INDUCTION

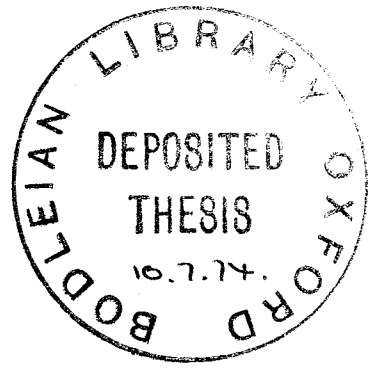
AND

NON-DEMONSTRATIVE

INFERENCE

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Introduction

This thesis is concerned with a certain kind of argument or reasoning: the kind of argument or reasoning which involves non-demonstrative inference. Up to date, most accounts of the nature of non-demonstrative inference have represented it as a species of inductive inference (1). However, I shall argue not only that most of our non-demonstrative inferences are non-inductive in character, but that we rely upon such inferences to justify all our inductive arguments. It may, then, seem rather paradoxical to say that the theory of non-demonstrative inference which I shall advance is substantially based on the ideas of Hume, who is commonly, and no doubt rightly, regarded as the founder of modern inductivism.

Now, in any process of argument or reasoning, one may distinguish a conclusion and a set of premises, though 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 (2). The primary task of any theory of rational inference is to elucidate the nature of such relations, which we may call 'inference-relations' (3).

An example of an argument involving demonstrative inference would be the syllogism: 'Socrates is a man; all men are mortal; therefore Socrates is mortal'. What characterizes 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 (4). Entailment is usually 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 (5).

Now, arguments involving non-demonstrative inference are negatively characterized by the fact that in them the conclusion is not entailed by the premises. An example of such an argument would be this: 'The kettle's been on the fire for the last ten minutes, so it should be boiling by now' (6). That this inference is non-demonstrative is shown by the fact that it would not constitute a logical contradiction at once to assert the premise and deny the conclusion (7). However, it is not enough 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 a positive characterization of the kind of relation which <u>is</u> involved in such inference. And this will be the primary task of this thesis.

However, some philosophers consider, in opposition to the views which I shall advance hereafter, that <u>all</u> genuine arguments must involve demonstrative inference, at least implicitly: in other words, that deductive reasoning is the only possible kind of reasoning (8). But it is not immediately clear on what grounds such philosophers hold this view. For there seems to be nothing in the concept of rational inference which necessarily restricts us to deduction as the paradigm of reasoning. Of course, the deductivist may allege that there is, as a matter of fact, no other kind of relation but that of entailment in virtue of which a conclusion may be said to follow from a set of premises. But how would the deductivist support such a claim? If one were to suggest a kind of inference-relation other than that of entailment, on what grounds would he object that it was not suitable? To object that it is always <u>possible</u> that inferences based on such relations might turn out to be mistaken would be to beg the question against

non-demonstrative inference, if the species of 'possibility' which is being appealed to is simply logical possibility. Yet the tacit belief that reasoning must be infallible in this logical sense is, I think, an assumption which underlies much deductivist opposition to the notion of non-demonstrative inference (9). However, in fairness to the deductivist, it must be acknowledged that all inductivist accounts of the nature of non-demonstrative inference have been singularly implausible and inadequate.

The structure of this thesis will be as follows. In Chapter 1, I shall discuss Hume's account of reasoning (10), which has had immense influence on modern theories of induction and which has led some philosophers to doubt the very possibility of non-demonstrative inference (11). In particular, I shall examine how Hume attempts to reduce causal inference to inductive extrapolation, by relying on certain sceptical assumptions which are really unwarranted (12). In Chapter 2, I shall discuss and criticise these sceptical assumptions. Finally, in Chapter 3, I shall try to revive the importance of causal inference and examine some of the presuppositions of inductivism in the light of this.

Chapter 1: Hume's Reduction of Causal Inference to Inductive Extrapolation

Preliminary Remarks Concerning Terminology

Section IV of the 'Enquiry Concerning the Human Under-standing' begins with the words: "All the objects of human reason or enquiry may naturally be divided into two kinds, to wit, Relations of ideas and Matters of Fact." Hume explains that the former constitute, typically, the subject-matter of mathematics and include "every affirmation which is either intuitively or demonstratively certain." It is hard to assign any very precise meaning to 'intuitive certainty' because of the subjective element which enters into this notion; but the nearest modern equivalent seems to be that of 'analytic truth'. Hume characterizes 'matters of fact' by saying that "the contrary of every matter of fact is still possible; because it can never imply a contradiction." Thus, in so far as it is possible to compare Hume's terminology with that of modern philosophers, Hume may be said to have concurred with the modern distinction between analytic and synthetic propositions (1).

Another important distinction made by Hume which is not, however, clearly reflected in modern terminology is to be found later in Section IV, where he 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" (2). Hume often also called the former 'reasoning apriori' and the latter 'probable reasoning'. This distinction of Hume's must not be confused with the modern distinction between demonstrative and non-demonstrative inference: Hume's distinction primarily concerns the subject-matter of arguments, that is to say, the logical status of their premises

and conclusions. In 'demonstrative' reasonings the premises and conclusions are analytic propositions, whereas in 'probable' reasonings they are synthetic propositions (3).

Indeed, it is not at all clear to what extent Hume could be said to have concurred with the modern distinction between demonstrative and non-demonstrative inference. It is certainly clear from many passages that Hume insisted that in any process of reasoning or argument there must be some inference-relation (or 'medium', or 'connexion' , as he usually calls it) 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 ..... an inference, if indeed it be drawn by reasoning and argument" (4). Hume also evidently believed that in the case of 'demonstrative' reasonings, such as mathematical demonstrations, this relation or connexion is 'intuitively certain' or 'necessary': or, as we should say, that it is the logical relation of entailment. The difficulty lies in interpreting Hume's views concerning the kind of inference which is typically employed in 'moral' reasonings, or 'probable' arguments. His overall conclusion seems to be the paradoxical one that in reality no such reasonings are possible at all, and that the only genuine arguments are 'demonstrative' ones (5). This is the position commonly referred to as 'inductive scepticism' (6). Nevertheless, at the outset of his account of reasoning in the Enquiry, Hume advances a very plausible theory of 'probable' reasoning: a theory which, however, he subsequently undermines by certain sceptical assumptions which, as we shall see, are really unwarranted.

Near the beginning of Section IV of the Enquiry, having made 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 a distinction, familiar to readers of modern works on induction and scientific method, between observation or memory statements on the one hand, and predictive or retrodictive statements on the other, and the familiar notion that in order to justify the latter, we must find some way of inferring them from the former. Such an argument I shall refer to as a 'transexperiential' argument (7); its premises I shall refer to as 'experiential' propositions and its conclusion I shall refer to as a 'transexperiential' proposition (8). 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 explain.

Two things may account for Hume's opinion. Firstly, although there are many syllogistic arguments concerning matter of fact and existence which are clearly not transexperiential arguments - such as 'Socrates is a man; all men are mortal; therefore Socrates is mortal' - Hume doubtless considered that all such arguments ultimately depend upon transexperiential arguments and can, perhaps, even be 'reduced' to them. For the universal premise of such a syllogism would be regarded by Hume as a transexperiential proposition itself. However, the paramount importance which Hume ascribed to transexperiential arguments can itself only be explained in terms of the assumption, 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.

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 argument which did not take us 'beyond' our experience, because such an argument could not tell us anything which we did not already know. This notion is founded upon the assumption, which is closely related to the empiricist principle I have just mentioned, that our experiential data are perfectly 'clear and distinct', requiring no sort of interpretation.

Both of these assumptions of Hume's - that factual knowledge must arise from experience, and that experiential data are clear and distinct - have become tacit presuppositions of inductivist theory. But Hume's adherence to them, unlike that of modern indictivists, could be explained and to some extent even justified by his adherence to the doctrine of ideas. The entire distinction between the 'experiential' and the 'transexperiential' which this doctrine helped to foster is, however, an invidious one, and productive of much confusion in philosophy even at the present time. This is despite the fact that today it is a mere commonplace to remark that observation is a 'theory-laden' activity (9).

However, in paragraph 22 of the Enquiry, Hume makes a most interesting claim: "All reasonings concerning matter of fact seem to be founded on the relation of cause and effect". What he appears to mean by this is that in all 'probable' arguments - and by this he means, as we have seen, only transexperiential arguments - the relation between the premises and the conclusion, in virtue of which the latter is said to follow from the former, is a causal relation. Hume gives several examples to substantiate this claim. It is interesting that all his examples are of transexperiential arguments to particular conclusions, rather than to universal or nomological conclusions, such as primarily interest modern inductivists (10). It is also interesting that the

relation or 'connexion' of which he speaks is conceived of as existing not so much between the propositions, as such, which form the premises and conclusions of such arguments, but rather between the matters of fact referred to by those propositions (11). Thus he says: "here it is constantly supposed that there is a connexion between the present fact and that which is inferred from it. Were there nothing to bind them together, the inference would be entirely precarious." This, incidentally, is one of the passages I referred to as indicating that Hume insisted upon some relation or connexion between the premises and conclusion of any genuine argument or process of reasoning.

According to Hume, then, whenever we infer the existence of some object, event or state of affairs A, which is 'beyond' our experience (either because we have not witnessed it, or because it has not yet occurred), from the existence of another object, event, or state of affairs B, of which we do have experience (either because we are presently observing it, or because we remember having observed it), we always make this inference upon the presupposition that a causal connexion 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 effects of the human make and fabric, and closely connected with it." Hume 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, in fact, highly plausible: transexperiential arguments - at least, to particular conclusions - certainly do seem to be founded on causal relations in the way he suggests (12). Secondly it is remarkable how far removed this account of transexperiential arguments appears to

be from any inductivist account. Nevertheless, Hume manages, in the course of a superficially lucid, but really rather confused argument, to reduce his account of causal inference to a substantially inductivist account of transexperiential reasoning. It is necessary to examine this argument, for two reasons. Firstly, because it helps to explain the origin of modern theories of ampliative induction; and secondly, because, by identifying the errors in the argument, we may be able to salvage, at least, the theory of causal inference, by showing that it need not necessarily be reduced to induction.

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." Hume is asking, then, how we can and do discover that a causal relation exists between two objects, events, or states of affairs A and B. The importance of this question, however, must now be explained.

One obvious reason why any particular argument might be regarded as unjustified or invalid would be if it were suspected that the conclusion did not really follow from the premises. In such a case, the only way to justify the suspected argument would be to produce a further argument supporting the claim that the required inference-relation really did subsist between the premises and the conclusion of the original argument. Thus, if probable arguments are founded, as Hume suggests, upon causal relations, they will only be 'justifiable' in this sense if it is possible to produce further arguments supporting or justifying causal propositions (that is, propositions asserting the existence of causal relations) (13).

Now, it must also be pointed out that while all processes of reasoning are potentially heuristic procedures (that is, methods of discovery),

not all heuristic procedures are processes of reasoning (14). But Hume is not, apparently, disputing that we do discover causal relations ("arrive at the knowledge of cause and effect"), but is only inquiring how we discover them. For if it should turn out that we cannot discover them through any process of reasoning, but must rely upon some other kind of heuristic procedure, it will follow that causal propositions can never be justified by rational argument, and from this it will follow that all 'probable' arguments are unjustifiable in the sense I explained (15).

Hume expresses his own opinion on this matter in paragraph 23. He declares: "I shall venture to affirm, as a general proposition, which admits of no exception, that the knowledge of this relation (of cause and effect) is not, in any instance, attained by reasonings apriori; but arises entirly 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 Hume's 'general proposition', he asserts that we can never discover causal relations by demonstrative arguments or reasonings apriori. The rest of Part I of Section IV is devoted to proving this claim and, as I do not have many remarks to make upon Hume's proof, I shall make them here.

The principal contention upon which Hume bases his proof that causal relations cannot be discovered by reasonings apriori is that the objects, events, or states of affairs which, in any given instance, we consider to be causally related, are always quite distinct - at least, as far as we can perceive (16). "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 given object, event, or state of affairs. Such thought-experiments, being wholly subjective, are, of course, worse than useless. But what Hume seeks to show is simply that given two events A and B which are supposedly causally related, we can perceive no 'necessary connexion' between them, such as would render the one, in modern terms, a logically necessary consequence of the other. For example, if one billiard ball hits another, there is nothing in the nature of this event which would render it a logical contradiction to say that it was followed by another event of whatever description we may care to imagine. To say that both billiard balls on impact were immediately transformed into pineapple jelly would not constitute a logical contradiction: we might regard such an outcome as impossible, but the impossibility is not logical(17). Of course, it may be objected to this that many words do carry extensive logical implications: if a 'ball' were to change into pineapple jelly we might hesitate to call it a 'ball' at all. However, not every feature of an object's possible behaviour enters into its very definition - if it did, we should have continually to be changing our definitions of things. With certain qualifications, then, we may agree with Hume's general point here (18). The consequence of it is that since demonstrative argument or reasoning apriori is only concerned with 'relations of ideas' - that is, with logically necessary or analytic propositions - such reasoning cannot in general inform us of causal relations between events, because the only kind of relations of which it can inform us - namely, necessary connexions or logical relations - cannot in general be perceived or imagined to exist between causally related events (19).

Now, this might be thought to imply that, in Hume's view, causal relations are not 'relations of ideas', since these are the

subject-matter of demonstrative reasonings. Then, given Hume's division of "all the objects of human reason or enquiry" into relations of ideas and matters of fact, we might take causal relations to be matters of fact; and, this being so, causal propositions would be justifiable, if at all, by 'probable' arguments (arguments concerning matter of fact and existence). Such an assumption concerning Hume's views might be very reasonable; but how far it would be correct is a matter I shall discuss in the next chapter.

In the second part of Hume's 'general proposition', which I quoted earlier, he asserts two things: firstly, that our knowledge of causal relations must always 'arise from experience' (20); and, secondly, that the kind of experience from which such knowledge arises must be experience of constant conjunctions of particular objects. I shall discuss these two points in turn.

Firstly, it should be remarked that in saying that our knowledge of causal relations 'arises from experience', Hume cannot be excluding the possibility that we may, at least sometimes, acquire such knowledge as a result of reasoning from experiential premises, since that is clearly one way in which knowledge may 'arise from experience'. (This, indeed, is the whole basis of transexperiential arguments.) However, it should be pointed out that if causal relations could be discovered by reasoning from experiential premises, the arguments employed would, of course, be 'probable' arguments (since experiential premises are synthetic propositions) and this would imply that causal relations are matters of fact. This point should be borne in mind.

The second point concerns the kind of experience from which our knowledge of causal relations arises, according to Hume. Now, one way in which knowledge may arise from experience is through simple

observation or inspection: some things we may discover simply by opening our eyes and looking (21). But Hume is insistent that causal relations cannot be discovered in this way: that we can never observe or experience a causal relation as such. Why Hume believed this is a matter I shall discuss later. However, what it means is that when two objects, events, or states of affairs, A and B, are causally related, all we can observe, according to Hume, is that they are conjoined - that is, that they exist or occur together in space and time (22). As far as observation or experience is concerned, then, we can never really perceive any difference between the conjunction of two causally related objects A and B, and the conjunction of two objects C and D which just happen, by coincidence, to exist or occur together. This is, prima facie, a remarkably implausible claim, and I shall discuss it in detail later. However, because Hume believed this, he considered that the only 'experience' from which our knowledge of causal relations could arise was "when we find that any particular objects are constantly conjoined with each other." That is, because we can never distinguish, by mere observation, a causal conjunction from a coincidental conjunction, all that betrays the existence of a causal relation to our senses is the fact that objects similar to A and B have, as far as we have observed, always been conjoined, whereas objects similar to C and D have not always been conjoined in our experience (23).

Now, a little while ago I remarked that in saying that our knowledge of causal relations 'arises from experience', Hume could not 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 experience in question, if such knowledge were to arise through reasoning in any instance, the premises of the arguments employed would have to refer to past constant conjunctions of particular objects which we have observed (24).

However, it was Hume's belief that knowledge of causal relations could not arise through reasoning, even from such premises. This he declares in paragraph 28: "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." Hume has already denied that causal propositions may be discovered or justified by demonstrative reasonings, or reasonings apriori. Now he is denying that they may be discovered or justified by probable reasonings, even by inferences from premises referring to observed past constant conjunctions. And this he promises to prove ("to explain and to defend"). In fact, as I shall show, he does nothing of the sort (25).

Although the argument which Hume produces takes up most of Part II of Section IV, it is in fact substantially contained in paragraphs 29-30. It opens with the remark: "It must certainly be allowed that nature has kept us at a great distance from all her secrets, and has afforded us only 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' (26). Thus Hume next says: "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."

Since Hume relies heavily upon this example in his subsequent argument, it would be as well 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 his subsequent argument. So he is taking it for granted that X is not only unobservable, but that we cannot 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. But the very question at issue is whether or not we ever can discover causal relations by reasoning from experiential premises (27). So, by relying on the example he has chosen, and by assuming a sceptical position with regard to 'secret powers', Hume has simply taken for granted from the outset the very point which his argument is supposed "to explain and to defend". Why Hume should have taken this for granted is a matter I shall discuss in the next chapter.

Hume's argument, then, is already a non-starter. However, it is of some interest to follow it through to its conclusion. Hume's next remark is: "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 mind or thought of which I would willingly know the foundation." In this passage one may see how Hume finally reduces his account of causal inference to a substantially inductivist account of transexperiential argument.

It will be recalled that according to Hume's account of causal inference, when we infer the existence of an object or event A, which is beyond our experience, from the existence of another object or event B, of which we have experience, we do so upon the presupposition that A and B are causally related. But now Hume tells us that this presupposition cannot be justified by any process of argument or reasoning. Having observed, in the past, a constant conjunction between objects or events similar to A and B, we simply assume A and B to be causally related and infer the existence of the one from the existence of the other, without being able in the slightest degree to justify our assumption (28). Causal inference thus simply amounts to an extrapolation from past experience: having had experience of a constant conjunction between certain kinds of objects or events, in the past, we simply expect a conjunction of similar objects in the future (29).

Having reduced causal inference to inductive extrapolation, Hume remarks, as we have seen: "Now this is a process of the mind or thought, of which I would willingly know the foundation." And strangely enough, he now proceeds to argue that inductive extrapolation cannot be a process of reasoning - even though it was only upon the assumption that causal inference could not be a process of reasoning that Hume reduced it to inductive extrapolation (30). Nevertheless, it is worth looking at Hume's argument.

In fact Hume seems to have two arguments against the rationality of inductive extrapolation, though he does not clearly distinguish them. 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 does not imply that all inductive arguments are really irrational or invalid: it only implies that all inductive arguments must be supported by arguments of another sort, as we shall see (31).

Hume's second argument against the rationality of inductive extrapolation is rather more complicated. 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." (32) It may be remarked that Hume gives no very clear reason for this (33). However, in paragraph 30 of the Enquiry, Hume produces a rather perplexing argument, paralleled in the Treatise, designed to show that such a supposition or principle 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 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 characterizes '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 demonstrative reasoning, which only concerns 'relations of ideas' (34).

But this means, as Hume points out, that "if we be, therefore, 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, says 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 extraordinary thing about this puzzling argument, which has, of course, convinced a good many philosophers, is that it is itself evidently circular. To demonstrate this more clearly, I shall once again

run over the steps which have led Hume to this conclusion.

At the outset of Section IV of the Enquiry, Hume advanced his account of 'probable' arguments, according to which they were founded upon causal inference. Then, in paragraph 29, we saw how Hume reduced causal inference to inductive extrapolation, on the assumption that we could not discover causal relations by reasoning from experiential premises. Next he tells us (without explaining why) that inductive extrapolation could only be a process of reasoning if it proceeded on the principle that the future will be conformable to the past. Finally, he argues that such a principle is indefensible because it cannot be supported either by demonstrative reasoning or by probable reasonings. But the only reason why it apparently cannot be supported by probable reasonings is that Hume has already assumed that probable arguments are reducible to inductive extrapolation, and that the latter could only be a process of reasoning if it proceededon the principle that the future will be conformable to the past. But neither of these assumptions has been independently justified - and, as we shall see, neither is, in fact, true (35).

We may conclude that neither of Hume's arguments against the rationality of inductive extrapolation is valid. However, what his arguments do seem to prove is that inductivism is an incoherent doctrine. They show that given the presuppositions of inductivism, inductive arguments are unjustifiable. Hume, who accepts these presuppositions uncritically, imagines that he has demonstrated the absolute unjustifiability of inductive arguments. But in fact, as I shall argue later, there are many circumstances in which inductive arguments are justifiable - and, moreover, in these cases inductive arguments do not proceed upon "the supposition that the future will be conformable to the past".

I have only discussed this argument of Hume's in detail because it is rather well-known and has convinced many philosophers. But, as I pointed out to start with, it in no sense fulfils Hume's promise "to explain and to defend" the view 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 must conclude that Hume has not proved, after all, that causal propositions cannot be both discovered and justified by 'probable' arguments. Indeed, if causal relations are 'matters of fact', even if they are unobservable, as Hume suggests, there is prima facie no reason why they should not be discovered by 'probable' arguments, at least in some instances. I am assuming, here, of course, that at least some 'probable' arguments do involve causal inference in the way Hume suggested.

It is impossible to carry the analysis of Hume's account of reasoning any further without settling the question, which I have so far evaded, of Hume's views concerning the logical status of causal relations - that is, whether they are 'relations of ideas' or 'matters of fact'. To this subject I shall now turn in the next chapter. As we shall see, Hume himself gives no very clear answer to this question.

When I discussed, earlier, Hume's proof 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 necessary connexions or 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, of course, have its original in Locke's 'real essences' (1); and 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 causal relations cannot be discovered by reasoning from experiential premises. We saw that that argument was a non-starter because it took 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, took causal relations to be opaque relations of ideas, can one 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, whether or not from experiential premises. So the fact that Hume could devote much of Part II of Section IV to a circular argument

to prove this very point can only suggest that he was not really entirely clear about the logical status of causal relations. A similar 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 the term 'causal relation' - that is, to the analysis of our concept of a causal relation. Thus, paragraph 49 of the Enquiry 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 meaning of these terms, and thereby remove some part of the obscurity, which is so much complained of in this species of philosophy". However, what Hume says here, and indeed the very title of the section, 'Of the Idea of Necessary Connexion', seems to imply that causal relations must, if anything, be relations of ideas.

For Hume, meaning and concept were 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 is the most extreme form of the doctrine 'nihil est in intellectu quod non fuerit in sensu', and it 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. The consequences of this mistaken doctrine 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 connexion: any quality, which binds the effect to the cause, and renders the one an infallible consequence of the other." 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 objects or events which we consider to be causally related, we can perceive no necessary or intuitively certain connexion, such as would permit us to infer the one from the other demonstratively. The second is the notion that causal relations are not possible objects of experience or observation. Hume's failure to distinguish these points can only be explained in terms of a supposition on his part that the only kind of connexion or relation between different matters of fact could be an analytic or necessary one, and thus that since we failed to perceive any such relations between objects or events which are considered to be causally related, causal relations, if they exist at all, must simply be opaque or 'secret' relations of ideas.

That Hume did indeed assume that the only kind of relation between matters of fact could be a necessary one 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."

However, if we separate these two points, we can see that while Hume's suggestion is perfectly plausible that we can perceive no necessary or analytic connexion between objects or events considered to

be causally related (2), his suggestion that we cannot ever experience or observe a causal relation is highly implausible, as I remarked earlier. This would imply, as we have seen, that we cannot, by mere observation, distinguish between a causal conjunction and a purely coincidental conjunction of events. As Hume remarks later, it means that "All events seem entirely loose and separate. One event follows another: but we never can observe any tie between them. They seem conjoined, but never connected" (3).

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 lighting (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? Surely, we can actually see the stone 'making a splash'?

The Humean would reply to this, no doubt, that we only imagine we can perceive a connexion between A and B which is lacking between A and C or C and B 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 has generally 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 coincidental conjunctions, it is hard to see how human infants could ever learn at all: the Humean theory of learning - associationism - is so evidently inadequate and implausible. The human mind must have an 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 (4). This is the 'reductio ad absurdum' of the Humean position. But, furthermore, the standard Humean reply naturally prompts the question as to why anyone should go to such extreme lengths to deny that we can ever really perceive a causal relation. Once again, the answer seems to be that adherents of this view imagine that if we could perceive causal relations, we would be able to fortell 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 necessary or analytic connexions(5).

Why did Hume and his followers neglect the possibility that causal relations could be synthetic connexions - that is to say, non-necessary or matter-of-fact relations? One might be able to experience or observe such relations between matters of fact, without thereby being able to fortell the outcome of events with demonstrative or logical certainty. Indeed, what other 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 (6). This notion, and Hume's neglect of synthetic connexions can, perhaps, only be explained by his adherence to the doctrine of ideas, and particularly to 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 implies

that the only relations really perceivable (and hence conceivable) by us must be relations of ideas. But the kind of connexion 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 relation between colours, shapes, smells or sounds(7). However, because of this, Hume felt compelled to deny that we could have any concept of such a relation or connexion. But, if we consider the matter without prejudice, what is there at all objectionable in the notion of a synthetic connexion, or non-necessary relation, between distinct objects, events or states of affairs?

But at this point it may be asked what exactly is a causal relation, granted that it is a synthetic or matter-of-fact connexion between objects, events, or states of affairs? We might, of course, simply characterize it as a relation of 'production' or 'generation': when two events or states of affairs are causally related, one may be said to 'produce', 'generate' or 'bring about' the other, in suitable circumstances. The Humean will, of course, interpose here that such a definition is quite empty, because 'cause', 'produce', 'generate' and 'bring about' are all synonymous. Hume, indeed, says exactly this (8). But this is no valid criticism of the view which I am advancing. I do not care 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 save the most hardened positivist would suggest that the lack of a definition in this latter case renders the concept in question meaningless or unintelligible. In other words, the concept of a causal relation, although it cannot be explicitly defined in terms which do not tacitly presuppose it, may be learned and understood through experience of particular causal relations, such as that between the falling stone and the subsequent splash.

Of course, the view which I am advancing by no means suggests that between any two causally related events we can always perceive the causal connexion; or even that when we can perceive the causal connexion, we can always perceive all there is to know about that connexion. Far from it. My view only demands that we should be able to perceive some causal connexions, such as that between the stone and the splash. But even here, there may be many features of this causal connexion, known, perhaps to science, which we cannot perceive, any more than we can perceive all there is to know about a material object, such as a stone. But no one supposes that the latter fact implies that we cannot ever really perceive stones at all! (9) However, all this would be quite evident were it not for the philosophical prejudice in favour of those relations which are 'relations of ideas' - that is, logical relations. Logical relations are indeed 'transparent'; Hume correctly saw that causal relations lack this quality, but erroneously assumed that they must therefore simply be 'opaque' logical relations.

So far I have only discussed causal relations as 'synthetic connexions' in a highly abstract way: in a way which the 'plain man' would never employ, any more than he would talk about 'material objects' as the philosopher does (10). But in order to bring the discussion a little more down to earth, we must delve a little way into the philosophy of perception.

Now, many philosophical theories of perception tend radically to oversimplify the nature of 'the objects of the senses'. Thus the idealist tends to say that what we 'really' or 'immediately' perceive are colours, shapes, smells, tastes, sounds and textures, - often referred to generically as the 'sensible qualities' (11); and that when we speak of perceiving objects, such as trees and books and tables, we

are really referring to a rather more complicated act of mind than simple perception - an act of mind which certainly involves the perception of sensible qualities, but which also involves some unconscious process of inference or association of ideas (12). The so-called 'naïve realist', reacting against this analysis, asserts that what we 'really' or 'primarily' perceive are, after all, 'material objects' such as trees and books, and tables. But this view, too, is excessively restrictive. The features of the world which we may be said to perceive fall into many diverse categories, and to say that we 'really' or 'primarily' perceive material objects is just as absurd as to say that we 'really' or 'immediately' perceive sensible qualities. (13) Thus, if we consider first of all perceptible particulars, it is evident that we perceive not only things, such as trees and books and tables, but also a variety of rather ephemeral phenomena, such as rainbows shadows, reflections and flames; then we perceive diverse spatial features, such as holes, cracks, corners and edges, which are clearly neither 'things' nor 'phenomena'; and finally we perceive various particulars such as rivers, pools, drops, bundles and heaps, which do not fit comfortably into any of the previous categories. But quite apart from particulars, we also perceive substances, attributes, and relations (to use a somewhat scholastic terminology). Thus, we perceive substances such as gold, water, blood and wood. We perceive qualities, such as redness, roundness, pungency, loudness and warmth. We perceive properties, such as elasticity, malleability, ductility, brittleness, stiffness and fluidity. We perceive spatial relations, such as when we perceive that one particular is behind another, underneath another, inside another, or between two others. Finally, we also perceive the behaviour or activity of things, such as when we perceive a thing falling, rolling, flying, wriggling, crawling or shaking. Now it is to this last category of perceptible features of the world that

'causal relations' properly belong. The 'activities' which I have so far mentioned are all non-causal in character, in that they typically involve only a single particular: but other 'activities' are causal or relational in character, in that they involve the interaction of two or more particulars. Examples of 'relational behaviour' would be: one thing pushing another, pulling another, breaking another, hitting another, bending another, melting another, or dissolving another. Recalling for a moment our earlier example, we may also say that 'making a splash', or 'splashing' is a causal or relational activity (14).

When 'causal relations' are simply regarded as 'relational activities', they appear far less mysterious and occult than they do when discussed in an abstract way by philosophers. To say that causal relations are 'synthetic connexions' is simply to say that whether or not, for example, one billiard ball is pushing another, is a matter of fact, to be determined empirically. And it no longer seems remotely plausible to deny that we can ever perceive causal relations. After all, few would seriously deny that we can perceive non-causal activities: we can perceive a stone falling or a billiard ball rolling. Why, then should it be denied that we can perceive a stone making a splash, or one billiard ball pushing another? (15)

Let us, however, look a little more closely at Hume's famous example of the billiard balls. I am asserting that we may perceive here not merely certain sensible qualities, such as the redness and roundness of the balls and the greenness and rectangularity of the table; nor merely certain material objects, namely the billiard balls and the table; nor merely certain motions of these objects (motion being a typically non-causal activity): we may perceive all these things, but we also perceive one billiard ball hitting and pushing another. But the most that Hume and his followers will admit is that we may perceive

the billiard balls and the table and their various qualities and relative motions. It is even denied that there is anything more to the situation than various objects in relative motion: as though not even the deepest scientific investigation could reveal anything more than this (16). But this is simply false. Science is not concerned merely with 'matter in motion', that is merely with kinematics: it is also concerned with dynamics and 'energetics'. It is not enough, in physics, to say how the billiard balls moved: it is necessary also to consider the various interchanges of energy involved. Two situations may be kinematically identical, and yet differ radically energetically. Thus all the motions of the billiard balls might equally be produced by a complicated system of electro-magnets hidden under the table: but in that case the 'energetics' of the situation would be radically different. Now, what physics describes as a certain kind of energy-interchange, the 'plain man' simply describes as hitting and pushing (17). So not only is there more to the situation than merely 'matter in motion': this 'more' may actually be perceived (18).

It seems, then, that the uneasiness which many philosophers have felt over the nature of causal relations and our ability to perceive them, has stemmed in large measure from a tendency towards an unduly restrictive ontology (19). However, it should be remarked that there is no one identifiable relation which is the causal relation, any more than there is one identifiable quality which the quality of things: causal relations - or, causal activities - belong to a 'family', having 'family resemblances' (20), but no unique and determinate (and hence rigidly definable) character. This is why it is futile to seek, as Hume sought, for a definition of the term 'causal relation'.

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 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" (21). 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 objects or events of a particular kind. 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 arise, 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, historically, even though it would appear that Hume himself considered the subjective element the more important (22). According to this first 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" (23). This definition would imply that whenever we assert the existence of a causal relation between two objects or events A and B, the objective content of our assertion amounts to the claim that A and B belong to classes of objects or events such that, as a matter of fact, members of these classes have been and will be always and everywhere conjoined (24). This amounts to a claim that a certain uniformity exists throughout the universe in both space and time. I need hardly remark how far-fetched it is to suppose that we really intend such a claim whenever we assert that two objects or events are causally related (25).

Nevertheless, many philosophers of the positivist school followed Hume in adopting precisely such a definition of causal relations (26). As I remarked earlier, such a definition makes causal relations matters of fact, but of a rather strange kind. For they are not conceived of as particular synthetic connexions between particular objects or events, in the way that I and common-sense suggest they should be, but rather as matter-of-fact universal uniformities. What is strange about such uniformities is that, in the absence of particular synthetic connexions, they are utterly inexplicable and unintelligible. This is a matter I shall discuss further in the next chapter.

However, it is evident that Hume, unlike the later positivists, considered that his two definitions of 'cause' were not really adequate - in the sense of reflecting the real nature of the connexions between matters of fact, if such there be (and this, of course, we can never know, according to Hume): these, he was still convinced, must really be secret relations of ideas or necessary connexions. His definitions were simply meant to represent the best concepts of causation 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 surprizing 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" (27). Perhaps if Hume had perceived the error of the very attempt to define 'cause', he would not have wreaked such havoc in philosophy.

Chapter 3: Induction and Non-Demonstrative Inference

In the first chapter of this thesis I attempted to show that Hume's reduction of causal inference to inductive extrapolation was unwarranted. In the second chapter I argued that causal relations should properly be regarded as 'synthetic connexions' and, moreover, that causal relations are, at least sometimes, perceptible. Combining these conclusions, we may begin to revive Hume's account of causal inference as a non-inductive theory of non-demonstrative inference. In doing so, however, we shall have to reject two more of Hume's assumptions: the assumption that transexperiential arguments are the only important species of arguments concerning 'matter of fact and existence'; and the assumption that in all transexperiential arguments the premises must refer, either explicitly or tacitly, to observed constant conjunctions between particular kinds of objects, events, or states of affairs. A transexperiential argument, it will be recalled, we defined as an argument from experiential premises - observation or memory reports - to a transexperiential conclusion - a predictive or retrodictive proposition (1).

I am proposing then, that when we are arguing about matters of fact, whether or not our premises are experiential and our conclusion transexperiential, we generally argue on the basis of known or supposed causal relations between the matters of fact referred to in our premises and those referred to in our conclusions (2). For instance, consider an example I mentioned earlier: 'The kettle's been on the fire for the last ten minutes, so it should be boiling by now'. Our inference here appears to be founded simply upon the fact that fire, given time, makes water boil. Now, there are certainly occasions on which this argument

could be classified as 'transexperiential': for example, when we remember having put the kettle on the fire ten minutes ago, and are predicting that it will be boiling by now. However, it should be remarked that even when this argument is transexperiential, its premise clearly does not refer, either explicitly or tacitly, to an observed or remembered constant conjunction between fires and boiling kettles: it simply states the fact that this kettle has been on the fire for the last ten minutes (3). But, while the argument may be transexperiential on some occasions, on others it may clearly not be. It is not necessary that the premise be an experiential proposition - we may not have observed or may not remember the kettle being put on the fire, but may have inferred, by means of another argument, that this was done. Nor is it necessary that the conclusion be a transexperiential proposition: we may simply be pointing out 'So it should be boiling by now', even though we can perfectly well see that it is boiling. Moreover, as I remarked earlier, it is simply fallacious to suppose, as many empiricists have supposed, that experiential propositions can never stand in need of rational justification or evidential support. It is easy to imagine a situation in which one person reports that he sees the kettle is boiling but another person doubts the veracity of this report; or a situation in which a person thinks he sees that the kettle is boiling but 'doubts the evidence of his own eyes'. In such situations an 'experiential' proposition may even be supported by 'transexperiential' or 'theoretical' considerations, such as estimates of 'probability'. On the whole, then, the distinction between experiential and transexperiential propositions is of doubtful significance for the theory of non-demonstrative inference. There can be no hard and fast line between matters of fact which we discover by observation or experience and

matters of fact which we discover only through the mediation of reasoning or argument. This, of course, has important repercussions not only for the theory of rational inference, but also for the theory of perception.

However, let us return to the causal basis of non-demonstrative inference. I have just asserted that when we argue 'The kettle's been on the fire for the last ten minutes, so it should be boiling by now', our inference is founded upon the fact that fire, given time, makes water boil. The inductivist will immediately ask: but how do we know this fact? by observation or by inference? But implicit in this question is the assumption that knowledge must be founded either upon observation or upon inference: the very assumption which I have just rejected. In reality no clear-cut answer is possible to such a question. Can we see that fire makes water boil? I should say that we can. But in saying this I do not mean to imply that a new-born infant could see this. In fact, it seems to me neither more nor less problematic to say that we can see that fire makes water boil than to say that we can see that fire is red and water transparent. In saying this, I am both denying that causal relations are occult and mysterious, and asserting that our perception of 'qualities', such as redness and transparency, is by no means as straightforward as many empiricists seem to have supposed. But to explain precisely how it is that we come to see that fire makes water boil or that fire is red, is a matter which calls more for the skills of the psychologist than for the speculations of the philosopher.

Now, I remarked earlier that the justifiability of inferences founded upon causal relations depends upon the justifiability, in general, of causal propositions, and that this, in turn, only requires that it should at least be possible to discover causal relations by means of reasoning

or argument (4). However, while it may well be that most people discover or come to know that fire makes water boil simply through observation or experience, this does not imply that it would be impossible to discover or come to know this fact through some process of reasoning or argument. Indeed, since causal relations are simply matters of fact, there can in principle be no reason why any causal proposition should not figure as the conclusion of a causal inference. That is to say, it is in principle no more problematical to justify by reasoning the proposition 'the fire is making the water boil' than it is to justify by reasoning the proposition 'the kettle should be boiling by now'.

Of course, so far I have only discussed everyday patterns of inference and have not touched upon scientific theorizing. However, I see no reason to suppose that scientific inference is fundamentally any different from the reasoning of everyday life. There, too, 'relational activity' is of paramount importance, as was indicated by the 'energetics' of the billiard-ball example. Consider the physicist who infers, from the motion of a magnet in the vicinity of a coil of copper wire, than an electic current is flowing in the wire. The inference is founded upon the fact that the moving magnet induces an electric current in the coil. It may be asked: can this fact ever be actually perceived? The instrumentalist will naturally answer: 'No - electric currents are imperceptible, and all that we may ever perceive in such a case is a concommittance between the motion of the magnet, and the motion of a pointer on the scale of a galvanometer.' However, in recent years philosophers have increasingly doubted whether we can coherently speak of any sharp dichotomy between 'observable' and 'non-observable' entities, and it has been suggested that it is perfectly legitimate to speak of observing such things as electric currents, magnetic fields and atomic particles (5). And if it is possible to perceive electric

currents and magnetic fields, then equally it may be possible to perceive a moving magnet inducing an electric current in a coil. However, whether or not such things are 'perceptible', it is clear that the scientist does discover, in one way or another, that moving magnets induce electric currents. And what he discovers here is a certain relational activity involving magnets and wires and electric currents: he discovers a certain kind of interaction between these entities, not a mere 'concommittance' between phenomena.

This still, of course, leaves open the question as to precisely how the scientist discovers that moving magnets induce electric currents. But I shall not attempt to answer this in any detail. However, it is clear that experiment and reasoning are both involved: but not in such a way that one can clearly separate what is 'observational' from what is 'inferential'. It is also clear that much of the reasoning involved will be of the non-deductive variety which we have been discussing, and will thus be based upon a knowledge of causal relations. But, as I have said, there is nothing paradoxical in the fact that we may discover one causal relation through reasoning which is based upon a knowledge of other causal relations. The growth of knowledge clearly exhibits a hierarchical structure. Of course, a 'complete' answer to the question 'How did scientists discover that moving magnets induce electric currents?' would require a book in itself.

Here it may be objected that in discussing the discovery of electrical induction, I am neglecting the question of justification. It is fashionable in contemporary philosophy of science to distinguish between a 'context of discovery' and a 'context of justification' (6). Many philosophers of science consider that the way in which scientific theories have been discovered is purely a matter for the psychologist and the historian, and that scientific reasoning solely concerns the

deductive consequences of theories and their correspondence with observation and experiment (7). However, while I would agree that the discovery of scientific knowledge is partly a matter for the psychologist, I do not regard this as implying that discovery is a wholly irrational or arational affair. The antithesis between the 'logical' and the 'psychological' is a spurious one, derived from an unwarranted regard for deductive reasoning as the paradigm of 'logicality' (8).

Naturally, I do not mean to deny that there is a 'context of discovery' and a 'context of justification'. I only deny that 'reasoning' is restricted to the latter context - though I also repudiate the view that the 'logic of discovery' and the 'logic of justification' are distinct modes of reasoning (9). The fact is that we use reasoning or argument for various different purposes - for example, to justify claims or to persuade people to accept them, and to explain known facts or to discover unknown ones. But the same argument may, on different occasions, serve any one of these purposes (10). We may see this even with our simple argument about the boiling kettle. Thus, on one occasion I may claim that the kettle should be boiling by now, but someone else may query this. In this case I may justify my claim by pointing out that the kettle has been on the fire for the last ten minutes. But on another occasion I may not know whether the kettle is boiling until I suddenly remember it being put on the fire ten minutes ago, and in this case I make a discovery. On yet another occasion someone may ask me why the kettle is boiling, and by telling him that it has been on the fire for the last ten minutes I shall be giving him an explanation (11). But in all these cases, and in various others which we can imagine, the same argument is being used, albeit for different purposes.

In conclusion, those very arguments by whose aid electrical

induction was discovered may serve, in another context, to justify our acceptance of the theory of electrical induction - though, no doubt, other arguments may also be adduced.

In the remainder of this chapter I shall contrast the theory of causal inference with the approach of inductivism. For this purpose I shall begin by concentrating on Russell's well-known account of induction in 'The Problems of Philosophy', as this brings out very clearly many of the presuppositions of the inductivist approach to knowledge and reasoning.

It is immediately clear from Russell's introductory remarks to the chapter on induction that he adheres unquestioningly to the empiricist principle that our knowledge may be sharply divided into the experiential and the transexperiential, and that the latter is dependent upon some form of inference from the former (12). Russell employs as his prime example of inductive reasoning the kind of argument we might, in his opinion, advance in support of the predictive proposition that the sun will rise tomorrow.

Russell first declares: "It is obvious that if we are asked why we believe that the sun will rise tomorrow, we shall naturally answer 'Because it always has risen every day'. We have a firm belief that it will rise in the future, because it has risen in the past." However, what Russell says here is, to all appearances, simply an account of the causes of our belief that the sun will rise tomorrow. As such, it may indeed be correct in some cases, though clearly not in all. People may acquire such a belief not merely through 'custom or habit' but also through being taught or through reasoning it out for themselves. A belief may even be instilled by 'brainwashing', or be acquired in a moment of 'inspiration'. But all this is obviously irrelevant to the question of how such a belief may be justified by

reasoning or argument. We may notice the ambiguity which lies in any question as to why we have a given belief. For this may be interpreted either as a question concerning the causes of that belief, or as a question concerning the reasons we might advance in support of that belief. And even if a belief has been acquired irrationally, this does not imply that it cannot subsequently be rationally justified (13).

However, Russell next says: "If we are challenged as to why we believe that it will continue to rise as heretofore, we may appeal to the laws of motion: the earth, we shall say, is a freely rotating body, and such bodies do not cease to rotate unless something interferes from outside, and there is nothing outside to interfere with the earth between now and tomorrow." Here Russell is apparently no longer discussing the causes of our belief, but is offering a genuine argument in support of the proposition that the sun will rise tomorrow. This argument, indeed, superficially seems to involve causal inference: it is founded upon the supposition that something external must interfere with the earth in order to stop it rotating - and 'interfering' and 'stopping' are evidently terms denoting 'relational activities', that is, causal relations.

However, the fatal twist in Russell's account appears immediately afterwards. He remarks: "Of course, it might be doubted whether we are quite certain that there is nothing outside to interfere, but this is not the interesting doubt. The interesting doubt is as to whether the laws of motion will remain in operation until tomorrow. If this doubt is raised, we find ourselves in the same position as when the doubt about the sunrise was first raised. The only reason for believing that the laws of motion will remain is operation is that they have operated hitherto, so far as our knowledge of the past enables us to judge".

We should notice here first that if we take Russell at his word and suppose that he <u>really</u> means that with regard to our beliefs concerning the laws of motion "we find ourselves in the same position as when the doubt about the sunrise was first raised", then we can only suppose that in saying that "the only reason for believing that the laws of motion will remain in operation is that they have operated hitherto", Russell is once again merely offering an account of the <u>causes</u> of our belief, since that was what he was apparently offering "when the doubt about the sunrise was first raised." But in that case, what he says would be quite irrelevant to the question of how we might justify by reasoning or argument our belief in such laws.

But, furthermore, and more importantly, what Russell says presupposes that when we justify by reasoning a predictive proposition, such as that the sun will rise tomorrow, we in general do so by inferring it demonstratively or deductively from nomological propositions, such as the laws of motion, which have previously been established by induction (14). This is why I said that the argument he offered in support of this prediction was only superficially an example of causal inference. In fact, it is a case of deductive inference from 'laws'. Now, it is my view that deductive inference from laws is logically posterior to causal inference. In order to establish this, however, we must discuss the notion of a 'natural law'.

Now, the nomological propositions of science vary from the very simple to the highly complex. Two of the more complicated forms of laws are so-called statistical generalizations and so-called laws of functional relationship. The latter, in particular, are frequently to be found in the more advanced sciences: indeed, the 'laws of motion' are such laws (15). But the very simplest kind of natural law is one of the form 'All A's are B', where A and B are two distinct properties, qualities, classes of events or species of objects - often simply called 'characters'

or 'characteristics' in inductivist literature (16). A frequently cited example of such a law is 'All ravens are black'. Such laws may be called classificatory or taxonomic laws. Simply because they are the least complex kind of law, much attention has been paid to them by philosophers of science, even though they do not possess a comparable importance in actual scientific procedure. However, such laws have an added philosophical importance in that they are often interpreted as causal laws, that is laws stating that a character A is a cause of another character B (17).

However, the analysis of nomological propositions has been a matter of considerable controversy in modern philosophy. Some philosophers, such as F.P. Ramsey, have held that nomological propositions are really disguised 'prescriptive rules' (18). However, this view is generally considered to be unsatisfactory (19). Most philosophers appear to be divided into two camps. Many, such as K.R. Popper, consider that nomological propositions are merely propositions of 'unrestricted universality', asserting the existence, as a matter of fact, of some uniformity or regularity amongst phenomena, which extends throughout the universe in both space and time (20). Others, such as W. Kneale, argue that this analysis is inadequate, particularly since it fails to account for the fact that scientists appear to speak as though nomological propositions entail 'counter-factual conditionals', which mere statements of uniformity apparently cannot do (21). Kneale accordingly regards nomological propositions as 'principles of necessitation' (22). It is interesting that in this division between modern philosophers over the analysis of nomological propositions, we may see a reflection of Hume's ambivalent attitude towards the nature of causal relations: whether they should be

interpreted in terms of 'constant conjunctions' or in terms of 'secret necessary connexions'. Needless to say, just as I have repudiated both of these Humean views, so I shall repudiate both of these modern analyses of natural laws. However, we may remark that according to both of these modern analyses, nomological propositions are necessarily 'transexperiential': according to the first view, because no finite being can have experience of the entire universe; and according to the second view because the necessity of natural laws is, supposedly, incorrigibly opaque to the human understanding (23).

That Kneale's analysis of natural laws is mistaken is, I think, evident from our discussion of Hume's doctrines (24). Such a view could, I believe, only arise, as in Hume's case, from a prejudice against the possibility of 'synthetic connexions' between matters of fact. But the other, and more popular view is also mistaken; and, though it is mistaken in a less obvious way it is, I think, ultimately mistaken for the same reason, namely, from a prejudice against the possibility of 'synthetic connexions'. For this view assumes that all that can possibly connect two particular events is the universal fact, if it is a fact, that events of these kinds are always, as a matter of fact, to be found together. But how could such a universal uniformity be explained? According to the 'deductive-nomological' account of explanation, which is typically espoused by the philosophers whose views we are examining, it may be 'explained' only by inferring it deductively from other nomological propositions of wider scope or generality (25). However, it is evident that this will not satisfy our curiosity, but only stave it off a little further.

Now, I would suggest that a universal uniformity can only be explained satisfactorily as being a consequence of the nature of the objects, events, or states of affairs to which it refers, or which

'fall under it'. For example, consider the uniformity of blackness amongst members of the raven species. This may be explained by showing that there exists some sort of causal connexion between being a raven and being black. The precise nature of this causal connexion may be discovered by geneticists and biochemists. Locke was not wrong in supposing that the 'superficial' qualities of things 'flow' from their 'inner constitutions' - in the case of ravens, from their genetic structures - but only wrong in supposing that these 'inner constitutions' are forever indiscoverable, and that the 'superficial' qualities 'flow' from them with mathematical or logical necessity. Here, then, lies the distinction between 'accidental' and 'lawlike' uniformities. It is difficult for philosophers of Popper's school to account for such a distinction, because of their prejudice against the possibility of causal relations, conceived of as particular 'synthetic connexions' rather than in terms of universal uniformities. For if lawlike uniformities are to be distinguished from accidental uniformities in virtue of causal relations, this distinction will be quite empty if causal relations themselves are construed in terms of universal uniformities (26).

Indeed, I would advance it as a general principle, which ironically seems to accord better with the tenets of empiricism than do the other views I have mentioned, that so-called natural laws are no more than abstractions from particular matters of fact, and as such can tell us no more about the nature of the world than is, in principle, discoverable in any of the particular matters of fact to which they refer, or which 'fall under them'. As such, there is no special significance in the fact that natural laws are commonly expressed in the form of 'all-statements', as Popper calls them (27). For example, consider again the putative natural law 'All ravens are black'. According to the view which I am advancing, this law, despite its

misleading form, tells us no more about all ravens than it tells us about any particular raven. It merely implies that there is some sort of causal relation between being a raven and being black. But this causal relation exists as a particular 'synthetic connexion' in each and every member of the raven species (28): indeed, it is a feature of the genetic structure of every creature which can rightly be said to belong to the raven population (29). One may thus in principle discover the law simply by examining, in a suitable way, any particular raven (30). Of course, this is not to deny that in general the truth of the law becomes apparent to us in virtue of the fact that all the various ravens which we see are conspicuously black. However, it is fallacious to identify the meaning of a proposition with the evidence which leads us to regard it as true (31).

We may now return to Russell's account of the way we might justify the proposition that the sun will rise tomorrow. Implicit in his account, it seems to me, is the bizarre notion that natural laws exercise some sort of active role in determining the course of natural phenomena. This superstition may perhaps derive from the older concept of a natural law as an edict of God, in accordance with which His creatures would obey His divine will. But scientific laws are not 'operative' - to use Russell's term - in the sense in which the laws of the land may be said to be. Scientific laws do not constrain objects to behave in certain ways, in the sense that the laws of the land might be said to constrain persons to behave in certain ways (32). On the contrary, as I have suggested, laws are determined by facts, not facts by laws. That is to say, a given natural law is discoverable because certain particular objects behave in certain ways: but that those objects should behave in those ways is determined not by laws, but by their own natural constitutions and by the behaviour of other objects.

Thus, when Russell asks how we may justify our belief that the laws of motion will continue in operation until tomorrow, this seems to be like asking how we may justify our belief that the laws of the land will not fall into abeyance before tomorrow. The latter we may do by pointing to the statute-book. But there is no statute-book of Nature. In fact, to ask whether the laws of motion will continue in operation until tomorrow is to ask nothing over and above whether particular objects will move and interact in certain ways in the near future. Therefore, having argued that the sun will rise tomorrow on the grounds that 'there is nothing outside to interfere', we do not, as Russell imagines, have to produce <u>further</u> arguments in support of the laws of motion. This would be quite evident if it were not erroneously supposed that predictive propositions are in general to be justified by deducing them from laws. On the contrary, given the logical priority of particular facts to laws, predictions must <u>in general</u> be justified by causal inference from particular facts (33).

Rather than continuing any detailed commentary on Russell's account of induction, I shall now turn to the question of the character and status of inductive arguments. For I by no means deny that we often employ inductive arguments, and do so quite justifiably. By an 'inductive argument' I mean, roughly speaking, an argument from premises asserting that certain members of a given class of objects, events or states of affairs possess a certain characteristic to a conclusion asserting that other members or all members of that class possess that characteristic (34). Such arguments are often called arguments of 'enumerative' or 'ampliative' induction (35).

A typical use of inductive argument 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, mathematically speaking the variety of possible curves is infinite, no matter how many observations are made (36). This is the so-called 'curve-fitting problem'. The question is: on what grounds does the experimenter base his selection? The usual answer in indictivist literature is that his selection is based on considerations of mathematical 'simplicity': of the infinite variety of possible curves it is rational always to select the 'simplest' (37). However, considerable controversy has arisen as to the proper criteria of simplicity and even as to whether there <u>are</u> any objective standards of simplicity (38). But in any case such a solution to the curve-sitting problem must in general be inadequate as an explanation of actual selection criteria in scientific practice. 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 particular experimental results amd as regards the selection of the best curve, the scientist relies heavily upon assumptions which do not concern 'simplicity', under any interpretation of that term (39).

We may conveniently relate the 'curve-fitting problem' to the more general problem of the 'justification' of inductive arguments. I earlier claimed 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 arguments

are unjustifiable (40). The 'presuppositions' are, broadly speaking, the following:

(I) That our knowledge may be divided into two kinds, the experiential and the transexperiential, and that the latter is dependent upon some form of inference from the former.

(II) That the only relations between things that we may either perceive or discover are spatio-temporal relations and relations of similarity or class-membership.

(III) That causal inference is reducible to inductive inference and that inductive arguments are the primary kind of arguments concerning matter of fact.

(IV) That all inductive arguments are founded upon a principle of inductive inference.

(However, as we shall see, the fourth presupposition is not a necessary feature of inductivism.)

Now, despite Hume, many inductivists have attempted to justify inductive arguments even while accepting some form of these presuppositions. These attempts seem to fall into three classes (41). Some philosophers have simply claimed that inductive inference is paradigmatically rational: that what we <u>mean</u> by a rational transexperiential argument is simply an argument corresponding to inductive criteria (42). To this I merely reply that <u>causal inference</u> does not correspond to these criteria, since I have shown that it is not reducible to inductive inference: so it <u>cannot</u> be the case that what we <u>mean</u> 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 (43). These philosophers, however, accept the fourth presupposition which I mentioned above, namely, that all inductive

arguments are founded upon a principle of inductive inference. (This principle corresponds in most ways to Hume's 'supposition that the future will be conformable to the past' (44)). Their aim is thus to show that an inductive justification of such a principle would not be viciously circular. But this approach is very much open to attack since it effectively centres upon the question of whether or not a certain kind of circularity is really 'vicious'. However, a little later I shall point out how an inductive justification of induction could escape this criticism by dropping the presupposition, which is really unnecessary, that all inductive arguments are founded upon a principle of inductive inference. But, as we shall see, an inductive justification of induction will still suffer from another, and unavoidable, defect.

The third and last group of philosophers attempt to justify induction pragmatically. Adherents of this approach simply claim that 'inductive policies' are the best possible way of estimating the nature of whatever lies 'beyond' our experience: that is, the best way of discovering the unknown (45). However, this approach seems to me to be fundamentally mistaken. As I pointed out earlier, all processes of reasoning are potentially heuristic procedures, but not all heuristic procedures are processes of reasoning. So even if it could be shown that inductive policies represent the best way of discovering the unknown - which I would dispute, in any case (46) - this would at most imply that it was reasonable to adopt inductive policies for this purpose, but could by no means imply that induction was itself a justifiable 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: this, at least, was what Hume was discussing.

So far, then, it would appear that all inductivist attempts to

justify inductive arguments are 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. 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' (47). But the point is that this relation is not an inference-relation. This requires some explanation. In the case of a deductive inference, if I accept that p entails q, I shall not hesitate to infer q from p. Similarly, in the case of a causal inference, if I accept that an event A causes an event B, I shall not hesitate to infer the occurrence of B from the occurrence of A (48). But consider now an inductive inference: for example, an inference from premises asserting that a large number of ravens are black to a conclusion asserting 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 that a similar situation arose in the case of the 'curve-fitting problem'.

In order to see the nature of these further assumptions, let us consider an inductive argument which all would agree is 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 derived not 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 spirits. It should be noticed, moreover, that the kind of knowledge which justifies an inductive inference 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 reasoning is applied in each particular case (49). In conclusion, then, inductive inferences are only justifiable when they can be backed up by specific causal inferences (50).

However, this raises a point to which I alluded earlier. The inductivist supposes that all causal inferences are really reducible to inductive inference: so here there seems to be a possibility of justifying inductive inferences inductively in a non-circular way. 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 inference could be justified by particular causal inferences which were themselves reducible to other particular inductive inferences. In this way he might envisage a complex hierarchy of inductive arguments in which the lower levels gave support to the higher (51). However, this picture is still unsatisfactory. In the first place, of course, I would deny that causal inference is reducible to inductive inference. But, since the question at issue is the coherence of inductivism, I can hardly appeal to this (52). The fundamental

defect in such a hierarchical inductive system would be not that it involved circularity, but that it implied an infinite regress. For, according to the inductivist creed, if we are to arrive at transexperiential knowledge, an inductive inference must have been performed, even if only 'unconsciously'. But if every inductive inference requires the prior performance of at least one other inductive inference, and generally speaking many more than one, it is clearly impossible to explain how human beings can ever have begun to acquire transexperiential knowledge. Our knowledge ought still to be confined to a 'limited and fragmentary' experience (53). The source of the trouble is, once again, the fact that there is no inference-relation between the premises and conclusion of an inductive argument. This need not imply, as Hume seemed to think, that inductive arguments are irrational: but it does imply, in my view, that inductive arguments must always be backed up by arguments of another kind and that, therefore, they cannot possessthe primary and fundamental role assigned to them by the inductivist philosophers. Causal inference, not inductive inference, is the primary kind of argument concerning 'matter of fact and existence'.

Appendix: Eliminative Induction, Confirmation Theory, and Falsificationism

This thesis is principally about non-demonstrative inference. However, in this Appendix I shall discuss a number of theories according to which demonstrative inference should be regarded as the primary mode of reasoning about 'matter of fact and existence'. My general object will be to show how all the modes of argument described in these theories are logically posterior to causal inference.

(1) Mill's Theory of Eliminative Induction

There are, broadly speaking, two kinds of theory of eliminative induction: those in which the conclusions of inductions are nomological propositions simpliciter, and those in which they are propositions concerning the probability of nomological hypotheses. Here I shall discuss the former. However, I shall only consider the application of the theory to the simplest kind of nomological propositions, namely, those of the form 'All A's are B'.

Now, the object of eliminative induction in Mill's system is to establish the truth of a certain nomological hypothesis by eliminating all of its possible 'rivals'. An hypothesis of the form 'All A's are B' may have as possible rivals hypotheses of the form 'All A's are X' and hypotheses of the form 'All Y's are B', depending on whether our interest lies in A or B. Elimination works on the principle, first clearly enunciated by Bacon, that a nomological hypothesis may be refuted by finding a single counter-instance to it: "Maior est vis instantiae negativae' (1). Thus, a 'rival' hypothesis of the form 'All A's are X' may be refuted by finding an A which is not X.

Of course, strictly speaking the process of refutation is, as K.R. Popper has stressed (2), by no means as straight-forward as some

inductivists have tended to assume. The idea underlying Bacon's principle is that a singular proposition of the form 'This A is not X' formally contradicts a nomological proposition of the form 'All A's are X', so that we cannot accept both as true on pain of inconsistency. (This is why eliminative induction is based on demonstrative inference.) However, while it is the case that if faced with two contradictory propositions we cannot accept both as true on pain of inconsistency, we still have a choice as to which we should reject as false: we may reject the singular proposition rather than the nomological hypothesis (3). However, here I shall disregard this problem and speak though a direct demonstrative inference may be made from a singular observation statement to the falsity of a nomological hypothesis.

Now, it is evident that no number or variety of singular propositions of the form 'This A is not X' or 'This Y is not B' can alone enable us to infer demonstratively the truth of a law of the form 'All A's are B', no matter how many 'rival' hypotheses are thereby eliminated. A certain additional premise is required for this. However, the precise form which this premise must take if it is to perform the role technically required of it is a matter upon which inductivists have been by no means entirely in agreement.

Perhaps the easiest way of approaching this technical problem is to conceive of eliminative induction as a method of searching for necessary conditions and sufficient conditions (4). It certainly seems clear that Mill, who regarded eliminative induction as a method of searching for causes and effects, conceived of causes as sufficient conditions and effects as necessary conditions (5). We shall thus be interpreting a law of the form 'All A's are B' as stating that A is a sufficient condition of B and that B is a necessary condition of A.

Now, of Mill's various inductive 'methods', only two are

fundamental: the Method of Agreement and the Method of Difference. We may interpret the Method of Agreement as stating that only those 'characters' may be necessary conditions of a given character A, which are copresent with A in all the observed instances in which A occurs. This is simply to say that if A occurs with X in some instance, then X is eliminated as a possible necessary condition of A. Similarly, we may interpret the Method of Difference as stating that only those characters may be sufficient conditions of a given character B which are coabsent with B in all the observed instances in which B does not occur. This is simply to say that if Y occurs without B in some instance, then Y is eliminated as a possible sufficient condition of B.

However, it is evident that the mere elimination of such characters as X and Y does not alone enable us to infer demonstratively what character is a necessary condition of A, or what character is a sufficient condition of B.

Suppose we are seeking a sufficient condition of a given character B (or in Mill's terms, that we are seeking a cause of B). Suppose, further, that a number of characters have been eliminated already, by the Method of Difference. In order to be able to infer demonstratively that a sufficient condition of B exists amongst the remaining characters which have as yet not been eliminated, must postulate that B does possess at least one sufficient condition (or cause). Mill met this requirement in the form of his 'Law of Universal Causation'. However, as Nicod pointed out, Mill's postulate was, in one respect, more powerful than it need to be: for even if B were the only character in the universe which had at least one sufficient condition (that is to say, was 'caused'), it might still be possible to use eliminative induction to discover such a sufficient condition. But in another respect Mill's postulate was, on the contrary, not

powerful enough. Since our object is to discover a sufficient condition of B simply by eliminating all but one of various possible alternatives, or rival hypotheses, we must evidently be able to ascertain exactly what all these possible alternatives are, and they must be finite in number. If there were infinitely many alternatives, elimination could never be completed; and if we were ignorant of some of the alternatives it would always be possible that all the sufficient conditions were as yet unconsidered possibilities. Thus we must further postulate that a certain limited and specific class of characters contains at least one sufficient condition of B. Let us call this class of characters $\mathcal{C}$. (We may describe it as a class of possible sufficient conditions of B).

However, even this strengthened postulate is inadequate. We now have to contend with the problem which Mill called the problem of the Plurality of Causes. Suppose the class $\mathcal{C}$ contains the characters P, Q, R, and S. If it should happen that more than one of these characters are in fact sufficient conditions of B - suppose, for example, that both P and Q are - then we shall never be able to infer, by eliminative induction, that either P or Q is a sufficient condition of B. For, P and Q both being, ex hypothesis, sufficient conditions of B, neither can ever be eliminated: but since all that we have claimed to know is that at least one of the characters in the class $\mathcal{C}$ is a a sufficient condition of B, we can never determine whether our failure to eliminate P or Q stems from the fact that both really are sufficient conditions of B, or merely from the inadequacy of our experiments so far.

Suppose, however, that we were to modify our postulate so as to avoid the problem of the Plurality of Causes. Then we should still have to contend with the problem which Mill called the problem of the

Complexity of Causes. Besides the individual characters P, Q, R and S the class θ contains as possible sufficient condition of B all the various conjunctions of two or more of these characters. Suppose now that P is a sufficient condition of B, and that we have succeeded in eliminating Q, R, and S. It will still be the case that besides P itself, every conjunction of P with one or more of the other characters will also be a sufficient condition of B. However, simply because all these conjunctions of characters will be sufficient conditions of B, they can never be eliminated, and this will mean that we can never infer demonstratively even that P itself is a sufficient condition of B. We must therefore modify our postulate further so as to rule out the possibility that B possesses only complex sufficient conditions.

Our conclusion must be that if we wish to use eliminative induction to infer demonstratively a law assigning a sufficient condition to a given character B, from premises referring to counter-instances of rival hypotheses, we must have as an additional premise the following postulate: that a certain limited and specific class of characters θ contains one and only one non-complex sufficient condition of B.

Such, then is the form of the additional premise required in any argument of eliminative induction. However, it is evident that knowledge of a proposition of this form is on the same level as knowledge of natural laws themselves. Such a proposition can in no sense be a report of the experience or observations of an individual. And this means that eliminative induction cannot possibly be the primary mode of reasoning about 'matter of fact and existence'. Mill realized this and maintained that the postulates presupposed by arguments of eliminative induction could ultimately only be supported by enumerative induction. Of course, we have already seen that enumerative induction is itself logically posterior to causal inference. But in fact arguments

of eliminative induction depend upon causal inference in a much more direct way, as an example will clearly show.

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, the observer may reasonably conclude that the spectrum lines are caused by sodium. This is an example of the Method of Difference. But we have just seen that the possibility of employing that method depends upon an assumption to the effect that one and only one of a limited number of alternative possibilities must be true. Thus, in this instance, the observer assumes that the yellow spectrum lines must be caused by one or other of the chemical elements. Such an assumption, however, may only be justified in terms of modern atomic theory, according to which all atoms emit radiation of characteristic frequencies depending on their internal structures. It is these internal 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 connexion between the distinguishing chemical properties of an element and the frequency of the light it emits that the Method of Difference may justifiably be employed to discover which element actually causes the yellow lines, by eliminating all the possible alternatives.

(II) The Probabilistic Theory of Eliminative Induction

In the probabilistic theory of eliminative induction (6), the conclusions of eliminative inductions are not simply nomological propositions or laws as such, but are rather propositions concerning the probability of nomological hypotheses. What exactly may be meant by the probability of an hypothesis in this context is a matter I shall

discuss later.

However, despite this distinction, the probabilistic theory is fundamentally similar to Mill's system. But the object of the eliminative procedure is now to 'raise the probability' of the hypotheses which remain after each successive elimination. For this purpose a special postulate is required, similar to that required in Mill's system, concerning a limited and specific class of alternative hypotheses. But now, rather than requiring the postulate that at least one of these hypotheses is true, we may postulate merely that there is a certain probability p that at least one of them is true. However, this will mean that even if all but one of the alternative hypotheses are eliminated, the probability of the remaining hypothesis will only amount to p. If we wish to raise the probability of an hypothesis as near to unity as we like, we must postulate that $p = 1$.

According to the probabilistic theory, the procedure of eliminative induction works as follows. All the possible hypotheses are conceived of a possessing apriori or initial probabilities which may be assigned to them before the eliminative procedure has begun. As elimination proceeds, by means of Mill's Methods of Agreement and Difference, the total probability initially attaching to the hypotheses which have been eliminated is transferred to the remaining hypotheses, and is shared amongst them in proportion to the relative initial probabilities of these remaining hypotheses, assuming that their relative probabilities are unaffected by the eliminative process. The ultimate objective is, naturally, to reduce the remainder to a single law which has a probability of unity (assuming that $p = 1$). If this is achieved, the result is exactly the same as in Mill's theory, since to say that the probability of the hypothesis 'All A's are B'

equals unity is merely to affirm that all A's are B. However, if the eliminative procedure is incomplete, that is, if several hypotheses remain in the field, it will only be possible to assign a definite probability to any one of these remaining hypotheses if we also know all the initial probabilities of the original set of alternatives. Only if elimination is complete can initial probabilities be ignored. What could be meant by an 'initial' probability and how it could be established are matters for later discussion.

So far I have not mentioned how the problems of the Plurality and Complexity of Causes are to be overcome in the probabilistic system. Nicod's solution to these problems is as follows. Let us consider the case of raising the probability of an hypothesis ascribing to a given character B a certain sufficient condition A (that is, the hypothesis 'All A's are B'). Suppose we find two instances of the character B which have only A in common: that is to say, of all the characters copresent with B in the first instance, A is the only character which is also copresent with B in the second instance. This will not suffice to show, of course, that A is a sufficient condition of B, since B might have two different sufficient conditions neither of which might be A. But now suppose we go on finding further instances of B any two of which have only A in common. Say we have a number N of such instances. This will still not imply that A is a sufficient condition of B, but it will mean that if A is not a sufficient condition of B, then B must have at least N different sufficient conditions. Furthermore, the probability that A is a sufficient condition of B must consequently equal the probability that B does not have at least N different sufficient conditions. Nicod accordingly advances, as a means of evading the problems of the Plurality of Complexity of Causes, a postulate to the effect that the higher the value of N, the greater is the probability

That A is a sufficient condition of B, and that as N tends to infinity the probability tends to unity. In this way, by collecting instances of B any two of which have only A in common, we apparently raise the probability of the hypothesis 'All A's are B' as near to unity as we like (assuming the truth of the postulate).

Now this procedure is, as Nicod points out, rather reminiscent of enumerative induction in which the circumstances of the positive or confirming instances are varied. Indeed, it should be noted that Nicod is really no longer appealing to an eliminative mechanism at all: by finding 100 instances of B any two of which have only A in common, we are not eliminating any hypotheses concerning a possible sufficient condition of B. Every single one of the characters which occurs in these 100 instances, besides A itself, is still a possible sufficient condition of B, because the only way to eliminate a possible sufficient condition of B is to examine instances in which B is absent - this is the Method of Difference (7). We may conclude that Nicod is no longer discussing a theory of eliminative induction and that his proposed postulate to avoid the problems of the Plurality and Complexity of Causes is irrelevant. In fact, the postulate which is required for this purpose is, as we shall see more clearly shortly, substantially the same as that found to be required in Mill's system.

It is thus clear that the use of the probabilistic theory of eliminative induction is at least as dependent as the use of Mill's theory was found to be, upon assumptions which require to be justified independently, and hence that it could not possibly be advanced as the primary mode of reasoning about 'matter of fact and existence'.

(III) The Probabilistic Theory of Confirmation

The Probabilistic Theory of Confirmation was developed from certain ideas first advanced by Keynes and Nicod. Here I shall discuss these original conceptions rather than later developments of philosophers such as Carnap (8). Prima facie, this is a theory of enumerative induction. Keynes, however, apparently believed that it was really based on an underlying eliminative mechanism. Nicod disagreed with this. But I shall argue that Keynes was right, but not quite in the way he imagined, and not for the reasons which he advanced.

The object of the Probabilistic theory of confirmation is, once again, to raise the probability of a nomological hypothesis through the accumulation of empirical data. But this time the data are supposed to consist not of counter-instances to rival hypotheses, but rather of confirming instances of the hypothesis in question. Thus, if this hypothesis is a nomological proposition of the form 'All A's are B', the data or premises will consist of singular observation statements asserting that certain A's have been found to be B (9).

In order to see how the process works, we have first to discuss what is meant by the probability of a nomological hypotheses. The probabilistic theory of confirmation is based on the so-called Logical Theory of Probability, first advanced by Keynes himself. The central principle of this theory is that probability is relative to evidence. Keynes himself actually conceived of probability as a kind of logical relation between propositions, not unlike the relation of entailment. Indeed, inasmuch as he regarded entailment as the upper limit of probability, it would not be misleading to say that he regarded probability as a 'partial entailment'. Of course, such a conception is really absurd, since entailment is an all-or-nothing relation. However, many philosophers, while quietly dropping the notion that probability is

literally a logical relation, have retained the basic structure and symbolism of Keynes' theory, and in particular the doctrine that probability is relative to evidence.

It has been plausibly argued that normal linguistic usage sanctions the idea that we can only speak of the probability of an hypothesis relative to a given body of evidence (10). For example, we say that in the light of current experimental evidence it is quite probable that Einstein's General Theory of Relativity is true. It would seem to be absurd to say that such a probability judgment was inaccurate merely because it does not take into account per impossibile, any unfavourable evidence which may happen to turn up in the future. This seems to imply that probability judgments are 'non-empirical' in the sense that even if subsequent evidence should falsify an hypothesis, this would not upset our former estimate of its probability relative to the previously existing evidence. Thus, for example, although on the evidence available to us it is virtually certain that the Earth is spheroidal in shape, this does not alter the fact that on the evidence available to primitive man it must have seemed very probable that the Earth was flat. Of course, a point which may cast some doubt on the supposed 'non-empirical' character of probability judgments is that when new evidence causes us to change our opinion concerning the probability of an hypothesis, we tend to say (as in the foregoing example) that on the old evidence the hypothesis only seemed as probable as it did, not that it was that probable. Indeed, as Toulmin has pointed out, the argument that probability is relative to evidence seems to rest upon a confusion between 'mistaken claims' and 'improper claims' (11).

However, if once we accept the doctrine that probability is relative to evidence (though I am not suggesting that we should), it is not hard to accept the idea that we may speak of varying degrees of

probability possessed by different hypotheses relative to different bodies of evidence, or by different hypotheses relative to the same body of evidence, or by a single hypothesis relative to different bodies of evidence.

We must now turn to the kind of probability judgments which we make in games of chance; for example, when it is said that the probability of throwing a 'six' with one cast of a true die equals 1/6, or that the probability of drawing the king of spades from a well-shuffled pack equals 1/52. Mathematicians have constructed a calculus of probabilities for use in such situations, which enables unknown probabilities to be derived from known ones. This calculus is, however, for a reason which I shall give shortly, in fact merely a species of class-ratio arithmetic or set-theory.

Now, an important theorem of class-ratio arithmetic states the following. Suppose we have a class C which contains two sub-classes A and B. Then the theorem states that 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. If we symbolize the proportion of members of C which are members of A as A/C, and so on for the other proportions, we have the following equation:

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

Furthermore, by considerations of symmetry, we also have:

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

Now this theorem is obviously of use in games of chance simply because in such games we estimate degrees of probability on the basis of class-ratios or relative frequencies. Thus, we say that the probability of dealing a spade from a normal pack equals 1/4, because this is the proportion of cards which are spades in such a pack. We

can then use the theorem in question to calculate one probability from two other, known probabilities. For example, what is the probability of being dealt the king of spades from a normal pack? It equals the probability of being dealt a king, multiplied by the probability that a king is a spade. That is: $1/13 \times 1/4 = 1/52$. But it should never be forgotten that the theorem employed here is really a theorem of class-ratio arithmetic, and can only be used for the calculation of probabilities in those cases in which probabilities are estimated on the basis of class-ratios or relative frequencies.

All this seems quite straightforward, but it tends to be obscured by the Logical Theory of Probability. According to Keynes' original conception, as we have seen, probability was a relation between propositions. As a consequence, according to the theory, the proper way to express the fact that the probability of being dealt a spade from a normal pack equals 1/4 is 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'. In the Logical Theory, such a probability judgment is then symbolized thus: $P(p/q) = 1/4$.

Now this is, in my view, a rather obscure way of expressing the fact in question, but it is certainly possible to use such symbolism for the calculation of probabilities in games of chance. For example, we can write out an equation, using these symbols, which is formally equivalent to the theorem of class-ratio arithmetic which I set out earlier, and this equation will enable us, in like manner, to calculate the probability of being dealt the king of spades from a normal pack. This equation is:

$$P(pr/q) = P(p/q) \times P(r/pq) = P(r/q) \times P(p/qr)$$

where r is the proposition 'This (same) card is a king'.

We must now return to the kind of probability judgments in which nomological hypotheses are said to be probable relative to empirical data. The Logical Theory of Probability employs exactly the same symbolism in these cases as it does in the case of probability judgments in games of chance. For example, according to the theory, if we have two nomological hypotheses g and h and certain body of empirical data or evidence, k, and we wish to say that g is more probable than h relative to k, then we may symbolize this probability judgment thus:

$$P(g/k) > P(h/k)$$

And, granted that we accept the doctrine that probability is relative to evidence, such symbolism may be harmless enough in itself. But the danger is that one may be tempted to imagine, from the similarity between this symbolism and that employed in games of chance, that we may similarly employ theorems of class-ratio arithmetic in the case of probability judgments concerning the probability of nomological hypotheses relative to empirical data. This is precisely what Keynes and Nicod assumed, and the whole edifice of the probabilistic theory of confirmation is founded upon this error - the error, that is, of assuming that the axioms of the so-called probability calculus are in principle applicable to <u>all</u> kinds of probability statements. Added to this was the equally misleading assumption that between <u>any</u> two self-consistent propositions there must exist some relation of probability.

Nicod, following Keynes, adopted as the fundamental theorem of the probabilistic theory of confirmation the following equation, which is really simply the theorem of class-ratio arithmetic which I set out earlier (12).

$$P(he/k) = P(h/k) \times P(e/hk) = P(e/k) \times P(h/ek).$$

However, Nicod entirely neglected the fact that not just <u>any</u> propositions can be substituted meaningfully into this equation, but only such

propositions as are related exactly as p, q, and r were in our earlier example concerning a card game. But, according to the probabilistic theory of confirmation, in the above equation h represents a nomological hypothesis, such as 'All A's are B', k represents certain background data or evidence which renders h probable to some degree, and e represents one or more singlular observation statements asserting that certain A's have been observed to be B. It is next pointed out that $P(e/hk) = 1$, because it is certain that if all A's are B, then any given A will be B, and certainty (meaning in this context 'entailment') equals a probability of unity. We are thus left with the following equation, which is the foundation of the probabilistic theory of confirmation:

$$\frac{P(h/k)}{P(h/ek)} = P(e/k)$$

This equation is interpreted as meaning that the probability of a nomological hypothesis is <u>necessarily</u> raised by the discovery of a 'confirming instance', provided that $0 < P(h/k) < 1$ and $0 < P(e/k) < 1$ - that is, provided that the apriori or initial probability of the hypothesis is finite and that the new evidence e, is not already contained within, and hence entailed by, the background data k. Exactly what k might be, however, has never been at all clearly indicated. This, and the related question as to what might be meant by the apriori or initial probability of an hypothesis have been a source of endless discussion - all of which, however, is quite pointless simply because the above equation, with the values of h, k and e above stated, is really meaningless. The theorem of class-ratio arithmetic upon which it is based can only be employed with reference to the probability of a nomological hypothesis if we are prepared to estimate this probability on the basis of a class-ratio or relative frequency. As I shall shortly show, this <u>can</u> in fact be done: but even then, as will be seen, we

have to be very careful as to what propositions we substitute for k and e in the equation. The result, however, is rather interesting since it shows that the only intelligible interpretation of the probabilistic theory of confirmation in fact simply reduces it to eliminative induction of the probabilistic variety. I should add, however, that although there is a way of estimating the probability of a nomological hypothesis on the basis of class ratios, this is utterly dissimilar to the normal sense in which we speak of an hypothesis being probable in the light of experimental data, as, for example, when we say that in the light of current experimental evidence it is quite probable that Einstein's theory is true.

Suppose we have a nomological hypothesis h, of the form 'All A's are B'. Let us interpret this as the hypothesis that the character A is a sufficient condition of the character B. How could we assign a probability to this hypothesis on the basis of a class-ratio? The obvious thing to do would be this. Consider a class θ of characters which, for some reason, we consider apriori to be possible sufficient conditions of B. (How we may arrive at this judgment need not concern us here (13)). Suppose that A belongs to this class θ. Suppose, furthermore, that of the members of this class a proportion p actually are sufficient conditions of B. Then it seems reasonable to define the apriori probability that A is a sufficient condition of B simply as p - just as the apriori probability of dealing a spade from a normal pack equals the proportion of cards in such a pack which actually are spades.

We can now say that the hypothesis h (that A is a sufficient condition of B) has a probability p relative to, or given, the datum k (that A is a member of the class θ). That is, $P(h/k) = p$.
This seems to be all that can be understood by the apriori probability of such an hypothesis, in terms of a class-ratio.

However, if we wish to employ the theorem:

$$P(h/k) \times P(e/hk) = P(e/k) \times P(h/ek)$$

we must, as I remarked earlier, be careful what sort of proposition we take e to be. In fact, it is clear that if h is a proposition about a possible sufficient condition of B, then e must also be a proposition about a possible sufficient condition of B - just as, if h were a proposition about a card from a normal pack, e would also have to be a proposition about a card from a normal pack. In particular, e clearly cannot be about a confirming instance of the nomological hypothesis h - that is a proposition stating that the character A was copresent with the character B in a certain observed instance. Because, if e were to mean this, the symbol $P(e/k)$ would simply be meaningless. If probabilities are here to be estimated in terms of class-ratios, there can be no such thing as the probability of A being copresent with B in a certain instance relative to the datum that A is a member of the class θ of possible sufficient conditions of B.

An example will make this important point quite clear. Suppose h were the hypothesis that a certain coin C had two heads. (This is like saying 'All the faces of C are heads'). The apriori probability $P(h/k)$ would be given by the proportion of two-headed coins in a certain class of coins. Now suppose that e is the proposition that in a certain instance the coin C lands 'heads'. (This is like saying 'This face of C is heads'). The apriori probability $P(e/k)$ ought to be given simply by the proportion of 'heads' amongst the faces of the coin C - thus, if C is a normal coin, this will equal 1/2. But then it is evident that the k in $P(h/k)$ cannot be the same proposition as the k in $P(e/k)$. The probability that all the faces of the coin are heads is relative to a certain class of coins; but the probability that a particular face of the coin in heads is relative to

to the class of faces of this coin. In exactly the same way, the probability of the nomological hypothesis 'All A's are B' is relative to a class θ of hypotheses of the form 'All X's are B'; but the probability of the proposition that a particular A is B in a certain instance is relative to the class of individual A's.

This means then that when h is a nomological hypothesis, e cannot be a proposition about a confirming instance of that hypothesis, in the way that Keynes and Nicod would like - not, at least, if one wants to employ the theorem of class-ratios. In fact, e must clearly be about another hypothesis in the class θ. This immediately suggests the idea of eliminative induction. If we let e be the proposition 'X is not a sufficient condition of B' (where X is a character belonging to the class θ) then, even though e itself is not about particular instances, it can be verified conclusively or demonstratively by finding a single instance in which the character X occurs without the character B.

However, an important step in the Keynes-Nicod argument was that in which it was argued that $P(e/hk) = 1$. This no longer holds, since h no longer entails e. However, if we add to k the special postulate that in the class θ there is one and only one non-complex sufficient condition of B, then we can indeed say that $P(e/hk) = 1$, since then 'A is a sufficient condition of B' would, together with k, entail 'X is not a sufficient condition of B', whatever X might be. But this postulate is, of course, precisely the same as the one which was found to be required in Mill's system of eliminative induction to avoid the problems of the Plurality and Complexity of Causes. With the help of this postulate we really can write:

$$\frac{P(h/k)}{P(h/ek)} = P(e/k)$$

provided that $0 < P(h/k) < 1$ and $0 < P(e/k) < 1$. These provisos, however, simply mean that some but not all of the characters in the class θ

must be sufficient conditions of B, and this is already guaranteed by our special postulate.

The interesting result of this inquiry is that if the main theorem of the probabilistic theory of confirmation is to be used at all, it must be used in such a way as to reduce the theory simply to eliminative induction of the probabilistic variety (14). But this should not surprise us, for the following reason. The species of inference which is employed in the probabilistic theory of confirmation is demonstrative inference - as is evident from the fact that it is based upon certain mathematical axioms and theorems. But it should by now be evident that the only kind of demonstrative inference from particular instances to a proposition conerning a universal hypothesis is that of demonstrative refutation. Thus, if Confirmation Theory is to be presented as a theory of enumerative induction it must abandon the connexion with the 'probability calculus' and accept that enumerative induction is a process of non-demonstrative inference.

The mistake of adherents of the probabilistic theory of confirmation was to be misled by bad symbolism into imagining that certain theorems of class-ratio arithmetic could be applicable to the sort of probability judgments in which we say that a scientific hypothesis is probable to some degree in the light of current experimental evidence. It is not at all apparent, according to the probabilistic theory of confirmation, why such hypotheses should not have precise numerical degrees of probability. And, indeed, some adherents of the theory have actually held that this is so, but that for some reason we are unable to discover these precise numerical degrees with any precision. (Keynes himself, however, explicitly rejected this idea.) As we have seen, this view is the only one that is consistent with the use of the theorems of class-ratio arithmetic in connexion with the probabilities of

nomological hypotheses. For these theorems can only be used in connexion with probabilities which are estimated, at least in principle, on the basis of class-ratios or relative frequencies and which are thus, at least in principle, assignable precise numerical degrees.

Keynes himself, it should be mentioned, supposed that the axioms of the so-called probability calculus were applicable not merely to numerical probability relations but also to non-numerical probability relations. Of course, it is not at all apparent how non-numerical probabilities may be added and multiplied: but Keynes assumed that the 'multiplication' and 'addition' axioms of the calculus defined certain primitive operations for combining probability relations (whether or not of the numerical variety) and that these operations only became multiplication and addition in the usual sense in the case of numerical probability relations. But then, of course, this leaves the nature and origin of these 'primitive operations' utterly obscure, as Ramsey was quick to point out. It is infinitely more plausible to explain the origin of the 'probability calculus' in terms of an adaptation of theorems of class-ratio arithmetic.

Now, some philosophers may feel that there is nothing absurd in the notion of scientific hypotheses possessing numerical degrees of probability which we cannot, for some reason, ever discover. But even if they can accept this notion with equanimity, they still have to explain why the probability of scientific hypotheses should be conceived of in this way when it is perfectly clear that no one has ever attempted, or ever would attempt, to estimate the probability of such hypotheses on the basis of class-ratios or relative frequencies. We have seen, indeed, that this can, in principle, be done: but clearly not in a way that remotely resembles the actual procedures by which we assess the probability of scientific hypotheses, such as Einstein's Theory of Relativity.

It is often suggested that there are two radically different kinds of probability: a quantitative kind to be found in games of chance and a non-quantitative kind which is spoken of in other contexts (15). But despite what I have said which may suggest that I concur with such a distinction, I do not think that the words 'probable' and 'probability' really have radically different meanings in these different contexts (16). Such a notion would ultimately be inexplicable. I do not intend to attempt to analyse the meaning of the word 'probability' here, but it does seem to me that the difference in question is not one of meaning, but lies rather purely in the different ways in which we estimate probabilities in these different contexts. In games of chance it appears that we may proportion the probability of a proposition to a class-ratio or relative frequency. (What leads us to make such a judgment is another matter (17)). It is the class-ratio or relative frequency which is originally quantitative, even though in virtue of this the notion of quantifiability is transferred to the probability itself. But in those cases where it would not be appropriate to proportion probability to a class-ratio or relative frequency, as in the case of scientific hypotheses, probability is naturally non-quantitative. Thus, while we may always speak of degrees of probability, we may only meaningfully speak of numerical degrees of probability when the criteria upon which our probability judgment is based are of a special kind.

IV Falsificationism (18)

Falsificationism is founded upon the supposed asymmetry between verification and falsification which was first clearly pointed out by Bacon ('Maior est vis instantiae negativae'). This asymmetry supposedly arises from the fact that a nomological hypothesis, while it can never be demonstratively 'verified' by any finite number of observed

confirming instances, can de demonstratively 'falsified' or 'refuted' by a single observed counter-instance. Of course, as I pointed out earlier, as K. R. Popper himself stresses, this is a dangerously oversimplified statement of the case. An observation as such cannot 'falsify' a nomological hypothesis: rather, it may be the case than an observation statement and a nomological hypothesis are inconsistent, and this presents us with the choice of rejecting one or the other. This choice, according to Popper and his followers, will involve methodological decisions, and much of the theory of falsificationism is devoted to explaining the criteria of such decisions (19). All this means, as Popper admits, that a conclusive (that is, demonstrative) disproof of a nomological hypothesis is no more possible, on the basis of observational data, than is a conclusive verification. But my criticism of the theory does not turn on this point: rather, it attacks it at a more fundamental level.

To appreciate this criticism, it is perhaps best to consider falsificationism in terms of an 'answer to Hume' - which, in fact, appears to have been the historical origin of the theory (20). The problem which Hume is supposed to have presented to the philosophical world is the question as to whether we can ever make rational predictions - that is, predictions founded upon rational arguments. According to Hume, as we have seen, our predictions are actually based on inductive extrapolation, and inductive extrapolation could only be a process of rational argument if it were founded upon the principle that 'the future will be conformable to the past'. This principle, however, cannot be defended or justified inductively, on pain of circularity.

To this the falsificationist replies that, while it is true that inductive extrapolation is not a process of rational argument, for the reasons which Hume gives (21), it is not the case that our predictions are based on inductive extrapolation. Rather, they are

deduced from nomological hypotheses. And this is why prediction is founded upon rational argument - namely, upon deduction.

However, the natural objection to this is that it is not explained how it is that we arrive at nomological hypotheses themselves by rational argument, or how such hypotheses may subsequently be justified or established. But the falsificationist replies that such an objection is misconceived: Hume has shown (or so the falsificationist thinks) that it is logically impossible to 'justify' or 'establish' a nomological hypothesis on the basis of observational data, and hence to require that we should be able to do this is nonsensical. Moreover, we do not arrive at such hypotheses by reasoning of any sort: ratherwe arrive at them through 'flashes of insight' and 'inspired guesses'.

However, it will now be objected that such a procedure is wholly arbitrary. Take any event you may care to imagine, no matter how bizarre: it will always be possible to imagine an equally bizarre nomological hypothesis from which this event may be predicted deductively. To this the falsificationist replies that his procedure is not, in fact arbitrary: whenever we make a prediction we must, it is true, deduce it from some hypothesis; and it is also the case that we can never establish whether an hypothesis is true, or even that it is at all probable; but we can, at least sometimes, establish that an hypothesis is false. The progress of science consists in weeding out the false hypotheses: in this way we gradually advance towards truth. While it would be arbitrary indeed if we deduced predictions from any hypothesis we liked, our actual procedure is not arbitrary since it would not be rational to deduce a prediction from a falsified hypothesis. This, then, constitutes the required restraint upon arbitrariness.

But this answer will not do. According to the falsificationist it would be irrational to make any prediction by deducing it from a

falsified hypothesis: we should always prefer unfalsified hypotheses as our guides to the future. But why should we? Is there not a tacit assumption here that if an hypothesis has met with counter-instances in the past, it is likely to meet with them in the future; or at least, that it is more likely to do so than is an hypothesis which has not, so far, been falsified? But if this is the real reason why we should prefer predictions deduced from unfalsified hypotheses - and the falsificationist cannot, it seems, offer any other (22) - Hume has not been 'answered' after all, since this is simply the familiar 'supposition that the future will be conformable to the past'. My objection, then, is that the falsificationist cannot explain in what sense it would be irrational to deduce predictions from falsified hypotheses, and because of this he cannot evade the charge of arbitariness.

To all this the falsificationist might retort that it is perfectly evident that it would be irrational to deduce predictions from hypotheses known to be false, simply because, in such cases, even if the inference were perfectly valid, we should have no reason whatever to expect the predictions to be true. However, the falsificationist can hardly lay claim to this argument since, according to his theory, even if we deduce predictions from unfalsified hypotheses, we can still have no reason whatever to expect them to be true. Thus the falsificationist can in no way explain why unfalsified hypotheses are to be preferred to falsified ones as guides to the future.

The moral to be drawn from all this is simply that the 'Hypothetico-deductive method', while it may certainly characterize many aspects of scientific theorizing, cannot be supposed to exhaust the entire field of human rationality. On the contrary, it is a rather sophisticated species of reasoning which is ultimately dependent upon a substantial background of causal inference (23).

V General Conclusion

In this Appendix I have examined various theories concerning the rationale of scientific method which represent it as being founded upon demonstrative or deductive inference. These theories really reduce to two: Mill's methods of eliminative induction and the hypothetico-deductive method. I by no means seek to reject these methods as unimportant, but only wish to maintain, and claim to have shown, that neither of them can possibly be advanced as our primary mode of reasoning about 'matter of fact and existence', any more than enumerative induction can be supposed to play this role. Causal inference alone can claim this primacy.

Notes and References

Introduction

(1) Not all the species of arguments commonly referred to as 'inductive' involve non-demonstrative inference. Thus, summative induction or induction by complete enumeration, recursive or mathematical induction, and induction by elimination, are all forms of argument involving demonstrative inference. The sort of induction which involves non-demonstrative inference is often called 'ampliative' induction: see, e.g. W. Kneale, 'Probability and Induction', Oxford, 1949, p 44, and G.H. Von Wright, 'The Logical Problem of Induction', Oxford, 1957, p 9.

(2) Cf. F.P. Ramsey: "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" ('Foundations of Mathematics', London, 1931, p 195). However, I do not believe, as Ramsey appears to, that this is equivalent to saying that "Whenever I make an inference, I do so according to some rule or habit" (ibid). I would not deny that there are rules of inference, of course, but I would distinguish these from the particular inference-relations from which such rules are, in my view, abstractions.

(3) It will become clear later that a fundamental distinction is that between those inference-relations which are relations between propositions as such - formal and conceptual relations - and those inference-relations which are relations between the references of propositions - in particular, causal relations.

(4) Entailment is clearly a relation between propositions: it is a formal or conceptual relation. But there may evidently be formal or conceptual inference-relations which are not relations of entailment: for instance, 'analogical' relations may qualify as such.

(5) Analytic propositions are usually regarded as belonging to two different categories: those propositions which are formally true, or true merely in virtue of their logical form - such as 'No unmarried man is married' - and those propositions which are conceptually true, or true merely in virtue of the meanings of the concepts contained in them - such as 'All bachelors are unmarried'. Now, the proposition 'It is not the case that while Socrates is a man and all men are mortal, Socrates is not mortal' is a formal truth, so that we may call the inference 'formal' inference, as opposed to 'conceptual' inference. Similarly, we may distinguish between 'formal' entailment and 'conceptual' entailment.

(6) I borrow this example from P.F. Strawson, who cites it as an instance of 'non-deductive reasoning': see, 'Introduction to Logical Theory', London, 1952, p 235.

(7) 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" (ibid.).

(8) K. R. Popper is perhaps the foremost representative of this 'deductivist' school of thought. Popper himself espouses the term 'deductivism': see'The Logic of Scientific Discovery', London, 1959, p 30.

(9) Later we shall see that many of Hume's sceptical arguments turn upon a similar misuse of the notion of 'possibility'.

(10) I shall principally refer to the account in the 'Enquiry Concerning Human Understanding', ed. L.A. Selby-Bigge, Oxford, 1902, as this seems to be clearer than the corresponding account in the 'Treatise of Human Nature'.

(11) For the influence of Hume on the views of K.R. Popper, see Popper's book, 'Objective Knowledge', Oxford, 1972, Chapter 1.

(12) I should perhaps point out that although my examination of Hume's arguments will keep fairly close to the actual text of the Enquiry, I shall at certain points 'rationalize' what he says, because he often confuses issues which should be kept distinct. To attempt to sort out all these confusions in the main body of this thesis would, however, produce too many complications. The principal object of this thesis is not simply an exercise in exegesis.

(1) This distinction has, of course, been criticised by a number of modern philosophers, notably W.V. Quine: see 'Two Dogmas of Empiricism', reprinted in 'From a Logical Point of View', New York, 1953. But many distinguished philosophers continue to defend it: Eg. H.P. Grice and P.F. Strawson, 'In Defence of a Dogma', Philosophical Review, 1956.

(2) Enquiry, p 35.

(3) D.C. Stove, also, points out that 'demonstrative' reasoning was, for Hume - and for other philosophers of his time - reasoning from non-contingent or necessarily true premises: see 'Probability and Hume's Inductive Scepticism', Oxford, 1973, p 35. Of course, the matter is complicated somewhat by the fact that Hume clearly believed that all demonstrative arguments (in his sense) involve demonstrative inference (in the modern sense) : that is, that there can be no sort of non-demonstrative inference in mathematical reasoning. This view is, however, rather questionable: cf. I. Lakatos, 'Proofs and Refutations', British Journal for the Philosophy of Science, 1963-4. Stove also points out that by a 'probable' argument Hume meant an argument from contingent premises to a contingent conclusion (ibid, p 37).

(4) Enquiry, p 34.

(5) Of course, Hume would have allowed that many valid syllogisms '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 such syllogisms as unimportant and trivial.

(6) The principal difference between Hume's position and that of K.R. Popper seems to be that while Hume concluded, apparently, that mathematical demonstration was the only genuine kind of reasoning, Popper believes that deductive reasoning concerning 'matter of fact and existence' is not trivial and unimportant, because of his advocacy of the 'hypothetico-deductive method'.

(7) Hume later refers to such arguments as 'arguments from experience': see, Enquiry, p 36.

(8) I adopt this terminology because we require generic terms to refer to observation and memory statements on the one hand and predictive and retrodictive statements on the other. As we shall see, modern inductivists also regard nomological propositions - propositions expressing 'natural laws' - as 'transexperiential'.

(9) See eg. N.R. Hanson, 'Patterns of Discovery', Cambridge, 1958, p 19.

(10) As we shall see, modern theories of inductive argument are mostly concerned with 'the induction of laws'.

(11) In other words, Hume is not referring to formal or conceptual relations. It should be noted that, in contrast to this, in all systems

of modern 'inductive logic', it is precisely formal or conceptual relations which are held to be of principal importance in non-demonstrative reasoning.

(12) 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 evident that we draw this predictive conclusion upon the supposition that fire causes water to boil. Strawson, however, assumes that such an inference is really 'inductive' in character: but this is the very point which I shall be questioning.

(13) For example, it will no doubt be accepted that if fire really does cause water to boil, then we may justly infer from the fact that the kettle has been on the fire for the last ten minutes that it will be boiling by now: but if there were no way of supporting the claim that fire does cause water to boil, such an inference would clearly never be justifiable.

(14) All processes of reasoning are potentially heuristic procedures in that it is always at least possible that we should be ignorant of the conclusion of an argument prior to making the actual inference.

(15) Of course, I do not allege that Hume was entirely conscious of the line of thought which I have just sketched: but it does seem to explain certain features of his general argument, as we shall see.

(16) This contention is expressed in the 'Treatise' in the famous dictum: "There is no object which implies the existence of any other, if we consider these objects in themselves, and never look beyond the

ideas which we form of them" ('A Treatise of Human Nature', ed. L.A. Selby-Bigge, Oxford, 1888, pp 86-7).

(17) Cf. P.F. Strawson, op. cit. p 235.

(18) Cf. A.J. Ayer, 'Probability and Evidence', London, 1972, especially pp 6ff on 'the assumption of atomicity'.

(19) I say 'in general' simply becuase, as we have remarked, many words carry logical implications. But what Hume has shown is that reasoning apriori could not be our primary way of discovering causal relations by reasoning, and this is all he needs to prove.

(20) This, too, might 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'.

(21) In saying this, I do not mean to deny the 'epistemic' features of seeing: cf. F.I. Dretske, 'Seeing and Knowing', London, 1969.

(22) Actually, of course, Hume insists on the temporal precedence of the cause to the effect: but this point need not concern us.

(23) I shall not discuss here the problems raised by the fact that even our experience of causally related events is not entirely 'coherent', owing to the fact that we blink our eyes, fall asleep, lose attention, and so on. In the Treatise, Hume only turns to these problems after he has discussed the nature of rational inference (in the section

entitled 'Of Scepticism with regard to the Senses'); arguably, he should have dealt with them first.

(24) When, later on, I shall argue that causal propositions can be justified by 'probable' arguments, I shall not, however, accept Hume's restriction on the nature of the premises of such arguments. As we shall see, Hume's restriction effectively limits 'probable' arguments to inductive arguments.

(25) Even though I claim to show that Hume's proof is a failure, I do not wish to suggest that we do or can in general justify causal propositions by arguments whose premises refer solely to observed past constant conjunctions. For, as I remarked in the previous note, I do not accept Hume's restriction on the possible nature of the premises of the arguments by which we may seek to justify causal propositions. Incidentally, it might be queried whether, in paragraph 28, Hume really is just claiming that we cannot discover causal relations through reasoning from premises referring to constant conjunctions: rather, he seems to be denying the possibility of inductive arguments, in general. But these two issues are not kept clearly separate by Hume: I must admit that I am to some extent 'rationalizing' Hume's argument.

(26) Thus Locke considers 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" ('Essay Concerning Human Understanding', Bk III, Chap III, para. 17).

(27) But it must be admitted that Hume often seems to lose sight of the fact that this is the question at issue.

(28) Hume illustrates this in the 'Treatise' as follows: "Thus we remember to have seen that species of object we call flame, and to have felt that species of sensation we call heat. We likewise call to mind their constant conjunction in all past instances. Without any further ceremony, we call the one cause and the other effect, and infer the existence of the one from that of the other" ('Treatise', p 87). The crucial phrase is, of course, "without any further ceremony", as this is meant to indicate the lack of any process of ratiocination.

(29) This 'expectation' Hume subsequently attempts to explain, of course, in terms of 'custom or habit'.

(30) This point may require some clarification. I pointed out earlier that a causal inference could not ever be valid if it were impossible to support the proposition that the causal relation in question existed. By associating causal relations with unknowable 'secret powers' Hume therefore was adopting a position according to which causal inferences must be deemed invalid - that is, not genuine processes of reasoning. It was with this situation in mind that Hume subsequently reduced causal inference to inductive extrapolation. So that in Hume's system inductive extrapolation is ex hypothesi not a genuine process of reasoning.

(31) Of course, modern philosophers would point out that there is a relation between the premises and conclusions of inductive arguments: the relation of 'confirmation' or inductive 'support' - see, especially, C.G. Hempel, 'Studies in the Logic of Confirmation', reprinted in 'Aspects of Scientific Explanation', New York, 1965, pp 3 ff. However, the point which Hume should be taken to be making is that this relation

of 'support' is not an inference-relation. This is a matter I shall return to later.

(32) Enquiry, p 35. 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" ('Treatise', p 89). This resembles, of course, the modern notion of a 'principle of inductive inference' - cf. B. Russell, 'The Problems of Philosophy', Oxford, 1912, chapter 6.

(33) Indeed, since in Hume's system inductive extrapolation is, ex hypothesi, not a process of reasoning, it seems somewhat vacuous for Hume to speculate about what conditions would, per impossibile, have to be satisfied in order that it should be a process of reasoning. However, D.C. Stove has suggested that what Hume meant was that the supposition that "the future will be conformable to the past" is required to turn inductive extrapolation into a deductively valid inference (see D.C. Stove, op. cit. pp 43-4). This suggestion seems quite plausible. But it may be remarked 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.

(34) D.C. Stove points out that "the only ground which Hume gives for saying that there can be no demonstrative arguments for the Resemblance thesis (as I will call it), is that this thesis is a contingent proposition" (Stove, op. cit. p 35). Stove cites this as confirmation that "Hume meant by a 'demonstrative argument' a '(valid) argument from necessarily true premisses'" (ibid.). By the 'Resemblance

Thesis' Stove means the proposition that the future will resemble the past.

(35) However, although I reject Hume's supposed proof that the 'principle of the uniformity of nature' cannot be supported by reasoning, I do not mean to suggest that such a principle is, in fact, defensible: on the contrary, it appears to me to be so vague as to be virtually meaningless.

Chapter 2

(1) 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" ('Essay Concerning Human Understanding' Bk. IV, Chap VI, para. II.) Locke was insistent that we could never know the 'real essences' of substances, however.

(2) The only exception to this is the one I referred to earlier, stemming from the fact that many terms for objects and events carry certain logical implications - 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 or events, and very often these descriptions include a tacit or explicit reference to their causal properties" (op. cit. p 7).

(3) Enquiry. p 74.

(4) The attempt to suggest that these sensations 'cohere' through the agency of 'custom or habit' - or even through the agency of 'imagination' - is surely wild.

(5) In other words, such philosophers believe that the only kind of restraint upon possibility is that of logical contradiction: this is commonly referred to as the view that there is no 'natural necessity'.

(6) Cf. C.J. Ducasse, 'Truth, Knowledge and Causation', London, 1968, pp 30-1. 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 perceived or otherwise 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 (such as a rather viscous fluid), and there were no splash, this might logically exclude the possibility of our calling this liquid 'water'. But one can only explain why we have incorporated this logical implication into the concept of 'water' in terms of our having observed that stones do easily 'make splashes' in certain kinds of liquids. 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 observed to exist between those events. It is not difficult to see how this may have given rise to confusions as to the logical status of causal relations.

(7) Cf. Ducasse, op. cit. p 8. By 'necessary relations' between colours, shapes, smells or sounds, I mean such notorious relations as that between being 'red all over' and being 'green all over'.

(8) E.g. "I begin with observing that the terms of efficacy, agency, power, force, energy, necessity, connexion, and productive quality, are all nearly synonymous; and therefore 'tis an absurdity to employ any of them in defining the rest" ('Treatise', p 170).

(9) Actually, of course, some phenomenalists do seem to suppose exactly this. But if we drop the phenomenalist analysis of material objects, as surely we must, should we not also drop the phenomenalist analysis of causal relations?

(10) Cf. J.L. Austin, 'Sense and Sensibilia', Oxford, 1962, pp 7-8.

(11) Thus Berkeley asks, rhetorically, "whether we immediately perceive by sight any thing 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" ('Three Dialogues Between Hylas and Philonous', Dialogue I).

(12) Thus Berkeley again: "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" (ibid.)

(13) As J.L. Austin puts it: "There is no one kind of thing that we 'perceive' but many different kinds, the number being reducible, if at all, by scientific investigation and not by philosophy" (op. cit. p 4).

(14) This distinction between causal and non-causal activity is

roughly reflected in language by the distinction between transitive and non-transitive verbs.

(15) For a similar view concerning the perceptibility of causal relations, see C.J. Ducasse, op. cit. : "...every person has perceived - and I say perceived, not inferred - that, for example, a particular tree branch was being caused to bend by a particular bird's alighting on it; that a particular bottle was being caused to break by the fall on it of a particular rock..." (p 26). Even more similar to my view is that of A. Michotte: see 'The Perception of Causality', London 1963. Michotte writes: "The thesis which I put forward.....was in direct disagreement with all theories of the traditional kind which we have just been considering, since I expressed the opinion that certain physical events give an immediate causal impression, and that one can 'see' an object act on another object, produce in it certain changes, and modify it in one way or another. I quoted various examples in this connexion eg. that of a hammer driving a nail into a plank, and that of a knife cutting a slice of bread" (p 15).

(16) Thus Hume declares: "The impulse of one billiard ball is attended with motion in the second. This is the whole that appears to the outward senses" (Enquiry, p 63). A.J. Ayer shares exactly the same view: "...we may take a simple illustration of Hume's, that of a cannon in billiards. We think of the cue striking the billiard 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 wholly described in purely undramatic 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" (op. cit. p 11). However, Ayer then goes on to say, somewhat incongruously: "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 linked are of the same character. The only relations involved in them are spatio-temporal" (ibid, my underlining). Ayer's uneasiness is readily understood: the 'more' we are saying besides describing the various motions of the billiard balls is quite simply that these motions are produced in a certain way, namely, by one billiard ball hitting and pushing the other. We can easily imagine exactly the same motions being produced in a completely different way eg. by a complicated system of electro-magnets hidden under the table. It will not do to say, as Ayer does, that the 'causal' aspect of the situation merely 'brings in an element of greater generality'.

(17) Cf. A. Michotte: "It still remains, of course, to consider

whether thereis any sort of parallelism between these causal impressions and the physical events which normally give rise to them eg. the impact of one billiard ball on another.... It would be quite wrong to try to discover the answer to our question by considering only the physical movements of these objects, for the causal impression is something quite other than a mere perception of movements... The solution immediately becomes obvious. The impression of 'force' clearly corresponds to the kinetic energy of the ball which is the motor object, and the impression of 'pushing away' corresponds to the mechanical work done ie. to the expenditure of kinetic energy in the displacement of the projectile" (op. cit. pp 226-7).

(18) Of course, there are many processes of energy-interchange in nature which we cannot perceive.

(19) Naturally, the idealist, who essentially adopts an ontology restricted purely to 'sensible qualities', must find causal relations puzzling: but this is simply because activities are not qualities. It may be remarked, incidentally, that properties are also distinct from qualities, and are, in fact, closely connected with activities; for a property is, in general, a disposition to act or behave in a certain way. Thus we have causal and non-causal properties, corresponding to causal and non-causal activities. For example: brittleness is a causal property - it is a disposition to be easily shattered; buoyancy is a non-causal property - it is a disposition to float. (Notice that 'shatter' is, in general, a transitive verb, whereas 'float' is, in general, non-transitive).

(20) Cf. L. Wittgenstein, 'Philosophical Investigations', Oxford, 1958, 66,67.

(21) Enquiry, p 74.

(22) Thus, Hume says "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" (Treatise, p 94). It may be noted that in the Treatise, though not in the Enquiry, Hume describes his two definitions of cause as considering causation "either as a philosophical or as a natural relation" (Treatise, p 170).

(23) Enquiry, p 76. It is commonly pointed out that the second part of this definition is not, in fact, equivalent to the first, since mere statements of uniformity do not entail counterfactual conditionals. See eg. A. Flew, 'Hume's Philosophy of Belief', London, 1961, pp 130 ff.

(24) It may be noticed that this definition of causal relations explicates them in terms of spatio-temporal relations and relations of resemblance or similarity (class-membership). This is a typically reductivist move: an attempt to economise on one's ontology by assimilating things which are in reality quite distinct.

(25) 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, so that it would constitute a logical contradiction to assert that A and B were causally related and yet to deny that other objects similar to A and B were not. But this argument is quite trivial, since the positivist will inevitably let all the work be done by the crucial word 'similar'. Thus, if I were to point to two objects

'similar' to A and B and claim that they were not causally related, the positivist would simply retort that these objects were not sufficiently 'similar' to A and B. Indeed, perhaps the most serious single defect in the positivist definition of causation is this dependence upon the vague notion of 'similarity'.

(26) Notably, of course, J.S. Mill. See 'A System of Logic', Bk. III, Chap V.

(27) Enquiry, p 76.

Chapter 3

(1) Modern inductivists also regard 'nomological' propositions as possible conclusions of transexperiential arguments. This is a matter I shall discuss later.

(2) Here I am only considering non-demonstrative reasoning about matters of fact: deductive reasoning about matters of fact need, of course, have nothing to do with causal relations.

(3) Of course, the inductivist will claim that the conclusion really is implicitly founded upon just such an observed constant conjunction, though perhaps in a rather indirect way: but this reduction of causal inference to inductive extrapolation from past experience is precisely what I have combatted in my analysis of Hume's doctrines.

(4) See p 9 supra.

(5) See, eg. P. Achinstein, 'Concepts of Science', Baltimore, 1968, chapter 5: "....there are numerous ways to describe what one observes in a given situation, some more infused with concepts employed in various theories than others" (p 178).

(6) These terms were first introduced by H. Reichenbach in 'Experience and Prediction', Chicago, 1938, pp 5-7.

(7) K.R. Popper, notably, holds such a view. See 'The Logic of Scientific Discovery' and elsewhere. (The title of this book seems

somewhat incongruous in the circumstances).

(8) Concerning this supposed antithesis, see K. R. Popper's writings. Eg. "The logical justification of the preference for one theory over another...must be sharply distinguished from all genetic, historical and psychological questions" ('Objective Knowledge', p 67).

(9) For this view see, eg. N.R. Hanson, op. cit. pp 85 ff. For a criticism of Hanson's views see P. Achinstein, 'Law and Explanation', Oxford, 1971, pp 137 ff.

(10) Cf. P. Achinstein: "The distinction between discovery and justification, so far as reasoning is concerned, depends not on the mode of reasoning but on the state of knowledge of the reasoner and on his purpose, or lack of it, in reasoning" ('Law and Explanation, p 141).

(11) Obviously, in some circumstances this would not be the sort of explanation desired, however. The inquirer may realize that the kettle has been on the fire for some time, and really want to know <u>for what purpose</u> it is boiling.

(12) "In almost all our previous discussions we have been concerned in the attempt to get clear as to our data in the way of knowledge of existence... So far, our answer has been that we are acquainted with our sense-data and, probably, with ourselves. These we know to exist... But if we are to be able to draw inferences from these data - if we are to know of the existence of matter, of other people, of the past before our individual memory begins, or of the future, we must know general principles

of some kind by means of which such inferences can be drawn... If this were not known to us, we could never extend our knowledge beyond the sphere of our private experience" (Russell, op. cit. p 33). It may be remarked that Russell's approach is precisely paralleled even in so recent a book as S. Blackburn's, whose introductory remarks are: "The subject of this book is a certain way of reasoning. This way of reasoning provides the basis for some of our most familiar expectations and beliefs... The experience of every one of us is limited and fragmentary, for we each perceive only a little of the world, as it now is, and remember only a fraction of the world as it once was. Yet the scope of our beliefs is wider than this. We award ourselves the right to believe a great variety of things whose truth is in no way a matter of our experience" ('Reason and Prediction', Cambridge, 1973, p 1).

(13) Russell was apparently confused over this point. Thus, at one point he asks: "Is this belief a mere blind outcome of past experience, or can it be justified as a reasonable belief?" - as though these were mutually exclusive alternatives. But later he remarks that we have to "distinguish the fact that past uniformities cause expectations as to the future, from the question whether there is any reasonable ground for giving weight to such expectations after the question of their validity has been raised."

(14) Actually, Russell himself also allows that predictive propositions may be directly supported by inductive arguments (see op. cit. p 37). But most inductivists believe that predictions may only be deduced from laws previously established by induction: see, eg. W. Kneale, op. cit. p 45, and G.H. Von Wright, op. cit. p 11.

However, S. Blackburn argues against this view op. cit. pp 7 ff. Of course, K.R. Popper, who repudiates induction altogether, believes that predictions may only be deduced from nomological hypotheses: see 'The Logic of Scientific Discovery' pp 59 ff.

(15) Other examples are: Boyle's law, Hooke's law, Snell's law, Ohm's law and Bragg's law.

(16) Thus J. Nicod uses the term 'character' in 'Geometry and Induction', California, 1970, p 193; and J.M. Keynes uses the term 'characteristic' in 'A Treatise on Probability', London, 1921, p 22.

(17) However, 'cause' is usually understood here in the sense of 'sufficient condition': see, eg. J. Nicod, op. cit. p 193. But J. S. Mill, who included succession in his concept of causation (as did Hume) regarded a 'cause' as an antecedent sufficient condition, so that on this interpretation most taxonomic laws, such as 'All ravens are black', would not qualify as 'causal laws' (cf. G.H. Von Wright, op. cit. pp 61-2). W. Kneale would also maintain that not all taxonomic laws can regarded as causal laws: "...there are many laws of nature which cannot be expressed as causal connexions, for example, that all mammals are vertebrates" (op. cit. p 62). But this is, again, simply because according to Kneale "...the most primitive use of the word 'cause' in application to events" is the sense in which "a cause is some happening earlier than the effect on hearing of which we find the occurrence of the effect intelligible" (op. cit. p 61). Now, I shall later maintain that a law such as 'All ravens are black' does refer to a causal connexion between ravenhood and blackness: but this is not because I agree with Nicod in using the term 'cause' to mean 'sufficient condition'

rather than with Mill in using it to mean 'antecedent sufficient condition'. For both of these analyses presuppose the 'universal uniformity' interpretation of causality.

(18) F.P. Ramsey, op. cit. pp 237 ff.

(19) See, eg. W. Kneale op. cit. p 77. Kneale gives a clear exposition of the various views concerning natural laws.

(20) See 'The Logic of Scientific Discovery' pp 62 ff, and elsewhere. This view is, of course, closely connected with the positivist concept of causation which we discussed earlier.

(21) See W. Kneale op. cit. p 75. See also W. Kneale, 'Natural Laws and Contrary-to-Fact Conditionals', reprinted in 'Philosophy and Analysis', ed. M. Macdonald, Oxford, 1954, pp 226 ff.

(22) See W. Kneale, 'Probability and Induction', p 71, pp 78 ff and pp 254 ff. It is not absolutely clear what Kneale means by a 'principle of necessitation', save that (1) he is emphatic that such principles do not state mere contingent matters of fact (p 75), (11) he considers that such principles cannot be known apriori, in the manner of mathematical principles, (p 254) and (111) his views are similar to, or even derivative from, Locke's notion of 'real essence' (p 71).

(23) This, at least, is what I take Kneale to mean by saying that 'principles of necessitation' cannot be known apriori. The supposedly 'transexperiential' character of nomological propositions corresponds, of course, to Hume's assumption that causal relations are never possible

objects of perception. Indeed, the whole controversy over 'natural laws' simply repeats the old controversy over the nature of causal relations.

(24) Kneale's is, in any case, very much a minority view, and has received much criticism elsewhere: see eg. A.J. Ayer, op. cit. pp 15-17.

(25) See, eg. C.G. Hempel, op. cit. pp 297 ff: "...the method of deductive subsumption under general laws lends itself also to the explanation of what might be called 'general facts' or uniformities, such as those expressed by laws of nature... Thus, an explanation of the regularities expressed by Galileo's law can be achieved by deducing the latter from the Newtonian laws and from statements specifying the mass and the radius of the earth" (p 299).

(26) Cf. R. Harre, 'The Principles of Scientific Thinking', London, 1970, p 153. It was largely from a feeling that the deductivist philosophers could not explain this distinction that Kneale was led to construe natural laws as 'principles of necessitation' (see 'Probability and Induction', p 75). We can now see that Kneale's solution was unnecessarily drastic.

(27) K.R. Popper, 'The Logic of Scientific Discovery', p 63.

(28) Cf. C.J. Ducasse: "The causal relation is essentially a relation between concrete individual events; and it is only so far as these events exhibit likeness to others, and can therefore be grouped with them into kinds, that it is possible to pass from individual causal facts to causal laws" (op. cit. p 6).

(29) I am not just saying here that ravens are by definition black - though this might indeed be so; but if it were so, it would only be because blackness is so inextricably causally related with the genetic structure of a raven that the genetic changes required to prevent blackness would produce such extensive changes in other features of the bird, that it would no longer bear any resemblance to ravens, would not be able to interbreed with ravens, and so on. Thus, if ravens were 'by definition' black, it would only be because we tailor our definitions to fit the facts, that is to reflect the causal restraints really existing in Nature. (Cf. chapter 2, note (6)). In fact, of course, since we admit the possibility of albino ravens, it seems that ravens are not black 'by definition'.

(30) In fact, of course, the exigencies of experimentation require us to use a fairly large number of specimens. But the point is that, pace Popper, we may discover the truth of the law without having to observe, per impossibile, an infinite number of ravens.

(31) Cf. C.J. Ducasse: "...uniformity of succession thus constitutes not at all the meaning of the cause-effect relation, but at the most only evidence of the existence of some causal connexion, perhaps very remote and indirect, and yet to be discovered, between the terms of the succession. A causal connexion explains the regularity of the succession, but is not constituted by such regularity." (op. cit. p 7). Cf. also R. Harre, op. cit. chapter 4, pp 92 ff.

(32) However, it is easy to see why those philosophers who explicitly deny the existence of causal ties at the level of particular objects

should tacitly reintroduce an element of causality at the higher level of natural laws. 'Constraining' is itself a causal relation.

(33) This is not to deny that predictions may legitimately and usefully be deduced from laws: but is only to assert that such inferences are logically posterior to causal inferences from particular facts.

(34) For comparable characterisations of the term 'inductive argument' see the following: Russell, op. cit. p 37; Keynes, op. cit. p 219; Nicod, op. cit. p 178; Kneale, op. cit. p 226; Von Wright, op. cit. p 159; Blackburn op. cit. pp 6-7. Some philosophers, however, seem to characterise an 'inductive argument' simply as an argument involving non-demonstrative inference - that is, as a non-deductive argument: see, eg. R. Swinburne, 'An Introduction to Confirmation Theory', London, 1973, p 180. But this is clearly mistaken.

(35) Some philosophers, such as J.M. Keynes, distinguish between 'pure' enumerative induction and induction involving 'variation of circumstances'. See Keynes, op. cit. pp 233 ff and L.J. Cohen 'The Implications of Induction', London, 1970, pp 106 ff. This distinction, important though it is, does not at present concern me, however. Induction involving 'variation of circumstances' should, incidentally, be distinguihsed from the theory of induction by elimination developed by J. S. Mill, which is a species of demonstrative inference: see Von Wright, op. cit. pp 60 ff.

(36) Cf. W. Kneale, op. cit. pp 228 ff.

(37) Cf. W. Kneale, op. cit. pp 228 ff and K.R. Popper, 'The Logic

of Scientific Discovery', chapter VII, pp 136 ff.

(38) Eg. R. Swinburne argues that judgments of mathematical simplicity are dependent upon the mathematical notation in current use: see Swinburne, op. cit. pp 108 ff.

(39) Cf. R. Ackermann, 'Inductive Simplicity', Philosophy of Science, 1961; reprinted in 'The Philosophy of Science', ed. P. H. Nidditch, Oxford, 1968, pp 124 ff.

(40) See pp 17-19 supra.

(41) Cf. R. Swinburne's introduction to 'The Justification of Induction', Oxford, 1974, pp 10 ff. Swinburne calls these three approaches the 'analytic', the 'inductive' and the 'pragmatic' justifications of induction.

(42) P.F. Strawson takes this view: "So to ask whether it is reasonable to place reliance on inductive procedures is 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" (op. cit. p 257). See also P. Edwards, 'Russell's Doubts About Induction', reprinted in 'The Justification of Induction', ed. R. Swinburne, pp 26 ff.

(43) For this view see, eg. R.B. Braithwaite, 'Scientific Explanation', Cambridge, 1953, pp 264 ff. Braithwaite calls this the 'predictionist' justification of induction.

(44) A 'principle of inductive inference' and Hume's supposition' certainly differ in certain respects: but as far as the charge of circularity is concerned, they play exactly the same role. For, in either case, to support such a 'principle' or such a 'supposition' by inductive arguments founded on the self-same 'principle' or 'supposition' would constitute, at least prima facie, a case of vicious circularity.

(45) Eg. W. Kneale: "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" (op. cit. p 235). See also H. Reichenbach, 'Wahrscheinlichkeitslehre', Leiden, 1935. Reichenbach was the first to conceive of the 'pragmatic' justification of induction.

(46) I might concede that in the <u>total</u> absence of any knowledge of the causal properties or likely behaviour of the objects under consideration, an 'inductive policy' might be the best way to make predictions. But otherwise, I would claim, <u>causal inference</u> is the best way of discovering the unknown.

(47) See chapter 1, note (31).

(48) Naturally, in a causal inference, the certainty with which I infer the conclusion from the premises will not be <u>logical</u> certainty. But, clearly, no sane man would hesitate to infer, for example, that this stone will fall to the ground if I let it go, merely on the grounds that is is <u>logically</u> possible that this should not occur. This kind of Humean scepticism would only be satisfied by making causal inference deductively valid.

(49) It is interesting here to compare the meteorological case with that of the discovery of Boyle's law. This law was, apparently, discovered inductively by interpolation and extrapolation of observations of the pressure and volume of specimens of air. In this case the inductive inference was justified by certain very elementary knowledge about the causal properties of air - knowledge which we acquire through everyday experience. But the law can also be derived purely through causal inference from the molecular constitution of gases (the kinetic theory of gases). However, here we clearly require a much deeper knowledge of the causal properties of gases. In the case of weather-forecasting we do not, as yet, have such deeper knowledge and must accordingly rely heavily upon inductive arguments.

(50) In saying this I am not denying that it may be possible, and even useful, to abstract from inductive arguments certain general criteria of 'inductive support' without any regard to the specific causal inferences which are tacitly or explicitly presupposed by any particular inductive inference. The simplest account of such criteria is that inductive support varies in strength according to two main factors: the number of 'positive instances' taken into account and the variety of their circumstances. (See eg. P.F. Strawson, op. cit. p 246. For a more comprehensive account see L. J. Cohen, op. cit.)

(51) This would be rather similar to the deductive systems of hypotheses which, according to some philosophers, characterise a 'science'. Cf. R.B. Braithwaite, op. cit. pp 12-21.

(52) Of course, I did appeal to this when I argued against the view,

held by Strawson and others, that induction is paradigmatically rational. But in that case I was justified since the question at issue was not merely the coherence of inductivism, but the meaning of the term 'rational argument'.

(53) It might be objected here that causal inference, too, implies an infinite regress in that in order to justify any particular causal inference we may be required to produce further arguments supporting the claim that a certain causal relation exists. However, the point is that given the perceptibility of causal relations many causal inferences will never require to be justified in this way. We are only required to justify a causal inference when the existence of a certain causal relation is actually called into question. But in the case of inductive inference we are always required to specify the further assumptions which justify the inference simply because of the absence of any inference-relation between the premises and conclusion of an inductive argument. Given the perceptibility of causal relations, however, there is no insurmountable problem as to how human beings can have begun to acquire transexperiential knowledge, since experience itself leads us 'beyond' experience.

Appendix

(1) F. Bacon, 'Novum Organum', I, 46.

(2) See K.R. Popper, 'The Logic of Scientific Discovery'.

(3) Perhaps the majority of observations and experimental results in science are, indeed, rejected simply because they do not accord with the prevailing theory.

(4) Nicod and Von Wright both take this approach. See J. Nicod op. cit. and G.H. Von Wright, op. cit.

(5) See chapter 4, note (17).

(6) Eg. the system discussed by J. Nicod, op. cit.

(7) The tendency, of which this is an example, to confuse eliminative induction with enumerative induction in which the circumstances of the confirming instances are varied, is a fairly common one. Keynes' account of 'Argument by Analogy' tends to suffer from this confusion. See also P. F. Strawson, op. cit. p 246.

(8) R. Carnap, 'The Logical Foundations of Probability', Chicago, 1950. For a more recent account see R. Swinburne, 'An Introduction to Confirmation Theory'.

(9) I shall not consider the various 'paradoxical' confirmations (see C.G. Hempel, op. cit.) since whatever else may or may not constitute

a 'confirming instance' of an hypothesis such as 'All ravens are black', it is agreed on all hands that a black raven is one: and any deficiencies in the probabilistic theory of confirmation discovered in the case of 'normal' confirmations will extend a fortiori to the case of 'paradoxical' confirmations.

(10) See, eg. W. Kneale, op. cit.

(11) S.E. Toulmin, 'The Uses of Argument', Cambridge, 1956.

(12) Nicod's symbolism was actually slightly different.

(13) In fact it will turn out that, as in the case of eliminative induction, such a judgment must be made on the basis of causal inferences.

(14) This may be regarded as a proof that inductive 'support' or 'confirmation' cannot be mapped onto the probability calculus. Cf. L. J. Cohen, op. cit. pp 13 ff.

(15) See, eg. W. Kneale op. cit. and R.B. Braithwaite, op. cit.

(16) Cf. S.E. Toulmin, op. cit.

(17) In fact it would appear that probability judgments in games of chance are ultimately based on causal arguments, which should not surprise us. For example, how do we judge that the probability of a normal die falling with a given face uppermost may be proportioned to the reciprocal of the number of faces possessed by the die? Clearly, we reason as

follows: the die, if it is a normal one, is a cubical object made of homogeneous material. When shaken and thrown, momentum is imparted to it and it is subject to the force of gravity and various frictional forces. These factors ultimately determine how the die will come to rest. But, owing to the structure of the die, we know that a very slight differential in momentum or force in one direction or another will be sufficient to cause the die to fall on one face rather than another. Now, for this reason, and because we are ignorant of the 'initial conditions' and of the precise quantities of momentum and force affecting the die in a given throw, we estimate that all the various possible outcomes of a given throw are equiprobable. Thus, by saying that the probability of throwing a 'six' with the next throw of the die equals 1/6, we are not merely claiming that the frequency of 'sixes' in a long series of throws will approximate to 1/6. On the contrary, we would predict and explain such a frequency by much the same arguments as we would use to justify our probability judgment; and the observation of such a frequency would merely constitute additional evidence for the homogeneity and symmetry of the die. Of course, in the case of some natural phenomena, such as the radioactive disintegration of atoms, a probability judgment may not simply reflect our ignorance of some of the causal factors affecting a certain situation, but may reflect some natural 'indeterminacy' in the objects under consideration - that is, may reflect the actual absence of causal factors affecting a certain situation.

(18) I refer principally to the doctrines of K.R. Popper. See 'The Logic of Scientific Discovery', and elsewhere.

(19) See, especially, I. Lakatos, 'The Methodology of Scientific

Research Programmes', in 'Criticism and the Growth of Knowledge', ed. I. Lakatos and A. Musgrave, Cambridge, 1969.

(20) See K.R. Popper, 'Objective Knowledge', chapter 1.

(21) One of the most serious defects in Popper's theory of knowledge is his virtually uncritical acceptance of Hume's sceptical arguments. Popper believes that he can accomodate both rationalism and scepticism but, as we shall see, he underestimates the corrosive power of the latter.

(22) Of course, once we acknowledge the validity of non-demonstrative inference, and especially of causal inference, it is easy to give reasons why falsified hypotheses should be rejected.

(23) This simply confirms our conclusion in chapter 3, that deductive inference from laws is logically posterior to causal inference from particular facts.

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