deflected, the reading on the ammeter may or may not increase. We cannot make the outcomes conform to our theories. It was because the physical world is the way it is that the experiment conducted by Hertz yielded no deflection of cathode rays and the modified experiment conducted by Thomson did. It was the material differences in the experimental arrangements of the two physicists that led to the differing outcomes, not the differences in the theories held by them. It is the sense in which experimental outcomes are determined by the workings of the world rather than by theoretical views about the world that provides the possibility of testing theories against the world. This is not to say that significant results are easily achievable and infallible, nor that their significance is always straightforward. But it does help to establish the point that the attempt to test the adequacy of scientific theories against experimental results is a meaningful quest. What is more, the history of science gives us examples of cases where the challenge was successfully met.

Further reading

The second half of Hacking (1983) was an important early move in the new interest philosophers of science have taken in experiment. Other explorations of the topic are Franklin (1986), Franklin (1990), Galison (1987) and Mayo (1996), although these detailed treatments will take on their full significance only in the light of chapter 13, on the "new experimentalism". The issues raised in this chapter are discussed in a little more detail in Chalmers (1984).

CHAPTER 4

Deriving theories from the facts: induction

Introduction

In these early chapters of the book we have been considering the idea that what is characteristic of scientific knowledge is that it is derived from the facts. We have reached a stage where we have given some detailed attention to the nature of the observational and experimental facts that can be considered as the basis from which scientific knowledge might be derived, although we have seen that those facts cannot be established as straightforwardly and securely as is commonly supposed. Let us assume, then, that appropriate facts can be established in science. We must now face the question of how scientific knowledge can be derived from those facts.

"Science is derived from the facts" could be interpreted to mean that scientific knowledge is constructed by first establishing the facts and then subsequently building the theory to fit them. We discussed this view in chapter 1 and rejected it as unreasonable. The issue that I wish to explore involves interpreting "derive" in some kind of logical rather than temporal sense. No matter which comes first, the facts or the theory, the question to be addressed is the extent to which the theory is borne out by the facts. The strongest possible claim would be that the theory can be logically derived from the facts. That is, given the facts, the theory can be proven as a consequence of them. This strong claim cannot be substantiated. To see why this is so we must look at some of the basic features of logical reasoning.

Baby logic

Logic is concerned with the deduction of statements from other, given, statements. It is concerned with what follows from what. No attempt will be made to give a detailed account and appraisal of logic or deductive reasoning here. Rather, I will make the points that will be sufficient for our purpose with the aid of some very simple examples.

Here is an example of a logical argument that is perfectly adequate or, to use the technical term used by logicians, perfectly valid.

Example 1
1. All books on philosophy are boring.
2. This book is a book on philosophy.

3. This book is boring.

In this argument, (1) and (2) are the premises and (3) is the conclusion. It is evident, I take it, that if (1) and (2) are true then (3) is bound to be true. It is not possible for (3) to be false once it is given that (1) and (2) are true. To assert (1) and (2) as true and to deny (3) is to contradict oneself. This is the key feature of a *logically valid* deduction. If the premises are true then the conclusion must be true. Logic is truth preserving.

A slight modification of Example (1) will give us an instance of an argument that is not valid.

Example 2
1. Many books on philosophy are boring.
2. This book is a book on philosophy.

3. This book is boring.

In this example, (3) does not follow of necessity from (1) and (2). Even if (1) and (2) are true, then this book might yet turn out to be one of the minority of books on philosophy that are not boring. Accepting (1) and (2) as true and holding (3) to be false does not involve a contradiction. The argument is invalid.

The reader may by now be feeling bored. Experiences of that kind certainly have a bearing on the truth of statements (1) and (3) in Example 1 and Example 2. But a point that

needs to be stressed here is that logical deduction alone cannot establish the truth of factual statements of the kind figuring in our examples. All that logic can offer in this connection is that *if* the premises are true and the argument is valid *then* the conclusion must be true. But whether the premises are true or not is not a question that can be settled by an appeal to logic. An argument can be a perfectly valid deduction even if it involves a false premise. Here is an example.

Example 3
1. All cats have five legs.
2. Bugs Pussy is my cat.

3. Bugs Pussy has five legs.

This is a perfectly valid deduction. If (1) and (2) are true then (3) must be true. It so happens that, in this example (1) and (3) are false. But this does not affect the fact that the argument is valid.

There is a strong sense, then, in which logic alone is not a source of new truths. The truth of the factual statements that constitute the premises of arguments cannot be established by appeal to logic. Logic can simply reveal what follows from, or what in a sense is already contained in, the statements we already have to hand. Against this limitation we have the great strength of logic, namely, its truth-preserving character. If we can be sure our premises are true then we can be equally sure that everything we logically derive from them will also be true.

Can scientific laws be derived from the facts?

With this discussion of the nature of logic behind us, it can be straightforwardly shown that scientific knowledge cannot be derived from the facts if "derive" is interpreted as "logically deduce".

Some simple examples of scientific knowledge will be sufficient for the illustration of this basic point. Let us consider some low-level scientific laws such as "metals expand when heated" or "acids turn litmus red". These are general statements. They are examples of what philosophers refer to as universal statements. They refer to all events of a particular kind, all instances of metals being heated and all instances of litmus being immersed in acid. Scientific knowledge invariably involves general statements of this kind. The situation is quite otherwise when it comes to the observation statements that constitute the facts that provide the evidence for general scientific laws. Those observable facts or experimental results are specific claims about a state of affairs that obtains at a particular time. They are what philosophers call singular statements. They include statements such as "the length of the copper bar increased when it was heated" or "the litmus paper turned red when immersed in the beaker of hydrochloric acid". Suppose we have a large number of such facts at our disposal as the basis from which we hope to derive some scientific knowledge (about metals or acids in the case of our examples). What kind of argument can take us from those facts, as premises, to the scientific laws we seek to derive as conclusions? In the case of our example concerning the expansion of metals the argument can be schematised as follows:

Premises

1. Metal x_1 expanded when heated on occasion t_1.
2. Metal x_2 expanded when heated on occasion t_2.
n. Metal x_n expanded when heated on occasion t_n.

Conclusion

All metals expand when heated.

This is not a logically valid argument. It lacks the basic features of such an argument. It is simply not the case that if the statements constituting the premises are true then the conclusion must be true. However many observations of expanding metals we have to work with, that is, however great n might be in our example, there can be no *logical* guarantee that some sample of metal might on some occasion contract when heated. There is no contradiction involved in claiming both that all known examples of the heating of metals has resulted in expansion and that "all metals expand when heated" is false.

This straightforward point is illustrated by a somewhat gruesome example attributed to Bertrand Russell. It concerns a turkey who noted on his first morning at the turkey farm that he was fed at 9 am. After this experience had been repeated daily for several weeks the turkey felt safe in drawing the conclusion "I am always fed at 9 am". Alas, this conclusion was shown to be false in no uncertain manner when, on Christmas eve, instead of being fed, the turkey's throat was cut. The turkey's argument led it from a number of true observations to a false conclusion, clearly indicating the invalidity of the argument from a logical point of view.

Arguments of the kind I have illustrated with the example concerning the expansion of metals, which proceed from a finite number of specific facts to a general conclusion, are called *inductive* arguments, as distinct from logical, *deductive* arguments. A characteristic of inductive arguments that distinguishes them from deductive ones is that, by proceeding as they do from statements about *some* to statements about *all* events of a particular kind, they go beyond what is contained in the premises. General scientific laws invariably go beyond the finite amount of observable evidence that is available to support them, and that is why they can never be proven in the sense of being logically deduced from that evidence.

What constitutes a good inductive argument?

We have seen that if scientific knowledge is to be understood as being derived from the facts, then "derive" must be understood in an inductive rather than a deductive sense. But what are the characteristics of a good inductive argument? The question is of fundamental importance because it is clear that not all generalisations from the observable facts are warranted. Some of them we will wish to regard as overhasty or based on insufficient evidence, as when, perhaps, we condemn the attribution of some characteristic to an entire ethnic group based on some unpleasant encounters with just one pair of neighbours. Under precisely what circumstances is it legitimate to assert that a scientific law has been "derived" from some finite body of observational and experimental evidence?

A first attempt at an answer to this question involves the demand that, if an inductive inference from observable facts to laws is to be justified, then the following conditions must be satisfied:

1. The number of observations forming the basis of a generalisation must be large.
2. The observations must be repeated under a wide variety of conditions.
3. No accepted observation statement should conflict with the derived law.

Condition 1 is regarded as necessary because it is clearly not legitimate to conclude that all metals expand when heated on the basis of just one observation of an iron bar's expansion, say, any more than it is legitimate to conclude that all Australians are drunkards on the basis of one observation of an intoxicated Australian. A large number of independent observations would appear to be necessary before either generalisation can be justified. A good inductive argument does not jump to conclusions.

One way of increasing the number of observations in the examples mentioned would be to repeatedly heat a single bar of metal or to continually observe a particular Australian getting drunk night after night, and perhaps morning after morning. Clearly, a list of observation statements acquired in such a way would form a very unsatisfactory basis for the respective generalisations. That is why condition 2 is necessary. "All metals expand when heated" will be a legitimate generalisation only if the observations of expansion on which it is based range over a wide variety of conditions. Various kinds of metals should be heated, long bars, short bars, silver

bars, copper bars etc. should be heated at high and low pressures and high and low temperatures and so on. Only if on all such occasions expansion results is it legitimate to generalise by induction to the general law. Further, it is evident that if a particular sample of metal is observed not to expand when heated, then the generalisation to the law will not be justified. Condition 3 is essential.

The above can be summed up by the following statement of *the principle of induction.*

> If a large number of **A**'s have been observed under a wide variety of conditions, and if all those **A**'s without exception possess the property **B**, then all **A**'s have the property **B**.

There are serious problems with this characterisation of induction. Let us consider condition 1, the demand for large numbers of observations. One problem with it is the vagueness of "large". Are a hundred, a thousand or more observations required? If we do attempt to introduce precision by introducing a number here, then there would surely be a great deal of arbitrariness in the number chosen. The problems do not stop here. There are many instances in which the demand for a large number of instances seems inappropriate. To illustrate this, consider the strong public reaction against nuclear warfare that was provoked by the dropping of the first atomic bomb on Hiroshima towards the end of the Second World War. That reaction was based on the realisation of the extent to which atomic bombs cause widespread destruction and human suffering. And yet this widespread, and surely reasonable, belief was based on just one dramatic observation. In similar vein, it would be a very stubborn investigator who insisted on putting his hand in the fire many times before concluding that fire burns. Let us consider a less fanciful example related to scientific practice. Suppose I reproduced an experiment reported in some recent scientific journal, and sent my results off for publication. Surely the editor of the journal would reject my paper, explaining that the experiment had already been done! Condition 1 is riddled with problems.

Condition 2 has serious problems too, stemming from difficulties surrounding the question of what counts as a significant variation in circumstances. What counts as a significant variation in the circumstances under which the expansion of a heated metal is to be investigated? Is it necessary to vary the type of metal, the pressure and the time of day? The answer is "yes" in the first and possibly the second case but "no" in the third. But what are the grounds for that answer? The question is important because unless it can be answered the list of variations can be extended indefinitely by endlessly adding further variations, such as the size of the laboratory and the colour of the experimenter's socks. Unless such "superfluous" variations can be eliminated, the conditions under which an inductive inference can be accepted can never be satisfied. What are the grounds, then, for regarding a range of possible variations as superfluous? The commonsense answer is straightforward enough. We draw on our prior knowledge of the situation to distinguish between the factors that might and those that cannot influence the system we are investigating. It is our knowledge of metals and the kinds of ways that they can be acted on that leads us to the expectation that their physical behaviour will depend on the type of metal and the surrounding pressure but not on the time of day or the colour of the experimenter's socks. We draw on our current stock of knowledge to help judge what is a relevant circumstance that might need to be varied when investigating the generality of an effect under investigation.

This response to the problem is surely correct. However, it poses a problem for a sufficiently strong version of the claim that scientific knowledge should be derived from the facts by induction. The problem arises when we pose the question of how the knowledge appealed to when judging the relevance or otherwise of some circumstances to a phenomenon under investigation (such as the expansion of metals) is itself vindicated. If we demand that that knowledge itself is to be arrived at by induction, then our problem will recur, because those further inductive arguments will themselves require the specification of the relevant circumstances and so on. Each inductive argument involves an appeal to prior knowledge, which needs an inductive argument to justify it, which involves an appeal to further prior knowledge and so on in a never-ending chain. The demand that all knowledge be justified by induction becomes a demand that cannot be met.

Even Condition 3 is problematic since little scientific knowledge would survive the demand that there be no known exceptions. This is a point that will be discussed in some detail in chapter 7.

Further problems with inductivism

Let us call the position according to which scientific knowledge is to be derived from the observable facts by some kind of inductive inference *inductivism* and those who subscribe to that view *inductivists*. We have already pointed to a serious problem inherent in that view, namely, the problem of stating precisely under what conditions a generalisation constitutes a good inductive inference. That is, it is not clear what induction amounts to. There are further problems with the inductivist position.

If we take contemporary scientific knowledge at anything like face value, then it has to be admitted that much of that knowledge refers to the unobservable. It refers to such things as protons and electrons, genes and DNA molecules and so on. How can such knowledge be accommodated into the inductivist position? Insofar as inductive reasoning involves some kind of generalisation from observable facts, it would appear that such reasoning is not capable of yielding knowledge of the unobservable. Any generalisation from facts about the observable world can yield nothing other than generalisations about the observable world. Consequently, scientific knowledge of the unobservable world can never be established by the kind of inductive reasoning we have discussed.

This leaves the inductivist in the uncomfortable position of having to reject much contemporary science on the grounds that it involves going beyond what can be justified by inductive generalisation from the observable.

Another problem stems from the fact that many scientific laws take the form of exact, mathematically formulated laws. The law of gravitation, which states that the force between any two masses is proportional to the product of those masses divided by the square of the distance that separates them, is a straightforward example. Compared with the exactness of such laws we have the inexactness of any of the measurements that constitute the observable evidence for them. It is well appreciated that all observations are subject to some degree of error, as reflected in the practice of scientists when they write the result of a particular measurement as $x \pm dx$, where the dx represents the estimated margin of error. If scientific laws are inductive generalisations from observable facts it is difficult to see how one can escape the inexactness of the measurements that constitute the premises of the inductive arguments. It is difficult to see how exact laws can ever be inductively justified on the basis of inexact evidence.

A third problem for the inductivist is an old philosophical chestnut called the problem of induction. The problem arises for anyone who subscribes to the view that scientific knowledge in all its aspects must be justified either by an appeal to (deductive) logic or by deriving it from experience. David Hume was an eighteenth-century philosopher who did subscribe to that view, and it was he who clearly articulated the problem I am about to highlight.

The problem arises when we raise the question of how induction itself is to be justified. How is the principle of induction to be vindicated? Those who take the view under discussion have only two options, to justify it by an appeal to logic or by an appeal to experience. We have already seen that the first option will not do. Inductive inferences are not logical (deductive) inferences. This leaves us with the second option, to attempt to justify induction by an appeal to experience.

What would such a justification be like? Presumably, it would go something like this. Induction has been observed to work on a large number of occasions. For instance, the laws of optics, derived by induction from the results of laboratory experiments, have been used on numerous occasions in the design of optical instruments that have operated satisfactorily, and the laws of planetary motion, inductively derived from the observation of planetary positions, have been successfully used to predict eclipses and conjunctions. This list could be greatly extended with accounts of successful predictions and explanations that we presume to be made on the basis of inductively derived scientific laws and theories. Thus, so the argument goes, induction is justified by experience.

This justification of induction is unacceptable. This can be seen once the form of the argument is spelt out schematically as follows:

The principle of induction worked successfully on occasion x_1
The principle of induction worked successfully on occasion x_2 etc.

The principle of induction always works

A general statement asserting the validity of the principle of induction is here inferred from a number of individual instances of its successful application. The argument is therefore itself an inductive one. Consequently, the attempt to justify induction by an appeal to experience involves assuming what one is trying to prove. It involves justifying induction by appealing to induction, and so is totally unsatisfactory.

One attempt to avoid the problem of induction involves weakening the demand that scientific knowledge be proven true, and resting content with the claim that scientific claims can be shown to be probably true in the light of the evidence. So the vast number of observations that can be invoked to support the claim that materials denser than air fall downwards on earth, although it does not permit us to prove the truth of the claim, does warrant the assertion that the claim is probably true. In line with this suggestion we can reformulate the principle of induction to read, "if a large number of

A's have been observed under a wide variety of conditions, and if all these observed A's have the property B, then all A's probably have the property B". This reformulation does not overcome the problem of induction. The reformulated principle is still a universal statement. It implies, on the basis of a finite number of successes, that all applications of the principle will lead to general conclusions that are probably true. Consequently, attempts to justify the probabilistic version of the principle of induction by an appeal to experience involve an appeal to inductive arguments of the kind that are being justified just as the principle in its original form did.

There is another basic problem with interpretations of inductive arguments that construe them as leading to probable truth rather than truth. This problem arises as soon as one tries to be precise about just how probable a law or theory is in the light of specified evidence. It may seem intuitively plausible that as the observational support for a general law increases the probability that it is true also increases. But this intuition does not stand up to inspection. Given standard probability theory, it is difficult to avoid the conclusion that the probability of any general law is zero whatever the observational evidence. To make the point in a non-technical way, any observational evidence will consist of a finite number of observation statements, whereas the general law will make claims about an infinity of possible cases. The probability of the law in the light of the evidence is thus a finite number divided by infinity, which remains zero by whatever factor the finite amount of evidence is increased. Looking at it in another way, there will always be an infinite number of general statements that are compatible with a finite number of observation statements, just as there is an infinity of curves that can be drawn through a finite number of points. That is, there will always be an infinite number of hypotheses compatible with a finite amount of evidence. Consequently, the probability of any one of them being true is zero. In chapter 12 we will discuss a possible way around this problem.

In this and the preceding section we have revealed two kinds of problem with the idea that scientific knowledge is derived from the facts by some kind of inductive inference. The first concerned the issue of specifying just what an adequate inductive argument is. The second involved the circularity involved in attempts to justify induction. I regard the former problem as more severe than the latter. The reason that I do not take the problem of induction too seriously is that any attempt to provide an account of science is bound to confront a problem of a similar kind. We are bound to run into trouble if we seek rational justifications of every principle we use, for we cannot provide a *rational argument* for rational argument itself without assuming what we are arguing for. Not even logic can be *argued for* in a way that is not question begging. However, what constitutes a valid deductive argument can be specified with a high degree of precision, whereas what constitutes a good inductive argument has not been made at all clear.

The appeal of inductivism

A concise expression of the inductivist view of science, the view that scientific knowledge is derived from the facts by inductive inference which we have discussed in the opening chapters of this book, is contained in the following passage written by a twentieth-century economist.

> If we try to imagine how a mind of superhuman power and reach, but normal so far as the logical processes of its thought are concerned … would use the scientific method, the process would be as follows: First, all facts would be observed and recorded, *without selection* or *a priori* guess as to their relative importance. Secondly, the observed and recorded facts would be analysed, compared and classified, without *hypothesis or postulates*, other than those necessarily involved in the logic of thought. Third, from this analysis of the facts, generalizations would be inductively drawn as to the relations, classificatory or causal, between them. Fourth, further research would be deductive as well as inductive, employing inferences from previously established generalizations.[1]

We have seen that the idea that the collection of facts can and should take place prior to the acquisition and acceptance of any knowledge does not bear analysis. To suggest otherwise is to believe that my observations of the flora in the Australian bush will be of more value than those of a trained botanist precisely because I know little botany. Let us reject this part of our economist's characterisation of science. What remains is an account that has a certain appeal. It is summarised in figure 2. The laws and theories that make up scientific knowledge are derived by induction from a factual basis supplied by observation and experiment. Once such general knowledge is available, it can be drawn on to make predictions and offer explanations.

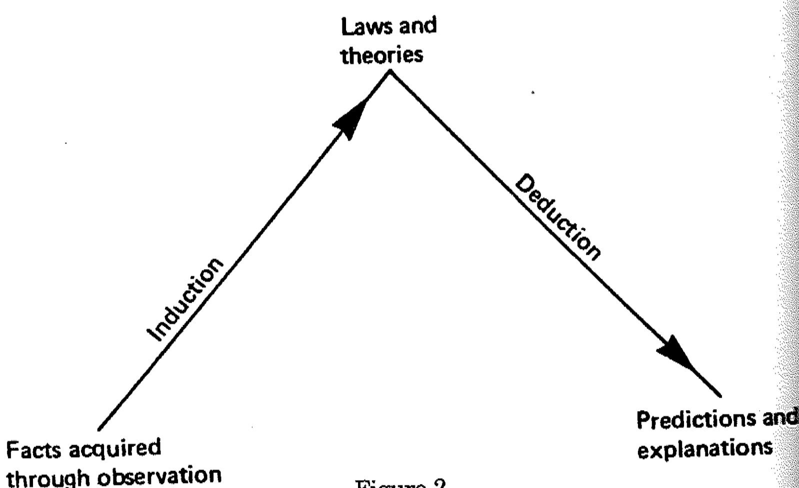

Figure 2

Consider the following argument:

1. Fairly pure water freezes at about 0°C (if given sufficient time).
2. My car radiator contains fairly pure water.

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3. If the temperature falls well below 0°C, the water in my car radiator will freeze (if given sufficient time).

Here we have an example of a valid logical argument to

deduce the prediction 3 from the scientific knowledge contained in premise 1. If 1 and 2 are true, 3 must be true. However, the truth of 1, 2 or 3 are not established by this or any other deduction. For the inductivist the source of scientific truth is experience not logic. On this view, 1 will be ascertained by direct observation of various instances of freezing water. Once 1 and 2 have been established by observation and induction, then the prediction 3 can be deduced from them.

Less trivial examples will be more complicated, but the roles played by observation, induction and deduction remain essentially the same. As a final example, I will consider the inductivist account of how physical science is able to explain the rainbow.

The simple premise 1 of the previous example is here replaced by a number of laws governing the behaviour of light, namely the laws of reflection and refraction of light and assertions about the dependence of the amount of refraction on the colour of the light. These general laws are to be derived from experience by induction. A large number of laboratory experiments are performed, reflecting rays of light from mirrors and water surfaces, measuring angles of refraction for rays of light passing from air to water, water to air and so on, under a wide variety of circumstances, until whatever conditions are presumed to be necessary to warrant the inductive derivation of the laws of optics from the experimental results are satisfied.

Premise 2 of our previous example will also be replaced by a more complex array of statements. These will include assertions to the effect that the sun is situated in some specified position in the sky relative to an observer on earth, and that raindrops are falling from a cloud situated in some specified region relative to the observer. Sets of statements like these, which describe the set-up under investigation, will be referred to as *initial conditions*. Descriptions of experimental set-ups will be typical initial conditions.

Given the laws of optics and the initial conditions, it is now

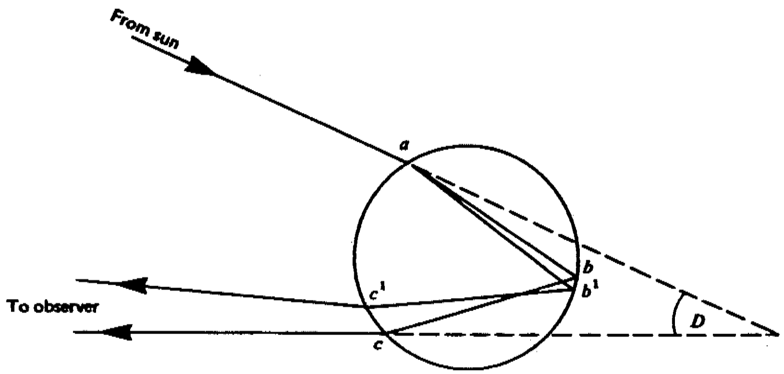

Figure 3

possible to perform deductions yielding an explanation of the formation of a rainbow visible to the observer. These deductions will no longer be as self-evident as in our previous examples, and will involve mathematical as well as verbal arguments. The derivation will run roughly as follows. If we assume a raindrop to be roughly spherical, then the path of a ray of light through a raindrop will be roughly as depicted in figure 3. For a ray of white light from the sun incident on a raindrop at *a*, the red light will travel along *ab* and the blue light along *ab'* according to the law of refraction. The law of reflection requires that *ab* be reflected along *bc* and *ab'* along *b'c'*. Refraction at *c and c'* will again be determined by the law of refraction, so that an observer viewing the raindrop will see the red and blue components of the white light separated (and also all the other colors of the spectrum). The same separation of colours will be visible to our observer for any raindrop that is situated in a region of the sky such that the line joining the raindrop to the sun makes an angle *D* with the line joining the raindrop to the observer. Geometrical considerations yield the conclusion that a coloured arc will be visible to the observer provided the rain cloud is sufficiently extended.

I have only sketched an explanation of the rainbow here, but it should suffice to illustrate the general form of the

reasoning involved. Given that the laws of optics are true (and for the unqualified inductivist this can be established from observation by induction), and given that the initial conditions are correctly described, then the explanation of the rainbow necessarily follows. The general form of all scientific explanations and predictions can be summarised thus:

1. Laws and theories
2. Initial conditions

---

3. Predictions and explanations

This is the step depicted on the right-hand side of Figure 2.

The basic inductivist account of science does have some immediate appeal. Its attraction lies in the fact that it does seem to capture in a formal way some of the commonly held intuitions about the special characteristics of scientific knowledge, namely its objectivity, its reliability and its usefulness. We have discussed the inductivist account of the usefulness of science insofar as it can facilitate predictions and explanations already in this section.

The objectivity of science as construed by the inductivist derives from the extent to which observation, induction and deduction are themselves seen as objective. Observable facts are understood to be established by an unprejudiced use of the senses in a way that leaves no room for subjective opinion to intrude. As far as inductive and deductive reasoning are concerned, these are adequate to the extent that they conform to publicly formulated criteria of adequacy, so, once again, there is no room for personal opinion. Inferences either conform to the objective standards or they don't.

The reliability of science follows from the inductivist's claims about observation and both inductive and deductive reasoning. According to the unqualified inductivist, observation statements that form the factual basis for science can be securely established directly by careful use of the senses. Further, this security will be transmitted to the laws and theories inductively derived from those facts provided the conditions for adequate inductive generalisations are met.

This is guaranteed by the principle of induction which is presumed to form the basis of science.

Attractive as it may have appeared, we have seen that the inductivist position is, at best, in need of severe qualification and, at worst, thoroughly inadequate. We have seen that facts adequate for science are by no means straightforwardly given but have to be practically constructed, are in some important senses dependent on the knowledge that they presuppose, a complication overlooked in the schematisation in figure 2, and are subject to improvement and replacement. More seriously, we have been unable to give a precise specification of induction in a way that will help distinguish a justifiable generalisation from the facts from a hasty or rash one, a formidable task given nature's capacity to surprise, epitomised in the discovery that supercooled liquids can flow uphill.

In chapter 12 we will discuss some recent attempts to rescue the inductivist account of science from its difficulties. Meanwhile, we will turn in the next two chapters to a philosopher who attempts to sidestep problems with induction by putting forward a view of science that does not involve induction.

Further reading

The historical source of Hume's problem of induction is Hume's *Treatise on Human Nature* (1939, Part 3). Another classic discussion of the problem is Russell (1912, chapter 6). A thorough, technical investigation of the consequences of Hume's argument is Stove (1973). Karl Popper's claim to have solved the problem of induction is in Popper (1979, chapter 1). Reasonably accessible accounts of inductive reasoning can be found in Hempel (1966) and Salmon (1966), and a more detailed treatment is found in Glymour (1980). See also Lakatos (1968) for a collection of essays, including a provocative survey by Lakatos himself, of attempts to construct an inductive logic.

CHAPTER 5

Introducing falsificationism

Introduction

Karl Popper was the most forceful advocate of an alternative to inductivism which I will refer to as "falsificationism". Popper was educated in Vienna in the 1920s, at a time when logical positivism was being articulated by a group of philosophers who became known as the Vienna Circle. One of the most famous of these was Rudolph Carnap, and the clash and debate between his supporters and those of Popper was to be a feature of philosophy of science up until the 1960s. Popper himself tells the story of how he became disenchanted with the idea that science is special because it can be derived from the facts, the more facts the better. He became suspicious of the way in which he saw Freudians and Marxists supporting their theories by interpreting a wide range of instances, of human behaviour or historical change respectively, in terms of their theory and claiming them to be supported on this account. It seemed to Popper that these theories could never go wrong because they were sufficiently flexible to accommodate any instances of human behaviour or historical change as compatible with their theory. Consequently, although giving the appearance of being powerful theories confirmed by a wide range of facts, they could in fact explain nothing because they could rule out nothing. Popper compared this with a famous test of Einstein's theory of general relativity carried out by Eddington in 1919. Einstein's theory had the implication that rays of light should bend as they pass close to massive objects such as the sun. As a consequence, a star situated beyond the sun should appear displaced from the direction in which it would be observed in the absence of this bending. Eddington sought for this displacement by sighting the star at a time when the light from the sun was blocked