

CHAPTER THREE

Evidence and Hypothesis

IN THIS chapter I approach the subject of hypothesis acceptance through thinking about inference, that is, by thinking about the intellectual practice of reasoning. Rather than start by analyzing the structure or language of a theory, I begin by considering the activity that has theories as (one of) its outcomes and then consider the consequences of understanding that activity for our understanding of that outcome.

Reasoning consists in determining the logical relations that exist among sets of propositions. While we often caricature reasoning as a kind of linguistic calculation that moves through and produces sequences of logical implications, this seems only one expression (and a rare one) of a much more general ability that includes determining consistency, inconsistency, contradiction, implication, and so forth. Reasoning may result in a judgment or in the suspension or withholding of judgment. This distinguishes it from inferring, which consists in the inferrer's coming to a conclusion on the basis of a logical relation the conclusion (or a sentence expressing it) purportedly bears to some presumed bit of information and/or other beliefs. Inference is thus a sometime result of the activity of reasoning.

Presumably we make inferences, accept or reject hypotheses, or assess their relative acceptability on the basis of evidence. This is to say that evidence and evidential relations are at the heart of inference and reasoning about empirical matters. Perhaps not all of our beliefs have evidential support, but at least those adopted in rational, reflective moments either have evidential support or are thought by us to have evidential support (else, these being reflective moments, we wouldn't have adopted them). This seems simple enough, until we ask what it means to say of some real or imagined state of affairs that it is or would be evidence for some hypothesis.

PHILOSOPHICAL PRELIMINARIES

In attempting to answer this question it may be useful first to distinguish three kinds of relations with which one might be concerned in discussing evidential support:

(1) A relation between a sentence describing a state of affairs said to be evidence and a sentence, the hypothesis, for which the state of affairs is said to be evidence. An example would be the relations between the members of the pair "table A is plastic" and "all tables are plastic" and of the pair "table A is not plastic" and "not all tables are plastic." In the first pair the first sentence is a consequence of the second. In the second pair, the second sentence is a logical consequence of the first.

(2) A relation between a state of affairs said to be evidence and another state of affairs described by a hypothesis for which the former is said to be evidence. For example, this table's being made of wood might be said to be (part of the) evidence that all tables are wooden, or that some tables are wooden, that not all tables are plastic, that at least one tree has been destroyed, and so on. Similarly, a line of condensation in a cloud chamber might be said to be evidence for the passage of some type of particle through the apparatus. The first three evidential relations hold, if they hold, in virtue of class inclusion and exclusion relations, the fourth and fifth in virtue of presumed causal relations.

(3) A relation between a state of affairs said to be evidence and a statement or proposition, the hypothesis, for which the former is said to be evidence, as this table's being made of wood might be someone's evidence for the hypothesis that a tree was destroyed.

A goodly amount of philosophical discussion, for example Hempel's work on confirmation and commentary on it, has been directed toward the analysis of relations in the first category. One problem with thinking about evidential relations as relations between sentences is the restriction of examples, and consequently the analysis, to sentences related by logical relations such as entailment. As we saw in the previous chapter, this is too restrictive. The second and third categories are, however, the kind of relation being referred to in nonphilosophical discussion of evidence. Scientists, lawyers, persons engaged in reasoning, are concerned about objects, events, and states of affairs and what inferences can be made, or conclusions drawn, from them. Although relations in the third category may depend in some way on relations in the second category, ordinary talk makes little distinction between locutions belonging to the two: both "Smith's sudden disappearance is evidence that Smith is guilty" and "Smith's sudden disappearance is evidence for the hypothesis that Smith is guilty" reflect acceptable usage (and, to this ear, a distinction primarily of degrees of certainty). This study follows, for the most part, the nonphilosophers' usage: it is concerned with evidential relations as relations between an object, event, or state of affairs said to be evidence and that (for convenience,

the hypothesis) for which it is said to be evidence. It is worth noting that the hypothesis, that for which there is or is said to be evidence, is not a sentence (as in 1 above) but a proposition or statement whose content can be expressed in different sentences of the same or different languages.

My concern with evidence in this chapter is primarily descriptive—that is, I am concerned not with what constitutes good evidence or with what makes one hypothesis better-supported than another but with what determines that something is or is taken to be evidence in the first place. My assumption in thus proceeding is that normative questions, questions concerning what criteria should govern our assessment of evidence, require as their basis a nonnormative analysis of the character of evidential relations. Even to talk about something being evidence independently of its being so taken by some person is to introduce an implicit assessment of its value. In order, therefore, to avoid (what would be at this stage) contamination by normative considerations I shall approach the description of evidential relations by considering what is involved in taking something to be evidence. Since it is we who, in our search for support of beliefs, hypotheses, theories, assign the status of evidence to objects and states of affairs, an analysis of the structure of this assignment will also be an analysis of evidential relations. The conceptual points concerning evidence will be illustrated by citing cases from the history of science. In addition I shall discuss the two standard approaches to evidential relations, some of whose problems were analyzed in the previous chapter and some more recent expressions of those approaches in light of the analysis developed here. This alternative analysis will put us in position to address the questions about scientific objectivity and scientific value neutrality that form the philosophical core of the discussion of biological politics. While my concern is an understanding of the role of evidence in scientific inquiry, I begin by discussing evidential relations in nonspecialized contexts.

EVIDENTIAL REASONING AND BACKGROUND ASSUMPTIONS

Any singular state of affairs, such as the level of mercury in a glass tube, of itself, points nowhere. The same is true of a pair of states, such as the levels of mercury in the tube at successive times, or the levels of different mercury samples in two different tubes. States of affairs, that is, do not carry labels indicating that for which they are evidence or for which they can be taken as evidence. Any attempt to find some unique or direct relation between states of affairs and those hypotheses for which they are taken as evidence reveals, in fact, that there is no such

relation and that anything that is the case or is imagined to be the case can be taken to be evidence that something else is the case. What determines whether or not someone will take some fact or alleged fact, x , as evidence for some hypothesis, h , is not a natural (for example, causal) relation between the state of affairs x and that described by h but are other beliefs that person has concerning the evidential connection between x and h .¹ To put it another way, states of affairs are taken as evidence in light of regularities discovered, believed, or assumed to hold. The evidential relations into which a given state of affairs can enter will thus be as varied as the beliefs about its relations with other states, or as the beliefs about the connections between a class of states to which it can be said to belong and other states (or classes of states). Some examples will make this clearer.

Consider someone, myself, coming to believe that an eight-year-old child has the measles, and suppose I base this belief on the fact that her stomach is covered with red spots. What explains why I come to believe she has the measles rather than that, say, the moon is blue, is some belief that I have about the relationship between having a red-spotted stomach and having the measles in light of which I take her red-spotted stomach to be evidence that the child has the measles. Ordinarily we might suppose that the relevant belief is that a red-spotted stomach is a symptom of the measles, but it is entirely possible that I should come to believe that she has the measles because a crystal ball reader told me that if this child's stomach appeared covered with spots on a given day she would have a disease called measles. We can even imagine the crystal ball reader going on to say that the disease is a systemic viral infection and that it can afflict the brain, without saying anything about any regular or symptomatic connection between red spots and measles. In both cases what is taken as evidence is the same: red spots on the child's stomach. What explains why it is taken as evidence differs: in the one case I believe that red spots are a symptom of measles and in the other, presuming that crystal ball readers are a reliable source of information, I believe what the reader told me. The alleged regularities in light of which the child's rash is taken as evidence of her having the measles are different: in the first case there is a regularity thought to hold between having the measles and having a red-spotted stomach, in the second there is a regularity thought to hold between the reader's predictions and what eventually happens.

¹ That is, it is not relations of the second type mentioned above but beliefs about relations of the second type that determine whether one takes some state of affairs to be evidence for some hypothesis, h .

The same state of affairs can also be taken as evidence for different hypotheses. Consider again this unfortunate child's red-spotted stomach. If one believes that red spots are a symptom of the measles, then one will take the presence of red spots as evidence that she has the measles. Suppose, however, that one believes not that stomach rashes are a symptom of the measles but that they are symptomatic of some gastric disorder. One will then take the red-spotted stomach as evidence that she has some gastric ailment. Once it is seen that the same state of affairs can be evidence for different hypotheses, depending upon what further beliefs are brought to bear upon the situation, it becomes clear that the same state of affairs can be taken as evidence for conflicting hypotheses. It is entirely possible, though perhaps improbable from our point of view, that someone will associate a red-spotted stomach with good health and good fortune and so take the red spots as evidence that she is in good health—a hypothesis not at all compatible with that attributing measles to her or that attributing a gastric disorder to her.

A quite different sort of example shows how different aspects of the same state of affairs can be taken as evidence for the same hypothesis, or, of course, for different hypotheses. Just as states of affairs do not stand in unique evidential relations with hypotheses, so, too, there is not a uniquely correct description for each object of description. A given item, event, or state of affairs can be correctly described in different ways depending on the points of view and interests of those describing it. For instance, "the gray hat" and "the hat on the banister" are descriptions that can be used to refer to the same hat. The consequences of emphasizing one aspect of the hat rather than another include the possibility of its receiving under one description an evidential assessment different from that received in a context in which some other aspect is emphasized. Suppose two men walk into a house and, upon seeing a gray hat on the banister, exclaim "Nick is here." It seems obvious that both have taken the presence of the gray hat on the banister as evidence that Nick is in the house. Suppose each is asked why he came to believe that Nick is in the house. One replies that Nick is the only person he knows with a hat just that shade of gray, the other that Nick always throws his hat on the banister in just that way. Thus it is not simply that both have taken the hat as evidence that Nick is present. More precisely, one has taken the presence of a gray hat, regardless of its location, as evidence that Nick is also present in light of his belief that Nick is the only person who owns a hat just that shade of gray, while the other has taken the hat's being on the banister, regardless of its color, as evidence that Nick is present in light of his

belief that Nick always tosses his hat on the banister in just that way. It is the gray hat on the banister that is evidence of Nick's presence, yet were it to be on the hatrack or cocked at a different angle, though the same shade of gray, it would not be evidence for one, and were it a different shade of gray, though sitting on the banister in just that way, it would not be evidence for the other. Their differing (though not conflicting) beliefs lead each to pick a different aspect of the same state of affairs as evidentially relevant.

This example, too, can be suitably modified to show that different and possibly conflicting hypotheses can be thought to be supported by the same state of affairs. Suppose that one man, as before, believes that Nick is the only person with a hat just that shade of gray, but the other believes that James is the only person who tosses hats on the banister in just that way. The first, paying attention only to the color of the hat, will take it as evidence that Nick is in the house, but the second, paying attention only to the location of the hat, will take it as evidence that James, rather than Nick, is in the house.²

What these examples show is that how one determines evidential relevance, why one takes some state of affairs as evidence for one hypothesis rather than for another, depends on one's other beliefs, which we can call background beliefs or assumptions. Thus, a given state of affairs can be taken as evidence for the same hypothesis in light of differing background beliefs, and it can be taken as evidence for quite different and even conflicting hypotheses given appropriately conflicting background beliefs. Similarly, different aspects of one state of affairs can be taken as evidence for the same hypothesis in light of differing background beliefs, and they can serve as evidence for different and even conflicting hypotheses given appropriately conflicting background beliefs.

The function of background beliefs is analogous to the function of background conditions in causal interactions. In an atmosphere that contains oxygen if one rubs two dry sticks together a flame (or at least a spark sufficient to ignite combustible materials such as straw or paper) results. In most contexts (those in which it is not surprising that there should be oxygen in the atmosphere) we pick out the rubbing of the sticks as the cause of the spark. We acknowledge that if there were no oxygen in the atmosphere, nor any functional equivalent of oxygen,

² It might be claimed that color is more central as evidence than location in this example, as the grayness of the hat is more important to its being the hat than is the location and spatial attitude of the hat at any given moment. The features of the hat are being used as evidence not that the hat is the hat that it is but that James, owner of the hat, is in the house. In this respect color and location are equally central as evidence.

the sticks' friction would not cause the spark, but we do not regard the presence of oxygen as a cause of the spark. The presence of the oxygen is rather an enabling condition of the causal interaction. So it is with evidence and background beliefs: a state of affairs will only be taken to be evidence and background beliefs if the case in light of some background belief or assumption asserting a connection between the two. The background belief is an enabling condition of the reasoning process in much the same way that environmental and other conditions enable the occurrence of causal interactions. The two processes are, however, disanalogous in that, while the end result of a causal interaction is an event or occurrence distinct from its cause, in the case of reasoning from evidence the end result need not be belief in, or inference to, the hypothesis detached from beliefs concerning the state or states of affairs taken to be evidence for it. The end result is often the more modest but complex belief that the hypothesis in question is probable or plausible in light of that state or states of affairs.

Background beliefs are what some philosophers would call principles of inference, but they are not principles in the sense that they can be abstracted from the sequence of reason for believing and belief. The examples above show that background beliefs can vary even though the pair "reason for believing" and "belief" or the pair "evidence" and "hypothesis" is the same. Rather than principles abstracted from a sequence they are beliefs in light of which one takes some x to be evidence for some h and to which one would appeal in defending the claim that x is evidence for h . Background beliefs function as they do because they are beliefs or assumptions about connections (actual or presumed, correlational or causal) between particular kinds of states of affairs and other kinds (of states of affairs). A given evidential relation may be determined by just one background belief or by a set of assumptions of varying degrees of generality and complexity, but in the absence of any such beliefs no state of affairs will be taken as evidence of any other.³

³ The beliefs or assumptions mentioned so far have been about relations between particular kinds of things, events, or states. Not all beliefs that function as background beliefs in inferring are of this apparently synthetic or empirical nature. There are some, such as the beliefs expressed by such formulae as "if all observed a 's have been F , then (it is likely that) all a 's are F " and "if a is analogous to b in that it possesses properties F , G , and H , which are possessed by b , then (it is likely that) a also possesses the property I which is possessed by b ," that seem much more abstract and schematic. The impetus for talk of principles of inference may have come from considering these beliefs, which are like principles of nondeductive reasoning. Because these principles function as a more empirical background beliefs do in inferring—that is, they allow one to identify a certain state of affairs as evidence that something else is the case—it seems better to drop

Consideration of the examples also shows that there is no unique or intrinsic relation between states of affairs in virtue of which they possess evidential status. The connections or regularities that we believe to hold between states of affairs and in light of which evidential relations are assessed are connections or regularities from some point of view, which is always susceptible to change. As will be argued more fully below, it follows from the fact that taking some state of affairs, x , to be evidence for some h is a function of background assumptions and that x 's actually being evidence for h is similarly a function of background assumptions. Evidential support of hypotheses is thus a relative matter: while in the context of one set of beliefs or assumptions x will be evidence for h , in the context of a different set x will be evidence not for h but for some hypothesis, h' , or for no hypothesis at all.

The dependence of evidential assessments on background assumptions might be conceded for nonspecialized contexts but denied for scientific contexts. This is in fact a common way of distinguishing science from nonscience. It can be argued that precisely what distinguishes scientific reasoning from ordinary, everyday reasoning is that in scientific inquiry inferences relying on hidden background assumptions are disallowed. Whereas one is perhaps sloppy about evidence in everyday contexts, evidential relations in science are clear, fixed, and absolute, independent of further assumptions. Recent work in the history of science, however, has persuaded many that such an attitude is unjustifiable.

As an initial illustration consider what might be inferred from the (supposed) fact that day and night alternate at a steady rate. (I oversimplify the actual situation for illustrative purposes.) This fact considered by itself suggests nothing. In the light of a heliostatic cosmology however, it can be taken as evidence that the earth is spinning round its axis at a steady rate. In the light of a geostatic cosmology, the evidential relation is quite different: the steady alternation of day and night would be evidence that the sun is moving round the earth at a steady rate. In both cases it must also be assumed that exposure to the sun and daylight are causally related. Considering the hypotheses in question independently of the theories with which we might associate them, we find no immediate relation to states of affairs that could count as evidence for them: "the earth is spinning round its axis at a steady rate" implies no statements that could describe evidence for the claim independently of something like the heliostatic theory. Only in the appellation *principle* (at least in this context). We are then left simply with a distinction between more and less abstract background beliefs.

light of the latter is the steady alternation of day and night evidence for the hypothesis. The point is that it's not a matter of not thinking of the appropriate evidence for the hypothesis but a matter of having any reason at all to think that there is a relation between the earth's spinning on its axis at a steady rate and the steady alternation of day and night. The evidential relevance of the latter (supposed) fact depends on the background assumptions operative in any given context.

This example shows both the dependence of evidential status on background beliefs and the resulting possibility that the same state of affairs can be taken as evidence for conflicting hypotheses. To the extent that no one is said to have actually made the inferences involved, it may be said to be fictitious. However, examples of the use of a given state of affairs as evidence for conflicting hypotheses abound in the past (and the present) history of scientific inquiry, particularly at those junctures of major theory change, when experiments and discoveries are subject to multiple interpretations. Two well-known instances are the Michelson-Morley interferometer experiments in the late nineteenth century and Priestley's experiments with mercury oxide in the late eighteenth century. A schematic discussion of these will have to suffice to show how the uses to which these experimental results were put exemplify the account of evidential relations defended here.

The Michelson-Morley experiments were carried out to measure the relative motion of the earth in the ether, this to be determined by the difference in the speed of light beams sent out at 90° angles to each other, this in turn to be determined by shifts in the interference fringe system displayed by the interferometer. The Michelson-Morley apparatus showed no experimentally significant displacement. Perhaps because this null result was influential in the eventual acceptance by the scientific community of Einstein's special theory of relativity, it is often regarded as the experimental proof of the theory. However, given the assumptions of nineteenth-century classical physics, it could equally well be, and was by some, viewed as decisive evidence for the Lorentz-Fitzgerald contraction hypothesis, the hypothesis that bodies moving at speeds close to the speed of light contract in length along the direction of their motion.

According to Einstein, once the notion of absolute space is abandoned, a "luminiferous ether" is superfluous. Thus, that no effect of the ether on the motion of light could be detected by means of the interferometer showed that there was no ether to affect it. In the context of the assumptions of relativity theory, then, the experimental results are taken as evidence that there is no ether. In the context of the classical physics, in which the ether played so crucial a role, it simply

was not possible to take the experiment in that way. It was troubling, it indicated that the theory needed alteration, but it was not, in a logical, as opposed to psychological, sense, a crucial experiment between the classical and relativistic theories. That is, it could not be assessed independently of the governing assumptions of one or the other of the two points of view, and hence its evidential relevance was determined by these assumptions.⁴

The "discovery" of oxygen by Joseph Priestley and Antoine Lavoisier provides an illustration of the role of background beliefs in highlighting different aspects of a given state of affairs in such a way as to have it support conflicting hypotheses. Priestley formed a mercury calx (now, oxide of mercury) by heating the metal in atmospheric air. He then found that by concentrating sunlight on the calx in a test tube it would become mercury again (decalcinat). In addition the decomposing calx released air in which combustible objects burned more brightly and quickly than in atmospheric air, in which mice were more frisky, and which gave Priestley a slight "high" when he inhaled it.

Lavoisier performed the same manipulation of mercury following instructions provided by Priestley in his description of the experiment and got similar results. The two thus had the same experimental information but approached it with different background beliefs. Priestley was still working with the phlogiston theory, while Lavoisier had rejected it and the qualitative theory of elements of which it was a part. Priestley's argument that the gas was "dephlogisticated air" rested primarily on the livelier combustion occurring in it, which was taken as evidence that the air had been depleted of its phlogiston and hence absorbed it more quickly from burning objects. Lavoisier, however, believing that a specific proportion of the atmospheric air combined with metals in calcination, took the fact that Priestley's air was released in the course of decalcination, together with the fact that it had properties that distinguished it from ordinary air, as evidence that a wholly new gas had been isolated, one that must combine with others to constitute atmospheric air. After further experimentation revealed its acidifying properties, he came to call it "oxygen gas." In the context

⁴ This discussion must be schematic as there is considerable disagreement concerning the role and necessity of background beliefs in the development and acceptance of the special theory of relativity, as well as disagreement as to just which assumptions must be made. Compare Zahar (1973) and the ensuing discussions by Feyerabend (1974), Miller (1974), and Schaffner (1974), as well as Alan Musgrave (1974). The account presented here is intended as a framework within which such disputes can take place.

For defenses of the contraction hypothesis from the charge of being ad hoc see Holton (1973), pp. 261-352, and Grünbaum (1963), especially pp. 386-397.

of their differing background beliefs and assumptions different aspects of the same state of affairs became evidentially significant.⁵

EVIDENTIAL REASONING AND THEORY ACCEPTANCE

I have discussed the dependence of evidential relations on background beliefs or assumptions and shown that given appropriately differing background beliefs the same state of affairs can be taken as evidence for different and conflicting hypotheses. The cases of the Michelson-Morley experiment and of the discovery of oxygen show that this dependence holds even for scientific experiments and the hypotheses they are alleged to support, that is, they show that experimental results can be taken as evidence for hypotheses only in the context of some set of background beliefs. Reflection on these cases and their outcomes raises a number of related questions. Can reasoning and inference be shielded from influence by social and individual values, interests, and subjective preferences? Upon what is the acceptance of the background beliefs operative in the contexts discussed based? What sorts of criteria are relevant to deciding between different or competing (sets of) background beliefs? It is clear that if the criterion is evidential support, there must be some further set of background beliefs in light of which whatever is taken as evidence is so taken. On the other hand, to require that acceptance of all hypotheses, theories, assumptions, or beliefs be evidentially based is to fall into an infinite regress. I shall approach these questions by first contrasting the analysis of evidence and inference developed here with the accounts discussed in the previous chapter and more recent variations on them.

The Positivist Tradition

The relation described by Hempel is a formal one. As detailed in Chapter Two, evidence, on that view, is described in observation reports whose relevance to the hypotheses they confirm is secured by their entailing instances of these hypotheses. What would count as evidence for a hypothesis is determined by the form of hypothesis sentences and evidence sentences, not by their content. This means that inference to a hypothesis is not mediated by possibly value-laden assumptions. The only background assumption that might be at work is the principle of enumerative induction and then only in those cases where a universally quantified hypothesis is the object of confirmation. The problem of criteria of acceptance of background assumptions reduces to the justifi-

⁵ For more details see Lavoisier (1952) and Conant (1957).

fication of deductive and inductive inference. It becomes a philosophical and not an empirical or scientific problem.

While this might seem an ideal answer to the problems posed by background assumptions, it will not do as an analysis of evidential relations in the sciences. As argued in the previous chapter, the sentences describing evidence for hypotheses and those expressing hypotheses are typically not related in the appropriate ways. It is true that some laws, for example Boyle's law relating the pressure, volume, and temperature of a gas and Galileo's law of free fall, are of this nature. Thus, some evidential reasoning can be accounted for as inductive generalization or as reliance on empirical generalizations. Boyle's law is a generalization of a finite set of observed correlations between measured properties of gasses. It is supported by those correlations in light of the principle of induction. We can use the observation of changes in one of these parameters as evidence for claims about changes in another in light of Boyle's law. A description of the evidential relation confined in its scope to empirical generalizations is, however, of limited utility.

One solution would simply be to restrict the hypotheses and theories proposable in scientific contexts to propositions expressing only relationships among observables. This was the course taken by Blaise Pascal in his work on aero- and hydrostatics.⁶ It is also one of the reasons his name does not appear in short lists of the seventeenth-century founders of modern science. Pascal urged and followed a rigorously empiricist research program, which effectively prevented him from distinguishing atmospheric weight from atmospheric pressure.

A slightly more permissive solution is to try to accommodate the use of nonobservables by scientists. The logical positivists' criterion of significance was one attempt to do this: terms purportedly referring to nonobservable, theoretical entities are admissible in a theory only as long as it is possible to eliminate such terms in a "rational reconstruction" of the theory. There is an extensive literature documenting the failure of this proposal in its many forms.⁷

In one of the most ambitious recent contributions to the literature of scientific confirmation Clark Glymour has proposed what he calls the "bootstrap" strategy to overcome the problem of determining the evidential relevance of observation reports to hypotheses containing

⁶ See "Preface to the Treatise on the Vacuum," "New Experiments Concerning the Vacuum," and Pascal's letters to Pere Noel and Le Pailleur in Pascal (1952). See also Pascal (1937), pp. 2-66, 91-112.

⁷ For example, Maxwell (1962); Putnam (1975); Scriven (1958).

terms other than those in the reports.⁸ The central idea of the bootstrap strategy is this: a hypothesis *h* is confirmed by using other hypotheses (from the theory *T* of which *h* is a part) to deduce instances of *h* from data obtained by established observational and experimental procedures. The idea is worked out in its greatest detail for those cases where an instance of *h* is a sentence assigning a particular value to some theoretical quantity, like pressure or temperature, but is generalizable to nonmathematical reasoning as well. A hypothesis *h* is said to be directly confirmed by evidence *e* relative to *T* if instances of *h* are deduced from *e* and a set of auxiliary hypotheses of *T*. An auxiliary hypothesis of *T* is essential to the direct confirmation of *h* by *e* if indirectly confirmed relative to *T*.

Detailed discussions of possible and actual argumentation in four episodes in the history of science illustrate the deployment of the bootstrap strategy in scientific reasoning. Bootstrap confirmation is, in at least some cases, a better representation of actual reasoning. Glymour also argues that it is better able to give reason and coherence to many nostrums common among scientists, such as the preference for a variety of evidence, than some previous accounts. Although intended as a modification of the Hempelian model of confirmation that does not collapse into Kuhnian wholism, bootstrap confirmation does not solve the problems posed by the introduction of background assumptions (a.k.a. auxiliary hypotheses) as mediators of evidential relations.

For application of bootstrapping in its strict form a theory must be axiomatized or axiomatizable. That is, it must be developed at least well enough that auxiliary hypotheses are available for the bootstrapping of other hypotheses.⁹ The influence of values or subjective preferences could be ruled out on the simple inductive model because confirmation depended only on independently established observation reports. In the bootstrap model confirmation of a hypothesis *h* is relative to the auxiliary hypotheses used in deducing instances of *h* from observation reports. Thus, ruling out the influence of values or subjective preferences on the model depends on the degree to which the auxiliary hypotheses are themselves free of or confirmed in a manner that rules out such influence.

Regarding the confirmation of an auxiliary hypothesis, there are several possibilities. (1) All the hypotheses of a theory *T* may be confirmable in the sense that each is confirmed or confirmable relative to some

⁸ Glymour (1980).

⁹ As I understand it, auxiliary status is relative to particular tests in which a hypothesis is used for bootstrapping and is thus not an intrinsic or permanent character of some hypotheses as opposed to others.

subset of the hypotheses of *T*. Thus, the auxiliary hypotheses are confirmed relative to some subset of *T*. (2) All the hypotheses of *T* may be confirmable in the sense that those auxiliary hypotheses not themselves confirmed or confirmable relative to *T* are established independently of *T*, for example by being part of and confirmed relative to some other theory *T'*. (3) Some of the auxiliary hypotheses of *T* are neither confirmed relative to *T* nor established independently of *T*.

The third possibility, of course, offers no guarantee against influence by subjective preference. The first possibility can be subdivided into two subcases: (a) all the hypotheses are directly confirmed relative to *T* and (b) all the hypotheses of *T* are either directly or indirectly confirmed relative to *T*. Subcase (a) generates a strong criterion and subcase (b) a weak criterion for the admissibility of hypotheses. While the strong criterion would make contextual motivations for the adoption of some hypothesis irrelevant, it represents a state that is rarely, if ever, realized in practice. When a theory is being developed, the criterion for the inclusion of specific hypotheses or principles is not that they are directly confirmed relative to (other hypotheses of) the theory but that they are relevant to the explanation of the phenomena comprehended by the theory. In the terms of the current discussion we could say that we admit hypotheses if they are relevant to the confirmation of other hypotheses of the theory without requiring that they themselves be directly confirmed relative to the theory. To require direct confirmation of all hypotheses places constraints on the practice of science comparably unreasonable to those imposed by the older account of confirmation. In practice, then, the first possibility regarding the confirmation of auxiliary hypotheses must be interpreted as its second subcase, the weak criterion: some hypotheses are directly confirmed relative to the theory and others are not. The latter are indirectly confirmed. Since a value- or interest-laden hypothesis could be essential to the direct confirmation of another hypothesis, the weak criterion is not sufficient to block contextual values, interests, and subjective preferences from the assessment of evidential relevance and hence from hypothesis acceptance.¹⁰

Thus, a scientific methodology that guaranteed noninterference by evaluative concerns would have to restrict admissible theories to those satisfying the second set of possibilities. Is the rule or set of rules contained in this set adequate for the sciences we have? One major prob-

¹⁰ I take this to be an implication of Glymour's remark that "virtually any hypothesis in a theory can be worked into a computation of almost any quantity"; Glymour (1980), p. 374.

lem with adopting this as a constraint on our theories is that procedures for independently establishing the requisite auxiliary hypotheses are often not available, if at all, until well after a theory has been developed and adopted. Such a constraint, like the strong version of the first possibility, would eliminate any science more sophisticated than Pascal's.¹¹

Thus, an empiricism that grants independent meaningfulness to nonobservational statements and that acknowledges the logical gap between hypotheses and evidence statements has no a priori grounds for eliminating contextual values from scientific reasoning. The methodological constraints that might have eliminated value-laden auxiliary hypotheses or background assumptions are the very ones shown to be too restrictive for the analysis of evidential relations. Similar considerations can be applied to the possibility of distinguishing evidence *simpliciter* from what is taken as evidence. To insist on this possibility presupposes that we have a way, in the case of differing evidential assessments owing to different background assumptions, of deciding which assumption is correct and which false. This would have to involve appeal to evidence, and what is offered as evidence will in turn be so in light of further background assumptions. Thus the relativity of evidential relations is thorough: if a set of data *e* is taken to be evidence for hypothesis *b* in light of background assumption *b*, then we cannot with any finality determine whether it is correctly so taken by examining the evidential support for *b*, since whatever (data *e'* is taken to be evidence for *b* is so in light of some further assumption *b'*). To maintain that there is a distinction between what is taken to be evidence and what is *really* evidence is to suppose that there is some nonempirical way to discover the truth or falsity of background assumptions. The bootstrap model does draw our attention to the discrete character of the auxiliary hypotheses or assumptions required to secure the evidential relevance of particular bits of data to particular hypotheses. Thus, even though observational and experimental data relevant to the direct confirmation of auxiliary hypotheses may not be available (either in fact or in principle), we are not precluded from inquiring as to what kinds of reasons *can* be offered in their support. That is, we may have reasons to accept a set of background assumptions that are short of reasons demonstrating the truth of those assumptions.

¹¹ This seems to be the substance of Feyerabend's complaints against methodology. See, for example, Feyerabend (1970a).

The Wholist Tradition

In the account of evidential relations emerging from the Wholist view there is no comparable problem about admitting certain types of theories. The denial of a theory-independent observation language and of theory-neutral observation, however, leads, as we saw above, to paradox. Charges of paradoxicality can be dismissed if there is no other way to account for the cases of theory change to be found in the history of science. If what seem to us to be the same states of affairs are taken as evidence for different and sometimes conflicting theories, then it must be that the meaning of terms used to describe those states of affairs have changed and that what is seen is different. The situation that prompts the paradoxical analysis, however, is not at all surprising in light of the analysis of evidential relations developed in this paper. If some state of affairs is evidence for a hypothesis only in light of some further background belief or assumption, then changes in background beliefs will result in changes in evidential status. Thus, it is not necessary to suppose that we must account for all cases of apparently conflicting theories supported by what seems to be the same body of evidence by saying that terms in the two theories have different meanings. Rather we can say that the relevant background beliefs have changed.

Kuhn's own examples are quite amenable to this kind of treatment. He says, for example, that an Aristotelian and a Galilean physicist looking at a swinging stone or pendulum see different things: the Aristotelian sees a body falling with difficulty, a case of constrained fall, while the Galilean sees oscillatory motion, a pendulum. The visual experience of each is incommensurable with that of the other to such a degree that if they were to begin talking about the stone they would be talking about different things. Clearly there is a profound difference between what the Galilean and the Aristotelian want to say about the swinging stone. Alas, Kuhn's attempt to explain and describe that difference leaves us finally unable to say that there is a difference since there is no common referent of their potential remarks.

Using the notion of background beliefs, however, a more satisfactory analysis of the situation is possible. The Aristotelian believes that the natural motion of all items (elements) in the sublunary sphere is in a straight line to their natural place. This belief about motion determines what features of the swinging stone are going to be important, and clearly what is important in the situation is that the stone (whose natural place is at the center of the earth) eventually comes to rest at a position which is as close to the center as it can get (given that it is constrained by the string or chain) and hence that the stone, swinging,

is in a state of unnatural motion until it comes to rest at its final position.

The Galilean, in accounting for this same phenomenon, is operating with the impetus theory of motion that already had a theory of the oscillatory motion of vibratory strings. In the context of this theory, features other than those that strike the Aristotelian become important, in particular the repetitive and oscillatory character of the stone's motion. In this respect the motion of the stone is analogous to that of the vibrating string and is given an analogous explanation.

It is not, therefore, necessary to say that the Aristotelian and the Galilean are seeing different things. Rather we can say that they are seeing the same thing but attending to different aspects of it. It is true that the aspects singled out become the focus of explanation and can be used as evidence for the differing hypotheses about the motion of the swinging stone, but there is no need to suppose that the Galilean or the Aristotelian must fail to see aspects that interest the other, nor to suppose that there is no description of the situation that both could accept and that would then form the basis for discussion of differences.¹² To use the notion of background beliefs as opposed to gestalt-type notions for analysis of this example and similar ones shows also that it is not always the case, in theory change, that exactly the same body of evidence supports conflicting theories. In the pendulum case different features constitute evidence for different hypotheses, so they are not strictly speaking supported by the same evidence, even though the different features are features of what is identifiable as the same state of affairs.

Kuhn analyzes the discovery of oxygen by both Priestley and Lavoisier in similar fashion. Priestley and Lavoisier, when looking at the new substance, saw different things: Priestley saw dephlogisticated air and Lavoisier saw oxygen. But though he later repeated the experiment on his own, Lavoisier initially constructed his different account on the basis of Priestley's own "theory-laden" description of the experiment. Priestley's description, then, cannot have been as "theory-laden" as one would have to suppose on Kuhn's account, that is, he must have described his experiments in such a way that they could be viewed as having different evidential relevance to one with different background beliefs and information. Thus, in addition to eluding the philosophical difficulties attendant upon Kuhn's analysis the account of this case offered earlier in the chapter seems to accord better with the actual facts.

¹² I am relying in the above on Kuhn's own reading of the impetus theory's account of vibration. See Kuhn (1970a), p. 120.

Mary Hesse, in the essay "Theory and Observation," has recast one of Feyerabend's examples to make a parallel point about meaning.¹³ Hesse rejects the idea that there is or could be a stable and theory-neutral observation language. Her argument depends neither on historical example nor on theories of perception but on remarks about learning the meaning and use of (and assigning these to) referring terms and predicates. Referring terms, she argues, are used to denote members of classes of similar things. The relation of similarity is not, however, transitive; therefore the application of such a term to a class of objects, in particular the specification of the class to which the term is applied, eventually and inevitably involves some loss of information. As the stock of referring terms and the generalizations into which they enter grows, conflicts arise among the generalizations. These conflicts are resolved by reclassifying objects, that is, changing the meanings of terms in order to preserve consistency among generalizations or to preserve the applicability of a generalization. Thus, Hesse concludes, as does W.V.O. Quine on the basis of similar considerations, that no predicates are immune to revision, no predicates can be part of a stable and theory-neutral observation language.

Hesse goes on to argue that the theory ladenness of observation terms does not, however, support theses of radical theory incommensurability. In any given case two proponents of different theories will find areas of intersection between their theories, aspects of experience in which they are inclined to use terms in the same way. Their theories overlap here, and if they are talking about phenomena both within and outside of their area of common experience, it's quite possible that they will make predictions that coincide in the common area but diverge in the area beyond. Feyerabend's example is "fall." While there is a range of experiences about which an Aristotelian and a follower of Anaximenes will agree in their use of "fall," there is also a range of possible experiences (which neither in fact had) about which they would fail to agree. As long as their discourse remains within the boundaries of their common experience, however, they cannot know that they are using terms in different ways. On the other hand, the overlap is the basis that enables them to communicate, to know, for instance, when each is signifying negation and when agreement. Thus, Hesse argues, it enables them ultimately to discover that regarding some area outside the overlap they do disagree and to reconstruct from their common language whether their disagreement lies in the theories they hold, in the meanings they have assigned to terms occurring in

¹³ Hesse (1980), pp. 63-110, especially pp. 95-99.

those theories, or both. As was established above, however, theory ladenness does not account for all cases of conflicting evidential assessment. I would add to this list, therefore, the background assumptions in light of which they assess the evidential relevance of their common experience.

Several useful lessons can be gleaned from Hesse's discussion. One emerges from her particular way of arguing against the existence of a theory-neutral observation language. Suppose some term o occurs in the description of data used as evidence for some hypothesis h belonging to theory T . The theory with which o is laden, and with respect to which the use of o is not neutral, may not be T but some other theory. It does not follow from the theory ladenness of meanings, therefore, that observations cannot be used as independent tests of theories but only that they cannot be so used in connection with theories presupposed by the descriptions of those observations. Secondly, her discussion is a good reminder that even when proponents of incompatible theories agree on the description of what they are observing, it doesn't follow that we can label their description as theory-neutral. Their theories may in that one instance coincide with each other but could well conflict with a third when it is brought to bear on the same phenomena. The thesis of theory ladenness, as Hesse reworks it, therefore, does not imply that incompatible theories are incommensurable or that incompatible hypotheses belong to incommensurable theories. A theory-laden description of a set of observations is not the only description that can be given of that set, and the theory with which the description is laden may not be the theory in whose support that set of observations is cited.

Summary

The model of evidential relations that I have defended has points of resemblance with both the positivist and the wholist accounts. Like the positivist account, it presupposes that data can be specified independently of the hypotheses and theories for which the data have evidential relevance. This is not to deny that some data (or evidence) may only be conceived or sought in the context of investigating some hypothesis, but it is to insist that the description of data cannot presuppose the truth of the hypothesis for which they serve as evidence. The model also resembles Kuhn's in that some third, distinct, element is appealed to as providing the context in which the assessment of the evidential relevance of experiments and observed (or alleged) states of affairs takes place. The character of this further element differs. In this analysis, while background beliefs determine what states of affairs

count as evidence for a hypothesis, all three of these elements—state of affairs, hypothesis, background beliefs—are independently specifiable. For Kuhn, however, the further element, the paradigm, so determines the context of assessment that one's perception of the world changes with the theories one adopts in such a way that one sees it as confirming the theory. This creates a bond between evidence and hypothesis impossible to break and even destroys, ultimately, the concept of evidence as something to which one can appeal in defending a hypothesis. The limitations on incommensurability on which Hesse insists restore meaningfulness to the concept of evidence, although evidential relations must be understood relative to some context of assumptions.

EVIDENTIAL REASONING AND RATIONALITY

Kuhn's and Feyerabend's examination of historical cases led them to say that theory choice is not determined by evidence, since there is no theory-independent way to describe the evidence, that is, there is no set of neutral and stable evidence statements to which one can appeal in deciding between two conflicting theories. To the charge that on this account science is irrational Kuhn responds by claiming that insofar as theory choice is determined by values, in particular such internal values as simplicity or greater problem-solving ability, it is not irrational. Feyerabend, on the other hand, has embraced and encouraged irrationality: Galileo was irrational to accept Copernicanism because it was less strongly supported by the available evidence than the Ptolemaic theory, but Galileo was right: flying in the face of evidence made him a better, more creative scientist, therefore it's better to be irrational. To adhere to theories just because they are evidentially supported hinders scientific progress.

Whatever one may think of these strictures, it seems clear that what leads both Feyerabend and Kuhn to their respective assessments of rationality in science is not just historical cases but an implicit acceptance of the formal, positivist (Hempelian) conception of evidence.¹⁴ Because the relationship of evidence to hypothesis, in the Kuhn and Feyerabend accounts, is not direct and unequivocal, because neither states of affairs nor hypotheses stand in unique relationships with each other, hypothesis acceptance is thought not to be based on evidence and hence to be, to that extent, irrational. For both, the positivist analysis is a

¹⁴ One could respond to Feyerabend, for instance, that Galileo did not fly in the face of evidence but that he chose (in light of other theoretical commitments) which data to take seriously and which to ignore.

description of an ideally rational procedure to which science, for better or for worse, fails to measure up. Many of their followers in the sociology of science seem to have absorbed the same attitude.¹⁵

So to regard the formal positivist analysis is to assume that a procedure such as it describes is possible, but just not the way things are, for one reason or another. And in fact the arguments against positivist forms of empiricism presented by both Kuhn and Feyerabend rely heavily on historical example and, in Kuhn's case, on a psychological hypothesis about perception. The account of evidential relations defended in this chapter, while it may have implications for psychology and while it suggests a new approach to the analysis of historical cases, rests on considerations of a logical character. Thus, the positivist account is not even an ideal and must be rejected not because the vagaries of human psychology prevent us from realizing it but because it involves a radical misconception of the way in which hypotheses and evidence are related to one another. In that account hypotheses and evidence are abstracted from their dynamic context of inquiry, of observation and reasoning, and treated as elements in a static, unchanging construction, as museum pieces.

In this chapter I have argued that the relation between hypotheses and evidence is determined by background assumptions operative in the context in which data are being assessed. Before going on to the questions of values, subjectivity, and objectivity that such an account clearly provokes, let me summarize the main features and additional advantages of this account.

However we end up characterizing observational data, *they* are what serve as evidence for hypotheses (and theories). Data—even as represented in descriptions of observations and experimental results—do not on their own, however, indicate that for which they can serve as evidence. Hypotheses, on the other hand, are or consist of statements whose content always exceeds that of the statements describing the observational data. There is, thus, a logical gap between data and hypotheses. In some cases, as noted above, they are related as instance or instances of generalization. Even instances, however, are *evidence* for the truth or plausibility of their generalizations only in light of some version of the principle of enumerative induction.¹⁶ As was also

¹⁵ See, for example, the introductions to two collections of essays in the sociology of science: Barnes and Edge, eds. (1982), pp. 1–12; and Knorr-Cetina and Michael Mul-kay, eds. (1983), pp. 1–17.

¹⁶ The empiricist account of confirmation can, then, be seen as a limiting case describing the relation of hypotheses *h* and evidence *e* when *e* is assigned evidential relevance to *h* by the principle of simple induction. The abstractness of the principle of induction

noted above, in the interesting cases of scientific reasoning, for example, that concerning the characterization of and relations among subatomic particles, hypotheses contain (as essential components) expressions not occurring in the description of the observations and experimental results serving as evidence for them. But causal hypotheses, too, contain expressions (“cause,” “influence”) that do not occur in the statements of correlation that serve for evidence for them. In these cases reliance upon background beliefs or assumptions that assert relations (of causality or other forms of interrelation) between these different classes of phenomena is much more apparent than it is in cases of (relatively) simple inductive or probabilistic reasoning.

Background beliefs or assumptions, then, are expressed in statements that are required in order to demonstrate the evidential import of a set of data to a hypothesis. As such, they both facilitate and constrain reasoning from one category of phenomena to another. Although I shall use these terms interchangeably, it is appropriate to speak of beliefs when these statements are more or less explicitly adopted as tenets and of assumptions when their necessity to a bit of evidential reasoning is not explicitly acknowledged. Relativizing evidential import to background assumptions thus involves abandoning the attempt to specify the relation between evidence and hypotheses by means of syntactic criteria and seeing this relation as involving substantive assumptions instead. Evidential relations are not autonomous or eternal truths but are necessarily constituted in the context in which evidence is assessed. I've considered above some cases in which different background assumptions facilitated varying interpretations of the same experimental data. In later chapters devoted more specifically to the discussion of particular scientific research projects I shall look more closely at the ways in which both the immediate context of research and its social environment interact in the interpretation and assessment of data.

If rationality is, at least in part, the acceptance or rejection of beliefs on the basis of evidence, then theory and hypothesis choice is, when based on evidence, rational. Rationality, however, is not the infallible road to truth or away from error that it is often claimed to be. Both the Aristotelian and the Galilean are being rational when they defend their respective accounts of the swinging stone. What explains why it serves as evidence for different hypotheses is not that the two see it

(cf. n3), it should be noted, does not make it a formal or syntactic principle. As has been noted since Hume, it involves a substantive claim about the persistence of regularities in the natural world. And a Popperian falsificationist would urge that we cease to rely on it.

differently and in ways determined by those hypotheses in question but that they hold different background assumptions in light of which its evidential relevance is differently assessed. Once it is accepted that the evidential relation is always determined by background assumptions, then it is easy to see that there could be a neutral description of a given state of affairs, that is, one agreed to by both parties to a dispute, and no agreement on the hypotheses for which it is taken as evidence. It is also easy to see that both parties are being perfectly rational. It is rational to take some state of affairs as evidence for a hypothesis in light of background assumptions one accepts. It would be irrational to assess evidential relations in a manner inconsistent with such background assumptions and antirational or nonrational to accept or reject hypotheses with no regard for evidence.

CONCLUSION

This approach to evidence solves a number of problems. Substantive background assumptions can bridge the gap between hypotheses and evidence that the formal ties of the positivist analysis cannot. Thus, the fact that conflicting hypotheses and theories have seemed to be supported by the same state of affairs can be explained by appealing to the different background beliefs in light of which the state is assigned evidential relevance to one or another hypothesis. The appeal to incommensurability, with its attendant logical difficulties, becomes unnecessary. Background assumptions may not always be explicit, but they are articulable.

Besides offering a solution to these logical problems the analysis has two additional strengths. In focussing discussion on the relation between evidence and hypotheses rather than on that between evidence and theories it can be used to examine the structure of reasoning in current and ongoing research projects that may not yet have developed a system of explanation comprehensive enough to be called a theory. A great deal of contemporary biological research is of this nature and is still structured by background assumptions that mediate inferences between data and hypotheses. Thus, the contextual analysis can be used to compare inference and argument in the formation of theories as well as in the defense of finished theories.

Its second strength consists in its ability to accommodate the interests of the different communities that have a stake in any given scientific research program. In particular, it is sensitive to the demands that nonspecialists who look to scientific inquiry for an account of the natural world (ought to) pose to any seriously proposed hypothesis: What

is the evidence? And why is this data evidence for this hypothesis? In other words, Why should I believe (or accept) this?¹⁷ However incommensurability is understood, it implies the meaninglessness of this question asked by one who is not part of the community advancing a particular theory or hypothesis. The formal requirements of data and a rationale for assigning to the data evidential relevance to the hypothesis proposed at least provides a framework within which externally (and internally) generated questions of belief and justification make sense.

While this approach solves some problems, it generates others. In particular, by relativizing what counts as evidence to background beliefs or assumptions, hypothesis acceptance on the basis of evidence is also thus relativized. This invites the question: How can science so described come close to meeting the ideal of objectivity? The intrusion of subjective preference into evidential relations can, it seems, only be blocked by some absolute, nonrelative means of determining which hypotheses are supported and which not. If background beliefs mediate the relation between hypotheses and their evidence, then if any states of affairs are evidentially relevant to them, that is, to the background beliefs, this relevance can itself only be ascertained in light of further background beliefs. In the light of this argument the promise of external or theory-independent standards of evaluation seems to vanish. In the absence of that clear and distinct perception of the truth of assumptions and fundamental propositions posited by Descartes and other rationalists, the choice of background assumptions is as relative as the determination of evidential relations. Without some absolute and nonarbitrary means of determining acceptable or correct background assumptions there seems no way to block the influence of subjective preference. The next chapter, therefore, addresses the problem of objectivity posed by a contextualist analysis of evidence.

¹⁷ For a defense of antispecialization see Maxwell (1980).