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THE PHILOSOPHICAL BASIS OF KNOWLEDGE IN SOCIAL SCIENCE: AN EPISTEMOLOGICAL DISCUSSION OF CAUSAL EXPLANATION

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CONTENTS

ACKNOWLEDGEMENTS		5
ABSTRACT		6
	PART 1	
CHAPTER ONE	INTRODUCTION	7
	Philosophy knowledge and science	8
CHAPTER TWO	IS KNOWLEDGE POSSIBLE ?	18
	Epistemological Individualism	21
	a) Objections to Hume's theory of ideas	28
	Wittgenstein's theory of language	30
	a) Objections to Wittgenstein's theory of language	33
	Epistemological sociologism	36
	a) Language and reality	37
	b) The social 'definition' of reality	40
	c) Durkheim's theory of ideas	41
	d) Kuhn's theory of 'paradigms'	44
	e) Popper's theory of 'objective knowledge'	49
	f) Knowledge and science	50
	Conclusion	51
CHAPTER THREE	HOW KNOWLEDGE IS POSSIBLE	53
	Science knowledge and universal laws	55
	Durkheim's formulation of epistemological sociologism	60
	Perception and conception	62
	Conceptions and the concept of truth	66
	Conceptions and language	70
	A refutation of epistemological sociologism	84

	The concept of a Paradigm	88
CHAPTER FOUR	CAUSATION AND EXPLANATION	98
	Paradigms of causation	100
	Paradigms of explanation	111
	a) Covering laws and explanation	111
	b) Causal explanations	117
	c) Durkheim's conception of explanation	121
	Causal explanation and family resemblances concepts	123
	Causal explanation and verification	128
	Conclusion of Part 1	137
	PART 11	
CHAPTER FIVE	THE PHILOSOPHY OF HUMAN BEHAVIOUR	141
	The need for a philosophy of human behaviour	142
	Central-State Materialism	147
	a) Objections to Central-State Materialism	154
	Individual and social 'action'	160
	a) Objections to the causal explanation of human action	167
	Conclusion	173
CHAPTER SIX	THE PHILOSOPHY OF SOCIAL PHENOMENA	179
	The ontological status of social phenomena	180
	a) Social phenomena and 'black boxes'	186
	Social phenomena and causation	189
	a) General and particular causal explanations	189
	b) The fallacy of sociologism	192
	Methodological individualism	203
	a) Methodological individualism and social explanation	204

b) Linguistic methodological individualism 205
c) Ontological methodological individualism 207

CHAPTER SEVEN CONCLUSION 209

APPENDIX FUNCTIONALISM IN SOCIAL SCIENCE 224

BIBLIOGRAPHY 232

ACKNOWLEDGEMENTS



ABSTRACT

The relationship between philosophy, knowledge and science, it is argued, has commonly been misrepresented. Various influential theories of the role of philosophy in the acquisition of knowledge are considered and suggested to be defective. Epistemology is held to be properly concerned with the philosophy of knowledge rather than with the logic, philosophy or sociology of science, which is not necessarily an epistemologically important category; we should attempt to distinguish knowledge from conjecture rather than science from non-science. Contrary to much prevalent opinion, philosophy is said to have an active part to play in the acquisition of knowledge which can be obtained only within a system of philosophical assumptions.

In Part 1, the need for and general nature of such systems is considered and the adoption of one particular system is prescribed. Central to this system is a particular concept of causation and causal explanation. When seeking to understand the world, we are advised to try to discover the causes and partial causes of effects rather than, for example, to try to identify constant conjunctions or universal laws. In Part 11, an attempt is made to apply this prescription in the case of social science; various objections to and problems raised by this attempt are considered in order to present an acceptable framework of philosophical assumptions within which knowledge and explanation of human behaviour and social phenomena might be successfully sought.

PART 1

CHAPTER ONE

INTRODUCTION

I aim to consider the relationship between knowledge and philosophy; I shall consider particularly the role of philosophy in the acquisition of knowledge of social phenomena and human behaviour. This relationship, it will be argued, has generally been misrepresented.

It is a peculiarity of most philosophical theories that their significance can be appreciated only once we have understood the philosophical positions to which they stand opposed. If we say that all swans are white then it is usually perfectly clear what the significance of the claim is; we mean to deny that there exist any non-white swans. But, if someone said, 'Material objects do not exist', then although we would have no difficulty in grasping what the words used mean, we could not understand the full significance of the theory if we did not understand what philosophical assumptions the theory was intended to oppose. The significance of the claim could be discovered only if we realised that it was voiced by someone who denied the existence of an external world and who believed that there exists nothing but 'ideas'. Philosophy offers a variety of alternative interpretations of our experience and no one interpretation can properly be appreciated in isolation; the recognition of an alternative interpretation as an alternative interpretation is a prerequisite of its being adopted as a philosophical theory. Hence much of this thesis will be devoted to the exposition of theories which it is intended to oppose. In this present Chapter, I shall sketch a general background which will highlight the

the significance of that opposition.

Philosophy, knowledge and science

Nowadays, it is widely believed that the philosopher cannot play an active part in the pursuit of knowledge. The philosopher, it is argued, can merely define words and elaborate on the logical relations which arise from his particular definitions; but, it is held that such manipulation of concepts can never produce knowledge of the world; knowledge of the world can only be produced by the scientist who leaves his armchair and goes out and actually observes the world. For example, Rex argues that since the theories of science are not now thought to have the same form as the rational demonstrations of deductive logic but rather to be conjectural and uncertain attempts to explain and predict our experience of the world ' ... philosophers have abandoned any attempt to legislate to the scientist'. (1970, P.2) In this view, the scientist has no need for the philosopher. On the contrary, philosophy is a second-order discipline parasitic upon the activities of science. Rather than prescribing for science, philosophers ' ... turn to the man whose work has led to relatively successful prediction and adjustment, and ask him what his methods and arguments were, in order to discover which arguments and methods really work'. (Rex, 1970, P.2) Hence, the activity of the philosopher must await the prior activity of the scientist; any philosophy of science must be a philosophy _of_ science rather than a philosophy laid down _for_ science. We can have a philosophy of science just as we can have a history of a war but in both cases the historian and the philosopher are essentially _hors de combat_ and play no constructive part in the performance. This is what Schutz means when he claims that 'Methodology is not the preceptor or the tutor of the scientist. It is always his pupil .... In this role, the methodologist has to ask intelligent questions about the technique of his teacher.' (1943)

In a modified version of this account of philosophy, it is held that although philosophy cannot of itself increase our knowledge of the world, it can still be a useful activity.

Philosophy is thought to be useful not as a means of directly increasing our knowledge but in solving conceptual and linguistic problems which might otherwise be obstacles to the scientist in his search for knowledge. Thus, Locke regards himself in the 'commonwealth of learning' not as a 'master-builder' but as an 'under-labourer' engaged in:

> ... removing some of the rubbish that lies in the way to knowledge; which certainly had been very much more advanced in the world, if the endeavours of ingenious and industrious men had not been much cumbered with the learned but frivolous use of uncouth, affected, or unintelligible terms. (Locke, 1964, P.58)

In this conception, the role of language in relation to science is thought to be of crucial importance and the rubbish lying in the way of knowledge is thought to consist largely of linguistic confusions arising from 'Vague and insignificant forms of speech and abuse of language ...'. (Locke, 1964, P.59)

In this interpretation, although philosophy is thought to have a limited usefulness in solving problems that arise in the course of scientific activity, the intervention of the philosopher is thought to be purely ad hoc; he is called for only when some specific occasion demands it. The relationship between the scientist and the philosopher is thought to be somewhat akin to the relationship between a scientist and his doctor: the philosopher is consulted by the scientist only in an emergency and he is consulted only with a view to solving specific problems which the scientist cannot solve for himself. But the healthy and successful scientist is thought to be in no more need of philosophy than he is of medicine. To claim that the required intervention of the philosopher is purely ad hoc is to deny that the scientist needs a systematic philosophical framework within which to operate. Indeed it is regarded as an indication of scientific maturity for a discipline to have broken free from the systems of philosophy. For example, Durkheim writes when considering the distinctive characteristics of sociology as a scientific discipline:

> First it is entirely independent of philosophy. Because sociology had its birth in the great philosophical doctrines, it has retained the habit of relying on some philosophical system and thus has been continuously overburdened with it. It has been successively positivistic, evolutionary, idealistic, when it should have been content to be simply sociology .... Sociology does not need to choose between the great hypotheses which divide metaphysicians .... All that it asks is that the principle of causality be applied to social phenomena. (1964, P.141)

Wisdom presents an interpretation of philosophy which also emphasises its concern with language but which allows the philosopher to be considered as something more than a mere linguistic maintenance man, whose function is to discover and correct deviations from proper linguistic usage and to try to eradicate 'vague and insignificant' forms of speech. He considers that the philosopher can, so to speak, be a linguistic engineer as well as a technician. Wisdom makes the apparently startling claim that most philosophical claims are false, if not even downright ridiculous if they are considered at face value. (See, for example, Wisdom, 1953, Pp. 254-260) Consider a claim such as that we can never actually know the state of another person's mind or, say, that science is the attempt to discover falsifiable but, as yet unfalsified laws. Given what we normally mean by the words 'knowledge' and 'science' both claims are simply false: in everyday life we often quite correctly say that we <u>know</u> how an other person feels or thinks and we correctly classify activities such as, say, social science as <u>science</u> while it is, to say the least, far from obvious that social scientists spend many of their professional hours trying to discover or to falsify universal laws. Are such philosophical theories then completely useless ? The theories are paradoxical because they use familiar words in unfamiliar ways and thereby display apparently startling conclusions: it almost appears as if what we thought of as knowledge is not <u>really</u> knowledge and activities which we had previously been quite happy to call science are not <u>really</u> science. But there is a <u>point</u> to these paradoxical claims. According to Wisdom, we should not simply take such philosophical claims at face value; they are of more interest and use to us if we <u>reinterpret</u> them as necessary truths

which embody suggestions as to how we might modify our use of language. For example we could reinterpret the false claim that we cannot know the mental state of another person as the necessary truth that we cannot 'know' the mental state of another person by redifining the term 'know'. (See Wisdom, 1952) Then the claim that I cannot 'know' John's feelings would be akin to the claim that I cannot be the progenitor of John's offspring or the author of his autobiography. If to 'know' a mental state means to be aware of it in the way in which we are aware of our own mental states then we certainly cannot 'know' the mind of another person. After such a reinterpretation we might find that:

> ... the metaphysical paradoxes appear no longer as crude falsehoods about how language is actually used, but as penetrating suggestions as to how it might be used so as to reveal what, by the actual use of language is hidden. And metaphysical platitudes appear as timely reminders of what is revealed by the actual use of language and would be hidden by the new. (Wisdom, 1953, P.100)

There is a point in saying that we do not know the mind of another person. While we talk of knowing our own mental states and also of knowing the mental states of other people, we might fail to notice that we verify statements about our own feelings very differently from statements about the feeling of another person and that our authority for commenting on our own mental states differs from our authority to comment upon the thoughts and feeling of other people. We could conceivably be mistaken about someone else's feelings, but how could we mistake our own conscious feelings ? These differences are vividly brought to our notice by the paradoxical claim that we can never actually 'know' the state of other minds. But, on the other hand, there is also a point in continuing to say that we can have 'knowledge' of other minds since this reminds us that some of our beliefs regarding other people's mental states are rationally well founded and we might forget this important point if we were to refuse to call such beliefs 'knowledge'. When we reinterpret philosophical claims in this way so that they are considered to express tautologies, it is no longer appropriate to ask whether they are true or false. Conceptual revisions and tautologies are, rather, advantageous or disadvantageous, illuminating or misleading, desirable or undesirable.

On Wisdom's account, philosophy can be a useful activity. When we are brought up within a particular language community it is very difficult not to regard the linguistic usages we learn as some sort of 'brute facts'. One of the most difficult tasks of the philosopher is to persuade us to take seriously the platitude that we could choose to adopt different usages. It is not arbitrary what linguistic usages we adopt since some are more illuminating or misleading than others for various purposes. For the purpose of trying rationally to understand the world, some linguistic usages and habits of thought may be more useful than others even when these usages are all perfectly proper habits of speech i.e. they are not abuses of language. Philosophers can therefore aid us in the acquisition of knowledge by suggesting and helping to foster habits of speech which are fruitful and illuminating just as they may hinder us by suggesting and fostering habits of speech which are misleading and fruitless. Yet, although philosophers might indirectly affect the quest for knowledge, on Wisdom's account, they cannot qua philosophers have an active role in the production of knowledge. Wisdom concludes that 'A philosopher since he has no news and also cannot amuse and amaze us in the way a mathematician can must be either paradoxical or boring'. (1953, P.255) We may note as a point to be returned to that he does not merely say that the philosopher will be paradoxical or boring insofar as he is considered to be presenting tautologies but that the philosopher must be either paradoxical or boring. When he says that the philosopher has no 'news', Wisdom means that the philosopher never conveys any fresh information to us: 'A metaphysician never tells us anything we haven't heard before'. (Wisdom, 1953, P.270) Philosophy may be, in a sense, revealing but it only reveals what we have been looking at all along; it only induces us to see it in a different way. Wisdom allows that there is a sense in which there may be philosophical discovery but this 'discovery' is quite different from scientific discovery which provides us with new knowledge about the world. (See Wisdom, 1953, P.266) After a philosophical discovery, we do not possess any additional information about the world, in Wisdom's view; we merely come to regard our knowledge in a different light.

We regroup particular bundles of facts in different ways, or, at least, we come to see the possibility of such regrouping, but we do not possess any additional facts.

These differing theories of the general scope of philosophy tend, in some respects, to misrepresent the relationship between philosophy and knowledge, as I shall try to demonstrate in the following chapters. Rex and Schutz are, in my view, mistaken when they suggest that philosophy is parasitic upon science. Rex argues that the philosopher looks to the successful scientist in order to discover what types of argument and inquiry are appropriate; but how is the philosopher to recognise successful science ? To say that science has been successful is to say that it has proceeded as it should have done and that it has reached some goal. But surely assumptions regarding the prescriptive standards and goals science should reach are properly considered as philosophical assumptions. The actual activities of the scientist cannot of themselves set prescriptive standards for science. Similarly, Durkheim appears to me to be mistaken when he declares that scientific endeavour is, or, at least, should be quite independent of philosophical assumptions. If science is to be successful, there must be a systematic relationship between scientific theory and philosophical assumptions. I shall try to argue that knowledge of the world logically presupposes a framework of philosophical assumptions. Indeed, we shall see that Durkheim himself has presented one such philosophical framework although it is not one which should be accepted in its entirety.

Philosophy can be useful in the way that Locke and Wisdom suggest; it can help to eliminate linguistic confusions and it can suggest alternative linguistic usages. I aim to apply Wisdom's technique to various issues in the philosophy of science. Yet, I think that philosophy has a far wider scope than this; not all philosophy is concerned with linguistic abuses or the presentation of rival tautologies. For example, although philosophers offer definitions of general terms such as 'knowledge', 'explanation' and 'causation' they do not merely formulate

tautologies; they try to persuade other people to accept them. What definitions of the terms 'knowledge', 'explanation' and 'causation' should we adopt ? This is quite a common type of philosophical question. Yet, even if the question is about tautologies, the question itself is hardly a tautology and nor could it rationally be answered by one. I do not think that Wisdom would intend his analysis of philosophy which I have quoted to be an account of all philosophical claims although this might appear to be implied by his statement that the philosopher must be either paradoxical or boring since, qua philosopher he is or can be interpreted as a presenter of tautologies. That the presentation of tautologies does not exhaust the activities of philosophy might be suggested if we were to apply Wisdom's type of analysis to the philosophical claim that the philosopher qua philosopher presents tautologies and is therefore paradoxical or boring. Is this philosophical claim itself a tautology ? If it is not, then not all philosophical claims are interpretable as tautologies. If it is a tautology we could still claim that not all philosophical claims (in the usual meaning of 'philosophical') are interpretable as tautologies since Wisdom's claim might involve a redefinition of the term 'philosophy' so that it becomes a necessary truth that 'philosophy' is interpretable as dealing merely in tautologies. As I shall try to show, there are claims which, in terms of current linguistic usage, are properly considered as philosophical claims and yet which are not interpretable as tautologous, nor are they concerned with correcting linguistic abuses. Some philosophical claims and assumptions are either true or false where such truth or falsity does not arise merely from the definitions of the terms used to express them; and some philosophical claims and assumptions can be known to be true or known to be false. Some such assumptions, notably certain types of prescriptive, interpretative, and evaluative assumptions are indispensable to our rational understanding of the world.

Philosophy plays an active part in our acquisition of knowledge of the world. Philosophical assumptions are logically prior to such knowledge which is obtainable only within a frame-

work of systematically related philosophical assumptions. In Part 1, I shall mainly be concerned to show the need for and illustrate the nature of such a philosophical framework while in Part 11, I shall concentrate on a particular philosophical framework which, it will be suggested, might profitably be adopted in social science. Central to Part 11 will be a discussion of the question of why and how we should try causally to explain human conduct and social phenomena.

Before trying to fulfil these tasks, it might be appropriate to try to indicate what I mean by the terms 'philosophy', 'knowledge' and 'science' particularly since I aim to consider the philosophy of knowledge rather than to consider the philosophy of science, which nowadays is the more conventional approach of epistemologists. It might also be worthwhile to mention two caricature pictures of philosophy and science which have been fairly influential in philosophy and which have tended to cause confusion. Philosophers have traditionally heavily emphasised the distinction between analytic and synthetic statements and the distinction between deductive and inductive arguments. An analytic statement is one such as 'All bachelors are unmarried'. (See Ryan, 1970, Pp.27-43) This can be known to be true simply by virtue of the meanings of the terms it contains. This is distinct from a synthetic statement such as 'Bachelors tend to smoke' which could be known to be either true or false only by actually observing bachelors to see whether or not they tend to smoke. In a deductive argument, the conclusion logically follows from the premises of the argument. For example, we could deduce that if Ian Smith is a bachelor then Ian Smith is unmarried. This is distinct from the type of argument we would use to try to demonstrate that Ian Smith tends to smoke less than other bachelors do; we would have to support this type of conclusion on the basis of empirical evidence, noting how much he tends to smoke and noting how much bachelors in general tend to smoke. This, in many ways useful distinction between deduction and induction and between analytic and synthetic statements has tended to foster caricature pictures of philosophy and science whereby there is considered to be a radical gulf between

philosophy, thought to be concerned with deductive arguments and analytic statements which cannot of themselves increase our knowledge of reality and science which is thought to be concerned with synthetic statements, inductive arguments and the increase of our knowledge of the world. These caricature pictures rest on the assumptions that meaningful statements are either analytical or synthetic and that rational arguments must be either deductive or inductive and these are assumptions which have been disputed, notably by Wisdom. (See, for example, Wisdom, 1953, Pp. 266-270) If Wisdom is correct, and I think that in the following chapters we shall see that he is, then we need not accept that philosophy and science are fundamentally radically different types of activities. We might quite correctly consider them to be distinguishable yet very closely related types of activities particularly if it is the case that some philosophical statements and also some scientific statements are neither analytic nor synthetic and also that some philosophical and scientific arguments are neither inductive nor deductive.

I aim to use the terms 'philosophy, 'science' and 'knowledge' in the way that people normally use them when they are not trying to make some philosophical point by restricting their scope. The term 'knowledge' is the easiest to define. By 'knowledge' is meant true, justified belief as opposed to opinions, prejudices, myths, conjectures and falsehoods. It would be far more difficult to express the usual meanings of 'philosophy' and 'science' but this need not prevent us from actually distinguishing philosophy from science. As we normally use these terms, philosophical activities and assumptions form a 'family' and scientific activities and theories form a 'family' in the Wittgensteinian sense of the term 'family' which will be explained later. I think it will be allowed that the questions discussed in the following chapters are, in the usual sense of the term, philosophical even if we are not able completely to articulate what this normal usage is. When we consider the writings of Durkheim for example, we will generally have no difficulty in deciding which of his theories are sociological and scientific and which are properly considered as philosophical.

My aim is to discuss the presuppositions of our knowledge of the world. For this purpose there is no need rigourously to stipulate a redefinition of 'science' or even to focus attention upon the activities and theories of those who, in the usual sense of the term would be called scientists. Not all knowledge of the world is scientific knowledge and there is no necessary epistemological cleavage between scientific and non-scientific knowledge. The presuppositions of non-scientific knowledge do not necessarily differ from the presuppositions of scientific knowledge. We can know, for example, that many of the buildings in Aberdeen are made from granite and I can know that there is a pound note in my wallet where this would hardly be classed as scientific knowledge. The amateur bird-watcher might know more than the professional scientific ornithologist. It would be false to imagine that scientific theories are necessarily particularly well attested. Some beliefs remain non-scientific beliefs simply because they are so well attested that they do not require the scrutiny of professional science. We all know 'the facts of life'. They are very well attested although they are not particularly attested by scientists. Although they are known facts, we would not normally call them scientifically known facts. I am twenty five years old; I know that I am. How many scientific claims could be voiced with such justified confidence ? Epistemology is properly concerned with the philosophy of knowledge rather than merely with the philosophy of science. There may very well be important sociological differences between scientific and non-scientific attempts to gain knowledge but these sociological differences need not reflect or produce different epistemological principles. Since the epistemological status of a belief is independent of its sociological pedigree it is quite appropriate to consider the philosophy of knowledge quite independently from the sociology of science.

CHAPTER TWO

IS KNOWLEDGE POSSIBLE ?

Perhaps the most basic presupposition of our knowledge of the world is that it is possible for us to attain such knowledge. As we shall see this is not an insignificant nor indeed an uncontroversial assumption. Explicitly to claim that we know, say, that smoking can tend to cause cancer is implicitly to claim that it is possible for us to know. If the philosophical claim that knowledge of the world is possible is false then any claim to know specific facts about the world will also be false. Knowledge is a particular type of justified belief. How could we justifiably claim to know any specific facts about the world if we could not justify the logically prior claim that knowledge is possible ? How could we justify the philosophical claim that knowledge is possible ? It does not appear as though we could rationally justify or confute this claim either by a deductive or a straightforward inductive argument. It does not appear to be necessarily true in the way that 'All bachelors are unmarried' is necessarily true; it does not appear to be necessarily false like the claim: 'Some bachelors are married'. Simple induction would hardly justify the assumption. It would not do simply to say that we know that smoking can tend to cause cancer and we know 'the facts of life' hence knowledge is possible since whether we actually could know such things is precisely the point at issue. Similarly, we would hardly refute the claim inductively by showing that some things are unknown or that often when we have claimed knowledge we have actually been quite mistaken. Yet, as we shall see it is possible to argue rationally for and against the claim.

In this chapter, I would like to consider two general ways of trying to indicate how knowledge is possible. In one account, knowledge is considered to be the property of individual human knowers and in the other, knowledge is considered to be a social product whose existence and epistemological status are quite independent of the ignorance, belief and general mental states of individual people. We could call both approaches epistemological individualism and epistemological sociologism respectively; 'sociologism' is an inelegant term but it avoids the political connotations of the more natural term 'socialism'. Both approaches seem initially to be plausible but, as we shall see, neither individualism nor sociologism seems able to account for the possibility of knowledge. Indeed, it appears that if we adopt either approach then we must conclude that knowledge of the world is not possible. How this conclusion might be avoided will be considered in the next chapter.

Epistemological individualism is characterised by the common-sense view that all knowledge must be somebody's knowledge; nothing could be known if it were not known by specific people. We shall find such a view implicit in Hume's empiricism where the basic data of knowledge are considered to be 'impressions' and 'ideas', which are the property of individual minds. Locke, the founding father of modern empiricism similarly takes epistemological individualism for granted and casts his analysis in terms of the relationship between the individual knowing subject and the objects of his knowledge. Thus, he writes:

> Since the mind in all its thoughts and reasonings hath no other immediate object but its own ideas, which it alone does or can contemplate, it is evident that our knowledge is only conversant about them .... Knowledge then seems to me to be nothing but the perception of the connexion and agreement, or disagreement and repugnancy of any of our ideas. In this alone it consists. Where this perception is, there is knowledge, and where it is not, there, though we may fancy, guess, or believe, yet we always come short of knowledge. (1964, P.320)

Knowledge is considered to be a particular kind of mental product; it is the recognition of a logically necessary relationship. We might want to call it a particularly well

founded belief had he not specifically contrasted belief, which could conceivably be false with 'knowledge', where that which is 'known' could not conceivably be false.

On this account, thoughts, ideas and knowledge are the mental products of individual people and since language is, in an obvious sense, a social phenomenon, Locke has the task of explaining the relationship between thought and language. Locke makes a sharp distinction between ideas and thoughts, which are mental phenomena, and language, which is a social device whereby we record and communicate our ideas and thoughts. Language is considered by Locke to be the external sign of our internal thoughts. By social convention, certain words become the mark or sign of particular ideas:

> Words in their primary or immediate significance, stand for nothing but the ideas in the mind of him that uses them .... When a man speaks to another, it is that he may be understood; and the end of speech is that these sounds, as marks, may make known his ideas to the hearer. (Locke, 1964, P.259)

Epistemological sociologism also has a certain common-sense plausibility. After all, most of the knowledge that any person has was not gleaned from his own sensory experience. We know about the battle of Hastings because we were told about it at school; similarly, we know that atoms can be split although we are unlikely ever to have split one ourselves. What do we generally do if we want to know some specific fact ? Well, we might have an introspective examination of our own 'ideas' but it would generally be far more sensible to consult the common stock of knowledge available at the local library. Nevertheless, before we can fully appreciate the significance of saying that knowledge is essentially social, we must consider epistemological individualism in more detail and be aware of the difficulties to which the theory appears to lead.

Epistemological Individualism

As an example of an individualistic epistemology, I want to consider the empiricist philosophy of Hume although I do not mean to imply that all empiricist theories are necessarily individualistic nor, indeed, that all individualistic theories are empiricist. The two elements which I particularly want to emphasise in Hume's philosophy are the assumptions that all mental content is derived from perception and that the meaning of language is such mental content.

Hume argues that ' ... there is nothing ever present to the mind but impressions and ideas ...'. (1969, P.393) and all ideas are regarded as being images, akin to photographs or pictures in the gallery of the mind. Hume presents a theory of all mental content. Ideas differ in degree of 'force and liveliness' from impressions; they are the fainter and fading images of impressions. In Locke's use of the term, 'ideas' include both Humean 'ideas' and 'impressions'. What Hume means is this. When we are actually looking at something what appears in the mind is an impression; when we close our eyes and recall the scene, what appears in our mind is an idea. The impression need not be the product of sight, of course; it may be a product of hearing, touch or any of the senses. The distinction between impression and idea is that between mental content as an immediate sensory experience and mental content as a replica of such a prior experience. The distinction is not, as we might suppose, the distinction between feeling and thought. Thoughts and feelings might be either ideas or impressions. All ideas are said to be derived from impressions which they replicate in every respect excepting degree of force and liveliness. In the standard example, if we had never looked at something which is red, we could have no idea of 'red': thus the congenitally blind could have no idea of 'red'.

Our idea of red is, according to Hume, a replica of our prior impression of red. He considers that some of our ideas, our 'complex' ideas are not so simply formed and do not correspond to prior impressions. For example, we have never

experienced an impression of Glasgow or London. Hume considers that such 'complex' ideas are ultimately derived from impressions. He argues that they are formed from simple ideas such as, say, the idea of Buckingham Palace or Big Ben which are derived from and are replicas of prior impressions. He also makes a further distinction between impressions of 'sensation' and those of 'reflexion' what he sometimes calls 'internal' impressions:

> The first kind arises in the soul originally, from unknown causes. The second is derived in a great measure from our ideas, and that in the following order. An impression first strikes upon the senses, and makes us perceive heat or cold, thirst or hunger, pleasure or pain of some kind or other. Of this impression there is a copy taken by the mind, which remains after the impression ceases; and this we call an idea. This idea of pleasure or pain, when it returns upon the soul, produces a new impression of desire and aversion, hope and fear, which may properly be called impressions of reflexion because derived from it. (Hume, 1969, P.55)

Hume's claim (a claim which he considers to be generally, although not quite invariably true) is that all mental content is, or is derived from perception. (See Hume, 1969, Pp.49-73)

According to Hume, the meaning of language is mental content. He accepts Locke's view that words ' ... stand for nothing but the ideas in the mind of him that uses them'. (Locke, 1964, P.259) What we mean by the word 'red', for example, is our idea of 'red', according to Hume. Had we never had an impression of 'red' then we could never use the word 'red' meaningfully. Hence he writes that 'When we talk of self or substance, we must have an idea annex'd to these terms, otherwise they are altogether unintelligible'. (1969, P.675) In essence, this is a precursor of the 'verification principle' whereby an attempt is made to relate all meaningful statements to sensory experience. This theory is very relevant to the activity of science. Consider, for example, some of the terms that social scientists use: 'society'; 'social action'; 'social class'; 'social norms'. What ideas, if any, are annexed to these terms ? Have we ever perceived a society; can we discover in our minds an impression of a social class ? Do we have a complex idea of a society or a social class;

if so, from what impressions is it ultimately derived ? Are such terms perhaps unintelligible ? Before trying to answer these questions, let us consider some other aspects of Hume's philosophy.

He considers that:

> There is an evident absurdity in pretending to demonstrate a matter of fact or prove it by any arguments a priori. Nothing is demonstrable unless the contrary implies a contradiction. Nothing that is distinctly conceivable implies a contradiction. Whatever we can conceive as existent we can also conceive as non existent.
> (1935, Part 1X, P.232)

Consider any statement of fact, say, 'All cows are brown'. Now, deny it: 'Not all cows are brown'. Since neither the assertion nor the denial of the claim is contradictory, the statement can neither be proved a priori to be true nor proved to be false, according to Hume. In a sense, it sounds extremely strange to say that we cannot logically prove a statement of fact since, after all, statements of fact feature as premises and conclusions in deductive arguments. Suppose we assume that all acids are corrosive and that liquid X is an acid. From this we can deduce that liquid X is corrosive. Would this not be a logical proof of the statement of fact, 'Liquid X is corrosive' ? Hume would argue that it is not. He would argue that such proofs are conditional proofs. What we can logically prove is that if all acids are corrosive and if liquid X is an acid, then liquid X is corrosive; but this is not logically to prove that liquid X is corrosive. A peculiar feature of deductive arguments is their apparent 'all or nothing' nature. A conclusion can be deduced from its premises or it cannot be; a statement is either contradictory or it is not contradictory, it would appear. Hence, Hume concludes that deductive reasoning cannot favour either acceptance or rejection of a statement of fact since it is not contradictory either to assert or deny a statement of fact. Consider a statement such as 'All swans are white'. Popper can be interpreted as trying to suggest that although such a statement could not be proved to be true, it could be proved to be false; the observation of even one non-white swan would be sufficient to refute or 'falsify' the statement. This is the basis for his conception of 'science':

' ... it must be possible for an empirical scientific system to be refuted by experience ...'. (Popper, 1968, P.41) Hume would not accept that we can refute a statement of fact where by 'refute' we mean 'logically disprove'. He would argue that we can deduce that if a non-white swan exists or if a non-white swan has been observed then the statement 'All swans are white' is false; but this is not logically to prove that the statement is false just as to deduce the statement from the assumption that swans exist and no non-white swans exist is not logically to prove it to be correct. We cannot logically prove that a non-white swan exists or has been observed, according to Hume, since it is not contradictory to deny it.

Hence, since, apparently, deductive reasoning cannot establish matters of fact, we must look for some other avenue to knowledge. We naturally look to experience and observation, particularly to observation of causal sequences since through a knowledge of causes we hope to bridge the gap between a knowledge of observed and unobserved events. But again Hume's conclusions appear disappointing. Hume asks, 'What does it mean to say that A caused B ?'. In accordance with his theory of meaning and his theory of ideas, he attempts to trace causal claims to their roots in sensory experience. Hence he asks, 'What do we perceive when we perceive a cause and an effect ?'. He concludes that we perceive that the cause and effect are contiguous and that the cause is prior to the effect. But, when we say that A caused B we have some notion that A necessitated the occurrence of B. As Hume says: 'There is a Necessary Connexion to be taken into consideration; and that relation is of much greater importance than any of the other two above-mention'd'. (1969, P.125) Most of Hume's analysis of causation is devoted to accounting for this notion of necessity.

We might think that this idea of necessity was simply a realisation of logical necessity but Hume swiftly dispels this view. He applies his maxim that we cannot logically prove matters of fact; causal claims are matters of fact. Firstly,

we cannot logically prove that an event requires a cause in order to take place. Secondly, we cannot logically demonstrate that any particular cause must produce a particular effect. A priori anything might cause anything else. For example, smoking might cause cancer but that smoking causes cancer is not a logically necessary truth. There is no contradiction in saying that smoking causes the measles or that smoking causes us to run faster, although both claims are false. And similarly we cannot logically demonstrate that any particular event must be the result of a particular cause.

Whence the idea of necessity ? Hume seeks a precedent impression of this idea and cannot find one. Suppose we observe two billiard balls in collision and we say that the one previously at rest was caused to move by the one which was in motion. What do we observe ? According to Hume, we observe only the motion of the balls. We observe only that the cause and effect are contiguous and that the cause is prior to the effect. We cannot perceive the cause necessitating the occurrence of the effect. We cannot observe that, given the cause, the effect somehow had to happen; we can merely perceive that it did happen. Hume gives a psychological explanation of our idea of necessity. He concludes that since there is no impression conveyed by the senses which could produce this idea of necessity, 'It must, therefore, be derived from some internal impression, or impression of reflection'. (1969, P.216) From an experienced 'constant conjunction' of a cause with its effect, when we experience an impression of the cause we come to expect an impression of the effect. According to Hume, it is from this expected constant union that our idea of a necessary relationship arises. Hence his conclusion that 'Upon the whole, necessity is something, that exists in the mind, not in objects ...'. (1969, P.216)

It is important not to get confused between Hume's psychological and his epistemological conclusions. The epistemological significance of his psychological account of necessity is this. Since we can have no impression of causal

necessity from observing a causal relationship, the claim that causes physically necessitate the occurrence of their effects cannot be intelligible, in Hume's view. In more modern parlance, we might say that the claim that causes necessitate the occurrence of effects is meaningless or 'metaphysical'. What Hume's analysis purports to demonstrate is that when we say that A causes B, all that we can meaningfully claim is that A and B are contiguous in time and space; that A is prior to B; and that A and B are constantly conjoined.

This analysis of causation leads us to Hume's theory of induction. Hume argues that we experience merely ideas and impressions; our reason can demonstrate no necessary relationship between these ideas and impressions. Suppose we experience a constant conjunction between A and B, say between smoking and cancer. From this experience of conjunction, what valid conclusion can we reach regarding unobserved instances of smoking and cancer ? According to Hume, we can draw no such valid conclusions. From the statement 'A and B were constantly conjoined on X occasions', we cannot validly conclude anything regarding the conjunction of A and B on occasions other than these particular X occasions. Hume's case is simple and compelling:

> ... there can be no demonstrative arguments to prove, that those instances, of which we have had no experience resemble those, of which we have had experience. We can at least conceive a change in the course of nature; which sufficiently proves, that such a change is not absolutely impossible. (1969, P.137)

At this stage, people sometimes say that although from an experience of a finite conjunction of, say smoking and cancer we cannot know for certain that cancer and smoking will be conjoined in the future, we can validly conclude that they will probably, be so united. To say this is to have misunderstood the entire problem of induction, according to Hume. As he points out:

> ... probability is founded on the presumption of a resemblance betwixt objects of which we have had experience, and those, of which we have had none; and therefore 'tis impossible this presumption can arise from probability. (1969, P.138)

If we accept Hume's assumptions, must we not conclude that it is not possible for us to attain systematic knowledge of the world since neither inductive nor deductive arguments appear to give adequate grounds for either the acceptance or rejection of statements of fact ? How can we have a rational preference for any belief rather than its opposite ? For example, if causal claims cannot be substantiated by induction or deduction, how can we answer Hume's claim:

> ... that all our reasonings concerning causes and effects are deriv'd from nothing but custom; and that belief is more properly an act of the sensitive, than of the cognative part of our natures (1969, P.234)

how can we reasonably argue that we know our causal claims to be true ?

Two particular problems arise if we consider 'impressions' and 'ideas' to be the basic data of knowledge. We want to try to show how it is possible for us to attain knowledge of a world whose existence is, in some sense, quite distinct from and independent of our perception of it. Yet, if we are acquainted merely with 'impressions' and 'ideas' it appears impossible for us to emerge from the net of our mental content and gain knowledge of a reality external to such mental content. Indeed, on Hume's account, the notion of an 'external world' existing independently of our perception of it is held to be quite unintelligible. How could we possibly have a Humean 'idea' of a world external to and independent of our 'ideas' and 'impressions' ? (See Hume, 1969, P.239) In terms of Hume's assumptions, talk of such an external reality appears to be meaningless. Secondly, how could we systematically relate our fleeting 'impressions' and 'ideas' to form a coherent body of knowledge applicable to our past and future as well as to our present experience ? Our beliefs are not the fruits of our reasoning, according to Hume but basically the non-rational product of our psychological constitution:

> Without this quality, by which the mind enlivens some ideas beyond others (which seemingly is so trivial, and so little founded on reason) we cou'd never assent to any argument, nor carry our view beyond those few objects,which are present to our senses. Nay, even to these objects we cou'd never attribute any existence, but what was dependent on the senses; and must comprehend them entirely in that succession of perceptions, which constitutes our self or person. Nay, farther, even with relation to that succession, we cou'd only admit of those perceptions, which are immediately present to our consciousness, nor cou'd those lively images, with which the memory presents us, be ever receiv'd as true pictures of past perceptions. The memory, senses and understanding are, therefore, all of them founded on the imagination or the vivacity of our ideas. (Hume, 1969, P.313)

a) Objections to Hume's theory of ideas

We could avoid Hume's sceptical conclusions if we were to reject some, or all of his premises. For example, we could deny that all valid arguments are deductive, that all mental content is either impressions or ideas or that the meaning of language is the mental content of language users. Consider, first of all, the second of these assumptions.

If all mental content was either impressions or ideas, then not only would we lack justified beliefs, we would have no beliefs at all. Hume considers beliefs to be particularly enlivened ideas but this theory is totally implausible. Consider any idea, for example the idea of 'red'. No matter how 'enlivened' such an idea of red might be, it could never of itself constitute a belief. If we consider any belief relating to this idea of red - 'This is an after image' ... 'I am looking at a patch of blood' - then the belief goes beyond the experience of the idea of red. Beliefs can be true or false and different beliefs can contradict each other. No impression or idea could be true or false. The smell of a rose may be pleasant or unpleasant but it would make no sense to say that the experience of smelling a rose was either true or false. And similarly, the experience of smelling a rose may differ from the experience of smelling a thistle but we could not say that the impressions were contradictory in the sense that the

belief that it is now Monday morning contradicts the belief that it is Saturday night.

If all mental content was either impressions or ideas then we could never recognise this content as either impressions or ideas and we could never distinguish impressions from ideas. To classify phenomena is to suggest a relationship between them which could never be derived from any impression of any one of them or of any number of them. For example, it is to suggest a relationship of similarity, a relationship of common class membership and the sharing of one or a variety of features or characteristics. Suppose we have the impression of a row of dots ..... We can conceive this as a row or as a collection of dots yet neither conception could be reduced to an impression of a series of impressions of the dots. Stare as we may, the words 'dot' and 'row' cannot be derived from looking at the dots yet we realise that they are appropriate words to use. We recognise a row and we recognise dots and the conception of 'row' and of 'dot' is logically prior to the recognition of rows and dots. If all mental content was either an impression or an idea, then our experience of seeing or hearing (or remembering having seen or heard) the words 'dot' and 'row' would be an impression or an idea. How then could we realise that the word 'dot' refers to actual dots ? We would be just as likely to imagine that the impression of a dot referred to or was the name of the impression of the word 'dot' as to imagine that actual dots were known as 'dots'. If Hume's theory of 'ideas' and 'impressions' was correct, then our thoughts could be neither true nor false and we could not name or classify the objects of our experience. Hence we have good reason for rejecting Hume's theory.

Before returning to the question of whether only deductive arguments are valid, I would like to consider Wittgenstein's theory of language as this provides an important impetus for epistemological sociologism. Wittgenstein rejects the assumption that we should consider the meaning of language to be any sort of 'ideas' in the minds of language users and he

also tends to reject Locke's emphasis upon a sharp cleavage between thought and language. Wittgenstein considers thoughts to be in language rather than something independent of language.

Wittgenstein's theory of language

His theory is based on two related props; that the meaning of a word is, or often is its use in particular 'language-games'; and that a private language is a logical impossibility where a private language is one in which the meanings of words are objects to which individual people have a private access. We could paraphrase the theory by saying that words could not be meaningful unless there was a way of telling when they were being used correctly, otherwise we could never teach children the use of words. Hence words could not be meaningful unless there were public criteria governing their use and at least for a large number of cases, ' ... the meaning of a word is its use in the language'. (Wittgenstein, 1958, P.20:43) Using words correctly is logically related to the notion of following a rule. 'To understand a sentence means to understand a language. To understand a language means to be master of a technique.' (Wittgenstein, 1958, P.81:199) The relevant technique is the ability to obey the rules of the particular 'language-game'. Obeying a linguistic rule is a mechanical type of act. 'When I obey a rule, I do not choose, I obey the rule blindly.' (Wittgenstein, 1958, P.85:219)

To follow a rule, so the argument runs, it must, in principle, be possible to check whether the rule is being followed and such a check must be in terms of publicly observable phenomena. It must be possible to observe events which would count as breach of the rule. If everything will count as conformity with a rule then it makes no sense to talk of conformity with a rule. Following a rule is related to the logical possibility of making a mistake and, in Wittgenstein's view, it must in principle be possible to discover when a mistake has been made. I take his point to be that without such a public check, there would be no difference between following a rule and merely mistakenly imagining that one is following a rule.

Hence, in Wittgenstein' view, there could not be a language where the meanings of words are phenomena to which we have a private access, such as sensations or emotions; there could be no rules governing the use of such words since there could be no public check on whether they are being used correctly. If mental terms such as 'pain' or 'pleasure' are to have a use and thereby a meaning, they cannot be the names of the unobserved mental accompaniment of 'pain-behaviour' and 'pleasure-behaviour'. It is only by virtue of such publicly observable events as tears or smiles that sensation words like 'pain' and 'pleasure' can have a linguistic use. Wittgenstein does not deny the existence of private sensations accompanying observable behaviour or attempt to define 'pain' as crying or 'happiness' as smiles. Of such accompanying sensation he says: 'It is not a something but not a nothing either ! The conclusion was only that a nothing would serve just as well as a something about which nothing could be said.' (1958, P.102: 304) He offers this as an illustration: imagine that everyone had a box with something in it but that no one could look into anyone else's box (cf. our private experience of mental states). (See Wittgenstein, 1958, P.100) If the word 'beetle' were introduced to refer to whatever was in our boxes and this word had a use in our language, then the meaning of the word 'beetle' would be completely unaffected by whatever happened to be in our boxes and whether our individual 'beetles' were similar or not. Similarly, if we regard 'pain' as the name of the sensation which we experience when we exhibit 'pain-behaviour', then the meaning of the word 'pain' will be completely unaffected by the nature of this experienced sensation and whether or not one such sensation is similar to any other. It would seem to be misguided to try to classify actions in terms of private mental states or to talk of mental states which somehow lurk behind our overt behaviour.

Wittgenstein argues that when we consider language we should always try to ' ... get rid of the idea of the private object'. (1958, P.207) whether the private object be 'beetles', sensations or 'meanings' lurking behind our use of

language. We sometimes tend to think that language involves two independent processes, firstly that there is thought or mental content or 'meanings' and, secondly that there is language which we use to express this mental content. Wittgenstein denies that there is such a dichotomy between thought and language; it would seem that to be consistent he must voice this denial since mental content or 'meanings' would appear to be private objects and he believes that private objects could be no part of the meaning of words and hence they could not possibly be expressed by such words. Wittgenstein believes that there are not two processes, thought and the use of words; he believes there is only one process involved i.e. the use of words. 'When I think in language, there aren't "meanings" going through my mind in addition to the verbal expressions: the language is itself the vehicle of thought.' (Wittgenstein, 1958, P.107:329) As if to back up this claim, Wittgenstein says: 'Make the following experiment: say "It's cold in here" and mean "It's warm in here". Can you do it ?' (1958, P.140: 510) I take his point to be that we cannot do this and hence we cannot divorce thought from language, the meaning of words from the use of words. If thought simply exists in language then it would seem plausible to argue, as Wittgenstein does, that there cannot be any genuinely profound problems with which the philosopher can grapple; what pass for 'philosophical problems' are merely linguistic puzzles which are to be solved by looking into the workings of our language.

Wittgenstein's theory is extremely relevant to the activities of scientists and perhaps particularly to the activities of social scientists. He writes that:

> 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 beginning. (Rather with that of certain branches of mathematics. Set theory.) For in psychology there are experimental methods and conceptual confusion. (As in the other case conceptual confusion and methods of proof.) (1958, P.232)

Such conceptual confusion might be eliminated, in Wittgenstein's view by applying his analysis of language. Rickman writes that

'Understanding is the grasping of some mental content to which an expression points' and that ' ... its successful conclusion on the highest possible level is the goal of the human studies'. (1967, P.30) According to Wittgenstein's philosophy, the social scientific activity which Rickman recommends would appear to be misguided. Would 'mental content' not be some sort of private 'object' ? How could it play a part in the meaning of words ? How could we 'grasp' anyone else's mental content any more than we could know the nature of his private 'beetles' ? If, as Wittgenstein suggests, thought lies in language rather than behind or beyond language, is it not pointless or meaningless to try to grasp mental content which is thought somehow to lurk behind the use of linguistic expressions ? Social scientists frequently try to classify actions in terms of the mental states and attitudes of the persons involved and they often try to explain actions in terms of mental states such as motives. Should we perhaps conclude that such mental phenomena are private 'objects' and hence cannot be part of the meaning of words ? Can we meaningfully talk about private mental states ?

a) Objections to Wittgenstein's theory of language

If we say that the meaning of a word is an 'idea' in the mind of the user of the word then we might almost tend to think that the best way to discover what is meant by a word is somehow to peer inside the user's skull. Of course, we do not do this nor would this strategy help us to learn a language. We discover the meaning of a word by watching how people actually use the word and learning how to use it properly ourselves. Wittgenstein tends to conclude that the notion that the meaning of a word is an 'idea' in anybody's mind must be mistaken. It is, however, surely rather strange to say that the meaning of a word is its use. Is it not quite simple to imagine instances where meaning and use are distinct ? Can we not give a stipulative definition of a word, say, 'blueno', the word thereby having a meaning but no use since we refuse to tell anyone what the word means ? Similarly, some words have a use but no linguistic meaning. Consider the

expression 'boom-boom' often delivered by comedians such as Basil Brush and Jimmy Tarbuck after pronouncing the punch line of a joke. This expression obviously has a use but, in the usual linguistic sense of the term, it is meaningless. We could not say that its meaning was its use. Jimmy Tarbuck may be aware that some people dislike the expression and he may use it to annoy them but we clearly would not say that 'to annoy them' was the meaning of 'boom-boom'. If the meaning of words is simply 'in the language' and in the use of words, how could we ever distinguish expressions like 'boom-boom' from expressions like 'democracy' or 'I am Scottish' which not only have a use but also have a meaning ? Can we really say that 'Where words ... are concerned, their meaning consists in rules for their correct use' ? (Hudson, 1970, P.162) After all, there could be rules for the correct use of 'boom-boom' - you must say it after the punch line of a joke - just as there are rules for the use of meaningful expressions.

Events which are quite commonplace in our experience would seem totally mysterious if Wittgenstein's philosophy of language were correct. Do we not often know exactly what we want to say yet momentarily forget the appropriate words to express ourselves ? Might we not want to arrange to meet a friend at Piccadilly Circus and yet momentarily forget that it is called 'Piccadilly Circus'; might we not even have the thought of Piccadilly Circus (statue of Eros ... flashing neon lights ... 'Snows' public house ...) present in our mind as we search for the name of the proposed meeting place ? Does this not suggest that our thoughts are, to an extent independent of our expressions of them ? In fact, do we not often do what Wittgenstein seems to imply cannot be done ? We may not be able consciously and purposely to say 'warm' and mean 'cold'; but we often, through a slip of the tongue, mean something other than what is in our language at face value. We might say, 'I am going to Glasgow tomorrow' when what we mean is 'I am going to Edinburgh tomorrow'. Thought and language can diverge; the thought is not just simply in the language. Similarly, we often 'wrongly learn' the use of words especially

when we are children; we misdiagnose linguistic rules. For example, when I was a child, I thought that 'blended' was a mild swear word because I had wrongly learned the use of the word from an advertisement claiming that a particular brand of cigarettes were 'blended better'. Yet, in such cases when we do not realise that we are using words incorrectly we surely mean something by their use. I meant something when, as a child, I said 'I am blended hungry'. In such cases, could we say that the meaning of our words is simply there in the language, in the correct linguistic rules for their use ? Perhaps, in an important sense, words have no more 'meaning' than have tables or chairs. In a sense, only individual people can mean anything by their use of words. And, perhaps, when we talk of linguistic meaning we should distinguish the meaning which a word conventionally has in a given language community from the meaning which particular language users attach to it on specific occasions. (See Grice, 1968)

We meet with difficulties if we try to think of language simply in terms of the use of words and deny that there is an independent thought process accompanying the use of words. For example, suppose we take any word at random; for the illustration, consider the word 'brother'. What does this word mean ? Imagine what would happen if we try to give the meaning of the word simply in terms of other words. We might say that 'brother' means 'male sibling'. What does 'male sibling' mean ? 'Male sibling' means 'brother'. If words only meant other words, then would we not be faced with a verbal circle out of which we could never break ? Suppose I say that X means the same as Y and Y means the same as Z. This will not tell you what any of the letters mean; it will only tell you that they are synonyms. Even if I tell you that they all mean the same as A and 'achly', you still could not know what they mean. And does it not appear that if words were merely defined in terms of other words that we could never know what any words mean; would we not merely know which terms were synonymous ? On the other hand, if we were to agree with Rickman that although mental content can be expressed by language, it is,

in a sense, quite distinct from language, then we may be able to imagine how this verbal circle can be broken. We could then look for the meaning of at least some linguistic expressions in this mental content. We might want to argue that in some circumstances, we are able intellectually to 'grasp' what some words mean. We might then deny that 'brother' means 'male sibling' and say, rather, that the word 'brother' means the same as the words 'male sibling' but that this meaning is something other than mere words and linguistic usages. We shall return to this issue in the following chapter.

Epistemological sociologism

The epistemological sociologism which I want to consider does not form such a unified theory as Hume's empiricism. I want to consider the theories of several writers where these theories share some characteristic features. They tend to stress the social context of knowledge and appear to lend weight to the view that the philosophy of knowledge cannot and should not be considered independently of the sociology of science. Such theories even seem to put the notion of fixed, socially immutable standards of rationality in doubt. With individualism, it appeared as thought we could not hope to attain knowledge of a reality which somehow objectively exists 'out there' because we seem to be trapped within the web of our own individual 'ideas'. With sociologism, we appear to be similarly trapped behind 'ideas' concepts and assumptions although in this case they are considered to be social rather than individual in nature and origin. It looks as though we can never attain knowledge of 'reality as it really exists' if, indeed, we can even talk intelligibly of such a thing. Furthermore, if, as it might appear, we are socially endowed with our concepts, theories and assumptions, can we really choose which concepts, theories and assumptions to adopt ? And, if we cannot exercise such choice, can we sensibly talk of being able to justify our beliefs ? As we shall see, Wittgenstein's assumptions and conclusions have a pivotal role in the theory of epistemological sociologism and, where appropriate his own questions or conclusions will be quoted where they particularly relate to

those of the writers being considered.

Wittgenstein stresses that all reasoning and quests for knowledge take place within a system of language and thought much of which must simply be taken for granted while the reasoning and search for knowledge proceeds. Indeed, there appear to be some assumptions which we could not question without infringing the rules of the 'language-game'. Suppose, for example, that a blind man were to ask whether or not you had two hands. How would you check ? Would you look to see whether two hands appeared to be attached to your arms ? Well, if you did, of what would this be a check ? It depends what is taken for granted during the test. It might be a test of your eyesight or perhaps of your ability to understand English. (See Wittgenstein, 1969, P.18:125) All testing and reasoning takes place within a system of thought and it appears as though we could never step completely outside this system in order to test it:

> All testing, all confirmation and disconfirmation of a hypothesis takes place already within a system. And this system is not a more or less arbitrary or doubtful point of departure for all our arguments: no, it belongs to the essence of what we call an argument. The system is not so much the point of departure, as the element in which arguments have their life. (Wittgenstein, 1969, P.16:105)

a) Language and reality

Winch extends Wittgenstein's theory of language to cast doubt on the view that science can study a reality which somehow objectively exists 'out there'. He argues that we cannot consider science or scientific beliefs independently of the language in which they are expressed. His argument might be paraphrased as follows. What a belief or concept means is its use in language; language usage is an aspect of a form of life; different forms of life are incommensurate and hence so are beliefs expressed in different language communities. For example, Winch argues against Evans-Pritchard who, in discussing Zande witchcraft, says that their beliefs are false; they are not ' ... in accord with objective reality'. Winch's theory is that we cannot talk of 'reality' as something

conceived of as beyond or outside of language. 'Reality is not what gives language sense. What is real and what is unreal shows itself in the sense that language has.' (Winch, 1964, P.309) We have, as it were two 'language games' and the 'language games' are incommensurate. We can compare and evaluate particular beliefs of our culture, say, religious and scientific beliefs in terms of other of our own cultural beliefs; this is a legitimate move in the 'language game'. Moves in a language game can be checked in terms of the rules of that 'language game' but not in terms of the rules of another. There are no extra-linguistic rules for evaluating or comparing entire 'language-games'. Belief in witchcraft may be false in terms of reality as conceived in our language but we cannot thereby maintain that it is false in terms of reality as conceived in Zande language: ' ... both the distinction between the real and the unreal and the concept of agreement with reality themselves belong to our language'. (Winch, 1964, P.309) The opposing beliefs only make sense by virtue of their use in different 'language-games' and different 'language-games', different forms of life are incommensurate.

It is not clear whether or not Wittgenstein would agree with Winch's conclusions. He does say:

> To say of man, in Moore's sense that he knows something; that what he says is therefore unconditionally the truth, seems wrong to me. - It is the truth only inasmuch as it is an unmoving foundation of his language-games. (1969, P.52:403)

And this might lead us to think that he would have been sympathetic to Winch's view. He even envisages a situation where our beliefs clash with those of primitive people. Suppose that we consult a physicist while they consult an oracle. 'Is it wrong for them to consult an oracle and be guided by it ? - If we call this "wrong" aren't we using our language-game as a base from which to combat theirs ?' (Wittgenstein, 1969, P.80:609) He appears here to be making a point similar to that made by Winch but, on the other hand, he sometimes does appear quite ready to allow that different systems of thought are, at least in some respects, commensurate. Writing around 1950, he says that we believe that it is not possible

for us to travel to the moon although it might be that there exist people who believe such a thing to be possible. Of such people he would be prepared to say: ' ... they are wrong and we know it. If we compare our system of thought to theirs then theirs is evidently the poorer one by far.' (1969, P.37:286)

Concepts, according to Winch, are products of social life; indeed, they are contained in language which is a form of social life. For example, the concept of 'man' is the way the word man is used in our particular 'language-game'. As we have seen, we have difficulty in denying that language has a meaning which is in some sense external to language. For example, Gellner considers the problem of translation between language communities. He writes:

> The situation facing a social anthropologist who wishes to interpret a concept, assertion or doctrine in an alien culture is basically simple. He is, say, faced with an assertion S in the local language. He has at his disposal the large or infinite set of possible sentences in his own language. His task is to locate the nearest equivalent or equivalents of S in his own language. (1970, P.121:5)

Gellner's account reflects Winch's in maintaining that there can be nothing external to the languages acting as a bridge between them. 'Naively, people sometimes think that reality itself could be this kind of mediator and "third language" ' but such a ' ... mediating third party is simply not to be found'. (Gellner, 1970, Pp.121:5; 122:5)

Could we not say that what 'mediates' between such different languages is what the particular expressions mean where this meaning is something external to the languages in which it is expressed ? Is the procedure which Gellner outlines logically possible ? Suppose we try to translate a foreign expression S into English. What would be the nearest equivalent to S in English: what English expression would mean the same as S ? Well, we could never discover an appropriate equivalent until we know what S means; but if we knew what S means then we would not have to translate it into English in order to know what it means. If we do not know what S means prior to

translating it into English, then we never could translate it into English since we could never recognise an appropriate English equivalent of S. If we argued that there is a distinction between language and the thought that is expressed in language then we might argue that the truth or falsity of any belief is totally unaffected by the language used to express it. Hence the existence of different language communities need not embarrass us if we claim that science should study a reality which objectively exists 'out there'. I may lament, 'Je suis un perroquet'. How could expressing myself in, say, Gaelic have altered my present state and hence the truth or falsity of the belief expressed ?

MacIntyre uses the following argument to refute Winch's claim that beliefs are somehow embodied in language and hence that beliefs expressed in different language communities are incommensurate:

> Consider the statement made by some Zande theorists or by King James VI and I, "there are witches" and the statement made by some modern sceptic, "there are no witches". Unless one of these statements denies what the other asserts, the negation of the sentence expressing the former could not be a correct translation of the sentence expressing the latter. Thus if we could not deny from our own standpoint and in our own language what the Zande or King James asserts in theirs, we should be unable to translate their expression into our language. Cultural idiosyncracy would have entailed linguistic idiosyncracy and cross-cultural comparison would have been rendered logically impossible. But of course translation is not impossible. (1973, P.31)

b) The social 'definition' of reality

There is a very simple yet popular argument that reaches the Winchian conclusion that reality and truth are somehow 'socially variable' and that we cannot and should not consider the philosophy of science independently of the sociology of science; this argument suggests that to be real is to be socially defined as real; reality is a product of social definition and therefore varies as that definition varies. This argument is very clearly expressed by Berger and Luckman. For example, talking of religious beliefs in Haiti

and of Freudian psychology in western civilisation, they write: 'Neither the Voudun gods nor libidinal energy may exist outside the world defined in the respective social contexts. But in these contexts they do exist by virtue of social definition and are internalised as realities in the course of socialization'. (1971, P.197) Again, they illustrate their conviction that 'Reality is socially defined' (1971, P.134) by a different example. They argue that to have knowledge is to be socially defined as a 'knower':

> ... both "knowing" and "not knowing" refer to what is socially defined as reality, and not to some extra-social criteria of cognitive validity. To put this crudely, maternal uncles do not transmit this particular stock of knowledge because they know it, but they know it (that is, they are defined as knowers) <u>because</u> they are maternal uncles. (1971, P.88)

c) <u>Durkheim's theory of ideas</u>

Durkheim presents a theory of the relationship between thought and society which is in some ways similar to that of Winch's; indeed, in his insistence upon a relationship between forms of thought and forms of social life, Durkheim could properly be regarded as a precursor of Wittgenstein, as has already been noted, for example, by Gellner. (See Gellner, 1970, P.120) Durkheim considers the traditional rival epistemological theories of empiricism and innate ideas; according to the former theory, our ideas are derived from sensory perception while according to the latter, we are by nature born equipped with our mental content just as we are naturally endowed with a heart and a pair of lungs. Concluding that we are led to absurdity when we accept either of these theories which regard 'ideas' as the property of <u>individual</u> minds, he argues that the mental 'categories' which provide the framework of our knowledge are essentially <u>social</u> in nature and origin. (See Durkheim, 1961, Pp.13-20)

Durkheim first presents his theory in <u>Primitive Classification</u> (1963) which he wrote with Mauss but the theory is developed with more precision in Durkheim's

The Elementary Forms of the Religious Life (1961). Durkheim argues that the first logical categories were social categories: the first classes of things were classes of men. He denies that men classify naturally ' ... by a sort of necessity of the individual understandings'. (Durkheim and Mauss, 1963, P.8) The 'categories' (by which Durkheim means general ideas such as time, space, class, number, cause and substances although he sometimes does not distinguish these more fundamental categories from what we would simply call concepts or ideas) are said to be products of social life; they are collective in nature and origin. These ideas have an existence which is independent of their appearance in any individual mind. Society does not merely serve as a model for classification, there is held to be a causal relationship between forms of social life and forms of thought; between social forms and the existence and nature of the categories. Writing of the systems of thought of a particular primitive tribe, Durkheim and Mauss say: 'Changes have taken place in the social structure which have altered the economy of these systems'. (1963, P.32) From the social origins of the categories, it is concluded that the classifications of primitive societies are produced by a logic which differs from our own. 'They are governed by laws which we do not suspect.' (Durkheim and Mauss, 1963, P.21) Hence they conclude that methods of scientific thought, logic and philosophy are ' ... veritable social institutions whose origin sociology alone can retrace and explain'. (1963, P.3)

Durkheim is vehement in his claim that the categories of thought are not the product of any individual minds:

> The nature of the concept ... bespeaks its origin. If it is common to all it is the work of the community. Since it bears the mark of no particular mind, it is clear that it was elaborated by a unique intelligence, where all the others meet each other, and after a fashion, come to nourish themselves. (Durkheim, 1961, P.434)

There is no reason to imagine that Durkheim is talking metaphorically when he says of the categories that ' ... they correspond to the way in which this very special being, society,

considers the things of its own proper experience ... they are the work of society ...'. (1961, P.435) Durkheim specifies that ideas are not merely social but that they are of religious origin. And just as religious ritual is held by him to be imposed upon the individual by society, the categories are similarly imposed: 'The authority of each is derived from the same source: society'. (Durkheim, 1961, P.368) We are socially endowed with our mental content just as surely as we are naturally endowed with a heart and a pair of lungs. According to Durkheim, the categories are axioms ' ... which the mind accepts and must accept, without control and without reservation ...'. (1961, P.369) And, according to Durkheim, we should not imagine that we choose which concepts to accept or that we can rationally evaluate differing socially imposed systems of thought. In such circumstances, we can claim to have knowledge ? Can we claim that our beliefs are, even sometimes, objectively true ? Can we rationally justify our beliefs ? Would such 'justification' not merely be in terms of socially relative standards of rationality ? Would this be 'real' justification ? These are the kinds of questions which Durkheim's views pose for us.

It would be quite interesting to look at Durkheim's application of his theory to the origin of the category of causation; this would give us a better understanding of the theory and it would also be of interest in view of the role that the category of causation will play in later chapters. The category of causation is, according to Durkheim's theory, produced not by individual minds but by society; ' ... the mind is not its artisan'. (Durkheim, 1961, P.367) Durkheim isolates certain features of the concept of causation; there are the notions of efficacy, active force and of power. These notions are held to be socially produced. For example, ' ... the prototype of the idea of force was the mana, wakan, orenda, the totemic principle or any of the various names given to collective force objectified and projected into things'. (Durkheim, 1961, P.363) It is in the image of power in society that physical power has been conceived:

> It is society which classifies beings into superiors and inferiors, into commanding masters and obeying servants; it is society which confers upon the former the singular property which makes the command efficacious and makes power. (Durkheim, 1961, P.366)

Durkheim's theory appears to rest upon a simple error. The existence of power in society cannot simply produce in us the idea of power by offering us a model of social power since, without the prior conception of power, we could never recognise the existence of social power. He writes: 'There are societies in Australia and North America where space is conceived in the form of an immense circle, because the camp has a circular form ...'. (1961, P.11) Now, it would be implausible to say that it was the camp's being in this form which directly produced the idea of circularity. The natives would surely have to observe the camp's shape before their category of space would emerge. But without the categories of space and circularity, the people could not observe that the camp was spatially located in a circle. The conception of circularity is logically prior to the recognition of circularity. Similarly, Durkheim argues that our concepts are not individual but social; but surely any concept must be somebody's concept. For example, consider my concept of a 'haggis'. Now, no doubt if I had been brought up in some other society I might have lacked that particular concept and to that extent the concept is socially induced. Yet, in a very important sense, the concept is my concept. By virtue of possessing this concept, I can recognise particular objects as haggis. Where is 'this very special being, society' which can recognise haggis ?

d) Kuhn's theory of 'paradigms'

Many of the conclusions of Winch and Durkheim are reinforced by the writings of Kuhn. Kuhn's arguments point towards the conclusion that the philosophy of science cannot, indeed should not be considered independently of the sociology of science and perhaps even of the psychology of scientists. He concludes that 'Scientific knowledge, like language, is intrinsically the common property of a group or else nothing

at all. To understand it we shall need to know the special characteristics of the groups that create and use it.' (1970a, P.210) To the question of how science progresses, Kuhn answers that ' ... it should be clear that the explanation must, in the final analysis, be psychological or sociological'. (1970b, P.21)

Kuhn defines 'science' more or less in sociological terms. We have a 'science' when there is a social group sharing a 'paradigm'. This 'paradigm' sets 'puzzles' for the scientific community and the standards of success in solving these 'puzzles'. Such puzzle solving is the normal activity of the scientific group. By 'paradigms', Kuhn means ' ... universally recognised scientific achievements that for a time provide model problems and solutions to a community of practitioners'. (Kuhn, 1970a, P.viii) By 'puzzles', Kuhn means ' ... that special category of problems that can serve to test ingenuity or skill in solution'. (Kuhn, 1970a, P.36) When a communtiy acquires a paradigm, it acquires ' ... a criterion for choosing problems that, while the paradigm is taken for granted, can be assumed to have solutions'. (Kuhn, 1970a, P.37) Since Kuhn assumes that puzzle solving is the normal activity of scientific communities, he argues that a paradigm is a prerequisite of science. Thus, Kuhn argues that astrology is not science because it lacks such a paradigm since without puzzles for a community of astrologers to solve ' ... astrology could not have become a science even if the stars had, in fact, controlled human destiny'. (1970b, P.9)

Kuhn rejects the view that science discovers the truth about some independent reality which exists 'out there'. There is, he argues ' ... no theory-independent way to reconstruct phrases like "really there"'. (1970a, P.206) Science cannot be regarded as a hoard of truth since scientist see 'reality' in terms of their particular paradigms. People with different paradigms see 'reality' differently. Hence, Kuhn compares changes in the accepted paradigms of scientific communities - what Kuhn calls 'scientific revolutions' - with gestalt-switches.

And ' ... paradigm changes do cause scientists to see the world of their research-engagement differently ... we may want to say that after a revolution scientists are responding to a different world'. (Kuhn, 1970a, P.111) We cannot simply appeal to the 'facts', to 'reality' to test rival paradigms since what will appear as the facts and as reality will be determined by our paradigm. We can never step outside of our paradigms in order to evaluate our paradigm. He also believes that the concept of truth is rendered meaningless by the fact that rival theories must be linguistically formulated and observation statements relating to the confirmation of these rival theories must be expressed in language since there is no 'neutral observation language' in which such theories and observations could be expressed. (See Kuhn, 1970c, P.266)

Kuhn suggests that we should abandon the notion of truth in relation to science but he does not feel ' ... that anything is lost, least of all the ability to explain scientific progress, by taking this position'. (Kuhn, 1970c, P.265) Kuhn does not argue that any paradigm is as good or as bad as any other or that rival paradigms are totally incomparable. He does not deny that there can be good reason for preferring one paradigm to another. Yet, such good Kuhnian reasons are essentially sociological rather than epistemological. He argues that 'One scientific theory is not as good as another for doing what scientists normally do'. (1970a, P.264) Scientist, we may recall, normally solve puzzles.

Kuhn shares several conclusions with Wittgenstein and, on many occasions, they seem to be asking very similar questions although, partly because of Wittgenstein's slightly rhetorical style, it is not clear that both men would want to give similar answers to these questions. Like Kuhn, Wittgenstein stresses that all testing of hypotheses takes place within a system of assumptions many of which must simply be taken for granted at least while we are testing these hypotheses:

> "But is there then no objective truth ? Isn't it true, or false, that someone has been on the moon ?" If we are thinking within our system, then it is certain that no one has ever been on the moon ... our whole system of physics forbids us to believe it. (Wittgenstein, 1969, P.17:108)

Wittgenstein, like Kuhn suggests that there are at least some assumptions which we cannot be 'forced' to abandon either by logic or observation:

> Isn't the question this: "What if you had to change your opinion even on these most fundamental things ?" And to that the answer seems to me to be: "You don't have to change it. That is just what their being 'fundamental' is." (Wittgenstein, 1969, P.67:512)

Where Kuhn talks of paradigms, Wittgenstein talks of 'world-pictures':

> But I did not get my picture of the world by satisfying myself of its correctness; nor do I have it because I am satisfied of its correctness. No: it is the inherited background against which I distinguish between true and false. (Wittgenstein, P.15:94)

Again, anticipating Kuhn's arguments, Wittgenstein writes:

> Lavoisier makes experiments with substances in his laboratory and now he concludes that this and that takes place when there is burning. He does not say that it might happen otherwise another time. He has got hold of a definite world-picture - not of course one that he invented: he learned it as a child. I say world-picture and not hypothesis, because it is the matter-of-course foundation for his research and also goes unmentioned. (1969, P.24:167)

According to Kuhn, we tend to 'see' the world in terms of our paradigms. Wittgenstein seems to be making a similar point when, talking of our view that the earth is round, he says: 'We shall stick to this opinion, unless our whole way of seeing nature changes .... Further experiments cannot give the lie to our earlier ones, at most they may change our whole way of looking at things.' (1969, P.38:291,292) Kuhn believes that different paradigms tend, in a sense, to produce facts to support them since we tend to see 'reality' in terms of our paradigms. Wittgenstein raises, yet apparently rejects a similar point:

> Indeed, doesn't it seem obvious that the possibility of a language-game is conditioned by certain facts ? In that case it would seem as if the language-game must "show" the facts that make it possible. (But that's not how it is.). (1969, P.82:618)

An extremely puzzling feature of Kuhn's theories is his apparent lack of distinction between questions of description and prescription; of matters of socio-historical fact and matters of philosophical advice. His theory is basically a socio-historical account of how scientists have in fact behaved and yet he frequently talks of scientific 'success' and 'progress'. But we can only talk of 'success' or 'progress' in terms of some prescriptive standard. Kuhn fails explicitly to offer such a prescriptive standard; how then can Kuhn talk of success or progress ? In attempting to answer such criticism, he writes: 'If I have a theory of how and why science works, it must necessarily have implications for the way in which scientists should behave if their enterprise is to flourish'. (1970c, P.237) But to make this reply is surely to have missed the point of the criticism. Is science flourishing ? This is the point at issue. We could only determine whether science is flourishing by comparing what science is actually doing with what it <u>should</u> be doing. To show that science is successful, it is not enough merely to recite what scientists actually do. If scientists <u>should</u> be spending their time solving Kuhnian puzzles, then according to Kuhn's analysis, science could be said to be flourishing. Yet, if science should be doing something else, then, given that Kuhn's factual analysis is correct, science is a dismal failure. No reason is given why we <u>should</u> accept the prescription implicit in Kuhn's account rather than some opposing prescription.

Kuhn claims that in considering scientific progress, nothing is lost by abandoning the view that our theories approach nearer the truth about an independently existing reality. Yet, if we accept the prescription that science should seek such truth then <u>everything</u> would be lost if we jettisoned this concept of truth. How can we ever expect the question of which prescription we should accept to be settled by sociology as Kuhn appears to suggest ? Surely nothing other than philosophical argument can tell us what prescription regarding science we <u>should</u> accept. Should science seek truth or should it try to solve puzzles ? How could this question ever be

answered by sociology, or by history or by psychology ? The activities of science cannot of themselves set a prescriptive standard for science. The prescriptive question of what science should do must surely be a matter for philosophical reflection. This gives us one indication of the need for a systematic relationship between philosophy and science. We require a framework of philosophical prescription regarding what science should do; and we require the activity of a philosophical evaluation of scientific endeavour to see if the work of science has reached this prescriptive mark. Of course, this philosophical activity need not be that of a professional philosopher; for example, it may be done by someone who is a scientist acting qua philosopher.

e) Popper's theory of 'objective knowledge'

If, as Wittgenstein argues, thought is embedded in language, and the meaning of language is the way words are used rather than any 'ideas' in people's minds, does it not follow that knowledge must be essentially social ? If we accept that concepts and thoughts are inherent in language, then should we not accept that knowledge must be embedded in language and that epistemological individualism must be false ? Popper assumes that truth is a property of language; he assumes that truth is ' ... the correspondence of statements to the facts they describe'. (1972, P.324) If truth is a property of language then it might seem to follow that knowledge resides in language. Thus, Popper compares what he calls subjective knowledge, that is, knowledge which is known by an individual subject with what he calls objective knowledge; and:

> Knowledge in this objective sense is totally independent of anybody's claim to know; it is also independent of anybody's belief, or disposition to assent; or to assert, or to act. Knowledge in the objective sense is knowledge without a knower: it is knowledge without a knowing subject. (Popper, 1972, P.109)

The contents of a book would be a good example of objective knowledge; this will exist as objective knowledge whether or not anyone believes the contents of the book and, indeed, whether or not the book is even read. Popper believes that objective knowledge rather than subjective knowledge is the

proper concern of epistemology since subjective knowledge ' ... which consists of dispositions and expectations' (Popper, 1972, P.66) is not subject to rational criticism unlike objective knowledge ' ... which consists of linguistically formulated expectations submitted to critical discussion'. (Popper, 1972, P.66)

Popper talks of 'objective knowledge' as being inherent in linguistically formulated theories. Yet, since lies, myths and guesses can in this sense be similarly 'objective' we can hardly use Popper's criterion of 'objective knowledge' to distinguish knowledge from lies, myths and guesses. We have already seen that Popper claims that we cannot justifiably claim that our scientific theories are true. Hence, should we conclude that when Popper talks of 'objective knowledge', he is not using the term 'knowledge' as it is normally used ? Is Popper's distinction between 'subjective knowledge' and 'objective knowledge' of any real epistemological importance ? It is false that only 'objective knowledge' can be rationally criticised. I might believe that there is a handkerchief under my pillow or that many of the buildings in Aberdeen are made of granite, or that my name is H.McL. These beliefs are quite easy to test; in fact, they are known by me to be true. Having linguistically formulated these beliefs, they are now, presumably, in Popper's terms 'objective knowledge'. But, why should we want to say that there is a fundamental epistemological difference between these beliefs merely as my 'subjective' beliefs and these beliefs as 'objectively' expressed in language ? How could the mere depression of certain keys of a typewriter change the epistemological status of my beliefs ?

f) Knowledge and science

How is knowledge possible; how do we know ? What can be known by us ? Epistemological sociologism almost seems to evade these questions or often to replace them with the question: 'What is science ?'. But, if our aim is to try to differentiate between true, justified beliefs and opinions, conjunctures and and myths, is this latter question of particular importance ?

For example, consider the controversy between Popper and Kuhn regarding the proper bounds of 'science'. Their differing perspectives can be seen by considering whether astrology is a 'science'. Popper argues that astrology is not a science, on the grounds that its theories are not set in the form of falsifiable universal laws. Kuhn retorts that certain astrological theories actually are falsifiable but that astrology is not science, on the grounds that astrologers do not have a 'paradigm': 'Though they had rules to apply, they had no puzzles to solve and therefore no science to practice'. (Kuhn, 1970b, P.9) Whether or not astrology is 'science' might be a matter of much concern to the sociologist but it is surely not a question that the epistemologist need puzzle over. The crucial epistemological question is not whether individual astrologers are engaged in the social activity of 'science' but whether astrological theories should be considered to provide us with knowledge. We might even allow that astrology is a science. The crucial question is: 'Is it a successful science; does it produce knowledge ?'. Which of its theories, if any, provide us with knowledge ? For some specific purposes it might be very useful to do what Popper and Kuhn have done and redefine the term 'science' to produce particular rarefied conceptions of 'science'. As Wisdom would insist there is a point to such (implicit) redefinition; the new ways of speaking will reveal features of our experience and of our language which might have been rendered inconspicuous by our previous way of speaking. But, if our purpose is to consider the possibility and presuppositions of knowledge, is such redefinition necessary or even desirable ?

Conclusion

In Chapter One, we considered the view that successful science is in no need of philosophy and that the philosopher is only required to intervene to solve particular problems arising largely from linguistic confusions. This view is rendered implausible not only by the fact that there is an element of philosophical interpretation in the claim that science is either successful or unsuccessful but by the fact

that it is a matter of philosophical interpretation whether or not a linguistic confusion exists and how it might be remedied. For example, Wittgenstein and Locke would not agree about what is involved in a word having a 'meaning' and they might well disagree as to whether or not language was being abused on any particular occasion. Is language abused when we use words without appropriate 'ideas' in our minds; is it being abused only when the rules to the 'language-game' are broken ? Is it part of the rules of 'language-games' that we have 'ideas' in our minds when we speak meaningfully ?

In this chapter, we considered two general ways of trying to indicate how knowledge is possible, and neither way seemed able to show that it is possible to have knowledge, i.e. true, justified beliefs about a 'reality' which is, in some sense, external to us and independent of us. Both individualism and sociologism seem to suggest that such knowledge is <u>not</u> possible. If we are conversant only with our own 'ideas', how can we possibly gain knowledge of a reality external to them ? With sociologism we have a corresponding problem. Here too we are trapped within a web of 'ideas' but here the web is a social rather than an individual one. As Kuhn would have it, we appear to be trapped within socially imposed 'paradigms' through which we see 'reality'; hence, there appears to be no sense in asking whether our theories are objectively 'true' or whether they correspond to what 'reality' is 'really' like. If only a deductive proof could justify our acceptance of a belief, it appears that we could never justify our beliefs about the world since it is not contradictory to deny such beliefs. From the arguments in this chapter, we might conclude that knowledge is not possible; how we should react to this conclusion will be considered in the following chapter.

CHAPTER THREE

HOW KNOWLEDGE IS POSSIBLE

It would appear to be platitudinous to say that we actually do know very many things when this knowledge is of an 'external world' i.e. a reality whose existence is distinct from and independent of our awareness of it. We seem to have very many beliefs which we are quite justified in accepting as true. It would seem very strange to deny that we actually know 'the facts of life' or that gasses generally expand when heated or that the sun will rise again tomorrow or that those who can see see with their eyes rather than with their ears or any other part of their bodies. We know most of these things with totally justified certainty; or, at least, this is what most of us would generally suppose. There is a host of other beliefs which we imagine we are quite justified in accepting even although we might not want to say that it could be held with justified certainty. For example, we would probably want to say that we know that smoking can tend to cause lung cancer even although it is perhaps not certainly true that smoking can cause lung cancer. We imagine that we can distinguish knowledge from opinions, superstitions, myths and mere conjectures. And it is obviously of considerable importance to us that we can make or that we appear to be able to make such a distinction. Yet, it would appear that if either the epistemological individualism or sociologism which we have considered is correct, then we cannot have knowledge of an 'external world'. Both theories seem to suggest that the belief that we can and do have knowledge is mistaken. It would appear that to accept either theory would be to concede that what passes for knowledge is delusory.

How should we react to these epistemological theories ? Should we concede that when we thought we actually knew many things about 'reality' we were quite mistaken ? Should we say that we actually do have knowledge and that any argument which suggests that we cannot have knowledge must be mistaken, even if we cannot detect its precise mistake ? Or should we simply 'sit on the fence' assenting to neither the epistemology nor the supposed knowledge ? Neither the first nor the last reaction seems appropriate. It is important to us, in very many circumstances, to say that we have knowledge of certain things. Particularly when we are trying rationally to plan our lives and to make moral decisions, we want to consider that we have a basis of knowledge rather than conjectures in terms of which to produce our plans and decisions. Why then should we give up our claim actually to have knowledge in the face of epistemological theories which are, to say the least, no better attested than our supposed knowledge ? Similarly, we would be conceding a lot if we were to relinquish our claim often to possess knowledge by 'sitting on the fence' and, as I hope to show, there are better reasons for considering much of our supposed knowledge actually to be knowledge than there are for sitting on the fence. Should we simply assert that we do know various things about an 'external world' ? Knowledge logically entails justification: how could we justify any claim to know a specific fact if we were unable to show that knowledge is in principle possible or, at least, that theories which seem to show that knowledge is not possible are false ? We have considered various epistemological theories which apparently cannot accomodate our conviction that often we actually do have knowledge. Would it not be rational either to retract our claim to possess knowledge or else to produce an acceptable set of epistemological assumptions which can accomodate our claim to know ? It would surely be irrational to accept an epistemological theory which purports to show that knowledge is not possible and also to claim to know any specific facts.

Science, knowledge and universal laws

Consider the conception of science as the quest for universal laws. Humean empiricism, in a modified form, provides the basic philosophical framework for this idea of science and the 'universal laws' which scientists are imagined or implored to seek are akin to Humean 'constant conjunctions'. For example Rex discusses such a conception of social science and writes in his opening chapter: 'Most of the later chapters of this book will, in fact, be concerned with the problems of conceptual analysis which have arisen in the course of the attempt to discover the laws of society'. (1970, P.22) He is well aware of the philosophical pedigree of the laws which he seeks. He specifically refers to Hume's analysis of causation and accepts a Humean conception of causal laws in terms of constant conjunctions. 'Thus when we say that A caused B, rather than merely saying that B followed A, what we mean is that B followed A, and that this accorded with our expectations derived from accepted laws.' (Rex, 1970, P.23)

It is perhaps to Mill more than anyone else that we are indebted for the popularisation among social scientists of this conception of science. In Mill's view 'Any facts are fitted in themselves, to be a subject of science, which follow one another according to constant laws'. (Book VI, Chapter III) It is the task of the scientist to discover these laws. Yet, if such laws merely express constant conjunctions, how can we know that we have ever discovered such a law ? Those accepting this aspect of Hume's system generally suppose that the question of induction does not have the epistemological significance that Hume imagined it to have. For example, Rex contends that:

> The failure of philosophers to solve the "problem of induction" has not prevented scientists from discovering laws and, what is more, being able to recognise the difference between a law which has been "established" and other universal propositions which have not. (1970, P.16)

Mill believes that generalisations based on induction are indeed justifiable and defends his view by claiming that 'Whatever be the most proper mode of expressing it, the proposition that the course of nature is uniform is the fundamental principle, or general axiom, of induction'. (Book III, Chapter III)

There is surely something profoundly unsatisfactory about this position. Rather than answering Hume's question regarding induction, this approach seems to beg the question. We cannot just say that the uniformity of nature justifies our inductive inferences since the justification for believing in the uniformity of nature on the basis of inductive experience is precisely the point at issue. Similarly, to say that scientists have succeeded in establishing laws despite the problem of induction is to beg the question since Hume's argument purports to show that such laws never could be established. Indeed, properly to have understood the 'problem' of induction is to realise that it is not the sort of problem which ever could be solved. We could no more solve the problem of induction than we could produce a square triangle. To imagine that at some future date Hume's problem will be solved is to have misunderstood the nature of the problem. What Hume demonstrates is that if we accept certain premises then certain conclusions necessarily follow. If we accept that knowledge should consist in justified acceptance of laws stating constant conjunctions then we must conclude that knowledge of the world is unattainable. Unless we can show that Hume has made a logical error and that his conclusions do not follow from his premises then it is surely unreasonable to accept the premises of Hume's argument and yet deny the conclusion. If we want to maintain that knowledge of the world is obtainable, then, surely we must reject the premises of Hume's argument. Is it not unsatisfactory to do what Mill and Rex appear to have done and attempt to frame our knowledge in terms of philosophical assumptions from which we can conclude that knowledge is impossible ?

Perhaps it is difficult for us always fully to appreciate that we do have a <u>choice</u> as to the concepts and assumptions we adopt. For example, consider Popper's version of the theory that science is concerned with universal laws. Popper's epistemology has its logical roots in the acceptance of Hume's analysis of induction. (See Popper, 1972, Pp.1-31) It is for this reason that his approach is <u>hypothetical</u> and <u>deductive</u>. Popper considers that the problem of induction is merely a facet

of the problem of 'demarcation' and it is upon this question of demarcation that his entire epistemology is founded. He argues that the problem of demarcation is the main problem of epistemology and is 'The problem of finding a criterion by which we can distinguish between assertions (statements, systems of statements) which belong to the empirical sciences, and assertions which might be described as "metaphysical" '. (1968, P.313) Popper's solution to this problem is the criterion of 'falsifiability'. He concludes that science consists of statements of universal laws when such statements can, in principle be refuted by observation. For example, consider the statement, 'All swans are white'. Popper agrees with Hume that we cannot 'verify' or 'justify' this statement since the statement outsteps our observations of a finite number of swans; but Popper believes that the observation of even one non-white swan would be sufficient to refute or falsify this statement. Popper considers such refutable statements to be 'scientific'. Hence Popper's definition of 'science': 'it must be possible for an empirical scientific system to be refuted by experience'. (1968, P.41)

Unlike Hume, Popper believes that we can have a rational preference among beliefs; we can have a rational preference for those universal laws which are in principle falsifiable but have, as yet, not been falsified. Nevertheless, Popper agrees with Hume in these important respects. Both Popper and Hume conceive of causation essentially in terms of de facto empirical regularities. Popper writes that 'To give a causal explanation of an event means to deduce a statement which describes it, using as premises of the deduction one or more universal laws, together with certain singular statements, the initial conditions'. (1968, P.59) Popper accepts Hume's analysis of induction and he also accepts Hume's belief that arguments are either deductive or defective; that beliefs are justifiable only if supported by a deductive argument. Hence, Popper concludes not merely that we can never logically prove that we have discovered a universal law but he further concludes that, therefore, we can never justify the belief that we have discovered such a law. He writes:

> I think that we shall have to get accustomed to the idea that we must not look upon science as a "body of knowledge", but rather as a system of hypotheses; that is to say, as a system of guesses or anticipations which in principle cannot be justified but with which we work as long as they stand up to tests, and of which we are never justified in saying that we know that they are "true" or "more or less certain" or even "probable". (1968, P.317)

Popper appears to accept Hume's conclusion that we can never have 'knowledge' of empirical regularities where by 'knowledge' we mean true, justified beliefs.

It is misleading to say that we 'have' to get used to the idea that 'science' is not a 'body of knowledge' but rather a system of conjectures which cannot be 'justified'. What Popper shows is that, if we accept certain premises, certain conclusions follow necessarily; but it is not true that we must accept these premises. If we assume that science is a system of falsifiable universal laws and if we assume that only a deductive argument can justify acceptance of a hypothesis then we must conclude that the acceptance of scientific theories cannot be justified. Similarly, if we define the term 'science' as 'a system of falsifiable universal laws' and if we define the terms 'to know' or 'to justify' as 'to be able deductively to prove' then it follows that 'science' is not a body of 'knowledge'. For some purposes, it might be useful to use the definitions of the terms 'science' and 'knowledge' embodied in Popper's philosophy. For example, it might focus our attention on the difference between statements which can quite convincingly be shown to be false on the basis of particular observations and statements such as, say; 'God exists'; 'We should not drop litter on the streets'; 'Every substance is necessarily infinite' (Spinoza, 1930, P.98); 'Smoking sometimes causes cancer'. Whether or not these latter statements can indeed be verified, it would seem to be apparent that no observation or finite number of observations could show them to be false quite as convincingly as a Popperian universal law might, in principle, be shown to be false. Nevertheless, there is a point in not accepting these definitions of 'science', 'justification' and 'knowledge'. For example, if we were to adopt these definitions, even just for some very specific purposes, we might forget that we were not using the

words 'science', 'justification' and 'knowledge' as we generally used to do. We may, perhaps, be misled into thinking that Popper has shown that science (in the conventional sense of the term) cannot be a body of knowledge i.e. true, justified beliefs. What Popper has shown is that 'science' cannot be a body of 'knowledge' where the terms 'science' and 'knowledge' are redefined. Furthermore, the acceptance of these redefinitions of 'science', 'justification' and 'knowledge' might lead to the important distinction between beliefs which are guesses or mere opinions and beliefs which are rationally well substantiated being overlooked.

In any case, if we consider many of the things we would claim to know, it is far from obvious that our knowledge could be expressed in terms of universal laws. We know 'the facts of life' but could we express this knowledge completely in terms of any unfalsified universal law, or laws ? Perhaps there are unfalsified universal laws pertaining to 'the facts of life' but we appear to know them without even thinking about such laws. If we were informed by biologists of some hitherto undiscovered universal law invariably holding in all acts of sexual intercourse, would we be more sure of 'the facts of life' than we are now ? How would we react to an anomaly like a well attested virgin birth ? Would we say that our supposed 'facts of life' had been falsified ? Would we simply say that the progeny involved was a god or a freak ? We generally all know our own names and our own ages. How does this relate to universal laws ? We know that it is very difficult for advanced industrial economies to maintain full employment, a significant rate of economic growth and a zero rate of inflation over a long period. But do we know of any invariant laws governing the relationship between employment, inflation and growth ? Some knowledge may be able to be expressed in the form 'All As are Bs' or 'No As are Bs' but perhaps this is not the form that all knowledge takes. Perhaps, on some occasions, it takes the form 'Some As are Bs' or 'As are almost always Bs' or 'As are very rarely Bs' and, indeed, on other occasions, it might assume a quite radically different form.

Durkheim's formulation of epistemological sociologism

Since we have reached an epistemological impasse with both individualism and sociologism, it would seem appropriate to retrace our path to try to discover where either theory may have misled us. Durkheim is perhaps the founder of epistemological sociologism and he reached his epistemological conclusions via the following route. (See Durkheim, 1961, Pp.9-20) He considers ' ... the categories of the understanding: ideas of time, space, class, number, cause, substance, personality etc. They correspond to the most universal properties of things. They are like the solid frame that encloses all thought.' (Durkheim, 1961, P.9) He looks at the two traditional ways of accounting for the categories. According to empiricism, they are derived from sensory perception. According to what he calls 'apriorism', the categories are ' ... immanent in the human mind by virtue of its inborn constitution'. (Durkheim, 1961, P.13) This latter theory is more or less the doctrine of innate ideas. Durkheim argues that both theories are untenable. For example he rejects empiricism because the categories are qualitatively different from other ideas which are derived from perception: 'As a matter of fact they are distinguished from all other knowledge by their universality and necessity'. (Durkheim, 1961, P.13) We cannot help but think and experience the world in terms of the categories. He rejects apriorism since ' .... the categories of human thought are never fixed in any one definite form; they are made, unmade and remade incessantly; they change with places and times'. (Durkheim, 1961, P.15)

Durkheim assumes that if the categories are individual mental products then either apriorism or empiricism must be true. Since neither apriorism nor empiricism is a plausible theory, Durkheim concludes that the categories cannot be individual, they must be social. Having discovered the difficulties of both apriorism and empiricism, he writes, ' ... if the social origin of the categories is admitted, a new attitude becomes possible, which we believe will enable us to escape both the opposed difficulties'. (1961, P.15)

Durkheim believes that the compulsive nature of the categories is to be explained by their social imposition. And he believes that his theory is supported by the fact that the categories are socially variable. He also presents the following argument. He distinguishes the categories and what we might generally refer to as concepts from sensual representations, such as sensations, images and perceptions and notices several fundamental distinctions between them. (See Durkheim, 1961, P.433) Consider a concept such as 'causation' and a sensual representation such as the idea of 'red' or the perception of regarding the various objects in the room. A concept such as causation is stable and durable; we have no difficulty in focusing our attention upon it at will. By contrast, sensual representations are fleeting and transitory. Our perceptions are in a constant state of flux. Consider how difficult it is consciously to conjure the idea of 'red'. If we cast our eyes round a room we cannot retain any perception for long; there is a continual stream of sensual representation while one perception dissolves into another. Furthermore we can communicate our concepts, but we cannot communicate sensations or perceptions. No ammount of conversation could communicate the idea of 'red' to the congenitally blind. Durkheim attempts to account for the differences between concepts and sensual representations by claiming that the latter are individual while the former are social in nature; they are the product of our inter-personal life. On this account, concepts are stable and durable because the social group which produces them is stable and durable. And similarly, concepts, unlike sensual representations, are communicable within a social group since they are the product and property of that social group. This is what Durkheim means when he writes: 'A concept is not my concept, I hold it in common with other men ...'. (Durkheim, 1961, P.433)

I think that we can allow Durkheim's conclusion that neither empiricism nor the theory of innate ideas is tenable. We have already seen the difficulties faced by empiricism and the theory of innate ideas fares no better. Locke argues forcefully against the theory by suggesting that there is no evidence in its

favour and that there is evidence against it; for example, children lack many of the principles and ideas held by adults and we find a greater diversity of thoughts and opinions than we might be led to expect on the supposition of innate ideas. (See Locke, 1964, Pp.67-78) But, perhaps these arguments are not totally convincing. Ideas could be innate and yet appear only in adulthood. Similarly, to say that ideas are innate is not to imply that we can expect thoughts and opinions to be standardised. Ideas might be both innate and socially variable. It is important not to confuse the epistemological and the psychological aspects of the theory of innate ideas. It is not possible totally to separate them, of course, since if the theory were false as a psychological theory i.e. if it did not properly account for the genesis of our ideas then it could not be an adequate epistemological theory; it could not account for our knowledge. For our purposes, the main reason for rejecting the theory is an epistemological one. Even if it were true as a psychological theory, it could not be an acceptable epistemological theory since it cannot account for our knowledge, or supposed knowledge of the world. If our ideas were simply innate, then how could we attain knowledge of a world external to those ideas ? Imagine that the idea of 'red' were innate so that even the blind had such an idea. The crucial epistemological point is that the blind could not recognise this idea as an idea of 'red'; they could not recognise red objects. And the sighted would be similarly placed. Suppose that my ideas of 'haggis' and of 'cause' are innate. How could I ever recognise these ideas as the idea of 'haggis' and the idea of 'cause'; how could I ever gain knowledge of actual haggis and of actual causes ?

Perception and conception

Having accepted the premises of Durkheim's argument, how can we avoid his conclusion and the epistemological sociologism which we have already found to be defective ? Suppose that someone had an ulcer in his stomach; would we argue that the ulcer must have been innate, something the person was born with, or else something which was transplanted into his stomach, perhaps being swallowed during a meal ? We would surely

allow the possibility of the ulcer's being produced in the stomach. And similarly, we need not conclude that either the theory of innate ideas or empiricism must account for the mental content of individual people. It is quite possible that thoughts are, in some sense, produced by the mind. And we need not assume that all mental content is of the same type. Perhaps some ideas are innate; perhaps some ideas are derived from sensory perception and perhaps some ideas have a different source. Bearing in mind the possibility that some mental content might be a mental product, it might be worthwhile to reconsider Durkheim's distinction between 'sensual representations' and the 'categories' in terms of which our knowledge is framed. According to Hume, all our mental content is sensual representation and it was from this assumption that we were led to an epistemological impasse. Perhaps Hume's conclusions could be avoided if we were to allow for the possibility of non-sensual mental content. Durkheim notes that sensual representations are fleeting and incommunicable while the categories are durable and communicable and tries to account for the difference by suggesting that whereas sensual representations are individual mental products, categories are social. Might it not be plausible to account for the difference by suggesting that although sensual representations are a product of people's senses, categories are, as we might say, a product of the intellect ? This might explain both the durability of categories and concepts and the fact that they can be communicated. Perhaps the 'idea' of 'red' is a sensual representation which cannot be communicated to a blind man while we can communicate a 'concept' such as the concept of 'democracy' although no one has ever perceived a democracy. Intellectually grasping the concept of a 'democracy' is a different sort of mental exercise from experiencing or being able to conjure the sensual representation of 'red'.

Talking of human thought, Spinoza writes: 'I use the word conception rather than perception because the name perception seems to indicate that the mind is passive in its relation to the object. But the word conception seems to express the action of the mind.' (1930, P.144) It might be useful to try to consider

the distinction between perception and conception and between percepts and concepts. Not all mental content is of the 'photograph in the mental album' variety; by 'percepts' I mean such content that is. 'Percepts' are what Durkheim calls 'sensual representations' and what Hume calls 'ideas' and 'impressions'. Percepts are the experiences of or the memories of sensory perception. Concepts, for example the concepts of 'cause', 'knowledge', 'explanation', 'man' or 'dog' are not necessarily derived from sensory perception or, at least, they are not necessarily derived totally from perception. As Spinoza might say, they are at least partly formed by the mind. Although there is some sort of resemblance between a percept and a concept of, say, a dog, they are quite distinguishable. Our concept of 'dog' i.e. what we mean by the word 'dog', is distinct from the perceptual experience obtainable by seeing, hearing, touching or smelling any particular dog or pack of dogs. Our concept of 'democracy' is distinct from any perceptual experience we might have when actually living in a democracy. We may want to distinguish between 'pure' and 'empirical' concepts. A 'pure' concept is produced entirely by the intellect; it is mental content which is independent of any sensory experience. Perhaps concepts such as 'explanation' and 'knowledge' come into this category. Knowledge is true justified belief and neither the concept of truth nor that of justification appears able to be derived from or even to be related to sensory experience. 'Empirical' concepts are the product of both the senses and the intellect. Empirical concepts require precedent 'impressions', to use Hume's terminology. Had we never seen a face or the colour 'red' we would lack the concept of having a red face; we could not recognise red-faced people. Yet, as we saw when we examined Hume's system, empirical concepts cannot arise from impressions alone. As we shall see when we come to discuss language and classification, logically prior to any empirical concept is a pure concept of class membership.

The difference between conception and perception is best indicated ostensively. For example, a thought that something is the case is a conception. An experience of having an impression

or an idea is a perception. Suppose we say, 'I think it is the case that X' where 'X' means, for example 'James is never late for his work'. 'That X' will be true or false. It will be true if James is never late for his work; if not, it will be false. Suppose we sit in front of a red screen and say, 'I sense, or experience Y' where 'Y' is our impression of the red screen. It would make no sense at all to say that 'that Y' is either true or false. Perception could be neither true nor false. We can appreciate the radical difference between perception and conception by trying to interchange the 'X' and the 'Y' in the above sentences. It would make no sense at all to say 'It is the case that Y i.e. an impression of "red" '. And similarly, it would make no sense to say 'I experience X i.e. James is never late for his work'.

If we think, 'I sense Y' then 'Y' will be virtually beyond our will; we have very little choice as to what percepts we shall experience. Yet, if we think, 'It is the case that X' we can mean almost anything we want by 'X'. We can produce conceptions more or less at will. This would tend to suggest that conceptions and perceptions have different origins. Similarly, if we close our eyes and heed whatever impression we are conscious of and then open our eyes and let them wander round the room, we shall generally find this original impression obliterated by the flood of perception which greets us; and this flood of perception will be swamped in turn as we regard some other part of the room. Yet, if we close our eyes and summon a conception, say, that it is the case that Perth is a very fine city and then open our eyes and gaze around the room, we usually find that our conception is not obliterated; even while looking intently round the room, we generally have no difficulty in maintaining the conception that Perth is a very fine city. This too illustrates the difference between perception and conception and suggests that both are of differing origins.

One last example should suffice to distinguish perception from conception. We might well say of some human behaviour that a person's conception of what he is doing is in a sense constitutive of what he is doing. For example, consider the activity of prayer. Without the conception of a god and of possible communication with god, one could not pray. When we pray our conception of our action as communication with god is partly constitutive of what we are doing. Yet, we would never say of a praying person that his perception of what he is doing is partly constitutive of what he is doing. Our conception of our actions is an intellectual orientation towards them; it is quite different from the effect of our actions upon our senses.

It seems quite plausible to say, as Hume does, that some mental content, for example, our 'idea' of 'red' is derived from sensory perception. Yet, there is no reason to conclude that all mental content is so formed. To show that concepts and conceptions are in many ways very different from percepts and perceptions is to give reason to suspect that they are different types of mental content with, perhaps, different origins. Furthermore, as we have seen, if we assume that all mental content is derived from sensory perception, it appears that we cannot account for the possibility of knowledge of an 'external world'. If the assumption that not all mental content is derived from perception allowed us to conclude that knowledge of an 'external world' is possible, we would have a strong reason for accepting it.

Conceptions and the concept of truth

People sometimes imagine that truth is a property of language; we find it perfectly natural to talk of true or false sentences or true or false statements. But, we can have true or false beliefs which are never expressed in language; we can have true thoughts that we tell to no one. Truths are expressed in language. Yet, we might express truth with a nod of our head - we might be asked if it is raining outside and nod our head to signify that it is - but we surely would not want to say that truth or falsity is a property of nods of the head. Furthermore,

sentences and statements can be ambiguous but it would be absurd to say that any one sentence or statement was both unequivocally true and unequivocally false. It would seem more plausible to suggest that truth is a property of thought rather than of language. It is what is expressed or stated in language, rather than language itself, which is either true or false.

Not everything that is expressed in language will be either true or false. If we say 'Hello' or 'Please come for a drink' then what we say could be neither true nor false. Truth and falsity occurs only when we think or state that something is the case. Only conceptions can be either true or false. For truth or falsity to occur, the expression 'is the case' need not be made explicitly; it need only be implicit in the claim. For example, if we say 'All cows are brown', what we mean to claim is that it is the case that all cows are brown. Any claim 'X' will be true or false or partly true and partly false if it makes sense to say 'It is the case that X' or 'That X is the case'. It makes no sense at all to say 'It is the case that Hello' or 'That please come for a drink is the case'. 'Hello' and 'Please come for a drink' could be neither true nor false. Truth is, in an important sense, a coincidence. Truth is the coincidence of what is thought to be the case and what is the case; falsity is their divergence. A conception is true when it is the thought that X is the case when X is the case; the same conception will be false when it is not the case that X. It is true that all cows are brown if it is the case that all cows are brown. The statement 'It will rain tomorrow' will be true, or, rather, it will express a truth, if it is the case that it will rain tomorrow. It will be true to say that we should accept epistemological individualism if it is the case that we should accept epistemological individualism.

It is not mere pedantry to talk about truth in relation to 'what is the case' rather than in relation to 'what is'. This 'coincidental theory of truth' stands opposed to what is known as the 'correspondence theory of truth'. For example, Popper writes that not only can truth be defined

' ... as correspondence to the facts; but we can also define reality as that to which true statements correspond'. (1972, P.329) This theory has its roots in the writings of Hume. He writes: 'Truth or falsehood consists in an agreement or disagreement either to the real relations of ideas, or to real existence and matters of fact'. (1969, P.510) In this account, there are two types of truth; there are necessary truths such as the truths of mathematics and logic; and there are truths regarding reality. In the latter sense of truth, truth is the correspondence between real existence i.e. what is and what is said to exist. The statement 'There is a mouse in the kitchen' will be true only if it corresponds to reality, if the mouse and the kitchen are 'real existences'.

Even a moment's reflection should make us uneasy with this correspondence theory. Is the theory true: could we demonstrate that it is a necessary truth; if it is not a necessary truth, to what 'real existence' could it correspond ? That it will rain tomorrow could very well be true, but if it is true, it will be a present truth; there is no 'real existence' or 'reality' to which this possibly true belief could correspond. When we voice the true claim that Hume is dead we are precisely denying the existence of a 'reality' to which the true claim corresponds. We are led into various paradoxes and antinomies if we accept the correspondence theory. We have great difficulty in allowing for hypothetical truths and for moral and aesthetic truths since there does not seem to be a reality to which these statements could correspond. And suppose we say 'The Loch Ness monster does not exist'. If we accept the correspondence theory, then we seem to have the problem of accounting for the existence of a non-existent monster to which this statement, if true, could correspond. In terms of the coincidental theory, we can account for such truths quite easily. It is true that we ought not to steal if it is the case that we ought not to steal; it is true that I would not have seen the cup final had I not gone to Hampden if it is the case that had I not gone to Hampden, I would not have seen the cup final. It is true that the Loch Ness monster does not exist if it is the case that there does not

exist a monster in Loch Ness. Wittgenstein once said that 'The world is everything that is the case'. (1951, P.31) This is misleading. We should not confuse 'what is' with 'what is the case'. The world is everything that *is*. Yet, if it is the case that Hume is dead and if it is the case that it will rain quite soon, then that this is the case will form no part of reality, of *what is*. We should not regard truth as correspondence with *what is*; we should regard truth as the coincidence of what is thought *to be the case* and what *is the case*.

It is worth noting several points regarding this theory of truth. It is possible to detect a certain logical affinity between the correspondence theory of truth, for example, as presented by Hume, and the view that true statements are either synthetic or analytic. Analytic statements might be thought to relate to the 'agreement or disagreement to the *real* relations of ideas' while synthetic statements relate to '*real* existence and matters of fact'. The coincidental theory of truth seems to be able to account for both types of truth. For example, suppose we present the mathematical statement '2+2=4'. This statement will state a truth if it is the case that 2+2=4; otherwise, it will state a falsehood. Furthermore, the theory seems to be able to allow that statements which would not normally be thought to be either synthetic or analytic might state truths or falsehoods. As we shall see, *prescriptive*, *evaluative* and *interpretative* statements belong to this category. It seems plausible to argue that the coincidental theory of truth is necessarily true. The theory will be true if what it states is the case; it will be false if what it states is not the case. If we accept that what the theory states is the case then we will accept that the theory is true. Suppose we were to assert that the theory is false. Would this not be to assert that what the theory states is not the case ? But, surely to assert this would be to assume that a true theory states what is the case while a false theory states what is not the case and to assume this is to assume that the coincidental theory is true. Does it not appear that explicitly to reject the theory is implicitly to have accepted it as true ?

Or, is this not the case ?

Conceptions and language

A statement will state a truth if it states that X is the case when X is the case. How will a statement be verified ? It makes no sense to talk of verifying statements since statements can be neither true or false. We can only verify what statements state. To verify what a statement states will be to try to discover whether or nor X is the case. Since whether X is the case and whether X can be discovered to be the case are quite serarate questions, the truth of any claim will be independent of its verification if, indeed, the claim is capable of being verified. Similarly, since truth is not a property of language, the truth or falsity of any conception will be totally unaffected by the language uses to express the conception if it is expressed in language. To substantiate these claims we must look more closely at the relationship between conceptions and language.

Language is a social device whereby people's conceptions are expressed, communicated and recorded; language has other uses as well but these are the functions of language that I want to consider. The 'people' referred to need not be solitary individuals. For example, committees and cabinets can express beliefs, thoughts and opinions. The point is that mental content expressed in language is distinct from the language used to express it. A language is a system of words, signs and expressions which can be used with the intention of communicating, expressing or recording mental content. Locke writes that words are the sensible signs of our internal ideas. To use more modern terminology, we might say that the meaning of the language we use is, or can be the mental content which we intend that language to express communicate or record. Such mental content will not be in Locke's terms 'ideas' i.e. percepts; it will, rather, be concepts and conceptions. Not all language will be in this sense 'meaningful'. If we say 'Ouch' or 'Goodbye', we are not necessarily trying to communicate or express any mental content. In such cases, we can say that these linguistic expressions are meaningful in some other sense. They are,

meaningful say, in the sense that they actually do reveal mental content even if unintentially or in the sense that their use is in accordance with conventional rules. Or, if we prefer, we can simply say that not all language is meaningful; some language is meaningless and some is perhaps merely non-meaningful. To say that the meaning of language might be mental content is not to say, as Locke appears to do, that words signify merely our mental content. When we use words such as 'table' or 'elephant' we are referring to actual tables and elephants rather than to our own thoughts or 'ideas' of tables or elephants. There is a distinction between saying that language expresses mental content and saying that language refers to such content. I might express the point that I am presently in quite considerable pain. To what am I referring; must I be referring to anything ? I am certainly not referring to my mental content or to my pain since I am in no pain at all. Even when language obviously does have a reference, it need not refer to the mental content which it expresses. We may express the thought that Churchill was a great war leader by saying 'Churchill was a great war leader'. We are referring to Churchill and the fact, if it is a fact that he was a great war leader but we are not referring to the thought that Churchill was a great war leader. (See Frege, 1952)

Rickman writes that 'Understanding is the grasping of some mental content to which an expression points ...'. (1967, P.30) where by 'expressions' he means not merely linguistic expressions: 'Though they are physical facts or events, expressions are distinguished from ordinary facts and events by having a reference to mental processes'. (1967, P.xv) His theory relates to the grasping of any mental content and not merely that expressed in language. In relation to linguistic expressions, he distinguishes two senses in which we can 'understand'. We can understand words and expressions and we can understand people's thoughts and feelings. Thus we might say 'I understand every word of his letter but I still do not understand what he is fussing about'. (Rickman, 1967, P.28) There is a distinction between understanding what words mean and understanding what on specific occasions any person means by

his use of language. For example, I have no knowledge at all of the German language yet I might be taught to recite a number of German phrases. On hearing me, a German speaker might understand the meaning of the words which I was reciting but he would not necessarily understand my thoughts; my use of German phrases would not involve the deliberate expression of mental content. Expressions are physical facts or events and we can be aware of them only through sensory perception but Rickman insists that although understanding involves perception, it is not reducible to perception. 'In perception expressions are not given to us as expressions. We only realise that what we perceive is an expression when we grasp that it has a meaning, i.e. that it expresses or refers to something.' (Rickman, 1967, P.38) We do not perceive what words mean nor what people mean by their use of words; we do not even perceive that words are meaningful or that they are words rather than mere sounds. To use an analogy, how can we distinguish a wink from a blink or a nervous twitch when the perceptible features of winks, blinks and nervous twitches are more or less identical ? When ' ... a man winks at a girl we recognise it as an expression because, and only if, we realise that a mental act links what he sees and what he does'. (Rickman, 1967, P.39) Consider the following deliberately contrived conversation between two women at the entrance to a tenement building.

A: 'Excuse me, I must go and clean the apples.'
B: 'That is very wise. I always wash fresh fruit before eating it.'
A: 'No, I am going to clean the apples and pears.'
B: 'You are going to have a fruit cocktail ?'
A: 'No, it is a cake ....'
B: 'I do not understand.'
A: 'It is a cake and a meringue - rhyming slang. Apples and pears - stairs. I am going to clean the stairs.'

How do we understand the meaning of the words used in this mock conversation; how do we understand what is on A's mind ? Clearly, we do not do this by perception alone. We must intellectually grasp precisely what is going on. To understand the conversation we must 'see' the point of the 'language-game' and we cannot see this with our eyes or any of our other organs of perception. The understanding of language involves not only

perception but also the mental activity of conception.

How do we 'understand'; how can we know that we have correctly 'understood' ? What are the epistemological conditions of 'understanding' ? Since we can only understand through a knowledge of expressions, ' ... the epistemological conditions of perception must be fulfilled before we can understand anything and to this extent understanding involves the same processes as knowledge of the physical world'. (Rickman, 1967, P.37) In addition to this, Rickman adds three specific conditions of understanding: ' ... familiarity with human nature, knowledge of the cultural background and awareness of context ...'. (1967, P.47) What is meant by the expression 'apples and pears' ? Well, it depends upon the cultural background of the speaker and the context within which it is spoken. The activity of understanding is commonplace although talk of the general condition of understanding perhaps makes it sound slightly mysterious. But if we consider any specific instance of understanding, the mental activity involved appears quite simple:

> An acquaintance asks me to look him up some time. How do I know that I have been invited ? Obviously by understanding both his words and his intention. To be able to do so I must have knowledge of the language, the social conventions of our society and the character of the speaker; these involve understanding. If an element of doubt remains I can test my understanding i.e., verify or falsify my hypothesis - by further acts of understanding. If I think I have misunderstood his words I can ask for his invitation to be repeated or rephrased; if I doubt the genuineness of his intention I can turn up on his doorstep and judge by his expression of surprise, pleasure or irritation. (Rickman, 1967, P.63)

We understand in a rather unexciting way by forming conceptions and then testing them in the light of our perceptual experience. I might say 'I like the apples'. How would anyone discover the meaning of this expression ? He may form the conception that I want to express a fondness for a particular type of more or less spherical fruit which grows on trees or perhaps the conception that I am expressing an admiration for the way in which the stairs have recently been painted. And both conceptions can be quite easily tested. I might be asked to rephrase my comment or to point at that which I claim to like or I might be offered an

apple and my reaction noted. The context of the remark would obviously be important. For instance, did it occur in an orchard ?

According to Rickman, there is a sort of epistemological circle in understanding expressions. We do not start from an epistemologically pure base or found our understanding upon any elements of indubitable knowledge. For example, understanding proceeds on the assumption that human beings share a certain common nature and this assumption is supported on occasions where we successfully understand. Similarly, we must assume that we have correctly perceived an expression before we attempt to understand it and the subsequent understanding of the expression supports the assumption that, in general, we are able correctly to perceive expressions and that, on this particular occasion we have correctly perceived an expression. Given that we must take certain assumptions for granted in any situation where we wish to understand an expression, where should we start; what should we take for granted; what should we consider problematical ? Rickman writes:

> In practice we solve this problem by a kind of shuttlecock movement. An approximate understanding of some expressions leads us to an approximate understanding of features of the cultural world. This, in turn, helps us to understand these expressions more fully or to understand a wider range of expressions. Our knowledge of the cultural world is thus extended and so we move to and fro, progresively deepening our understanding of the cultural world and of its particular manifestations. (1967, P.45)

When we say that language can be used as a social device for the expression of individuals' mental content, we encounter Wittgenstein's argument that a 'private language' is an [illegible] of impossibility, where in a 'private language' the meaning of words would be objects to which individual people had a private access. If the meaning of language were considered on any occasion to be the mental content of individual people, how could it be possible publicly to check what the meaning of any particular expression was ? Would an individual's mental content not be akin to the 'beetles' referred to in Wittgenstein's imaginary 'language-game' ? We have already partly answered these questions. Personal mental content is quite unlike a person's private 'beetle' where this content is concepts and conceptions rather

than perceptions. Intellectually grasping what someone might be thinking about is a different type of mental exercise from trying to conjure a perception of his unseen 'beetle' or his brain cells. As we have seen, it is possible publicly to check whether people possess a particular mental content. I might form a new word and attach a stipulative meaning to it. This meaning could sensibly be considered to be, at present, my personal mental content. But, it would be possible for other people to discover the stipulative meaning I had attached to the word. I might form the word 'blueno'. What do I mean by a 'blueno' ? It would be possible to discover this by asking me, say, whether it related to religion or politics and noting what I proceed to call 'bluenos' and my reaction to them.

We can readily agree with Wittgenstein that if words and expression are to have a use in language then they must be used in some sort of stable manner; they must, in general, be used in accordance with rules. Otherwise we could never have any confidence that any expression would express or was used with the intention of expressing any specific mental content. Similarly, if there was no way to check whether a particular expression had a particular meaning we would lack this requisite confidence. Yet, it does not follow that the meaning of language actually is such rules and public tests. In any case, although confidence may be a prerequisite of our attaching meaning to an expression, such confidence is often misplaced. For us to attach a particular meaning to an expression, it is enough for us to imagine that we are using the expression in accordance with the appropriate rules. This is one of the reasons why it is possible to distinguish what words mean or what they are generally taken to mean from what people on any particular occasion mean by their use of words. Reconsider too Wittgenstein's illustration of the 'private beetles'. Imagine that everyone had a box with something in it but that no one could look into anyone else's box. Wittgenstein argues that if the word 'beetle' were introduced to refer to what was in everyone's box then the meaning of the word 'beetle' would be quite unaffected by the nature of whatever happened to be in our boxes. Suppose that everyone

assumes, perhaps correctly, perhaps incorrectly, that everyone else has something in his box which is similar to or even identical with what he has in his box. If people were to make this assumption, then it is plausible to suggest that whenever anyone uses the word 'beetle' he will mean by this 'an object similar in nature to X' where 'X' is the object within his own box. The meaning of the word 'beetle' will not be a private object; but it will not be a public object either. It will not be an object of any sort. Nevertheless, it will surely be false to say that what people mean by the word 'beetle' will be independent of the nature of the inhabitants of their boxes. (The meaning of 'beetle' will vary as 'X' varies.) Does a similar situation not hold in our actual language, say, when we see someone kicked on the shin and say that he is in 'pain' ? Might we not mean to say that he is experiencing a particular subjective state similar to the state which we generally experience when we are kicked on the shin ?

There is an intimate relationship between language, concepts and classification. Classification is obviously crucially important for the acquisition of knowledge; it is difficult to see how a systematic coherent knowledge of the world would be possible if we could not classify the objects of our experience into groups and types. Furthermore, to call certain beliefs 'knowledge' in contrast to 'conjectures' or 'myths' is to have been engaged in an act of classification. Perhaps the two most fundamental questions regarding classification are these. Firstly, what is involved in being a member of a conceptual class; what is it to be an X ? Secondly, how do we recognise that particular things belong to particular classes ? How do we recognise, say, tables and chairs as tables and chairs ?

Locke answers both these questions in terms of his theory of ideas. Locke argues, that to be an 'X', for example, a 'man', is to be correctly called a 'man' and to be correctly called a 'man' is to correspond to or agree with our 'idea' of 'man':

> For when we say this is a man, that a horse; this justice, that cruelty; this a watch, that a jack; what do we else but rank things under different specific names, as agreeing to those abstract ideas, of which we have made those names the signs ? (Locke, 1964, P.268)

According to Locke, we recognise objects such as tables and chairs as tables and chairs because they agree with our 'ideas' of tables and chairs. How do we form 'ideas' such as the 'idea' of a table and the 'idea' of a chair ? Locke argues that we form such 'ideas' by a process of mental abstraction; having observed numerous tables and numerous chairs, we abstract from the features peculiar to particular tables and to particular chairs and form our 'ideas' of 'table' and of 'chair' with the features which are common to all chairs and the features which are common to all tables. Thus when people form an 'idea' of, say, 'man' they ' ... only leave out of the complex idea they had of Peter and James, Mary and Jane, that which is peculiar to each, and retain only what is common to them all'. (Locke, 1964, P.265)

Locke speaks as though our class concepts such as 'table' and 'chair' must be what we might call 'essentialist ideas'. He writes of an 'idea' such as 'table' that ' ... everything contained in that idea is essential to that sort'. (1964, P.283) This is similar to the view expressed by Durkheim. Durkheim believes that we should seek the 'essential properties' of things when we want to define them. In this spirit, he argues:

> The essential properties of a thing are those which one observes universally whenever that thing exists and which pertain to it alone. If, then, we wish to know what crime essentially is we must extract the elements of crimes which are found similar in all criminological varieties in different social systems. (1969, P.70)

In terms of this 'essentialism', we might say that to be an X is to possess the <u>essential</u> features necessary to being an X; to be an X, for example, a man, is to share the features which are necessarily common and peculiar to men. Locke's theory seems extremely plausible until we actually try to produce a Lockean definition. For example, we can all recognise 'mankind'; we do not confuse people with crocodiles or with apes or tables

and chairs. Yet, if we try to discover what features are common and/or peculiar to human beings we have great difficulty. If we leave out of our 'ideas' of James, Mary and Jane the features peculiar to each and retain only that which is common to all it is difficult to see what we could retain and it is far from obvious that such a residue actually would approximate to our 'idea' of 'man'. Wittgenstein offers a solution to this 'problem of universals'. (See Bambrough, 1960)

Wittgenstein did not specifically ask the logical question 'Must all Xs share features common and/or peculiar to Xs ?' He, rather, approached the question by actually looking at the way class concepts are used in language. He considered the way we use the word 'game' and concluded that the rules of our language are such that properly to be called a 'game' need not be to share features which are common and/or peculiar to games. Our language is such that properly to be called a 'game' is to share to a greater or lesser extent a variety of features.

> I can think of no better expression to characterize these similarities than "family resemblances" ; for the various resemblances between members of a family: build, features, colour of eyes, gait, temperament, etc., etc., overlap and criss-cross in the same way. And I shall say: "games" form a family . (Wittgenstein, 1958, P.31: 66-7)

Games do not necessarily share features common and/or peculiar to games: to be a game is to possess some or all of a variety of features; there need not be any particular feature which an activity must have in order to be a game, nor need the possession of any particular feature be sufficient for an activity to be a game. Games might share features - eg. being expensive or providing pleasure - with activities which are not games.

Must all Xs share features which are common and/or peculiar to Xs ? Wittgenstein implicitly answers this logical question with a 'No'. Is this a correct answer ? Suppose we say:

> Let us define the term "X" in such a way that to be an X is to share a variety of the features A, B, C and D where there is no particular feature in this list which an object must have in order to be an X and where the features A, B, C or D may be possessed by objects which are not Xs.

Is there any logical contradiction involved in suggesting that we

might define 'X' in this way ? If not, Wittgenstein answered the question correctly. It appears to me that there is no contradiction in outlining a possible family resemblances definition of a word. Of course, Wittgenstein does not argue that definitions must be of the family resemblances type. He does not suggest that it is not possible to produce Lockean essentialist definitions; he only demonstrates that it is not necessary to do so. We shall consider the prescriptive question of whether we should adopt family resemblances type definitions or essentialist definitions in the next chapter.

What do all Xs have in common ? As a matter of fact they may all share a common feature; perhaps they were all made in Britain. Yet, in order for them all to be Xs, they need have nothing in common other than the fact that they are Xs (although we could choose to define the term 'X' such that to share features in common with other Xs became a logically necessary condition of being an X). Wittgenstein argues that games have nothing in common other than being games. What is the point of saying that all games are games. Is this not merely a trivial platitude ? It is a platitude but it is a significant platitude. The point of Wittgenstein's saying that all games are games can be fully appreciated only when we realise what he is denying when he says it:

> Now the platitude that all games have in common is that they are games is denied by the nominalist who says that all games have nothing in common except that they are called games. And it is not only the nominalist, but also his opponent, who misunderstands the central importance of the platitude that all games have in common is that they are games ... he feels that he must look for something that games have in common apart from being games. This feeling is entirely misplaced. (Bambrough, 1960)

Wittgenstein denies that all members of a class must necessarily share a feature or features in common; he also denies that all members of a class have nothing in common other than being called by the same name.

Wittgenstein specifically developed his theory in terms of the use of words but it is quite possible to consider family

resemblances in terms of meaning. In so doing we can produce a marriage between some of the theories developed by Locke and those developed by Wittgenstein. We might modify Locke's theory slightly and say that what the word 'man' means is our concept of 'man'. To call it a concept rather than a percept is to stress that it is not a mental picture; it is mental content which is not totally derived from sensory perception. Let us retrace Locke's argument. What is it to be an X ? It is to be correctly called an X, or, rather, since not everything which might correctly be called an X actually will be called an X, it is to be such that if it were called an X, it would be correctly so called. What is it to be correctly called an X ? It depends what the word 'X' means. What the word 'X' means is our concept on an 'X'. What the word 'man' means is our concept of a 'man'. To be a man is to correspond to or agree with our concept of 'man'. Yet, our concept of 'man' need not be an essentialist concept: we may have a family resemblances concept of man; we may have a concept of man such that to be a man is to share to some extent a variety of features; we may define the word 'man' such that to be a 'man' means to share some of such features. How do we recognise objects such as men as men ? We have a concept of 'man' such that to be a man is to share certain features. We recognise that certain objects are men because we recognise that they possess these appropriate features. How do we recognise our concept of man as a concept of man ? We do this through learning the use of words in language. We simply learn and remember that a particular word 'man' is related to a particular concept of 'man'. Logically prior to any act of classification is the pure concept of class membership. To classify anything as a man or a crocodile or an X is implicitly to have answered the logical question 'What is it to be an X; what is involved in belonging to a conceptual category ?'.

There are three linguistic activities which we should be careful to distinguish; namely, naming, classifying and defining. People often talk about defining things; for example, sociologist often talk about an actor's definition of his situation. (See Thomas, 1951, P.88) But, if we are to think

clearly about the operations of language, we should perhaps say that we define words rather than things. To define a word is to say what the word means; to define the word 'man' is to express our concept of 'man'. If we define a word in a certain way, then the word has, by definition, the meaning we ascribe to it. In a sense, it is quite arbitrary how words are defined. The words 'table' and 'chair' both have a particular meaning yet there is no reason why our language could not have developed differently, so that the meanings of both words might have been reversed. In a sense, it is more or less arbitrary that tables and chairs are called 'tables' and 'chairs' rather than vice versa. In this regard, we must distinguish between naming and classifying. It is perhaps arbitrary that the word 'table' has been defined in a particular way, but, given that the word has been so defined, it is not arbitrary that particular objects are called tables; such objects actually are tables. In terms of our definitions of words such as 'table' we classify actual objects as, for example, tables.

We might simply think that 'table' is the name of actual tables, but it is important not to confuse naming with classifying. For example, I am named Hugh and I am classified as a man. I am named Hugh, but I could just as readily have been named Peter, Alasdair or James. I was not classified as a Hugh. At birth, my parents did not examine me and call me Hugh because they thought I possessed features associated with Hughs. We would hardly say that they could have made a mistake when they called me Hugh and that it would have been more accurate to have called me Alexander. On the basis of meeting a great number of Peters, we could not approach a total stranger and reasonably say, 'Excuse me, I recognise that you are a Peter'. I was arbitrarily named Hugh but I am correctly classified as a man; I actually am a man. I possess features associated with men. If someone classified me as a crocodile, a salmon or a woman then he would have made a mistake; he would have incorrectly classified me. I would still be a man even if no one ever called me a man. If I was not called Hugh then I would not be a Hugh. A process of re-naming could turn a Hugh into a Peter instantly but no process

of re-naming or re-classifying could turn me from a man into a stone. Of course, Hughs do form some sort of a class; in a sense, I would now be correctly classified as a Hugh. But this is a class of a very peculiar type: the necessary and sufficient condition of membership of this class is to be called Hugh; it is what we might call a 'nominal class'. This is quite different from the membership qualifications for a class such as 'tables', 'men' or 'societies'. To be a table or a society is to possess certain features. All tables and societies actually are tables and societies even although they may not all be called tables and societies. They would continue to be tables and societies even if the words 'tables' and 'societies' ceased to be used. All Hughs are called Hugh. It is irrelevant what features they possess: men, dogs, cats, horses, continents, planets or teddy bears could all share the name of 'Hugh'.

We define words rather than things; we classify and name things. We can classify things correctly or incorrectly while the naming of things is more or less arbitrary. At least, such naming is arbitrary from a philosophical or epistemological point of view. For example, I was not named totally without reason. I was named after my grandfather and, for obvious reasons, not given a girl's name nor a name comprising one thousand letters. It is possible to classify phenomena in various different ways depending on the features whose possession we consider to be important for particular purposes. People are often quite impressed by the fact that people in different societies often use systems of classification which are quite different from our own. Yet, we should not conclude from the possible and actual diversity of classificatory systems that it is arbitrary which one we adopt for any particular purposes or that there is no rational justification for adopting any classificatory system. Similarly, we should not conclude that ony one system must be better than another system for all purposes, nor, indeed, that any system must be just as good as any other system for any or all purposes.

> In no case will it appear that we must choose between rival systems of genuine classification of a set of objects in such a sense that one of them is to be recognised as the classification for all purposes. There is no limit to the number of possible classifications of objects. (The nominalist is right about this.) There is no classification of any set of objects which is not objectively based on genuine similarities and differences. (The realist is right about this.) (Bambrough, 1960)

To say that the meaning of class terms such as 'table', 'chair' or 'society' is a concept rather than percepts and to say that language can express conceptions is to say that we should not expect the meaning of language to be reducible to sensory perception. Hence, social scientists should not necessarily discard such terms as 'society', 'social class' 'the spirit of capitalism' merely because the meaning of such terms does not seem to derive from sensory perception. As we saw in Chapter Two, if the meaning of words were simply other words, then it is difficult to see how we could break out of this verbal circle and discover what any word means. We can see how this verbal circle can be broken if we allow that we are able intellectually to grasp what words mean. And we need not imagine that such intellectual grasping of mental content involves any sort of mystical intuition. As Rickman shows, such 'understanding' is a commonplace rational activity. We can grasp the point of distinguishing epistemological sociologism from individualism not by any mystical means but basically by noting how both terms are actually used. In Chapter Two, we considered Wittgenstein's argument regarding the possibility of a 'private language' and it looked as though we should perhaps conclude that personal mental states must be unrelated to the meaning of language. If this were the case, then it would seem to follow that social scientists should not try to classify human behaviour in terms of the mental states of the people involved, nor should they attempt to explain human behaviour in terms of private mental states such as hopes, aspirations or motives since it would appear that the meaning of the language which we use will be quite unrelated to the nature of these mental states just as, in Wittgenstein's imaginary 'language-game', the meaning of the word 'beetle' is held to be unaffected by the nature of people's individual 'beetles'. If we agree with

Rickman that we can grasp mental content, then we can avoid Wittgenstein's conclusion. If personal mental content can be expressed and grasped through language then we can reasonably try to classify and explain human behaviour in terms of mental states.

It might be illuminating to apply Wittgenstein's theory of family resemblances to the class terms 'science' and 'philosophy'. We may want to use an essentialist concept of 'science' such as that presented by Popper but we do not have to do so. We may use a family resemblances concept of 'science' : we need not conclude that all scientific activities necessarily share anything in common other than being scientific activities. Similarly, perhaps all activities properly called philosophical have nothing in common other than being philosophical. Some philosophy is concerned with the elimination of linguistic confusion. Some philosophy issues in definitions and embodies conceptual revisions. On some occasions, philosophers merely try to show that particular arguments or particular types of arguments are invalid. At other times, philosophical activity results in the presentation of prescriptive, evaluative or interpretative assumptions and conclusions. And perhaps there are other defining features which activities possess by virtue of which they are philosophical activities.

A refutation of epistemological sociologism

With the aid of the conclusions we have reached in this chapter, we can try to refute some of the theories of epistemological sociologism which were presented in Chapter Two.

We can use the distinction between naming, classifying and defining to refute the theory of Berger and Luckman. They argue that the existence of things is not independent of the social definition of things and that 'reality' is socially defined. As we have seen, we do not define things, we define words or symbols. We could not define reality; we could define only the word 'reality'. What the word 'reality' means is a matter

of definition but reality itself is quite unaffected by our use of the word 'reality'. Nothing at all could exist by virtue of social definition. The definition of words could not affect the existence of things. Berger and Luckman argue that to be a 'knower' is to be socially defined as a knower. We might be tempted to think that if someone is 'socially defined' as a knower then, by definition, he is a knower. If we talk of naming and classifying rather than defining things, then this temptation should disappear. Certain people may be socially classified as 'knowers' yet it does not follow that they actually are 'knowers'. They may have been misclassified as knowers just as countless people in the Seventeenth Century were called witches when they were not witches and were incorrectly classified as witches.

The distinction between perception and conception should enable us to solve some of the problems which were highlighted by Kuhn. Kuhn argues that we cannot talk of 'reality' as something independent of our 'paradigms'. People with different paradigms see the world differently. For example, after a particular paradigm shift, Uranus which was previously seen as a star came to be seen as a planet. (See Kuhn, 1970a, P.115) Similarly, Lavoisier and Priestley are said to have been operating in different 'worlds'; where Lavoisier saw oxygen, Priestley is said to have seen de-phlogisticated air. (See Kuhn, 1970a, P.118) The superficial plausibility of Kuhn's theory stems from an ambiguous use of the word 'see'. We do not 'see' oxygen or de-phlogisticated air in the same sense that we 'see', say, tables or red flowers. How could we see oxygen if, as we are all told at school, it is gaseous and colourless ? What Lavoisier conceived of or classified as oxygen, Priestley classified as de-phlogisticated air. Yet, we need not conclude that there was any difference in perception between them or that in the visual sense of the term that they saw the world differently. We might look at a jar and classify its contents as oxygen and then we might change our mind and classify the contents as de-phlogisticated air. Yet, we need not imagine that our change in conception necessarily produced a change in perception.

The impression (in the Humean sense of the term) that we experienced in both instances need not have altered. Similarly, we might have thought that a particular object was a star and then have changed our mind and imagined that it would be better classified as a planet. It would not follow that on looking at what we now consider to be a planet we will experience a different impression from that experienced when we looked at what we previously thought was a star. Kuhn also suggests that a gestalt switch divides his readers into two camps, roughly, those who agree with him and those who do not. (See Kuhn, 1970c, P.231) This surely indicates a fundamental confusion between conception and perception. It is quite plausible to suggest that different readers have a different conception of the meaning and worth of Kuhn's books. Yet, surely those who accept his conclusions and those who reject them do not perceive his books in a radically different way ? If they did, how could they ever understand anything that Kuhn had to say ?

If we consider the distinction between perception and conception then we can deny that what appears to us as the 'world' is necessarily socially produced and that ' ... when paradigms change, the world itself changes with them'. (Kuhn, 1970a, P.111) After a 'paradigm shift', the scientists conception rather than ' ... the scientists perception of his environment must be re-educated ...'. (Kuhn, 1970a, P.112) No doubt, the conceptions we have are influenced by our social environment, but we need not conclude that they are completely socially imposed upon us. Different classificatory and conceptual schemes can be compared and rationally evaluated. We can test whether Uranus is better classified as star or as a planet. We can ascertain whether the features it possesses are associated more with the concept of a planet (i.e. what the word 'planet' means) than with the concept of a star. Differing concepts and differing conceptions can be communicated and hence people with different paradigms can communicate their differing conceptions and try to evaluate them rationally.

Kuhn also writes that knowledge must be social rather than private for the same reasons that language is the common property of a social group. (See Kuhn, 1970c, Pp.270-277) It is not clear precisely what is the point of this argument especially since we can surely allow that people might have secret knowledge which they never tell to anyone. Perhaps if we accept Rickman's claim that through 'understanding' we can grasp personal mental content, then Kuhn's argument would appear beside the point. Indeed, much of the sting vanishes from epistemological sociologism if we assume that personal conceptions might be known to be either true or false and that although such conceptions can be expressed in language, they are not vested in language; conceptions are distinct from the language used to express them.

According to Winch, what a belief or concept means is its use in language; different language usages are incommensurate; hence the beliefs found in different language communities are incommensurate. Consider the Zande belief in witchcraft as expressed in Zande language. What does this belief mean ? Beliefs do not mean anything. What has a meaning is the words used to express the belief; they mean the belief. Because of their particular use in Zande language, these words will have the particular meaning that they have. Winch is obviously correct in maintaining that it is a matter of social convention which words express which beliefs but it is a mistake to imagine that this linguistic convention is the belief. Suppose that we want to compare our views on witchcraft with those of the Zande, and that, unlike their belief, ours is expressed in English. What problem do we have ? Our differing use of words might correspond to differing ways of life, but what difference does this make ? Our beliefs are not the use of words although they are expressed by our use of words. The truth or falsity of any beliefs will be totally independent of the language used to express such beliefs. Beliefs which are conceptions that something or other is the case must be either true or false. Such truth or falsity is unaffected by the sociological pedigree of the belief or the words used to express it. The existence of

different language communities and different forms of social life does not affect the possibility of non-socially variable truth and falsity. Mental content is an individual product; the forms of expressing it are social.

The concept of a Paradigm

By 'knowledge' is meant true, justified belief. It seems plausible to suggest that beliefs are a type of conception rather than, as Hume claims, a particular type of percept. When we believe, we believe that something is the case; beliefs are either true or false. Although the means of expressing them are social, beliefs are a type of mental content hence it seems quite reasonable to claim that knowledge might be the property of individual 'knowers'. Epistemological individualism is a perfectly tenable theory. The mind plays an active part in the acquisition of knowledge by the production of concepts and conceptions. Knowledge of the 'external world' is the outcome of a dialect or of an interplay between conception and perception. Our only experience of an 'external world' is through our 'impressions' and 'ideas' but we can gain knowledge of a reality external to these 'impressions' and 'ideas' since we can form conceptions relating these impressions and ideas to a supposed 'external world' and we can test such conceptions. We can thereby attain knowledge of what the world is 'really' like. For example, we might look at a kettle and experience 'impressions' and 'ideas'. We can form the conception that we are actually looking at a physical object which is external to our 'impressions' and 'ideas'; we can classify our sensory experience as 'ideas' and 'impressions' of an external, independently existing kettle and we can test our classification. For example, we might touch the kettle, we might throw it into a pool and listen for a splash; we might photograph the kettle and compare the photograph with our 'impressions' and 'ideas' of the kettle. As we have already seen, the 'understanding' of 'expressions' involves a similar activity of testing conceptions in the light of our perceptual experience. By such an interplay between our conceptions and perceptions we can gain knowledge of an actual physical object external to our 'impressions' and 'ideas'.

We can thus have good reason to believe in the real existence of physical objects and we can have good reason to believe that they actually do behave in particular ways. Epistemological individualism need not imply that we are trapped within a web of our own 'ideas'.

Philosophical assumption is logically prior to knowledge of the 'external world' i.e. the reality which is external to the knowing self including the physical body of the knowing self. For example if we assume that we have gained such knowledge we must assume that such knowledge is attainable and as we have seen from discussing the theories of Popper and Hume, this is not an uncontroversial assumption. We must assume that an 'external world' exists and we must accept, at least implicitly, a theory of perception which posits some relationship between our 'impressions' and 'ideas' and the 'external world'. Logically prior to the recognition of knowledge is the concept of knowledge; we could not discover that any of our beliefs were true without the prior assumption of the concept of truth. No systematic knowledge of the world would be possible without classification and any act of classification must implicitly assume a concept of class membership. We might classify an object as an A: do we mean that it shares features which are common and peculiar to As; do we mean that it shares some of a cluster of features ? We could not have knowledge of As, of tables or chairs without having implicitly answered this question. Knowledge is a particular class of belief; it is justified belief. Justification logically presupposes the evaluation of the particular beliefs. Such evaluation will be in terms of logically prior prescriptive standards. To say that a belief is justified is to say that it conforms to the canons to which beliefs should conform if they are to be considered justified. Such canons and such evaluation are therefore logically prior to the attainment of knowledge. Concepts and conceptions are logically prior to knowledge and we have a measure of choice as to which conceptions and concepts we accept. We can rationally evaluate competing logically possible conceptions and concepts. For instance, as I shall try to show in the next

Chapter, there are different conceptions of causation and explanation which we could choose; we can give good reason for choosing a particular conception of causation and give good reason for accepting a particular conception of explanation. Again, Locke defines 'knowledge' in such a way that, by definition, we can only 'know' that which we can logically prove. Since we cannot logically prove matters of contingent fact, we have a good reason for replacing Locke's concept of knowledge with the concept of true, justified belief when we are discussing the possibility of 'knowledge' of matters of contingent fact. Such is the dialectic between our knowledge and our philosophical assumptions that, although logically, prior to knowledge of contingent facts, such assumptions themselves become knowledge. For example, the assumption that knowledge is possible is logically prior to the attainment of knowledge but the consequent attainment of knowledge justifies this assumption and renders it knowledge. Similarly, a presupposition of our knowledge of the 'external world' is the assumption of a more or less stable and systematic relationship between our percepts and the 'external world'. We assume that when we look at, say, a kettle, we generally experience similar impressions on the different occasions when we do so or if we do not experience similar impressions, it is possible to account for the difference; perhaps our senses are impaired through tiredness, drink or drugs or perhaps we are looking at the kettle in a different light. This assumption renders knowledge of the 'external world' possible and such obtained knowledge justifies the theory of perception.

Knowledge is the conception that X is the case when X is the case and we believe and are justified in believing X to be the case. But how can we justify such conceptions if, since it is not logically contradictory to deny them, we cannot logically prove them to be true ? Such conceptions similarly could not be logically proved to be false. Would anyone argue that we should believe every contingent proposition because it might conveivably be true ? It is hardly more rational to doubt every contingent proposition because it might conceivably be

false. Doubt is inherently no more rational than belief; neither is it necessarily particularly rational to 'sit on the fence'. Belief is often far more rational than either doubt or sitting on the fence. For example, any adult member of our society would be acting totally irrationally were he seriously to doubt that the earth is, more or less, spherical or that sex often contributes to the production of babies. It would be quite wrong to imagine that some form of 'philosophical doubt' regarding these matters was in the least rational. Popper and Hume both suggest the view that we can only justify a belief which can be logically proved. This is not merely a speculative belief, it is surely self refuting since it is a belief which cannot itself be logically proved. It is not contradictory to deny it. To show that a proposition cannot be logically proved to be true or to be false is not to prove that the proposition cannot be known to be true or to be false; it is not even to give an adequate reason for believing that it cannot be known to be true or to be false. We justify our conceptions by giving reasons for their acceptance. We cannot logically prove that the earth is more or less spherical but there are very good reasons for believing that it is so shaped. For example, those who have been fired into space have testified that from a distance, the earth always appears as a circle although it is known to revolve on its axis. We can set off from any point on the earth and arrive back at the point approaching it from the opposite direction to which we set off. These are very good reasons for believing that the earth is more or less spherical although they do not constitute a deductive proof. And similarly, the very good reasons for believing that an interaction between male sperm and a female egg contributes to the production of babies are too well known to require retelling.

Philosophers of science often lay great emphasis on constant conjunctions regarding the verification of beliefs. But observed constant conjunction is neither a necessary nor a sufficient condition for holding justified beliefs; it is often of little or no importance. For example, we know that the earth is more or less spherical but the justification of this has

little to do with observed constant conjunctions. It is, rather, the juxtaposition of a host of facts which give us reason to believe that the earth is virtually spherical. Given that the earth is a sphere then a whole host of facts 'fit'; taken together they produce a coherent structure. And similarly there is no generally known constant conjunction between sex and babies. Our knowledge here is verified not by the observed constant conjunction of events but by the systematic variation of and the juxtaposition of a number of facts. The belief that sperm and eggs produce babies coheres totally with a mass of other facts and it is this overall coherence which is the chief justification of our beliefs. We do not think that astrology produces knowledge because we do not think that we have any good reason to believe that astrological theories are true. Yet, we could be quite prepared to believe that astrologers are able to produce volumes of attested astrological constant conjunctions; they might even be able to furnish libraries with unfalsified universal laws. We are justified in denying that astrology produces knowledge on the grounds that astrological theories simply do not cohere with what we already know about human behaviour. Given what we know, the theory that the spatial arrangement of physical objects placed at a distance from us can affect our personalities and behaviour does not make much sense. Of course, it is logically possible that astrological theories are true and that what passes for knowledge is completely false but, given the evidence presently available to us, we have better reasons for accepting what is considered as knowledge of human behaviour that we have for accepting the speculations of astrologers.

The major justification for accepting any element of knowledge as knowledge is its coherence with the other elements of our knowledge, just as the justification for imagining that we have correctly placed any particular jig-saw piece is the part it plays in the overall picture. Often beliefs do cohere with prevailing evidence and what is accepted as knowledge and yet prove to be false. One such belief may have been the belief in the existence of 'phlogiston'. But, in such cases, we can show

the falsity of coherent beliefs only by showing that they do not particularly cohere with an expanded or modified framework of knowledge and evidence. No knowledge is sought from scratch; all knowledge is sought within a framework of knowledge and a context of facts and evidence. Given the role that coherence plays in the justification of beliefs, I think we should accept as a general principle that any claims to fresh knowledge should cohere with what is already known. We should not classify any belief as knowledge if it cannot be shown to cohere with our current framework of knowledge and our current context of facts and evidence. Of course, to say this is not to discourage bold speculation at variance with what is accepted as knowledge or to deny that bold speculation is very often crucially important to the expansion of knowledge: it is merely to say that such speculation is speculation.

Of particular relevance to the justification of our beliefs is causation, a concept we shall consider in greater detail in the following Chapter. The epistemological importance of causation is this. As Hume shows, from the fact that A and B have been observed to be conjoined on any number of occasions, we can deduce nothing as to the unobserved instances of A and B. Furthermore, as I hope to show in the following Chapter, mere observed conjunction of itself gives no reason to believe that conjunction has occurred or will occur in unobserved instances. The mere fact that sperm and egg have been conjoined with the emergence of a baby in any number of cases gives no reason to imagine that future interaction of sperm and egg will be conjoined with the production of a baby. But, suppose we introduce the idea of causation where by a cause we mean something that 'physically necessitates' or 'produces' an effect. If we could justify the conception that A causes B, say, that the interaction of sperm and egg causes the production of a baby, then we could know that whenever A occurs then B will occur; whenever sperm and egg interact a baby will be produced. In such a way we might product a systematic knowledge of the world and our knowledge would not be limited to knowledge aquired by direct observation. That such causal conceptions can be justified I aim to show in

in the next chapter.

Philosophy plays an active part in the acquisition of knowledge. Philosophy has a prescriptive, an interpretative and an evaluative role to play. Science is an activity which can fail or succeed. In order to judge any scientific activity a success i.e. in order to judge that knowledge has been thereby acquired, we need a prescriptive standard against which the activity of science can be measured. We also require the philosophical activity of evaluation; we need to evaluate the fruits of science in order to decide whether it is appropriately classified as knowledge. Philosophy plays an active part in the acquisition of knowledge, yet such philosophical assumptions need not be produced by someone who happens to be a professional philosopher. Philosophy rather than philosophers is logically prior to knowledge. To accept any beliefs as known to be true is implicitly to have accepted certain philosophical assumptions no matter what the professional status of the believer is. For example, suppose that professional scientists were in some doubt as to whether or not to classify Einstein's theories as knowledge. We would not necessarily imagine that the evaluation of Einstein's theories would be better done by professional philosophers. The point is that to accept any theory as known to be true is to have indulged in the activity of philosophy. It is to have accepted a certain standard to which a theory should conform to be accepted as knowledge and it is to have assumed that the theory meets this prescriptive standard. It is to assume that the theory would be correctly evaluated as knowledge.

Philosophy prescribes that we adopt certain 'categories' in order to understand the world. We interpret and classify our experience of the world in terms of these 'categories'; for example, we classify certain beliefs as knowledge and we classify certain mental contents as perceptions of an 'external world'. We evaluate our acts of classification in order to tell whether we have correctly classified our thoughts and experiences. There is thus a systematic relationship between the philosophical

activities of prescription, interpretation and evaluation and hence there must be a systematic relationship between philosophy and knowledge. Philosophy prescribes that beliefs should take a particular form to be considered as knowledge; it prescribes a particular category of knowledge. We have seen that we can give reasons for prescribing this particular category of knowledge. Science has traditionally been concerned with offering explanations. When would we allow any particular theory to be explanatory ? Well, it depends upon our prescriptive standard of explanation; it depends what category of explanation we adopt. I aim to show in the next Chapter that it is not arbitrary what category of 'explanation' we adopt, nor is it a matter of linguistic convention; we can give reasons for adopting a particular concept of explanation. I similarly want to show that we can give good reasons for prescribing that in trying to gain knowledge and explanations of events in the world, we should seek causes rather than constant conjunctions or universal laws. We should interpret our experience in terms of causes and effects and interpret our theories in terms of the categories of knowledge and explanation. As we shall see, certain interpretative and evaluative problems will be raised by an attempt to meet this prescription.

I want to call the prescriptive, interpretative and evaluative assumptions implicit in the attainment of knowledge 'paradigms', borrowing the term from Kuhn. For example, the assumption that events in the world are the effects of causes is an interpretative paradigm and the assumption that we should explain events by quoting their causes is a prescriptive paradigm. An overall system of prescriptive, interpretative and evaluative assumptions within which knowledge can be attained I aim to call a 'Paradigm'. A Paradigm is logically prior to the attainment of knowledge. In Part 11, I want to consider one such Paradigm which might be used in social science.

A Paradigm is a system of concepts, such as the concepts of 'knowledge', 'cause' and 'explanation' and of conceptions, such as the conception that our percepts are percepts of an

'external world'. The prescriptive, interpretative and evaluative conceptions comprising a Paradigm must be either true or false since they are conceptions that something is the case. For example, if we prescribe that we should seek causes rather than universal laws, what we prescribe will be either true or false: it will be true if it is the case that we should seek causes rather than universal laws; if it is not the case that we should seek causes, our prescription will be false. Although philosophical claims can share with straight forward empirical and scientific claims the feature of being either true or false, I do not think that we shall find much difficulty in distinguishing paradigms from empirical and scientific claims. For instance, philosophical theories tend to be more abstract and general than empirical and scientific theories: we would call the general assumption that mental phenomena can affect physical phenomena a philosophical assumption while considering a more specific theory such as that worry can cause stomach ulcers an empirical theory. Paradigm construction is a rational activity; we can give reasons for preferring a particular paradigm to its rivals. I hope to give a demonstration of this point by rationally evaluating rival paradigms.

To talk of the system of assumptions implicit in the attainment of knowledge as a Paradigm might tend to suggest a static relationship between philosophy and knowledge. To talk of a Paradigm is to take, as it were, a 'snapshot' of our knowledge at a particular time. There is a constant dialect between our philosophical assumptions and our knowledge. In the light of our philosophical assumptions, knowledge is apprehended and in the light of such knowledge we modify our philosophical assumptions. Knowledge is the outcome of a constant interplay between our Paradigm or paradigms and our perceptual experience. It is perfectly true that we can never step completely outside our Paradigm in order to test our Paradigm but it does not follow that our Paradigm cannot be rationally evaluated. We cannot check our paradigms all at once but we can systematically check and modify individual paradigms in the light of our perceptual experience. Let us reconsider Wittgenstein's illustration of

a blind man asking us whether we have two hands. If we want to check that we have two hands, we might touch one hand with the other and try to fumble for a possible third hand or we might simply look to see whether we have two hands or we might ask a friend to tell us how many hands we have. No matter what test we use, we must take certain assumptions for granted during the test. We must assume, for example, that our sense of touch is not deceiving us or that our eyesight is not impaired or that our friend is trustworthy. Yet these assumptions which are taken for granted during this specific test can themselves be tested on other occasions although other assumptions will have to be taken for granted during these tests. This does not show that what we accept as a body of knowledge cannot be rationally evaluated; it shows only that no one specific test is sufficient for its assessment. Similarly, no one specific test is sufficient for us to be able rationally to assess an entire Paradigm but it can be rationally evaluated through the evaluation of different individual paradigms on different occasions. In any case, why should we assume that it is always irrational to take assumptions for granted; why take that particular assumption for granted ? Generally, it is not rational to assume that our senses are not deceiving us unless we have some specific reason for <u>doubting</u> it ? Think of someone driving a car on a busy road. Are there not assumptions which it would generally be irrational <u>not</u> to take for granted ?

CHAPTER FOUR

CAUSATION AND EXPLANATION

In the previous Chapter, it was argued that philosophy has an active part to play in the acquisition of knowledge. Among the numerous writers who oppose this view, none is more vociferous than Durkheim who believes that an alleged separation of sociology from philosophy is an indication of the scientific autonomy and of the scientific maturity of the discipline of sociology. Writing of the method appropriate to sociological study, he concludes:

> First, it is entirely independent of philosophy .... Sociology does not need to choose between the great hypotheses which divide metaphysicians. It needs to embrace free will no more than determinism. All that it asks is that the principle of causality be applied to social phenomena. Again, this principle is enunciated for sociology not as a rational necessity but only as an empirical postulate, produced by legitimate induction. (1964, P.141)

But, how could 'legitimate induction' establish the principle of causality ? Even if we lay aside the question of whether induction ever could be 'legitimate', Durkheim's view appears implausible. As we have seen, causation is imperceptible and since we cannot perceive a cause necessitating the occurrence of an effect of any occasion, how could causality be established by induction from any number of such occasions ? Causality is not simply inductively established by the repitition of the perceptual experience of causality: causality is, rather, a conception; the principle of causality offers a particular interpretation of our perceptual experiences. It is quite possible to reject causality as an interpretation of our experience of the world. To deny that what are called causes

actually physically necessitate the occurrence of what are called effects is to adopt a perfectly respectable philosophical position, a position which we have already discovered in the writings of Hume. Hence, if Durkheim is to accept that 'causes' physically necessitate the occurrence of 'effects', then surely this acceptance must be the adoption of a rival philosophical position.

Durkheim's book on the philosophy of sociology, The Rules of Sociological Method (1964) could perhaps be best interpreted as an attempt to formulate a Paradigm for sociology. In this book, Durkheim presents a philosophical framework within which he believes sociological study can proceed and prescribes the adoption of this framework of assumptions. For example, he presents the prescriptive paradigm: 'Consider social facts as things'. (1964, P.14) He further prescribes that we should try to explain 'social facts' by giving their causes (although, as we shall see, Durkheim's prescription is ambiguous on this point). Among Durkheim's interpretative paradigms is that 'social facts' are always the effects of other 'social facts' and from this is derived the prescription that 'The determining cause of a social fact should be sought among the social facts preceding it and not among the states of the individual consciousness' (1964, P.110) and the evaluative paradigm that ' ... every time that a social phenomenon is directly explained by a psychological phenomenon, we may be sure that the explanation is false'. (1964, P.104) To complete this skeleton of Durkheim's Paradigm we might note his evaluative paradigm. According to Durkheim, the method which the sociologist must use to evaluate his attempts causally to explain social facts is ' ... the method of concomitant variations or correlation'. (1964, P.130) This method is considered by him to be particularly fruitful since 'In order to obtain results, a few facts suffice. As soon as one has proved that, in a certain number of cases, two phenomena vary with one another, one is certain of being in the presence of a law.' (1964, P.133) Not only does adherence to Durkheim's 'rules' imply the adoption of philosophical assumptions, but the assumptions to which one is thereby

committed are often controversial. For example, the assumption that we can be certain to have established a sociological law on the basis of a few facts is radically opposed to the evaluative paradigms of both Hume and Popper who argue that we could never be certain that we have discovered such a law.

Consider this quotation from Durkheim's study of suicide. After discounting various possible explanations of suicide he writes:

> Wholly different are the results we obtained when we forgot the individual and sought the causes of the suicidal aptitude of each society in the nature of the societies themselves ... the social suicide-rate can be explained only sociologically. (1952, Pp.151,299)

Here Durkheim talks of the causes and of the explanations of suicide. He believes that he has discovered the cause and the explanation of variations in rates of suicide. Logically prior to the recognition, or supposed recognition of any factors as causes and explanations are the concepts of 'cause' and 'explanation'. What are Durkheim's categories of 'causation' and 'explanation' ? What are the reasons for and against adopting them ? Could we formulate any better justified paradigms of 'causation' and 'explanation' ? First of all, let us look at his paradigm of 'causation'.

Paradigms of causation

For Durkheim, causation is not merely a matter of de facto empirical regularity. He conceives not merely that an effect invariably follows its cause but that it is produced by its cause. Given a particular cause, not only will its particular effect occur, it will be 'forced' to occur. Thus Durkheim distinguishes causation from mere regularity or constant conjunctions arguing that in the latter cases 'The antecedent state does not produce the subsequent one, but the relationship between them is exclusively chronological'. (1964, P.118) Durkheim imagines that the bond between causes and effects has not been adequately appreciated by philosophers; he considers that as well as being produced by its cause, an effect also affects its own cause:

> Indeed, the bond which unites the cause to the effect is reciprocal to an extent which has not been sufficiently recognized. The effect can doubtless not exist without its cause; but the latter, in turn, needs its effect. It is from the cause that the effect draws its energy; but it also restores it to the cause on occasion, and consequently it cannot disappear without the cause showing the effect of its disappearance. (1964, P.95)

In Durkheim's account, not only must a particular cause produce a particular effect but any particular effect must be the result of a particular cause; one class of effect must be produced by one class of cause. '<u>A given effect has always a single corresponding cause</u>. Thus ... if suicide depends on more than one cause, it is because, in reality, there are several kinds of suicides.' (Durkheim, 1964, P.128) Durkheim totally rejects the view that a particular effect might on different occasions be produced by a different cause. He believes that such a conception of the causal bond would remove all determinism from it and that it would virtually render a scientific sociology impossible because of the consequent difficulty in verifying causal theories: ' ... it would be necessary to conduct the experiment in an isolation practically unrealizable in sociology'. (Durkheim, 1964, P.127) This supposition of the plurality of causes is a negation of the principle of causality, according to Durkheim, and results from a failure to realise the intimacy of the relationship between cause and effect and to regard them as quite unrelated elements which, as a matter of contingent fact, happen to be causally conjoined. He writes:

> If, on the contrary, the causal bond is something logical, it could not be indeterminate. If it consists in a relation resulting from the nature of things, a given effect can maintain this relationship with only one cause, for it can express only one single nature. (1964, P.127)

According to Hume, causes and effects are events which are constantly conjoined where causes are prior and contiguous to their effects. Hume discounts the view that there is a physically necessary bond between cause and effect and it is this notion of causes 'necessitating' or 'producing' their effects which is central to Durkheim's concept of causation. Hume's reason for trying to remove this notion

of necessity from causation is that such necessity is imperceptible and since no 'impression' of causal necessity could be discovered, talk of causal necessity must be meaningless; but as we saw in the previous chapter, there is good reason to believe that the meaning of language is not completely reducible to sensory perceptions. Since causation is a concept and not a percept the fact that we cannot discover an 'impression' of causal necessity is no good reason for rejecting the notion.

A main advantage of Durkheim's concept of causation is that it enables us to distinguish mere constant conjunction from what we would normally want to consider as a causal relationship. For example, we might find in men a more or less constant conjunction between wrinkles' appearing on their face and their hair's beginning to thin but we would not want to say that the wrinkles caused the balding or that the balding caused the wrinkles. We would want to say that both were caused by some other factor such as growing old. If causation were merely constant conjunction then it could not possibly make sense to distinguish between mere conjunction and what we would naturally think of as causation. Furthermore, if we consider causation simply in terms of constant conjunction it appears impossible ever to account for such conjunction. If we argue that old age actually produces wrinkles and baldness then the conjunctions can be accounted for. The observed regularity in much of our experience gives reason to believe that causation is a good interpretation of this experience. Every day life abounds with experiences for which causation seems to be the only plausible interpretation. If we watch a sculptor carving a figure, what sense does it make to talk of a constant conjunction between the movement of his chisel and the appearance of the figure ? Can we not 'see' that he is producing the carving, that he is causing a particular form to appear ? If we are in a losing tug-of-war team, can we not 'feel' the force which necessitates our unwilling movement ? In a crowded street, our eyes are often drawn towards attractive girls rather than towards old men. Are we not aware of elements in such situations which cause us to focus our attention in particular directions ?

There is a strong epistemological reason for considering causation in terms of causal necessity. Fully to appreciate the important relationship between causation and knowledge let us consider a particular definition of the term 'cause'. Let us define a 'cause' as conditions which are sufficient to necessitate the production of some further state or event which we can call the 'effect' of the cause. As Hume indicated, from the mere fact that two events A and B have been observed to have been constantly conjoined on any number of occasions, we can distil no justified beliefs regarding unobserved instances of A and B. Yet, suppose we know even on one occasion that A caused B; simply because of the meaning of the term 'cause' we can deduce from this knowledge that whenever A occurs B will be caused to occur.

A has been observed to follow B on a large number of occasions; when A has been observed, B has always followed. What reason does this give us for believing that A and B will continue to be conjoined ? In itself, this inductive evidence offers no justification for believing that A and B will continue to be conjoined or that they will cease to be conjoined. Consider specific examples. Suppose we are playing tennis and every time our opponent has served, he has served to our backhand. Are we justified in concluding that he will continue to do so for the rest of the match ? We are playing dice and for two hundred and fifty consecutive throws, the die lands with a six uppermost. Do we have any reason to suppose that subsequent throws will result in similar behaviour from the die ? If a friend goes on holiday and fails to send a postcard to us on five consecutive days does this give a reason to suppose he will not send a postcard on the sixth or seventh day ? If we are in a train heading north from London and frequently look out the window to observe that we are in England, are we justified in concluding that subsequent glances through the window will reveal to us parts of England ?

Observation of regularities in itself does not justify our believing that the regularities will continue or

cease. Yet, such regularities can be evidence for our beliefs when they are considered against the background of a knowledge of causation. What causes our opponent to serve to our backhand ? Is our backhand notoriously weak ? If the die is a 'fair' one and the extraordinary sequence of two hundred and fifty consecutive 'sixes' is the result of chance then we have no justification for believing that the die will continue to fall with a six uppermost. Yet, if we can discover why the die has behaved in this peculiar way, we may be justified in assuming either that it will continue to behave in this way or that it will cease to behave in this way. Is the die 'loaded' ? Will it remain 'loaded' ? In some circumstances, the longer a regularity persists, the more justified we are in assuming that it will cease. Perhaps the more frequently the postman fails to deliver a friend's postcard the more we are justified in assuming that he will do so fairly soon just as we may become more justified in believing that we will soon see Scotland again the more often we observe from the train that we are still in England.

Through knowledge of causes, we can deduce knowledge of unobserved events from knowledge of observed events. The central question now is whether we can actually gain knowledge of causes, whether we can actually know that A caused B rather than simply followed B. And I think we can gain such knowledge. In tug-of-war competitions, we do not merely know that there is generally a conjunction between a team's being stronger and its winning; we know that its superior strength causes it to win when it chooses to use it. Consider too our moral and legal judgements. Suppose we send someone to jail for throwing a bottle at a football match. It makes sense and it appears just to punish and blame the person only if we can know that he actually caused the bottle to fly from his hand and land on the pitch. But surely such punishment is just and such moral blame does make sense simply because we can determine that people cause particular events to take place. There is a further case for adopting this paradigm of causation. If we assume that the world has in

this sense a causal order and that the events within it are the effects of causes then we can assume that the world can be known by us. On the other hand, if we assume that the world has no causal order and that it does not contain effects which are produced by causes and that the events which take place within it simply happen, how could we ever gain knowledge of the world ? Suppose, for example, the world was such that babies simply appeared on the earth totally uncaused, unheralded by midwives and not born to mothers. Even if these babies appeared at regular intervals in regular places so that statistical laws could be seen to describe their appearance how could we ever have knowledge of the future occurrence of babies ? How could we possibly attempt to explain the occurrence of babies ? Hence we might say that to assume that knowledge is possible is to assume that the world is subject to causation. Causation is a category whose acceptance is logically prior to the attainment of knowledge. The knowledge subsequently attainable justifies the category of causation which made it possible. To quote Wittgenstien, ' ... one might almost say that the foundation walls are carried by the whole house'. (1969, P.33:248)

There are two particular problems produced by the constant conjunction view of causation which disappear if we consider causation in terms of the conditions sufficient for the production of an effect. Firstly, if causation means constant conjunction, how can unique events be accounted for since, by definition, unique events cannot feature in constant conjunctions ? If we consider that a cause is the conditions sufficient to produce an effect, then, in principle, unique events can be accounted for. In practice it would be very difficult to verify a causal theory of a unique event but in principle such an event could be accredited to some unique cause or series of causes. Secondly, constant conjunction does not allow for <u>degrees</u> of causation; with constant conjunction causation is an all or nothing affair; events are either constantly conjoined or they are not. This is what Hume means when he writes that there is no medium between liberty or

chance and necessity i.e., constant conjunction. (See Hume, 1969, Pp.447-455) But surely in a tug-of-war competition we want to say that the twenty stone muscle man was causally more responsible for dragging the opposing team to the ground than was his nine stone team mate. Furthermore if we do look for strictly constant conjunctions we are unlikely to find many. We normally would say that sex is a cause of babies but we certainly would not discover a constant conjunction between copulation and pregnancy. Would we therefore say that the relationship between sex and babies is simply the result of chance ? If we consider a cause as conditions sufficient for an effect, we can talk about partial causes where such conditions tend towards the production of an effect but are not of themselves sufficient conditions. For example, we might say that a tug-of-war team caused its opponents to be dragged off their feet. This team is composed of several individuals who were all partial causes of this effect and some of these people were causally more significant than others. Rather than say that smoking causes cancer or sex causes babies we should say that smoking can be a partial cause of cancer and sex can be a partial cause of babies. As we shall see when we come to consider explanation and the verification of causal theories, it is important not to confuse partial causes with what we could call complete causes.

In the paradigms of both Hume and Durkheim there is the notion that a particular class of effect can have only one class of cause. This is implied in Hume's theory of constant conjunction and it is argued for quite strongly by Durkheim. It is a simple matter of logic that if A is a sufficient condition for B then whenever A occurs B will occur. Yet, it does not follow that only A could be a sufficient condition for B and hence it does not follow that whenever B occurs A must have caused it. Various other conditions might have constituted sufficient conditions for the occurrence of B, whatever B might have been. What possible reasons are there for discounting this logical possibility ? Durkheim argues that a proposed plurality of causes would remove determinacy

from the causal relationship, but why should we accept this ? We could easily say that A determined the occurrence of B but that C, D or E on other occasions have determined the occurrence of B. Even in our every day experience, do we not discover classes of effects which have different causes on different occasions ? On different occasions, are there not different causes of our happiness or sadness ? The effect of contraception is surely produced by different methods on different occasions. And similarly, if we take an effect like death it seems obvious that on differing occasions there are different causes of death. We may want to argue that in all these cases there is one ultimate cause of death but what possible reason could we have for believing that there must be some such single ultimate cause of death. And if we say this then surely we must concede that this single ultimate cause of death itself has different causes on different occasions. Even if we discover this ultimate single cause of death how could we possibly know that on some future occasion deaths might not be caused differently ? How could we discount the possibility that there might be conditions which would cause death if they were ever realised but through chance never come about ? Furthermore, even if we take one single event like a victory of a tug-of-war team it seems apparent that there are numerous classes of causes in operation. The fact that the losing team had the sun in its eyes and that it had spent the morning in a public house were very likely causally important factors. Durkheim argues that one class of effect must have one class of cause since the relationship between cause and effect stems from the nature of things and can consequently express only one nature. But in what way is the unity of nature affected by the possibility of the same effect's having different causes ? Why can we not simply say that nature is such that on different occasions the same class of effect can have a different class of cause ? Durkheim also maintains that there is a unique relationship between cause and effect since the bond between them is 'logical' and this leads us to consider the nature of the relationship between causes and effects.

One of the central features of Hume's conception of causation is that the relationship between cause and effect is purely empirical and that a priori anything might be the cause or effect of anything else. What that means is that in the absence of any knowledge of the behaviour of an object or an event, mere intellectual reflection upon this object or event could produce no knowledge of its causes or effects and that such knowledge cannot be produced by our powers of reasoning but only through an observation of the behaviour of the object or event in question. From experience we can discover that A causes B, but a priori reasoning cannot tell us why A caused B rather than C, D or E. It might be said that we can know what caused a particular tug-of-war team to win - they were stronger than the opposition - and we may think that we can see why their superior strength and fitness produced their victory. To this we could reply that such apparent intelligibility between a cause and an effect only occurs through regarding the instance in the light of other knowledge of causal sequences which are themselves unintelligible. Durkheim seems to want explicitly to deny this when he says that there is a 'logical' bond involved. But what possible form could this bond take ? Is Durkheim's assumption of such a bond consistent with his claim that the principle of causality is not a rational necessity but that it is simply 'an empirical postulate, produced by legitimate induction' ? I do not see how Hume's conclusion can be avoided. I think that the relationship between a particular cause and a particular effect can be intelligible or at least appear to be so in the context of knowledge of other causal relations but if we consider any causal relationship in isolation from such knowledge, it appears to me to be unintelligible i.e. we cannot tell why this particular cause has this particular effect. And given that this is so, it does not seem justifiable to discount the possibility that the same effect might have different causes on different occasions.

Furthermore, if the relationship between cause and effect is in this sense 'empirical' we have good reason to

question Hume's view that cause and effect are contiguous and that the cause must be prior to the effect. If we cannot rationally demonstrate what effects any cause must have, why should we assume that its effect must be contiguous and must follow it in time ? How could we possibly demonstrate that cause and effect must be contiguous ? Do we not commonly suppose that the moon causally affects the flow of the tides even although the moon and the sea are not contiguous ? When we consider partial causes the assumption of contiguity seems quite inappropriate. Smoking can be a partial cause of cancer and sex can be a partial cause of babies yet neither smoking and cancer nor sex and the delivery of babies are contiguous. The assumption of contiguity between cause and effect might appear plausible if one is considering a causal relationship between physical objects such as colliding billiard balls but it might be unduly restrictive to consider causation in social science in such terms. An attractive girl might turn our head in a busy street even although she is standing several yards away from us. Sociologists want to study the effect upon human behaviour of social factors such as public opinion. Does it make sense to talk of people and public opinion being contiguous ? And similarly it appears unduly restrictive to assert that a cause must be prior to its effect. A cause could not occur after its effect yet there is no reason to imagine that cause and effect could not be contemporaneous. The pulling of the tug-of-war team causes the rope to move but the team's pull need not be prior to the movement of the rope; both cause and effect could occur at the same time. We can grip a pen tightly and wave our fist in the air; this will cause the pen to move but the movement of our fist need not precede the movement of the pen. Similarly, perhaps some social causes and social effects are contemporaneous; a priori there seems to be no reason to discount such a possibility.

If the causal bond is 'empirical' rather than 'logical' then there seems to be no reason to accept Durkheim's assumption of a causal feedback between cause and effect. If a cause is a condition sufficient for the occurrence of an

effect, what possible reasons are there for believing Durkheim's postulate of causal reciprocity and that a cause 'needs its effect' ? Of course, it is quite possible that, as a matter of fact, the effects of some causes are such that they tend to perpetuate the occurrence of their causes. Unhappiness might cause excessive drinking which results in further unhappiness and further excessive drinking. To incorporate such a notion of reciprocity into the paradigm of causation seems needless; there is no reason to define the terms 'cause' and 'effect' such that a causal reciprocity is implied.

Before passing from causation to explanation we must clarify the distinction between partial causes and complete causes. Let us give the tug-of-war teams a rest. Suppose that they are standing in a line and that a bottle of beer is handed from team mate to team mate along their ranks; they all take a gulp of beer from the bottle. We might then ask, 'What caused the bottle to be in the hands of the outer-most member of one of the teams ?'. What were sufficient conditions for this state of affairs ? We might then say that the complete cause of this state was his neighbour's passing the bottle to him. We might say that the complete cause was the passing of the bottle from his neighbour's neighbour to his neighbour and then to him. If we were more historically minded, we might say that the sufficient conditions were the production of the bottle in a glass factory, its transportation to a public house, its transportation to the tug-of-war competition and its being passed along the team. Which of these states should be regarded as the cause of the event; how far back in the causal narrative should we go ?

We have given a complete cause when we have given sufficient conditions for an effect. A complete cause is sufficient conditions but not necessarily <u>the</u> sufficient conditions for an effect. All the causes mentioned might be sufficient conditons for the event to have taken place; they differ merely in being historically richer and consequently causally more embracing. As we retrace the causal chain and

produce historically richer complete causes, every complete cause we mention will subsume the later complete causes. For example, we could give a complete cause of the event merely by mentioning the actions of the teams and ignoring the manufacture of the bottle, but if we were to start our causal tale with the production of the bottle, we could not furnish a complete cause of the event without including the action of the team in passing the bottle. Merely to recount the manufacture of the bottle and its transportation to a public house would be to offer a partial cause of the event. It is extremely difficult to discover complete causes; there is usually such a host of causal factors operating in any situation that we must be satisfied with the discovery of various partial causes. For example, even in the above case of the passage of the bottle we might think that we would have to elaborate further on the causal factors in operation in order to recount conditions sufficient for the effect. Nevertheless, complete causes can perhaps be discovered especially when we concentrate on historically shallow rather than on historically deep conditions. Why is the bottle now empty ? We might be able to give a complete cause of this event. The contestants individually partially caused and jointly completely caused this state of affairs.

Paradigms of explanation

a) Covering laws and explanations

Historically and logically related to the view that the search for knowledge should be the search for universal laws or constant conjunctions is the belief that we should try to explain particular events by showing that their occurrence is in accordance with these laws. The main feature of this type of explanation is held by its proponents to be its deductive nature. On this account, we have explained a state or event when we have deduced that it will occur from a universal law together with statements about the particular circumstances of the state or event. For example we might ask, 'Why did this object fall to the ground ?'. On this account, we could explain this event by deducing from the universal law, 'All objects

dropped from a height fall to the ground' and from the statement of particular circumstances, 'This object was dropped from a height', the statement, 'This object will fall to the ground'. This statement predicts the occurrence of the event we wanted to explain and for this reason Hempel writes: 'The explanation fits the phenomenon to be explained into a pattern of uniformities and shows that its occurrence was to be expected, given the specific laws and the pertinent particular circumstances'. (1966, P.50)

It has been frequently protested that this type of 'explanation' does not actually explain anything. It does not tell us why events occur in a particular way but only tells us that events always do occur in that way. It does not tell us why a particular dropped object fell to the ground; it only tells us that dropped objects always do fall to the ground. Suppose we discover a black raven and ask 'Why is this raven black ?'. Is it not rather unsatisfactory merely to be told that all ravens are black ? Rather than remove our puzzlement, this type of 'explanation' seems almost to increase it. If we were puzzled by the appearance of one black raven will we not become even more perplexed when we are told that not only this raven but all other ravens are black ? (See Campbell, 1921, P.80)

It might perhaps be thought that this 'covering law' model of explanation appears to be unsatisfactory only because of the particular examples which I have used. For example, it might be objected that the universal law relating to ravens was not a causal law. Such a criticism appears to me to be beside the point. Since the raven example fits the covering law model, this criticism would be criticism of this model of explanation and not of the illustration of it. Furthermore, if causation is considered simply in terms of constant conjunction, it does not make sense to try to distinguish causal constant conjunction from non-causal constant conjunction. If causation is constant conjunction then the structure of such 'causal' explanation would be identical with the structure of covering law explanations. We can see that this is so by

considering Popper's model of 'causal' explanation. Popper presents a more or less Humean conception of causation:

> The (singular) event A is called a cause of the (singular) event B if and only if from a set of true universal statements (laws of nature) a material implication follows whose implicans designates A and whose implicate designates B. (1961, P.123)

According to Popper:

> ... to give a causal explanation of a certain specific event means deducing a statement describing this event from two kinds of premises: from some universal laws, and from some singular or specific statements which we may call the specific initial conditions. (1961, P.122)

It is important to realise that on Popper's account, we do not 'explain' specific events by their 'specific initial conditions': 'Universal law and specific events are together necessary for any causal explanation ...'. (Popper, 1961, P.146) The structure of this type of 'causal explanation' is thus identical with the covering law type of 'explanation'.

The raven example would surely conform to this account of 'explanation'. From the universal law, 'All ravens are black' and the specific initial conditions, 'This is a raven', we can deduce that this raven will be black. We could imagine numerous examples which would conform to Popper's model where we would be unlikely to consider that we had been given an adequate explanation. Suppose that someone with red hair and a large nose dies. From the true universal law, 'All people with red hair and large noses die', and the initial conditions, 'This person had red hair and a large nose', we can deduce that this person will die. We would hardly allow that this was an explanation of the person's unfortunate death. We would rather tend to account for his death by the fact that he was run down by a bus although we know that it is not a universal law that all people who are run down by buses die. It might appear more relevant to account for his death in terms of the universal law that when people's brain's become damaged to a certain extent they die and the initial conditions that this person's brain was so damaged. But in terms of this model of explanation, we have no reason for distinguishing between relevant and irrelevant explanations. In any case even with such relevant

explanations, the basic objection remains. We are not told why this person who had brain damage died; we are told only that all people who suffer such brain damage die.

What reasons are there for considering covering law type 'explanations' to be genuinely explanatory ? One supposed reason, given by both Hampel and Ryan, is that given a covering law type of 'explanation', the event in question is rationally to be expected since we can deduce its occurrence from the 'explanation'. Thus Ryan writes that when we seek an explanation of some event ' ... what we look for is that information which, when appropriately put together, yields an argument to the effect that the event in question was what we should rationally expect'. (1970, P.48) But, given that all ravens are black and given that we are presented with a particular raven, we would rationally expect that the raven will be black; would we on that account imagine that we had been given an explanation of why the particular raven is black ? Similarly, since I know that I am a man and I know that all men are mortal, I rationally expect to die; it would be irrational to expect otherwise. Yet I do not know why I will die. Although I rationally expect to die, I certainly do not imagine that I have at present an explanation of my death. Will I die of old age; will I have a heart attack; will I fall from a cliff ? That acceptance of an argument leads us to conclude that a particular event will occur is no reason to conclude that the argument explains the event in question.

Ryan argues that covering law types of 'explanations' are genuinely explanatory on the grounds that they show not merely that an event will happen but that they show that the event had to happen and if we can show why an event had to happen we would appear to be justified in considering the event explained. He notes that in the covering law type of deductive explanation, we cannot, without contradiction, accept the premises of the argument (the 'explanation') and deny the conclusion, the statement that the event to be explained will occur. He paraphrases this deductive form of 'explanation':

' ... if all A are B and this is an A, then it must be a B'. (1970, P.51) And hence, he concludes, we can explain why a particular object or event is a B by showing that it must be a B. Similarly, given the appropriate universal law and initial conditions, we could 'explain' why a particular firm's unit price fell by showing that, given the assumptions, ' ... it would have to be the case that the firm's unit price fell ...'. (1970, P.52) Given that the assumptions are true, ' ... the price per unit has to go down'. (Ryan, 1970, P.51)

There is perhaps a confusion between logical and physical necessity in Ryan's argument. Given that all A are B, it follows that it must be true to say of any A that it is a B. But it does not follow from the assumption that all A are B that all A or any particular A must be a B. To show that all ravens are black is to show that it will be true to say that any particular raven is black, but it is not to show that any particular raven must be black. We cannot conclude from the assumption that all ravens are black that all ravens must be black. Hence we cannot conclude that any particular raven must be black. Suppose we want to explain why a particular firm lowered its unit-price. We might present the universal law that any firm which raises its output in a market with constant demand will lower its unit price and the initial conditions that this firm increased its output in conditions of constant demand. What can we deduce from this 'explanation' ? We cannot deduce that the firm must lower its unit price. We can only deduce that, given that the assumptions of the argument are true, then it must be true that the firm will lower its unit-price. But we already know that it is true that the firm will lower its unit-price since this is the event we want to be explained. Since we can deduce nothing from this covering law argument that we did not already know, why should we imagine that the argument is explanatory ? All that we can deduce from the argument is the occurrence of the state of affairs whose occurrence we want to explain.

There seems to be good reason to be dissatisfied with this type of covering law 'explanation'. But if we reject this conception of 'explanation', what alternative conception should we adopt ? How should we define the term 'explanation' ? This is not a matter of linguistic convention regarding the way people actually use the word 'explanation'. The question is,'Are there any reasons for preferring one conception of 'explanation' to another ?'.

The 'explanations' we are concerned with arise in response to the question of why such and such an event occurred. Their job is to remove our puzzlement or curiosity at the occurrence of particular states or events. We might then measure putative explanations in terms of their ability to remove this puzzlement or curiosity. Of course, it is an unfortunate matter of psychological fact that people are often satisfied with nonsensical answers to their questions and have been satisfied when they should have remained puzzled. The question is: 'When should we accept that a particular answer has adequately answered a question regarding the occurrence of a particular state of event; when should we be satisfied with an attempted explanation ?'. When is remaining puzzlement or curiosity appropriate; when is it inappropriate ? We want an answer which will properly silence the question, 'Why did X occur ?'. When will this question itself become inappropriate ? If we are given an answer such that acceptance of the answer is logically inconsistent with the persistence of the question then the question would become inappropriate; we should be satisfied with the answer to our question. Nevertheless, it would perhaps be unduly restrictive to require an attempted explanation to take this form in order to be considered genuinely explanatory especially if it is the case that not all rational arguments are deductive. It would perhaps be better to say that an explanation of an event or state X is an answer to the question, 'Why did X occur ?' such that it is irrational to accept this answer and to continue to raise the question. We should consider an event or state to have been explained when it is irrational to continue to ask why the event or state

occurred.

We can apply this principle to covering law types of attempted explanations. Suppose we ask, 'Why is this raven black ?' and we are told that all ravens are black. Is reiteration of the question still appropriate ? I would suggest that it is: we can say quite rationally, 'I am quite aware that all ravens are black, but why is this raven black; was it born black; was it painted black; is it covered with soot ?'. Similarly, we can say, 'I know that all people who suffer a particular degree of brain damage die, but why did this man who suffered such brain damage die ? Did the brain damage actually cause him to die ? Was his death caused by a heart attack ?' Since the question of why a particular event occurred is just as appropriate before or after a covering law type of answer, we have good reason to consider such answers to lack explanatory force.

b) Causal explanations

With causation, as previously defined, the situation is different. Suppose we say, 'Why did B occur ?' and we are given the answer, 'Conditions A completely caused it to occur; A was a complete cause of B'. Since a complete cause constitutes conditions which are of themselves sufficient to necessitate the production of an effect, it would be irrational, even if perhaps not contradictory to accept this answer and to reiterate the question. It would be absurd to say, 'I accept that conditions A caused B to occur, but why did B occur ?'. We could ask, 'Why did A occur ?' but that would be a different question, requiring a different answer. Again, we might ask, 'What caused this baby ?' and answer, 'It was partly produced by male sperm'. Sperm is claimed only to be a partial cause of babies and it is not irrational to accept this answer and continue to ask the question. The question has not become totally inappropriate, yet suggesting this partial cause is a step towards properly silencing it. The question would become totally inappropriate when, by the adducing of a host of such partial causes, a complete cause of the event had been suggested.

Complete causes completely explain; partial causes partially explain. As there are degrees of causation there are degrees of explanation. We saw that when we considered causation in terms of constant conjunctions it is difficult, or perhaps even logically impossible to accomodate degrees of causation. Similarly, if we consider explanation in terms of deduction from universal laws and initial conditions, how could we allow for degrees of explanation ? Surely we would have to say that either the statement of the event in question does follow from the laws and conditions in which case it is totally 'explained' or it does not follow in which case the laws and conditions are totally unexplanatory or else abandon deduction as a necessary feature of 'explanations'. (Cf. Hempel, 1966, P.59)

In this type of causal explanation, we completely explain particular events by giving a complete cause of these events and we partially explain events by giving a partial cause of these events. General laws have no role in the explanation of particular events although they may play a part in the verification of these explanations. Indeed, if we offer a complete causal explanation of a particular event we can deduce and explain a general law from this particular explanation. If we say that particular conditions A completely produced conditions B, we can derive from this statement the universal law, 'Whenever A then B' and we can also explain why conditions B invariably accompany conditions A by saying that conditions A cause conditions B. Universal laws are therefore, in this account, in principle explicable. The situation is different if we imagine that we should explain universal laws by deducing them from more general universal laws since there must be at least one universal law which is more general then any other general laws and hence could not be deduced from and thereby 'explained' by a more general law. Merely to offer a partial explanation of a particular event is not to imply a universal law. Sperm is a partial cause of babies hence it does not follow that whenever sperm is produced a baby is produced. It does not even follow that there must be a correlation

between the number of babies and the amount of sperm produced.

This paradigm of causal explanation stands somewhat opposed to the view that science is necessarily concerned with general occurrences rather than with unique or particular ones. For example, causal explanations are unlike statements of 'Laws of Nature' whose discovery Lessnoff considers to be the primary aim of physical science. A ' ... Law of Nature is a statement of unrestrictedly universal form. It asserts something about all instances of a class of phenomena, and the class is defined without any restriction of time or place'. (Lessnoff, 1974, P.12) To say that conditions A completely cause conditions B is to say that A will always cause B but not, of course, that B will always be produced by A. But to say that conditions A were on a particular occasion a partial cause of B is not to say that A will always be a partial cause of B. Suppose we have a general class of effects, say human deaths or instances of social order. Since the same type of effect can, on different occasions, be produced by different causes, we can never know the cause of a general class of events where this class ' ... is defined without any restriction of time or place'. We might note the causes of particular instances of death or social order but we would have no warrant to conclude that other instances of death or social order will be similarly produced. Furthermore, we completely explain a particular effect by citing its complete cause; it is quite irrelevant to the explanation of this particular effect whether or not other members of this class of phenomena could be similarly explained. We may partially explain a particular death by saying that the person involved suffered a heart attack. It is quite irrelevant to the adequacy of this explanation whether or not other particular deaths are produced by heart attacks. We need not produce an adequate explanation of the general class of 'deaths' in order to be able to explain particular instances of death. The significance of saying that we need not focus attention on the production of 'general theories' and explanations of general classes of events will become more apparent in the

following chapters.

It is sometimes argued that it is a necessary feature of a scientific explanation that the sentences constituting the explanation must be true. (See Hempel and Oppenheim, 1948) What is the point of saying this; is there some further point in distinguishing explanations from scientific explanations ? Lyon argues that normally ' ... we speak of explanations being correct or incorrect, right or wrong, but if Hempel and Oppenheim's account were correct, then there could not be such a thing as an incorrect scientific explanation'. (1974, P.233) Often, it might be quite useful to be able to distinguish explanations from correct explanations. It may be objected that an attempted explanation incorporating false premises simply could not explain anything which actually happens:

> Suppose I see someone go into a shop and ask why he went in. I may be told: 'He wanted to buy a tie'. Suppose that in fact he went in not to buy a tie but to see his girl-friend. Then there is a tendency to say that what I was told does not explain why he went in, because he did not go in to buy a tie. Thus there is a tendency to use the term 'explanation' in a manner similar to that in which we use achievement words such as 'see' and 'proof'. (Lyon, 1974, P.234)

But, as Lyon points out, we may choose to resist this tendency. We may say that ' ... the claim "He wanted to buy a tie" provided an explanation, but the wrong one. All we can do here is to report these conflicting tendencies, and then decide which way we are going to use the word in some particular context, and for what reasons'. (1974, P.234) Let us try to clarify the distinction between explanation and correct explanation. We could completely explain event B by saying that it was completely caused by conditions A, whether or not conditions A did cause B; but this would be a correct explanation only if A did cause B. Similarly, we could partially explain B by saying that it was partially caused by A but this would be a correct partial explanation only if A did partially cause B. Hempel and Oppenheim talk specifically about 'scientific' explanations. For some purposes, it may be useful to distinguish what we might want to call 'scientific explanations' from explanations but, for my present purposes, I do not think there is a need to

make such a distinction.

c) Durkheim's conception of explanation

To appreciate this conception of explanation more fully, let us consider another rival conception, that of Durkheim. Durkheim's explicit prescription regarding explanation is ambiguous. He writes: 'When, then, the explanation of a social phenomenon is undertaken, we must seek separately the efficient cause which produces it and the function it fulfils'. (1964, P.95) For Durkheim, the 'function' of a social fact ' ... consists of the production of socially useful effects'. (1964, P.110) The 'function' of a social fact is the correspondence between it and the needs of the social organism. But why should we seek functions at all, why should we not simply seek causes ? Durkheim believes that ' ... to explain a social fact it is not enough to show the cause on which it depends; we must also, at least in most cases, show its function in the establishment of social order' (1964, P.97) because the reciprocal bond between cause and effect has not been sufficiently realised. 'Indeed, if the usefulness of a fact is not the cause of its existence, it is generally necessary that it be useful in order to maintain itself.' (Durkheim, 1964, P.97) In a similar spirit, Bierstedt writes, 'Indeed, cause and effect are correlative phenomena, each depends upon the other and both are necessary for the complete explanation of a phenomenon'. (1966, P.90) Bierstedt gleans this prescription directly from Durkheim's writings. On other occasions, Durkheim appears to contradict himself, for example when he says that ' ... sociological explanation consists exclusively in establishing relations of causality, that it is a matter of connecting a phenomenon to its cause, or rather a cause to its effects'. (1964, P.125) Does this mean that explanation is solely a matter of adducing causes rather than functions ? Or does it mean that explanation involves showing both the cause and the function (i.e. a particular type of effect) of the phenomenon to be explained ? On either interpretation, Durkheim's prescription is at least ambiguous if not contradictory. Again, Durkheim is

quite explicit that 'To show how a fact is useful is not to explain how it originated or why it is what it is .... The need we have of things cannot give them existence, nor can it confer their specific nature upon them.' (1964, P.90) Yet, how is this compatible with the explanatory role Durkheim claims for 'functions' ? Often in his substantive sociological theories, Durkheim appears to violate his own prescription by attempting to explain social phenomena by showing their functions i.e. their social usefulness. For example, he writes: ' ... we have explained the constant development of the division of labor by showing that it is necessary in order that man may maintain himself in the new condition of existence as he advances in history'. (1964, P.92)

Since we have rejected the notion of a necessarily reciprocal bond between cause and effect we can reject the view that explanation requires the citing of both the cause and effect of the phenomenon in question because of the existence of this supposed reciprocal bond. It is not necessary to quote the function or any other effect of a phenomenon in order totally to have explained it. When we have offered a complete cause of a phenomenon, the phenomenon has been completely explained; we do not also require to suggest any effects of the phenomenon to render the explanation complete. Furthermore, effects and functions are in themselves unexplanatory. Suppose we ask, 'Why did A occur ?' and we are told, 'Because A produced B'. We can quite consistently say, 'I accept that A produced B, but why did A occur ?'. The question, 'Why did A occur ?' is just as appropriate before or after the effect of A has been cited and hence we have good reason to believe that attempted explanations in terms of effects or functions of the phenomenon to be explained are unexplanatory. Suppose we ask, 'Why is the street wet ?'. It is something approaching a complete explanation to say, 'Because it has been raining'. But suppose we ask, 'Why did it rain ?'. It would be quite absurd to try to answer this question by saying, 'Because the street is now wet'. We shall consider functionalist explanations in relation to social science in more detail in an appendix.

Through these paradigms of explanation we can see an active role that philosophy has in the acquisition of knowledge. Philosophy prescribes what form scientific theories should take if they are properly to be considered explanatory. Similarly, we can see the practical consequences of implicitly adopting false philosophical assumptions in our scientific endeavours. For example, to try to explain a phenomenon by showing it to be in accordance with universal laws or by showing its 'function' might be to waste time which could be more fruitfully employed in seeking the causes of the phenomenon.

Causal explanation and family resemblances concepts

In the previous chapter, we considered the distinction between 'essentialist' concepts and 'family resemblances' concepts. There we saw that it is possible to use family resemblances concepts; the question to be considered now is whether we should use such concepts or whether we should try to define our terms differently. Through a knowledge of causes, we want to bridge the gap between a knowledge of observed events and a knowledge of unobserved events. If we know that A completely caused B, even on one occasion, then we can know that A will always cause B, we can predict with justification that B will always accompany A and, given that we know how to produce A, we have the power to produce B at will. And if we know that A can be a partial cause of B then, although we cannot deduce that A will always accompany B, we can often have good reason for believing that A will tend to produce B. For example, we have some knowledge of a causal relationship between smoking cigarettes and developing lung cancer. Smoking cigarettes appears to be able to be a partial cause of lung cancer. Although we cannot conclude that everyone who smokes cigarettes will suffer from lung cancer, it is rational to conclude that people who inhale an inordinate number of cigarettes are generally more likely to suffer from lung cancer than those who do not smoke at all, or smoke very little.

When he wrote about induction, Hume stressed that it is conceivable that the course of nature might change. Given the meaning which has been given to the term 'cause', if A caused B today then it is literally inconceivable that A should not always cause B. It is of course conceivable that A did not cause B; it is conceivable that causation does not operate in the world, but Hume's induction problem at least appears to be reduced to manageable proportions. If A did cause B then it might in principle be possible to justify the conception that it did. Similarly, if A did not cause B, it might be possible to justify the conception that it did not. Yet, as Hume demonstrated, even if A and B were always merely constantly conjoined, we could never know that they were so conjoined on the basis of any finite number of observations of their conjunction. Complete causal explanation is, of course, an epistemological ideal. Most correct explanations will at best be merely partial and our knowledge and powers of prediction and production will be correspondingly partial, yet complete causal explanation is an ideal at which we can aim when we seek knowledge of the world.

It is against this background of the epistemological role of causation that the problem apparently posed by family resemblances concepts can best be appreciated. The problem is this. From the premise, 'An A caused a B', we want to deduce that all As cause Bs, but whether or not this deduction is valid will depend upon our concept of an 'A' and what it means to be a member of that particular conceptual class. If members of a class need share no particular feature or features in common with other members of that class, what reason have we for assuming that all the members of the class will produce the same or a similar effect ? Suppose we have a family resemblances concept of 'A' such that three different As might have the following defining features: A1 comprises features a, b and c; A2 comprises features a, c and d; A3 comprises features b, d and e. We might discover that A1 causes B but we could not conclude from this that all As cause Bs since it does not follow from the fact that a, b and c

jointly cause a B that a, c and d or b, d and e will cause a B. We could only conclude that all A1s will cause Bs where all A1s comprise features a, b and c. We do not even appear to have a justification for believing that all As will tend to produce Bs. Is the fact that a, b and c cause a B a reason for believing that a, c and d or b, d and e will in combination produce or tend to produce a B ? Hence, it appears that family resemblances concepts, at least in their pure form seem to thwart the attempt to gain knowledge of unobserved events from a knowledge of observed events through a knowledge of causation.

A possible solution to this problem is to discard family resemblances concepts for the purpose of causal explanation and totally to adopt essentialist concepts. If to be an 'A' is to share features common to As and to possess no other features, then we could validly deduce that if an A causes a B then all As cause Bs. A solution of this sort seems to have appealed to Durkheim and it is perhaps for this reason that he advocates essentialist conceptions such as his conception of 'crime': 'If, then, we wish to know what crime essentially is, we must extract the elements of crime which are found similar in all criminological varieties in different social systems. None must be neglected.' (Durkheim, 1964, P.70) Durkheim says of different types of crime. 'As different as they appear at first glance, they must have a common foundation, for they everywhere affect the moral conscience of nations in the same way and produce the same result'. (1964, P.70)

The trouble with this solution is that it is extremely difficult, if not even impossible to form such essentialist concepts which are in conformity with reality. If we consider a class of phenomena whether it be 'games' or 'societies' or whatever, it is extremely difficult to abstract features which are necessarily common to even a significant majority of the class members. And when we have the added difficulty of defining 'A' such that to be an A is to share certain features and these alone the task is even more daunting. This added qualification has to be made since otherwise we might have As

sharing features with other As yet also possessing features which thwart or preclude the production of Bs and the entire point of the exercise is validly to be able to conclude from the premise 'An A caused a B' that all As cause Bs. We might allow that As possess features other than those common to As as long as these features were causally neutral, or at least causally neutral to the production of Bs. For example, the colour of billiard balls might be causally neutral to their effect in collision and an essentialist conception of 'billiard ball' might allow that billiard balls can be any colour at all. But, in order for our causal deduction to remain valid, we would have to incorporate such stipulations within the definition of our terms and this would create practical difficulties.

Durkheim seems to imply that essentialism is the only proper course open to us and that to adopt anything other than essentialist concepts is 'unscientific', but this is not so. There is at least one other solution to the problem. We could adopt a type of concept which is a cross between an essentialist and a family resemblances concept. Let us return to the example of the three As where A1 comprises the features a, b and c; A2 comprises a, c and d; A3 comprises b, d and e. We discover that A1 produces B and from this we can conclude that all A1s will cause Bs providing that to be an A1 is to comprise the features a, b and c or at least to possess these features and no others which are causally antithetical to the production of Bs. Hence, for the purposes of causal explanation, we could discard the family resemblances concept of As and talk of A1s or redefine 'A' in terms of the causally relevant features a, b and c. Later, we might discover that an object comprising the features a, b and f also causes a B and we could redefine 'A1' or 'A' to allow that objects comprising the features a, b and f are A1s or As. And later, we might broaden the concept to encompass objects comprising the features b, c and g when they are found to produce Bs. We could thus enjoy the advantages of both essentialist and family resemblances concepts by formulating definitions such that for an object to be an 'A' it is <u>essential</u> that it possess features which in conjunction

will produce or tend to produce a B but it is not essential what these features are; to be an A is to share some or all of a variety of causally relevant features. With such concepts we could conclude that if an A causes or tends to cause a B then all As cause or tend to cause Bs.

Such a strategy is probably quite commonly adopted even in our every day thinking. Quite often, when we classify object, we are less interested in what features the objects possess than in what the objects actually do. For example, we call various assortments and arrangements of materials 'engines' not primarily because we think that they share features in common but because they produce similar effects, for example, the effect of propelling a vehicle. Similarly, we classify various objects as 'hearts' not because they look similar or share particular features but largely because they have the same effect of pumping blood round a body. If a plastic object bearing virtually no perceptible likeness to a human heart replaced a human heart, we would have no hesitation in calling it an artificial heart.

It might have been thought that a possible solution tothe problem is simply to define an 'A' as possessing features which will produce a 'B' without specifying what the particular features are but, for a variety of reasons, this strategy would be useless. For example, unless we can indicate what features are possessed by As we could never recognise As; we could never predict the occurrence of Bs and we could never contrive to produce Bs. The essentialist and the essentialist/family resemblances concepts which were sketched out are ideal concepts to which actual empirical concepts will conform to a greater or lesser extent. The actual content of empirical concepts is, of course, a matter to be dealt with by those actually involved in practical scientific endeavour as is the question of whether essentialist or essentialist/family resemblances concepts are best suited to the task. It is, however, important to be aware of which type of concepts are being used in any particular investigation as we shall see when we consider the verification

of causal claims.

Causal explanation and verification

This paradigm of causation offers a particular interpretation of our experience of the world. People are often perplexed by occasions where 'Rival philosophers agree about the empirical facts, and agree that there is no need for further deductions from these facts yet continue to debate a philosophical point'. (Newell, 1967, P.4) On such occasions, where neither logical deduction nor the citing of new facts can settle the argument, in what sense can we claim to have a proper rational disagreement at all ? If the argument cannot be settled by deduction or observation, then it surely cannot be a rational argument at all, we might think. And on such occasions, where empirical facts are not directly in dispute, what knowledge, beyond knowledge of the agreed facts, could we ever hope to emerge from such disputes ?

Suppose people agree that there exist bodily states and they agree that there exist pains, sensations, thoughts and dreams, what possible point of substance could be at issue in the mind/body dispute ? In such a dispute, where the facts are largely agreed upon by rival participants, what is often at issue is the proper interpretation of the agreed facts. For example, are pains and other mental states best interpreted as a species of bodily states or are they best interpreted as being radically distinct from physical things ? Similarly 'causation' offers an interpretation of accepted facts in terms of the categories of cause and effect. Such accepted facts will themselves be particular interpretations of perceptual experience which will itself be interpreted as perceptual experience. Different interpretations are not arbitrary; interpretations are true or false. The interpretation of mental states as a type of physical state will be false if it is not the case that mental states are a species of physical states. Rival interpretative paradigms can be rationally evaluated. We can give reasons for the acceptance of one interpretation rather than another just as, in this chapter, we considered various

reasons for adopting a particular paradigm of causation rather than, for example, conceiving the happenings of the world in terms of universal laws. And similarly, we have considered reasons for explaining these happenings in terms of causation rather than, for example, in terms of conformity to universal laws.

How can we tell when a correct causal or partial causal explanation of a particular event has been furnished ? What criterion can we use to tell whether we have discovered the cause of an event ? If all valid arguments are either inductive or deductive, how could causal explanations ever be verified ? Since causal claims are matters of fact, they could not be verified deductively; causal claims are not necessary truths. They could not be verified by induction since causation is imperceptible. We cannot observe causes physically necessitating the occurrence of their effects on any one occasion hence causal claims cannot be verified by a catalogue of such experiences.

Related to the thesis that rational arguments are either inductive or deductive is the thesis that it is the logical or linguistic form of an argument rather than its content which is the sole guarantee of its validity. This thesis has inspired philosophers to try to formulate rules of logic, to outline valid forms of argument such as the syllogism and, perhaps to present criteria of 'science' such as that of falsification. And this thesis can be challanged. It may even be self refuting since the thesis itself does not seem to betray a basic logical or linguistic form which would render it an a priori truth. Consider what is involved in justifying a belief or an act. We justify acts by giving reasons for doing them and we justify beliefs by giving reasons for accepting them. But how do we distinguish genuine from spurious reasons ? Suppose we sought to justify or condemn denying the right to vote in parliamentary elections to certain categories of people, for example children. We might say that the fact that children are inexperienced, immature and relatively

ignorant is a good reason for denying them the vote. And I think that this would generally be allowed as a good reason for denying children a vote. Suppose we said the fact that children are soft skinned, hirsute and relatively small is a good reason for denying them a vote. Can we not 'see' that this argument is irrational and that it does not cite a good reason for denying children a vote. Yet, the linguistic and logical form of the argument citing a good reason and the argument citing a spurious reason is identical. Here, we do not appear to distinguish between a good and a spurious reason on the basis of a formal linguistic or logical criterion; some other type of intellectual judgement seems to be at work. But surely we can distinguish between genuine and spurious reasons, even in the absence of any formal criterion. Furthermore, although offering reasons to justify acts or beliefs is a rational type of argument, the logic of giving reasons is the logic of neither induction nor deduction. Comparing it with induction, we do not say that children's immaturity and lack of experience is a good reason for depriving them of the vote because we have observed on a number of other occasions that this was a good reason for denying them a vote. Similarly, although it would be irrational to deny that children's immaturity and lack of experience is a good reason for denying them a vote, it would not be logically inconsistent or contradictory to voice such a denial.

We have verified a causal explanation when we have justified the belief that the explanation is correct. And this justification will proceed on the basis of neither induction nor deduction; this verification will consist of giving reasons for considering the causal claim to be true. There are not necessarily any formal linguistic or logical criteria which we use to distinguish a good reason from a spurious reason, but, as we have seen, we do not necessarily require such criteria in order to distinguish good reasons from spurious reasons. Individual people in the context of the evidence for and against any particular causal claim can use their own judgement to ascertain whether they have better reason for

accepting or rejecting the causal claim. Some people might want to argue that we must be able to furnish a criterion of a good reason if we are to be able to identify good reasons for accepting beliefs. But this view is difficult to sustain. The argument would seem to lead to an infinite regress. If we require a criterion of a good reason in order to identify good reasons, would we not similarly require a criterion of a criterion of a good reason and a criterion of a criterion of a criterion of a good reason and so on ad infinitum ? If we could not recognise good reasons, how could we recognise a criterion of a good reason; how could we have good reason to believe that we had recognised a criterion of a good reason ? It would seem to be a presupposition of rational argument that good reasons for accepting or rejecting assertions can be identified. How could we rationally doubt this without assuming that we are able to recognise reasons for doubting it ?

When verifying causal explanations, it is important to be aware what type of concepts one is dealing with in order fully to appreciate precisely what the suggested causal claim is. We must be particularly careful when using family resemblances concepts. We can illustrate this point with a simple example. Suppose we want to explain an instance of tennis elbow and we suggest that it was produced by the sufferer's playing the game of tennis, where we have a family resemblances concept of 'games'. We might then have the theory that playing a game caused or partially caused this case of tennis elbow. Is it relevant to the verification of this explanation whether or not excessive playing of other games such as poker, snooker or chess cause tennis elbow ? It is not necessarily relevant since to say that playing tennis might tend to cause tennis elbow is not to imply that the playing of other games will produce a similar effect. Is it relevant to the verification of the explanation whether or not all games of tennis can tend to cause tennis elbow. This is not necessarily relevant either. Perhaps games of tennis form a family like games. Perhaps playing lawn tennis might tend to cause tennis elbow while playing table tennis does not.

In order to facilitate the verification of the claim that this instance of tennis elbow was partially caused by playing tennis, it would be advisable to indicate what specific features of the game are considered to be causally responsible. We are perhaps unlikely to be misled or confused when trying to verify this particular causal claim, partly because we are well aware that there are very many different types of games and we are quite well aware that different games often possess quite different features. Nevertheless, we may perhaps become confused in our attempt to verify other causal claims.

For example, suppose that we want to explain social phenomenon X (an instance of shared moral values within a community) and social phenomenon Y (a social revolution). We might explain X by the 'society' in which it occurred and we might explain Y by the prevailing state of the industrial economy in that country. How would we verify these causal claims ? Is it relevant to their verification whether or not shared moral values emerge in other societies and whether or not social revolutions occur in other countries with industrial economies ? We may mistakenly think that this must be relevant. Well, it might be relevant, but it might not be: it depends on our concepts of 'society' and 'industrial economies'. Our concepts might be such that 'societies' and 'industrial economies' form families like 'games'. Other 'societies' and other 'industrial economies' might compare with the society and industrial economy in question as poker compares with the game of tennis; 'societies' and perhaps even 'industrial economies' might share no causally relevant defining features in common. Perhaps the best advice which can be given to combat this problem is simply to be aware of it. We might also try to form our concepts in terms of causally relevant features but if we do this and, say, define a 'society' as a group possessing certain specific causal features, it becomes a matter for investigation whether any social group which we might previously have called a 'society' actually possesses the stipulated causal features.

When verifying causal explanations, it is also important to realise whether we are trying to verify a complete causal claim or a partial causal claim. Complete causal claims are falsifiable in the Popperian sense that if they are false, then it is in principle possible for there to be a true observation statement which is inconsistent with them. Partial causal theories are unfalsifiable; we cannot deduce that they are false from the assumption that any one particular observation statement is true. Of course, partial causal theories can be shown to be false. For example, it is known to be false that drinking tea is generally a partial cause of babies althought this partial causal claim has not been falsified. There are reasons for considering the claim to be false. The systematic correspondence and coherence of a vast number of facts rather than any one particular observation strongly suggests that the claim is false. If we assume that A completely causes B, we can deduce that 'When A, then B' and we could falsify the theory by observing A unaccompanied by B. Hence, if we said that smoking is a complete cause of cancer we would have a falsifiable, though a false theory. If we say that A is a partial cause of B, what can we deduce from this ? Can we deduce that any particular observation would falsify the theory ? Can we not say that 'other things being equal', A will produce B or, 'other things being equal', the more there is of A, the more there will be of B ? We cannot even say this unless we stipulate what are the other things which must remain 'equal' and this is to furnish a complete cause of B. Causes produce effects but partial causes do not always produce partial effects. Sperm can be a partial cause of babies but sperm of itself will not produce a partial baby; we could not deduce from the fact that sperm can be a partial cause of babies that the more sperm is produced in a society, the more babies will be produced. Partial causal claims are unfalsifiable and, hence, in Popperian terms 'metaphysical' since, even if they are false, there cannot be any one true observation statement which contradicts them.

Since the assumptions relating to the verification of complete causal claims differ in some respects from those appropriate to the verification of partial causal claims, we should be aware whether we are trying to verify a partial or a complete causal claim. We should also be aware of the degree to which factors are claimed to be causally significant. When we say that smoking is a partial cause of cancer, do we mean that it tends very slightly to produce cancer, that it has an overwhelming tendency to produce cancer or is the claim somewhere between these extremes ? It is important to know precisely what the theory is in order to be able to tell whether the evidence supports the theory or suggests that the theory should be modified or abandoned. In general, the more complete a causal explanation is, the more it approaches being falsifiable and the more easily it can be verified; the more partial a causal theory is, the more difficult is its verification. Hence, it might be wise to prescribe that causal explanations be made as complete as is practically possible. For example, instead of saying that smoking is a partial cause of cancer, it might be better to focus our attention upon what smoking itself completely causes or upon what combines with smoking completely, or almost completely to cause cancer.

We justify causal claims by giving reasons for their acceptance. Since there are degrees of justification, there are degrees of knowledge. Some causal claims are held with more justified assurance than others. But how sure can we justifiably be that any causal claim is true ? Talking of causal laws, Durkheim argues that, on some occasions, ' ... one is certain of being in the presence of a law'. (1964, P.133) Could we ever know with certainty that any particular causal claim was true ? Philosophers often argue that we can distinguish analytic statements which are necessarily true from synthetic statements which are contingently true and that, unlike synthetic statements, analytic statements can be known to be certainly true although such statements convey no factual information about the world. Analytic

statements are considered to express tautologies and, if they tell us anything at all, they are thought merely to tell us what particular words mean and that particular terms are synonymous. To quote Berlin, 'Propositions are either certain and uninformative, or informative and not certain. Metaphysical knowledge which claims to be both certain and informative is therefore in principle not possible.' (1956, P.180) The suggestion is that we can never justifiably be quite so sure that a synthetic statement is true as we can that an analytic statement is true; it is rational always to have reservations about any synthetic statement. We can never be completely sure that any claim we make about the world is true, or so Berlin appears to suggest. Causal claims are synthetic rather than analytic since it is not contradictory to deny that any particular causal claim is true. Should we conclude that Durkheim is mistaken and that, in principle, we could never be certain that any causal claim is true ?

Berlin's argument rests on the assumptions that that which is necessarily true can be known to be certainly true and that which logically could be false cannot be known to be certainly true and both these assumptions can be challanged. Consider the first assumption. Suppose that we were presented with a complex mathematical theorem which is necessarily true. Can we be certain that it is true ? We might consider the premises of the theorem and try to judge whether the various conclusions follow from the premises yet is always logically possible that we have made a mistake no matter how many times we recheck the proof. (See Hume, 1969, Pp.231-238) Consider even a simple necessary truth such as that all bachelors are unmarried. Can we be certain that all bachelors are unmarried ? It is logically possible that we have made a mistake of some sort when judging that all bachelors are unmarried. On this account should we conclude that we cannot be certain that necessary truths are true ? If we allow that necessary truths can be known with certainty even although it is always logically possible that we have made a mistake when we claim that a particular statement is necessarily true why

should we not also allow that non-necessary truths might be known with certainty even although it is logically possible that we have made a mistake when we claim that a particular non-necessary truth is true ? In any case, we can be certain that the statement 'All bachelors are unmarried' is true only if we know what the words 'All bachelors are unmarried' mean. It is always logically possible that we have made a mistake when we claim that the words 'bachelor' and 'unmarried' have a particular meaning. We could only be certain that the statement 'All bachelors are unmarried' is true if we could be certain what the words 'bachelor' and 'unmarried' mean. It is a contingent matter that particular words have a particular meaning. As Wittgenstein reminds us: 'If you are not certain of any fact, you cannot be certain of the meaning of your words either'. (1969, P.17:114) It is perhaps partly for this reason that he argues that 'If the proposition 12 x 12 = 144 is exempt from doubt, then so too must non-mathematical propositions be'. (1969, P.86:653)

I can see no reason for denying that statements of fact such as causal claims might be known to be true just as certainly as analytic statements might be known to be true. How could we deduce that statements of fact cannot be known to be certainly true; how could we be certain that the premises from which we reached such a conclusion were true ? Would we not want to say that when we see, it is logically possible that we see through our nostrils but <u>certain</u> that we see with our eyes ? Why should we be inclined to argue that Wittgenstein is <u>necessarily</u> mistaken when he claims that 'We know, with the same certainty with which we believe <u>any</u> mathematical proposition, how the letters A and B are pronounced, what the colour of human blood is called, that other human beings have blood and call it "blood".' ? (1969, P.43:340)

Conclusion of Part 1

Rex considers three conceptions of science. He considers: science as classification; science as the quest for laws; and science as the search for causes. Of these rival conceptions of science he says, 'It is not suggested that any of these is more valuable or important than the others. All in fact have a part to play in the overall plan of scientific enquiry.' (1970, P.25) It is possible to argue that one of these conceptions is more valuable than the others. In my view, we should conceive of successful science as the discovery of causes of effects rather than as either the classification of objects and events or the discovery of universal laws. If we wish to seek knowledge and to be able to explain the world then the prescription that we should seek causes is better than the prescription that we should seek universal laws or that we should classify, or so I would like to suggest. I would also suggest that this prescription, like the other prescriptive, interpretative and evaluative paradigms which have been advocated, is true. If we accept a Humean conception of 'truth' then it does not seem possible to accomodate the view that prescriptions, interpretations and evaluations can be true since they do not appear to be necessary truths and there does not appear to be any 'real existences' to which they could correspond or with which they could agree. Hence, it does not seem to make sense to say that we should accept Hume's conception of truth.

With the coincidental theory of truth, we can accomodate the possibility that prescriptions, interpretations and evaluations are true or false. For example, the prescription that we should seek causes rather than laws or classification will be true if it is the case that we should seek causes rather than laws or classification. Furthermore, I would like to claim not merely that this prescription is true but that it can be known to be true; it is knowledge. Like other prescriptive, interpretative and evaluative paradigms, this prescription can be verified; we can justify its acceptance. It is justified in a similar way to causal claims and any other factual claims.

It is justified by neither induction nor deduction but by giving reasons for believing it to be true. And like causal claims, it is also justified by its relationship to other elements of our knowledge, with which elements it combines to form a coherent system of assumptions.

The specific reasons for preferring the prescription that we should seek causes to the prescription that we should seek laws have already been considered. Causes can adequately explain their effects; we cannot explain an event by showing it to be in accordance with a universal law. If we seek laws then, as Hume's treatment of induction shows, we could not reasonably claim to know that we have discovered any whereas we might justifiably claim to know that we have discovered causes, or at least partial causes.

Although classification is indispensable in the production of knowledge, classification alone will not provide us with an adequate understanding of the world. We can realise the inadequacy of the prescription that we should seek to classify if we recall the discussion of classification from Chapter Three. Unlike a knowledge of its cause, a knowledge of the conceptual class or classes to which an object or event could correctly be said to belong will not explain the occurrence or existence of the object or event, nor will it give us the power to predict or to control its appearances. Suppose that we are presented with a particular object; let us say that the object is a chair. Suppose that to be a 'chair' is to possess features a, b and c. We might look at the object and observe that it possesses the features a, b and c. We would then be able to classify the object as a chair. Would this act of classification add to our knowledge of the object in front of us ? Would we not have to know that the object was a chair in order to classify it as a chair; is to say, after the act of classification, 'This is a chair' to say anything about the chair which we did not already know ? Of course, we could know the features of an object and not know that it was properly called a 'chair' but to be told that it is properly called a

'chair' is to acquire fresh knowledge about word usage; it is not to acquire fresh knowledge about the chair. To classify an object is to say what the object is; it is to say that it is a table, a motor car or a chair. But to say what an object is is neither to explain why it exists or to be able to predict its future appearances. If we ask, 'Why does this object exist ?' we will hardly be satisfied with the reply, 'It is a chair.' If we ask 'Why did that event occur ?', we are unlikely to consider the event to be explained by our being told that the event is a social revolution or an economic depression. And hence, I think that Rex is mistaken when he says that ' ... classification is itself a comprehensive mode of scientific explanation. It explains by showing the logical relations of one thing to another.' (1970, P.11)

I have tried to substantiate the claim that philosophy has an active part to play in the acquisition of knowledge and that philosophical assumption is logically prior to knowledge. To assume that causation operates in the world is to assume that events in the world are explicable and that a systematic knowledge of the world is possible. By a 'systematic knowledge' I mean a coherent pattern of justified beliefs as opposed to quite isolated fragments of knowledge such as 'This is a chair' ... 'That is a greyhound'. Even such fragmentary knowledge embodies philosophical assumptions notably the concepts of truth and class membership and a theory of perception. The philosophical activities of interpretation, prescription and evaluation are logically prior to the acquisition of knowledge. For example, there is the [illegible] interpretation of the world in terms of causes and effects; there is the prescription that the search for knowledge should be the search for causal explanations; and there is the activity of evaluating causal claims to decide whether they could properly be classified as knowledge. We can further prescribe that we should try to make such explanations as complete as possible since the more complete is the suggested cause, the more completely will it explain. Furthermore, the

more complete are our causal claims, the more readily can they be verified and the more secure can our knowledge become; the more complete is our causal knowledge then the more surely can we predict and control subsequent events. In Part 11, I shall try to apply this prescription to social science and to consider the interpretative, prescriptive and evaluative questions which are raised by this attempt. I shall then try to consider what further interpretative, prescriptive and evaluative assumptions should be made in order to have a philosophical basis, a Paradigm within which knowledge could be attained in social science.

PART 11

CHAPTER FIVE

THE PHILOSOPHY OF HUMAN BEHAVIOUR

In Part 11, I shall consider the presuppositions of of knowledge pertaining to social science. It will be argued that in social science, as in other sciences, knowledge is produced by an interplay between our conceptions and our perceptions and that we should seek knowledge through the search for causes and effects. In social science we should consider phenomena and occurrences to have been correctly explained when we have discovered their causes and correctly partially explained when we have furnished their partial causes. To say that the basic presuppositions of knowledge are the same in natural and social science is not to say that the appropriate methods of study are the same in both cases and neither is it to imply that social science might be 'reduced' to natural science in the sense that the laws of, say, chemistry or physics will eventually subsume the laws of social science. There is a distinction between asserting that there is a unity in the presuppositions of knowledge and advocating a unity of science. In any case, although the basic presuppositions of knowledge are the same in natural as in social science, I want to consider some further presuppositions which are peculiar to social science in the following chapters.

It would perhaps be a justifiable oversimplification to say that there are basically two types of social science. One type is the study of individual behaviour and types of behaviour. Psychology is largely of this type as is much history and much economics could reasonably be called the study of

economic behaviour. The sociology of this type is usually concerned with the specific category of meaningful social action rather than with the general umbrella category of behaviour. The second type of social science is concerned with 'social phenomena', which are in some sense distinct form the behaviour of specific individual people; they are often the unintended consequences of such behaviour. Thus, the historian does not try only to recount the behaviour of, say, Bismarck or James VI, he describes and tries to account for social phenomena such as capitalism, the Boer War and the Great Depression. The economist studies phenomena such as trade cycles and the Gross National Product and although such things are dependent upon the behaviour of specific people, they seem to be, in an obvious sense, distinct from such behaviour. In this chapter, I want to consider the presuppositions of our knowledge relating to the first type of data, human behaviour. In the next chapter, we shall turn our attention to social phenomena, the second type of data.

The need for a philosophy of human behaviour

In trying to explain human behaviour, social scientists have often made the mistake of failing to realise the importance of philosophical assumptions in relation to their task. They have often been unaware of the philosophical assumptions to which they have been implicitly committed by acceptance of their theories and have misconceived philosophical assumptions as straightforward 'scientific' theories to be settled by observation when the philosophical interpretation of such observations has been a matter of dispute. They have often failed to realise that no knowledge of human behaviour is possible without a logically prior philosophy of human behaviour.

To illustrate this, I want to consider an expression of behaviourist philosophy and then look at the implicit philosophy of Weber and that of much economic theory. We are told in a psychology text book that human beings are organisms whose behaviour can be explained as responses to the stimuli of their environment; these organisms are conditioned to

respond in particular ways:

> The experimental data that we have presented in connection with our conditioning studies have, however, been limited to rather simple responses such as salivation (in dogs) and bar-pressing (by rats). In our everyday life, we seldom spend much time thinking about such isolated responses, usually thinking of more gross activities, such as learning a poem, carrying on a conversation, solving a mechanical puzzle, learning our way round a new city, to name only a few. While the psychologist could study these more complicated activities, as is done to some extent, the general approach of psychology is to bring simpler responses into the laboratory for study. Once the psychologist discovers the principles of learning for simpler phenomena under the more ideal conditions of the laboratory, it is likely that he can apply these principles to the more complex activities as they occur in everyday life. The more complex phenomena are, after all, nothing but a series of simpler responses. Speaking to a friend is a good example of this. Suppose we have a conversation such as the following:
> He: 'What time is it ?'
> She: 'Twelve o'clock.'
> He: 'Thank you.'
> She: 'Don't mention it.'
> He: 'What about lunch ?'
> She: 'Fine.'
> Now, this conversation can be analysed into separate S-R units. 'He' makes the first response which is emitted probably to the stimulus of the sight of 'She'. When 'He' emits the operant [i.e. a particular type of response], 'What time is it ?' the muscular activity, of course, produces a sound, which also serves as a stimulus for 'She'. On the receipt of this stimulus, 'She' emits an operant herself: 'Twelve o'clock', which in turn produces a stimulus to 'He'. And so on .... In such complex activity, then,we can see that what we really have is a series of S-R connections. The phenomenon of connecting a series of such S-R units is known as chaining, a process that should be apparent in any complex activity. (Calvin, 1961)

This behaviourist psychology offers a clear domonstration of the need for a philosophy of human behaviour logically prior to knowledge of such behaviour. This need is partly hidden by the writer's linguistic tone and specifically by his failure to distinguish between perception and conception. That human behaviour consists of stimuli to responses is not a perception: it is a conception; it is a particular philosophical interpretation of human behaviour which could, logically, be either true or false. Suppose that the account of this

conversation is perfectly true; suppose it to be known to be true. How could such knowledge be possible ? It could not arise from perception alone since we can perceive 'stimulation' no more than we can perceive causation. We can perceive 'He' saying 'What time is it ?' and we can perceive the 'She' but we cannot perceive that the sight of the 'She' stimulates the request for the time. This stimulus-response theory is an interpretative paradigm of human behaviour. Since this theory states that something is the case, the theory will be either true or false. Suppose that the theory were false. Suppose that the very same events which are described took place but that they were differently caused or suppose that they simply occurred by 'chance' quite unprompted by any stimuli or causes. Would the perceptible features of the events necessarily have been any different than if the events had been the responses of stimuli ? No, they would not. Hence knowledge of stimuli and responses could not arise from perception alone. The theory that human behaviour is response to stimuli is a philosophical theory of behaviour.

We are led to believe that this psychological theory is a straight-forward 'scientific' theory which can be settled simply in terms of observation; this gives the impression that this psychology has no philosophical basis. For example, we are told that the process of 'chaining' should be 'apparent' in any complex activity. But this process is not apparent in the sense that we must have made a perceptual mistake if we do not notice it; that this process occurs is simply one possible interpretation of complex human activity. Similarly, we hear of the hypothetical conversation: 'In such complex activity, then, we can see that what we really have is a series of S-R connections'. We do not see such connections in the same sense that we see tables and chairs or patches of red. Such connections, if they exist, are not directly conveyed to us in sensory experience. The S-R theory cannot be established or refuted simply by a catalogue of observations of human behaviour; the S-R theory is rather a philosophical interpretation of such observations.

In trying to account for human behaviour, the social scientist must accept, at least implicitly, a philosophy of human behaviour. For example, it is logically possible that human behaviour might proceed in such a manner that no systematic knowledge of it is possible. Human behaviour might be totally uncaused and occur in a purely fortuitious manner. To imagine that we possess knowledge of human behaviour is implicitly to have accepted various philosophical presuppositions. To assume that knowledge of human behaviour is possible is to assume that it is not totally uncaused; that it is not fortuitous or without pattern. Such assumptions commit us to further assumptions. To assume that we have knowledge of the causes of human behaviour is to make assumptions regarding the sorts of things which could causally affect human behaviour. For example, economists often try to account for economic behaviour in terms of people's motives and their desire to attain as high a level of economic well being as possible. Sociologists such as Weber also consider that knowledge of motives is an appropriate way to attain knowledge of behaviour, or, at least of some behaviour since not all behaviour is prompted by motives or purpose. Thus he writes that sociology ' ... is a science which attempts the interpretive understanding of social action in order thereby to arrive at a causal explanation of its course and effects'. (1964, P.88) It is generally assumed that by 'interpretive understanding', Weber means an appreciation of a persons motives for his 'social action' i.e. behaviour which is motivated and makes allowance for other people's behaviour and expectations of behaviour.

That Weber and economists are here committed to philosophical assumptions is perhaps best appreciated by considering assumptions to which theirs are opposed. For example, Ryle interprets mental phenomena such as motives in such a way that it does not appear to make sense to consider mental phenomena as causal agents. To assume that motives are the sorts of things which could be causes is to assume that they are 'things' or events and perhaps this is a false assumption. Ryle argues that 'Motives are not happenings and are not therefore

of the right type to be causes'. (1949, P.113) He suggests that ' ... to explain an action as done from a certain motive is not to correlate it with an occult cause, but to subsume it under a propensity or behaviour trend'. (1949, P.110) According to Ryle, there are no such 'things' as what are commonly thought of as mental phenomena. Mental phenomena exist in a sense but they do not exist as 'mental things'; they are, rather, physical dispositions to behave in particular ways; such dispositions do not produce actions, on Ryle's account. The belief that there exists mental non-physical 'things' arises from a misleading use of words, according to Ryle. If I said that I go to work on an average once a week, the unwary might be misled into thinking that there exists a physical object, an 'average' on which I sometimes go to work (perhaps using a bicycle on other days). Similarly, according to Ryle, the unwary are misled by talk of, say, 'pain behaviour' and 'motivated behaviour' into thinking that there actually are such mental things as 'motives' and 'pains' lurking behind overt behaviour. In Ryle's view, on such occasions, there exists only physical behaviour and events; to use mental terms is to refer to dispositions and tendencies towards particular behaviour.

Weber does not interpret motives as behavioural dispositions; he regards a motive as a 'complex of subjective meaning'. (See Runciman, 1972, P.43) Although it may not be clear precisely what a 'complex of subjective meaning' is, it would appear to be some sort of 'mental thing'. Weber's conception appears to differentiate between motives and physical phenomena which, like tables, chairs, arms and legs presumably lack 'subjective meaning'. There is obviously a philosophical interpretation involved in considering mental phenomena such as motives as being non-physical or immaterial. Furthermore, to regard such immaterial motives as possible causes of human behaviour is implicitly to commit one's self to further philosophical assumptions. Human action and behaviour obviously involves the movement of a physical body even if it does not comprise such physical movement.

But, if mental entities such as motives are immaterial and lack the properties of physical objects how could mental entities and physical entities ever causally interact ? How could motives causally affect the physical world of which human action and behaviour is a part ? To assume that mental entities such as motives are the sort of things which are able causally to affect human behaviour is to have made a controversial philosophical assumption.

Causation is imperceptible. Causally to explain human behaviour and human mental and physical states, we require a philosophy of human behaviour and human mental and physical states. We require this in order to be able to interpret particular phenomena as partial and direct causes of human behaviour and states. We could never recognise a cause or partial cause of any human behaviour without a logically prior conception of the sorts of things which could be a cause or a partial cause of human behaviour. It is perhaps best to approach such a philosophy of behaviour via a discussion of the mind/body problem, which is probably the central disputed issue.

Central-State Materialism

One of the clearest presentations of the questions at issue is that offered by Campbell in Body and Mind (1971). He presents the problem in this way:

1) The human body is a material thing.
2) The human mind is a 'spiritual' thing.
3) Mind and body interact.
4) 'Spirit' and matter do not interact.

We seem to have good reason to believe each single proposition of this tetrad. The evidence of professional science suggests that our bodies are constituted of the same stuff as the subject matter of physics while our common sense experience suggests that mental phenomena such as love, hate and conceptions lack at least some of the basic properties of material objects; they lack, or appear to lack such properties as solidity, velocity, mass and volume. Common sense experience suggests mind/body interaction as when physical fire appears to cause us mental

anguish or mental comfort or when mental pain appears to cause physical screams. Yet, on reflection, if mental and physical phenomena are radically distinct types of 'things' it seems difficult to imagine that they could ever causally interact. The physical world seems to be a closed, self-contained system whose events are explicable in terms of other physical events in the system; how and where could 'spirit' get a causal foothold in this physical system ?

The mind/body problem is a classic example of philosophical activity as the choosing between rival paradigms. Each individual proposition could logically be true; there is no logical contradiction involved in voicing any single one of the propositions. Each single paradigm must be either true or false since each one states that something is the case. For example,(3) will be false if it is not the case that mind and body interact. All four paradigms could not be true; it would involve a logical contradiction simultaneously to accept all four conceptions since, for example, (1), (2) and (4) together entail that mind and body do not interact and this contradicts (3). The philosophical problem is, 'Which of the above propositions, if any, should we accept and which ones would we be justified in rejecting ?'. This choosing between rival paradigms is a rational activity since, although we cannot deductively prove that we should accept or reject any particular proposition, we can give reasons for accepting or rejecting the rival paradigms. It is not arbitrary which paradigms we choose nor is it a matter of linguistic or grammatical convention. And we need not conclude that the mind/body problem is a pseudo-problem arising, as Ryle suggests from 'category mistakes' which stem ' ... from inability to use certain items in the English vocabulary'. (1949, P.17) Is the mind material or 'spiritual'; do mind and body interact ? These appear to be perfectly legitimate questions worthy of and capable of rational answers.

Various solutions to the mind/body problem are possible depending upon which of the rival paradigms are

accepted. What we are required to do is to choose paradigms which are mutually consistent and for which we can give good reasons. Paradigm (1), 'The human body is a material thing', can be readily accepted; it is the only one of the tetrad which is uncontroversial. Given this assumption, the solution which seems initially to be the best justified and which appears to offer the most fruitful source of scientific advancement is the doctrine known as Central-State Materialism (C.S.M.). This solution involves the acceptance of (1) and (3) and the rejection of (2) and (4); it is asserted that mind and body interact but denied that the mind is 'spiritual' or immaterial. The theory allows that the existence of non-material mental phenomena is a logical possibility but states that, as a matter of contingent fact, such phenomena do not exist. Mental phenomena such as sensations, thoughts and emotions are believed to exist but they are believed to exist as physical phenomena, namely as states of the central nervous system. The core of C.S.M. is 'the mind-brain identity theory'. This is the theory that the mind and the brain are identical. The term, 'identity theory' is confusing since we might imagine that the theory is that the mind and the brain are 'identical' in the sense that two twins might be identical, but this is not the case. The claim is that what are called 'mental phenomena' actually are physical phenomena: mind 'stuff' is brain 'stuff'. Mind and brain are the same thing; they are identical in the sense that the morning star and the evening star are identical; the morning star is the evening star and vice-versa. (See Borst, 1970)

An obvious attraction of C.S.M. is that it allows a causal role for mental phenomena; that our mind is able causally to affect our behaviour is an assumption generously supported by our everyday experience. For example, surely what I am now thinking affects what I write; and surely my longer term motives and intentions are also partly responsible for my present actions ? Similarly, the theory allows that physical objects and events might causally affect our mental states as, for example, when we read a physical book and are caused to experience a particular sequence of thoughts. Furthermore,

there is overwhelming neuro-physiological evidence that our brains are peculiarly important regarding the causes of our physical movements and states; the central nervous system causes the movements of our bodies where such movement is not produced by physical forces external to our bodies. C.S.M. initially appears extremely attractive because of the way it is supported by both common sense knowledge and scientific knowledge. The mind causes our behaviour and our brain causes our body to move. This is because our mind is our brain, according to the theory. The mind/body problem dissolves on this account. The mind is part of the body, that part which controls behaviour. The relationship between body and mind is akin to the relationship between a vehicle and the vehicle's engine. What causes a vehicle to move when it is not propelled by an external force ? The state of the vehicle's engine. What causes voluntary human movement ? States of the central nervous system. And this is a causal theory of all human activity whether such activity is solitary, unreflective and motiveless or purposeful, motivated and orientated towards the behaviour of other people. On this account, all such behaviour which is voluntary, in the sense of not being totally produced by external physical causes is produced by states of the individual's central nervous system.

Before considering objections to C.S.M., I would like to consider a few minor alterations to the theory in order to reach certain important conclusions which will still apply even after the suggested modification of C.S.M., and in order to illustrate two basic roles which a paradigm of 'how man works' has in relation to social science.

It seems implausible simply to say that the mind is the brain. It would be more plausible to say that mental phenomena were particular parts of the brain or particular states of the brain. Perhaps all 'mind-stuff' is 'brain-stuff' but it seems less likely that all 'brain-stuff' is 'mind-stuff'. We would not expect our brains totally to disappear when our minds go 'blank'. What of the brain of a dead person or segments of his disected brain; are they also mental phenomena ?

More importantly, the theory is surely mistaken in trying to isolate the mind/brain as the cause of human behaviour and bodily movement. To use an analogy, the engine of a vehicle may be particularly causally relevant to its movement but it is not the only causally relevant factor. In giving an explanation of the behaviour of a vehicle we would particularly mention the vehicle's engine but this would not furnish a complete explanation; we would also have to mention the state of the tyres, condition of the chassis, and so on. Similarly, although we know that the brain is particularly causally relevant in human behaviour, it does not operate in isolation. The brain is rather the 'Clapham Junction' of the causes and effects of bodily states. The brain is affected by the rest of the body, for example, by the flow of blood maintained by the actions of the heart and by the input of information from the senses. The brain in turn affects the rest of the body sending out signals which control the activity of the muscles, limbs, tissues, organs and glands. Human activity is thus the result of a complex causal chain and it is misleading to consider any cause as the ultimate cause of such activity since no matter what cause we focus upon, it too will be the product of some even more 'ultimate' cause. We should seek, rather, partial and complete causes of human activity. There is a host of factors other than states of the brain which could be cited as a partial cause of human activity. States of the brain will be important in any complete explanation of human activity when such an explanation is at all historically deep but such brain states will not be a complete cause of the activity since subsequent and consequent bodily states, for example, passage of brain signals, movement of tissue, muscle and limb will be causally relevant. Indeed, it would be possible to furnish a complete thought historically very shallow explanation of any human act without retracing the causal chain as far back as the contribution of the brain and quoting merely the subsequent and consequent bodily states and events.

This conception of human behaviour in terms of causally related bodily states enables us to interpret particular

factors as causes or as partial causes of human behaviour. What sorts of things could be causes or partial causes of human behaviour ? The sorts of things which occur in the above human causal chain. Could motives be considered as causes of human behaviour ? In terms of this slightly modified C.S.M., motives are not conceived as behavioural dispositions as they are by Ryle; they are conceived to be actual 'things', namely states of or part of the brain. Hence, although they could not be complete causes of human behaviour they could be considered to be partial causes of such activity.

This account of 'how man works' also has another related function. Durkheim writes that when we undertake any scientific venture, 'All preconceptions must be eradicated' and that this supposition is ' ... the basis of all scientific method'. (1964, P.31) Superficially, this advice seems almost platitudinous since no one would want preconceptions to thwart his attempted 'scientific objectivity'. Yet, a moment's reflection should make us uneasy with this prescription. If we eradicate all preconceptions then we eradicate the preconception that we should eradicate all preconceptions. Furthermore, we have already seen the need for categories such as 'cause' and 'explanation' and a conception of the sort of things which could be causes and explanations, where such conceptions are logically preconceptions. There is another reason why preconceptions should not be eradicated. A crucially important justification for considering any belief to be true is its coherence with other elements of our knowledge. Generally, we are not justified in accepting any belief as knowledge unless it can be shown to cohere with other relevant beliefs which are known to be true. For example, consider the 'theory' which is sometimes expressed in London buses: 'Having the correct fare makes the bus go faster'. Suppose that we can produce a well attested generalisation to the effect that when people have the correct fares on London buses, buses can be observed to go faster. Would we be justified in considering this theory to be known to be true ? Of course, a priori, anything might cause anything but we already know what makes buses go and what makes

buses go faster; the driver presses the accelerator, the flow of petrol to the engine increases and the engine drives the wheels at a faster rate. And, on the face of it, having a particular sum of money in our hands or in our pockets does not cohere with this known causal mechanism. The contents of our pockets seem irrelevant to the behaviour of the bus. We are not justified in considering the belief that having the correct fare makes the bus go faster to be true unless we can show that it coheres with our relevant prior knowledge. In the case of one-man operated buses, this can be done quite easily: we can appreciate that if the driver/conductor has to spend a long time giving change to the passengers who do not tender the correct fare, then the bus will be stationary when it might otherwise have been moving and hence the bus journey will take longer than it might otherwise have done. In the case of two-men operated buses, there does not seem to be any coherence between the theory and our prior knowledge of bus behaviour. No matter how constant a conjunction be observed between passengers' having the correct fare and the speed of two-men operated buses, we would not be justified in claiming knowledge of a causal relationship between these phenomena although we could claim such knowledge in the case of one-man operated buses.

In trying to account for any particular instances of 'bus behaviour', it would be foolish to try to eradicate what we already know of the behaviour of buses. And similarly, we should not try to eradicate what we know of 'how man works' when we try to account for any instance of human behaviour. Indeed, we are not justified in considering any explanation of human behaviour to be known to be correct if it could not be shown to cohere with our prior knowledge of 'how man works'. That an explanation of human behaviour coheres with our prior knowledge of human behaviour is a prerequesite of its being <u>known</u> to be correct. Of course, what passes for knowledge of buses and human beings might be false; bold speculation at variance with this supposed knowledge might turn out to be true, but, in the context of current available evidence we are more justified in accepting what passes for knowledge of buses and

human beings than we are in rejecting it in favour of mere speculation. Although the precise relationship between mind and body might be a matter for doubt, there is a hard core of knowledge in the above account of C.S.M. We do have knowledge of the causal mechanism through which the body works. We know the role of the brain and the nervous system in relation to bodily movement and we know how the body is causally related to the brain. We know the causal mechanism by which the external environment affects the body and we know the sorts of things which can affect the brain via an effect upon the senses. We are not justified in accepting any account of human behaviour as knowledge if it cannot be shown to cohere with this knowledge of human behaviour.

a) Objections to Central-State Materialism

This C.S.M., philosophy of behaviour is in many ways a very attractive theory. Although it has an admirable simplicity and economy, it is less crude and schematic than much materialist philosophy. For example, it is far preferable to the simplistic S/R theory in that it allows a clear causal role to mental phenomena and it emphasises the vast range of factors which are able causally to affect human behaviour. Some human behaviour might be partially caused by external stimuli and C.S.M. can account for this but it also accounts for behaviour which does not seem to be properly classified merely as a response to the external physical environment. Nevertheless, the theory still appears to be inadequate. There is a certain apparent purposefulness and intelligibility in human behaviour which seems difficult to account for in purely 'physical' terms. The analogy between vehicle behaviour and human behaviour seems to be too close for the theory to be entirely acceptable. People often act with an intention and with a conception of what they are doing when such behaviour is something which we would consider to be, in principle, quite different from anything we would expect from, say, a bus. A bus does not intend to grind to a halt, it just does whereas there is a certain intellectual direction to much human behaviour. For example, reconsider the S/R analysis of a hypothetical conversation:

He: 'What time is it ?'
She: 'Twelve o'clock.'
He: 'Thank you.'
She: 'Don't mention it.'
He: 'What about lunch ?'
She: 'Fine.'

Even in a rather stilted conversation such as this, there is an obvious intelligiblity; we can, to an extent, appreciate why the people said what they did. But, as we have already seen, where physical causation is concerned, anything might be the cause of anything; the relationship between physical cause and physical effect is simply one of de facto regularity and it is opaque to reason. Hence, if we consider human behaviour to be simply a physical effect which is the result of blind physical causes would we not expect conversations like the following ?

He: 'What time is it ?'
She: 'Bacon and eggs ?'
He: 'Don't mention it ?'
She: 'Virtually every Thursday.'
He: 'Accidents of bread and cheese.'

But, apart from in the music hall, we do not come across such conversations.

Consider too the difference between conception and perception. If we cast our eyes around the room, we experience a sequence of percepts and they can quite plausibly be considered to be the effects of the external physical environment. There is no intellectually intelligible relationship between the various percepts; each percept is simply an element in a causal chain. Yet, with conception, the situation is different. Suppose that we read Spinoza's Ethic considering his various definitions and then attending to the various propositions which are to be deduced from them. Our thoughts follow a pattern which is radically distinct from a stream of percepts; there is an intelligible relationship involved; we can appreciate why the sequence of thoughts follows the pattern that it does and this intellectual intelligibility seems incompatible with purely physical causation.

Similarly, although in perception we can plausibly imagine ourselves to be passively affected by external causes, in conception we have at least an illusion that 'we' are directing our own thoughts. And this notion of 'us' of the intellectual self which thinks and knows and is the author of its own actions, is crucially important in any acceptable philosophy of behaviour. Yet, how can this intellectual self plausibly be considered as a physical thing ? Suppose that I drive the hypothetical one-man bus and deliberately drive into a herd of sheep. Who/what was responsible for this act ? We would surely say that 'I' was the author of this act and that 'I' am responsible, where it would not make any sense to attribute such responsibility to the bus, to the accelerator, to the steering wheel or to my foot or leg or to any other part of my body. We say of our actions, thoughts and bodies that they are our thoughts actions and bodies; but how could this relationship of ownership be plausibly considered as a physical thing or as a physical relationship ? Similarly, I can say of parts of my body that this is my arm or that is my leg but how could it possibly make sense (as opposed merely to being false) to say of any of my bodily parts or states: 'That is my intellectual self; that is my "me" ?'

We encounter several other difficulties if we try to consider mental phenomena to be physical states. For example, any empirical claim which we make about the physical world could, logically, be false. This applies even to claims about our own bodies: we might imagine that there was a pain in our leg and be mistaken; our leg may have been previously amputated and may now be painless. This is not so with sincere claims about our own mental states. We may sincerely report, 'I am cold' when we simply mixed up our words and when we meant to say 'I am warm'. Nevertheless, what we meant to say could not possibly be false. This gives us reason to believe that since reporting our mental states is quite different from reporting physical states, including our own, mental states are somehow of a different nature from physical states.

This argument partly rests upon the theory of language which it presupposes and which was discussed in Chapter Three. There, it was argued that we often use language with the intention of conveying or expressing mental content. For example, we might wish to convey the mental content 'Scotch people are brave'. If mental content were a physical object then it should make sense to attribute to it physical qualities, at least those which are attributable to 'brain-stuff'. Yet, it would be totally senseless to say that 'Scotch people are brave' is green or is heavy or exists between my ears or has ceased to exist. Similarly, consider any concept, such as the concept of 'democracy'. This concept could be said to be clever, paradoxical, confused or self-contradictory yet it would not make sense to attribute such qualities to physical objects. We talk of knowledge as justified belief, but how could a physical object be justified ? Knowledge is also true belief and if we reconsider the concept of truth, I think we shall find that mental materialism is difficult to defend. Truth is the coincidence of what is thought to be the case and what is the case. A thought is true when it is the thought that X is the case, and X is the case. This relationship of coincidence is quite different from any relationship between physical objects or physical states. It is quite different from the relationship between a table and a chair or between a table and an identical table or even between a table and its self. Furthermore, as we have seen, there are not always any 'objects' to which objectively true thoughts can 'correspond'. If it is true that there does not exist a Loch Ness Monster then there is no object to which the true thought 'corresponds'. How could we ever try to analyse such a true thought (if it is true) in terms of a relationship between a physical object i.e. the thought and the absence of some other physical object ?

If we were to accept C.S.M., it would be quite natural for us to conclude that it is appropriate to try to explain human behaviour in the same way in which we would seek to explain any physical occurrences, that is, in terms of causation. Similarly, we would tend to imagine that mental

states could be partial causes of human behaviour although we would not necessarily think that there was anything epistemologically peculiar about mental states in relation to human behaviour; they would be considered simply as partial causes among other partial causes. Nevertheless, as we have seen, there are objections to C.S.M. If we were to concede that these objections are valid, could we still reasonably claim that it is appropriate to try to explain human behaviour causally ? If we were to concede that mental phenomena are non-physical or 'spiritual' could we still reasonably claim that the mind and the body causally interact ? Could we still maintain that mental states are not epistemologically peculiar in relation to human behaviour and that they feature simply as partial causes or would we be led to conclude that the relationship between a mental state and human behaviour is radically different from that existing between a physical cause, or partial cause and a physical effect ?

I want to argue that even if we concede that the mind is, in some sense 'non-material' and distinct from the brain, it is still appropriate to try to explain human behaviour causally and to consider the relationship between a mental state and human behaviour to be that between a partial cause and an effect.

It is not unreasonable to accept the following solution to the mind/body problem:

1) The human body is a material thing.
2) The human mind is a 'spiritual' thing.
3) Mind and body interact.

We could try to graft a causal non-material mind on to the previous C.S.M., theory; the mind could be added as a further link in the causal chain. The external world affects the mind through its effect upon our bodies and the consequent effect upon the brain. Thus our moods and thoughts are affected by, say, alcohol via its effect upon our body and brain. Our minds are able causally to affect our bodies through affecting the brain. Thus, when our brains are adversely affected by alcohol our attempts to walk in a straight line or to speak intelligibly are thwarted. Our mental states can therefore be

partial causes, although not complete causes of our actions and behaviour.

Although in every day experience it is almost impossible to doubt that our thoughts, motives and aspirations causally affect what we do, the notion of interaction between mental phenomena and physical phenomena has been considered to be quite problematical if not even unintelligible by some philosophers. If mental and physical phenomena are radically distinct how could they ever causally interact; how could phenomena which share no features in common ever 'come together' in order to interact ? Such philosophers consider that the causal relationship between, say, colliding billiard balls is quite intelligible; we can visualise, they imagine, the impact of the balls and the transfer of 'force' or 'momentum' from one ball to the other. If, however, we consider the relationship between our physical bodies and non-physical mental entities we, cannot visualise an intelligible causal bond; there seems to be no possible 'point of impact' between the mental and the physical spheres.

As a prelimenary answer to these objections, we could recall the discussion of classification in Chapter Three. To say that mental and physical phenomena are distinguishably different need not be to say that they share no features in common. To be 'mental' or to be 'physical' may mean to possess some or all of a variety of defining features where no defining feature of a mental phenomenon is also a defining feature of a physical phenomenon and it may be the case that mental and physical phenomena also share other features in common. The fact that 'mental' and 'physical' phenomena can significantly be distinguished does not <u>a priori</u> preclude this possibility. Hume presents a forceful answer to those who say that a causal bond between the physical and mental sphere is unintelligible. According to Hume, <u>a priori</u>, anything might be the cause of anything; we cannot rationally demonstrate why any cause should produce one particular effect rather than another; we can merely observe that it does. We cannot find an

intellectually intelligible nexus between any cause and effect; we can only be familiar with causal relationships. He argues that we are just as familiar with a conjunction between certain motives and certain actions as we are with any other relationship of conjunction and hence there is no reason why we should deny that mental and physical phenomena are often causally related. (See Hume, 1969, Pp.447-455) Hume framed his argument in terms of an analysis of 'causation' as constant conjunction, but I think that the argument still applies when causation is considered as causal necessity although we would probably prefer to say that motives can be partial causes rather than causes of actions. Furthermore, as we shall see, some people want to argue that there actually is some sort of intelligible bond between some mental states and human behaviour and this leads us to the topic of motivated behaviour or 'action'.

Individual and social 'action'

So far, we have talked about human behaviour in general and it is perhaps not a very controversial claim that unreflective mere behaviour should be explained causally. The controversial issue concerns the relationship between mental states and reflective or intellectually directed human action, where 'action' would generally be considered as behaviour orientated towards a goal. (See, for example, Parsons and Shils, 1951) In view of the concern among some social scientists for 'meaningful' and 'motivated' behaviour and their conviction that this category of behaviour is in principle different from all others, requiring different principles of explanation, I want to consider the category of 'human action' and to try to show that it is appropriate to seek to understand this class of behaviour in the same way as any other, namely, through a knowledge of causes.

Social scientists often define the term 'action' as 'meaningful' behaviour or behaviour to which the actor attaches 'emotional significance', but such definitions might tend to cause confusion and fail to highlight the distinction which social scientists want to make between events happening to us

and our actually doing something. For example, we would surely attach emotional significance to being hit on the head by a brick and being pushed from a cliff yet in such circumstances we would be the object of external action rather than the author of our own action. Action is sometimes held to be 'meaningful' in precisely the same sense that language is meaningful but this too seems confusing. (See Cicourel, 1973, P.42) As we have seen, words or symbols have a linguistic meaning and this is the mental content which such words or symbols are intended to express or communicate. Although we may sometimes act in a symbolic sense as when we nod our heads to express a particular thought, most behaviour which we would want to call 'action' such as jumping from a cliff or engaging in a pub brawl would be in this linguistic sense of the term quite meaningless. According to White, it is an essential feature of action that ' ... it is the bringing about of something'. (1970, P.11) Some behaviour which we would want to call 'action' accords with this definition. For example, the action of voting a particular way in a election brings about or tends towards bringing about a particular state of affairs, namely, the result of the election. Nevertheless, is this an essential feature of action ? An action such as prayer may produce some effect, but why should we say that prayer could not be an action if it did not ? Perhaps actions form a Wittgensteinian family like games. We need not expect that there are necessarily any significant features which are common to all actions.

Bearing in mind this possibility and allowing that a correct interpretation of actions might not account for all actions, I would like to consider the category of 'action' in terms of the previous distinction between conception and perception. A distinctive feature of conscious human behaviour is that it is accompanied by percepts and concepts. This is true of mere behaviour as well as of action. When pushed from a cliff, we might still perceive the waves rushing to meet us and we might still have conceptions; we might conceive, 'I have just been pushed off a cliff'. A distinctive feature

of much 'action' is that we have a conception of what we are doing rather than of what is happening to us and that, in a sense, our conception of what we are doing is partly constitutive of what we are doing. If we were falling from a cliff then we would still be falling from a cliff whether or not we had the conception that we were falling from a cliff. Yet, consider an action such as prayer. Without the conception of a god and of possible communication with god, we could not pray. We could not be praying if we did not have the conception that we were trying to communicate with god; to have such a conception is to pray.

Our conception of what we are doing, like any other, will be either true or false. We might conceive that we are diving into the sea and be mistaken and land on a pile of rocks or be fast asleep in bed. We may conceive that we are communicating with god and yet be deceiving ourselves. Perhaps there is no god; perhaps he cannot hear our prayers. This is all perfectly obvious but it is worth stressing in view of the confusion which might arise from the celebrated phrase of Thomas about an actor's 'definition of his situation'. (See Thomas, 1951, P.81) If we talk in this way rather than talking about a person's conception of what he is doing, we may be misled into thinking that an actor's definition' must be true, by definition we might say, just as Schutz is led to say that ' ... social reality contains elements which are real because they are so defined by the participants ... '.(1943) In an obvious sense, our conceptions are still just as 'real' and causally operative whether or not they are true as long as we imagine them to be true but it is a mistake to imagine that the truth or falsity of a person's conceptions is necessarily of no causal significance. For example, the effect of landing on a pile of rocks is not cushioned by the mistaken belief that we are diving into the sea and the effects of actually communicating with god may not be the same as merely imagining that we are communicating with god; in the former instance, our prayers might be answered.

McIntyre argues that 'An agent can only do what he can describe'. (1962) An agent's action ' ... is identified fundamentally as what it is by the description under which he deems it to fall'. (McIntyre, 1962) An action ' ... must fall under some description which is socially recognisable as the description of an action'. (McIntyre, 1962) Thus, he concludes: 'To identify the limits of social action in a given period ... is to identify the stock of descriptions current in that age'. (1962) The corollary is that the sociologist can only attain knowledge of action insofar as he possesses a description under which the action could be socially deemed to fall.

I think that this account of action is misleading; it might cause us to confuse language with the mental content which is expressed by language. We can 'do' what we can conceive to be possible whether or not we can express such conceptions in language. The limits of our action are the limits of our conceptions rather than of our language. Consider any novel action such as, for example the action of a work-in when this was a novel action. (See Buchan, 1972) To have a work-in is not just to go through certain physical motions, just as there is more to 'going on strike' than simply not attending one's work. Someone who did not grasp the point of 'going on strike' could remain absent from his work but he could not 'go on strike'. Similarly, anyone who pointlessly 'went through the motions' of having a 'work-in' without realising the significance of the activity could not properly be described as taking part in a work-in. The conception of what one is doing in a work-in is an integral part of the work-in. If, and only if we can grasp the point of a work-in , (that is, if we can form the concept of a 'work-in') is the activity of a work-in one in which we can engage. It does not matter whether or not we possess the language to express or communicate the concept of a 'work-in'. Indeed, if we had to await a socially deemed description of an action before we could perform that action, we would wait forever. How could we 'socially describe' something which is supposed not to exist prior to the description ?

This role of conceptions in relation to action has repercussions on the ability of the social scientist to know what an action is and to explain and predict it. Suppose that a social scientist lacked the concept of a 'work-in'. How could he ever recognise, explain or predict work-ins ? The problem is exacerbated by the possibility of novel concepts and consequently the possibility of novel actions. Prior to the occurrence of the novel action of a work-in at Upper Clyde Shipbuilders, how could this novel action have been predicted ? If someone had said, prior to the work-in, 'There will occur a "work-in" ' then what he said would have been meaningless. It is in principle impossible to predict or even to talk about a novel concept since, if we could do this then, by definition, the concept could not be a novel one. (See Cranstoun, 1967, Pp.117-118)

There is another objection to the claim that 'An agent can only do what he can describe'. With some actions, we can offer correct descriptions of what an agent is doing which are quite independent of the agent's conception of what he is doing. We can procrastinate, for example, without realising that we are procrastinating and without realising what the word 'procrastination' means. We could still procrastinate even if we were mentally incapable of forming the concept of 'procrastination'. 'What I do may be the act of a madman, an act of bigamy or trespass, without my having the faintest idea that this is what I am doing.' (White, 1970, P.12)

Motives, which are important in relation to action can be interpreted in terms of conceptions. To have a motive is to desire some state of affairs and to have the conception that some action of our own will serve to produce it. Motive is distinct from desire. I might desire a glass of water or desire to go to Edinburgh but motives only arise with my conceiving that heading for the nearest tap and the nearest railway station will serve to meet these desires; and these conceptions, like all others will be either true or false, or

partly true and partly false.

Motives do not always produce actions. We can have motives that we are ashamed of and repudiate. We might have a motive to climb Ben Nevis and yet consider the requisite activity not to be worth the bother. We often have conflicting motives; we might have a motive to go to London as well as a motive to go to Edinburgh and our motive to go to London need have no effect upon our subsequent trip to Edinburgh. We are often aware of our own motives although we are rarely aware of the causes and effects of the desires and conceptions involved. Consider an instance of motivated action, for example, my heading towards the nearest tap in order to quench my thirst. In such cases, the 'actor' involved, in this instance myself, gets a very distorted view of the causal sequence which is involved. I am aware of my motive in walking towards a tap and I am aware of my action of walking towards a tap but since causation is imperceptible, even in the case of our own action and behaviour, I am not conscious of the causes of my motive or of what caused this motive from all my various competing motives to have been heeded and neither am I conscious of the host of other mental and physical causes which intercede between my motive and my action. Consequently, I tend to get the false impression that my motive completely causes my action when in fact it is only a relatively partial cause. I also tend falsely to imagine that there is a sort of uniquely peculiar causal relationship between my motive and my action; I am almost tempted to conclude that my motive is the 'ultimate' cause of the action and nothing else could have any explanatory relevance to it. Yet, on reflection such a view of the relationship between motives and actions is implausible. Motives are simply elements in a wider causal chain and from the point of view of causation they own no peculiar or especially important place. On reflection, we can place my thirst and motive within an wider causal framework. My thirst was a result of my particular bodily state which was in turn the result of my body's being de-hydrated after the previous night's drinking which was the consequence of my attending a friend's stag party

which was imposed upon me as a social obligation as a result of our long standing friendship. Similarly, my conception of how to quench my thirst was not produced in a causal vacuum; from experience I know the effect of water upon my thirst and I also know the sort of places where water can usually be obtained, Motives are a peculiar element of human experience precisely because we are often so aware of them. Furthermore my motives are peculiarly mine. Motives are extremely important in relating to morals. The moralist is not concerned with all the physical causes of my drinking at my friend's stag party although he is particularly concerned with my motives for doing so since it is appropriate to allocate moral praises and moral blame in terms of people's motives for action. Nevertheless, from an epistemological point of view, motives and other mental states need have no favoured place; they are epistemologically important insofar as they are causally relevant and the causal relevance of particular motives can only be ascertained from the particular occasions on which they arise. (See Spinoza, 1930, Pp.135, 209-213)

If we consider mere behaviour, action and social action then it is possible to argue that there is no fundamental epistemological distinction between them; the only difference is the range and complexity of the factors which can be causally relevant in the respective cases. The causal chain involved with action is wider than that involved in the case of mere behaviour since the choices, decisions, conceptions, motives and desires of the 'acting self' enter into the picture. Similarly, the causal chain is wider in the case of social action than in the case of individual action since the behaviour, expectations, thoughts, motives and desires of other people are able causally to affect an individual person's action. For example, when I am seeking the nearest tap or drinking fountain the causal sequence involved in this action will be more complex if the streets are busy and I have to negociate heavy traffic as I cross roads than if the streets are empty of people other than myself since, in the former instance of social action, people other than myself will influence my action as, for

example, when I hesitate at the kerb, afraid of being killed by a car.

a) Objections to the causal explanation of human action

A popular objection to this philosophy of behaviour could be paraphrased as follows. Consider any purposeful action such as praying or going to a newsagent's to buy a newspaper. Such 'meaningful' action, it is suggested, could not be explained by 'non-meaningful' factors. It is granted that for this action to have taken place, certain other biological and physiological events had to take place but it is argued that 'meaningful' action cannot be reduced to 'non-meaningful' factors such as chemical reactions or the laws of physics. 'Meaningful' action must be considered on its own level of 'meaningfulness' and not on the level of 'non-meaningful' events. Human action takes place within the physical world and is therefore to some extent influenced by physical factors but physical laws only set the boundaries of our actions; laws of human action operate within their own autonomous, or at least partly autonomous sphere. It is the task of the social scientist to discover these particular laws.

I agree that it does not seem to make any sense to try to reduce a 'meaningful' action to 'non-meaningful' factors. Furthermore, I would agree that we should not try to explain social or individual action by subsuming it under the laws of physics or chemistry. It must remain an open question whether or not this subsumption would be possible but, in any case, I have argued against the view that we should try to explain anything in terms of general laws. To say that A causes, or partially causes B is not to say that A is reducible to B or that B is reducible to A. Sperm is a partial cause of babies but it would not make sense to say that babies are reducible to sperm or that sperm is reducible to babies. Similarly, in a sense, having the correct fare is on a different 'level' from the length of a bus journey although we know that the latter can be affected by the former. Hence, to say that action is not reducible to 'non-meaningful' factors is not to give good

reason to believe that action is not affected by 'non-meaningful' factors. When we seek to explain through seeking for causes, then it hardly makes sense to talk of different independent 'levels' of explanation. There cannot be different 'levels' of causes. There can be only complete and partial causes; hence there can be only complete and partial explanations.

A related objection to this interpretation of human behaviour is made by Socrates. Socrates refused to flee from Athens because of his moral principles and consequently was imprisoned in Athens. His being in prison, he maintains, cannot be explained in terms of the movement of his bones, muscles and sinews but only in terms of his motives, intentions and his judgement of what is right and wrong. This Socratic objection seems to me to rest on a confusion between explaining an action or event and morally justifying it. Regarding the moral appraisal of Socrates's action, the movement of his sinews and bones is indeed irrelevant and such appraisal must relate primarily to his motives, intentions and moral principles. Yet, it does not follow from this admission that Socrates's 'non-meaningful' biological and physiological states are irrelevant to the explanation of his action in remaining in Athens and going to prison. They, like his motives, intentions and moral principles were partial causes of this action. Similarly, we might have an occasion where our one-man operated bus mounted the pavement and bumped into a wall. What explains the presence of the bus upon the pavement ? In morally appraising this state of affairs, the motives and intentions of the driver are particularly relevant. Did he swerve with the intention of avoiding a pedestrian; was he driving too quickly, trying to show off to his girl friend ? Yet, in the causal explanation of this state of affairs, the driver's morally relevant motives and intentions feature merely as one other element in a causal sequence of events.

People sometimes imagine that reasons for actions are radically distinct from the causes of actions and even that to say that an action was done for a reason is to preclude the

possibility of a causal explanation of that action. This too is a confusion, in my view. 'Reason' and 'cause' are not mutually exclusive categories. That they might appear to be so perhaps arises from a confusion between the 'reason' for an action and the 'reason' for accepting a particular belief. There is obviously a distinction between my reasons for accepting or rejecting a belief, for example C.S.M., and the causes of my accepting or rejecting it. Reasons, unlike ordinary causes serve to justify the acceptance of rejection of beliefs. This type of justificatory reason is related to yet distinct from our reasons for doing things. My reasons for accepting C.S.M. (in this latter sense of 'reasons') might be quite distinct from my justification for believing C.S.M. to be true; I might accept C.S.M. in order to shock my religiously inclined friends. Thus we could say that my reasons (justification) for saying that C.S.M. is true are quite distinct from my reasons (purposes, motives, intentions) for saying that it is true. A consideration of our reasons (in this latter sense of 'reasons') is often relevant to the moral appraisal of our actions. The reasons for our actions are a particular type of partial cause of our actions; hence we say that we acted because of particular reasons. Reasons sometimes justify what we do, just as Socrates's reasons for remaining in Athens served to justify his action; yet, it would be implausible to say that Socrates's reasons for acting did not causally affect what he did. To say that our reasons justify what we do is to presuppose that such reasons affect our acts. It would be absurd to say that Socrates's reasons for not leaving Athens justified his not leaving Athens but that they were causally irrelevant to this action and that this action was completely caused by factors unrelated to his reasons. How could we reasonably say that Socrates's action would have occurred whether or not he possessed these reasons and also claim that these reasons serve to justify the action ?

Leat argues that since human action cannot be defined in physical terms then it cannot be explained in physical terms. She argues that ' ... since the concept of action logically

involves the notion of giving reasons ' ... it follows that explanation in these terms will necessarily count as explanation of actions'. (1971) As we saw when we were discussing language, we do not define things; we define words and symbols. We do not define action, rather, we define the word 'action'. And we may, as Leat has done, define the term 'action' in such a way that the concept of 'action' logically involves the notion of reasons. We may say that 'action', by definition, is behaviour which is done for a reason and it will then become a matter for investigation whether any particular instance of behaviour actually is an action. If reasons are causes of actions then it follows that reasons will help to explain actions but it does not follow that only reasons will explain actions; it does not follow that physical factors cannot explain actions. These latter conclusions would follow only if we defined an 'action' as behaviour which is completely caused by the actor's reasons and which is caused by nothing but the actor's reasons for acting. Again, it would be a matter for investigation whether anything actually was such an 'action'. Nevertheless, it seems almost inconceivable that such 'action' could ever occur and such 'action' is radically distinct from the action and behaviour which we encounter in everyday life and of which we have knowledge. I know that my action of walking towards the nearest tap was done for a reason but I also know that this reason alone did not completely produce my action and I know that numerous physical factors (eg. my physical state of thirst) were causally relevant to the action. Merely having a reason could not propel me from one side of a street to the other.

The following two arguments against the causal explanation of human action are presented by Melden. (1961) He considers the case of a man in a car signalling by raising his arm. He says that such an intentional event cannot be explained causally since we cannot identify an event which is found to be common and peculiar to such cases. In other words, we cannot find a Humean cause which is found to be constantly conjoined with its effect. But to say that we have not as yet found such an event is not to show that such an event does not

always occur when people signal in motor cars. In any case, there is no need to expect cause and effect to be constantly conjoined since the same effect might, on different occasions, be produced by different causes. Furthermore, since a person's 'intention' in raising his arm to signal would be a partial rather than a complete cause of an act of signalling, we need not expect a constant conjunction between such 'intentions' and acts of signalling. Melden further argues that a cause must be 'logically distinct' from its alleged effect and that since a reason for acting is not logically distinct from the resultant action, reasons cannot be causes of actions. This argument seems to me to be unfounded. Causes need to be logically distinct from their effects merely in the sense of being conceptually separable from them. For a causal bond to exist between phenomena it must be the case that we have at least two phenomena and not merely one phenomenon. This is a necessary condition for a causal nexus to exist. In a sense, 'radio transmitters' and 'radio waves' are not 'logically distinct' just as there is a logical relationship between the 'sun' and 'sunlight'; but, it makes sense to say that radio transmitters produce radio waves and that the sun produces sunlight because all four can be conceived of as separately existing phenomena. Similarly, there is obviously some sort of logical connection between my seeking the nearest tap and my reason for doing so but this does not preclude the possibility of a causal relationship between them since both the reason and the action are logically separable. That they are logically separable can be seen by noting that reasons for actions are usually prior to the resultant actions and that we can have reasons for actions which we never perform.

McIntyre argues that:

> If beliefs and actions stand in a causal relationship ... then it is purely contingent which beliefs are related to which actions. If we are looking for the cause of some action in the realm of beliefs, we can have no a priori expectation about where to look. (1962)

For example, the action of spraying roses might well be the

effect of the belief that the Pope is the anti-Christ or the belief that speed kills; it is purely contingent that it is the effect of the belief that spraying roses helps to ward off greenfly. The apparently nonsensical nature of this conclusion leads McIntyre to deny that there is a causal relationship between our beliefs and our actions. To combat this objection, it is necessary partially to accept it. A priori, anything might cause anything; a priori, the belief that the Pope is the anti-Christ might cause us to spray the nearest rose bed. This admission only sounds absurd, or at least paradoxical due to an ambiguity of term a priori. 'A priori', strictly speaking, means prior to or independent of all experience. Prior to all experience, we could not know what beliefs would cause any particular actions. We sometimes use the term a priori more loosely to mean prior to particular experiences. And it is perhaps from this sense of the term that McIntyre's argument acquires its plausibility. Prior to particular instances of believing that speed kills or instances of spraying roses it would seem nonsensical to suggest a causal bond between them but this is simply because we have knowledge of the sorts of things that cause people to spray roses and the sort of effects that believing that speed kills has. The suggestion seems nonsensical simply because it does not cohere with our knowledge of human behaviour and this knowledge is certainly not a priori. McIntyre's objection highlights the fact that there is a certain intelligibility in human actions which does not seem to be found in the behaviour of inanimate objects. For example when the sun shines, the mercury in the thermometer rises; when a policeman wants the traffic to stop, he raises his hand. (See Schutz, 1943) In an obvious sense, we seem to be able to understand why the policeman raised his arm although we cannot similarly understand why the mercury 'raised itself'. But to say this is just to reaffirm that unlike inanimate objects, people are 'acting agents' who have desires, purposes, motives and conceptions and who also have a knowledge of their environment such that they often know how to fulfil their desires. It is simply because people have a certain intelligence that there is often an intelligible

relationship between their beliefs and their action rather than a relationship of mere de facto regularity. To say this is not to say that such intelligibility precludes a causal explanation of human beliefs and actions and neither is it to say that intellectually intelligible relationships between beliefs and actions or between motives and actions need be particularly causally important.

Conclusion

This philosophy of behaviour has an interpretative, a prescriptive and an evaluative role. We can use it to interpret particular factors as causes or as partial causes of human behaviour and human action; it prescribes what sort of factors we should consider to be causes or partial causes of behaviour and action. When any explanation of human behaviour has been attempted, we can evaluate it in terms of this philosophy to see whether the attempted explanation might reasonably be considered to be known to be correct. Since knowledge is justified belief, its reflective evaluation is a necessary thought not a sufficient condition of any belief's being known to be true. Suppose, for example, that an economist tried to explain the behaviour of some group of producers by suggesting that it was caused by their motive to try to equate the marginal revenue and the marginal cost of their product in order to maximise their profit. Could this theory be known to be true ? In terms of this philosophy of behaviour, motives and desires are the sort of things which could be partial causes of behaviour. Hence, their motives and desires could not explain their economic behaviour although they could be interpreted as partial explanations of that behaviour and such partial explanations might be known to be correct.

We can apply a similar type of assessment to Weber's sociology of action. Weber believes that to explain an action, we must grasp the actor's motive for his action. The explanation must be adequate 'at the level of meaning' i.e. it must be plausible that an actor might have such a motive and that it might produce the action to be explained:

> On the other hand, even the most perfect adequacy at the level of meaning has causal significance from a sociological point of view only in so far as there is some kind of proof for the existence of a probability that action normally takes the course held to be meaningful. (Weber, 1964, P.199)

In other words, we must discover a generalisation to the effect that there is some sort of conjunction between the grasped motive and the action to be explained. There are a number of ambiguities and confusions in Weber's sociology which are worth mentioning. For example, Weber seems to fail to realise that the same effect might be produced on different occasions by different causes and that the same action might similarly be provoked by different motives when he prescribes that we must show that an action normally follows the course held to be meaningful. Does Weber mean that without the appropriate generalisation, our putative explanation cannot be an explanation or, rather, that it cannot be a correct explanation ? It is not clear whether Weber believes that an action is to be explained by its motive in which case the generalisation that that type of motive and that type of action are normally conjoined is only relevant to the verification of the explanation or whether the action is to be explained in a Popperian manner by deducing a statement of its occurrence from the generalisation and the initial conditions of the motive.

The philosophy of behaviour which we have been considering would allow that motives might be partial causes of actions and hence that they might partially explain actions and it is important not to misdiagnose partial explanations as complete explanations. Weber almost appears not to have distinguished between explanations and partial explanations and is led to misrepresent the role and significance of a general conjunction between motives and actions. If a motive completely caused an action then we would always find that the action followed the motive althought the motive might not always precede the action. If the motive almost completely caused the action, we might expect that the action usually followed the motive. But the verification of partial causal claims is far more complex and difficult than the verification of complete

causal claims. There is no reason to expect that an effect normally follows its partial cause. An act of sex can be a partial cause of a baby yet it would be false to say that an act of sex is normally followed by the production of a baby; for more often than not, this conjunction does not obtain. Since motives are only partial causes of actions, and perhaps causally relatively insignificant in many cases, Weber is mistaken when he stipulates that we must discover the existence of a conjunction between motives and action in order to establish that a causal relationship exists between them. Indeed, it would probably be extremely difficult to establish a normal conjunction between any motive and an action. Consider even a general motive like wanting to eat food to alleviate hunger. I doubt whether this motive is normally followed by an act of eating. More often than not, when we have such a motive we are either working and unable to eat or we are in a situation where there is no food to which we have access, or there is food available and we are too lazy to cook it or we are caught between meals and we do not want to lose our appetite or we are trying to lose weight and consciously avoid eating. We often eat when we are not hungry perhaps simply because we enjoy the taste of food or because we do not want to offend a host who has prepared an unappetising meal for us. Consider an action such as theft. We often have an opportunity and a motive for theft. We often desire more money and possessions and realise that stealing from someone else would serve to increase our own stock of money and possessions. Perhaps, on some occasion, this motive might lead us to steal and to cite this motive would be partially to explain our act of theft. Nevertheless, we do not normally steal whenever we have a motive for doing so. Even a professional thief, surely, does not usually steal on the numerous occasions when he has a motive for stealing. Generalisations like these Weber talks of are generally irrelevant to the verification of partial explanations of behaviour in terms of motives.

Perhaps another criticism could be made of Weber. We saw from the paradigm of 'how man works' that there is a host of factors which are able causally to affect human behaviour. Anything which might find a place within this known general scheme is the sort of thing which could be accepted as a known cause, or partial cause of human behaviour. Social scientists sometimes tend to be irrationally selective in their choice of causal factors. Weber focuses his attention on human motives yet there is a great variety of factors other than motives which could explain human action and behaviour. To focus attention exclusively upon human motivation is permanently to block the path to complete explanation of human action and behaviour. Human motives and other 'meaningful' factors might partially explain particular instances of human action but there is an abundance of 'non-meaningful' factors which might very well be causally far more significant. Similarly, some economists tend irrationally to focus undue attention on motivation and upon the causal efficacy of human 'rationality'. There are no reasons either a priori or a posteriori thus to restrict causal enquiry. Rex writes of the explanation of action within a social system that ' ... motivation ... is the ultimate determinant of any behaviour occurring within the system'. (1970, P.88) He even appears to chide those who look for explanations of human behaviour in other directions. For example, he critices 'structuralism' on the grounds that, although this type of explanation may have its uses, ' ... it does not take into account the subjective meaning of a situation for an actor'. (1971, P.32) There are no ultimate explanations of human behaviour; any cause or partial cause which we quote can be considered as an element in a wider causal chain. For practical reasons, any explanation of human behaviour which we are able to furnish is likely to be merely a relatively partial explanation. We can rightly criticise people who mistreat partial explanations as complete explanations but there is no reason to decry any partial explanation simply because it is a partial explanation. Complete knowledge can emerge only from the summation of partial knowledge. There is no more reason to

criticise a structural partial explanation because it ignores 'meaningful' factors than there is to criticise a partial explanation in terms of an actors 'subjective meaning' on the grounds that it does not mention 'structural' factors or the state of the weather or the state of the actors digestive system all of which might causally influence his behaviour.

Human action presupposes an 'acting self' just as knowledge presupposes a 'knowing self'. Hence philosophies such as crude behaviourism which conceive human behaviour simply as a series of physical responses to external physical stimuli are rightly to be criticised as too mechanistic. Nevertheless, it does not follow that human behaviour, including more specific categories of behaviour such as 'action' and 'social action' is not to be correctly explained as the effect of causes. Insofar as human behaviour can be known, it is known in terms of causation.

The explanation of human behaviour is subject to the same strictures as any other causal explanation. For example, classification alone can give us no knowledge of causes. To have classified any behaviour as, for example, 'rational', 'traditional', 'affectual', 'specific', 'diffuse', 'universalistic' or 'particularistic' is hardly to have explained the behaviour or to have added to our knowledge of it. Similarly, we cannot have knowledge of the causes of behaviour in general or of the causes of a general class of behaviour where the members of this class cannot be individually specified. As we have already seen, we might know that conditions A always cause conditions B but we could not know that conditions B are always caused by conditions A. We can only know the causes of specific instances of behaviour. We cannot give an explanation of a general class of behaviour which could be known to be correct. We can only thus explain particular instances of behaviour by citing their particular causes and partially explain them by citing their partial causes. For example, we might explain or partially explain an instance of a certain type of economic activity in terms of the religious ethic to which those engaged in this

economic activity adhered. (See Weber, 1930) We might be able to discover that our partial explanation of this particular instance of a type of economic activity is correct, but we could not thereby conclude that we have discovered a correct partial explanation of all instances of this type of economic activity. Other instances of this type of activity might very well be produced by different causes. Hence, any causal theory of action which is not a theory of specific instances of action cannot reasonably be considered to be known to be true.

CHAPTER SIX

THE PHILOSOPHY OF SOCIAL PHENOMENA

It has been argued that knowledge of 'the external world' is to be obtained through an interplay between our perceptions and our conceptions of it and that systematic knowledge is possible through a knowledge of causes and the effects they produce. It might perhaps be thought that these theses do not apply to phenomena which are characteristically social and that different epistemological principles are involved simply because social phenomena, unlike tables, chairs and clergymen cannot plausibly be considered to belong to 'the external world'. Social phenomena are, in an important sense internal constructions: as we have seen, such a phenomenon as a work-in is not merely a movement of bodies, arms and legs but is partly constituted by the participants' inner conception of what they are doing. Social phenomena cannot be considered solely to occupy what, since the time of Descartes, has been called the 'extended world' of physical objects. Yet, as possible objects of knowledge, it is perfectly proper to regard social phenomena as part of 'the external world' since, although their existence is in some cases not independent of anyone's conception of them, their existence is quite independent of the perceptions and conceptions of the knowing self which seeks knowledge of them. When I confront something which exists quite independently of my perception or conception of it, then I can quite properly consider it to be part of my 'external world'; the possession of such an ontological status is a precondition of anything's being a possible object of my knowledge.

The remainder of this chapter comprises three related sections. The first one deals with the ontological status of social phenomena. The question will be raised: 'How should we interpret social phenomena when we are considering them either as objects of study or as that to which we refer in our theories?'. In the following section, we shall consider social phenomena and causation and ask: 'When should we consider social phenomena to have been correctly explained?'. These central question will re-appear in the last section, which is a discussion of methodological individualism.

The ontological status of social phenomena

Durkheim is particularly keen to insist upon the objectivity of social phenomena and particularly of what he calls 'social facts' which belong to the category of facts which ' ... consists of ways of acting, thinking and feeling, external to the individual, and endowed with a power of coercion, by reason of which they control him'. (Durkheim, 1964, P.3) On another occasion, Durkheim says that a 'social fact' is ' ... every way of acting which is general throughout a given society, while at the same time existing in its own right independent of its individual manifestations'. (1964, P.13) In Durkheim's Paradigm for sociology, 'The first and most fundamental rule is: Consider social facts as things'. (Durkheim, 1964, P.14) In stressing the objectivity of social phenomena, Durkheim is voicing his concern that sociologists might study their own ideas of what exists rather than what actually does exist. He bemoans the alleged fact that ' ... in truth, up to the present, sociology has dealt more or less exclusively with concepts and not with things'. (Durkheim, 1964, P.18) Durkheim seems to believe that if social phenomena are not objectively existing things, then they are not objects of possible knowledge: 'To treat phenomena as things is to treat them as data, and this constitutes the point of departure of science'. (Durkheim, 1964, P.27) His point appears to be that if social phenomena are not objectively existing things, then there cannot be a science of sociology, or, perhaps, as we might prefer to say, that sociology cannot be a successful science.

Durkheim is using language in a rather strange way when he says that social facts are things. For example, it is rather strange to call a fact a 'thing'. It is a fact that that smoking can cause lung cancer but we would hardly say that smoking can cause lung cancer is a 'thing' in the sense that lungs, potatoes and cigarettes are 'things'. Facts are often, although not always, about things. Furthermore, it is strange to call social phenomena, including what Durkheim calls 'social facts', 'things'. Rex accuses Durkheim of reifying the concept of 'social facts' and notes several differences between social phenomena and what would ordinarily be thought of as 'things'. For example, social phenomena are not tangible or directly observable and they do not appear to be independently existing entities unlike what we normally call 'things'. (See Rex, 1970, P.46) We could add that they are not discrete objects unlike tables and chairs and that they cannot be dissected unlike most of what we normally call 'things'.

In response to criticism such as this, Durkheim replies:

> We assert not that social facts are material things but that they are things by the same right as material things, althought they differ from them in type. What precisely is a 'thing' ? A thing differs from an idea in the same way as that which we know from without differs from that which we know from within. Things include all objects of knowledge that cannot be conceived by purely mental activity, those that require for their conception data from outside the mind, from observations and experiments .... To treat the facts of a certain order as things is not, then, to place them in a certain category of reality but to assume a certain mental attitude toward them on the principle that their characteristic properties, like the unknown causes on which they depend, cannot be discovered by even the most careful introspection .... One might even say in this sense that, with the possible exception of mathematical units, every object of science is a thing. (1964, Pp.xliii, xliv)

Durkheim seems to want to distinguish that which can be known a priori from that which can be known only by observation; he wants to distinguish mathematics from empirical science and to claim that all empirical sciences require things as possible objects of knowledge.

Unless Durkheim is using the word 'thing' in an unusual way, then this claim is surely false. We might well agree that for science to be successful, it requires something to study; in order to acquire knowledge, we must study something of which knowledge is possible. But it does not follow that that which the successful scientist studies need be in the usual sense of the term a thing. For example, scientists can successfully study heat and electricity although neither heat nor electricity are things in the sense that fires or electric blankets are things. Not everything which exists or occurs exists or occurs as a thing. Curiously, our language is such that although it would be quite in order to say that there is such a thing as courage and moral virtue, it would be rather quaint, to say the least, to claim that courage and moral virtue are things. Similarly, we can concede that sociologists must study something in order to be successful scientists without necessarily claiming that we should consider social phenomena to be things. We might simply say that we should consider social phenomena to occur or exist, although not necessarily as things. Perhaps, if we were to accept the correspondence theory of truth, we might falsely imagine that the sociologist requires 'things' for his statements to correspond to in order for his statements to be true, although there is no positive indication that Durkheim accepts such a theory. We could modify Durkheim's prescriptive paradigm and suggest that the sociologist should study what actually is the case rather than what is imagined to be the case; he should study actual social states of affairs rather than imagined social states of affairs and real, as opposed to imaginary social phenomena.

We seem to have good reason to believe that social phenomena actually do exist. For example, it seems quite appropriate to ascribe features to social phenomena as when we say that the Gross National Product is high or low or rising and when we say that a work-in was effective or badly supported. It is perhaps worth considering the ontological status of social phenomena in terms of causation. It would seem

reasonable to suggest that that which is a cause or an effect must be something rather than nothing. Social phenomena appear to be caused and to produce effects. We can feel the effects of variations in the Gross National Product just as we can be aware of the effects of public opinion on our own actions and upon other people's actions. Work-ins can have effects as when the work-in at Upper Clyde Shipbuilders affected government policy and the subsequent history of the shipbuilding industry. That social phenomena have causes seems equally apparent. The work-in on Clydeside did not appear in a causal vacuum: it was partially caused by the poor employment prospects in the area; it was partially caused by the acumen and intelligence of the workers' leaders and partially caused by the widespread sympathy and support that they received.

The significance of stressing the objective reality of social phenomena can be adequately appreciated only when we consider an opposing paradigm such as that presented by Popper and Rex. They maintain that the objects of study in social science are not actually existing 'things' but that they are 'theoretical entities'. Popper writes that:

> ... most of the objects of social science, if not them all, are abstract objects; they are theoretical constructions. (Even "the war" or "the army" are abstract concepts, strange though this may sound to some. What is concrete is the many who are killed; or the men and women in uniform, etc.). (1961, P.135)

These theoretical constructions or 'models' have an explanatory function for the social scientist but, according to Popper, we should be careful not to ' ... mistake our theoretical models for concrete things'. (1961, P.136) As we have seen, we can deny that social phenomena are either 'theoretical models' or 'concrete things'.

Perhaps Popper may be confusing the questions of whether social phenomena exist and whether they can be perceived to exist. Those who stress the objective reality of social phenomena often tend to talk as if social phenomena could be directly perceived in the same way that we can perceive an

apple or a table. For example, Radcliffe-Brown writes that when we observe human beings living in societies:

> ... direct observation does reveal to us that these human beings are connected by a complex network of social relations. I use the term 'social structure' to denote this network of actually existing relations .... Social structures are just as real as are individual organisms. (1952, P.190)

The truth of Radcliffe-Brown's second claim would not entail the truth of the first of his claims and his first claim is either ambiguous or false. We might say that we can directly observe objects like tables and apples in the sense that we can experience a percept which can properly be called the percept of a table or the percept of an apple. Such direct observation is not always possible. For example, we cannot directly observe oxygen or the Gross National Product in the sense that a percept which we experience could properly be called a percept of oxygen or a percept of the Gross National Product. Oxygen and the Gross National Product are indirectly observed. Yet, they are no less real than that which can be directly observed: their reality is perhaps most obvious when we consider that that which cannot be directly observed, like oxygen or electricity, can be just as causally efficacious as that which can.

Most, if not all social phenomena are indirectly rather than directly observed; when we indirectly observe a work-in in a shipyard, we directly observe moving arms and legs, ships and tall cranes. Yet, why should we want to say on that account that social phenomena do not actually exist ? We have concepts rather than percepts of social phenomena such as 'wars' and 'armies'; but why should we say that such concepts are necessarily fictitious ? Do wars not often produce noticeable effects such as an increase in the death rate and a reduction in the official unemployment rate; do armies not sometimes sweep across Europe despoiling the countryside ? There seems no more reason to say that social phenomena such as 'wars' and 'armies' are mere theoretical construction than there is to say that 'oxygen', 'heat' and 'electricity' do not actually exist

but are mere theoretical constructions.

Rex adopts a view akin to Popper's. Talking of Durkheim's 'social facts', he claims that we should ' ... insist on greater methodological clarity in recognising that these are not things but theoretical constructs through which we explain uniformities of behaviour'. (1970, P.47) This is in line with Rex's general conception of social phenomena: ' ... such entities as social relations which seemed to be the basic data of sociology [are] not simple observable entities but theoretical constructs used to explain observed behaviour'. (Rex, 1970, P.176) It has been agreed that social phenomena cannot be directly observed; that any social phenomenon exists is a conception. Nevertheless, if we imagine that social phenomena require an explanation, then surely we must assume such conceptions to be <u>true</u>. If the data of sociology are 'social phenomena' which do not exist objectively but are merely the product of the sociologist's imagination, then how could they be correctly explained any more that the present existence of unicorns and dodos could be correctly explained ?

It would seem reasonable to suggest that social phenomena must be assumed actually to exist or to occur if they are assumed to be possible objects of knowledge or the subjects of correct explanations. Nevertheless, could we reasonably concede the claim made by Rex and by Popper that although social phenomena are not assumed to exist, they can be used to <u>explain</u> that which is assumed by them to exist, namely observed behaviour ? This claim is resisted by Durkheim who argues that we should explain 'things' in terms of other 'things'. He argues that: 'A thing is a force which can be engendered only by another force. In rendering an account of social facts, we seek, then, energies capable of producing them.' (1964, P.144) Should we consider that that which exists or occurs can be explained, or can be correctly explained, only in terms of that which exists or occurs ?

a) Social phenomena and 'black boxes'

By casting social phenomena in the role of 'theoretical entities', Rex and Popper could be seen to be advocating the use of 'black box' type theories in social science. The entities referred to in such theories are not necessarily assumed to exist; they are assumed to have a heuristic value. Ryan explains the nature of such theories althought he does not completely accept that they are generally genuinely explanatory. On this view:

> Theories are essentially instrumental in that they are good means for linking together statements about observations. They are heuristically useful because they suggest to us new experiments which make us aware of connections between observable states that we should not otherwise have noticed. Thus the idea of 'unconscious projection' may serve a useful purpose in suggesting to us how we should link the anger a man displays now to fears he had when he was a child .... We do not have to claim that there really is an unconscious mind at work; the network of successful analogy is its own justification. And correspondingly the failure to generate consistent and interesting experimental consequences is what spells death to a theory. In this sense the theory is a black box .... A good box is one which transforms true statements as inputs into true statements as outputs; it should not, in principle, disturb us if we were somehow able to break open the box and find it empty. And plainly, for anyone who holds this view, concepts such an 'economic man' or 'the spirit of capitalism' are seen as complex ways of shuffling our information and arguing about our data. (1970, Pp.92,93)

Such 'black boxes' might be useful in stimulating our minds to generate fruitful hypotheses; but, are 'black box' type theories genuinely explanatory ?

Suppose that we were to try to explain a social phenomenon such as the emergence of capitalism by a 'black box' type of theory and to show that a statement of the emergence of capitalism can be deduced from certain assumptions and concepts such as the 'spirit of capitalism'. First of all, it is not clear that the deduction could be valid unless we were to assume that 'the spirit of capitalism' actually existed. Secondly, unless 'the spirit of capitalism' is assumed by us actually to have existed, we do not seem to have a good reason to consider the theory to afford us an explanation of the

emergence of capitalism. Furthermore, even if this 'black box' were to explain the emergence of capitalism, it is difficult to see how it could be a correct explanation unless 'the spirit of capitalism' actually existed.

Let us consider a 'black box' type of theory as presented by Schutz. He argues that in the study of social phenomena, ' ... the scientist replaces the human beings he observes as actors on the social stage by puppets created by himself and manipulated by himself'. (1943) The 'puppet' or 'personal idea type' is only concept; its existence is confined to the social scientist's imagination. According to Schutz, we should deduce a number of conclusions from this ideal type along with some other assumptions and then compare our conclusions with what actually happens in the real world:

> This homunculus or puppet is supposed to be interrelated in interaction patterns with other homunculi or puppets constructed in a similar way .... As the next step, the circumstances within which such a model operates may be varied, that is, the situation which the homunculi have to meet may be imagined as changed, but not the set of motives and relevances assumed to be the sole content of their consciousness. I may, for example, construct a model of a producer acting under conditions of unregulated competitions and another of a producer acting under cartel conditions, and then compare the output of the same commodity of the same firm in the two models. In this way, it is possible to predict how such a puppet or system of puppets might behave under certain conditions and to discover certain 'determinate relations between a set of variables, in terms of which ... empirically ascertainable regularities ... can be explained'.(Schutz, 1970, Pp.16-17)

Suppose that we try to construct such a conceptual model. We might assume that a producer aims to maximise his profits and that he is operating in a situation of perfect competition. Suppose that we were to add to the model the assumption that the producer does not exist. What could we deduce from the model ? What can we deduce from the assumptions that a producer aims to maximise his profits and that he does not exist ? The model surely becomes absurd. The model is rational only if we assume that a producer were to exist in certain stipulated circumstances. Similarly, what can we

validly deduce from the twin assumptions that 'the spirit of capitalism' promotes a particular type of economic behaviour and that 'the spirit of capitalism' does not exist ?

We might observe a situation where a producer increases the output of a particular product and lowers its price and seek an explanation of this occurrence. We might then produce a conceptual model from which we can deduce a statement of this occurrence. The basis of the model could be the assumption that the producer in question is so rich that he is quite unconcerned about his profit from this particular product and that he lowers its price and increases its output to annoy his less prosperous rivals whenever they offend him and the assumption that they have offended him. Should we consider the occurrence to have been explained ? It is surely quite rational to believe that the model is logically consistent and to continue to inquire why the price was lowered and the output of the product increased. The fact that a statement of the occurrence can be deduced from the model is no reason to believe that the occurrence has been explained. Of course, the situation would be different if we were to imagine the model to be <u>true</u>; but then we would not be considering the theory to be a 'black box' type of theory. If we were to accept that, in fact, the producer was so rich that he was not concerned with his profit from this product and that he lowered its price and increased its output to annoy his rivals we would have reason to consider the occurrence to have been at least partially explained. If the model were assumed to be true, then it would offer a partial explanation of the occurrence in question, even if an incorrect one.

Let us imagine that the particular producer is not so rich that he is unconcerned about the profit he makes from his product and that his overriding aim, at least where his economic activity is concerned, is to maximise his profit from this product. Suppose that he is operating in a situation of oligopolistic competition and that he discovers a new technique which enables him to produce his product more cheaply than

previously and that he lowers the price of his product and increases its output in an attempt to maximise his profit. In such circumstances, how could we reasonably claim that the suggested conceptual model offers a correct explanation of the observed rise in price and fall in output ?

We should consider social phenomena actually to exist or occur (although not necessarily as 'things') when we are regarding them as objects of study or as that to which we refer in our social theories. We shall return to the question of the ontological status of social phenomena at the end of this chapter after we have considered how social phenomena might be correctly explained.

Social phenomena and causation

As we saw in Chapter Four, Durkheim's prescription regarding explanation is ambiguous. Nevertheless, on some occasions at least, he explicitly states that we should seek to explain social phenomena in terms of their causes. The reasons for considering causal explanations to be genuinely explanatory and for believing that knowledge of causes and the effects they produce can provide us with systematic knowledge of 'the external world' have already been discussed and do not need to be repeated here. In this section, I shall consider what sorts of factors are able causally to affect social phenomena and attempt to evaluate Durkheim's prescription that we should seek to explain 'social facts' only in terms of other 'social facts'. Before doing so, however, I would like to consider briefly the attempt to produce general theories of social phenomena.

a) General and particular causal explanations

Durkheim imagines that there is a unique relationship between causes and effects in the sense that one class of cause will always produce another particular class of effect and that any class of effect will always be produced by some other particular class of cause. As we have seen, he explicitly rejects the view that one type of effect might be produced on

different occasions by different types of causes. From these assumptions, he is led to conclude that he can produce general theories of particular classes of social phenomena and that such general theories might be known to be true. He believes that to find the cause of any member of a class of social phenomena is to have discovered the cause of every member of that class and thereby to have correctly explained that particular class of social phenomena. Thus, when Durkheim discusses the division of labour in society and claims that it is caused by an increase in the volume and moral density of society, he does not stipulate what particular instances of the division of labour were so caused; his discussion is directed towards divisions of labour in general. Similarly, in the light of a consideration of Australian Aboriginal society, he produces a theory of religion. Yet, he does not merely claim that his theory correctly accounts for Australian Aboriginal religion; he seems to imagine that his theory thereby correctly accounts for religion in general.

Radcliffe-Brown not only tries to present general theories, such as 'The sociological theory of totemism', but he argues that theories are unsatisfactory if they cannot account for an entire class of phenomena. His point seems to be that where we have a class of phenomena, 'A' where A comprises a1, a2, a3, a4 etc., then no theory of a1, or a2 can be an adequate theory of either a1 or a2 unless it also accounts for a3, a4 etc. He appears to argue that to be a satisfactory theory, a theory must be 'general'; it must account for an entire class of events. He considers Durkheim's theory of religion or 'totemism' and suggests that since totemism can be considered to be a member of the wider class of 'ritualised relations', then Durkheim's theory of totemism must be defective because it cannot account for other instances of ritualised relationships. He notes that in Australia there is a particular ritualised relationship which is akin to totemism: 'Whether we call this totemism or not, it is quite evident that it is closely related to totemism and that any theory of totemism, to be satisfactory, must take it into account'. (Radcliffe-Brown,

1952, P.122) Since totemism is a sub-category of the wider category of ritual relations, he concludes that the problem of the existence of totemism is part of the wider problem of accounting for the existence of ritual relations: 'It is obvious that no solution of the lesser problem of totemism can be satisfactory unless it conforms with or is part of a general solution to the wider problem, i.e. a theory of ritual relations in general'. (1952, P.124)

It has already been argued that we should seek to explain particular phenomena in terms of their particular causes and seek partially to explain them in terms of their partial causes. Any particular type of effect might, on different occasions have a different type of cause. Perhaps in every single case, instances of, say, totemism are differently caused. Only explanations of particular instances of totemism could be known to be correct. Consider too, the previous discussion of family resemblances concepts. Perhaps all members of the class 'totemism' share nothing in common other than being members of that class. If this were the case, what justification would we have for believing that totemism always produces a similar effect or that it is always produced by a similar cause ? How could an explanation of totemism in general, as opposed to one of particular instances of totemism be known to be correct ?

It is difficult to see why Radcliffe-Brown argues that theories must be general in order to be satisfactory explanatory theories. If we discover a complete cause of an example of totemism then why should we not consider that this example of totemism has been correctly completely explained; why should we be dissatisfied with the suggested explanation ? The explanation may not account for other examples of totemism but how does that affect its adequacy as an explanation of this particular example of totemism ? We might want to account for the existence of different games such as table tennis or rugby football. Would we say that no account of table tennis is satisfactory if it does not also account for rugby football ? Why should we want to say that no theory of games is satisfactory

unless it applies to all games ? If any one suggests that he is able correctly to explain social phenomena such as, say, the division of labour, totemism or inflation, we might be wise to inquire: 'What particular instances of the division of labour, totemism or inflation do you have in mind ?'.

b) The fallacy of sociologism

Given that social phenomena are to be explained in terms of their causes, what sort of things should we interpret as causes and partial causes of social phenomena ? When should we consider social phenomena to have been correctly explained or correctly partially explained ?

In answering these questions, Durkheim propounds a point of view which might be called 'sociologism'. According to him, ' ... a social fact can be explained only by another social fact'. (1964, P.145) 'The determining cause of a social fact should be sought among the social facts preceding it and not among the states of the individual consciousness.' (Durkheim, 1964, P.110) He argues that since the main characteristic of what he calls 'social facts' is their power of influencing individual human beings, such social facts cannot be explained in terms of the nature of individual people and that 'Consequently, every time that a social phenomenon is directly explained by a psychological phenomenon, we may be sure that the explanation is false'. (1964, P.104) It is not clear what particular distinction Durkheim wants to make between an explanation and a direct explanation. It is also not clear whether the use of the term 'social phenomenon' rather than the more characteristic 'social fact' was intended by Durkheim: does he mean that one specific type of what we would normally call 'social phenomena', namely 'social facts' are to be explained only in terms of other social facts or does he mean that all social phenomena are to be explained only in terms of other social phenomena ? After all, we would consider such a practice as calling a friend by a particular nickname a social phenomenon although it lacks the coercive nature and generality of Durkheimian social facts.

There is a contradiction in Durkheim's formulation of sociologism. He claims that a social fact can be explained only in terms of another social fact. Yet, how can we reconcile this with the claim that when a social phenomenon is explained in terms of psychological factors the explanation is false ? Presumably, what Durkheim wants to say is that, although social facts or social phenomena can be explained in terms of non-social phenomena, they can be correctly explained only in terms of social facts or social phenomena.

Sociologism might be expressed in such a way that no reference is made to the specific Durkheimian category of 'social facts'. It might be expressed as the claim that 'Social phenomena or occurrences can be correctly explained only in terms of other social phenomena or occurrences' or that 'Social states of affairs can be correctly explained only in terms of other social states of affairs'. A watered down version of sociologism might be the claim that social phenomena and social occurrences are particularly, although not necessarily exclusively relevant to the explanation of social phenomena and occurrences.

Sociologism can be interpreted as a logical or as an empirical claim. As a logical claim, it is fallacious; as an empirical claim it is implausible. If, as Durkheim maintains, society is a reality sui generis and social facts are a particular type of phenomena different in nature from other types of phenomena such as biological or psychological phenomena, then does it not follow as a matter of logic that only social facts can cause and correctly explain other social facts and that society is ' ... a system of things existing separately, by reason of causes peculiar to itself.' ? (Durkheim, 1964,P.122)

A priori, anything might be the cause of anything else. There is no contradiction in saying that only social phenomena or social facts can cause and correctly explain other social phenomena or social facts but, similarly, there is no contradiction in saying that only non-social phenomena can

cause and correctly explain social phenomena or social facts. We cannot logically demonstrate that any type of phenomena must be causally relevant in the production of social phenomena. Consider any specific catagory of phenomena, which we might call 'x-type' phenomena. What will cause x-type phenomena ? We could never know the answer to this a priori, nor could we know that other x-type phenomena need have any causal effect at all upon the production of x-type phenomena. I think that we might still reasonably reject sociologism as an a priori claim even after considering the arguments which Durkheim brings to its defense. He argues that there is a difference of kind between individual and social phenomena and that when individuals congregate in social groups, there emerges an entity ' ... of a new sort. It is, then, in the nature of this collective individuality, not in that of the associated units, that we must seek the immediate and determining causes of the facts appearing therein.' (1964, P.103) This argument is defective. We might well say that sperm and an egg combine to form a being 'of a new sort' yet it would not follow that a baby could not be partially explained correctly in terms of an egg or sperm; we need not look only to the resultant baby for the causes of its existence. Similarly, we could recall the one-man-operated bus and say that there is a difference in kind between this individual bus and the phenomenon of 'traffic' resulting from the interaction of a host of vehicles. We could say things of traffic which we could not say of individual vehicles as when we say that the traffic is dense or has been dispersed. Individual vehicles, like individual members of society, come and go while traffic and societies can continue as enduring entities apparently unaffected by their changing composition. Yet, it does not follow from this that 'traffic facts' can be correctly explained only by other 'traffic facts' or that 'social facts' can be correctly explained only by other 'social facts'. 'Traffic facts' are to be correctly explained by factors which can find a place within the known scheme of how individual vehicles comprising the traffic 'work'.

Durkheim protests that 'A whole is not identical with the sum of its parts'. (1964, P.102) Perhaps in some instances this is true: a flow of traffic might well possess properties which none of the vehicles involved in the flow possess; yet it does not follow that traffic flows cannot be completely and correctly explained in terms of factors relating to how individual vehicles 'work'. We saw in the last chapter that it is often maintained to be 'illegitimate' to try to reduce 'meaningful' facts to 'non-meaningful' facts. Similarly, Durkheim maintains that social facts cannot be 'reduced' to facts about individual people. As we saw, to say that A causes or partially causes B is not to say that A is reducible to B or that B is reducible to A. To say that social phenomena can be partially or completely caused by non-social phenomena is not to say that statements about these non-social phenomena logically entail or can be reduced in meaning to statements about the appropriate social phenomena and vice versa.

Durkheim stresses that 'social facts' have a power of coercion over individual people; they form part of their 'external world'. From this externality, he argues that social facts must be explained by other social facts and ' ... that sociology is not a corollary of individual psychology'. (1964, P.101) All that follows from Durkheim's premises is that a psychological account of social phenomena might be incomplete if it ignores the effect of external social facts upon individual psyches. If we imagine a number of buses in a traffic flow then we might say that the movement of all the other buses confronts each bus and bus driver as an external 'traffic fact', yet it would be a mistake to conclude that the flow of the traffic could be correctly explained only in terms of external 'traffic facts' rather than in terms of how individual buses 'work' (albeit allowing for how individual bus drivers react when confronted by external 'traffic facts').

He restates his case by saying:

> The group thinks, feels, and acts quite differently from the way in which its members would were they isolated. If, then, we begin with the individual, we shall be able to understand nothing of what takes place in the group .... Consequently, every time that a social phenomenon is directly explained by a psychological phenomenon, we may be sure that the explanation is false. (1964, P.104)

Again, all that this argument shows is that in accounting for the behaviour of groups comprising individual people, we should be aware that the people are acting within a group; it does not show that factors relating to individual behaviour, including psychological factors cannot completely and correctly explain group behaviour. It is sometimes said that people behave quite differently when they are behind the wheel of a car than when they are mere pedestrians and that they drive their cars differently, perhaps more aggresively when they are in dense traffic rather than in deserted country lanes. But what would follow from this ? It would not follow that only 'traffic facts' could correctly explain the behaviour of dense traffic. To alter the analogy, we might say that we could not deduce a statement of the behaviour of a vehicle's engine from a knowledge of the way its component parts would behave if the engine were dismantled; yet, facts relating to these individual components and their reactions when juxtaposed with other components will completely and correctly explain the activities of the engine.

Could we not claim that, as a matter of contingent fact, social phenomena are particularly or even exclusively causally relevant to the production of social phenomena ? To appreciate the implausibility of this claim, let us consider a specific example of its defence.

In his study of Suicide (1952), Durkheim tries to follow his own prescription and to ' ... seek the explanation of social life in the nature of society itself'. (1964, P.102) His contention is that ' ... the social suicide rate can be explained only sociologically'. (1952, P.299) He tries to establish this conclusion by showing that other possible

explanations of the suicide rate are not correct. To do this he shows that there is no concomitant variation between rates of suicide and non-social factors such as psychological, racial, ethnic, genetic or climatic factors while there is a concomitant variation between 'social facts' regarding the integration of social groups and the 'social fact' of the suicide rate. Adhering to his prescriptive paradigm that one class of effect must have one class of cause, he classifies suicides into three types: egoistic, altruistic and anomic; he then cites three types of social facts as the causes of the three types of suicide. For example, he writes: 'If we agree to call this state egoistic in which the individual ego asserts itself to excess in the face of the social ego and at its expense, we may call egoistic the type of suicide springing from excessive individualism'. (1952) He tries to explain egoistic suicide by the 'social fact' of a relative lack of integration of particular social groups and he tries to show that these two 'social facts' are covariant. He argues that there is a higher suicide rate among Protestants than among Roman Catholics because there is more integration of Roman Catholics within their social group; individual Protestants are more free to question religious authority and are more insulated from their social group than are Roman Catholics. Similarly, Durkheim quotes other 'social facts' as the causes and correlatives of altruistic and anomic suicide.

What renders Durkheim's argument implausible is partly his mistaken theory of the verification of causal claims. He imagines that:

> We have only one way to demonstrate that a given phenomena is the cause of another, viz., to compare the cases in which they are simultaneously present or absent, to see if the variations they present in these different combinations of circumstances indicate that one depends on the other. (1964, P.125)

This is what he calls the method of 'concomitant variation or correlation'. But such concomitant variation or correlation is neither a necessary nor a sufficient condition for the establishment of a causal relationship.

To say that A completely or partially causes B is not to say that A and B are covariant or that every change in A will produce or be correlated with a change in B; and neither is it to say that ceteris paribus the more there is of A, the more there will be of B. Thirst might partially cause me to leave my chair and go towards the nearest source of drinking water but we would scarcely expect a positive statistical correlation between degrees of my thirst and degrees of my going towards a tap, if such things could be measured. If I have a slight thirst, I will not slightly go to the nearest tap. Lack of water can lead to our death while too much water can also be fatal. Would we expect there to be a positive correlation between our water intake and degrees of our being alive or dead ? It would be absurd to suggest that we would become more alive and more dead as we increased our intake of water. Sperm can be a partial cause of babies, yet we need not expect there to be a positive statistical correlation between the amount of sperm and the number of babies produced. We need not expect that ceteris paribus, the more sperm that is produced, the more babies that will be produced. With 'other things remaining equal' i.e., with no change in male or female biology and no change in the contraceptive or sexual habits of the nation, we need not necessarily expect an increase in the amount of sperm produced to lead to a corresponding increase in the number of babies produced. Hence, to show that non-social factors do not concomitantly vary with social factors is not to show that such non-social factors do not partially or completely cause the particular social factors.

Moreover, Durkheim's claim that social facts can be explained, or correctly explained only in terms of other social facts is at variance with the previously discussed prescription that we should not consider beliefs to be known to be true if they do not cohere with our prior knowledge. Although it is true that in many ways our knowledge of societies is lamentably scanty, Durkheim is not quite correct when he says of the social scientist that 'When he penetrates the social world, he must be aware that he is penetrating the unknown ... '.(1964, P.xlv)

A similar mental attitude to sociological enquiry is suggested by Thomas and Znaniecki: ' ... the only safe method is to start with the assumption that we know absolutely nothing about the group or problem we are to investigate'. (1927, P.19)

We do have knowledge of the sorts of things which produce social phenomena. We know that social phenomena like Gross National Products, trade cycles, prostitution and marriage are, at least partly, the result of the action and behaviour of individual people although they are obviously not always the intended result of such activity. It is hardly controversial to say that if no more individual people were to marry, then the institution of marriage would cease to exist although, as we shall see, this is distinct from the controversial and probably false claim that social phenomena such as marriage can be 'reduced' to factors relating solely to individual people. Similarly, we know that social phenomena are produced within the context of the natural world which can affect and is affected by them. The Gross National Product and the life styles of numerous families are likely to be affected by the presence of oil in the North Sea. Napoleon's retreat from Moscow was greatly affected by the weather. According to newspaper reports, the installation of natural gas in Cambridge resulted in a decrease in the suicide rate; apparently, natural gas is non-toxic, unlike its predecessor, and this led to the frustration of some potential suicides.

As an empirical claim, sociologism is implausible. There seems to be no reason to believe that only social phenomena should be considered as the causes or partial causes of social phenomena nor to believe that social phenomena are generally particularly significant in the production of other social phenomena.

We are not justified in considering our explanations of social phenomena to be correct unless such explanations can be shown to cohere with our prior knowledge relating to the production of social phenomena and particularly with our

knowledge of human behaviour. What sorts of things should we consider as causes and partial causes of social phenomena ? There is no reason either a priori or a posteriori to stipulate that only certain categories of phenomena should be considered as partial or complete causes of social phenomena. We should be prepared to consider anything as a partial or complete cause of a social phenomenon provided that the appropriate causal claim is such that there are good reasons for its acceptance and, specifically, provided that the claim coheres with our relevant prior knowledge. For example, should an occurrence such as a failure of the public electricity supply in New York be considered as a partial cause of an increase in the birth rate there several months later ? Perhaps it should. Perhaps the appropriate causal claim could be shown to cohere with our relevant knowledge of human biology and behaviour.

Should Durkheim's explanations of suicide and variations in rates of suicide be considered to be correct ? For example, is a relative lack of social integration the sort of thing which might cause or partially cause people to commit suicide and thereby influence the suicide rate ? If it were considered to be a complete explanation then it could scarcely be reasonably thought to be correct. The claim that a relative lack of social integration completely caused a particular rate of suicide to obtain does not cohere with our knowledge of the steps that potential suicides must take in order to kill themselves. Interpreted as a partial explanation, there may be grounds for considering it to be correct. We may claim that a relative lack of integration of a social group might partially affect the rate of suicide experienced by that group. This claim might cohere with our relevant prior knowledge. A relative lack of social integration might affect a potential suicide by making him more lonely and unhappy than he might otherwise have been and removing from him a possible source of comfort and psychological support. He might be more likely actually to commit suicide in such an atmosphere than in circumstances where he was well integrated within his social group. If we imagine that numerous potential suicides are so

affected, we might plausibly suggest that the degree of integration of a social group might partially affect the rate of suicide experienced by the group. Of course, the degree of social integration will only operate as a partial cause in combination with other types of partial causes. For example, it will affect the suicide rate by affecting the psychological states of potential suicides and it would be just as appropriate to cite such psychological states as partial causes of suicide and rates of suicide as to cite a social partial cause such as the degree of social integration.

Durkheim himself implicitly violates his own explicit prescription when he tries to show that his explanations of different types of suicide are correct. For example, he considers the three factors of suicide rate, levels of education and extent of religious traditionalism concomitantly to vary and wonders which factor or factors are causes and which effects. He reasons that the extent of religious traditionalism is the cause of both the suicide rate and the level of education since:

> ... it is impossible to understand how erudition can lead to suicide; such an explanation is in contradiction to the laws of psychology. Education, especially the elementary branches of knowledge, reaches only the more superficial regions of consciousness; the instinct of self preservation is, on the contrary, one of our fundamental tendencies. It could not, then, be appreciably affected by a phenomenon as far removed and of so feeble an influence. Thus we come to ask if both facts are not the consequence of an identical condition. This common cause is the weakening of religious traditionalism, which reinforces both the need for knowledge and the tendency towards suicide. (1964, P.132)

In effect, Durkheim appears to be saying that the claim that the level of education affects the suicide rate does not cohere with our relevant prior knowledge while the claim that the extent of religious traditionalism affects both the level of education and the suicide rate does and hence that the suicide rate is to be correctly explained in terms of the extent of religious traditionalism rather than in terms of the level of education. To say this is implicitly to deny that when we

seek to explain social phenomena, we are in the presence of the unknown and that, in the course of scientific enquiry, all our preconceptions should be eradicated.

Furthermore, why do we say that the claim that a decline in religious traditionalism affects the suicide rate coheres with our relevant knowledge ? We say so because it is plausible to suggest, for example, that with a decline in religious traditionalism, a source of psychological support for the potential suicide is removed and that the impeditive belief that suicide is sinful becomes less strongly held. It is plausible to suggest that a decline in religious traditionalism might affect the suicide rate because it is plausible to suggest that the psychological states of potential suicides will be affected by this social phenomenon and that these psychological states will subsequently influence the rate of suicide. Durkheim's partial explanation of the suicide rate might reasonably be considered to be correct only because it is reasonable to suggest that non-social factors such as emotional and intellectual states are also partial causes of the suicide rate and when trying to explain the suicide rate, there is no reason to accord social factors, such as a decline in religious traditionalism an epistemologically favoured place.

It was suggested in the previous chapter that social scientists are often irrationaly selective in their choice of factors which they are prepared to consider as causes and partial causes of human behaviour and action. It might be suggested that they are sometimes similarly selective when they attempt to account for social phenomena; Durkheim, as we have seen, is particularly keen to restrict the range of causal enquiry in sociology. Thomas and Znaniecki also appear to want to restrict and <u>a priori</u> direct the scope of causal enquiry in social science when they say that we should seek an individual and a social phenomenon when we seek to explain any particular individual or social phenomenon and that this is:

> The fundamental principle of both social psychology and sociology - the principle without which they can never reach scientific explanation .... The cause of a social or individual phenomenon is never another social or individual phenomenon alone, but always a combination of a social and an individual phenomenon. (1927, P.44)

What reasons are there for accepting these claims ? Why should we say that a social phenomenon must have a social cause and must have an individual cause; why should we accept the implicit assumption that social phenomena must be caused either by social or by psychological factors ?

Rex appears to criticise attempts to explain social phenomena when such attempts are not specifically geared to producing sociological explanations. He maintains that it is a defect of much social research that ' ... it does not differentiate social from other determinants of behaviour'. (1970, P.40) Talking of some studies to seek the causes of social phenomena like delinquency, he bemoans the fact that by such studies ' ... we are not taken much further in our attempt to understand the social structure, or the social determinants of behaviour'. (1970, P.34) Why should we imagine that there is particular merit in seeking and trying to identify the specifically social causes of social phenomena ?

Methodological individualism

In this section, I want to consider the topic of methodological individualism, which relates to several of the issues considered in this chapter. For example, it might mistakenly be thought that to reject sociologism is necessarily to advocate methodological individualism. Lukes argues that there is a variety of issues which are rarely properly distinguished regarding this topic and I want to consider three such issues cited by him and consider methodological individualism as a theory of social explanation, a theory of linguistic meaning and lastly as an ontological theory. None of these theories, it will be argued, is plausible.

a) Methodological individualism and social explanation

As a theory of social explanation, methodological individualism is the theory that ' ... facts about society and social phenomena are to be explained solely in terms of facts about individuals'. (Lukes, 1968) It is the theory that ' ... all attempts to explain social and individual phenomena are to be rejected (or ... rejected as rock bottom explanations) unless they refer exclusively to facts about individuals'. (Lukes, 1968)

The theory of social explanation which has been defended in this chapter is quite different from this. To say that, in order to be considered to be correct, explanations of social phenomena should be such that they cohere with our knowledge relating to the production of social phenomena and particularly with our knowledge of the role of individual people in producing social phenomena is not to imply that such explanation need refer exclusively, nor even refer at all to facts about individual people. It is perfectly in order to cite a social factor such as a relative absence of social integration as a partial cause of a suicide or of the suicide rate; this explanation could cohere with our knowledge of individual people without referring exclusively to facts about individual people. Furthermore, there is no reason why we should not give correct and complete explanations of some social phenomena without referring to any facts about individual people. We might perhaps completely and correctly explain the existence of particular legislation in terms of our legal system and Acts of Parliament. If it is objected that facts about decisions of Parliament can be 'reduced' to facts about individual people, namely Members of Parliament, then this objection, even if accepted, would be irrelevant since, if statements about social phenomena mean the same as certain statements about individual people, then there could be no merit in quoting the latter rather than the former statements. But this leads us to methodological individualism as a theory of meaning.

b) Linguistic methodological individualism

The theories that:

> ... every statement about social phenomena is either a statement about individual human beings or else it is unintelligible and therefore not a statement at all ... and that statements about social phenomena are translatable without loss of meaning into statements that are wholly about individuals (Lukes, 1968)

constitute linguistic methodological individualism. It is illustrated by Jarvie, one of its proponents as follows: ' "Army" is merely a plural of soldier and all statements about the Army can be reduced to statements about the particular soldiers comprising the Army'. (See Lukes, 1968) As we saw in Chapter Three, there is a distinction between the meaning of a word or expression and to what, if anything, it refers. For example, if I were to talk of the present manager of Glasgow Celtic, I would be referring to Jock Stein; yet, the expression 'the present manager of Glasgow Celtic' does not mean 'Jock Stein'. There are two claims involved with linguistic methodological individualism. There is the claim that statements about social phenomena mean the same as statements about individual people and there is the claim that statements about social phenomena are statements about individual people i.e. the statements about social phenomena refer to individual people. Lukes rejects both claims, arguing that ' ... although statements about armies are true only in virtue of the fact that other statements about individuals are true, the former are not equivalent in meaning to the latter, nor a fortiori are they "about" the subject of the latter'. (Lukes, 1968) There is good reason to accept Lukes's conclusions.

Mandelbaum casts doubt on linguistic methodological individualism. He argues that:

> ... it is impossible to escape the use of societal concepts in attempting to understand some aspects of individual behaviour: concepts involving the notions of status and role cannot themselves be reduced to a conjunction of statements in which these or other societal concepts do not appear. (1955)

His point is that often, when we imagine that social phenomena can be described solely in terms of statements about individuals,

such individualistic description implicitly appeals to statements about social phenomena. For example, suppose that we were to try to describe an army in terms of statements about individual soldiers. Mandelbaum would argue that it only makes sense to talk of 'soldiers' in the context of the social framework provided by an army. Similarly, suppose that we were to try to describe the activities of Parliament in terms of statements about individual people. We might produce statements about Members of Parliament and the speeches they made and the way they voted, the effect of such voting and so forth. Would it make sense to refer to individual people as Members of Parliament outwith the specific social context of our political system ? It is because the particular people concerned have the social status of Members of Parliament and are recognised to have such a status that their individual actions are of significance in the production of legislation.

Suppose that we say, 'I am going to join the Army' or 'I am going to join the Navy'. A perfectly intelligible meaning can be attached to these statements. Yet, it is far from clear that we could re-express the same points by saying that we were going to form a particular association with a stipulated selection of soldiers or sailors. In any case, since the meanings of the statements are quite clear as the statements stand, there is no obvious point in trying to re-express them in such terms, far less in saying that they must be expressible in such terms. We might say, 'Glasgow Celtic won the European Cup in 1967'. This statement is true by virtue of the fact that eleven men whom we could individually name won a particular football match. Nevertheless, that Celtic won the cup cannot mean that these particular individuals won the relevant game since it could still have been the case that Celtic won the cup even if they had fielded eleven different players. Statements about the former eleven individuals would not be synonymous with statements about the latter eleven players; and hence, statements about Celtic are not necessarily synonymous with statements about individual members of the team.

Statements about social phenomena might always refer to individual people even although statements about social phenomena cannot always be translated into statements about individual people. Nevertheless, it is not plausible to argue that statements about social phenomena always refer to individual people. We would not be talking about individual people if we were to suggest that Celtic will be relegated next season. We might say, 'In one hundred years time, Celtic will win the European Cup again'. This statement is quite intelligible; yet, it is surely a statement about 'Celtic' rather than a statement about specific people who are, presumably, as yet, unborn. If we say that Celtic has a proud history or that its home ground is in the east side of Glasgow, to whom are we referring ?

c) Ontological methodological individualism

Ontological methodological individualism is the claim that ' ... in the social world only individuals are real'. (Lukes) This presents an interpretation of social phenomena which was criticised at the beginning of this chapter where we considered Popper's view that, for example, 'armies' are not 'real' and that what is real are the individual soldiers comprising them. It is, of course, true that if there were no individual soldiers there would be no armies just as there would be no football teams if there were no individual football players, yet it does not follow that armies or football teams are less real than soldiers or football players. Consider cities such as Glasgow or London. Walking around these cities, what we see is houses, parks, factories, people and so on; yet, it would be somewhat quaint to say that Glasgow and London do not 'really exist' and that it is only the houses, parks, factories and people which are real. Furthermore, is there not a case for saying that the existence of Glasgow and London cannot be 'reduced' to the existence of particular buildings simply because the identity of these cities is not affected by the removal and construction of any particular buildings ? Durkheim presents a similar case for saying that the existence of social phenomena cannot be 'reduced' to the existence of

individual people and their activities. Nevertheless, even if it were the case that the reality of social phenomena is merely the reality of individuals and their behaviour, why should we say that, unlike individuals and their behaviour, social phenomena are not real; what additional ontological status would social phenomena lack ?

We would surely say that, for example, buses are real. Yet, buses are composed of individual parts such as seats, wheels and engines. Should we modify our views and say that, after all, buses are not real and that it is things like seats, wheels and engines which are real ? Does a bus have an existence over and above the existence of its component parts ? We might reply that it has an existence as a bus which is distinct from the existence of, say, the chassis as a chassis. Ontological methodological individualism will lead to an absurd regress even if the regress is not infinite. If we were to say that social phenomena are not real because they comprise individual people and their activities, why should we be prepared to allow that individual people are real ? Why should we not say that people are merely theoretical entities and that it is arms, legs, bowels, lungs and fingers which are real or that fingers are merely theoretical entities and that it is folds of skin, knuckles, tufts of hair and fingernails which are real or, yet again, that fingernails do not really exist but are merely theoretical entities ? Given what the words 'real' and 'exist' generally mean, it is true to say that social phenomena such as armies or football teams exist and that they are real. Of course, we could choose to redefine the words 'real' and 'exist' so that it became necessarily true that social phenomena are not 'real' and do not 'exist'; we could perhaps choose to say that only that which can be directly observed actually 'exists' and is 'real'. But what would be the point of using these words in such an unusual way ?

CHAPTER SEVEN

CONCLUSION

In Chapter One, it was suggested that the relationship between philosophy and knowledge has commonly been misconceived and that philosophy has an active part to play in the acquisition of knowledge, there necessarily being a systematic relationship between our knowledge and our implicit philosophical assumptions. Knowledge presupposes what has been called a 'Paradigm', whose component parts include interpretative, prescriptive and evaluative paradigms. Such assumptions relate to our conceptions rather than to our perceptions. For example, we need not assume that there is any difference in perceptual experience between those who regard, say, armies or football teams as existing phenomena or 'things' and those who conceive them merely to be 'theoretical entities'. Similarly, there is no reason to assume that Central State Materialists would, in the purely visual sense of the term, 'see' people differently were they to adopt a different interpretation of human behaviour and mental states. Since it has been argued that a systematic knowledge of the world is available through a knowledge of the effects of particular causes and also that particular events and states are to be correctly explained in terms of their causes, it has been convenient to combine a discussion of the presuppositions of knowledge and of the presuppositions of explanation; yet, it is perhaps in relation to explanation that the distinctive role of philosophy in our understanding of the world is most apparent. Suppose we were to accept that an increase in the volume and moral density of society explained an increase in the social division of labour. This acceptance would not only imply

interpretative assumptions regarding the nature of the division of labour and the volume and moral density of society, it would imply the acceptance of a particular prescriptive standard to which a theory should conform in order to be considered to be genuinely explanatory and it would also imply an evaluative assumption to the effect that the theory conformed to this prescriptive standard.

We cannot decide whether a theory is explanatory simply through perception. We cannot perceive that an increase in the volume and moral density of society would explain the division of labour; we cannot perceive that an event or occurrence is explained by its suggested cause and is partially explained by its suggested partial cause. By perception, we cannot discover whether or not a social institution can be explained by suggesting that it has a particular 'function'. Linguistic convention cannot decide what prescriptive standard of 'explanation' we should accept. It is not sufficient to ask how the term 'explanation' is defined in any particular community since it is always appropriate to enquire whether any particular linguistic convention should be accepted. By a philosophical convention, to 'explain' means to deduce a statement of the occurrence of the event requiring an explanation from a covering law and certain initial conditions; but it is quite appropriate to ask whether this convention should be accepted. Indeed, as we saw in Chapter Four, it is possible to give good reasons for rejecting this conception and offering a conflicting paradigm of explanation. Hence, even if it were possible to obtain knowledge without commitment to philosophical assumptions and if, as the empiricists assume, knowledge could be obtained through perception alone, we would still require philosophical assumptions in order rationally to understand the world since the acceptance of any theory as an explanation implies the acceptance of prescriptive and evaluative paradigms.

The philosopher can correctly be said to make a positive contribution to our knowledge. Briefly to review this conclusion, let us reconsider a conflicting view of the scope of

philosophy. Ayer bases his philosophy on the assumption that statements are either empirical hypotheses or tautologies or else they are literally meaningless, and the assumption that only statements which are literally meaningful ' ... can properly be said to be either true or false'. (Ayer, 1971, P.20) Empirical or factual statements ' ... must refer to sense experience'. (Ayer, 1971, P.96) It is the mark of a genuinely empirical statement that ' ... some experiential propositions can be deduced from it in conjunction with certain other premises without being deducible from these other premises alone'. (Ayer, 1971, P.52) Empirical statements are held to be verified simply by observation; by the evidence of our senses. In Ayer's view, doubt regarding their truth or falsity is appropriate only in the face of insufficient sensory evidence and, in principle, their truth or falsity, or at least their probability could be established by producing further sensory evidence. Corresponding to the two types of meaningful statements, there is considered to be two types of rational discourse, the inductive and the deductive. The philosopher is thought to be concerned with deduction, which is thought to involve anaytical statements, which are tautologically true rather than statements of fact. Induction, it is claimed, is the province of the scientist and he alone can produce knowledge of matters of fact. Ayer considers that science ' ... is essentially predictive' and that empirical hypotheses are ' ... designed to enable us to anticipate the course of our sensations'. (1971, Pp.130,129) He concludes accordingly that the justification of empirical hypotheses is their success in predicting our perceptual experience: 'What justifies scientific procedure, to the extent to which it is capable of being justified, is the success of the predictions to which it gives rise: and this can be determined only in actual experience'. (Ayer, 1971, P.67) Philosophy is said to have no role in the validation of empirical hypotheses, whose ' ... validity would be independent of the validity of any philosophical thesis'. (Ayer, 1971, P.96)

If science can succeed independent of philosophy, what is the scope of philosophy ? According to Ayer, philosophy

issues in definitions, albeit definitions of a particular sort, namely, 'definitions in use', which are contrasted with the 'explicit definitions' found in dictionaries. We define a symbol in use ' ... by showing how the sentences in which it significantly occurs can be translated into equivalent sentences, which contain neither the definiendum itself, nor any of its synonyms'. (Ayer, 1971, P.81) The pronouncements of the philosopher are considered to be a particular type of statement which is true by virtue of the meaning of the words it contains and conveys no factual information. Ayer concludes from his own principles that there is no warrant for the existence of rival philosophical theories or 'schools' of thought. He argues that we can reasonably dispute a proposition only when there is a lack of evidence to determine its truth but philosophical propositions can never be so placed since it is the task of philosophy merely ' ... to elicit the consequences of our linguistic usages'. (Ayer, 1971, P.176) And hence ' ... the questions with which philosophy is concerned are purely logical questions; and although people do in fact dispute about logical questions, such disputes are always unwarranted' (Ayer, 1971, P.176) since, according to Ayer, at least one of the disputants must be denying a proposition which is necessarily true or be asserting one which is necessarily false.

If Ayer's conception of philosophy were correct, then the entire venture of constructing a Paradigm would be senseless since paradigms are neither necessary truths nor are they verifiable by the type of sensory experiences which Ayer's verification principle requires. Consider a paradigm like 'We should consider an event to have been explained when we have suggested its cause and to have been correctly explained when the suggested cause is its cause' or 'Social phenomena should be interpreted as objectively real phenomena'. Neither of these statements could be considered to be 'analytic'; nor for that matter do they appear to conform to what are normally considered as 'synthetic' statements. We could not decide their truth or falsity merely through perceptual experience. We could not decide whether social phenomena should be interpreted as real

phenomena or as 'theoretical entities' merely by a further series of observations of social phenomena since what is at issue is how such observations should be interpreted. The conclusion that paradigms must thereby be meaningless and hence neither true nor false can be avoided by denying the premise of Ayer's argument and affirming the theory of meaning and truth that was suggested in Chapter Three. There it was argued that meaning and truth relate to conceptions rather than to perceptions and hence we need not require meaningful statements to refer solely or even partly to sensory experience. Truth relates to what is the case rather than to what is and this apparent verbal quibble is important. It is only if we consider truth to relate to what is, to what Hume called 'real existence' that the verification principle is plausible since it seems reasonable to suggest that what is must, at least in principle, be capable of being perceived and hence to suggest that statements which do not relate to perceptions cannot relate to what is and hence can be neither true or false. Individual paradigms can be considered to be either true or false insofar as they state what could intelligibly be considered to be the case. And it is surely quite intellectualy conceivable (though not, of course, perceptible) that it is the case that we should consider an event to have been explained when we have suggested its cause and that we should interpret social phenomena as objectively real phenomena.

Similarly, we can deny that all rational arguments are either deductive and tautological or inductive in the sense of being established by the repetition of particular perceptual experiences. We justify individual paradigms by giving reasons for their acceptance and in terms of their coherence with other paradigms and other relevant elements of our knowledge. An overall Paradigm is not a deductive system in the sense that acceptance of any one individual paradigm logically commits us to accepting all the other paradigms and neither is a Paradigm an inflated tautology. The structure of a Paradigm and the reciprocal bonds between its component paradigms is well accounted for by Schopenhauer's description of his own philosophy:

> ... if it admits of being broken up into parts to facilitate its communication, the connexion between these parts must yet be organic, i.e. it must be a connexion in which every part supports the whole just as much as it is supported by the whole, a connexion in which there is no first part and no last, in which the whole gains in clearness from every part, and even the smallest part cannot be completely understood until the whole has already been comprehended. (1883, P.viii)

We have already seen that although Durkheim explicitly claims that sociological knowledge is independent of philosophical assumptions, much of his writings can be interpreted as the presentation of a philosophical Paradigm for sociology. Similarly, even in the empiricist philosophy of Hume where he appears merely to be offering a particular definition of a term or elucidating some aspect of linguistic usage, he is perhaps better understood to be offering prescriptive, evaluative or interpretative paradigms. We may recall his analysis of 'causation' and his theory of 'impressions' and 'ideas'. He obviously does define the terms 'impression' and 'idea' in a particular way but he does far more than indulge in a linguistic exercise. He claims that all that there is in the mind is either an impression or an idea and this claim is neither an analytical truth nor an empirical hypothesis verifiable by induction. Hume's theory can plausibly be considered as a particular <u>interpretation</u> of mental phenomena. Similarly, we can interpret his analysis of causation as a prescriptive paradigm to the effect that we should not conceive that certain events actually <u>physically necessitate</u> other particular events but merely that some types of events are constantly conjoined with other types of events. Again, this prescription appears to be neither a tautology nor a straightforward empirical hypothesis.

Despite Ayer's specific theory of the nature of philosophy, some of his claims can be regarded as interpretative or prescriptive paradigms. For example, he writes that prediction is the essence of science and that empirical hypotheses are rules for the anticipation of sensations. These hardly seem to be tautologies. They could perhaps be regarded as factual descriptions of the way science usually proceeds, but if we

interpreted them in this way, Ayer would appear to have made the mistake that Kuhn was accused of making in Chapter Two. We cannot judge that science is successful or conclude that its success should be judged on the basis of its predictive ability merely by observing what scientists actually do. Not only is there usually considerable interpretation required in order to judge whether science actually has predicted (or explained) accurately, but we are required neither by logic nor by sensory experience to conclude that ability to predict accurately should be the standard by which the success of science is judged. If science is a matter of fact essentially predictive, it is still quite appropriate to ask whether it should be essentially predictive. Should it not, perhaps, be essentially explanatory, we might ask ? To assume that science is a successful venture insofar as it successfully predicts is surely to have accepted a particular prescriptive paradigm regarding how we should preceed in order rationally to understand the world. Similarly, some of Ayer's 'definitions' have the appearance of disguised interpretations and prescriptions. For example, he considers that the question, 'What is the nature of a material object or a mental state ?' is a request for a particular definition of the terms 'material object' or 'mental state'. In Chapter Five, where we considered a similar question, it was argued that we were not looking for any particular linguistic elucidation of the term 'mental state' ; rather, we wanted to know how we should interpret actual mental states since whether mental states could correctly be considered as possible causes or partial causes of human behaviour depends on how they are correctly interpreted. Yet, the actual definition which Ayer gives of the term 'material object' could readily be regarded simply as a particular empiricist interpretation of material objects akin to Hume's empiricist conception of mental states. Ayer claims that material objects are reducible to percepts or what he calls 'sense-contents'. Material objects, he claims are logical constructions from our sense-contents. Similarly, the British State is considered to be a logical construction from individual British people. By this he means that the symbol 'state' is definable in terms of certain symbols which stand for individual people, not explicitly but

'in use'. It is totally clear that Ayer is describing logical relations between types of sentences rather than offering a particular way of conceiving of material objects and social phenomena such as states ?

In the light of the conclusions of the preceding chapters, Ayer's claim that the truth of empirical hypotheses is quite independent of the truth of philosophical assumptions hardly seems plausible. Consider an empirical hypothesis such as, that the division of labour was caused by an increase in the volume and moral density of society or that the emergence of capitalism was partially caused by the Protestant Ethic. The truth of these claims is not independent of the truth of philosophical assumptions. Can any event actually cause the production of another event ? Is the division of labour the sort of thing which could be the effect of a cause or is it merely a 'theoretical entity' ? Are the volume and moral density of society the sort of things which could causally affect the division of labour ? Could an apparently mental entity such as an "ethic' actually produce an effect; could it produce a phenomenon such as capitalism which is at least partly physical, involving physical people, physical factories and physical machines ? The abovementioned sociological empirical hypotheses assume that these philosophical questions have been answered in the affirmative. Hence the truth of these philosophical answers must be relevant to the truth of the empirical hypotheses. It cannot be true that the Protestant Ethic partially caused the emergence of capitalism if no event or state can actually cause the production of another event or state or if mental phenomena can have no effect upon physical phenomena. Furthermore, we might say that the division of labour is explained by an increase in the volume and moral density of society and this is surely rightly to be considered as a normal type of scientific hypothesis. Yet, this empirical hypothesis depends for its truth on the truth of the philosophical assumption that to suggest the cause of an event is to have explained the event and this assumption can hardly be considered as an empirical hypothesis.

Ayer is disturbed by the existence of conflicting schools of thought in philosophy and concludes that where these conflicts cannot be definitively settled either by logical deduction or by an appeal to sensory experience, then the apparent conflict is quite senseless. The philosopher is often engaged in problems which can be definitively solved by a logical deduction or by an elucidation of linguistic usages. Yet, philosophy is also concerned with other types of questions which, although they can be answered rationally, and such answers can be true or false, cannot be settled as definitively as logical questions. We have considered some such questions in the construction of a Paradigm for social science. It is endemic to the nature of prescriptive and interpretative questions that they have logically possible conflicting answers; no question could possibly be a genuine prescriptive or interpretative question if it could be dissolved by a purely linguistic or logical analysis since such a dissolution would show that the putative question was either unintelligible, tautologous or self-contradictory. Hence, when we realise that paradigms are not answers to questions which are purely logical or linguistic, we need not be disturbed by the existence of conflicting schools of thought in philosophy.

In the preceding chapters, I have attempted to evaluate various conflicting interpretative, prescriptive and evaluative paradigms and it is a peculiarity of such paradigms that they appear significant only when they are seen to be in conflict; when they are seen to be logically possible points of view rather than necessary truths or necessary falsehoods. A prescription such as 'Social facts should be explained by other social facts' must be amplified by adding 'rather than by factors relating to individual psychology or physiology' before the point of the prescription can be fully appreciated. However, I have not tried merely to review particular conflicting individual paradigms: I have tried to produce an overall Paradigm on the assumption that conflicts between individual paradigms can be resolved only within a wider systematic framework of assumptions and that the justification for

accepting or rejecting any particular paradigm is its coherence or lack of coherence with an accepted body of such assumptions. This stress upon the relevance of theories within a context of other theories is reminiscent of the writings of Kuhn and perhaps justifies my borrowing the term 'paradigm' from him. Yet, unlike Kuhn, I have argued that individual 'paradigms' can be either true or false and can be known to be either true or false. This conclusion was supported by appealing to other elements of the overall Paradigm particularly the distinction between perception and conception and a coincidental theory of truth. Furthermore, this conclusion could be seen to accord with the essence of Kuhn's argument since, if theories gain relevance and support only within a particular intellectual framework, then the theory that individual paradigms are neither true nor false will itself require the support of other assumptions which might, as I have tried to suggest, be false.

In Part 1, the presuppositions of knowledge in general were considered while in Part 11, we focused upon further presuppositions peculiar to knowledge in social science. In both cases, the Paradigm was formulated in opposition to what might be called moderate orthodox empiricism with which the basic assumptions of the Paradigm will now be contrasted. We should conceive of the world in terms of causes which necessitate the occurrence of their effects rather than as a province subject to universal or probabilistic constant conjunctions or laws. We should seek knowledge by trying to discover the effects which particular causes produce rather than by trying to discover universal laws. We should consider an event to have been correctly explained when its cause has been discovered and correctly partially explained when its partial cause has been discovered rather than when a statement of its occurrence has been deduced from a universal law together with certain initial conditions. The basic evaluative assumption is that we can verify causal theories by giving reasons for their acceptance i.e. the verification is neither simply inductive nor deductive. This conflicts with the three standard empiricist evaluative assumptions. The first is that universal laws can be

established neither by induction nor deduction and hence we can never justifiably claim that we know them to have been established. The second assumption is an appendix to this to the effect that, nevertheless, we can conclusively show, by the method of falsification, that particular theories do not state universal laws. Lastly, some empiricist assert that universal laws can be shown to be at least probably true by simple induction i.e. the repetition of particular perceptual experiences.

Although this Paradigm is suggested as appropriate to all knowledge and explanation, its conscious adoption might be particularly fruitful in social science simply because of the apparent relative lack of invariant laws governing human conduct and social phenomena. As a matter of fact, social scientists have had very little success in discovering universal laws. Perhaps this is at least partly due to the fact that there is a great range of factors which are able causally to affect human conduct and social phenomena. For example, the sorts of physical factors which can affect a billiard ball can generally also affect a man yet, since a man has feelings, thoughts and awareness, a host of other factors can affect a man which could not influence a billiard ball. In such circumstances, the social scientist might, at best, only be able to produce very partial correct explanations of his data and if this is the case, might his theories not be better cast in terms of partial causal claims rather than in terms of procrustean universal laws ?

Moderate orthodox empiricism presents a particular conception of science and of the relationship between individual sciences. I would like to conclude by suggesting that there is an anomoly in this model of science and by contrasting it with an alternative model which emerges from the view that knowledge and explanation are to be obtained through identifying causes and effects rather than invariant laws. If science should be the quest for laws, then it might seem to follow that 'Any facts are fitted, in themselves, to be a subject of science which follow one another according to constant laws; although these

laws may not have been discovered, nor even be discoverable by our existing resources' (Mill, Book VI, Chapter III) Thus a science of economics is possible if there are uniformities among economic phenomena; psychology can be a science if psychological data are similarly law-governed; and a science of sociology is possible if social phenomena are related by laws. Mill does not merely say that data must be law governed to be possible subjects of science: he claims that to be possible data for science, particular types of phenomena must follow one another according to constant laws.

This qualification has had a considerable impact in social science. Thus, in this conception of science, for, say, sociology to be an autonomous science, it is not sufficient that we can discover uniformities between certain social phenomena and, say, the movement of the stars or psychological, physiological or biological phenomena. To be an autonomous science, sociology must be able to discover laws relating one type of social phenomenon with another type of social phenomenon. Similarly, in this view, if other disciplines such as economics and psychology are to be fully fledged sciences, they must be able to discover uniformities between economic phenomena and psychological phenomena respectively. Understandably, social scientists, be they sociologists, psychologists or whatever, are usually keen to stress the status of their own disciplines as autonomous scientific ventures. Hence much has been written about the possibility of reducing the laws of one discipline to the laws of another. For example, there has been much discussion as to whether the laws of economics and sociology could be reduced to the laws of psychology on the assumption that such a possible reduction would reveal psychology to be the social science; and, for this reason, the classical raison d'etre of sociology from the time of Comte to the present day has been that sociological laws are not reducible to psychological laws. Thus, Mandelbaum is keen to demonstrate that 'societal facts' cannot be reduced to facts about individual people since, presumably, this would be to show that sociological laws cannot always be reduced to psychological laws.

Although Durkheim expresses an interest in causation rather than simply in the discovery of sociological laws, he too appears to have been influenced by this conception of scientific autonomy. He considers that a science ' ... can justify its existence only when it has for its subject matter an order of facts which the other sciences do not study. It is impossible that the same concepts can fit equally things of different natures.' (1964, P.146) Hence, Durkheim tries to show that 'social facts' can be correctly explained only by other 'social facts' since he thinks that he has thereby demonstrated that 'Sociology is, then, not an auxilliary of any other science; it is itself a distinct and autonomous science ...'. (1964, P.145) Furthermore, he seems to imply that it is necessary that 'social facts' be correctly explained by other 'social facts' in order for sociology to be a bona fide scientific venture.

Even if we should seek knowledge and explanation through discovering universal laws, there would appear to be an anomaly in this conception of science. Why was Mill not content to say that any body of facts is a fit subject of science if these facts appear in conformity with universal laws: why did he further stipulate that they must follow one another in accordance with laws ? It might be the case that a particular type of social phenomenon is not constantly conjoined with any other type of social phenomenon but is constantly conjoined with a type of, say, psychological or physiological phenomenon. If knowledge and explanation should proceed through the identification of universal laws, then this type of social phenomenon would be just as fit a subject of science as a type of social phenomenon which happens to be constantly conjoined with another type of social phenomenon. Similarly, economic or psychological phenomena could be possible subjects of science provided that they were constantly conjoined with some other type of phenomenon no matter what that type of phenomenon happen to be. And we need not assume that all economic phenomena will be conjoined with the same type of phenomenon; some may be conjoined with other economic phenomena, some with psychological phenomena, some with social phenomena and so on and so on.

We might differentiate disciplines in terms of the type of phenomena which they try to explain. Thus economics could try to explain economic phenomena, psychology psychological phenomena and sociology could explain social phenomena. We need not then assume that individual disciplines would be in Durkheim's sense scientifically autonomous in that they studied a range of phenomena which no other discipline studied since, for example, sociologists might need to cite psychological or even biological phenomena which might be found to be conjoined with some types of social phenomena. But why should we consider this type of autonomy to be of any epistemological importance ? What is important is that knowledge is gained and phenomena are explained, rather than whether or not this knowledge and explanation is produced by scientifically autonomous disciplines.

If we consider that we should seek knowledge and explanation in terms of causation and partial causation, then we need not imagine that phenomena must be governed by invariant laws in order for them to be subjects of a successful science. To be successful, the scientist requires to study phenomena of which knowledge and correct explanation are possible. For example, social science might be successful if human conduct and social phenomena are correctly explicable, no matter what types of factors serve correctly to explain them. We might want to consider individual social scientific disciplines as sciences. We might say that economics could be a successful science if economic phenomena are correctly explicable. There are perhaps so many social phenomena which are potentially explicable and so many different types of factors which could serve correctly to explain them that, for practical reasons, different social scientific disciplines are required to produce some sort of division of scientific labour. How should this partition of employment be arranged ? Does it matter what range of phenomena is allocated to each individual science ?

From an <u>epistemological</u>, as opposed, perhaps, to a <u>sociological</u> point of view the relationship between individual social sciences does not appear to be important. For example,

some people might want to try completely to explain some particular economic phenomenon in terms of various social, geographical, biological and climatic factors while other people want to find the purely social or purely economic causes of particular economic phenomena. Whether such people are considered as belonging to a particular social category of 'scientist' or 'economist' or whatever scarcely affects the epistemological status of their theories. A scientific discipline might be constituted in such a way that, on some occasions, its members try correctly to explain particular psychological phenomena and on other occasions study economic phenomena or try to discover the effects of social integration on the suicide rate or simply whatever they feel like discovering. We might want to say that such a collection of people acting in such a manner are scarcely members of a scientific discipline; yet the identity of particular sciences is surely a matter of concern to the sociologist rather than the epistemologist. If all knowledge and explanation were produced completely ad hoc would this necessarily be of epistemological importance ? Would the explanations produced necessarily be less explanatory than those produced by professional scientists; would the knowledge necessarily be less securely based than scientific knowledge ?

The nature of the social activity of science is unimportant; what matters is the epistemological status of particular beliefs, no matter what their social pedigree happens to be. We might repeat an assertion from Chapter One and say that epistemology is properly concerned with the philosophy of knowledge rather than with the logic of science; with the presuppositions of beliefs rather than the analysis of science as a social activity and that, contrary to the view of Kuhn, the philosophy of knowledge can and should be considered independently of the sociology of science.

APPENDIX

FUNCTIONALISM IN SOCIAL SCIENCE

Functionalism was considered in Chapter Three where it was argued that functionalist supposed explanations are not genuinely explanatory. Yet, functionalist theories are so common in social science, particularly in sociology and functionalism is so often thought to be able to offer not merely genuine explanations but explanations of a different sort from causal explanations that it is worth considering functionalism in some greater detail.

Radcliffe-Brown was largely instrumental in popularising this type of supposed explanation in social science. He begins his philosophy of functionalism by modifying Durkheim's definition of the term 'function':

> Durkheim's definition is that the 'function' of a social institution is the correspondence between it and the needs (besoins in French) of the social organism .... I would like to substitute for the term 'needs' the term 'necessary conditions of existence', or, if the term 'need' is used, it is to be understood only in this sense. It may be here noted, as a point to be returned to, that any attempt to apply this concept of function in social science involves the assumption that there are necessary conditions of existence for human societies just as there are for animal organisms, and that they can be discovered by the proper kind of scientific enquiry. (1952, P.178)

The analogy between biological and social 'organisms' is implicit in attempted functionalist explanations in social science and is quite explicit in Radcliffe-Brown's formulation. Attempted functionalist explanations are quite common in

biology and even in everyday speech as when we try to explain the existence of our hearts by showing that they have the function of circulating our blood which is conducive to our survival. It is sometimes argued that functionalist theories are genuinely explanatory in biology but that, for particular reasons, they are not applicable in social science. For example, Lessnoff argues that functionalist theories are explanatory in biology because the struggle for survival between competing organisms is so great that we can expect very few of them to survive:

> Thus, it can be expected that few dysfunctional traits will survive in biological organisms, and that the vast majority of existing traits are functional .... To point out the 'survival value' of a biological trait is therefore to indicate something without which it probably would not have survived, and so, in a very general way, to explain its survival. (Lessnoff, 1974, P.120)

He considers that this sort of argument does not apply in social science since there is no equivalent struggle for survival between competing societies to ensure that there is a tendency for only social practices which are 'functional' to survive. I want to argue that the functionalist <u>form</u> of attempted explanation is defective and that functionalist theories of any class of phenomena are never genuinely explanatory unless, of course, they are reinterpreted as causal theories. Since functions are particular types of effects, functionalist theories can always be interpreted as explanatory causal theories. For example, I could partially explain my survival by saying that my heart has the effect or function of circulating blood round my body. This 'functionalist' theory would be no more explanatory than any other causal theory. Nevertheless, those who present functionalist theories generally imagine that that which is said to have function is explained by its function and I shall consider functionalist theories in this light.

As has been noted, for example, by Lessnoff, there is an ambiguity in Radcliffe-Brown's formulation of functionalism. The 'necessary conditions of existence of the social organism'

may mean the conditions necessary for the maintenance of the social organism in a particular social state or, alternatively, the conditions necessary for the maintenance of the social organism in any social state at all. As Lessnoff expresses the point:

> ... what is necessary to preserve the system in its existing form has to be distinguished from what is necessary for the system to survive at all, in any form whatever - that is, from the general conditions of societal viability. (1974, P.112)

Corresponding to these two senses of the term 'function', Lessnoff isolates two types of functionalism which he calls respectively institutional relation functionalism, which is concerned with the contributions of social institutions to the maintenance of a particular sort of social system and societal survival functionalism which is concerned with the conditions of survival of social systems as such. That both types of functionalism are distinct can be clearly seen. We might argue that, in Ulster society, the institutions of the I.R.A., and the U.D.A. have an institutional relation function in that they tend to perpetuate a particular social order in Ulster but that they are more likely to be dysfunctional, in the other sense of the term, in that they thwart rather than promote the existence of human and social life as such. Lessnoff points out that both types of functionalism have frequently been confused in social science. Since both types of functionalist theories require different evidence for their verification, such confusion is unfortunate.

There are perhaps several ambiguities surviving in Lessnoff's exposition of functionalism. For example, there is surely a distinction which should be made between the conditions necessary for the survival of a particular system in any state whatsoever and the conditions requisite for the survival of social systems in general, yet this distinction is blurred in Lessnoff's account. An organised system for distributing food, clothing and supplies of power is arguably necessary for the survival of our advanced industrial society in any form whatsoever: such a system of distribution has a specific

societal survival function in the sense that it is a necessary condition (or so I am suggesting) for the survival, in any form, of specific forms of societies. Yet it does not have a general societal survival function: it is not a prerequisite of social life per se; some societies are able to survive without such a centrally organised distribution system, notably primitive societies. Once again, since different evidence is required to verify or confute both types of functionalist theories it is, important not to confuse them.

We might try to clarify the distinctions between the different types of functionalist theories in terms of biological analogies. Consider institutional relation functionalism. Its biological counterpart might be termed 'organic relation functionalism'. Such a functionalist theory might be that a left kidney maintains me, and organisms similar to me in a particular physiological state. Nevertheless, human life is possible both for me and for other people, even in the absence of a left kidney. A specific survival functionalist theory in biology might be that fish need the ability to acquire oxygen from water in order to survive in any form, even although other creatures can live happily without such an ability. A general survival theory might be that no creature can live for long without a digestive system.

We have now isolated three types of functionalism: institutional relation functionalism; specific societal survival functionalism; and general societal survival functionalism. By taking account of a further ambiguity in Lessnoff's exposition of functionalism, we can identify three more types. He writes:

> One can, then, attempt to list the necessary conditions of societal viability, and to identify the contribution that various social activities make to fulfilling them (this is sometimes called 'requisite analysis'). The effect of a social activity in contributing to societal viability can then be called the 'social function' of the activity, analogously to biological functions. (1974, P.111)

We have here two sense of function. There is a difference between an activity's having the effect of aiding societal

survival and being part of the necessary conditions of societal survival since to say that a social phenomena has a particular effect is not to say that it is the only thing which could produce that effect. To be a sufficient condition for the production of an effect is not necessarily to be a necessary condition of that effect. Hence, we can differentiate functionalism which considers the effect of social phenomena in promoting the existence of a particular social order or the continuation in any form of particular societies or the maintenance of social life in general from functionalism which seeks the necessary conditions for such things. Yet again, both these different types of functionalist theories - effect functionalist theories and necessary conditions functionalist theories - will require different evidence to confirm or confute them and hence they should not be confused. Unfortunately, they often seem to be confused. For example, in his discussion of 'joking relationships' occurring in some primitive societies, Radcliffe-Brown argues that they have the function of forestalling otherwise likely social conflicts and hence that they contribute to the maintenance of these societies. (See Radcliffe-Brown, 1952, Pp.90-104. It is not clear whether he intends this to be an institutional relation or a specific societal survival functionalist theory.) Yet, despite his proposed use of the term 'function', Radcliffe-Brown presents no evidence to suggest that joking relationships are necessary for the maintenance of these societies. He suggests only certain effects of joking relationships; he does not even try to show that nothing else could have produced a similar effect. Furthermore, it is far from clear how such an enquiry could have proceeded.

Radcliffe-Brown is quite clear that functionalism rests on the assumptions that there are necessary conditions for the survival of societies and that these conditions can be known to be necessary conditions. Even allowing that the former assumption is true, how can we substantiate the latter ? We might discover on some occasions that an effect such as social order is produced by, say, shared values, but how could we possibly discover that nothing else could have produced this

effect ? How could we know that a presently unobserved instances of social order will not be produced by a cause other than shared values ? Unless they are so vague as to be almost tautologous or trivial, how can necessary conditions functionalist theories, as opposed to effect functionalist theories be known to be true ? For example, we might say that food is necessary for human survival; but is 'food' really necessary ? Spacemen seem to survive, for some time anyway, on what we would not normally call 'food'. What we seem to need is vitamins, protein and so on. Is eating food the only way we can acquire such things ? People in hospitals are sometimes 'fed' intraveinously. Can we not get vitamins from sunlight ? To save the theory we would have to say something vague like, 'People cannot survive for long without an "intake" of some kind'. And what sort of an intake do we require; how can we say that vitamins, for example are actually known to be the only things capable of producing their effects unless we trivialise the claim by saying that anything capable of producing particular effects is by definition a 'vitamin' ?

None of these types of functionalist theories is genuinely explanatory; to show that a social phenomenon has a function in any of the six senses of the term which have been considered is not to have explained its existence or persistence nor even partially to have explained its existence or persistence. To illustrate this claim, let us interpret Radcliffe-Brown's functionalist theory of religion in each of the six ways. His theory is that religious rites have the function of regulating, maintaining and transmitting the sentiments upon which the existence of society depends. Let us consider first of all effect functionalism. We could say of religion that it has the effect of maintaining a particular society in a particular state or that it has the effect of maintaining a particular society or type of society in any state whatsoever or that it has the effect of maintaining all social life as such. Would an institution of religion be explained by adducing any of these effects ? I do not think so, since it is perfectly rational to say, 'I believe that religion

produces one or all of these effects, but why does it exist or persist ?'. The same conclusion applies to necessary conditions functionalism. It is perfectly rational to say, 'I believe that religion is a necessary condition for the survival of this society in its existing state and/or it is a necessary condition for the survival of this society in any state at all and/or it is a necessary condition for the survival of societies in general, but why does religion exist or persist ?'. To see that this is so, consider a necessary conditions theory of the third type. As Lessnoff points out, it might be the case that, say, religion is a necessary condition for the existence of social life as such even although religion, and consequently social life as such did not exist. Hence, the suggested necessity of religion for the existence of social life cannot explain the existence of religion since the necessity is quite compatible with its non-existence. (See Lessnoff, 1974, P.124) A similar argument can be used to show that the other types of necessary conditions functionalist theories are also not genuinely explanatory.

Lessnoff considers that it is possible to produce institutional relation functionalist theories which are genuinely explanatory, but I think that this view is perhaps confusing. He distinguishes two forms of such theories:

> ... unidirectional, where one institution is "functional for" another or others, i.e. favours the latter's development or persistence; and reciprocal, where each of the two institutions is "functional for" (favours) the other. (1974, P.125)

He argues that the unidirectional form is unexplanatory and that we cannot explain the existence of one institution by suggesting that it has a beneficial effect upon another but that the reciprocal form of functionalism does have explanatory power. He cites as an example of an explanatory reciprocal theory Parsons's suggested functional relationship between the nuclear family and the industrial economy. According to Parsons, the nuclear family promotes the industrial economy which requires geographical and social mobility while the industrial economy promotes the nuclear family by demanding

such mobility and by tending to break up the extended family:

> In other words, where a reciprocal functional relation is demonstrated between two coexisting institutions, A and B, each is at least partially explained. Furthermore, each is explained by its function; for the function of A is to promote B and B in turn promotes A - so A's function favours A's own existence. (Lessnoff, 1974, P.126)

It is true that both the nuclear family and the industrial economy are partially explained by what is suggested to be their functions, but it would be a mistake to imagine that we have discovered some form of functionalist explanation which can be distinguished from causal explanation. The entire force of this 'functionalist' explanation is derived from causation. The existence of the nuclear family is partially explained by its suggested function, the promotion of the industrial economy, only because the industrial economy is said causally to affect the nuclear family, tending to favour its existence. From the standpoint of explanatory force, it is quite irrelevant that a partial cause of the nuclear family also happens to be its function. If the industrial economy promotes the nuclear family, then we can correctly partially explain the existence of the nuclear family in terms of this causal relationship whether or not the nuclear family in turn causally affects the industrial economy. If the industrial economy were not said causally to affect the nuclear family, then we could not explain the existence of the nuclear family in terms of its function, the promotion of the industrial economy. Reciprocal institutional relation functionalism does not offer a type of explanation sui generis.

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