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Explanation in the Social Sciences

1. Introduction

Disagreements about explanation in the social sciences are closely bound up with views about whether or not the so-called social sciences really are sciences.

The dispute has a long history. J. S. Mill, following Hume and the philosophers of the French Enlightenment, maintains that a science of human nature is possible (1874, 586). He believes that the thoughts and feelings of humans are the causes of their actions. On this basis, he argues that we can investigate the causal connections between thoughts and actions by employing the same canons of inference (Mill's Methods) that we use to discover and justify causal regularities in the physical world.

Mill recognizes that the complexity of human behavior impedes the development of causal explanations. Nevertheless, he believes that at least an *inexact* science of human behavior is possible. Whether a science is "exact" or "inexact" depends on how accurate the predictions of the science are. Mill doubts that the science of human behavior will ever become as exact as the physical science of astronomy, for example, because human actions are subject to so many unknown, and possibly unknowable, circumstances. In addition, even when the circumstances surrounding behavior are known, we are sometimes unable to describe or measure them accurately.

Mill points out, however, that the accumulation and interaction of many minor causal forces simlarly hinder accurate prediction in some physical sciences. Thus, the main laws that govern the movement of tides are known, but, because of irregularities in shorelines and ocean floors, as well as changes in direction of winds, precise predictions of tidal movements are not possible. Still, Mill says, no one doubts that tidology is a science, and hence the failure of prediction is no barrier in principle to the development of an *inexact* human science. With this

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limitation in mind, Mill urges scientists to investigate human behavior with the aim of uncovering general laws.

Mill says that to discover patterns of connection between human thought and human action we must first study history to discern some regular connections. In Mill's version of what later came to be called a *covering-law model of explanation*, he suggests that the regularities revealed by historical studies ("the lowest kind of empirical laws") are themselves to be explained by showing that they are derived from laws of character development. Laws of character development, in turn, are to be explained by showing how they result from the operation of general laws of the mind (1874, 589).

Mill believes that fundamental causes of human behavior are probably mental rather than physical, but he does not think that this makes explanation of human behavior significantly different from explanation in the physical sciences. For Mill, subsumption under causal generalizations is at the heart of explanation. When we explain an event (either a physical occurrence or a human action) we must subsume it under an appropriate causal generalization; when we explain a generalization we must subsume it under a more general law or set of laws. *Explanation* thus is possible in inexact as well as exact sciences, though in the latter more precise *predictions* are possible.

C. G. Hempel's work on explanation in the social sciences lies squarely in the tradition of Mill. Hempel argues that insofar as explanations in history and other social sciences are complete, rather than elliptical or partial, these explanations require relevant universal or statistical generalizations. Hempel recognizes that many of the generalizations invoked in explanations in the social sciences are vague, common-sensical claims, unlike the more precise and well-confirmed generalizations in the physical sciences. He points out, though, that when the generalizations are not well founded, the explanations that they underlie are accordingly weakened (1962, 15–18). Both Hempel and Mill insist that explanatory laws have empirical content, but Hempel, unlike Mill, countenances *noncausal* explanatory laws.

Hempel agrees with Mill that human action can be explained by reference to mental causes, such as motives, beliefs, desires, and reasons. Similarly, he agrees with Mill that future investigations might show that mental concepts are "reducible" in accord with some materialist program. However, Both Hempel and Mill recognize that even if succesful materialist reductions were forthcoming, explanations in terms of mental causation would not thereby be rendered obsolete. Just as in the physical sciences, an explanation at one level (such as, for example, explanation of the behavior of gases in terms of pressure and temperature) can be correct and informative even when the phenomena can also be explained at a "deeper" level (as when the behavior of gases is explained in terms of molecular motion).

Although both Mill and Hempel embrace nonmaterialist causal explanations of behavior, their shared belief that explanations of human behavior are fundamentally similar to explanations of physical phenomena is challenged by critics who argue that the ability of humans to exercise free choice sets them apart from the rest of nature. Since humans are able to make decisions and carry out plans in accord with their own reasons rather than some external constraint, the critics say, voluntary human behavior, unlike physical phenomena, cannot be subsumed under laws. Thus, the critics conclude that covering-law explanations cannot account for human behavior.

Mill was aware of this objection, but thought that it was based on a misunderstanding about the possibility of *accurate prediction* in science. He believed he had solved the problem by showing that even in the physical sciences precise predictions are not always possible. He argued that since the absence of precise predictions is not an impediment to the construction of inexact physical sciences, it cannot prevent the construction of inexact human sciences.

Hempel responds differently to the criticism that some distinctive form of explanation is required to account for human actions. He argues that explanations in terms of mental causes, such as motivating reasons, have the same *logical* structure as covering-law explanations in the physical sciences. A human action, like a physical event, he says, is explained when it is shown to follow from explanatory facts that include at least one law statement. Hempel presents the following model for explaining behavior that is a result of rational deliberation:

Agent A was in a situation of type C. [Initial Condition]
A was disposed to act rationally. [Initial Condition]
Any person who is disposed to act rationally will, when in situations of type C, invariably (with high probability) do X. [General law]

A does X. [Event to be explained] (1962, 27)

In Hempel's model, agents are considered rational if they are disposed to take appropriate means to achieve their chosen ends. In contrast to Dray's (1957) account, Hempel does not assume that agents always act rationally, but instead he regards the attribution of rationality to an agent as an explicit initial condition in the explanation. Hempel adds that covering-law explanations with the same logical form as explanations of rational behavior can be framed for behavior that is not a result of "rationality and more or less explicit deliberation, but . . . other dispositional features, such as character and emotional make-up" (1962, 27).

Hempel's models display clearly what he means when he says that explanations in physical and social sciences are similar to one another: Each has the logical structure of an *argument* (inductive or deductive) in which the event to be explained is the conclusion and some initial conditions and law or laws constitute the explanatory premises. Hempel's models so aptly clarify and refine respectable

popular intuitions about the nature of explanations in the physical sciences that they have become a focus for discussion of alternate views about explanation in social sciences. In what follows, I will look at some objections to his approach and some alternative accounts of explanation in the social sciences.

The objections to Hempel's account that I will discuss fall roughly into three categories. The first position, *interpretativism*, regards explanations of human purposive action as having an entirely different structure from causal explanations or any other explanations that appeal to *laws*. Interpretativists deny that there are empirical laws connecting reasons with actions; they say that to suppose there could be such laws involves committing a *logical* error.

The second type of objection can be called "nomological skepticism." It is dominated by the worry that there are no *laws* in social science available for constructing covering-law explanations. Some nomological skeptics admit the eventual possibility of discovering laws, while others are less sanguine. The skeptics' doubts about the possibility of finding appropriate laws are pragmatic in contrast to the logical concerns of the interpretativists. Skeptics believe that the great variability and complexity of human behavior pose practical barriers to framing generalizations that are at once informative and true. Although both Mill and Hempel have addressed these practical concerns, the skeptics remain unconvinced.

The third type of objection, proposed by *critical theorists*, sees lawful explanation as a threat to human autonomy. Critical theorists worry about the ethical implications of trying to explain human behavior in the same manner that we explain the actions (or movements) of nonconscious physical objects.

Aside from the three types of objections just described, other fundamentally important criticisms of Hempel's models raise questions about whether laws are *parts* of explanations or whether instead laws justify or underlie explanations (Scriven 1959; Humphreys 1989), whether explanations are arguments (Jeffrey 1969), whether a statistical explanation must show that the event to be explained is highly probable (Salmon 1965), and whether the pragmatic features of explanation have been adequately addressed (van Fraassen 1980). As the present volume attests, the models originally proposed by Hempel more than twenty years ago cannot survive these criticisms intact. Not surprisingly, an adequate account of scientific explanation must go beyond that early work.

Nevertheless, we can look at criticism of explanation in the social sciences without exploring these refinements. For insofar as newer models are causal models of explanation or covering-law models of explanation (see, for example, Glymour et al. 1987) the objections raised to them by interpretativists, nomological skeptics, and critical theorists will not be very different from their objections to Hempel's original models. Since this paper focuses on lawful explanation in the social sciences, I will limit the scope of this discussion to the criticisms raised by interpretativists, nomological skeptics, and critical theorists.

2. Interpretativism

Interpretativists reject as logically confused the claim that explanations in the social sciences are fundamentally similar to those in the physical sciences. Their position goes back at least to Dilthey, whose main work begins just after Mill's death. A forceful exposition of interpretativism is found in R. G. Collingwood (1946).

Collingwood uses the terms "history" and "historical thought" to refer to all studies of human affairs, including not only history, but also (parts of) anthropology, sociology, political science, psychology, and economics. The contrast class, called here "physical science," includes physics, chemistry, and even the so-called historical sciences, such as evolutionary biology and geology.

The events studied by history have, according to Collingwood, an "inside" as well as an "outside." The outsides of events consist of everything belonging to them that can be described in terms of bodies or their movements. The insides of events can be described only in terms of the thoughts of the agent (or agents) who are responsible for the events. Human behavior requires for its complete description not only the account of bodily motions involved but also an account of the beliefs and desires of the agent. For example, the physical description of one person cutting another with a knife does not distinguish an act of surgery from an act of assault, a ritual act, or an accidental cutting. A more detailed physical description of the "outside" may provide clues that will help to ascertain the intention of the cutter, but until that intention is uncovered, Collingwood would say, we simply do not know what action took place.

Collingwood argues on the basis of the difference between events with only an outside and those with both an inside and an outside that there is a fundamental difference between the search for so-called causes of human behavior (the *reasons* for the behavior) and causes of physical events. In the physical sciences, the event is perceived, and its cause (a separate event) is sought. This investigation takes the scientist beyond the original event in order to relate it to other separate events, thus bringing it under a general law of nature.

In contrast, when studying human actions, the historian must look inside the event to discover the thought that is expressed *in* the event. The event studied is not really separate from the thought, but is the mere expression of the thought. The thought is the event's "inside," that which makes the event what it is. From his claim that the reasons for actions are not external to the actions, Collingwood concludes the relation between the two cannot be governed by laws of cause and effect. Thus, he says, causal laws play no essential role in the explanation of human behavior.

When an action is described as the sort of action it is, the description itself includes the reason for the action. In the case of purposive human behavior, the acts of describing and explaining are therefore one and the same. The reason for the

act is not viewed as some cause that is separate from it, but rather is *logically* or *meaningfully* related to the act. The reason gives meaning to the act, and makes it the sort of act it is. Accordingly, Collingwood would say that the model of searching for regular connections between actions and reasons, as proposed by Mill and, later, Hempel, simply makes no sense.

Among contemporary philosophers, Peter Winch (1958) is a leading advocate of the interpretativist position. Like Collingwood, he denies the role of anything analogous to a law of nature in explaining human behavior, and he regards the term "social science" as misleading for this reason. Winch's view is that "social relations really exist only in and through the ideas that are current in a society; [and] . . . that social relations fall into the same logical category as relations between ideas" (1958, 133). Causality is thus no more an appropriate category for understanding social relationships than it is for understanding mathematical relationships. Whereas Collingwood pays special attention to individual beliefs and desires, Winch, following Wittgenstein, emphasizes the social character of human action and thus he focuses on the importance of *rules* or norms of behavior.

Winch's broad notion of a rule covers not only formal regulations, such as traffic rules and tax deadlines, but also unstated cultural norms or conventions, such as those governing the appropriate distance between speakers engaged in a face-to-face conversation. In addition, "rule" embraces practices and institutions such as religion, democratic government, and money. Some rules are regulative, such as the rule to stop for red lights, while others are constitutive of the practices they embrace.

Consider, for example, the act of offering a sacrifice. This act is possible only within a certain type of institution in which particular kinds of behavior, such as killing animals in a prescribed way and under certain conditions, count as offering sacrifices. Without such an institution or social practice, it makes no sense to call the killing of the animal in that way a sacrifice. The point of these constitutive rules is that whether an action is performed and what kind of action it is depends not only on individual intentions, but also on the social set-up. Social relations, however, according to Winch, do not *cause* the act; they rather constitute it by giving it the meaning it has.

Obviously, we cannot understand what is going on in a sacrifice if we do not have the concept of a sacrifice. To say that the concept of sacrifice depends on the social set-up, however, goes far beyond the claim that, as individuals, we can acquire the concept only through socialization. For, not only how we come to learn the concept, but also the very meaning of the concept depends on the possibility of social relations of a certain type.

To understand human behavior, Winch says, we require more than just abstract knowledge of the rules of a society. We also need to understand what counts as following a rule in a particular case. To grasp this, he says, we must somehow

come to share the viewpoints, attitudes, and feelings of the actors. For example, consider a society which has a norm that requires showing respect to elders. Knowing this rule and having a physical description of some bit of behavior are not enough to figure out whether or not this behavior in the presence of an elder counts as showing respect. We also require some knowledge of the beliefs and attitudes of the person engaging in the behavior. Was the person aware an elder was present? Did the person know that form of address was considered disrespectful? Was the person merely careless in his manner? Was the form of address considered disrespectful by the speaker or the elder?

Even in one's own society, working out such matters can require complicated negotiations. Understanding human behavior in exotic societies is doubly problematic, and those who attempt it must be on guard against ethnocentrism. Winch's sensitivity to the importance of rules and how they are applied, as Papineau notes (1978, 96–97), accords well with the concerns of contemporary cognitive anthropologists. Ward Goodenough (1957), for example, says that culture consists not of "things, people, behavior or emotions but the forms or organizations of these things in the minds of people" (quoted in Frake 1969, 38). Cognitive anthropologists like Goodenough see their task as uncovering these forms (i.e., rules) primarily on the basis of what people say about how they categorize and organize the furniture of their worlds in applying the rules.

Winch does not deny that we can predict human behavior. He admits that with a knowledge of the rules of a society and an understanding of how the rules are applied, reliable prediction is often possible. He insists, however, that successful prediction of behavior is unlike predictive success in the physical sciences. The difference is not just that physical science yields more accurate predictions, for in some cases, as in tidology, it does not. The point is that in the physical sciences, *causal regularities* are the basis of the predictions, whereas insofar as behavior is rule-governed, it is not subject to causal laws. Rules, according to interpretativists, function as standards or norms of behavior; they give meaning to behavior, but do not cause it. Although we can predict behavior from a knowledge both of the rules of a society and of how those rules are translated into actual behavior, the prediction is not based on a causal relation between the rule and the behavior. The concepts of cause and effect, Winch insists, simply do not apply to the relation between a reason for action and the action, any more than the concepts of cause and effect apply to the relation between being a Euclidean triangle and having internal angles with a sum of 180 degrees.

Interpretativists acknowledge that discerning the rules that underlie human behavior is an important goal. They insist, however, that these rules have a logically different character from causal laws—or other empirical laws—discovered in the physical sciences. Because interpretativists see the relation between reasons and behavior as a logical relation rather than a causal relation, they reject the covering-law models of explanation.

Drawing support from interpretative philosophers, some social scientists have also rejected covering-law models of explanation. Clifford Geertz, for example, claims to do so in *The Interpretation of Cultures* (1975).

Anthropology, for Geertz (in contrast to cognitive anthropologists like Goodenough), is ethnography. Ethnography, he says, consists in interpreting the flow of social discourse. This interpretation itself consists of inscribing or recording the flow, "fixing" it so that it can be shared and reexamined long after the actual events take place. A major problem for the ethnographer is that of finding the appropriate general concepts for describing or classifying the observed behavior. For example, an ethnographer might observe that meetings are taking place and that political issues are being discussed. Further features noted might include low voices and concern with arrangements to secure secrecy. Is the appropriate *thick description* (Geertz borrows Ryle's expression) or interpretation of such activities that of "fomenting a rebellion" or something less inflammatory?

Geertz pointedly calls the ethnographer's activity of fixing the flow "interpretation" rather than explanation. Following Dray's (1957) advice to historians, Geertz believes that anthropologists should primarily be concerned with assigning observed behavior to appropriate concepts. Like Dray, he denies that covering laws play any role in this activity. He adds that any true statements that are general enough to serve in covering-law explanations in anthropology are either hopelessly vague or trivial. In such passages, Geertz's rejection of the possibility of lawful explanation in anthropology echoes Scriven's criticism of Hempel's models in "Truisms as the Grounds for Historical Explanation" (1959), and also reflects the position I have characterized as nomological skepticism.

In this same work, however, Geertz also says that anthropologists are interested not only in particular ethnographic studies (which he calls microscopic work) but also in going beyond ethnographies to compare, to contrast, and to generalize. Indeed, he acknowledges that some generalization occurs even within microscopic studies. As Geertz says, "We begin with our own interpretations of what our informants are up to, or what they think they are up to, and then systematize these." In another place, where he is talking about doing ethnography, he says:

> Looking at the ordinary in places where it takes unaccustomed forms brings out not, as has so often been claimed, the arbitrariness of human behavior . . . but the degree to which its meaning varies according to the pattern of life by which it is informed. Understanding a people's culture exposes their normalness without reducing their particularity. . . . It renders them accessible; setting them in the frame of their own banalities, it dissolves their opacity. (1975, 14)

However, Geertz's remarks are ambiguous, for they lend themselves either to an interpretativist or to a nomothetic (covering-law) construction. The nomotheti-

cally inclined reader can say: "What can it mean to expose the normality of a people if we do not know what normality is and have no concern with the normal? Some account of what is normal provides the framework for our recognition and understanding of traits in another culture. But to have an account of what is normal is to be in possession of at least some statistical laws of human nature, to know that humans usually believe, say, or do thus and so under such and such circumstances. Moreover, at least part of what it is to interpret or to understand something is to place it within an intelligible framework or to see how it fits into a pattern. But intelligible frameworks and patterns are the sorts of regularities that, according to Hempel, are expressed in statistical or universal laws.

Winch could object to such a nomothetic reading of Geertz's position on the grounds that the reading involves a serious misunderstanding of the type of regularity or normality present. The appropriate regularities, according to the interpretativist, are not causal regularities, but rules or norms of behavior. A grasp of these is crucial for understanding behavior, but norms and rules are not explanatory laws, and so cannot play that role in explanation.

Geertz, however, in contrast to Winch, does not want to deny that anthropology is a science, albeit a different and "softer" kind of science than physics. In discussing how anthropological theories are constructed, Geertz says that whereas in covering-law models individual cases are subsumed under *laws*, the generalizations that constitute theory in anthropology are generalizations *within* cases rather than generalizations *across* cases. Amplifying this point, Geertz goes on to say that the type of generalization he refers to is called, in medical science, "clinical inference." He says that anthropologists should look for "intelligible patterns" into which otherwise unrelated bits of information can be fitted rather than "universal laws" that can be combined with initial conditions to *predict* cultural patterns.

Geertz's rejection of prediction as a goal for anthropology provides an important clue to his dismissal of the covering-law models. He apparently believes that covering-law explanation commits one to an objectionable view about the close relationship between explanation and prediction.

Hempel, it is true, argues for a kind of symmetry between scientific explanation and scientific prediction. He regards scientific explanations of individual events as arguments to the effect that the event to be explained was to be expected on the grounds of laws and initial conditions, and scientific predictions as arguments to the effect that the event predicted is to be expected on the grounds of laws and initial conditions. Given Hempel's analysis, explanation and prediction do not differ in their logical structure, but only in the temporal order of the events to be explained or predicted and in the starting point of our knowledge of the situation. In explanation, we are aware that the event has occurred, we search for the relevant laws, and we try to reconstruct the initial conditions from which the event can be derived. In prediction, we are aware of the initial conditions, and

with knowledge of the relevant laws, we derive the occurrence of the predicted future event.

It is important, however, not to misread Hempel's thesis of the symmetry of explanation and prediction as asserting that whenever an event can be explained it could have been predicted. Hempel, in response to criticisms of his symmetry thesis by Scriven and others, defends "the conditional thesis that an adequate explanatory argument must be such that it could have served to predict the explanandum event *if* the information included in the explanans had been known and taken into account before the occurrence of that event" (1965, 371). At the same time, he insists that his symmetry thesis does *not* require that we always can know independently of the occurrence of the explanandum event that all the conditions required for an explanation are realized. There are clearly situations in which it is only "after the fact" that we are aware that some of the initial conditions were present. For example, before a murder occurs there may have been no way to recognize that the rage of the murderer was sufficient to motivate him to commit the act, or to know that he was capable of murder.

Hempel's version of the symmetry thesis does *not* claim that because the act can be explained in terms of laws and initial conditions, that these laws and initial conditions could have been discovered before the act occurred. To hold that any act which can be explained after the fact could have been predicted before the fact (without further qualification) requires a much stronger symmetry thesis. This stronger version of symmetry is indefensible, and should not be attributed to Hempel. (Hempel's symmetry thesis is closely linked with his understanding of explanations as *arguments*. For a weaker version of symmetry, compatible with the view that explanations are not arguments, see Salmon 1970.)

Geertz apparently regards the indefensible strong symmetry thesis as a feature of "the covering-law model of explanation." He specifically rejects attempts to establish covering laws in anthropology to *predict* behavior because he sees the goal of social science as understanding rather than prediction. He then goes on to reject explanation as a goal because, guided by the strong symmetry thesis, he erroneously identifies lawful explanation with the ability to make predictions.

Ironically, when Geertz says that clinical inference provides a model for understanding anthropology, he seriously undercuts his interpretativist stance. Clinical inference involves diagnosis of symptoms, such as the diagnosis of measles on the basis of its telltale signs. Diagnosis, as Geertz correctly notes, is not concerned with predicting. It is directed toward analyzing, interpreting, or explicating the complex under consideration. Geertz sees this activity as the essential concern of anthropologists and therefore argues that anthropology is a diagnostic science rather than a nomothetic one.

Hempel shows, however, that cases of diagnosis or clinical inference fall easily within the scope of covering-law explanation (1965, 454–55). Moreover, in several places where Hempel discusses "explanation by concept" and other forms

of explanation in the social sciences, he offers an account of explanation that deemphasizes the relation between explanation and prediction, and focuses instead on an explanation's power to increase our understanding. Near the end of "Aspects of Scientific Explanation," for example, he says that explanation "seeks to provide a systematic understanding of empirical phenomena by showing how they fit into a nomic nexus" (1965, 488). The same emphasis on understanding rather than prediction is apparent in "Explanation in Science and in History" (1962, 9).

Hempel's arguments strongly support the position that scientific "understanding" is just as dependent on laws as scientific explanation. If Hempel is correct, Geertz's form of interpretativism is very different from Winch's, for it is just as dependent on laws as nomological explanation. Geertz, however, as noted earlier, has also questioned whether there can be any nontrivial laws in anthropology. This nomothetic skepticism is a sticking point for many social scientists. In the next sections, we will consider some attempts to find and characterize appropriate laws for explaining (and understanding) human behavior.

3. Rationality and Explanations of Behavior

When Hempel wrote "The Function of General Laws in History" (1965, 251–52), he believed that it would be possible to discover causal laws of human behavior though he did not specify any particular form that these laws would take. At that time there was widespread hope that behaviorism would soon deliver what it promised. Hempel reflects this optimism when he suggests that we may someday find behavioristic stimulus-response laws or laws of learned behavior that connect circumstances and actions. If statements describing such regularities—which I take to be causal—were available, they would be suitable covering laws for constructing explanations of human actions. Similarly, though Hempel does not say so here, if causal laws connecting brain states and human *actions* (not mere bodily movements) were available, these could also be used in covering-law explanations of behavior. However, neither of these materialist programs has so far succeeded in supplying the sorts of laws which the programs' early adherents hoped would be forthcoming.

Hempel clearly regards instances of his proposed law schema—"Any person who is disposed rationally will, when in circumstances of type C, invariably (with high probability) do X" (1962, 28–29)—as having empirical content, whether or not the law is causal. In keeping with his understanding of rationality and other character traits as *dispositional* properties, Hempel does not regard them as completely definable in terms of manifest behavior. He says instead that they may be partially definable by means of reduction sentences. Following Carnap's account of reduction sentences, Hempel says that the connections between dispositional properties (e.g., rationality, fearlessness) and the actions that are symptomatic

of these dispositions are stated in claims that express either necessary or sufficient conditions for the presence of the given disposition in terms of the manifest behavior.

Reduction statements seem to be analytic, for they offer at least partial definitions of dispositional properties. Yet not all these statements can qualify as analytic, Hempel says, for in conjunction they imply nonanalytic statements of connection between various manifest characteristics. For example, if a specific form of behavior *A* is a sufficient condition for the presence of rationality, and another specific form of behavior *B* is a necessary condition of rationality, then the claim "Whenever *A* is present *B* will be also," which is a consequence of the conjunction of the two reduction sentences, and which asserts that two types of behavior are always found together, "will normally turn out to be synthetic" (1962, 28–29). On this basis, Hempel claims that the generalizations which state that in situations of a certain sort, rational agents will act in specified ways are empirical.

The success of Hempel's defense of the empirical character of the laws that connect dispositions with behavior is obviously tied to the possibility of finding suitable reduction sentences for characterizing dispositional properties such as rationality, fearlessness, and the like. This is, I believe, so closely linked with behaviorism that it inherits the problems associated with that program. Moreover, Hempel admits in "A Logical Appraisal of Operationism" (1965, 133) that the ability to derive synthetic statements from pairs of reduction sentences casts some doubt on the "advisability or even the possibility" of preserving the distinction between analytic and synthetic sentences in a logical reconstruction of science. Despite his concession to the blurring of this traditional empiricist distinction, he maintains his original standard requiring empirical content in explanatory laws.

It is fair to say that behaviorism no longer enjoys the sway it once held. In a more contemporary attempt to understand the nature of rationality, many philosophers have rejected the behaviorist approach and turned to a decision-theoretical analysis of rationality. However, if we try to understand "rationality" in decision-theoretic terms rather than behavioristic terms, the empirical content in the law (or law schema)—"Any person who is disposed rationally will, when in circumstances of type C, invariably (with high probability) do X"—remains elusive.

The decision-theoretic approach to rationality is intended to be applicable under the assumption that an agent is acting independently and with only probabilistic knowledge of the outcomes of various actions. Under such circumstances, a person is said to behave rationally just in case the person acts so as to maximize expected utility. If we adopt this criterion, then to say that if an agent is disposed to act rationally in circumstances C, the agent will do X, is equivalent to saying that in circumstances C, action X maximizes expected utility (or, in Papineau's (1978) phrase "expected desirability").

The expected desirability of an action is the sum of the products of the probabilities and the values for each of the possible outcomes of the action. Maximizing

expected desirability just means choosing the action with a sum that is not lower than the sum of any other action. The statement that an agent is in circumstances C (i.e., the agent holds various beliefs to which probabilities are assigned and also has desires with values attached to them) is clearly empirical. However, whether or not actions maximize expected desirability under such specified circumstances is not an empirical matter, but rather a judgment based on a calculation which is determined by the criterion of rationality.

The decision-theoretic account of rationality does not actually require agents to perform calculations. However, to be rational, actions must accord with the results of such calculations, had the calculations been performed. This means that *rational* agents must choose actions that maximize expected desirability, for they could not do otherwise and still be rational agents.

Thus, if we use the decision theorist's analysis of rationality, instances of Hempel's schema for a law ("Any person who is disposed to act rationally will when in circumstances C invariably (with high probability) do X") fail to meet his criterion of empirical content. But if the laws have no empirical content, they cannot ground genuine scientific explanation, even though they may be the basis for another form of explanation, similar to explanation in mathematics, where the laws are not empirical.

The criterion of rationality offered by decision theory is minimal in the sense that agents need only take some available means to achieve whatever ends they desire, or at least to avoid frustrating those ends. The decision-theoretical account of rationality does not assume that agents make good use of available evidence when forming beliefs. Even if agents' beliefs are based on prejudice or ignorance, or if their desires are peculiar or hard to comprehend, their behavior can be rational. Moreover, as mentioned before, to be rational in this sense, agents need not assign explicit probabilities to beliefs or quantify values, or even make rough or precise calculations of expected desirability. It is enough for agents to act *as if* they were maximizing desirability, given their beliefs and desires.

If we strengthen the decision theorist's standard of rationality to require that rational agents use an objective physical basis instead of a purely personal consideration to assign probabilities to various possible outcomes of an action, we invoke some probabilistic laws. However, these are not covering laws; the probabilistic laws are used only to assess the truth of the initial condition which states that the agent is disposed to act rationally. As such they are not even part of the covering-law explanation itself.

David Papineau adopts the decision-theoretical approach to rationality in his (1978) attempt to reconcile the interpretativist position with nomothetic explanation of human action. Papineau presents a covering-law model of explanation of human action that differs from Hempel's in several respects.

According to Hempel, an agent's disposition to act rationally is an initial condition that must be established empirically. For Papineau, agents always act ratio-

nally. According to him, the lawlike generalization that grounds explanations of behavior is: "Agents always perform those actions with greatest expected desirability" (1978, 81). This law, he believes, is implicit in ordinary explanations of individual human behavior. Accordingly, for Papineau, acting out of a character trait, such as fearlessness, would not contrast with his acting rationally, as it might for Hempel. (For Hempel, humans may be rational at some times, and not at others; for Papineau, it is a contingent, but universal truth that humans behave rationally. We must remember though that the two differ about the meaning of rationality.)

Papineau agrees with Winch that rules do not *cause* behavior, for otherwise, he says, humans would be mere puppets of their cultural milieu, unable to violate social norms. Obviously people can disregard norms, whereas causal laws cannot be violated. Papineau acknowledges that the existence of rules plays an important role in forming agents' beliefs and desires, and that the interpretativist has something important to say about this (1978, ch. 4; also see Braybrooke, 1987, 112–16). However, Papineau says that the information which guides our attributions of beliefs and desires to agents is not actually part of the explanation of human behavior. All that is required for explanation, in addition to the generalization about maximizing expected desirability, is an account of what beliefs and desires an individual has (these are the initial conditions), not an account of how the agent came to have them. Papineau does not deny the importance and relevance of the interpretativists' concerns, but he sees them as supplementing, rather than conflicting with, lawful explanation.

Papineau is aware of the apparent lack of empirical content in his proposed law: "Agents always perform those actions with greatest expected desirability," and he tries to defend his model of explanation against this criticism. Although, he says, it is true that we use this general principle to infer the nature of agents' beliefs and desires, it is legitimate to do so as long as we do not include the action we are trying to account for in establishing those attitudes. We infer beliefs and desires from agents' past actions, aided by knowledge of the norms of their society, and then—attributing to them those attitudes—explain their present actions in the light of that knowledge.

Nevertheless, Papineau recognizes that sometimes people do not act in accord with the beliefs and desires that have been correctly attributed to them in the past. And when this happens, he admits that we do use the anomalous action to infer the beliefs and desires that underlie it. His argument to legitimize this move appeals to Lakatos's understanding of a theoretical "core statement" that is maintained in the face of presumptive counterexamples by revising various auxiliary assumptions (see, for example, Lakatos 1970). It is a commonplace that people's beliefs and desires do change over time, so we must be prepared to take account of such revisions. However, he says, "this preparedness does not condemn the overall theory as unscientific—none of the most revered theories in the history

of science would ever have survived if their proponents had not been similarly prepared to defend their central tenets from the phenomena by revising auxiliary hypotheses" (1978, 88).

Papineau does not mean to countenance ad hoc revisions to save his generalization that agents act always so as to maximize expected desirability. He says that if we attribute to an agent extraordinary desires or beliefs that we would not expect to be available to the agent, we should be prepared to give some account of the circumstances that will lead to new and independently testable propositions. We are bound to do this, just as any physical scientist is required to do so in a "progressive" research program (1978, 88).

Ultimately, however, Papineau urges us to accept this theory of human action because we do not have a better theory available. Behaviorism is impoverished, he says; physiological accounts are woefully undeveloped, and the rule-governed account of behavior must be integrated into the decision-theoretical model if humans are not to be understood as cultural puppets who rigidly conform to norms, rules, and conventions.

Even if interpretativists were willing to accept Lakatos's account of the nature of theories in physical science, I doubt that Papineau's arguments would undermine their insistence that human behavior is not subject to causal laws, for he does not address the claim that reasons for behavior are *logically* different from causes. At the same time, those who urge nomological explanations of human behavior will be disappointed in Papineau's failure to put forth an explanatory generalization with empirical content.

One can accept a decision-theoretical account of the nature of rationality, while nevertheless recognizing, as Donald Davidson (1980) does, that no *criterion* of rationality, however satisfactory it is, can function as a law in covering-law explanations of human behavior.

In a series of papers (1980, especially essays 1, 7, 12, 14), Davidson refutes the interpretativist claim that it is a logical error to suppose that reasons can cause actions. Interpretativists have typically argued that if things are logically related (or connected by relations of meaning), then they cannot be causally related. Davidson shows that whether or not two events are related causally depends on what the world is like, whereas various linguistic accounts of the relationship may be classified as "analytic" or "synthetic." For example, suppose that a rusting understructure causes a bridge to collapse. The sentence "The rusting of the iron understructure caused the bridge to collapse" would normally be regarded as synthetic, whereas "The cause of the bridge's collapse caused the bridge to collapse," would normally be regarded as analytic. Yet the causal relationship between rusting and collapsing is a feature of the world, not of either sentence.

However, Davidson does not believe that reason explanations can be understood as covering-law explanations that invoke an implicit law connecting *types of reasons* with *types of behavior*. Thus, suppose that Jason's running in the mara-

thon can be (truly) explained by his desire to prove his self-worth. Davidson does not deny that there is a causal law connecting Jason's reason (his desire to prove his self-worth) for running the marathon with his running, for he agrees with Hempel that "if *A* causes *B*, there must be descriptions of *A* and *B* which show that *A* and *B* fall under a law" (1980, 262), and he also agrees that explanations in terms of reasons are—if correct—causal explanations. However, he does say that we hardly ever, if ever, know what that empirical law is, and also argues that the (unknown) law does not have the form of a regular connection between a psychological cause and an action, such as "Whenever anyone wants to prove his worth, he runs a marathon." He argues further that no matter how carefully the circumstances surrounding, for example, someone's wanting to prove his worth and the opportunities to do so may be qualified, the resulting expression is not a psychophysical law.

Obviously it is not possible here to develop or to criticize Davidson's arguments with the care they deserve. It is worth pointing out, though, that whereas Davidson refutes a major premise of the interpretativists (i.e., that events cannot be causally related if descriptions of them are logically related), he seems to agree with Winch in saying that Mill was barking up the wrong tree in his search for explanatory *laws* that have the form of connecting mental causes with behavioral effects.

Davidson argues that explanations that cite reasons are informative because they tell us a lot about the individuals whose acts they are invoked to explain, rather than a lot about general connections between reasons and actions. For example, if the explanation of why Jason ran the marathon is correct, then we have achieved some understanding of what motivates him and how he expresses this. Furthermore, with this knowledge, we can make reliable conditional predictions of how he would behave in other circumstances.

Although Davidson says that his reflections reinforce Hempel's view "that reason explanations do not differ in their general logical character from explanation in physics or elsewhere" (1980, 274), his arguments against the possibility of covering laws that connect descriptions of psychological states and behavior in such explanations represent a significant departure from the tradition of Mill and Hempel. Although Davidson delineates the special character of reason explanations differently from the interpretativists, they can perhaps take some comfort in his recognition of the special or anomalous nature of explanations in terms of reasons.

4. The Existence of Appropriate Laws

Those who deny that there is a strong similarity between *reason* explanations of human action and explanations in physical science should remember that not all explanations in the social sciences appeal to laws connecting reasons (or dis-

positional properties such as fear) with actions. Consider, for example, the tentative laws concerning social structure proposed by G. P. Murdock (1949). These statements do not refer to beliefs or desires of any individuals. The generalizations relate various systems of kinship in different societies to differing forms of marriage, to patterns of postmarital residence, to rules of descent, and to forms of family. These generalizations are not causal, for they do not attempt to assign temporal priority, nor do they cite any mechanisms for the regularities they describe. In many cases, Murdock's generalizations state the coexistence of some rules with other rules, but they do not seem themselves to express norms or rules of any society. By analogy with some physical *structural* laws, such as "All copper conducts electricity," Murdock's proposed laws can also be classified as "structural laws." Other candidates for structural laws in the social sciences are the "law of evolutionary potential" (the more specialized the system, the less likely it is that evolution to the next stage will occur) proposed by Sahlins and Service (1960) and the "law of cultural diffusion" (the greater the distance between two groups in time and space, the more unlikely it is that diffusion will take place between them) (Sanders and Price 1968).

Murdock's proposed laws are generally regarded by anthropologists as problematic. He tried to verify his generalizations by using information that had been recorded by scores of anthropologists who differed widely in methods and theoretical presuppositions. As a result, serious questions can be raised about whether terms used to characterize the data on which Murdock based his generalizations were employed consistently. While anthropologists have justifiably criticized the design of Murdock's studies, there seems to be no reason in principle why structural generalizations in the social sciences cannot be framed and tested.

Not all social regularities that are well established are good candidates for *explanatory laws*. Some regularities raise more questions than they answer; they require rather than provide explanations. This may be true of Murdock's generalizations. Current examples of nonexplanatory generalizations are those puzzling but well-supported statements that describe patterned connections between birth order, sibling intelligence, and achievement. However, when correlations like these are strongly supported across many cultures, social scientists are stimulated to search for deeper regularities to explain them (Converse 1986). The deeper regularities, if discovered, might be causal laws or other structural laws. It is not clear in advance of their discovery whether these deeper generalizations will refer to any individual human reasons for acting.

In opposition to Geertz's pessimism about the ability of the generalizations framed by social scientists to "travel well," that is, to apply to situations other than those which gave rise to their formulation, Converse is optimistic. He believes that social scientists will eventually discover useful high-level generalizations. He says, on the basis of his own (admittedly limited) experience, that "we shall discover patterns of strong regularity, for which we are theoretically quite unpre-

pared, yet which reproduce themselves in surprising degree from world to world and hence urgently demand explanation" (1986, 58).

Anthropologist Melford Spiro also says that it is too early to abandon hope of finding any panhuman generalizations that are not either "false—because ethnocentric—or trivial and vacuous." He says:

> That any or all of the generalizations and theories of the social sciences (including anthropology) may be culture-bound is the rock upon which anthropology, conceived as a theoretical discipline, was founded. But the proper scholarly response to this healthy skepticism is not, surely, their a priori rejection, but rather the development of a research program for their empirical assessment (1986, 269).

Despite the optimism of Spiro and Converse about the future of the social sciences, neither they nor anyone else can deny the superior status of the laws now available to contemporary physical sciences. Two standard responses to the paucity of interesting, well-supported generalizations in the social sciences are that the data are far more intractible than in the physical sciences, and that we have not been trying long enough. Converse, for example, says that given the complexity of the data, "it would not surprise me if social science took five-hundred years to match the accomplishment of the first fifty years of physics" (1986, 48). However, Alasdair MacIntyre (1984), analyzes the situation rather differently.

First of all, MacIntyre claims that the salient fact about the social sciences is "the absence of the discovery of any law-like generalizations whatsoever" (1984, 88). MacIntyre thinks that this reflects a systematic misrepresentation of the aim and character of generalizations in the social sciences rather than a failure of social scientists. He says that although some "highly interesting" generalizations have been offered that are well supported by confirming instances, they all share features that distinguish them from *law-like* generalizations:

(1) They coexist in their disciplines with recognized counterexamples;
(2) They lack both universal quantifiers and scope modifiers (i.e., they contain unspecified *ceteris paribus* clauses);
(3) They do not entail any well-defined set of counterfactual conditionals (1984, 90–91).

MacIntyre presents four "typical" examples of generalization in the social sciences including Oscar Newman's generalization that "the crime rate rises in high-rise buildings with the height of a building up to a height of thirteen floors, but at more than thirteen floors levels off."

In response to a rather obvious objection to (1)—that most laws of social science are probabilistic rather than universal—MacIntyre replies that this misses the point. He says that probabilistic generalizations of natural science express

universal quantification over sets, not over individuals, so they are subject to refutation in "precisely the same way and to the same degree" as nonprobabilistic laws. He concludes from this that

> [W]e throw no light on the status of the characteristic generalizations of the social sciences by calling them probabilistic; for they are as different from the generalizations of statistical mechanics as they are from the generalizations of Newtonian mechanics or of the gas law equations. (1984, 91)

MacIntyre's position represents a serious misunderstanding of the nature of statistical laws. These laws do not state some universal generalization about sets, they state the *probability* (where this is greater than 0% and less than 100%) that different types of events will occur together. Of course, the probabilistic connections in the physical sciences are usually stated numerically, and in this they differ from many generalizations in the social sciences. But statistical generalizations need not be stated numerically; the probabilistic connection can be conveyed with such expressions as "usually" and "for the most part."

Moreover, statistical generalizations in physics, or in any other field, are not confirmed or disconfirmed in exactly the same way as universal generalizations. A universal generalization can be overthrown by a single genuine counterexample that cannot be accommodated by a suitable revision of auxiliary hypotheses. In contrast, *any* distribution in a given sample is compatible with a statistical generalization.

MacIntyre is correct in saying that statistical generalizations in social science are different from those in statistical mechanics. But this is because the probabilistic laws in statistical mechanics are based on a theory that is well tested and supported by far more elaborate and conclusive evidence than is presently available for any statistical generalizations in the social sciences. The retarded development of theories in the social sciences may be at least partly attributed to a scarcity of resources for investigation along with greater complexity of data.

MacIntyre's point about "scope modifiers" is one that has received much attention from those concerned to point out the differences between the social and physical sciences (e.g., Scriven 1959). In the physical sciences, the exact conditions under which the law is supposed to apply are presumably explicit, whereas in the social sciences, vague clauses specifying "under normal conditions" or some such equivalent, are substituted. This difference can be interpreted in several ways. MacIntyre regards the *ceteris paribus* clauses as required because of the ineliminability of *Fortuna*, or basic unpredictability, in human life. Hempel points out, however, the widespread use in physical science of *provisoes*, which is his term for assumptions "which are essential, but generally unstated presuppositions of theoretical inferences" (1988). Hempel supports his point with an example from the theory of magnetism:

> The theory clearly allows for the possibility that two bar magnets, suspended by fine threads close to each other at the same level, will not arrange themselves in a straight line; for example if a strong magnetic field of suitable direction should be present in addition, then the bars would orient themselves so as to be parallel to each other; similarly a strong air current would foil the prediction, and so forth.

Hempel says that the laws of magnetism neither state precisely how such conditions would interfere with the results, nor do they guarantee that such conditions will not occur. Yet such *ceteris paribus* clauses are surely implicit.

Ceteris paribus clauses sometimes result from inadequate information (the complexity of the data again) about the precise boundary conditions under which a given lawlike statement is applicable. *Fortuna* plays no role here. With *ceteris paribus* clauses, proposed laws can be stated tentatively, while research proceeds to attempt to sharpen and refine the spheres of application. As Converse points out (1986, 50), such tidying up occurs in the physical sciences as well, as shown by work that had to be done by astrophysicists as a result of information brought back from recent space explorations of distant planets. Whether or not *ceteris paribus* clauses are *stated*, then, rather than whether they are required, seems to distinguish the social sciences from the physical sciences.

MacIntyre's third point—the failure of generalizations in the social sciences to support counterfactuals—raises a complicated issue. One common way of attempting to distinguish genuine laws from "coincidental" generalizations (a thorny, and as yet unresolved problem) is by appealing to the former's ability to support counterfactuals. So, on this view, saying that a generalization cannot support counterfactuals is just another way of saying it is not a law. However, since we do not have any widely accepted account of what it is to support counterfactuals that is independent of our understanding of causal laws, it is not clear what MacIntyre's remarks about the inability of generalizations in the social sciences to support counterfactuals adds to his claim that these generalizations are not laws.

In any case, MacIntyre admits that the probabilistic laws of quantum mechanics do support counterfactuals. It is reasonable to claim that these laws also contain elements of "essential unpredictability," for the laws cannot predict the behavior of individual atoms or even sets of atoms.

Furthermore, if we do not equate the possibility of explanation with that of accurate prediction (and MacIntyre agrees that the strong symmetry thesis is indefensible) then essential unpredictability poses no barrier to lawful explanation in the social sciences.

Nothing that is said here supports the view that Oscar Newman's generalization about crime rates in high-rise dwellings, quoted above, is a genuine law or that it could play a role in a covering-law explanation. (However, it may have

considerable practical predictive value for those contemplating designs of housing projects.) As it stands, Newman's generalization states an interesting correlation. We want to know how well it stands up in new situations. More than that, even if it does not apply beyond the observed instances – if the generalization is no more than a summary – we want to know *why* the correlation exists for those instances. This is the kind of generalization that can lead us to form interesting and testable causal hypotheses about connections between criminal behavior and features of living situations. These can stimulate the acquisition of new data and further refinements of the hypotheses, or the formulation of additional hypotheses. Ultimately, this process could lead to the establishment of laws that are very different in form (not merely refined in terms of scope) from the generalization that initiated the inquiry.

5. Ethical Issues

Central to MacIntyre's discussion of the character of generalizations in social science is an attack on systems of bureaucratic managerial expertise. He says that those who aspire to this expertise misrepresent the character of generalizations in social science by presenting them as "laws" similar to laws of physical science. Social scientists do this, MacIntyre claims, to acquire and hang on to the power that goes along with knowledge of reliable predictive generalizations.

The ethical problems that trouble MacIntyre are of paramount importance to the critique of social science put forth by a group of philosophers, known as critical theorists, who are associated with the Frankfurt School. While the views of this group – which includes Horkheimer, Adorno, Marcuse, Habermas, Apel, and others – are not monolithic, certain themes are pervasive. These writers regard any attempt to model social science on the pattern of the physical sciences as both erroneous and immoral. Like the interpretativists, they believe that covering-law explanations in social science involve a fundamental confusion between natural (causal) laws and normative rules. In addition, they complain that explanations in the physical sciences are divorced from historical concerns, and that this cannot be so in the social sciences. Most critical theorists would agree, for example, with Gadamer's characterization of physical science: "It is the aim of science to so objectify experience that it no longer contains any historical element. The scientific experiment does this by its methodological procedure" (1975, 311, quoted in Grünbaum 1984, 16).

Grünbaum refutes this characterization, using examples from classical electrodynamics and other fields (1984, 17–19) to show that laws of physical sciences do embody historical and contextual features. Grünbaum also criticizes the attempts of critical theorists to argue that the historical elements in physical theories are not historical in the relevant sense (1984, 19–20), and points out that

Habermas bases this so-called lack of symmetry between physics and psychoanalysis on the platitude that Freudian narratives are *psychological* (1984, 21).

Karl-Otto Apel's defense of the asymmetry between history and physical science departs somewhat from the statements of Habermas that Grünbaum criticizes. It brings out, perhaps, more clearly the worry about loss of human autonomy that is the real concern of critical theorists:

> It is true, I think, that physics has to deal with irreversibility in the sense of the second principle of thermodynamics. . . . But, in this very sense of irreversibility, physics may suppose nature's being definitely determined concerning its future and thus having no history in a sense that would resist nomological objectification.
>
> Contrary to this, social science . . . must not only suppose irreversibility—in the sense of a statistically determined process—*but irreversibility, in the sense of the advance of human knowledge influencing the process of history in an irreversible manner.* (Apel 1979, 20, emphasis mine)

Apel then goes on to talk about the problem presented to social science (but not to physical science) by Merton's theorem concerning self-fulfilling and self-destroying prophecy.

Apel's remarks suggest that critical theorists' talk about the special "historic" character of social science, here and elsewhere, really amounts to the recognition that humans are often able to use their knowledge of what has happened to redirect the course of events, and to change what would have been otherwise had they not been aware of what was going on and had they not formed goals of their own. Since humans are agents with purposes, they enter into the molding of their own histories in a way not possible by any nonthinking part of nature.

Grünbaum does include examples of "feedback" systems in his account of how past states count in the determination of present behavior (1984, 19), but it is at least arguable that the concept of "purposive behavior" is not entirely captured in the descriptions of mechanical feedback systems (Taylor 1966). Apparently, the critical theorists use the term "history" in a special way to refer to accounts of autonomous human behavior. In this, they are similar to Collingwood, who does not apply the term to any processes of nature—even geological and evolutionary processes—that do not involve human intentions:

> The processes of nature can therefore be properly described as sequences of mere events, but those of history cannot. They are not processes of mere events but processes of actions, which have an inner side, consisting of processes of thought; and what the historian is looking for is these processes of thought. All history is the history of thought. (Collingwood 1946, 215)

In addition to using "history" in this special way, critical theorists are concerned that the "regularities" observed in our (corrupt) social system—that are the

result of unfortunate historical circumstances and that can be changed—are in danger of being presented by a nomothetic social science as exactly analogous to unchangeable laws of nature. The reaction of critical theorists to the attempt to discover laws and to construct nomological explanations in the *physical* sciences ranges from Horkheimer's acceptance of the goal to Marcuse's outright condemnation (see Lesnoff 1979, 98). Critical theorists, however, agree in rejecting nomological explanation in the social sciences.

Deductive-nomological explanations (covering-law explanations in which the laws are universal generalizations) are supposed to show that the event to be explained *resulted from* the particular circumstances, in accord with the relevant laws cited in an explanation of it (Hempel 1962, 10). Since the description of the event in a successful deductive-nomological explanation follows logically from the explanatory statements, it is plausible, using a *modal* conception of explanation, to say that, *given the circumstances and the operative laws, the event had to occur*. Leaving aside the point that in the social sciences explanations are much more likely to be probabilistic than deductive-nomological, the critical theorists, I believe, mistakenly read this feature of "necessity" in deductive-nomological explanations as an attempt to take what is the case (i.e., the event to be explained), and show that it *must be* the case, in the sense that the event was inevitable and could not have been otherwise, even if circumstances had been different. The mistake here is similar to the incorrect belief that any conclusion of a correct deductive argument is *necessary* just because the conclusion follows *necessarily* from the premises. If deductive-nomological explanation is misinterpreted in this way, it seems to present a challenge to humans' abilities to intervene and change circumstances.

However, such an understanding of scientific explanation is mistaken. A social science that is committed to providing scientific explanations is not thereby committed to serve the ends of regimes that want to maintain their dominance by making any existing social arrangements seem *necessary*.

In the same vein, critical theorists protest that scientific explanations are merely descriptions of the status quo, since scientific explanations fail to present a range of possible alternatives to what is in fact the case. However, it is not at all clear that explanations should tell us what could be or might have been; the goal seems rather to say *why* things are as they are. Understanding why things are as they are is, after all, often a prerequisite for changing the way things are.

In part, the complaint that nomological science is oriented only toward description rather than understanding or explanation is based on incorrectly identifying science with technology, and mistaking the goals of technology—prediction and control of the environment—for the goals of science. In the grips of this mistake some version of the following argument is adopted by critical theorists:

> Physical science aims only at prediction and control of the physical environment. Therefore, a social science that is similar to physical science in its methods and aims has as its goal the prediction and control of the behavior of other humans.

Such a science would be an inherently manipulative—and thus ethically unsavory—enterprise (Habermas 1984, 389).

The picture of science as mere technique—prediction and control—is obviously inadequate as well as somewhat at odds with the critical theorists' own view that science is committed to the status quo and insensitive to what might be. Manipulation is, after all, often directed toward other ends than the maintenance of the status quo.

We have already discussed the differences between explanation and prediction, and have rejected the strong symmetry thesis. We can sometimes reliably predict outcomes on the basis of regularities that give no understanding of the situation. We can also have significant understanding and be unable to use this knowledge for reliable prediction. It is difficult to make the case that all of science is directed toward prediction and control of the environment. Certainly those scientists who are engaged in pure research cannot always spell out immediate practical applications of that research when asked to do so.

If, as I believe, the critical theorists' assessment of the nature of physical science is grossly inaccurate, their ethical worries may nevertheless be well founded. For certainly predictive knowledge and control are highly valued byproducts of scientific knowledge. Furthermore, if scientists pretend to have the power of prediction and control when they do not, or if they capitalize on the laymen's respect for science to claim that scientific expertise grants them moral expertise as well, then they behave unethically.

It would be naïve to suppose that an increase in understanding is the only aim of scientists or even the chief aim of most scientists. Fame and money motivate scientists as they do all humans. It has been argued (Bourdieu 1975; Horton 1982) that the struggle for status rather than a pure concern for truth is dramatically more pronounced in the social sciences than in the physical sciences. Concern for status is often shown in attacks on credentials and other forms of name-calling. Horton recognizes these features in the conduct of social scientists and blames this behavior on the comparative lack of agreement about what constitutes normal social science, and consequently what counts as outstanding achievement. However, this cannot be the whole story, for among physical scientists as well, attempts to increase one's status by denigrating the credentials of others is all too common. *The Double Helix* (Watson 1968) shocked many nonscientists with this revelation, but it came as no surprise to those working in the field. The heated dispute about whether or not the impact of a comet caused the extinction of the

dinosaurs provides a current public example of name-calling among physical scientists that can hardly be overlooked by anyone who reads newspapers.

Social scientists are painfully aware that it would be a serious deception to put forth the present findings of their disciplines in the same light as well-founded physical theories. Unfortunately, overconfidence in and misuse of the predictive power of science is a feature of bureaucracy, as MacIntyre notes. However, bureaucratic overconfidence is not confined to the pronouncements of social scientists, as the investigation into the tragic failure of the space shuttle in 1986 attests. MacIntyre and the critical theorists do raise our awareness that such abuses occur, and that is helpful. But the occasional occurrence of abuses does not prove that a search for scientific laws and scientific explanations in physical science or in social science is unethical.

6. Conclusion

Hempel's account of covering-law explanation in the social sciences, which is similar to his account of explanation in the physical sciences, was chosen because of its clarity and importance as a point of departure for discussion of contemporary views of explanation in the social sciences. Responses to Hempel's models of explanation by interpretativists, nomological skeptics, and critical theorists were presented and criticized.

From the array of accounts of scientific explanation presented in this volume, it should be apparent that no consensus about these matters exists or is likely to be reached any time soon. In the absence of a completely acceptable account of scientific explanation, we have only approximations. Yet, despite protests of the critics of causal and nomological explanation in the social sciences, the best approximations to a satisfactory philosophical theory of explanation seem to embrace successful explanations in the social sciences as well as successful explanations in the physical sciences. None of the critics, I believe, has demonstrated that the admitted differences between our social environment and our physical environment compel us to seek entirely different methods of understanding each.

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