EVIDENCE AND HYPOTHESIS: AN ANALYSIS OF EVIDENTIAL RELATIONS*

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The subject of this essay is the dependence of evidential relations on background beliefs and assumptions. In Part I, two ways in which the relation between evidence and hypothesis is dependent on such assumptions are discussed and it is shown how in the context of appropriately differing background beliefs what is identifiable as the same state of affairs can be taken as evidence for conflicting hypotheses. The dependence of evidential relations on background beliefs is illustrated by discussions of the Michelson-Morley experiment and the discovery of oxygen. In Part II, Hempel's analysis of confirmation and the contrasting model of theory acceptance provided by philosophers such as Kuhn and Feyerabend are discussed. It is argued that both are inadequate (on different grounds) and the problems addressed by each are shown to be more satisfactorily approached by means of the analysis developed in Part I. In Part III, it is argued that if there are objective criteria for deciding between competing theories, these cannot be simply that one theory has greater evidential support than another. Finally, some further methodological questions arising from the analysis are mentioned.

Presumably we make inferences, accept or reject hypotheses, or assess their relative acceptability, on the basis of evidence. Perhaps not all of our beliefs have evidential support, but at least those adopted in rational, cool, moments either have evidential support or are thought by us to have evidential support (else, these being cool moments, we wouldn't have adopted them). This seems simple and desirable 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.

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

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(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;
(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;
(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.

A goodly amount of philosophical discussion has been directed toward the analysis of relations in the first category and will receive attention below. The second and third categories include, however, the kind of relation being referred to in non-philosophical discussion of evidence. Scientists, lawyers, persons engaged in reasoning, are concerned about objects, events and states of affairs and what inferences can be made 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 essay follows for the most part the nonphilosophers' usage and is concerned with evidential relations as relations between an object, event, or state of affairs said to be evidence and that (which for convenience I shall call the hypothesis) for which it is said to be evidence. It is worth noting that the hypothesis, 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.

The concern with evidence in this paper 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 hope in proceeding thus is that normative questions, questions concerning what criteria should govern our assessment of evidence, can be both more clearly asked and better answered in the context of a non-normative analysis of the character of evidential relations. To talk about something being evidence independently of its being so taken by some person is to introduce an implicit assessment of its value. 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 two standard approaches to evidential relations—the logical empiricist approach of Hempel and the more subjective approach associated with Thomas Kuhn—in light of the analysis developed here. I shall close with some brief remarks on the consequences of the analysis for such notions as scientific objectivity and rationality and with some suggestions as to directions for further methodological inquiries. While my concern is an understanding of the role of evidence in scientific inquiry, I begin by discussing evidential relations in non-specialized contexts.

I

States of affairs 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 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 any natural (e.g. 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*. That is, it is not relations of the second type mentioned above, but beliefs about relations of the second type which determine whether one takes some state of affairs to be evidence for some hypothesis, *h*. 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 my daughter 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 my daughter 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 gypsy fortune teller told me that if my daughter's stomach appeared covered with spots on a given day she would have a disease called measles. We can imagine the fortune teller 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 connection between red spots and measles. In both cases, what is taken as evidence is the same: red spots on my daughter'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, believing that gypsies are a reliable source of information, I believe what the gypsy told me. The alleged regularities in light of which my daughter's red spots are 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 gypsy's predictions and what eventually happens.

The same state of affairs can also be taken as evidence for different hypotheses. Consider again my unfortunate daughter'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 red spots 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 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—an 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. "The grey hat" and "the hat on the bannister" are descriptions that can be used to refer to the same hat, though the consequences of emphasizing one aspect of the hat rather than another include the possibility of its receiving 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 grey hat on the bannister, exclaim "Jones is here." It seems obvious that both have taken the presence of the grey hat on the bannister as evidence that Jones is in the house. Suppose each is asked why he came to believe that Jones is in the house. One replies that Jones is the only person he knows with a hat just that shade of grey, the other that Jones always throws his hat on the bannister in just that way. Thus it is not simply that both have taken the hat as evidence that Jones is present. More precisely, one has taken the presence of a grey hat, regardless of its location, as evidence that Jones is also present in light of his belief that Jones is the only person who owns a hat just that shade of grey, while the other has taken the hat's being on the bannister, regardless of its color, as evidence that Jones is present, in light of his belief that Jones always tosses his hat on the bannister in just that way. It is the grey hat on the bannister which is evidence of Jones's presence, yet were it to be on the hat rack or cocked at a different angle, though the same shade of grey, it would not be evidence of Jones's presence for one, and were it a different shade of grey, though sitting on the bannister in just that way, it would not be evidence of Jones's presence 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 Jones is the only person with a hat just that shade of grey, but the other believes that Smith is the only person who tosses hats on the bannister in just that way. The first, paying attention only to the color of the hat, will take it as evidence that Jones is in the house, but the second, paying attention only to the location of the hat, will take it as evidence that Smith, rather than Jones, is in the house.[1]

[1]It might be claimed that the hat's color is 'better' evidence in this example than its position. If the color of the hat is assigned greater evidential weight than its location,

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 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 which contains oxygen, if one strikes a match a flame results. In most contexts (those in which it is not surprising that there should be oxygen in the atmosphere), we pick out the striking of the match as the cause of the match igniting. We acknowledge that if there were no oxygen in the atmosphere, nor any functional equivalent of oxygen, the striking of the match would not cause the match to ignite, but we do not regard the presence of oxygen as a cause of the match's igniting. 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 that something else is 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 the hypothesis detached from beliefs concerning the state(s) 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 those state(s) of affairs.

Background beliefs are what some philosophers would call principles

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this too, is in light of certain background assumptions, e.g. to the effect that the shade of grey in question is less easily reproducible than the position. I am grateful to the referee of *Philosophy of Science* who, in bringing this to my attention, suggests that not only is identification of state of affairs as evidence a function of background beliefs—the assignment of evidential weight is similarly dependent.

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 that *h*. Background beliefs function as they do because they are beliefs or assumptions about connections (actual or presumed regular associations, causal connections) between particular states of affairs and other kinds. 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.[2]

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 which 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, that *x*'s 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 non-specialized but denied for scientific

[2]The beliefs or assumptions mentioned so far have been about relations between particular kinds of things, events, or states. Not all beliefs which function as background beliefs in inferring are of this apparently 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, which are like principles of non-deductive reasoning. Because these principles function as the more empirical background beliefs do in inferring, i.e. they allow one to identify a certain state of affairs as evidence that something else is the case it seems better to drop the appellation 'principle' (at least in this context). We are then left simply with a distinction between more and less abstract background beliefs.

contexts. This is in fact a common way of distinguishing science from non-science. 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 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 heliocentric 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 geocentric 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 which could count as evidence for them: "the earth is spinning round its axis at a steady rate" implies no statements which could describe evidence for the claim independently of something like the heliocentric theory. Only in the 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 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 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. While this is the most notable, other interpretations of the experiment consistent with the assumptions of classical physics included the proposal that the earth dragged the ether along with it and Ritz' emission theory.

According to Einstein, once the notion of absolute space is abandoned, a 'luminiferous ether' is superfluous. Thus, 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, indicating that something was wrong, but 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 relevence was determined by these.[3]

The 'discovery' of oxygen by Joseph Priestley and Antoine Lavoisier provides an illustration of the role of background beliefs in highlighting

[3]This discussion must be schematic as there is considerable disagreement as to 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 (Zahar 1973, pp. 95–123; Feyerabend 1974, pp. 25–28; Miller 1974, pp. 29–45; Schaffner 1974, pp. 45–78; Musgrave 1974, pp. 1–23). 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 also Grünbaum 1963, pp 386–397.

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. In addition combustible objects burned more brightly and quickly in the air released by the decomposing calx than in the atmospheric air; mice were more frisky, and Priestley received a slight 'high' when he inhaled it.

Priestley and Lavoisier 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 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 which distinguished it from ordinary air, as evidence that a wholly new gas had been isolated, one which must combine with others to constitute air. After further experimentation revealed its acidifying properties, he came to call it 'oxygen gas'. In the context of their differing background beliefs and assumptions, different aspects of the same state of affairs became evidentially significant (Lavoisier 1952; Conant 1957).

II

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, i.e., 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 the questions: 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? Clearly, 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. In this section I approach these issues through a discussion of two major competing models of the relation between hypothesis and evidence in science: the empiricist approach typified by Carl Hempel's work on confirmation and the anti-positivist, historical, approach typified by the work of Thomas Kuhn and P. K. Feyerabend.

In his celebrated essay, "Studies in the Logic of Confirmation (Hempel 1965, pp. 3–51)," Hempel is engaged in a search for "general objective criteria determining whether . . . a hypothesis H may be said to be corroborated by a given body of evidence E" (1965, p. 6). His aim in the essay is to provide definitions of the concepts of confirmation and disconfirmation which characterize those relations in a purely formal way: the criteria of confirmation "should contain no reference to the specific subject matter of the hypothesis" (1965, p. 10). For Hempel, one ought to be able to tell simply by looking at the logical forms of a hypothesis-sentence and an evidence-sentence whether the confirmation relation holds between them, just as one can tell simply by inspecting the logical forms of premise-sentences and conclusion-sentences whether the implication relation holds between them. The search, then, is for formal syntactic criteria of confirmation analogous to the formal criteria for the validity of deductive arguments.

The requirements developed by Hempel are met by his satisfaction criterion of confirmation. The relation of direct confirmation is characterized thus:

> An observation report B directly confirms H if B entails the development of H for the class of objects mentioned in B (Hempel 1965, p. 37).

And the relation of confirmation:

> An observation report B confirms a hypothesis H if H is entailed by a class of sentences each of which is directly confirmed by B (Hempel 1965, p. 37).

We need only concern ourselves with the notion of direct confirmation. As the development of a hypothesis H for some class C is what H would assert if there existed only those objects which are members of C, the development of the hypothesis '$(x)(Ax \rightarrow Bx)$' for a class {a} would be '$Aa \rightarrow Ba$.' All observation reports which entailed '$Aa \rightarrow Ba$', e.g. '$-Aa$', 'Ba', '$Aa \ \& \ Ba$', '$-Aa \ \& \ -Ba$',

'Aa & Ba & Ca' would directly confirm '(x)(Ax → Bx).' As Hempel explains, this definition of confirmation is a satisfaction definition, because it holds an hypothesis H to be confirmed by a report B if H is satisfied by the items mentioned in B.

This definition provides the syntactic, formal, criterion for which Hempel was searching since we need only check to see that certain entailment relations hold in order to determine whether a given sentence confirms another. The question, however, is: can this definition of the confirmation relation be the source of a description of the relation between evidence and hypothesis?

Extending the analysis to evidence and not just sentences describing evidence we find the following characterizations. For any universal conditional hypothesis, e.g., '(x)(Ax → Bx)', any state of affairs which can be described as satisfying neither the antecedent nor the consequent (e.g. '$-A$a & $-B$a') will be evidence for that hypothesis. Thus, a white elephant, being neither grass nor green will be evidence for the universal greenness of grass. This seems somewhat removed from our ordinary conception of evidence—certainly the whiteness of an elephant (in the absence of some very strange background assumptions) would not normally be taken as evidence of the grass's greenness—but, though untoward, it is a consequence Hempel accepts. This, the socalled Raven Paradox, results from just barely acknowledging that things, events, and states of affairs are not subject to uniquely correct descriptions: 'white' and 'not green' are both true of the elephant. Secondly, since the relation described by Hempel is a formal one, there seems no need for the background beliefs and assumptions discussed in earlier sections. What would count as evidence for an hypothesis is determined by the form of hypothesis-sentences and evidence-sentences, not by their content. The only background assumption which 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 situation seems from an epistemological point of view ideal: the justification of hypotheses becomes a very straightforward matter and philosophers have only to solve the problem of induction in order to finish tidying the house of science. Reality, however, has a habit of eluding the ideal; in this instance no less than in others, for actual evidential relations in science are not captured by the analysis of confirmation. To see this, one need only consider arguments Hempel himself has advanced in a different context. In the course of arguing against the inductivist view of the formulation of hypotheses, i.e. the view that hypotheses are formulated, developed, by being inferred inductively from observations, he remarks:

> Take a scientific theory such as the atomic theory of matter. The evidence on which it rests may be described in terms referring to directly observable phenomena, namely to certain macroscopic aspects of the various experimental and observational data which are relevant to the theory. On the other hand, the theory itself contains a large number of abstract, non-observational terms such as 'atom', 'electron', 'nucleus', 'dissociation', 'valence' and others, none of which figures in the description of the observational data (1965, p. 6).

The evidence for statements about atoms, electrons, etc., is not described in statements about, 'observation reports' of, individual atoms, but in statements about cloud chambers, lines observed in spectrographic analysis, etc. The examples considered above exhibit the same features. Evidence that the earth rotates on its axis at a steady rate is described using language not found in the hypothesis in question. The Michelson-Morley experiment is described using terms occurring in none of the hypotheses it was thought to support. Hempel takes this as showing that it is impossible to devise rules that would enable one to infer new hypotheses from observations, as the inductivists hoped. Such rules could be devised, if at all, only if the same predicates occurred in the hypotheses as occurred in the descriptions of observations. As this is patently not the case, such rules cannot be constructed.

The implications of this state of affairs are, however, equally devastating for Hempel's analysis of confirmation, if intended as a description of the evidential relation. The analysis he provides is of a formal, syntactic relation between sentences. This relation holds only between sentences containing the same predicates: the development of a hypothesis for a class contains only those predicates occurring in the hypothesis and as an observation report only confirms a hypothesis if it entails its development for the class of objects mentioned in the observation report, it too must contain at least one of the predicates occurring in the hypothesis.[4] That is, the confirmation relation makes the same impossible demand upon science as does the inductivist conception. Hypotheses forming part of the atomic theory of matter are not evidentially supported by statements about atoms, by statements containing the same terms as occur in the hypothesis, but by statements containing quite different kinds of terms. The same is true for most, if not all, interesting scientific theories.

[4]While an observation report may contain predicates not occurring in the hypothesis, only those occurring in both observation report and hypothesis are relevant to confirmation (Hempel 1965, pp. 37–38).

What makes events observed in cloud chambers, lines observed in spectrographic analysis, evidence for or against hypotheses concerning atoms and electrons is not a syntactic relation holding between the relevant sentences but some (set of) background belief(s) or assumption(s) linking what is macroscopically observable with the microscopic and relatively unobservable. Thus, Hempel was right in claiming that one could not, from scrutiny of observations alone, develop in a rule-governed way hypotheses which would account for or explain the observations, but he was wrong in his implicit claim that one could simply by scrutinizing an hypothesis, once developed, determine in a rule-governed way the observation reports which would confirm the hypothesis.[5]

Both the inductivist program and the Hempelian analysis of confirmation could apply to hypotheses and theories which were merely generalizations of observation reports and while it is true that some laws, e.g. Boyle's law relating the pressure and volume of a gas, are of this nature, a description of the evidential relation confined in its scope to these is of limited utility. Another way of putting this is to say that both programs deal with evidential situations in which one particular background belief or assumption is operative, namely the principle of enumerative induction, but are inapplicable to those contexts in which other background beliefs or assumptions are operative. Putting matters this way is then not to say that the Hempelian approach is totally incorrect, but rather that, to the extent that it works at all, i.e. is defensible against attacks on enumerative arguments of the Hume or Goodman type, it is applicable only to a restricted set of evidential relations.[6]

By contrast, philosophers such as Kuhn and Feyerabend seem to have rejected the logical empiricist approach of Hempel *in toto* (Kuhn 1970a, 1970b; Feyerabend 1962 and 1970). They are struck by the fact that the history of science repeatedly reveals that apparently inconsistent theories seem adequately supported by the data they seem intended to explain. Thus both the Ptolemaic and the Copernican theories, rival cosmologies, and medieval impetus theory and Newtonian physics, rival theories of motion, were attempting to explain and

[5]The use of 'bridge principles' to leap over the gap between experimental/observational and theoretical language, is subject to well-known objections, and in this context seems highly *ad hoc*.

[6]Hempel has, in recent work, retreated from the position discussed here and adopted the Duhemian view that a test or experiment never conclusively confirms or disconfirms or falsifies an hypothesis, but rather is relevant to the hypothesis in conjunction with certain assumptions. These assumptions, as discussed by Hempel and by Duhem, are of a different character than background beliefs as discussed here, but considerations of space prevent an exploration of these differences.

were evidentially supported by more or less the same data. This feature of successive scientific theories leads these philosophers to deny that the cumulative model of scientific growth, (i.e. the model according to which successive theories differ only in accounting for a wider and wider range of phenomena and are consistent with earlier theories accounting for the same data) is an adequate one.

To account for what they see as the non-cumulative character of the development of science, Kuhn and Feyerabend use notions of 'theory dependence'. Both observation and the meanings of terms used to describe observations are theory-determined. Kuhn has drawn an analogy between the adoption of a new theory (or paradigm) and visual gestalt shifts. To change one's theory (or paradigm) involves changing one's world view and hence one's world; to change one's theory is to change what one sees, and apparently, what there is to be seen. In adopting a new theory, one adopts a way of seeing the world that confirms the theory; thus there is no independent or neutral description of evidence to which one could appeal in the course of theory choice. The objectivity assumed in Hempel's analysis, the straightforward determination of confirming, disconfirming, or irrelevant, observation reports is thus rejected for an account of theory choice much vaguer and more subjective. On the accounts offered by Kuhn and Feyerabend, theory choice in science is no longer a high expression of rationality and objectivity, but is described as non-rational or irrational, and certainly not evidence-determined.

Both Kuhn and Feyerabend have convincingly pointed out that science as a historical phenomenon does not conform to the model developed by logical empiricists such as Hempel. The models with which they propose to replace the empiricist account have, however, been shown to be at best paradoxical (Shapere 1964; Achinstein 1968). The paradox lies in the supposition that two theories could be both mutually incommensurable and mutually contradictory. It has been argued, for instance, that if we regard the meaning of a term occurring in one theory as changed when it occurs in some other theory, then we cannot say that any theories contradict one another: given two theories which appear to be inconsistent, on this view, scientists asserting hypotheses associated with the two theories are using the common terms in different ways and hence could not be said to be contradicting one another. Just as theories could not be said to be contradictory, so they could not be said to be in agreement or consistent with one another.

Objections of paradoxicality can be met with the claim that 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 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 which 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 an 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 wants to say, 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 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 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 which already had a theory of the oscillatory motion of vibratory strings. In the context of this theory, features other than those which strike the Aristotelian become important, in particular the repetitive 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 (Kuhn 1970a, p. 120).

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's no need to suppose that the Galilean or the Aristotelian must fail to see aspects which interest the other, nor to suppose that there is no description of the situation which both could accept and which would then form the basis for discussion of differences. 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, strictly speaking, they are not 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, i.e. 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 this paper seems to accord better with the actual facts.

The model of evidential relations being defended here 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 an 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 an hypothesis. The gestalt metaphor, as Kuhn realizes, renders evidential considerations either useless or self-deceptive. Taken seriously, it also makes the fact that some theories work, while others do not, utterly mysterious.[7]

III

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, i.e., 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 (such as simplicity, 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 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 empiricist (Hempelian) conception of evidence. Because the relationship of evidence to hypothesis 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 empiricist analysis is a description of an ideally rational procedure to which science, for better or for worse, fails to measure up.

So to regard the empiricist 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 empiricism presented by both Kuhn and Feyerabend rely heavily on historical

[7]In criticizing Kuhn and Feyerabend for assimilating what I take to be cases of change of belief to cases of change of meaning, theory dependence, etc., I do not wish to be understood as claiming that meanings never change. Sometimes they do, but such change is not capable of bearing the burden of explanation thrust upon it by Kuhn and Feyerbend. For a way of distinguishing change of meaning from change of belief, see Achinstein 1968, ch. 3.

example, and in Kuhn's case, on a psychological hypothesis about perception. The account of evidential relations defended in this paper, 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 empiricist 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.

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 thought 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 differently and in ways determined by the hypothesis 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, i.e., 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 an 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 a-rational or non-rational to accept or reject hypotheses with no regard for evidence.

In these cases of differing evidential assessments owing to different background assumptions, there is a temptation to say that, since one must be right and the other wrong (unless both are wrong) one of the background assumptions must be at fault, i.e. one must be true and the other false. This is analogous to the claim that while what is taken to be evidence for an hypothesis is relative to background assumptions, nevertheless whether or not some state of affairs actually is evidence for an hypothesis is not a relative matter. However, in deciding which assumption is correct (or which of the supposed evidential relations is the actual evidential relation), one must look to see what evidence is offered in support of each. What is offered as, or taken to be, evidence, will in turn be so in light of further background assumptions. Thus, the relativity of evidential relations

is thorough: if e is taken to be evidence that h in light of b, then we cannot with any finality determine whether it is correctly so taken by examining the evidential support for b, since whatever state of affairs e' is taken to be evidence for b, is so taken in light of some further assumption b' (and so on). If there is some way of determining in an absolute way the truth of background assumptions, it cannot involve appealing to evidence. To maintain that there is a distinction between what is taken to be evidence and what is evidence is to suppose that there is some non-evidential means of discovering the truth or falsity of background assumptions. Since the empiricist view rules out non-evidential means of ascertaining the truth of a belief other than immediate perceptual awareness, an empiricist must accept a thorough-going relativism with respect to evidential relations.

What the account of evidential relations defended in this paper offers is a way to accept such relativism and at the same time preserve the view of scientific inquiry as being, at least in principle, a form of rational and objective inquiry. If the different evidential assessment of the same state of affairs is a function of differing background assumptions rather than of differently 'seeing' or of having incommensurable languages or incommunicable world views, then objectivity must consist not only in being rational, i.e. assessing evidential relations consistently with one's background assumptions, but also in articulating and thus exposing to evaluation and criticism the background assumptions in light of which such assessment takes place.

Whether one can ultimately detach evidential relations from the contexts defined by the beliefs of an individual or of a community depends on whether criteria for the acceptability of background beliefs other than evidential support can be articulated (cf. Hempel 1965, p. 248; Achinstein 1971).[8] This question, with others, lies beyond the scope of this essay. Further methodological studies, both historical and logical, could clarify the role of background beliefs and assumptions and their relation to objectivity and rationality in science along several dimensions. From a descriptive point of view much can be gained by examining in particular cases what the origin of background assumptions has been, the degree to which given background beliefs pervade other areas of research, and what criteria, if any, determine their acceptance.[9] From a normative point of view, there seem to

[8]This is analogous to, but more acute than, the problem of detaching explanations from the contexts in which they are explanations. In connection with the latter, see (Hempel 1965, p. 248) and (Achinstein 1971, pp. 61–84).

[9]Holton (1973) offers good examples of the historical pursuit of questions of this sort. Also, Glymour (1975, pp. 403–425) shows that auxillary hypotheses from the same or different theories are needed to deduce from an hypothesis exact quantitative expressions of the data required to support it. This is a good example of their pursuit from a logical point of view.

be at least two sorts of questions to be asked. On the one hand, it seems important to understand the criteria governing the evaluation of evidence within given frameworks of background beliefs. Can criteria developed for simple inductive frameworks be extended to other frameworks? Is it possible to articulate criteria that are or should be operative in all frameworks? On the other hand, it seems important to investigate the criteria that govern the evaluation and acceptance of background beliefs themselves. Even though the prospect of an infinite regress prevents one from supposing that the adoption of all beliefs could be evidentially based, the is no *a priori* reason to suppose that there are no criteria at all. One can ask whether, and if so, which, criteria should determine their acceptance.[10]

Some theories, some hypotheses, just are better than others. Experience tells us this much. How we determine which are better is not a matter of simply counting instances and counterinstances. To propose the above questions is to express the faith that it is more than a matter of luck.

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[10]I include the first part of the question since Feyerabend can be understood as claiming that no criteria should govern the acceptance of background beliefs. A case must be made that the development of scientific knowledge requires or is enhanced by such criteria.

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