INDUCTION

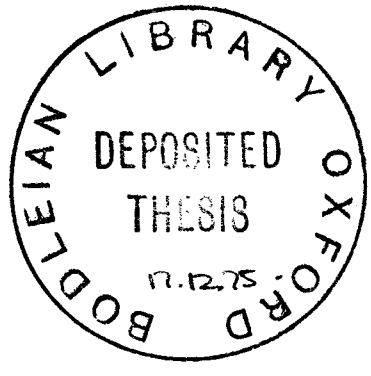
AND

CAUSAL INFERENCE

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ABSTRACT

The general intention of the thesis is to provide an alternative to traditional inductivist and deductivist accounts of reasoning 'concerning matter of fact and existence' which not only more accurately characterises the inferential procedures which we actually employ, but is also free from some of the logical problems and paradoxes which beset these traditional accounts. To this end an attempt is made to depict causal inference as a procedure quite distinct from both induction and deduction. Throughout the thesis, special emphasis is laid upon scientific method.

The thesis is divided into three parts, which treat of their subject-matter as follows.

The first part of the thesis consists of a discussion of Hume's account of 'probable' reasoning (chiefly as he expounds it in Section IV of the 'Enquiry Concerning Human Understanding') and a critique of his analysis of the causal tie. Here it is argued that the key to Hume's scepticism with regard to the powers of human reason lies in his reduction of causal inference to inductive extrapolation, and that if the 'problem of induction' is to be resolved it is this reduction which must be questioned. Hume's reductivist position is shown to be founded upon his assumption that causal propositions cannot be justified by means of the procedures which we normally employ to justify our beliefs in particular matters of fact, and this assumption is explained in terms of his equivocal attitude towards the nature of causal relations, whereby he sees these sometimes as matter-of-fact connections and sometimes as indiscoverable 'relations of ideas'. It is suggested that the latter view is dominant in Hume's thought (contrary to the opinion of most commentators) and that its origin is to be found in Locke's notion of 'real essence'. The contribution of the doctrine of 'ideas' to Hume's account of causation - and especially to his scepticism with regard to the perceptibility of causal relations - is also considered.

In the second part of the thesis an attempt is made to establish the ontological and epistemological foundations of a theory of causal

inference which depicts this mode of reasoning as a procedure quite distinct from both induction and deduction. This begins with a discussion of various issues in the philosophy of perception, the purpose of which is to show that scepticism with regard to the perceptibility of causal relations has the same philosophical roots as scepticism with regard to the perceptibility of material objects, the suggestion being that since the latter sort of scepticism is by now almost universally rejected, it is anomalous that the majority of contemporary philosophers should still incline towards the former. The next topic dealt with is that of dispositions or 'powers', and here it is argued that a 'realist' - rather than a 'reductivist' - account of such properties is to be preferred. For this purpose it is suggested that a fundamental and irreducible distinction between the 'actual' and the 'potential' should be recognised - a suggestion which consciously draws upon certain ideas of Aristotle. At the same time a related distinction is made between 'accidental' and 'essential' properties, and an explanation is proposed for the fact that the 'powers' of objects are generally regarded as 'essential' - that is, as being largely definitive of the 'natural kinds' to which those objects belong. In the ensuing discussion an attempt is made to interrelate the concept of 'power' with the concepts of 'natural' necessity and possibility, and with the help of these modal concepts various different kinds of powers are distinguished. In the light of all these considerations an analysis is proposed of our use of the terms 'cause' and 'effect' and this is utilised in a rudimentary account of the formal structure of causal inference. Here the discussion returns to some of the issues raised in the earlier examination of Hume's doctrines. Finally, the second part of the thesis concludes with an investigation of the concept of a natural law, and here it is suggested that this concept is intimately connected with the concept of a natural kind. This, in the light of earlier findings, is seen to relate the concept of a natural law with causal concepts. At this point causal inference is finally shown to be not only quite distinct from induction, but altogether independent of inductive procedures.

The third and last part of the thesis consists of a critique of

various inductivist and deductivist accounts of reasoning 'concerning matter of fact and existence', the general objective being to demonstrate that the applicability of both inductive and deductive reasoning to matters of fact presupposes the possibility of causal inference. First of all 'ampliative' induction is considered, and an attempt is made to explain why the 'justification' of induction is impossible within the framework of inductivist presuppositions. Next 'eliminative' induction is briefly discussed with a view to establishing the general conditions of its application. An examination is then made of the probabilistic theory of confirmation, and here the views of contemporary adherents of this approach are discussed in the light of the work of earlier theorists, particularly J. M. Keynes. It is argued that certain of Keynes' original arguments connecting confirmation with probability were founded upon questionable assumptions, and it is suggested that there is a serious error in supposing that the probability calculus may be 'interpreted' as a 'logic of confirmation'. Finally, the thesis concludes with some brief remarks concerning K. R. Popper's doctrine of 'falsificationism' considered as an 'answer to Hume'.

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INTRODUCTION

The central concern of this thesis is a certain kind of argument or reasoning: the kind of argument or reasoning which is founded upon non-demonstrative inference or, in other words, non-deductive reasoning. Up to date, most accounts of the nature of non-demonstrative inference have represented it as a species of inductive inference. Of course, not all the species of arguments commonly referred to as 'inductive' are non-demonstrative in character. Thus summative induction, or induction by complete enumeration, recursive or mathematical induction, and induction by elimination are all forms of deductive argument. The sort of induction which supposedly involves non-demonstrative inference is usually referred to as 'ampliative' induction.[1] When I refer to 'inductive arguments' subsequently in this thesis, I shall always mean arguments of ampliative induction, unless I specifically state otherwise.

Now, I shall be arguing not only that most of our non-demonstrative inferences are non-inductive in character, but also that we rely upon such (non-inductive) inferences to justify all our inductive arguments. It may, then, seem rather paradoxical if I say that the theory of non-demonstrative inference which I shall advance takes its starting point from certain ideas of Hume, who is commonly, and no doubt rightly, regarded as the founder of modern inductivism. Such, nonetheless, is the case.

My approach to rational inference is based upon the following simple analysis. In any process of argument or reasoning, one may distinguish a conclusion and a set of premises, although these may not always be explicitly stated. The conclusion is said to follow from the premises, and is said to do so in virtue of some known or supposed relation between the premises and the conclusion. As F. P. Ramsey put it: "An inference is not completely given when we are given the premises and conclusion; we require also to be given the relation between them in virtue of which the inference is made."[2] The primary task of any theory of rational inference, as I see it, is to elucidate the nature of such relations, which we may call 'inference-relations'.

An example of an argument involving demonstrative inference

1. See, e.g., Kneale (1), p. 44 and Von Wright (1), p. 9.
2. Ramsey (1), p. 195.

would be the following syllogism: 'Socrates is a man; all men are mortal; therefore Socrates is mortal.' What characterises the inference as demonstrative is the fact that the relation between the premises and the conclusion, in virtue of which the latter is said to follow from the former, is the logical relation of entailment. Entailment is often defined as follows: given two propositions p and q, to say that p entails q is to say that the proposition 'It is not the case that both p and not q' is analytic, or logically necessary.[1] (This definition of entailment as 'strict implication' has been questioned, but where it has been questioned it has usually been on the grounds that entailment is a stronger relation than mere strict implication.[2]) Thus it is commonly said that the premises of a deductive argument logically necessitate the conclusion or, in other words, that given the truth of the premises, no alternative is logically possible but that the conclusion is also true. Of course, I do not want to suggest that the notions of logical necessity and possibility are perfectly straightforward and uncontroversial: but this thesis is not concerned with the problems of demonstrative inference.

Now, arguments founded upon non-demonstrative inference are negatively characterised by the fact that in them the conclusion is not entailed by the premises. An example of such an argument would be the following one, cited once by P. F. Strawson: 'The kettle's been on the fire for the last ten minutes, so it should be boiling by now.'[3] That this inference is non-demonstrative is indicated by the fact that it would not constitute a logical contradiction at once to assert the premise and deny the conclusion. As Strawson puts it: "It would not be self-contradictory to say 'The kettle has been on the fire for ten minutes, but the water is stone cold.' One might be inclined to say that this would be impossible: but the impossibility is not logical."

Now, this last sentence of Strawson's is illuminating, even though he himself does not follow up its implications. It is not enough, of course, simply to define non-demonstrative inference negatively as a mode of inference in which the relation between the premises and the conclusion, in virtue of which the latter is said to follow from the former, is not the logical relation of entailment: one must give

1. See, e.g., Strawson (1), p. 23.
2. See, e.g., Von Wright (2) pp. 166 ff.
3. Strawson (1), p. 235.

a positive characterisation of the kind of relation which is involved in such an inference. Now, what Strawson's last sentence seems to imply - and rightly, I think - is that if we wish to develop a theory of non-demonstrative inference, we must attempt to articulate concepts of possibility and necessity which are quite distinct from logical possibility and necessity. For I would suggest that what is central to the concept of rational inference is the notion that if the premises of an argument are true, no alternative is possible but that the conclusion is also true.[1] This view is confirmed by the fact that we all naturally consider it to be a fatal weakness in someone's argument if he has failed to perceive that other alternatives besides his conclusion are compatible with his premises. But this means that what distinguishes demonstrative inference from non-demonstrative inference is the kind of 'possibility' involved.

Of course, there are many philosophers who believe that demonstrative inference is the only genuine form of reasoning: for example, K. R. Popper.[2] If the preceding analysis is correct, the central assumption of this 'deductivist' school must be that there is no kind of necessity but logical necessity: that there are no constraints upon what may or may not happen in the physical world save those implied by the logical laws of excluded middle and non-contradiction. Expressed thus, deductivism may appear to be an absurd doctrine: as, indeed, I believe it is. Its absurdity has been obscured by philosophical dogma. Of course, the inductivist school, which Popper apparently opposes, is in exactly the same position; and this, as we shall see, is why inductive arguments can never be justified given the presuppositions of inductivism.

The sceptic will, no doubt, urge that there can be no kind of necessity but logical necessity. But I shall argue that it is logically necessary that we should operate with a concept of 'natural' or 'physical' necessity if we are to be able to conceive of any kind of objective world at all, and this, I hope, will cut the ground from under the sceptic's feet. I shall be arguing, in effect, that we cannot possibly do without a theory of non-demonstrative inference.

The structure of this thesis is briefly as follows. The work falls naturally into three parts. In the first part I discuss and

1. This is to say, in effect, that 'inference-relations' are relations of 'necessitation'.
2. See, e.g., Popper (2), chap. 1.

criticise Hume's account of reasoning and his views on causation: for Hume's ideas and presuppositions are largely shared by modern philosophers of both the inductivist and the deductivist schools. Of course, criticism of Hume is well-covered ground: but I hope to have made some new points and clarifications in a subject which is still very confused despite numerous works of exegesis. Furthermore, as I mentioned earlier, there are certain of Hume's ideas upon which I build positively. In the second part I attempt to lay the foundations of a theory of non-demonstrative inference which represents this mode of argument as being quite independent of inductive procedures. The success of this task hinges largely upon the successful articulation of concepts of natural necessity and possibility, and this inevitably involves many matters pertaining to ontology and the philosophy of perception. Finally, in the third part, I discuss inductive and deductive modes of argument, with a view to showing why these cannot be exhaustive of scientific method or of our everyday reasoning about matters of fact.

I : HUME'S ACCOUNT OF PROBABLE REASONING

Hume's main claim to fame as a philosopher is his denial of the rationality of inductive procedures on the grounds that the appeal to 'past experience' as a guide to the future cannot be given a non-circular justification. However, it is frequently forgotten that an essential element in this famous argument is Hume's reduction of causal inference to an inductive procedure. If there is a solution to the 'problem of induction', it lies, I believe, in questioning the validity of this reduction.

In presenting Hume's views, I shall follow the account he gives in the 'Enquiry Concerning the Human Understanding' (principally Section IV), as this seems rather more lucid and coherent than the corresponding account in the 'Treatise of Human Nature'. In presenting these views I shall repeat some points which have only too frequently been made before by numerous commentators: but this is necessary in order to give a balanced account of the structure of Hume's argument.

As a preliminary to the discussion of Hume's argument, a few remarks are called for concerning his terminology. In particular, we ought to mention two important distinctions to which Hume continually refers. The first, which he formulates at the beginning of Section IV, is the distinction between 'relations of ideas' and 'matters of fact'. According to Hume, the former include "every affirmation which is either intuitively or demonstratively certain", while the latter are characterised by the fact that "the contrary of every matter of fact is still possible; because it can never imply a contradiction." Hume proposes that the criterion whereby a proposition may be judged to be non-contradictory is that such a proposition, even though it should be false, may be "conceived by the mind with the same facility and distinctness as if ever so conformable to reality." By thus characterising the distinction between relations of ideas and matters of fact in terms of 'intuitive certainty' and 'facility and distinctness' of conception, Hume was appealing to criteria which many modern philosophers would condemn as 'psychologistic'.[1] However, despite this we may, I think, legitimately translate Hume's first distinction in

1. See, e.g., Popper (2), chap 1.

terms of the modern distinction between analytic and synthetic propositions. Such a translation does not do too much violence to Hume's original ideas and is, in any case, presumed by those modern philosophers who claim to share Hume's views concerning induction.

Of course, the modern distinction between analytic and synthetic propositions has itself been criticised by a number of contemporary logicians, notably W. V. Quine.[1] But I do not propose to enter this debate here, since any evaluation of Hume's arguments which does not dismiss them outright as invalid presupposes an acceptance of some such distinction as that between analytic and synthetic propositions. It is not my object to refute Hume by questioning the 'dogmas of empiricism': rather, I want to argue that Hume's scepticism was unwarranted even in the context of these 'dogmas'.

The second important distinction which Hume makes is that between 'demonstrative reasoning' (or 'reasoning apriori') and 'moral reasoning' (or 'probable reasoning'). According to Hume, "All reasonings may be divided into two kinds, namely, demonstrative reasoning, or that concerning relations of ideas, and moral reasoning, or that concerning matter of fact and existence."[2] It is vital not to confuse this distinction with the modern distinction between demonstrative and non-demonstrative <u>inference</u>. Hume's distinction simply relates to the logical status of the premises and conclusions of arguments: in demonstrative reasonings the premises and conclusions are analytic propositions, while in moral or probable reasonings they are synthetic propositions.[3] Thus this distinction leaves entirely open the question as to whether or not there may be valid processes of non-demonstrative inference either from analytic or from synthetic premises.

I have just said that Hume's distinction between demonstrative and probable reasoning should not be confused with the modern distinction between demonstrative and non-demonstrative inference. One of the main problems in understanding Section IV of the 'Enquiry', however, is to discover to what extent Hume acknowledged this modern distinction at all. It is certainly clear from many passages that Hume insisted that in any process of reasoning or argument there must be some relation (or 'medium' or 'connexion', as he usually calls it)

1. See Quine (1).
2. Hume (2), p. 35.
3. Cf. Stove (1), p. 35 and p. 37.

between the premises and the conclusion, in virtue of which the latter may be inferred from the former. Thus, at one point he says: "There is required a medium, which may enable the mind to draw such an inference, if indeed it be drawn by reasoning and argument."[1] Hume also evidently believed that in the case of 'demonstrative' reasonings, such as mathematical demonstrations or proofs, this relation is always one of 'intuitive certainty' - or, as we should say, that it is the logical relation of entailment. (Hume would not have sympathised with the modern notion of non-deductive reasoning in mathematics.[2]) Now, at the beginning of Section IV, Hume advances a theory of 'moral' or 'probable' reasoning (reasoning concerning matter of fact and existence) which seems to imply that this kind of reasoning involves non-demonstrative or non-deductive inference. But his scepticism consists in his ultimately denying that 'probable' arguments are really genuine processes of reasoning at all. (Of course, Hume would have allowed that valid syllogisms may 'concern matter of fact and existence', in that their premises and conclusions are synthetic propositions: but, for reasons which will become clear shortly, he would have regarded all such syllogisms as trivial and unimportant.[3]) Thus we may say that his final position is to reject the distinction between demonstrative and non-demonstrative inference, on the grounds that we only really employ reasoning in subjects like mathematics.

We may now proceed to examine in detail Hume's theory of 'probable' reasoning. Having formulated the distinction between relations of ideas and matters of fact, Hume goes on to say: "It may, therefore, be a subject worthy of curiosity to enquire what is the nature of that evidence which assures us of any real existence and matter of fact, beyond the present testimony of our senses, or the records of our memory." Here we see another distinction, and one central to the empiricist tradition, between observation and memory statements on the one hand, and predictive and retrodictive statements on the other. It is further suggested that in order to justify statements of the latter kind, we must find some way of inferring them from statements of the former kind. (That the issue of justification is involved here is indicated by Hume's use of the word 'assure'.) Such an argument

1. Hume (2), p. 34.
2. See, e.g., Lakatos (1).
3. This, of course, is where Popper differs from Hume.

I shall refer to as a 'transexperiential' argument. Its premises I shall refer to as 'experiential' propositions, and its conclusion I shall refer to as a 'transexperiential' proposition. Now, a transexperiential argument, since it concerns matter of fact and existence, is evidently a species of 'probable' argument, in Hume's terms. However, Hume speaks as though it were the only species of probable argument, and this we must endeavour to explain.

Two things may account for Hume's opinion. The paramount importance which Hume ascribed to transexperiential arguments is partly explained in terms of the assumption, which was held generally by philosophers of the empiricist school, that all our knowledge of matters of fact must somehow or other 'arise from experience': nihil est in intellectu quod non fuerit in sensu. This rather vague principle by no means implies, of course, that such knowledge should always arise from experience by processes of reasoning, as Hume well understood: but it does imply that any factual knowledge which we acquire through processes of reasoning must ultimately be based upon experiential premises. This is why Hume would have dismissed syllogistic reasoning about matters of fact as trivial: for the universal premise of any syllogism would be regarded by him as a transexperiential proposition, which could only be justified by transexperiential argument. It need not concern us here, incidentally, that this empiricist principle is quite plainly false. (For example, it neglects the non-observational and non-inferential knowledge which we derive through the performance of intentional actions.[1])

The second thing which accounts for Hume's assumption that transexperiential arguments are the only species of probable arguments, or arguments concerning matter of fact, is the notion that there could be no use for any such argument which did not 'take us beyond our experience', because it could not tell us anything which we did not already know. This notion is founded upon the assumption that all our experiential data are perfectly 'clear and distinct', requiring no sort of interpretation. As a corollary of this, it has usually been assumed within the empiricist tradition that experiential propositions, unlike transexperiential propositions, in general stand in need of no

1. See, e.g., Anscombe (1), pp. 49 ff.

rational justification, or justification by means of reasoning or argument: experience itself is what justifies an experiential proposition. This assumption is, of course, quite as dubious as the principle that all our factual knowledge 'arises from experience', and has increasingly been criticised by those philosophers who regard observation as an irreducibly 'theory-laden' activity.[1] But, again, this need not concern us here as it is not my object to refute Hume by questioning the 'dogmas of empiricism'.

We may now return to our main theme. Hume has posed the following question: What is the general nature of transexperiential arguments? And his initial answer to this question is summed up in the sentence: "All reasonings concerning matter of fact and existence seem to be founded on the relation of cause and effect." This simply suggests that all transexperiential arguments are founded upon causal inference. According to Hume, whenever we infer the occurrence of some event A, which is 'beyond' our experience (either because we have not witnessed it, or because it has not yet occurred), from the occurrence of another event B, of which we do have experience (either because we are presently observing it, or because we remember having observed it), we make this inference upon the presupposition that a causal connection or relation exists between A and B. For example: "The hearing of an articulate voice and rational discourse in the dark assures us of the presence of some person: why? because these are the effect of the human make and fabric, and closely connected with it." Hume also points out, however, that A and B need not be related as immediate cause and effect: the relation may be "either near or remote, direct or collateral."

Two things may be remarked at this point. Firstly, what Hume says here is strikingly plausible: transexperiential arguments of the sort he describes certainly do seem to be founded upon causal relations in the way he suggests. Thus, if we consider Strawson's example - 'The kettle's been on the fire for the last ten minutes, so it should be boiling by now' - it seems very reasonable to say that we draw this predictive conclusion upon the supposition that fire, given time, causes water to boil. Strawson himself, of course, assumes that such an inference is really 'inductive' in character. But this leads to

1. See, e.g., Hanson (1), p. 19.

my second point: it is remarkable, at first sight, how far removed Hume's account of transexperiential arguments appears to be from the familiar inductivist account. Nevertheless, Hume manages, in the course of a superficially lucid, but really rather confused argument, to reduce his theory of causal inference to a substantially inductivist account of transexperiential reasoning. It is this reduction which I shall be questioning.

Having given his brief account of probable arguments in terms of causal inference, Hume goes on to say: "If we would satisfy ourselves, therefore, concerning the nature of that evidence which assures us of matters of fact, we must enquire how we arrive at the knowledge of cause and effect."[1] Now, Hume is asking here how we discover the existence of causal relations, and he is asking this in the context of an enquiry into the justification of transexperiential propositions. This passage thus illustrates an important confusion which pervades the whole of Section IV of the 'Enquiry', namely, the confusion between what Reichenbach called the 'context of discovery' and the 'context of justification'.[2]

Now, as far as the justification of transexperiential propositions by means of causal inference is concerned, we may say the following. If probable arguments are founded, as Hume suggests, upon causal inference, such arguments may only be used to justify transexperiential propositions if there is some way in which we may justify causal propositions in turn. (This point does not concern the validity of such arguments, but only the possibility of our using them to justify the assertion of transexperiential propositions.) For example, it will no doubt be accepted that if we can justify the assertion that fire, given time, causes water to boil, then we may use causal inference to justify the assertion that the kettle is boiling, on the grounds that it has been on the fire for the last ten minutes. But if there were no way of justifying the claim that fire causes water to boil, we clearly never could use this argument for this purpose. Hume's scepticism simply consists in his belief that it is impossible to justify the assertion of causal propositions, and hence that it is impossible to justify transexperiential propositions by means of

1. My underlining.
2. See Reichenbach (1), pp. 5-7.

probable arguments. Our main concern is thus to discover whether Hume advances cogent reasons for his scepticism on this point.

Now, what, according to the principles of empiricism, could be the ways in which we might seek to justify causal propositions? The answer to this question depends, in the first instance, upon whether causal relations are conceived to be 'relations of ideas' or 'matters of fact'. If the former, then causal propositions will only be justifiable by means of demonstrative reasoning (or 'reasoning apriori'), in the manner of mathematical theorems; but if the latter, they will only be justifiable by immediate experience or by inference from immediate experience (that is, by transexperiential argument). Of course, it might be interposed here that it would be utterly misguided to attempt to justify causal propositions in a manner appropriate to relations of ideas, since causal propositions are evidently synthetic. This is no doubt correct: yet, as we shall see, Hume (contrary to popular belief) was by no means sure that causal relations are properly to be conceived of as 'matters of fact'. However, leaving this point aside, it is evident that what Hume must prove, in order to warrant his scepticism, is that causal propositions cannot be justified in any of these ways.

It will be recalled, however, that Hume actually asks not how we can justify causal propositions, but how we can discover causal relations. As I remarked, this illustrates his tendency to confuse the 'context of discovery' and the 'context of justification'. The point is this: even if it should be the case that we generally discover causal relations by means of processes which could not be represented as justifying our assertion of the propositions expressing the existence of these relations (i.e. causal propositions), this would not preclude our subsequently justifying these propositions by some other means. Thus the fact that children and animals acquire extensive knowledge unreflectively and without ratiocination (a fact which Hume makes much of), is really quite irrelevant to the question of whether their beliefs are rationally justifiable. Hume's genetic approach to the problems of empirical knowledge tends to confuse this point.

Nevertheless, heuristic considerations can be relevant to the

problem of the justification of causal propositions. For justificatory procedures - and in particular, processes of argument or reasoning - can be used for heuristic purposes. But if a given justificatory procedure cannot be used to discover causal relations, it cannot be used to justify causal propositions either. Thus although Hume's argument is misleadingly couched in terms of the context of discovery, it is possible to translate it into terms of the context of justification.

Concerning the origin of our knowledge of cause and effect, Hume says the following: "I shall venture to affirm, as a general proposition, which admits of no exception, that the knowledge of this relation is not, in any instance, attained by reasonings apriori; but arises entirely from experience, when we find that any objects are constantly conjoined with each other." This important statement has two parts, which I shall discuss in turn.

In the first part of his 'general proposition', Hume asserts that causal relations cannot be discovered by demonstrative reasonings, or reasonings apriori. Hume bases this assertion upon the contention that the events which we consider to be causally related in any given instance are always quite distinct: at least, as far as we can discover. "The mind can never possibly find the effect in the supposed cause, by the most accurate scrutiny and examination. For the effect is totally different from the cause, and consequently can never be discovered in it." Hume attempts to convince us of this by a number of thought-experiments, in which he challenges us to imagine, "without consulting past experience", what might be the cause or effect of some particular event or state of affairs. Thus: "A stone or piece of metal raised into the air, and left without any support, immediately falls: but to consider the matter apriori, is there anything we discover in this situation which can beget the idea of a downward, rather than an upward, or any other motion, in the stone or metal?"[1]

Such thought-experiments, of course, smack of the 'psychologism' which modern philosophers have found so unacceptable in Hume. However, translated into less psychologistic terms, what Hume seeks to show is simply that between any two events which we consider to be causally related, we can never discover any connection which would

1. Hume (2), p. 29.

render the one a logically necessary consequence of the other. For example, if a moving billiard ball hits a stationary billiard ball, there is nothing in the nature of this event which would render it a logical contradiction to predict that the second billiard ball will not subsequently move off in the same direction as the first. To borrow Strawson's expression, one might be inclined to say that such an outcome would be impossible, but the impossibility is not logical.

Of course, it might be objected to this that our descriptions of physical states of affairs often carry extensive logical implications, at least some of which involve the causal properties of objects. Thus if the 'billiard balls' were to explode or change into white rabbits upon impact, we might hesitate to call them 'billiard balls' at all, since this is not behaviour appropriate to such objects: so these, perhaps, are outcomes of the impact which can be excluded as logically impossible. However, not every feature of an object's possible behaviour can be included in its very definition. If it were, either we should continually have to be changing our definitions of things, or else we should encounter problems concerning the applicability of our definitions to the real world. This excludes any 'conventionalist' solution to Humean scepticism.[1]

With a few qualifications, then, we may agree with Hume's general position here. What he has shown is that demonstrative reasoning could not possibly be our primary way of discovering causal relations: at best it can only make explicit those causal properties which we have tacitly ascribed to objects, but which we must have discovered in some other way. And this means that demonstrative reasoning cannot ultimately be used for the justification of causal propositions.

Now, all this might be thought to imply that, in Hume's view, causal relations are not relations of ideas. Then, given his division of "all the objects of human reason or enquiry" into relations of ideas and matters of fact,[2] we might assume him to have regarded causal relations as matters of fact. Such an assumption would be very reasonable: but whether it would be correct is something which I shall discuss later.

1. Cf. Ayer (1), pp. 6 ff., on 'the assumption of atomicity'.
2. Hume (2), p. 25.

In the second part of Hume's 'general proposition', which I quoted earlier, he asserts two things. Firstly, he asserts that our knowledge of causal relations must always 'arise from experience'. (And this, incidentally, might also be taken to imply that causal relations were, in Hume's view, matters of fact, since it is precisely our factual knowledge which, according to the principles of empiricism, is supposed to 'arise from experience'.) Secondly, he asserts that the kind of experience from which such knowledge arises must be experience of constant conjunctions of particular sorts of events. I shall discuss these two assertions in turn.

It should be remarked that in saying that our knowledge of causal relations 'arises from experience', Hume cannot yet be excluding the possibility that we may, at least sometimes, acquire such knowledge as a result of reasoning from experiential premises, since this is clearly one way in which knowledge may 'arise from experience'. Such reasoning would merely be a form of transexperiential argument. However, it should be stressed that if causal relations could, in any instance, be discovered by reasoning from experiential premises, the arguments involved would necessarily be 'probable' arguments, and this would imply, again, that causal relations are matters of fact.

Hume's second assertion concerns the kind of experience from which, in his view, our knowledge of causal relations arises. Here it is important to remark that Hume believed that we can never simply observe or experience a causal relation itself (and this, of course, excludes for Hume the possibility of our justifying causal propositions by appealing to 'direct' or 'immediate' experience). Why Hume believed this is a matter I shall discuss later. But what it means is that when two events are causally related, all we can observe, according to Hume, is that they are conjoined: that is, that they occur together in space and time. As he puts it later, in Section VII of the 'Enquiry', "All events seem entirely loose and separate. One follows another: but we never can observe any tie between them. They seem conjoined, but not connected."[1]

As far as observation or experience is concerned, then, we can never, in Hume's opinion, really perceive any difference between the

1. Hume (2), p. 74.

conjunction of two causally related events A and B, and the conjunction of two events C and D which just happen, by coincidence, to occur together. Because he believed this, Hume considered that the only 'experience' from which our knowledge of causal relations could arise was that which we acquire "when we find that any objects are constantly conjoined with each other." That is, because we can never distinguish, by mere observation, a causal conjunction from a purely coincidental conjunction, all that betrays the existence of a causal relation to our senses is the fact that events similar to A and B have always been conjoined in our experience, whereas events similar to C and D have not always been conjoined in our experience.

It may be remarked that even at this point Hume's argument has certain weaknesses. In the first place, he does not attempt to explain why 'constant conjunction' should be taken to indicate the existence of a causal relation when the latter is, at least prima facie, a particular relation between particular events. This is a matter we shall return to later. Secondly, Hume does not, in the 'Enquiry', deal with the problems raised by the fact that even our experience of causally related kinds of events is not entirely uniform and coherent, owing to the fact that we blink our eyes, fall asleep, lose attention and so on. In Hume's highly restricted epistemological framework of 'impressions' and 'ideas', it is exceedingly difficult to articulate the distinction between purely subjective non-uniformities of experience and objective non-uniformities in the 'external world'. Even in the 'Treatise', Hume only turns to these problems after he has discussed the nature of causal inference,[1] whereas, arguably, he should have dealt with them first. I shall later try to argue that an adequate philosophical analysis of the nature of the 'objects of the senses' dispels many of the sceptical ideas which helped to undermine Hume's theory of causal inference.

Now, not long ago I remarked that in saying that our knowledge of causal relations 'arises from experience', Hume could not yet be excluding the possibility that we should, at least sometimes, acquire such knowledge through reasoning from experiential premises. We can now see that, given his further stipulation concerning the kind of

1. See Hume (1), pp. 187 ff.

experience in question, if such knowledge were to arise through reasoning in any instance, the premises of the arguments involved would have to refer to constant conjunctions of certain species of events within our past experience. This is the first real step in Hume's reduction of causal inference to an inductive procedure.

An inductive argument typically takes the following form. We possess a collection of 'data' concerning the constant conjunction within our past experience of certain species of events S_a and S_b and we possess evidence of the occurrence of a further event, A, belonging to the species S_a but not included in the 'data'. From all this we then infer the occurrence of another event. B, belonging to the species S_b and conjoined with A. On the other hand, a causal inference typically takes this quite different form: from evidence of the occurrence of one event. A, we infer the occurrence of another event, B, upon the supposition that A and B are causally related. Now, what Hume has stipulated concerning the kind of experience from which our knowledge of causal relations arises, implies that the supposition that A and B are causally related could only be justified by rational inference from a collection of 'data' concerning the constant conjunction within our past experience of certain species of events, S_a and S_b, to which A and B respectively belong. It is not difficult to see how the two processes of (1) the causal inference from the occurrence of A to the occurrence of B and (2) the justification of the causal proposition upon which this first inference is based, could be collapsed into a single process of inductive inference from the occurrence of A, plus data concerning the constant conjunction of observed members of the species S_a and S_b, to the occurrence of B. However, it should be stressed that while it may have been quite natural for Hume to have collapsed these two processes into one, such a move is not necessarily valid, and certainly requires some sort of justification. We shall see that through overlooking this fact Hume was led into a number of errors and confusions.

It should be recalled that our principal problem is the question of whether causal propositions may be justified, and if so how. So far Hume has ruled out, as possible justificatory procedures,

(1) demonstrative reasoning, or 'reasoning apriori' and (2) the appeal to 'direct' experience (since he denies that causal relations are possible objects of perception). This leaves as the only possibility (given Hume's empiricist assumptions) some kind of inference from 'direct' experience. Regarding this possibility we may interpret Hume's position so far to be that causal propositions could at best only be justified by inference from 'direct' experience where that experience is of constant conjunctions of certain species of events. In order to establish his sceptical belief that we can never use causal inference to justify transexperiential propositions, Hume must prove that causal propositions cannot. after all, be justified in this last way.

Now, it might seem at first sight that this is the task to which Hume addresses himself in Part II of Section IV of the 'Enquiry'. For he says at the outset: "I say, then, that even after we have experience of the operations of cause and effect, our conclusions from that experience are not founded on reasoning, or any process of the understanding. This answer we must endeavour both to explain and to defend." By 'the operations of cause and effect', Hume clearly means simply the constant conjunctions between particular species of events which are, in his view, the only sensible manifestations of causation. However, as we shall see, Hume is not very clear as to the character of the 'conclusions' which we supposedly draw from such experience. What these conclusions ought to be are causal propositions, if Hume is really to address himself to the task in hand. But, owing to his tendency to collapse the separate processes of causal inference and the justification of causal propositions into the single process of inductive inference, he actually takes these 'conclusions' to refer simply to future or unobserved conjunctions of events: that is to say, he takes them to be inductive conclusions. We shall see that owing to this confusion Hume fails, after all, to show that causal propositions cannot be justified by 'probable' arguments. and hence fails to establish his sceptical position.

Let us now proceed to examine the argument which Hume offers in Part II. Paragraph 29 opens with the remark: "It must certainly be

allowed that nature has kept us at a great distance from all her secrets, and has only afforded us the knowledge of a few superficial qualities of objects; while she conceals from us those powers and principles on which the influence of those objects entirely depends." This is a clear reference to the Lockean distinction between sensible qualities and 'real essences'. Locke had considered the 'real essences' of corporeal substances to be the "real, but unknown, constitution of their insensible parts; from which flow those sensible qualities which serve us to distinguish them one from another."[1] In the same spirit Hume continues: "Our senses inform us of the colour, weight and consistence of bread; but neither sense nor reason can ever inform us of those qualities which fit it for the nourishment and support of a human body."[2]

Since Hume relies heavily upon this example in his subsequent argument, it is important to look at it more closely. Let us call the sensible qualities of the bread (its colour, weight and consistency) A, and its nourishing property B. Both A and B are observable and have, in common experience, been 'constantly conjoined'. Hume is now suggesting, in Lockean fashion, that the causal relation between A and B consists in their being collateral effects of a 'secret power' or 'real essence', which we may call 'X'. However, it should be noticed that Hume has already declared that "neither sense nor reason" can ever inform us about X, and he takes this for granted throughout the subsequent argument. So he is taking it for granted not only that X is unobservable, but that we can never even acquire knowledge of X by reasoning from experiential premises. But since the causal relation between A and B supposedly consists solely in their being collateral effects of X, and since X cannot be discovered by reason, it follows inevitably that reason can never discover to us the causal relation between A and B. Yet, the very question at issue is whether or not we ever can discover causal relations by reasoning from experiential premises. So, by relying upon the example of the bread, and by assuming a sceptical position with regard to 'secret powers', Hume was really taking for granted from the outset the very point which he apparently undertook "to explain and to defend". Why Hume should

1. Locke (1), Bk. III, Chap. III, para. 17.
2. My underlining.

have taken this for granted is a matter I shall try to explain later.

Despite this unsatisfactory beginning, it is interesting to pursue Hume's argument to its conclusion. His next remark is this: "But notwithstanding this ignorance of natural powers and principles, we always presume, when we see like sensible qualities, that they have like secret powers, and expect that effects, similar to those which we have experienced, will follow them. If a body, of like colour and consistence with that of bread, which we have formerly eat, be presented to us, we make no scruple of repeating the experiment, and forsee, with certainty, like nourishment and support. Now this is a process of the thought or mind of which I would willingly know the foundation." In this passage we may see quite clearly Hume's reduction of causal inference to an inductive process. The causal inference in this instance is from the existence of A (the sensible qualities of the bread) to the existence of B (its nourishing property). This inference is founded (according to Hume) upon the supposition that A and B are causally related as collateral effects of a 'secret power', X. But this supposition itself could, in Hume's opinion. only be justified by inference from our past experience of a constant conjunction between the occurrence of qualities like A and the occurrence of properties like B. However, Hume has now decreed that "neither sense nor reason" can ever inform us about X, and hence that the supposition in question cannot be justified by such an inference. He therefore suggests that the original causal inference from A to B in reality merely amounts to an inductive extrapolation, from our past experience of a constant conjunction of the kind described, to a prediction that a further conjunction of the same sort will occur. In this way, the causal character of the original inference from A to B is completely eliminated.

Having effected this reduction of causal inference to inductive extrapolation, Hume remarks, concerning the latter, "Now this is a process of the thought or mind of which I would willingly know the foundation." And, curiously enough, he now proceeds to argue that inductive extrapolation itself cannot be a process of reasoning or argument. This is curious because, of course, it was only upon the

assumption that causal inference as he originally described it is not a justifiable procedure that Hume reduced it to inductive extrapolation. Now, it is this argument against the rationality of inductive procedures for which Hume is so famous. Yet, it is clear that in producing this argument Hume imagined that he was keeping his promise "to explain and to defend" his assertion that "even after we have experience of the operations of cause and effect, our conclusions from that experience are not founded upon reasoning." It is easy to see, however, that if these 'conclusions' are taken to be causal propositions, no such argument can fulfil the promise in question, since the 'conclusions' of inductive extrapolations are not causal propositions, but are mere predictions of conjunctions. Hume was evidently led into this confusion by his reduction of causal inference to inductive extrapolation. What is worse is the fact that the famous argument against the rationality of inductive procedures is itself untenable unless this reduction can be justified, as we shall see.

We are now in a position to evaluate Hume's critique of induction. In fact Hume appears to have two arguments against the rationality of inductive procedures, of which the second is the famous one. The first argument seems to be simply that there is no inference-relation between the 'premises' and 'conclusion' of an inductive extrapolation. Thus he says: "These two propositions are far from being the same, 'I have found that such an object has always been attended with such an effect' and 'I forsee, that other objects, which are, in appearance similar, will be attended with similar effects'.... But if you insist that the inference is made by a chain of reasoning, I desire you to produce that reasoning. The connexion between these propositions is not intuitive. There is required a medium, which may enable the mind to draw such an inference, if indeed it be drawn by reasoning and argument. What that medium is, I must confess, passes my comprehension." Now, I shall later support this contention of Hume's that there is no inference-relation between the premises and conclusion of an inductive argument. But I shall also argue that this need not imply that inductive procedures are just irrational, provided that they can be supported by non-inductive arguments of a certain kind.

Of course, modern philosophers would point out that there is a relation between the premises and conclusion of an inductive argument: the logical relation of 'confirmation' or 'inductive support'.[1] However, the point which Hume should be taken to be making is that this relation of 'confirmation' is not an inference-relation. This is a matter to which I shall return in a later chapter.

We may now turn to Hume's second and more famous argument. This is the argument which has frequently been hailed as a masterpiece of philosophical analysis. Thus A. J. Ayer: "Whatever we may think of the conclusion, this is a marvellous chain of arguments: one of the most brilliant examples of philosophical reasoning that there has ever been, and one of the most influential."[2]

Hume seems to maintain that if inductive extrapolation were a process of reasoning, it would have to "proceed upon the supposition that the future will be conformable to the past."[3] Similarly, in the 'Treatise', Hume declares: "If reason determin'd us, it would proceed upon that principle, that instances, of which we have no experience, must resemble those, of which we have had experience, and that the course of nature continues always the same."[4] This closely resembles, of course, the more modern notion of a 'principle of inductive inference'.[5] But there is also an important difference. It may be remarked that Hume does not explicitly state why he thinks that inductive extrapolation could only be a genuine process of reasoning if it were founded upon the principle which he mentions. D. C. Stove has suggested that what Hume meant was that the "supposition that the future will be conformable to the past" is required in order to transform an inductive extrapolation into a deductively valid inference.[6] This suggestion seems quite plausible in the light of Hume's undoubted tendency to anticipate his ultimate conclusion that deductive inference is the only genuine form of reasoning. Yet it may be observed at once that it is doubtful whether so vague a principle as that of the 'uniformity of nature' really would transform an inductive argument into a valid deductive inference.[7] And, certainly, modern inductivists do not want to attribute such a role to their 'principle of inductive inference', since they want to stress

1. See, e.g., Hempel (1), pp. 3 ff.
2. Ayer (1), p. 6.
3. Hume (2), p. 35.
4. Hume (1), p. 89.
5. See, e.g., Reichenbach (1), p. 340.
6. Stove (1), pp. 43-4.
7. Cf. Katz (1), pp. 44-5.

the non-demonstrative character of inductive arguments. However, leaving this point aside for the moment, we may turn to paragraph 30 of the 'Enquiry', which contains the kernel of Hume's argument: his attempt to prove that the supposition or principle which he mentions could not be justified by any species of argument or reasoning.

It is at this point that Hume declares: "All reasonings may be divided into two kinds, namely, demonstrative reasoning, or that concerning relations of ideas, and moral reasoning, or that concerning matter of fact and existence." Hume seeks to show that neither demonstrative nor moral, or probable, reasoning can be used to support the proposition that "the future will be conformable to the past". He proceeds as follows.

He first points out: "That there are no demonstrative arguments in the case seems evident, since it implies no contradiction that the course of nature may change." That is to say, the proposition in question is a synthetic one (recalling that Hume characterises 'matters of fact' by saying that "the contrary of every matter of fact is still possible; because it can never imply a contradiction.") And this means that it cannot possibly be justified by means of demonstrative reasoning, which only concerns 'relations of ideas'.

If demonstrative arguments are ruled out, it follows, as Hume says, that "if we be .... engaged by arguments to put trust in past experience, and make it the standard of our future judgment, these arguments must be probable only, or such as regard matter of fact and existence, according to the division above mentioned." However, according to Hume, there can be no such arguments in this case since "we have said that all arguments concerning existence are founded on the relation of cause and effect; that our knowledge of that relation is derived entirely from experience; and that all our experimental conclusions proceed upon the supposition that the future will be conformable to the past. To endeavour, therefore, the proof of this last supposition by probable arguments, or arguments regarding existence, must be evidently going in a circle, and taking that for granted, which is the very point in question."

But the trouble with this famous argument is that it begs several

questions itself. The first clause is, of course, unexceptionable: it merely asserts that all probable arguments are "founded on the relation of cause and effect" - that is, involve causal inference. The second clause is, however, ambiguous. It states that our knowledge of causal relations is "derived entirely from experience". Now, as I pointed out earlier, this cannot exclude the possibility that such knowledge should be 'derived from experience', at least sometimes, by means of reasoning from experiential premises - that is, by means of probable arguments, involving causal inference. For, provided that causal propositions are synthetic - that is to say, provided that causal relations are 'matters of fact' - there is, prima facie, no reason why a causal proposition should not figure as the conclusion of a probable argument, just like any other synthetic proposition. (Such an argument would not be circular, provided that the causal proposition upon which the argument was founded was different from the causal proposition which formed the conclusion.[1]) However, Hume tended to anticipate his position of 'inductive scepticism' and to assume a simple dichotomy between 'Reason' and 'Experience'. Thus there is already a strong suggestion in saying that our knowledge of causal relations is "derived entirely from experience" that this knowledge is not, therefore, in any instance acquired through processes of reasoning. This suggestion, of course, Hume has altogether failed to justify.

The next clause in Hume's argument is the assertion that "all our experimental conclusions proceed upon the supposition that the future will be conformable to the past." This assertion is, as we have seen, quite inappropriate if by 'experimental conclusions' Hume means causal propositions. And it is still unjustified if by 'experimental conclusions' Hume means inductive conclusions (as in this context he evidently does): for he gives no explicit reason for supposing that inductive arguments would have to proceed upon the supposition in question, and his tacitly held reason appears to be the question-begging one that deduction is the only legitimate mode of rational inference.

Finally, we cannot even agree with Hume's conclusion, that

1. It might still be urged that the principles of empiricism would demand that at least some causal relations would have to be possible objects of 'direct experience': but this is a matter which I take up in the subsequent chapters.

"the proof of this last supposition by probable arguments .... must be evidently going in a circle." The only reason why the supposition that the future will be conformable to the past cannot be supported by probable arguments in Hume's scheme is simply that Hume has already assumed (1) that all probable arguments are reducible to inductive extrapolations and (2) that the latter could only be processes of reasoning if they were founded upon the supposition in question. But we have seen that Hume has justified neither of these assumptions. However, I should hasten to add that while I reject Hume's supposed proof that the 'principle of the uniformity of nature' cannot be supported by probable arguments, I do not mean to suggest that such a principle is, in fact defensible: on the contrary, it appears to be so vague as to be virtually meaningless.

We may conclude that Hume does not succeed in demonstrating the irrationality of all inductive procedures. However, what his arguments do seem to indicate is the incoherence of inductivism. They show that <u>given</u> the presuppositions of inductivism, inductive procedures are unjustifiable.[1] I shall later explain why there are, in fact, many circumstances in which inductive procedures <u>are</u> perfectly justifiable. I shall also point out that where inductive arguments are used legitimately, they do <u>not</u> "proceed upon the supposition that the future will be conformable to the past."

It will be recalled that our principal object in studying Hume's arguments was to discover whether he offered any cogent reasons for doubting the justifiability of causal propositions, since it is upon this doubt that his scepticism really rests. I think we may conclude from our examination of the 'Enquiry' that his scepticism was not warranted, even within the unsatisfactory framework of traditional empiricism. For even if we accept that causal relations are not possible objects of perception (a view which I shall, however, shortly come to criticise), none of Hume's arguments succeeds in showing that causal propositions cannot be supported by <u>probable</u> arguments. Of course, this does not mean that the theory of causal inference is now entirely problem-free. There are still many problems, some of them closely related to the doubts which Hume raised.

1. I return to this subject in a later chapter.

II : HUME'S ANALYSIS OF THE CAUSAL TIE

When I discussed Hume's argument that causal relations cannot be discovered by demonstrative reasonings, or reasonings apriori, I remarked that one might assume from this that in Hume's view causal relations are matters of fact, rather than relations of ideas. But, as I hinted then, such an assumption, however reasonable, would be mistaken. In fact, all the indications are that in Section IV of the 'Enquiry', at least, Hume took causal relations to be relations of ideas which are, for some reason, opaque to the human understanding; and thus that it is their opacity, rather than their being matters of fact, which prevents them from being discovered by demonstrative reasonings.

Such a notion would presumably have its original in Locke's notion of 'real essence'. Locke considered that the sensible qualities of substances 'flowed' from their 'real essences' quite as necessarily as the properties of a triangle follow from its definition and the axioms of geometry. Thus he says: "Had we such ideas of substances as to know what real constitutions produce those sensible qualities we find in them, and how those qualities flowed from thence. .... to know the properties of gold, it would be no more necessary that gold should exist and that we should make experiments upon it, than it is necessary for the knowing of the properties of a triangle, that a triangle should exist in any matter."[1] Locke was insistent, however, that we could never discover the 'real essences' of substances.

Now, we have already seen what an important part 'real essences' or 'secret powers' played in Hume's discussion of the causal relation between the sensible qualities of bread and its nourishing properties. Upon this example Hume based his argument that "even after we have experience of the operations of cause and effect, our conclusions from that experience are not founded on reasoning, or any process of the understanding." We saw that that argument begged the question, because Hume took it for granted that "neither sense nor reason" can ever inform us of the secret power, and hence of the causal relation in question. But only by assuming that Hume, at this point at least, was taking causal relations to be opaque relations of ideas, can we

1. Locke (1), Bk. IV, Chap. VI, para. 11.

understand why he should have taken this for granted.

However, if causal relations are relations of ideas, it should be perfectly obvious that they can never be discovered by 'probable' arguments, since these only concern 'matters of fact'. So the fact that Hume could devote much of Part II of Section IV to an argument about this very point can only imply that he was far from clear in his own mind about the logical status of causal propositions. The same ambiguity may be found in Section VII of the 'Enquiry', to which I shall now turn.

Section VII is apparently devoted to establishing the meaning of causal terms - that is, to the analysis of our causal concepts.[1] Thus paragraph 49 begins: "There are no ideas, which occur in metaphysics, more obscure and uncertain, than those of power, force, energy, or necessary connexion, of which it is every moment necessary for us to treat in all our disquisitions. We shall, therefore, endeavour, in this section, to fix, if possible, the precise meanings of these terms, and thereby remove some part of the obscurity, which is so much complained of in this species of philosophy."

For Hume, 'meaning' (in the sense of 'concept') is synonymous with 'idea', which is rather unfortunate since he next declares: "It seems a proposition, which will not admit of much debate, that all our ideas are nothing but copies of our impressions, or, in other words, that it is impossible for us to think of anything, which we have not antecedently felt, either by our external or internal senses." In fact, far from being indisputable, this proposition is quite evidently false and absurd, at least in Hume's interpretation of what it means. His interpretation represents the most extreme version of the doctrine nihil est in intellectu quod non fuerit in sensu, and would imply that we have no concepts which are not ultimately analysable in terms of 'simple ideas' of colours, shapes, smells, sounds, tastes, textures, and various bodily sensations. This doctrine is another legacy of Locke. Its consequences will become clear shortly.

In paragraph 50, Hume remarks: "When we look about us towards external objects, and consider the operation of causes, we are never able, in a single instance, to discover any power or necessary

1. Arguably, Hume should have dealt with this before examining the nature of causal inference.

connexion: any quality, which binds the effect to the cause, and renders the one an infallible consequence of the other." Now, what Hume says here appears to confuse two points which we have already noticed in his account of reasoning. The first is that between any two events which we consider to be causally related, we can discover no connection which renders the one a logically necessary consequence of the other. The second is the notion that causal relations are not possible objects of perception. Hume's confusion of these two points can only be explained by a supposition on his part that the only possible kind of relation or connection between different events (apart from spatiotemporal relations) is an analytic one (a 'relation of ideas'), and hence that since we fail to discover any such relations between those events which we consider to be causally related, causal relations, if they exist at all, must simply be opaque or 'secret' relations of ideas, and hence imperceptible.

That Hume did indeed assume that if causal relations exist they must be relations of ideas is implied by what he says next: "But were the power or energy of any cause observable by the mind, we could forsee the effect, even without experience; and might, at first, pronounce with certainty concerning it, by mere dint of thought and reasoning." (Compare this passage with the one quoted from Locke a little earlier. Notice also the recurring tendency to oppose 'reason' to 'experience'.)

However, if we separate these two points, we can see that while Hume's first suggestion is perfectly plausible - that we can discover no analytic connection between events which we consider to be causally related - his suggestion that we can never experience or observe a causal relationship is highly implausible. The only qualification which needs to be made to the first suggestion is the one I mentioned earlier, concerning the fact that many of our terms for objects and events carry logical implications of a causal character - that, as A. J. Ayer puts it, "For the most part we describe objects or events at least partly in terms of their actual or potential relations to other objects and events, and very often these descriptions include a tacit or explicit reference to their causal properties."[1] But the

1. Ayer (1), p. 7.

second suggestion would imply, as we have seen, that we can never, by mere observation, distinguish between a causal conjunction of events and a purely coincidental conjunction of events. As Hume himself puts it, it means that "All events seem entirely loose and separate. One follows another: but we never can observe any tie between them. They seem conjoined, but not connected."[1]

This is highly counter-intuitive, not to say paradoxical. For example, consider the following situation. Suppose it is a stormy day and I am walking by the river. For idle amusement I pick up a stone and throw it into the water (event A), and at the moment of the subsequent splash (event B), there is a flash of lightning (event C). Are we really to say that we cannot perceive A and B to be connected any more than we can perceive A and C or B and C to be? Surely, we can actually see that the stone's falling into the water 'has something to do with' the splash, whereas we cannot see that either of these events 'has anything to do with' the flash of lightning?

The Humean would reply to this, no doubt, that we only imagine that we can perceive a connection between A and B which is lacking between A and C or B and C because we are familiar with a splash following the projection of a stone into water, whereas we are not familiar with either of these events being associated with a flash of lightning. In reality, there is nothing objective which we perceive in the one case, but which is absent in the others.

This sort of reply, strangely enough, has silenced most critics. But to me it appears wildly far-fetched. If it really were the case that we could never distinguish, in perception, between causal conjunctions and purely coincidental conjunctions, it would be hard to see how human infants could ever learn at all. The Humean theory of learning - associationism - is itself quite inadequate and totally implausible.[2] The human mind must, it seems, have the ability to discriminate, at least sometimes, between the causally relevant and the causally irrelevant. Otherwise, experience would simply be a meaningless flux of unconnected sensations. The suggestion that these might 'cohere' through the agency of 'custom or habit' - or even

1. Hume (2), p. 74.
2. Cf. Chomsky (1), chap. 3.

through the agency of the 'imagination' - is surely quite wild. This is the reductio ad absurdum of the Humean position.

But, in any case, the standard Humean reply to those who argue that we can perceive causal relations naturally prompts the question as to why anyone should go to such lengths to deny that we can ever really perceive such relations. And once again the answer seems to be that adherents of this view imagine that if we could perceive causal relations, we should be able to predict the outcome of events with demonstrative certainty - that is, without even the logical possibility of error. But this can only mean that such philosophers tacitly suppose that causal relations could only be analytic connections, if they exist at all. In other words, such philosophers believe that the only kind of restraint upon the 'possible' behaviour of objects is that provided by the logical laws of excluded middle and non-contradiction. This belief is commonly referred to as the view that there is no such thing as 'natural' necessity.

Why did Hume and his followers neglect the possibility that causal relations might be synthetic connections - that is, matter-of-fact relations, rather than secret relations of ideas? One might be able to observe or experience such relations between events without thereby being able to predict the outcome of events with demonstrative certainty. Indeed, what kind of relations would one expect to find between matters of fact, but matter-of-fact relations? There is an inherent absurdity in the notion of logical relations between matters of fact.[1] Logical relations can only exist between our descriptions of matters of fact. But then it is impossible to explain why we have incorporated specific logical relations into our descriptions of matters of fact, save in terms of our having discovered that these matters of fact really are related, in a matter-of-fact way. For example, our term 'water' may carry the logical implication that water is a liquid which splashes easily, so that if we threw a stone into a liquid and there were no splash, this might logically exclude the possibility of our calling the liquid in question 'water'. But we can only explain why we have incorporated this logical implication into our concept of 'water' in terms of our having observed that stones and other similar objects

1. Cf. Ducasse (1), pp. 30-1.

do, as a matter of fact, cause a splash in certain liquids, but not in others. The logical relations between our descriptions of events, while not actually being causal relations, reflect to a large extent the causal relations which we have discovered to exist between events of those descriptions. It is not difficult to see how this may have given rise to confusions about the logical status of causal propositions.

This, then, is one possible source of the neglect of synthetic connections. But a more important one, in the case of Hume, was probably the doctrine of ideas, and particularly the view that all our concepts must ultimately be analysable in terms of 'simple ideas' of colours, shapes, smells, sounds, and so on. Such a view inevitably suggests that the only relations really perceivable (and hence, given Hume's assumptions, conceivable) are relations of ideas. But the kind of connection between a stone falling into water and the subsequent splash clearly cannot be analysed in terms of colours, shapes, smells or sounds, nor in terms of any 'necessary connection' between colours, shapes, smells or sounds.[1] (By the latter I mean such philosophically notorious relations as that between 'being red all over' and 'being green all over'.) However, because of this, Hume apparently felt compelled to deny that the connection between the falling stone and the splash is really intelligible to us. He furthermore assumed that this supposed lack of intelligibility derives from the opacity of causal relations - when in fact it derives from the inadequacy of the doctrine of ideas.

But at this point it may be asked what a causal relation then is, granted that it is a synthetic or matter-of-fact relation between events, rather than a secret relation of ideas. We might, of course, simply characterise it as a relation of 'production' or 'generation': when two events are causally related, one may be said to 'produce' or 'generate' or 'bring about' the other, in suitable circumstances.[2] But the Humean will interpose here that such a definition is quite empty, because 'cause', 'produce', 'generate' and 'bring about' are all synonymous. Hume, indeed, says exactly this in the 'Treatise': "I begin with observing that the terms of efficacy, agency, power, force, energy, necessity, connexion, and productive quality, are all

1. Cf. Ducasse (1), p. 8.
2. Cf. Harré (1), pp. 103 ff.

nearly synonymous; and therefore 'tis an absurdity to employ any one of them in defining the rest."[1] However, this is no valid criticism of the view which I am advancing, as I am not attempting to define the term 'causal relation'. Indeed, to demand a definition of 'causal relation' would be as absurd as to demand a definition of 'material object'. But no-one but the most extreme positivist would suggest that the lack of a definition in the latter case renders the concept in question meaningless or unintelligible. In other words, the general concept of a causal relation, like that of a material object, though it cannot be explicitly defined, may be learned and understood through an acquaintance with particular instances.[2] Nevertheless, it may be admitted that it is not very enlightening to be told that a causal relation is a relation of 'production' or 'generation', or that we are all perfectly well familiar with a large variety of different particular causal relations. A philosophical treatment of causation cannot rest content with statements like these, even if they are true as far as they go. I shall therefore attempt to improve upon these statements in subsequent chapters.

However, there are one or two points which should be clarified before we move on. The view which I am advancing by no means implies that we can always perceive the causal relation between two causally related events, or even that when we can perceive a causal relation, we can perceive all there is to know about that relation.[3] Thus, while it seems reasonable to suggest that one can perceive a causal relation between a stone falling into water and the subsequent splash, there are clearly many features of this relation which are known to science, but not to the 'plain man'. But the same is true of material objects: no-one supposes that the fact that the 'plain man' does not know all there is to know about an object such as a stone, implies that he cannot really perceive stones. (Actually, some phenomenalists do seem to have supposed just this: but if we are to abandon the phenomenalist analysis of material objects, as surely we must, should we not also abandon the phenomenalist analysis of causal relations?) But all this would seem quite evident, but for the philosophical prejudice in favour of logical relations.

1. Hume (1), p. 170.
2. Cf. Mackie (2), p. 134.
3. Nor does it imply that we cannot mistakenly think we perceive a causal relation where none really exists.

The consequences of Hume's prejudices are to be seen in Part II of Section VII of the 'Enquiry'. He acknowledges the difficulty into which he has forced himself: "And as we can have no idea of anything which has never appeared to our outward sense or inward sentiment, the necessary conclusion seems to be that we have no idea of connexion or power at all, and that these words are absolutely without meaning."[1] But, oddly enough, Hume's solution to this dilemma is to define the concept of a causal relation in such a way that causal relations do become, after all, matters of fact, but of a rather strange kind.

According to Hume, as we saw earlier, the only kind of objective experience which can betray to us the existence of a causal relation is the experience of a constant conjunction between certain species of events. In addition to this we have, according to Hume, an internal feeling or sensation associated with the habit which we acquire, after experience of a constant conjunction, to expect similar conjunctions in the future. This feeling, apart from the observation of the constant conjunction itself, is the only sensible experience from which our concept or idea of a causal relation can be formed, in Hume's view. He accordingly gives us two definitions of the causal relation: the first in terms of the constant conjunction, which may be said to contain all that is objective in our concept of a causal relation: and the second in terms of the internal feeling or sensation, which may be said to contain the subjective or psychological element in our concept of a causal relation.

It is the objective definition which has had the more influence, philosophically, even though it would appear that Hume himself considered the subjective element the more important, since he remarks, in the 'Treatise': "Thus tho' causation be a philosophical relation, as implying contiguity, succession, and constant conjunction, yet 'tis only so far as it is a natural relation, and produces an union among our ideas, that we are able to reason upon it, or draw any inference from it."[2] This is simply because Hume reduces causal reasoning to inductive extrapolation, founded upon 'custom or habit'. (Incidentally, we may notice his ambivalent attitude in this last passage, where he persists in speaking of causal reasoning, even though

1. Hume (2), p. 74.
2. Hume (1), p. 94.

he has done his best to prove that this process is quite irrational.)

According to the objective definition, "We may define a cause to be an object, followed by another, and where all the objects similar to the first are followed by objects similar to the second. Or, in other words, where, if the first object had not been, the second would never have existed."[1] It is commonly pointed out that the second sentence in this passage is not, in fact, equivalent to the first, since mere statements of uniformity do not entail counterfactual conditionals.[2] But it is the first sentence which expresses Hume's true opinion, even though the second sentence betrays his common-sense inclination towards a more sensible notion of causation.[3]

This first definition would imply that whenever we assert the existence of a causal relation between two events A and B, the objective content of our assertion amounts to the claim that A and B belong to certain species of events, such that, as a matter of fact, members of these species have been and will be always and everywhere conjoined. This amounts to a claim that a certain uniformity or regularity exists throughout the universe in both space and time. Now, I need hardly remark how far-fetched it is to suppose that we really intend such a claim whenever we assert that two particular events are causally related. The positivist will maintain, of course, that while we may not intend such a claim, such a claim is a logical implication of what we say, if we suppose ourselves to be saying anything meaningful at all. But this implies that it would be a logical contradiction to assert that A and B are causally related, and yet to deny that certain other events similar to A and B are thus related as well. However, it is very often the case that this is precisely what we do say. It will not do for the positivist to retort that in all such cases the events in question are not similar to A and B in the right respect. For in what respect should these other events be 'similar' to A and B? This dependence upon the notion of similarity, which is quite explicit in Hume's first definition of 'cause', is one of the many weaknesses in the 'regularity' theory of causation.

As I remarked earlier, Hume's first definition makes causal

1. Hume (2), p. 76.
2. See, e.g., Flew (1), pp. 130 ff.
3. Cf. Mackie (2), p. 30.

relations matters of fact, but of a rather strange kind. For they are not conceived of as particular synthetic connections between particular events, as I and common sense suggest that they should be, but rather in terms of matter-of-fact universal uniformities. What is strange about such uniformities is that, in the absence of particular synthetic connections between the particular events which instantiate them, they are utterly mysterious and inexplicable. This is a problem to which I shall return in a later chapter.

However, it is evident that Hume, unlike the later positivists, never considered that his two definitions of 'cause' were really adequate, in the sense of reflecting the real nature of the connections between matters of fact, if such there be (and this, of course, we can never know, according to Hume): these, he was apparently still convinced, must really be secret relations of ideas. Universal constant conjunction could at best be the outward sensible effect of the workings of secret inner causes or powers, and it therefore could not constitute causation 'as it is in the objects'. Hume's definition was simply meant to represent the best concept (or the most 'adequate idea', in Lockean terms) to which the human mind, in its state of incurable ignorance, could ever attain. Thus he says: "And what stronger instance can be produced of the surprising ignorance and weakness of the understanding than the present? For surely, if there be any relation among objects which it imports us to know perfectly, it is that of cause and effect .... Yet so imperfect are the ideas which we form concerning it, that it is impossible to give any just definition of cause, except what is drawn from something extraneous and foreign to it."[1]

Perhaps if Hume had perceived the error of the very attempt to define 'cause', he would have wreaked less havoc in philosophy. For, as we shall see, there is in reality no such thing as the causal relation, but rather a family of relations, having 'family resemblances', in Wittgenstein's sense, but no simple definable character.[2] Yet Hume was right in seeing the inadequacy of the 'regularity' theory, and in perceiving that, whatever else, a causal relation must be a particular connection between particular events.

1. Hume (2), p. 76. It seems to me that Hume was far closer to Locke in his general philosophical outlook than many twentieth-century commentators have supposed (perhaps through a misguided desire to see him as the forerunner of logical positivism).
2. Wittgenstein (1), para. 66, 67.

III : THE OBJECTS OF THE SENSES

In the previous chapter I argued that it is not unreasonable to suppose that we can actually perceive causal relations. In this chapter I shall try to show that scepticism with regard to the perceptibility of causal relations has exactly the same roots as scepticism with regard to the perceptibility of material objects, and that it therefore anomalous that modern philosophers, who have almost universally rejected the latter sort of scepticism, should still largely sympathise with the former.

'Perceive' is not, of course, a word much employed in ordinary language: it is a philosopher's term. And in the hands of philosophers it has received various different uses, changing over the generations as fashions in theories of perception have changed. Being partly a technical term, its meaning has never been entirely independent of the theories in which it has been employed. Thus for Locke and other early empiricists, 'perceiving' was synonymous with 'having ideas',[1] which sounds very odd to modern ears, owing to a change in the meaning of the word 'idea'. I should emphasise, then, that when I speak of 'perception', I mean sense-perception. For earlier philosophers often included thought and imagination under the general term 'perception',[2] a usage which is retained in certain expressions, as when one speaks of 'perceiving' the truth of someone's words, or of 'perceiving' the error of one's ways: 'perceiving' here has the sense of 'realising' or 'coming to understand'.

By sense-perception I mean seeing, hearing, smelling, tasting and feeling, the first four modes of perception being closely tied to specific sense-organs - the eye, the ear, the nose and the tongue or 'palate' - whereas the fifth, 'feeling', is much more diversified and, indeed, only by convention, I think, spoken of as a single 'sense'. For 'feeling' includes the tactile sense (as when we 'feel' the shape or texture of something), the kinaesthetic sense (as when we 'feel' our own bodily movements), and the senses of warmth and pressure. Moreover, particularly in the case of 'feeling', as usage itself indicates, there is a shading off from perception to sensation. We

1. See Locke (1), Bk. II, Chap. I, para. 9.
2. See, e.g. Locke (1), Bk. II, Chap. IX, para. 1, 2.

speak of 'feeling' external objects and their qualities of texture, shape and warmth (and this is perception), and we also speak of 'feeling' aches and pains, tickles, giddiness and nausea (and this is sensation). Sensation itself shades off into emotion, for we also speak of 'feeling' anxiety, pleasure, fear, misery and so on, all of which states are not purely 'sensational', but include 'intellectual' elements (such as anticipation and memory). Because of the danger of confusion, it may be wise, in the philosophy of perception, to concentrate upon sight and hearing, the least 'sensational' of the senses, and thereby avoid the common philosophical error of assimilating perception to sensation (an error much inveighed against by Ryle[1]).

By 'perceiving', then, I mean seeing, hearing, smelling, tasting, and feeling or touching. But what do we perceive? What are the 'objects of the senses'? It may be doubted whether this question is really a meaningful one. There certainly does not seem to be any one kind of thing which we perceive.[2] However, while it would doubtless be mistaken to suppose that there is a special class of entities whose essential property is that of 'being perceived' (a supposition which many idealists and phenomenalists have been inclined to make), it would not necessarily be mistaken to think that the things which we perceive may be categorised in certain ways, perhaps in ways which have nothing whatever to do with the fact that they are perceived by us.

Our question may at least begin to make sense if we examine how we ordinarily employ the words 'see', 'hear', 'smell', 'taste' and 'feel' or 'touch'. By discovering what sort of 'objects', in the grammatical sense, these verbs ordinarily take, we may hope to elucidate the nature of the 'objects', in the ontological sense, which we ordinarily conceive ourselves to perceive. Such an examination of ordinary linguistic usage discloses that we at least speak of perceiving a considerable variety of different categories of things. These categories are deeply embedded in the grammatical structure of our language, and consequently form the basis of our common-sense ontology. Thus we have common nouns denoting various kinds of particulars, adjectives denoting the qualities and properties of

1. See Ryle (1), pp. 200 ff.
2. Cf. Austin (1), p. 4.

these particulars, verbs denoting their activities, and prepositions denoting spatial and temporal relations between them. And all of these different categories of things we speak of perceiving - that is, of seeing, hearing, smelling, tasting and feeling.

Now, while it would be wrong to suggest that philosophy can or should do nothing more than analyse 'ordinary language', there have been many philosophers who have pronounced dogmatically upon what we can and cannot perceive, without sufficiently taking into account the extent to which our use of perception-verbs is interwoven with our most fundamental beliefs concerning 'what there is'. The bulk of the present chapter will be devoted to criticising some of these dogmas.

I shall first of all discuss perceptible particulars, and later go on to talk about perceptible attributes and relations. It is the latter which are of primary interest in a discussion of causation, but, as I mentioned earlier, I want to draw a parallel between scepticism with regard to the perceptibility of causal relations and scepticism with regard to the perceptibility of certain sorts of particulars.

It is impossible to define what is meant by a 'particular'. It might be suggested that the defining characteristics of a particular are (1) that a particular can only be at one place at a given time. (2) that if a given particular has been at two different places at two different times, then it must have been at intervening places at intervening times, and (3) that two particulars of the same sort cannot exist at the same place at the same time. These are the familiar criteria of spatiotemporal continuity and diversity. But these criteria clearly cannot provide a definition[1] of the term 'particular', since the concepts of space and time, in terms of which these criteria are framed, are themselves not independent of the concept of a particular. In fact, however, none of the basic categories of our common-sense ontology are definable. It is possible to state general characteristics of any given category, but such generalisations must not be looked upon as definitions, as they always presuppose a prior, even if only tacit, understanding of the categorial framework which they are supposed to explicate.[2]

1. I am using the term 'definition' in the sense in which we found Hume to be using it in the last chapter: in this sense, the definiens must not include concepts which are tacitly dependent upon concepts included in the definiendum. In a later chapter I shall refer to such a definition as a 'reductive analysis'.
2. Cf. Quine (2), pp. 48 ff. I borrow the term 'categorial framework' from Körner: see Körner (1).

The most important category of particulars is the category of material objects, or 'things': material objects are, in Strawson's phrase, our 'basic particulars'.[1] Some examples of the membership of this category are: apples, trees, tables, books, animals, houses, stones, stars, etc. That this is a very broad category is indicated by the fact that it includes natural inanimate objects such as stones, living beings such as trees and animals, and artefacts such as houses, tables and books. Yet all these diverse objects have certain general features in common. In particular: all material objects or 'things' are three-dimensional and possess fairly definite spatial boundaries; they all persist in time for periods of varying lengths, during which they undergo processes of change; and they are, generally speaking, perceptible by means of all the senses severally. To illustrate the last point, we may <u>see</u> an apple, <u>taste</u> it, <u>smell</u> it, <u>touch</u> it, and even <u>hear</u> it (e.g. if it falls from the tree): if a 'thing' is not perceptible by a given sense (as, for instance, a diamond has no smell or taste), this is always a purely contingent matter (which, as we shall see, is often not so in the case of other categories of particulars).

As I remarked earlier, many philosophers have held dogmatic views concerning the perception of material objects. As an illustration we may cite Berkeley. Thus at one point he asks (with the obvious implication that a negative answer is undeniable) "whether we immediately perceive by the sight anything beside light and colours and figures; or by hearing any thing but sounds; by the palate any thing besides tastes; by the smell, besides odours; or by the touch, more than tangible qualities?"[2] Of course, one puzzling term in this passage is 'immediately': for the 'plain man', as Austin pointed out,[3] rarely speaks of seeing or feeling anything 'immediately'. But it would be absurd to insist that a philosopher should never depart from 'ordinary language', provided that he can make clear the meanings of any new or technical terms which he may introduce, and provided that he does not trade upon ambiguities. Now, Berkeley's view was simply that the perception of 'sensible qualities', as he calls them - that is, colours, smells, sounds, and so on - is radically different from the perception of 'things', such as tables and chairs and apples

1. See Strawson (2), p. 40.
2. Berkeley (2), p. 203.
3. Austin (1), pp. 15 ff., pp. 133 ff.

and books, in that the latter involves a process of inference (albeit usually an unconscious inference). Thus he says: "When I hear a coach drive along the streets, immediately I perceive only the sound; but from the experience I have had that such a sound is connected with a coach, I am said to hear the coach."[1] So Berkeley is not denying that the 'plain man' may correctly speak of hearing such things as coaches, rather than just sounds: he is asserting, rather, that in saying this the 'plain man' is overlooking an important distinction which does not escape the philosopher. Nevertheless, despite this qualification, and despite the relative plausibility of the example of the coach, Berkeley's view really does conflict quite strongly with ordinary usage and common sense, and seems to derive from a number of important confusions.

It is undoubtedly the case that the verbs 'see', 'hear', 'smell', 'taste' and 'feel' often take as their grammatical objects such nouns as 'colour', 'shape', 'sound', 'odour', 'flavour', 'texture' and so on. But we should be wary of supposing that these nouns therefore denote entities of any sort, having an independent ontological status as particulars. We should be wary of reifying 'sensible qualities'. This was precisely the mistake which Berkeley made: he treated 'sensible qualities' as things - things, indeed, of a very elementary kind, out of which are composed all the various material objects, such as tables and chairs, which the 'plain man' speaks of perceiving. Thus he says: ".... a certain colour, taste, smell, figure and consistence, having been observed to go together, are accounted one distinct thing, signified by the name apple."[2]

Now, it is instructive to discover exactly when, in general, we speak of hearing 'sounds' (as opposed to coaches, bells, guns, etc.) or of smelling 'odours' (as opposed to apples, flowers, gas, etc.) or of tasting 'flavours' (as opposed to oranges, salt, beer, etc.) The answer seems to be that we generally do so when we are uncertain as to exactly what sort of thing we are confronted with. Thus, when we are uncertain as to whether we can smell a rose or a violet, we may say that we can smell a fragrant 'odour'; and when we are uncertain as to whether we can hear the cat or a burglar moving about downstairs in the

1. Berkeley (2), p. 237.
2. Berkeley (1), p. 113.

night, we may say that we can hear a clattering 'noise'. But are there really fragrant 'odours' in addition to flowers which smell fragrant, or clattering 'noises' in addition to cats and burglars who clatter about in the night? In Berkeley's ontology there are, ultimately, only odours and sounds and colours and flavours, whereas our common-sense ontology only allows an existence of a sort to such entities when it is not immediately apparent that they can be reduced to the status of attributes of material objects. Thus we allow a semi-independent existence to a 'bang' when it is not immediately apparent that any particular thing has 'banged'. For example, when a door 'bangs', we are not tempted to allow an independent existence to the 'bang', but treat it as an attribute of the door. But in the case of thunder, where there is, apparently, no thing which 'bangs', we do accord an independent existence to the 'bang'.[1]

It is certainly the case that our common-sense ontology includes various categories of particulars besides that of material objects, and that perhaps the most important of these secondary categories is that of such 'phenomena' as thunder, lightning, rainbows, shadows, echos, reflections and patches of light. All such phenomena share certain characteristics which distinguish them clearly from material objects. For example, they need not be three-dimensional and may not have well-defined spatial boundaries, or even an exact location in space at all; they are often very ephemeral in nature, so that their reidentification is frequently problematical;[2] and they are usually only perceptible by one or two of the senses (e.g. we can only see rainbows, only hear thunder, and only see and feel shadows). As I mentioned earlier, this latter feature of 'phenomena' is by no means contingent, as it is in the case of the diamond which has no smell.

Now, what Berkeley and other philosophers have done, in effect, is to take phenomena, rather than material objects, to be our 'basic particulars'. Yet we have observed that in the ontology of common sense there is a strong tendency to reduce phenomena to the status of attributes of material objects. Phenomena only acquire an independent ontological status as particulars either when they become 'detached' from the appropriate material objects (as, for example, when

1. Cf. Strawson (2), p. 46.
2. Cf. Strawson (2), p. 55.

food has been eaten but the 'smell' remains) or when there appear to be no appropriate material objects to which they can be 'attached' (as in the case of the 'colours' of the rainbow). This reductivist tendency, already apparent in our common-sense ontology, is carried even further in the ontology of modern science, by the introduction of imperceptible, microscopic material objects: many macroscopic 'phenomena' are reducible to the status of attributes of such objects. Thus, for example, rainbows are explained in terms of the refraction of light by water droplets, and odours are explained in terms of the chemical properties of complex molecules. This reductivist tendency in our ontological thought has certain epistemological advantages which I shall discuss later. In effect it is an unconscious exercise of Ockham's Razor.

Berkeley was not wrong to suppose that we at least sometimes infer the presence of things from the perception of noises, smells, and so on. His example of the coach may be a case in point. Again, this generally occurs when we are uncertain as to exactly what sort of thing we are confronted with - for example, a coach in the street or someone rattling at the door. Where he was mistaken was in supposing that the perception of things such as coaches always involves this inferential element - as though our senses always only provide us with evidence of the nature of the objects in our environment. For one thing, we are by no means always uncertain as to exactly what sort of thing we are confronted with: when, for instance, we have a coach right in front of us and we are looking at it in good light, we do not infer the presence of the coach from the perception of a certain colour and shape and so forth, nor are we in any degree uncertain as to its being a coach.[1]

Berkeley's mistake was, no doubt, at least in part a consequence of misleading linguistic analysis, whereby from the fact that our perception-verbs often take as their grammatical objects such nouns as 'odour' and 'flavour', it was supposed that these nouns always denote particulars of some sort. Linguistic analysis can be a useful guide in ontology, but only if used with caution. But a possibly even more important source of Berkeley's mistake was probably a

1. Cf. Austin (1), p. 115.

misapprehension of the import of certain physiological facts about perception, and this deserves some discussion.

We see with our eyes, hear with our ears, smell with our noses and so on: and yet the eyes are sensitive only to light, the ears to sound, and the nose to smell. Hence arises a simple confusion between what we may perceive by a given sense, and what the relevant sense-organ is sensitive to. Here we see the danger I mentioned earlier of assimilating perception to sensation. This confusion is encouraged by the fact that we use the same words to denote perceived things and scientific entities which are not, properly speaking, perceived. Thus we use the term 'light' to signify such phenomena as flashes, glows, and reflections, and also to signify electromagnetic waves or photons; and we use the term 'sound' to signify such phenomena as bangs, rumbles and whistles, and also to signify waves in a medium such as air. But the fact that the retina of the eye is sensitive only to electromagnetic waves - that is, to 'light' in the scientific sense - by no means implies that by means of the visual sense we can only immediately perceive flashes and glows and colours and so on - that is, 'light' in the 'plain man's' sense of the term. Of course, I am not denying that there may be a derivative sense in which we may, sometimes, be said to perceive photons - namely, the sense in which we may sometimes be said to perceive protons and electrons in a cloud-chamber, or an electric field in an electroscope.[1] What I am asserting is that in general when we speak of perceiving 'light', we do not and could not mean 'light' in the sense of photons, but only 'light' in the sense of such phenomena as flashes and glows. And it is simply not true to say that light in the latter sense is all that we can immediately perceive by the visual sense.

It may be remarked that in part what distinguishes our perception of something by a particular 'sense' from the mere sensitivity of the corresponding sense-organ is the fact that perception always involves an epistemic element. For although we may sometimes be mistaken as to what we perceive, we must, I suggest, always perceive whatever we perceive as being something or other.[2] This, indeed, is why we can make mistakes in perception: only where there is knowledge can there

1. See, e.g., Achinstein (1), p. 162.
2. Cf. Hanson (1), chap. 1.

be the possibility of error. For example, one may mistakenly think that one is seeing a lemon, when in fact what one sees is a piece of yellow soap: but one cannot <u>see</u> something and yet not see it <u>as being</u> this or that kind of thing. (The notion that there is a species of 'non-epistemic' seeing is one which I shall criticise more fully in due course.[1]) When one is <u>completely</u> baffled as to what sort of thing one is confronted with, one does not see some elementary uninterpreted 'datum': one does not 'see' anything at all. It is in such cases, if at all, that one can properly speak of having visual <u>sensations</u>. (Such sensations are, quite often, distinctly uncomfortable - which is hardly the way we would describe our <u>perceptions</u>.)

This epistemic element in perception, the element of recognition, seems to be closely related to another essential element: the element of 'awareness'. It might, I suppose, be queried whether it really is necessary that we should be aware whenever we perceive. For instance, if a person walks down a muddy street, engaged in heated conversation, and 'automatically' steps over a puddle, of which he later shows no sign of remembering consciously, should we not still say that he must have <u>seen</u> the puddle, albeit <u>unconsciously</u>? The answer to this is, I think, that the 'paradigm' cases of perception certainly <u>do</u> involve some level of awareness, although we may extend the use of perception-verbs to cases in which it is completely absent. In the same way we may extend the use of perception-verbs to describe the operations of certain machines: thus we might speak of a robot 'seeing' with its electronic eye, even though we would not attribute awareness or consciousness to a robot. I conclude that recognition and awareness are two essential features of true perception - features, moreover, which distinguish it both from sensation and from any mere mechanical reaction to a stimulation of the sensory organs.

So far I have suggested two sources - one linguistic and the other physiological - of the Berkeleian view that we can never perceive material objects 'directly' or 'immediately', but only by means of some process of <u>inference</u> from the perception of certain 'immediate objects of the senses'. But there are other sources besides these, and one of them is particularly important: this is philosophical

1. For this notion, see, e.g., Dretske (1), pp. 4 ff.

doubt, or scepticism.

Many philosophers have thought it relevant to the question 'What do we perceive?' to ask the further question: 'What can we doubt about what we perceive?' Thus H. H. Price once wrote: "When I see a tomato there is much that I can doubt. I can doubt whether it is a tomato that I am seeing, and not a cleverly painted piece of wax. I can doubt whether there is any material thing there at all. Perhaps what I took for a tomato was really a reflection; perhaps I am even the victim of some hallucination. One thing however I cannot doubt: that there exists a red patch of a round and somewhat bulgy shape, standing out from a background of other colour-patches."[1] Thus it is inferred that what we 'immediately' perceive in such a case is simply a reddish, roundish, bulgy something, or 'sense-datum'. But we may criticise both the premises and the logic of this argument. For in the first place, it is frequently the case that when we are looking at a tomato, or other material object, we cannot seriously doubt that this is what we are seeing.[2] And secondly, even in those cases (e.g. in bad light, or when we only catch a brief glimpse) in which we can only be certain that we are seeing something red and round and bulgy, without being able to specify exactly what, we cannot legitimately infer the existence of a certain 'something', or 'sense-datum', which is supposedly the 'immediate object of perception', and which merely possesses the properties of looking red and round and bulgy.

Much the same reply may be made to those phenomenalists who have appealed to the various 'arguments from illusion' in an attempt to prove that we do not perceive material objects 'directly'. The fact that a straight stick, partly immersed in water, appears to be crooked, by no means implies that when we look at such an object what we 'immediately' see is really a crooked 'appearance', or sense-datum. It is simply absurd to suggest that we always infer the real nature and qualities of the things we are confronted with, by judging from their 'appearances': that, for instance, whenever we judge a thing to be red, we do so on the grounds that it appears red to a 'normal' percipient in 'normal' conditions. (A. J. Ayer once adhered

1. Price (1), p. 3.
2. Cf. Austin (1), p. 10.

to this view: "What we actually do is to define the real qualities of a material thing in terms of the qualities of certain privileged appearances."[1]) For, in general, to say that an object appears red is merely to say that seeing this object is an experience rather similar to that of seeing an object which is red (and hence that the object which appears red may or may not actually be red). Thus an ability to perceive the appearances of things 'directly' presupposes an ability to perceive their 'real' qualities 'directly' - for how else could we know that a particular experience is similar to that of seeing an object which is red?[2] So, for instance, an ability to perceive that a straight stick, partly immersed in water, appears to be crooked, presupposes an ability to perceive (at least sometimes) that an object is ('really') crooked: and this is sufficient to prove that the 'immediate objects of perception' cannot always be mere 'appearances'. The age-old dichotomy between 'Reality' and 'Appearance' is a spurious one: nor could a 'language of appearances' possibly constitute a 'foundation of empirical knowledge', since it would necessarily be logically posterior to (or 'parasitic upon') discourse in terms of material objects and their attributes.

The confusions which we have just been discussing are evidently intimately connected with what I referred to earlier as the 'epistemic' element in perception. The point is that a perceptual judgment such as 'I can see an apple' has implications which extend beyond what is immediately 'verifiable': for instance, it implies that the object will be edible, will not suddenly explode, weighs a few ounces rather than a few pounds, has a white interior rather than a red one, and so forth. If any of these 'predictions' should turn out to be wrong. there will be a prima facie case for rejecting as false the claim that what I saw was really an apple. However, it does not follow, from the fact that it is possible that a statement should subsequently turn out to be false, that it is in any way improper to make it: the propriety of the assertion depends entirely upon the character of the evidence upon which it is made. Moreover, as I have remarked before, we are very often certain - and justifiably certain - as to the nature and qualities of the things which we perceive, and hence also very often

1. Ayer (2), p. 31.
2. Cf. Ryle (1), p. 209.

justifiably certain of all the 'predictive' consequences of our perceptual judgments. But from the fact that we are sometimes mistaken in our perceptual judgments the sceptic fallaciously draws the conclusion that we can never be justifiably certain of such judgments. It is of some importance to elicit the precise nature of this fallacy.

The fact that we are sometimes mistaken in our perceptual judgments certainly seems to imply that in any particular perceptual judgment it is logically possible that we should be mistaken: this is merely another way of expressing the fact that our perceptual judgments have predictive implications which extend beyond what is immediately verifiable. However, this does not imply that we can never be justifiably certain of such a judgment, unless it is quite unwarrantably assumed that we can only ever be justifiably certain where there is not even the logical possibility of error. It is true that we cannot be justifiably certain if we know that we might be mistaken: but the kind of possibility indicated by the 'might' must be appropriate to the kind of judgment we are making, and it is evident that mere logical possibility is not appropriate in the case of perceptual judgments. This is a subject to which I shall return in a later chapter. Meanwhile it should be remarked that while it is logically possible that we should be mistaken in any particular perceptual judgment - a fact which merely indicates the epistemic element in perception - it is not even logically possible that we should be mistaken in all (or even most) of our perceptual judgments: yet it is upon a paranoiac doubt of this sort that the sceptic largely bases his case.

I pointed out earlier a certain reductivist tendency in our ontological thought, whereby we incline, if possible, to regard 'phenomena' as attributes of material objects. I also remarked that this reductivist tendency has certain epistemological advantages. These advantages derive from the fact that 'thing'-discourse has far greater epistemic content (in the form of predictive implications) than does 'phenomenon'-discourse. This fact was recognised by many positivist philosophers and yet, perversely enough, they chose to regard paucity of epistemic content as a desirable feature of a truly

'scientific' discourse, on the grounds that observations described in terms of a discourse with low epistemic content are less liable to be mistaken. But in their desire to eliminate the possibility of error, these philosophers overlooked the fact that where there can be no error there can be no knowledge either. As it turns out, of course, practicing scientists have taken a course exactly opposite to that recommended by the positivist philosophers: the ontology of modern science is even more 'thing'-oriented than is the ontology of common sense. But it should be remarked that while 'phenomenon'-discourse has less epistemic content than 'thing'- discourse, there can be no way of describing what we perceive that is entirely devoid of epistemic content. All sensory experience contains an interpretive or hermeneutic element, even the perception of the so-called 'sensible qualities', as is indicated by cross-cultural studies of colour-perception.[1] Since, then, the quest for elementary non-epistemic sensory 'data' is doomed to failure, there can really be no objection to our treating material objects as 'immediate' objects of perception.

There is, however, one further source, worth mentioning, of the view that we can never perceive material objects 'directly', and this, too, is connected with the epistemic element in perception. This is simply the fact that human beings have to learn to perceive material objects such as apples: a new-born infant cannot perceive an apple as an apple. Now, empiricist philosophers have tended to conceive of the infantile mind as a 'tabula rasa', from which the adult mind differs merely through the acquisition of 'experience'. This might be called the 'closet' theory of the mind since Locke, who clearly held this view, likened the mind to "a closet wholly shut from light, with only some little openings left, to let in external visual resemblances, or ideas of things without."[2] According to this theory of the mind, when an adult perceives a material object such as an apple, his actual sensory experience is identical to that of an infant confronted with the same object, and his perceptual judgment only differs from that of the child because this sensory experience is associated in his mind with a greater variety of other 'ideas'. However, modern

1. See, e.g., Toulmin (2), pp. 437-9.
2. Locke (1), Bk. II, Chap. XI, para. 17. This is what Popper calls the 'bucket' theory of the mind: see Popper (2), pp. 60 ff.

psychological studies suggest that this view is quite mistaken, and that the sensory experience of infants is in fact radically different from that of adults.[1] It is clear, then, that the fact that infants have to learn to perceive material objects cannot imply that the perception of such objects is 'indirect', for the learning process consists not so much in a mere accumulation of sensory experience, as in a qualitiative change in sensory experience.[2]

This concludes my discussion of the perception of particulars, and we may now go on to discuss perceptible attributes and relations. Under the general term 'attribute' I include the so-called 'sensible qualities' (e.g. redness, sweetness, roughness, roundness, pungency, coldness, etc.) as well as activities and 'dispositional' properties (such as malleability, solubility, fragility, elasticity, etc.) We have already sufficiently discussed 'sensible qualities' under the head of 'phenomena', however. It is the latter two categories of attributes which are of most importance in a discussion about causation.

The 'activities' of particulars are denoted by verbs, and they fall into two classes, 'relational' and 'non-relational' activities, a distinction which corresponds to the linguistic division between transitive and intransitive verbs. Relational activities essentially involve interactions between particulars, which is why they are of central importance in a discussion about causation. However, it would be as well to begin with by discussing the perception of non-relational activities.

Some examples of non-relational activities which we commonly speak of perceiving are the following: rolling, falling, wriggling, glowing, flickering, sparkling, ringing, buzzing, expanding, burning. Thus we may see a diamond sparkling, a flame flickering, or charcoal glowing; we may hear a bell ringing, or a bee buzzing; and we may see and feel a worm wriggling, or a baloon expanding.

Now, just as it has often been held that we cannot perceive material objects 'directly', so it has frequently been maintained that we cannot perceive certain activities 'directly'. As we shall see, this scepticism has been particularly strong regarding the perceptibility

1. See. e.g., Piaget and Inhelder (1), pp. 28 ff.
2. Cf. Hanson (1), chap. 1.

of relational activities; but it has also been directed towards non-relational activities, especially those which may be classified as species of movement (e.g. falling, rolling, wriggling, spinning, crawling, shaking, expanding, etc.) The notion that movement is an illusion may be traced back as far as Zeno and Parmenides, of course. It is quite often suggested that the movements of objects are never perceived 'directly', but are only inferred from perceived differences in position or size or shape.[1] Indeed, it might even be alleged that modern science endorses this conception of motion, in view of the use of the differential symbols 'ds/dt' and 'dθ/dt' to represent linear and angular velocity respectively. However, we should be wary of drawing such an inference from the symbolism used in mechanics. For it is arguable that the concept of the time-series is derivative from, and hence logically posterior to, the concept of the activity of a material object. After all, all our measurements of time involve the use of clocks, which are, in effect, simply material objects which undergo various processes of regular, and generally cyclical, change. Thus a sand-glass involves the movement of sand from one compartment to another, and a mechanical clock involves the movement of a pointer on a dial. Now, whenever a material object exhibits some activity, it undergoes a certain change in state. For example, when a baloon 'expands' it changes in size and shape, and when a stone 'falls' it changes in position, and when a lamp 'brightens' it changes in luminosity. But it would be wrong to imagine that the notion of a change in state of an object is simply equivalent to the notion of a temporally ordered series of different states of that object. For, as I have just said, it seems likely that the notion of temporal order is logically posterior to the notion of change, and that the latter is quite fundamental to our concept of a material object. Thus, if anything, it is change which is perceived 'directly', and the temporal order which is 'inferred'. Bearing this point in mind, we may now go on to discuss relational activities.

Here are some examples of relational activities which we commonly speak of perceiving: pushing, pulling, hitting, melting, burning, dissolving, squashing, cutting, breaking, bending. Thus we may see

1. See, e.g., Russell (1), pp. 143 ff.

and hear a stone breaking a window, or a hammer hitting a nail; we may see and smell a red-hot iron burning cloth, or a flame melting wax; and we may see water dissolving a lump of salt, or see and hear water extinguishing a fire. Again, to cite an example I used in the previous chapter, we may see and hear a stone making a splash in (or 'splashing') a pool of water.

Now, it is my contention that our perception of relational activities such as the ones just cited is quite as 'direct' and 'non-inferential' as is our perception of non-relational activities and material objects.[1] Consider, for example, Hume's famous example of the billiard balls, which we discussed in an earlier chapter. I am maintaining that in this situation we 'immediately' perceive not merely certain 'sensible qualities', such as the redness and roundness of the balls and the greenness of the table; nor merely certain material objects, namely, the billiard balls and the table; nor merely certain movements of these objects (movement being, of course, a non-relational activity, denoted by intransitive verbs). We perceive all these things, but we also perceive one billiard ball hitting and pushing another: we perceive an interaction between these objects. Yet according to Hume and his followers, all we can perceive, strictly speaking, are the 'sensible qualities' - colours, shapes, sounds, smells, textures, etc. Little wonder, then, that Hume denied the perceptibility of causal relations.

Even amongst modern philosophers, scepticism with regard to the perceptibility of relational activities is very strong. It is even suggested that relational activities do not really exist, in the sense that they are 'reducible' to something else, such as actual or potential movements. Thus, in the case of the billiard balls, it is suggested that there really is nothing more to this situation than various objects in relative motion: as though not even the deepest scientific investigation could reveal anything more than this.

A good example of this prejudice occurs in a recent book by A. J. Ayer, in a passage which deserves quoting at some length. He writes: ".... we may take a simple illustration of Hume's, that of a cannon in billiards. We think of the cue striking the billiard

1. Relatively few philosophers have endorsed this view: but see, e.g., Ducasse (1), p. 26 and Michotte (1), passim.

ball and so imparting motion to it, of the ball hitting a second and rebounding on to a third. All these forceful expressions, 'striking', 'hitting', 'imparting motion', 'rebounding' are expressions of causal agency, and their use in this context is perfectly correct. But everything that actually happens can be wholely described in purely undrammatic spatio-temporal terms .... there is nothing in the situation that calls for anything more than the identification of various objects and the specification of their changing spatio-temporal relations."[1] Having said all this, however, Ayer remarks, somewhat lamely, "Of course, I am not maintaining that, when we say that the impact of the first ball causes the second ball to move, we are saying no more than that there is first a period in which the first ball is in motion and the second at rest .... then an instant at which they share a common spatial boundary, and then a period in which the balls are both in motion .... Exactly what more we are saying is not easy to specify, except that the answer has to do with the fact that the use of causal expressions brings in an element of greater generality. We are in some way implying that this situation is typical; that this is what normally happens or would happen in these circumstances. But the point is that the other situations with which this one is likened are of the same character. The only relations involved in them are spatio-temporal."[2]

Now, Ayer's uneasiness in this passage is readily understandable. The fact is that it is simply false to suppose that the science of mechanics is concerned merely with 'matter in motion'. Mechanics is not purely concerned with kinematics: it is also concerned with dynamics and energetics. It is not enough, in mechanics, to say how the billiard balls moved: it is necessary also to consider the various interchanges of energy involved. Two physical situations may be kinematically identical, and yet differ radically energetically. Thus all the motions of the billiard balls might equally well have been produced by a complicated system of electromagnets hidden under the table: but in that case the energetics of the situation would have been quite different. The 'more' we are saying besides describing the various motions of the billiard balls is not, as Ayer would have

1. Ayer (1), p. 11.
2. Ibid., my underlining.

it, that the situation is somehow 'typical'. It will not do to say that the causal aspect of the situation merely 'brings in an element of greater generality' (a phrase which, of course, displays Ayer's adherence to the 'regularity' theory of causation). No: the 'more' we are saying is simply that the motions of the billiard balls were produced in a certain way, namely, by one billiard ball hitting and pushing another (rather than, say, by the operation of concealed electromagnets). And, of course, what the 'plain man' describes as 'hitting' and 'pushing', the physicist describes as a certain kind of energy-interchange.

So, not only is there more to such a situation than merely 'matter in motion', this 'more' may actually be perceived. To generalise, we may say that all 'relational activities' of material objects are, from the physicist's point of view, simply processes of energy-interchange. And some (but not all) of these processes we may perceive.[1] Of course, it might be objected that since, for example, 'kinetic energy' is represented as half the product of mass and the square of the second differential of distance with respect to time (or, symbolically, as '$\frac{1}{2}.m.(d^2s/dt^2)^2$'), energy-interchange itself can be described in 'purely undrammatic spatio-temporal terms'. But we should remember that, as in the case of motion itself, the notation employed in mechanics is not a safe guide to the ontological presuppositions of that science. Moreover, ever since Einstein discovered a relationship between energy and mass (embodied in the famous equation '$E = mc^2$'), it has been evident that the concept of 'energy', far from being reductively eliminable, is quite the most fundamental concept of physical science. A process of energy-interchange is, then, just about the most 'real' thing that one could hope to discover in the physical world.

Now, one rather obvious objection to the view which I have been advancing would be the following. As I have remarked, all the motions of the billiard balls could equally well have been produced by a complicated system of electromagnets hidden under the table: but if that had been the case, the casual observer might still have imagined that the motions were being produced by one ball hitting and pushing

1. Cf. Michotte (1), pp. 226-7.

another. And the fact that we can be mistaken in such a case might be taken to imply that we cannot, after all, perceive relational activities such as 'hitting' and 'pushing' directly or immediately. An instance of precisely this argument may be found in a recent book by J. L. Mackie. Mackie writes: "Admittedly we can, in the most ordinary sense of 'see', see a stone break a window. But seeing this, like almost all seeing, includes an element of interpretation. And, of course, we might be wrong. It might be that a vacuum somehow suddenly produced on the inside of the window made it collapse a tenth of a second before the stone arrived. The point of mentioning such fantastic possibilities is not to promote scepticism .... but to bring out the fact that something which we can call interpretation is involved."[1] However, as Mackie himself acknowledges, this interpretive element is in no way peculiar to the perception of relational activities: in fact, as we have seen, no perception is entirely devoid of it. So, although Mackie claims not to be promoting scepticism, this is really precisely what he is doing. Thus he says, just before the passage just quoted: "If I see someone .... peel a potato, I see the relative movement of the potato and the knife .... but .... I don't see the knife making the peel come up."[2] But we have heard this sort of thing said only too often with regard to the perception of material objects. Thus if Mackie concurs with contemporary philosophical opinion in rejecting such arguments when applied to the perception of material objects, it is anomalous that he should continue to apply them to the perception of relational activities.

This concludes my discussion of our perception of the activities of material objects. However, in the philosophy of causation, a still more important category of the attributes of material objects is that of the so-called 'dispositional' properties, and these will constitute the subject-matter of the next few chapters.

1. Mackie (2), p. 134.
2. Mackie (2), p. 133.

IV : DISPOSITIONS AND CONDITIONALS

Here are some examples of common 'dispositional' properties:

fragility	elasticity
malleability	ductility
flexibility	solubility
permeability	inflammability
miscibility	adhesiveness

The 'manifestations' or 'displays' of dispositional properties - such as breaking, bending, dissolving, burning, stretching and squashing - are simply what we have referred to in the previous chapter as activities of material objects. For reasons which will soon become apparent, I restrict the term 'disposition' to denote only those properties which are 'manifested' as relational activities. It should also be remarked that 'activities', 'qualities' (such as shape, colour, texture, etc.) and spatial relations are often referred to generically as 'occurrent' properties, in contradistinction to 'dispositional' properties (and to certain other kinds of property which, as we shall see, bear certain resemblances to 'dispositional' properties). This, as will be made clear, is a very important distinction.

Attempts have often been made by philosophers to 'analyse' dispositional predicates. Such analyses seem to fall into two categories. There are semantic analyses which merely set out to elucidate what we ordinarily mean or understand by dispositional predicates; and there are reductive or ontological analyses which attempt to show that dispositions are not 'real' properties of things, even if we ordinarily think of them as being such. I take it that a reductive analysis of dispositions would be successful if it could be shown that disposition-statements are logically equivalent to truth-functions of statements mentioning only non-dispositional or 'occurrent' properties. A number of such reductive analyses have been proposed, mostly in terms of material conditionals.[1] Thus, if we take as our example the predicate 'water-soluble', and write:[2]

1. See, e.g., Carnap (1), pp. 52-3.
2. A note on symbolism. In the ensuing chapters I use the following symbols to denote the truth-functional connectives: conjunction = '&', negation = '-', alternation = 'v', and material implication = '$\supset$'. I use '$\longrightarrow$' to denote entailment.

'x is water-soluble at t' = p

'x is in water at t' = q

'x is being dissolved by water at t' = r

(where x is some particular material object and t is a certain time), then the suggested reductive analysis would be:

$$p \leftrightarrow q \supset r.$$

Now, such an analysis fails because it implies that any object not actually in water at t is water-soluble at t (because the material conditional is true if its antecedent is false). Various attempts have been made to improve upon this analysis, but without success.[1]

However, although attempts to 'analyse' disposition-statements in terms of material conditionals have failed, it seems indisputable that disposition-statements are logically equivalent to, or at least entail, non-material conditionals (including 'subjunctive' and 'counterfactual' conditionals). For example, it seems clear that in some sense of 'If..., then....', 'x is water-soluble at t' entails 'If x is in water at t, then it is being dissolved by water at t' or, symbolically:

$$p \longrightarrow \text{If } q \text{ then } r.$$

That this is true seems to be accepted on all hands.[2] But the question is what this fact signifies. Even if we grant that disposition-statements not merely entail, but are logically equivalent to non-material conditionals,[3] this in no way advances the cause of a reductive or ontological analysis of dispositions, since it is notoriously the case that non-material conditionals are not truth-functional. Of course, the equivalence might still be taken to indicate that dispositions are 'unreal' in a weaker sense, if it were also suggested that non-material conditionals are somehow 'subjective': for instance, if they were construed as 'suppositions',[4] or as 'inference-licences'.[5] But such interpretations of non-material conditionals have never appeared to be entirely satisfactory, particularly since they seem to imply

1. For fuller discussion, see Mackie (1), pp. 124 ff.
2. See, e.g., Mackie (1), p. 127 and Mellor (1), p. 168.
3. Mackie appears to hold this view, but Mellor rejects it: see Mellor (1), p. 178.
4. See, e.g., Mackie (1), p. 93.
5. See, e.g., Ryle (1), p. 119.

that such conditionals do not, in general, have definite truth-values. And, in any case, the equivalence of disposition-statements to non-material conditionals (supposing it to obtain) could be appealed to for an analysis of conditionality quite as well as for an analysis of dispositionality. In fact, it is arguable that the concept of dispositionality, being well-entrenched in natural languages in virtue of the common dispositional predicates, is semantically less opaque than that of conditionality. This view - that dispositionality is the prior concept - may be strengthened later when I explain why I think that we have no option but to regard dispositional properties as an irreducible category in our ontology.

It should be remarked that while disposition-statements are clearly not logically equivalent to material conditionals, it is the case that they entail them, simply because the non-material conditional is itself stronger than (entails) the material conditional.[1] That is:

$$\text{If } q \text{ then } r \longrightarrow q \supset r.$$

(Why this should be the case requires explanation, which I shall try to give later.) This fact has the consequence that the conjunction of a disposition-statement with the antecedent of the corresponding conditional entails the consequent of that conditional. That is:

$$p \,\&\, q \longrightarrow r.$$

The proof of this is very simple, as follows:

(i)	$p \longrightarrow$ If q then r	(assumed)
(ii)	If q then $r \longrightarrow q \supset r$	(assumed)
(iii)	$p \longrightarrow q \supset r$	(from (i) and (ii))
(iv)	$(p \longrightarrow q \supset r) \longrightarrow (q \,\&\, p \longrightarrow q \,\&\, q \supset r)$	(theorem of prop. calc.)
(v)	$p \,\&\, q \longrightarrow q \,\&\, q \supset r$	(from (iii) and (iv))
(vi)	$q \,\&\, q \supset r \longrightarrow r$	(theorem of prop. calc.)
(vii)	$p \,\&\, q \longrightarrow r$	(from (v) and (vi))

Q. E. D.

1. See, e.g., Mackie (1), pp. 74-5 and Mellor (1), p. 169.

Now, some philosophers have professed to find this entailment incompatible with the 'reality' of dispositional properties. Thus J. L. Mackie appeals to "Hume's principle that there can be no logical connections between distinct existences" for this purpose.[1] Now, in terms of our example, the entailment reads in words as follows:

'x is water-soluble at t' & 'x is in water at t'
$\longrightarrow$ 'x is being dissolved by water at t'.

The argument thus is that according to 'Hume's principle', these three propositions cannot each refer to distinct existences, in virtue of this entailment. However, it should be remarked that any argument based on this entailment must be quite symmetrical as regards both 'x is water-soluble at t' (p) and 'x is in water at t' (q), since it is only the conjunction of p and q which entails r ('x is being dissolved by water at t'). Thus this entailment, if it can be held to undermine the 'reality' of the property 'water-solubility', could equally well be held to undermine the 'reality' of the ('occurrent') property 'being in water'. This is plainly absurd. In fact, of course, one must sharply distinguish questions of 'real' existence from questions of 'distinct' existence. For instance, there seems to be little doubt both that r entails q and that r entails p, that is, that:

'x is being dissolved by water at t' $\longrightarrow$ 'x is in water at t'

and that:

'x is being dissolved by water at t' $\longrightarrow$ 'x is water-soluble at t'.

(I shall discuss these entailments later in more detail.) Thus, x's being in water at t is not a distinct existence from x's being dissolved by water at t: and yet we do not therefore want to deny the reality either of x's being in water at t or of x's being dissolved by water at t. And, of course, the same applies to the property of 'water-solubility', which may be 'real' even if its possession by x at t is

1. See Mackie (1), p. 137.

not logically 'distinct' from x's possession at t of certain other properties.[1] It should, incidentally, also be remarked that p and q are quite 'distinct', even if their conjunction is not 'distinct' from r: the fact that x is in water at t is logically totally unconnected with the fact that x is water-soluble at t. We shall see later that this fact is of some importance for the theory of dispositions.

In view of the foregoing discussion, I would reiterate my suggestion that as far as logical analysis is concerned, the 'reality' of dispositional properties would only be undermined if it were possible to give a truth-functional analysis of disposition-statements in terms of statements mentioning only 'occurrent' properties, like 'being in water at t' and 'being dissolved by water at t'. That such an analysis is impossible seems to be almost universally accepted by now. But that dispositions are 'real' seems to be much less accepted.[2] I can only attribute this attitude to a persistent hankering after an impossible reductivist ideal. As we saw in the previous chapter, there is an exact parallel in the case of the 'logical analysis' of material object-statements - save that by now scarcely any philosopher still wants to deny the 'reality' of material objects. Of course, there may still be other arguments suggesting that dispositional predicates are, in the context of modern science, in some sense primitive or crude or unuseful. My views on this issue will emerge in the ensuing chapters.

There is one more point. Mackie suggests that the above-mentioned entailment (p & q $\longrightarrow$ r) could not be used as an explanatory schema if the disposition-statement (p) is taken to refer to a 'real' property, because any explanation it might be held to offer would be 'too good'.[3] However, as Mellor points out, this objection is no more valid here than it would be if it were directed against the Hempel-Popper 'deductive-nomological' model of explanation.[4] An explanation is only 'too good' if it cannot be falsified by any possible experience: but anyone who employs the explanatory schema 'p & q $\longrightarrow$ r' is prepared to withdraw his assertion either of p or of q if subsequent experience should indicate the falsity of r.

1. Cf. Mellor's answer to Mackie's argument: see Mellor (1), pp. 179 ff. Mellor, who also affirms the 'reality' of dispositions, questions the truth of 'Hume's principle'. I do not question its truth, but only its relevance to the issue of the 'reality' of dispositions.
2. Cf. Mellor (1), p. 157.
3. See Mackie (1), p. 143.
4. See Mellor (1), p. 179. I discuss this model more fully later.

V : THE ACTUAL AND THE POTENTIAL

In the previous chapter I maintained that a reductive analysis of dispositions is impossible. We cannot analyse the dispositional properties of a thing in terms of its 'occurrent' properties (its activities, qualities and spatial relations with other things). But a semantic analysis of dispositionality is still philosophically desirable. However, I suggested that a semantic analysis in terms of non-material conditionals would be unenlightening since the concept of conditionality is, if anything, semantically more opaque even than that of dispositionality.

The etymology of many dispositional predicates suggests another line of analysis. Many such predicates end with the suffix '-able' or '-ible' or '-uble' (e.g., 'malleable', 'flexible', 'soluble'). The obvious suggestion is that a term like 'water-soluble' simply means '<u>able</u> to be dissolved by water', so that a 'water-soluble' substance is simply one that '<u>can</u> be dissolved by water'. But what does the 'can' signify when we say of a thing that it 'can' be dissolved or burnt or broken? It might be suggested that the 'can' here denotes quite simply a <u>physical or natural possibility</u>. But, as we shall see in the next chapter, such an interpretation is unsatisfactory. Moreover, for reasons which will become clear later, I want to maintain that the concepts of natural possibility and necessity are ultimately derivative from a more fundamental concept: the concept of 'potentiality' or 'power'.

When we say of a thing that it 'can' exhibit a certain sort of activity, we are, I suggest, ascribing to it a certain 'power'. Dispositional properties are merely one species of power: we may call them 'relational powers', because their 'manifestations' are relational activities.[1] There are also <u>non</u>-relational powers, such as 'explosiveness' ('explode' is an intransitive verb in its normal use). It should be remarked that non-relational power-statements, unlike disposition-statements, do <u>not</u> entail specific non-material conditionals. Thus, 'x is explosive at t' does not entail any specific conditional of the form 'If x is.... at t, then it is exploding at t'. (We should not

1. We shall see later that dispositions are not the <u>only</u> species of relational power.

be misled by the fact that the term 'explosive' is sometimes used to denote a relational power, such as the disposition to be detonated by a spark or jolt.) This fact, incidentally, is a further reason for rejecting a semantic analysis of dispositionality in terms of non-material conditionals - simply because such an analysis is quite unable to cope with properties like explosiveness, despite the fact that these are evidently closely related to dispositional properties. By appealing to the more fundamental notion of 'power' we are able account for this circumstance - namely, in terms of the distinction between relational and non-relational powers.

But what is this notion of 'power'? The moral which we should draw from the fact that a reductive analysis of dispositionality is impossible is that we should admit to our ontology something more than the merely 'actual'. What we must recognise is a fundamental and irreducible distinction between the 'actual' and the 'potential' - between what a thing is actually doing and what it 'can' do, or is 'capable' of doing. Actuality and potentiality are distinct and mutually irreducible 'modes of being': we should not think of potential existence as being less 'real' or more shadowy than actual existence. All this was, of course, said long ago by Aristotle.[1] As he pointed out, it is futile to seek to define the difference between the actual and the potential: rather, we can only grasp the distinction in general from a consideration of particular cases.[2]

The belief that only the 'actual' is real has a long history in philosophy. This belief ('actualism', as M. R. Ayers has called it[3]) implies that it is only true to say that a thing 'can' do this or that when it is actually doing it. That such a view leads to manifest absurdities was pointed out somewhat scornfully by Aristotle,[4] but his strictures have often gone unheeded. To say that a thing 'can' do this or that is not to say anything whatever about what it is actually doing. Nor is it to say anything whatever about what it actually has done (even though what a thing has done certainly is relevant to the question of what it can do - a point which we will discuss more fully later).

I am suggesting, then, that the dispositional properties of things

1. See Aristotle (1), Bk. θ, passim.
2. See Aristotle (1), 1046a, 1046b.
3. See Ayers (1), p. 6.
4. See Aristotle (1), 1046b.

should be regarded as potentialities or powers, which are quite as 'real' as their activities and other 'occurrent' properties. Now, that the powers of a thing are exceedingly important - even to the point of determining what kind of thing it is - has frequently been observed.[1] This, indeed, is why Quine's suggested analysis of dispositionality fails. He attempts to define a 'water-soluble' thing as the sort of thing which belongs to the same natural kind as the things which actually dissolve in water.[2] But since dispositional properties are themselves largely definitive of natural kinds, Quine's analysis is doomed to circularity. (And this indicates once again the futility of attempting to define the potential in terms of the actual.)

That powers are as a matter of fact largely definitive of natural kinds has, as I say, frequently been observed. What has less often been observed, however, is that this state of affairs is not purely contingent, or a matter of pragmatic convenience: for it is arguable that we could not operate at all with the concept of an objective world of reidentifiable particulars if natural kinds were not defined with reference to potentialities.

First of all, it must be recognised that particulars cannot be identified independently of some system of classification into kinds: a thing is identifiable as the thing it is at least in part on account of the kind of thing it is.[3] However, in a world which changes, that is, which contains particulars which remain self-identical over a period of time and which are at least in principle reidentifiable, there must be some restrictions of a non-logical sort upon the changes which any given particular may undergo and still remain the same kind of particular as before. (By restrictions of a 'non-logical' sort, I mean restrictions other than those provided by the logical laws of excluded middle and non-contradiction.) Without such restrictions, reidentification would be impossible. Now, what such restrictions imply is that for a particular of any given kind there must be certain changes which it can undergo, and certain changes which it cannot undergo. However, as we noted in an earlier chapter, to say that a particular is undergoing a certain 'change' is simply to say that it

1. Notably by Locke: see Locke (1), Bk. II, Chap. XXIII, para. 7-10. Also by Aquinas: see Anscombe and Geach (1), pp. 101 ff.
2. See Quine (3), pp. 130 ff.
3. Cf. Wiggins (1), p. 69.

is exhibiting a certain 'activity' - or, in common parlance, that it is 'doing something' (e.g., if it is changing from being soft to being hard, it is 'hardening', and if it is changing from being hot to being cool, it is 'cooling'). Thus, what we have shown is that in an objective world of reidentifiable, persisting particulars, these particulars must be classifiable into different kinds which are distinguished largely in terms of what particulars of these kinds can and cannot do, that is, in terms of their potentialities or powers. If a particular could change in any way logically possible (i.e. in any way not resulting in its becoming both A and not-A at the same time), we could not distinguish particulars into different kinds, so particulars would not be re-identifiable, and the conception of an objective world would be impossible.[1]

Philosophers of phenomenalist leanings might well doubt the cogency of this argument, imagining that an objective world of particulars could be conceived in which the various different kinds of particulars were distinguished purely in terms of differing occurrent properties, such as colour and shape and texture. But they would be mistaken in imagining that this could adequately describe a changing world of reidentifiable particulars: for once change is admitted, we must place some form of restriction on the changes which a particular of any given kind may undergo. It will thus not do to differentiate kinds purely in terms of colour and shape and so forth: we must also specify how a particular of a given kind may change in colour or shape etc. The phenomenalist is therefore restricted to an unchanging world. But the notion of an unchanging world is probably incoherent, is certainly not our conception of the world, and is undoubtedly incompatible with the concept of a conscious, percipient being - for the concept of a conscious percipient is not independent of the concept of action.[2]

This, then, is probably the nearest we can get to a 'transcendental' argument for the necessity of our distinguishing different kinds of particulars in terms of their 'powers'. Certainly, I think this argument indicates the absolutely fundamental and irreducible character of the notion of 'power' in our common-sense ontology of persisting material objects, even if it does not prove that no other

1. On the connection between reidentifiability and objectivity, see Strawson (2), Part I, passim.
2. Cf. Hampshire (1), pp. 45 ff.

ontological scheme is workable.

A thing's powers, we have said, are largely definitive of the natural kind to which it belongs. Another way of putting this (in scholastic or Aristotelian terminology) is to say that a thing's powers are essential properties. The 'essential' properties of a thing are those which it cannot lose and remain the same kind of thing. Powers, however, are not the only essential properties of things (even if they are the most important of them): thus in the case of many things shape is also essential. Contrasted with a thing's essential properties are its accidental properties (or 'accidents'), and principal amongst these are (1) the activities of a thing which correspond to its powers, and (2) the spatial relations of a thing with other things. It is obvious why the activities of a thing which correspond to its powers are non-essential: it is precisely because the corresponding powers are essential. Thus, precisely because water-solubility is an essential property of a piece of salt, the actual dissolving of the salt at a given moment is 'accidental' to its nature: if it were not accidental, the salt would always be in a state of dissolution, and it would therefore be meaningless to call it 'water-soluble'. In short, if a particular, x, has a certain power, $\emptyset$, it cannot be essential to the nature of x that $\emptyset$ should be actualised (and, equally, it cannot be essential to the nature of x that $\emptyset$ should not be actualised).

We are now in a position to understand more clearly the connection between dispositionality and conditionality. Dispositions, we have said, are relational powers and therefore are manifested as relational activities. Relational activities, as we have seen, are denoted by transitive verbs - such as 'dissolving', 'hitting', 'bending', 'burning', and 'squashing'. (Some of these verbs also have intransitive forms, of course.) When describing a situation in terms of such activities, we distinguish between agent and patient (e.g. we speak of water (agent) dissolving salt (patient)). And, corresponding to such activities, we ascribe to the agent an 'active' power and to the patient a 'passive' power (e.g. we ascribe the property of being a salt-solvent to water, and the property of water-solubility to the salt). It is, of course, necessary for the actualisation of a given 'passive' power that a

suitable agent should exist (the same holding, mutatis mutandis, for the actualisation of an 'active' power). If no water existed, no salt would ever actually be dissolved by water, even though it might still possess the appropriate 'passive' power. However, it cannot be sufficient for the actualisation of a given passive power that a suitable agent should merely exist: for if that were the case, salt in Oxford would dissolve merely because water exists on Mars. It is evident that a necessary condition of the actualisation of a relational power must be that agent and patient should be related in some way. Moreover, this relation must be accidental - for if it were essential the power would always be actualised (which, as we have remarked, is contrary to the nature of a power). Furthermore, the relation must be relatively specific (or at least there must be a restricted class of suitable relations), since if any accidental relation sufficed, the power would, again, always be actualised. We have thus come to the conclusion, from relatively apriori considerations, that it must be in the nature of a relational power only to be actualised when the possessor of that power is related in some specific and accidental way with some other specifiable kind of object.

It is easy to see how this begins to explain the connection between dispositionality and conditionality. We saw earlier that the disposition-statement 'x is water-soluble at t' entails the non-material conditional 'If x is in water at t, then it is being dissolved by water at t'. The antecedent of this conditional, however, simply refers to a certain specific but accidental relation between x and a body of water (viz. the spatial relation of 'immersion'), while the consequent refers to the actualisation (or 'manifestation') of the dispositional property. This is precisely what we might expect from the considerations of the previous paragraph, and tends, I think, to vindicate my suggestion that of dispositionality and conditionality the former is the prior concept.[1] However, there are certain further features of non-material conditionals which I shall discuss in the next chapter.

It should be remarked, incidentally, that the character of the accidental relation which is appropriate to the actualisation of a

1. It also explains why non-relational power-statements do not appear to entail specific non-material conditionals.

given relational power is by no means logically independent of the character of that power: rather, it is as if the power itself determines what sort of conditions are appropriate to its actualisation (which should hardly surprise us). This is illustrated by the fact, which we noticed earlier, that x's being dissolved by water at t actually entails that x is in water at t. Our concept of 'dissolving' is simply of a certain kind of interaction between a liquid and a solid immersed in that liquid. (We have no concept of 'dissolving at a distance'.) Consideration of other dispositional properties will disclose that this feature is not peculiar to the case of solubility. Consider, for example, the property 'malleability'. When we call a piece of lead 'malleable', we imply that if it is struck by a heavy object, such as a hammer (in Latin, malleus), it will be dented or flattened by that object. But it should be noted that flattening a piece of lead with a hammer is an activity which is not logically distinct from the activity of striking it with a hammer: it is not as though we could use a hammer to flatten lead without striking it. Flattening the lead is striking it (plus something more): 'flattening' entails 'striking' (but not vice versa). This example, incidentally, illustrates another point: the 'accidental' relation which is necessary for the actualisation of a relational power need not be a simple spatial relation, as in the case of solubility - it may, as in the case of malleability, be a relational activity ('striking').

VI : POSSIBILITIES AND POWERS

It is no part of my present purpose to investigate the concepts of logical possibility and necessity, though these are by no means uncontroversial. But I shall often have occasion to refer to the logical modalities for the purpose of contrasting them with other species of possibility and necessity. For it seems reasonable to suggest that the logical modalities constitute merely one species of a generic notion of possibility and necessity.[1] Another species is, of course, constituted by the natural or physical modalities, and these we shall discuss in some detail. It would seem that what chiefly differentiates one species of necessity from another is the field of discourse in which it is characteristically employed.[2] Thus, logical necessity is pertinent to issues of word-meaning and natural necessity is pertinent to issues of physical interaction between objects. Seen in this light, it seems not unreasonable to suppose that there may be a further distinct species of necessity especially pertinent to issues of intentional human action (to the extent that human beings cannot be looked upon as mere physical objects); but of this more later. Yet another species of necessity may be constituted by the ethical 'must' or 'ought' of moral discourse.

It is particularly important that both logical and natural possibility (and, indeed, the other species of necessity to which I have just referred) should be distinguished from what may be called 'epistemic' possibility.[3] The use of this latter concept is illustrated by a statement such as: 'It is possible that the train will arrive late'. It is noteworthy that the correlate of epistemic possibility is not normally expressed as a 'necessity', but rather as certainty: thus the statement just cited is equivalent to 'It is not certain that the train will not arrive late'.[4] That we do employ a quite distinct concept of epistemic possibility may be demonstrated by the following example. A mathematician would be perfectly well understood if he were to say 'It is possible that Goldbach's theorem is true'. Now, clearly, in saying this he cannot be implying anything about physical or natural possibility, since he is talking about a mathematical

1. Cf. Von Wright (2), p. 112.
2. Cf. Toulmin (1), pp. 11-43.
3. See Ayers (1), pp. 12-13.
4. Cf. Ayers (1), p. 55.

proposition. Nor, however, can he be maintaining that Goldbach's theorem is a logically possible proposition: for the theorem, being a proposition of mathematics, is either logically necessary or else logically impossible, and which of these is the case is unknown to the mathematician. Clearly, what he does mean to say is simply that so far no proof has been established either of the truth or of the falsity of Goldbach's theorem. Similarly, when it is said that it is possible that the train will arrive late, what is meant is that as far as we know ('on available evidence') no water-tight argument can be produced to show that the train will arrive on time. Epistemic possibility, then, is concerned with the relations between assertions and the evidence available to support them (by means of rational argument). The evidence in question may well include - indeed, must include - statements mentioning logical or natural modalities (or some other species of modality), simply because rational arguments are founded upon such modalities. This is why we should be so careful to distinguish epistemic possibility from these other modalities. (As a matter of fact, it is misleading to call epistemic possibility a 'modality' at all, in the usual sense.)

I shall now turn to the issue of natural necessity and possibility. For reasons which will become clear shortly, I want to maintain that the natural modalities are relative modalities: that is, that we can never properly speak of a particular state of affairs being absolutely or intrinsically naturally necessary or possible, but only of it being naturally necessary or possible relative to some other specifiable state of affairs. We may, for convenience, introduce the following modal operators, 'N(/)' and 'P(/)' (taking propositions or propositional variables, p, q, r,...., as arguments[1]), to represent natural necessity and possibility respectively.[2] Thus 'N(p/q)' will be read as 'p is naturally necessary relative to, or given, q' or, alternatively, as 'q naturally necessitates p'. ('P(p/q)' may be read as 'q naturally possibilifies p'.)

We may provisionally suggest the following definition and principles (or axioms) to govern the use of these modal operators:

1. It should be understood that suitable propositions are restricted to the physical field of discourse.
2. Cf. Von Wright's system of dyadic modal logic: Von Wright (2), pp. 89 ff.

Def. $N(p/q) = -P(-p/q)$

P(1) $N(p/q) \longrightarrow P(p/q)$

P(2) $N(p/q) \ \& \ N(q/r) \longrightarrow N(p/r)$

P(3) $P(p/q) \longleftrightarrow P(q/p)$

P(4) $P(p \vee q/r) \longleftrightarrow P(p/r) \vee P(q/r)$

P(5) $P(p \ \& \ q/r) \longrightarrow P(p/r) \ \& \ P(q/r)$

P(6) $p \ \& \ q \longrightarrow P(p/q)$

Some remarks on these principles are called for.[1] The definition and P(1) are uncontroversial, since they are common to all systems of modal logic. P(2) states that natural necessitation is a transitive relation (it is also asymmetrical), and this seems plausible enough, though it might conceivably be questioned in some instances. P(3) states that natural possibilification is a symmetrical relation (it is also non-transitive - but not intransitive), which again seems plausible, especially in view of the plausibility of P(6), which states that if p and q are both, as a matter of fact, true, then they are naturally possible relative to each other (which corresponds to the old principle ab esse ad posse valet consequentia). P(4) requires no discussion. The reason why P(5) states a one-way entailment is, of course, simply that from $P(p/r) \ \& \ P(q/r)$ we cannot infer $P(p/q)$, and if p is not possible relative to q, p and q cannot be possible relative to r.

From these principles it is possible to derive various theorems which seem to confirm the intuitive plausibility of the system. For example (I omit proofs):

T(1) $N(p/r) \ \& \ N(q/r) \longleftrightarrow N(p \ \& \ q/r)$

T(2) $N(p/r) \vee N(q/r) \longrightarrow N(p \vee q/r)$

T(3) $N(p/q) \vee N(p/r) \longrightarrow N(p/q \ \& \ r)$

T(4) $P(p/q \ \& \ r) \longrightarrow P(p/q) \vee P(p/r)$

Natural necessitation should not be looked upon as being somehow 'weaker' than logical necessitation: rather, it is quite different in kind. In particular, it would be wrong to imagine that if p entails q, then q must be naturally necessary relative to p.[2] It is easy to see

1. It may be noted that they are rather different from Von Wright's.
2. For this view, see, e.g., Lewis (1), p. 8.

that such a view leads to absurdities. For example, suppose we were to write:

'The cow jumped over the Moon at t' = p
'The cow jumped at t' = q,

then, clearly, p entails q. But if it were also the case that:

$$(p \longrightarrow q) \longrightarrow N(q/p),$$

then it would follow that:

$$(p \longrightarrow q) \longrightarrow P(p/q) \qquad \text{(By P(1) and P(3)).}$$

In other words, it would follow that the cow's jumping at t naturally possibilifies its jumping over the Moon at t, which is plainly quite absurd.

Now, so far all we have is a purely formal modal calculus. In order, however, to give it content as a logic of natural modality, we must give it some sort of 'interpretation' in terms of physical interactions between material objects (since this is the field of discourse of the natural modalities). For this purpose the obvious suggestion is to appeal to the dispositional properties or relational powers of such objects. This, indeed, is why I suggested that the natural modalities are essentially relational - namely, because it seems that our concepts of natural necessity and possibility are ultimately derived from our acquaintance with the relational powers of common physical objects. That this is indeed the case seems to be confirmed by the fact that if we are asked to explain what we mean by 'necessitation', we invariably find ourselves using words like 'compelling', 'forcing', 'making', 'inducing', 'constraining' and 'restricting', all of which are primarily employed to denote certain kinds of relational activity between material objects endowed with the appropriate 'active' and 'passive' relational powers.

The natural suggestion is, then, that every disposition-statement

entails a specific statement of relative natural necessity (though not every statement of relative natural necessity need entail a specific disposition-statement[1]). Thus if we write, as before:

'x is water-soluble at t' = p
'x is in water at t' = q
'x is being dissolved by water at t' = r,

then the suggestion is that the following logical equivalence obtains:

$$p \longleftrightarrow N(r/q).$$

In other words, we are proposing that 'x is water-soluble at t' is logically equivalent to 'Given that x is in water at t, it is naturally necessary that x is being dissolved by water at t' or, alternatively, to 'x's being in water at t naturally necessitates its being dissolved by water at t'. (If we use the latter mode of expression, however, we must be careful to remember that the statement in question in no way implies that x is actually in water at t: as I remarked in the previous chapter, to ascribe a power to an object is not to imply anything whatever about what that object is actually doing at the time in question.[2])

The statement of relative natural necessity 'N(r/q)' may also be looked upon as an interpretation of the non-material conditional 'If q then r',[3] in the sense that the following logical equivalence obtains:

$$\text{If } q \text{ then } r \longleftrightarrow N(r/q).$$

This would account for the commonly held belief that the non-material conditional differs from the material conditional chiefly in implying some sort of consequential relation between the protasis and the apodosis. It would also account for the related belief that the non-material conditional is stronger than (entails) the corresponding material conditional: for it is easy to see that the statement of

1. This asymmetry is connected with the fact that natural necessitation is a transitive relation. I shall return to this point later.
2. We shall see later that some modal statements do carry implications about the actual conditions of the relevant material objects.
3. This analysis is, of course, only adequate for those non-material conditionals which are employed in the physical field of discourse.

relative natural necessity 'N(r/q)' must itself entail the corresponding material conditional, the proof of which is as follows:

(i)	$q \,\&\, -r \longrightarrow P(-r/q)$	(By P(6))
(ii)	$-P(-r/q) \longrightarrow -(q \,\&\, -r)$	(from (i) by contraposition)
(iii)	$N(r/q) \longrightarrow q \supset r$	(from (ii) by Def.)

Q. E. D.

This, incidentally, serves to explain a point I made at the beginning of the previous chapter. We noticed that a dispositional predicate like 'soluble' seems to mean, in some sense, '<u>able</u> to be dissolved', so that a 'water-soluble' substance is simply one that '<u>can</u> be dissolved by water'. But I rejected the obvious suggestion that the 'can' here simply denotes a physical or natural <u>possibility</u>. The reason for this is now plain: if 'x is water-soluble at t' were logically equivalent merely to a statement of relative natural <u>possibility</u>, that is, if it were the case that:

$$p \longleftrightarrow P(r/q),$$

then it would not entail the material conditional $(q \supset r)$ and hence could not entail the non-material conditional (If q then r) as is required.

In suggesting, as I am doing here, that the distinctive features of non-material conditionals may be accounted for if they are interpreted in terms of statements of relative natural necessity, I am merely reiterating, in effect, my suggestion of the previous chapter, that of dispositionality and conditionality it is the former which is the prior concept (contrary to the assumptions of most philosophers[1]). It has always been acknowledged that the concepts of dispositionality, conditionality and natural necessitation are all closely related, but the distinctive feature of the present account is that it takes the first member of this trio (dispositionality) as fundamental, and interprets disposition-statements as predicating 'real' properties (namely, 'relational powers') of material objects.

1. Interestingly enough, Bradley appears to have been an exception to this rule: see Bradley (1), Bk. I, Chap. II.

However, while it seems that dispositionality is the prior concept, it is frequently very convenient to discuss physical interactions in terms of the natural modalities, since in this way we are better able to abstract from particular instances and so achieve a high degree of generality (and this is where a symbolic modal calculus is especially helpful). According to the present account, whenever we say that some particular physical state of affairs is 'naturally necessary' or 'naturally impossible', we imply - without, however, being at all specific - that the objects involved are endowed with certain powers and liabilities in terms of which the occurrence or non-occurrence of this state of affairs might be explained. The advantage of modal discourse is that it obviates reference to <u>specific</u> powers and liabilities: it enables us to hint at explanations without having to articulate them in detail. For example, when it is alleged that it is 'impossible' that a conjuror should really saw a woman in half and subsequently produce her alive and well, it is <u>suggested</u> - without, however, any <u>specific</u> argument being developed - that human bodies are so constituted that the process of sawing them in half results in death. Only a biologist could explain in detail why this is so. In saying that the conjuror <u>must</u> be performing a trick, we are simply affirming our confidence that such a biological explanation exists.

We are now in a position to understand a further distinction between kinds of power or potentiality. (We have already distinguished between relational and non-relational powers.) I have proposed that a disposition-statement, such as 'x is water-soluble at t', is logically equivalent to a statement of relative natural necessity. But it would seem that there is a class of relational power-statements which do <u>not</u> entail statements of relative natural necessity, even though they do entail statements of relative natural <u>possibility</u>. I am thinking in particular of the sort of statements used to express the <u>capacities and capabilities</u> of human beings to perform <u>intentional actions</u>: for example, the statement 'A is capable of playing (can play) the piano at t'.

'Playing the piano' is, of course, a relational activity, so that the ability to play is a relational power. But it is not at all

evident that the statement just cited entails any specific conditional of the form 'If A is...., then he is playing the piano'. Rather, if it entails a conditional at all, it must be one something like the following: 'If A is seated at the piano, with his hands free etc., then he may be playing the piano'. Suppose we write:

'A can play the piano at t' = p
'A is seated at the piano, etc. at t' = q
'A is playing the piano at t' = r,

then the ability to play the piano is similar to a dispositional property like water-solubility in that the following two entailments seem to obtain:

$$r \longrightarrow p$$

(if A is playing the piano, then he can play it), and

$$r \longrightarrow q$$

(if A is playing the piano, then he is seated at it etc. - because this is part of what we mean by saying that someone is 'playing the piano'). However, the ability to play the piano is dissimilar to a disposition in that we apparently cannot assert that:

$$p \longrightarrow N(r/q).$$

This seems to be true however carefully we specify the 'accidental' conditions (which are covered by the 'etc.' in proposition 'q'): there appears to be no set of such conditions sufficient to necessitate the 'manifestation' of A's ability to play. All that we can apparently assert is that there is a set of conditions which possibilify but do not necessitate A's actually playing, that is, that:

$$p \longrightarrow P(r/q) \;\&\; -N(r/q).$$

This fact is, of course, relevant to the problem of 'Free Will', to which I shall return in a moment.

Now, many philosophers of 'behaviourist' leanings have tended to assimilate the capabilities of human beings to perform intentional actions to the dispositional properties of physical objects.[1] Our foregoing observations suggest that this assimilation is mistaken, and that there is an important distinction to be made here.[2] It may be, of course, (though this does not, at present, seem at all likely) that with advances in psychophysiology we shall one day be able to eliminate this distinction (by discovering a set of 'accidental' conditions - perhaps including certain 'brain-states' - which is sufficient to necessitate the manifestation of such a capability). But it cannot be doubted that the way we speak of human powers at present is distinctly different from the way we speak of the powers of inanimate physical objects. This is why I suggested earlier that if there is a notion of necessity appropriate to the field of intentional action, it is distinct from the notion of natural or physical necessity.

It has sometimes been maintained, for instance, that if a person has ability, opportunity and desire (or intention), then he must act.[3] According to this view, if A not only can play the piano (p) and is seated at the piano etc. (q), but also wants to play the piano (s), then he must be playing the piano (r). That is:

$$p \,\&\, q \,\&\, s \longrightarrow r.$$

However, even if this is true, it would be most implausible to suggest that it is true in virtue of the capability-statement entailing a statement of relative natural necessity - that is, in virtue of:

$$p \longrightarrow N(r/q \,\&\, s).$$

For, quite apart from anything else, it is by no means clear that the statement 'A wants to play the piano at t' (s) expresses any sort of physical state of affairs, in the way that 'A is seated at the piano etc.' evidently does. Again, it would be highly speculative to suggest that

1. See, e.g., Ryle (1), pp. 112 ff.
2. Cf. Aristotle's distinction between rational and non-rational potencies: Aristotle (1), 1046b, 1048a. Cf. also Ayers' distinction between natural powers and personal powers: Ayers (1), pp. 102 ff.
3. See, e.g, Aristotle (1), 1048a.

advances in psychophysiology will one day establish that 'wanting' or 'intending' is identifiable with some particular 'brain-process'.

However, the implausibility of identifying 'intending' with a brain-process has suggested to some philosophers another way of explaining the above-mentioned entailment ($p \,\&\, q \,\&\, s \longrightarrow r$).[1] Perhaps 'intending' is not any kind of 'process' at all (not even a 'mental' one): perhaps it is 'reductively eliminable' by means of some such logical equivalence as the following:

$$s \longleftrightarrow (p \,\&\, q \supset r).$$

However, a reductive analysis of intention is no more adequate than is a reductive analysis of dispositionality, and for the same reason: according to the logical equivalence just suggested, we should have to say that A 'intends' or 'wants' to play the piano just so long as he cannot play it or does not have the opportunity to play it. This, however, suggests another line of analysis. The entailment which we are trying to explain ($p \,\&\, q \,\&\, s \longrightarrow r$) could also be accounted for if we supposed that:

$$s \longleftrightarrow N(r/p \,\&\, q).$$

This would make 'intending' or 'wanting' a disposition (and, moreover, a 'second-order' disposition, since it would be defined with reference to a capability, which is itself a non-'occurrent' attribute). But this suggestion, too, is not very convincing. For, while it is undoubtedly the case that many 'intentional' actions are not preceded by any identifiable mental act of 'intending' or 'wanting',[2] it does sometimes seem something rather like this occurs, which seems to rule out a purely dispositional analysis of intention.

Altogether, then, it seems that the entailment '$p \,\&\, q \,\&\, s \longrightarrow r$' (supposing it to obtain) is not to be explained in terms of natural necessity. Thus, while it would be wrong to multiply the species of necessity unduly, there does seem to be a case for speaking of a third kind of necessity, distinct from both logical and physical necessity,

1. For fuller discussion, see Von Wright (3), chap. 3.
2. Cf. Anscombe (1), passim.

and especially appropriate to the field of intentional action. This kind of necessity would be exemplified in such statements as 'I must catch the eight o'clock train this morning' and 'He can't put that book down'. It is quite evident, for example, that the latter statement is quite different in kind from a statement of natural necessity, such as 'He can't lift that rock'. Again, as I remarked earlier, the 'must' or 'ought' of ethical discourse (e.g., 'You must keep your promise') may constitute yet another distinct species of necessity.

I do not really want to enter the 'Free Will' debate here, but a few remarks relevant to this issue may be in order. Some philosophers have argued that the key to this problem lies in the distinction between 'actions' and mere 'bodily movements': an 'action', such as opening the window, may well involve a bodily movement, but is not merely a bodily movement (as is shown by the fact that one and the same bodily movement may be 'interpreted' as a number of different possible 'actions'). It is then argued that neurophysiologists could at best only give a causal explanation of bodily movements (in terms of processes occurring in the brain and muscles), so that our 'actions' are 'uncaused' simply because causal explanations are quite inappropriate to the field of 'action'.[1] However, while the distinction between 'action' and 'bodily movement' is valid and important, it is not, I think, directly relevant to the 'free will' issue. For if all our movements can, ultimately, be given a causal explanation by neurophysiology, the distinction in question would leave us only with the merest shadow of 'freedom'.[2] Thus if, when a person closes the window, the bodily movement involved is completely determined by neurophysiological processes, his 'freedom' would apparently merely consist in the fact that this movement might be 'interpreted' in various different ways as an 'action' (e.g., as closing the window, or as excluding a draught, or as securing the house against burglars). But, surely, genuine 'freedom' must involve freedom of movement as well as of 'action'.

While not attempting to offer a solution to the 'free will' problem, I must say that it seems to me that this problem centres around the question of whether human beings are just very complicated physical objects: and this turns, at least in part, upon the question of whether

1. See, e.g., Melden (1), pp. 184 ff. The connection between 'causal' explanation and dispositional properties will be clarified in the next chapter.
2. Cf. Lucas (1), pp. 17 ff.

the distinction between human 'capabilities' (such as the ability to play the piano) and the dispositional properties of material objects (such as water-solubility) can ultimately be eliminated by advances in neurophysiology.

However, it may not only be the powers of human beings which differ (apparently) from ordinary relational powers like water-solubility in not entailing statements of relative natural necessity. If adherents of the so-called 'propensity' theory of probability are correct, certain inanimate objects also display such powers.[1] This, besides complicating the 'free will' issue, brings to light a certain difficulty in the account of dispositionality which I have been advancing.

I suggested some time ago that 'x is being dissolved by water at t' (r) entails 'x is water-soluble at t' (p), and this certainly seems very plausible. But if it is also the case that:

$$p \longrightarrow N(r/q)$$

(where q = 'x is in water at t'), how can the former entailment ($r \longrightarrow p$) really obtain? Surely, all we are entitled to infer from r is P(r/q), and for the following reason:

(i)	$r \longrightarrow q$	(assumed)
(ii)	$(r \longrightarrow q) \longrightarrow (r \longrightarrow r \,\&\, q)$	(theorem of prop. calc.)
(iii)	$r \longrightarrow r \,\&\, q$	(from (i) and (ii))
(iv)	$r \,\&\, q \longrightarrow P(r/q)$	(by P(6))
(v)	$r \longrightarrow P(r/q)$	(from (iii) and (iv))

Q. E. D.

That is, all we can apparently infer from 'x is being dissolved by water at t' is that x's being in water at t <u>naturally possibilifies</u> (rather than naturally necessitates) x's being dissolved by water at t.[2]

As far as it goes, this argument seems water-tight. And yet, is it not absurd to suppose that x's being dissolved by water does <u>not</u>

1. For the 'propensity' theory, see Mellor (2) and Popper (3).
2. Mellor himself denies that disposition-statements are entailed by the relevant 'display'-statements for this very reason, viz. because the 'display' <u>may</u> only indicate a mere 'propensity': see Mellor (1), p. 178.

imply that x is water-soluble (in the accepted sense of 'water-soluble', in which the disposition-statement entails the non-material conditional)? It is easy to explain why we feel this to be absurd. It is acknowledged that:

$$r \longrightarrow P(r/q),$$

but on what conceivable grounds would we be inclined to assert:

$$P(r/q) \ \& \ -N(r/q)?$$

As we have seen, assertions of this sort are normally reserved to describe the capabilities of human beings to perform intentional actions. The reason, then, why we feel it would be absurd to suggest that it might be possible but <u>not</u> necessary for an inanimate object, such as a piece of salt, to dissolve when immersed in water, is that this suggestion seems to liken the object to a <u>human agent</u>: as if the salt could consciously <u>choose</u> whether or not to dissolve on any given occasion. If salt <u>can</u> be dissolved by water, why should it <u>not</u> be dissolved by it when they come into contact?

It may be retorted that our feeling that this is absurd is purely subjective and irrational. This, at any rate, is what a proponent of the 'propensity' theory might maintain. And yet, as far as is known, the only inanimate objects to which a propensity view really seems appropriate belong to the esoteric field of atomic physics. (The propensity theory does <u>not</u> seem appropriate to such objects as dice and roulette-wheels, which apparently behave in a perfectly 'deterministic' manner.) It should, furthermore, be borne in mind that our very concept of natural necessity is derived from our experience of the physical interactions of macroscopic inanimate objects. so that dispositional properties like solubility, malleability and so forth are in fact <u>paradigmatically</u> exemplary of what we mean by 'natural necessitation'. Altogether, then, I do not think that it is mistaken to suppose that a relational activity-statement concerning an inanimate object (such as 'x is being dissolved by water at t')

implies the corresponding disposition-statement (such as 'x is water-soluble at t'). I say 'implies' rather than 'entails' because I think that the argument which I outlined earlier is strictly speaking correct. But it should be understood that the 'implication' of which I am speaking, even if it falls short of entailment, is very strong: it is, for example, quite as strong the relation between a material object-statement (such as 'x is an apple') and all the various 'predictive' consequences 'implied' by such a statement.[1]

It may be remarked, indeed, that the contrary view - that a relational activity-statement in no way implies the corresponding disposition-statement[2] - savours of the scepticism which we have already met in one or two other contexts. It is typical of such scepticism that the real possibility of error or doubt on particular occasions is inflated into an imagined possibility of general and systematic error or doubt. Such inflated views are always incoherent. Thus, in the present case, it is arguable that the suspicion that all the powers of material objects might merely be 'propensities' is incoherent. This follows from the 'transcendental' argument which I put forward in the previous chapter. According to that argument, it is not a purely contingent matter that we distinguish particulars into different kinds largely in terms of what they can and cannot do. If all powers were 'propensities', however, we could only speak of what a thing can do, and never of what it cannot do. For to say that a thing cannot act in a certain way is to say that it is naturally necessary (not merely naturally possible) that it should not act in that way. Thus when we say that diamonds are water-insoluble we imply that it is naturally necessary that diamonds should not be dissolved by water. But it is clear that we need the notion of what a thing cannot do, as well as the notion of what it can do, in order to distinguish particulars into different kinds: for if we could never be sure that a thing could not behave in certain ways, we could never be sure that two apparently quite disparate things were not really of the same kind. Thus we could never be sure that diamonds might not one day display all the properties of salt, or that chalk might not become cheese. What all this means, briefly, is that a completely 'indeterministic' world is

1. As we remarked in an earlier chapter, the 'implications' of a material object-statement are far more extensive than the set of statements which it actually entails.
2. I do not wish to imply that proponents of the 'propensity' theory actually hold this view, however.

unthinkable - or, in other words, the concept of such a world is incoherent.

The foregoing discussion brings us to another important issue: that of the perceptibility of powers. It is frequently denied that powers are perceptible[1] - indeed, the contrary view is scarcely ever voiced. We have already seen how strong this scepticism was in the early empiricists, such as Locke and Hume. But it is hard to see on what grounds this opinion can be maintained, so long as it is acknowledged (1) that the 'display' or 'manifestation' of a power implies the possession of that power (e.g. that 'x is being dissolved by water at t' implies 'x is water-soluble at t') and (2) that we can perceive the activities of material objects (both relational and non-relational). For, given these two facts, it seems evident that simply in perceiving the manifestation we are perceiving the power. Thus, we may perceive that a piece of salt is soluble simply in seeing it being dissolved; and we may perceive that a piece of lead is malleable simply in seeing it being flattened. Of course, a power cannot be perceived when it is not being manifested: but this is not to say that it cannot be perceived at all.

It might be objected here that while it may be true to say that we may perceive that a piece of salt is water-soluble, it is incorrect to say that we may perceive the water-solubility 'itself'. However, I think that the distinction between 'perceiving' and 'perceiving that' is a spurious one. Surely, the source of this supposed distinction is the fact that perception-verbs such as 'see' and 'hear' are normally used transitively, taking nouns or noun-phrases or noun-clauses as their objects, whereas attributes, including powers, are normally expressed adjectivally. Thus one says 'I see the lead', but 'I see that the lead is malleable' (rather than 'I see the malleability of the lead'). But this is not peculiar to powers. One also says 'I see that the ball is red' in preference to 'I see the redness of the ball'. But no-one is going to advance this as a reason for saying that we do not really perceive the redness 'itself'. The only truth in the notion that we cannot perceive a power 'itself' is the trivial one that a power, since it is an attribute, is not a particular - is

1. See, e.g., Ayers (1), pp. 59-62. Apart from this point, I am largely in agreement with Ayers' account of powers.

not an 'it'. (Perhaps it seems less odd to say that we can perceive redness 'itself' simply because the 'sensible qualities', such as colours, are sometimes reified as 'phenomena', whereas powers are apparently never reified.)

We may now finally move on to discuss a curious objection raised by D. H. Mellor against the view that the dispositional properties of objects are potentialities or powers. According to Mellor, "dispositions are real properties in a sense that rules out any account of them as mere potentialities or possibilities.... A fragile glass is one that does break (if dropped), not one that can break. Whether it can break depends inter alia on whether it can be dropped, and its being fragile entails nothing about that."[1]

There seem to be a number of confusions here. Mellor is right to deny that a disposition like fragility is a 'mere potentiality' if, as appears to be the case, he equates potentiality with mere natural possibility. Thus, when he says that a fragile glass is "one that does break (if dropped), not one that can break", he appears simply to be making the point that a dropped glass (assuming it to be fragile) must break. However, this merely shows that a disposition like fragility is not equivalent to a mere relative natural possibility (a point which we noticed earlier), not that dispositions are not potentialities or powers. It is not as though Mellor has obviated our appeal to the notion of potentiality in saying that a fragile glass is "one that does break (if dropped)", since here he is merely appealing to the non-material conditional, and the very problem at issue is the meaning and ontological implications of such conditionals. Can we perhaps detect the remnants of a prejudice towards 'actualism' in what Mellor says here?

However, what Mellor says next is rather puzzling. He asserts (1) that whether a glass can break depends on whether it can be dropped, and (2) that a glass's being fragile entails nothing about whether or not it can be dropped. But both of these assertions involve more complications than Mellor appears to realise, and neither is unrestrictedly true. I shall discuss (2) first.

If one means, in saying that a particular glass can be dropped,

1. Mellor (1), p. 173.

simply that this glass is by nature a droppable object, then it would seem that, pace Mellor, saying that the glass is fragile does entail saying that it 'can be dropped'. For it would hardly be proper to say of any particular glass that it is by nature at once fragile and undroppable: for its undroppability would entail that it would never actually break, and, as we argued earlier, an object cannot possess a power $\emptyset$ if it is essential to the nature of that object that this power should never be actualised.

However, in normal usage, to say that a glass 'cannot be dropped' would not be to assert that it is by nature an 'undroppable' object. Rather, it would be to imply (a) that the glass is actually in a certain situation S (e.g. it might be locked up in a cupboard) and (b) that relative to this situation S it is naturally impossible that the glass should be dropped. Now, it might indeed be suggested that to assert (a) and (b) is still in some sense to assert that the glass is 'undroppable'. But the point is that the glass is not, in such a case, asserted to be undroppable qua 'glass', but only undroppable qua 'object in situation S'. And this is a very different matter, since it is not essential to the glass that it should be in situation S (this is purely accidental: the glass just happens to be locked up in the cupboard). In this sense of 'cannot be dropped' what Mellor says is perfectly true: that the glass is fragile entails nothing whatever about whether it can or cannot be dropped, since it entails nothing whatever about what accidental situation the glass may be in.[1]

We may now turn to Mellor's assertion (1) cited above, namely, that whether a glass can break depends on whether it can be dropped. If by 'can break' and 'can be dropped' are meant respectively 'is fragile' and 'is droppable' then, as we have seen, whether a glass can break (is fragile) does indeed depend on whether or not it can be dropped (is droppable). But we have also seen that there is another sense in which no such dependency obtains, because it need not be inconsistent to say of a particular glass both that it can break and that it cannot be dropped. This is so if by saying that the glass 'can break' we are simply making a statement of relative natural possibility, to the effect that relative to some specifiable state of

1. This is simply the point that when we ascribe a power to an object we are not normally implying anything about the actual state of that object. The only exception to this rule is in the case of 'accidental' powers, which I shall discuss shortly.

affairs (such as, of course, its being dropped) it is naturally possible that the glass should break, while by saying that the glass 'cannot be dropped' we mean that the glass is actually in some situation S which is such that relative to S it is naturally impossible that the glass should be dropped. To say that the glass 'cannot be dropped' in this sense implies, indeed, that the glass will not actually be dropped, and hence will not actually break. But to assert this is quite consistent with the statement that relative to some hypothetical state of affairs[1] (such as the glass's being dropped), it is naturally possible that the glass should break, since the truth of this statement in no way depends upon whether or not the glass is ever actually dropped.

One final point: I have suggested that a fragile glass cannot be undroppable qua 'glass', although it may be undroppable qua 'object in situation S' (e.g. qua 'object locked up in a cupboard'). This example should warn us that not all the 'powers' of an object need be essential properties of that object. An object x possesses an accidental power ϕ if x is accidentally identifiable as the kind of object which possesses ϕ essentially. Thus the glass is accidentally undroppable because it happens to be identifiable as an 'object locked up in a cupboard', and such objects are (essentially) undroppable.[2] Of course, 'objects locked up in cupboards' do not constitute a natural kind: the essential powers of such non-natural kinds are to be accounted for in terms of the essential powers and accidental relations of objects which do belong to natural kinds. Thus the undroppability of objects locked up in cupboards is to be accounted for in terms of the impenetrability and durability of the pieces of wood and metal from which cupboards are constructed - for it is owing to these properties that one is unable to remove an object from a locked cupboard and hence unable to drop it.

1. In calling a given state of affairs 'hypothetical', one is simply neither asserting nor denying the actuality of this state of affairs.
2. This example is, perhaps, a little artificial: but the principle is all that matters.

VII : CAUSE AND EFFECT

Let us consider once again the entailment:

'x is water-soluble at t' & 'x is in water at t'
$\longrightarrow$ 'x is being dissolved by water at t'.

Now this particular entailment is merely one instance of a general type of entailment, in which a proposition ascribing some relational power ϕ to an object x at time t, and a proposition asserting that x is in some accidental relation R to another object y at t, together entail a proposition ascribing to x and y at t the relational activity P which corresponds to ϕ. It is always implicit in such a case that y possesses at t the appropriate complementary power ϕ^* (i.e. if ϕ is the 'active' power, then ϕ^* is the corresponding 'passive' power, and vice versa). Such an entailment might be represented symbolically as follows:

$$\phi x(t) \;\&\; \phi^* y(t) \;\&\; xRy(t) \longrightarrow xPy(t)$$

(It is always legitimate, of course, to substitute 'yP*x(t)' for 'xPy(t)', where 'P*' represents the same activity as does 'P', but expressed in the alternative voice: e.g., if 'P' denotes 'is dissolving', then 'P*' denotes 'is being dissolved by'.)

It may be remarked that the propositions represented by 'xRy(t)' and 'xPy(t)' respectively express what are in common parlance called 'cause' and 'effect'. Thus, x's being in water at t is the 'cause' of its being dissolved by water at t (the 'effect').

Now, in the first chapter we discussed a notion made famous (or notorious) by Hume: the notion of causal inference. This is the notion of a special kind of inference from 'cause' to 'effect'. The general type of entailment described above is not, however, exemplary of 'causal inference' in the Humean sense: in the first place because it is not simply an inference from 'cause' to 'effect' (since the left-hand side of the entailment includes expressions referring to

powers of objects, and a 'power' is not a 'cause'[1]), and in the second place because causal inference in the Humean sense is non-demonstrative and so cannot be exemplified by an entailment. Nevertheless, we may, for convenience, call this general type of entailment a 'causal entailment', provided that we are careful to distinguish this from causal inference in the Humean sense.

Suppose, however, that we introduce the symbol '$\underset{c}{\rightarrow}$' to represent the kind of implication between 'cause' and 'effect'. ('$\underset{c}{\rightarrow}$' may be read as 'causally implies'.) Then the following would appear to represent more or less what Hume had in mind by the notion of causal inference:

$$xRy(t) \underset{c}{\rightarrow} xPy(t).$$

A specific example would thus be:

'x is in water at t' $\underset{c}{\rightarrow}$ 'x is being dissolved by water at t'.

If we recall for a moment the example of 'non-deductive reasoning' quoted from Strawson earlier - 'The kettle's been on the fire for the last ten minutes, so it should be boiling by now' - we can see that this too exemplifies the form of causal inference symbolised above, since we could rewrite Strawson's example something like this:

'The kettle has been on the fire for ten minutes at t'
$\underset{c}{\rightarrow}$ 'The kettle is being boiled by the fire at t'

It is easy enough to see what '$\underset{c}{\rightarrow}$' must mean according to the views which I have been advancing. It simply means that the 'cause' necessitates - but does not entail - the 'effect'. This 'necessitation' is simply natural (as opposed to logical) necessitation. Thus we may define '$\underset{c}{\rightarrow}$' by the following equation:

$$p \underset{c}{\rightarrow} q = N(q/p) \qquad \text{(Def.)}$$

1. Cf. Mellor (1), pp. 179-80.

Of course, as we have seen, statements of relative natural necessity of this kind are entailed by relational power-statements. Thus we have:

$$\phi x(t) \ \& \ \phi^{*}y(t) \longrightarrow N(xPy(t)/xRy(t))$$

and hence:

$$\phi x(t) \ \& \ \phi^{*}y(t) \longrightarrow xRy(t) \underset{c}{\longrightarrow} xPy(t).$$

In view of this the following fact becomes intelligible, namely, that whenever we employ any particular 'causal inference' we do so upon the presupposition that an appropriate relational power-statement is true. Thus, we are prepared to assert that a piece of salt is dissolving, given that it is in water, because we know or believe the salt to be water-soluble; and we are prepared to assert that the kettle is boiling, given that it has been on the fire for the last ten minutes, because we know or believe that fire has the power to make water boil. This, we can now see, is simply because the appropriate relational power-statement actually entails the statement expressing the causal inference.

All this, of course, ties up with what we discovered in the first chapter of this thesis. We saw that Hume considered that all arguments concerning matter of fact - all 'probable' arguments, as he called them - are "founded upon the relation of cause and effect", by which he meant that whenever we infer the occurrence of one event, A, from the occurrence of another event, B, we do so on the presupposition that A and B are causally related. Now, according to our foregoing analysis, to say that one event is the 'cause' of another is, in effect, simply to ascribe certain relational powers to certain material objects. Thus the 'causal propositions' upon which 'probable' arguments are founded, and which we discussed at such length in the first chapter, are simply what we have now come to call 'relational power-statements'.

It will be recalled that we isolated as the key to Hume's scepticism his belief that causal propositions can never be justified.

We can now see that this scepticism was entirely unwarranted. For if, as we are proposing, causal propositions are just equivalent to relational power-statements, then it is clear that even in Hume's terms they can be 'justified'. A relational power-statement is, in Hume's terms, a statement 'concerning matter of fact and existence' and is hence to be 'justified', if at all, either by direct experience or else by inference from direct experience. Now, contrary to Hume's sceptical assumptions, both of these forms of 'justification' are open to us in the case of relational power-statements. For, on the one hand, it appears that we can perceive the powers of material objects; and, on the other, it is clear that there can be no objection to a relational power-statement figuring as the conclusion of an argument 'concerning matter of fact and existence'. Power-statements raise no more problems in these respects than do statements about the 'occurrent' properties of material objects.

Nevertheless, I can well imagine how convinced inductivists might attack the line of thought which I have adopted. Suppose we consider a specific instance of causal inference, such as Strawson's example of the boiling kettle. The inductivist may grudgingly acknowledge that we can perceive the power of a fire to boil a kettle of water. But he still has room for scepticism. We may, he might say, have perceived in the past that this or that particular fire has had the power to boil this or that particular kettle of water, but how can we be sure that the particular fire and the particular kettle of water with which we are now concerned possess the requisite relational powers? Don't we just assume that this is so through a process of inductive extrapolation from our past experience of particular fires and particular kettles of water?[1]

Now this objection, while mistaken in its sceptical implications (as I shall make clear later), does bring out an important point. This is that when we employ a causal inference such as the one about the boiling kettle, even though the inference is technically speaking founded upon a singular causal proposition - namely, a statement ascribing certain relational powers to particular material objects - we obviously do not in general attempt to verify or justify these

1. A. J. Ayer advances just such an objection as this: see Ayer (1), p. 12.

singular causal propositions, but rely instead upon general causal propositions which we regard as being already adequately justified. Thus we do not attempt to discover whether this fire really does have the power to make this water boil: we simply rely upon the general proposition that fire, in general, has the power to make water, in general, boil.[1] The role of general causal propositions in our reasoning about 'matter of fact and existence' is of the greatest importance. But this is a matter which I shall discuss in the next chapter.

Nevertheless, despite the importance of general causal propositions, the fact should not be overlooked that many causal inferences only presuppose particular causal propositions. Thus, in the case of the boiling kettle we could, if we had wanted, have ascertained whether this particular fire had the power to boil this particular water (futile though such a procedure would have been). However, this still seems to leave a loophole for the sceptic. Again he might acknowledge that we could have ascertained that this particular fire had the power to boil this particular water at some time in the past: yet how can we be sure that it still possesses this power, save by actually observing the fire boiling the kettle? But if we are driven to this expedient in order to justify our causal inference, what sense is there in our employing such an inference at all? The whole point of the inference was that it seemed to save us the trouble of actually going into the kitchen in order to know whether or not the kettle was boiling: but if we have to go there anyway in order to justify our use of the inference, there is really no sense in our employing it at all.

Now this objection, unlike the previous one, really has very little of a fruitful nature to offer. For it is really quite incoherent to be in perpetual doubt about whether the particular material objects with which we are acquainted will continue to possess the properties which they currently have. The notion of a world in which this paranoiac sort of doubt were justifiable is quite unthinkable.[2] An objective world of reidentifiable particulars must exhibit a considerable degree of permanence and stability.

The inductivist may allege, of course, that we have only come to

1. Sometimes, however, we do find it necessary to ascertain the powers of particular material objects before making a causal inference: e.g., if I want to know whether this rubber ball will bounce when I drop it, I may have to test its elasticity first.
2. Such doubts are literally paranoiac, in that they are often actually exhibited by unbalanced people.

assume that material objects must display this stability through a process of inductive extrapolation from past experience. But what is this 'past experience' from which we supposedly extrapolate? If our earlier arguments are correct, our experience is simply experience of material objects and their attributes: there simply are no more primitive or elementary 'data' from which any such 'extrapolation' could proceed. We cannot think outside of our categorial framework of material objects.

I am not denying that it is, in some rarefied sense, logically 'possible' that the entire objective universe should suddenly plunge into a nightmarish chaos in which rational thought would be impossible.[1] But this in no way impinges upon the rationality of the procedures which we actually adopt within the scope of our current categorial framework: it does not, in particular, impinge upon the rationality of causal inference. For, as we have seen, it is no accident or matter of pragmatic convenience that we think in terms of the category of cause and effect - in terms, that is, of powers or potentialities. That we should think in this way is quite indispensable to our ability to think coherently about the world at all.

I shall return to the subject of causal inference in the next chapter; the present chapter I shall conclude with some further observations on the use of the terms 'cause' and 'effect'.

There is, of course, a formal analogy between the notion of a 'causal entailment' as described above and the 'deductive-nomological' model of explanation advanced by philosophers like Popper and Hempel. The most important substantive difference lies in the fact that in the D-N model singular power-statements are replaced by statements expressing universal uniformities. This, of course, is intimately connected with the 'regularity' theory of causation. Perhaps the principal advantage of the account in terms of powers is that it obviates appeal to propositions which transcend any possible experience. (Powers are perceptible, if my earlier arguments are correct, but a universal uniformity could only be perceived by God.) In view of this, it is somewhat ironical that such philosophers should pride themselves on being 'empiricists'. There are various other deficiencies in the

1. It is arguable, of course, that this is not even logically possible, in the sense that such a degree of chaos can only be described as state of individual insanity, and not as a state of the 'world'.

Popper-Hempel account which I shall discuss in the next chapter.

As regards the 'regularity' theory of causation itself, a similar point may be made. Perhaps the chief advantage of the present theory of causation is that, unlike the regularity theory, it explains the terms 'cause' and 'effect' in such a way that the fact that some particular event is the 'cause' of another particular event is made to depend purely on the particular circumstances of these events at the time in question, and not in any way upon relations between other events at other times (and let alone upon the past and future history of the entire universe). This, I suggest, is more in keeping with our common-sense conviction that we can often pick out the 'cause' or 'effect' of some event, without the slightest difficulty, from a mere scrutiny of its immediate circumstances.[1]

Many philosophers - some of them adherents of the regularity theory and others not - have attempted to analyse the notions of 'cause' and 'effect' in terms of necessary and sufficient conditions.[2] This kind of approach can be traced back to J. S. Mill.[3] Now, while such an approach is not altogether mistaken, it tends to lead to confusions and unnecessary complications, simply because the general notions of 'necessary' and 'sufficient' conditions are far too unspecific to capture the precise meanings of the terms 'cause' and 'effect'.

A few examples will illustrate the sort of complications which arise when this kind of approach is taken. The most obvious suggestion would be that a 'cause' is simply a sufficient condition of its 'effect', which is in turn a necessary condition of its 'cause'.[4] And, indeed, there is a sense in which this is correct, as we shall see. But such an analysis appears to be upset by an example such as the following. The presence of animals like ourselves on the surface of a planet may clearly in a sense be said to be a 'sufficient' condition of the existence of oxygen in the atmosphere of that planet - yet it would evidently be absurd to suggest that the presence of such animals is a cause of the existence of oxygen in the atmosphere.

It might be suggested that this is because a 'cause' is an antecedent sufficient condition - that is, that a 'cause' must temporally precede its 'effect'.[5] But this will not do, for lightning precedes thunder

1. Cf. Ducasse's theory: see Ducasse (1), chap. 1-4.
2. See, e.g., Mackie (2), chap. 2; Hart & Honoré (1), chap. 5; and Von Wright (3), chap. 2.
3. See Mill (1), Bk. III, chap. V.
4. See, e.g., Nicod (1), p. 193.
5. I shall discuss this question of temporal precedence more fully later.

and is also in a sense a 'sufficient' condition of thunder - but again it would be incorrect to call lightning a 'cause' of thunder.

Again, it often appears that what we do call the 'cause' of some event is in some sense not a sufficient condition of that event. Thus, one might say that A's consuming a certain poison was the 'cause' of his death: and yet this alone was not 'sufficient', in the sense that it was also necessary that A had a certain constitution susceptible to the poison, and his possession of this in turn depended on the existence of various environmental conditions, in the absence of which, indeed, A might never have existed at all. Indeed, ultimately it might be said that only the entire history of the universe up to the moment of A's taking the poison may have been a really sufficient condition of his death, in that that event might never have occurred had other events, even quite remote in time and space, been different. Now, there is evidently something radically wrong with this line of thought: but it is clear that considerations of this sort have led some philosophers to maintain that what we call the 'cause' of an event is merely one member of a large conjunction of conditions which are jointly sufficient for the 'effect', and that our selection of this one member is usually guided by subjective or anthropocentric considerations (e.g., this event may particularly catch our attention or involve some human action, such as taking the poison).[1]

It is one of the advantages of the theory of causation which I have been advancing that it clears up some of these complications and confusions. Let us reconsider the general form of a 'causal entailment', as described earlier:

$$\emptyset x(t) \;\&\; \emptyset^* y(t) \;\&\; xRy(t) \longrightarrow xPy(t).$$

The proposition represented by 'xRy(t)' expresses the 'cause' while that represented by 'xPy(t)' expresses the 'effect'. (This may be regarded as illustrating the fundamental or central meanings of the terms 'cause' and 'effect': it is not intended as a definition, nor does it exclude the possibility of derivative or peripheral meanings.) Now, in view of the fact that:

1. See, e.g., Mill (1), Bk. III, chap. V, sect. 3.

$$\phi x(t) \;\&\; \phi^{*}y(t) \longrightarrow N(xPy(t)/xRy(t)),$$

there clearly is a sense in which the 'cause' is a 'sufficient' condition of its 'effect': namely, the sense in which it 'naturally necessitates' the 'effect'. But it should not be forgotten that the 'cause' only naturally necessitates the 'effect' in virtue of the existence of the appropriate relational powers ϕ and ϕ^{*}. Thus, x's being in water at t is not in itself a sufficient condition of x's being dissolved by water at t: it is only so in virtue of the fact that x is water-soluble at t. It is hardly surprising, then, that those philosophers who attempt to analyse 'cause' and 'effect' in terms of necessary and sufficient conditions while ignoring the role of powers in any such analysis, run into confusions and complications. For, far from the concepts of necessary and sufficient conditions providing adequate materials for an analysis of 'cause' and 'effect', it turns out that we have to attain an understanding of causation before we are able to say in what sense a 'cause' is a 'sufficient' condition and an 'effect' a 'necessary' condition.

As to the argument that our designation of the 'cause' of an event is affected by subjective and anthropocentric considerations, we can see why it is quite spurious. There is an objective difference between the 'cause' and the multitude of 'standing' conditions without which the 'effect' could not occur. Consider, for instance, the case in which we say that the 'cause' of a lump of salt's dissolution was its immersion in water. It is perfectly true that the 'effect' would not have occurred if, for example, there had been no atmosphere: but only because if there had been no atmosphere there would have been no liquid water in the first place. The 'environmental' or 'standing' conditions are simply those which are necessary for the very existence of the objects possessing the powers ϕ and ϕ^{*}, while the 'cause' is merely an accidental relation between those objects, in the absence of which the objects could still have existed in their normal state. Moreover, the 'cause' is not just any arbitrarily chosen accidental relation between the objects in question since, as we have seen, it is not (pace Hume and his followers) logically distinct from the 'effect'.

Thus it is absurd to say, with Mill, that to know the true 'cause' of any event we must discover all the relevant 'environmental' conditions: all we need to be able to do is to identify the objects possessing the relevant powers ϕ and ϕ^*, and for this we do not need to know what 'environmental' conditions are necessary for their existence. One can recognise a liquid as being water without knowing that its existence in a liquid state depends upon atmospheric pressure.

There are, of course, several senses in which an event E_1 may be said to be a 'sufficient' condition of another event E_2. One sense - which may be called the 'extensionalist' sense[1] - is the sense in which E_1 belongs to a certain class of events C_1 while E_2 belongs to another class of events C_2, and all members of C_1 are members of C_2 (i.e., C_1 is a sub-class of C_2). Another sense - or, rather, family of senses - may be called 'intensionalist' and is based on modal concepts: this is the sense in which E_1 necessitates E_2. But here we must distinguish between the different species of necessity: logical, natural, and perhaps others as well.

Now it may be remarked that if an event E_1 is a sufficient condition of another event E_2 in the causal sense, then it will also be a sufficient condition of E_2 in the extensionalist sense, and for the following reason. We pointed out earlier that powers are essential properties of objects - that is, are largely constitutive of the natural kinds to which objects belong.[2] Now, it is because a particular lump of salt is water-soluble at t that its being in water at t (E_1) is the 'cause' of its being dissolved by water at t (E_2).[3] But water-solubility is an essential property of the salt and hence is common to all specimens of salt whatever. It follows that the class of events describable as 'immersions of salt in water' is co-extensive with the class of events describable as 'dissolvings of salt by water', and hence that x's being in water at t (which belongs to the first class) is a sufficient condition in the extensionalist sense of x's being dissolved by water at t (which belongs to the second class). But in view of the possibility of purely accidental uniformities, the reverse relationship does not hold: the fact that an event E_1 is a sufficient condition of another event E_2 in the extensionalist sense does not

1. Cf. Von Wright (3), p. 40.
2. We must, of course, remember to qualify this in the light of our remarks about 'accidental' powers. But there is no problem in distinguishing a thing's accidental powers from its essential powers.
3. Cf. Mellor (1), p. 179.

imply that it is also a sufficient condition of E_2 in the causal sense - though this may, of course, constitute good evidence of a causal relationship. This, of course, illustrates one of the many failings of the 'regularity' theory of causation: for that theory, in taking the evidence of a causal relationship to be constitutive of the very meaning of the causal proposition which it is supposed to support, renders it unintelligible why we should regard co-extension as evidence of a causal relationship, by making this a matter of definition. I shall return to this subject in the next chapter.

It should perhaps be remarked that even if an event E_1 naturally necessitates another event E_2, it may not be correct to call E_1 the 'cause' of E_2. This is because while relations of natural necessity are transitive, our use of the terms 'cause' and 'effect' is not unrestrictedly transitive, owing to the fact that the central meanings of these terms are such that, in any particular instance, 'cause' and 'effect' are designated with reference to specific dispositional properties. Thus, x's immersion in water at t will have been necessitated by some other event: but this other event will not therefore be designated the 'cause' of x's being dissolved by water at t, even though it necessitated the latter (transitively). This helps to clear up some of the confusions I referred to earlier.

I shall now finally move on to discuss the question of temporal relations between 'cause' and 'effect'. Philosophers have frequently debated whether or not a 'cause' must precede its 'effect' in time, some arguing that a 'cause' cannot possibly occur simultaneously with its 'effect',[1] others arguing that it is in principle even possible that a 'cause' should occur after its 'effect'.[2]

The theory of causation which I am advancing seems to imply that strictly speaking a 'cause' is simultaneous with its 'effect', but that there is a sense in which it may be said to precede it. That a 'cause' must, according to the present account, strictly speaking be simultaneous with its 'effect' is implied by the fact that the activity which is the 'effect' (e.g. the salt's being dissolved by the water at t, or the lead's being flattened by the hammer at t) is not logically distinct from the 'cause' (e.g. the salt's being in water at t, or the

1. See, e.g., Russell (2), p. 135.
2. See, e.g., Mackie (2), pp. 161-2 and pp. 173 ff.

lead's being struck by the hammer at t), despite the common assumption that 'cause' and 'effect' must be 'distinct existences'. This assumption derives, of course, from Hume. But Hume was really only concerned to show that the 'necessity' of causation is not logical necessity - that the 'cause' does not entail its 'effect'. We can allow this while noticing that the 'effect' does entail its 'cause'. This latter entailment should not shock us: the fact is that it has generally simply gone unnoticed that, for example, part of what we mean by 'x is dissolving y' is that x is a liquid and y a solid and y is immersed in x.[1]

However, as I remarked, there is a sense in which the 'cause' may be said to precede its 'effect', as can clearly be seen from this example of water dissolving salt. Evidently, if x is being dissolved by water at t, it must be in water at t: but it cannot always have been in water prior to t (in view of the accidental nature of its immersion). Therefore, at some time not later than t, x must have been immersed in the water. (I am speaking now of 'immersion' in the sense of a process - the process of introducing a solid object into a liquid - rather than in the sense of a state.) Now, the process of immersion necessarily takes time: at the beginning of this process the object is not yet in the liquid, and at the end of it it is. Hence it is true to say that some part of the process of immersion must precede in time x's being dissolved by the water - namely, that part of the process during which x is not yet actually in the water. Now it is not unnatural - if strictly incorrect - to call the process of immersion the 'cause' of x's being dissolved by water at t, rather than x's state of immersion at t. And in this sense it is thus true to say that the 'cause' precedes (at least in part) the 'effect'.

The reason why it is strictly incorrect to call the process of immersion the 'cause' is simply this. While it is true to say that x would not have been dissolving at t if it had not been immersed in water prior to t, the fact that it had been immersed in water prior to t would not of itself imply that x was dissolving at t, since x might have been removed from the water subsequent to its immersion and prior to t. It is only x's being in water at t which ensures that it is

1. Chemists also speak of gases dissolving and being dissolved. This is an extended sense of the term 'dissolve' - but even here it is implied that there is physical contact between solute and solvent.

being dissolved by water at t.

Thus we see that there are natural enough reasons for the commonplace belief that a 'cause' must precede its 'effect', and these are ultimately founded upon the accidental character of the relation which is designated the 'cause'. For since the relation is accidental, it has to be brought about by some process, which takes time: at the beginning of this process the relation does not yet subsist, and at the end of it it does, so that the beginning of the process must temporally precede the event which is designated the 'effect'.

But as for the notion of 'backward causation' (as Mackie calls it), I really cannot understand what this could mean. It is quite conceivable that one event should be a sufficient condition of another in the extensionalist sense, while also occurring after that event. And it is also true that we usually take evidence of co-extension as an indication of causal relationship. But I really cannot understand in what sense we might be induced to speak of an effect preceding its cause. We have seen that it is strictly incorrect even to speak of a 'cause' preceding its 'effect', although there is a loose sense in which this is permissible. But I cannot imagine even a loose sense in which an 'effect' might be said to precede its 'cause'. I do not, however, consider it to be a weakness in my theory of causation that it cannot make sense of the notion of 'backward causation': on the contrary, since this somewhat bizarre notion seems to belong purely to philosophical discussions without having, apparently, any application either in ordinary life or in science, it is rather to be taken as a sign of the accuracy and good sense of the present theory that it has no place for such a notion.

VIII : NATURAL LAWS AND NATURAL KINDS

In the previous chapter I remarked upon the importance of general causal propositions in our reasoning about matters of fact. The point was that while all causal inferences whose conclusions are singular propositions (and most of our predictive arguments take this form) are technically speaking founded upon singular causal propositions ascribing relational powers to particular material objects, in practice we very often rely upon general causal propositions. Thus, in the case of the boiling kettle we do not attempt to verify the singular causal proposition that this particular fire has the power to make this particular kettle of water boil, but rely instead upon the general causal proposition that fire, in general, has the power to make water, in general, boil.

Now, the problem is by what right we may rely upon such general causal propositions. The inductivist will claim that we arrive at such general propositions through inductive procedures founded upon our past experience of 'constant conjunctions': that, for example, we believe that fire, in general, has the power to make water, in general, boil, simply because we have observed a great many particular fires boiling particular kettles of water. However, there are numerous defects in this view. The most serious defect, which we shall discuss later, is that this doctrine totally misconstrues the logical character of the general propositions with which we are here concerned. But before discussing this point, I want to draw attention to the fact that this inductivist doctrine neither is nor could be a true account of the way in which we normally or primarily justify general causal propositions.

That the inductivist doctrine is not as a matter of fact a true account of the way in which we normally justify general causal propositions is easily seen. Philosophers have often noticed with some puzzlement that very frequently even the most careful scientist will consider a single well-conducted experiment to be an adequate justification of a general proposition. Hume himself remarked upon this - but failed to account for it satisfactorily.[1] Now, it is to be observed that the general propositions for which this experimental

1. See Hume (1), p. 104.

procedure is particularly appropriate are general causal propositions: and the reason for this will shortly become evident. For example, if one were required to justify the general proposition that lead is malleable or that salt is water-soluble, it would be considered (by all except, perhaps, the sceptical inductivist) quite adequate simply to show that a particular piece of lead can be flattened by a particular hammer, or that a particular piece of salt can be dissolved by a particular flask of water. Neither the scientist nor the 'plain man' repeats such experiments simply in order to increase the sheer number of 'confirming instances', in the manner which the inductivist proposes.[1] Such an experiment is only repeated if the experimenter has reason to doubt the 'purity' of his specimens - if, for example, he has reason to suspect that his piece of lead is alloyed with other metals, or that his flask of water is contaminated with other solvents.

It should furthermore be remarked that even if the experimenter does have reason to doubt the 'purity' of his specimens in a given experiment, the reason why he repeats the experiment with other specimens is not that he does not consider the first experiment to justify a general causal proposition, but rather that he is not sure as to which general causal proposition it justifies: that the experiment does justify some general causal proposition is never brought into question. Thus, for example, if the experimenter suspects that his flask of water is contaminated with other solvents, he may not consider that his experiment justifies the general causal proposition that salt is water-soluble, but he will consider it to justify the general causal proposition that salt can be dissolved by a certain (perhaps unknown) mixture of solvents.

So much, then, for the point that we do not as a matter of fact normally attempt to justify general causal propositions in the way which the inductivist suggests is appropriate. Why we adopt the procedure described above is not, however, a purely contingent state of affairs, and I shall attempt to explain it shortly. But before doing this it would be as well to point out that the procedure which the inductivist proposes could not possibly be our primary way of justifying such propositions. Consider, for instance, the suggestion

1. Cf. Cohen (1), p. 68 and pp. 106 ff. See also Keynes (1), pp. 233-4.

that we only believe that fire, in general, has the power to make water, in general, boil, because we have observed a great many particular fires boiling particular kettles of water. Now this suggestion, even though, as we have seen, it conflicts with our actual procedures in such matters, does not appear entirely absurd (especially if one has been well indoctrinated in the inductivist school of thought) - and this, perhaps, is because it undoubtedly is the case that we use inductive procedures to establish some general propositions (a point which I shall discuss more fully in a later chapter). Where the inductivist doctrine becomes absurd is when it suggests that the only way, or the primary way, of justifying general causal propositions is by inductive procedures. This can be seen when we consider what sort of knowledge is presupposed by our ability to make particular observations or experiments. I remarked in an earlier chapter that all perception contains an uneliminable epistemic element: whenever we perceive or observe something, we can only perceive or observe it as being this or that kind of thing. Now, this epistemic element in perception - the element of 'recognition' - implies that the ability to make particular observations or experiments always presupposes some general knowledge. For we cannot perceive a particular barely as a particular: we can only perceive it as being a particular of some general kind.[1] For instance, the ability to observe a particular fire boiling a particular kettle of water presupposes, on the part of the observer, the possession of sufficient general knowledge to recognise fire and water. However, if, as the inductivist proposes, all general knowledge must arise through inductive procedures from particular observations, it is clear that we should never know anything at all, and never perceive anything - we should all be in the condition of the lowest forms of sentient life, in which our experience would consist merely of a flux of sensations. The fact that this is not the case therefore refutes the inductivist position.

It is simply not possible for the inductivist to account for the difference in sentience between human beings and lowly forms of life such as worms and insects. For this difference is not merely one of degree: the sensory experience of human beings, virtually from the

1. This is simply a corollary of Kant's famous dictum: "Gedanken ohne Inhalt sind leer, Anschauungen ohne Begriffe sind blind" (Kant (1), p. 75).

moment they are born, is thoroughly saturated with general knowledge. I must confess that I am quite unable to explain this fact - a fact which is a good deal more mysterious than many philosophers seem to have supposed. But it is clear that the inductivist cannot explain it either. And this being the case, I do not think that it is incumbent upon me to suggest a solution to this problem before proposing to replace inductivism with an alternative account of the character of our reasoning about matters of fact.

So far we have seen that the inductivist doctrine neither is nor could be a true account of the way in which we normally justify general causal propositions. Now, as I hinted earlier, the underlying reason for this is simply that this doctrine totally misconstrues the logical character of such propositions. What we should ask ourselves is why it has seemed so self-evident to 'empiricist' philosophers - even though it conflicts sharply with actual scientific experimental procedure - that no general proposition can possibly be justified by a single observation or experiment. The answer seems to be, quite simply, that such philosophers have tended to assume that all general propositions concerning 'matter of fact and existence' are to be construed as statements concerning collections of particulars: for if this is assumed, it seems quite evident that the discovery that one individual particular in a collection possesses a certain characteristic has very little bearing upon the question of what characteristics are possessed by other individual particulars in that collection. Now I shall be arguing that, on the contrary, while some general propositions certainly do concern collections of particulars, by far the most important category of general propositions concern kinds, and that the notion of a kind is not reducible to the notion of a collection of particulars.

This, perhaps, is the place to introduce the subject of 'natural laws'. For while, as I say, empiricist philosophers have on the whole tended to assume that all general propositions concern collections of particulars, it has not escaped their notice that some general propositions are regarded, both by the scientist and by the 'plain man', as having a very different status from others - and these are commonly

referred to by such philosophers as 'natural laws', or 'nomological' propositions. The problem has been to give an analysis of such propositions which adequately accounts for the special esteem in which they are held.

Some philosophers, such as F. P. Ramsey, have suggested that nomological propositions are not really general propositions at all, but are disguised 'prescriptive rules'.[1] But for various well-known reasons this view is now generally rejected.[2] Another group of philosophers, exemplified by K. R. Popper, consider that what distinguishes nomological propositions from other general propositions is that the former are propositions of 'unrestricted universality', asserting the existence of some uniformity or regularity which extends throughout the universe in both space and time.[3] Yet other philosophers, such as W. Kneale, have argued that this analysis is inadequate, because it fails to account for the fact that both the scientist and the 'plain man' seem to assume that nomological propositions support counterfactual and subjunctive conditionals.[4] Kneale, in an attempt to account for this, has suggested that nomological propositions should be construed as 'principles of necessitation'.[5] However, it is not altogether clear what Kneale means by this save that (1) he is emphatic that such principles do not purport to state mere contingent matters of fact, (2) he considers that such principles nevertheless cannot be known apriori, in the manner of mathematical theorems, and (3) his views are similar to, and perhaps even derivative from, Locke's notion of 'real essence'. It is interesting that in this division between Popper and Kneale we may see a reflection of Hume's ambivalent attitude towards the nature of causal relations - his uncertainty as to whether these should be interpreted in terms of 'constant conjunctions' or in terms of 'secret necessary connections'.

Needless to say, I reject both Popper's and Kneale's analyses of nomological propositions. It is noteworthy that what these analyses have in common is the sceptical implication that a nomological proposition can never be justified by a single experimental test or observation. Popper's analysis implies this simply because it construes nomological propositions as concerning collections of particulars:

1. See Ramsey (1), pp. 237 ff.
2. See, e.g., Kneale (1), p. 77.
3. See Popper (1), pp. 62 ff., and also Appendix X, pp. 420 ff. It is not clear to what extent the doctrine of the appendix represents a change in Popper's basic view of natural laws.
4. See Kneale (1), p. 75 and Kneale (2), passim.
5. See Kneale (1), p. 71, pp. 78 ff., and pp. 254 ff.

Kneale's analysis implies it because it construes nomological propositions as being somewhat like mathematical theorems which can only be 'confirmed' by particular instances, but never conclusively proven. I shall not discuss Kneale's view any further as it appears to suffer from all the defects of Locke's notion of 'real essence' and, besides, it has received sufficient criticism at other hands.[1] But views not unlike Popper's continue to dominate contemporary opinion in the philosophy of science, particularly amongst self-styled 'empiricists'. Of course, it is by now commonly accepted, in response to Kneale's criticisms, that something is lacking in the Popperian analysis as regards the connection between nomological propositions and counterfactual and subjunctive conditionals. But it generally seems to be supposed that this is to be accounted for in terms of some additional factor which is 'subjective' in the sense that it resides in our attitude towards those general propositions which we term 'natural laws', rather than in any peculiar logical or semantic features of these propositions which might distinguish them 'objectively' from other general propositions.[2] It is still widely supposed that a nomological proposition, whatever additional factors may be involved in its analysis, is fundamentally a proposition about a collection of particulars.

Now, the Popperian view of natural laws suffers in particular from one very serious defect. Consider, for example, the (notorious) proposition 'All ravens are black'. The problem is to explain what we mean in suggesting that this proposition expresses a 'natural law'. According to the Popperian analysis, this proposition concerns a collection of particulars or individuals falling under a certain description, and asserts of each member of this collection that it possesses the attribute of blackness. The 'nomological' aspect of the proposition resides in the absence of spatiotemporal restrictions on the collection and also, perhaps, in our 'attitude' towards the proposition. The proposition thus merely asserts the existence of a universal uniformity of blackness amongst members of the raven population. But the problem is how such a uniformity can be explained in the Popperian scheme. For the natural answer to this question would be to say that there must be some sort of causal connection between being a

1. See, e.g., Ayer (1), pp. 15-17.
2. See, e.g., Ayer (3), pp. 230 ff. and Rescher (1), pp. 97 ff. This view is obviously connected with the common opinion that non-material conditionals are themselves in some sense 'subjective' (see p. 55 above).

raven and being black - a connection which explains why all particular ravens happen to be black.[1] But according to the Popperian analysis, to assert the existence of such a connection is merely to reassert the general proposition concerning the uniformity of blackness amongst members of the raven population, which is the very circumstance which we want to explain. Nor will it do for the Popperian to brush this problem aside by contending that the only proper way to 'explain' a universal uniformity is to deduce it from another universal uniformity of wider scope or generality:[2] for this is only to explain one uniformity in terms of another - it does not at all explain why the universe should exhibit those particular uniformities which it does exhibit, a fact which is just allowed to remain an accident of cosmic proportions.

This fatal defect in the Popperian analysis of nomological propositions simply serves to bring out the fundamental logical point that it is not possible that all general propositions should simply concern collections of particulars. At least some general propositions must concern kinds of particulars. Of course, 'nominalist' philosophers may contend that our knowledge of any general kind of particular is ultimately simply knowledge of particular objects which may be collected into a class on the basis of similarities which we discover between them. But it is impossible that all knowledge of general kinds should be of this nature, since the very concept of a particular presupposes the concept of some general kind to which it belongs. Just as we cannot perceive a particular save as being a particular of a certain kind, so we cannot conceive of a particular save in these terms. The notion that we can conceive of a particular in its bare particularity - that a particular might have properties which are essentially peculiar to its being the particular it is - is evidently incoherent.[3] This is not, of course, to deny that there may, as a matter of contingent fact, happen only to be one particular of a given general kind: it is only to deny that it can be essential to a given particular that it should be the unique representative of its kind. Nor am I, in saying this, committing myself to a 'reification of universals': we are still at liberty to maintain the common-sense view that all existents are

1. Cf. Ducasse (1), p. 7.
2. See, e.g., Hempel (1), pp. 297 ff.
3. Locke seems to have appreciated this point: see Locke (1), Bk. III, Chap. VI, para. 6.

particulars, since this is quite consistent with maintaining the 'reality' of kinds. In short, the notions of 'particular' and 'kind' are inextricably interdependent and mutually irreducible.[1] Our very ability to conceive of an objective world at all depends upon our grasp of these notions.

Now, what I want to propose is that what are commonly called 'nomological' propositions or 'natural laws' are general propositions concerning kinds - and, in particular, general causal propositions of this sort. The causal character of such propositions serves, of course, to explain the connection between nomologicality and counterfactual or subjunctive conditionals, since we have already explained the connection between such conditionals and relational power-statements (which are 'causal' propositions, according to our previous analysis). The causal character of nomological propositions is also evidently closely connected with the fact that causal properties - that is, relational powers - are 'essential' properties of objects, and hence are definitive of the kinds to which objects belong. Again, yet another element which fits into this analysis is the relationship, which we discussed earlier, between 'sufficient' conditions in the causal sense and 'sufficient' conditions in the extensionalist sense.[2] For it is in terms of this relationship that the difficulties encountered by the Popperian analysis of nomologicality are to be resolved: the uniformity of blackness amongst members of the raven population is to be explained in terms of a causal connection between ravenhood and blackness - a connection which renders blackness an 'essential' property of ravens, and hence common to all individual members of the species.

We can also now understand why it is that both the scientist and the 'plain man' commonly regard a single experimental test or observation as a sufficient justification of a general causal proposition. This is simply because such propositions concern kinds of particulars, and one exemplar of a given kind is as good as another. If such a test is repeated, it is only because it is suspected that the experimental objects were not exemplars of the required kind. But that the experimental objects are exemplars of some kind or other cannot be questioned since, as we have remarked, the notion that a particular

1. Cf. Wiggins (1), p. 69.
2. See pp. 93-4 above.

might have properties which are essentially peculiar to its being the particular it is, is incoherent. This also accounts for the fact that when a scientist does repeat an experiment, he often deliberately 'varies the circumstances'. It should further be remarked that, while it may sometimes be reasonable for an experimenter to doubt whether his experimental objects are exemplars of the right kind, it cannot be reasonable for him always to entertain such doubts - doubt on this scale is doubt of the 'paranoiac' sort. This is why it is not merely in principle, but also in practice, possible to justify a general causal proposition by means of a single experimental test.

Of course, the proposition 'All ravens are black' is not itself a general causal proposition, in the sense that 'All salt is water-soluble' is - namely, because it does not ascribe a relational power to ravens. Nonetheless, in suggesting that 'All ravens are black' is a natural law we are, I am proposing, suggesting that this proposition is a consequence of certain (perhaps unknown) general causal propositions (concerning, no doubt, the genetic make-up of ravens). But this helps to explain a peculiarity of certain 'nomological' propositions. We have seen that a general causal proposition, such as 'All salt is water-soluble' may be justified by a single experimental test. But it is evident that a proposition like 'All ravens are black' cannot be justified in this way - at least in the sense that the observation of a single black raven is clearly not sufficient proof of the general proposition in question. (This, incidentally, is, I suspect, one reason why philosophers like Popper and Hempel have - no doubt quite innocently - concentrated upon such 'taxonomic' laws, despite their relative unimportance in science.) The reason for this resides in the fact that this proposition is not itself a causal proposition: we do not regard the colours of objects as causal properties (that is, as relational powers). On the whole, we do not regard differences in colour and other 'sensible qualities' as essential differences (nor, for that matter, do we generally regard differences in size in this light): we are only prepared to accord an essential status to such differences when we know that they derive from specific relational powers in the objects concerned (for example, in the genetic structure

of living organisms) rather than from 'accidental' circumstances (such as 'environmental' differences).[1] This is why the proposition 'All ravens are black' is a very bad example of a 'natural law', and one which has helped to divert the attention of many philosophers of science from the real problems at issue in the question of nomologicality.

In conclusion, let us return to the problem with which I opened this chapter. I remarked that in our reasoning about matters of fact we very often rely upon general causal propositions, even when our arguments are technically founded merely upon singular causal propositions. The problem was to explain by what right we may rely upon such general causal propositions. What I wanted to deny was not that this right derives from 'past experience', but that it derives from 'past experience' exclusively through inductive procedures. And for this reason it is that I have argued that it is possible to justify a general causal proposition by means of a single experimental test. Once such a proposition has been justified, our right to rely upon it subsequently in particular arguments simply depends upon our ability correctly to identify the kinds of objects we are arguing about - and this ability, while not infallible, must, as we have argued elsewhere, normally be exercised successfully. For instance, once it has been established by experiment that fire has the power to make water boil, our right to rely upon this proposition in any particular argument (such as the one cited by Strawson) depends simply upon our ability to recognise certain particular material objects as 'fire' and 'water'.

The reason why the arguments of the present chapter are so important for the views being advanced in this thesis is, of course, simply that only if they are correct can it be maintained (as I want to maintain) that causal inference is a species of reasoning entirely independent of inductive procedures. I hope by now to have substantiated this claim. In the remaining chapters I shall attempt to prove that no other species of reasoning about 'matter of fact and existence' (including inductive arguments) is, conversely, independent of causal inference (in the sense that all such species of reasoning presuppose the possibility of justifiable causal inferences).

1. This, of course, reminds one of the more fruitful aspects of Locke's doctrine of 'real essences' - in particular, the notion that material objects have 'inner constitutions' which are partly responsible for their 'sensible qualities'.

IX : AMPLIATIVE INDUCTION AND ITS 'JUSTIFICATION'

As I pointed out in the Introduction, while the term 'inductive argument' is normally assumed to signify arguments of ampliative induction, we should be careful to distinguish this kind of induction from others such as mathematical induction and eliminative induction, which involve deductive inference. 'Ampliative' induction is so called simply because it is non-deductive: the conclusion of such an argument is said to 'go beyond' its premises (whereas the conclusion of a deductive argument is said to be already 'contained within' its premises). Ampliative induction is also often referred to as 'enumerative' induction (but it is only one species of enumerative induction, since it must be distinguished from induction 'by complete enumeration', which is a form of deductive inference). This name derives from another important feature of such arguments - namely, that such an argument is characteristically an argument from premises asserting that certain members of a given class of objects or events possess a certain characteristic, to a conclusion that other members, or all members, of that class possess that characteristic. Thus the premises of such an argument characteristically enumerate instances conforming to a certain pattern.[1] It may be mentioned that some philosophers, such as J. M. Keynes, have distinguished between 'pure' enumerative induction and ampliative induction involving 'variation of circumstances'.[2] This is an important distinction, but only one within the general notion of enumerative induction, since even where the 'circumstances' of the 'confirming instances' are varied, the method is still basically one of collecting confirming instances (and must therefore not be confused with eliminative induction). However, I shall later concur with Keynes' view that variation of circumstances is really essential to the rationale of ampliative induction (a view which is supported by the considerations of the previous chapter).

I must emphasise from the outset that in spite of my insistence upon the central importance of causal inference in both scientific method and everyday thought, I by no means deny the invaluable role played by inductive arguments. As I shall point out later, there are

1. For similar definitions of the term 'inductive argument', see, e.g., Russell (3), p. 37, Keynes (1), p. 219, Nicod (1), p. 178, Kneale (1), p. 226, and Von Wright (1), p. 159.
2. See Keynes (1), pp. 233 ff. See also Cohen (1), pp. 106 ff.

many fields in which, for the present at least, we have to rely upon inductive arguments, because our knowledge of the relevant causal properties of the objects involved is insufficient. To admit this, however, is not to retract our conclusions of the previous chapter, that causal inference is a species of reasoning entirely independent of inductive procedures.

A typical use of inductive argument in science is the discovery of functional relationships between variable quantities by graphical methods. When observation or experiment discloses a number of different coinciding values of two or more variables, such as the volume and pressure of a specimen of gas, and these values are plotted as points on a graph, a curve representing the inferred functional relationship between these variables may be drawn by interpolation and extrapolation of these points. However, it is frequently pointed out that, while the experimenter generally has no difficulty in selecting just one curve to 'fit' the points, the number of curves mathematically possible is always infinite, no matter how many observations are made.[1] This is the so-called 'curve-fitting problem'. The problem is: On what grounds does the experimenter make his selection, and are these grounds 'rational'? The usual answer in inductivist literature is that his selection is based upon considerations of mathematical 'simplicity': of the infinite variety of mathematically possible curves, it is rational always to select the 'simplest'.[2] However, considerable controversy has arisen as to the proper criteria of simplicity, and even as to whether there are any objective standards of simplicity. Thus Swinburne has argued that judgments of mathematical simplicity are dependent upon the mathematical notation in current use.[3] But, in any case, such a solution to the curve-fitting problem must in general inadequate as an explanation of the actual selection-criteria adopted by practicing scientists, which clearly do not merely involve mathematical considerations. For example, the experimenter rarely, if ever, supposes that his final curve will fit all the points he has plotted; and he will often suspect that his results favour the more 'complex' of two possible curves, such as an exponential curve rather than a straight line. So, both as regards the acceptability of

1. See, e.g., Kneale (1), pp. 228 ff.
2. See, e.g., Kneale (1), pp. 228 ff. and Popper (1), pp. 136 ff.
3. See Swinburne (1), pp. 108 ff.

particular experimental results and as regards the selection of the best curve, the scientist relies heavily upon assumptions which do not concern 'simplicity', in any accepted sense of that term.[1]

We may conveniently relate the 'curve-fitting problem' to the more general problem of the 'justification' of inductive arguments. In an earlier chapter I remarked that Hume had two different arguments against the rationality of inductive extrapolation, but that these served only to show that <u>given</u> the presuppositions of inductivism, inductive procedures are unjustifiable.[2] These 'presuppositions' are, expressed briefly, the following:

(1) That our knowledge of matters of fact may be divided into two kinds, experiential knowledge and transexperiential knowledge, and that knowledge-claims of the latter sort depend upon some species of inference from knowledge-claims of the former sort.[3]
(2) That the only relations between things which we may either perceive or in any other way discover are spatiotemporal relations and relations of 'similarity'.[4]
(3) That causal inference is reducible to inductive inference and that inductive argument is our primary species of reasoning about matters of fact.
(4) That all inductive arguments are founded upon a 'principle of inductive inference'.[5]

Now, despite Hume, many inductivists have attempted to justify inductive arguments even while accepting these presuppositions in some form or other. These attempts seem to fall into three distinct classes, which Swinburne has called the 'analytic', the 'inductive' and the 'pragmatic' justifications of induction.[6] I shall discuss these three approaches in turn, and argue that none of them succeeds.

Some philosophers have simply claimed that inductive inference is paradigmatically rational: that what we actually <u>mean</u> by a rational transexperiential argument is simply an argument conforming to inductive criteria. P. F. Strawson takes this view. Thus he says: "So to ask whether it is reasonable to place reliance on inductive procedures is

1. Cf. Ackermann (1), passim.
2. See p. 24 above.
3. Cf. Russell (3), p. 33.
4. Cf. Ayer (1), p. 11. This excludes logical relations, of course.
5. Cf. Reichenbach (1), p. 340.
6. See Swinburne (2), pp. 10 ff.

like asking whether it is reasonable to proportion the degree of one's convictions to the strength of the evidence. Doing this is what 'being reasonable' means in such a context."[1] To this view I merely reply that causal inference does not conform to inductive criteria, since I have argued that causal inference is neither reducible to, nor in any way dependent upon, inductive procedures: so it is not necessarily the case that what we actually mean by a rational transexperiential argument is simply an inductive argument from experiential premises.

A second group of philosophers dispute the claim that Hume proved an inductive justification of inductive arguments to be viciously circular.[2] These philosophers, however, accept the fourth presupposition which I mentioned above, namely, that all inductive arguments are founded upon a 'principle of inductive inference' (although they are not entirely in agreement as to the precise form which this principle should take). Such a 'principle' obviously corresponds in most ways to Hume's 'supposition that the future will be conformable to the past', save that modern inductivists do not look upon their 'principle' as the major premise required to turn an inductive extrapolation into a deductively valid inference, but rather simply as the guiding rule governing all particular inductive extrapolations. The aim of this second group of philosophers is thus to prove that an inductive justification of such a principle would not be viciously circular. But, of course, this approach is very much open to attack, since it effectively centres upon the question as to whether or not a certain kind of circularity is really 'vicious'. This is a very weak position to have to defend, and it is therefore not surprising that most inductivists reject this approach. However, a little later I shall point out how an inductive justification of induction could present a much stronger case by dropping the presupposition, which turns out to be unnecessary, that all inductive arguments are actually founded upon a 'principle of inductive inference' (which is not, however, to deny that all inductive arguments may exhibit a conformity to a rule, but only to deny that they are founded upon that rule[3]). But, as we shall see, an inductive justification of induction will still suffer from another, and this time irremediable, defect.

1. Strawson (1), p. 257. See also Edwards (1), passim.
2. See, e.g., Braithwaite (1), pp. 264 ff. Braithwaite calls this the 'predictionist' justification of induction.
3. To say that an argument is not founded upon a given rule is simply to say that that rule would not be appealed to in order to justify the inference made in that argument.

The third and last group of philosophers attempt to justify induction pragmatically. Adherents of this approach simply claim that 'inductive policies' provide the best possible way of estimating the nature of whatever lies 'beyond' our immediate experience. Thus Kneale declares: "Primary induction is a rational policy, not because it is certain to lead to success, but because it is the only way of trying to do what we want to do, namely, make true predictions."[1] However, this approach seems to me to be fundamentally mistaken. As I pointed out in an earlier chapter, while all processes of reasoning are potentially heuristic procedures, not all heuristic procedures are processes of reasoning.[2] So even if it could be shown that inductive policies provide the best way of discovering transexperiential matters of fact, this would at most imply that it would be reasonable to adopt inductive policies for this purpose, but it could by no means imply that induction itself is a genuine process of reasoning. It may be, of course, that some inductivists would be quite content with this. But it remains the case that the question at issue is whether induction really is a process of reasoning: whether the truth of inductive premises accords us any right whatever to have confidence in the truth of inductive conclusions. It is by no means apparent that the 'pragmatic' justification of induction answers this question (which is the question which Hume himself posed). For even if it can be shown that induction is in general the best policy for arriving at the truth, this still does not imply that in any specific instance in which this policy is applied we can have reason to suppose that it will supply us with the truth: and this is what we require of any genuine process of inference. But, in any case, I would of course deny that an inductive policy is in general the best way of arriving at the truth.[3] I would acknowledge that very often we must rely upon inductive procedures - a point which I shall elaborate later - but would contend that when we have sufficient knowledge of the causal properties of the relevant physical objects, causal inference is a far superior way of predicting matters of fact which lie 'beyond' our immediate experience. Moreover, even within the framework of inductivist presuppositions, it has been very hard to prove that inductive policies really are the best heuristic procedures.[4]

1. Kneale (1), p. 235. See also Reichenbach (1), pp. 348 ff.
2. See p. 11 above.
3. Kneale's contention that it is the only way is blatantly false: cf. Braithwaite (1), p. 272.
4. See, e.g., Katz (1), passim.

So far, then, it would appear that all inductivist attempts to justify inductive arguments are quite unsatisfactory. But at this point we may ask why the question of the 'justification' of induction ever arises. After all, there is no similar controversy about the 'justification' of deductive arguments, as adherents of the 'analytic' justification of induction rightly point out. Hume, I believe, clearly saw the source of the problem. It is simply that there is no inference-relation between the premises and conclusions of inductive arguments. Of course, there is a relation between the premises and conclusions of such arguments: the so-called relation of 'inductive support' or 'confirmation'.[1] The nature of this relation is a matter of some dispute,[2] but what seems clear is that it is not an inference-relation. This requires a little explanation.

In the case of a deductive inference, if I know that a proposition p entails another proposition, q, I shall not hesitate to infer q from p. Similarly, in the case of a causal inference, if I know that an event A causes another event, B, I shall not hesitate to infer the occurence of B from the occurence of A. But consider now an inductive argument: for example, an argument from premises asserting that a large number of ravens are black to a conclusion that other ravens, or all ravens, are black. The premises 'support' or 'confirm' the conclusion: yet every sane and rational man would hesitate to infer such a conclusion from such premises without a great many further assumptions. We saw a similar situation arise in the case of the 'curve-fitting problem'.

It will not do, incidentally, to argue that I am here overlooking the 'probabilistic' nature of inductive inference. For it is not even true to say that a sane and rational man would, without further assumptions, infer from the fact that many ravens are black that it is probable that other ravens, or all ravens, are black. This is simply because every sane and rational man acknowledges the possibility that his experience may have been untypical. Of course, some philosophers would argue that it is part of the very meaning of the concept of probability that a man should infer, from the fact that many ravens are black, that it is 'probable' that other ravens, or all ravens, are black.[3]

1. See, e.g., Hempel (1), pp. 3 ff. and Goodman (1), pp. 67 ff.
2. See, e.g., Cohen (1), pp. 6 ff.
3. I shall discuss this view more fully in a later chapter.

But this simply takes us back to what Swinburne calls the 'analytic' justification of induction. Moreover, the consequence of this view would be that inductive inference could no longer be looked upon as non-demonstrative: inductive inference would simply become a species of deductive argument (and hence lose its 'ampliative' character).

It should be clear from the preceding chapters of this thesis that the failure of inductivism stems from its dismissal of non-logical species of possibility and necessity. To say that there is no inference-relation between the premises and conclusion of an inductive argument is simply to say that those premises in no sense necessitate that conclusion (or that they do not exclude the possibility of other conclusions). The fact that the premises of an inductive argument do not logically necessitate the conclusion is quite acceptable, since inductive arguments are supposed to be non-demonstrative. But this does not imply, as the inductivist seems to imagine, that in a non-demonstrative argument the premises need in no sense necessitate the conclusion.

In order to resolve these difficulties, let us look at an inductive argument which everyone would feel to be quite 'justifiable'. A meteorologist may predict the weather in England next August by extrapolating from the weather statistics of the past fifty years, taking into account any long-term trends and cycles apparent in the data. But this prediction is only deemed reasonable because the meteorologist has reason to suppose that the weather will not undergo radical changes from one year to the next, and that long-term trends and cycles will not alter abruptly. But this knowledge is not derived merely from observing that no such radical changes and abrupt alterations have occurred in the past, but rather from an understanding, however partial and inadequate, of the causal factors involved in climatic change - such as atmospheric conditions, the intensity of solar radiation, the orbital and axial motions of the Earth, and so on. Without such an understanding, we really would have no reason to reject the possibility of snow and ice next August, any more than would the primitive savage who believes that the weather is controlled by capricious gods and demons. It should be noticed, moreover, that the kind of knowledge

which renders an inductive inference 'reasonable' or 'justifiable' cannot be expressed in terms of some vague 'principle of the uniformity of nature', since it quite specifically concerns the causal properties of the objects to which the inductive argument refers in each particular case. The function of such knowledge is to set strict limits on the range of possibilities which the inductive argument must take into consideration. In this way, for example, most of the mathematically possible curves which 'fit' a set of points plotted on a graph may be excluded as physically or naturally impossible - that is, may be excluded as being incompatible with the known causal properties of the objects concerned. It is thus clear that inductive arguments are only 'justifiable' in a context in which causal inference is possible, since the 'reasonableness' of an inductive argument derives from a multitude of (often tacit and unarticulated) causal inferences which serve to narrow down a certain range of possibilities.

Now, in saying this I am not denying that it may be possible, and indeed even quite useful, to abstract from inductive arguments certain general criteria of 'inductive support' without regard to the specific causal inferences which are presupposed (either tacitly or explicitly) by any particular inductive argument. The simplest account of such criteria is that inductive support varies in strength according to two principal factors: the number of 'confirming instances' taken into account in the data, and the 'variety' of their 'circumstances'.[1] However, as I remarked earlier, I would not distinguish very sharply between these criteria since, like Keynes, I would hold that the mere repetition of experiments or observations is worthless, and that repetition only adds support through the variation of circumstances which it normally creates. This follows from our considerations of the previous chapter. The only real difference between these two criteria is that the first reflects the experimenter's hope that the repetition of an experiment will involve some variation of circumstances while the second reflects his intention to vary, by deliberate intervention, specific circumstances which he suspects to be relevant.

It is interesting to compare the meteorological example discussed above with the discovery of Boyle's Law. This law was, apparently,

1. See, e.g., Strawson (1), p. 246. For a more comprehensive account, see Cohen (1), passim.

discovered inductively by interpolation and extrapolation of numerous observations of the pressure and volume of specimens of air. In this case the inductive inference was rendered justifiable by certain very elementary knowledge of the causal properties of air - knowledge which we all acquire through everyday experience. This knowledge was sufficient to rule out the more bizarre of the mathematically possible functional relationships between pressure and volume. But the law can also be derived purely through causal inference from knowledge of the causal properties of the molecules constituting a gas. (This derivation is a well-known feature of the kinetic theory of gases.) However, here we clearly require a much deeper knowledge of the causal properties of gases than was available to Boyle. In the case of weather-forecasting it is precisely because we still lack a comparable deeper knowledge of the determinative causal factors that we must rely heavily upon inductive arguments. This distinction between inductive inference from statistical data and causal inference from the properties of 'theoretical entities' is frequently met in science and, indeed, is sometimes said to distinguish 'immature' or 'new' sciences (such as sociology and psychology) from 'advanced' sciences (such as physics and chemistry).

Now, I contended earlier that inductive arguments are only justifiable or reasonable when they are advanced within a framework of specific causal inferences which serve to narrow down a certain range of possibilities. But this raises a point to which I alluded earlier. The inductivist supposes that all causal inferences are really reducible to inductive inferences. So here there seems to be the possibility of a non-circular inductive justification of induction. Rather than insisting that all inductive arguments are founded upon a 'principle of inductive inference', and attempting to justify such a principle inductively, the inductivist might claim simply that any particular inductive argument is rendered justifiable by particular causal inferences, which are themselves reducible to other particular inductive arguments. There is no circularity here. In this way the inductivist might envisage a complex hierarchy of inductive arguments, in which the lower levels give support to the higher. However, this picture is

still unsatisfactory. In the first place, of course, it may be objected that causal inference is not reducible to inductive inference. But since the question at issue is only the internal coherence of inductivism, this objection is out of place. The fundamental defect in a hierarchical inductive system of the sort envisaged is not that it involves any kind vicious circularity, but that it involves a vicious infinite regress. For, according to the presuppositions of inductivism, all our trans-experiential knowledge is founded upon inductive inference: but if every inductive argument presupposes other, prior inductive arguments, it is clearly impossible to explain how we can even have begun to acquire transexperiential knowledge. This objection is really just the same as the one which we met in the previous chapter, to the effect that not all general knowledge can be acquired inductively. The moral to be drawn is simply that induction alone cannot perform all the tasks demanded of it by the inductivist, and hence that at least some of the inductivist's presuppositions are false.

X : ELIMINATIVE INDUCTION

In previous chapters I have argued that inductivism is inadequate as a general account either of scientific method or of our everyday reasoning about matters of fact: that ampliative induction is neither the only, nor the most important, species of argument involving non-demonstrative inference. However, there have been many philosophers who have considered that the notion of non-demonstrative inference is altogether spurious and that it is possible to characterise our reasoning about matters of fact - and, in particular, scientific method - purely in terms of deductive inference. In the remaining chapters of this thesis I shall discuss some of these theories, with a view to showing why the applicability of deductive reasoning to matters of fact is, in fact, dependent upon the possibility of non-demonstrative (and, more especially, causal) inference.

I begin with the theory of eliminative induction. I shall discuss eliminative induction only in its simplest form, partly because there are numerous comprehensive accounts of it available,[1] and partly because what I have to say concerning the applicability of eliminative induction is unaffected by this simplification.

J. S. Mill, the first systematic exponent of eliminative induction, regarded this mode of argument as an experimental method (or group of methods) for the discovery of causes and effects.[2] However, Mill was an adherent of the 'regularity' theory of causation,[3] according to which a 'cause' is a sufficient condition, in the extensionalist sense, of its 'effect'. We have seen that this is inadequate as a theory of causation. It is therefore better to look upon eliminative induction simply as an experimental method (or group of methods) for the discovery of sufficient conditions and necessary conditions. As we shall see, it will then depend upon a further stipulation, in any particular case, as to whether these conditionship-relations are to be understood in the extensionalist sense or in some intensionalist sense.

In a formal discussion of eliminative induction it is usual to refer to the 'conditions' in question as 'characteristics'. In practice, a 'characteristic' may be any kind of object, attribute, relation, event,

1. See, e.g., Nicod (1), pp. 192 ff., Von Wright (1), pp. 60 ff., and Mackie (2), pp. 297 ff.
2. See Mill (1), Bk. III, chap. VIII.
3. See Mill (1), Bk. III, chap. V.

or state of affairs. For instance, the characteristic 'ravenhood' may be said to be a sufficient condition of the characteristic 'blackness', which is in turn a necessary condition of the former. 'Eliminative' induction is so called simply because its objective is the discovery of a sufficient condition or a necessary condition of some given characteristic, C, by means of 'eliminating' any characteristics which are discovered not to be sufficient conditions or necessary conditions of C.

Of Mill's various inductive 'methods', only two are really fundamental: the 'method of agreement' and the 'method of difference', the former being used for the discovery of necessary conditions and the latter for the discovery of sufficient conditions. The method of agreement works on the principle that a characteristic may only be a necessary condition of a given characteristic, C, if it is always copresent with C, so that if an instance is discovered in which C is present but this other characteristic is not, then the latter may be 'eliminated' as a possible necessary condition of C. The method of difference works on the principle that a characteristic may only be a sufficient condition of a given characteristic, C, if it is always coabsent with C, so that if an instance is discovered in which C is absent but this other characteristic is not, then the latter may be 'eliminated' as a possible sufficient condition of C. To avoid prolixity, I shall only discuss the method of difference, as these two methods are really just opposite sides of the same coin.

Now, it is evident that the mere elimination of various characteristics as possible sufficient conditions of a given characteristic, C, cannot by itself lead to the discovery of a characteristic which is a sufficient condition of C. Whenever eliminative induction is employed, certain further assumptions are required over and above the mere collection of experimental data. It is of some importance to establish the precise nature of these assumptions.

In the first place it must be assumed, of course, that C does have some sufficient condition (or, as Mill would put it, that C is 'caused'). Mill met this requirement in the shape of his 'Law of Universal Causation'.[1] However, as Nicod pointed out, we do not need to postulate

1. See Mill (1), Bk. III, chap. V.

anything as grandiose as this in order to apply eliminative induction: we only need to be able to assume that the particular characteristic with which we are currently concerned possesses a sufficient condition.[1] Nevertheless, this assumption is still inadequate in another respect. Since our objective is to discover a sufficient condition of C through the elimination of various characteristics, it is evident that we must assume not only that C does possess a sufficient condition, but that such a condition is one of a finite number of known characteristics. However, even this assumption turns out to be inadequate in view of the possibility of what Mill called the 'plurality of causes'.[2] For if there should exist, amongst our finite number of known characteristics, more than one independent sufficient condition of C, it will follow that we shall be unable to eliminate all but one of these characteristics, and hence be unable to establish any particular characteristic as being a sufficient condition of C. This means that what we must be able to assume, in order to discover a sufficient condition of C by eliminative induction, is that a certain finite number of known characteristics includes one and only one such condition.[3]

Now, it is obvious from the nature of this assumption that eliminative induction could not possibly be our primary mode of reasoning about matters of fact, nor even the primary basis of scientific method (although it cannot be denied that eliminative induction is an important constituent in scientific method). For the knowledge presupposed by any process of eliminative induction cannot always itself have been a product of eliminative induction, on pain of a vicious infinite regress. (There is an obvious parallel between this argument and the objection against inductivism which I raised in the previous chapter.) Mill himself perceived this and argued that the assumption presupposed by any argument of eliminative induction - namely, according to him, the 'Law of Universal Causation' - could ultimately only be justified by enumerative (or ampliative) induction.[4] Of course, we have already seen that ampliative induction is itself logically posterior to causal inference: but in fact it is clear that arguments of eliminative induction are dependent upon causal inference in a quite direct way, as a simple example will show.

1. See Nicod (1), p. 194.
2. See Mill (1), Bk. III, chap. X.
3. It is in this assumption that we must stipulate, in any particular case, what sort of 'sufficient' condition we are seeking.
4. See Mill (1), Bk. III, chap. XXI.

If certain characteristic yellow lines are observed through a spectroscope when it is directed at a flame known to contain sodium, and if these lines disappear when the sodium is removed from the flame but not when other substances are removed, an observer may reasonably conclude that these spectrum lines are caused by sodium or, in other words, that the presence of sodium is a sufficient condition in the causal sense of these spectrum lines. This is an example of the 'method of difference'. But we have seen that the possibility of applying this method depends upon an assumption to the effect that one and only one of a finite number of known 'characteristics' is a sufficient condition of the 'characteristic' with which we are concerned (namely, the yellow spectrum lines). Thus, in this instance, the observer assumes that the yellow spectrum lines must be caused by an incandescent state of one or other of the chemical elements. Such an assumption, however, may only be justified in terms of the modern atomic theory, according to which all atoms emit radiation of characteristic frequencies depending on their internal structures. It is these structures, furthermore, which give rise to the various distinguishing chemical properties of the elements. Thus, it is only in virtue of the theoretical background, which establishes a causal connection between the distinguishing chemical properties of an element and the frequency of the light it emits, that the method of difference may justifiably be applied to discover which element actually causes the yellow lines by eliminating all the possible alternatives.

In general, we may say that the application of eliminative induction on any specific occasion always presupposes our ability to specify a certain limited range of mutually exclusive and collectively exhaustive 'alternative possibilities'. Just as in the case of ampliative induction, mere logical or mathematical possibility will not suffice here since it will always leave us with infinitely many 'alternatives'. Once again, causal inference from the known causal properties of the objects involved is the only means whereby we can achieve our objective.

One final point: we have seen that the conditions necessary for

the application of eliminative induction and ampliative induction - namely, the specification of a limited range of alternative possibilities - are very similar. But we should not therefore be tempted to confuse these two distinct modes of argument: in particular, we should not confuse the function of 'elimination' with the function of 'variation of circumstances' in ampliative induction, as some philosophers have tended to do.[1] Ampliative induction always proceeds by the collection of confirming instances, and is non-demonstrative, whereas eliminative induction proceeds by the collection of negative instances, and is deductive. Bacon, who may perhaps be said to have given the earliest account of eliminative induction, evidently appreciated this distinction, since his advocacy of eliminative induction as the method of science was based on his famous, if questionable, doctrine: maior est vis instantiae negativae.[2]

1. See, e.g., Strawson (1), p. 246. Keynes and Cohen, I think, also tend to confuse this distinction.
2. Bacon (1), Bk. I, para. 46. I call this 'questionable' in view of our findings in chapter VIII above.

XI : THE PROBABILISTIC THEORY OF CONFIRMATION

I briefly touched on the notion of 'confirmation' or 'inductive support' in an earlier chapter, where I discussed the point that the discovery of a large number of black ravens may be said to 'confirm' or 'support' the hypothesis that other ravens, or all ravens, are black.[1] However, as I remarked then, I reject the view that in virtue of the very meaning of the concept of 'probability', one can infer, from the discovery of a large number of black ravens, that it is 'probable' that other ravens, or all ravens, are black. This would be to reduce ampliative induction to a species of deductive argument. Yet there are many contemporary philosophers of science who hold this sort of view, in one form or another - although it is by no means always apparent that they appreciate that such a theory of confirmation robs inductive arguments of their 'ampliative' character.

The notion of 'confirmation' or 'support' is undoubtedly one which is actually employed by practising scientists, and not merely by philosophers of science. Thus scientists would no doubt agree that the discovery in 1919 that the light of stars is deflected by the sun's gravitational field confirmed or supported Einstein's General Theory of Relativity. Now, if we were to examine a large number of historical cases of this sort, in which scientists have spoken of empirical evidence confirming or supporting an hypothesis, it is quite conceivable that we should be able to abstract certain general principles or rules which seem to underlie their thinking in these contexts. For example, such a general principle might be that empirical data are only said to confirm or support a hypothesis when those data are predictable by that hypothesis but not by its rivals. Thus the deflection of light by the sun would not have been held particularly to confirm or support Einstein's theory if it had already been predictable on the basis of Newtonian gravitational theory. How useful such general principles of confirmation would be, it is difficult to say; but it is hardly likely that scientists would explicitly appeal to them, or that scientific theorising and experimental procedure would be improved or rationalised by reflection upon them.

1. See pp. 112-13 above.

However, many philosophers, not content with merely abstracting certain general principles of this sort, seek to formalise them and present them in terms of an axiomatic system. This approach is well exemplified in a recent book by M. Hesse. This author lists eight "intuitive rules which should be taken into account by any adequate theory of inductive confirmation", and then goes on to declare that "The second requirement for an explication of induction is a postulate system in which to systematise the inductive rules."[1] Now, given that philosophers feel this need for an axiomatic system of postulates, it is not surprising that they should turn to the probability calculus. For there exists, of course, a long philosophical tradition, with a very extensive literature, associating probability and induction. In fact it is, I think, this tradition, rather than any genuine reflection upon the 'intuitive' judgments of practising scientists, which is the primary source of the modern axiomatic systems of probabilistic confirmation. Philosophers of previous generations attempted to construct principles of inductive reasoning on an apriori basis: it is only relatively recently (under the influence of Carnap) that philosophers have begun to construe their axiomatic systems as 'explicating' our 'intuitive' judgments concerning the confirmation of hypotheses by empirical evidence. But modern philosophers, despite this apparent shift in approach, continue to build substantially upon the foundations laid by these earlier theorists (and especially upon the work of J. M. Keynes).

There are, of course, a great many modern philosophers who reject the probabilistic theories confirmation: in particular, we may mention K. R. Popper and L. J. Cohen. However, both of these philosophers take what seems to me to be, for reasons which will become clear shortly, a relatively weak approach in criticising these theories. They both attempt to show that certain intuitively plausible judgments concerning the confirmation of hypotheses by empirical evidence are contradicted if 'confirmation' or 'support' is explicated in terms of the probability calculus.[2]

Popper's main object is to refute the view that:

1. Hesse (1), pp. 98-9.
2. Actually, as we shall see, Popper is not so radical a critic as Cohen.

$$C(x,y) = Pr(x,y),$$

that is, that the degree of confirmation accorded to hypothesis x by evidence y is simply identical to the so-called 'logical' probability of hypothesis x relative to, or given, evidence y.[1] He does this by showing, by means of a simple example, that it is possible to find certain propositions x, y and z which are such that, apparently,

$$C(x,y) < C(x \,\&\, z,y) > C(z,y),$$

whereas we may derive from the probability calculus the theorem

$$Pr(x,y) \geq Pr(x \,\&\, z,y) \leq Pr(z,y),$$

and if this is so then, plainly, we cannot have

$$C(x,y) = Pr(x,y).$$

Popper does, however, have another, rather different kind of argument against the identity of C(x,y) and Pr(x,y), and this is simply that it is not, in his view, the aim of science to advance hypotheses which have a <u>high</u> probability: on the contrary, according to Popper, if x is a universal hypothesis and y is empirical evidence, it is always the case that

$$Pr(x,y) = 0.$$

Therefore, if C(x,y) were identical with Pr(x,y), we should have the counter-intuitive consequence that empirical evidence can never confirm or support a universal hypothesis. However, this argument depends on Popper's analysis of nomological propositions, which I criticised in an earlier chapter: this is a point to which I shall return later on.

It may be remarked, however, that although Popper denies that C(x,y) is identical to Pr(x,y), he does want to define C(x,y) as a <u>function</u> of certain 'logical' probabilities involving x and y, the test

1. See Popper (1), pp. 387 ff. I have slightly modified Popper's symbolic notation. I shall discuss the notion of 'logical' probability more fully later.

of this definition being that it apparently conforms to certain intuitively acceptable 'desiderata'. In this respect Popper differs from Cohen, who wishes not merely to deny the identity of a degree of confirmation or support with a 'logical' probability, but also to deny that a degree of confirmation or support may be defined as any function of the relevant 'logical' probabilities. Cohen bases his case upon two principles of inductive support which he takes to be intuitively plausible, the 'equivalence principle' and the 'instantial comparability principle', from which he is able to show that the 'syntax' of inductive support functions cannot be mapped on to the probability calculus.[1]

There is little point in my discussing the arguments of Popper and Cohen in any detail, since there can be no doubt that they draw the correct conclusions in the light of their own assumptions. Rather, I want to point out the general weakness in their line of criticism of the probabilistic theories of confirmation. This may be illustrated by Swinburne's response to Cohen's argument.[2] Swinburne naturally selects as the object of his attack Cohen's 'instantial comparability principle'. What is significant, however, is the way in which he criticises this principle: this is, once again, by citing examples in which Cohen's principle appears to give rise to counter-intuitive results. Thus he maintains that this principle would have the counter-intuitive consequence that evidence of a single black raven better supports the prediction that another raven will be black than does evidence of a million black ravens and one white raven. How relevant Swinburne's argument really is to Cohen's position does not concern me here:[3] what does concern me is the kind of argument which arises between the opposing camps. For this kind of argument inevitably degenerates into a disagreement as to what our 'intuitive' judgments concerning confirmation really are, and how far they are to be relied upon. Adherents of the probabilistic theory will argue that a proper 'explication' of the entire corpus of our 'intuitive' judgments concerning confirmation will inevitably indicate that some of these judgments are unreliable and must be modified in order that the system as a whole should be consistent. As Hesse puts it: "As with all

1. See Cohen (1), pp. 13-30. The second of these two principles indicates certain affinities between Cohen's position and the views expressed in chapter VIII above.
2. See Swinburne (1), pp. 92 ff.
3. In fact I think that this is another example of the misleading consequences of regarding the raven hypothesis as typical.

explications, this does not imply that intuition is accepted blindly. It implies that, on the whole, what we take to be valid inductions are explicated as such, but it does not exclude the possibility that some intuitive types of inference may turn out to be inconsistent or useless or misleadingly specified."[1] Thus it appears to look as though it will be difficult prove, to those philosophers who insist on explicating confirmation in terms of probability, that they are mistaken, if we adopt the line of criticism taken by Popper and Cohen. For whenever the probabilistic scheme appears to endorse some 'intuitive' judgment, this will be claimed as a vindication of that scheme; but whenever the scheme appears to support some paradoxical or counter-intuitive proposition, it may be maintained that this simply indicates the inconsistency or confusion of some of our 'intuitive' judgments. Of course, I do not want to accuse adherents of the probabilistic theory of deliberate equivocation: rather, I want to point out that their allegiance to this theory is more aprioristic than they might be willing to admit.

This leads to my next point. I remarked earlier that it is only relatively recently that it has become fashionable to construe an axiomatic system of inductive postulates as 'explicating' our 'intuitive' judgments. The primary source of such systems seems in reality to be the work of earlier philosophers - and, especially, J. M. Keynes - who attempted to found inductive reasoning on an apriori basis. In the rest of this chapter, therefore, I shall discuss some of Keynes' original ideas on probability and induction, with a view to showing that, from the very outset, the connection of confirmation with probability rested upon certain very dubious assumptions. I shall briefly return to the question of 'explication' later.

Now, Keynes offers an argument which purports to prove that it follows as a necessary consequence of the axioms of the probability calculus that, provided certain conditions are fulfilled, the probability of a universal generalisation must be raised by any evidence of hitherto unknown instantiations of that generalisation: for example, that any new discovery of a black raven necessarily raises the probability that all ravens are black. Such an argument, if correct, would

1. Hesse (1), p. 100.

obviously constitute a strong endorsement of the probabilistic theory of confirmation.

Keynes' argument is couched in terms of the so-called 'logical' theory of probability, which was itself largely his own production. According to this theory, probability is a relation between propositions, or, as some would put it, probability is relative to evidence. Thus Keynes would maintain that to say of any proposition, such as a universal hypothesis, that it is 'probable' to some particular degree, only makes sense in relation to some specific body of evidence. Therefore, one and the same hypothesis may very well have quite different degrees of probability relative to different bodies of evidence. Kneale, who sympathises with Keynes' theory, has argued that this characteristic of our probability concepts is manifested in the everyday use of terms like 'probable' and 'probably'. For we often account some hypothesis highly probable on the available evidence and then later discover it to be false: but, it is alleged, this does not deter us from maintaining that this hypothesis really was, nevertheless, highly probable in the light of the previously available evidence.[1] To this it may be objected that usually in such a situation we do not maintain that our hypothesis really was highly probable, but only that it seemed highly probable, and that we were quite justified in supposing it to be highly probable. Indeed, Toulmin has argued very plausibly that the doctrine that probability is relative to evidence is founded upon a confusion between 'improper' claims and 'mistaken' claims. Our earlier estimate that our hypothesis was highly probable was mistaken, but not improper: Keynes and Kneale wrongly imagined that because our estimate was not improper, it could not have been mistaken.[2] This objection, however, does not seem to have made much impression upon adherents of the Keynesian approach. Thus Swinburne concludes that our tendency to say that our hypothesis only seemed, rather than was, highly probable is " a quirk of ordinary language without philosophical significance."[3] Once again we seem to be contending with apriorism. In fact - although this is not a subject I want to discuss in any detail here - it seems to me that there is partial truth on both sides of this argument. For it would seem that just as we must distinguish between

1. See Kneale (1), pp. 9 ff.
2. See Toulmin (1), pp. 57 ff.
3. Swinburne (1), p. 25.

'epistemic' and non-'epistemic' or 'objective' possibility, the former being concerned with the relations between assertions and the evidence available to support them,[1] so we must make a similar distinction with respect to probability (which is, of course, a concept intimately connected with the concept of possibility).[2]

For the time being, however, I shall overlook any objections to the 'logical' theory of probability. It then seems possible to speak of the probability of different hypotheses relative to the same body of evidence, or of the same hypothesis relative to different bodies of evidence, and so on. At this stage, adherents of the theory find it convenient to introduce symbolism. Thus, in many modern accounts, 'Pr(h,e)' represents the probability of hypothesis h relative to, or given, evidence e, and the formula

$$Pr(g,e) > Pr(h,e)$$

represents the fact that hypothesis g is more probable than hypothesis h relative to, or given, evidence e; and so on. Now all this seems harmless enough, perhaps. But next it is supposed that these symbols may be operated with in accordance with the axioms and definitions of the so-called 'probability calculus' (a not unnatural assumption, it may be felt, but in fact a very dangerous one). Thus it is commonly alleged that the 'syntactical' structure of the probability calculus may be given various different 'semantic' interpretations, one of which is in terms of these supposed 'logical' probability-relations between propositions. For example, one particularly important axiom of the calculus, the so-called 'multiplication axiom', is given the following 'interpretation':

$$Pr(p \,\&\, r,q) = Pr(p,q) \times Pr(r,p \,\&\, q) = Pr(r,q) \times Pr(p,q \,\&\, r),$$

where p, q and r are certain propositions. However, it is not often inquired as to whether there are any restrictions upon the sort of propositions which may meaningfully be substituted into this formula. Indeed, it is generally assumed that p, q and r may be any self-consistent

1. See pp. 66-7 above.
2. To be fair, Swinburne himself acknowledges such a distinction.

propositions whatever. We shall see that upon this assumption rests Keynes' argument that the probability of a universal generalisation must be raised by any evidence of hitherto unknown instantiations of that generalisation.

Rather than discuss Keynes' argument in the form in which he himself presents it,[1] I shall deal with Nicod's version, which is preferable from the point of view of simplicity.[2] Suppose we let h be the universal generalisation 'All F's are G's (where F and G are certain 'characteristics'), let e be an observation-statement to the effect that some particular F is a G, and let k be certain 'background evidence' which renders h and e probable to certain degrees. Then, substituting h, e and k into the formula just set out above, and putting

$$Pr(e,h \,\&\, k) = 1,$$

since a universal generalisation entails any statement asserting its particular instantiation, we arrive at the equation

$$\frac{Pr(h,k)}{Pr(h,e \,\&\, k)} = Pr(e,k).$$

This equation apparently implies that the probability of h must be raised by the addition of e to k, provided that

$$0 < Pr(h,k) > 1 \quad \text{and} \quad 0 < Pr(e,k) > 1.$$

These latter probabilities are usually referred to as the 'initial' or 'apriori' probabilities of h and e respectively.

Of course, the Popperian would object that if h is a universal generalisation and k, like e, is evidence of an empirical nature, then we inevitably have

$$Pr(h,k) = Pr(h,e \,\&\, k) = 0.$$

1. See Keynes (1), pp. 235-6.
2. See Nicod (1), p. 230.

However, it seems to me that this objection involves exactly the same misconceptions as those underlying the Keynesian argument itself, as I hope to make clear.

As I remarked earlier, it is commonly alleged that the 'syntactical' structure of the probability calculus may be given various different 'interpretations', of which one is supposedly in terms of 'logical' probability-relations between propositions. Thus another 'interpretation' is said to use the calculus as a system of 'rational belief'.[1] But it should not be forgotten that this calculus was originally derived from considerations of a fairly elementary sort concerning class-ratios. Consider, for example, the 'multiplication axiom' of the calculus. This is derived from an elementary theorem of class-ratio arithmetic, according to which, if we have a class C containing two sub-classes, A and B, then the proportion of members of C which are members of both A and B equals the proportion of members of C which are members of A multiplied by the proportion of members of A which are also members of B. We might express this theorem symbolically as follows:

$$AB/C = A/C \times A/BC = B/C \times B/AC.$$

Now, this theorem is of obvious utility in games of chance and similar situations, where we estimate probabilities on the basis of class-ratios. Thus we say that the probability of dealing a spade from a normal pack equals 1/4, because this is the proportion of spades in such a pack. Then we may use the above theorem, for example, to calculate the probability of dealing the king of spades, given the probability of dealing a spade and the probability that a spade is a king:

$$1/4 \times 1/13 = 1/52.$$

However, according to the 'logical' theory of probability, as we have seen, probability is a relation between propositions. Thus, according to this theory, the proper way of expressing the fact that the probability of dealing a spade from a normal pack equals 1/4 is

1. See, especially, Ramsey (1), pp. 166 ff.

to say that the proposition p, 'This card is a spade', has a probability of 1/4 relative to, or given, the proposition q, 'This (same) card is dealt from a normal pack'. This is then written symbolically as follows:

$$Pr(p,q) = 1/4.$$

If we next let r be the proposition 'This (same) card is a king' and substitute all three propositions into the formula which represents the multiplication axiom when the probability calculus is interpreted in terms of 'logical' probability-relations between propositions, then we can, as before, calculate the probability of dealing the king of spades, in virtue of the equation

$$Pr(p \& r,q) = Pr(p,q) \times Pr(r,p \& q).$$

This equation, when p, q and r have the meanings just assigned to them, is perfectly acceptable. But it would appear that this is only so because in this case the probabilities in question are estimated in terms of class-ratios. We have no reason whatever to suppose that this equation would be valid or even meaningful if p, q and r were just any self-consistent propositions whatever.

Nicod did not discuss this problem, but simply assumed that any self-consistent substitutions into the above formula are both meaningful and valid. Keynes, however, did discuss the matter in an oblique way. According to Keynes, not every probability-relation between propositions can be assigned, even in principle, a numerical value between 0 and 1.[1] And, indeed, this seems evident. For even if we accept the doctrine that probability is relative to evidence, no one is seriously going to suggest that, for example, Einstein's General Theory of Relativity has some exact numerical degree of probability relative to current experimental evidence. But if not every probability-relation is numerical, the problem arises as to how it can be said that all probability-relations can be operated with in accordance with the axioms and definitions of the probability calculus. For it is not at all clear

1. See Keynes (1), chap. III.

how non-numerical probabilities can be said to be multiplied and added. Keynes' solution to this problem - if we may call it a solution - was to maintain that the multiplication and addition axioms of the calculus serve to define certain primitive operations for combining probabilities, and that these operations only become multiplication and addition in the accepted sense when the probabilities in question are numerical.[1] But the objection to this is that these primitive operations are utterly mysterious and inexplicable, as is their metamorphosis into multiplication and addition. Ramsey was quick to point out such deficiencies in Keynes' theory, but his remarks seem to have been largely ignored.[2]

Thus it seems to me that Keynes gave us no good reason whatever to suppose that just any self-consistent propositions may meaningfully be substituted into the formula representing the multiplication axiom. Indeed, I see no reason to suppose that any propositions may meaningfully be substituted into that formula which are not analogous to the propositions p, q and r in our example concerning the pack of cards. The probabilities in question must be estimated in an exactly analogous fashion on the basis of class-ratios. After all, this formula, as we have seen, is simply an 'interpretation' of a theorem of class-ratio arithmetic, and hence any use which may make of it is only possible in virtue of some reference to class-ratios. Ramsey, who 'interpreted' the probability calculus as a system of 'rational belief', seems to have appreciated this point, since he acknowledged that the validity of his interpretation depended upon his being able to articulate a concept of degree of partial belief which carried a tacit reference to relative frequencies.[3]

However, if this is the case, it is relatively easy to show that Keynes' argument that the probability of a universal generalisation must be raised by any evidence of hitherto unknown instantiations of that generalisation, is invalid - or, rather, that it is only valid in circumstances which render it useless for the purposes to which adherents of the probabilistic theory of confirmation want to put it.

Let us recall Keynes' argument. We have the universal generalisation h ('All F's are G's'), the observation-statement e (asserting

1. See Keynes (1), chap. XII.
2. See Ramsey (1), pp. 160-66.
3. See Ramsey (1), p. 188. A relative frequency may, of course, be represented as a class-ratio.

that some particular F is a G), and certain 'background evidence' k. The contention is that it is possible to derive from the formula representing the multiplication axiom of the probability calculus the following equation

$$\frac{Pr(h,k)}{Pr(h,e\ \&\ k)} = Pr(e,k).$$

Now, I have just argued that propositions can only be meaningfully substituted into the formula in question under certain strict conditions: in particular, the probabilities concerned must be estimated on the basis of class-ratios. Thus, for instance, Pr(h,k), the so-called 'initial' or 'apriori' probability of h, must be estimated in some way on the basis of a class-ratio, and hence must be, in principle at least, a numerical probability. This stipulation in itself tends, of course, to undermine the relevance of Keynes' argument to scientific method since, as we have already remarked, many scientific hypotheses, such as Einstein's General Theory of Relativity, cannot seriously be supposed to possess numerical degrees of probability. Nevertheless, in the case of simple universal generalisations of the form 'All F's are G's' it is, in principle, possible to assign 'initial' or 'apriori' probabilities on the basis of class-ratios. In fact, there seem to be two quite different ways of assigning a probability to a universal generalisation on such a basis, both of which seem to have influenced adherents of the probabilistic theory of confirmation. But we shall see that only one of these ways is acceptable, and that this way is incompatible with the Keynesian argument.

Suppose that we are trying to confirm the universal generalisation 'All ravens are black'. Suppose also that we are able to establish (in some way which need not concern us at present) the following two conditions: (1) that the universe contains n ravens and (2) that for any individual raven there are m different colours which it may be (so that these colours form a finite set of mutually exclusive and collectively exhaustive alternatives). For simplicity let us suppose that m = 2, and that each raven may be either black or white. Then

if we calculate how many different possible constitutions this raven population may have, we shall find that they number 2^n in all. These different constitutions correspond to what Carnap calls 'state-descriptions'.[1] But if we count as identical all those constitutions which do not differ in respect of the proportions of black and white ravens which they contain, but only in respect of which individual ravens are black and which white, we shall find that the number of different constitutions is $(n + 1)$ in all. These different constitutions correspond to what Carnap calls 'structure-descriptions'.[2] One might accordingly define the 'initial' or 'apriori' probability of the hypothesis 'All ravens are black', under such conditions, either as $1/2^n$ or as $1/(n + 1)$, depending on whether one regards the different state-descriptions or the different structure-descriptions as being initially equiprobable. This, then, seems to be one way in which probabilities might be assigned to universal generalisations on the basis of class-ratios, in a fashion analogous to that employed in games of chance. And it seems clear that this is the model adopted by Carnap, although in a rather more sophisticated form.

However, it is evident that this model is quite inappropriate if we are dealing with the kind of universal generalisations with which science is principally concerned, namely, nomological propositions. In the first place, unless we assume that the extension of a universal generalisation is finite, it will follow, according to this model, that its 'initial' probability is zero. And since it is not implied by any nomological proposition that its extension is finite, Keynes' argument must be ineffective with regard to such propositions if we adopt this model, because it is a condition of that argument that

$$0 < \Pr(h,k) > 1.$$

This is one of Popper's main criticisms of the probabilistic theory of confirmation.

However, both this criticism and Carnap's model exhibit a misconception of a more fundamental nature. This is simply that it is quite mistaken to construe nomological propositions as concerning mere

1. See Carnap (2), pp. 70 ff.
2. See Carnap (2), pp. 116 ff.

collections of particulars, either finite or infinite in extension.[1] Thus the hypothesis 'All ravens are black' must be construed as asserting the existence of some causal connection between the two 'characteristics' ravenhood and blackness. Such an hypothesis cannot accordingly be regarded as being merely one of a set of different state- or structure-descriptions, as Carnap's model proposes.

For this reason, if we wish to assign an 'initial' probability to a nomological proposition on the basis of a class-ratio, we must adopt an entirely different model. Suppose that we are construing the universal generalisation 'All F's are G's' as a nomological proposition, that is, as a proposition asserting that the characteristic F is a sufficient condition, in the causal sense, of the characteristic G. Suppose also that we are able to establish (in some way which need not concern us at present) that F is one of a finite number of known characteristics, at least some of which are sufficient conditions, in the causal sense, of G. Let the total number of characteristics be n, and let the number of these which are sufficient conditions of G be m. Then we might define the 'initial' probability of the hypothesis 'All F's are G's' as m/n, that is, as being equal to the proportion of the total set of characteristics which are sufficient conditions of G (just as we define the 'initial' probability of dealing a spade from a normal pack as being equal to the proportion of cards in such a pack which are spades). We can thus say that the hypothesis h ('All F's are G's') has a 'logical' probability of m/n relative to, or given, the proposition k, that F is a member of the set of characteristics in question. This model for assigning 'initial' probabilities to universal generalisations has been adopted by Von Wright.[2]

This model is in principle perfectly acceptable. However, if we employ it, we must be very careful how we construe the proposition e which, together with h and k, is to be substituted into the formula representing the multiplication axiom. In fact, it is clear that if h is construed as a proposition concerning a sufficient condition, in the causal sense, of G, then e must also be construed as just such a proposition (just as, if h were a proposition about a card from a normal

1. See chap. VIII above.
2. See Von Wright (1), pp. 127 ff.

pack, e would also have to be a proposition about a card from a normal pack). In particular, e clearly cannot be construed as a proposition asserting the existence of a confirming instance of the universal generalisation h, that is, as asserting that some particular F is a G. Because if e were construed in this way, the symbol 'Pr(e,k)' would be quite meaningless. If probabilities are here to be estimated in terms of class-ratios, there can be no such thing as the probability that some particular F is a G relative to, or given, the proposition k, that the characteristic F is a member of a finite number n of characteristics, m of which are sufficient conditions, in the causal sense, of the characteristic G. This is a fact which Von Wright apparently fails to appreciate, since he persists in taking e to refer to a confirming instance of h.

In this model, then, we can only construe e as a proposition concerning one of the characteristics in the set to which F belongs, and either affirming or denying of this characteristic that it is a sufficient condition of G. This immediately suggests the idea of eliminative induction, however. For if we let e be a proposition asserting of some other characteristic in the set that it is not a sufficient condition of G, then we can indeed use the multiplication axiom of the probability calculus to show that the addition of e to k must raise the probability of h. This is quite obvious, since the 'posterior' probability of h, Pr(h,e & k), will equal m/(n - 1), which is greater than m/n, its 'initial' probability. It will be recalled, however, that an important step in Keynes' argument was the assumption that

$$Pr(e,h \,\&\, k) = 1.$$

This no longer holds, since h no longer entails e. Nonetheless, we can easily secure this condition simply by stipulating that of the finite number n of known characteristics, one and only one is a sufficient condition, in the causal sense, of G (thus rendering the characteristics mutually exclusive). It may be noted that this is precisely the assumption required, in eliminative induction, to

avoid the problem of the 'plurality of causes'.[1] Given this extra assumption, it is indeed permissible to write the equation

$$\frac{Pr(h,k)}{Pr(h,e\ \&\ k)} = Pr(e,k).$$

But this equation can no longer be used, of course, to support the Keynesian claim that the probability of a universal generalisation must be raised by any evidence of hitherto unknown instantiations of that generalisation.

It is a fact of some significance that the probability calculus can only be 'interpreted' as a system of eliminative induction, and not as a logic of confirmation. For this serves to stress the point that while eliminative induction is a process of deductive inference, ampliative induction (to which the concept of 'confirmation' is appropriate) is non-demonstrative.[2] The axioms of the probability calculus are, as we have seen, derived from certain mathematical theorems concerning relationships between classes: it is not surprising, then, that they can only be used for purposes of deductive inference. Thus the calculus can only be 'interpreted' as a system of enumerative induction when we are speaking of induction by complete enumeration, which is a deductive process. (This is why Carnap's model of confirmation only works with generalisations of finite extension.)

At this point it may be objected that the preceding arguments do not really affect the modern theories of probabilistic confirmation. Adherents of these theories conceive their purpose to be one of 'explicating' our intuitive judgments concerning the confirmation of hypotheses by empirical evidence. Now, it might be urged that this purpose is served very well by an equation such as

$$\frac{Pr(h,k)}{Pr(h,e\ \&\ k)} = \frac{Pr(e,k)}{Pr(e,h\ \&\ k)},$$

even if we construe h, e and k in the usual fashion, namely, as universal hypothesis, instantiation-statement, and 'background evidence'.

1. See p. 119 above.
2. See p. 121 above.

For, as I acknowledged earlier, there seems to be a general principle of confirmation that empirical data are only said to confirm a hypothesis when those data are predictable by that hypothesis but not by its rivals.[1] And from the above equation we can derive what appears to be a form of this principle, namely, the biconditional

$$Pr(h,e \,\&\, k) > Pr(h,k) \text{ if and only if } Pr(e,h \,\&\, k) > Pr(e,k).$$

For this biconditional simply asserts that the addition of certain empirical evidence e to the 'background data' k only raises the probability of h if the addition of h to k raises the probability of e (that is, makes e more 'predictable' than it was on the basis of k alone). Swinburne derives precisely this biconditional (which he calls 'principle C') from the equation set out above, and then proceeds to give an 'independent justification' of the biconditional by means of an intuitively plausible example.[2] He points out that Robinson's fingerprints on the safe will only be said to confirm the hypothesis that Robinson was the thief, if his fingerprints would not otherwise be expected to be there: once we know that Robinson is the manager of the shop, we do not regard the presence of his fingerprints as being evidence against him.

However, there are two points which we might make in objection here. Firstly, we may notice that while Swinburne derives his 'principle C' from the multiplication axiom of the probability calculus, it is clear that no 'independent justification' of that principle, such as Swinburne gives, can in any way justify his derivation of it from that axiom: it can at most justify the principle itself. Secondly, and more importantly, while Swinburne's example of Robinson and the fingerprints certainly does seem to illustrate a general principle underlying our judgments concerning the confirmation of hypotheses by empirical evidence, we should not overlook the fact that Swinburne actually expresses his 'principle C' in terms of inequalities between certain 'logical' probabilities. For this kind of example does nothing whatever to support the assumption that when we speak of empirical evidence e confirming or supporting an hypothesis h,

1. See p. 122 above.
2. See Swinburne (1), p. 48.

this is simply equivalent to saying that the addition of e to certain 'background data' k raises the 'logical' probability of h. Only this assumption can justify the move from

$$C(h,e \,\&\, k) > C(h,k) \text{ if and only if } C(e,h \,\&\, k) > C(e,k)$$

to

$$Pr(h,e \,\&\, k) > Pr(h,k) \text{ if and only if } Pr(e,h \,\&\, k) > Pr(e,k).$$

This, of course, was precisely the move objected to by Popper. In fact, it embodies the very notion to which I objected at the beginning of this chapter, namely, the notion that in virtue of the very meaning of the concept of 'probability' one can infer, from the discovery of empirical evidence which confirms a certain hypothesis, that that hypothesis is more probable than it was prior to that discovery. In view of this, I cannot agree with the modern opinion that the probability calculus may be used to 'explicate' our intuitive judgments concerning the confirmation of hypotheses by empirical evidence. The overall conclusion to be drawn from the investigations of the present chapter is that the probabilistic theory of confirmation is no serious rival to the theory of causal inference as an account of the fundamental processes of reasoning upon which scientific method is based.

XII : FALSIFICATIONISM

I shall conclude this thesis with some brief remarks concerning K. R. Popper's doctrine of 'falsificationism'. It would seem that one of Popper's principal objectives in advancing this theory was to resolve the 'problem of induction' by demonstrating that rationalism is compatible with Humean scepticism. Indeed, in the first chapter of his recent book, 'Objective Knowledge', Popper presents his doctrine precisely in the form of an answer to Hume. It is in this form that I shall criticise it.

According to Popper, Hume poses the following 'logical problem' and its solution: "Are we justified in reasoning from instances of which we have experience to other instances of which we have no experience? Hume's answer is: No."[1] Popper then 'reformulates' this problem and gives his own solution to it as follows: "Can the claim that an explanatory universal theory is true be justified .... by assuming the truth of certain test statements or observation statements? .... My answer to this problem is the same as Hume's: No (it) cannot."[2] I shall say more about this 'reformulation' a little later. Here we need merely notice Popper's readiness to accept Humean scepticism, a readiness which may be accounted for by his conviction that he can prove that rationalism is compatible with such scepticism. This conviction is based on his answer to a second 'logical problem' which he formulates as follows: "Can the claim that an explanatory universal theory is true or that it is false be justified by .... the assumption of the truth of test statements? To this problem my answer is positive: Yes, the assumption of the truth of test statements sometimes allows us to justify the claim that an explanatory universal theory is false."

What Popper wants to argue is that we can sometimes appeal to past experience to make a rational choice between competing theories. He explains: "Russell says that without solving the problem of induction we could not decide between a good scientific theory and a bad obsession of a madman."[3] Popper therefore asks: "Can a preference with respect to truth or falsity for some competing theories over

1. Popper (2), p. 4.
2. Popper (2), p. 7.
3. Popper (2), p. 8.

others ever be justified by .... 'empirical reasons'?" And his answer is: "Yes, sometimes it can, if we are lucky. For it may happen that our test statements may refute some - but not all - of the competing theories; and since we are searching for a true theory, we shall prefer those whose falsity has not been established."

This, then, is Popper's answer to Hume: although we cannot argue rationally from past experience to the truth of a universal theory, we can argue rationally from past experience to the falsity of a universal theory, and this at least enables us to choose rationally between competing theories. Furthermore, according to Popper, this allows us to say, in opposition to what he takes to be Hume's position, that we can make rational choices between different courses of practical action. Popper points out that "a man of practical action has always to choose between some more or less definite alternatives, since even inaction is a kind of action."[1] He then contends that although it would not be rational to rely on any theory as a basis for practical action, it would be rational to prefer as a basis for practical action the best-tested of our theories. As he says elsewhere, "the best-tested of our theories .... are the ones for which we have good reasons for believing .... to be the best available from the point of view of a search for truth or verisimilitude - (they are) the best among competing theories, the best approximations to the truth."[2] Thus, since the practical man must act on the basis of some theory or other, rational action consists in prefering that that theory which we have reason to suppose is the 'best approximation to the truth'.

Now, in the light of our earlier findings it is relatively easy to see that Popper's doctrine is defective as a solution to what he calls 'Hume's problem' and that he fails to appreciate all the logical consequences of his acceptance of Humean scepticism. In order to see this we must first of all recall how Popper 'reformulates' Hume's problem in terms of 'explanatory universal theories', for this reformulation helps to give Popper's 'solution' a measure of superficial plausibility while disguising its evasion of the real difficulty posed by Humean scepticism.

By an 'explanatory universal theory' Popper means a general

1. Popper (2), p. 21.
2. Popper (2), p. 95.

proposition or set of general propositions concerning one or more spatiotemporally unrestricted collections of particulars.[1] Such a general proposition typically simply ascribes a given 'characteristic' to each and every member of the collection concerned, and hence may be 'falsified' by the discovery of a single member of that collection which lacks the characteristic in question. But, of course, as Hume's formulation of the problem makes clear, we are chiefly interested in those members of the collection which have not been tested or observed: and it is evident that the 'falsification' of such a general proposition tells us no more about these than does its 'confirmation' by particular tests or observations. In short, if we are 'men of practical action', why should we prefer to use an 'unfalsified' theory as the basis of practical action, when it seems that the 'falsification' of a theory no more implies that that theory will be a bad guide to the future than its 'confirmation' could imply that it will be a good one? How can we call this a rational choice between competing theories? In what respect can we say with any assurance that an 'unfalsified' theory is a better 'approximation to the truth' than is a 'falsified' theory? Only, it seems, in respect of the tests and observations which we have actually made in the past. In view of this, Popper's attempt to combine rationalism with Humean scepticism must be regarded as a failure.

Popper's doctrine only assumes a superficial plausibility if we adopt an equivocal attitude towards the sort of general propositions which figure in 'explanatory universal theories'. I argued in an earlier chapter that Popper's analysis of such general propositions - 'nomological' propositions - is defective, and that these must in fact be construed as concerning not collections of particulars, but kinds of particulars.[2] Now, it is only if we construe nomological propositions, and hence 'explanatory universal theories', in this latter sense, that their 'falsification' really becomes significant as a 'guide to the future'. But, of course, if we do construe nomological propositions in this sense, their 'verification' by means of particular tests or observations (and even, in principle, by means of a single test or observation) becomes an acceptable notion.[3] The

1. See p. 101 above.
2. See p. 103 above.
3. See pp. 104-5 above.

supposed 'asymmetry' between 'falsification' and 'verification' as regards general propositions, of which Popper makes so much,[1] is really quite spurious: it seems clear that this doctrine has arisen through a failure to appreciate that there are two quite different sorts of general proposition 'concerning matter of fact and existence'.

Of course, Popper's objective in advancing the doctrine of 'falsificationism' was not purely to resolve the 'problem of induction'. Another important objective seems to have been that of providing a more realistic alternative to the traditional inductivist account of scientific method, by emphasising the creative character of scientific theory-construction. In this respect Popper's theory is more successful, and certainly seems to have appealed to practicing scientists.[2] However, while acknowledging this more fruitful aspect of Popper's work, we must point out, in view of the considerations of the present chapter, that the 'hypothetico-deductive method' cannot alone be exhaustive of rational procedures available to the scientist (or, indeed, to the 'plain man'). Popper's rigid refusal to countenance any form of non-deductive reasoning seems to have blinded him on this point.

1. See, e.g., Popper (1), p. 70.
2. See, e.g., Medawar (1), pp. 45 ff.

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