

The Rationality of Psychology

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In accordance with the Thesis Regulations of the University of Edinburgh I declare that I alone am responsible for the design and the execution of the research on which this thesis is based.

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Abstract

A basic theme of this thesis is that different approaches to psychology are legitimated by different philosophies of science or wider philosophical perspectives. Several influential contemporary arguments about whether and how a scientific psychology ought to be pursued are first introduced. Next the attempt to reconstruct the recent history of the discipline in line with T.S. Kuhn's theory of science is critically discussed as is that theory itself. This leads to a case study of the influence of Logical Positivism on Behaviourism, illustrating the historical importance of philosophy of science for psychology. The case study also provides the background for the examination of Karl Popper's philosophy of science and its associated methodology. Historical objections to Popper's epistemology are then discussed in some detail, with special attention paid to Imre Lakatos's Methodology of Scientific Research Programmes. Popper's views are defended against these objections but substantially modified in response to normative criticisms by Nicholas Maxwell. The different approaches to psychology, already discussed, are then evaluated in the light of this modified Popperian philosophy of science. The mentalist programme, as articulated by Noam Chomsky, is given a qualified endorsement but the claim that it vindicates rationalist epistemology is rejected. Finally, in a more speculative vein, Popper's metaphysical theory of Worlds 1, 2 and 3 is examined in the light of various criticisms and an interactionist alternative to mentalism is given favourable consideration.

Chapter One

Approaches to a Scientific Psychology

The pioneers of rationalism inveighed against the traditional dogmas, ridiculed popular superstitions, campaigned against priests and sorcerers, and castigated them for fostering and preying upon the ignorance of the masses - hoping that a final victory of science would banish for ever the evils of unreason and organised deception. Little did they suspect that a Trojan Horse would appear in the camp of enlightenment, full of streamlined sorcerers clad in the latest paraphernalia of science.

Stanislaw Andreski in
Social Sciences as Sorcery¹

Introduction

Arguments over the aims and achievements of psychology are as diverse and polemical today as they have ever been. The consensus, such as it was, over the discipline's proper subject matter, problems, aims and methods has been steadily eroded during the past two decades by an increasing number of penetrating philosophical and methodological critiques. So fragmented has academic psychology become that there is even disagreement as to whether there is such a lack of consensus. We shall see, however, that conflicting answers are still given by professional psychologists to such basic questions as whether the subject is the science of behaviour or a science of mind or whether it can be a science at all.

We shall also see that some eminent and experienced researchers, disillusioned by what they see as chronic failure to progress, talk of a 'crisis in psychology'. If there is a crisis then it would seem

1 Andreski, S. (1974) p249

to have been there from the start, especially if constant disputes over fundamental issues are taken to mark its presence. Indeed it is now just a half century since Karl Buhler's famous work² with that title first appeared and from that time to this no psychological movement has ever had an exclusive hold on the allegiance of the discipline. Even when the behaviourist hegemony was firmly established in the Anglo-American world it was engaged in constant skirmishes with rival approaches, from psychoanalysis to Gestalt psychology. Now behaviourism, despised and denounced by its critics as a demeaning and pretentious pseudo-science, aggressively defended and promoted by its more loyal adherents in a vigorous, if vain, rearguard action has declined in influence to be but one perspective among many.

A major issue to be considered in this thesis is whether behaviourism or any other putative science of mind or behaviour, irrespective of its degree of support within contemporary psychology meets or can be so formulated as to comply with the minimal intellectual criteria demanded of any empirical science. Or, in other words, can a scientific psychology be a rational enterprise?

The search for an answer to this question will require an extensive investigation of the issues which dominate debate in contemporary philosophy of science, a discipline itself so tormented by internicine strife that some would even have it turn to psychology for help in settling its own fundamental difficulties. Twentieth century philosophy of science, we shall see in the next chapter, has very largely determined the course of modern psychology and it is from that field, I will argue, that the key to a resolution of current conflicts in psychology can be found. However, the attempt to find a satisfactory answer to the

2 Buhler, K. (1927)

above question is made doubly difficult by the failure of the philosophers of science to agree on a satisfactory notion of what exactly would constitute a rational enterprise.

To begin my enquiry I will now review what I take to be the most important arguments and counter-arguments within and about psychology that have appeared in recent debates over the status of the discipline. I shall be particularly interested in the answers which different theorists have given or might legitimately be expected to give to the following question: Should psychology, taken as a conscious attempt to apply the methods of natural science to problems of human experience and behaviour, now be abandoned as a naive if well-intentioned failure or can it be reconstructed as a viable, coherent and successful empirical science? In the interests of explication I shall consider the different standpoints in a sequence and only refer to criticisms each has made of the other where clarity demands. My intention is, at this stage, primarily to map out a field of operations or range of viewpoints which will then be studied more critically in later chapters after a comparative study of the different evaluative criteria which competing philosophies of science provide for the assessment of scientific theories. However, I will attempt in this survey to spell out tacit or explicit philosophical assumptions underlying these different standpoints; these assumptions- epistemological, linguistic, methodological - often play a very important role in psychological theories, not least among those whose proponents deny any philosophical debts whatsoever.

Behaviourism

Put at its simplest this view is that psychology is the science of behaviour. That is the form in which it was launched in 1913 by

John B. Watson and, depending on one's sympathies, it can be said to have undergone progressively more sophisticated and precise reformulations during the past sixty years or to have suffered from that ignominious fate of, what Flew³ (in another context) described in a celebrated phrase as, 'death by a thousand qualifications'.

Today, behaviourism is a term used to refer to several distinct philosophical doctrines and associated psychological positions. These usages reflect the historical development of the movement, especially the consequences of its intimate involvement with the Logical Positivism of the Vienna Circle and later, that philosophy's American descendant, Logical Empiricism. In a 'case study' illustrating the implications of the philosophy of science for theorising and research in psychology I will document, at some length, in the next chapter, the influence of these philosophies on behaviourism. Here I will attempt to explain what behaviourism signifies in current psychology.

In a recent essay, notable mainly for its high polemics, H.J. Eysenck⁴ distinguishes three usages of the term, two of which he considers relatively unimportant to the psychologist. The first, metaphysical behaviourism, is but a disguised version of old-style materialism which can be dismissed, he says, 'like so many other metaphysical problems ... for it ... seems pretty murky, insoluble and rather meaningless.'⁵ The second usage, analytic (or logical) behaviourism, says Eysenck, describes a 'very useful' way of solving a pseudo-problem like the existence of mind or consciousness by means of a linguistic purge. More interesting for my purposes is the third position namely, methodological behaviourism, a specifically psychological standpoint of which Eysenck wholeheartedly approves.

3 Flew, A.N. and MacIntyre, A. (1955)

4 Eysenck, H.J. (1972)

5 " " " p288

Mind, consciousness, mental events, private awareness and related notions cannot, on this view, be investigated by the methods of science. Because Eysenck's position, as he himself characterises it, has been directly attacked by contemporary cognitive psychologists it will be best to state his position in his own words.

'... we must agree with J.B. Watson and his many followers that our primary datum is behaviour, not consciousness, and that our laws must be laws about observable behaviour, not about non-observable states of consciousness. This does not imply (although some behaviourists write as if it did) that states of consciousness do not exist; as T.R. Miles puts it, 'the case of the methodological behaviourist is rather that if there are such things as minds or mental events they cannot as a matter of methodology be regarded as proper objects for scientific study'. Such a statement is subject to disproof and is hence a meaningful scientific statement; all you have to do if you do not agree with the statement and wish to disprove it, is to put forward a method which would render mental states suitable for scientific study.'⁶

Eysenck further claims that 'almost all psychologists are now behaviourists in the methodological sense.'⁷

It is scarcely surprising that Eysenck can make this further claim because, with the above formulation, he is able to characterise as methodological behaviourists a great variety of psychologists who would object to the view that they are behaviourists. The confusion arises from an ambiguity in the use of the terms 'mind' and 'mental events'.

The confusion can be dispelled if we anticipate some of the conclusions of the case study to follow. Behaviourism, as developed by Watson, was a science of behaviour advanced in opposition to the conception of psychology as the science of mind or consciousness. Watson, as is well known, attacked the idea of a science of mind - where that is taken to mean a science of conscious mental events - and the

⁶ Eysenck, H.J. (1972) p292

⁷ " " " " p297

associated introspective method. Watson was thus anti-mentalist.

Behaviourism as influenced by the logical positivists and logical empiricists however, was much more than an anti-mentalist doctrine. It became the representative in psychology of those philosophies of science and, as such, was wedded to a variety of principles about the status of theoretical terms, the nature of explanation, observation languages, confirmation procedures and other views dealt with in the case study. As it evolved this philosophically-supported behaviourism continually modified the initial Watsonian formulation and relaxed the anti-mentalist strictures. Mental terms were readmitted as legitimate provided they could be fully defined in terms of the (allegedly, objective) observation language. Later, mental terms were held to be only partially definable in this language and, later still, even that greatly modified position was abandoned so that it was no longer held that mental terms could be provided with logically necessary and sufficient criteria of application in the observation language.

To some extent the point at which behaviourism is so qualified and modified that it is something else is a matter of stipulation. I shall turn to that point in a moment. The issue at this point is that the 'mental terms' readmitted into what I call philosophically-supported behaviourism could, in principle, refer either to conscious or unconscious processes or events. The emphasis was on the nature of their 'logical connection' to the observable behaviour; behaviourism was thus permitted to include reference to the mental and even to conscious mental events provided that the theoretical terms which mediated that reference were used in conformity to the methodological principles of the day. During the 1940's and 1950's, behaviourists were much preoccupied with questions concerning these 'logical connections'. Thus their laws were not just about public observable

behaviour but were also about what Eysenck calls 'non-observable states of consciousness'⁸ - provided, and only provided, the empiricist criteria of significance for the use of such terms were so respected.

Methodological behaviourism, as defined by Eysenck (quoting Miles), most closely resembles Watsonian behaviourism in its anti-mentalism. When he says that 'minds or mental events cannot as a matter of methodology be regarded as proper objects for scientific study'⁹ he is objecting to a science of mind or consciousness, as conceived of before Watson. He does not, I take it, claim that minds or mental events cannot be studied in the sense in which they were readmitted into the philosophically-supported varieties of behaviourism. For each one of these varieties was just as opposed as Eysenck is to any attempt to construct a science of mind or consciousness per se, a science which flouts the empiricists' methodological demands. Thus methodological behaviourism is a doctrine that was held by all those behaviourists of the 1940's and 1950's who accepted the various conceptions of empirical significance for mental terms as introduced by the philosophers. But Eysenck's usage is confusing for his term equally applies to those psychologists who consider themselves anti-behaviourist by virtue of their rejection of such empiricist significance criteria but who do not wish to espouse a science of mind or mental events, where such expressions are taken to be synonymous with the traditional conception of a science of consciousness. Modern mentalists, we shall see later in this chapter, steer a middle course between adoption of what they take to be an empirically indisciplined science of consciousness and commitment to recent empiricist constraints on psychological theories. And since they thus oppose what they correctly take to be behaviourist methodology they would, appropriately, reject the suggestion that they

8 Eysenck, H.J. (1972) p292

9 " " " "

are 'methodological behaviourists'. In conclusion, then, I reject this usage of 'methodological behaviourism' for it fails to express the doctrines that have characterised behaviourism during the past three decades and because it is confusing in that it embraces psychologists who are, if they are anything, anti-behaviourists.

Judged in terms of the number of its adherents the most important version of contemporary behaviourism is, perhaps, that of B.F. Skinner. This is something of an anomaly in that it is based on a once-fashionable philosophy of science now long abandoned by its originators namely, operationism. 'Behaviourism has been', writes Skinner with approval, '(at least to behaviourists) nothing more than a thorough-going operational analysis of traditional mentalistic concepts.'¹⁰ Skinner is equally opposed to attempts to explain behaviour by reference either to physiology or to mental events, conscious or unconscious. He is particularly hostile to such allegedly unscientific approaches as that of Karl Popper, inherent in such statements as:

'What we want is to understand how such non-physical things as purposes, deliberations, plans, decisions, theories, tensions and values can play a part in bringing about physical changes in the physical world.'¹¹

The history of science teaches us, Skinner suggests, that such seemingly reasonable 'common sense' approaches to the problems of human behaviour will prove a dead end. He writes:

'We can follow the path taken by physics and biology by turning directly to the relation between behaviour and the environment and neglecting supposed mediating states of mind. Physics did not advance by looking more closely at the jubilation of a falling body, or biology by looking at the nature of vital spirits, and we do not need to try to discover what personalities,

10 Skinner, B.F. (1945) p271

11 Popper, K.R. (1972) p229

'states of mind, feelings, traits of character, plans, purposes, intentions or the other perquisites of autonomous man really are in order to get on with a scientific analysis of behaviour.' 12

In fact this scientific analysis of behaviour is, he says, well established and there is an urgent need to press ahead, to probe further the nature of environmental control of behaviour. Radical behaviourism is, in the words of two of its contemporary advocates, Boakes and Halliday¹³, 'by far the most promising approach to the understanding of behaviour'. To the question whether psychology can be reconstructed as a viable, coherent and successful empirical science they would, I presume, reply that a reconstruction is unnecessary for it is these things already.

Just as radical behaviourism reflected operationist standards so weaker versions of behaviourism were developed in response to modifications of those standards. So, for example, when logical empiricism was refined to require merely that theoretical terms be such that logically necessary and sufficient conditions of application be specified in observation language a parallel situation arose in psychology: mental terms were held to be legitimate entities in behaviourist theories provided that logically necessary and sufficient conditions of application were specified in terms of overt behaviour. Before its eventual demise logical empiricism underwent continual attenuation which, in turn, gave rise to numerous versions of weak behaviourism. It is, I have said, partly a stipulative affair when we can correctly say behaviourism is so qualified that it is not behaviourism any longer.

Fodor¹⁴ has suggested, after noting the variety of these weak

12 Skinner, B.F. (1973) p20

13 Boakes, R.A. and Halliday, M.S. (1970) p373

14 Fodor, J.A. (1968)

behaviourist formulations, that we employ the following criterion:

'To qualify as a behaviourist in the broad sense of that term that I shall employ, one need only believe that the following proposition expresses a necessary truth: For each mental predicate that can be employed in a psychological explanation, there must be at least one description of behaviour to which it bears a logical connection. I shall henceforth refer to this proposition as P.' 15

I intend to use Fodor's definition of a minimal behaviourism because, unlike Eysenck's usage of methodological behaviourism, it respects important distinctions between those psychologists who call themselves behaviourists and those who do not. Fodor himself, we shall see later in this chapter, opposes a traditional science of mind in the sense of a science of consciousness yet does not think that a sufficient reason to merit being labelled a behaviourist. For he also opposes even the very weak behaviourist requirement that each mental event be logically connected to a description of behaviour. Another reason for accepting this definition is that when the terms 'logical connection' and 'description of behaviour' are interpreted in different ways it enables us to account for the heterogeneity and complexity of historical behaviourism.

Finally, the epistemological and methodological sources of support for behaviourism will be discussed in the case study in the next chapter.

Apostasy

One of psychology's most virulent critics during the past decade has been the distinguished American ex-behaviourist Sigmund Koch. His disillusionment with the possibility of a successful scientific psychology, or as he calls it his 'apostasy'¹⁶, set in after he produced

15 Fodor, J.A. (1968) p51

16 Koch, S. (1974) p3

a systematic critique of the work of Clark Hull.¹⁷ Though Koch now boasts that that critique 'is probably the most mercilessly sustained analysis of a psychological theory on record'¹⁸ he also thinks that critical analysis is unlikely to persuade psychologists to abandon their efforts. Hence, the violence of his apostasy has culminated in a fierce polemic in which he suggests that psychology students are the victims of a cruel confidence trick when they are informed that they are studying a coherent discipline or set of specialties. He writes:

'Whether as a 'science' or any kind of coherent discipline devoted to the empirical study of man psychology has been misconceived. Though a massive hundred-year effort to erect a discipline given to the positive study of man has here and there turned up a germane fact or thrown off a spark of insight, these 'victories' have had an adventitious relation to the programmes believed to inspire them, and their sum total over time is overwhelmingly counterbalanced by the harvest of pseudo-knowledge that has now been reaped.'¹⁹

'Many legitimate and important domains of psychological study, then, can be called 'science' in no significant sense and continued application of this misleading metaphor can only vitiate, distort or pervert research effort ... I am saying that in fields as close to the heart of psychological studies as perception, cognition, motivation and learning, and certainly social psychology, psychopathology, and personology, and, of course, aesthetics, the study of 'creativity' and the empirical study of phenomena relevant to the domains of the extant humanities - that in all these areas such concepts as 'law', 'experiment', 'measurement', 'variable', 'control', 'theory', do not behave sufficiently like their homonyms in the established sciences to justify their extension to them of the term 'science'.²⁰

Alas, Koch here fails to provide arguments which would justify his conclusions. He does not illustrate how psychology - which he

- 17 Koch, S. (1954)
18 " " (1974) p4
19 " " " p6
20 " " " p25

seems to identify with behaviourism in its various forms - fails to meet the canons of scientific method as they are applied in the established natural sciences. And apart from a passing reference to Michael Polanyi's philosophy of science Koch's own views on the nature of science remain a mystery. Thus, despite Koch's eminence and despite the possibility, which I am prepared to believe, that he is only one among many professional psychologists disenchanted with what they take to be the sterility of behaviourism I shall move on to consider some other long-standing opponents of psychology whose arguments are, at least, a little more explicit.

A Priorism

The idea of a scientific psychology is, according to some philosophers simply a contradiction in terms. If it is psychology it is not science; if it is science it is not psychology. Thus it is only to be expected, they argue, that behaviourism in all its forms and all other efforts to construct a science of psychology will prove dismal failures. For they will all be exercises in conceptual confusion. Wittgenstein has inspired many such a priorist attacks on psychology and other social sciences. Though he did not himself offer a formal argument against the possibility of a science of psychology he did write, at the very end of his Philosophical Investigations:

'The confusion and barrenness of psychology is not to be explained by calling it a 'young science'; its state is not comparable with that of physics, for instance in its beginnings. (Rather with that of certain branches of mathematics. Set Theory) For in psychology there are experimental methods and conceptual confusions.' 21

Fortunately we have a better idea of what Wittgenstein intended thanks

21 Wittgenstein, L (1958) p232

to the work of Winch in his The Idea of a Social Science.²² Though, as its title suggests, Winch was not primarily concerned with psychology much of his case would, if successful, prove very damaging to the credibility of any science of psychology.

The basic mistake which social scientists make, he says, is that they assume that all explanations have the same logical form. That is to say they assume that the principles of explanation which hold for the natural sciences can be applied, simpliciter, to the human social realm. So psychologists, for example, will recognise that human beings are highly complex organisms and will consequently devise complicated experiments to probe the underlying laws of behaviour. But they would insist, he argues, that the recalcitrant issues involved are merely empirical. Failure to find the presumed and desired laws is taken to reflect lack of ingenuity and calls for more sophisticated research or intensification of effort. Only time will tell, psychologists believe, how successful their efforts are likely to be.

Such an approach is doomed from the outset, says Winch, for the issues involved are not empirical at all; they are, he argues, conceptual. And he writes:

'It is not a question of what empirical research may show to be the case but of what philosophical analysis reveals about what it makes sense to say. I want to show that the notion of a human society involves a scheme of concepts which is logically incompatible with the kinds of explanation offered in the natural sciences.'²³

As members of a human society individual human beings cannot be understood in terms of the concepts of natural science. Though human beings are more complex than other organisms they are not just more complex. He adds:

22 Winch, P. (1958)

23 " " " " p72

'For what is from one point of view, a change in the degree of complexity is, from another point of view, a difference in kind: the concepts which we apply to the more complex behaviour are logically different from those we apply to the less complex.' 24

Now though his thesis is largely devoted to showing that human social action is qualitatively different from events in the natural world it appears to hold equally for our understanding of animal actions. For Winch asks us to consider a cat 'writhing in pain'.²⁵ If we were to describe the cat's reactions, his very complex gyrations in purely mechanical terms, using a set of space-time co-ordinates - much as we would describe the motion of a stone hurtling through the air - then we would have described what was going on just as much as the statement that the cat was 'writhing in pain'. Winch continues:

'But the one statement could not be substituted for the other. The statement which includes the concept of writhing says something which no statement of the other sort could approximate to. The concept of writhing belongs to a quite different framework from that of the concept of movement in terms of space-time co-ordinates; and it is the former rather than the latter which is appropriate to the conception of the cat as an animate creature. Anyone who thought that a study of the mechanics of the movement of animate creatures would throw light on the concept of animate life would be the victim of a conceptual misunderstanding.' 26

Winch's argument rests on the alleged differences between the principles of explanation in the natural sciences and those appropriate to understanding social action. If it could be shown that his view of how the natural sciences work - how they explain physical events - is untenable, or that his account of what is involved in understanding social life is flawed, or both, then his thesis would be correspondingly weakened or undermined.

24 Winch, P. (1958) p72
25 " " " p74
26 " " " "

It is interesting that Winch develops his case against a natural science of psychology in opposition to the views of John Stuart Mill. He approaches his opponents in this way, he says, because Mill's position 'underlies the pronouncements of a large proportion of contemporary social scientists'²⁷ and because 'some rather more sophisticated interpretations of the social studies as sciences ... can be best understood as attempts to remedy some of the more obvious defects in Mill's position.'²⁸ What is revealing about this is that Winch is attacking the application of a positivist conception of natural science to human behaviour.²⁹ And he suggests that the only alternative to such a conception is the neo-Wittgensteinian analysis of social life which he himself advocates.

Thus, for example, in criticising Mill's approach to what he called the 'moral sciences' Winch notes the attempt to make motive explanations a species of causal explanation. And he adds:

'The conception he (Mill) wishes to advocate, though he is not very explicit, seems to be something like this. A motive is a specific mental occurrence (in a Cartesian sense of 'mental' implying that it belongs wholly to the realm of consciousness) ... what we can do, Mill argues, is to study the causal relation between motives, considered as purely conscious events and the actions to which they give rise.'³⁰

A contemporary positivist version of Mill's position is found, says Winch, in that of the social psychologist T.M. Newcombe. For Newcombe, as for Mill, motives are a species of causal explanation. However, Newcombe differs from Mill in his further claim that these motives can be identified with physiological states of the organism. (Mill, says Winch, regards that claim as non-proven.) Newcombe is forced to his radical reductionist conclusion because, Winch argues, the only apparent

27 Winch, P. (1958) p66

28 " " " "

29 Positivism is briefly examined in the case study, in Chapter Two

30 Winch, P. (1958) p79

alternatives seem unacceptable - namely, that a motive is either a 'figment of the imagination' or that it is but a 'synonym' for the very behaviour it is alleged to cause.

Both Mill and Newcombe misunderstand the role of motives in social life, according to Winch. And he implies that such misunderstanding is a consequence of their attempt to apply the principles of natural science to the problems involved in explaining human behaviour. Some psychologists deny, we shall see in a moment, that we must accept either of the alternatives Winch presents. They deny, in other words, that we must agree with either Mill's or Newcombe's analysis on the one hand, or that we need accept Winch's own positive arguments on the other. In short, they argue that it is possible to implement a coherent scientific psychology and doing that entails the rejection of these spurious alternatives.

It involves rejection, first, of the claim that a science of psychology is required, for example, to describe the behaviour of the writhing cat in terms of movement in space and time. This demand is seen to be the consequence of a quite unnecessary and, it is argued, indefensible empiricist account of science. Part of this account tries to isolate the 'inductive risk' allegedly involved in all scientific theorising from the observation language which is, on this account, the base on which theory is supposedly grounded. Hence, an attempt is made to purge the so-called basic observation language of theory, to make it somehow neutral, obvious and indubitably fundamental. Different psychologists recommend various alternatives to this empiricist formulation, as we shall see. The point I wish to make here is that psychological theories and theories about psychology are very often tied up with tacit, but very potent, ideas about what a scientific theory must look like. Winch seems to assume, or so it appears from

his examples, that a scientific theory is limited to an observation language that meets such empiricist demands. Such an assumption is unnecessary, some argue; indeed they say it inhibits the emergence of truly explanatory theories. So, in effect, they are saying that it is Winch, in his equation of science with the positivist interpretation of science, who is himself the victim of a conceptual misunderstanding.

And it is that misunderstanding, they continue, which leads him to claim that his own neo-Wittgensteinian philosophy is the only acceptable alternative which the social scientist possesses. An adequate treatment of this philosophy would require a thesis in itself so I shall here merely sketch Winch's positive argument. He begins with the espousal of a form of the famous Wittgensteinian objection to the possibility of a private language, an objection based on the view that language use is a special case of rule-following behaviour. Learning the definition of a word, learning to use the word correctly is, he argues, the equivalent of learning a rule. A crucial move in the argument, a move which numerous philosophers have criticised, is taken by Winch when he writes:

' ... the notion of following a rule is logically inseparable from the notion of making a mistake. If it is possible to say of someone that he is following a rule that means that one can ask whether he is doing what he does correctly or not. Otherwise there is no foothold in his behaviour in which the notion of a rule can take a grip; there is then no sense in describing his behaviour in that way, since everything he does is as good as anything else he might do, whereas the point of the concept of a rule is that it should enable us to evaluate what is being done.' 31

Next, Winch generalises this familiar argument from its employment in the attempted elucidation of language use to show, or try to show, how

31 Winch, P. (1958) p32

it can illuminate the nature of human action. Just as learning the definition of a word is to learn a rule governing its future use so, he says, 'action with a sense ... commits the agent to behaving in one way rather than another in the future'.³² To illustrate the analogy Winch asks us to consider a person placing a slip of paper between the pages of a book. The person can be said to be 'using a bookmark', says Winch, only if he acts with the idea of using the slip to determine where he shall start re-reading. He then adds:

'The notion of being committed by what I do now to doing something else in the future is identical in form with the connection between a definition and the subsequent use of the word defined ... It follows that I can only be committed in the future by what I do now if my present act is the application of a rule. Now ... this is possible only where the act in question has a relation to a social context: this must be true even of the most private acts, if, that is, they are meaningful.'³³

All meaningful behaviour is social, he concludes, since it can be meaningful only if governed by rules - and rules presuppose a social setting.

Assuming then that the behaviour of human beings is typically meaningful behaviour Winch proceeds to argue that the kinds of explanation found in natural science are 'logically incompatible' with those appropriate to understanding human action. He continues by noting that a regularity or uniformity is the constant recurrence of the same kind of event on the same kind of occasion. Statements of uniformities presuppose, he says, judgements of identity. And this is where he attempts to drive an impenetrable wedge between the 'logics' of natural and social science. For a natural scientist can identify successive events as the same or different merely by reference to

32 Winch, P. (1958) p32

33 " " " p50

criteria of identity provided by the natural science involved, say physics. The social scientist, in contrast, must recognise that the criteria of identity which his science provides are secondary to those which govern the actions of the agents being studied. The social scientist cannot identify two successive social events as 'the same', whether they be strikes, baptisms, auctions or whatever, merely by appealing to the criteria of his social science. The criteria which the participants employ in their social life take priority. If an observer ignores what the actors conceive themselves to be doing, says Winch, then he is no longer a social scientist. For the events he is studying have been stripped of their social character. To illustrate the point Winch considers the parable of the Pharisee and the Publican and argues:

' ... the appropriate criteria for deciding whether the actions of these two men were of the same kind or not belong to religion itself.' 34

But if an observer eschews a positivist orientation and instead investigates the theories, ideas, propositions etc which the actors-being-studied actually employ then, Winch says, he is no longer a social scientist. He writes:

' ... what the sociological observer has presented to his senses is not at all people holding certain theories, believing in certain propositions, but people making certain movements and sounds. Indeed, even describing them as 'people' really goes too far, which may explain the popularity of the sociological and social psychological jargon word 'organism': but organisms, as opposed to people, do not believe propositions or embrace theories. To describe what is observed by the sociologist in terms of notions like 'proposition' and 'theory' is already to have taken the decision to apply a set of concepts incompatible with the 'external', 'experimental' point of view. To refuse to describe what is observed in such terms, on the other hand, involves not treating it as having social significance. It

'follows that the understanding of society cannot be observational and experimental in one widely accepted sense.' 35

The study of social life, he concludes, is more properly the preserve of the philosopher where that is taken to mean elucidation of 'forms of life'.

Winch is led to this conclusion by his own analysis of the relation between philosophy and, in this case, science. Philosophy, à la Wittgenstein, leaves everything as it is. It is, says Winch, a peculiarly uncommitted form of enquiry in contrast to science or religion, for example, which are each committed forms of enquiry. Science is but one way of making the world intelligible; religion is another. Each has criteria of intelligibility or criteria of logic peculiar to itself. 'Criteria of logic', says Winch, 'are not a direct gift of God, but arise out of, and are only intelligible in the context of, ways of living or modes of social life.'³⁶ A scientist is committed to the criteria of logic peculiar to science by virtue of his immersion in a scientific community.³⁷ Since criteria of logic are only intelligible in the context of a way of life it follows, for Winch, that they cannot be applied to modes of social life as such.

Given his commitment to the criteria of one way of life it is disastrous for the scientist, he argues, to try to use his criteria of logic 'in the investigation of a human society, whose very nature is to consist in different and competing ways of life, each offering a different account of the intelligibility of things.'³⁸ To do this is

35 Winch, P. (1958) p110

36 " " " p100

37 The close similarity between this Wittgensteinian view of the nature of science and the more recent, influential account of Thomas Kuhn (1962) will be discussed in Chapter Two.

38 Winch, P. (1958) p103

to misunderstand the nature of science. No, such is properly the job of philosophy; its *raison d'être* is the elucidation and comparison of the ways in which different intellectual disciplines, different 'forms of life' make the world intelligible. Part of this job is, says Winch, to be 'alert to deflate the pretensions of any form of enquiry to enshrine the essence of intelligibility as such, to possess the key to reality'.³⁹ Part of the philosopher's job, then, is to prevent the social scientist imposing the criteria of logic which science recognises on other 'forms of life' and judging their own criteria against the standards of science. Where the scientist is committed to seeing his criteria of logic triumph by making everything intelligible to science, the philosopher, on the other hand, is uncommitted in that he has no axe to grind for one form of life rather than another. The philosopher recognises that 'intelligibility takes many and varied forms'⁴⁰ and consequently must remind the scientist that his form of life is on a par with its competitors. The philosopher, on this view, embraces and enforces a total relativism. Finally, given that even the apparently most 'private' human activity is, as Winch has argued, supposedly parasitic on a social context the general argument against a social science applies with undiminished force to any putative science of psychology. It is simply a conceptual blunder, on this view, to apply the 'criteria of logic' appropriate to science to the 'forms of life' which constitute human activity. Hence, a scientific psychology is held to be, in plain language, sheer nonsense.

Since what is actual is possible the best reply to Winch is to point to a coherent, flourishing science of psychology that avoids the dilemma he presented. Some psychologists do just that, I have suggested,

39 Winch, P. (1958) p103
40 " " " p102

rejecting both the positivist view of science which leads to describing behaviour in terms of movement and the alleged alternative, namely Winch's neo-Wittgensteinian view. They agree that it is important to discover the rules, intentions, plans, criteria etc which agents use in their private and social behaviour; and they agree among themselves, at least, that descriptions of behaviour must make reference to the mental state which that behaviour is meant to express. But they deny that in so doing they are forced to violate legitimate canons of scientific methodology. Winch's argument is, they imply, a non-sequitur for he has only argued against a positivist-behaviourist approach to psychology. What is of value in Winch's case can be incorporated, they suggest, in a non-positivist psychology.

In conclusion, I want, again, to draw attention to the fact that arguments within and about psychology are closely associated with philosophies of science or, in this case, a more general philosophy. These philosophies dictate the criteria which are used to settle such questions as whether psychology is possible and, if possible, how it should be carried on. Hence, I suggest, it is essential to debate the merits of such criteria in any discussion of the status of psychology.

Mentalism

Associated in particular with the pioneering work of Noam Chomsky mentalism, also known as 'New Mentalism' or 'New Rationalism', is now generally used to characterise the activities of the influential psycholinguistic and cognitive psychology movements. The principles of the mentalist movement in psychology have been clearly articulated by Fodor⁴¹, whose criterion for defining a minimal behaviourism I have already discussed. In fact, mentalism is defined by Fodor as the

41 Fodor, J.A. (1968)

denial of proposition P. (See Page 10) In other words, the mentalist denies what a minimal behaviourist asserts, namely that for each mental predicate employed in a psychological theory there is at least one description of behaviour to which it bears a logical connection.⁴²

Mentalism is to be sharply distinguished from traditional dualism which asserts that mind and matter are substances of different logical kinds. Mental predicates apply, according to a dualist, to an ontologically distinct substance called mind which is irreducibly different from the substance matter to which behavioural and/or physical predicates apply.

Dualists are unlikely to be and in some cases cannot be - as a matter of logic - behaviourists. Hence, they are likely to be mentalists. But there is no reason why mentalists are likely to be dualists, for mentalism is compatible with either dualism or traditional materialism. This requires some clarification for there is considerable and continuing confusion over these issues. A.R. Luria, for instance, opposes Chomsky's research programme because '17th century philosophy, with its dualistic approach is more appealing to him (Chomsky) than objective, materialist epistemology with its socio-historical searchings for data.'⁴³ It is simply a mistake, I believe and will later argue, to view Chomsky as a Cartesian.

Dualists are unlikely behaviourists because anyone who holds that mental and behavioural predicates apply to logically distinct kinds of substance will probably not envisage a logical connection between the distinct kinds of predicates. Dualism is logically incompatible with certain kinds of behaviourism - for example, radical behaviourism. It is logically impossible to hold both that mental and

42 Fodor, J.A. (1968) p55

43 Luria, A.R. (1976) p380

behavioural predicates apply to different kinds of substance and at the same time claim that the mental can be eliminated in favour of behavioural terms.

Mentalists, however, need not be dualists for it is quite reasonable to hold that mental and behavioural predicates are not logically connected and yet hold that they apply to substances of the same kind. Thus mentalists might be dualists; but they equally well might be materialists. And in fact the leading mentalists in contemporary psychology and linguistics do subscribe to materialism in this sense. They subscribe to some version of the Identity Theory, the doctrine that mind and brain states are contingently identical.

Mentalists, then, are generally committed to physicalism in that they hold each mental event contingently identical to a brain or neural event. The cognitive psychologist, on this view, postulates abstract mechanisms which are held to mediate the production of the behaviour to be explained. Now, in opposition to the behaviourist, the mentalist recognises that these postulated mechanisms are often very complex and thus cannot be 'tied' to observable behaviour in any simple, direct way. That is not to say that such mechanisms are not empirically constrained; the mentalist simply asserts that there is no reason why in constructing his theories the psychologist should shackle himself with further empirical constraints than does, say, the physicist. The mechanisms which are held to determine a person's behaviour are, he says, much too abstract to have behavioural criteria of application for each postulated entity. Even the 'weak behaviourist' demand that each mental term be logically connected to behavioural terms is, says Fodor, a much too severe constraint. He writes:

' ... we may regard a commitment to behaviourism as involving a speculation about the complexity of those phenomena that psychological explanations

'will have to account for ... if it is possible to demonstrate the occurrence of psychological phenomena for which the simplest available explanation requires us to hypothesize the occurrence of mental events that do not exhibit behavioural correlates, then, since even the weakest variety of behaviourism requires at least that such correlates exist for each type of mental event, we shall be in a situation of forced choice. In particular, we shall be required either to abandon the explanation or else to abandon the methodological principle that forbids explanation of that type.' 44

The methodological principle in question is, says Fodor, but a consequence of the discredited operationalist account of scientific theories. Hence it should be abandoned and the explanation retained.

In embracing physicalism the mentalists are admitting that psychological theories are, in the last resort, about physical systems. But this does not in any way commit them to the view that the laws of psychology, whatever they may be, will in the end be reducible to the laws of physics. Physics is the basic science in the sense that psychology is about physical systems but that does not licence the classical reductionist inference, associated with the 'Unity of Science' movement stemming from the Vienna Circle, that psychology will eventually be replaced by an ideal physics. The traditional reductionist programme, which views the sciences as a hierarchical system resting on physics, is - say the mentalists - not to be defended on ontological grounds. If physicalism is true it provides no support for the classical reductionist aim of reducing psychology to physics.

Psychological or classical or radical reductionism is defined by Fodor as the conjunction of physicalism (the claim that every psychological event is a physical event) and 'the assumption that there are natural kind predicates in an ideally completed physics which correspond to each natural kind predicate in an ideally completed

44 Fodor, J. (1968) p79; Fodor, J. (1975) p1/2

special science' (eg psychology).⁴⁵ In short, the claim is that every kind is or is co-extensive with a physical kind. Since reductionism, thus defined, is an empirical claim it cannot be dismissed a priori but there are, says Fodor, three good reasons why it seems highly unlikely to be true.

The first reason is that 'interesting generalisations (eg counterfactual supporting generalisations)',⁴⁶ can often be made about events with very different physical descriptions. Second, he says, whether the physical descriptions of the events described by such generalisations have anything in common is very often irrelevant; and third, 'the special sciences are very much in the business of formulating generalisations of this kind'.⁴⁷ To illustrate the point he asks us to consider that a law of the special science of economics is true - namely, Gresham's Law which deals with monetary exchanges. Fodor says he will accept that every event which consists of a 'monetary exchange' has some true physical description - eg an ounce of gold moved from A to B - but that any physical description of all such monetary exchanges would be 'wildly disjunctive'.⁴⁸ It is a logical possibility, he admits, that a kind like 'monetary exchange' could turn out to be co-extensive with a physical kind but it would be 'an accident on a cosmic scale'.⁴⁹ Similarly, while he assumes that for each psychological

45 Fodor, J. (1975) p13. To explain his usage of 'natural kind' Fodor notes that every science employs a taxonomy consisting of theoretical and observational predicates with which it classifies events; events fall under the laws of a science by virtue of satisfying its predicates. Now, not every true description of an event, he says, falls under a scientific law; predicates which express such true descriptions, he says, are not natural kind predicates. However, he adds: '... roughly, the kind predicates of a science are the ones whose terms are the bound variables in its proper laws. I am inclined to say this even in my present state of ignorance, accepting the consequence that it makes the murky notion of a kind viciously dependent on the equally murky notions of law and theory. There is no firm footing here.' p14

46 Fodor, J. (1975) p15

47 " " " "

48 " " " "

49 " " " "

event there is some corresponding neurological event it is also, in his view, highly improbable that psychological kinds could turn out to be co-extensive with neurological kinds.

Fodor's case should be distinguished from that of another leading mentalist and cognitive psychologist, N.S. Sutherland. In his paper Is the Brain a Physical System?⁵⁰ he begins by stressing that even if the brain is a physical system the psychologist may need to use new concepts and principles in explaining behaviour; this is analogous to the procedures adopted in the physical sciences, he says, where new concepts and principles were once employed to explain, for example, the behaviour of the thermostat, of a gas or of the stars. He continues:

'These three systems are, however, all mechanistic systems and the principles used in explaining the behaviour of the whole system can be inferred from a knowledge of the laws governing the component parts of the system together with a knowledge of how these component parts are organised. Our claim is not that the explanation of behaviour does not involve new principles but only that if the brain is a physical system then these new principles are reducible to the laws governing the behaviour of elementary particles in the same way that the laws governing the molar behaviour of a gas are reducible to laws governing the behaviour of the molecules that compose it.' 51

It presumably follows from this that the brain is not a physical system if the new principles are not reducible to the laws of physics in the same way as the gas laws were found reducible to the laws governing the behaviour of the molecules of which the gas is composed. In taking this line it is likely that Sutherland is going to have to admit that the brain is not a physical system. I say likely because only empirical evidence could decide this issue. It had to be empirically discovered that the gas laws were in fact reducible to

50 Sutherland, N.S. (1970)
51 " " " " p97/8

molecular laws; it was a logical possibility that they were not so reducible. Similarly, it will have to be discovered whether psychological laws can be reduced in the same way to molecular laws; it is a logical possibility that they cannot be. Indeed, according to Fodor as we have seen, it is highly improbable that such a reduction will ever take place. If Fodor is correct Sutherland will have to infer that the brain is not a physical system. Fodor, however, may in that event continue to make the physicalist claim because that claim is, for him, independent of possible future reductions. Reductionism would be a sufficient but it is not a necessary condition, in his view, for the truth of physicalism.

An account of the behaviour of an organism is greatly impoverished, mentalists say, if that behaviour is described in terms of movement. For it loses sight of the fact that the sequence of behaviour is significant in so far as it is the expression of some mental state. When a man writes his name, sings a song or plays football he is not just moving his fingers, his vocal chords or his feet. Any psychological theory which attempts to explain what is going on will have to determine the intention, plan, rule or concept that the person meant his behaviour to satisfy. To this extent the mentalist endorses Winch's objection to the behaviourist's descriptions in terms of movement. However, by breaking free of logical empiricist constraints, the mentalist proceeds to specify the concepts that the person (or organism) possesses and tries to discover the mechanisms whereby those concepts are put to use.

Mentalists decline to fight on the ground defended by earlier generations of mentalists; they do not attempt to make conscious mental states the subject matter of psychology nor do they plead for

special epistemological status for the associated method of introspection. In fact they recognise that the mental processes which underlie behaviour are generally as mysterious to the person employing them as they are to the experimenter.

Perhaps the closest intellectual predecessors of contemporary mentalists were the Gestaltists who emphasised the distinction, among others, between the proximal and the distal stimulus. To explain a person's behaviour, they argued, it is necessary to determine what properties he takes any physical stimulus to have. Conversely, to find out what the behaviour means it is necessary, mentalists emphasise, to find out what description it was intended to satisfy. Mentalists differ from Gestaltists, however, in that they have developed much more precise and sophisticated theories with which to specify the nature of the representations or descriptions with which an organism categorises his experience; conversely, they use the same theories to specify the descriptions behavioural output is intended to fall under. Theoretical linguistics, especially transformational generative grammar, has made an enormous contribution to the mentalists' efforts to achieve their goals, by providing an account of sentence structure. This account, some claim, provides a systematic and detailed characterisation of the categories or concepts a person uses in the perception and production of language. Psycholinguistics will be, if successful, a model for all cognitive psychology - or so some mentalists argue.

Cognitive theories in general treat psychological phenomena - perception, decision making, concept learning etc - as computational processes taking place in a representational system or, as Fodor calls it, a 'language of thought'.⁵² As I intend to examine contemporary

52 Fodor, J. (1975)

mentalism at some length in Chapter Five I will, in conclusion, note here just one further aspect of this approach.

Cognitive psychology, by analogy with computer processes, views mental activity as a series of transformations of information carried out in the representational system. In doing so it cuts across distinctions found both necessary and important in common sense and in disciplines other than psychology. As I have noted already, it does not insist on making conscious events the subject matter of investigation. Instead, it recognises that some of the mental states it characterises are conscious while others are not. Nor does mentalism insist on distinctions between what happens to an organism and what the organism does - though it recognises that such distinctions may be very important in, say, a court of law. Fodor writes:

'... the ordinary distinction between what the organism does, knows, thinks and dreams, and what happens to and in its nervous system, does not seem to be frightfully important. The natural kinds, for purposes of theory construction, appear to include some things that the organism does, some things that happen in the nervous system of the organism, and some things that happen in its environment. It is simply no good for philosophers to urge that, since this sort of theory does not draw the usual distinctions, the theory must be a muddle. It cannot be an objection to a theory that there are some distinctions it does not make; if it were, it would be an objection to every theory.'⁵³

Hence the mentalist may agree that the philosophical distinction between actions and happenings merits the vast literature on the topic but as far as he is concerned the distinction is not a psychological one.

Interactionism

Interactionists hold, at least, that private conscious mental

53 Fodor, J. (1975) p53

states exist and causally determine behaviour. It is the business of any science of psychology, they argue, to discover generalisations relating such mental events to behaviour.

Interactionists do not really form a unified position for, the above claims apart, there is considerable variation in the range of further theses they adopt. Some, like Beloff⁵⁴, are dualists in that they award distinct ontological status to both mind and matter. Others are pluralists like Popper⁵⁵ who confers full ontological credentials not just on matter and mind but also on theories, poems, symphonies and other products of the human mind. There are interactionists like Whiteley⁵⁶ who recognise that consciousness is an ineliminable feature of the human mind, yet eschew the label 'dualist' on the assumption that it implies that a study of mind can form no part of science. However, if we drop that assumption there is no reason why Whiteley should not be co-opted to the dualist club. At any rate he is an interactionist whose arguments illustrate how this position contrasts with the other standpoints considered so far.

First of all, Whiteley is concerned to defend the idea of a science of conscious mind against the Wittgensteinian private language argument, a form of which we have already seen employed by Winch. Wittgenstein attacked the idea of a sense datum language, a language which could be held to describe an individual's private experiences. The first part of this argument equates learning the definition of a term, learning to use the term correctly with the learning of a rule. If there are rules there must, the argument continues, be a difference between using them correctly and incorrectly. Now, in a public language there are public rules and conventions governing the use of

54 Beloff, J. (1962), (1976)

55 Popper, K.R. (1972)

56 Whiteley, C.H. (1973)

the terms in the language. These rules and conventions enable one person to check whether or not another person has learned to use a term correctly. If, standing in Princes Street in Edinburgh I point to the Scott Monument and say 'Waverley Station', I could obviously be said to have made a mistake. I would not be using the rule governing the use of 'Waverley Station' correctly.

There can be no private language, according to Wittgenstein, because, in contrast to the above situation, there is no way by which another person could ensure that I was employing a term correctly when I used it to refer to a private experience. Hence, it is alleged, there is no difference between using the term correctly and using it incorrectly. But if that is the case it follows that there is no sense in saying that I am using a rule at all. Nor, therefore, is there any sense in saying I am using private rules in a private language.

The gist of Whiteley's reply to this argument is that it is a non-sequitur. To begin, he acknowledges that Wittgenstein's account of language use as a special case of rule-following constitutes an advance on previous behaviourist theories of language. However, the fact that there are no public rules or conventions which can be used to check that I am using a term correctly when I use it to describe a private mental experience only means that there is no way by which another person can know for certain whether the term is being correctly applied. The absence of public rules and conventions does not licence the inference that there is in fact no difference for me between using the term correctly and using it incorrectly. Hence, says Whiteley, it does not follow that there is no sense in saying I am using a private rule. Nor does it follow that I cannot be using a private language.

Whiteley identifies the flaw in Wittgenstein's argument by considering the famous dictum: 'An 'inner process' stands in need of

outward criteria.'⁵⁷ The word 'criteria' can be used in two different ways, he says. It can mean either 'the necessary and sufficient conditions for the correct application of a descriptive term'⁵⁸ or 'the evidence or clues which may be relied on to assure us that the term is being correctly applied.'⁵⁹ The trouble arises when these two senses of 'criteria', which often amount to very different things, are confused. In particular, it is a serious error, says Whiteley, to argue that the necessary and sufficient conditions for the correct application of a term like, say, 'pain' are or include publicly observable behaviour. The necessary and sufficient conditions for the correct use of the word 'pain', he says, are simply that the person concerned be aware of this private experience. There may or there may not be observable 'pain-behaviour' which would constitute evidence that the term is being correctly applied. Such evidence is a necessary condition for checking my usage of the term but it is not necessary for such usage. Says Whiteley:

'The confounding of these two sorts of criteria is a relic of the verification principle, which the later Wittgenstein and his followers do not explicitly accept and may explicitly repudiate, but often employ tacitly without acknowledgement. I think it is the main source of the persistent tendency towards behaviourism in contemporary analyses of mental terms.'⁶⁰

Having defused the Wittgensteinian objection⁶¹ he asserts not only

57 Wittgenstein, L. (1958) p153

58 Whiteley, C.H. (1973) p15

59 " " " "

60 " " " "

61 It is interesting to note that Whiteley's rebuttal of the private language argument - a rebuttal with which I am in full agreement - is also employed by Fodor, although for very different purposes. He is concerned to defend, in his aptly titled book The Language of Thought (1975) not the idea of a sense datum language but the notion of a private representational language in terms of which an organism specifies the stimuli it encounters and the description it intends its behaviour to satisfy. p55. He argues: 'The private language argument - at least as

that we can inform one another about events that are necessarily private but that statements about such events are, contra behaviourism, legitimate data for science. Let us now examine his defence of this claim.

It is often argued, he points out, that private, conscious mental states are not suitable for scientific study because they cannot be checked by other people. 'The practical consequence of this view for the study of mind is', he says, 'that this should be a study of behaviour only.'⁶² Behaviourism is the only alternative, he suggests, to a psychology of conscious mental events. This is clear from his reference to the doctrine that a science of conscious mind is impossible. He writes:

'It also seems to say that in strictly scientific discourse no animal can be described as pursuing, fleeing, hiding, courting, or challenging, and no person as buying or selling, bringing a lawsuit, making a gift, teasing or befriending; for none of these words is a pure description of behaviour.'
(My emphasis) ⁶³

Whiteley then proceeds, by way of defending a science of conscious mind,

I have been construing it - isn't really any good. For, as many philosophers have pointed out, the most the argument shows is that unless there are public procedures for telling whether a term is coherently applied, there will be no way of knowing whether it is coherently applied. But it doesn't follow that there wouldn't in fact be a difference between applying the term coherently and applying it at random. A fortiori, it doesn't follow that there isn't any sense to claiming that there is a difference between applying the term coherently and applying it at random. These consequences would, perhaps, follow on the verificationist principle that an assertion can't be sensible unless there is some way of telling whether it is true, but surely there is nothing to be said for that principle.' p70. The fact that the private language argument has no force does not mean, however, that psychology must make use of a sense datum language - or so we have seen the mentalist maintain. His hypothesis is that both conscious and unconscious mental states will provide, if suitably characterised, a satisfactory account of the determinants of action.

62 Whiteley, C.H. (1973) p18

63 " " " " p19

to argue that behaviourism is 'internally inconsistent and impossible to carry out.'⁶⁴ Before discussing these further arguments it is worth noting, again, that mentalists object to the view that behaviourism and dualism (here, science of conscious mind) exhaust the possibilities facing the psychologist. The argument is usually presented the other way round, with the behaviourist claiming that he has the only viable alternative to dualism.⁶⁵ Either way, the mentalist replies, the argument ignores the via media.

Against the behaviourist Whiteley argues that all science rests on experience and not just on the experience of one investigator. The publicity of science lies in the fact that each investigator treats what his colleagues say as reliable testimony as to what they have observed. Hence, Whiteley argues, there is no justification for the distinction drawn by behaviourists between their fellow-observers (whose testimony they accept) and their fellow-human experimental subjects (whose testimony they ignore). And so, he concludes, there is no justification for excluding reports of private experiences taken as reliable descriptions of those experiences.

Different philosophies of science award different epistemic status

64 Whiteley, C.H. (1973) p19

65 The most recent assertion of this behaviourist claim that I have seen was made by Blackman (1976). Any psychologist, he wrote, who says that 'what counts' is not just observable behaviour but the causes which lie behind the behaviourist is faced with an awkward dilemma. Such a psychologist will be forced, he argued, either to give up science or to give up psychology. If 'what counts' is held to be private, subjective mental events then, since they allegedly cannot be studied by scientific method (according to Blackman), they cannot play any part in a scientific psychology. If 'what counts' is held, on the other hand, to be the biological causes of behaviour then the discovery of those causes is properly the task of the physiologist, neurologist, etc in Blackman's view. The only 'scientifically respectable' (p31) option, he maintained, is Watsonian behaviourism.

to introspective reports. What one makes of this dispute between the dualist and the behaviourist will depend on the criteria one's philosophy of science provides. I will return to this issue later in the thesis but I shall now, in conclusion, note an objection to his position which Whiteley does not mention. Rather there are two objections, one of an empirical nature, the other philosophical. First, it is quite possible that a taxonomy based on consciousness will not yield significant generalisations. Indeed, it might be argued that old-style mentalism failed to find them and there is no reason to expect more today. Of course, this is not to say in advance that such a science of conscious mind is doomed to failure. New-style mentalists suggest that a taxonomy based on conscious, semi-conscious and unconscious states of mind are more likely to provide a natural kind for a psychological theory. Second and more important is the objection that if the dualist were to find laws relating conscious mental states to either behaviour or other conscious mental states that would not explain anything. We would still need an explanatory theory. This objection has been raised by Chomsky against dualism. He writes:

'... the proposals of the Cartesians were themselves of no real substance; the phenomena in question are not explained satisfactorily by attributing them to an 'active principle' called 'mind', the properties of which are not developed in any coherent or comprehensive way.' 66

Teleology

Not every dualist tries to defend claims about the uniqueness of man by employing ontological arguments in favour of the existence of mind or consciousness. More influential in recent times than the traditional dualism is a view that man is special not in terms of the stuff of which he is allegedly made but in terms of the laws by which

66 Chomsky, N. (1972) p14

his activities are to be understood. This position, what I call methodological dualism, asserts that there is a fundamental divide over methods between the physical and biological sciences on the one hand, and the behavioural and social sciences on the other.

This methodological dualism, which opposes in different ways the approaches to psychology already examined, has been developed with great sophistication and at length by Georg Henrik von Wright⁶⁷ and it is his argument that I propose to examine in this section. His book, Explanation and Understanding, derives its title from a distinction, attributed to the nineteenth century German historian-philosopher J.G. Droysen, between Erklären and Verstehen. It was held that erklären ('explanation') is the method appropriate to the study of nature while verstehen ('understanding') was deemed suitable to the study of man.

In his attempt to give philosophical muscle to this dichotomy von Wright advances a complex, if schematic, argument which cuts deep into the philosophy of action, the philosophy of history and problems surrounding the nature of causality, natural necessity and determinism. It may be foolhardy to attempt a brief summary of his position but since it introduces issues which will be dealt with in later chapters and is, itself, an interesting argument I will try.

Two traditions in the history of ideas, he says, can be identified in terms of their different views on the criteria which a scientific explanation must meet if it is to be held worthy of respect. These traditions, the 'galilean' and the 'aristotelian', have been at loggerheads for centuries with the first asserting that only causal or mechanistic explanations were satisfactory, the latter choosing to defend a teleological or finalistic conception of explanations. Since

67 von Wright, G.H. (1971)

the mid-nineteenth century the dispute between these traditions, says von Wright, has been a central issue in the philosophy of science.

The galilean tradition has received its most refined presentation, he says, at the hands of the neo-positivists of the Logical Empiricist movement. Positivism has been, traditionally, an ardent supporter of methodological monism and, in particular, of the view that all scientific explanations must be causal in nature. Teleological explanations, which are typically of the form 'A occurred in order that B would be achieved' are, on the positivist account, held to be ultimately reducible to causal explanations in which, in this case, necessary and sufficient conditions for A's occurrence can be specified. von Wright agrees that the galilean tradition scored a major victory when Rosenbleuth, Wiener and Bigelow⁶⁸, in their classic paper of 1943 - 'Behaviour, Purpose and Teleology' - showed how teleological explanations in biology could be eliminated in favour of causal accounts. However, von Wright contests the claim, which galileans are prone to make, that intentional (aristotelian) explanations in the behavioural and social sciences will also be eliminable in favour of causal explanations.

Before examining von Wright's arguments in favour of methodological dualism I wish to draw attention to a criticism of his identification of methodological monism (or naturalism) with positivism. Such an equation, says Giedymin⁶⁹, is both parochial and misleading. It is parochial since it is, he says, inapplicable to all those philosophers and scientists who have never expressed any systematic views on the aims and methods of social science yet who accept characteristically positivist positions. It is misleading, he says, since it does not allow for the fact that many of the positivists' leading critics have

68 Rosenbleuth, A., Wiener, N., and Bigelow, J. (1943)

69 Giedymin, J. (1975) p284

themselves been methodological monists. For example, Popper has been a life-long opponent of positivism, says Giedymin, yet he advocates a methodological monism. Consequently, it is unhelpful to view the galilean-causalist position as positivist; not every methodological monist is a positivist; nor is every positivist committed to methodological monism.

Giedymin's criticism of von Wright is very similar to the mentalists' criticisms of Winch, which we have already discussed. In both cases von Wright and Winch identify the methods of natural science with a positivist interpretation of natural science and it is this mistaken equation, so runs the criticism, which leads each to develop his own alternative. Since both claim that the methods of natural science oblige the psychologist to describe human behaviour in terms of movement they recommend, respectively and independently, that the social scientist does Wittgensteinian philosophy or develops a distinct methodology appropriate to understanding human action. The mentalist opposes both positions, as we have seen, by simply denying that natural science must be positivistically interpreted and further denying that behaviour must be described as movement.

Von Wright does not say that it is impossible to advance a causal account of behaviour. He merely argues that such an account will be an explanation of movement, not action. And it is action, where that is taken to be behaviour linked to intentionality, which is the proper subject matter of the behavioural sciences. Social life, human behaviour, even 'private' actions are different in kind from movement since they are thoroughly imbued with intentionality. Actions-qua-actions, he argues, are non-causal. Hence he seems to hold - though it is not clear how he defends his adoption of - some version of the Compatibility Thesis. This asserts that the same sequence of behaviour

can be causally explained in terms of movement and non-causally, teleologically explained in terms of actions. In other words, von Wright seems to claim that scientists of all kinds do not explain events but rather explain descriptions of events.

Nothing is to be gained in this debate, he suggests, by linguistic legislation. In everyday life, court-rooms, psychiatric wards, economic institutes and a myriad other situations people explain their behaviour in terms of causes. 'I killed him because he hit me', 'I bought it because it was cheap' etc are perfectly legitimate ways of talking provided, says von Wright, we do not think that this usage of 'cause' is the same as its usage and employment in the natural sciences. He calls this usage 'quasi-causal' to indicate that it is not really a causal explanation at all; it is but a disguised form of teleological explanation. By the same token quasi-teleological explanations, such as those which explain the workings of a thermostat or biological evolution in terms of end-results or goals, are really causal explanations in disguise.⁷⁰ Psychologists, then, are free to employ causal language but they must realise, he says, that they are not using causal concepts.

The argument that actions-qua-actions are not causally explicable is developed in two stages. First, von Wright borrows the Aristotelian notion of practical inference (or syllogism) as resurrected by Anscombe⁷¹ and then suggests that this notion can be employed as an explanation model for actions. Second, in a crucial move for his methodological dualist thesis, he defends a refined version of what has become known as the Logical Connection Argument to prove that explanations which meet the requirements of the practical inference model are non-causal.

70 von Wright, G.H. (1971) p200

71 Anscombe, G.E.M. (1957)

One way of reconstructing the main idea in the practical syllogism, he says, is the following:

'The starting point or major premise of the syllogism mentions some wanted thing or end of action; the minor premise relates some action to this thing, roughly as a means to the end; the conclusion finally consists in use of this means to secure that end. Thus, as in a theoretical inference the affirmation of the premises leads of necessity to the affirmation of the conclusion, in a practical inference assent to the premises entails action in accordance with them.' 72

A practical syllogism, in its simplest form, has the following structure:

(PI) A intends to bring about P.

A considers that he cannot bring about P
unless he does a.

Therefore A sets himself to do a.

This schema of practical inference is, von Wright says, that of a teleological explanation "turned upside down". Normally the starting point in a teleological explanation is the fact that someone has performed an action or started to perform an action. If asked 'Why?', the answer is simply 'In order to bring about P'. This explanation assumes that the agent considers the behaviour causally relevant to the bringing about of P and that the bringing about of P is what he is aiming at or intending with his behaviour. In von Wright's opinion:

'Practical reasoning is of great importance to the explanation and understanding of action. It is a tenet of the present work that the practical syllogism provides the sciences of man with something long missing from their methodology: an explanation model in its own right which is a definite alternative to the subsumption-theoretic covering law model. Broadly speaking what the subsumption-theoretic model is to causal explanation and explanation in the natural sciences, the practical syllogism is to

'teleological explanation and explanation in history and the social sciences.' 73

Methodological monists, whether positivist or antipositivist, argue in reply that actions can be causally explained. Hence, they suggest that von Wright's practical syllogisms or inference schemas can be recast in causal form. Indeed, we have seen that mentalists attempt to do just this. Both the intention (major premise in PI) and the cognitive belief (minor premise) can be so characterised, on that view, so that they can be seen to play a causal role in the production of behaviour.⁷⁴

Before examining von Wright's version of the Logical Connection Argument and his associated argument that actions cannot be causally explained I want to draw attention to a criticism made by Weinryb.⁷⁵ It is, simply, that practical inferences are not inferences at all. To infer, says Weinryb, is to find the logical consequences of a given set of statements. It is misleading for von Wright, he says, to use the term inference since he admits that it is possible for an agent to have both an intention to do something and the required beliefs and yet do nothing. To quote von Wright:

'... the premises of a practical inference do not with logical necessity entail behaviour. They do not entail the "existence" of a conclusion to match them. The syllogism when leading up to action is "practical" and not a piece of logical

73 von Wright, G.H. (1971) p27. Von Wright's claim that the subsumption-theoretic covering law model of causal explanation is typical of the natural sciences is to be expected in the light of his positivist orientation. For details of this model of explanation and its development see Suppe, F. (1974); positivism is further discussed in Chapter Two, Part Two.

74 Fodor, we have seen (p30), denied that action, taken to mean conscious intentional behaviour as opposed to unconscious, unintentional behaviour, is a psychological concept. I think that von Wright's usage of action can, however, be reconciled with mentalism. For, according to von Wright, not all actions result from conscious intentions; he also construes as action intentional behaviour which is not, however, the result of conscious intention. p35

75 Weinryb, E. (1974) p331

'demonstration. It is only when action is already there and a practical argument is constructed to explain or justify it that we have a logically conclusive argument. The necessity of the practical inference schema is, one could say, a necessity conceived ex post actu.' 76

With this von Wright not only contradicts his earlier claim that the premises of a practical inference do entail action but offers a very weak alternative methodology for the social sciences in comparison with that which he takes to be appropriate for natural science.

In essence the Logical Connection Argument is this. (1) In a Humean causal connection the cause and effect are necessarily logically independent of one another. (2) An intention is never logically independent of the action which it is meant to explain. (3) Therefore an intention can never be the cause of an action. It is then, by this argument, simply a conceptual blunder to 'explain' actions as caused by intentions. Cartesian dualists and interactionists like Whiteley make just such blunders, on this view, as do holders of the mind-brain Identity Theory for the latter believe that mental states such as intentions can be contingently identified with states or processes in the body or the brain. (So this Logical Connection Argument can also be employed against those mentalists who also subscribe to the Identity Theory.)

Though von Wright thinks that those who advocate the Logical Connection Argument are "substantially right"⁷⁷ he does not think the above presentation convincing. For, while he is prepared to defend the first premise he thinks the second is not proven and thus proceeds to develop an alternative formulation of it. In passing, however, I wish to point out that von Wright's acceptance of the first

76 von Wright, G.H. (1971) p117
77 " " " " p77

premise, the Humean demand for the logical independence of cause and effect is neither justified nor explained. And it certainly calls for explanation in the light of the fact that an entire chapter of his book is devoted to combatting the Humean (positivist) conception of causality.⁷⁸ He does not explain how his opposition to the positivist view of causation squares with his espousal of the positivist requirement of logical independence. The above presentation of the argument is unsatisfactory, says von Wright, because it does not follow from the fact that acts of will, intentions and other mental events can only be defined by making reference to their objects (ie intended results of actions), that they could not, nevertheless, be (Humean) causes of behaviour. 'The logical dependence of the specific character of the will on the nature of its object is', he says, 'fully compatible with the logical independence of the occurrence of an act of will of this character from the realization of its object.'⁷⁹ There may, in other words, be a contingent relation between intentions and the occurrence of what fulfills them even though it is necessary to characterise intentions through reference to their objects.

Von Wright's novel attempt to bolster up the second premise of the Logical Connection Argument is in terms of verification and his reasoning is in the form of a kind of Catch-22. To verify the conclusion of a practical syllogism it is, he says, necessary to verify

78 The thrust of the second chapter of von Wright (1971) is to show that the notion of cause is dependent on the concept of action. 'We cannot understand causation, nor the distinction between nomic connections and accidental uniformities of nature, without resorting to ideas about doing things and intentionally interfering with the course of nature.' p65. 'We can be as certain of the truth of causal laws as we can be of our abilities to do and bring about things.' p73. For criticism arguing that the notion of action is dependent upon the notion of cause see Kim, J. (1972) and Brittan, G.G. (1972).

79 von Wright, G.H. (1971) p94

the major premise, is a description of the agent's intentions; to verify the major premise, however, it is necessary to verify the conclusion, is a description of the agent's actions. It is impossible to verify either the conclusion or the premises of a practical inference independently of each other and consequently, he concludes, the facts which one tries to establish are not logically independent of one another. Hence, an intention cannot be a (Humean) cause of behaviour. 'In this mutual dependence of the verification of premises and the verification of conclusions in practical syllogisms consists,' he says, 'the truth of the Logical Connection Argument.'⁸⁰

The first part of von Wright's argument runs like this: if an agent performs an action it is not possible for an observer to say with certainty that the actor did, in fact, perform the action; the agent might well be unaware that his behavioural movements are taken to signify action or he might even have taken himself to perform some other action. Says von Wright:

'We must also establish that what took place was intentional on A's part, and not something that he brought about only accidentally, by mistake, or even against his will. We must show that A's behaviour, the movement which we see his body go through, is intentional under the description "doing a".'⁸¹

In case A fails to perform a, he says, we need, a fortiori, to verify the fact that A had the intention to perform a. In sum, whether A performs or fails to perform a, 'we shall also have to establish the intentional character of the behaviour or of the accomplishment, that it is "aiming" at a certain achievement, independently of whether it accomplishes it or not.'⁸² To verify the conclusion of a practical

80 von Wright, G.H. (1971) p116
81 " " " " p108
82 " " " " p109

sylllogism, therefore, it is necessary, he argues, to verify the major premise.

Next, von Wright turns to the claim that the verification of the premises in a practical syllogism depends on the verification of the conclusion. There is, first, a discussion of the alleged impossibility of gaining direct access to another person's inner states or intentions. This is followed by an argument to the effect that I do not even have access to my own intentions - independently of my behaviour. But, as Weinryb⁸³ has pointed out, von Wright provides no support for his above claim and merely asserts that he has established that verifying the descriptions of other people's intentions depends on verifying the descriptions of the actual actions which are explained by these intentions. Thus, in conclusion, von Wright has not substantiated his version of the Logical Connection Argument and so, says Weinryb, he has not justified his thesis that intentions cannot be offered as causal explanations of actions.

Even if von Wright had provided arguments to support the second leg of his thesis he would still not have made a convincing case for the Logical Connection Argument. For, says Weinryb, in his espousal of the first part of his version of the Logical Connection Argument von Wright blurs an important distinction which he has himself made in other parts of his book, the distinction between 'intentional acting' and 'having an intention to do a certain thing'. Von Wright had warned:

'One must distinguish between intentional acting and intention to do a certain thing. Everything which we intend to do and also actually do we do intentionally. But it cannot be said that we intend to do everything we do intentionally.'⁸⁴

If we accept this distinction it is possible, says Weinryb, to

83 Weinryb, E. (1974) p335

84 von Wright, G.H. (1971) p89

distinguish between the criteria for considering an action as intentional and the criteria for detecting an intention in the agent. Against von Wright he writes:

'If this distinction is kept, then the term 'intentionality', so often used by him, becomes ambiguous. Anyway von Wright's argument from the fact that the conclusion of a practical inference must describe an intentional action to the assertion that, therefore, its verification depends on ascertaining the presence in the agent of a specific intention is, even on his own assumptions, a non sequitur.' 85

Weinryb correctly concludes that von Wright, having ultimately failed to provide a coherent defence of his thesis of interdependence of verifiability of premisses and conclusion in a practical inference, has not shown the Logical Connection Argument to be 'substantially right'. Consequently, he has not made out a convincing case for methodological dualism nor eliminated the possibility of a causal explanation of intentional behaviour.

More influential among psychologists than von Wright's view have been those of the philosopher Charles Taylor who attempted to rehabilitate teleological-purposive explanations as scientifically respectable in psychology. 'The "Galilean spirit",' he wrote in his book, The Explanation of Behaviour, 'has been abroad in psychology for quite some time, and, if it hasn't produced anything very solid in experimental psychology this may be because current approaches are wrong.' 86

The key to Taylor's argument lies in the word 'may' for, unlike von Wright, he does not think it a priori impossible that action or intentional behaviour will be eventually explained in mechanistic-causal terms. In reply to criticism he has made his position clear:

85 Weinryb, E. (1971) p336

86 Taylor, C. (1964) p272

'I don't believe that there is any argument in principle which can show that mechanistic explanation is impossible, that in other words such an explanation is 'inconceivable' ... there is no necessary incompatibility between our describing and explaining behaviour by purpose in ordinary life or in the context of scientific theories of teleological-intentional type (like, for example, psychoanalysis) on one hand, and our being able to give a mechanistic neurophysiological account of them on the other ... I believe that such a mechanistic explanation would be related to our ordinary purposive ones on the model of ... 'deeper level' explanations.' 87

Taylor's espousal of methodological dualism is, therefore, much less radical than von Wright's and may be viewed as little more than a short-term research strategy. He advocates peaceful co-existence between both mechanist and purposive explanations provided 'that they are related as more and less basic explanations.'⁸⁸ Psychologists are recommended to try to establish teleological and intentional laws in the first instance and this would help, he says, in the further search for a global mechanist theory. Methodological dualism may, then, on this view, eventually lead to a methodological monism.

Humanism

All of the arguments and counter-arguments reviewed so far reflect the debate about the status of psychology as it has been conducted within professional academic psychology and philosophy. I cannot pretend that the positions considered exhaust either what is important or interesting in the relevant literature but I do claim that they are representative of the range of conflicting views within and about psychology. However, there is another strain in contemporary thinking on these matters, which we might loosely call 'humanist', that

87 Taylor, G. (1970) p89

88 " " " p94

often finds its most forceful expression outside academia. Humanists try, in diverse ways, to defend and develop an image of man as an autonomous, conscious, creative moral agent - an image which is threatened, they claim, by the march of scientific knowledge. These modern humanists, therefore, unlike their forebears, aim their fire not so much at organised religion as at organised science. Psychology, in particular, when viewed as the application of the scientific method to the study of man, is singled out as a pernicious enemy of the humanist vision.

Pre-eminent among such humanist works is Arthur Koestler's fierce polemic against behaviourism, The Ghost in the Machine.⁸⁹ He attempts in what seems, at times, almost a moral crusade to reassert man's freedom and dignity against the 'pseudo-science' of atomorphic psychology.⁹⁰ Unlike other humanists Koestler's strategy is to advocate a better scientific theory and produce evidential support for it which simultaneously vindicates our own assessments of human nature. Alas, as Gellner⁹¹ has pointed out, any scientific explanation must be unacceptable to a humanist for such an explanation will be both de-humanising and morally offensive. This fact, he suggests, is recognised by many other humanists who oppose scientific psychology - for example, by both Ryle and Wittgenstein. Instead of offering better scientific explanations than do the behaviourists they deny that any explanations are called for at all. Hence, in a much quoted passage, Ryle wrote:

'Let the psychologist tell us why we are deceived; but we can tell ourselves and him why we are not deceived. The ... diagnosis of our mental impotences requires special research methods. The explanation

89 Koestler, A. (1971)

90 " " " p33

91 Gellner, E. (1974) p106

'of the exhibition of our mental competences often requires nothing but ordinary good sense.' 92

But, Gellner replies, not only do mental competences call for scientific explanation but such explanations have already been advanced by Chomsky and others. And the salient feature of such explanations, he says, is that they postulate impersonal structures, theoretical mechanisms whose operations cut through commonsense and destroy the very notions in terms of which we conceive our own identity. 'Cognition and identity,' he says, 'are incompatible.'⁹³ As we shall see, however, this is not a consequence of the mentalist position that Chomsky would be prepared to accept. He holds out a theory of human nature which, while scientifically respectable, allegedly conforms to the traditionalist, rationalist conception of man as a free, creative agent. Accordingly he would, I suspect, be more in sympathy with Koestler than Gellner. But these are issues to which I will return.

92 Ryle, G. (1949) p326

93 Gellner, E. (1974) p101

Chapter Two

Psychology and the Philosophy of Science

Part One: The Kuhmian Proposal

Scientific psychology, then, is a priori a contradiction in terms, a special kind of science with its own unique brand of teleological non-causal explanation, a dismal and expensive failure, a well-founded and fast developing science of behaviour, a demeaning and dehumanising pseudoscience or, finally, it is, at last, becoming an experimental mechanism which is both empirically disciplined and capable of revealing the complexity and richness of the mental mechanisms which cause behaviour. Faced with such diversity of argument over the most fundamental issues relating to contemporary psychology the problem is how we are to decide which, if any, of these conflicting standpoints is correct or, at least, preferable. If we are to be able to defend a decision to adopt one or other of these views we will, clearly, be obliged to point to tenable criteria against which the competitors are adjudged. Different criteria will motivate different decisions.

These criteria are often provided by general philosophical perspectives or by what has come to be called 'philosophy of science'. Some people oppose all attempts to construct a scientific psychology, we have seen, on the grounds of the alleged inability of scientific method to get to grips with mental phenomena or human action. Implicit in such arguments which derive from a more comprehensive philosophy is a 'philosophy of science' which denies that mind, action, intention and other specified concepts belong to the same sphere of discourse or 'form of life' as natural science. Alternatively, we have seen



that some assert that behaviour alone, publicly observable and hence, so runs the argument, objectively describable in contrast to private, subjective mental experience, can form part of the scientific enterprise. In this case the philosophy of science is usually explicitly stated, namely one or other version of logical empiricism. Indeed, according to Skinner¹:

'Behaviourism, with the accent on the last syllable, is not the scientific study of behaviour but a philosophy of science concerned with the subject matter and methods of psychology.'

A good part of this chapter will be concerned with showing that different philosophies of science provide the basis for different judgements about psychology. Accordingly we need to debate the merits of current philosophies of science.

Historically, psychology has been enormously influenced by developments within philosophy of science, especially in this century. As they struggled to construct a viable science psychologists were continually looking over their shoulders in search of those pearls of wisdom, the presumed canons of scientific method formalised and advertised by their contemporary colleagues in the tradition which was to become philosophy of science. That this is something of an oversimplification we shall see later in this chapter where behaviourism's intimate involvement in the career of logical empiricism provides a case study of the influence a philosophy of science can have on a psychological movement. Such a case study will have the added benefits of providing background information necessary for an understanding of the sources of the ferment in current philosophy of science and it will also introduce, in the context in which they were formulated, the ideas of Karl Popper whose work is the topic of the next chapter.

1 Skinner, B.F. (1964) p79

Contemporary discussions about psychology's proper course of development can be illuminated, I will argue, by such a case study. It can throw light, for example, on a recent proposal which suggests that psychology can best be understood if viewed through the perspective on scientific theorising provided by the historian and sociologist of science Thomas Kuhn. Kuhn's widely influential philosophy of science played a very significant role in logical empiricism's decline. In line with Kuhn's account Palermo² claims that 'experimental psychology has had two paradigms already, with the appropriate scientific revolution between them', and that the discipline may very well be enjoying yet another revolution at present, with a switch away from an anomaly-ridden behaviourism to a new paradigm of cognitive psychology or psycholinguistics based on the pioneering work of the linguist-psychologist Noam Chomsky. Palermo has less to say about the effect such a reconstruction would have on the practice of psychology if it were seriously considered - but more of that later. Let us, therefore, examine the issues involved in the debate Palermo has started before we turn to the case study to see what lessons it holds out for the student of psychology and its relation to current philosophy of science.

Psychology's first Kuhnian paradigm, says Palermo, emerged during the late nineteenth and early twentieth centuries and was centred on the work of Wundt and his followers in Germany. This paradigm, he says, was characterised by its subject matter and its method. In Kuhnian language, within Wundtian 'normal science' the task of the psychology student was to study immediate experience (subject matter) by the (method) of pure introspection. Wundt's paradigm, according to

2 Palermo, D.S. (1971) p138

Palermo, was self-consciously contrasted with the paradigms of the natural sciences which studied what was termed mediate experience by the method of inspection. During the reign of the alleged Wundtian paradigm the 'act psychology' of the day agreed with the use of the introspective method but opposed, he says, the aim of analysing immediate experience into its constituent elements. Nevertheless, Wundt dominated psychology, says Palermo, until Watson's famous 'revolutionary statement' launched behaviourism as 'the new paradigm for experimental psychology - at least, for American experimental psychology.'³

If behaviourism was psychology's second paradigm, as Palermo claims, then he is, on his own admission, obliged to find the source of pre-revolutionary crisis because 'as Kuhn has noted, revolution can only succeed in the presence of a crisis.'⁴ Consequently Palermo offers three reasons why Wundt's paradigm ran into crisis. The first, he says, is the unreliability of the introspective method.

'Each laboratory found, in the introspective reports of its own subjects, the kind of data which the scientist in the laboratory was looking for to support his theoretical conception of the contents of consciousness. This, of course, is not out of line with the typical efforts of normal science, but the fact that experiments in one laboratory could not be replicated in others made the procedure suspect.'⁵

The second reason for the crisis was the gradual realisation by animal psychologists that 'it seemed unnecessary to bring in consciousness to describe or account for (their) results.'⁶ Given that Wundt's paradigm centred on immediate experience the animal psychologists felt obliged to anthropomorphise and thus postulate

3 Palermo, D.S. (1971) p140
4 " " " "
5 " " " "
6 " " " p142

animal experiences on analogy with their own; by embracing behaviourism they were immediately freed from what they came to see as unnecessary complications. Third, the Wundtian paradigm ran into crisis, he says, because it was incapable of making any contribution to education or mental health - primary concerns of many American psychologists in the pragmatic tradition. Functionalism failed to conquer Wundtian psychology because it 'had no strong evangelical spokesman to make its case'⁷ for a discipline that would have greater influence on practical affairs. Thanks largely to Watson's charisma behaviourism triumphed over Wundtian structuralism and such would-be-paradigms as Gestalt theory which lacked a propagandist of Watson's talent - or such, at least, is Palermo's reading of the birth of behaviourism.

When the majority of practitioners within the discipline lined up behind Watson, Palermo continues, the behaviourist revolution was, ipso facto, triumphant. And despite initial opposition - 'true to Kuhn's analysis of the other sciences'⁸, he says, such is only to be expected - behaviourist normal science was soon in full swing.

'The research efforts of Tolman, Guthrie, Hull and their followers marked the period of normal science within the behaviourist paradigm. Data were collected at fever pitch. Different theoretical points of view led to controversy, but all played the game of psychology by essentially the same ground rules: all accepted the behaviourist paradigm and thought little of questioning the paradigm.'⁹

In time, of course, the expected Kuhnian anomalies soon started to appear and as the paradigm-defined puzzles failed to yield to paradigmatic solutions so disenchantment grew and is still growing. In fact, says Palermo, all of the historical and social elements central to a Kuhnian revolution are now visible in psychology. 'There seems

7 Palermo, D.S. (1971) p143

8 " " " "

9 " " " p144

little doubt', he concludes, 'that experimental psychology is ripe for a revolution - if it is not, in fact, currently in the midst of one.'¹⁰

Sharp and effective criticism of Palermo's account was rapidly and independently advanced by Warren¹¹ and Briskman¹². Parochialism coupled with distorted vision, says Warren, is the source of Palermo's misapplication of Kuhn. Parochialism leads Palermo to ignore large areas of psychology and perceive in behaviourism a paradigm which triumphed over Wundtian mentalism. Before Watson published his revolutionary manifesto, says Warren, psychology was a diverse enterprise constituted by many unrelated fields of study. These included, he says, in the years from the turn of the century to the First World War, Gestalt psychology in Germany, Freud's psychoanalysis emerging in Austria, the discovery of the conditioned reflex in Russia, Binet's invention of psychological tests in France and the first appearance of social psychology textbooks in the United States. It is true, says Warren, that behaviourism became the dominant movement in America but it did not triumph over a single, unitary Wundtian paradigm.

'There has never been a unitary discipline of psychology with one major paradigm at a time. At any one time there has been a fair number of paradigms, each of which commanded the allegiance of many psychologists. It is only by parochial limitation to the American scene that Palermo can perceive paradigmatic science.'¹³

Warren further argues that even when behaviourism became the dominant movement in American psychology it did not eliminate competitors or totally supplant its predecessors. Gestaltism, ethology, psychometrics, and 'almost every other European development was given a lease of life

10 Palermo, D.S. (1971) p155

11 Warren, N. (1971)

12 Briskman, L.B. (1972)

13 Warren, N. (1971) p410

in the New World¹⁴, he says, during the supposed heyday of behaviourist normal science. Palermo's distorted vision, he suggests, leads him to ignore these features of the discipline as well. Since a Kuhnian paradigm is supposed to be all-pervasive within a discipline, especially during the normal science period, Warren concludes that behaviourism does not meet the appropriate Kuhnian criteria. Nor does it, he says, square with Kuhn's formulation in another important respect. A new paradigm's success lies, in part, in its ability to provide solutions for those problems which proved intractable to the old paradigm's methods and techniques. Behaviourism, says Warren, solved no problems which supposedly embarrassed its predecessors. Watson merely excluded mind, he says, and with it the associated introspective method by fiat; there was no scientific growth in the implementation of behaviourism. So behaviourism, he concludes, was not a paradigm.

Warren's criticisms do not, however, extend to the Kuhnian theory of science itself. He merely opposes the extension of Kuhn arguing, instead, that psychology is in a pre-paradigmatic state. The desired first paradigm, which would signal the 'onset of maturity'¹⁵, has yet to emerge. Palermo's analysis leaves him dismayed because he had thought psychology was at last becoming 'more of a unitary international science - more the kind of enterprise on which Kuhn's theory could genuinely be brought to bear.'¹⁶ Instead it looks as if they are as divided as ever. But he is insistent that the establishment of a Kuhnian paradigm would be an advance. He writes:

'It is desirable ... that psychologists be an esoteric, closed community which advances its

14 Warren, N. (1971) p410
15 " " " p413
16 " " " "

'science within a common paradigm - with, of course, the inevitable revolutions arising out of normal science.' 17

Like Warren, Briskman's strategy is to unveil a number of historical claims, some in conflict with Palermo's, which do not square with the minimal requirements of any Kuhnian analysis. Thus he challenged the identification of what he calls Wundtian introspectionism with a Kuhnian paradigm. Wundtian structuralism, as it is more usually called, was characterised by Palermo merely by its subject matter and methodology. This is insufficient, Briskman¹⁸ pointed out, to constitute a paradigm in Kuhn's sense - though what would be sufficient was admitted to be problematic. We have seen Palermo admit that Wundt was opposed by 'act psychology' over the proper subject matter for study. Such disagreement, says Briskman, is not compatible with the Kuhnian requirement of unanimity within a discipline on fundamental issues like what the discipline is about. Thus he writes:

'Basically one cannot help but feel that as employed in such cases the notion of a 'paradigm' serves more to conceal than to reveal; that it serves as an excuse for not having to analyse the historical situation in all its rich complexity.' 19

Similarly, Briskman argues that the three reasons offered by Palermo in his attempt to explain the 'crisis' within the alleged Wundtian paradigm do not square with Kuhn's account of crisis in science. For Kuhn a crisis results from the failure of normal science, which is carried on within a paradigm, to solve the puzzles which the paradigm defines. For Palermo, by contrast, the 'crisis' resulted from three factors which, Briskman suggests, could only sound irrelevant to any member of the supposed Wundtian paradigm. Take Palermo's first reason

17 Warren, N. (1971) p413
18 Briskman, L.B. (1972) p88
19 " " " "

for the 'crisis' - the unreliability of the introspective method. Since the Wundtian 'paradigm' was supposed, by Palermo, to be defined in contrast to the paradigms of the natural sciences Briskman rightly stresses that anyone working within a Wundtian paradigm, had there been one, would ignore standards of experimental reliability imported from another paradigm - in this case, a paradigm in the natural sciences. He would ignore, downgrade in significance or merely learn to live with the fact - if it is a fact - that experiments conducted with the method of pure introspection could not be replicated from one laboratory to another. It would certainly not lead him to abandon Wundtian psychology. It could similarly be easily shown, Briskman adds, that the other two reasons offered by Palermo to account for the Wundtian crisis are also inadequate. The concerns of animal psychologists and American pragmatists could not provoke a 'crisis' within Wundtian psychology.

In denying paradigm-status to behaviourism Briskman's argument is complementary to Warren's. Whereas Warren had drawn attention to the variety of psychological movements thriving inside America and outside both before and during behaviourism's career Briskman detailed the disagreements within the behaviourist movement itself. Palermo's assertion that 'the research efforts of Tolman, Guthrie, Hull and their students marked the period of normal science within the behaviourist paradigm'²⁰ he described as 'sheer nonsense and an extremely good indication that Palermo has fallen so in love with Kuhnian ideas that he has lost touch with the reality of his subject.'²¹ Normal science, as characterised by Kuhn, only begins when debate over fundamentals comes to an end, Briskman correctly pointed out. Behaviourism, however,

20 Palermo, D.S. (1971) p144

21 Briskman, L.B. (1972) p91

was characterised by persistent disputes over such fundamental issues as what the organism actually learns, the mechanism by which he learns and especially over the role of reinforcement in learning. He concludes:

'These are not simply minor disagreements; rather they concern fundamental issues for the conceptualisation of learning processes. In other words there really has never been anything like behaviourist normal science, at least not if the work of Tolman, Guthrie and Hull are meant to mark it.' 22

In his paper Briskman restricts his comments, by and large, to Palermo's application of Kuhn's theory to psychology rather than mounting a direct assault on that theory itself. He does point out, however, that Kuhn's analysis displays a 'fundamental weakness'²³ in failing to recognise, as noted by Feyerabend²⁴, that a 'crisis' in any paradigm may result only as a consequence of the emergence of a competing paradigm or theory, consciously developed in advance of any breakdown in the old paradigm. Briskman ends his paper by sketching a reconstruction of behaviourism in terms of the methodological-cum-metaphysical research programmes, familiar to students of Lakatos's philosophy of science, which I shall examine in Chapter Four.

Both Warren and Briskman, then, base their objections to Palermo on what they take to be a serious discrepancy between the facts of history and the criteria which they take Kuhn to specify for the existence of normal science, paradigms, crises and so on. To be effective any reply to these criticisms is obliged either to show that the historical claims are false or that Kuhn's analysis is different from Kuhn as understood by these two writers or both. If Kuhn's theory is still to be applied to psychology then some indication should be provided that satisfactory answers to the objections raised to Kuhn's

22 Briskman, L.B. (1972) p92

23 " " " " p89

24 Feyerabend, P. (1965)

account are forthcoming. In a lengthy article which attempts to counter the criticisms reviewed here Weimer and Palermo²⁵ assert that both Warren's and Briskman's arguments are, from Kuhn's point of view, based on misconceptions. Had they understood Kuhn correctly, they imply, these critics would not have objected as they did. Despite Kuhn's popularity, they warn, 'there is evidence of considerable misunderstanding'²⁶ - a claim which they themselves then proceed to exemplify, as we shall see. It will be necessary to discuss Kuhn's theory itself, to identify his distinctive doctrines, to review the counterarguments to those doctrines and to measure their weight in order to judge whether Weimer and Palermo are embarrassed by either Warren or Briskman or both. But first let us examine the reply Weimer and Palermo have made to their opponents.

We can best appreciate their line of attack by focusing on the way they deal with Warren's objection that psychology is, according to Kuhn, a pre-paradigmatic science. Warren notes that it was the difference which Kuhn perceived between the physical and social sciences that led him to introduce the notion of paradigm in the first place. Warren even quotes Kuhn's remarks on this in the preface to the latter's 1962 essay:

'The practice of astronomy, physics, chemistry or biology normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists or sociologists. Attempting to discover the source of that difference led me to recognise the role in scientific research of what I have since called "paradigms".' ²⁷

Warren argued, in brief, that since psychology was pre-paradigmatic it obviously could never have had a ^dparadigm. To this Weimer and Palermo reply, quite simply, that this 'is not now compatible with Kuhn's

25 Weimer, W.B. and Palermo, D.S. (1973)

26 " " " " " " " " p211

27 Warren, N. (1971) p408

position.²⁸ Hence if Warren wishes to claim that psychology has never had a paradigm, they reply, it will not suffice merely to quote Kuhn but rather require that he show how psychology's various movements fail to meet the Kuhnian criteria for paradigms. It is clear from this that Weimer and Palermo are not supporting the extension of Kuhn's initial theory to psychology, as carried out by Palermo, but are defending the application of the more cautious, modified Kuhnian theory as developed by 1969.

A similar strategy is employed in dealing with the criticisms that neither structuralism nor behaviourism held unanimous support within psychology and consequently the discipline of psychology never had an all-pervasive paradigm. It will be remembered that Palermo had provoked these criticisms with the claim that 'experimental psychology has had two paradigms already, with the appropriate scientific revolution between them.'²⁹ Such a claim, it is now admitted, is simply a misreading of Kuhn for paradigms do not characterise entire disciplines at any specific moment. Instead Weimer and Palermo emphasise that, according to Kuhn, there may be several competing paradigms within a discipline at any one moment, each of them involved in normal science activity. Hence, it is no argument against the claim that behaviourism and structuralism were paradigms to point to contemporaneous psychologists opposed to each of these respective movements. And with the recognition that paradigms are not all-pervasive the obstacles to a Kuhnian analysis are removed. They write:

'A genuine problem that faces a 'paradigm' analysis in any discipline is that of isolating the relevant research community, as the sociological unit of analysis; paradigms are the possession of, and the governors of, a group of practitioners - not a discipline. With this realisation, the charge of

28 Weimer, W.B. and Palermo, D.S. (1973) p215

29 Palermo, D.S. (1971) p138

parochialism vanishes, and the real problem of isolating relevant research communities emerges.³⁰

Given this emphasis on permitted diversity within a discipline and combined with the greatly liberalised notion or, perhaps more accurately, more explicit characterisation of the already liberal notion of a paradigm, discussed below, it is a wonder that the authors still wish to claim that psychology has had only two paradigms to date or, possibly, three. Piagetians, Gibsonians, Freudians, Skinnerians - to name but a few - all form groups with their own distinctive set of commitments which, with a little ingenuity, could easily be presented as plausible paradigms. Before looking at Weimer's and Palermo's other arguments it is only fair to note that Warren³¹ has rightly rejected their argument based on a revised version of Kuhnian theory. Palermo's case, says Warren, was based on Kuhn's initial theory and the fact that Kuhn has changed his views does not correct Palermo's mistakes. On that original theory psychology could not be said to have a paradigm.

To the objection that behaviourism was not a paradigm because it was racked by disputes over fundamentals Weimer and Palermo reply here, too, that this is another misconception. Within any single paradigm during a period of normal science, they say, 'there is plenty of room for controversy, including in certain cases, "debate over fundamentals".'³² Such controversy, however, is merely over theoretical issues within the paradigm and 'what is "fundamental to the paradigm" is not questioned during normal research.'³³ More specifically, the furious debates between Tolman, Guthrie, Hull and their followers, they now claim, were merely theoretical disputes but did not call the behaviourist paradigm into question.

30 Weimer, W.B. and Palermo, D.S. (1973) p216

31 Warren, N. (1974)

32 Weimer, W.B. and Palermo, D.S. (1973) p217

33 " " " " " " "

To the objection that behaviourism was not a paradigm because it solved none of the problems that troubled its predecessor, merely banishing them by fiat, Weimer and Palermo provide no reply at all. So let us finally consider what they have to say to their critics on the role of 'crisis' in Kuhnian theory.

Palermo offered three reasons for the crisis in Wundtian structuralism prior to the behaviourist revolution asserting that there was indeed a crisis because 'revolution can only succeed in the presence of a crisis.'³⁴ While Briskman replied that these three reasons could not be fitted into Kuhn's analysis he did agree, quoting Feyerabend in criticism of Kuhn, that for Kuhn alternative theories (hence, revolutions) emerge or triumph only after the previous paradigm has run into crisis. Again, Weimer and Palermo reply that this is another misconception stating:

'Kuhn has not insisted that crises were indispensable precursors to revolutions.'³⁵

As this is offered as a correction of Palermo and his critics and is also advanced as a restatement of one of the constant claims of Kuhn's argument it shows, ironically, that Weimer and Palermo have missed one of the central doctrines of the entire theory. Kuhn clearly states that in the absence of crisis a new paradigm-candidate will not initiate a revolution.

In sum, however, the general strategy of Weimer's and Palermo's reply is that their critics have both misunderstood Kuhn's original theory and have ignored subsequent clarifications and modifications of his views. Thus, they say, the critics dismiss a legitimate and potentially useful reconstruction of the history of modern psychology.

34 Palermo, D.S. (1971) p140

35 Weimer, W.B. and Palermo, D.S. (1973) p237 (footnote a)

Immediately the question arises whether these authors or their critics are correct and has one or other side in the dispute misread Kuhn. The fact that the disagreements result from different interpretations of Kuhn is not very surprising for the theory is ambiguous, necessarily elusive, in part self-contradictory and admittedly incomplete. Nevertheless, Weimer and Palermo do misread Kuhn as well.

Thomas S. Kuhn's Theory of Science

In The Structure of Scientific Revolutions³⁶ Kuhn, a physicist by training and a historian of science by profession, launched a vigorous attack on the then dominant, traditional conception of scientific method as developed over thirty years by the logical empiricists and stemming from the work of the Vienna Circle. Since the rise and fall of that movement will be documented, in some detail, in the case study it will suffice here to say that Kuhn was challenging the view of science as a cumulative, progressive, knowledge-building enterprise whose theories could be given precise and exhaustive logical formulations and grounded on solid empirical foundations by equally precise and complete rules of interpretation. Logical empiricists were concerned with the ideal, finished product of scientific thinking rather than the theory in its half-articulated form or, indeed, in any other stage of development. History of science, accordingly, was deemed on this view as irrelevant to philosophy of science for how a theory was generated and nurtured to its adult expression, while of possible intrinsic interest, could throw no light on its epistemological status. Such was one effect of the influential distinction, introduced

36 Kuhn, T.S. (1962)

by Reichenbach³⁷, between 'the context of discovery' and 'the context of justification'.

This distinction and associated positivist dichotomies, such as the theoretical-observational divide, were brushed aside by Kuhn as 'extraordinarily problematic'³⁸ when applied to the facts of history.

'Rather than being elementary logical or methodological distinctions, which would thus be prior to the analysis of scientific knowledge, they now seem integral parts of a traditional set of substantive answers to the very questions upon which they have been deployed.'³⁹

History of science was not merely relevant to philosophy of science, he argued, but could, if taken seriously, result in a decisive transformation in the popular, accepted picture of science. It could lead us to replace the propaganda which presented science as a linear, ever-growing body of established knowledge with a cyclic account of considerable complexity wherein consensus fragments, chaos takes over and peace returns only after periodic and inevitable revolutions in the most basic concepts of each and every science. In taking this view, Kuhn was firmly embedded in a tradition which, originating with Duhem, had reinstated the old, abandoned theories of former generations as much less than error, prejudice, superstition and ignorance if not equally meritorious as their successors.

The Cambridge historian, Herbert Butterfield, partially anticipated Kuhn's theory back in 1949 when he opposed the idea that science was an awe-inspiring series of success-stories, ingenious discoveries and careful fact-gathering. Looked at from a historical perspective, he said, science exhibits an uneven and fitful course of development with sudden leaps of insight following long periods of

37 Reichenbach, H. (1938)

38 Kuhn, T.S. (1962) p9

39 " " " "

stagnation thanks to the scientist donning, as it were, 'a different kind of thinking cap' with which he handles 'the same bundle of data as before, but placing them in a new system of relations with one another by giving them a different framework.'⁴⁰

This 'thinking cap' idea is, in Kuhn's theory, included in the much more elaborate concept of 'paradigm', a term which has perhaps received closer scrutiny than any other in modern philosophy of science. This is understandable for the 'paradigm' is central to Kuhn's theory and the theory itself has been turned into something of a cult with a ritual search for paradigms, crises, revolutions and other Kuhnian phenomena in such varied disciplines as economics, bibliography, anthropology and linguistics. Kuhn's use of paradigm is a reflection of his sociological-cum-historical approach to science. Philosophers of science traditionally offer what are called 'rational reconstructions' of scientific theories which are not meant to coincide with what the actual scientists, in the laboratory as it were, took themselves to be doing. These reconstructions spell out exactly which propositions are involved in each theory, how they relate to each other, to evidence marshalled in what are called 'basic statements' or some such, and so the philosophers attempt to explain the content of each theory. Kuhn, in contrast, uses the idea of a paradigm to convey what he takes to be an important omission from the traditional account, namely that there are global and fundamental presuppositions of a highly intricate nature which unite the scientists working within a tradition.

Nowhere in his essay does Kuhn define a paradigm for the very good reason that it is part of his thesis that a paradigm cannot be

40 Butterfield, H. (1949) pl

expressed in words. He does, however, offer many partial characterisations of a paradigm to get his message across. Paradigms are initially introduced as 'universally recognised scientific achievements that for a time provide model problems and solutions to a community of practitioners.'⁴¹ If we look at science, says Kuhn, we are struck by the fact that for long periods scientists are amazingly united in what they take to be important problems, how those problems ought to be tackled and what a solution to those problems would look like. Kuhn introduces the notion of a paradigm to emphasise this unity on fundamentals, this unquestioning commitment to 'global'⁴² presuppositions. What happens, he says, is that some particular scientific achievement is:

'... sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it (is) sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve.

Achievements that share these two characteristics I shall henceforth refer to as 'paradigms', a term that relates closely to 'normal science'. By choosing it, I mean to suggest that some accepted examples of actual scientific practice - examples which include law, theory, application and instrumentation together - provide models from which spring particular coherent traditions of scientific research.'⁴³

Normal science springs, he says, from commitment to a paradigm; once committed the scientist is provided with a pair of blinkers which simultaneously focuses attention on specific, relatively esoteric puzzles for detailed investigation and blinds the scientist to problems which are not defined by the paradigm. Kuhn refers to paradigm 'puzzles' because, by analogy with jig-saw puzzles, the scientist is convinced that there is indeed a solution if only he is sufficiently

41 Kuhn, T.S. (1962) p10
42 " " " p43
43 " " " p10

ingenious or lucky to hit upon it. Failure to find the solution reflects directly on the researcher in contrast to any potential problem solver; failure to solve a problem, says Kuhn, is not likely to reflect badly on a student if there is no guaranteed solution to the problem. The initial promise of the paradigm is tested during normal science, the activity in which most scientists are professionally engaged most of the time. Normal science is, he says, constituted by 'mopping-up operations'⁴⁴ which extend 'the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between these facts and the paradigm's predictions, and by further articulation of the paradigm itself.'⁴⁵ Normal science is 'an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies.'⁴⁶

But there is more to paradigms than this. It is true, says Kuhn, that members of the same paradigm share largely the same rules and standards for scientific practice. But they also share something deeper, more fundamental, something that cannot be put into words.

'Normal science is a highly determined activity, but it need not be entirely determined by rules. That is why ... I introduced shared paradigms rather than shared rules, assumptions, and points of view as the source of the coherence for normal research traditions. Rules, I suggest, derive from paradigms, but paradigms can guide research even in the absence of rules.'⁴⁷

When the historian looks at a scientific tradition, says Kuhn, he experiences an acute difficulty if he tries to express in propositional form the rules and principles which every member of the tradition subscribes to. It is doubtful if the historian could ever produce a set of rules that would win universal support from the members of that

44 Kuhn, T.S. (1962) p24
45 " " " "
46 " " " "
47 " " " p42

tradition. This leads Kuhn to argue that behind the open diversity of opinion on what the rules and principles are lies an invisible uniformity, a harmony of inexpressible commitments that binds paradigm members together. The rules which each paradigm-member can state are abstractions from this hidden inexpressible paradigm.⁴⁸ Without such a paradigm a scientific tradition would not exist.

'Scientists can agree that a Newton, Lavoisier, Maxwell or Einstein has produced an apparently permanent solution to a group of outstanding problems and still disagree, sometimes without being aware of it, about the particular abstract characteristics that make those solutions permanent. They can, that is, agree in their identification of a paradigm without agreeing on, or even attempting to produce a full interpretation or rationalisation of it. Lack of a standard interpretation or of an agreed reduction to rules will not prevent a paradigm from guiding research. Normal science can be determined in part by the direct inspection of paradigms, a process that is often aided by but does not depend upon the formulation of rules and assumptions. Indeed, the existence of a paradigm need not even imply that any full set of rules exists.'⁴⁹

A good indication of what Kuhn intends to convey in his use of the paradigm idea is seen in a reference he makes to a 'very similar theme'⁵⁰ developed by the philosopher-scientist Michael Polanyi. Scientific knowledge, according to Polanyi, is personal; it has an ineluctable, subjective element which he calls 'tacit knowledge'. Science is a continuation of perception, he says. Perception takes place through the unconscious integration of formerly disparate phenomena; scientific knowledge grows through a similar integration of hitherto unknown coherences in nature. 'Our recognition of these coherences is largely

48 Kuhn is inconsistent on the within-paradigm agreement or disagreement on rules and standards. At times he says that members share the same rules (p11). At times, he says that they do not (p44).

49 Kuhn, T.S. (1962) p44

50 " " " "

based,' he writes, 'like perception is, on clues of which we are not focally aware and which are indeed often unidentifiable.'⁵¹ What we take to be scientific knowledge is ultimately but a personal decision. Knowledge is, then, a psychological response by an individual scientist, a response resulting from perceptual or intellectual processes of which the scientist is unaware. Kuhn's theory differs from Polanyi's in that scientific knowledge is not identified with a decision by the individual scientist but with the social activities of the community that shares the paradigm. Polanyi's thesis is psychological; Kuhn's is sociological. Knowledge is, respectively, what the individual or the group take it to be. On both accounts scientific knowledge is ultimately unfathomable.

In stressing the social basis of science Kuhn acknowledges the similarity between his own views and those of the Wittgenstein of the Philosophical Investigations.⁵² Consideration of how a child learns to use the word 'game' leads Wittgenstein to deny that there is a set of characteristics applicable to all and only members of what we come to call games. The child learns to apply the term correctly because he sees what Wittgenstein calls, for short, 'family resemblances' between the event in question and previous activities which he has learned to call 'games'. Something similar, says Kuhn, may happen when a scientist is introduced to a paradigm; in an elusive manner the new recruit learns to recognise which problems may yield to solutions similar to those he has studied through immersion in the paradigm. The scientist does not learn an explicit set of rules with which to recognise a problem or with which he can produce a solution; yet he can still recognise problems and produce solutions which he knows are

51 Polanyi, M. (1972) p49

52 Wittgenstein, L. (1958)

legitimate. In Wittgensteinian language, the scientist knows more than he can say. Says Kuhn:

'That scientists do not usually ask or debate what makes a particular problem or solution legitimate tempts us to suppose that, at least intuitively, they know the answer. But it may only indicate that neither the question nor the answer is felt to be relevant to their research. Paradigms may be prior to, more binding, and more complete than any set of rules for research that could be unequivocally abstracted from them.' 53

In the case of both child and scientist the knowledge is acquired, according to these theorists, through involvement in a complex, social process which cannot be fully articulated. Kuhn thus admits that his theory is elusive in so far as he cannot spell out exactly what a paradigm consists of; he believes that psychology and especially the study of perceptual processes may eventually provide further illumination of how a paradigm determines a scientist's thought.

Paradigms also play a central role in Kuhn's account of the dynamics of scientific change. True to his sociological perspective this change results, he says, not from the intentional activity of the individual scientist but from the unintentional effects of individual paradigm-members each attempting to solve specific puzzles.

'The scientific enterprise as a whole does from time to time prove useful, open up new territory, display order, and test long-accepted belief. Nevertheless, the individual engaged on a normal research programme is almost never doing any one of these things. Once engaged, his motivation is of a rather different sort. What then challenges him is the conviction that, if only he is skilful enough, he will succeed in solving a puzzle that no one before has solved or solved so well.' 54

In brief, Kuhn argues that once a group of scientists is drawn to work on what the members take to be a promising scientific achievement they

53 Kuhn, T.S. (1962) p46

54 " " " p38

attract, as the newly initiated normal science gets up steam, more and more support from fellow scientists. However, because the commitments which all paradigm-members share retain, in Kuhn's words, 'an element of the arbitrary',⁵⁵ it is inevitable that sooner or later the paradigm will run into trouble. When the paradigm's predictions do not square with experimental results we have what Kuhn calls 'anomalies'. Such anomalies are, at first, set aside as likely to yield to further, more ingenious research but if, after an unspecified time, they remain recalcitrant to repeated efforts to eliminate them then the paradigm has run, he says, into crisis. At this point what agreement there was over the rules which ought to govern scientific practice is fractured and the individual members are forced into a radical rethink of the most fundamental issues, puzzle-solving ceases and polemics or philosophy often take over. A battle ensues between rival paradigm-candidates and normal science emerges once again only after one of the rivals attracts enough support to repeat the process already outlined.

Before studying the difficulties which arise from Kuhn's theory I would first like to resolve the dispute between Weimer and Palermo and their critics, a dispute resulting, as we have seen, from differing interpretations of Kuhn's theory. First of all there is disagreement over the admissibility or otherwise of more than one paradigm within a discipline at any one time. Weimer and Palermo, it will be remembered, assert that paradigms do not belong to a discipline but to a community of practitioners. Their opponents hold that a discipline is supposed to have only one paradigm, at least during a normal science period - hence behaviourism, they continue, was not a Kuhnian paradigm. There

55 Kuhn, T.S. (1962) p5

is some support for Weimer and Palermo in Kuhn's original essay. In introducing the paradigm idea Kuhn, we have seen, asserted that it belonged to a community of practitioners. And this is a view he has emphasised in his later writings.⁵⁶ However, there is much more support for the alternative interpretation that a paradigm is all-pervasive within a discipline during a normal science period. This is evident both from the examples of paradigms Kuhn uses in the history of science and from the account of paradigm formation and change which occupies most of the initial argument. For example, he writes that once a successful normal science period is under way, '... the profession will have solved problems that its members could scarcely have imagined and would never have undertaken without commitment to the paradigm.'⁵⁷ Or, referring to a new paradigm's redefinition of scientific problems and standards, he says: 'Other problems, including many that had previously been standard, are rejected as metaphysical, as the concern of another discipline or sometimes as just too problematic to be worth the time.'⁵⁸ But the imperialism attributed to paradigms within disciplines is most clearly seen in Kuhn's view of scientific revolutions.

'At the start a new candidate for paradigm may have few supporters, and on occasions the supporters' motives may be suspect. Nevertheless, if they are competent, they will improve it, explore its possibilities, and show what it would be like to belong to the community guided by it ... More scientists will then be converted and the exploration of the new paradigm will go on ... Still more men, convinced the new view's fruitfulness, will adopt the new mode of practising normal science, until at last only a few elderly hold-outs remain. And even they, we cannot say, are wrong. Though the historian can always find men - Priestley, for instance - who were unreasonable to resist for as long as they did, he will not find a point at which resistance becomes

56 Kuhn, T.S. (1972)
 57 " " (1962) p25
 58 " " " p37

'illogical or unscientific. At most he may wish to say that the man who continues to resist after his whole profession has been converted has ipso facto ceased to be a scientist.' 59

The entire thrust of his argument is that science progresses in a cyclical manner from one paradigm to another with each successful paradigm defining the issues for the discipline concerned and excluding as not part of the discipline those individuals who refuse, despite being in a tiny minority, to throw in the towel. Since Kuhn's is a descriptive and not (at least initially) a normative theory of science he is not concerned to justify or defend such exclusion. Kuhn offers no logical arguments against the views of this minority - indeed, as we can see, it is part of his theory that there can be no such arguments. Thus, if a paradigm was defined by, or constituted by, a set of propositions or logical arguments there could be no objection to the view that a discipline may, on Kuhn's theory, possess more than one paradigm. But as a paradigm is such that it cannot be defined and is used, albeit vaguely, to describe scientific activity as a social process we must accept that Kuhn's thesis allots each discipline one paradigm during a normal science period. Peripheral malcontents who refuse to accept the paradigm do not themselves form another paradigm until the predecessor has run into crisis and they win over defecting revolutionaries. Consequently it is only by ignoring a central feature of Kuhn's theory that Weimer and Palermo can claim that structuralism and behaviourism were psychology's first two paradigms.

Consider now another issue which divides Weimer and Palermo from their critics, namely the role of a crisis in Kuhn's theory. This dispute can easily be resolved for Weimer and Palermo are, quite simply, mistaken in their claim that 'Kuhn has not insisted that crises were

indispensable precursors to revolutions.⁶⁰ The short answer to this is that Kuhn has insisted, over and over again, that crisis is a necessary, if not sufficient, condition for a revolution. Thus he writes:

'If awareness of anomaly plays a role in the emergence of new sorts of phenomena, it should surprise no one that a similar but more profound awareness is prerequisite to all acceptable changes of theory. On this point historical evidence is, I think, entirely unequivocal.'⁶¹

Kuhn immediately goes on to argue that Ptolemaic astronomy was 'a scandal' before Copernicus, Aristotle's theory of motion was 'in difficulties' before Galileo, the wave theory of light followed the discovery of anomalies in Newton's theory; in sum, he writes:

'... in all these cases except that of Newton the awareness of anomaly had lasted so long and penetrated so deep that one can appropriately describe the fields affected by it as in a state of growing crisis.'⁶²

Further, he claims that a new theory or paradigm-candidate will only get attention if the paradigm it hopes to replace has run into a state of crisis. It often happens, he says, that when a new paradigm does emerge triumphant from a period of extraordinary science it is found to have been anticipated by a theory which was ignored simply because it had not made its debut during a breakdown in normal science. So Aristarchus' anticipation of Copernicus was ignored because 'the vastly more reasonable geocentric system'⁶³ had no difficulties that a heliocentric system would solve. Emphasising the role of crisis he says:

'... these examples share another characteristic that may help to make the case for the role of crisis impressive: the solution to each of them had been at least partially anticipated during a period when there was no crisis in the corresponding science; and in the absence of crisis those anticipations had been ignored.'⁶⁴

60 Weimer, W.B. and Palermo, D.S. (1973) p237

61 Kuhn, T.S. (1962) p67

62 " " " "

63 " " " p75

64 " " " "

Not only does Kuhn argue that a crisis is a prerequisite to a revolution but he insists that that crisis must be generated from within normal science. As Palermo's critics pointed out the reasons he offered for structuralism's alleged crisis all point to origins outside normal science. Again, only by distorting Kuhn's theory can it be applied to structuralist psychology.

Finally, let us examine the dispute between Weimer and Palermo and their opponents on whether there can be 'debate over fundamentals' within a paradigm. Controversy is permitted, according to Weimer and Palermo, provided that 'what is "fundamental to the paradigm" is not questioned during normal research.'⁶⁵ This allows them to characterise behaviourism as a paradigm with controversy raging over fundamental theoretical issues, but fundamental issues not basic to the paradigm. However, Weimer and Palermo go even further and allow controversy over the commitments which, as they put it, 'constitute the sociological and/or metaphysical usage of paradigm.'⁶⁶ They write:

'Researchers may share the same paradigm even though they need not accept exactly the same commitments, and even if they do not interpret those commitments which they do accept in a uniform manner. There is thus room for considerable disagreement within the paradigm. Assent to a sociological paradigm is never an all-or-none matter across individuals: one researcher's position on the relevant cluster of commitments may be totally unique with respect to another researcher's, yet they may both share the 'same' paradigm.'⁶⁷

Given that they agree that "what is fundamental to the paradigm" is not questioned during normal research and that both explicit paradigm commitments as well as theoretical issues may be disputed it is odd that Weimer and Palermo do not claim that what is fundamental to the

65 Weimer, W.B. and Palermo, D.S. (1973) p217
66 " " " " " " p220
67 " " " " " " "

paradigm is the invisible, inexpressible web of presuppositions which are prior to any open, expressed commitments or theories. Unlike Kuhn they ignore this presumed hidden uniformity lying behind all paradigm articulations and offer, instead, a characterisation of both structuralism and behaviourism in terms of a cluster of commitments.⁶⁸ But such a characterisation is, for Kuhn, impossible. Even if it were possible the admission that individuals who are held to belong to the 'same' paradigm can openly disagree about such commitments robs the paradigm of any explanatory value it may have. For then it can be attributed to a group which shares specified views and equally to an individual who disagrees with that group.

It is difficult to settle this dispute with finality because it is not at all clear what is and what is not fundamental to a paradigm in Kuhn's theory. Hence, it is not clear what kinds of dispute are permissible between individuals who are said to belong to the 'same' paradigm. Nevertheless, the emphasis in Kuhn's theory is on agreement on problems, methods, and solutions resulting from shared presuppositions imbibed through communal research based on a substantive scientific achievement. Weimer and Palermo, in contrast, tolerate and even emphasise disagreement within a paradigm as a result of minimising the primary Kuhnian usage of paradigm-as-exemplar.⁶⁹ As Mackenzie⁷⁰ has

68 In this attempt Weimer and Palermo ignore the implication of their own position on permitted controversy over commitments between members of the same paradigm. If people who disagree on commitments can be said to belong to the same paradigm it is strange that these authors should go on to write: 'No structural psychologist could deny associationism ... and remain within the 'New Psychology' ' (paradigm). (1973) p222

69 Kuhn now distinguishes between two senses of paradigm which, he says, were originally confused. These are, respectively, the sense of paradigm as exemplar or concrete scientific achievement accepted by a community and as a disciplinary matrix or shared presuppositions which account for the relatively unproblematic nature of scientific judgements over problems, methods and acceptable solutions.

70 Mackenzie, B.D. (1972)

noted, in criticism of Palermo, behaviourism cannot be construed as a paradigm because it was not based on a major scientific achievement or exemplar; by focusing on the secondary usage of paradigm as a set of commitments or disciplinary matrix to which the members are only loosely attached and by minimising the primary usage of paradigm Weimer and Palermo are led astray. In sum, only by distorting Kuhn's theory can it be made to apply to the heterogeneous activities that constituted neobehaviourism.

In conclusion, therefore, the extension of Kuhn's theory to psychology, as outlined by Palermo and later modified by Weimer and Palermo, is rejected. Both accounts advance flawed versions of Kuhn which, as we have seen, misunderstand the role of crisis, the exclusiveness of paradigms within disciplines, the extent of fundamental agreement and the source of that agreement. In defending the theory against Weimer and Palermo's interpretation of it I do not wish to suggest that, accurately presented, it would prove enlightening or useful to contemporary psychologists. On the contrary, Kuhn's theory - if taken seriously by psychologists - is a recipe for confusion and combined with its associated methodological implications would lead to a demand for conformity to those confused doctrines. To defend my assertion I now turn to examine the problems raised by the theory itself.

Is Kuhn simply offering a description of what he thinks scientists have actually done in the past or is he advancing a methodological prescription as to what science ought to be like? Is Kuhn suggesting to scientists that they ought to behave in certain ways? Several of Kuhn's readers have admitted to considerable puzzlement as to how his work should be read. Among these readers is the self-styled epistemological anarchist Paul Feyerabend. He reports that many social scientists have taken Kuhn to be telling them how they

might turn their struggling, factious and apparently stagnant fields into mature science which would then be on a similar footing with the envied natural sciences. Says Feyerabend:

'The recipe, according to these people, is to restrict criticism, to reduce the number of comprehensive theories to one, and to create a normal science that has this one theory as its paradigm. Students must be prevented from speculating along different lines and the more restless colleagues must be made to conform and 'to do serious work'. Is this what Kuhn wants to achieve?' 71

Feyerabend goes on to resolve his own puzzlement by concluding that Kuhn intends his theory to be an ambiguous one so that he can, at one and the same time, 'give solid, objective, historical support to value judgements which he just as many other people seem to regard as arbitrary and subjective'⁷² and leave himself a safe second line of retreat to the claim that his view is purely descriptive and no implied derivation of values from facts is intended.

In response to criticism Kuhn has made his position clear. He says:

'The structure of my argument is simple and, I think, unexceptionable; scientists behave in the following ways; those modes of behaviour have (here theory enters) the following essential functions; in the absence of an alternative mode that would serve similar functions, scientists should behave essentially as they do if their concern is to improve scientific knowledge.'⁷³

In other words Kuhn denies that his theory is ambiguous, intentionally or otherwise and claims, merely to 'tell it as it is'. He is well aware that he evaluates the behaviour he claims to have uncovered and in the light of that evaluation, given certain aims, he recommends that scientists ought to behave in the way he described. Now this analysis of what Kuhn is doing is open to serious criticism which, in effect,

71 Feyerabend, P. (1972) p198

72 " " " p199

73 Kuhn, T.S. (1972) p237

denies that he is primarily advancing historical discoveries with a methodology based upon them and asserts instead that Kuhn is, in reality, proposing a philosophy of science based on an incoherent, dogmatically held theory of meaning. The structure of his argument, I suggest, is not as simple as he suggests and every part of it is unsatisfactory.

Consider, first, the claim that he is merely describing the behaviour of scientists. Before Kuhn can study any scientist or scientific event in the past he must, initially, employ some standard or criterion which allows him to classify the person or event concerned as part of the history of science. Before he can do history of science he thus needs a philosophy of science or, less grandly, some intuitive notion to help him decide what is and what is not part of scientific history. Using this tacit standard to select his material for more detailed study Kuhn then goes on to argue that scientists join paradigms, practise normal science, work themselves into a crisis before being converted during revolutions and so on. These are part of the complex explanatory theory which Kuhn offers to account for the phenomena he is investigating. He is not merely describing the behaviour of scientists and making startling factual discoveries; he is both proposing a criterion of what constitutes scientific activity and offering a theoretical analysis of that activity.

Long before Kuhn's essay was written this entire approach to the study of science was attacked by Popper⁷⁴, directing his fire against those who sought to show that historically a principle of induction was used in science. This naturalistic approach to the problem of method in science, as he termed it, tries to solve its problems by studying the actual behaviour of scientists or the actual procedures

74 Popper, K.R. (1934)

of 'science'. This is a mistake, he wrote:

'What I call 'methodology' should not be taken for an empirical science. I do not believe that it is possible to decide, by using the methods of an empirical science, such controversial questions as whether science actually uses a principle of induction or not. And my doubts increase when I remember that what is to be called a 'science' and who is to be called a 'scientist' must always remain a matter of convention or decision.

... Thus I reject the naturalistic view. It is uncritical. Its upholders fail to notice that whenever they believe themselves to have discovered a fact, they have only proposed a convention. Hence the convention is liable to turn into a dogma.' 75

Popper is here arguing that history of science cannot yield the kind of results Kuhn is claiming to have found. Embedded in the historian's or sociologist's approach is a theoretical standpoint which prevents him from directly examining the 'facts of history'. In short, Popper denies that there are neutral historical facts which can be directly inspected to find out whether science uses a principle of induction, exhibits cyclical revolutions or works by a method of conjectures and refutations. Which 'facts' are selected for study and what is regarded as significant - what is learned from history - depends on the criteria or conventions which are built into the observer's perspective.

A very similar line of argument has been followed, though independently, by Kuhn's critics. They have asserted that paradigms, normal science, crises and the other phenomena Kuhn has 'discovered' do not exist at all and are merely reflections of his own philosophical perspective. The linchpin of Kuhn's thesis, the paradigm, has been the focus of the attack but some investigators have turned their criticism to other aspects. Thus Feyerabend⁷⁶ objects to the periodicity in Kuhn's account with normal science dominating a paradigm before it gives way to extraordinary or crisis science. Dissent,

75 Popper, K.R. (1959) p52/53

76 Feyerabend, P. (1972) p208

proliferation of competing theories, recourse to fundamentals and other allegedly crisis features are, he says, present throughout science and exist alongside what he - Feyerabend - calls boring, uncritical normal science activity. However, I shall concentrate on the paradigm idea which, we have seen, is introduced by Kuhn to solve what he sees as a dilemma. On the one hand, he is struck by the amazing uniformity among scientists working within a tradition on what are the important problems to be solved, the legitimate methods to be employed and the solutions which will be acceptable; on the other hand, he experiences acute difficulty in finding a form of words to summarise the tradition in a way which he feels would satisfy every member of it. Each scientist expresses the theory they are all working on in a slightly different way from everyone else. Clearly, Kuhn concludes in what is a crucial move in his argument, behind this open diversity there is a deeper, more fundamental, invisible, inexpressible 'paradigm' of which each scientist's formulation is an 'articulation' or partial expression.

Kuhn then allots this paradigm a key role in his account of scientific change. Paradigms define the standards and rules which govern research during 'normal science'. Different paradigms provide different standards and rules; thus they direct their members to different puzzles and differ in the solutions they deem 'scientific'. Reflecting the Wittgensteinian nature of a paradigm as a social process or 'form of life' for a community Kuhn paints a vivid picture of pre-revolutionary debate:

'To the extent, as significant as it is incomplete, that two scientific schools disagree about what is a problem and what is a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms. In the partially circular arguments that regularly result, each paradigm will be

'shown to satisfy more or less the criteria that it dictates for itself and to fall short of a few of those dictated by its opponent. There are other reasons too, for the incompleteness of logical contact that consistently characterises paradigm debates.' 77

This 'incompleteness of logical contact', characteristic of 'forms of life', leads Kuhn to the conclusion that different paradigms are often 'incommensurable'.⁷⁸

Scientists can settle their problems by discussion and reference to the same standards only when they belong to the same paradigm. But a cool, disinterested appraisal of two paradigms is not possible. Since each person argues from within his own framework he is unable, says Kuhn, to provide 'logically or even probabilistically compelling'⁷⁹ reasons why a paradigm should be adopted or abandoned. There is no possibility of choosing between paradigms by reference to overarching standards.⁸⁰

Typically scientists arguing across the paradigm divide talk past one another and what is good reasoning or sound argument for one cuts no ice with the other. Thus, Kuhn insists, argument does not play a decisive role in persuading a member of an old, crisis-ridden paradigm to join a new one. He repeatedly claims that the scientist is merely converted to the new way of seeing things which the parvenu provides. Just as a paradigm cannot be reduced to a list of logical propositions so conversion cannot be reduced to a set of arguments. 'The competition between paradigms,' he says, 'is not the sort of battle that can be resolved by proofs'.⁸¹ With this not-yet-understood conversion sequence the upshot of Kuhn's argument is a complete relativism which

77 Kuhn, T.S. (1962) p110
78 " " " p112
79 " " " p94
80 " " " "
81 " " " p148

he presents, reluctantly and with attempts at modification, as a historical revelation.

Shapere⁸² argued, in a highly critical review, that such relativism is a consequence not of an investigation of the history of science but of Kuhn's own theoretical approach. Shapere drew attention to the necessity of paradigms as the sine qua non of research, according to Kuhn. 'Such views,' wrote Shapere, 'appear too strongly and confidently held to have been extracted from a mere investigation of how things have happened.'⁸³ Paradigms tell us more about Kuhn, he implied, than they do about science. They tell us, in Shapere's view, about Kuhn's theory of meaning. Where the logical empiricists had emphasised meaning-invariance, the unproblematic use of the same concepts in different theories, Kuhn emphasises meaning-variance - the view that a concept has two radically different meanings in different paradigms - with a similar, fatal excess. This theory of meaning emerges clearly, says Shapere, with the assertion that different paradigms are incommensurable. In Shapere's words:

'Two expressions or sets of expressions must either have precisely the same meaning or else must be utterly and completely different. If theories are not meaning-invariant over the history of their development and incorporation into wider and deeper theories, then those successive theories (paradigms) cannot really be compared at all, despite apparent similarities which must therefore be dismissed as irrelevant and superficial. If the concept of the history of science as a process of "development-by-accumulation" is incorrect, the only alternative is that it must be a completely noncumulative process of replacement.'⁸⁴

The corollary of the claim that there is absolute difference between paradigms is that there is absolute sameness or uniformity within a paradigm. Thus Kuhn assumes that there is a deep and fundamental unity

82 Shapere, D. (1964)

83 " " " p386

84 " " (1966) p68

lying behind the surface differences between individuals in a scientific tradition. Indeed, we have seen that he claims there could not be a tradition without such unity. Thus Kuhn assumes that communication is only possible between individuals who are agreed on fundamentals. This is the logical thesis of relativism and it thus is built into Kuhn's theory of science in the guise of the paradigm. As Popper had argued, the naturalistic approach to the study of method in science leads to the 'discovery' of those very conventions or theories which the historian brings to his task.

One of the oddest features of Kuhn's essay is the claim that paradigms can be 'directly inspected' (see quotation on page 70 above) by the scientists who join them and by the historian who studies them. It is odd because one of the basic motives for using the paradigm, as Shapere points out, is to deny that any scientist can ever study neutral observational data directly. All data are examined through a paradigm. Yet the historian or sociologist, it is claimed, can dispense with paradigms or metaparadigms in his own research. Why Kuhn should liberate the historian of science from the constraints he places on other empirical investigators is not clear. But he gives no reason why it should be possible to inspect and identify paradigms directly if it is impossible to similarly examine other empirical phenomena.

In summary, then, when Kuhn claims that the first part of his argument is simply that 'scientists behave in the following ways'⁸⁵ he distorts what he is in fact doing. First, he is employing some implicit criterion of what constitutes scientific activity and who is to be considered a scientist. Second, he is advancing a complex explanatory theory to account for the activity he has circumscribed.

85 Kuhn, T.S. (1972) p237

It seems to me that the general argument advanced by Popper that history of science cannot explicate the nature of science and is dependent on conventions or criteria provided by a philosophy of science, is correct. Different historians produce very different accounts of the 'same' episode. A historian who holds a philosophy which views successive scientific theories as a continuous, series of more and more comprehensive viewpoints with each earlier one merely an approximation to the later version, will produce a cumulative historical account of science. Similarly, we can read in Agassi⁸⁶ a historiography of science generated by Popper's fallibilist epistemology. Kuhn's history of science with its revolutionary, relativist conclusions similarly reflects the conventions he implicitly adopts. Histories of science are based on philosophies of science whether the historian is conscious of the fact or not; the historian cannot escape the need to implement selectional and evaluative criteria before he starts his research. The root of Kuhn's relativism and of his revolutionary thesis lies in the imposition of paradigms with their built-in relativist philosophy.

The second part of Kuhn's argument, as he sees it, is that the scientific behaviour he describes has the function of promoting the growth of knowledge - though this entails, he admits, a change in what we mean by such a conception. Since there is no way by which we might compare successive, incommensurable paradigms against all-embracing objective standards, says Kuhn, 'we may well have to abandon the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth.'⁸⁷ The growth of knowledge is best understood, by analogy with the Darwinian

86 Agassi, J. (1963)
87 Kuhn, T.S. (1962) p170

model of evolution, as a development away from primitive beginnings by 'a process whose successive stages are characterised by an increasingly detailed and refined understanding of nature.'⁸⁸ Like biological evolution scientific development is, he says, 'unidirectional and irreversible.'⁸⁹

In the face of Kuhn's admission that different paradigms cannot be compared and especially his assertion that one paradigm cannot be said to be closer to the truth than another it is necessary to ask what sense can be given to the ideas of development, evolution and progress in this context? In epistemology, to use these terms is to imply that one theory or paradigm is an improvement over its predecessors and to say that is to imply that they can be compared. But, according to Kuhn, no such comparison is possible. Hence, his critics asked, is Kuhn saying that every and any paradigm switch which scientists make is automatically a progressive change? In the absence of some means of comparison against common standards is Kuhn's argument that change is progressive per se? This has drawn the reply that it is possible, after all, to compare competing theories or paradigms against the same criteria to determine which is latest in the evolutionary chain. Now though this contradicts Kuhn's own incommensurability thesis it does not provide a satisfactory defence of the use of the notions of progress, development and evolution. Kuhn wrote:

'I believe it would be easy to design a set of criteria - including maximum accuracy of predictions, degree of specialisation, number (but not scope) of concrete problem situations - which would enable any observer involved with neither theory to tell which was the older, which the descendant.'⁹⁰

Kuhn's answer is unsatisfactory because he does not provide an aim for

88 Kuhn, T.S. (1962) p170
89 " " (1972) p264
90 " " " "

science. A scientist, who aims (among other things) to arrive at a true theory, will specify criteria which will allow him to judge whether or not any particular theory is helping him achieve his aim. Thus he will, for example, say that the theory is superior to another if it passes tests (meets the criteria) which the other has failed. The reason why it is an improvement for the scientist is that it has, in his judgement, brought him closer to achieving his aim than any other alternative theory. Kuhn argues, in contrast, that individual scientists do not have aims such as promoting the growth of knowledge or the pursuit of absolutely true theories; rather, growth of knowledge is an unintended outcome of community-based normal science. Hence the criteria which Kuhn introduces as an index of progress are not criteria which measure the extent to which aims are judged to have been achieved. In fact, he gives no reason why we should accept these criteria as an index of progress. And since his theory is not a prescriptive one - it does not tell us what science ought to be like - any criteria such as he introduces will be arbitrary and hence there is no reason why we should accept them as an index of evolutionary progress. Thus I reject the second part of what Kuhn takes to be the structure of his argument (see page 80 above) - namely that the scientific activity which his essay describes has the function of promoting the growth of knowledge. If the 'scientific behaviour' which he claims to have discovered as historical phenomena were accepted as correct description then the conclusion would have to be that science does not progress by reasoned argument nor is it judged against objective standards but is, instead, as Feyerabend insists, irrational. If it is the case that scientists dogmatically commit themselves to a view to which they have been blindly converted and stubbornly persist with such narrow-minded intolerance until suddenly converted again then, I

do not think Feyerabend's usage is unfair. But I will leave consideration of his philosophy to a later chapter. At this point, having rejected the first two planks of Kuhn's argument, I will now, briefly, dismiss the final part - the methodological recommendation that scientists ought to do what Kuhn claims they have, in fact, done throughout their history.

'Acquisition of a paradigm', writes Kuhn⁹¹, 'and of the more esoteric type of research it permits is a sign of maturity in the development of any given scientific field.' The search for Kuhnian paradigms in the social sciences, a growth industry during the past decade, is but the latest manifestation of the traditional envy with which the natural sciences are copied by their struggling counterparts in social fields. As with Weimer and Palermo this has remained at the level of after-the-fact appraisal of the state of a discipline, the reconstruction of past movements along Kuhnian lines; to my knowledge there has been little discussion of the methodological implications of Kuhn's theory for the disciplines concerned. However, since it is normal science which results in progress - through its pursuit of detailed research on 'esoteric' puzzles and indirectly, through its generation of crises and revolutions, via the inevitable anomalies - Kuhn recommends that scientists practise normal science. 'Scientists should behave essentially as they do', he says, in the third part of his argument (p80 above), 'if their concern is to improve scientific knowledge.' They should, that is, put their deeper, philosophical differences aside and conform to a single point of view which thus becomes dominant.

But there is a difficulty here since paradigms are not so much something which an individual scientist actively embraces as something

91 Kuhn, T.S. (1962) p11

which happens to the individual. The scientist is, after all, converted to the paradigm in an as yet little understood process. Normal science, thus, is not something which an individual can consciously decide to practise; it is something which happens to him, something which he finds himself doing. Maturity comes to a discipline and it is not at all clear that the maturation process can be forced. For those disciplines which are judged to already have a paradigm and an associated normal science tradition there is a similar difficulty in actually implementing Kuhn's recommendation. Suppose, for example, that we assume a discipline can be correctly said to have a paradigm and suppose further that it has become diseased by the pile up of anomalies. Kuhn's recommendation would now be, I take it, to create and then commit oneself to a new paradigm. However, he tells us that he has no idea how new paradigms come forward and so we have, again, to wait until the paradigm appears. He does not tell us what to do if we wish to escape from the old paradigm's crisis. Watkins⁹² has argued that, on Kuhn's own account, a scientist belonging to the old paradigm could never invent an alternative and thus the entire Kuhnian theory collapses. Watkins' argument is that since a paradigm is supposed to have a monopoly on a scientist's thinking, since 'there is little or no interregnum between the end of the old paradigm's reign over a scientist's mind'⁹³ and the start of the new paradigm's reign, and since the two paradigms are supposedly incompatible or incommensurable the new one would have to be invented and adopted in an instant. Says Watkins:

'We must remember that the new paradigm is immediately powerful enough to induce our scientist to turn against the well-articulated and unrefuted paradigm that has dominated his scientific thinking hitherto. This means, I take it, that the new paradigm cannot begin as just a

92 Watkins, J.W.W. (1972)

93 " " " " p34

'few fragmentary ideas, but must at the outset be large and definite enough for its striking potentialities to be fairly apparent to its inventor.

If that is so, the Instant-Paradigm thesis seems to me to be barely credible on psychological grounds. I do not know how much a single genius might achieve in the middle of the night, but I suspect that this thesis expects too much of him.' 94

At any rate there is no method which Kuhn recommends for developing new paradigms as rivals for the old; his position seems to be that the best way for scientists to progress is to wait until a first paradigm emerges or, if it has already emerged and run into crisis, to wait until a successor appears. Feyerabend, as we have seen, reports that Kuhn is taken to mean that scientists must take active steps to start normal science by inducing conformity to one dominant theme. But this does not, strictly speaking, follow from Kuhn's passive account. Conformity to a single theory is not sufficient to institute normal science. But once a paradigm emerges then he can be correctly said to recommend conformity to the paradigm and, as a corollary, a cessation of critical debate about possible alternatives. In sum, the methodology which flows from Kuhn's argument does promote conformity but the main lesson seems to be that there is little positive action which scientists can take to force the progress of science. This may explain the absence of proposals as to how best to develop their disciplines by those scientists who have implemented Kuhn's theory.

Finally, acceptance of any Kuhnian methodology would depend on prior adoption of the first two parts of his argument. Since I have rejected both his claim that he is accurately describing scientific behaviour and that the function of that behaviour is progress or the growth of knowledge there is obviously no reason to support methods based on such claims. However, in discussing Popper's methodology in

a later chapter, I will argue that some of the features of normal science which Kuhn says are 'uncritical' and whose continuance he recommends, can be absorbed into Popper's theory without difficulty.

In common with other historical relativists such as Hanson,⁹⁵ Toulmin⁹⁶ and Feyerabend, Kuhn has played an important part in the destruction of the once orthodox logical empiricist philosophy of science. That philosophy of science determined the course of mainstream Anglo-American psychology for a quarter of a century and exerts a continuing influence to this day. In the case study examined below I will review and critically, though briefly, discuss the origins and development of the logical empiricist movement and study its incorporation into behaviourist psychology. I will attempt to assess the strengths and weaknesses of that philosophy and the lessons to be learned for future research in psychology from its involvement with that philosophy. One of the lessons will be that psychologists, if they were to implement Kuhn's approach to science and, for example, urge commitment to cognitive psychology because it is a new paradigm would be repeating the mistakes of their predecessors. For, despite Kuhn's role in the rejection of logical empiricism, there are very close connections between Kuhn and the early logical positivists. Both are subjectivist epistemologies and, later in this thesis, both these views will be contrasted with their common enemy - the objectivist epistemology of Karl Popper which I will recommend as providing a basis for the approach psychology should follow.

95 Hanson, N.R. (1958)

96 Toulmin, S.E. (1967)

Part Two: A Case Study

Logical Positivism and Behaviourism

'With the behaviourist point of view now becoming dominant, it is hard to find a place for what has been called philosophy. Philosophy is passing - has all but passed and unless new issues arise which will give a foundation for a new philosophy the world has seen its last great philosopher.' 97

When John Watson wrote these words in 1928 he had good reason to think that psychology was well on its way to becoming a respectable natural science of behaviour, free - at last - from the debilitating and divisive influence of the philosophers. Within fifteen years following the publication of his 'Behaviourist Manifesto' the traditional introspective psychologies had largely withered away within the United States and American psychologists were no longer exercised by the perplexities of, what he called 'those time-honoured relics of philosophical speculation'⁹⁸ such as, for example, the mind-body problem. 'Most of the younger psychologists realise,' he wrote, with satisfaction, 'that some such formulation as behaviourism is the only road leading to science.'⁹⁹ However, in that same year, a great philosopher writing on the other side of the Atlantic published the first of a series of works for which behaviourists were soon to find a place. The philosopher was Rudolf Carnap and the works were to constitute the contribution of the Vienna Circle, a philosophical movement which was to transform the character of behaviourism and govern its fate for nearly thirty years. My primary concern, in this case study, is to illustrate the historical importance of philosophy of science for

97 Watson, J.B. (1928) p14
98 " " (1913) p166
99 " " (1924) pvii

psychology by spelling out the consequences for behaviourism of its involvement with logical positivism, as the philosophy of the Vienna Circle was soon to become known, and with that philosophy's American descendant, logical empiricism.¹⁰⁰

To understand the nature of classical behaviourism, which in common usage covers the period from 1913 to 1930, it is necessary to discuss two largely independent intellectual traditions which shaped the movement's initial formulation. These are functionalist comparative psychology which flourished in America at the beginning of this century and positivism, a set of philosophical doctrines which have enjoyed fluctuating support throughout history, at least as far back as the fourteenth century and, arguably, beyond.

Behaviourism started life as a revolt within American comparative psychology but very quickly made a take-over bid for the entire discipline. The background to behaviourism was, in brief, provided by functionalist psychology which was an American adaptation of the introspectionist, structural psychology practised in Germany by Wundt. Where Wundt had focused on the contents of human consciousness functionalist psychology was more concerned with the adaptive uses of consciousness by humans and by other animals. The method employed by comparative psychologists within the functionalist tradition combined observations of behaviour, inferences to capacities on the basis of that behaviour and detailed interpretation of those capacities by analogy with human experience. Given the aim of specifying what was going on in an animal's mind when it performed a piece of behaviour the

100 According to Herbert Feigl, one of the early members of the Vienna Circle, logical empiricism was the appellation preferred by many logical positivists after their emigration to the United States. It was, in part, meant to signify a move towards a more realist standpoint and a rejection of the criticism that in its emphasis on the role of experience in science this philosophy was "yet another version of subjective idealism." See Feigl's article, Wiener Kreis in America, (1968) p657.

functionalist psychologist could not rest content after postulating capacities which would produce that behaviour but proceeded to attribute specific conscious states to the animal by analogy with what a human would feel or experience in a similar situation. The method, as practised by functionalist psychologists from the late 1890's to around 1915, has been summed up by Boring. He writes:

'The rule of functional animal psychology of that date was that, when you have finished your observations of behaviour, you use the results to infer the nature of the animal's consciousness and then show how those processes function in the animal's behaviour.' 101

Since there is no way by which an experimenter can gain access to the supposed mental states of an animal and thus no means of testing claims about what the animal is feeling or thinking the functionalist comparative programme was doomed from the outset. How British comparative psychologists, who initially aimed to probe the mental states of animals, gradually subordinated and eventually eliminated the 'subjective' or analogical aspect of their investigations and hence avoided producing a situation such as that which produced behaviourism is told at length in Mackenzie's Blinded with a Great Hope.¹⁰² He also relates how frustration built up within functionalist comparative psychology, frustration resulting from the central aim of detailing the conscious content of the animal mind and from increasing constraints on uncontrolled speculation in experimental work. In short, if they were to fulfill their aims or even attempt to fulfill them these comparative psychologists were forced to indulge in uncontrolled speculation; if they did that then they violated their growing attachment to experimental rigour. The upshot was a polemical outburst by a man raised in the functionalist comparative tradition (and author of the

101 Boring, E.G. (1929) p556

102 Mackenzie, B.D. (1977, forthcoming)

revealingly titled work, The Psychical Development of the White Rat¹⁰³⁾ in the now famous paper of 1913 - Psychology as the Behaviourist Views It.

Henceforth psychology would be the science of behaviour and its goal prediction and control of that behaviour. Mind and consciousness as studied by the functionalist and introspective psychologists was to be eliminated from the subject matter of the discipline; behaviour, however, as studied by the functionalists was retained and therein lies a feature of behaviourism which was to lead, eventually, to the movement's demise. For the functionalists had enforced a rigid dualism between mind and consciousness, on the one hand, and behaviour on the other. The former was held responsible to its environment; behaviour itself was a very impoverished notion, stripped of mentality, adaptive and functional significance and amounting to nothing more than movement in space. It was this conception of behaviour which Watson passed on to his behaviourist movement. Behaviourism, Watson later wrote, 'attempted to make a fresh, clean start in psychology, breaking both with current theories, and with traditional concepts and terminology.'¹⁰⁴ In his dismissal of mind and consciousness he might be said to have made a fresh, clean start but there was nothing new in his formulation of behaviour. Nor, though this is much less important, was there anything new in his environmentalism, his assertion that 'in a system of psychology completely worked out, given the response the stimuli can be predicted; given the stimuli the response can be predicted.'¹⁰⁵ For this too was inherited from the 'passive organism' model of the later functionalist psychologists, the main difference being that the

103 Watson, J.B. (1903) This was Watson's doctoral thesis.
104 " " (1929) p4
105 " " (1913) p167

latter argued that behaviour was controlled via the environment's relection in consciousness.

In summary, Watson's behaviourism was at first a revolt against the view of psychology held by his fellow functionalist comparative psychologists but a revolt which, despite its seemingly radical nonsense flavour was heavily indebted to its predecessor.

Behaviourism's claim to fame, however, did not rest on the expulsion of mind and consciousness from the subject matter of animal psychology but on the further suggestion and policy for purging human psychology of subjective experience. 'The position is taken here,' he wrote, 'that the behaviour of man and the behaviour of animals must be considered on the same plane; as being equally essential to a general understanding of behaviour. It can dispense with consciousness in a psychological sense. The separate observation of 'states of consciousness' is, on this assumption, no more a part of the task of the psychologist than of the physicist.'¹⁰⁶ A scientific explanation of human activity will only be obtained, Watson maintained, when we 'bury subjective subject matter.'¹⁰⁷ Behaviour, where that is taken to be publicly observable - and hence, it was argued - objectively describable movement was asserted to be the only proper subject matter for a scientific psychology.

To justify the extension of the mentalist purge throughout all of psychology Watson appealed to the supposed practices of the physical sciences. He did not carry out a systematic or prolonged investigation of either the philosophy of science or of its methodology but merely contrasted existing psychology with the relatively successful natural sciences, such as physics and chemistry. For example, Watson argued:

106 Watson, J.B. (1913) p176

107 " " quoted in Koch, S. (1964) p7

'Psychology, as it is generally thought of, has something esoteric in its method. If you fail to reproduce my findings, it is not due to some fault in your apparatus or in the control of your stimulus, but it is due to the fact that your introspection is untrained. The attack is made upon the observer and not upon the experimental setting. In physics and in chemistry the attack is made upon the experimental conditions.' 108

Psychology, he went on, could be brought into line with the methods of natural science by focusing on observables alone.

'This suggested elimination of states of consciousness as proper objects of investigation in themselves will remove the barrier from psychology which exists between it and the other sciences.' 109

The appeal to presumed external standards of scientific practice as justification of behaviourism's claim to encompass the entire discipline was a very significant step because it linked the behaviourist programme to positivism, a philosophy of science then enjoying a revival and soon to be given its most sophisticated expression by the members of the Vienna Circle. Watson wanted a basis or rationale for the expulsion of unobservables like consciousness from every branch of psychology; positivism was taken to provide that rationale and hence to guarantee behaviourism's scientific status.

Whether or not Watson and his followers were correct in believing that positivism backed up their position is not so easy to establish; it all depends on what we mean by positivism. Today that term covers a variety of philosophical standpoints which reflect the movement's past and there is a persistent danger, in writing of positivism at any particular period, that we may attribute to that period 'positivist' arguments which were only developed at a later time. 'Each phase of positivist thought,' says a historian of that philosophy Leszek Kolakowski, 'is a specific variation of a dominant intellectual style.'¹¹⁰

- 108 Watson, J.B. (1913) p163
109 " " " p177
110 Kolakowski, L. (1972) p241

That dominant intellectual style is one which strongly recommends caution in claims to knowledge, is hostile to speculative and metaphysical thinking, urges clarity and precision in debate and steers clear of insoluble problems - preferring to tackle issues of a more pragmatic character. Nevertheless it is possible to identify certain rules or norms about knowledge which have characterised positivism in the past. Four such rules are singled out by Kolakowski and a brief examination of them will clarify the nature of classical behaviourism's positivism. These are the rule of phenomenalism, the rule of nominalism, the rule that refuses to call value judgements and normative statements knowledge and the rule that scientific method shares an essential unity in all spheres of enquiry.

The rule of phenomenalism states that 'there is no real difference between 'essence' and 'phenomenon','¹¹¹ This rule was aimed at metaphysicians who said the observable world of our experience is but a manifestation of a hidden reality which we can never know. For the phenomenalist anything which is in principle unobservable can play no role in science. Says Kolakowski:

'According to positivism, the distinction between essence and phenomenon should be eliminated from science on the ground that it is misleading. We are entitled to record only that which is actually manifested in experience; opinions concerning occult entities of which experienced things are supposedly the manifestations are untrustworthy. Disagreements over questions that go beyond the domain of experience are purely verbal in character.' 112

This rule is much less clear-cut than it at first appears for positivists do not object to all investigations of hidden causes of observable phenomena. They would not oppose, for example, the search for the cause of a disease of which specific symptoms are a manifestation.

111 Kolakowski, L. (1972) p11
112 " " " p11/12

What they do object to is the explanation of phenomena in terms of occult entities which are in principle inaccessible to human investigation. The rule is vague, however, for positivists have never been able to provide a criterion for distinguishing the occult from legitimate non-observables.

Watson's expulsion of mind and consciousness from psychology and his focus on observables would certainly correspond, I think, with the spirit (if I may use such a word) of this rule. But whether psychologists who might subscribe to this positivist rule are forced to become behaviourists is arguable. This rule of phenomenalism outlaws the admission of such entities as the Kantian Ding an sich for they are, in principle, unknowable. If mind and consciousness are held to be similarly occult then they would have to be eliminated in the same way. But a positivist might argue that consciousness is not as occult as say, the Ding an sich, on the grounds that each man's consciousness is capable of being investigated by each man himself. So he could consistently subscribe to the rule of phenomenalism and yet refuse to adopt behaviourism. Such refinements, according to Kolakowski, did not trouble behaviourists unduly and 'the older introspective psychology is dismissed as a web of irresponsible fantasies concerning the 'soul' and 'spiritual' faculties.'¹¹³

The rule of nominalism, which may be regarded as a consequence of the previous rule, says 'we may not assume that any insight formulated in general terms can have any real referents other than individual concrete objects.'¹¹⁴ Science makes use of a variety of concepts in its theories to which we may be tempted to award some existential status. Nominalists permit the construction of theoretical

113 Kolakowski, L. (1972) p220
114 " " " " pl3

concepts, e.g. the perfect gas, ideal resistance, etc, which may serve as a more precise and general description of reality, but they argue that such concepts are but human creations, mere aids to the understanding. Scientific knowledge, they say, is but a record of observed facts and the abstract concepts science may employ are but a shorthand notation. To yield to the temptation of regarding such constructs as really existing is to fill the world with metaphysical fictions. What is important in science is only that which experience obliges us to recognise.

Now this rule is also vague for it fails to provide a criterion for recognising metaphysical fictions, while permitting some theoretical concepts to eventually achieve existential status. However it reinforces phenomenism's emphasis on observables in science and may thus be said to support, in a general way, Watsonian behaviourism. But it does not seem to me that attachment to nominalism entails acceptance of behaviourism; for a man may admit descriptions of mental states as legitimate psychological data yet simultaneously assert that theoretical concepts employed in any explanatory theory are merely of instrumental status. In other words explanations are but summaries of observed facts and the facts, in this case, are held to be privately observed mental states.

Nor do the other two positivist themes singled out by Kolakowski require a psychologist to subscribe to behaviourism. The first of these denies that value judgements and normative statements are knowledge; they are but arbitrary decisions. Strictly speaking this rule follows from a combination of the previous two rules, with phenomenism denying that values or norms belong to this world and nominalism denying that they exist 'in themselves' or in some Platonic realm. So this rule is also compatible with behaviourism but it might

equally well be defended by an introspectionist psychology.

Finally, positivism affirms the unity of scientific method. We saw earlier that Giedymin has criticised the identification of a belief in unity of scientific method with positivism as both parochial and misleading. (See page 38) His point was that it is parochial in that many thinkers who hold other positivist viewpoints have not expressed any view on this topic; it is misleading in that some anti-positivists, like Popper, also affirm a similar unity-of-method thesis. Hence while it is true that Watson saw behaviourism as scientific because it employed a method which he presumed all the sciences shared, that is not particularly positivist in itself.

Positivism, then, as characterised by these four themes provides only general and uneven support for classical behaviourism. The expulsion of mind and consciousness is not actually required by this positivism but in its stress on the primacy of observable behaviour Watson's move may be fairly said to accord with positivism's general tenor. And, historically, behaviourism was defended by appeal to positivist standards. But since those standards were not submitted to any detailed scrutiny we can conclude that classical behaviourism's relation to positivism was, at first, adventitious and unsystematic. Only later, as we shall see, did that relation become explicit and more calculated.

For more than a decade after its birth behaviourism defended its claim to be the objective approach to psychology by advertising its anti-mentalism and emphasising its publicly observable data base - namely, behaviour. Gradually, however, the behaviourists realised that they needed more than this if they were to emulate the achievements of psychology's traditional idols, the physicists. As Koch, who has

written about this phase of behaviourism in some detail, puts it:

'By the late twenties, there was much 'objective' experimentation but few bodies of clearly stated predictive principles comparable with the crowning achievement of physics: its theories. Instead, experimentation seemed aimless, 'theoretical' hypotheses but loosely related to date, and debate idle. The search for a 'decision procedure' thus became a search for a formulary of techniques for constructing rigorous theory.' 115

Neobehaviourism, which covers the period from 1930 to the late 1950's, was preoccupied with the attempt to attain theoretical advance through the implementation of a 'formulary of techniques', derived partly from operationism, an American-based philosophy of science, but mainly from the logical positivists. Commenting on the psychologists' growing faith in the philosophers' confident claims to possess the key to scientific progress Koch adds:

'It should be observed that psychology's selections from this cluster of commitments were spotty, adventitiously determined and not supported by especially expert scholarship in the relevant sources.' 116

What the neobehaviourists wanted was a set of rules which would enable them to develop explanatory theories, as successful as those of physics, but which would preserve the objectivism of early behaviourism and prevent the readmission of troublesome metaphysical fictions and, more specifically, of entities which are, in principle, unobservable. To build explanatory theories they clearly needed to make use of theoretical concepts. What the psychologists thus wanted from the philosophers of science and what these philosophers claimed to provide was a guaranteed method of discriminating between empirically significant theoretical constructs and metaphysical ones. The

115 Koch, S. (1964) p9
116 " " " " p10

psychologists believed that they already possessed an empirically sound data base but they were also pleased to hear philosophical arguments which appeared to make that base even more secure.

The picture, however, is greatly complicated by the fact that the criteria of empirical significance were constantly modified and, on a few occasions which we shall examine in more detail in a moment, radically changed throughout the life of logical positivism and logical empiricism. So, by way of reply to Koch, it could be argued that the psychologists might have been more consistent and adept in their employment of empiricist criteria had those criteria been more stable. Nevertheless we shall see that there is a good deal of substance in Koch's charge for psychologists are to be found, even today, defending positions with arguments long recognised to be inadequate by their creators.

The first phase of the Vienna Circle, dominated by Carnap's Der Logische Aufbau der Welt,¹¹⁷ does not seem to have had any impact on the behaviourists; the phase was, in fact, abandoned by Carnap himself even before logical positivism was given this 'international trade name',¹¹⁸ and introduced into the United States in 1931 by Feigl and Blumberg as 'A New Movement in European Philosophy'.¹¹⁹ Despite its lack of influence on psychology it is worth looking a little more closely at this work of Carnap for it throws light on future contributions within the Circle. In the Aufbau Carnap tried to show how the entire range of concepts used in the empirical sciences - from sociology to physics - could be constructed out of one basic phenomenalist concept or, more accurately, out of moments of unanalysed and unprocessed experience. In a series of definitional steps he hoped

117 Carnap, R. (1928)

118 Feigl, H. (1968) p630

119 Feigl, H. and Blumberg, A.E. (1931)

to offer a logical reconstruction of empirical knowledge thus fulfilling the age-old empiricist promise of reducing all knowledge in science to an experiential base. Hence, Carnap's programme aimed, at this stage, to reduce not only theoretical concepts of the type the neobehaviourists wanted to introduce into psychology but also their believed-to-be-basic, behavioural data to an ultimate, primitive phenomenal base - to what Feigl and Blumberg were to call 'the ground-floor of knowledge, the given.'¹²⁰

It is tempting to speculate that behaviourists, had they been exposed to Carnap's phenomenalism would have regarded it as incurably metaphysical and joined those many critics who, according to Goodman, regard 'those who spend time on phenomenalist constructions as ... stubborn and old-fashioned crack-pots who shut their eyes to the facts of life and science.'^{121,122}

Carnap, who was as opposed to metaphysics as any behaviourist, embarked on his programme, however, because he wanted to demonstrate that the superiority of scientific knowledge lay in its absolute certainty. As he later wrote in his autobiography:

120 Feigl, H. and Blumberg, A.E. (1931) p292

121 For a brief, clear and balanced assessment of Carnap's Aufbau, see N. Goodman's paper, 'Significance of Der Logische Aufbau' in Schilpp, P. (1963); in particular his defence of logical constructions against arguments that these do not accurately reflect our cognitive psychological methods of thought. Goodman points out that these logical constructions are not offered as psychology but as, what the name suggest, logical constructions.

122 The neobehaviourists were not the first psychologists to turn to philosophers of science for help. In 1892 the English psychologist, statistician and polymath Karl Pearson published 'The Grammar of Science' in which the views of Ernst Mach were favourably discussed. Mach, for Pearson, was an idealist and it is, he said with approval, 'very needful to bear in mind...that an external object is in general a construct - that is, a combination of immediate with past or stored impressions.' (p.50) Pearson's idealist interpretation of Mach was later to provide valuable ammunition for Lenin in his campaign against the Russian Machists. Mach's philosophy, he wrote in his highly polemical work, 'Materialism and Empiric-Criticism' (1947), is 'idealism vainly seeking to hide the nakedness of its solipsism under the cloak of a more objective

'Under the influence of some philosophers, especially Mach and Russell, I recognised in the Logischer Aufbau a phenomenalist language as the best for a philosophical analysis of knowledge. I believed that the task of philosophy consists in reducing all knowledge to a basis of certainty. Since the most certain knowledge is that of the immediately given, whereas knowledge of material things is derivative and less certain it seemed that the philosopher must employ a language which uses sense-data as a basis.' 123

Carnap was gradually persuaded, however, that a physicalist attitude was more suitable. Its leading advocate at that time, Otto Neurath, had championed physicalism against the prevailing view of German contemporary philosophy that there was a fundamental difference between the natural sciences and the mental or cultural sciences in which the human mind plays an essential role. He opposed the associated claim that there were two radically different methods of investigation corresponding to each - explanation by causes for the natural sciences, hermeneutical or meaningful understanding for the mental and cultural sciences. (This was but an earlier, cruder version of the methodological dualism advanced by von Wright and discussed in Chapter One.) Where Neurath combatted such views with a blunt retort that psychology and sociology tried to explain the operations of complex physical systems

terminology.' (p.56) 'The idea of the neutrality of the elements of experience in relation to the 'physical' and 'psychical' ... is the basic error of Machism' (p.58), a basic error shared, says Lenin, by that 'idealist and fideist' Wundt. In brief, when positivists endorse a scientific world-view, Lenin said, they merely contradict their fundamental idealist premises and smuggle in materialist assumptions. 'Mach and Avenarius, in their philosophy, combine,' he wrote, 'fundamental idealist premises with individual materialist deductions for the very reason that their theory is an example of that 'pauper's broth of eclecticism' of which Engels speaks with just contempt.' (p.64) Carnap, like Mach before him, denied that he was offering any metaphysical theses about reality - whether it was really physical or psychical. Both men claimed that they were phenomenalist or solipsistic only in a methodological sense.

123 Carnap, R. (1963) p50

and so was a natural science Carnap preferred to make physicalism a thesis about language and so he formulated it as the claim that any sentence of any branch of empirical science could be translated into physical language 'without loss of content'.¹²⁴ So construed physicalism is preferable to phenomenalism, he said, because 'the events described in this (physical) language are in principle observable by all users of the language.'¹²⁵ Carnap's aim, however, remained the same during this second phase of his thought - that of demonstrating the certainty of scientific knowledge by reducing all concepts to physical concepts describing observables.

Physicalism, so formulated, has given rise to considerable confusion and to the mistaken belief that Carnap had condemned as meaningless statements referring to 'other minds' or to mental states in general. We can avoid confusion if we recognise that this early version of physicalism rests on the adoption of two different empiricist meaning criteria. The first of these, what Hempel¹²⁶ calls the wider empiricist criterion of meaning, specifies the conditions under which a statement has meaning or empirical content; the second narrower empiricist criterion, as Hempel calls it, tries to state wherein lies the meaning or content of a statement. Using these meaning criteria we will see that statements about 'other minds' may have a meaning for Carnap.

The wider criterion states that a sentence is only meaningful or has empirical content if it is, in principle, testable (verifiable) by means of observations or, more precisely, if it implies 'observational sentences' or what are called 'protocol sentences' which describe observable events. By this criterion statements about, for example, the 'soul', or 'the spirit of the age', 'essences', 'Zeitgeist' are

124 Carnap, R. (1932) p166

125 " " (1963) p52

126 Hempel, C. (1969) p177

all meaningless for they do not imply observational sentences and, as such, are not held to be translatable into physical language.

The narrower criterion further states that the meaning or empirical content of a statement is completely determined by its observational implications. In Carnap's words:

'If the same sentences may be deduced from two sentences, the latter two sentences have the same content. They say the same thing, and may be translated into one another.' 127

Clearly, the narrower criterion implies the wider one but not vice versa.

To see how Carnap's meaning criteria apply to psychology let us consider the statement 'John is angry'. That statement can be translated without loss of content, according to Carnap, into a sentence in physical language which implies the same observational or protocol sentences. The sentences in the physical language would, in this case, describe the state of John's body - blood pressure, heart-beat, agitated movement etc - and thus point to 'the existence of a physical structure characterised by the disposition to react in a specific manner to specific physical stimuli.'¹²⁸ Further, the specific manner of bodily reaction would also be described in physical terms. Anger is not something over and above this physical structure and its dispositions, according to Carnap, any more than the 'firmness' of a wooden support - his example - is a property existing over and above the physical structure of the wood.

Anyone who claims that 'anger' is not to be identified with a physical structure disposed to react in certain ways but possesses some further non-physical component is, says Carnap, simply talking nonsense. For that further component would lack any observational consequences

127 Carnap, R. (1932) p166
128 " " " p172

and would, hence, be meaningless - on the wider criterion of meaning; if it did have observational consequences these could be physically described, hence the further component would not be non-physical. With this argument Carnap rebutted to his own satisfaction the objection that 'meaningful' behaviour was not identical to some set of mere movements in space. By way of example, he pointed out that a 'meaningful' term like 'beckoning motion'¹²⁹ could be exhaustively described in physical terms by enumerating the class of arm-movements to which it corresponds.

Psychological or mental terms were not held, however, to be analytically translatable into physical language. Just what behavioural manifestations constitute 'anger', 'excitement' or any other state is said to be a matter for empirical investigation. What was asserted was that for every psychological term there must be some observational and, hence, physical implication; otherwise the psychological term would have no content and be meaningless.

In summary, in 1932 Carnap held that mental or psychological terms were meaningful because they could be fully defined in or translated into physical terms; such a definition would often require an empirical investigation. Claims that such terms possessed some 'extra' component apart from their physical accompaniments were held to be meaningless. Finally, if anyone rejected this physicalist account of psychological terms or sentences then, Carnap asserted, the terms or sentences are simply rendered meaningless.

Carnap's entire physicalist thesis was sharply criticised by Karl Popper whose own philosophy of science, itself partly developed in opposition to the Vienna Circle, is examined in the next chapter. First, he argued that Carnap's demarcation and meaning criterion between

129 Carnap, R. (1932) p182

science and metaphysics, sense and nonsense, was untenable. The difference between science and metaphysics cannot lie in the reducibility of science to experience, whether that experience is expressed in either phenomenalist or physicalist language. Carnap had written, in support of his physicalism, that:

'Chiefly because of the efforts of Mach, Poincare and Einstein, physics is, by and large, practically free of metaphysics. In psychology, on the other hand, the work of arriving at a science which is to free of metaphysics has hardly begun.' 130

By 'free of metaphysics' Carnap meant reducibility to 'observation statements' based on observations. Psychology, except for radical behaviourism (which was much less influential in Germany than in America during this period) was, he maintained, metaphysical in so far as it admitted mental states which were held to be different from physical accompaniments i.e. not definable in physical terms.

To this Popper argued that no physical theory could be translated into statements about observations and hence, on Carnap's own criterion, all science was metaphysics. This is but another way of saying, as we shall see later, that Popper accepted Hume's view that the problem of induction is insoluble; scientific theories involve universal statements which can never be reduced to a series of particular statements. Popper wrote:

'The point is that all physical theories say much more than we can test. Whether this 'more' belongs legitimately to physics, or whether it should be eliminated from the theory as a 'metaphysical element' is not always easy to say.' 131

Popper then went on to employ his own demarcation criterion in such a way that science was not turned into metaphysics. Against Carnap he simply pointed out that there could be no objection to the use of

130 Carnap, R. (1932) p174

131 Popper, K.R. (1963) p266

mental or psychological terms on the grounds that they were not reducible to physical, behavioural terms. Psychological terms - not definable in physical terms - were as legitimate components of scientific theories as theoretical terms in physics. Physicalism, as outlined by Carnap, swept away too much.

Next Popper argued that in another respect Carnap's physicalism was not physicalist enough. For Carnap held that the sentences which formed the empirical basis of all tests in science, the 'protocol sentences', are reports of our own observational experiences, although expressed in physical language. In other words Carnap attempted to base all scientific knowledge on experience, just as he did in his phenomenalist phase.

Popper, by contrast, argued that all scientific theories - including psychological theories - must be tested 'by first deriving from them statements about the behaviour of physical bodies.'¹³² These simple descriptive statements are then compared with statements of what are taken to be the 'facts', where the latter statements describe physical facts. Popper adds:

'Thus according to my view we do not, for the purpose of such basic tests, choose reports (which are difficult to test intersubjectively) about our own observational experiences, but rather reports (which are easy to check) about physical bodies ... which we have observed.'¹³³

Popper's view is radically different from Carnap's because he denies that it is possible to construct the world of science out of private experience. Now exactly what role Popper allots to experience and perception in scientific testing is much debated and will be considered later (in Chapter Four) but the point to note here is that Carnap

132 Popper, K.R. (1963) p267

133 " " " "

accepted Popper's criticism of this aspect of his physicalism. Popper had presented, Carnap wrote in 1932, 'the most adequate of the forms of scientific language at present advocated ... in the theory of knowledge.'¹³⁴

Finally, to come to a point of great significance for psychology, the physicalist theory of testing which Popper advocates differs in a crucial respect from Carnap's. For Carnap, we have seen, psychological terms must be translated into physical language; applied to behaviour that requires the psychologist to conceive of all behaviour as movement in space - just like a falling stone. For Popper, on the other hand, psychological theories are tested by comparing their derivations with statements about behaviour where that behaviour is always seen to be interpreted in the light of theories. Hence, where Carnap would describe a piece of behaviour as, say, 'right arm moves to horizontal position, right index finger juts forward one inch, makes contact with button etc', Popper sees nothing wrong with, nothing unscientific about, the statement, 'he rang the bell'. Popper spells out his position thus:

'The behaviour of men, predicted by psychological theories, nearly always consists, not of purely physical movements, but of physical movements which, if interpreted in the light of theories, are 'meaningful'. (Thus if a psychologist predicts that a patient will have bad dreams, he will feel that he was right, whether the patient reports 'I dreamt badly last night', or whether he reports 'I want to tell you that I have had a shocking dream'; though the two 'behaviours', i.e. the 'movements of the lips' may differ physically more widely than the movements corresponding to a negation may differ from those corresponding to an affirmation.' 135

With this argument against Carnap's physicalism Popper anticipated, I suggest, an objection that the 'New Mentalists' were to make against behaviourism, especially as defended by Skinner. There is no

134 Carnap, R. (1932) pp223-8

135 Popper, K.R. (1963) p267

obligation on a psychologist, mentalists argued, to classify behaviour merely in terms of movement; to demand that it be so classified is to condemn psychology to sterility for the significance of a piece of behaviour lies in the fact that it is meant to satisfy some mentalist description, to fall under some concept, plan or rule. The task of the psychologist is then to advance a theory which will characterise the concepts, plans or rules and explain the production of the behaviour which is taken to fall under them. Popper's argument was that any and every description of behaviour is an interpretation in the light of theory. And it is futile to attempt to find some basic, non-theoretical description such as movement. Since all descriptions involve the use of universal terms the descriptions are irredeemably theoretical; and this holds for description-as-movement just as much as for meaningful or mental descriptions like, 'he rang the bell.' (Incidentally, physicalism and behaviourism in their retreat to a presumed atheoretical level of description are rejected by Popper as subjectivist - despite their claim to be the objective approaches to psychology. They are subjectivist, he says, because they accept a subjective theory of knowledge, the view that scientific knowledge can be reduced to subjective experience.)

Despite Popper's criticism, however, and despite Carnap's partial acceptance of it logical positivism in this physicalist version gradually filtered into American behaviourism providing the psychologists with what they took to be a sound methodology enabling them to implement their 'objectivism' at the theoretical level. Where classical behaviourism's emphasis on observables was, as we have seen, positivist in an unsystematic way neobehaviourism's rationale and methodology was increasingly based on explicit logical positivist recommendations. This is not to say that these recommendations were implemented in their

entirety; rather they were incorporated piecemeal, adapted to the behaviourists' own requirements. But they were certainly taken to provide general support for the behaviourist tradition and to provide the key to further advance. Referring, in 1939, to Carnap's physicalism the influential American behaviourist S.S. Stevens summed up its presumed message:

'It is the Logical Positivist's way of saying that psychology must be operational and behaviouristic.'¹³⁶

This 'new view' of science, as Stevens called it, was taken to support the original emphasis on the need for a public, 'objective', data base - namely, behaviour. So this emphasis was preserved in the transition from classical to neobehaviourism. Next, it seemed to provide what the psychologists had been looking for, a guaranteed method of building theories without importing unobservable, metaphysical notions which would undermine the objectivity of the discipline; in contrast to Watsonian behaviourism it was now admitted that unobservables were, after all, legitimate components of psychological theories. As demanded by Carnap these theoretical entities or dependent variables had to be fully definable in or translatable into physical language. The immediate effect of this was a relaxation of the anti-mentalist posture of early behaviourism for it was held that mental terms, suitably disinfected, could enter theories as legitimate unobservables. The advantage of this move was that it gave behaviourists an answer to the common-sense objection that behaviourism was absurd in its denial of something which everybody knew to exist - namely, their own consciousness. 'We do not deny the existence of mind,' the neobehaviourists could now reply, 'we merely say that mind and consciousness as traditionally conceived is occult and can play no role in science.'

136 Stevens, S.S. (1939) p240

By the mid-1930's Carnap started to weaken his physicalist thesis in line with his recognition that it was impossible to obtain a full definition of theoretical or psychological concepts in terms of observables. Dispositional terms in particular, he realised, could not be so reduced. His reasoning was that a dispositional term like, say, 'brittle' was supposed to be definable in this way:

x is brittle \equiv x is struck \supset x breaks

Such a definition, however, applies not merely to objects which are struck and break but to objects which are never struck. On this definition all objects which are never struck are held to be 'brittle'; but since some objects which are never struck are not brittle we have not defined the term correctly. Hence, explicit full definition does not work for theoretical terms. Applied to psychological terms this means that they are not eliminable in favour of observable movements.

Before considering Carnap's proposals for modifying physicalism it is worth pointing out that 'operationism', a philosophy of science fathered by the physicist P.W. Bridgman and favoured by neobehaviourists around 1930, was a variant of this thesis of the full definability of theoretical terms. Bridgman argued:

'The concept of length is therefore fixed when the operations by which length is measured are fixed; that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; the concept is synonomous with the corresponding set of operations.' 137

Hence, in subscribing to the full definability of terms, operationists face the same difficulties as did the early physicalists.

To avoid these difficulties Carnap suggested in 1936-37 that the requirement of full definability be given up and replaced by 'partial

definition' via 'reduction sentences'.¹³⁸ In brief - for the modifications are argued for at length - reduction sentences do not completely define what it is for an object to be, say, 'brittle'; they merely specify test conditions which apply only when specific circumstances obtain - in the case above that would be when the object is struck. With reference to objects for which the circumstances do not obtain no criteria of application are given. Applied to psychology Carnap's modified physicalism now claimed that for every mental concept there are publicly observable, behavioural criteria associated with that concept which allow an investigator to say whether or not a person is in the state referred to. Not only do I have access to my mental states via introspection but, in principle, anyone else, Carnap said, can gain access as well by observing my movements. But Carnap no longer maintained that it was possible to fully reduce such mental terms to observables - there is more to such terms than can be defined.

Behaviourists did not react in a uniform manner to these modifications. Some, in line with Carnap, changed their views on the status of theoretical terms. Others continued to defend the initial physicalist view or indeed only started to embrace that position after it had been abandoned by its philosophical sponsors. As a result the pattern of influence of logical positivism within behaviourism becomes more complicated and, combined with the already noted fact that what techniques or norms which were imported into the discipline were adapted to psychology's specific needs, difficult to assess. What can be stated, however, is that many influential psychologists were, at least, indifferent to the modifications and changing formulations of logical positivist and empiricist thought. Typical of such behaviourists

138 Carnap, R. (1936, 1937)

is B.F. Skinner who has continued to call for full operational definitions of all psychological concepts and furthermore asserts, without further argument, that this is the only way to implement a scientific psychology. In the face of this it is difficult to resist Koch's conclusion that psychology's relations with the 'new view' of science were 'spotty, adventitiously determined, and not supported by especially expert scholarship in the relevant sources',¹³⁹ and 'throughout this entire sequence and down to this very day, no great clarity was achieved about these imported ideas.'¹⁴⁰

After their emigration to the United States the logical positivists, thereafter the logical empiricists, continued to refine their meaning criteria and in the 1940's and 1950's further liberalized the associated physicalist doctrines. Partial definition of terms was no longer demanded and Carnap proposed that mental terms be introduced by first specifying a set of theoretical principles in which they function and second, a set of 'correspondence rules' which provide partial observational criteria for some of the theoretical terms. In short, mental terms are not held to be dispositions to behave nor are they held to possess necessary or sufficient conditions of application that can be stated in an observational vocabulary. This has followed from the fact that the wider and narrower meaning criteria have been given up.¹⁴¹

Today Carnap's bold, radical physicalism has given way to Feigl's physicalistic identity theory which claims - as a scientific hypothesis - that every mental state is coextensive with some neurophysiological state, which a future unified science will identify. This physicalism,

139 Koch, S. (1964) p10
140 " " " p11
141 Carnap, R. (1956)

which is but a pale, apologetic descendant of the early Vienna physicalism, no longer dictates to psychology the status it may award theoretical terms. As such, as we have seen, it is even adopted by leading contemporary anti-behaviourists, the modern mentalists. Behaviourism, we have also seen, has been similarly diluted and has even been used by Quine¹⁴² to define the position that psychological hypotheses must be tested by observations. To which Chomsky has retorted: 'Quine's proposal signifies the demise of behaviourism as a substantive point of view, which is just as well.'¹⁴³

To conclude, this brief case study has attempted to show how important philosophy of science has been in psychology's history. Behaviourism as formulated by Watson was shaped and justified by an unanalysed and implicit positivism that prepared the movement for its subsequent reconstruction by the logical positivists of the Vienna Circle and later, the logical empiricists. These philosophers of science developed and exported to psychology 'decision procedures' or 'methodological rules' which, they claimed, would enable the behaviourists to put their discipline on a secure, objective scientific footing. I have pointed out that these methodological rules and the epistemology on which they rested were incisively criticised at the time of their presentation by Karl Popper with arguments which were admitted to be destructive by the logical positivists themselves. Yet Popper's arguments were ignored by the behaviourists who quickly and somewhat naively implemented the Carnapian physicalism. I have then briefly traced the subsequent dilutions of physicalist doctrines and the increasingly complex implications of those positions for psychology due to attachment to different philosophical views. Given the size

142 Quine, W.V.O. (1969)

143 Chomsky, N. (1973)

of the literature on both logical positivism and behaviourism this case study could not be other than schematic and the main point has yet to be demonstrated: that psychology, had it been based on the philosophy of science developed by Karl Popper rather than on the logical positivism of his opponents, would have looked very different today. But it will be the task of the rest of this thesis to make good that claim.

Chapter Three

The Objectivist Epistemology of Karl Popper

After the lecture there was a discussion, and Ayer encouraged me to say something. So I said first that I did not believe in induction at all, even though I believed in learning from experience, and in an empiricism without those Kantian limits which Russell proposed. This statement, which I formulated as briefly and pointedly as I could with the halting English at my disposal, was well received by the audience who, it appears, took it as a joke, and laughed. In my second attempt I suggested that the whole trouble was due to the mistaken assumption that scientific knowledge was a species of knowledge - knowledge in the ordinary sense in which if I know that it is raining it must be true that it is raining, so that knowledge implies truth. But, I said, what we call "scientific knowledge" was hypothetical, and often not true, let alone certainly or probably true (in the sense of the calculus of probability). Again the audience took this for a joke, or a paradox, and they laughed and clapped. I wonder whether there was anybody there who suspected that not only did I seriously hold these views, but that, in due course, they would be widely regarded as commonplace.

Karl Popper
Intellectual Autobiography¹

Introduction

'The main philosophical malady of our time is an intellectual and moral relativism, the latter being at least in part based upon the former.'² Popper's central concern throughout his life has been to combat relativism and defend the objectivity of scientific knowledge. Perhaps his greatest achievement has been to provide a method for fighting relativism successfully, an antidote to a malady which has threatened to turn into an epidemic in recent years.

Traditionally philosophers have come to espouse relativism with great reluctance. Usually they started their epistemological quest

1 Popper, K.R. (1974) p87
2 " " (1945) Vol.2 p369

with a conviction that some beliefs, at least, were superior to others and had a rational basis which it was their task to make clear. However, these attempts to promote some beliefs to the status of 'knowledge' as opposed to mere 'opinion' quickly led, as we shall see, to an even deeper relativism. Popper's strategy for avoiding this unfortunate consequence was to adopt more modest epistemological standards than previous anti-relativists. In particular, he embraced fallibilism and recognised that every claim to knowledge may be completely mistaken. His move may be viewed as a case of reculer pour mieux sauter, a retreat from standards that were unattainable to better defend the notion of objective knowledge.

Before examining Popper's epistemology in detail I would like to make my own strategy clear. I have argued that behind each of the competing approaches to psychology, already reviewed, lie more comprehensive philosophies, each with its own distinct views on scientific method, the role of theoretical concepts, the nature of explanation and understanding, the logic of social action and so on. These different philosophies provide very different criteria for assessing claims about whether and how psychology should be practised. Different philosophies legitimate different approaches to psychology. Now I wish to suggest that Popper's 'fallibilist absolutist'³ epistemology, discussed below and subject to various modifications and qualifications introduced in the next chapter, provides the most satisfactory perspective from which to judge the merits and demerits of the conflicting arguments about psychology. It provides an enlightening analysis and vigorous defence of the rationality of scientific enquiry and enables us to affirm, in principle, the viability

3 Popper, K.R. (1945) Vol.2 p377

of a scientific psychology. So, after explicating Popper's philosophy of science in this chapter I shall, in the next, review criticisms and objections which have been levelled against it. Then, after recognising the force of some of these arguments and rejecting others, I shall employ this modified Popperian philosophy to evaluate the worth of the aspirant psychologies. Finally, I will discuss in the last chapter of this thesis, the implications for psychology of Popper's more recently published metaphysical theories, theories which he has developed in his efforts to further strengthen his defence of objectivity in science.

Protagoras is usually credited⁴ with the classic statement of relativism when he asserted that man is the measure of all things. What is true depends, on this account, on what the individual chooses to believe; he makes his own truth. More commonly, relativism is the term used to refer to the doctrine that groups, communities or societies decide what is to constitute truth for them. In neither case is there any sense to the notion that the beliefs of either an individual or a society can be measured against some external, objective standard. It is nonsensical, relativists maintain, to say that a belief is true or false independent of what any person or community thinks about it. In up-to-date psychological language relativism may be expressed like this: 'Each person is a member of a community or social group, conditioned throughout his life by an intricate web of positive and negative reinforcements which shape his norms, expectations, values and principles. What anyone therefore values or chooses to call true or false, good or bad, ugly or beautiful etc

4 Trigg, R. (1973) p3. According to Plato, says Trigg, Protagoras held that anything 'is to me as it appears to me and is to you as it appears to you.'

is completely determined by the social forces and authorities in his group or society. Therefore what is good, true, beautiful etc is simply a matter of social convention. Furthermore no group or community is privileged in being exempt from the operation of such social mechanisms. Scientific communities, despite claims to the contrary and a camouflage which tolerates dissent of a purely conformist kind i.e. arguments, have no less effective power structures than other highly organised communities and such power structures enforce the law - determine what is true, what is false - as rigidly as any other.⁵ Finally, any epistemologist who believes in the existence of objective truth and say, the superiority of scientific knowledge is himself the product of a social framework and hence his belief is as socially determined as any other. All beliefs are socially produced and hence derive what value they have from the individual, group or community for which they function; they have no value independent of the social unit which created them.'

Justificationism

The history of epistemology can be reconstructed as a series of attempts to overcome relativist arguments of this kind. And in fact Popper has reconstructed traditional epistemological debate so that he could both identify the source from which relativism has sprung and prevent future thinkers from, as he sees it, sinking into the relativist mire. Historically, philosophers who often disagreed among themselves on whether knowledge of any kind was possible have, in his view, shared one fundamental assumption. They agreed that if any

5 'The authority of truth is the authority of society.' Thus writes a sociologist of science, David Bloor, in his 'Popper's Mystification of Objective Knowledge'. 1973 p17. Bloor's argument is examined in Chapter Six.

belief could be justified then it was absolutely or objectively true and could be legitimately awarded the title 'knowledge'. Which is to say that the traditional epistemologists shared, in Popper's view, a 'justificationist' theory of knowledge. If a belief could not be justified or proven or established or found indubitable or made certain, either totally or partially, then it remained a mere belief.⁶

Adoption of justificationism is a disastrous way to fight relativism, Popper argues, for it quickly plunges its proponents into an even deeper relativism. To see that this is so let us briefly consider how justificationists tried to combat the sceptical, relativist claim that there is no knowledge, that no belief can be justified, that every belief is on a par with every other.⁷ Sceptics started off by pointing out that a statement of belief could only be justified by producing another statement in its support. The latter, in turn, obviously requires a further statement in its support - or so the sceptics argued. The uncomfortable consequence of this, they triumphantly asserted, is either an endless regress or a circular argument. But it is not knowledge.

To overcome this argument some dogmatists abandoned the view that a statement of belief could be justified only by another statement and proclaimed instead that the regress of justifications could be ended by tracing all beliefs to some fundamental self-evident first principles. That is to say they took some subjective or psychological property which they associated with the belief to either guarantee or even define

⁶ Popper, K.R. (1972) p128

⁷ Following Musgrave's usage let us call those who deny that any belief can be justified the 'sceptics'. Sceptics were usually justificationists in that they equated knowledge with justified belief; they merely denied that any beliefs could be justified. For reasons that will soon become obvious those who assert that beliefs can be justified are called 'dogmatists'. Musgrave, A. (1974) p561

its objective truth. In other words they employed a subjective theory of truth in that a subjective experience was held to guarantee or define the truth of the statement. All other beliefs and statements were then held to be justified relative to these 'fundamental' first principles.

To the delight of the sceptics the dogmatists exhibited considerable variation both in the beliefs which they took to be fundamentally true and in the subjective attributes of belief which were held to be infallible criteria of truth. Rationalists typically opted for such subjective feelings as clarity, distinctness and indubitability resulting from the contemplation of some belief or statement;⁸ empiricists, on the other hand, preferred feelings of obviousness or perceptual assurance generated by sensory experiences.

The sceptics in fact had very little difficulty in coping with these dogmatist variations. Is it not a fact, the sceptics asked, that what is clear, distinct, indubitable, obvious and certain to one man is none of these things to another? Rationalists and empiricists, they pointed out, disagree not only with each other but differ among themselves over which beliefs are to be held to be true. And these feelings of conviction, these psychological impressions vary from person to person, time to time and for the same person at different times. Surely truth, the sceptics asked, is not held by dogmatists (of all

⁸ Descartes illustrates this type of argument:

'After this I considered in general what is requisite to the truth and certainty of a proposition; for since I had just found one that I knew to have this nature, I thought I must also know what this certainty consists in. Observing that there is nothing at all in the statement 'I am thinking, therefore I exist' which assures me that I speak the truth, except that I see very clearly that in order to think I must exist, I judged that I could take it as a general rule that whatever we conceive very clearly and very distinctly is true; only there is some difficulty in discerning what conceptions really are distinct.' (1954) p32.

people) to be dependent on such unreliable and whimsical phenomena? Far better, they replied to their own question, to admit that there are no fundamental, universally agreed first-principles at all nor are there any infallible criteria which can be employed either to guarantee or define the truth of beliefs. Let us admit, they concluded, that what 'first-principles' or 'starting points' we take to be true and relative to which we 'justify' our beliefs are nothing but arbitrary, irrational assumptions. Let us not delude ourselves that we possess genuine knowledge. Since no beliefs can be justified, nothing can be known.

What the sceptics in this reconstruction are rejecting, in Popper's eyes, is what rationalists and empiricists have traditionally subscribed to - what Popper calls the dogma that truth is manifest.

'By the doctrine that truth is manifest I mean ... the optimistic view that truth, if put before us naked, is always recognizable as truth. This truth, if it does not reveal itself, has only to be unveiled or discovered. Once this is done, there is no need for further argument. We have been given eyes to see the truth, and the 'natural light' of reason to see it by. This doctrine is at the heart of the teaching of both Descartes and Bacon. Descartes based his optimistic epistemology on the important theory of the veracitas dei. What we clearly and distinctly see to be true must indeed be true; for otherwise God would be deceiving us. Thus the truthfulness of God must make truth manifest. In Bacon we have a similar doctrine. It might be described as the doctrine of the veracitas naturae, the truthfulness of Nature. Nature is an open book. He who reads it with a pure mind cannot misread it. Only if his mind is poisoned by prejudice can he fall into error.' 9

With this last remark Popper draws attention to a second theory held by dogmatists, a corollary of the doctrine that truth is manifest and a reply usually made to sceptics amused by the disharmony within the dogmatist camp. This is what he calls the 'conspiracy theory of

error'.¹⁰ To sceptics who pointed out that not everyone finds the same beliefs evident, clear, etc and hence, on the dogmatists' criteria, a specific belief must be regarded as true for one man but not for another, dogmatists often replied that some people are just plainly wrong. Subjective, psychological guarantees of truth are universal, they answered, and so everyone ought to find the same beliefs clear, distinct, etc. The fact that they do not do so only shows that some minds are biased, prejudiced, misled or otherwise impure. Hence dogmatists were forced to employ what Musgrave calls a psychotherapy, a method 'to cleanse the mind of bias and prejudice so that it could receive the truth i.e. so that their subjective criteria should never deceive.'¹¹ Other dogmatists preferred to argue that the disagreement over which beliefs are self-evident, fundamental, etc is due to the fact that some people only think they have employed the criterion of truth correctly. Not every belief which is clear and distinct to the person who holds it is actually clear and distinct. 'Hence,' the sceptics were quick to respond, 'we need a further criterion for distinguishing genuine psychological feelings from erroneous ones.' And this leads either to an endless regress or plain dogmatism, to an authoritarian faith in one's own feelings reinforced by a psychotherapy which would make these feelings 'self-evident' to everybody else.

In summary, dogmatists set out to combat relativism by attempting to justify specific beliefs, thereby affirming their truth and turning them into genuine knowledge. Justificationism leads directly to subjectivism, the claim that a subjective psychological feeling supposedly invariably associated with a belief provides its ultimate justification. Not only does this fail to embarrass the relativist but it defeats

10 Popper, K.R. (1963) p7

11 Musgrave, A. (1974) p563

the dogmatist's aim of defending the power of rational argument. For if two people disagree over 'first-principles', if they have different feelings about beliefs, then the difference cannot be settled by discussion - feelings cannot be argued about so the disagreement invites an authoritarian settlement. One person must be led to a 'correct' view of things. Further, by insisting that rational discussion is only possible between people who share the same 'first-principles' dogmatists are actually saying that we cannot discuss genuine disagreements.¹² If two people genuinely disagree, no discussion can ever get started. Hence, dogmatists set out to defend the power of argument and end up by sharply constricting its role - and to no avail because the dogmatists fall back on the very relativism they tried to avoid.

Knowledge: Subjective and Objective

To fight relativism effectively Popper challenges the assumption, shared by both dogmatists and sceptics alike, that knowledge worthy of the name consists of beliefs which have been or may be justified. He develops a method whereby it makes sense to talk of beliefs or, more accurately, of statements meeting objective standards even though we cannot justify our claims about their epistemological status. Popper rejects justificationism because it conflates two areas of enquiry which ought, he says, to be kept distinct. We may, for the moment, call these the areas concerned with the genesis and justification of beliefs respectively. Questions such as 'How did this man come to believe this?'

¹² This claim that rational discussion is only possible between people who share the same 'first-principles', 'assumptions', etc. is what Popper calls 'the Myth of the Framework' which is 'in our time the central bulwark of irrationalism.' (1970) p56. Despite its 'revolutionary' guise (see Kuhn's theory of science, already discussed) this claim is but one consequence of the traditional justificationist conception of knowledge.

or 'Why does this man find this belief so obvious or certain?' are very different from and call for a very different kind of investigation from the question which asks 'Is this man's belief true?' or 'Despite the fact that nobody seems to believe this statement - is the statement true?'

Dogmatists do not make this distinction which Popper recommends because they believe it is possible to establish the truth or falsity of a statement by examining its genesis or source. This reflects their twin attachments to the theory that truth is manifest and to the conspiracy theory of error. Both of these theories are false, Popper argues, because most human conjectures are false:

'For the simple truth is that truth is often hard to come by and that once found it may easily be lost again. Erroneous beliefs may have an astonishing power to survive, for thousands of years in defiance of experience, with or without the aid of any conspiracy.' 13

Identifying knowledge with beliefs of a certain kind obscures, he suggests, the ambiguity of that word. A belief can refer either to the contents of a proposition or statement, or to the psychological act by which it is either generated or understood. It is perfectly legitimate, Popper says, for a man to concern himself with either or both of these fields of enquiry. The contents of a belief may, for example, be studied to find out whether they support or contradict the contents of some other belief; or to find out whether they are true or false; or whether they are self-contradictory. On the other hand, that same belief in the mind of a specific person may be investigated to establish whether it is held with conviction, whether it is liable to be discarded on a whim or whether it is part of a more complex belief system. What is not legitimate, Popper argues, is to attempt to answer questions

13 Popper, K.R. (1963) p8

about the status of knowledge by studying the source of knowledge or vice versa. In short, he draws a sharp distinction between the logic of knowledge and the psychology of knowledge.

Armed with this distinction between knowledge considered as a subjective belief in a person's mind and knowledge viewed as the contents of statements Popper reformulates and then dissolves Hume's Problem, the problem of induction. Knowledge, for Hume, was a species of belief - beliefs which could be justified. When, after due reflection, he concluded that no beliefs could be justified he was held to have undermined all claims to knowledge and dealt a lethal blow to human rationality. Beliefs men must have, Hume admitted, but they are the result of blind habit - they are not established by reason.

'If I ask you why you believe any particular matter of fact, which you relate, you must tell me some reason; and this reason will be some other fact connected with it. But as you cannot proceed after this manner, in infinitum, you must at least terminate in some fact, which is present to your memory or sense; or must allow that your belief is entirely without foundation.

What then is the conclusion of the whole matter? A simple one; though, it must be confessed, pretty remote from the common theories of philosophy. All beliefs of matter of fact or real existence is derived merely from some object, present to the memory or sense, and a customary conjunction between that and some other object. Or, in other words; having found, in many instances, that any two kinds of objects - flame and heat, snow and cold - have always been conjoined together; if flame or snow be presented anew to the senses, the mind is carried by custom to expect heat or cold, and to believe that such a quality does exist and will discover itself upon a near approach. The belief is the necessary result of placing the mind in such circumstances.' 14

In modern parlance Hume here rejects the principle of induction and denies that it is ever justifiable or rational to reason from instances of which we have experience to instances of which we have no experience. Beliefs about the world, including beliefs to which science

14 Hume, D. (1902) p46

has led us, have - he says - no rational basis. What beliefs we do hold about the world are but the result of associations or repetitions; our minds are conditioned to expect future events simply because the same kind of events have occurred on the same occasions in the past. But such beliefs are not justified; they are not knowledge.

The consequences of Hume's argument were not always accepted by later empiricists or, indeed, even by Hume himself. Though not answered the sceptical conclusions were conveniently forgotten. Bertrand Russell, however, stated them with characteristic lucidity:

'The growth of unreason throughout the nineteenth century and what has passed of the twentieth is a natural sequel to Hume's destruction of empiricism.

It is therefore important to discover whether there is any answer to Hume within the framework of a philosophy that is wholly or mainly empirical. If not, there is no intellectual difference between sanity and insanity. The lunatic who believes that he is a poached egg is to be condemned solely on the ground that he is in a minority, or rather - since we must not assume democracy - on the ground that the government does not agree with him. This is a desperate point of view, and it must be hoped that there is some way of escaping from it.' 15

Popper's unique contribution to epistemology has been to offer an escape from Hume's depressing and pessimistic legacy. He dissolves Hume's Problem by arguing that induction is not the method of science.

Fallibilism

Translated into the logical mode of speech Hume's logical problem of induction is presented by Popper thus:

'Can the claim that an explanatory universal theory is true be justified by 'empirical reasons'; that is by assuming the truth of certain test statements or observation statements (which, it may be said, are 'based on experience').' 16

15 Russell, B. (1948) p699

16 Popper, K.R. (1972) p7

His answer is the same as Hume's. No, such a claim can never be justified; a finite number of particular statements can never entail a universal theory. Induction, he agrees, is not logically possible. Unlike Hume, however, he does not conclude that scientific knowledge is thereby deprived of a rational basis. Instead he proposes that we locate the rationality of science in the attempt to disprove universal theories. Popper exploits the asymmetry between verification and falsification to propose that the logic of science lies in the vulnerability of universal theories to refutation by just one counter-instance. With this proposal Popper breaks the age-old identification of knowledge with justified belief. Scientific knowledge, he proposes, consists of those theories which are judged not to have been refuted.

Popper then employs the distinction between the logic and the psychology of knowledge to attack the justificationist (and in particular, the empiricist) account of the relation between science and experience. Traditional empiricists had tried to reduce all scientific knowledge to an 'empirical base'. On this view all statements in science were justified by the perceptual experience of the 'knower'. Perceptions, observations, experiences were held to justify or establish perception or observation statements. These statements in turn were the basis from which all other scientific statements were deductively inferred. We have seen that this was the view of the logical positivists of the Vienna Circle; they wanted to make scientific knowledge secure and prevent future revolutions or upheavals by making certain that all theoretical statements could be reduced to observation statements, themselves based upon or justified by experience.

Popper denies that perceptions or experiences can be taken to justify the decision to accept an 'observation statement'. The traditional empiricist view overlooks a simple logical point. It is

this. Every scientific statement contains universal terms which go far beyond immediate experience.

'Every description uses universal names (or symbols, or ideas); every statement has the character of a theory, of a hypothesis. The statement, 'Here is a glass of water' cannot be verified by any observational experience. The reason is that the universals which appear in it cannot be correlated with any specific sense experience. (An 'immediate experience' is 'only once' 'immediately given'; it is unique.) By the word 'glass', for example, we denote physical bodies which exhibit a certain law-like behaviour, and the same holds for the word 'water'. Universals cannot be reduced to classes of experiences; they cannot be 'constituted'.' 17

Perceptions, observations and experiences are psychological affairs; observation sentences are logical entities and so their truth or falsity cannot be established by experience. Hence, says Popper, observation statements are conjectures, conventionally (and tentatively) agreed to be true by a scientist or group of scientists. Since they cannot be justified by experience (or in any other way) we must admit that they may well be false. It is at this point that Popper's logic of science becomes irredeemably fallibilist.¹⁸ As this part of his argument has been much criticised let us see what he says about the role of experience in science.

'And finally, as to psychologism: I admit, again, that the decision to accept a basic statement, and to be satisfied with it, is causally connected with our experiences - especially with our perceptual experiences. But we do not attempt to justify basic statements by these experiences. Experiences can motivate a decision, and hence an acceptance or rejection of a statement, but a basic statement cannot be justified by them - no more than by thumping the table.' 19

17 Popper, K.R. (1959) p95

18 It is at this point, say his critics, that Popper's theory of science becomes non-empirical. If we cannot rely on our senses as justification for observation statements then talk about 'scientific knowledge' is pretence. A.J. Ayer's criticisms of what he takes to be a fundamental flaw in Popper's analysis are examined, together with other objections, in Chapter Four, Part Two.

19 Popper, K.R. (1959) p105

Where the empiricist justificationists tried to defend the objectivity of science by resting all theoretical statements on a hard-core of observables or certain perceptions, on what Feigl and Blumberg²⁰ call 'the ground-floor of knowledge, the given' and consequently fell into relativism Popper tries to defend the objectivity of science without having to claim that any specific empirical assertion is true. Fallibilism may be viewed as the price paid for avoiding relativism.

What happens in science then, on this view, is that the scientist advances a conjecture about the natural or social world. How the scientist came to think up the conjecture is deemed to be a problem for empirical psychology, not for the epistemologist whose sole concerns are the logical analysis and appraisal of theories. Just as it is not the psychologist's job, qua psychologist, to explain the logic of science so it is not the epistemologist's role to explain why someone produced a particular theory.²¹ Popper's work, The Logic of Scientific Discovery, is thus oddly named for he holds that there can be no logic of discovery. To search for a logic of discovery is to violate the distinction he draws between the logic and the psychology of knowledge.

Once a conjecture is advanced it is, henceforward, to be assessed independently of its source. The scientist deduces a prediction from the conjecture together with a statement of initial conditions and possibly with the help of auxiliary background theories. Next, the scientist carries out an experiment to test the prediction and, indirectly, the initial conjecture. On the basis of the observed outcome of the experiment the scientist accepts an 'observation statement' or 'basic statement' which either falsifies or corroborates the prediction. To

20 Feigl, H. and Blumberg, A.E. (1931) p292

21 Against this view of Popper's Medawar argues that there is some 'internal censorship' which eliminates absurd hypotheses from any consideration. Hence, psychology has an epistemological function. Medawar, P. (1969) p53. See Chapter Four, Part Two.

accept this 'basic statement' is, in effect, to cease criticising it for the time being; the philosophy of science which Popper advocates thus incorporates a conventional component at the level of these singular observation statements. But it is a critical conventionalism, says Popper, (in contrast to the conventionalism of Poincare and Duhem) for there is no question of any 'basic statement' being beyond repeal, beyond refutation. The quotation marks simply mean that the accepted statement is not basic in the traditional empiricist sense. Though the scientist is led to adopt such a 'basic statement' by his experiences he cannot use his experience as a logical justification for his acceptance.²²

Now it is important to realise that if the scientist regards the prediction (and hence, the conjecture) as falsified by the 'basic statement' which he has accepted, he may well be mistaken. The original conjecture may be a true theory and the scientist's mistake may lie in his decision to accept the 'basic statement'. So science is, in Popper's eyes, fallible to the core. Neither conclusive proof nor conclusive disproof is possible in science.²³ We can never know for certain that we have not made an error when we either accept or reject a theory. Everything that we call scientific knowledge may well be false. This, therefore, is the argument behind the long quotation at the beginning of this chapter and behind Popper's famous statement about the foundations of objective knowledge:

22 Popper writes: 'My term, 'basis' has ironical overtones: it is a basis that is not firm.' (1959) p111

23 Popper repeatedly stresses the fallibilism of his logic of science. However it must be said that he sometimes writes as if we know for certain that we have made the right decision. For example, he suggests that a theory which has failed severe tests is false. See (1963) p235, paragraph one. Given his argument for fallibilism he ought to conclude that the theory may be false. This may help explain the mistaken but widespread assumption that he takes falsifiability to be decisive - an assumption made, for example, by Bronowski (1974) p616/7.

'The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon rock bottom. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and when we cease our attempts to drive our piles into a deeper layer, it is not because we have reached firm ground. We simply stop when we are satisfied that they are firm enough to carry the structure, at least for the time being.' 24

In terms of the reconstruction of epistemological debate developed earlier Popper can be seen to side with the sceptic in his denial that any belief can be justified. Knowledge in the traditional sense is unattainable. However we can see his affinity with the dogmatist in his insistence that the notion of objective truth plays an essential role in science. We shall see too that Popper sides with both kinds of dogmatist, both rationalist and empiricist, in that he gives a role to both the intellect and the senses in conjectures and refutations respectively. It is not, of course, the role which the rationalists and empiricists traditionally defended. So, then, scientific knowledge emerges from this account as a series of theories which have not yet, as far as we can judge, been falsified. Scientific theories are seen to be, in Ayer's phrase, on 'indefinite probation'.²⁵

Objective Standards

The fallibility of scientific knowledge thus undermines the justificationist defence of its objectivity. How, then, does Popper defend his claim that science is objective? How does he defend the view that scientific theories, though all equally fallible, are not all equal? The answer is that he does so by developing objective standards in the light of which theories may be appraised, standards which make

24 Popper, K.R. (1959) p111

25 Ayer, A.J. (1974) p688

it possible to rationally evaluate and choose between competing theories, without justifying the chosen theory as the true one. His epistemology is thus dependent upon an elaborate logical analysis of both theories and the ideals which they should approach. The two most fundamental standards in this analysis are those of content and truth, standards which are now examined in turn.

Given that he takes a basic aim of science to be the growth of knowledge Popper proposes that, other things being equal (such as the degree of empirical support, an issue which will be examined later), that theory is to be preferred which tells us more, which has the greater amount of empirical information or content. Empirical content of theories is a logical notion and Popper has developed a measure of content which allows us to say of a theory even before it has been tested whether, should it pass specific tests, it would be an improvement on its predecessors and competitors.

'To put it a little differently, I assert that we know what a good scientific theory should be like, and - even before it has been tested - what kind of theory would be better still, provided it passes certain crucial tests. And it is this (meta-scientific) knowledge which makes it possible to speak of progress in science, and of a rational choice between theories.'²⁶

A good scientific theory has high content and a better one has more content still. Popper has shown that the higher the content the more improbable the theory which means, in turn, a higher probability of being falsified.²⁷ If we take science to have as its aim the growth of knowledge, then we must admit, he argues that we want our theories to be highly improbable and thus highly falsifiable or testable. Testability, he suggests, is a criterion of progress (or potential satisfactoriness) in that we prefer a more testable theory - even before

26 Popper, K.R. (1963) p217

27 " " (1959) Sections 31-46; appendix IX.

we submit it to criticism and testing.

But high content is not enough. Science would not progress very far if scientists were content with content alone; if they just dreamt up more and more improbable theories, indifferent to their truth and indulged in a rapid demolition of their own wild ideas science would soon appear, Popper argues, as a pointless and time-wasting activity. He thus does not hesitate to demand that scientific theories ought to aim at truth. We want both highly informative theories which will hopefully stand up to criticism and which may thus be true. In his early writings²⁸ Popper avoided any lengthy discussion of this demand for truth but since learning of Tarski's objective or correspondence theory of truth he has made free use of it as a regulative idea in his logic of science.

Tarski's theory of truth dovetails with Popper's analysis of science for it separates the question of what it is for a statement to be true from the question of how we know whether in fact a particular statement or theory is true. To talk of a statement or theory being true or 'corresponding to the facts' without simultaneously generating various semantic paradoxes it is necessary, Tarski points out, to distinguish between two sorts of statement found in natural languages - such as English. Statements which refer to the world, such as 'Snow is white', he calls statements of the object-language; statements which refer to linguistic entities, such as 'The statement 'Snow is white' has three words', he calls statements of the metalanguage. Now the notions of 'truth' and 'falsity', which apply to statements and theories, must occur in the metalanguage if we are to avoid contradictions and paradoxes.

28 Popper, K.R. (1959) p273/4. 'In the logic of science here outlined it is possible to avoid using the concepts 'true' and 'false'.'

Such problems do arise if these notions are expressible in the object-language.²⁹ Using a metalanguage we can then refer to both the statement in the object-language and the facts to which it refers.

This enables us to define or give a complete account of what it is for a particular object-language sentence to be true. It is this:

The statement 'Snow is white' is true if and only if snow is white.

This theory of Tarski's has given rise to various criticisms which include the claim that it is not a version of the correspondence theory at all,³⁰ the objection that it is circular and/or that it is trivial. None of these criticisms have been accepted by Popper who applies Tarski's version without qualification. The criticisms themselves have been examined at length by Musgrave³¹ who, before he dismisses them, explicates the confusions on which they are based. Incidentally, he also isolates the 'rational kernel' in the later Wittgenstein's view that it is not possible to speak about but merely to show the relation between language and the world, the view behind his famous dictum:

'What we cannot speak about we must pass over in silence.'³²

The rational kernel is the claim that the relation between a language and the world cannot be discussed in the same language. From there, however, Wittgenstein proceeded to relegate, says Musgrave, the entire

29 Tarski's theory of truth emerges from his solution of the well-known Paradox of the Liar. Any language which can be a metalanguage of itself and which contains the notions 'true' and 'false' will give rise to the paradox. By introducing the rule that 'no language must be a metalanguage of itself' and thereby modifying natural language by making the object-language/metalanguage distinction Tarski allows us to avoid the paradox and retain the objective theory of truth from which the paradox seemed inevitably to arise. For a full and clear account of Tarski's theory and the objections which have been made against it see Musgrave's chapter on 'The Objective Theory of Truth' in his Objective Knowledge: A Criticism of Subjectivism in Epistemology (1969)

30 Black, M. (1949) p105

31 Musgrave, A. (1969) p163ff

32 Wittgenstein, L. (1961) p151

theory of meaning and truth to the ineffable and to inspire a school whose theses could be indicated but not stated. Tarski's distinction between the object-language and the metalanguage paves the way, in contrast, to an explicit investigation of the relations between language and the world, including the theories of meaning and truth. In other words Tarski paves the way to semantics where Wittgenstein falls into irrationalism - the assertion that such relations cannot be openly stated nor rationally discussed. As Musgrave puts it:

'Semantics speaks where Wittgenstein said we must be silent.' 33

Tarski's theory reinforces the distinction between the objective and subjective dimensions of knowledge. Whether or not a theory is true or false is now seen to be completely independent of whether or not anyone thinks it true or false. A theory may be true even though nobody believes it; or it may be false even though it meets with general assent. Tarski provides no criterion for recognising true theories; what his theory does is give a precise account of correspondence and thus allows the idea of truth to play a regulative role in science.

Says Popper:

'The status of truth in the objective sense, as correspondence to the facts, and its role as a regulative principle, may be compared to that of a mountain peak usually wrapped in clouds. A climber may not merely have difficulties in getting there - he may not know when he gets there, because he may be unable to distinguish, in the clouds, between the main summit and a subsidiary peak. Yet this does not affect the objective existence of the summit; and if the climber tells us 'I doubt whether I reached the actual summit', then he does, by implication, recognise the objective existence of the summit. The very idea of error, or of doubt (in its normal straightforward sense) implies the idea of an objective truth which we may fail to reach.' 34

33 Musgrave, A. (1969) p168

34 Popper, K.R. (1963) p226

Fundamental though truth in Tarski's sense is to Popper's logical analysis of scientific knowledge it is very remote, Popper admits, from the situation that confronts the scientist when he has to choose between competing theories - all of which may eventually turn out to be false. With a view to clarifying the logic of such a situation Popper has recently combined the two objectivist notions of truth and content (or logical consequence) in his theory of verisimilitude, a theory which enables him to talk precisely of one theory being closer to the truth even though both theories may eventually turn out to be false. It is meaningful to say that a new theory, though false, is closer to the truth than its (false) discarded predecessor.

Verisimilitude, which is a logical as opposed to an epistemological notion, is based upon the fact that every false statement has true logical consequences. If a statement 'a' is true, then the class of all the logical consequences of 'a' - the content of 'a' - consists only of true statements because the truth is always transmitted from a premise to its conclusions. Now, since a false statement will always have both true and false consequences, we can say that in every statement there may be more or less truth in what it says. Popper calls the class of the true logical consequences of 'a' the truth-content of 'a'; the class of the false consequences of 'a' is the falsity-content of 'a'.

If we compare two theories T_1 and T_2 we can say that T_1 has less verisimilitude than T_2 if (1) their truth-contents and falsity-contents are comparable (2) the truth-content of T_1 , but not the falsity-content, is smaller than that of T_2 , or else (3) the falsity-content of T_1 but not the truth-content, is greater than the falsity-content of T_2 . In other words, the theory T_2 is said to be nearer to the truth if, and only if, more true statements follow from it than from T_1 - but provided more false statements do not follow from T_2 than T_1 .

The ideal theory on this account, the one with maximum possible verisimilitude would be, Popper agrees, both true and comprehensively true - it would correspond with all facts. But the point behind the entire theory of verisimilitude is not to specify an ideal theory; rather it is to defend the claim that, in comparing competing theories, which may both turn out to be false, we are appraising an objective property with the same regulative character as the idea of objective truth itself. In Popper's words:

'This comparative use of the idea is its main point; and the idea of a higher or lower degree of verisimilitude seems less remote and more applicable and therefore perhaps more important for the analysis of scientific methods than the - in itself much more fundamental - idea of absolute truth itself.' 35

Popper's logic of science thus allows us to say whether or not a theory, by virtue of its content, would constitute an advance upon its predecessors if it were to stand up to test. His theory of verisimilitude, which is still undergoing further development, allows us to talk of a refuted theory being nevertheless an advance on a previous refuted theory, by virtue of being closer to the truth. Now it is important to stress that this is a logical analysis of the notion of progress in science; we can say, in the abstract, that a theory with a higher degree of verisimilitude is an advance on a theory with a lower degree. Popper's analysis spells out the sense in which there can be progress in science.

However, just as Tarski provides no criterion for recognising true theories or statements so Popper insists that we have no means of knowing for certain whether one theory has greater verisimilitude than another. Appraisal of the verisimilitude of competing theories remains a fallible conjecture of an objective property based upon the severity

35 Popper, K.R. (1963) p234

of the tests the theories have withstood to date. So when we choose between competing theories, preferring one because it has, or is conjectured to have, a higher degree of verisimilitude we may well be mistaken. Despite his elaborate logical analysis of theories and the standards they are said to approach Popper has never provided nor attempted to provide a criterion for deciding correctly between competing theories.³⁶

Explanation

Popper's account of explanation is heavily dependent upon the logical analysis of theories and standards as discussed thus far. By linking the notion of the explanatory power of a theory to its content or testability he is able to give an objective formulation of the relation between explicans and explicandum in terms of the logical relations between them. In his important paper The Aim of Science³⁷ he argues that if an explicans is to be neither circular nor ad-hoc it must be rich in content, which means that it must have a variety of testable consequences - some of which are different from the event to be explained, the explanandum. These different testable consequences then allow independent testing of the explicans. However, independent testability of the explicans is not alone sufficient to ensure that the explanation is not ad-hoc; for it always is possible to explain an explanandum such as 'The sea is rough today' (his example) by deducing it from the independently testable but nevertheless ad-hoc conjunction, 'The sea is rough today and those plums are juicy'. Hence, he argues:

³⁶ Thus Ayer is mistaken in criticising Popper for failing to provide such a criterion - for to have even tried to do so would have run counter to his entire programme. Ayer, A.J. (1974) p691

³⁷ Popper, K.R. (1972) p191ff

'Only if we require that explanations shall make use of universal statements or laws of nature (supplemented by initial conditions) can we make progress towards realizing the idea of independent, or non ad-hoc explanations. For universal laws of nature may be statements with a rich content, so that they may be independently tested everywhere, and at all times. Thus if they are used as explanations, they may not be ad hoc because they may allow us to interpret the explicandum as an instance of a reproducible effect. All this is only true, however, if we confine ourselves to universal laws which are testable, that is to say, falsifiable.' 38

So a good scientific explanation would be one, he suggests, in which the event to be explained or, more accurately, a statement describing it is deducible from a testable and falsifiable universal statement or law together with a statement of initial conditions. It would be a good scientific explanation if, and only if, it had been severely tested and had survived such tests. In line with Popper's fallibilism, as I have now repeatedly stressed, an explanatory theory which has survived such tests must be recognised as possibly false; similarly, an explanatory theory which has not survived such severe tests may nevertheless be true.

Now every explicans - i.e. universal statement, law or conjecture - may itself be further explained by being deducible from theories of a higher and higher level of universality. Hence every scientific explanation may itself be further explained and this implies, Popper argues, that there is in principle no end to science. 'Thus the task of science,' he says, 'constantly renews itself.'³⁹ At this point we can see how his account of explanation dovetails with his theory of

38 Popper, K.R. (1972) p193

39 He rejects the 'essentialist doctrine' that there are ultimate explanations not in need of further explanation. (1972) p195/6. He does accept that he may be called a 'modified essentialist' in that he does 'not doubt that we may seek to probe deeper and deeper into the structure of our world or, as we might say, into properties of the world that are more and more essential, or of greater and greater depth.'

verisimilitude. The latter combined the two fundamental aims of science, high content and truth, specifying the ideal to which our theories ought to approach as that of maximum verisimilitude. Scientific explanations are now analysed as progressive (but fallibly appraised) approximations to that goal. We hope that by continually developing conjectural theories of higher and higher degrees of universality and submitting them to the severest tests we can devise that they will probe deeper and deeper into the structure of the world and that the secrets they reveal will be true.

Popper's analysis of explanation is not, however, as neat and tidy as it may appear from this discussion. Though he has given a clear sense to the notion of one theory providing a better explanation than another he recognises that there is much more to be said. He talks of the depth of a theory as a guide to our intuitions but doubts if it is ever likely to be given an exhaustive logical explication. Despite this, however, he offers a partial logical analysis of depth on the basis of which he advances regulative standards to help us choose the better of two competing theories; as I understand him, these new standards are compatible with the view already discussed which recommends that we choose the explanatory theory with higher content and which has survived severe tests. But they go further - they are, he says, even better standards which we ought to try to attain.

In brief, he argues that a good explanatory theory will not only explain the problem (explanandum) which gave rise to it; it will actually contradict or correct it. By way of example he shows that Newton's dynamics contradicts Galileo's and Kepler's laws.⁴⁰ This allows him to claim that progress in science is revolutionary. On the

40 Popper, K.R. (1972) p202

other hand, he can claim that progress - although revolutionary - is, in a sense, conservative for a new theory must always be able to fully explain its predecessors' success.

'In all those cases in which its predecessor was successful, it must yield results at least as good as those of its predecessor and, if possible, better results. Thus in these cases the predecessor theory must appear as a good approximation to the new theory; while there should be, preferably, other cases where the new theory yields different and better results than the old theory.' 41

And so we can say that a theory which contradicts its predecessor in some way and yet which can explain why its predecessor had at least some success will, if it passes severe tests, constitute a better explanation. This provides, says Popper, a criterion of progress and means that theory-change in science can be rationally assessed. In that sense science is a rational enterprise.

Popper's objective or logical analysis of explanation contrasts sharply with the notion of explanation that results from adopting a subjective approach to knowledge - a position which is, we shall see, no stranger to psychology. From a psychological standpoint we may be said to explain an event to another person if we reduce or eliminate his perplexity or feelings of puzzlement, if we can make the person feel that he understands the cause of the event that troubled him. What is wrong with this approach, in Popper's view, is that what will reduce one man's sense of bewilderment may increase another man's and vice versa; or what dissolves my puzzlement tonight (when I am tired) may not do so tomorrow (when I am wide awake). A good scientific explanation in the objective sense, an abstract high-content theory that meets the standards discussed above is more likely, he argues, to cause considerable confusion and bewilderment - just because it is a creative

41 Popper, K.R. (1974a) p83

leap into the dark, a break with commonsense or common knowledge.⁴²

A corollary of this analysis which insists that the objective dimension is much more important for the appraisal of scientific explanation is the recognition that problems too can be looked at from two perspectives; again, it is the objective side of problems that is, says Popper, the more important. This point is stressed in his more recent work where he talks of Epistemology Without a Knowing Subject.⁴³ But the objective problem-situation that faces every student in every scientific field has been a central theme of his philosophy right from the start. Indeed this is clear from the opening lines of the preface to the first edition of the The Logic of Scientific Discovery.

'A scientist engaged in a piece of research, say in physics, can attack his problem straight away. He can go at once to the heart of the matter; to the heart, that is, of an organised structure. For a structure of scientific doctrines is already in existence; and with it, a generally accepted problem-situation.'⁴⁴

Only when he acquaints himself with the objective problem-situation will the student, perhaps, manage to feel puzzled or perplexed and thereby motivated to produce an objective explanation. But puzzlement in itself is of no real consequence here for, after all, there may be much puzzlement where no objective problem exists; in that case we talk of a pseudo-problem. Conversely, as Musgrave points out, a genuine problem may fail to provoke feelings of puzzlement for years; 'One mark of a

⁴² Perhaps the classic statement of the thesis that explanation in science is reduction to the familiar, reduction of the unknown to the known is that of Sir William Thomson (later Lord Kelvin). He wrote:

'I never satisfy myself until I can make a mechanical model of a thing. If I can make a mechanical model I can understand it. As long as I cannot make a mechanical model all the way through I cannot understand it.'

Quoted in Hempel (1965) p434 Popper, on the other hand, takes the opposite point of view:

'Thus scientific explanation, whenever it is a discovery, will be the explanation of the known by the unknown.' Popper, K.R. (1972) p191

⁴³ Popper, K.R. (1972) p106ff

⁴⁴ " " (1959) p13

scientific genius is to discover a new and important problem which had previously been undetected.⁴⁵

Methodology

The method which Popper recommends to the scientist, if he wishes to fulfill his aims of discovering highly informative theories which are also true or, at least, closer to the truth than their predecessors is that of conjectures and refutations. The scientist is advised to advance bold, imaginative hypotheses about the natural or social worlds - for Popper is a monist in this respect, as Giedymin pointed out (p39) - and then submit them to a policy of ruthless criticism and testing.

Now, in a sense, this is not a method at all. We have already seen, in the case study, that the logical positivists of the Vienna Circle claimed to have a method which would, if followed, guarantee success and progress in science. They offered a 'blueprint' for the growth of knowledge. In contrast, Popper's methodology is a much less ambitious affair. First of all he admits that there is no method for dreaming up, discovering or otherwise arriving at successful, true conjectures or theories. Every hypothesis is a wild leap in the dark and the wilder the leap the more significant will be the advance - if and only if that hypothesis resists the most severe tests we can devise against them.⁴⁶ So, as I have already said, Popper admits that there is no logic of discovery; there is no 'blueprint' for success. (Attempts at explaining the amazing success of science are, he says, in danger of

45 Musgrave, A. (1974) p572. A leading advocate of the subjective approach to knowledge, says Musgrave, is Polanyi, for whom 'nothing is a problem or discovery in itself; it can be a problem only if it puzzles or worries somebody, and a discovery only if it relieves somebody from the burden of a problem.' Polanyi (1958) p122

46 Perhaps I should say may be an advance for passing severe tests is no guarantee of a theory's truth.

proving too much. By which he means, I take it, that the 'amazing success' may turn out to be failure - the believed-to-be-true-theories may well be false. I shall return to this issue in Chapter Five where I examine Chomsky's suggestion that there may be a 'science-forming' capacity which it is the task of the cognitive psychologist to explain.⁴⁷⁾

So his methodological recommendations are really concerned with what happens once the scientist has produced a conjecture. Popper's advice is to then attempt to falsify it by constructing the most stringent tests we can think of. Again, in line with his objectivist account, he stresses that the severity of tests is an objective matter. Whether or not tests are severe is totally independent of such psychological matters as whether the scientist thinks them to be or wants them to be severe.

'The severity of our tests can be objectively compared; and if we like, we can define a measure of their severity.' 48

The objectivity of tests, incidentally, is one reason why the claims of such sociologists of science as Kuhn about the commitment scientists allegedly make to their theories fails to impress Popper. Even if it is the case, he can reply, that scientists are deeply committed to the truth of their theories and have a strong faith that they will triumph they may nevertheless manage to submit them to tests which are, from an objective standpoint, severe. On the other hand, if Kuhn exaggerates the narrowmindedness of the scientific community and scientists do try to criticise and test them may, from an objective standpoint again, fail to do so. They may wish to test them severely but never manage to invent a severe test.

47 Chomsky, N. (1976) p24

48 Popper, K.R. (1963) p388

Now Popper has written extensively on the method of science and as with the notion of verisimilitude there is a lively, ongoing debate about how we can measure the severity of tests. To review these issues in full would require a long digression and so I shall restrict myself to what I take to be his main recommendations.

Once the scientist has arrived at a theory he should, first of all, ask himself whether it is falsifiable. If it is not falsifiable either because it is, in principle, compatible with every and any eventuality⁴⁹ or because there is no method of testing it (at present) then it is not to be regarded as scientific. If the theory is falsifiable then the scientist should state, prior to any experimental tests, the conditions under which he will give it up. And should the experiments refute the theory's predictions then he should not try to immunise it by resorting to what Popper calls 'conventionalist stratagems' or ad-hoc theories which would explain away the embarrassing results. So Popper lays down a supreme methodological rule:

'It is the rule which says that the other rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification.' 50

From a methodological point of view - as opposed to a logical one - no theory is clearly falsifiable. Even when we have ruled out evasive ploys by the adoption of the above rule we must recognise, says Popper, that when we test a high-level (i.e. high content) theory we never test it alone. We test a complex of the initial high-level theory, initial conditions and auxiliary theories. And when we obtain an

49 This was the case with Adler's psychology, Popper decided. It was one of the inspirations for his proposal that falsifiability be adopted as the criterion of demarcation between science and non-science. See Popper (1974) p31

50 Popper, K.R. (1959) p54

experimental refutation we must decide which part of the complex is responsible; such decisions are, again, fallible conjectures and so we may mistakenly abandon a true (high-level) theory or mistakenly accept a false one. 'We can never,' he says, 'be certain that we challenge the right bit (of background knowledge); but since our quest is not for certainty, this does not matter.'⁵¹ This provides, incidentally, Popper's answer to Quine who (following Duhem) argues that 'our statements about the external world face the tribunal of sense experience not individually but only as a corporate body.'⁵² To which Popper replies that while this is true it does not embarrass a fallibilist - though it might turn a positivist or verificationist, who wants to know for certain which part of the complex-under-test is responsible for the refutation, into a sceptic.

Finally, as I have now stressed perhaps ad nauseam, Popper accepts that we can never justify any theory i.e. a claim that it is true, no matter how severe the tests it has withstood nor for how long it has been successful. What we can do is justify our preference for a theory by choosing the one which has emerged from the struggle for survival, the one which has best stood up to our attempts to destroy it.⁵³ And if we choose to act on this theory in the future we do not assume that it is true - in which case we would, as Popper's critics imply, be appealing to a tacit inductive principle; we simply hold

51 Popper, K.R. (1963) p238

52 Quine, W.V.O. (1953) p41

53 After a theory has survived a severe test that test passes into background knowledge and will, henceforth, not constitute a severe test. We may come to feel that a very successful theory, after much severe testing, after many failures to find falsifications, is nothing but a set of implicit definitions or conventions - as, for example, Poincare felt about Newton's theory. But once we do find a refutation we know that it was indeed empirical. 'De mortuis nil nisi bene: once a theory is refuted, its empirical character is secure and shines without blemish.' Popper, K.R. (1963) p240

that the most rational course we have is to act on the best corroborated hypothesis - in the hope that it is true. In Popper's words:

'To put it in a nutshell: we can never rationally justify a theory - that is, a claim to know its truth - but we can, if we are lucky, rationally justify a preference for one theory out of a set of competing theories, for the time being; that is, with respect to the state of the discussion. And our justification, though not a claim that the theory is true, can be the claim that there is every indication at this stage of the discussion that the theory is a better approximation to the truth than any competing theory so far proposed.' 54

Evolutionary Epistemology

I have tried in this chapter to show how Popper defends the objectivity of scientific knowledge without resorting to dogmatism. By embracing fallibilism he shows that there is no necessary connection between dogmatism and absolutism. Scientific knowledge, as he characterises it, can be viewed as a system of tentative conjectures which we guess not to be false and hope to be true. It has to be recognised that this is indeed a considerable retreat from the traditional idea of objective knowledge. On that view objective knowledge was not only definitely true but it was known to be definitely true. We might say that where the justificationists sought objectivity by reducing knowledge to solid foundations, to a 'ground-floor', Popper seeks it by reaching higher and higher to a 'ceiling' of truth.

But his analysis is not so rarefied that it is beyond the reach of working scientists. For with his account of content, truth, verisimilitude and his associated methodology he offers both a precise analysis of progress in science and a set of rules as to how best to try to achieve it. We will only achieve it when we recognise that knowledge has an objective dimension in its problems, tests, explanations, aims and standards. If we choose to ignore this dimension and look at

knowledge from a subjective, - psychological or sociological - perspective then we will fall into relativism.

Recently Popper has emphasised this objective dimension of knowledge by distinguishing between three levels of reality, a metaphysical theory which recognises World 1 (the world of physical objects), World 2 (the world of subjective, mental states or mind) and World 3 (the world of the objective contents of theories, statements etc). It is World 3 which is, from an epistemological point of view, of decisive importance. It takes priority over World 2 so that, for example, before we could have a psychology of knowledge we must have an epistemological analysis of the kind Popper has developed. What we take to be knowledge, in his case - unjustifiable conjectures - results from a decision; knowledge is a normative idea. Only when we have decided what we are to value as knowledge can we have an empirical study of how such knowledge is possible.⁵⁵ So Popper remarks:

'I suggest that one day we will have to revolutionise psychology by looking at the human mind as an organ for interacting with the objects of the third world (World 3); for understanding them, contributing to them, participating in them; and for bringing them to bear on the first world (World 1).' 56

The concentration on the objective aspects of knowledge, I have suggested, is a natural development of a distinction Popper made from the start. (See page 148) Just how this distinction, as developed in the Three Worlds theory, would 'revolutionise' psychology is further considered in the last chapter of this thesis where I discuss Popper's evolutionary epistemology.

55 Similarly, a psychology of creativity is dependent upon a normative analysis of what is to be considered creative. For a discussion of this see L.B. Briskman (1977, forthcoming).

56 Popper, K.R. (1972) p156

Before turning my attention to Popper's critics, however, I would like to forestall confusion by recognising that the term 'evolutionary epistemology' is ambiguous. It can refer to the evolution of knowledge in the objective sense, the process described in this chapter in which 'scientists try to eliminate their false theories, they try to let them die in their stead.'⁵⁷ Or it can refer to the fact that evolution itself can be viewed as a knowledge process. That is to say we can look upon the increasingly complex sense-organs with which animals, as they ascend the phylogenetic scale, adapt to their environment as 'theories' which have not yet been falsified. In this case the inborn dispositions, expectations or knowledge is knowledge in the subjective sense; it is something which the organism contains or believes (unconsciously). Most important, this subjective or a priori knowledge is not a priori valid. It is a set of expectations which, unlike human conjectures in science, have not been explicitly formulated and criticised, and which - should environmental change occur - may well be disappointed. So, finally, we can say that when a scientist investigates these inborn expectations or 'innate knowledge' what he is doing is framing fallible objective theories about subjective theories. And if successful, if his theories stand up to test, he will have objective knowledge of what we might call subjective knowledge.

57 Popper, K.R. (1972)

Chapter Four

For and Against Popper's Epistemology

(1) Introduction

Popper's theory of scientific knowledge has been at the centre of a vigorous and wide-ranging debate which has become increasingly cantankerous and confusing during the past few years and which shows little sign, as yet, of satisfactory resolution. Some writers have focused on particular aspects of his position which they believe to be inadequate, while others have attempted to dismiss the entire theory as a complete irrelevance to any reasonable account of scientific enquiry. Some have chosen to employ descriptive arguments drawn from the history of science or from the alleged practices of contemporary scientists, while others have advanced explicitly normative considerations of supposedly deadly import. A few contributors have generated confusion by relying on historical arguments while ostensibly making a normative point; conversely, others have employed tacit definitions or proposed conventions while claiming to be merely presenting historical discoveries.

Further confusion has arisen because there has not been unanimity of intention among critics using one type of argument. Thus we find among the historians and sociologists deep disagreement over whether history or contemporary practice shows science to be a rational enterprise; and among those philosophers who have considered history of science a pointless diversion there are similar disputes over Popper's claim to have demonstrated the rationality of science.

Nevertheless, the debate has served a number of useful functions. Specific issues have been examined in detail and some problems have been clarified; the debate has led to a discussion of what criteria a satisfactory methodology should meet; and the clash between Popper and

his critics has shown that no philosophy of science is sufficiently problem free to be immune from the objections of competitors. Popper's theory of science, I will argue in this chapter, constitutes a significant advance on its predecessors but must be substantially modified and supplemented if it is to show that science can be a rational enterprise and if it is to be of practical benefit to working scientists.

To begin, I propose to divide criticisms of Popper's epistemology into two categories. First, I shall consider the issues raised by his 'historical' opponents and their attempts to either modify or destroy his position. Then, after assessing the relevance of history of science to Popper's philosophy of science, I shall turn my attention to the much more important arguments and objections of his 'normative' critics.

(2) Historical Objections

Kuhn's Critique

Just when Popper's philosophy of science was beginning to weaken the support for inductivist views of science came, in 1962, the publication of Kuhn's famous essay, which sought to sweep away all attempts to understand science by building abstract models, logics of research or rational reconstructions. Only by close study of specific episodes in the history of science could we understand the mechanism and dynamics of the scientific process. Kuhn's theory has already been examined at some length and so I shall now merely summarise the descriptive, historical claims which he made and contrast them with the prescriptive, normative proposals advanced by Popper. Then I shall trace the development of this debate through the writings of Lakatos and Feyerabend. Before that let us, however, remember that Kuhn has said, in response to Feyerabend, that he is not just making descriptive

claims but also methodological recommendations based upon them.

Above all, science for Popper is a critical activity, a never-ending search for error in our theories. For Kuhn, by contrast: 'in a sense, to turn Sir Karl's view on its head, it is precisely the abandonment of critical discourse that marks the transition to a science.'¹ The central aim of science, for Popper, is maximum verisimilitude, the discovery of true theories with the highest possible content. For Kuhn, on the other hand, individual scientists do not have aims at all. Once committed to a paradigm the scientist's 'motivation is of a rather different sort'², namely the blinkered activity of puzzle-solving. The scientist must spell out, Popper demands, the conditions under which his theory will be given up - riskily - as false; in advance of experimental testing he must specify those 'basic statements' or 'potential falsifiers' which will refute his theory. According to Kuhn, however, real science does not work that way at all:

'No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature ... The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgement leading to that decision involves the comparison of both paradigms with nature and with each other.' ³

Contra Popper, says Kuhn, counterevidence does not and must not, by itself, falsify theories. The scientist, wedded to his paradigm, merely sets discrepancies between theoretical predictions and experimental evidence aside as problematic instances or anomalies which will, his convictions assure him, fade away in the end. Theory-change, for Popper, as we have seen, is rational in that it is always possible to make a direct comparison of two competing theories with the same evidence and

1 Kuhn, T.S. (1972) p6
2 " " (1962) p38
3 " " " p77

judge which of the two has better survived empirical testing. What characterises paradigm-change for Kuhn, on the other hand, is the 'incompleteness of logical contact'⁴ between the paradigms and the fact that each paradigm is judged by different evidence and standards. Hence, paradigm-change cannot be explained in purely logical terms and Kuhn falls back on sociological and psychological explanations of opinion change; paradigm switches are, typically, conversion processes. Finally, to complete this survey of the major differences between Popper and Kuhn, they also disagree over the nature of progress in science. Ideally, as theories with higher and higher content survive more and more stringent tests they hopefully, for Popper, approach the truth; therein lies progress. For Kuhn, 'we may, to be more precise, have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth.'⁵ Nevertheless, Kuhn claims, science does progress but in such a way that we have to reinterpret the notion of progress.

Popper's Reply

In 1934 Popper had already declared his opposition to any attempt to understand the methods of science by appeals to history. (Ch.2 p82) Such an approach failed to realise, we have seen him argue, that what facts it uncovered were but a reflection of the philosophy of science or, less grandly, the evaluative criteria the historian employed. Popper's specific reply to Kuhn was, in effect, exactly that which we found Shapere arguing as well.⁶ Kuhn's historical discoveries, with their relativist consequences, were not discoveries at all but the

4 Kuhn, T.S. (1962) p110

5 " " " p170

6 Shapere, D. (1964)

consequence of his attachment to the logical thesis of relativism. History of science, Popper suggested, can only begin after a philosophy of science has been proposed, for only then can we decide who is to be deemed a scientist and what activities are to constitute scientific enquiry. He replied:

'... to me, the idea of turning for enlightenment concerning the aims of science, and its possible progress, to sociology or to psychology (or...to the history of science) is surprising and disappointing. ...How can the regress to these often spurious sciences help us in this particular difficulty? Is it not sociological (or psychological, or historical) science to which you want to appeal in order to decide what amounts to the question 'What is science?' ' 7

Kuhn's Reply

These remarks puzzled Kuhn for he thought that in this area - namely, the relevance of history of science to philosophy of science - he and Popper saw eye to eye. He replied:

'If he means that the generalizations which constitute received theories in sociology and psychology (and history?) are weak reeds from which to weave a philosophy of science, I could not agree more heartily. My work relies on them no more than his. If, on the other hand, he is challenging the relevance to the philosophy of science of the sorts of observations collected by historians and sociologists, I wonder how his own work is to be understood. His writings are crowded with historical examples and with generalizations about scientific behaviour ... He does write on historical themes, and he cites those papers in his central philosophical works. A consistent interest in historical problems and a willingness to engage in historical research distinguishes the men he has trained from the members of any other current school in the philosophy of science. On these points I am an unrepentant Popperian.'⁸

I have quoted Kuhn at some length because I have some sympathy with his puzzlement. Popper does not make it entirely clear how his philosophy

7 Popper, K.R. (1972a) p58

8 Kuhn, T.S. (1972) p235/6

of science relates to history of science or the actual practices of working scientists. But that is not to say, as Kuhn suggests, that there are no differences between them in this area. Popper's falsifiability criterion did not result from an extensive, historical investigation of how scientists have actually behaved; nor when he advises us, for example, to prefer a theory with greater 'empirical content' than its predecessors does he do so because that is what scientists have typically done. All Kuhn's methodological recommendations, on the other hand, are abstracted from the alleged practices of scientific communities.

How then are we to regard Popper's frequent excursions into the history of science and his reconstructions of past theory-changes in terms of his own model of science? I will later argue that they are but illustrations of the relevance of Popper's theory to the concerns of working scientists; they do not provide a warrant for his methodological prescriptions, as Kuhn's historical examples are intended to do for his. We will see that Popper's definition of science or falsifiability criterion and the associated methodology are based on an epistemological argument, on what McMullin calls a 'general theory of rationality'.⁹ Whether that argument succeeds and whether Popper provides a satisfactory defence of his methodology is thus an issue which must be considered independently of what methods scientists have or have not used in the past. And I will consider that issue later in this chapter. So even if Popper's illustrations were, as Kuhn claims, historically false that would not, I will argue, be a very serious matter. If there were independent considerations favouring Popper's theory it would not be very embarrassing if those whom we choose to call past scientists did not obey Popperian rules.

9 McMullin, E. (1970) p31

Lakatos's Attempted Resolution of the Popper-Kuhn Debate

'Philosophy of science without history of science is empty; history of science without philosophy of science is blind.'¹⁰ With this paraphrase of the Kantian dictum one eminent member of Popper's school, Imre Lakatos, sums up his own response to Kuhn, a response very different from that of Popper himself and from the argument I intend to develop and alluded to above. In a series of long papers¹¹ Lakatos has tried to present a philosophy of science which would preserve the advances he finds in Popper - especially his objectivism, fallibilism and his conventionalist-empiricist criticism - while abandoning those features he finds unacceptable in the light of Kuhn's critique. Then, after elaborating his own theory of science, what he calls a 'methodology of scientific research programmes', Lakatos develops a further metatheory for the assessment of competing philosophies of science. On that metatheory, which he chooses to call a 'methodology of historiographical research programmes', he attempts to demonstrate that Popper's 'methodological falsificationism' (as he terms it) is an advance on such philosophies of science as inductivism, conventionalism and 'dogmatic falsificationism',¹² but is in its turn superseded by his own methodology of scientific research programmes - henceforth MSRP. Lakatos, therefore, claims to have provided not only a theory of science superior to both Popper's and Kuhn's but to have provided a criterion for recognising progress at the metatheoretical level. Let us, therefore, see how he develops his rather complex position.

He begins by recalling how Popper, according to his own account,¹³ arrived at his falsifiability criterion. Newton's theory, though refuted, was a major achievement in Popper's eyes as in those of most

10 Lakatos, I. (1971) p91

11 " " " ; (1972); (1974)

12 " " (1971) p95

13 Popper, K.R. (1974) p31/2

scientists of his day; so too was Einstein's general theory of relativity. On the other hand, Popper was unimpressed by the ability of Marxists, Freudians and Adlerians to find easy verifications of their theories. These latter theorists had a ready explanation for everything that might occur while Einstein, Popper noted, argued very differently. He did not have an explanation for everything that might happen and, in contrast, even stated that he would reject his general theory if specific experimental results were obtained. Popper's problem was thus, says Lakatos, to find a definition of science which would yield his 'basic judgements' about these theories, which would define as 'scientific' those he admired and as 'unscientific' those he did not. Popper's solution, says Lakatos, was falsificationism.

Lakatos next exploits what he has decided was Popper's strategy, to propose a provisional metacriterion for the assessment of theories of scientific rationality. He suggests that, 'a rationality theory - or demarcation criterion - is to be rejected if it is inconsistent with the accepted 'basic value judgements' of the scientific community.'¹⁴ He argues, in support of this proposal, that though there is little agreement over a universal criterion of the scientific character of theories there is considerable agreement concerning single achievements in science during the last two centuries. He writes:

'While there has been no general agreement concerning a theory of scientific rationality, there has been considerable agreement concerning the rationality of a particular step in the game - was it scientific or crankish? A general definition of science thus must reconstruct the acknowledgedly best games and the most esteemed gambits as 'scientific'; if it fails to do so it has to be rejected.'¹⁵

'Quasi-empirical' is the label that he hangs around his metatheory of science, a metatheory to be contrasted with the apparently 'aprioristic'

14 Lakatos, I. (1974) p246
15 " " " "

Popperian approach and, I presume, the empirical approach he would attribute to Kuhn. What Lakatos is denying is that it is possible to understand science either by laying down abstract rules against which the 'game of science' (as he repeatedly calls it) is adjudged or by recording the activities of scientists as historians and sociologists try to do. In adopting this procedure which we will see him modify in a Socratic manner in the next few pages, Lakatos argues that in fact his is the very same procedure as Popper's. Though Popper appears to propose an 'apriori' definition of science, says Lakatos, he is - were he aware of it - a quasi-empiricist too. For, as we have seen Lakatos note, Popper starts by observing and assessing the claims of scientists, loosely characterised, and then proceeds to propose a sharp demarcation criterion which will support the judgements he has already made about these claims. The major difference between the two approaches, on this reading, is that the 'basic value judgements' Popper's definition of science must reconstruct are his own, while those which Lakatos's theory demands of an ideal definition or demarcation criterion are those of the scientific community.

Once we accept the above metacriterion we must, says Lakatos, reject Popper's criterion because in its light the best scientific achievements in the past were 'unscientific' and because 'the best scientists, in their greatest moments, broke the rules of Popper's game of science.'¹⁶ In other words, Popper's definition of science fails, he suggests, to reconstruct as rational the 'basic value judgements' of the scientific community. We must reject Popper's theory first and foremost, he says (echoing Kuhn), because no theory in the history of science has ever been falsifiable at all. 'Exactly the most admired

16 Lakatos, I. (1974) p246

scientific theories simply fail to forbid any observable state of affairs.¹⁷ So if Popper denounces Freudian theory as unscientific because criteria of refutation have not been laid down before any experimental investigations are carried out then he must also agree that Newtonian theory, which similarly lacked such criteria, was 'unscientific' as well.¹⁸ Lakatos defends this claim with a 'characteristic story' in which a typical scientist continually refuses to abandon his pet theory even though its predictions clash with experimental evidence. Such a scientist will continue to invent auxiliary theories which, together with the initial theory and initial conditions of testing, will account for the evidence. Lakatos argues that since a scientist always tests an initial theory in conjunction with initial conditions and auxiliary theories he always has the option, on the appearance of apparently 'falsifying' evidence, of saving his pet theory and replacing either the initial conditions or any (or all) of the auxiliary theories. Lakatos, therefore, agrees with Kuhn that the history of science is a history of theories which are not falsifiable in the way which Popper demands.

Should Popper retreat, in the light of this criticism, to the demand that systems of theories, including initial conditions and auxiliary or observational theories, be falsifiable, we will, says Lakatos, still have to reject his criterion. For the fact is, he says, that all theories in the history of science have progressed amid an ocean of 'anomalies'.¹⁹ Where Popper demands that some part of the system of theories under test be conjectured to be false what has actually happened, says Lakatos (in complete agreement with Kuhn), is

17 Lakatos, I. (1972) p100
18 " " (1974) p247
19 " " (1972) p135

that refuting instances or counter-examples have been either set aside as anomalies to be solved at a later stage - or else they were dissolved with the help of ad hoc solutions which violate Popperian rules. And even when anomalies which have been set aside pile up and resist repeated efforts to remove them, they eventually only 'falsify' the initial theory or paradigm, Lakatos argues (again, in full agreement with Kuhn) if an alternative was proposed which was able to explain them. He concludes:

'That in their choice of problems the greatest scientists 'uncritically' ignore anomalies (and that they isolate them with the help of ad hoc stratagems) offers, at least on our metacriterion, a further falsification of Popper's methodology. He cannot interpret as rational some most important patterns in the growth of science.'²⁰

Moreover, Popper's methodological demand that we ought not to work on an inconsistent theory (because it fails to forbid any observable state of affairs) is, says Lakatos, also 'falsified' by the history of science. And he provides numerous examples where research programmes progressed on inconsistent foundations. Only by breaking the rules which Popper demands, he says, were scientists able to let their theories outgrow such 'infantile diseases' as inconsistency, anomalies, ad hoc hypotheses etc. Accordingly, Lakatos concludes that Popper's demarcation criterion fails to reconstruct as rational many of what he believes to have been crucial moves made by scientists in the past, judges our most admired theories 'unscientific' and, consequently, must be rejected.

When we reject Popper's theory we have, Lakatos continues, two alternatives. We may abandon the attempt to give a rational account of the success of science on the grounds that every 'logic of science' to date has been a caricature of the actual course of scientific history. Inductivism, the attempted reconstruction of science in terms of hard

20 Lakatos, I. (1971) p112

facts and proven theories was, he says, 'falsified' by - among others - Duhem, Popper and Agassi²¹; conventionalism, the attempted explanation of scientific revolutions as the replacement of cumbersome frameworks by simpler ones, has been, he says, historiographically falsified by Kuhn²²; and now Popper's falsificationism has been, he adds, undermined both by his own and by Kuhn's historical studies. Instead of proposing yet another 'logic of science' we may follow the paths of Polanyi, Kuhn and the sociologists of science who merely explain, as he puts it, 'changes' in belief. For reasons very similar to those I have used in evaluating Kuhn, Lakatos concludes that this approach results in a view of science as a basically irrational enterprise²³ and eventually it results in scepticism.²⁴ Consequently he chooses the second alternative and tries to develop an improved methodology which offers a better reconstruction of science than its predecessors are said to have provided.

Conventionalism plays a key role in Popper's theory of science, at the level of 'basic statements'; these are provisionally accepted and awarded falsifying power by convention. They are not, as we have seen Popper claim, justified in the least degree by experience. In Lakatos's MSRP conventionalism plays an even more important role for not only are singular 'basic statements' accepted by convention but universal statements are accepted by convention as well. The unit of appraisal in MSRP is not an individual theory or series of theories but what Lakatos calls a 'research programme' with a conventionally accepted 'hard core' - a universal statement which is, 'by provisional decision, irrefutable'²⁵; thus the hard core has, temporarily, a metaphysical

- 21 Lakatos, I. (1971) p114/5
- 22 " " " p115
- 23 " " (1972) p115
- 24 " " " p115, footnote 2
- 25 " " (1974) p248

status. It is not falsifiable by accepted basic statements. (For example, Newton's research programme, according to MSRP, consisted of his three laws of dynamics and his law of gravitation.) Every research programme also possesses, he says, a 'positive heuristic' which 'consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research programme, how to modify, sophisticate, the 'refutable' protective belt'²⁶ of auxiliary theories around the hard core. When discrepancies arise between the hard core's predictions and (conventionally accepted basic statements describing) experimental results the positive heuristic, in effect, provides a strategy for research. It promises to turn these apparent anomalies into eventual victories for the programme and thus simultaneously provides a rationale for setting anomalies temporarily aside and explains the relative autonomy of theoretical science in a way which, says Lakatos, Popper's theory cannot. The positive heuristic generates more and more complicated 'models simulating reality'²⁷, predicts that anomalies almost inevitably arise - since false simplifying assumptions are nearly always made - and spells out a programme (to varying extents) for digesting them.

'It should be pointed out,' says Lakatos, 'that the methodology of scientific research programmes has more teeth than Duhem's conventionalism: instead of leaving it to Duhem's unarticulated common sense'²⁸ to judge when a framework is to be abandoned, I inject some hard Popperian elements into the appraisal of whether a programme progresses or degenerates or of whether one is overtaking another.'²⁹ We can thus

26 Lakatos, I. (1972) p135

27 " " " "

28 Duhem, P. (1954)

29 Lakatos, I. (1971) p115

see that this MSRP is designed to wed Kuhn's 'discoveries' to Popper's objectivity demands. The hard core and positive heuristic correspond loosely to Kuhn's concept of paradigm but the introduction of criteria of progress and degeneration allows Lakatos to claim to be defending the rationality of science via comparative appraisal against objective standards. As long as a research programme's theoretical growth, (i.e. theoretical growth where the hard core is modified by the positive heuristic), anticipates its empirical growth, that is, continues to predict 'new facts' with some success it is progressing; a research programme is degenerating if it either gives post-hoc explanations of chance discoveries or facts predicted successfully by a rival or even if it predicts novel facts successfully 'in a patched-up development rather than by a coherent, pre-planned positive heuristic.'³⁰

Lakatos differs further from Kuhn in that the former recommends that a scientist ought, if possible, to work on rival programmes - especially if one of them is vague. The scientist may expose a programme's weakness by working on it, giving it a precise formulation and then showing a rival's superiority. 'Simultaneous work on rival programmes, of course, undermines Kuhn's thesis of the psychological incommensurability of rival paradigms.'³¹ Another reason for Lakatos's support of simultaneous work by scientists, either individually or as a group, on rival research programmes is that 'the progress of one programme is a vital factor in the degeneration of its rival.'³² If a research programme successfully predicts a 'new fact' which its rival can only 'explain' in an ad hoc manner then the rival is, by definition,

³⁰ This last kind of degenerating science is typical of much work in the social sciences, Lakatos maintains. (1971) p125; the work of Meehl, P. and Lykken, D.T. illustrates, he says, how it is possible to predict 'new facts' with some empirical success but in a non-progressive way. Meehl, P.E. (1967)

³¹ Lakatos, I. (1971) p125, footnote 37
³² " " " " " "

degenerating. Thus the more the first progresses the more its rival degenerates.

According to Lakatos MSRP is, at once, both more strict and more tolerant than Popper's philosophy. It is more strict in its standards in that a programme is not progressing if it merely predicts some new phenomena which are subsequently corroborated. MSRP further demands that auxiliary hypotheses added to the initial hard core and resulting in successful predictions be 'largely built according to a preconceived idea, laid down in advance in the positive heuristic of the research programme.'³³ It is more tolerant in its appraisal of whether a research programme is meeting its standards; for the research programme is allowed to outgrow such 'infantile diseases' as inconsistent foundations, anomalies and ad hoc manoeuvres. Where Popper's code of scientific honesty demands, as Lakatos supposes, the rejection of a falsified theory at once³⁴ his MSRP permits such signs of degeneracy for a time. They do not lead to the immediate rejection of a research programme:

'The old rationalist dream of a mechanical, semi-mechanical or at least fast-acting method for showing up falsehood, unprovenness, meaningless rubbish or even irrational choice has to be given up. It takes a long time to appraise a research programme: Minerva's owl flies at dusk.'³⁵

Since MSRP does not demand that a research programme make progress at every step it is very difficult to decide when it has so degenerated that it should be abandoned. For a seemingly hopeless research programme can always, Lakatos insists, make a comeback and triumph against all expectation. Conversely, a research programme which has been much more progressive than its rivals may suddenly start to degenerate and be

33 Lakatos, I. (1974) p249
34 " " (1972) p108
35 " " (1974) p249

superseded by a competitor. Hence, in MSRP, there are no 'crucial experiments', no 'decisive refutations', no 'instant rationality'.³⁶ And if we should choose to identify some particular experimental result as crucial, as the result that turned the tide in favour of a programme, we could only do that, Lakatos suggests, long after the event when we have seen how the battle between the programmes turned out.

In contrast to Popper, therefore, Lakatos does not offer advice to the scientist who, aiming for maximum verisimilitude, is faced with two competing research programmes and wishes to know how to proceed. Where Popper recommends the fallible elimination (as a contender for the truth) of the theory which has been falsified (by an accepted basic statement) Lakatos insists that it is perfectly legitimate to stick to the degenerating programme in the hope that it will recover; and it is equally legitimate (rational) for him to do the opposite if he wishes, namely to abandon the degenerating programme for its more progressive rival. That is what lies behind Lakatos's statement that 'philosophy of science is more of a guide to the historian than to the scientist.'³⁷ All that the philosopher of science can do, he says, is enable an objective appraisal to be made of the comparative progress of rival programmes; he does not tell the scientist how to proceed. 'Thus methodology,' he says, 'is separated from heuristics, rather as value judgements are from ought statements.'³⁸

This provoked a retort from Feyerabend that Lakatos's objective standards were but 'verbal ornaments'³⁹ and that he had so modified Popper's initial position that this new MSRP was now compatible with his own epistemological anarchism. In other words, Lakatos is saying,

36 Lakatos, I. (1974) p249
37 " " " p252
38 " " (1971) p123
39 Feyerabend, P. (1972) p215

Feyerabend remarked, that: 'Anything goes.'⁴⁰ To which Lakatos replied:

'This does not mean as much licence as might appear for those who stick to a degenerating programme. For they can do this mostly only in private. Editors of scientific journals should refuse to publish their papers which will, in general, contain either solemn reassertions of their positions or absorption of counterevidence (or even of rival programmes) by ad hoc, linguistic adjustments. Research foundations, too, should refuse money.'⁴¹

How Feyerabend dealt with this claim will be discussed in a moment but first I will complete my review of Lakatos's methodology and metamethodology.

We have seen that Lakatos constructed his methodology so that it would evade the criticisms which 'falsified' Popper's falsificationism on his metacriterion. So MSRP is, in effect, an amended demarcation criterion: the 'game of science' is defined in such a way that it 'squares' with history of science as Kuhn and Lakatos describe it. The problem which faces us, Lakatos next suggests, is how we are to know that the 'game of science', played in line with MSRP, helps us promote the growth of knowledge? This is the very same problem which, he says, Popper has never solved for his 'game of science'. Popper has never given any reason for believing that his methodological rules best help him achieve his aim of detecting genuine error in his theories. Accordingly, Lakatos baldly states: 'Popper's demarcation criterion has nothing to do with epistemology. It says nothing about the epistemological value of the scientific game.'⁴² We shall see, in the second half of this chapter, that Lakatos has indeed put his finger on a major weakness in Popper's position - his ultimate failure to provide a rationale for his methodological rules. However, we shall see that Lakatos's proposed solution for Popper's and his own problem, a conjectural 'inductive principle' relating methodological appraisals with verisimilitude, is

40 Feyerabend, P. (1975) p28

41 Lakatos, I. (1971) p105

42 " " (1974) p254

unsatisfactory but that a solution can be developed more in line with the spirit of Popper's philosophy - but a solution that will necessitate substantial modifications both to the aims and the methodology we give to science.

Lakatos's next move in his argument in defence of the rationality of science is to use his metacriterion to assess MSRP. Very quickly, he says, he realised that MSRP, like all previous definitions of science, does not square completely with the 'basic value judgements' of the scientific community. 'My methodology too - and any methodology whatsoever - can be 'falsified', for the simple reason that no set of judgements is completely rational and thus no rational reconstruction can ever coincide with actual history.'⁴³ The 'common scientific wisdom'⁴⁴, embodied in the judgements which the scientific elite make about particular theories is not - he concludes - as wise as it might be. Therefore, instead of eliminating his methodology, MSRP, he suggests that we modify his metacriterion - 'and then replace it altogether with a better one.'⁴⁵ First, he says that if a universal rule should clash with a particular 'normative basic judgement' we should allow the scientific community time to reflect on the clash: they may give up their judgement and submit to the rule. He then proposes a new metacriterion, abandoning what he calls naive falsificationism in meta-method just as MSRP abandons it at the level of method. He continues:

'While maintaining that a theory of rationality has to try to organise basic value judgements in universal, coherent frameworks, we do not have to reject such a framework immediately merely because of some anomalies or other inconsistencies. We should, of course, insist that a good rationality theory must anticipate further

43 Lakatos, I. (1971) p116
44 " " " p121
45 " " " p116

'basic value judgements unexpected in the light of its predecessors or even that it must lead to the revision of previously held basic value judgements. We then reject a rationality theory only for a better one, for one which, in this quasi-empirical sense, represents a progressive shift in the sequence of research programmes of rational reconstructions. Thus the new - more lenient - metacriterion enables us to compare rival logics of discovery and discern growth in 'meta-scientific' - methodological knowledge.'⁴⁶

So, on this new metacriterion, Popper's falsificationism is not rejected just because it is 'falsified' by some 'basic judgements' of leading scientists. Lakatos rejects it because his own MSRP allegedly reconstructs as rational more of the actual historical judgements of the scientific elite. However, though rejected, Popper's falsificationism can be seen to have been an advance on previous methodologies such as inductivism and conventionalism. For he rehabilitated as rational, scientific activity which previous methodologies dismissed as irrational. For example, phlogiston theory, though false, was a scientific theory - rationally held for some time - from Popper's perspective; inductivism had deemed it pre-scientific. On the other hand, Lakatos says that MSRP can reconstruct as rational many actual scientific judgements which Popper's theory can only see as irrational. These include, he says, scientists' frequent defence of degenerating research programmes, selective blindness to embarrassing evidence (falsifications), tolerance of inconsistencies and recourse to ad hoc hypotheses. These moves are 'irrational' for Popper but not for MSRP, says Lakatos; and thus the latter methodology constitutes 'yet a further step forward.'⁴⁷

Finally, Lakatos argues that the lesson to be learned from all this is that both aprioristic approaches to methodology - such as

46 Lakatos, I. (1971) p116/7
47 " " " " p117

Popper's⁴⁸ - and empirical approaches - such as Kuhn's and Polanyi's - are mistaken. Against the aprioristic position he says that it has been scientific standards as applied 'instinctively' by the scientific elite that have been the main yardstick of the philosopher's universal laws. 'Methodological progress ... still lags behind common scientific wisdom.'⁴⁹ Against the empirical approach we have seen him argue that it is not 'completely rational'⁵⁰ and, he adds, 'the philosopher's statute law may occasionally be right when the scientist's judgement fails.'⁵¹ And so his new metacriterion - the methodology of historiographical research programmes - implies, he says, a pluralistic system of authority between aprioristic statute law and empirical case law; and it 'specifies ways both for the philosopher of science to learn from the historian of science and vice versa.'⁵²

Feyerabend's Critique of Lakatos

Lakatos's strategy was to defend the rationality of science by loosening up the definition of science introduced by Popper so that it would be immunized against the kinds of criticism made by Kuhn. A definition of science must, he proposed (initially), 'square' with the facts of history. Accordingly he developed a definition, MSRP, which is allegedly closer to the reality of actual science than any other and is therefore the best available methodology; and it demonstrates, he adds, that science can be a rational enterprise.

48 Popper's strategy can, he admits, be taken two ways. Popper insists that his is an aprioristic approach and it is in that sense that it is opposed by Lakatos. However, we have seen that Lakatos argues that it is in fact a 'quasi-empirical' approach - an interpretation Popper would reject as naturalistic and, therefore, mistaken.

49 Lakatos, I. (1971) p121

50 " " " "

51 " " " "

52 " " " "

His efforts, however, have failed to impress, at least in the intended manner, the self-proclaimed epistemological anarchist Paul Feyerabend who holds that science follows no methodological rules whatsoever and generates results which are in no way epistemologically superior to the findings of magicians, astrologers, witchdoctors and those who are otherwise dismissed as 'cranks' by our allegedly intolerant, science-obsessed society. Indeed, Feyerabend goes so far as to argue that Lakatos, *malgré lui*, has produced the most ingenious defence of epistemological anarchism to date. And so he recommends that epistemological anarchists, if they exist, ought to adopt Lakatos's position because his 'irrational theory falsely interpreted as a new account of reason will be a better instrument for freeing the mind than an out-and-out anarchism that is liable to paralyse the brains of almost everyone.'⁵³

Feyerabend begins his critique with an attack on Lakatos's definition of science, MSRP. We have seen that this methodology does not offer advice to a scientist, much less instruct him how to proceed. All it does is provide standards for the appraisal of moves already made. So any choice which a scientist makes, when faced with two rival research programmes - one progressing, the other degenerating - is rational, for it is compatible with the standards of MSRP. Since a degenerating programme may recover at any time a scientist may choose it, use it and defend his choice by saying that the degeneration is only temporary and not a sign that the programme's hard core is false. So Feyerabend concludes:

'Reason as defined by Lakatos does not directly guide the actions of the scientist. Given this reason and nothing else, 'anything goes'.'⁵⁴

53 Feyerabend, P. (1975) p214
54 " " " pl86

So liberal is MSRP, he says, that it is compatible with anarchism.

We have seen earlier that Lakatos, in reply to Feyerabend, said that his methodology does not legitimate every and any decision a scientist may make. And so he recommended that journal editors and research sponsors should refuse publicity and money for degenerating research programmes' supporters. Such advice is not irrational, Feyerabend concedes, for it does not conflict with the standards of MSRP; but since no advice would conflict with those standards - they do not, after all, forbid anything - it would not have been irrational for Lakatos to have given the opposite advice. He could equally well have encouraged support for degenerating programmes alone, without violating his standards. If we should, however, accept Lakatos's advice to withdraw support for degenerating programmes we should realise, Feyerabend asserts, that this policy is not based on reasons or arguments.

'Briefly, but not at all unjustly: research programmes disappear not because they get killed in argument but because their defenders get killed in the struggle for survival.' 55

That such a procedure is 'rational' for Lakatos, in that it does not conflict with his 'rationality standards', simply shows how liberal those standards are; this theory of rationality, Feyerabend concludes, is anarchism in disguise.

Nor has Lakatos shown, as he claims, 'rational change' where 'Kuhn and Feyerabend see irrational change.'⁵⁶ Historical data impress upon us inescapably, says Feyerabend, that theory-change in science is nothing but a 'power struggle' pure and simple, full of 'sordid personal controversy.'⁵⁷ Kuhn's researches bear out this interpretation

55 Feyerabend, P. (1975) p197

56 Lakatos, I. (1971) p118

57 Feyerabend, P. (1975) p199

too, he says, despite Kuhn's attempts to evade the consequences. Paradigms triumph not because their arguments are overpowering but because of 'mob psychology' and because the defenders of rival paradigms just fade away.⁵⁸ But this is exactly the picture painted by Lakatos, Feyerabend insists, a picture that is 'rational' in name alone.

The critique continues with a two-pronged assault on Lakatos's basic strategy, the attempt to make 'basic value judgements' of the scientific elite or the 'common scientific wisdom' the measure of method. First, Feyerabend argues that Lakatos exaggerates the degree of agreement in the history of science over specific achievements. 'He believes that uniformity of basic value judgements prevailed 'over the last two centuries' when it was actually a very rare event,' argues Feyerabend,⁵⁹ who lists numerous disagreements which, he says, undermine that claim. Second, he suggests that even where there was uniformity of judgement on a particular theory those judgements were 'rarely made for good reasons.'⁶⁰ So he concludes that "'common scientific wisdom' is not very common and it certainly is not very wise."⁶¹ Further, he points out - rightly in my view - that Lakatos fails to provide any argument at all to show that these 'basic value judgements', even if they were commonly held, would be worthwhile. Lakatos just assumes that such judgements establish the superiority of modern science over other fields of enquiry, he adds, when that is far from obvious.

'Rational reconstructions' take 'basic scientific wisdom' for granted, they do not show that it is better than the 'basic wisdom' of witches and warlocks. Nobody has shown that science (of 'the last two centuries') has results that conform to its own 'wisdom' while other fields have no such results ... To find the right method, one must reconstruct the right discipline. But what is the right discipline?'⁶²

58 Feyerabend, P. (1975) p199
59 " " " p203
60 " " " p202
61 " " " p203
62 " " " p205

Incidentally, it is Lakatos and not Popper who, according to Feyerabend, fails to realise what he is in fact doing in his theory of science. Lakatos, we have seen, argued that Popper's approach could be taken two ways, either as an aprioristic or as a quasi-empirical one; and Lakatos suggested it was in reality quasi-empirical like his own MSRP. Not so, says Feyerabend. Despite appearances, he says, Lakatos's methodology is a traditional 'logic of science' because the 'common scientific wisdom' is admitted to be a fallible judge of methodologies and can, in the last resort, be ignored. History of science does not play a decisive role after all, says Feyerabend - a point which has been independently made by McMullin: 'the uneasiness the reader feels with the over-all methodology of (Lakatos's) monograph is due mainly to the equivocal role assigned to history of science, at once emphasised and called upon as evidence, yet systematically 'reconstructed' in the service of a prior theory of rationality.'⁶³

To sum up, Feyerabend rejects Lakatos's claim to have demonstrated that science is a rational method of enquiry whose consequence is the growth of objective knowledge. First, he claims to have shown that Lakatos, in his liberalisation of Popper's theory so that it would be immune to Kuhn's historical criticisms, has in fact embraced an anarchistic conception of science in which 'anything goes.' And second, he denies that Lakatos has shown that the consequence of pursuing his methodology is objective knowledge. Lakatos merely asserts, without supporting argument, that science as practised is rational; whereas Feyerabend argues that real science is irrational because the crucial factors in actual theory-change are not reasons or arguments but social pressures, psychological trickery and propaganda. Historical

63 McMullin, E. (1970) p34

considerations do play a decisive role for Feyerabend as epistemological anarchist in contrast to the equivocal and ultimately inessential role they play for Lakatos, the merely illustrative role they are given by Popper and to Feyerabend's earlier position when, as an arch-Popperian critical rationalist he dismissed them as impotent criticisms of philosophy of science.

Historical Objections Assessed

Popper's demarcation criterion, the principle of falsifiability, is based - as I suggested earlier - on an epistemological argument. It is this: We must assume that most of our conjectures about the world (or any set of phenomena we are investigating) will be false; to assume otherwise is to hold that we are epistemologically privileged (in that we have, for example, innate a priori valid knowledge or that there is a pre-established harmony between Mind and Nature). Therefore, if our aim is to find out the truth about the world (phenomena), we ought to try to detect falsity in our conjectures. The best way to do that is to make them experimentally refutable. For we can then eliminate false theories by observation and experiment. And while we cannot know for certain that the remaining conjectures, which have resisted attempts to refute them, are true, we can hope that they are: they are the best 'knowledge' we can ever attain.

In the second part of this chapter I shall argue that there are serious flaws in Popper's defence of falsificationism via the above argument. But those flaws can be, we shall see, repaired in a way which, though it calls for major changes in Popper's theory, can nevertheless be said to conform to the spirit of his philosophy. Now I want to consider the historical objections already discussed.

Popper's demarcation criterion was proposed as a convention based on the above argument; it was not advanced as a conjecture about theories in the history of science. To undermine Popper's position, therefore, it is necessary to show that there is a flaw in the argument; one must challenge either the premises or the reasoning. Some philosophers have tried to do just this, as we shall shortly see. The objections drawn from the history of science, on the other hand, do not challenge either the premises or the inference of the argument. They do not attack falsifiability as convention but as alleged historical fact. But since Popper is not claiming that all or most theories in the history of science, however defined, were or must have been falsifiable the alleged historical discoveries, even if true, are simply irrelevant. For Popper could accept that no theory in the history of science was ever falsifiable⁶⁴ and yet still maintain that the best method for the scientist, who aims at discovering true theories, is to propose falsifiable theories and then try to find out if they might be true by seeking to refute them. Whether Popper could, in the circumstances, continue to call his logic and methodology an analysis of science is debatable. We might decide to reserve that term for whatever aims and methods we believe we have uncovered via historical research - an approach which Popper dismisses as naturalistic (for reasons already given). Or we might decide that the aim of science which Popper recommends, the discovery of maximum verisimilitude, is the aim which science ought to pursue and accordingly we may choose to call 'scientific' the associated methodological recommendations which he makes. Or we might decide that a different aim ought to be pursued, such as the

64 We are, thus, not obliged to agree with the claim that 'Popper's view of science slides on to its history like a glove.' Magee, B. (1973) p28

discovery of theories which are not only resistant to falsifying tests but which are also coherent, simple, intelligible etc, and we might then devise our methodology in order to have the best chance of achieving our aim; and then that methodology would be held to be 'scientific'. But in any case, whatever we call the methodology, it is to be defended not because it was in fact used in the past but because it best enables us, we believe, to achieve our aims.

So how are we to interpret Popper's frequent discussions of previous debates in science or of live issues in contemporary science? They should not, I suggest, be interpreted as saying that scientists were or are imbued with Popperian principles, selecting the most testable theory, shunning ad hoc hypotheses, dismissing theories on the appearance of counterevidence and so on. They should be seen, instead, as attempts to illustrate the relevance of Popper's convention and methodology to the assessment or evaluation of past debates in what we loosely call 'science'. Past theory changes are rational, in Popper's view, because they can be reconstructed in terms of his own theory of science and shown to conform to his normative proposals, not because the participants are held to have been unwitting Popperians.

Where Lakatos goes wrong, I believe, is in ignoring the epistemological argument on which Popper's convention is based and in concentrating, instead, on the latter's motivation in putting forward his proposal. Falsificationism is not to be defended because it supports personal feelings or judgements Popper had or made about Freud's and Einstein's theories but because it allegedly opens up a route to the discovery of error and to the possibility of progress towards the truth, to the achievement of aims which are held to be worthwhile. Lakatos, we might say, overlooks the objective, epistemological defence of falsificationism for a subjective, psychological account of its

origins. And it is this which leads him to develop a metamethodology in which methodologies are judged in terms of their correspondence with 'basic value judgements' of the scientific elite; but, as Feyerabend has correctly pointed out, agreement with such judgements (where they exist) does not speak in favour of a methodology unless we have independent reasons for thinking that they are sound; and, as Feyerabend has further and equally correctly argued, no such reasons are given by Lakatos. So there is no reason why we should accept his metamethodology.

What does speak in favour of a methodology, I suggest, is that it provides us with some reason for holding that it gives us a better chance of achieving the aims of science than any other methodology; and for that methodology to be valued we must, of course, be able to say that the specified aim is worth achieving. But since, on Lakatos's own admission, his MSRP does not tell the scientist how best to proceed if he wishes to achieve the specified aim of science - which is, for MSRP, maximum verisimilitude - I conclude that we have been provided with no reason for adopting Lakatos's methodology either.

Popper's theory, I conclude, if interpreted in the manner intended - as a convention based on argument - emerges from Lakatos's critique unscathed. Hence we do not need to replace it with his MSRP - which is just as well. For it offers no guidance to the practising scientist, a state of affairs that could only appeal to the epistemological anarchist.

(3) Normative Objections

The Problem of the Empirical Basis

Popper gives no logical role to observation in science and has thus failed both to demonstrate that science is a rational mode of enquiry and to escape from a radical scepticism. This is, in short, the

gist of a most important criticism of Popper's theory that has been developed independently by two philosophers, Ayer⁶⁵ and Deutscher⁶⁶. Their diagnosis and cure for Popper's problem are broadly similar and outlined below.

Statements can be justified only by statements, Popper insists; they cannot be justified by experience. Since every report of experience - a statement - contains universal terms that report, Popper maintains, inevitably goes beyond or says more than the single experience of the observer. 'Universals cannot be reduced to classes of experiences; they cannot be constituted.'⁶⁷ Experience may instead, he says, cause or motivate us to accept a 'basic statement', to agree provisionally and conventionally that a statement will be treated as 'basic' or unproblematic. This is, both Ayer and Deutscher agree, Popper's position.

The consequences of this position, they also agree, are disastrous for it quickly lands Popper in scepticism. Ayer begins by admitting that Popper leaves open the possibility that we may decide to accept a false statement or reject a true one. But this possibility is entirely abstract, Ayer rightly adds, because there is no way by which we could discover that we had been mistaken one way or the other. Should we change our minds about a basic statement and, say, accept a statement we had previously rejected that new decision could be no more justified than the previous one. Experience cannot provide any justification and

65 Ayer, A.J. (1974)

66 Deutscher, M. (1968)

67 Popper, K.R. (1959) p95. This is also, incidentally, Lakatos's view on the role of 'basic statements' in his MSRP and is, in part, the reason why he calls for a metaphysical inductive principle linking 'acceptances' and 'rejection' in methodology with verisimilitude in epistemology. Because his defence of the conventionalist basis of MSRP contrasts sharply with Ayer's and Deutscher's criticisms it is worth noting his argument here: 'No factual proposition can ever be proved from an experiment. Propositions can only be derived from other

so, Ayer concludes, there is no reason at all why basic statements should have to refer to observable events. 'One might have equally strong motives for accepting basic statements which did not refer to anything observable.'⁶⁸ Basic statements may be accepted for all kinds of motives or causes; in all cases they are mere conventions and no basic statement has any more justification for being accepted as true or basic than any other. Nor, therefore, have we any good reason for rejecting a universal theory just because it conflicts with an accepted basic statement.

Deutscher agrees with this and argues that since basic statements are held to be impossible of verification by experience Popper's demarcation criterion breaks down. 'General statements would be equally unverifiable and unfalsifiable, if no basic statement could be verified.'⁶⁹ And apart from the fact that Popper's conventionalist account of basic statements is a form of scepticism it cannot be maintained in any case, he says, without contradicting the view that experience cannot justify statements. Why, Deutscher asks, should any individual think he agrees with others over the basic statements he accepts? (We remember that, for Popper, basic statements are just those statements we can agree on with others.) Deutscher continues:

'If he has no reason to think this, then Popper's position collapses. But if he can have some basis for thinking that he agrees with others, then his basis for his belief in basic statements need not reduce to the fact (conjecture?) of his agreement with others. If he can have a basis for the belief that he agrees with others, it can only derive from his own observations. But if this can be an adequate basis

propositions, they cannot be derived from facts: one cannot prove statements from experience - 'no more than by thumping the table.' This is one of the basic points of elementary logic, but one which is understood by relatively few people even today.' Lakatos, I. (1972) p99

⁶⁸ Ayer, A.J. (1974) p687

⁶⁹ Deutscher, M. (1968) p280

'for such a belief, then why can it not provide at least part of his basis for acceptance of some basic statements? Indeed, belief that one agrees with others logically involves acceptance of some basic statements.' 70

Popper, I suppose, might reply to this that a scientist does indeed have no justification for believing that he agrees with others. Popper might say it was merely a conjecture by the scientist that he agreed with others on the basic statements he accepts. This reply, however, would be unsatisfactory, I suggest, because it does not provide any good reason or rationale for the acceptance of one particular basic statement and not others. Unless we have some reason for believing that we agree with others whatever basic statement we do accept will be arbitrary. And if we do have reasons for believing that we agree with others then, as Deutscher argues, those can only derive from observations and so we do not need Popper's conventionalist account. Statements in science will, in short, be justified by experience.

The flaw in Popper's argument, says Ayer, is 'the assumption that if what purports to be the record of an observation 'transcends' the experience on which it is based, we are left with no reason for accepting it.'⁷¹ The proper inference, he replies, is not that we have no grounds for accepting the record of observation or statement but that we have inconclusive grounds. The report may be false and the perceiver may have made a mistake in labelling his experience or he may have been the victim of a hallucination. 'None of this,' he concludes, 'prevents it from being true that my having this 'observational experience' supplies me not only with a motive but also with a ground for accepting the interpretation which I put upon it.'⁷² There is thus no good reason, he

70 Deutscher, M. (1968) p283

71 Ayer, A.J. (1974) p688

72 " " " p689

says, why we should not regard our observations as directly justifying our basic statements.

Deutscher comes to the same conclusion as Ayer after locating the source of Popper's 'difficulty'. Popper, he says, identifies a truistic thesis with an absurd one and, as it were, having thrown the baby out with the bath water is left with an even deeper absurdity. The truistic thesis is that statements can be justified by experience, the absurd one that scientific statements speak only of our experiences. So preoccupied, says Deutscher, has Popper been with combatting the latter sense-datum account of knowledge that he mistakenly thinks that a man who says we get scientific knowledge by observation is obliged to hold the further view that scientific knowledge (statements) refer only to experiences. He adds:

'Popper repeatedly says that those who maintain that perception can justify belief also think that all apparently objective statements refer mainly to experiences. While it must be admitted that many people do clutch these views closely together, I see no logical reason why they should not be separated. In fact, their separation would form a key part of any programme to reconstruct the thesis that perception can justify belief.' 73

As long as Popper mistakenly believes that to hold the truistic thesis is to espouse the absurd one he will be unable to give a satisfactory account of the role of observation in science, his demarcation criterion breaks down and he has failed to demonstrate the rationality of science.

What these critics have shown, I believe, is that Popper has indeed failed to give an epistemological role to observation in science. He has failed to explain why we should accept one basic statement rather than another and why we should decide to allow such an accepted basic statement to falsify a theory-under-test. We saw in the first part of this chapter that his demarcation criterion was based on an argument to

73 Deutscher, M. (1968) p287

the effect that we can eliminate false theories by observation and experiment. The criticisms reviewed in this section show that we have no good reason for believing this because we have no reason for holding that those basic statements we accept enable us to locate genuine error in our conjectures. We have no reason for believing that the basic statements we accept are more likely to be true than any others, given that we cannot justify them in any way by our observations.

Next I want to examine a rather more general criticism of Popper's philosophy than that developed by either Ayer or Deutscher. This position, argued at some length in a series of recent papers⁷⁴ by Maxwell will be seen, on the one hand, to support the criticism of Popper examined in this section but, on the other, to hold that the solution proffered by Ayer and Deutscher - giving a justificatory role to perception - is unsatisfactory. For it is highly problematic, Maxwell argues, how we could ever have good reasons for supposing a theory to be falsified. Even if we did have good reasons for accepting basic statements, even if observations could justify statements that would not give us sufficient ground, he argues, for falsifying theories.

From Standard to Aim-Oriented Empiricism

One way of putting Ayer's and Deutscher's point would be to say that Popper provides no rationale for the acceptance and rejection of basic statements, no good reason for accepting one basic statement rather than another. The central argument of Maxwell's penetrating and fundamental critique of Popper's views is that he fails to provide a rationale for any of his methodological rules; no good reason is given for believing that implementing Popper's rules gives us a better chance

74 Maxwell, N. (1972); (1974a); (1974b)

of achieving his aims than any other set of rules. Therefore, says Maxwell, Popper fails to solve his demarcation problem in a satisfactory manner and he further fails to demonstrate that science is a rational enterprise. We shall see that Maxwell does think, however, that a rationale can be given which does not do too much damage to the spirit of Popper's philosophy. If we are prepared to make substantial changes to Popper's theory of science then we can indeed, he says, show the rationality of scientific enquiry.

Maxwell begins his critique with an analysis of the solutions Popper advanced for his two fundamental problems, the problem of demarcation and the problem of induction. Put crudely, he says, Popper's solution of the demarcation problem is this: 'The distinctive and especially valuable feature of scientific theories is that they are experimentally falsifiable. Falsifiable theories are especially to be prized just because we can discover that they are false; in this field we can detect error, learn from our mistakes and so hopefully make progress.'⁷⁵ Having solved his demarcation problem in this way Popper then maintains, says Maxwell, that this solution makes it unimportant that the traditional problem of induction is insoluble. It would only matter if the feature of scientific theories which we especially prized was their provenness, verifiability or certainty. Once we replace the inductivist's demarcation criterion by Popper's we can forget about induction and prize falsifiability because it enables us, at least, to locate error.

It is clear, Maxwell concludes from this analysis, that for Popper a satisfactory solution of the demarcation problem must explain why we so prize theories which are scientific in the proposed manner.

75 Maxwell, N. (1972) p138

Again, we prize falsifiability because it helps us to detect error. A criterion which did not explain why we place such a high value upon theories to which it applies would, he says, be unsatisfactory. Now the important point to note is this. The reason why falsifiability is a satisfactory solution to the demarcation problem, the reason why it is so valued is that it enables us to achieve an aim for science which we have adopted in advance, the aim being that we wish, at the very least, to detect error. Not every aim for science that we might adopt would enable us to explain why we place such a high value on scientific theories; not every aim, therefore, would allow us to give a satisfactory solution to the demarcation problem; and so not every aim would allow us to dismiss induction as irrelevant to science.

Indeed, Maxwell argues that because of his choice of aim for science in The Logic of Scientific Discovery Popper failed to solve all of these problems. In that work Popper took the view that the aim of science was simply to put forward and reject theories in accordance with his methodological rules: 'Just as chess might be defined by the rules proper to it, so empirical science may be defined by means of its methodological rules.'⁷⁶ He steered clear of a more ambitious aim such as the elimination of error or achieving maximum verisimilitude, as pointed out in Chapter Three (p139. Also, see note 28, Chapter Three). Having adopted such a humble aim Popper obviously had no difficulty in providing reasons or, a rationale for his methodological rules. We employ those rules because that is just what science is, by definition, about. However, says Maxwell:

'This line of approach cannot provide an adequate solution to the problem of demarcation. For, as we have seen, in order to solve the problem of demarcation

76 Popper, K.R. (1959) p54

'it is not enough simply to specify necessary and sufficient conditions for a type of enquiry to be scientific: in addition one needs to show why we are justified in especially prizing the theories of a mode of enquiry that is scientific in the required sense. But clearly, the mere fact that a mode of enquiry proceeds in accordance with Popper's acceptance and rejection rules provides no reason whatsoever for especially prizing its theories.' 77

Once Popper accepted a more ambitious aim, however, an aim such as the achievement of maximum verisimilitude he was able to explain why he placed such a high value on falsifiability. But, at once, this gives rise to a new problem. What reason has Popper for holding that the methodological rules he recommends gives us the best hope of achieving the aims he gives to science? Only if he can provide reasons for holding this, only if he gives a rationale for his methodological rules can he claim to have solved his demarcation problem. We need to know that by following his methodological rules we have a better chance of rejecting only false theories than we have by following some other set of rules. But since, Maxwell rightly points out, Popper provides no rationale whatsoever 'he has failed to give an adequate solution to the problem of demarcation, and to that version of the problem of induction he would wish to claim he has solved, namely: What criteria ought to govern our selection of theories if our concern is to realize the fundamental aim of scientific enquiry? In addition he has failed to show that scientific enquiry can be viewed as a rational enterprise.'⁷⁸

Maxwell, I have already said, supports the objection, already discussed, that Popper gives no reason for accepting one basic statement rather than another. There is, he suggests, no reason for regarding a high-level theory T which conflicts with an accepted basic statement as

77 Maxwell, N. (1972) p140
78 " " " "

false; conversely, there is no reason for looking with any particular favour upon T if it proves compatible with the accepted basic statement.

Ayer and Deutscher both argued that we could, in effect, provide a rationale for accepting basic statements and for using them to test high-level theories, if we were to give a justificatory role to perception. Maxwell disagrees with this because he denies that accepted basic statements alone are sufficient to overthrow theories. He argues:

'For in accepting experimental results as refuting a theory one is committed to the possibility of explaining these results by some future theory. That is, one is committed to holding, at least as a conjecture, that the refuting experimental results constitute lawful occurrences. For if one denies this conjecture one thereby accepts experimental results which no future physical theory can conceivably explain - since it is only lawful occurrences that can be explained physically. ... to assert that a set of experimental results constitute lawful occurrences is in effect to assert a somewhat vague universal hypothesis. Thus singular hypotheses, however well-corroborated, do not suffice to refute a theory; it is only singular hypotheses, backed up by a universal hypothesis, to the effect that the experimental results in question constitute lawful occurrences, that can refute a theory - or rather a conglomeration of theories.' 79

To reinforce this argument Maxwell asks us to consider the law, 'All bits of copper expand when heated' and to suppose that 'This piece of copper, on the surface of the earth for one year, fails to expand when heated' is highly corroborated within the limits of space and time specified. Outside these limits, we can suppose this piece of copper does expand when heated - as do all other pieces of copper.

Now, in these circumstances, we could decide, he suggests, to retain the law 'All bits of copper expand when heated' and simply say that in the above example something was going on which we do not, as physicists, understand and cannot take into account. That we could

legitimately do this shows, he says, that a well-corroborated basic statement alone does not suffice to falsify a theory. Or we could conjecture that the anomalous bit of copper was, during the relevant time, in an unusual state and thus decide to reject the law. The crucial point in this case however, he argues, is that we would here be rejecting the law not as a consequence of accepting the basic statement - 'This piece of copper, on the surface of the earth for one year, fails to expand when heated' - but 'as a consequence of accepting the somewhat vague universal hypothesis 'All bits of copper, when in some specific unknown state, fail to expand when heated.' It is only if we accept tentatively some such hypothesis as this that the behaviour of the anomalous bit of copper can be seen as constituting lawful occurrences, which we may hope to explain by means of some future theory.'⁸⁰

Ayer's and Deutscher's solution to the problem of basic statement acceptance is thus inadequate for it does not provide any basis for the refutation of theories. In any case it is unsatisfactory, Maxwell implies, because it is not possible to justify theories by experience.

Finally, Maxwell argues that even if we did have a rationale for accepting basic statements and even if we did have a further rationale for allowing them to falsify high-level theories Popper's methodology would be inadequate. For he provides no reason, says Maxwell, for preferring one theory T to an infinity of easily constructed rivals which are just as highly falsifiable and as well corroborated as T, yet which differ drastically in their empirical consequences both from each other and from T itself. Maxwell then provides a procedure for generating rivals to T which are just as empirically successful; the procedure runs like this. First, select some type of experiment which

80 Maxwell, N. (1972) p144

has been performed endless times in the past. Let us next suppose that a theory T (perhaps, together with auxiliary theories) successfully predicts that if the experiment is performed the outcome is P. We then add to the specifications of the experiment 'certain entirely bizarre, ludicrous details'⁸¹ which we can feel pretty sure are irrelevant to the outcome of the experiment. Thus we might stipulate, he says, that the apparatus be painted red or that the sounds 'Abracadabra' be made near the apparatus during the experiment. He continues:

'In this way we specify in purely universal terms a type of experimental set-up (essentially, a set of initial conditions) which we can be reasonably sure has never obtained anywhere in the universe. We can now construct empirically successful rivals to T by means of the following rule: 'As long as E does not occur everything occurs in accordance with T; if E occurs then the outcome is Q.' We are here free to choose Q as we please. Q may differ only slightly from P (the prediction of T) or Q might be some quite drastic assertion such as that all phenomena in the universe occur in accordance with such and such a set of laws which are very different from our present physical theories (in which case our rival theory to T would in effect assert that if E occurs the laws of nature change.) By dreaming up different experimental set-ups E and different outcomes Q we can easily invent an unlimited number of aberrant versions of T - as we may call these theories - each of which will differ in predictive content from T, and yet will be just as empirically successful as T.'⁸²

Popper is unable to provide a rationale for his methodological rules, says Maxwell, because he makes a crucial mistake when he takes as the fundamental aim of science, the pursuit of maximum verisimilitude. If he were, instead, to adopt, as the basic aim of science, the search for explanatory truth it would open up the way, says Maxwell, to a rationale and thus to a genuine solution of the demarcation problem. Now Popper does agree, we have seen, that science does aim to arrive at true explanatory theories but he believes he can, and has repeatedly tried

81 Maxwell, N. (1974) p128

82 " " " "

to reduce this aim to a more fundamental one, namely to the pursuit of true theories with high empirical content. These attempts are doomed to failure, says Maxwell, 'for the simple reason that high empirical content cannot be equated with high explanatory power.'⁸³ It is always possible to increase the empirical content of a theory by adding on an independently testable postulate but this will decrease the simplicity or explanatory power of the theory. Popper is mistaken in equating explanatoriness with content.

Once we adopt as the fundamental aim of science the search for explanatory theories - theories which are simple, unifying, coherent, intelligible - it will be perfectly rational, Maxwell argues, to plan our strategy on the assumption that our search will meet with success. Now we will only meet with success, given that we have adopted this aim, if it is indeed the case that the world (or phenomena under study) are simple. If the world (or phenomena) are incredibly complex then there is no way we can succeed, for as we make our theories more and more explanatory, more and more simple etc, the further they will recede from the truth. Hence, says Maxwell, it will be perfectly rational, given our aim, to assume a priori that what he calls the metaphysical thesis of the structural simplicity of the world (phenomena) is true. And it will be perfectly rational to evaluate our observations and theories on that basis.

We have, he admits, no reason at all for thinking that such a thesis is true. It may well be false and so we may plunge deeper and deeper into error when we reject theories and/or observations which conflict with our assumption. In that case we will fail to make empirical progress. If the world is in fact extremely complex and we

83 Maxwell, N. (1974) p148

decide, on the assumption that it is simple, to reject a priori all complex or aberrant theories we remove the possibility of discovering, through empirical testing, that we may have hit upon the correct theory. If, says Maxwell, we have failed after a considerable period to make any empirical progress at all that, in itself, will suffice to call the assumption into question.

But how can it be rational to make such a metaphysical assumption, one may ask, if it is admitted that we have no reason whatsoever for holding that it is true? Maxwell replies that it is perfectly rational to search for something which may, admittedly, not exist quite simply because it is extremely important for our survival that we find it. He uses the analogy of a man dying of thirst in the desert and argues that his search for an oasis is supremely rational even though the odds against success are huge. The man has no other choice. Says Maxwell:

'I suggest that the situation is somewhat analogous to this as far as the search for intelligibility in science is concerned. From an intellectual standpoint (and ultimately also, I would wish to argue, from a practical, technological standpoint) our need for there to be intelligibility inherent in the universe is so enormous, so utterly irreplaceable, that we have no alternative but to take for granted that intelligibility does exist even though we have not the slightest reason for supposing this assumption to be true. To cast doubt on the existence of intelligibility is idle, not because we have such wholly convincing reasons for holding it to be true (quite the contrary!), but rather because if intelligibility does not exist at all, then our case is hopeless, and both science and, ultimately, life become impossible.' 84

Assuming that the world (or phenomena under study) are simple would only be irrational if we had, he says, any good reason for thinking that the opposite is more likely to be true.

What Maxwell denies is that the rationality of seeking the aim of explanatory truth requires that it is more rational to hold that the

84 Maxwell, N. (1974) pl40

metaphysical thesis of structural simplicity is true rather than false. All that is required, he says, is that we do not have any good reason for holding that it is more likely to be false than true; in those previous circumstances it would, indeed, be irrational to make such an assumption. This position is to be contrasted with the effort by Lakatos to provide Popper's methodology with a rationale via a metaphysical inductive principle linking corroboration with verisimilitude. Without some such principle, Lakatos argued (Chapter Four see p172/3), Popper's methodology is a mere game which has nothing to do with epistemology. 'By refusing to accept a 'thin' metaphysical principle of induction,' says Lakatos⁸⁵, 'Popper fails to separate rationalism from irrationalism, weak light from total darkness.' But the trouble with this traditional attempt to solve the problem of induction, or the problem of providing a rationale for methodological rules, Maxwell correctly argues, is that some reason must be provided for holding that the principle is true. Otherwise we have no reason for making it. And since no good reason can be provided for holding that such a principle is true rather than false Lakatos's traditional 'solution' collapses.

Once we adopt the aim of seeking explanatory truth and assume the metaphysical thesis that the world is structurally simple in some sense or other - to be discussed in a moment - it is then reasonable to suppose, Maxwell argues, that those regularities we observe in the world do in fact arise from or reflect the structural simplicity of the universe.⁸⁶ It is reasonable, therefore, to accept basic statements which we fallibly believe to accurately describe real regularities on the basis of both our metaphysical assumption and our observations. And if we

85 Lakatos, I. (1974) p261

86 For brevity I refer to 'the world' but that can be read to refer to any set of phenomena we are investigating.

have developed a high-level theory to explain those observed regularities and it either conflicts with accepted basic statements, which we believe to describe those regularities, or has to be patched up in a way which leads to considerable theoretical complexity it will be rational, given our assumption, to decide that it is on the wrong lines. And finally, our metaphysical assumption gives us a rationale for dismissing a priori the infinite number of possible aberrant rivals which are as empirically adequate as the simple theory we decide to accept. Maxwell, indeed, argues that only by making such a metaphysical assumption is it possible to prevent science coming to an immediate halt. For there is a potentially endless supply of aberrant, empirically adequate theories which we must reject in favour of a simple theory, independent of empirical considerations. If we were to start experimentally testing such aberrant theories, instead of dismissing them by licence of our a priori assumption, then we would be stopped in our tracks; as soon as one was refuted another would at once appear to take its place.

It must be admitted that Maxwell's provision of a rationale for Popper's theory of science represents a considerable modification. For, as we have seen, Popper insists that it is possible to evaluate theories in terms of empirical evidence alone. That theory is to be preferred which best stands up to test. Like other 'standard empiricists', as Maxwell calls them, Popper denies that scientists do or ought to make permanent metaphysical assumptions about the world or any other phenomena the scientist may study. He has, admittedly, stressed the influence of metaphysics throughout the ages in generating ideas in science. But these influences are felt to belong only to the 'context of discovery'; metaphysics is denied by Popper to have influence in the 'context of justification' or, more appropriately in Popper's case, in the 'context of criticism.' Lakatos too, incidentally, is a standard empiricist

despite giving a temporary metaphysical role to the hard core of his research programmes. He allows us to treat such a hard core as provisionally irrefutable but insists that in the end evaluation of research programmes is a purely empirical affair. Lakatos is, in this respect at least, fully in accord with Popper. Maxwell, in contrast, argues that science must be recognised to be thoroughly imbued with metaphysics if we are to have any basis for the rational assessment of theories.

The fundamental aim of science, says Maxwell, is the discovery of more and more about an underlying harmony, unity, simplicity or intelligibility conjectured to exist in the world, or to be inherent in the phenomena we are investigating. He writes:

'At the very least, there is the metaphysical conjecture that the phenomena are such that it is in principle possible to develop theories of increasing simplicity, unity, coherence or intelligibility which also meet with increasing empirical success. The fundamental aim of science is, in other words, to develop successive theories which progressively articulate and make precise more and more of a metaphysical conjecture M (which asserts, roughly that the phenomena are intelligible) in such a way that these theories meet with more and more empirical success.' 87

One way of looking at Maxwell's aim is to see it as a revision of Popper's demarcation criterion. Knowledge, for Popper, was defined as those theories which have not yet been falsified. Knowledge, for Maxwell, is constituted by those theories which are compatible with a metaphysical conjecture M (explained above) and which have not yet been falsified. Accordingly Maxwell does not prize or consider to be 'knowledge' any theory, no matter how empirically successful, which is incompatible with the conjectured metaphysical 'blueprint' for the science. The methodology which flows from this conception of knowledge,

87 Maxwell, N. (1974) p141

therefore, is to favour only those theories which are (a) compatible with M and (b) empirically successful. The rationale for these rules is that it is just empirically successful theories compatible with M that we want. Maxwell can therefore claim, correctly in my view, that he has provided a satisfactory solution to the demarcation problem and thereby demonstrated the rationality of scientific enquiry.

A further difference with Popper arises, Maxwell claims, over the question whether there is a 'logic of discovery'. Like the majority of contemporary philosophers of science Popper denies that there is any logic in proposing a hypothesis. Thus he argues:

'The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it ... The question how it happens that a new idea occurs to a man ... may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge.' 88

In opposition to this Maxwell asserts that once we accept that scientific theories must be evaluated, at least in part, in terms of their compatibility with a conjectured metaphysics - that is, once we accept what he calls 'aim-oriented empiricism' - then, 'rational discovery becomes the very heart of the rationality of science.'⁸⁹ By a rational method of discovery, however, Maxwell does not mean that he has a method for conceiving or inventing a theory. He means that it is a method for 'choosing rationally between conflicting, more or less vague metaphysical ideas for future scientific theories, future lines of development.'⁹⁰ The aim-oriented empiricist, who assumes that the world (phenomena under study) are intelligible in a more or less precise way, is thus able to eliminate all embryonic theories which do not

88 Popper, K.R. (1959) p31

89 Maxwell, N. (1974) p147

90 " " " p127

conform to the presumed metaphysical blueprint; and he rejects such theories a priori. The standard empiricist, Popper included, is unable, says Maxwell, to do this because he denies that we may make such metaphysical presuppositions.

To some extent, therefore, the difference between Popper and Maxwell is not as great as it would seem if we baldly stated that one asserts and the other denies that there is a logic of discovery. Maxwell asserts this not in the sense which Popper opposes, namely the claim that there is a rational method for generating ideas which are true. But he does claim, and I believe legitimately, that aim-oriented empiricism provides a rational method for the elimination of an infinitude of theories before any experimental testing. They can be rationally dismissed a priori in so far as they do not help articulate the metaphysical blueprint for the science which the aim-oriented empiricist just assumes at the outset.⁹¹ An important qualification which Maxwell makes to his claim to possess a rational method of discovery is the recognition of its fallibility; the a priori elimination of theories may prove disastrous because the metaphysical thesis that the world is intelligible may be quite false, either because the world is in fact very complex or it is intelligible but not in the way the conjectured blueprint specifies.

91 Medawar sees it as a 'defect of the hypothetico-deductive scheme that it sets no upper limit to the number of hypotheses we might propound to account for our observations.' Medawar, P.B. (1969) p53. Because the hypotheses that do enter our minds will, as a rule, be plausible and not, as in theory they could be, idiotic he concludes that there is a critical process, a psychological 'internal censorship' which edits out implausible ideas. Criticism in science is therefore, he says (contra Popper), not 'wholly logical'. Maxwell's espousal of a public, a priori blueprint which edits out hypotheses before testing makes Medawar's proposal unnecessary.

With hindsight we can see, Maxwell points out, that in order to provide a rationale for Popper's rules it was necessary to develop a theory of science with something approaching a logic of discovery. The procedure for generating aberrant theories, at least as or more empirically successful than existing theories, amounted to, he says, just such a method of discovery. 'In order to outlaw such theories, one has to outlaw certain procedures for the construction of new empirically successful theories. In other words, one has to place a priori (that is, nonempirical) restrictions on the field of new hypotheses worthy of consideration. But to delimit in this a priori fashion the field of new hypotheses is in effect to provide a more or less useful rational method of discovery.'⁹²

Maxwell's theory of science, therefore, differs from Popper's in two quite fundamental ways. Scientists must make some metaphysical conjecture about the way the world, nature, society, the mind or whatever is being studied is structured if they are, in Maxwell's view, to have any rational basis for the assessment of theories. If they do not make some such a priori assumption and instead adopt, like Popper, the aim of discovering more and more about the object of study, whatever that object may turn out to be like, then they will be faced with an infinitude of equally empirically successful theories and have no good reason for preferring one to all the rest. But if they do make such an assumption then, if they are able to develop an empirically successful theory which articulates the metaphysical blueprint, it will be perfectly rational to prefer that intelligible, empirically adequate theory to other aberrant, empirically adequate rivals. The second basic difference between Maxwell's and Popper's views, as we have seen, is that the first

92 Maxwell, N. (1974) p148

can legitimately claim, in a qualified sense, to have a rational method of discovery.

Maxwell's aim-oriented empiricism, however, is fully Popperian in two other, equally fundamental senses. It is both fallibilist and objectivist. It is fallibilist because, in short, we can never know we have not made a disastrous series of mistakes. Not only may we make, what is very likely, the wrong metaphysical guess but even if we should manage, having made our assumption, to develop an empirically successful programme of research which articulates that assumption we may still be mistaken. The metaphysical assumption may be an error and the empirical success thus only apparent. Maxwell's theory is thus fallibilist both in the a priori assessment of acceptable theories and in their experimental appraisal. It is objectivist because, says Maxwell, 'the demand for objectivity is to a considerable extent to be understood in terms of the more fundamental demand for intelligibility.'⁹³ He argues, at length, that the traditional criticism of the search for simplicity, intelligibility, etc, in science, the objection that it is irredeemably subjective is mistaken.⁹⁴

The methodological implications of aim-oriented empiricism are that, first and foremost, we must attempt to develop a metaphysical blueprint which will spell out the sense in which the phenomena we are studying are 'intelligible'. Maxwell lists blueprints which have powered physical research down the ages and which include, for example, Boscovich's point-atomistic view, the corpuscularian view, Galileo's view that 'the book of nature is written in the language of mathematics' and the Einsteinian view that the world is made up of one unified field.

93 Maxwell, N. (1974) p271
94 " " " p271ff

Other sciences must try to spell out some notion of how whatever it is they are studying is constituted. 'Without some kind of agreed aim or blueprint for a science,' Maxwell concludes, 'one hardly has a science at all.'⁹⁵ Only if we have assumed that whatever we are interested in has a specified structural simplicity will we have any basis for preferring one theory to another. Having taken the plunge, as it were, and made our metaphysical assumption we then proceed to devise testable theories, compatible with that assumption and which we hope will prove empirically successful. In this way we try to transform our metaphysics into empirical science. Should we fail to make any empirical progress then we may take that to indicate that we are working on a mistaken blueprint which we ought to replace. But we have no choice, in these circumstances, but to assume that whatever the new metaphysical blueprint will be it must also be intelligible in some sense or other. Should we succeed in our efforts to transform the blueprint into an empirically successful programme however, that is not, Maxwell insists, any basis for complacency. It could still be the case that another more intelligible blueprint - a metaphysics which better conforms to our ever improving standards of intelligibility - would lead to even greater success.

The implications of all this for psychology are, I fear, likely to meet stiff opposition. Metaphysics has been, for psychologists, a synonym for 'unscientific' and has, consequently, been suppressed with speed and with scorn.⁹⁶ Or more precisely, explicit debate about the role of metaphysics in any putative scientific psychology has been

95 Maxwell, N. (1974) p294

96 The behaviourist psychologist, in particular, is often very hostile. Thus, for example, Eysenck writes: 'Philosophy to the behaviourist is idle speculation about matters either unknowable or uninteresting or both; in this he would find support in some of the more able modern philosophers themselves who have relegated metaphysics to an academic backwater, or legislated it out of existence altogether.' Eysenck, H.J. (1972) p289

avoided. But such intolerance has not been based on any extensive investigation of the epistemological difficulties facing philosophies of science or on any closely argued thesis of the kind advanced by Maxwell. And it is at least possible that continuing the debate he has developed may throw light upon the conflicting arguments over the status of psychology which have already been examined in this thesis. We might even find, as I suspect, that some of the most intolerant opponents of metaphysics and philosophy will have smuggled in embarrassing assumptions, albeit unwittingly, into their theorizing. In which case only a discussion like that we have had in the last two chapters will enable us to disentangle the arguments involved.

Chapter Five

The Approaches Assessed

Behaviourism and Mentalism

Introduction

I now propose to evaluate these two movements together for two reasons. First because they are, as pointed out in Chapter One, each defined in opposition to the other with behaviourism asserting and mentalism denying that each mental predicate employed in a psychological explanation must be connected to at least one description of behaviour. The second reason is that I have decided to choose Skinner and Chomsky as the representatives of these approaches within contemporary psychology and to examine the clash between their views. Most of what I have to say about Skinner will be recapitulation of Chomsky and my primary concern, therefore, will be mentalism.¹ To deflect possible objections that there is much more - or less - to mentalism than is to be found in

1 Mentalism has at least three different usages in the literature and since this may easily cause confusion I had better make my own position clear. Mentalism may refer to: (1) again, the denial that each mental predicate in a psychological theory must be logically connected to some description of behaviour; (2) the view that introspective reports and/or intuitive judgements are admissible as evidence in linguistic and psychological theories; (3) the dualist/interactionist view that mind is both a distinct ontological substance and must be awarded causal efficiency in psychological explanations. Some mentalists in the sense of (1), accept (2) - e.g. Chomsky - while others do not - e.g. Sampson (1975). To make matters even more confusing Sampson calls his position 'behaviourist'. Throughout this thesis I use mentalism in the sense of (1), which was the definition adopted by Fodor (1968). Sampson is, therefore, a mentalist because he subscribes to (1) above. Finally, while it is possible as a matter of logic for a mentalist, in the sense of (1), to also subscribe to (3) I have suggested that, in fact, few do so. But see the discussion on p242 footnote 2.

Chomsky's writings on mind and language let me add at once that not all mentalists are Chomskians and that should Chomsky's scientific theories fail to find empirical support we need not conclude that mentalism would be thereby discredited. Nevertheless, since Chomsky is arguably the most important exponent of mentalism and his theories, if true, would constitute a major discovery about the human mind I shall focus my discussion on his scientific ideas. But first let us examine the psychology of language advocated by Skinner for it was against this behaviourist orthodoxy that Chomsky rebelled.

Chomsky's Critique of Skinner

In his now classic review of Skinner's Verbal Behaviour² Chomsky³ launched an attack on the general behaviourist strategy of trying to explain complex human behaviour by applying the principles and methods discovered and developed in laboratory experimental investigations of supposedly simpler behaviours of organisms more primitive than man. This had been Skinner's approach to what he called the problem of providing a 'functional analysis' of verbal behaviour. Man's ability to speak and to understand language was, for Skinner, an admittedly complex set of behaviours but one which was, in principle, no different from the behaviour of the rat in a maze-running experiment or the pigeon in the Skinner-box. His experimental studies had demonstrated, to his satisfaction, that these 'simple' animal behaviours resulted from the operation of physical stimuli which could be objectively measured and manipulated. Human verbal behaviour, he claimed in his book, was similarly well understood and so he set himself the task of showing that it could be both predicted and controlled by observing and manipulating

2 Skinner, B.F. (1957)

3 Chomsky, N. (1959), (1964)

the speaker's physical environment. In other words the principles and methods employed in the standard operant conditioning paradigm could be extended to the study of language without significant change.

Chomsky began his review by raising a difficulty that faces any attempt to extend the concepts of the laboratory conditioning experiments to real-life situations. Concepts such as 'stimulus' or 'response' have, Chomsky conceded, a fairly clear specification or reference in the laboratory where, for example, a stimulus might be the onset of a red light and the response say, a bar-press. But it is not clear in real-life situations, Chomsky argued, how such concepts are to be used. Is a stimulus, he asked, just any physical object to which an organism is capable of reacting or is it to refer to just those objects to which it in fact reacts; similarly, is any piece of behaviour to be called a response, he wondered, or is it just that behaviour which is lawfully connected to stimuli.⁴ If we adopt the former, broad definitions of stimulus and response we will have to admit that almost no behaviour is lawful, Chomsky argued. On the other hand, he said, if we adopt the latter, narrow definitions of stimulus and response - the procedure Skinner employed in his Behaviour of Organisms⁵, which dealt with the animal conditioning experiments - then behaviour is lawful by definition. Unfortunately, Chomsky continued, applying the narrow definitions to real-life problems will lead us to conclude that most of what the human does, including what he says, is not behaviour at all.⁶

Skinner fails to provide a satisfactory solution to the above difficulty, Chomsky pointed out, because he does not consistently follow one or other course. Instead he continually switches from the narrow

4 Chomsky, N. (1964) p551

5 Skinner, B.F. (1938)

6 Chomsky, N. (1964) p551

usage to the broad, with the former allowing him to claim scientific rigour for his system while the latter, amounting to but 'a metaphoric extension of the technical vocabulary of the laboratory'⁷, is alleged to provide evidence of its scope. The reality is, Chomsky maintained, that with a literal reading (narrow definitions) the book covers almost no aspect of verbal behaviour while with an analogic reading (broad definitions) it is not scientific at all. To support this conclusion Chomsky then submitted a large number of Skinner's 'explanations' of verbal behaviour to a merciless analysis which showed how little resemblance there was between Skinner's experimental terminology and the extension of that terminology to the explanation of language behaviour.

If a man were to stand in front of a picture for a couple of minutes and then utter 'Dutch' that would be for Skinner, Chomsky⁸ correctly argued, a response 'under the control of extremely subtle properties'⁹ of the painting. Chomsky continued:

'Suppose instead of saying 'Dutch' we had said Clashes with the wallpaper, I thought you liked abstract work, Never saw it before, Tilted, Hanging too low, Beautiful, Hideous, Remember our camping trip last summer?, or whatever else might come into our minds when looking at a picture (in Skinnerian translation, whatever other responses exist in sufficient strength.) Skinner could only say that each of these responses is under the control of some other stimulus property of the physical object.'¹⁰

Skinnerian functional analysis, therefore, explains a wide range of responses, Chomsky argued, by identifying controlling stimuli, properties of the object which determine the response. And since it is possible to attribute an infinity of properties to any object we can then account, with this functional analysis, for any response a speaker may make to any object. The trouble, however, with this simple,

7 Chomsky, N. (1964) p552

8 " " " "

9 Skinner, B.F. (1957) p108

10 Chomsky, N. (1964) p552

explanatory schema is, Chomsky asserted, that it is not an explanation at all; nor does it resemble the notion of stimulus control as that is used in Skinner-box experiments. He argued:

' ... the word stimulus has lost all objectivity in this usage. Stimuli are no longer part of the outside physical world; they are driven back into the organism. We identify the stimulus when we hear the response. It is clear from such examples, which abound, that the talk of stimulus control simply disguises a complete retreat to mentalistic psychology. We cannot predict verbal behaviour in terms of the stimuli in the speaker's environment, since we do not know what the current stimuli are until he responds. Furthermore, since we cannot control the property of a physical object to which an individual will respond, except in highly artificial cases, Skinner's claim that his system, as opposed to the traditional one, permits the practical control of verbal behaviour is quite false.' 11

The analysis of behaviour in a Skinner-box set-up is, in contrast, quite different. The stimulus is, at least, specified independently of the animal's response and can both be measured and manipulated. (Whether or not the Skinnerian explanation of such behaviour is the correct one, is, of course, another matter. It seems to me that it is necessary, if we wish to understand the actions of either rats or men, to develop theories which characterise, among other things, how the stimulus appears to the receiver. But that is getting ahead of the argument.)

Skinner's extension of the notion of response to the explanation of verbal behaviour is equally unsatisfactory in Chomsky's eyes. The unit of verbal behaviour - the verbal operant - is defined as a class of responses of identifiable form functionally related to one or more controlling variables. But this definition is useless, Chomsky argued, because 'no method is suggested for determining in a particular instance what are the controlling variables, how many such units have occurred or where their boundaries are in the total response.'¹² Skinner provides

11 Chomsky, N. (1964) p553

12 " " " " p554

no indication of how much variation is permissible between two sequences of behaviour if they are to be identified as examples of the same verbal operant. 'In short,' Chomsky concluded, 'no answers are suggested for the most elementary questions that must be asked of anyone proposing a method for the description of behaviour.'¹³

Even though he did not succeed in his aim Skinner's intention was to offer a functional analysis of verbal behaviour in terms of publicly observable and hence, in his view, purely objective stimuli and responses. Chomsky rightly objected that this concentration on input-output relations and the exclusion of any consideration of the 'contribution of the organism' to both learning and behaviour amounted, in fact, to the assumption that the function being investigated was very simple. Skinner merely assumed what ought to be a matter for empirical discovery namely, whether the contribution of the speaker is quite trivial and elementary - just as he assumed that external factors such as present stimulation and the history of reinforcement are of overwhelming importance. 'The magnitude of the failure of this attempt to account for verbal behaviour,' Chomsky concluded, 'serves as a kind of measure of factors omitted from consideration, and an indication of how little is really known about this remarkably complex phenomenon.'¹⁴

Chomsky then proceeded to argue that before a psychologist tries to develop a causal explanation of linguistic or any other behaviour he should pause to ask himself what it is he is trying to explain. A 'theory of learning' must begin with a consideration of 'what is learned' and so Chomsky, taking his cue from Lashley¹⁵, insisted that the psychologist give a detailed characterisation of behaviour. 'A

13 Chomsky, N. (1964) p554

14 " " " p549

15 Lashley, K. (1951), (1970)

consideration of the structure of the sentence and other motor sequences will show,' Lashley had argued, 'that there are, behind the overtly expressed sequences, a multiplicity of integrative processes which can only be inferred from the final results of their activity.'¹⁶ Before he studies the causes of language behaviour, therefore, the psychologist ought to employ the insights of the linguist for it is the task of the latter, Chomsky maintained, to devise for each language a grammar from which may be derived 'a statement of the integrative processes and generalised patterns imposed on the specific acts that constitute an utterance.'¹⁷ Since Skinner fails to provide even a rudimentary, preliminary characterisation of the language a speaker puts to use, his 'explanations' should be rejected, Chomsky insists, as not only vacuous but hopelessly premature.

Chomsky's attack has not silenced Skinner who has gone on to advocate the construction of a behavioural technology allegedly based on a science of behaviour which, we are told, reveals that all behaviour is largely under environmental control.¹⁸ This has, in turn, provoked a further critique by Chomsky¹⁹ which consists, in the main, of repetitions of the criticisms reviewed above and which calls both for modesty in our claims to understand why people act in the ways they do and for a more elaborate, indirect programme of research than Skinner permits. Before examining Chomsky's own positive programme I would like to add a few further criticisms of Skinner or repeat objections I have already made.

As suggested in the case-study Skinner's radical behaviourism is the representative in psychology of early logical positivist and

16 Lashley, K. (1970) p186

17 Chomsky, N. (1964) p576

18 Skinner, B.F. (1973)

19 Chomsky, N. (1973)

operationist views of science which require that all behaviour must be translated into physical language and, therefore, described in terms of movement in space. We have seen that Carnap, one of the early advocates of this position, had by 1932 accepted Popper's criticisms of this translation requirement. It is futile, Popper argued, to search for some basic or ultimate foundation for a science, such as 'movement in space', as a replacement for normal, everyday descriptions of behaviour. All descriptions make use of universal terms, he pointed out, and thus are irredeemably theoretical. It is, therefore, no less scientific to categorise behaviour in terms of the plans, concepts or schemas under which it falls. In making this point Popper was anticipating, I have suggested, the objections of mentalists like Chomsky that Skinner's methodological prescriptions condemned his enterprise to sterility and insignificance. Skinner is something of an anomaly, in my view, for he continues to defend a methodology long abandoned by its originators in response to criticism and for which he provides no convincing reasons. Ironically this Carnapian view of science, stemming as it does from the positivist search for certainty in scientific knowledge, is a truly subjectivist theory for it holds that, ultimately, all statements in science can be reduced to experience - ironic because it was subjective experience that the behaviourist revolt was supposed to have finally driven out of psychology.

Ultimately the only criticism that will silence the Skinnerians will be an empirically successful, theoretically coherent explanatory account of the mental processes which generate behaviour and it is such an account that contemporary mentalists are trying to produce. Until that far-off day arrives, if it ever does, when the mentalists have developed a comprehensive theory which is empirically supported the radical behaviourist will be at liberty to argue that all human behaviour

is, in principle, understood and to maintain that if we could only decode the reinforcement schedules in our environment we would have laid bare the determinants of action. So even though the Skinnerian analysis of everyday human behaviour is, depending on how it is interpreted, either irrelevant or vacuous, and even though it insists on methodological constraints for which it provides no defence it is likely that we will hear for some time yet that it is 'by far the most promising approach to the understanding of behaviour.'²⁰

Chomsky on Language and Mind

'Language is a mirror of mind in a deep and significant sense.'²¹ This statement of Chomsky's sums up the bold, metaphysical assumption which has inspired his scientific research programme in linguistics and psychology and which continues to motivate his distinctive doctrines on human nature and knowledge. Before his famous assault on Skinner's work drew him to the attention of psychologists at large he had attracted considerable interest two years earlier, in 1957, among psychologists of language with his book Syntactic Structures.²² He argued there, as he did against Skinner, that the linguist can perform an essential first step for the psychologist who studies language by providing an abstract, theoretical account of language which characterises 'what is learned'. Only when that initial investigation has been made ought psychologists to proceed to study the processes and mechanisms involved when 'what is learned' is put to use in the perception and production of speech.

Language is a mirror of mind, in Chomsky's view, because every language which humans naturally speak, as opposed to artificial languages

20 Boakes, R.A. and Halliday, M.S. (1973) p373

21 Chomsky, N. (1976)

22 " " (1957)

like, for example, Esperanto, possess unexpected characteristics which they have as a matter of biological necessity. He claims that when we have identified these features which all natural languages share we will have discovered features which reflect innate structures of the human mind. We will see, later, that Chomsky holds that the study of language may prove a suitable model for the investigation of other domains of competence and we will also see that he believes that such investigations will vindicate the traditional rationalist account of human knowledge and freedom against the empiricist vision which allegedly dominates contemporary thought.

Somewhat surprisingly for a theory with such far-reaching and controversial claims Chomsky's linguistic and psychological theories are not easy to pin down and formulate in a way which would satisfy all Chomskians. As the title of his first book suggests Chomsky's theory is primarily about syntax, that is, about how words are joined together to form sentences. Most of his followers would probably agree with that although there are some who, while adopting the Chomskian label to acknowledge their intellectual origins, would deny even that basic claim. One of the difficulties for any expositor of Chomsky's theory of language is that the position has been substantially changed over the years and Chomsky has abandoned positions that were characteristically his own. Fortunately, however, I am not required here to trace these alterations as my main concerns are his general strategy for doing psychology, how that strategy measures up to the criteria of scientific procedure as those were developed in the previous two chapters and with the implications he feels his scientific work has for epistemology and philosophy in general.

Once again, the theory is primarily about syntax. The sentences of a language are identified with what are called stringsets of

formatives, each sentence being viewed as a sequence of basic units or formatives, which are the stems and inflectional suffixes which, in English, are run together to form words. Each language, for Chomsky, amounts to a set of sentences; hence each language is viewed as a stringset of formatives. What the linguist must do, he continues, is to construct a grammar which will divide the set of all possible formatives in each language into two classes, the grammatical and the ungrammatical. To use two examples from Sampson's²³ excellent introduction to Chomsky's work, a satisfactory grammar of English ought to place the following two strings in separate categories. She walked to the door ought to be judged grammatical while Of the of of should be deemed ungrammatical.

The data against which a grammar ought to be tested is a point of disagreement among Chomskians. 'The empirical data that I want to explain,' says Chomsky²⁴, 'are the native speaker's intuitions.' The linguist's initial task, therefore, is - on this view - to devise a grammar which will classify all possible stringsets of formatives into two groups such that one group conforms to the speaker's intuitions about what is grammatical while the other group does not. Why Chomsky feels that it is the speaker's intuitions that must constitute the data for both the linguist and the psychologist will be examined in a moment but at this point I merely wish to point out that some linguists who otherwise adopt a Chomskian view of language, deny that intuition can play any evidential role. Linguistic and psychological theories must be tested against spoken or written utterances in their view on pain of making those theories unempirical. If Chomsky's theories must rely

23 Sampson, G. (1975) p38/9

24 Chomsky, N. quoted in Sampson, G. (1975) p60

on the speaker's private judgements then those theories are, they maintain, unscientific; but fortunately, they further conclude, what people say and what they write suffice as an evidential base for testing these theories.

Whatever the evidence he uses - utterances or intuitions - the Chomskian is not concerned merely to construct a grammar for each language which humans speak. His main aim is to develop a general theory of language, a body of principles that will divide the set of all possible stringsets or the set of all possible human languages into two classes, the natural and the unnatural. His theory of language defines a class of grammars - the natural class - and predicts that all the grammars which are actually worked out for contemporary or previously spoken languages such as English, Chinese, Latin, etc will fall under the natural class; they will share common characteristics or what Chomsky calls a 'universal grammar'. He argues that the grammars of natural languages and therefore of spoken or what we might call attested languages form a small sub-class of the class of possible grammars or, in other words, that there are strong universal constraints on human languages. We can see that the theory of language, which defines the class of natural grammars, is to be tested against the evidence provided by those grammars which have been worked out for the attested languages. Since these grammars are themselves based upon either utterances or intuitions, depending on one's viewpoint, the theory of language is also based, ultimately, on either utterances or intuitions.

The central claim of Chomsky's theory of language is, in brief, that the natural class of grammars has an essentially 'constituency structure' but which can be modified by specific kinds of transformational operation. Accordingly he claims that the attested human languages can be shown to have grammars which possess these constituency-cum-

transformational structures. Should we find that an attested language does not have this structure but possesses, instead, say, a finite-state grammar then Chomsky would admit that his theory of language has been falsified.

His critics might argue that there is little danger of that, given that the central claim of the theory is so vague. They may also point out, as in fact Searle²⁵ has pointed out, that there is very little agreement among transformational grammarians as to exactly what the constituency and transformational rules which characterise a natural language actually are. He comments:

'The effort to give a purely autonomous syntax has been going on now for over twenty years and it is not getting very far. There is no set of such rules that all or even most linguists can agree are the rules of syntax of any natural language or even of an interesting fragment of a natural language.' 25

He then goes on to argue that Chomsky's entire programme of attempting to give a formal account of the syntax of a language in terms of complex and abstract rules of the kind alluded to above is fundamentally misconceived and that a simpler and more convincing account of syntactic regularities can be given if we pay attention to both the function and the meaning of sentences. But this conflict between Chomsky and Searle over how best to account for the facts of language is, as the latter suggests, an empirical one and cannot be settled by a priori argument. If Searle could account for these regularities in terms of function then there would be no need of Chomsky's postulated rules but Searle has not so far provided any such account and it is Chomsky's conviction that he will be unable to do so that convinces him that his own programme is the more promising.²⁶

25 Searle, J. (1976) p1119

26 Chomsky, N. (1976) p57ff

Chomskians will admit, in reply to their critics, that they have not yet provided a complete set of rules of syntax but argue that they have developed a very elaborate and plausible outline of a grammar for English and gained sufficient insights into the syntactic structure of other languages to warrant further research. And they will also claim that all those languages which have been investigated do turn out to have grammars which possess constituency-cum-transformational structures; in other words they will argue that even though there is disagreement over the details of what these grammars are like there is sufficient accord to support the hypothesis of universal grammar.

Chomsky explains the existence of the postulated universal grammar by suggesting that it is, in effect, though he prefers not to use the term²⁷, innate. Therefore these alleged linguistic universals exist not because of some cultural consideration such as their common origin in our evolutionary past but because the human brain is so constituted that any language acquired and put to use through its operation must possess corresponding properties. We learn and speak the languages we do because of the way we are born. Chomsky employs a second argument in support of the 'innateness hypothesis', which runs like this. A human language is an amazingly complex and abstract entity as our syntactic analysis makes clear; an infant learns to speak it in a very short time; therefore, he must have had a head start, a brain richly endowed with structures amounting to a built-in universal grammar. Occasionally Chomsky backs up this argument with the claim that the language-learning child's exposure to linguistic experience is so fragmentary and impoverished that it is only his innate endowment that makes the achievement possible.

27 Chomsky, N. (1976) p13

Confusion has arisen over the status of a grammar in Chomsky's account of language acquisition and use. When the linguist constructs a grammar which accounts for those syntactical regularities he discovers, are we to take it as merely an abstract characterisation or are we to look upon it as psychologically real, the specification of a psychological competence?²⁸ It is a mistake, some argue, to look on a Chomskian grammar as a psychological description. Dismissing as ill-founded objections that Chomsky's constituency and transformational rules are not properly called rules at all (because they cannot be disobeyed) Sampson²⁹, for example, argues that 'it is not part of Chomsky's claims that the formulae describe or regulate whatever psychological processes result in our uttering individual sentences of our languages (though it would perhaps be surprising to discover that the psychological processes bore no relation whatsoever to the formulae).' Chomsky's recent presentation of his views, however, contradicts this interpretation and gives a clear psychological role to the grammar. He says that 'the grammar is put to use, interacting with other mechanisms of mind, in speaking and understanding languages.'³⁰ (Incidentally, I am not implying that Chomsky's rules ought not to be called rules if they are interpreted as psychologically real. As long as we are clear about their role in Chomsky's theory it matters little what he calls the procedure for deriving stringsets from a grammar.) An important phrase in Chomsky's quotation above is 'interacting with other mechanisms of mind' for his account of how we put a grammar to use is, we will see, more complicated than I have suggested.

28 Searle also asks this question when he wants to know if the postulated grammatical rules 'guide - as opposed to merely describe - his (the speaker's) speech' and observes: 'I have never seen anything like an adequate answer to this question.' Searle, J. (1976) p1120

29 Sampson, G. (1975) p100

30 Chomsky, N. (1976) p28

He makes it clear in Reflections on Language³¹ that his theory of language, outlined above, is merely the first step in a complex theory of the mental processes which generate and interpret language. The linguistic characterisation of 'what is learned' or 'knowledge of language' or 'grammar' constitutes an advance, he holds, on the 'premature' Skinnerian attempt to relate experience (in the form of what the child is exposed to i.e. verbal stimuli) to behaviour. What Chomsky holds we can attempt is to relate experience (in the form of what the child hears) to 'cognitive structure' or 'grammar' etc; but as to how that grammar interacts with other cognitive structures such as what he calls 'the domain of common-sense understanding'³² remains a mystery. Not only is it a mystery but it is one that he sees few prospects of studying scientifically for a very long time.

Finally, Chomsky believes that his work on transformational generative grammar will, if empirically successful, prove a model for investigations of other domains of human competence. He does not doubt that there are other innate faculties of mind which, on exposure to the relevant experience, construct other competences as abstract and complex as our postulated 'knowledge of language', and which may enter into 'the ability to recognise and identify faces on exposure to a few presentations, to determine the personality structure of another person on brief contact (thus, to be able to guess, pretty well, how that person will react under a variety of conditions), to recognise a melody under transposition and other modifications, to handle those branches of mathematics that build on numerical or spatial intuition, to create art forms resting on certain principles of structure and organization, and so on.'³³ In seeing his psycholinguistic theory as the model of the

31 Chomsky, N. (1976)
32 " " " p17
33 " " " p21

correct methodological strategy for psychology in general he is joined, as we saw in Chapter One, by others who see it as 'something of a test case for the possibility of an experimental mentalism.'³⁴ Chomsky thus endorses the general mentalist programme and views his own theory of language as a pioneering effort to give it detailed articulation. 'The proper way to exorcise the ghost in the machine,' he writes in support of that programme, 'is to determine the structure of the mind and its products.'³⁵

Evaluation of Chomsky's Mentalism

'The strongest argument against mentalism in psychology is simply that by and large it has not worked.' Thus write Fodor, Bever and Garrett³⁶ who attribute the movement's former failures to the equation of the view that psychology is the science of mental phenomena with the view that the proper methodology for that science is one which relies on introspective observations. In their view it is unnecessary to spend time defending the epistemological credentials of the introspective method because, as a matter of fact, most of the mental processes governing the understanding and speaking of a language and underlying other human activities are inaccessible to consciousness. They therefore proceed to investigate these mental processes, to exorcise the ghost from the machine, by developing abstract theories to account for publicly-observable behaviour. And should any of the causal mental processes be present to consciousness, these mentalists say, then they must be characterised by these theories in the same way as unconscious mental processes. In short, the 'New Mentalism' need not, these authors maintain, rely on introspective method.

34 Fodor, J., Bever, T. and Garrett, M. (1974) pxvii

35 Chomsky, N. (1976) p23

36 Fodor, J., Bever, T. and Garrett, M. (1974) pxi

Chomsky himself disagrees with this, we have already seen, in that he thinks a grammar must be tested against the speaker's introspective judgements or intuitions about the acceptability of sentences. And we have also seen that there are linguists, meriting the description Chomskian in that they propose and test constituency-cum-transformational grammars, who insist that only utterances are acceptable as linguistic evidence. They argue that the ghost can only be exorcised and psycholinguistics rendered an empirical science if only publicly observable data are admitted as evidence. They then proceed to show how Chomskian linguistics can be tested without reliance on intuitions.

One of the reasons some linguists - let us call them intuitionists - argue for intuitions is over the problem of negative instances. If a man utters a sentence then we can say it is a string belonging to the 'grammatical' set; but just because we have not heard a man utter some different sentence we cannot infer that that sentence is ungrammatical. Maybe we have not waited long enough or searched diligently enough to find it being uttered. Not all sentences which have not been observed are ungrammatical. To enable us to distinguish between the grammatical and the ungrammatical among sentences which have not been observed we must, intuitionists suggest, only declare to be ungrammatical those strings which speakers themselves judge to be ungrammatical.

Against this proposal Sampson argues that if we adopt the Popperian principle that our scientific theories ought to be as strong as possible through their prohibition of more and more possibilities we will want a grammar to exclude as many strings as possible. We want our grammar to exclude every string except those we actually hear and this provides us with a motive, he says, for excluding the unuttered. However, we also have a motive for classifying some non-observed strings as grammatical, Sampson correctly argues, if we also adopt the methodological requirement

that our linguistic theories be simple, in some a priori sense. He asks us to suppose, by way of example, that we have heard a speaker utter such strings as Boys like girls, Boys like pets, Girls like boys but that we have never heard anyone utter Girls like pets. He continues:

'Any English speaker knows that the latter sentence is fully as grammatical as the other three, but what motive can we have for constructing our grammar accordingly? A grammar which permits the string Girls like pets is that much less strong than one which forbids it; apparently we are justified in treating Girls like pets as grammatical only if we require the grammar to permit all the sentences which the native speaker 'knows' to be grammatical.' 37

But this, he adds, is not the case. Having heard the first three strings uttered by a speaker it would be plausible to suggest that there is a rule of grammar in English of the form 'A sequence of noun, -s, transitive verb, noun, -s is grammatical; boy, girl and pet are nouns; like is a transitive verb.' To exclude as ungrammatical the unobserved string Girls like pets would require that we insert a further clause to the above rule, reading 'except when the first noun, verb and second noun are respectively girl, like and pet.' Adding this clause would only be justified if it increased the strength of the grammar; but as it affects only one string its main effect is not to make the grammar stronger but to make it cumbersome. In general, the scientist must try to balance strength against simplicity in his theories and it is in this, Sampson rightly observes, that he may employ his judgement or intuition and not in deciding what the facts for the linguistic theory to explain will be.

Another possible argument in favour of using linguistic intuitions as evidence is that not all utterances which the linguist observes are held to be grammatical. Again, borrowing Sampson's example, we may hear

someone, who was about to say 'If the phone rings, could you answer it?', actually say only 'If the phone rings' because the person abruptly broke off in mid-sentence to deal with a boiling-over saucepan. The linguist ought, Sampson argues, to discount the half-finished sentence from the set of grammatical strings. But the intuitionist may object to this on the grounds that if behaviour alone is the evidence for a grammar we have no grounds for excluding anything that is uttered; he will argue that we can only exclude the uttered 'If the phone rings' as ungrammatical if we rely on our intuitive knowledge that it is incomplete.

This argument also fails because a grammar which judges 'If the phone rings' ungrammatical only predicts that it will not be uttered under normal circumstances. As Sampson points out, quite correctly in my view, the circumstances are not normal and we know, quite apart from issues in linguistics, that humans will often interrupt one task when a higher-priority task intervenes. We can therefore expect that we will sometimes hear the beginnings of strings but not their endings. 'Our theories confront reality in a body, not one by one; each individual branch of knowledge,' he says, 'makes predictions about observable facts only when we take into account other relevant pieces of knowledge.'³⁸

In sum, general methodological considerations provide us with a rationale for assuming that some utterances which we have not observed are grammatical while, when we take our more general knowledge about human behaviour into account, we have a good reason for ignoring some of the utterances we have observed. In neither case do we have to rely on a speaker's introspective judgements or intuitions. Chomsky's theories can be tested against publicly-observable, albeit theory-laden, behaviour.

Chomsky is not, incidentally, entirely consistent in espousing

³⁸ Sampson, G. (1975) p66

intuitions as an evidential base for linguistics and psychology because in his recent writings he stresses that these sciences share the same methodology as other natural sciences. Arguing against Quine's definition of behaviourism as simply the insistence that conjectures and conclusions must eventually be tested in terms of observations Chomsky remarks that 'any reasonable person is a behaviourist in this sense.'³⁹ In his insistence that linguistic theories must be tested against evidence as in any other science, Chomsky is, for some of his readers such as Gellner⁴⁰, an empiricist. Whether mentalism is empiricist depends, I presume, on what empiricism is taken to imply but there is little doubt that many of Chomsky's followers hold mentalism to be empirical.

Chomsky's research programme comes very close, I think, to the criteria which emerged in the last chapter as those which an empirical science ought to possess. The key idea in his theory of language is that syntactic properties, common to all natural languages, reflect the innate structural features of the infant brain. This is the first step in the articulation of the metaphysical assumption that language is a mirror of the human mind. We have seen that this assumption lends itself to articulation in diverse ways and that it allows for considerable disagreement among Chomskians over exactly what the principles of universal grammar are. We have also seen that when Chomsky does identify a grammar for a language he acknowledges that before we can explain linguistic performance or how the grammar is put to use we have to identify the other faculties of mind or cognitive structures which come into play in the generation of action. This makes his theory extremely complex, resistant to falsification by any crucial experiments and would

39 Chomsky, N. (1973) p111

40 Gellner, E. (1974) p94

even allow him to maintain his position, I think, in the face of prima facie embarrassing evidence. This is not to say that the theory that the transformational grammarians have developed is unscientific because unfalsifiable, merely that its success or failure in coping with the evidence will only be judged when the different cognitive structures are specified in much more detail. Until, for example, we have given some substance to the cognitive structure of 'common-sense understanding', posited by Chomsky, we will not be able to assess the status of his theory of linguistic performance.

My own belief is that further research will not support the claim that the child inherits a universal grammar or faculty of mind, specific to language. The burgeoning literature on language acquisition suggests instead, I think, that we will have to locate the basis for language learning in nonlinguistic cognitive structures. To understand the acquisition of knowledge of language we will have to begin by analysing how the child determines the meaning of what the speaker intends to convey.⁴¹ This is a reflection, in studies of language acquisition, of an empirical hypothesis about adult language, advanced by Searle⁴² in opposition to Chomsky, that if we wish to explain regularities in syntax we will have to take account of both the meaning and function of sentences. 'The claim is rather that,' he maintains, 'so far as we can tell structure, function and meaning in natural languages interact in all sorts of interesting and complex ways and it is extremely unlikely that all of the rules of structure can be stated completely independently of any of the rules for the use of the structures in question.'⁴³

This is not the place, however, to review the research findings in favour of Chomsky's theory or against it. Rather I have been concerned

41 Macnamara, J. (1972)

42 Searle, J. (1976)

43 " " " p1119

to examine the theory in order to see whether, in the words of the introduction to Chapter One, it complies with the minimal intellectual criteria demanded of any empirical science. And I have argued that Chomsky's mentalism is, in principle, a rational enterprise in that it meets the standards which I argued for in the last chapter. The metaphysical assumption which inspires his work, the thesis that language is a mirror of the human mind, has been translated into testable scientific theories which have, we have seen, met with some empirical support. However, as the linguistic research expands into the exploration of more and more languages it may be that we will have to either attenuate or abandon the claims for a universal grammar. Or it may be that psychological research into the child's acquisition of language will encourage us, as I have suspected it will, to drop the notion of an innate language faculty, a cognitive domain relatively isolated from the rest of the child's mind and to see language learning as an activity intimately involved with other mental capacities such as the capacities for perception and thinking.⁴⁴ In these cases we would be advised, I think, to conclude that our metaphysical assumption is false, that language - as Chomsky analyses it - is not a mirror of mind. The demise of Chomskian theory, however, would not discredit mentalism for we would, in such circumstances, be at liberty and indeed impelled to try to discover some hopefully true thesis about the mind and give it empirical expression.

Chomsky on Knowledge and Freedom

In his Bertrand Russell Memorial Lectures Chomsky⁴⁵ appropriately

44 In a recent article Bruner supports this 'integrated' research proposal and argues further that the reverse holds true for work on perception. Recent studies on colour perception show that the 'recognizability of colours to which one has been previously exposed is a function of their linguistic codability.' Bruner, J. (1976) p1590

45 Chomsky, N. (1972a)

considers a question that troubled the British philosopher throughout his life and to which Chomsky evidently feels his own work in linguistics and psychology provides a convincing if incomplete reply. 'How comes it,' Russell⁴⁶ asks, 'that human beings whose contacts with the world are brief and personal and limited, are nevertheless able to know as much as they do?' Dismissing as 'irrelevant' the possible response of the sceptic who holds that we do not have such knowledge as Russell suggests, Chomsky provides the controversial answer that 'we can know so much because in a sense we already knew it, though the data of sense were necessary to evoke and elicit this knowledge.'⁴⁷ Though, as indicated already, he prefers not to use the term Chomsky holds that human beings are born with 'innate knowledge' and claims to have vindicated the rationalist position against the empiricist in this centuries-old epistemological struggle.

He arrives at this conclusion, as we have seen, only after a pioneering investigation of the nature of language and its acquisition by the child. He builds up his case - here I summarise the logic of his argument - by first claiming that there are severe, arbitrary constraints on the diversity of human languages which amount to a set of linguistic universals. He then argues that these universals are no historical accident but the biologically necessary consequences of the operation of innate structures of mind which are, in turn, reflections of properties of the human brain. The child rapidly acquires a grammar or knowledge of language because he is born with an innate universal grammar, innate knowledge. Chomsky further holds that his linguistic studies will be a model for the study of other domains of competence and that we will have to develop novel theories to characterise these

46 Russell, B. (1948), quoted in Chomsky, N. (1972a) p13

47 Chomsky, N. (1976) p7

faculties. Only when such theories have been developed and proven empirically successful will we have provided, he believes, a satisfactory answer to Russell's question.

Chomsky's argument is, I suggest, fundamentally mistaken and his empirical researches provide no basis whatsoever for his claim that human beings possess innate knowledge in line with the rationalist tradition. His scientific theories are almost completely irrelevant to the problem which exercised Russell and epistemologists before and since. For their's is an epistemological problem and, as such, is primarily concerned with the validation of knowledge claims whereas Chomsky's substantive work is empirical, a psychological theory about the genesis of beliefs and cognitive structures.

To answer Russell's question properly it is necessary, I suggest, to draw a distinction between the logic or epistemology of knowledge and the psychology of knowledge. This is the distinction we saw Popper argue for against Kuhn when he objected to the latter's naturalistic treatment of knowledge. For Popper, knowledge is, again, a value and thus not something which can be empirically discovered; the value he chose, we have seen, prizes those falsifiable theories which have been adjudged not to be false. For Maxwell too, knowledge is a value; in his case only those theories which have an a priori simplicity, unity etc and which, though falsifiable, are adjudged not to be false are prized as knowledge. Following Popper and Maxwell I therefore suggest that before we can have a psychological investigation into the origins of knowledge we must first specify what we take knowledge to be. Which value we choose will then determine the scope of our psychological study.

Because Chomsky fails, like Kuhn, to make this distinction he treats knowledge naturalistically and identifies it with a set of beliefs and

cognitive structures. But even if his hypothesis that humans attain a knowledge of language so rapidly because they possess an innate universal grammar were true and even if it were to prove, as he hopes, a productive model for the investigation of other domains of human competence that would provide no support for a rationalist epistemologist who holds that humans possess a priori valid knowledge. All it would show would be that humans inherit a priori expectations, beliefs, dispositions etc with which they categorise their linguistic experiences and which enable them to quickly achieve a knowledge of language. We would have made a discovery about humans comparable to the finding by Hubel and Wiesel⁴⁸ that the cat inherits a priori expectations - to the effect that a tiny black object flying rapidly across its visual field is likely to be nourishing. Such a discovery of innate universal grammar in humans would not, however, establish the existence of a priori valid knowledge. It may prove to be an innate disadvantage for the infant if he 'knows', through his genetic inheritance, that human language has a specific structure; for if, for some reason, human beings found it desirable to speak some artificial language with a very different structure to that which allegedly characterises human natural languages then the infant's innate 'knowledge' would be the basis of his ignorance. For the cat, a parallel would arise, say, if due to some environmental change flies absorbed a substance which transformed them into a deadly poison; then the cat's 'a priori knowledge' would lead to his demise. In short, a priori knowledge is not a priori valid and Chomsky's failure to preserve the fact-value distinction in epistemology leads him to give a factual answer to a value question, to the mistaken conclusion that linguistic science vindicates traditional rationalism.

48 Hubel, D.H. and Wiesel, T.N. (1962) p106-54

In so far as Chomsky views mentalism in psychology as a natural science with no special methodology, a position he has recently favoured in common with other mentalists like Fodor, he must be bracketed with the traditional empiricists in epistemology. That rationalist-empiricist debate, as Searle⁴⁹ has noted in criticising Chomsky's 'misuse' of the terms, is primarily a debate about the validation of knowledge claims 'and not, except derivatively, of how they are acquired.' Chomsky scarcely bothers to debate the view that scientific theories can only be accepted if they have evidential support; he agrees that all cognitive claims must submit to the verdict of experience and the only discussion is over the nature of the evidence or experience. In epistemology he is an empiricist. In psychology, however, he calls himself a rationalist to contrast his theory of language acquisition with the impoverished behaviourist learning theory which he calls empiricist. This, too, is a misleading formulation because, as Searle⁵⁰ has also argued, 'the psychology of all of the classical empiricists, Locke, Berkeley and Hume, was introspectionist, and Behaviourism was founded by Watson and others at least partly in reaction against the empiricist tradition of introspectionism.' It would, therefore, be preferable if, to avoid confusion, Chomsky were to refrain from equating Skinnerian behaviourism with empiricism. One can perfectly logically and Chomsky does, very plausibly, espouse empiricism in epistemology and posit rich, innate mental mechanisms in psychology; but one may, equally logically, as Skinner does (highly implausibly, in my view) combine an empiricist epistemology with a radical behaviourism.

Though mentalism is, as noted in Chapter One, compatible with both dualism and materialism most leading mentalists do seem to subscribe to

49 Searle, J. (1976) p1120

50 " " " "

the latter with some, at least, explicitly espousing the Identity Theory. Psychological theories, therefore, are held to be ultimately about physical systems and the mentalists leave open the possibility of mental capacities being mapped on to the brain. But that does not imply, as we have also seen, that psychological laws will ultimately be reducible to the laws of physics. 'The point of reduction,' writes Fodor⁵¹, in the clearest formulation of the mentalist position I know, 'is not primarily to find some natural kind predicate of physics coextensive with each kind predicate of a special science. It is, rather, to explicate the physical mechanisms whereby events conform to the laws of the special sciences.' Since this is also Chomsky's position it is not, therefore, a distortion to maintain that he is not only an empiricist but a mechanist as well.

The idea that man is a complex machine is of interest not so much because of what it conveys but of what it promises. The mechanist is admired or feared not because he provides a complete, causal explanation of behaviour but because he asserts that it is possible, in principle, to provide such explanations. We may look upon Chomsky's linguistic theories as a tentative step towards the specification of the programme which governs language behaviour just as a computer programme governs the output of a machine. The analogy of the computer is appropriate because of the impetus programming has given to mentalism, as Fodor, Bever and Garrett point out. They write: 'If contemporary mentalists are better situated than their forebears to provide an account of the place of psychology among the sciences, the invention of the general-purpose digital computer is in part responsible. For even though the computer is undoubtedly a physical system, characterising its

51 Fodor, J. (1975) p19. As his argument is a general one we must, for our purposes, read 'psychology' for 'special science'.

transformations of information is quite as important as characterising its changes of physical state in any full description of the functioning of the machine.'⁵² Chomsky's programme of research, therefore, promises - once it has provided formal descriptions of cognitive structures and detailed their modes of interaction - to identify the causally necessary and/or sufficient conditions for what a person says and does. In adopting this aim, therefore, he seems to me to hold out the possibility of undermining our everyday, common-sense assumption that we are relatively free agents, largely responsible for our own actions. This is the point we saw Gellner make against Koestler when he challenged the latter's assumption that good science would vindicate the common man's view of himself, a view which contrasts sharply with the dehumanised caricature of 'pseudo-scientific' behaviourism. 'The price of genuine, powerful, technologically enriching science,' Gellner observes, 'is that its style of explanation ruthlessly destroys those very notions in terms of which we identify ourselves.'⁵³

Given Chomsky's aim, therefore, it seems to me odd to find him at the end of his first Bertrand Russell lecture arguing that his researches will advance the humanistic vision of man. Emphasising Chomsky's mechanist commitments Gellner remarks that he 'goes out of his way not to exclude the possibility that La Mettrie's mechanistic programme 'may be in principle correct'.⁵⁴ On the other hand it is possible to argue that Chomsky has his doubts that the mentalist or mechanist programme will ever get very far. For while discussing the genetic constraints on man's possible ability to develop theories which would explain his own actions Chomsky gives favourable consideration to a paper by Stent,

52 Fodor, J. Bever, T. and Garrett, M. (1974) pxciv

53 Gellner, E. (1974) p101

54 " " " p97; Chomsky, N. (1966) p81

significantly entitled 'Limits to the Scientific Understanding of Man'.⁵⁵

The gist of this paper is that the traditional mind-body problem haunts contemporary psychology in general and Chomsky's programme in particular in exactly the same form as it was spelled out by Descartes. The concept of self, says Stent, can neither be exorcised from psychological explanation nor is it accessible to scientific investigation. Descartes had argued that no matter how deeply we probe with our physiological investigations into the visual pathways we must eventually posit an 'inner man' who transforms the visual image into a percept. Stent adds:

'And, as far as linguistics is concerned, the analysis of language appears to be heading for the same conceptual impasse as does the analysis of vision. I think it is significant that Chomsky, who views himself as carrying on the line of linguistic analysis begun by Descartes and his disciples, has encountered difficulty with the postulated semantic component ... for man the concept of 'meaning' can be fathomed only in relation to the self, which is both ultimate source and ultimate destination of semantic signals.'⁵⁶

There is, he concludes, a 'fundamental epistemological limitation'⁵⁷ to the human sciences in that they must make reference to a 'Kantian transcendental concept', the self, a notion which will be forever resistant to scientific analysis.

If Chomsky were to align himself with Stent then there would obviously be some justice in Chomsky's claim to be a modern Cartesian. But, while he may have his doubts about the ultimate success of his enterprise, Chomsky's working assumption seems to me to be the mechanist one discussed above, an interpretation forcibly supported by Gellner. (Chomsky does, after all, aim to exorcise the ghost from the machine.) And if we take Chomsky's mentalism to be a form of mechanism, as I think we ought, then we must conclude that the connection between his technical

55 Stent, G.S. (1973) p1057
56 " " " "
57 " " " "

linguistic and psychological theories on the one hand and his wider conceptions of human nature, on the other, is, as his sympathetic commentator Bracken⁵⁸ writes, simply 'tenuous'. Or else we must conclude that he is defending an idiosyncratic humanistic vision.

Apriorism and Teleology

In endorsing mentalism as an, in principle, rational approach to a scientific psychology I have, by implication, rejected both Winch's apriorist arguments to the effect that a science of psychology is a contradiction in terms and von Wright's assertion that teleological and not causal explanations are appropriate to the study of intentional behaviour. Mentalism can be judged rational, I maintain, because it conforms or, more accurately, can be reconstructed so as to conform to the criteria of aim-oriented empiricism which I supported in the last chapter.

A constant theme of this thesis has been that different approaches to psychology are associated with different philosophies of science or with wider philosophical perspectives which legitimate the respective claims about whether a science of psychology is possible and, if possible, how it ought to be pursued. I am prepared to defend mentalism because it can be formulated in a way which conforms to those epistemological and methodological requirements I have been arguing for namely, the necessity to gamble on some metaphysical assumption about the nature of the human mind, to develop testable theories conforming to that assumption and to test them. Winch's and von Wright's independently argued but agreed conclusion that a natural science of psychology is impossible, on the other hand, is associated with two very different

58 Bracken, H. (1973) p242

epistemologies as I tried to make clear when I introduced their views.

Winch's neo-Wittgensteinian position has been subjected to an enormous number of varied critiques and since discussion of them would involve an interesting but unnecessary detour I shall restrict myself to those observations which are of particular relevance to psychology. I argued earlier (Chapter One p14) that we could dismiss Winch's case if we could show that he held an unsatisfactory theory of science or that his account of what is involved in understanding social life is flawed or both. I now suggest that indeed both prongs of his argument are blunt and that he is mistaken in his conclusion that the logic of natural science and the logic of social life are incompatible.

Let us recall that he built up his case by contrasting the kind of account of social behaviour which he believed a natural scientist is obliged to offer with what a sociological observer or philosopher ought to present. When the natural scientist describes people speaking, singing, dancing or whatever people do in social situations he must, Winch argued, restrict his explanation to be one about organisms making certain movements and sounds. 'To describe what is observed by the sociologist in terms of notions like 'proposition' and 'theory' is,' he wrote, 'already to have taken the decision to apply a set of concepts incompatible with the 'external', 'experimental' point of view.'⁵⁹ Winch then proceeded to recommend that we study social life in terms of concepts appropriate to the 'form of life' involved, rather than to subscribe to a kind of scientific imperialism which imposes the 'form of life' appropriate to science on competing 'forms of life'.

Winch's conclusion is mistaken because he has embraced an excessively narrow conception of science and of what an 'external, experimental

59 Winch, P. (1958) p110

point of view' entails. It does not entail, as we have seen, that the psychologist must conceptualise behaviour as movement nor that he refrain from using terms such as 'proposition' or 'theory' when describing what people take themselves to be doing. Logical empiricist meaning criteria do require that the natural scientist behaves as Winch suggests but we are not required to employ such criteria to remain scientific psychologists. Popper maintains, we have seen, that to insist on construing behaviour as movement is to subscribe to a verificationist theory of science which mistakenly aims to base scientific theories on an atheoretical, solid empirical foundation. And I have argued, following both Popper and Maxwell, that it is a sufficient constraint on scientific theorising that our theories be vulnerable to theoretically-interpreted observations. To use the example already employed, it is just as scientific to record as a statement of scientific observation that 'he rang the bell' as it is to say 'right arm moves to horizontal position, right index finger juts forward one inch, makes contact with button, etc'. And it is, of course, the former kind of statement of scientific observation that the mentalist or psycholinguist makes in testing his theories.

Like Winch, von Wright insisted that the application of the methods of natural science to social phenomena will result in a causal account of movement and not in an explanation of action. For von Wright also holds, as I have already pointed out, a conception of science which is as positivistic as is Winch's. And it is because he views the natural sciences in that way that he is led to construct an alternative schema of explanation for the social sciences. He is, therefore, vulnerable to exactly the same criticism as that proffered against Winch in the previous paragraph and to the corollary that if he had embraced a modified Popperian philosophy of science, as outlined in the last chapter,

he need not have constructed his practical inference schema. He could have, as Giedymin indicated, adopted a methodological monism without simultaneously committing himself to positivism.

Weinryb's incisive criticisms of von Wright's unnecessary alternative schema have been examined at some length in Chapter One and we have seen that he showed the case against a causalist account, via a refined version of the Logical Connection Argument, to be based upon a non-sequitur. Weinryb demonstrated clearly that von Wright failed to substantiate his case for a methodological dualism. Von Wright's position is, therefore, rejected because the two methodologies he advocates for the natural and social sciences respectively are unsatisfactory. Let us now return briefly to discuss Winch's alternative for the social sciences.

The second prong of Winch's argument, that since social action cannot be described by a natural scientist such action or, meaningful behaviour - along with 'private' mental activity (i.e. thinking) which is, in reality, allegedly parasitic on public behaviour - must be investigated by the philosopher, is no more convincing. We have, in fact, seen that there is a crucial flaw in Winch's development of his thesis when he holds that all meaningful behaviour (thinking included) is rule-following behaviour and therefore essentially bound up with the social context in which it was learned and is used. We need not concede, therefore, that to understand social action we must study it as a part of a distinct 'form of life' - with all the problems that such a philosophy entails. The flaw was pointed out independently and effectively by Whiteley and Fodor (Chapter One pp32-34) when they argued that though we need public procedures for discovering whether a rule is

being applied it is perfectly sensible to talk of rule-following in the absence of such procedures. Identifying the flaw allowed Whiteley to dismiss the Wittgensteinian objection to a private sense-datum language, leaving the way open to his espousal of a kind of dualism while Fodor considered himself freed to develop the case for an innate, representational language of thought.

In sum, therefore, I reject the two options which Winch claims to be the only alternatives facing the social scientist and, by extension, the psychologist. And the best answer to Winch, von Wright and, for that matter, to Skinner will be provided, I believe, by the development of testable theories in the mentalist programme as pursued within the requirements of aim-oriented empiricism. If we should be able to discover theories of mind which explain the behaviour we observe, even in a limited, defined sphere then we will have undermined these apriorist and teleological positions. In the meantime the most we can do is challenge the assumptions which inspire their arguments and focus on the lacunae in the arguments themselves.

I ended my discussion of Chomsky by noting Gellner's argument that the former was both a mechanist and a materialist. Should Chomsky find empirical support, Gellner wrote with approval, he would have demolished our everyday self-image, replacing it with a complex, causal account of behaviour. It is exactly this kind of account, Gellner wrote in an earlier paper⁶⁰, that Winch aims to immunise human and social life against. We ought to view Winch, he wrote⁶¹, as a 'negative anthropomorphist', a man trying to protect the idea of a 'meaningful moral order' in human affairs against a scientific account which gives the human no special status in the world.

60 Gellner, E. (1968), (1974)

61 " " (1974) p130

'In an important and extended sense,' says Gellner⁶², 'the Copernican revolution is well established; humanity is known not to be at the centre of things; human requirements are not allowed to limit, or even create presumptions in, the sphere of scientific theory.' In the fields of human and social studies, however, anthropomorphism has yet to be conquered, man has yet to be dethroned by science. Winch's strategy is to avoid any positive anthropomorphic doctrine which would, says Gellner, spell out what the moral order in the social studies ought to be and he resorts instead to an epistemological defence of human and social life. Winch's argument, according to Gellner, is 'that the very nature of knowledge, in the sphere in question, is such that no non-anthropomorphic theory can possibly be true. This leaves the field open for anthropomorphic theories, without however at the same time positively singling out any one of them.'⁶³

Gellner's counter-argument is, in part, that Winch's views are contradicted by a flourishing anthropology and sociology which constantly, if fallibly, compare cultures, societies and so-called forms of life and attempt to discover theories which are universally supported. Winch is further contradicted, he says, by features of human societies themselves, especially by their pluralist and self-critical characters; isolated, mutually exclusive, self-satisfied forms of life are, he says, a myth. Since what is actual is possible we ought, he implies, to regard existing social science and the nature of the societies studied as refutations of the Winch/Wittgensteinian position. He then strengthens his case with an assault on the epistemological argument itself.

62 Gellner, E. (1974) p129
63 " " " " p131

Chapter Six

An Interactionist Alternative

I am averse to turning the greatest miracle we know of in this universe into an epiphenomenon.

Karl Popper
Replies to My Critics¹

Introduction

Interactionists insist that the conscious mind, at least, exists, is as ontologically distinct as the material world and, as commonsense assumes, exerts a causal influence on that world. They therefore unite in opposition to mentalism which, they maintain, gives no special psychological or biological recognition or function to consciousness or mind, reducing it instead - as Popper's remark suggests - to the level of an epiphenomenon. They are especially opposed to a mentalism which is combined with a materialist ontology, a combination which is, I have suggested, favoured by the majority of contemporary mentalists. Should a mentalist, however, prefer to espouse a dualist ontology he would, I suspect, still incur opposition from an interactionist - who would further insist that the conscious mind be given causal efficacy in psychological explanations. I am forced to speculate on the latter because I have not encountered any psychologist who explicitly states an attachment to both mentalism and dualism.²

1 Popper, K.R. (1974) p1052

2 F.A. Hayek may, perhaps, be said to approach such a position in his paper, The Primacy of the Abstract, where he defends the contention that 'all the conscious experience that we regard as relatively concrete and primary, in particular all sensations, perceptions and images are the product of a superimposition of many 'classifications' of the events perceived according to their significance in many respects.' Subjective experience is the product of abstract mental operations. Hayek, F.A. (1972) p311

Interactionists do not, however, form a homogeneous group, having arrived at the same conclusion by identical routes and sharing a common programme for future research in psychology. The three interactionists introduced in Chapter One, Beloff, Whiteley and Popper all agree that the conscious mind is not to be reduced to, identified with or eliminated in favour of the physical but they present very different proposals on the basis of their arguments. Beloff³ advocates continued research in parapsychology because he accepts Randall's redefinition of that field as 'the science of mind-matter interaction.' Whiteley, on the other hand, proposes that the psychologist ought to investigate and, if possible, discover generalizations 'about the effects in consciousness of what happens outside it, and the effects of what happens in consciousness on human behaviour.'⁴ Popper develops a very different strategy for the investigation of mind and it is his interactionist alternative that will be my main preoccupation in this chapter. Before examining his position, however, I would like to reflect on Beloff's arguments for dualism and to assess their implications for mentalism.

Beloff⁵ identifies three features of mind or mental processes which, he says, are not found in the physical world and which suggest that nothing less than a dualist-interactionist psychology will suffice, even if that thwarts the ambitions of those who are committed to the unity of science movement. First, we must recognise that human and animal life in general are conscious, in contrast to material objects and probably, to plant life as well. The second feature he specifies is 'the referential aspect of the cognitive processes in man and the higher animals'⁶, by which he means that 'our thoughts, percepts, memories and so forth represent, symbolize or otherwise refer to something

3 Beloff, J. (1973)

4 Whiteley, C.H. (1973) pl14

5 Beloff, J. (1976)

6 " " " pl27

other than themselves.'⁷ Noting that Brentano and the 'act psychologists' took this as the cardinal feature of mind he points out that a physical process, however complex, is just a physical process and never refers to anything. The implications of this for the mind-brain identity theory are, he thinks, very embarrassing. He writes:

'For to claim that two entities are identical implies that whatever can be said about the one entity can equally be said about the other entity. However, while I can say what my thoughts are about, it seems to make no sense at all to say about the concomitant brain processes that they are about anything, they just are. But in that case there appears to be an insuperable difficulty about identifying my thoughts or my thinking with any set of brain states or with the electrical impluses in the neural circuits of my brain.'⁸

The third feature of mind distinguishing it from the physical is what he calls the 'intentional or purposive aspect of behaviour which transforms what would otherwise be a mere sequence of movements into a meaningful action.'⁹ No purely physical system can, he says, act in a sense which implies an intention. He asks us to consider a machine or robot capable of performing some perceptual type task, such as selecting an object of some shape or colour. We cannot, Beloff maintains, talk of the robot's perceiving 'because perceiving implies, among other things, having certain conscious percepts and if anyone were to suggest that the robot was conscious we would suspect him of prevaricating in his use of the word consciousness.'¹⁰ When we talk of machines 'speaking a language', 'reading', 'playing chess' etc we ought not to forget, he implies, that they only appear to be doing these things; they are, in fact, merely executing movements without awareness. 'The apparent intentionality of such performances,' he says, 'is not anything intrinsic but derives from the fact that we invest them with meaning.'¹¹

7 Beloff, J. (1976) p127
8 " " " p127/8
9 " " " p128
10 " " " "
11 " " " "

Though I have sympathy with these arguments none of them seems to me to constitute decisive objections to mentalism. Unlike some behaviourists mentalists do not deny that consciousness exists; they simply argue that it is in principle possible to give causal explanations of behaviour by characterising mental states through the construction of theoretical models of mind. Some of the characterised states are conscious while others are not and the mentalist may plausibly reply that if he is able to explain, predict and control observed behaviour then he will have been justified in treating consciousness as a kind of epiphenomenon. It must be admitted, however, that the mentalist programme has not come anywhere near such an achievement and as long as its achievements are so meagre it can expect to meet the criticism that it is consciousness that is, as commonsense assumes, the important determinant of behaviour and so must be appropriately recognised in a psychological theory. However, mentalists may respond to that with the observation that there are impressive empirical discoveries in, for example, speech perception which show that perception does not imply consciousness and that various 'mechanisms of mind' are being successfully studied by his methods.

As for the second criticism the mentalist may reply that the truth or falsity of the identity-theory is independent of his approach to psychology. The mentalist might opt for a more full blooded materialism or for dualism, pluralism or some other metaphysic. Beloff's criticism may, in short, be accepted without embarrassment. On the other hand the mentalist may continue to espouse the identity theory and try to meet the criticism itself. He might argue that it has no force because the postulated identity relation is contingent and it is well known that for contingent identity statements there are exceptions to Leibniz's Law of the identity of indiscernibles. Such is the kind of argument approved

by Borst¹² who takes as his example the argument that mental states could not be physiological states because we can have knowledge of the former without possessing any knowledge of the latter. He objects to this and notes, by way of reply, that 'people were able to speak about genes before anything was known about DNA molecules, but for all that genes are DNA molecules.'¹³ What his argument amounts to is that the so-called intentional properties (which include such propositional attitude phrases as 'knows that', 'believes that' etc) are not genuine properties at all. He continues:

'It may be true, for example, that Tom believes (or fears, etc) that the Morning Star is likely to explode, without its also being true that Tom believes (or fears, etc) that the Evening Star is likely to explode, even although, unknown to Tom, the Morning Star is the Evening Star.'¹⁴

'What Tom believes' is not, on this view, a genuine property of the planet Venus and we cannot, therefore, expect - as Beloff does - that 'whatever can be said about the one entity can equally be said about the other entity.' Whether this counter-argument removes Beloff's objection would then depend upon whether the referential aspect of a cognitive state is held to be a genuine property of that state. To say the least Beloff's mythical opponent would have a difficult task if he were to try to establish the possibility of mental states that need not refer at all. As I do not wish to get further embroiled in the debate on the identity theory I shall conclude by again noting that no matter what the eventual assessment of Beloff's criticism may be it is not decisive for mentalism, which is compatible with other ontologies.

As will be evident from my discussion of Popper's interactionism I have sympathy with Beloff's third argument which is, in effect, that

12 Borst, C.V. (1970)
13 " " " p25
14 " " " "

it is impossible to provide an explanation of human action in purely physical terms. We will see, however, that such a priorist objections are connected with wider metaphysical positions which cannot be settled with finality. A committed mechanist will dismiss the objections as based upon arbitrary limitations on the complexity of the robot's mental equipment and maintain that there is every possibility of what Beloff calls mind-like, intentional or purposive behaviour yielding to a mechanist analysis as he constructs an increasingly sophisticated representation of the brain and its environment. It is exactly along these lines that the mentalist N.S. Sutherland makes his case.¹⁵

Popper's Interactionism: World 1, World 2 and World 3

In his fight against relativism Popper has elaborated a metaphysical theory which reinforces his objectivist position. He begins by recognising the reality of three distinct entities which he calls World 1, World 2 and World 3. World 1 is the physical world of material bodies such as rocks and trees and includes apparently non-material entities like X-rays and lasers. World 2 is the world of subjective experiences or mental states, images, perceptions, feelings and so on. More controversial is his World 3 which refers to the objective contents of thoughts and which includes not only theories, arguments and the products of scientific thinking but also such diverse cultural objects as statues, poems, string quartets, symphonies and paintings; it also contains theories and arguments which are false or mistaken and, most important, it contains the infinite number of consequences which can be logically deduced from all theories. From both an epistemological and methodological standpoint this World 3 is of crucial importance.

15 Sutherland, N.S. (1970) pp106-109

Popper admits that his World 3 bears certain resemblances to Plato's theory of Forms or Ideas and to Hegel's theory of objective spirit but is, perhaps, most closely akin to Frege's objective contents of thought. In contrast to Plato's theory, however, World 3 is man made and had no existence before the emergence of human language; it is, Popper argues, the distinctive descriptive and argumentative functions of human language as opposed to the expressive and signalling functions of both animal and human languages that are responsible for the appearance of World 3. Unlike the Hegelian spirit World 3 is not, in any sense, conscious or a kind of 'Superhuman Being'. Popper's theory differs from Frege's in that it contains a much wider range of entities than theories and their consequences - provoking the charge that World 3 is overpopulated.

Popper has advanced three major arguments in support of his attribution of full ontological status to World 3. First, he asks us to consider a thought experiment in which we imagine that various aspects of our culture and civilization are destroyed. Imagine that as a result of some catastrophe all our industries, technology, institutions, etc have been wiped out, together with all our subjective knowledge and memories of these, but that our libraries and books containing the theories which gave rise to these along with our ability to learn from them have survived intact. In these circumstances our human civilization would, Popper maintains, be rapidly, if painfully, rebuilt. Had all our libraries and books been wiped out as well, on the other hand, he argues that it would take eons of time to reconstruct our achievements. He considers this a powerful argument in favour of recognising the reality of World 3. For since our subjective knowledge of World 3 had perished, World 3 is not mental; nor is it physical since all physical

artifacts (except libraries, books, etc) were also wiped out¹⁶; nor could it be held to be merely social since we have imagined that all our social institutions have disappeared as well.

Popper's second argument is that World 3 is autonomous, at least in part. Admittedly it is dependent upon its human creator in that if he did not exist World 3 would not have existed either. But once a theory has been produced then World 3 becomes partly independent in that there are always logical consequences of that theory which, unintended and unforeseen, are there to be discovered. Popper illustrates this argument with the example of the invention of the natural numbers. Once they were invented then the prime numbers, the odd and even numbers and many other relations were there for the mathematician to discover. Every scientific, artistic or musical theory has, says Popper, an infinity of consequences which no-one ever thought of before and perhaps will never think of. Lack of human ingenuity, opportunity or time, however, is insufficient reason, he implies, for denying the reality of such consequences and he therefore argues against the identification of scientific, artistic or musical knowledge with the thoughts that human beings happen to think. He insists that World 3 transcends the human minds which produced it and scientific knowledge, in particular, is not to be identified with what scientists happen to think.

It is with this argument that we can see how World 3 is an extension of the emphasis, in Popper's early work¹⁷, on the distinction between the subjective and objective dimensions of knowledge. (Chapter

16 On this argument World 3 is physical in that it must be encoded in some World 1 object, like a book or photograph. But the same theory may be encoded on any of an infinity of different physical objects. So, on this argument, World 3 is not ontologically independent of all but it is independent of any physical object.

17 Popper draws attention to the continuity between his metaphysical theory and the methodological defence of objectivity, as put forward in The Logic of Scientific Discovery. Reply to J.C. Eccles' comments on World 3. Popper, K.R. (1974) p1050

Three p129) He there argued that the truth or falsity of a theory was, among other things, independent of the perception and appraisal of its truth by anyone or of its reception by any scientific community. His initial distinction between the two approaches to knowledge was a methodological one, to be judged in terms of its fruitfulness. His new defence of objectivism, on the other hand, relies upon a metaphysic with ontology in the role of handmaiden to epistemology. We might say that his original proposal was that we treat the objective dimension as if it were real while his new theory is a bold conjecture that it is just as real as the physical and the mental.

Before introducing Popper's third argument for the reality of World 3 I wish to examine his views on how these three worlds are envisaged to interact. The human mind, World 2, can interact in a causal manner with World 1 and vice versa; what happens in World 2 - beliefs, intentions, decisions, acts of will, etc - can all produce effects in World 1 and similarly what happens in World 1 can give rise to such beliefs etc in World 2. Just how this interaction takes place remains a mystery though Popper¹⁸ does speculate that there is not a one-to-one mind-brain liaison, with the brain in the same state every time a person thinks the same thought. Popper rebuts the traditional objection to mind-body interaction which, based on the Cartesian admission that mental states have an intensity but no extension, concludes that such mental events cannot causally affect bodily movements. Such an argument, says Popper,¹⁹ rests on the false assumption that there is

18 Popper, K.R. (1974) p1052

19 Beloff also argues that this traditional objection to dualism rests on a false assumption about the monopoly of mechanistic causation and writes: 'Stated in its simplest terms, we can say that if we are satisfied that an event E would not have occurred under conditions C but for another event X then, ipso facto, we are entitled to call X the cause of E whatever might be the nature of X or E, whether one be mental and the other physical or whatever.' Beloff, J. (1976) p129

only one kind of physical causation, namely the action-by-contact theory presumed to explain the interaction between material bodies.

About this theory of causation and Popper's reaction to it Watkins writes:

'This simpliste theory of physical causation is, of course, quite obsolete today; yet many philosophers cling to its negative implications for mind-body interaction, despite the fact that both common sense and physics admit many kinds of causal interaction between empirically different kinds of thing. One of Popper's examples, here, is light and matter, light being essentially differentiated from matter by its velocity. Another example, which has been adduced in this connection by J.O. Wisdom, is electricity and magnetism, which seem radically different and mutually irreducible; yet there is electromagnetic interaction. (Philosophers seem never to have asked in this context embarrassing questions like, Just where in a dynamo does the interaction take place? Electromagnetic theory has had no pineal gland trouble.)' 20

World 2 does not, however, interact with World 1 alone. Indeed it is the fact that, as Popper argues, World 2 can causally affect World 3 and vice versa that provides his third argument in support of the reality of World 3. Once the human mind has formulated a theory in World 3 that 'third world product' can, he says, react upon World 2 and can lead eventually to World 2 acting on World 1. I think this way of talking is quite likely to mislead and, as I understand him, Popper's intention is not to posit some active World 3 'Being' but to draw out the implications of the autonomy of World 3. On this reading he is arguing that a scientist may invent a theory which has consequences which the scientist - or someone else - may hit upon only a long time after the initial theory's invention. These unintended and unforeseen consequences may well prove to be a startling discovery even for the mind which invented the initial theory and may constitute a solution to the problem which the initial theory was intended to solve. Hence we

may say that World 3 leads to what DeWitt²¹ aptly calls 'categorically novel effects' in World 1, effects resulting from a process which, in summary, works like this: the human mind (World 2) invents a theory (World 3) which has unforeseen consequences which constitute a solution to some objective problem (both in World 3) and those consequences are later perceived (in World 2) to be a creative and valued advance in the field in question. That advance may then be implemented by the human mind (World 2) which causally interacts with World 1. The discovery may take place in any of the scientific or artistic fields and examples of such discoveries, realised in World 1 as categorically novel effects, would be the discovery of counterpoint in music or the development of perspective in art. Popper²² himself and Gombrich²³ have independently applied this interactionist schema to both music and art history. In sum, Popper argues that World 3 can exert a causal effect on World 1 via the intervention of World 2 and this constitutes his third major argument in favour of its distinct ontological status.

In support of his interactionism Popper draws upon his previous work on another metaphysical issue and an associated scientific theory, the problem of determinism and the theory of quantum mechanics. From around 1950 he has championed the radical indeterminism of quantum mechanics which holds that there exist microphysical processes which are not further analysable in terms of causal chains but which consist of what are called 'quantum jumps'; a quantum jump is an event which is held to be absolutely unpredictable because it is not determined by causal laws -

21 DeWitt, Larry W. (1975) p205

22 Popper, K.R. (1974) pp47-57. An example of the unforeseen consequences of a musical invention which Popper provides and of the effect of the perception of those consequences is that of Haydn. Says Popper: 'There is a beautiful story about Haydn who, when listening to the first chorus of the Creation, broke into tears and said, 'I have not written this.' Popper, K.R. (1972) p181

23 Gombrich, E.H. (1971), (1972), (1975)

a quantum jump is not causally predetermined to occur before it in fact occurs. 'Quantum mechanics,' Popper argues²⁴, 'regards these absolute chance events as the basic events of World 1.'

Popper's main opponents in this controversy have been scientific determinists who are convinced, as was Einstein, that behind the apparently chance events which occur in microprocesses there exist rigid causal laws which determine the events in question. Scientific determinism is actually composed of two doctrines and Popper rejects them both. The first doctrine, what is called metaphysical determinism, holds that every event that occurs in the universe is causally determined. The second doctrine is the further epistemological claim that there is, in principle, no limit to the scientific knowledge we can obtain about the already fixed and determined future, from a knowledge of initial conditions and the laws of nature. These scientific determinists, therefore, argue that the chance events observed to occur in microprocesses only appear to be chance events because of the imprecision and incompleteness of our knowledge of both initial conditions and laws of nature. The ideal they aspire to attain is that of the mythical Laplacian Demon who is imagined to have a precise and total knowledge of the position of all bodies in the universe and of all the forces acting upon them. Were they to attain such knowledge then for them too, as Laplace wrote, 'nothing would be uncertain and the future, like the past, would be present to (their) eyes.'²⁵

Popper has consistently fought scientific determinism because, as Watkins²⁶ - drawing on Popper's unpublished postscript to The Logic of Scientific Discovery - puts it, it 'obliges us to put a special

24 Popper, K.R. (1973) p24

25 Quoted in Watkins, J.W.N. (1974) p375

26 Watkins, J.W.N. (1974) p376

interpretation - an essentially anthropocentric interpretation - upon (probabilistic) theories; these cannot be taken at their face value as statements about the world, for in nature (according to metaphysical determinism) nothing is chancey or merely probable; probabilities reside in the subjective world of our thinking, not in the objective world outside.¹ Because of his attachment to metaphysical determinism the scientific determinist is forced to interpret probabilistic theories as, in part, statements about our ignorance.

In opposition to this subjectivist interpretation of quantum mechanics Popper has pioneered a propensity theory of probability which enables him to give an objectivist interpretation of probabilistic theories. Such theories are not, on this interpretation, held to be about our ignorance but about the world itself. Only if he were to embrace metaphysical determinism would he be obliged, Popper argues, to accept or make a subjectivist interpretation, for it is that doctrine that prevents us from accepting observed indeterminacies as being about what they appear to be about, namely indeterminacies in the physical world. But since its main consequence, he says, is that it subjectivises physics Popper recommends the rejection of metaphysical determinism.²⁷

Popper does not claim to have refuted determinism. Since it is a metaphysical theory that would be impossible. What he does claim is that metaphysical determinism leads to undesirable consequences in physics and to various paradoxes, some of them first discovered by the quantum physicist-philosopher Alfred Landé.²⁸ Popper is so concerned to defend metaphysical indeterminism, the doctrine that there exists at least one event such that there was a time prior to its occurrence

27 The metaphysical determinist may plausibly reply, however, that subjectivism is not a necessary consequence of his position. His aim, he might reply, is to provide an objectivist account by eliminating probabilistic theories altogether.

28 Landé's thought-experiment arguments against determinism are rehearsed by Watkins (1974) p378; further references are cited.

such that it was not causally predetermined, because he considers it a necessary condition for World 1 being open to causal interaction by World 2. He accepts Laplace's conclusion that a physical world which is causally determined, closed to the intervention of World 2, implies that there is neither human freedom nor creativity. If World 1 is determined then everything which humans do is also determined.

Indeterminism, however, is not enough for interactionism which can, as it were, only get a foothold if World 1 is not closed to World 2. The admission that there are absolutely chance events in World 1 does not, in itself, allow either for human freedom or creativity. 'A closed indeterministic World 1 would,' he says²⁹, 'go on as before whatever our wishes or feelings are, with the sole difference from Laplace's world that we could not predict it, even if we knew all about its present state: it would be a world ruled only by chance.' He therefore conjectures that World 1 is not only a metaphysically indeterminist one but is also incomplete, open towards the causal influence of World 2.

Popper's interactionism, then, is a bold metaphysical thesis about what there is in the world and is dependent upon the truth of the further metaphysical thesis of indeterminism. It is upon these two theories that Popper has built his new defence of objectivism and his attempt to revolutionise psychology. Once we admit the reality of Worlds 1, 2 and 3 the proper methodology for psychology is not one which tries to probe the processes of thinking and acting in the way we saw the mentalist tries to do. We must instead shift to an analysis, he proposes, of the modes of interaction between Worlds 2 and 3. 'My central thesis,' he says, 'is that any intellectually significant analysis of the activity of understanding has mainly, if not entirely, to proceed

29 Popper, K.R. (1973) p25

by analyzing our handling of third-world structural units and tools.³⁰ We must, that is, analyse human thinking in terms of the interaction between World 2, the initial World 3 product, its myriad unintended consequences and the latter's perception in World 2. This analysis must then be complemented by an analysis of the interaction of World 2 on World 1. About this level of interaction Popper is admittedly vague and his account sketchy; but we have seen that he speculates that, in view of the incompleteness of World 1, physicalism in the sense of one-to-one mind-brain liaison is unsatisfactory. He conjectures that the relation between World 1 and World 2 has the character of a genuine interaction.

For and Against Popper's Interactionism

Popper's pluralistic metaphysics provoked the sociologist of science, David Bloor, to launch an attack on what he called the 'mystification' of objective knowledge which such a metaphysic allegedly creates. What is valuable in the notion of World 3 can be preserved and unnecessary puzzlement or mystification dispelled, he argued, if we employ the 'transformative method' as it was applied by Marx to Hegel. The rule of transformation which Bloor recommended was this:

'For 'Third World' read 'Social World'.³¹

'The method,' he added, 'is based on the assumption that metaphysical systems are not empty fantasies, but are about something real - namely, the social relations between men. It further assumes that such systems disguise, displace and invert what they perceive; that they make a mystery out of what might be said more plainly.'³²

30 Popper, K.R. (1972)

31 Bloor, D. (1973) p8. Popper now prefers the name 'World 3' to 'Third World' so I shall follow his usage.

32 Bloor, D. (1973) p8

Applying this method Bloor then produced a 'translation' which preserved the tripartite pattern of the original but with the individual now poised between the natural (physical) and social worlds. Thus instead of saying World 3 acts on World 1 via the intervention of World 2 Bloor writes that 'society acts on men, but collective purposes impinge on nature only through the agency of individuals.'³³ The mistake of identifying World 3 with World 2 (as Popper would have it) becomes, for Bloor, 'the more recognisable syndrome of being blind to social processes.'³⁴ Proceeding in this way he claims that he can systematically translate the 'obscure into propositions which are both clear and arguable.'³⁵ And so we can see, according to Bloor, that Popper's claim that World 2 processes are anchored in World 3 amounts to nothing but the claim that 'an individual's subjective acts of thought are anchored in the social world ... In short, Popper is saying in his language that psychology owes more to sociology than vice versa.'³⁶

Locating the objectivity of scientific knowledge in World 3 turns it, says Bloor, into a mystery. He continues:

'What exactly is the third world? What mode of being does it have? To say that it is the world of propositions, theories and problems still leaves the issue obscure, because an account is needed of what sort of entity these are.'³⁷

We need not bother with such questions, on the other hand, if we apply the transformative method - or so he argues. For, reading social world for World 3, we can see that the individual is a member of a social group, subject to group pressures and so conditioned in his beliefs by group concepts and norms. Recognising how the social group 'transcends

33 Bloor, D. (1973) p9
 34 " " " "
 35 " " " "
 36 " " " p10
 37 " " " p15

the individual and constrains him'³⁸ we can even appreciate why, Bloor implies, Popper felt impelled to award some 'real' ontological status to World 3. Group norms - stable, enduring and external to the individual - appear to have a distinct nature. But once they are seen for what they are it is clear, he concludes, that they do not inhabit some mystical, mystifying realm but have a more earthy, accessible abode. 'The authority of truth,' he concludes, 'is the authority of society.'³⁹

If all that Popper is trying to say is that society transcends the individual and that the authority of truth is the authority of society (whatever exactly that means) then we ought to welcome Bloor's sociological reductionism. For that is certainly not the message which I and Bloor's critics took from Popper's writings. But I now suggest that we ought not to welcome the attempted reduction because it either ignores or fails to grasp the central point of World 3, which is this: The objectivity of knowledge is not to be located either in what an individual, any group of individuals, a community or society either now or in the future might happen to think. The objectivity of knowledge lies in the fact that it transcends all social processes; it lies in the autonomy of World 3. We have seen Popper argue that one of the consequences of World 3 is that scientific knowledge is independent of the beliefs of any scientific community. As a consequence of applying his transformative method we find Bloor arguing, in contrast, that '*physics is what physicists (as a group) know.*'⁴⁰ (My italics) Since Bloor's transformative method, therefore, results in the denial of the most fundamental point about World 3 it ought to be rejected. If we were

38 Bloor, D. (1973) p17
39 " " " "
40 " " " p11

to accept Bloor's alleged translation of Popper's pluralism we would, apart from distorting the objectivist argument, be adopting a view of science very similar to Kuhn's; in particular, we would be committed to sociology - the mistaken identification of scientific knowledge with communal beliefs - and to its relativist consequences.

As for the repeated claim that Popper's pluralism is mystifying, disconnects us from 'processes that are real and accessible to investigation'⁴¹, is 'opaque' and both 'paralyses the imagination and stultifies research'⁴² I find myself in full agreement with DeWitt's fear of the authority of society. He writes:

'If the objectivity of World 3 is no more autonomous than the intersubjectivity of the society, then might not this stultify research in the feared manner? If 'the objectivity of knowledge resides in its being the set of accepted beliefs of a social group' then clearly this could, all too easily, stultify progress to the point of stagnation. That is, what if the social group never changes any of its accepted beliefs? If we then acquiesced in this stagnation we would be properly 'objective' (under Bloor's view) and presumably nothing more is to be said. But obviously we wish to mean more than this by 'objectivity'. ... Objectivity as mere consensus is simply too cheap. The threat to our imaginations and to progress seems, therefore, to emanate entirely from Bloor's corner.'⁴³

DeWitt makes the further point that Bloor's charge that World 3 is opaque and inaccessible to investigation arises from the latter's commitment to a materialist ontology and to his unexpressed presupposition that a minimal ontology is always desirable. He recommends that we ought, instead, to favour ontological boldness - a companion to Popper's epistemological boldness - 'provided we have relatively codifiable procedures for assessing ontological posits.'⁴⁴ And he replies that

41 Bloor, D. (1973) p17

42 " " " "

43 DeWitt, Larry W. (1975) p206

44 " " " " p202

what vagueness applies to World 3 applies with equal force to the physical world: 'What exactly is World 1? What mode of being does it have?'⁴⁵

Are there agreed criteria for assessing ontological posits? And does World 3 meet those criteria? If we make the criteria sufficiently stringent it will always be possible, I suppose, to exclude posits which we do not like. We saw that Popper advanced three arguments in favour of the existence of World 3 namely, his thought experiment, autonomy and the causal efficacy of World 3 on World 2 and, indirectly, on World 1. Taking these in reverse order, I have already said that I think it possible that to talk of World 3 causing effects is likely to mislead and that we can reformulate this claim in terms of World 2 discovering something unforeseen or unintended in World 3; in other words, World 3 does not alone cause any effects. But I do think, however, that the argument for World 3's reality by virtue of its autonomy is sound and that abstract, logical consequences of theories are there to be discovered; and they have been discovered, as Popper insists. As for Popper's thought experiment I think his point can also be subsumed under the autonomy argument. Thus I am prepared, tentatively, to conjecture that World 3 is as real as the other two.

Popper's claim, then, is that World 1 is incomplete and that it is, in principle, impossible to give a complete causal account in terms of physical causes. World 1 is open to the intervention of the human mind, World 2. On that conjecture it will, therefore, be impossible to give a complete causal account of human action if we rely on any account which excludes, in principle, any interaction by World 2. If the conjecture is, in fact, correct then mentalism will (together with even more spartan

45 DeWitt, Larry W. (1975) p203

strategies, like, say, radical behaviourism) be misconceived. If Popper is correct then, as Beloff argued (for different reasons), it will be impossible to provide an explanation of human action in physical terms - where that is taken to imply mentalism, behaviourism and non-interactionist approaches in general.

Conclusion

A central theme of this thesis has been the claim that different approaches to psychology are legitimated by different philosophies of science or wider philosophical perspectives. I have argued that Popper's philosophy of science, as modified by Maxwell, provides a reasoned and tenable account of the rationality of scientific enquiry and equips us with suitable criteria for the assessment of the rival arguments over whether psychology is viable and, if viable, how it ought to be pursued. In the light of these criteria I have endorsed the mentalist programme as an, in principle, rational enquiry provided it recognises the limits of empirical science and does not try to settle epistemological questions. Mentalism aims to provide an account of the necessary and sufficient conditions required for the explanation of human actions and so promises a mechanistic, causal account of the determination of actions. In view of the earlier discussion of indeterminism in the context of interactionism I should, perhaps, point out that the truth or falsity of determinism as a metaphysical thesis is of little concern to mentalist psychologists. If determinism is true then there can be no objection, in principle, to a search for an explanation of action in accordance with the mentalist strategy. Even if we are persuaded of the truth of metaphysical indeterminism there is no logical absurdity in pursuing the mentalist research programme. For,

as Lucas has neatly argued, it may be quite profitable for such an indeterminist to adopt determinism as a methodological principle.⁴⁶

He writes:

'Even if I know I shall not be able to find a scientific explanation of every event, I do not know that I shall not be able to find one of this event, and therefore it is reasonable to go on looking, since to abandon the search is to resign myself to failure, while to continue is not necessarily fruitless.'⁴⁷

There is no contradiction, as he puts it, in a Methodist lay-preacher who, on Sundays, preaches about sin and salvation - and assumes the freedom of the will - donning, on Mondays, his neurophysiological hat and assuming that every mental event has a physical cause. In summary, then, we can say that if mentalists should proceed to devise cognitive theories which enable them to explain, predict and control behaviour they will have turned psychology into a successful natural science in the sense in which any science, given Popperian arguments, may be called successful.

However, mentalism is not the only legitimate approach to psychology. It is at least possible that Popper's metaphysical conjectures are true and that the world consists of the physical, the mental and the objective contents of thought. If that is the case, if the further metaphysical thesis of indeterminism is, as is also possible, a true theory and if Worlds 1, 2 and 3 interact in the conjectured manner then we might gain the most valuable insights into the process of human understanding by focusing on the interaction between the mind and its products. Just because Popper's pluralistic philosophy is speculative, unfamiliar and incomplete that is no reason to abandon it before it has been properly developed and its implications for psychology drawn out. Though

⁴⁶ Lucas, J.R. (1970)
⁴⁷ " " " p168

admittedly schematic and novel it does have the valuable feature that the conscious mind is given a central biological function namely, that of interacting with the emergent products of the mind itself. And in comparison with some other forms of interactionism it has what seems to me an equally valuable characteristic namely, the recognition that man, though special, is a part of nature.

In conclusion, therefore, I advocate a methodological dualism of my own endorsing mentalism for those who seek to explain behaviour in terms of natural causes and a Popperian interactionism for those who doubt that man is what the mechanists hope. For, as Popper has said in reply to his physicalist critics Feigl and Meehl, 'there are more things in physics and biology than are dreamt of in all our philosophies.'⁴⁸

48 Popper, K.R. (1974) p1078

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