CHAPTER FIVE

General Laws and Explaining Human Behavior

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I

In this paper I argue for three major theses that are often thought to be antithetical to one another. The three theses are: first, that explanations of human behavior in terms of its reasons (beliefs, desires, motives, goals) rest upon general laws because such explanations are causal in nature; second, that it is extremely unlikely that these general laws are statable in the intentionalist vocabulary of the social sciences; and third, that the social sciences must be genuinely theoretical if they are to be at all viable.

My purpose in this is, in the first instance, to present a model of the role general laws and what I shall call causal generalizations have in the explanation of human behavior. But there is a second, larger purpose as well, namely, to outline a picture of social-scientific theory. Briefly stated, I hope to show that, although general laws properly so called will not emerge from the social sciences, a certain sort of genuinely theoretical science of human behavior is still possible. The "sort" I have in mind is what is sometimes called Critical Theory. The paper is ultimately concerned to show, therefore, that a proper understanding of the nature and basis of the causal explanations of human behavior leads to a critical metatheoretic conception of social-scientific theory.

II

There is a widespread belief among certain (mostly post-Wittgensteinian) philosophers in what I shall call the "singularity thesis of human action."[1] According to this thesis, reason-explanations can ac-

count for human action without invoking or presupposing any general law; in the words of Hart and Honore, this thesis consists of the claim that, "The statement that a given person acted for a given reason does not require for its defense generalizations asserting connections between types of events."[2] This thesis, if it were true, would have profound consequences for any science of action, for it would mean that the explanations in this science would be particularistic, and it would mean that such a science could not be genuinely theoretical.[3] Instead, the social sciences involved in explaining action would be confined to elaborating the character of the particular reasoning process that results in the performance of certain historically located events: They would be backward looking, ad hoc, and ideographic.

I wish to begin by examining what I take to be the two most compelling arguments for this belief, and to show why I think they are mistaken. These two arguments might be called the "logical-connection argument" and the "essential-nature argument." My aim in doing this is to demonstrate, in an indirect manner, that reason-explanations do rest on general laws.

One major support for the singularity thesis is the claim that the connection between that which explains an action and the action itself is a logical one, and that therefore this connection is both intuitively clear and qualitatively different from the relationship which exists between events which figure in causal explanations.

Thus, William Dray maintains that reason-explanations invoke principles of action (as opposed to empirical laws) to explain human behavior, and that the relationship between principles and their outcomes is not essentially one of a recurring pattern, but is rather one in which the outcome (in this case the action) is intrinsically (logically, conceptually) connected with the principle itself. Reasons give the grounds for which the action is a consequent, and, since the relationship between ground and consequent is logical rather than empirical, he argues that reason-explanations do not require general statements linking a kind of reason with a kind of action.[4]

To take a concrete case. Alasdair MacIntyre, in his well-known (though now self-repudiated) article, "A Mistake about Causality in Social Science,"[5] analyzed Max Weber's explanation of the rise of capitalist behavior in terms of certain theological beliefs of Protestants. He concluded that, just because the connection between these beliefs and actions is a conceptual one, Weber's tactic of supporting his thesis by embedding it in large-scale historical generalizations was irrelevant; as MacIntyre wrote:

> The use of Mill's methods is entirely out of place; we do not need to juggle with causal alternatives. India and China do not strengthen and could not have weakened his case about Europe. For it is not a question of whether there is a purely contingent relationship between isolable phenomena. And so constant conjunction is neither here or there.[6]

Now, in this argument I wish to support Weber against MacIntyre, for I want to maintain that explanations of particular kinds of action in terms of particular sorts of reasons do in fact rest on at least implicit general laws.

The crucial mistake in the logical-connection argument lies in its account of the way reason-explanations account for an action. According to it, explaining an action involves specifying the reasons that rationalize it, i.e., that show it to be the appropriate thing for the agent to have done, given his situation. However, this account is inadequate because it does not distinguish between those beliefs and desires that are a reason for the behavior but that did not cause it to happen ("a" reason for doing it) from those which in fact were responsible for its occurrence ("the" reason for doing it).

The distinction that is relevant here is between acting *and* having a reason and acting *because* of that reason. In the former case an agent may have a reason for his behavior, and it may therefore have been a rational and justifiable thing to have done. But unless the having of this reason was the cause of the agent's acting as he did, the reason does not explain the act, i.e., it does not show that the act occurred because the agent had the specified reason.

Broadly speaking, reason-explanations succeed in explaining when they show that it was because the agent thought that the act was the appropriate way to achieve his ends that he acted as he did. In other words, it isn't the reason that explains the act, but rather the agent's *having* this reason, and this having caused him to act in the way he did, that explains it. In another article, I have described this by saying that we explain the behavior in question in discovering the agent's practical reasoning processes that brought it about.[7]

If explaining an act by means of a reason-explanation is knowing the reasoning process which caused it, then I think it can be demonstrated that such explanations implicitly rest on general laws. Such a demonstration is a simple matter if one adopts a broadly Humean construal of causality. It is much more complex if one subscribes to a

broadly realist construal of causal explanation. I will discuss each of these in turn.

On a broadly Humean construal of causality, a claim that x causes y involves the claim, among others, that x is regularly related to y. Thus, to employ Mill's account of the regularity thesis, to say that x causes y is to say that x is a sufficient condition of y's occurring, and/or that it is a necessary condition as well. But relations of necessary and sufficient conditionship obviously rest on general laws. For any claim that x causes y in the sense that x is a necessary and/or sufficient condition of y involves the assumption that whenever x occurs y will occur, ceteris paribus (otherwise x cannot be a sufficient condition of y), or that whenever x does not occur y will not occur, ceteris paribus (otherwise x cannot be a necessary condition of y), or both.

Given a realist construal, the matter is somewhat different.[8] According to the realist, providing a causal explanation of the form "x causes y" is to relate x and y by means of an actual mechanism which, in suitable conditions captured by "x," generates the observed outcomes described as "y." (The realist doesn't intend anything specifically mechanical by the term "mechanism." Thus, a practical reasoning process could itself be such a mechanism. Indeed, my account of reason-explanations as causal in form is broadly realist in that, on my reading, the reasons of the actor are connected to his behavior by means of real psychological reasoning processes.) In the realist view, causal explanations may start with an observed regularity between x and y, but this regularity is only evidence that a causal relationship actually exists. Only when the underlying mechanism that has certain natural powers is discovered can a genuine causal explanation be said to have been given. It follows from this, according to the realist, that causal explanations do not therefore rest on general laws.

But does this follow? I think not.[9] The reason why is that the realist's account of causal explanation surreptitiously smuggles Humean regularities back in on another level. The realist invokes a basic mechanism as a way of explaining why a particular sort of event will occur in certain circumstances. This mechanism is meant to have a particular nature such that, subject to conditions of an appropriate kind, it will perform in a specific manner. But all of this presupposes that there is a regular operation of the mechanism, that under certain circumstances the mechanism will act in a predictable manner. If this were not the case, then invoking the mechanism would not be genuinely explanatory, because then one would need to know why the mechanism worked as it did to produce the effect y in the case at hand. It is only because

the notion of a causal mechanism carries with it the backing of a general law that such a mechanism can be a relevant part of a causal explanation.

To this the realist has an answer. He will claim that knowledge of the underlying mechanism does not consist of general laws that supposedly govern its operation; rather, such knowledge consists of knowing the "nature" or "essence" of the mechanism in question, and this includes knowing the powers that it has. This response is thus a variant of the second major argument which supports the singularity thesis, namely the essential-nature argument.

I will turn almost immediately to the essential-nature argument, but before I do so I want to point out that no matter how one interprets either the Humean or the realist construal of causal explanation, both accounts agree as to the relevance of generalizations in the assessment of causal explanations. In the case of the Humean, this is so because causal explanations just are generalizations of a certain sort. In the case of the realist, this is so because generalizations indicate that deeper causal mechanisms are at work. Thus, insofar as reason-explanations are a type of causal explanation, generalizations linking particular beliefs and desires with particular actions will be relevant in determining the worth of the explanation at hand. This is directly contrary to the singularity thesis.

Thus, to return to the example of Weber, it may well be the case that even though the matrix of beliefs, desires, values, and so on associated with Calvinism is an initially appealing explanation because it rationalizes capitalist behavior, and because it invokes elements in the experience and thought of a group of people that appear to be crucial motivational factors in their lives, it is indeed false. For it may well be the case either that many of the particular Protestants who did possess the relevant beliefs and desires nevertheless did not in fact act in the way that Weber thought they did, and/or that many people acted in a capitalist manner who were not in fact Protestants. If this turned out to be so, then Weber would have had to reject his interpretation as the explanation of capitalist behavior because he would now be in possession of evidence which indicated that "a" reason for the puzzling behavior was not, upon empirical investigation, "the" reason why people behaved as they did.[10]

So far I have argued that reason-explanations are causal in nature, and that on at least one construal (the Humean) of causal explanation this means that reason-explanations rest on general laws. I have also argued that another construal of causal explanation (the realist), though

apparently not nomological, actually is so at a deeper level. Insofar as these arguments are right, the singularity thesis cannot be correct. However, I did allow that the realist could salvage his case by invoking the essential-nature argument, and I said earlier that this argument was itself one of the most important supports of the singularity thesis. I must, therefore, consider this argument.

The essential-nature argument amounts to the claim that good explanations are those which ultimately rest on an account of the nature of the basic entities involved. Knowing that the essence of an entity is to act in a certain manner means that the operation of this entity does not require further explanation in terms of some general law under which one could subsume its fundamental dispositions. Thus, to turn to the case of a practical reasoning process, the essential-nature argument says that, because it is part of the very nature of such a process to result in an action, the relationship between coming to have a reason and the action which this event explains is immediately apprehensible without recourse to any generalizations. We don't need to see the occurrence of these two events as instances of some generally recurring pattern in order for the occurrence of one to explain the occurrence of the other once we know what a practical reasoning process is. Thus, for example, when one is told that a person crossed the street because he inferred from his belief that this was the only way to buy cigarettes, and from his desire to have a smoke now, that he ought to cross the street, this explanation appears satisfying in itself: One doesn't need any further information to the effect that the person was of type x, and type x people engaged in type y reasoning act in z type way, because one can understand the relationship between the reasoning process and the act immediately.

Now it *is* a fact that in explaining an action we grasp a connection between a singular *explanans* and a singular *explanandum* such that we do not feel the need to subsume them under some general statement. But this is not because reason-explanations are not causal in form (as the logical-connection argument would have it), nor because the causal powers of the mechanism involved in producing the act involve some sort of "natural necessity" that does not admit or require further explanation in terms of a general law (as the realist would have it). *The reason is that reasoning processes are partially defined in functional terms.* One doesn't feel the need for general laws when explaining act z by the agent's desire for y and his belief that his doing z is the best means for achieving y, any more than we feel the need for a general law when we explain why a person feels relaxed by the fact that he

ingested a tranquilizer. In neither case do we feel the further need for a general statement which supports the particular causal explanation, because the disposition to produce certain sorts of outcomes is built right into the concept "desire" and "practical reasoning process" just as it is built into the concept "tranquilizer." In both sorts of cases, discovering that the cause of an event is another event or object which is identified as one which characteristically produces events of the first type is (psychologically) satisfying in itself as an explanation.

Indeed, it is because reasoning processes (and poisons and tranquilizers) are entities specified at least partially in functional terms that scientists are generally interested in those cases in which they *don't* operate, i.e., when their causal force is defeated by external circumstances. In these cases, what is sought is some law which states that when a particular set of (necessary) conditions is not present, then the expected causal force of the entity will be inoperative. In other words, with functionally defined causes the general laws which are usually sought are those which explain their *breakdown* rather than their operation, just because in "normal" circumstances the entity's being of a certain kind is a sufficient condition for the occurrence of a certain event.

But it certainly does not follow from this that in the normal cases in which the appropriate effect is forthcoming the relationship between the explanans event and the explanandum event does not presuppose a general law to the effect that under normal circumstances these kinds of events are related in a specified way. On the contrary, as I argued above when I showed that the realist smuggles Humean regularities into his account of causality, it presupposes just this sort of statement. For to characterize entities functionally is partly to characterize them in terms of the general causal outcomes that they will produce.

Moreover, it is a deep mistake to think with the realists that explanations in terms of "basic nature" and "causal power" are as deep as science can go. For one of the ways that science progresses is specifying in more detailed and sharply defined terms under just what circumstances these sorts of events are related. Furthermore, in this process of articulating the general laws which govern the relations between functionally defined entities and certain events, it is quite often the case—contrary to Charles Taylor's thesis of asymmetrical explanation[11]—that the scientist will try to (causally) explain just why it is that a certain entity ordinarily produces the effects that it does.

Thus, while a relaxed condition is a natural and in some sense privileged outcome of a person's taking a tranquilizer—in Taylor's words,

there is a certain "bent or pressure of events towards a certain consummation," an outcome which "does not come about by 'accident' but is somehow part of its 'essential nature'"[12]—the neurophysiologist will certainly try to explain why this is the case by investigating the neurochemical processes through which the tranquilizer causes this result. And in the same way, a psychologist may well try to explain why it is that people of a certain sort who engage in particular reasoning processes will ordinarily or "naturally" behave in a certain manner, by referring, for example, to their schedules of reinforcement or to the development of their mental capacities. The reason for this *further* level of explanation is not only the commonplace one that science seeks to include phenomena in a wider and wider range explainable by a smaller and smaller number of principles, but as well the more pertinent one that explanations in terms of functionally characterized entities tend toward the vacuous (á la the explanation of Molière's opium in terms of its dormitive powers).

Thus, though it is often the case that we feel satisfied with an explanation of an action which specifies the particular desires and beliefs which brought it about, and that we feel it is unnecessary to invoke a general law in virtue of which the particulars of our explanation would be seen to be instances of a generally recurring pattern, these psychological facts should not blind us to the logic of the situation. For these psychological facts are rooted in the peculiar feature of our characterizations of the mental events which cause actions, namely, that they are partially functional characterizations; and functionally characterized events are so characterizable just because we believe that they regularly produce certain outcomes, and thus that some general law involving their description is in the offing. This is why singular explanations which invoke functionally characterized events do not seem to require subsumption—the general law is implicitly brought into the situation in the very meaning of the description of the particular causal event. Moreover, it should not be forgotten that although we often do not seek to elaborate these general laws, social scientists must try to discover the larger causal patterns in virtue of which these conditions hold.[13]

If what I have argued so far is correct, then two conclusions can be drawn. The first is that reason-explanations account for actions by seeing them as the causal outcome of certain mental events, namely, practical reasoning processes. The second is that because they are causal and because causal explanations are essentially nomological, reason-explanations necessarily rest on general laws, at least implicitly.

However, there is a glaring fact regarding the explanations of action that seems to conflict sharply with my whole analysis of the nomological foundation of reason-explanations. The fact is that we *do* presently have reason-explanations for all sorts of actions, but we do *not* have available to us any general laws properly so called which link the having of certain reasons with the performing of certain actions—indeed, we are far more certain of singular causal connections than we are of any putative law governing the cases in which we assert their existence. (Thus, for example, Weber's causal generalizations are not general laws.[14]) This fact lends credence to the singularity thesis, and it seems to undermine the nomological thesis that I am supporting.

However, the drawing of such a conclusion because of this fact would be a result of a deep misunderstanding of the nature of the nomological thesis.[15] For this thesis does not consist of the claim that for every particular causal explanation there is ready at hand a general law under which it can be subsumed; indeed, the thesis does *not* entail even that it be known what form the relevant general law would take if it were statable. All that the nomological thesis asserts is that there is *a* general law under which the events invoked in a causal explanation fall.

There are three important ways in which it can be seen that this is so. In the first place, there are many cases in which the claim that "x causes y," or even that "x's cause y's" is true, and yet the general laws under which such claims are subsumed involve no use of x and y at all. In such cases the events which we initially described as x and y are redescribed by means of a and b, and only then are they linkable by means of a general law. It is quite consistent with the nomological thesis that the general law which figures in a causal explanation be formulated in terms quite unlike those used to assert a particular causal connection or even a particular type of causal connection.

Indeed, it is normally the case that scientists have had to redescribe events which they believed (correctly) to be causally related in order to be able to formulate the general laws which govern them. Thus, for example, it was necessary to redescribe the event type originally described as "the production of warmth" as "the increase in molecular motion" in order to generate the causal laws governing heat.

In this discussion, an extremely important, if a relatively obvious, point to remember is that phenomena as such are never explained, *but only phenomena as described in some way*. And it is also important to remember that there may be any number of different descriptions of the same phenomenon. By keeping in mind these two considerations, one can easily see how events that are described in one set of terms,

and related to one another by means of these terms, may well be redescribed in conceptually quite dissimilar terms from those employed in the original description, and, as a result, only then be able to be seen as part of a generally recurring pattern of events.

Of course, in order for this to happen there must be a specific kind of relationship between the terms describing the events in question. In the first place, there must either be an equivalence in their extension, or at least the extension of the first term must be a subset of the extension of the second, redescribing term. In the second place, the redescribing terms must figure in a more comprehensive theoretical scheme that allows one to understand why the event as initially described could have the causal power ascribed to it, and that gives one the capability of articulating more general and more precise formulations of the causal relationships involved.

I should mention in passing that the sorts of considerations I have been discussing are particularly apt in the context of the philosophy of social science, since there seems to be a widespread belief among a number of its practitioners from quite divergent perspectives in what I call "the doctrine of superficial generalization." This doctrine holds that if one claims one event is the cause of another event, one is thereby claiming that the law upon which this explanation rests will consist of the very same terms as used in the particular descriptions of these singular events. Hart and Honore appear to be holders of this doctrine when they write: "To make such a singular causal statement is therefore to claim that the events which it relates are instances of such a universal connection between types of events."[16] And an instance of this doctrine can be found in Hempel's famous article, "The Function of General Laws in History"; there, in trying to demonstrate that the explanation of particular historical events requires a covering law, he writes:

> Now the assertion that a set of events . . . have caused the event to be explained, amounts to the statement that, according to certain general laws, a set of events *of the kinds mentioned* is regularly accompanied by an event (of the kind for which an explanation is sought).

And then he says by way of example:

> Consider, for example, the statement that the Dust Bowl farmers migrated to California "because" continual drought and sandstorms made their existence increasingly precarious, and because California

> seemed to them to offer so much better living conditions. *This explanation rests on some such universal hypothesis as that populations will tend to migrate to regions which offer better living conditions.*[17]

Here the law which Hempel adduces is simply a more general version of the singular explanatory statement itself.[18]

This doctrine has often been responsible for objections to the nomological thesis on the grounds that social scientists are quite often willing to accept a singular explanatory statement which asserts a causal relationship between two events, and yet to deny the truth of any putative law or causal generalization formulated by using the same terms found in the singular explanation.[19] Thus, no matter how Hempel formulates his "universal hypothesis," it seems extremely implausible that we would be willing to accept it, even though his particular causal explanation seems evidently to be true. And this wedge between the particular and the general is supposed to demonstrate that the nomological thesis is false.

But it shows no such thing. For this sort of objection is rooted in the mistaken assumption that the nomological thesis consists of the claim that the laws which a particular explanation instantiates will be formulated in the same sorts of terms as those to be found in the descriptions of the particular case. In fact, however, the nomological thesis only asserts that there must be a covering law in order for a singular causal statement to be true; and it is quite in keeping with this that the actual laws that do cover these instances will be formulated in terms other than those found in the particular explanation.

The second important way in which it can be seen that a holder of the nomological thesis is not committed to the truth of any available general law, even though he is willing to assert the truth of some singular causal explanations, is to see that it is perfectly consistent with this thesis that there not be available a law under which a true particular causal statement is subsumable. For, as I have already had occasion to mention, all that the thesis maintains is that there is a law; but it does not follow from this that this law be currently known. Thus, for example, it is perfectly consistent for someone to claim that smoking causes lung cancer (under certain circumstances), to believe that such a claim rests on a general law which links together the two events now described as "smoking" and "the development of lung cancer," and yet to admit that as of this moment no such law exists. Indeed, it is probably the case that a majority of those causal ascriptions

which both ordinary people and natural scientists currently make are not supported by fully adequate general laws. And thus it is no argument against the relevance of the nomological thesis for human behavior that although we are willing to believe a whole array of causal statements linking motives, beliefs, desires, and values with actions, we cannot provide a genuine general law under which they are subsumable.

Of course (as the example of smoking clearly shows), it does not follow from this that generalized statements and a whole range of empirical evidence are not therefore relevant to our making singular causal ascriptions. On the contrary, in order to provide an adequate causal explanation, we must have good evidence for believing that full-fledged causal laws which cover the relevant events actually exist. I will take this up again in Section IV; at this point I just wish to forestall a possible misinterpretation to the effect that, since the nomological thesis apparently does not require that there actually exists a formulated general law for every (true) causal explanation, it allows us to dispense with the need for generalizations of any sort.

The third important way in which it can be seen that the nomological thesis is consistent with the glaring fact that we presently do have reason-type causal explanations (some of which are undoubtedly true), but that we do not have available to us any general laws properly so called, is really only a product of the first two ways. This is that it is certainly possible for someone to be a proponent of the nomological thesis and at the same time believe that the general laws under which his singular causal explanations are subsumable will not be formulated in the same terms as those found in the causal explanations he presently gives.

Thus, for example, a historian may assert that soil erosion and the decline of agricultural production in a particular area are causally related, and he may assert this even though he also believes that it is impossible to formulate the laws which link these events by using such a gross term as "soil erosion." In fact, he may even believe that the event which he now describes as soil erosion will have to be redescribed in terms of a radically different sort before the appropriate law could be forthcoming—for example, it may be that he will have to introduce quite determinate physical concepts drawn from chemistry, such as the relative amounts of nitrogen, etc.

To borrow a distinction from Davidson,[20] the generalizations we have may be either *homonomic* or *heteronomic*. Homonomic generalizations are those whose positive instances give us reason to believe that the form and vocabulary of the finished law will be of the same type as

the generalizations themselves; heteronomic generalizations are those which lead us to believe that the precise law at work can only be stated by switching to a different vocabulary altogether. Thus, in terms of our example of soil erosion, this causal explanation involves a heteronomic generalization.

The important point about heteronomic generalizations is that they allow for the possibility of one's developing a whole range of causal stories without at the same time committing oneself to the belief that the general laws which underlie these stories will be formulated in the same terms that one is currently employing. We need not wait on the actual development of the relevant scientific theories before offering causal explanations of the events around us. And we may offer these explanations even though we might well expect that ultimately the laws which govern the phenomena involved will be expressed in a radically different terminology from what we currently employ.

With this understanding of the nomological thesis—and particularly the possibility of heteronomic generalizations—I wish to turn to the social sciences that explain human behavior, with an eye toward understanding the nature of the causal explanations which they can and do offer. In particular, I want to examine whether it isn't the case that the causal generalizations found in these social sciences aren't heteronomic. For if this is the case, it will point to a deep difference between the sciences of intentional action and the sciences of nature.

III

In this section I hope to demonstrate that there is a good reason to believe that the laws which underlie the causal processes of mental events that bring about actions will not be forthcoming at the level of discourse that social scientists use to describe and explain actions, namely, intentional discourse. I want to show that the generalizations they employ possess features which make them unusable in highly deterministic theories, and which make them incapable of being indefinitely refined so that they might become so usable; that is, that they are heteronomic generalizations. In the social sciences there are genuine causal explanations rooted in genuine causal generalizations about how certain kinds of people think and act in certain sorts of circumstances; but these generalizations are *not* genuine laws, nor is it at all probable that they ever will be purified into general laws properly so called.

In order to see why this is the case it is crucial to understand that the identity of intentional objects and events described as such is partially a function of the propositional attitudes which they embody. Another way of putting this is that intentional objects are what they are partially in virtue of their content, i.e., in virtue of the ideas they instantiate. Thus, for example, an arm extended from the window of a turning car is an act of signalling partly because of the beliefs and desires which it expresses. This means that the very identity of human actions, institutions, and psychological states is partially determined by the conceptual distinctions on which they rest. We might call these conceptual distinctions the "constitutive meanings" of an intentional object.

As an example of this from social science, take the nature of the political realm. What politics is in a given social setting (i.e., what the nature and function of government is; what political power is; what political relationships consist of; and so on) depends to a large extent upon the ideas which the actors themselves hold, at least implicitly. Thus, the nature of political behavior can be a profoundly different thing depending on whether one is referring to the political activity in an African tribe, or the ancient *polis*, or Elizabethan England, or twentieth-century America; and the reason for this is that the political realm in each of these societies is rooted in fundamentally different constitutive meanings. This is, of course, a fact well recognized by social scientists; and a book like Samuel Beer's *Modern British Politics* is an exceptionally good one in showing how the periods in the political life of modern Britain are in some sense discontinuous partially because each of them has been structured around different sets of beliefs about the nature of the political.

The situation is similar for the mental phenomena which figure in the explanations of social behavior. Mental states and events are representational states and, as such, are what they are by virtue of what they are about, i.e., their contents. Thus, one belief is distinguished from another by specifying the content of each belief—the belief that it is cold outside is distinguished from the belief that it is warm outside by indicating *what* a person is committed to by virtue of having either of these beliefs. And the same is true for desires, motives, perceptions, and the like.

Now, an extremely important fact is that the self-understandings which constitute social and psychological objects and events are inherently historical because they are subject to the constant change resulting from the various conceptual innovations which a group's

members introduce and come to accept.[21] These conceptual innovations assume a bewildering number of types and forms, and they may arise from any number of sources both internal and external to a given social group. (Of course it is true that rates of conceptual change may vary widely, that there are some kinds of societies which are more resistant to such changes than others, and that there are even some that institutionalize means by which such changes can be prevented. But *all* societies, even the most closed and isolated, are subject to the developments of thought occasioned by changes in what must be done to survive and prosper, by the shifting of relationships within the social whole, by contact with foreign groups, and by the widespread tendency of humans to ask further and further questions about their world.)

Thus, to return to our example of the political realm, starting in the seventeenth century in Western Europe, people gradually came to understand themselves and others as the possessors of individual rights. Now, this new self-understanding marks the occurrence of a real conceptual innovation that not only changed the way people—both theorists and laymen alike—talked *about* their political relationships and institutions, it also altered the very nature *of* these relationships and institutions as well. For example, governments had limits placed on their activity which they never had before, and they had duties to perform—such as defending the civil liberties of their citizens—that were essentially new. Such changes are themselves only moments in a continually evolving historical process in which the ideas that form the social space of people's lives alter and shift and combine in novel ways.

In fact, the kinds of historical changes that I am discussing are not merely accidental ones in human life (in the way in which a change in the average height of humans is accidental). For human beings are self-educable creatures capable of transforming the social and natural settings in which they live, and themselves in the process. That is, it is one of the distinguishing features of humans that they reflect on their experience and, within a certain range, alter the forms of this experience as a result of this reflection. Human life is essentially historical, not because changes in how it is lived have occurred, but because parts of these changes have been authored by the participants themselves in this historical process. I shall return to this point at the end of the paper, for it will serve as the foundation of my remarks on the critical theoretical character of social science.

The crucial point in all of this for my purposes lies in the pragmatic epistemic unpredictability of these sorts of conceptual innovations.[22] In a very well-known argument, Maurice Cranston has shown that it is

logically impossible for anyone to make predictions about the occurrence of conceptual innovations,[23] and though this is *not* the argument I wish to make here (for reasons which will become clear in a moment), a brief examination of it will be useful in order to bring out what would be involved in making predictions about the future course of human beliefs.

Stated simply, Cranston's argument is that in order to predict an invention (whether conceptual or mechanical is immaterial) one would already have to be in possession of it; but if this is the case, then one couldn't be said to predict its novel appearance at some time in the future. The example he employs is the corkscrew, which he supposes to have been invented in 1650. In order to be able to predict at 1650 - n that in 1650 the corkscrew would be invented, the predictor would have to know at 1650 - n what a corkscrew is; but if this were the case, then he himself would be the inventor of this gadget, not the poor fellow in 1650. Trying to predict the discovery of an invention puts the predictor in the self-contradictory position of predicting at time $t - n$ the invention at t of a device that he himself had known of at $t - n$!

The same situation would exist in the case of a predictor trying to forecast in 1890 that Albert Einstein would originate the Special Theory of Relativity in 1905. For in order to be able to do so, the predictor would have to know at least roughly the contents of the Special Theory, and this means that Einstein could not have been the discoverer of the theory. Once again, the predictor would be in the logical bind of predicting the creation in the future of something already in existence.

However, while this argument makes a clever logical point, it really is of limited interest. For there is nothing in the argument which makes it a logical impossibility for someone to predict that at a future date a particular object will be fashioned or a particular theory will be formulated. All that Cranston's argument rules out is the possibility of predicting the discovery of a *novel* or *original* theory or invention; it is against the possibility of predicting an event described as the *first* of its kind that his argument is telling. Even if a predictor knew the Special Theory of Relativity in 1890, for all of Cranston's argument there would be nothing contradictory in his predicting that Einstein would produce such a theory in 1905.

Nevertheless, the main thrust of this argument is useful because it demonstrates what would be involved in predicting the future course of human thought. For what the argument does reveal is that in order to accomplish such a task one would have to be able oneself to make

all the creative leaps that will recur later at some specified time. In order to have predicted in 1850 that the General Theory of Employment, Interest and Money would be proposed by Keynes in 1935, a scientist would have already had to have formulated the rough outlines of the theory himself.

And while there is nothing about such an event which would make it a priori impossible, from a pragmatic point of view such a Herculean effort is extremely unlikely. This is especially so for innovations which themselves depend on a whole range of other innovations, or for innovations which involve fundamental alterations in the basic theories and principles which underlie the broad mass of our knowledge. To predict in 1600 the emergence of Marx's social theory would require that the predictor be in possession of concepts which themselves depended on developments in philosophy ("dialectic," "alienation"), economics ("capital," "commodity"), sociology ("civil society," "bureaucracy"), and a whole range of other areas of thought, and so it would be necessary for him to elaborate these conceptual distinctions and intellectual strategies in order to predict for any distance into the future a social theory of the complexity of Marx's.

Moreover, the unlikeliness of such a situation is not based solely on the limits of the human mind; there is another reason such a situation is almost unthinkable. It is that, as Popper has forcefully pointed out,[24] successful prediction is only possible when one is dealing with a closed system, that is, with a system which is protected from external influences that would tend to upset the regular interaction of members in the system. All scientific predictions take the form, "if C, then E, in situation X, ceteris paribus; but C in situation X, ceteris paribus; therefore E;" and they are applicable to real situations only when the ceteris paribus clause has been satisfied, which is to say, when no untoward event occurs to upset the relationship between C and E.

But the collection of individuals who comprise a given social group (say, all atomic physicists, or all the members of the Ndembu tribe) is a most unlikely candidate to be a closed system. The possible influences on the minds of people are practically innumerable, and the amount and intensity of interaction between such collections so great, that the idea that a human group might be isolated enough so that a scientific prediction about its conceptual developments might be forthcoming sounds like a mad millenarian dream.

The difficulties involved in predicting conceptual developments are enormous; indeed, from a practical standpoint, it may be taken as a given that such predictions are so unlikely as to be almost certainly

not realizable. And this means that even if it were the case that the course of human thought is law governed in some fully deterministic way there is a *pragmatic* epistemic unpredictability about the development of human thought.

Moreover, it should be clear from what I said about the constitutive role that concepts play in human actions and institutions that this unpredictability is not confined to the history of human *thought*; human actions and institutions are also unpredictable insofar as they change as a result of people's coming to think of themselves, each other, and the natural world in novel ways. Thus, as the lessons of Keynesian theory came to be appreciated first by the leaders of government and industry, and then in an attenuated form by the populace at large, new demands on the government were made and were seen to be justifiable, and a whole new class of laws, regulations, institutions, and practices emerged. As a result, the very nature of the relationship between the government and its citizens changed.

The conclusion to be drawn from all of this is that the objects of social science are open-ended in a practically unpredictable way. Social institutions and practices, as well as the beliefs and desires of the members of particular social groups, are continually in a state of flux and evolution which will always appear to be indeterminate to those who wish to study them. To understand what this actually means, it might be useful to draw an analogy suggested by Alasdair MacIntyre[25] to some imagined geologist's attempting to study rocks which changed their shapes, sizes, colors, and chemical compositions in a manner which (even though lawful) always eluded his predictive capacity. Retrospectively he would be able to understand why it was that a class of rocks assumed the form that it did; but prospectively he would be unable to know what form it will take: The objects of his research would be constantly changing in surprising ways. Now, this imagined situation of the geologist is like the real situation of the social scientist interested in explaining intentional behavior, just because all such behavior is what it is by virtue of its place in a social community, and because the life history of social communities is constantly changing in unpredictable ways.

What does all of this have to do with the heteronomic character of the generalizations in a science of action? Just this. In order to frame general laws properly so called, it is necessary that one use concepts which refer to objects which are in some sort of steady state, or which change in some regular way which is apprehensible. The reason for this is that general laws are universal well-confirmed empirical hypotheses

which state that under situations X, if a then b, ceteris paribus; but if there is no way of describing an object or event so that it can be seen to be part of a regularly recurring sequence of events, then there is no way that one can know whether one's hypotheses are either universal or well-confirmed enough in order to be accorded the title of "general law."

It is as if the objects in the world will not stay still enough, or evolve in a predictable enough fashion, so that one can pick them out as part of a genuine natural pattern. To return to the example of the geologist and his rocks, if the objects to which his terms "rocks of type a" and "rocks of type b" refer are forever shifting in unpredictable ways, there is no way that the geologist could frame a general law using the concepts "rocks of type a" and "rocks of type b"; this is because there would simply be no way to know whether the purported general law which resulted by using such concepts held or not, and, if it did, over what range of phenomena. In other words, the basic problem about general laws in the social sciences of action is one of confirmation: In order to have general laws one must be able to predict outcomes as the result of the presence of a certain factor; but it is extremely unlikely that such predictions of social and psychological phenomena will occur, just because the concepts which partially make these phenomena what they are are subject to unpredictable change, and so the phenomena themselves are unlikely to be enough like the original to provide confirmatory instances.

Take, for example, the hypothetical case of a social scientist in 1800 who is trying to frame a general law about the political life of tribal societies, and yet who does not possess—and could not possibly possess, given the limits of his ability to predict conceptual changes—the concepts of "imperialism" and "socialism." No generalization that he formulates using the terms he has available to him could ever become a general law properly so called because the very nature of tribal politics in the nineteenth and twentieth centuries would be so deeply altered by the development and spread of these notions: His subject matter would change on him, and so his carefully wrought generalizations could never be tested and applied over a wide variety of situations so that they could become more than generalizations.

This hypothetical social scientist is in the same situation as was Marx in trying to formulate the iron laws of capitalist economy—say, the inevitable and immense pauperization of the working-class. Such an attempt was doomed to fail just because capitalist social systems are constantly evolving: For instance, Marx could never have known

of the theoretical innovations that would be made by Keynes, innovations which would irrevocably alter the set of institutions, practices, and beliefs about which Marx was trying to theorize. Such unpredictable changes inevitably made Marx's putative general law just that, a *putative* general law.

Nor can the social scientist avoid this situation by arguing that his generalizations can be transformed into law-like statements by recognizing their inherently statistical character. For the statistical generalizations found in the social sciences are not likely to become genuine statistical laws for exactly the same reason that causal generalizations in social science are not, namely, because to become so they must be confirmed in a wide variety of instances, and such confirmation is subject to the same sorts of difficulty I have been discussing. (Actually, the logic of confirmation for statistical laws is exactly the same as that for causal laws, except that in the latter case one is concerned with the occurrence of individual events, whereas in the former one is concerned with the occurrence of sets of events.)

It should be noted before proceeding that there is nothing in what I have said which would be incompatible with the social world being as deterministic as one pleases (deterministic in the sense of being fully law-governed)—or being indeterministic, for that matter. All that is required in order for my argument to work is that social phenomena be unpredictable because they are constituted by the self-understandings of the relevant actors, self-understandings which themselves change in unforetellable ways.

Nor should it be concluded from this that no general statements at all are possible in the social sciences of action. This would be the case if the sorts of changes I have been discussing occurred extremely fast and appeared to be totally random (such a situation would be analogous to our geologist's confronting a world in which the rocks in it changed in irregular ways every month or so). But fortunately this is not the situation in which we find ourselves. The structure of the social world is *relatively* stable, and its changes are usually confined to some roughly definable area; indeed, if this were not the case, it would completely undermine the possibility of sustained social interaction, and hence the possibility of there being some sort of genuine social order at all. There is a kind of regularity and continuity which must be present if there is to be social life; and it is on these facts that the generalizations that we do find in the social sciences rest.

Thus, for example, the sociology of knowledge tries to provide us with a general understanding of the relationship between certain sorts

of social structures and certain sorts of belief systems. Moreover, it also tries to provide some understanding of the sorts of intellectual innovations that are likely to find acceptance in societies of a certain description. In so doing, it gives us some understanding of the range of ideas that are likely to gain a foothold in a social order and thereby alter it. And on the basis of such knowledge, one can make certain genuinely testable generalizations about the rate and kind of change in various social systems.

The question is not, therefore, whether generalizations are possible—in the first place, they must be if there is going to be a social order at all, and, in the second, social scientists have already given us a whole slew of them. The question instead is whether or not we can expect these generalizations to be purified and rigorously stated so that they may thereby become genuine general laws. To this question, because of the constitutive role of concepts and beliefs in making human social life what it is, and because of the practical unpredictability of the development of these concepts and beliefs, we have every reason to believe that the answer will be "no." For these generalizations refer to what I hope to have shown to be epistemically anomalous phenomena, and such phenomena are not the sort requisite for formulations of a genuinely nomological character.

IV

My discussion about the possibility of general statements in social science leads directly into a discussion of the nature and role of causal theories in explaining actions, assuming it is the case that the causal generalizations in social science are indeed heteronomic. It is necessary to include this discussion because only then will it be clear what we can expect to be the nature of those sciences that try to give systematic causal explanations of human behavior. I want to conclude by emphasizing that although I believe that general laws properly so called will not emerge from the social sciences—and in this respect they differ from the natural sciences—I do not think it follows from this that a genuinely theoretical science of human behavior is impossible.[26] On the contrary, I believe that until the essentially theoretical character of social scientific explanation is appreciated, no account of the nature of the sciences of behavior will be adequate.

Toward the end of Section II, I mentioned that all causal explanations require the existence of causal generalizations; this is true in the natural as well as the social sciences. The reason for this is that in order to

justifiably claim that a particular (kind of) event causes another (kind of) event, one must have good reason to believe that the two events are not merely accidentally related, or even the joint outcomes of a third unknown (kind of) event. That is, one must have evidence that in certain circumstances the first (sort of) event actually is a necessary and/or sufficient condition for the other (sort).

The evidence called for here is of two kinds. In the first place, because the explanation rests, at least implicitly, on a general claim (that, ceteris paribus, the first [type of] event is enough to produce the other, and/or that the second [type] cannot occur without the first having occurred), the relevant evidence will involve generalizations which report other instances in which the two events are conjoined. In this regard, the more *unlike* the circumstances in which the relation is observed, the stronger the evidence that it is indeed the (type of) event in question, and not some other one in the environment, that is the cause.

But—and this leads to the second sort of evidence—generalizations of covariance alone cannot provide enough weight to support an imputation of a causal relationship. The two events might be causally unrelated to one another, and yet if they were the common effect of another, but unknown event, one would still have a generalization of covariance. This shows that something stronger than this sort of generalization is required. What is needed besides is a generalization which might be said to explain its instances, in the sense that implicit in it is an account of *why* the relationship between the events is indeed one of necessary and/or sufficient conditionship. It is only when one is in possession of this sort of generalization that one can with any degree of confidence make the contrary-to-fact and subjunctive conditionals that one must be able to make in order to claim that in certain circumstances one event is a sufficient and/or necessary condition of the other.

It is precisely at this point that theories are required, for it is from theories that such an account derives. Theories provide a systematic explanation of a diverse set of phenomena by showing that the events in question all result from the operation of a few basic principles. A theory goes beyond generalizations by showing why the generalizations hold, and it does this by specifying the basic entities which constitute the phenomena to be explained, and their modes of interaction, on the basis of which the observed generalizations can be inferred. One might say—with an acknowledgement to the realist theory of science, which I discussed earlier—that theories provide us with knowledge of the causal mechanisms that relate the events in question, and that are the

means by which one event brings about the other. It is knowledge of these mechanisms (understood in the broadest sense, such that the having of motives or beliefs might figure in such mechanisms) that supplements mere empirical generalizations and enables them to be evidence that a genuinely causal relationship is involved. It is thus that we are inevitably led from the desire to explain causally a particular (sort of) occurrence to the need for theories.

Of course, as science develops, these theories become much more rigorous and explicit. Moreover, it will naturally attempt to organize and structure the various causal explanations and causal generalizations upon which they rest by systematically interrelating them, and by subjecting them to experimental and other empirical verification. In this process the self-conscious development of "large-scale" theories is absolutely essential.

It is a very odd fact that the most sophisticated statements in the analytic tradition about the nature of social science have consistently failed to mention its essentially theoretical character[27]—a fact which has made much in this tradition appear irrelevant to many of its practioners. I say an odd fact because even a cursory glance reveals the kind of theorizing I am discussing. In neoclassical economic theory, in structural-functional theory in anthropology, in exchange theory in sociology, in structuralist theories of cognitive development in psychology—in these and countless other cases the fact that causal explanations require the development and articulation of large-scale theories is evident.

Of course, such theories have never lived up to the aspirations of Hobbes or Comte; that is, they have never approached the universal scope and precision of theories in the natural sciences. And if what I have argued in this paper is correct, they never will: The causal generalizations that figure in the theories of the social sciences are heteronomic (principally because their objects, human behavior and society, are intentional and historical entities), and consequently these theories are limited. These limits manifest themselves in a number of ways: in the precision of the terminology in these sciences; in the sharpness with which a scientist will be able to specify the conditions in which his theories hold; in the range of application in both space and time of such theories; and in their predictive power, and therefore their testability.

Moreover, there is one last limit on the causal theories of social science which deserves particular mention because of its bearing on the question of the sort of theory that social and psychological phenomena

call for. The limit I have in mind is that many of the causal generalizations in a social theory will be restricted to a given cultural context. The reason for this limitation is the constitutive role beliefs play in human life. If, as I have argued, social and psychological phenomena are what they are, and therefore have the causal relations they have, partly because of the beliefs of the actors involved, then these actors coming to have a radically different set of beliefs will likely mean a whole new set of relations among them, and this will consequently require a different set of causal generalizations to explain their social and psychological experience. Thus the causal theories a social scientist develops are likely to be more or less confined to particular cultures or types of culture.

This is an extremely important point, and not just for theoretical reasons alone. For if one interprets the causal generalizations produced by social science as if they were general laws which applied over a whole range of cultures, or even if one thinks that social science is capable of producing such laws, this may have the terribly unfortunate political repercussion of stifling political change. Let me explain, and in the process reveal why I think the theories in social science ought sometimes to be what has been called "critical."[28]

The causal generalizations in social science are about essentially conventional activity just because that activity is partly constituted by the beliefs of those involved. However, if one takes these generalizations to be actual or possible laws, one may be unwittingly reifying the particular conventions one is observing, i.e., treating them as if they were nature-like necessities such that the particular way a group of people interacts is taken to be the way it *must* interact. The reason why a commitment to general laws properly so called inevitably leads to this reification is the generality involved in such laws: If the generalizations one discovers are indeed (potential) general laws, then what might at first appear to be a local or idiosyncratic practice must be seen as an instance of something that is in the nature of things, and thus as something not alterable.

However, from the perspective of this paper, in which I have argued that causal theories in social science are limited in scope to a particular culture, or perhaps to particular sorts of culture, this reification is a form of ideological distortion. For in such cases the social scientist is illicitly transforming the generalizations which account for one particular way of doing things into purported general laws which supposedly govern human life as such. The effect of this concealed ideological transformation can be particularly oppressive, for it can reinforce the social

actors' acceptance of a status quo which may be deeply frustrating to them. It can do this both by giving them reason to believe that their social life must be as it is, and by failing to provide them with an analysis of their situation which might help them to change it (and so falsify the causal generalizations which now characterize their behavior!).

I have claimed that ultimately the nature of social-scientific theory (with its heteronomic generalizations, limitations in scope and specificity, and restriction to particular [sorts of] cultural setting) is shaped by the essential historicity of the objects it seeks to explain. And earlier in the paper I suggested that this historicity is itself rooted in the capacity for self-transformation characteristic of human beings. This suggests that any conception of social science which fails to take historicity into proper account will be defective. It will be theoretically defective because it will fail to appreciate the special character of causal theory in social science, and it will be practically defective in the oppression that it can cause in the way I have just indicated.

It is on just this feature of historicity that critical theory focuses. According to it, social-scientific theories not only must self-consciously recognize that they are limited because they are about creatures capable of self-transformation, but they must make this feature play an essential role in their construction. That is, critical theory insists that social science ought to be a means by which such transformation is fostered.

How can social theory do this? By assuming a particular form, namely, one that isolates in the lives of a group of people those causal conditions that depend for their power on the ignorance of those people as to the nature of their collective existence, and that are frustrating them. The intention here is to enlighten this group of people about these causal conditions and the ways in which they are oppressive, so that, *being enlightened, these people might change these conditions and so transform their lives* (and, coincidentally, transcend the original theory). Examples of critical theory are Marx's theory of capitalism and Freud's theory of neurosis.

A critical metatheoretical understanding of social science grows quite naturally out of the account of explanation, cause, action, law, generalization, and theory that I have given in this paper (although obviously it is not entailed by my account). The reason why it does is that both the heteronomic character of causal generalizations in social science and the idea of a critical social theory derive from the same special feature of human beings, namely, what I have called their historicity. It is because humans learn about themselves and their world that they are instrumental in transforming themselves and their relations, thereby

defeating the causal generalizations which a social scientist might have used to describe their lives. This is why such generalizations are heteronomic. But it is this very same capacity to be enlightened by these theories about the world and to alter their social arrangements partly on the basis of this enlightenment that makes humans fit subjects for a critical social science.

Moreover, while to a theory of social science that seeks to model it on the natural sciences the heteronomic character of social scientific causal generalizations is a pronounced liability, to a critical social scientist this heteronomy is a virtue. Heteronomy is a virtue for critical theory because it means that humans are capable of self-reflection and self-transformation, and it is just these that a critical social science is meant to foster. Indeed, a critical social scientist actually desires to see his causal generalizations made otiose by a group of actors who, having learned them, alter the way they live. He desires this because it means that he has been successful as a theorist in helping to alter the social world which he is studying.

A consideration of the nature of causal generalizations in explaining human behavior has lead me into a critical theoretic conception of social theory. This, it seems to me, is no accident. In the first place, such a conception is based on the belief that social science must be theoretical, but also on the self-conscious recognition of the heteronomic character of social scientific theories. Moreover, such a conception sees humans as natural creatures in a natural world of cause and effect, and thus as fit subjects for science; but it also sees humans as capable of a kind of initiative which distinguishes them from other natural creatures, and thus it argues that social science must be of a novel form. It is critical theory which understands the basis for the heteronomy of social science (namely, the historicity of human beings) and which builds this understanding into its account of social scientific theory.

PART THREE
Criticism and Advocacy

show, by implication, that conservative social forces, not rational inference or mere misinformation, remove the burden of argument from those who assume that blue-collar workers are conservative or that white workers, rather than their employers, benefit from racism. By right, these safe and standard propositions should be controversial in the extreme.

14. See Karl R. Popper, *The Open Society*, II (Princeton: Princeton University Press, 1963); J. W. N. Watkins, "Historical Explanation in the Social Sciences," *British Journal for the Philosophy of Science*, 9 (1957), pp. 104-117; George Homans, "Bringing Men Back In," *American Sociological Review*, 29 (1964), pp. 809-818.

15. G. M. Wilson, "A New Look at the Problem of 'Japanese Fascism'," in *Reappraisals of Fascism*, ed. Henry A. Turner (New York: Watts, 1975), p. 202.

16. Peter Bachrach and Morton Baratz, *Power and Poverty* (New York: Oxford University Press, 1970), p. 49.

17. See Watkins, pp. 104-117.

18. See Steven Lukes, "Methodological Individualism Reconsidered," *British Journal of Sociology*, 29 (1968), pp. 119-129, and M. Mandelbaum, "Societal Facts," *British Journal of Sociology*, 6 (1955), pp. 305-317, both reprinted in *The Philosophy of Social Explanation*, ed. Alan Ryan (New York: Oxford University Press, 1973).

19. His initial, highly influential statement is "The Function of General Laws in History," *The Journal of Philosophy*, 39 (1942), pp. 35-48. His more-or-less final elaboration, refinement, and defense is the title essay of *Aspects of Scientific Explanation* (New York: Free Press, 1965).

20. See, for example, Jürgen Habermas, *Knowledge and Human Interests*, trans. Jeremy J. Shapiro (Boston: Beacon Press, 1971), pp. 308-311.

21. The covering-law model requires, for good reason, that the laws in question employ only general, qualitative predicates, referring to no particular time, place, or person, for example, "male," "plantation-owner," "intelligent," but not "Confederate" or "nineteenth-century." After all, the model would be neither valid nor informative if "Whoever is Napoleon Bonaparte becomes Emperor of France" is counted as a law. But, among purely logical constraints, only the requirement of generality distinguishes that pseudo-law from real ones. Thus, the relevant covering-law connecting despair with surrender must be valid throughout the universe of military leaders, applying to Etruscan warrior-kings and Iroquois war chiefs, quite as much as Lee's compeers. At present, it is mere wishful thinking to suppose that such a law exists. On the requirement of qualitativeness, see Carl G. Hempel and P. Oppenheim, "Studies in the Logic of Explanation," originally written in 1948, in Hempel, *Aspects of Scientific Explanation*, pp. 268ff.

22. See Thomas S. Kuhn, "Concepts of Cause in the Development of Physics," in his *The Essential Tension* (Chicago: University of Chicago Press, 1977), pp. 21-30.

Chapter Five

1. See A. R. Louch, *Explanation and Human Action* (Berkeley: University of California Press, 1966), passim; R. Peters, *The Concept of Motivation* (New York: Humanities Press, 1958), ch. 1; D. Hamlyn, "Behavior," in *Philosophy*,

28 (1953); H. L. A. Hart and A. M. Honore, *Causation and the Law* (Oxford: Oxford University Press, 1959), esp. pp. 48-55; and William Dray, *Laws and Explanation in History* (Oxford: Clarendon Press, 1957), ch. 5.

2. Hart and Honore, p. 21.

3. This is the central thesis of Louch's book, for example.

4. Dray, ch. 5, part 4. Also cf. Hart and Honore, p. 52.

5. In *Philosophy, Politics and Society* (Second Series), ed. Peter Laslett and W. G. Runciman (Oxford: Blackwell, 1962), pp. 48-70.

6. Ibid., p. 55.

7. I have defended this distinction in detail in my "Practical Reasoning, Rationality, and the Explanation of Action," *Journal for the Theory of Social Behavior*, 8 (1978), pp. 77-101.

8. The Realist position has been developed by Rom Harre. See, for example, Harre and Peter Secord, *The Explanation of Social Behavior* (Oxford: Blackwell, 1972). See also Russell Keat and John Urrey, *Social Theory as Science* (London: Routledge and Kegan Paul, 1975).

9. I learned a great deal on this matter from David Miller of Warwick University. See Miller's review of Rom Harre's *The Principles of Scientific Thinking* (London: MacMillan, 1970), entitled "Back to Aristotle?" in the *British Journal of the Philosophy of Science*, 23 (1972), pp. 69-78.

10. This is just the sort of debate that developed over Weber's thesis. See, for example, the essays by Robertson, Samuelson, and Hansen, in *Protestantism, Capitalism, and Social Science*, ed. Robert Green (Lexington, Mass.: Heath, 1973).

11. *The Explanation of Behavior* (London: Routledge and Kegan Paul, 1964), pp. 21-25. There Taylor claims that the fact "that the system achieves this (normal) result-condition neither calls for nor admits of explanation" (p. 22).

12. Ibid., p. 24.

13. In the case of practical reasoning processes, I take it that this is just what developmental psychologists do; for example, see Jean Piaget, *The Origins of Intelligence in Children* (New York: Norton, 1963), particularly Part II.

14. Of course, in order for this to be so one must be able to distinguish between *causal generalizations* and *causal laws* properly so called. For this paper, general laws properly so called are universal well-confirmed empirical hypotheses of conditional form capable of supporting counterfactuals. They state that under certain specified boundary conditions every case in which events of the type N occur an event of the type E will occur, ceteris paribus. Causal generalizations, on the other hand, although of essentially the same form as that of general laws, are much more rough-and-ready: they are not universal, nor well confirmed; their boundary conditions are not well articulated; and their capacity to support counterfactuals is limited to a quantifiably unspecified range of events.

15. This part is a direct borrowing from Donald Davidson's "Causal Relations" in the *Journal of Philosophy*, LXIV (November 9, 1967), pp. 691-703.

16. Hart and Honore, pp. 13-14.

17. Reprinted in Carl G. Hempel, *Aspects of Scientific Explanation* (New York: Free Press, 1965), p. 232 and p. 236 (italics mine).

18. In the first half of his article, "Theory in History" (*Philosophy of Science*, 34 (1967), pp. 23-40), Leon Goldstein gives other instances of this doctrine to be found in the work of the philosophers Patrick Gardner (in *The Nature of*

Historical Explanation, pp. 57-89), Ernest Nagel (in "Determination in History," *Philosophy and Phenomenological Research*, 20 (1967), p. 307), and Karl Popper (in *The Open Society*, p. 448).

19. See, for example, Louch, chs. 1, 2.

20. See Donald Davidson, "Mental Events," reprinted in L. Foster and J. Swanson, *Experience and Theory* (Amherst: University of Mass. Press, 1970), pp. 79-101. See also William Alston, "Do Actions Have Causes?" in the *Proceedings of the Seventh Inter-American Congress of Philosophy* (Quebec: Laval University Press, 1970), pp. 256-276.

21. One might be tempted to interpret this as an excessively idealist account of social life because it seems to make ideas the crucial factor in social change. Such an interpretation would be a mistake, however. As "materialist" a theory of social change as one likes is compatible with what I say here, provided that this theory includes the assertion that social practices and psychological states are partly constituted by the self-understandings of the actors involved. (Marxism is such a materialist theory, for instance.)

22. The argument in this section was suggested by Karl R. Popper, *The Poverty of Historicism* (London: Routledge and Kegan Paul, 1957); and by Louis Mink, "Philosophical Analysis and Historical Understanding," *Review of Metaphysics*, XXI (June 1968), pp. 667-698.

23. Maurice Cranston, *Freedom* (London: Longmans, 1953), p. 118.

24. In *The Poverty of Historicism*, passim.

25. See Alasdair MacIntyre, "Predictability and Explanation in Social Science," *Philosophic Exchange*, 1 (Summer 1972), pp. 5-13. In chapter 8 of *After Virtue* (Notre Dame: University of Notre Dame Press, 1981) MacIntyre develops an argument quite like the one I am making in this paper.

26. This is in contrast to the views of MacIntyre, for example, who, after having shown that the particulars characteristically studied by social scientists are not predictable, claims to have shown thereby "that the aspiration to construct theories of scientific or a quasi-scientific sort in this area *must* fail" ("Predictability and Explanation in the Social Sciences," *Philosophic Exchange*, 1 (Summer 1972), p. 12). He makes the same claim in "Ideology, Social Science, and Revolution," *Comparative Politics*, 5 (April 1973), p. 336.

27. For example: Georg Henrik von Wright, *Explanation and Understanding* (Ithaca: Cornell University Press, 1972); A. R. Louch; Peter Winch, *The Idea of a Social Science* (London: Routledge and Kegan Paul, 1958).

28. See Jürgen Habermas, *Knowledge and Human Interests*, trans. Jeremy J. Shapiro (Boston: Beacon Press, 1972), chs. 10-12, and *Theory and Practice*, trans. John Viertel (Boston: Beacon Press, 1973), essay 7; and Brian Fay, *Social Theory and Political Practice* (London: Allen and Unwin, 1976), esp. ch. 5.

Chapter Six

1. Most notable among lesser known contemporary critical theorists are Alfred Schmidt, *The Concept of Nature in Marx* (London: New Left Books, 1971); Oskar Negt, *Öffentlichkeit und Erfahrung* (Frankfurt: Suhrkamp, 1972) (coauthor with Alexander Kluge); and Albrecht Wellmer, *Critical Theory of Society* (New York: Seabury Press, 1971).

2. Thomas A. McCarthy, *The Critical Theory of Jürgen Habermas* (Cambridge, Mass.: MIT Press, 1978) is an outstanding accomplishment, excellent in breadth and detail, but at times technically cumbersome, as is Trent Schroyer, *The Critique of Domination* (Boston: Beacon Press, 1973). Brief, accessible commentaries of Habermas's work written in English include Fred Dallmayr, "Reason and Emancipation," *Man and World*, 5 (1972): 79-109; Richard Bernstein, *The Restructuring of Social and Political Theory* (Philadelphia: University of Pennsylvania Press, 1978), pp. 171-236; Anthony Giddens, *Studies in Social and Political Theory* (New York: Basic Books, 1977), pp. 135-164; David Held, *Introduction to Critical Theory* (Berkeley: University of California Press, 1980), pp. 249-352; and Dick Howard, *The Marxian Legacy* (New York: Urizen Press, 1977), pp. 118-152.

3. Andrew Arato, "Political Sociology and Critique of Politics," in *The Essential Frankfurt School Reader*, ed. Andrew Arato and Eike Gebhardt (New York: Urizen Press, 1978), p. 15.

4. The Italian Antonio Gramsci was equally important in forging the reconstruction of Marxist theory along Hegelian lines, but his work was not as directly influential on the development of critical theory as that of Lukàcs and Korsch.

5. Arato, "Political Sociology and Critique of Politics," p. 6.

6. See Russell Jacoby, "Politics of the Crisis Theory," *Telos*, 23 (1975): 3-52.

7. While Marx himself was certainly ambivalent regarding this matter, the very presence of this ambivalence should be cause for concern among critical thinkers.

8. Russell Jacoby, "Toward a Critique of Automatic Marxism," *Telos*, 10 (1971), p. 145.

9. Max Horkheimer, "The Authoritarian State," *Telos*, 15 (1973), p. 9. In place of mass parties, Horkheimer in this essay endorses the workers' councils as the appropriate forms of revolutionary organization, although he shortly abandoned this position.

10. Ibid., p. 4.

11. Ibid.

12. See Dick Howard, p. 102.

13. James Miller, "Review of J. Habermas, *Legitimation Crisis*," *Telos*, 25 (1975), p. 214.

14. Max Horkheimer, *The Eclipse of Reason* (New York: Oxford University Press, 1947), p. 187.

15. Ibid., p. 178. Adorno expresses essentially the same point with reference to the gap between concepts and the reality they represent: "Reciprocal criticism of the universal and of the particular does justice to what it covers, and whether the particular fulfills the concepts." Theodor Adorno, *Negative Dialectics*, trans. E.B. Ashton (New York: Seabury Press, 1973), p. 146.

16. Marx quoted in Jürgen Habermas, *Theory and Practice*, trans. John Viertel (Boston: Beacon Press, 1973), p. 168.

17. Quoted ibid.

18. Ibid., p. 169.

19. See McCarthy, *The Critical Theory of Jürgen Habermas*, p. 147.

20. Habermas, p. 47.