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SINGULAR CAUSAL JUDGMENTS AND THE CAUSE-CONDITIONS DISTINCTION IN HISTORY

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SINGULAR CAUSAL JUDGMENTS AND THE CAUSE-CONDITIONS DISTINCTION IN HISTORY

Introduction

(A) The Problem

The cause-conditions distinction has been the subject of intermittent philosophical controversy for well over a century now. As far back as 1859 John Stuart Mill drew attention to the popular or common-sense appeal to such a distinction when explanations of phenomena were being requested or offered. In his System of Logic,[1] Mill observed that people often say things like "His eating that particular dish caused his death" or "The cause of that man's death was that his foot slipped in climbing a ladder," knowing full well that the condition cited as the cause was not the only condition requisite for the occurrence of the particular event in question. Mill hastily dismissed such appeals as philosophically or scientifically untenable, suggesting that when one of a number of conditions that were all necessary for the occurrence of an event is singled out as its cause, such causal judgments are arbitrary.

A number of writers have since contested Mill's view of the nature of the cause-conditions distinction. The question raised by some of Mill's early critics, such as C. J. Ducasse and R. G. Collingwood was this: Are there any objective criteria for distinguishing the cause of an event and other conditions equally necessary for its occurrence? Both Ducasse and Collingwood responded affirmatively to this question and claimed to have identified criteria for

drawing this distinction in a non-arbitrary manner. A detailed examination of their views on the nature of the cause-conditions distinction, as well as those of later writers who have raised and responded to the same question, will be one of the major objectives of this thesis.

Apart from reconstructing the debate over the nature of the cause-conditions distinction I will also be especially concerned with various attempts to identify applications of this distinction in different contexts of inquiry. Although virtually all of the authors who have written about the cause-conditions distinction recognize that it is primarily a common-sense distinction; that is, that the average man or woman on the street will often request explanations that assume this distinction and will generally accept singular causal judgments as satisfactory explanations, some writers have endeavored to show that it plays an important role in such fields as the law and in historical inquiry.

In their impressive work, Causation In the Law, H. L. A. Hart and A. M. Honoré analyze the ways in which causal issues arise and are resolved in common law and criminal courts and find that the judges who sit in these courts are often required to discriminate between causes and conditions in order to reach a verdict or decision in particular cases. Like Ducasse and Collingwood, these authors maintain that Mill's view of the arbitrary character of singular causal judgments is mistaken and they too claim to have discovered criteria that serve to distinguish between causes and conditions in both everyday and legal contexts. In Chapter III I will examine Hart and Honoré's description of the way in which these criteria function in some detail.

Inspired by Hart and Honoré's analysis of causal judgments made in legal inquiries, a number of authors with an interest in the methodology of historical research have sought to apply the cause-conditions distinction to the kinds of causal judgments historians offer their readers. Observing that historians sometimes single out one of a number of conditions that were involved in bringing about some event and declare it the cause of that event, both Morton White and William Dray have analyzed the singular causal judgments offered by historians along the lines suggested by Hart and Honoré. Most recently, though, Maurice Mandelbaum has mounted a vigorous attack on various aspects of Hart and Honoré's view of the nature of the cause-conditions distinction and the role it has been alleged to play in historical research. In his book, The Anatomy of Historical Knowledge, Mandelbaum develops an alternative view of the causal relation which, he claims, is both incompatible with yet superior to the one endorsed by Hart and Honoré and seeks to apply it to the causal explanations constructed by historians.

The debate over the role played by the cause-conditions distinction in diverse contexts of inquiry has not, in my view, been satisfactorily resolved to this date. While the controversy between Mandelbaum and Hart and Honoré has served to expose certain assumptions about the nature of the cause-conditions distinction and certain requirements for causal judgments offered in various contexts of inquiry to be certified as "objective," I believe that some important aspects of the grounds for ascribing causal status to one condition of an event have been neglected in this debate. Among these neglected aspects the most important concerns the role of the Mill's Method of Difference in the construction of causal explanation.

Although it has been known for quite some time that this method constitutes an objective criterion for validating causal hypotheses in the experimental sciences, few people have even considered the possibility that it supports the singular causal judgments made outside the laboratory. The claim that this possibility has indeed been actualized in historical research on many occasions is one I wish to develop and defend, for it explains what historians are up to when they advance causal judgments about events that took place in the past. While much analytical spadework will be required to strengthen this claim, I believe that this conceptual excavation will enable me to show that the Method of Difference plays an essential role not only in the formulation of singular causal judgments that employ the cause-conditions distinction, but also in those causal judgments that assert that one cause was more important than another in bringing about a certain event.

The foundation for the theory of causal explanation I want to develop consists of a certain view of the nature of the causal relation. For this reason I will now go on to briefly develop this view.

(B) The Nature of the Causal Relation

Even when it has been recognized that the conditions often cited as causes of events are necessary, but not sufficient for the occurrence of such events, there have remained certain puzzles about the nature of such causes. The question, "Must causes produce events or contribute to their production?" is one of these. The type of case which, perhaps more than any other, has ellicited this question is one in which an omission is judged to be the cause of some event. Mill, for instance, noted that people sometimes say things like "the cause

of the army's being surprised was the sentinel's being off his post." This response to such declarations is instructive. He writes: "His being off his post was no producing cause, but the mere absence of a preventing cause; it was simply equivalent to his non-existence. From nothing, from a mere negation, no consequences can proceed."[2]

The view that Mill is espousing here is certainly understandable. We ordinarily think that causes are productive, that fire can destroy forests, wind erode soil, and water turn electric turbines. Such physical conditions do produce changes in the world. And in the example proposed by Mill we would probably agree that the sentinel's being off his post did not produce the surprise felt by the members of the army; it did not make them be surprised. Nevertheless, one who made the kind of causal judgment that Mill proposed would probably be reticent about withdrawing his judgment even when he understood the point of Mill's remark. And if he reflected on the dilemma for awhile he might say something like this: "Well, I know that the sentinel's being off his post didn't cause the army to be surprised, the presence of the enemy did, yet his being off his post was the cause of the army's being surprised." While this clarification of the initial causal judgment may appear contradictory, I suggest that it is not. There is, I would maintain, a genuine distinction to be drawn between conditions that "cause" and others that "are the cause of" events. The former produce events, the latter, even if they do not produce them, at least explain them.

Once we recognize that there is a valid distinction between producing causes and explanatory causes[3] we can agree with Mill that "the sentinel's being off his post" is indeed no producing cause, it is an

explanatory one. But how, then, would we go about making sense of speaking of the statement "The cause of the army's being surprised by the enemy was the sentinel's being off his post" as making reference to an explanatory cause. I suggest that we would first want to know what question such a judgment was a response to. The most likely candidate would appear to be "Why was the army surprised by the enemy?" But this question does not tell the full story. A person who asked such a question would normally assume or imply that the state of affairs described as "the army's being surprised" is to be contrasted with another state of affairs, described perhaps as "the army's being prepared." Seen in this light the causal judgment would have to be understood as a response to the question "Why was the army surprised by the enemy rather than prepared to meet it?" Once the question which occasioned the particular response at issue is fully stated we can see that the subject of the causal judgment is a change, a difference between two comparable situations. Such contrasts or comparisons, I want to maintain, constitute the procedure through which all explanatory causes are identified. Whenever we ask why this particular event or change occurred we are asking "Why this instead of that?" or "Why now rather than then?" or "Why here rather than there?", and so on. In other words, in raising an explanatory causal question we always suppose, either explicitly or implicitly, an alternative situation. This is why we find no difficulty in accepting the judgment that the cause of the army's being surprised was the sentinel's being off his post. It was "his being off his post" that made the difference between the army's being surprised as opposed to its being prepared to meet the enemy, and so it is this condition which explains the change in question.

No doubt there is much more that needs to be said to give a complete account of what is involved when omissions are cited as the cause of certain events, e.g. suppositions about what normally happens when sentinels are at their posts. Moreover, we will have to ask what, in general, entitles us to speak of two situations being "comparable." What is important to recognize here, however, is that producing causes, as Mill calls them, are treated in exactly the same way as omissions when we try to explain the occurrence of particular events. This point can be brought out by considering another example Mill offers to illustrate the popular employment of the cause-conditions distinction, viz; "This man died as a consequence of his having eaten of this particular dish."[4] Now what this statement means is that the cause of the man's death was some element in the food that initiated a chemical reaction in his body that resulted in his death. In this instance the chemical substance that poisoned the man is definitely regarded as a producing cause. However, when we ask what question this singular causal judgment is a response to we find that it is something like: "Why did this man die now when he was in good health for some time before this occurred?" In this, as in the former example proposed by Mill, an explanation of the difference between two comparable situations is sought and what satisfies our desire to know the explanation of such change is the discovery of some condition which we accept as having made the difference between the presence of one, but not of the other situation. We may conclude, then, that the class of explanatory causes includes, but is wider than that of producing causes. Indeed, we shall find that there are a number of types of conditions that can count as explanatory causes, including events, standing conditions, beliefs, actions and omissions.

The aim of singular causal judgments is, as I have already said, to identify conditions that account for a difference or variation between two comparable situations. Most often the variations we seek to explain are relative in nature, not absolute. This is reflected in the language we use to describe such variations, e.g. we talk about increases and decreases, rises and declines, and so on. On some occasions, though, we are not interested in explaining the occurrence of a particular type of change as such, but in the rate of change. Thus, a historian writing about settlement patterns in 19th century America may ask: "Why were the Ohio lands settled only in the 1830's when equally fertile land further east and south was settled much earlier?" What is sought here is an explanation for the delay. Similarly, historians have asked why prices plummeted on the New York Stock Exchange on October 29, 1929. In this instance it is not an explanation of a decline in prices as such that is sought, but one for a rapid decline.

Against the view of causal explanation that I have outlined (albeit very roughly), it may be objected that the subject of explanation is not always a change or variation, sometimes we seek explanations for non-variations, for instance, for the persistence of a certain state of affairs or result. Thus, it may be pointed out that historians sometimes ask themselves questions like, "How did Prime Minister X manage to remain in power so long?" Here, it would appear, it is the persistence of a certain state of affairs that needs to be accounted for. My response to this objection is that while it is ostensibly a non-variation whose explanation is being sought we nevertheless view that state of affairs as a variation when we try to explain it. I submit that in asking "How did Prime Minister X remain in power so

long?" the historian would be contrasting that state of affairs with one in which, for example, previous Prime Ministers lost power very quickly or one where other heads of state did so. Here, again, it is the identification of a condition or set of conditions that differentiates between two comparable situations that is sought. In this way the concept of variation can be extended to include states of affairs that persist or endure.

(C) Types of Causal Inquiry in History

The field of history, like that of medicine, contains many departments and areas of specialization. One would think that any attempt to classify the division of labor in such large-scale enterprises would have to take into account differences in the nature of the problems encountered by practitioners engaged in different areas of specialization as well as differences in the methods of investigation used to resolve these problems. Nobody would deny, for example, that the problems with which a hemotologist must deal are far different than those with which an epidemiologist may be concerned or that their methods for resolving their respective problems are quite different. Unfortunately, when it comes to analyzing how the concept of causation functions in historical accounts such differences are often overlooked. Collingwood, for instance, contended that the subject matter of history is always actions and that historical causes are always reasons for actions. However much such a view might square with a reading of certain types of political and diplomatic history, it would appear to be completely out of line with some types of economic history. The error committed by Collingwood is of course that of hasty generalization. In order to reduce the risk of my committing this fallacy when

considering the role played the cause-conditions distinction in the causal judgments made by historians I think it advisable to acknowledge right from the start the diverse forms that causal inquiry may take in historical research. Towards this end I want to outline a number of types of causal inquiry that historians do engage in, without pretending that my list is exhaustive. Once the details of this outline have been filled in at a later stage of this thesis we will then be in a position to determine in what way, if any, the cause-conditions distinction functions in these different types of causal inquiry.

As the British historian A. J. P. Taylor once pointed out[5] there can be many answers to the question, "What caused the Second World War?" Among the conditions listed by Taylor were: (1) German grievances against the peace settlement of 1919 and the failure to redress them, (2) failure to agree on a system of general controlled disarmament, (3) failure to accept the principles of collective security and to operate them, (4) German strength, which destroyed the balance of power in Europe, (5) American aloofness from European affairs, and (6) Hitler's inordinate and unscrupulous ambition.

Although some historians think that the compilation of such a list of contributory conditions or factors is the most they can offer by way of causally explaining the outbreak of the Second World War others maintain that explanation has not been achieved unless there is some way of ranking these conditions on some scale of importance such that, even if we can't say that it was some one condition that caused the war at least we will be able to distinguish the more important from the less important conditions. I am not in a position to say which view is more prevalent among historians. However, it does appear to me that historians do try to limit, as far as possible, the number of

conditions they regard as important ones. This suggests to me that when historians attempt to explain why a particular event occurred they almost always discriminate between causes and conditions if not between cause and conditions. Nevertheless, there are historians, Taylor among them, who do claim to have isolated a single condition they accept as being the cause of some variation between two comparable situations and it is such judgments that will be of greatest concern to us in the pages that follow.

In order to understand what historians are doing when they isolate one or more conditions of an event and call it [them] the cause, we need to first take note of the fact that events, and the causal questions asked about them, can be distinguished in terms of the different levels of generality they manifest. As we have already seen, singular causal questions, e.g. "Why did this man contract throat cancer?" will evoke singular causal judgments. Sometimes, though, historians will ask more general causal questions, the answers to which may also be accepted as a satisfactory response to a singular causal question. For example, a historian or social scientist may, after scouring hospital records, ask: "Why was there a significantly higher incidence of throat cancer among 19th century German chimney-sweeps than among those engaged in other occupational roles?" To both the singular and more general causal question the statement, "exposure to a high level of carbon," may be taken as a correct and satisfactory explanation. Moreover, when attempting to explain social events, i.e. events referring to what has happened to or been done by groups of individuals, historians will sometimes issue comparative causal judgments rather than unequivocal singular ones. Thus, a historian trying to explain a precipitous drop in the non-marital birth rate in 19th

century England may conclude that "use of contraceptives was a more important cause of the drop in the non-marital birth-rate than was the influence of Victorian prohibitions against sex."

Whether one is inquiring into which factor was the cause or which factor was the most important cause of some event one thing seems clear, some form of quantification is often necessary for engaging in these forms of causal inquiry. This is even more apparent in historical works like Robert Fogel's Railroads and American Economic Growth. There Fogel attempts to determine the net contribution of the railroad to American economic growth in the year 1890. In order to accomplish this task he employs a standard to measure what he calls "social savings," this being "the difference between the actual level of national income in 1890 and that level of national income that would have prevailed if the economy had made the most efficient possible adjustment to the absence of the interregional railroad."[6]

In a later chapter we will explore in greater detail the character of Fogel's work and the sense in which it can appropriately be called a type of causal inquiry.

What I would consider another broad type of causal inquiry that historians and other social scientists engage in involves tracing the consequences of a given event rather than seeking for its cause. Here the direction of inquiry is not from effect to cause but from cause to effect(s). Whether or not we know the answer to the question "Why did this car stop running?", we can still raise the question, "What were the consequences of this breakdown?" In history such inquiries are sometimes large-scale enterprises where the whole of a historical work will constitute an attempt to identify and describe the heterogeneous consequences of a war or of an invention, e.g. in medicine, law,

agriculture, education, and so on; on other occasions the historian's interest may focus upon only one type of consequence, e.g. the impact of the automobile of the transportation industry. In addition to the types of consequences investigated, such inquiries can be characterized in terms of their space-time points of reference as well. By this I mean that while one historian may be interested in the diverse consequences of a war in a particular country another might trace such consequences on an international scale. Again, one historian may seek to trace the consequences of the automobile on the domestic transportation industry for a period of ten years subsequent to its introduction while another historian will do much the same for a longer stretch of time.

Seeking to identify the cause of an event, determining the magnitude of the contribution made by some factor to some net result, and tracing the consequences of an event are all types of causal inquiry that historians engage in. Consequently, if we are to determine the role played by the cause-conditions distinction in historical research we shall have to ask what role, if any, the cause-conditions distinction plays in each type of causal inquiry previously mentioned. As a move in that direction I shall now go on to consider the conception of the nature and role of the cause-conditions distinction as presented by J. S. Mill.

CHAPTER ONE: J. S. MILL AND THE CAUSE-CONDITIONS DISTINCTION

(A) Mill's View of the Cause-Conditions Distinction:

Book three of the System of Logic contains Mill's classic discussion of the cause-conditions distinction. One of the passages in which he expresses the view that such a distinction is philosophically untenable reads as follows:

> Since, then, mankind are accustomed with acknowledged propriety so far as the ordinances of language are concerned, to give the name of cause to almost any one of the conditions of a phenomenon, or any portion of the whole number, arbitrarily selected, without excepting even those conditions which are purely negative, and in themselves incapable of causing anything; it will probably be admitted without longer discussion, that no one of the conditions has more claim to that title than another, and that the real cause of the phenomenon is the assemblage of all its conditions. (97)

Mill was not the first English philosopher to reject the linguistic habits of ordinary speakers on the grounds that the distinctions they appeal to are without foundation. Hume, of course, made a career out of such scepticism. Noting that philosophers were apt to support various common-sense distinctions reflected in the everyday use of causal language, Hume, in The Treatise of Human Nature, proposes to "remove several prejudices and popular errors that have very much prevailed in philosophy." [1] In particular he claimed that "there is no foundation for that distinction which we sometimes make between efficient causes, and causes sine qua non; or betwixt efficient causes, and formal, and material, and exemplary and final causes."[2] Also to be rejected is "the distinction betwixt cause and occasion, when supposed to signify anything different from each other."[3]

For both Hume and Mill none of the common-sense causal distinctions listed are philosophically valid for the simple reason that all of them

are predicated on the belief that there are different kinds of causes when in fact all causes are of the same kind. According to Hume, only events of a certain type whose occurrence has been observed to be constantly conjoined with the occurrence of other types of events are entitled to the name cause, while for Mill, who was aware that concommitant variations may fail to signal any causal connection, a sequence must be unconditional as well as invariable for it to be deemed a causal one.

Aside from the fact that the cause-conditions distinction fails to conform to his monistic conception of cause, Mill argues that the distinction can be shown to be untenable merely by observing the manner in which causes are selected from among a wealth of other conditions. "Nothing can better show the absence of any scientific ground for the distinction between the cause of a phenomenon and its conditions," writes Mill, than the capricious manner in which we select from among the conditions that which we choose to denominate the cause. However numerous the conditions may be, there is hardly any of them which may not, according to the purpose of our immediate discourse, obtain that nominal preeminence." (198-199)

In spite of his insistence that causes are selected gratuitously, Mill does point to several criteria that serve to guide causal selections. "In practice," he notes, "that particular condition is usually styled the cause, whose share in the matter is superficially the most conspicuous, or whose requisiteness to the production of the effect we happen to be insisting upon at the moment." (199)

By maintaining that the criteria commonly employed to distinguish causes from conditions are conspicuousness and immediate interest Mill appears to be casting derision rather than light on the grounds upon

which singular causal judgments are drawn. Elsewhere, however, he does point to the use of a criterion of causal selection that is much less vague than the previous two. This criterion is what might be called the "last necessary condition" (LNC) criterion of causal selection. Mill observes that people often designate a necessary condition of an event that "completes the tale" as its cause. More strictly, he suggests that people often call that condition the cause of an event "which was requisite to complete the required concurrence of conditions." (198) Here, I think, Mill has put his finger on a criterion that is commonly utilized in making singular causal judgments. Since C. J. Ducasse makes this criterion the cornerstone of his defense of the cause-conditions distinction I shall postpone discussion of it until we come to examine Ducasse's views a little later in this chapter.

At the very least, however, Mill's recognition of the LNC criterion betrays his previously expressed conviction concerning the capricious or arbitrary manner in which causes are distinguished from conditions. But in gauging the consistency of Mill's views on the validity of the cause-conditions distinction there is, I believe, an even greater embarrassment to be noted. What I want to argue is that not only is the cause-conditions distinction not drawn on arbitrary grounds but that Mill himself, also in the System of Logic, provided a detailed outline of non-arbitrary grounds for drawing the distinction. Put simply, I believe that his methods of experimental inquiry (Mill's Methods) constitute the justifying grounds for many singular causal judgments.

(B) The Methods of Experimental Inquiry and the Cause-Conditions Distinction

The key to an understanding of how the concept of cause is used by historians is, as I have already said, to be found in variation. Whenever an event has occurred something has varied. Concommitant variations do not, in and of themselves, establish causation. Increased use of fertilizer may, for example, be followed by larger crops, but this concommitant variation does not prove that more fertilizer resulted in the greater yield. To establish causation other possible influences on the result (such as fluctuations in temperature, moisture, light, quality of seeds, presence of parasites, etc.) must be eliminated. The usual way of achieving such elimination is to hold these other conditions constant. Thus, two seeds of the same genetic quality may be planted in the same soil growing side by side in the same light, moisture, and temperature, with an equal amount of spraying and cultivation accorded each seed. Then fertilizer is applied to one seed but not to the other. The first seed is our experimental subject, the second, our control. Should the experimental plant produce more than the control then a causal connection between the use of this fertilizer and plant growth has been established.

To adopt the jargon of experimental science, what we would be trying to do in the above example is to establish a causal connection between two variables, a dependent variable (plant growth) and an independent variable (application of this fertilizer). Unless we have made sure that "other things are equal" our causal judgments lack adequate grounds or support. Indeed, various techniques for securing "factor control" can be employed, e.g. factor equation, frequency distribution, randomization. The role played by these techniques in

different kinds of experimental designs is well-documented in a variety of texts on scientific methodology and I shall not discuss them here.[4]

However, it is important to take note of Mill's view that there are two ways of obtaining the variations necessary to support our causal judgments. We may "either find an instance in nature, suited to our purposes, or, by artificial arrangement of circumstances, make one." (218) The former he calls "Observation," the latter "Experiment." The example outlined above is one in which variations were obtained via an "artificial arrangement of circumstances." What was artificial about our experimental design was the application of a (suspected) causal agent to one subject, the withholding it from another and the conscious attempt to control the influence of other relevant causal factors. Paradigmatic of such "artificial arrangement of circumstances" is, of course, the laboratory experiment, where the experimenter can exercise rigid control over relevant factors, use instruments to gauge the effects of the stimulus he himself introduces, and can repeat the experiment at will while varying all the circumstantial factors.

What Mill terms "Observation," on the other hand, can more appropriately be called a "natural experiment," for in these the necessary arrangement of circumstances is found rather than contrived. This is not to say that on the occasions when a "natural experiment" takes place no human intervention in the environment is permissible, only that such intervention must not have been intended as a means for testing a causal hypothesis. Thus, a natural experiment took place in my garden this past summer when I noted that there was a rather uniform decrease in the height of my stringbean, pepper, and basil plants; the ones situated at the back of their respective rows being taller than the ones situated at the front. The cause of this variation, as I soon

discovered, was that the plants growing at the front of the rows were shielded from the strong afternoon sunshine by the large tree in my neighbor's yard, whereas those plants growing at the back of the rows were not similarly shielded.

Now if I had been interested in testing the causal hypothesis that plants exposed to sunshine for various lengths of time would manifest corresponding variations in their rates of growth, other things being equal, I would have been more rigorous in controlling other possible influences on plant growth than was the case in the "natural experiment" and I would have constructed my own light shield or perhaps used an artificial source of light. Had these conditions obtained then I would have been engaging in what Mill called "Experiment." However, Mill rightly claims that irrespective of the mode in which circumstantial variations are obtained, "the value of the instance depends upon what it is in itself.... There is, in short, no difference in kind, no logical distinction between the two processes of investigation." (218) Thus, we are told that "the fact that light blackens chloride of silver might have been discovered either by experiments upon light, trying what effect it would produce on various substances, or by observing that portions of the chloride had repeatedly become black, and inquiring into the circumstances." (220)

There are, however, some important practical differences between these two modes of investigation that Mill takes stock of. Not only do artificially constructed experiments permit us to produce a greater number of variations in circumstances than nature presents us with, but they also allow us "to produce the precise sort of variation which we are in want of for discovering the law of the phenomenon; a service which nature, being constructed on a quite different scheme from that

of facilitating our studies, is seldom so friendly to bestow upon us." (218) Also, by producing a phenomenon artificially "we can take it, as it were, home with us, and observe it in the midst of circumstances with which in all other respects we are accurately acquainted." (218) This, Mill claims, makes it easier to become assured that the influence of other circumstantial factors has been eliminated. Add to this the fact that artificially created experiments can often be repeated at will while "natural experiments" cannot, and we see the immense advantage that experimentation has over observation. It is for the reasons just listed (and a few others) that Mill asserted that in social science "induction from direct experience is practiced at a disadvantage generally equivalent to impracticality...." (220)

Notwithstanding the important practical advantages that experimentation has over observation, Mill does recognize that artificially-constructed experiments must be uni-directional, that is, they apply only to cause-effect investigations and not to effect-cause ones. In his own words, "we can take a cause and try what it will produce; but we cannot take an effect, and try what it will be produced by. We can only watch till we see it produced, or are enabled to produce it by accident." (220) For this reason,

> Whenever, having nothing to guide us to the cause, we are obliged to set out from the effect, and apply the rule of varying the circumstances to the consequents, not the antecedents, we are necessarily destitute of the resource of artificial experimentation. (220)

Evidently this is the situation that obtains in historical investigations. Historians cannot somehow reach back into the past and physically manipulate conditions they suspect of having had a causal influence on certain events. It would appear that the most that an empirically-

minded historian could hope for is that he sometimes might find conditions so arranged in the past that they naturally fall into an experimental and a control group.

Even if this view were correct (which I do not think it is), I do not think that the prospects for grounding causal judgments on "natural experiments" are as grim as a reading of Mill would lead us to believe. Once I have explained what, in my view, is the relationship between the methods of experimental inquiry and the cause-conditions distinction, it will be seen that there are many opportunities for historians to find variations in circumstantial factors on which to base their causal judgments. However, it is not entirely clear whether, in fact, historians are "necessarily destitute of the resource of artificial experimentation." If by "artificial experimentation" is meant that the experimenter exercises physical control over the circumstantial factors he wishes to vary then historians obviously cannot engage in artificial experimentation. But physical control is not the only way that an experimenter can exercise control over such factors. In what has become known as the retroactive or ex post facto experiment, control is achieved by manipulating symbols that represent the circumstantial factors, rather than those factors themselves. For example, we may suspect that the cause of an increase in the incidence of juvenile crime is the increase in the number of broken homes. But in order to determine whether this is so it is unlikely that we would try to break up homes so that we can observe what effect this has upon the rate of juvenile crime. Instead of manipulating this factor in such a concrete way what we would probably do is try to gather as much information as we could about the living arrangements of a large number of adolescents. By obtaining this information and information about

other circumstantial factors (e.g. family income, religious denominations, etc.) we could then ask whether when these other factors are held constant, coming from a broken home is causally connected with being a juvenile delinquent. In the next chapter when I discuss R. G. Collingwood's "controllability" criterion for distinguishing between causes and conditions I shall have more to say about this type of experimentation and the role it plays in the quantitative causal analyses that have become fashionable among certain groups of historians.

By way of introducing the Methods of Experimental Inquiry it is important that we first recognize two kinds of comparisons, each of which can yield the variations required to engage in experimentation, whether of the "natural" or "artificial" type. The first kind of comparison can be called a simultaneous set-up, the second, a successional set-up. Both kinds of comparison are well illustrated by the work of Pasteur.

In experiments that manifest a simultaneous set-up the object is to introduce a stimulus into one individual or group of subjects, withhold it from another individual or group of subjects, and observe what happens. Thus, in an experiment designed to prove that anthrax vaccination produces immunity from the disease of the same name Pasteur took a group of twenty-four sheep, one goat and several cattle and vaccinated them with a weak form of anthrax. At the same time he left another group of twenty-four sheep, one goat and several cattle unvaccinated. Twelve days later his assistants injected a virulent dose of anthrax into both groups of animals. Two days later it was observed that not a single animal that had been vaccinated had so much as a fever whereas twenty-two of the twenty-four unvaccinated sheep had already expired and the other two were staggering about close to death.[5]

The object of the successional set-up, on the other hand, is to obtain chronological comparisons of effects in a single subject or group of subjects over a period of time. This experimental design is often referred to as a "before and after" experiment. A simple experiment conducted by Pasteur in order to disprove the theory of spontaneous generation illustrates this experimental design quite nicely. What Pasteur did was to boil meat broth in a flask with a very long, thin neck until no bacteria were left. This he determined by the fact that no changes were observed in the broth for several days. Then he proceeded to break off the neck of the flask and within hours the meat broth was in a state of almost total decay. After repeating the experiment a number of times Pasteur concluded that the microorganisms responsible for the decay were carried by the air.[6]

The method of experimental inquiry employed by Pasteur in these experiments was the Method of Difference. Mill's formal statement of this Method reads:

> If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance save one in common, that one occurring in the former; the circumstance in which alone the two instances differ, is the effect, or the cause, or a necessary part of the cause, of the phenomenon. (225)

It is fairly obvious how this method bears upon the two experiments discussed above. In Pasteur's anthrax experiment the two contrasting instances are represented on the aggregative level by the experimental group of animals and the control group, and on the individual level by each animal in the experimental group when paired with another animal in the control group. The one circumstance in which they are assumed to have differed is that the first group received the weakened dose of anthrax while the second did not. The second experiment, which

illustrated the successional set-up, has as its two instances the boiled meat broth in the hermetically sealed flask, on the one hand, and the same broth after the neck of the flask was broken, on the other. The one circumstance in which the two instances are assumed to differ is that of being exposed to air.

Consider now an example Mill introduces when he is discussing applications of the Method of Difference:

> When a man is shot through the heart, it is by this method (of difference) we know that it was the gun-shot which killed him; for he was in the fullness of life immediately before, all circumstances being the same except the wound. (225)

This (natural) experiment quite clearly manifests a successional set-up. The same subject supplies us with the two instances we require to make the necessary contrast and the gun-shot constitutes the single difference between them. Examples of this sort, it is asserted by Mill, illustrate "a logical process to which we owe almost all the inductive conclusions we draw in daily life." (225)

If this view of Mill is correct, (and I think it is) and if it can be established that the cause-conditions distinction is just another way of expressing the results of having applied the Method of Difference, then it will have been shown that Mill's claim about the arbitrary nature of singular causal judgments based on the cause-conditions distinction is inconsistent with his views on the Method of Difference and, more importantly, that it is a faulty characterization of the grounds upon which that distinction is often drawn. Such an argument is fairly easy to make out.

In order to show that the argument about the relationship between the Method of Difference and the cause-conditions distinction does indeed hold I now want to look briefly at the way that C. J. Ducasse went

about challenging Mill's view of the arbitrary nature of the cause-conditions distinction.

(C) A First Critique of Mill: C. J. Ducasse

In response to Mill's claim that "Nothing can better show the absence of any scientific ground for the distinction between the cause of a phenomenon and its conditions than the capricious manner in which we select from among the conditions that which we choose to denominate the cause," Ducasse flatly states that "he is wrong. There is no capriciousness about it."[8] Mill's mistaken characterization of the grounds upon which the cause-conditions distinction is drawn is traced by Ducasse to "the obsession under which he labors all along, namely that inquiry into causation is inquiry into laws."[9]

Not unlike the authors of many recent analyses of singular causal judgments, e.g. Donald Davidson, Ducasse allows that laws may be implied by such judgments, but this is not to say that causal inquiry is always, or even primarily an inquiry into laws. For Ducasse, on the contrary, causal inquiry is "directly and primarily an inquiry concerning single, individual, events."[10] I must say that I fully agree with the view of causal inquiry espoused by Ducasse and believe it has important consequences for the analysis of what I later call "species" of singular causal judgments. Now, however, consider an example that Ducasse thinks is typical of those situations that elicit singular causal judgments. Writes Ducasse:

> If the engine of my car stops, and I ask "why"....
> what I wish to discover is the single difference
> between the circumstances of the engine when it
> was running, and at the moment when it was not....
> If it is the cause that we seek, we look for a
> difference in those circumstances between the
> moment when the phenomenon occurred and the

> preceding moment. And the field among the entities of which the conditions lie is thereby also defined: It is that of circumstances which remained constant over the two moments.[11]

Notice, first of all, that this example is in all respects like Mill's gun-shot example. In the latter example it was the difference between the circumstances when the man was living and when he ceased to be alive that is singled out as the cause whereas in the former it is the difference between the circumstances when the engine was running and when it ceased to run that is designated the cause. Moreover, Ducasse's reference to a set of circumstances that remained constant over a period of time and his identification of the cause with a difference between two distinct sets of circumstances strongly suggests that it is the Method of Difference that is employed in discovering the cause of the breakdown. However, the fact that Ducasse views the two sets of circumstances that need to be contrasted in order to find the relevant difference solely in terms of their temporal proximity places a certain restriction on the application of the Method of Difference. What he has done, in short, is to identify "the cause of an event" with the LNC criterion, or, as he puts it, "the single change which occurred in the immediately antecedent and adjacent circumstances of that event."[12] This restriction limits the scope of his analysis for, as Samuel Gorovitz has pointed out, if

> A is in the habit of turning on his radio each morning when he gets into his car, and B one night wires the radio switch to a bomb so that the car explodes the next morning when A turns on the radio, we would be required by Ducasse to say that the cause of the explosion was A's turning on the switch.[13]

In Gorovitz' example we would want to say that A's action in turning the radio switch, though it was "the single change which occurred in

the immediately antecedent and adjacent circumstances" of the explosion and was a necessary condition of the explosion, was part of the normal functioning of his daily life and so, a mere condition of the explosion. On the other hand, B's action, though it occurred many hours before A's is, like A's action, a necessary condition of the explosion, but it is something more than this. And it is in virtue of this something more that we deem it the cause of the explosion and not A's action. Whatever the additional feature possessed by B's action may be, it clearly goes beyond anything that Ducasse had to say in his analysis of singular causal judgments. Consequently, this analysis, while it does indicate how singular causal judgments may have as their justifying grounds the Method of Difference, cannot be a fully satisfactory account of the basis on which a distinction between the cause and other necessary conditions of an event is drawn.

A second and perhaps better-known attempt to provide non-arbitrary grounds for drawing this distinction is to be found in Part III of R. G. Collingwood's Essay on Metaphysics. Though this work was published some sixteen years subsequent to the appearance of Ducasse's book, Collingwood seems to have been unaware of Ducasse's analysis of singular causal judgments. Consequently, while Collingwood, like Ducasse, took up Mill's challenge to identify non-arbitrary grounds for distinguishing between causes and conditions his treatment of the subject differs considerably from that of Ducasse. I shall now go on to examine Collingwood's views on the cause-conditions distinction in Chapter II.

CHAPTER TWO: R. G. COLLINGWOOD: THREE SENSES OF CAUSE

(A) Introduction

Today R. G. Collingwood is best known as one of the pioneers of contemporary analytic philosophy of history. In a number of published essays, and in the posthumously published book, The Idea of History, Collingwood examined, criticized, and revised earlier conceptions of the nature and aims of historical inquiry. Drawing a sharp distinction between scientific and historical inquiry, the gist of Collingwood's idealist interpretation of the nature of historical inquiry can be seen in the following two sentences: "All history is the history of thought.... The history of thought, and therefore all history, is the re-enactment of past thought in the historian's own mind."[1]

In the years since his premature death a number of philosophers have explicated what they take Collingwood to have meant by these words and have considered the implications that follow from such a view of historical practice. Whether sympathetic or hostile to Collingwood's basic orientation towards the analysis of historical inquiry, writers such as W. H. Walsh, Patrick Gardiner, W. H. Dray, Louis Mink, Alan Donagan, and Rex Martin all acknowledge that his ideas are seldom presented in an unobjectionable manner and, indeed, many of his most fertile ideas are insufficiently developed. In this chapter we shall find that this is especially true of his theory of causation in history. While Collingwood does have some important things to say about the way historians employ the concept of cause, it will become apparent as our examination of his work proceeds that his theory of causation is marked by several internal weaknesses and can be criticized for its limited scope of application.

Collingwood's theory of causation is developed in two of his published works, The Idea of History and the Essay on Metaphysics. In both books an attempt is made to clarify the notion of a particularly historical sense of "cause." In The Idea of History Collingwood contrasts the historical sense of cause with what he takes to be a uniquely scientific one; while in the Essay on Metaphysics he develops a complex theory of causation, distinguishing three senses of cause, the first of which he calls "the historical sense."

It is somewhat surprising to find that in his most celebrated work, The Idea of History, Collingwood devotes only two paragraphs to a discussion of the historian's use of the word "cause." What he says there about "the historical sense of cause" invites comparison with what he says about a specifically historical sense of cause in the Essay on Metaphysics. As far as I know, no one has yet to raise the question of whether Collingwood's treatment of the historical sense of cause is the same in both of these books. I shall argue that in one important respect it is not.

For the most part I shall be looking at the theory of causation that is developed in the Essay on Metaphysics. As the title of this work may suggest, the analysis of causal notions offered therein is supposed to illustrate the way the science of metaphysics, as Collingwood conceives it, should be carried out. For him, metaphysics is a historical science whose subject matter consists of "absolute presuppositions." The metaphysician's task is to

> find out what absolute presuppositions have been made by this or that person or group of persons, on this or that occasion or group of occasions, in the course of this or that piece of thinking. Arising out of this, it will consider (for example) whether absolute presuppositions are made singly or in groups, and if the latter, how the groups

> are organized; whether different absolute presuppositions are made by different individuals or races or nations or classes; or on occasions when different things are being thought about; or whether the same have been made semper ubique, ab omnibus.[2]

The nature and aims of metaphysics, as conceived by Collingwood or anyone else, is not my concern in this thesis. But it is important to realize that Collingwood believed he was doing metaphysics when he presented his analysis of causal notions. "All metaphysical questions," he tells us, "are historical questions." (EM 49) And so, when he examines, in order, his three senses of cause he also claims to be tracing the historical development of certain absolute presuppositions. In speaking about the relation between the three senses of cause he also claims that "sense II and III logically presuppose sense I." (EM 292) In what follows I shall be concerned with neither the historical nor the logical relations that these three senses of cause are said to exhibit. Instead, I shall focus on the first two senses with a view towards determining their internal strengths and weaknesses as well as the extent to which they capture important aspects of the historian's use of causal language. Sense III of cause, or what Collingwood refers to as the concept of cause as employed in "Theoretical Natural Science," will not be discussed in this chapter, though one aspect of it will be brought up in connection with Mandelbaum's theory of causation, presented in Chapter VI. My reason for not discussing this sense of cause is chiefly that I find Collingwood's discussion of it obscure. But in any case, Collingwood seems himself to have regarded it as defective, and certainly as having no application to historiography.

Having narrowed the scope of my examination to Collingwood's first two senses of cause I should mention that it is only in respect to sense II that he attempts to defend the validity of the cause-conditions distinction against Mill's previously discussed views on this subject. Collingwood says nothing about whether sense I of cause involves a contrast between cause and conditions; I think, however, that my discussion of some of the ways in which a cause in sense I may fail to be sufficient to explain some historical event will make it clear that Collingwood should have applied the distinction to sense I as well as to sense II.

(B) Sense I of Cause

> Definition: that which is 'caused' is the free and deliberate act of a conscious and responsible agent, and 'causing' him to do it means affording him a motive for doing it. (EM 285)

This sense of cause is what Collingwood calls the "historical sense," for "it refers to a type of case in which both C and E are human activities such as form the subject matter of history." (EM 286)

On many occasions when historians ask why some agent acted in a particular way on a particular occasion it is indeed sense I of cause that is at issue. Thus, to ask why Brutus stabbed Caesar or why, upon taking office, Queen Elizabeth "etceterated" herself in her official title,[3] is to request an answer in the terms specified by sense I of cause. Given the prevalence of this type of explanation in historical works it can hardly be denied that sense I of cause constitutes a paradigm of causal thinking in history. Nevertheless, one of the more important questions we shall have to ask about this sense of cause is whether or not it is the only concept of cause historians employ and, indeed, whether Collingwood is correct when he characterizes the

subject matter of history exclusively in terms of human activities. For the time being I want to postpone discussion of these questions for there are several others that require attention at the outset. Among these are (1) In what way does Collingwood explicate the terms he uses to qualify the effect-factor and the cause-factor in the above definition?, (2) Can causing (affording a motive) be regarded as a sufficient or even a necessary condition for explaining the occurrence of a particular act?, and (3) Is the historical sense of cause that is discussed in the Essay on Metaphysics equivalent to the historical sense of cause spoken of in The Idea of History?

We can begin to answer the first question and raise some problems concerning the second by noting that a cause in sense I is said to be composed of two elements, a causa quod and a causa ut. The former is defined as "a situation or state of things known or believed by the agent to exist," while the latter is defined as "a purpose or state of things to be brought about," but "not a mere desire or wish, it is an intention." (EM 292-93) Each of these elements constitutes a necessary part of the cause of an action for "neither of these could be a cause if the other were absent." (EM 292) To illustrate this point Collingwood offers the following example:

> A man who tells his stockbroker to sell a certain holding may be caused to act thus by a rumor about the financial position of that company; but this rumor would not cause him to sell out unless he wanted to avoid being involved in the affairs of an unsound business. And per contra a man's desire to avoid being involved in the affairs of an unsound business would not cause him to sell his shares in a certain company unless he knew or believed that it was unsound. (EM 292)

While Collingwood never employed the terminology of necessary and sufficient conditions it seems appropriate to ask whether the presence

of a causa quod and a causa ut in an agent's mind is sufficient or even necessary to explain an agent's performing a certain act. Let me ask first, whether the presence of the two is sufficient for that purpose. Taking the example cited above as my paradigm, I think it is relatively easy to show that it is not.

To begin, what is the relevant causa ut in Collingwood's example? It is, he tells us, the man's wanting to avoid being involved in the affairs of an unsound business. This was his "purpose or state of things to be brought about." Suppose, however, that there was some other purpose that this man had which conflicted with his wanting to avoid being involved in the affairs of an unsound business. Suppose further that this man deemed more important the fulfillment of the latter purpose than the former one. Given these suppositions we would have a situation where someone is inclined towards acting so as to fulfill a certain purpose or to attain a certain goal, but forbears from doing so because he realizes that by doing this he would be defeating the purpose which he deems more important. The point, however, is that if we want to say that an agent took certain steps in order to fulfill a certain purpose we must first assume that he did not regard the fulfillment of any other purpose as being more, or even equally important.

Assume now that this condition has been met; the man in Collingwood's example does not believe that any other purpose he has conflicts with or is more important than his wanting to avoid being involved in the affairs of an unsound business. Now we turn to the causa quod. What is this? It appears to be the man's belief that the rumor he heard about a company's financial position (a company in which he has an interest) is true. Can we, at this point, deduce the action in

question--his telling his stockbroker to sell his shares in company C--from the statements about his purpose and belief? Apparently not. Referring to one of the examples Collingwood discusses in The Idea of History, Alan Donagan has pointed out that "Caesar did not act simply because a certain causa quod and causa ut were both in his mind, but because he put them together in an act of practical reasoning."[4] If Donagan is right, and I think he is, we could not deduce the action performed by the man in Collingwood's example (telling his stockbroker to sell his shares in company C) from the two premisses containing the statement of his purpose and belief, we would need at least an additional premise stating that "he put them together."

Even if we accept Donagan's criticism and modification of Collingwood's view of the tie between motivation and action it still does not go far enough, for although an agent may "put together" a certain causa quod and causa ut in an act of practical reasoning there remain several conditions that must be fulfilled before we can deduce the statement referring to the action in question. In this connection it is important to realize that between the formation of an intention to take steps to realize a certain goal and the initiation of those steps other factors come into play. For example, selling one's shares in a company one believes to be unsound is only one of several possible means of avoiding being involved in the affairs of an unsound business. Perhaps the man in Collingwood's example could have realized his goal by phoning the president of the company in which he held an interest and getting him or her to reorganize the company so as to make it financially solvent again. Thus, in order to deduce the action of telling the stockbroker to sell his shares in company C another premise is needed. Not only must this man believe that telling his stockbroker

to sell his shares in company C is a means to attain his goal, he must also believe that there was no other action which, as a means to achieving that goal, was to be preferred over the former action.

Another consideration that would have to be formulated as a premise in our argument is one which, because it is so obvious, has often been overlooked. As William Dray has pointed out, "every thought explanation, if it is to yield strict deduction of an overt action, requires a premise about the actual situation." What this means is that

> If the agent's control over his own body, at least, is a generally necessary condition of action in the historically interesting sense, then clearly we can never (as a matter of mere logic) deduce individual actions from statements about the agent's thought alone; we can never, as Collingwood has generally been thought to use the term, deduce the 'outside' from the 'inside'. We shall always need an "efficacy" premise, asserting something about the agent's powers and opportunities.[5]

The point that Dray is making about an agent's powers and opportunities is, of course, well taken. In the absence of the relevant powers and opportunities (e.g. paralysis or sudden lack of speech) the most that the man in Collingwood's example could do was attempt to tell his stockbroker to sell his shares in company C, not actually tell him to do so.

Another point that bears mention is the fact that quite often people form intentions to perform certain actions at some time in the future. This element, the lapse of time between the formation of an intention and the initiation of an action, makes it possible for people to forget what they had intended to do. Perhaps no additional premise is needed to account for forgetfullness, we can include remembering or being aware of one's intentions as one of the relevant powers that is mentioned by Dray.

By way of summarizing the points I have been making I would suggest that the following list of conditions constitute what Collingwood ought to concede to be the premises of his explanatory argument. Thus, we can say that the man in his example told his stockbroker to sell his shares in company C because:

(1) he wanted to avoid being involved in the affairs of an unsound business.

(2) he believed that company C, in which he held an interest, was financially unsound.

(3) he had no other purpose that conflicted with or was deemed more important than (1).

(4) he believed that telling his stockbroker to sell his shares in company C was a means to achieve his goal and that there was no other action preferable to this one as such a means.

(5) he "put together" (1)-(4).

(6) he was <u>able</u> to tell his stockbroker to sell his shares in company C.[6]

If we remove the specific content of the wants, beliefs, etc., mentioned above we would have a list of conditions which, it seems to me, is jointly sufficient for underwriting any claim to the effect that "an agent performed a certain action because...." Given that the first two conditions are the only ones Collingwood singles out as providing the warrant for action-explanations I think it is justified to claim that his view of the connection between thought and action presupposes a distinction between <u>the cause</u> and other conditions relevant to the explanation of an action.[5] However, I think there are at least two further types of action-oriented explanatory contexts in which the distinction is drawn more explicitly and forcefully. Before examining these it is first necessary that we respond to the latter half of the question posed earlier: Is having a motive (a <u>causa quod</u> and a <u>causa ut</u>) a necessary condition for action?

An unambiguous response to this question requires that a distinction be drawn between conditions that are "necessary in the circumstances" and ones that are "generally necessary." Generally speaking, a condition can be regarded as being "necessary in the circumstances" when there are no readily available substitutes to replace it.* The major problem that this notion of "necessary" raises for us is whether we can justifiably say that, in the absence of some particular kind of motivation, a certain type of action would not have occurred. A common denial of this counterfactual points to the fact that motives can sometimes be substituted for one another without affecting the type, manner of occurrence, place or timing of a particular action. For example, a given action may be prompted by envy but could just as well have been done for revenge.

Collingwood, of course, never discussed or even raised this difficulty. Had he done so I think that he would have been forced to acknowledge two things: First, that there may be more than one motive that could lead an agent to perform an action of a particular type on a particular occasion, there sometimes being present in an agent's mind a plurality of such motives, and, secondly, that actions resulting from mixed motivation do exist, i.e. when motive A is insufficient for C and motive B is insufficient for C, but together A and B are sufficient for C. Granting these two points, I think Collingwood would probably want to add that in order for a particular action to occur, some motive(s) must have been adopted by the agent, and this is what is necessary.

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*See Appendix A for the detailed account of this issue I presented in "What's the Problem With Causal Overdetermination," Philosophy of the Social Sciences, Vol. 11, No. 1 (March 1981), pp. 33-36.

If the line of reasoning I have attributed to Collingwood was indeed his view then what he would have been telling us is that having a motive is a "generally necessary" condition of action, i.e. without a motive an agent can perform no action. With one important qualification this does seem to me to be his view. The qualification I have in mind has to do with the fact that Collingwood has only deliberate action in mind. He is quite emphatic on this point. He tells us that "There may be cases where mere desire leads to action without the intermediate phase of intention; but such action is not deliberate, and therefore has no cause in sense I of the word." (EM 293)

This last statement makes it clear that Collingwood wanted to draw a line between action and deliberate action. Unfortunately he provides us with no examples to indicate where the line is to be drawn, so we are left to speculate on how he might have drawn it. One plausible suggestion is that he was thinking of a distinction endorsed by some contemporary theorists of action, a distinction between "acting intentionally" and "intending." In his book Theory of Action, Lawrence Davis draws such a distinction, the implication being that an agent who acts intentionally may have done so without having formed an antecedent intention to do so. Two of Davis' examples of acting intentionally are: "Sue stepped on the brake when a child suddenly darted out in the path of the car" and "Sal kicked the door in anger."[7] With respect to the first of these examples Davis adds: "she may say afterward that stopping the car, stepping on the brake, and avoiding the child are all things she intended. But she did not intend these things before acting."[8]

Considering the emphasis Collingwood placed upon re-enacting the thought of historical agents it seems reasonable to conclude that by "deliberate act" he means action that occurs after the agent has formed

the intention to do it. Consequently, various types of impulsive and compulsive behavior will have no cause in sense I of the word. If this reading of Collingwood is correct, then we could take him to be saying that although having formed an antecedent intention is not a necessary condition for action, it is one for deliberate action.

In asking whether an agent's having a causa quod and a causa ut is a necessary condition for action we were forced to distinguish between action and deliberate action and to offer a preliminary analysis of the latter notion. While I would not claim that his analysis is anything more than preliminary, I do think it has served to clarify the issue with which we began. In addition, it has supplied us with a partial answer to one aspect of the first question posed at the outset, this being, how does Collingwood explicate the terms he uses to qualify the effect-factor? In order to complete the answer to this question we must now try to understand what Collingwood means when he characterizes the effect factor as a "free and deliberate act."

Collingwood's notion of a "free and deliberate act" is closely tied to a particular conception of what it is to be a "free agent." This is evident when he writes: "If A causes B to do an act ϕ, ϕ is B's act and not A's; B is a free agent in doing it, and is responsible for it." (EM 293) The problem for us is to determine the sense in which he is using the term "free" in the last sentence of this passage. When an armed-robber points a gun at a bankteller and demands the money from the till, do we want to say of the bankteller, when he complies with the demand, that he "is a free agent in doing it, and is responsible for it"? Much depends on the way we are using the terms "free agent" and "responsible."

Let us ask, then, in what sense is the bankteller a "free agent" in handing over the money to the robber and is "responsible" for that act? What Collingwood would have to say is that in handing over the money the bankteller was acting "freely" in the sense that he "chose" to do it. Faced with such a response one feels impaled on the horns of a dilemma. In a certain sense we are inclined to agree with Collingwood. The bankteller could have refrained from complying with the robber's demands, that is, he could have chosen to do otherwise, regardless of the consequences that might befall him should he so refrain. Thus, he acted "freely," and this at least makes it very different than a case of sheer physical movement.

On the other hand we recognize that the bankteller's action was not done voluntarily. Although he chose to hand over the money we feel that he opted for the course of action that constituted the lesser of two evils. In cases such as this we often say that the robber "made," "forced," or "compelled" the bankteller to act as he did. Indeed, Collingwood draws attention to this feature of the use of causal language when he writes: "For 'causing' we may substitute 'making', 'inducing', 'persuading', 'urging', 'forcing', 'compelling', according to the differences in the kind of motive in question." (EM 290)[9] However, given the fact that we have already admitted that the bankteller's act was "free" in that he chose to do it, why do we now feel inclined to say that he was <u>not free</u> in doing it and, indeed, not responsible for what transpired?

The answer to this question can only be understood in light of the sort of defense the bankteller would offer if responsibility were ascribed to him for giving the robber the money. What he would probably say on his own behalf is this: "It was unreasonable for me to have been

placed in a situation where I had to make that choice." As such, responsibility is shifted away from the agent who did the act to the one who created the situation where such an act was presented as the lesser of two evils. Going further, we might not only deny that the bankteller was responsible for handing over the money but also that he was acting "freely" in doing so, his action was constrained, not free.

It seems, then, that the notion of a "free act" is ambiguous. Given that Collingwood appears to have been aware that human action may occur under constraint it is surprising that he did not qualify his thesis that when A causes B to do an act, "B is a free agent in doing it" to take account of this fact. In any case, the idea that one person may "force" another person to perform a certain action, that the second person may act deliberately but under constraint, is certainly familiar enough to us through our own experience. What may escape our attention though, and what has certainly escaped Collingwood's attention, is that we often apply the cause-conditions distinction to explain the occurrence of such actions. Thus, we say that the cause of the bankteller's handing over the money was there being a gun pointed at his head. At the same time we recognize that other conditions were necessary for that action to occur, namely, those enumerated in our scheme for action-explanations. Paradigmatic of cases in which the cause-conditions distinction is drawn in this fashion are ones where one person is provoked by the words or deeds of another person to act in his own self-defense or to protect his vital interests.

In an article that we shall be examining at greater length in Chapter III, "Concepts of Causation in A. J. P. Taylor's Account of the Origin of the Second World War," William Dray has pointed out that this way of drawing a distinction between causes and conditions finds

expression in Taylor's attempt to explain the events leading up to the Second World War. Dray has found that on numerous occasions in Taylor's The Origins of the Second World War, Hitler is portrayed

> not only as failing to take the initiative himself, but as being forced continually to respond to the initiatives of others. He is maneuvered, induced, incited, and even driven to act by events and situations not of his own making.[10]

Thus, in separate incidents, it is the Austrian Nazis, the Sudeten Germans, and the Austrian Chancellor Von Schuschnigg who placed Hitler in a situation where he simply "had to act." "The cause of what ensued," writes Dray, "was therefore not what he did himself; it was what was done by those who put him in that unfortunate position, and whose own actions were not similarly forced."[11]

Dray goes on to point out that critics of Taylor's view of Hitler's role in bringing about the Second World War do not try to deny the applicability of the model of causal thinking Taylor has appealed to: rather, they assume the same model but adduce evidence and formulate arguments to undermine his judgment concerning who was forced to act and who was doing the forcing. As such, the focal point of the debate can be seen as a competition for causal status between two or more deliberate, but not equally free, human acts. Deciding which acts were free and which unfree also serves to differentiate cause from mere conditions.

As one who was a practicing historian, had some understanding of the cause-conditions distinction, and recognized the importance of dealing with the notion of a "free act," it would appear that Collingwood possessed the resources to make the kind of observations Dray has made. That he did not do so is unfortunate, for such omissions

limit the scope of both his theory of causation in history and his theory of action.

Before elaborating further on other external limitations of Collingwood's theory of causation in history I would like first to deal with the third question posed at the outset, for here we are confronted with the prior issue of whether Collingwood's notion of a particularly historical sense of cause is internally consistent. It was stated earlier that those who have had something to say about Collingwood's conception of historical causation (and there have been many) never raised the question of whether his discussion of a particularly historical sense of cause in the Essay On Metaphysics duplicates his discussion of a particularly historical sense of cause in The Idea of History. Consistency requires that this should be the case, any differences in the two accounts being incidental to the core content of the notion of a historical cause. However, as the reader may have already surmised from the whole of our previous discussion of Collingwood's views, the view of a historical cause that is outlined in the Essay On Metaphysics really contains two distinct concepts of cause, only one of these being the subject of Collingwood's discussion of historical causes in The Idea of History.

In the latter work Collingwood contrasts the notion of a historical cause with that of a scientific cause in this way:

> When a scientist asks 'Why did that piece of litmus paper turn pink?' he means 'on what kinds of occasions do pieces of litmus paper turn pink?' When a historian asks 'Why did Brutus stab Caesar?' he means "What did Brutus think, which made him decide to stab Caesar?' The cause of the event for him, means the thought in the mind of the person by whose agency the event came about....(IH 214-15)

This notion of what it is to be a historical cause is one we began examining at the outset of this section. Historical causes are thoughts in the minds of human agents who subsequently engage in action. Such thoughts, claimed Collingwood, can be broken up into a causa quod and causa ut. The schema for action-explanations that we offered was based solely on considerations arising from this claim. On the other hand, once we started looking at Collingwood's notion of a "free act" the emphasis shifted to a consideration of the view that the act of one person may be the cause of an act performed by another person. This concept of cause is surely different from the one which focuses on the thoughts of agents. The difference I am alluding to shows up quite clearly in a passage in the Essay On Metaphysics, though I am convinced that Collingwood did not recognize it as such:

> Causes in sense I of the word may come into operation through the act of a second conscious and responsible agent, in so far as he (1) either puts the first in a certain situation in such a way that the first now believes himself to be in that situation, or alternatively informs or persuades the first that he is in a certain situation; or (2) persuades the first to form a certain intention. In either of these two cases, the second agent is said to cause the first to do a certain act, or to 'make him do it'. (EM 293 italics mine)

The words I have italicized in this passage should have alerted Collingwood to the fact that he was dealing with still a further concept of cause. What he appears to be saying is that while the action of one agent has a cause (sense I), it may be an effect of an action taken by a second agent. This, however, introduces an entirely new sense of cause. Why Collingwood did not realize that he was dealing with two distinct concepts of cause is open to speculation. I shall refrain from expressing my own thoughts on this issue because I think

that it is more important to see that the "other" concept of cause that emerges from the passage cited above should not be restricted to human actions.

If a cause in sense I is the thought in the mind of the agent who performs a certain action, then whatever or whoever is responsible for putting that thought in the agent's mind will be a cause in, let us call it 'sense I(a)'. A cause in sense I(a) need not be restricted to the acts of "a second conscious and responsible agent." Anything that triggers a change in a person's volition-belief structure, including the behavior of non-conscious and non-responsible agents, can be a cause in sense I(a). Thus, a drought, earthquake or some other physical event may cause the members of a tribe to migrate from one geographical location to another. In such cases it is simply false to say that cause and effect "are human activities such as form the subject matter of history." Though a migration may be a human activity, an earthquake surely is not. There are still other types of causal explanations that do not cite human actions as causes. For example, consider a case where a historian is attempting to explain why one side was victorious and the other side defeated in a naval battle. Undoubtedly such a historian will mention a variety of human actions. Nevertheless, in the end he might choose to explain the outcome of the battle in terms of the state of technical competence exhibited by the kinds of weapons and means of transport utilized by each side. As such, it is the idea of being capable or incapable that is at issue here. The cause of victory and defeat, then, while it may pertain to the beliefs and volitions of the agents involved in the battle, is not a belief and/or a volition itself.

These last remarks bear directly upon that most important question raised at the outset of my examination of sense I of cause, i.e. whether, as Collingwood claimed, sense I of cause is the only concept of cause that historians appeal to. Pending further analysis I think that the last two examples of the sorts of conditions historians are apt to cite as the causes of certain events already render Collingwood's claim suspect. In order to see more clearly what the limitations of his view of historical causation are, let me ask why it is that historians do not always cite human beliefs, wants, and choices as the cause of occurrences to be explained, even when these are generally necessary conditions for such occurrences.

It seems to me that there is not one answer to this question, but several. One very obvious reply, though, is that historians are not always interested in determining the causes of human actions, sometimes they are interested in identifying the causes of what has happened to human beings. Such a view explains why historians will sometimes seek causal explanations for occurrences like epidemics. While it can be argued that such explanations are the exclusive concern of the epidemiologist I think that the very same type of explanation may be constructed by a historian in the course of giving an account of, say, the social effects caused by the introduction of new farming and irrigation techniques into a certain region of a country. Thus, it may be found that a dramatic increase in the number of deaths due to a certain disease was caused by the introduction of new irrigation techniques, for the creation of reservoirs provided an environment in which the infectious agent could flourish.

In some varieties of historical writings the fact that the historian is not interested in determining the causes of specific human

actions is quite apparent, the unit of historical interest being "populations" rather than individuals. By "population" I do not mean only the inhabitants of a circumscribed geographical region, but any group of people who possess certain common properties (e.g. American women over thirty-five years of age) or incidents involving human beings (e.g. homicides). Historians who adopt as their unit of study populations often seek to establish trends and to identify the causes of fluctuations in such trends. These trends are reflected in birth rates, mortality rates, and the like. While these rates do refer to human activities they are not themselves human actions.[12] Consequently, sense I of cause will be inadequate for analyzing the sort of causal connections depicted in historical writings of this type.

Causal analyses of changes in the incidence of certain types of occurrences are, of course, based upon statistical evidence. However, historians sometimes treat qualitative changes that populations undergo in exactly the same way. This tendency is especially prevalent in historical accounts that trace the development of a country from the time of its discovery or settlement to some later point in time. For example, the prominent Canadian historian, Arthur Lower, has, in a number of articles and books, tried to explain the particular character of Early Canadian political life by reference to the environment encountered by the French and English settlers. When contrasting New France with New England before 1763, Lower notes that both colonial societies were confronted by similar environmental conditions and responded to them in similar ways. Thus, in Colony To Nation, Lower claimed that

> French life and society in America departed considerably from authoritarianism and in spirit approached

> English life and society in America where conditions were so uniform as in the settlers' attack on the forest, and where it was the worth of a man as a man, as an axe-swinging, forest-clearing, crop-sowing, animal, that counted, the same qualities came to the fore, the same scale of values tended to prevail....North American democracy was forest-born. It carried with it a stubborn attachment to the rights and privileges that come from an independent life and a disinclination to coercion that was a strong defense against arbitrary authority.[13]

Lower's thesis, then, is that when we compare the character of life in France and England (characterized by "privilege" and "aristocracy") with that of New France and New England (characterized by social equality) the difference in the nature of social relations that we observe can only be explained by reference to the uniform influence exerted by the new environment upon the settlers. The question remains, however, why doesn't he cite the wants, beliefs, and choices of the settlers as the cause of the changes to be explained?

The answer, I suggest, is that he is implicitly assuming a certain model of causal connection that incorporates, but is much wider than sense I of cause. This model depicts a relation between three sets of factors, a factor that is an independent variable (the suspected cause), one that is a dependent variable (the effect) and other factors that are constant. Now the reason why Lower does not cite the wants, beliefs, and choices of the settlers as causes is that they fall into the category of factors that are deemed constant, and a constant cannot explain a variable, i.e. a change.

At this point the reader may be inclined to ask, what is the basis for this claim? Well, I must admit that it is not to be found in anything Lower says. But I suspect that what he was committed to saying goes something like this: Randomly select a group of people from eighteenth-century France and England, transport them to the eastern

seaboard of North America and leave them there to fend for themselves. What you will find is that their old social habits will change in much the same way as did those of the actual settlers in New France and New England. As such, it is not what the settlers were thinking that ultimately explains changes in their social relations, but the nature of the environment they had to adapt to if they were to survive.

The thought-experiment I have attributed to Lower explains the sense in which natural events and conditions may be cited as the causes of human actions or changes that involve human beings. The same model of causal connection is often used by social historians to explain how inanimate objects such as inventions cause human actions or the outcomes of such actions. In this connection consider how William Ogburn explicates the meaning of the statement "the automobile created the motel." Ogburn notes that even though it is human beings who choose to build motels and are responsible for the actual construction of these, the former expression is employed "because the [independent] variable is the automobile and not the human beings." He then goes on to say that

> the effect we are trying to explain is the new existence of motels. At one time there were no motels, at another time there were. This variation from no motels to motels must be explained by a variable, which was the automobile and not the drivers or users. Potential drivers and users existed before motels and after motels. They were always there in large numbers, hence are denoted as constant. So we do not say it was the users of automobiles but rather the automobile that created motels; although a factor in their creation was human beings.[14]

Ogburn's explanation, I submit, makes clear the sense in which inanimate objects can be cited as the cause of social change. That historians do employ this model of causal connection is incontestable. Patterns of settlement are explained by reference to railroad routes,

the alignment and re-alignment of world powers is explained by reference to the technology of war, the health of a population is explained by reference to new methods of farming.

To sum up, there are at least three considerations that explain why historians do not always cite human wants, beliefs, and choices as the causes of certain historical events, even when these are necessary conditions for the occurrence of such events. These are (1) the nature of the events to be explained, (2) the unit of historical study adopted, and (3) the model of causal connection employed. To avoid misunderstanding I should point out that I am not denying that, from an ontological point of view, much of what has happened to human beings can be reduced to the thoughts of individual agents. My point is methodological. What I am saying is that historians often find it unnecessary to cite these thoughts when they seek to establish causal connections that obtained in the past.

Does this mean that the three-factor model of causation outlined above is itself an expression of the cause-conditions distinction? Not quite. The view I want to develop--and it cannot be developed in detail until we have had a chance to examine the more sophisticated conception of the cause-conditions distinction advanced by Hart and Honore--is that the distinction is but one of a number of species of the basic pattern of causal reasoning depicted in that model. For now, however, we must remain content with having shown that there are three points at which the cause-conditions distinction bears directly upon Collingwood's sense I of cause. First, our schema for action explanations has made it clear that judgments which cite an agent's wants and/or beliefs as the cause of his action presuppose the fulfillment of a variety of other conditions, second, that the free and deliberate

act of one person may be cited as the cause of an unfree but deliberate act of a second agent and, third, that non-actions, e.g. physical events, may be cited as the cause of human actions. These considerations are enough to show that Collingwood's notion of a historical cause, though essential for understanding one very prevalent type of historical explanation, does not do justice to the scope and variety of causal connections that are to be found in the writings of historians. I conclude, then, that sense I of cause is not the only concept of cause employed by historians.

(C) Sense II of Cause

Had Collingwood been right in claiming that historians use the word cause only in sense I of that word it would not be necessary for us to consider sense II of cause at all. But I think I have already said enough to show that sense I of cause does not tell the whole story. Perhaps, then, sense II of cause can add something to it. I think it can, despite Collingwood's view that sense II of cause has nothing to do with the way in which historians talk about causes. This is not to say that I find his characterization of the elements composing sense II of cause totally acceptable. Indeed, I shall argue that it is not. Nevertheless I do think that the mistakes he makes are very instructive, for they force us to analyze in greater depth the key elements that characterize sense II of cause. In this respect one of the more important claims I want to establish in this section is that Collingwood's view of the role played by the concept of "control" in sense II of cause is but one aspect of the larger role played by that concept in various kinds of systematic inquiry into causal connections, including historical inquiry into such connections. Before I can even

begin to argue this claim we must first present and evaluate Collingwood's definition of sense II of cause and then look at the way in which he thinks this sense of cause involves a contrast between cause and conditions.

> Definition: A cause is an event or state of things which it is in our power to produce or prevent, and by producing or preventing which we can produce or prevent that whose cause it is said to be. (EM 296-97)

This sense of cause is one which Collingwood takes to be paradigmatic of what he calls "Practical Natural Science," e.g. medicine, agriculture, engineering. What all such sciences have in common is the express aim of manipulating physical things, events, and processes in such a way as to fulfill human wants and needs. The development of vaccines, hybrid plants, and steel alloys are examples of successful attempts to manipulate naturally occurring phenomena so as to achieve results that are in accord with such wants and needs. However, the fact that sense II of cause finds its most systematic expression in the practical natural sciences should in no way overshadow the point that this sense of cause is exhibited in the kinds of causal statements that non-scientists frequently make. This is quite evident from some of the examples Collingwood offers to illustrate the presence of sense II of cause, viz; "the cause of a furnace going out in the night is that the draught-door was insufficiently open." (EM 299)

The basic idea that underlies sense II of cause is that "there are certain ways in which natural things behave if left to themselves, but that man, being more powerful than they, is able to thwart their inclination to behave in these ways and make them behave not as they like but as he likes." (310) Stated differently, the point is that by harnessing natural forces human beings can make nature work for them.

Plant growth can be enhanced by the development and application of synthetic fertilizers; plant destruction can be prevented by the development and application of pesticides. In all cases of this sort two conditions must be fulfilled: (1) "The thing described as a cause is always conceived as something in the world of nature or physical world," and (2) "it is always something conceived as capable of being produced or prevented by human agency." (EM 299)

I think it is probably true to say that all of us can think of occasions when we have made judgments about the cause of some event that fulfills the two conditions laid down by Collingwood. Still, I think it is very important to ask the following question: Are these conditions sufficient to explain our propensity to accept or reject a wide variety of judgments that cite as the cause of some event a condition that is insufficient though necessary (in the circumstances) for that event to occur? Taking this question as my standard for assessing the extension of the word "cause" in sense II, I shall argue that Collingwood's definition of that word is both too broad and too narrow; too broad because it would apply to judgments of the cause which we find unacceptable, too narrow because it excludes judgments of the cause that we find completely acceptable though the condition so cited is not amenable to human control. In order to get on with the job of responding to the question I have posed we need to see in precisely what way Collingwood conceives of sense II of cause as involving the cause-conditions distinction.

Sense II of Cause and the Cause-Conditions Distinction

It will be recalled that in the view of J. S. Mill the "real cause" of any effect is its invariable unconditional antecedent. Such

antecedents, he recognized, are virtually always complex, being composed of several conditions. Moreover, while acknowledging that people quite often single out one of these conditions and call it "the cause," Mill protested that such selection is arbitrary.

Without denying that there is a sense of cause in which "all the conditions" of a given result are denoted by that term (sense III of cause), Collingwood believed that Mill's explication of the cause-conditions distinction was inadequate. According to Collingwood,

> closer inspection would have shown him that the 'selection' of one condition to be dignified by the name cause is by no means arbitrary. It is made according to a principle. The 'condition' which I call the cause (in sense II) of an event in which I take a practical interest is the condition I am able to produce or prevent at will. (EM 302)

Thus, it is suggested that when I turn a switch and a light comes on it is entirely appropriate to cite the position of the switch as the cause of incandescence even though there are other conditions that must be satisfied in order for that effect to occur, i.e. "the existence of an appropriate current and its maintenance by insulation and contacts." (EM 302) Another example presented by Collingwood to highlight his point that the condition cited as the cause of an event is one which is subject to human control is this:

> if my car fails to climb a steep hill, and I wonder why, I shall not consider my problem solved by a passer-by who tells me that the top of a hill is further away from the earth's center than its bottom, and that consequently more power is needed to take a car uphill than to take her along the level. (EM 302)

While Collingwood admits the truth of what the passer-by maintains and acknowledges that it would form one of the conditions in Mill's "real cause," he insists that an appropriate singular causal judgment is made

by an A. A. man when he discovers that a high-tension lead is loose and that, consequently, the car is running on only three cylinders. This judgment is said to be appropriate, but not the one made by the passer-by, because it informs him of the condition he can put right, after which the car will be able to climb the hill. But at the same time Collingwood concedes that if he were a person "who could flatten out hills by stamping on them" then the passer-by's remarks would also have been appropriate.

This concession only seems to reinforce the point that the condition cited as the cause of an event must be one that is amenable to human control. Going one step further, Collingwood then points out that people sometimes cite different conditions of an event they would like to control as the cause of that event. This situation is perfectly intelligible, for each person may actually possess the ability to produce or prevent the event for which they claim to have discovered the cause. With this in mind Collingwood introduces what he calls "the principle of the relativity of causes." This principle states that "for any given person the cause in sense II of a given thing is that one of its conditions which he is able to produce or prevent." (EM 304) To illustrate the operation of this principle in a concrete situation Collingwood invites us to consider the following example:

> A car skids while cornering at a certain point, strikes the curb, and turns turtle. From the car-driver's point of view the cause of the accident was cornering too fast, and the lesson is that one must drive more carefully. From the county-surveyor's point of view the cause was a defect in the surface or camber of the road, and the lesson is that greater care must be taken to make roads skid-proof. From the motor-manufacturer's point of view the cause was the defective design in the car, and the lesson is that one must place the center of gravity lower. (EM 304)

The principle of the relativity of causes does appear well suited to the example Collingwood has devised, for each of the three parties involved is assumed to have possessed the ability to prevent the condition which, from their respective points of view, is claimed to have been the cause of the accident. Yet it is not entirely clear what Collingwood wishes us to conclude from his statement of this principle. Are the driver, county-surveyor, and auto manufacturer making rival causal claims or are their statements of the cause to be construed as complementing one another? What we are asking, in other words, is whether any of the conditions regarded as the cause from the different points of view can be said to be the true, decisive, or most important cause?

Experience teaches us that parties involved in automobile accidents usually deny that they were responsible for such accidents. This fact alone would lead us to presume that the people involved in the accident Collingwood describes were making rival causal claims. Yet Collingwood explicitly rules out the suggestion that the causal judgments made by the driver, county-surveyor, and auto manufacturer can be rival ones so long as we are still using the word cause in sense II. Thus, he maintains that

> If the driver, the surveyor, and the manufacturer agreed in thinking they know the cause of the accident I have described, but differed as to what it was, and if each thought that it was a thing which one of the others could produce or prevent, but not himself, the result would be that none of them would do anything towards preventing such accidents in the future, and their so-called knowledge of the cause of such accidents would be a 'knowledge' that was not, and did not even bring, power. (EM 305)

Though I do not wish to dwell on this point, one way of impugning Collingwood's justification for denying that the causal judgments made

by the three parties are rival ones is to point out that it is not necessarily true that when people make rival causal judgments nothing is accomplished by way of preventing the sort of events they want to prevent. Suppose, for example, the driver charges that it was a defect in the design of the car that was the cause of the accident. Must we conclude from this that he is not doing anything towards preventing such accidents in the future? No, for if the driver's case is successful the courts may order the manufacturer to recall that make and model of automobile in order to correct the defective design. Similar arguments could be advanced by the county-surveyor and auto manufacturer as well. The point, however, is that if Collingwood wants to discount the view that judgments which fulfill the requirements of sense II of cause can be rival ones, the argument he offers fails to establish that conclusion.

It seems to me that once Collingwood introduces the principle of the relativity of causes his account of sense II of cause becomes increasingly implausible. One of the basic weaknesses of his account surfaces when, after denying that the driver, county-surveyor, and auto manufacturer could be advancing rival causal claims, he concludes: "Hence the folly of blaming other people in respect of an event in which we and they are together involved." (EM 305) What Collingwood does not seem to realize is that the nature of a person's "involvement" in an event is crucial to the causal judgments made in respect to that event. His failure to make this point, I would suggest, is what leads him to accept some strange implications that follow from his characterization of the principle of the relativity of causes.

In order to add some weight to my argument consider the conclusion Collingwood would be forced to accept if we add a further supposition

to his example. Suppose that it is found that the driver of the car which was in the accident had gas pumped into his almost empty gas tank only fifteen minutes before the occurrence of the accident. Now no one would deny that having sufficient fuel to enable the car to get to the point where the accident occurred was necessary (in the circumstances) for the accident to occur. Moreover, it seems quite clear that this condition was subject to control by the gas station attendant. Why is it, then, that we would not want to say, as Collingwood must, that from the gas station attendant's point of view the presence of fuel in the car was the cause of the accident? Again, consider the implications we would be forced to accept if we apply Collingwood's principle of the relativity of causes to an example discussed earlier, where A one night wires a bomb to the radio switch in B's car, knowing that B turns on the radio each morning when he drives the car. The next morning when B turns on the radio he detonates the bomb and is killed. Now if we follow Collingwood we should have to say that from B's point of view his turning the radio switch was the cause of the explosion that killed him for it was a condition that was subject to his control. I think that most people would find such a judgment preposterous and would probably reject the theory of causation on which it is based.

The upshot of the last two examples is that Collingwood's definition of sense II of cause is too broad, for there are conditions which are both insufficient and necessary (in the circumstances) for a particular event, and are subject to human control, but which we would not want to call the cause of the event with which it is associated. Taking my argument one step further, I would also suggest that Collingwood's definition of sense II of cause is too narrow, for there

are occasions when people appear to have good grounds for claiming that they have discovered, but are unable to control, an insufficient condition which they regard as the cause of some event or effect. As such, I think we should reject the corollary Collingwood draws from the principle of the relativity of causes, i.e. "for a person who is not able to produce any one of its conditions a given event has no cause in sense II at all....for a mere spectator there are no causes." (EM 306)

Based on this corollary, Collingwood maintains that it would be a "nonsense statement" for someone to profess to have discovered the cause of cancer if he acknowledged that he could not produce or prevent at will the condition cited as the cause. Someone who made such a claim would, in Collingwood's words, "be ridiculed by his colleagues in the medical profession." (EM 300) This is, of course, nonsense. A scientist may isolate some condition which causes a certain type of disease without thereby possessing either the knowledge, tools, or technique for controlling it. Taken to its logical extreme, Collingwood's view would have us deny that in the example discussed earlier, the passer-by discovered the cause of the car's failure to climb the hill if he had said exactly the same thing as the A. A. man but admitted that he lacked the ability to repair the malfunctioning cylinder. Such a denial would obviously be a mistake.

It seems to me abundantly clear that "mere" spectators can form quite intelligible and valuable opinions about the cause of some event they have witnessed or otherwise have studied, though they could do nothing to prevent that event from occurring. Historians are a good case in point, for they cannot control nor, in the vast majority of cases, could they have controlled conditions that occurred in the

past. Surely, though, no one would want to subscribe to the view that a historian could not know the cause of some event that occurred in the past. Yet, incredibly, Collingwood does seem committed to this view so long as he insists on his corollary, without realizing that his definition of sense II of cause sometimes applies to the singular causal judgments made by historians. What I mean is that historians sometimes cite as the cause of some historical event a condition which is selected because it was subject to some agent's control, e.g. when it is claimed that lack of ammunition was the cause of an army's defeat in battle. Given that such a causal judgment does conform to the definition of sense II of cause, I fail to see what Collingwood hoped to gain by making its scope of application subject to the corollary he formulates.

Anyone is free to give a stipulative definition to any term, including "cause." But the fact that one defines a term in a given way provides no assurance that such a definition is representative of actual usage. Now it seems to me that when Collingwood began his discussion of sense II of cause he was attempting to show how it conforms to causal judgments that are typical of the way people actually talk about causes. However, once he introduces the principle of the relativity of causes his account of sense II of cause begins to deviate from actual usage in several important ways. Since, to my knowledge, Collingwood was not recommending that actual usage be altered to conform to the conditions he imposes on the definition of sense II of cause, it seems to me that two conclusions are open to us. We may conclude that (1) the application of sense II of cause is restricted only to those occasions on which a claimant can produce or prevent the

condition cited as the cause of an event, or (2) there is another principle, more general in its scope than Collingwood's controllability criterion, which explains both the successes and failures of that criterion.

It seems to me that the latter conclusion is the one that we should adopt. In opting for this alternative I do not feel that I am proposing something that goes entirely beyond what Collingwood had to say about the grounds upon which singular causal judgments are made. There is a certain irony here, analogous to the situation that obtained with respect to Mill's rejection of the cause-conditions distinction while he appeared to make this distinction when presenting simple cases wherein the Method of Difference operates. What I have found is that in challenging Mill's view that the condition cited as the cause is "selected" at all, let alone arbitrarily selected, Collingwood makes a point which provides the basis for a principle that incorporates, yet goes beyond his controllability criterion. Thus, against Mill he asserts that:

> the 'condition' which is thus 'selected' is in fact not 'selected' at all; for selection implies that the person has before him a finite number of things from among which he takes his choice. But this does not happen. In the first place the conditions of any event are quite possibly infinite in number, so that no one could thus marshal them for selection even if he tried. In the second place no one ever tries to enumerate them completely. Why should he? If I find that I can get a result by certain means I may be sure that I should not be getting it unless a great many conditions were fulfilled; but so long as I get it I do not mind what these conditions are. If owing to a change in one of them I fail to get it, I still do not want to know what they all are; I only want to know what the one is that has changed. (EM 303 italics mine)

The process that leads to the identification of some condition as the cause of an event is not one of selection, but of elimination. One

condition after another is eliminated as a possible cause until we find a condition that has changed. We presume that a system operates normally unless and until something changes. When something changes in the system we then look for a corresponding change to explain it. This is why we would not want to say that in our three automobile examples previously discussed that gravity (the condition cited by the passer-by), the presence of gasoline in the car, and B's turning on the radio could be the causes of the events for which they were insufficient but necessary (in the circumstances). It also explains why we find it perfectly acceptable to say that "spectators" may know the cause of some event though they cannot or could not manipulate the condition so cited.

In writing the passage cited above Collingwood was apparently unaware of the fact that he was introducing a principle that was both different from and wider than his controllability criterion. Certainly he makes no attempt to restrict the notion of a condition that changes to one that is subject to human control, and this is as it should be. But notice also that Collingwood's example is virtually the same as the one proposed by Ducasse. For Ducasse, it will be recalled, the cause of the failure of the automobile to run was "the single change which occurred in the immediately antecedent and adjacent circumstances." Although it was necessary to criticize the temporal restriction Ducasse placed on the identification of the cause, we noted that his analysis does indicate how the Method of Difference supplies the justifying grounds for such singular causal judgments. It is entirely appropriate, then, that we apply the same conclusion to Collingwood's analysis of sense II of cause. Once we do this it then becomes clear that the role played by the idea of "control" in identifying the cause of an event is

different than the one Collingwood attributed to it. Its fundamental importance lies in the fact that one or more entities or processes is treated as a "control" subject or group of subjects, rather than these entities or processes being amenable to physical manipulation (control in Collingwood's sense).

If the revisions I have proposed to Collingwood's account of sense II of cause are sound, then the procedure whereby such causes are identified would constitute one aspect of what Ernest Nagel has called "controlled investigation." For Nagel, it seems to me, controlled investigation is simply investigation that utilizes controls. In his own words though, controlled investigation

> consists in a deliberate search for contrasting occasions in which the phenomenon is either uniformly manifested or manifested in some cases but not others, and in the subsequent examination of certain factors discriminated in those occasions in order to ascertain whether variations in these factors are related to differences in the phenomena.[15]

Applying Nagel's notion of controlled investigation to the automobile examples offered by Ducasse and Collingwood we would say that the driver contrasted the occasion on which the phenomenon was manifested (i.e. the car would not run) with previous occasions when the phenomenon was not manifested (i.e. when the car was running) in order to determine whether there was some variation in the factors present on the occasion when the phenomenon was manifested, but not on the previous occasions.

In the next chapter we shall examine the way that Nagel applies the notion of controlled investigation to what I have previously called ex post facto or retrospective experiments. There we will find that the

concept of control takes on an added dimension. For present purposes, however, it is enough that we have shown that Collingwood's manipulability criterion needs to be replaced by a different concept of control in order to account for many cases where a distinction between cause and conditions is drawn.

CHAPTER THREE: H. L. A. HART AND A. M. HONORÉ: CAUSES CONDITIONS, AND CONTEXTS OF INQUIRY

(A) Introduction

In the last chapter we saw that Collingwood challenged Mill's view that when we say that X was the cause of Y what we always mean is that X is "the sum total of the conditions, positive and negative, taken together, which being realized, the consequent invariably follows." (SL 200) While he was willing to allow that Mill's notion of the meaning of cause constitutes one sense of what it is to be a cause of some event (sense III) he proposed two further senses of cause. What distinguishes these two senses from Mill's is that they both involve a contrast between cause and conditions whereas Mill's notion of a cause does not. It was this claim, along with the criteria he proposed for the non-arbitrary application of the cause-conditions distinction, that constitutes Collingwood's contribution to the analysis of causal notions.

In turning our attention now to H. L. A. Hart and A. M. Honoré's analysis of causal concepts we may take as our starting point the observation of some important similarities between their approach to problems of causation and that of Collingwood. There are four points to be mentioned in this connection. First, both Collingwood and Hart and Honoré challenge Mill's rejection of the cause-conditions distinction, and at least one of the reasons they have done so is the same, viz. the recognition that there are rules or principles governing the discrimination of causes from conditions. Second, Hart and Honoré applaud Collingwood for having been the first one to recognize the "principle of the relativity of causes." Thirdly, like Collingwood, these authors dismiss the idea that what is called the cause of some

event is "selected" from an assortment of other conditions. And finally, they seem to share the view that explanations of causal connections between physical events require generalizations in a way in which explanations of connections between human actions do not.

Despite this not insignificant measure of agreement, Hart and Honoré are critical of Collingwood's views on what rules are most appropriate for distinguishing causes from conditions and of his explication of the principle of the relativity of causes. In addition, they explore a much wider range of issues bearing on the topic of causation than did Collingwood.

In a series of articles in the Law Quarterly Review[1] and in their much discussed book which bears the same title as their earlier articles, Causation In The Law[2], Hart and Honoré attempt to sort out and to resolve various enigmas surrounding the use of causal language in courts of law. Their views on the nature and role of causation in the law are developed against a double background. On the one hand they argue that traditional philosophical accounts of causation (primarily Hume's) have little or no bearing upon the particular causal issues faced by the judge and lawyer; on the other hand they are sceptical of what they see as a growing trend among jurists to try to eliminate the notion of cause altogether from the law or to reduce its scope to a minimum, substituting in its place "policy" considerations alone.

In attempting to realign problems of causation in the law within a broader philosophical context and to resist the reduction of causal issues to questions of legal policy, Hart and Honoré claim that the complex causal questions courts have to face can only be understood, though not always conclusively answered, in terms of certain principles latent in common sense notions of causation. Two claims that make their

analysis of causal issues in the law especially relevant to the topic of this thesis are (1) that "the causal statements of the lawyer and historian are like the causal statements most frequent in ordinary life; they are singular causal statements identifying in complex situations certain particular events as causes, effects, or consequences of other particular events," (9) and (2) that "the contrast of cause with mere conditions is an inseparable feature of all causal thinking." (11) Here I shall comment only briefly on these two claims for their implications will be developed in greater detail in subsequent sections of this chapter.

When reading through the introduction and the first chapter of Causation in the Law one is struck by the number of references the authors make to "the lawyer, historian, and ordinary man." They use this phrase, or some variation thereof, at least a dozen times. The major point they wish to convey in this connection is that unlike the scientist who strives to formulate laws or generalizations connecting types of events, the lawyer, historian, and ordinary man are "primarily concerned to make causal statements about particulars, to establish that on some particular occasion some particular occurrence was the effect or consequence of some particular occurrence." (8-9) Hart and Honoré then suggest that most philosophical thought about causation, stemming as it does from Hume's concern with the supposed necessary connection between cause and effect, is quite remote from the problems engendered by the singular causal judgments made by lawyers, historians, and "plain men." They argue, moreover, that "Hume's terminology is almost perfectly designed to conceal their existence." (15)

Two difficulties attending Hume's terminology that Hart and Honoré find particularly noteworthy are, first, that he invariably refers to

particular causes as "events" or "objects" when what is designated as a cause by lawyers, historians, and ordinary people include persistent states, the failure of events to occur, omissions, and the provision of opportunities. Secondly, Hume speaks as if it were pairs of single events that are causally connected when, as Mill properly noted, "it is seldom if ever between a consequent and the single antecedent that this invariable sequence consists. It is usually between a consequent and the sum of several antecedents, the concurrence of all of them being requisite to produce, that is to be certain of being followed by the consequent." (SL 200) Though Mill retained Hume's emphasis on invariable sequence; adding that such sequences must be unconditional as well as invariable, his observation that causes are complex sets of conditions provides the framework for analyzing the most important issue generated by singular causal statements, this being, upon what grounds is one member of a set of conditions, which set is sufficient for some effect, denominated the cause of that effect?

This last question becomes of great importance in the law when limits must be set upon tracing causal connections forwards and backwards in time. In many legal cases there is a question as to whether some condition or factor can be said to "negative causal connection" or "break the chain of causation" between some earlier act or omission and the occurrence of some subsequent harm. Two kinds of legal case serve to illustrate the nature of this problem; one involves "supervening causes," the other, "extraneous causes."

(1) Supervening Causes

Consider the following: A lights a fire, a mild breeze starts up, the fire spreads to an adjoining house and consumes it. In such a case the law holds that the breeze did not "break the chain of causation"

even though it was a condition that was independent of A's action and a necessary condition of the harm. Thus, A's action in lighting the fire is the cause of the subsequent harm. On the other hand, suppose the fire started by A was about to flicker out when another person, B, poured gasoline on it, such that the flames spread to the adjoining house with the same result as before. Given these facts B's action would be deemed a "supervening cause" and would be said to have broken the chain of causation between A's action and the subsequent harm.

(2) Extraneous Causes

Extraneous causes are conceived as events which, in conjunction with other occurrences, are of an extraordinary nature. Thus, A hits B with the intention of killing him. B suffers only minor injuries but faints. At that moment a huge tree crashes down on top of B, killing him. Upon these facts both common sense and the law would hold that A caused B's minor injuries but not his death. The extraneous factor, the fall of the tree, is what caused B's death. In this case the fall of the tree at that particular time and place in conjunction with B's being in the path of its fall would be deemed a coincidence. As Hart and Honoré point out, a thorny problem for the courts to resolve is "just how unlikely must a conjunction be to rank as a coincidence, and in the light of what knowledge is likelihood to be assessed?" (75)

Legal talk about supervening causes and extraneous causes has its roots in common sense notions of cause, argue Hart and Honoré, and no attempt to explicate legal notions of cause without regard to this fact can be successful. Their own analysis of common sense notions of cause takes as its point of departure the view that "there is not a single concept of causation but a group or family of concepts." (26)

They find, however, that one concept in particular can be identified as the central one, this being "cause and effect."

The source of the central notion of cause is located by Hart and Honoré in the knowledge acquired by human beings that they can, by making appropriate movements of their bodies, bring about desired alterations in both animate and inanimate objects in their environment. Such achievements are expressed by the use of simple transitive verbs, e.g. push, pull, bend, twist. Further, people have "learnt to extend the range of their actions and have discovered that by doing these relatively simple actions they can, in favorable circumstances, bring about secondary changes, not only in the objects actually manipulated, but in other objects." (26)

We use the correlative terms "cause" and "effect" rather than simple transitive verbs only when there exists such a three place relation, that is, action--primary change--secondary change. For example, "we cause one thing to move by striking it with another, glass to break by throwing stones, injuries by blows, things to get hot by putting them on fires." (27) In such cases the notions of cause and effect merge with those of means to ends and of producing one thing by doing another.[3]

What Hart and Honoré find particularly important about these simple cases is that they constitute paradigms for understanding the way causal language gets used in very different types of cases. This is so primarily because "expressions which have a literal use in the simple cases have come to be used in a metaphorical and sometimes baffling way in cases far outside their scope." (27) For this reason they claim that analogies with the central notion of cause and effect

"characterize the common-sense identification of causes in a wider field." (26)

What serves to characterize the simple cases to which the terms cause and effect most readily apply is the presence of a human action conceived as "an interference in the natural course of events which makes a difference in the way these develop." (27) Elaborating on this point Hart and Honoré write:

> Common experience teaches us that, left to themselves, the things we manipulate, since they have a 'nature' or characteristic way of behaving, would persist in states or exhibit changes different from those which we have learnt to bring about in them by our manipulation. The notion, that a cause is essentially something which interferes with or intervenes in the course of events which would normally take place, is central to the common-sense concept of cause....Analogies with the interference by human beings with the natural course of events in part control, even in cases where there is literally no human intervention, what is to be identified as the cause of some occurrence; the cause, though not a literal intervention, is a difference to the normal course which accounts for the difference in the outcome. (27)

Whatever the origin may be of the idea that the cause of an event is some condition that "makes the difference," this idea is of greater importance in analyzing the structure of explanations that involve a distinction between cause and conditions than even Hart and Honoré seem aware of. While much of this chapter will be devoted to an examination of their account of the way in which two contrasts serve as criteria for distinguishing causes from conditions--these being, "what is abnormal and what is normal in relation to any thing or subject matter, and between a free deliberate human action and all other conditions" (31)--I shall argue that it is often the Method of Difference, sometimes in conjunction with one or another of Mill's

Methods[4], that supplies the justifying grounds for many singular causal statements. Moreover, I want to argue that those singular causal statements that can be analyzed in the way I propose constitute but one species of the Method of Difference, another species being comprised of some varieties of judgments of causal importance (otherwise known as weighting causes). Since to my knowledge no one has yet made a sustained attempt to link the cause-conditions distinction with judgments of causal importance, the account I give of their mutual justifying grounds will hopefully serve to initiate debate on this much neglected aspect of causation.

(B) Types of Causal Inquiry: Explanatory and Attributive

The first of the two principal criteria for distinguishing causes from mere conditions is the contrast between normal and abnormal conditions. Before beginning our examination of this contrast it should be pointed out that Hart and Honoré contend that it functions in both "explanatory" and "attributive" causal inquiries. The distinction they draw between these two types of causal inquiry is somewhat complicated, for it seems to me that they mean at least three different things by "attributive inquiries." In order to avoid undue complications at the outset I will first examine the way in which the contrast between abnormal and normal conditions functions in explanatory inquiries alone. Once we have come to appreciate the manner in which this contrast operates in these causal inquiries we will be in a better position to deal with the special problems engendered by attributive causal inquiries.

(1) Explanatory Inquiries: Abnormal and Normal Conditions*

What serves to characterize both explanatory and attributive causal inquiries are the respective purposes of people who undertake them. In general, explanatory inquiries are motivated by a desire to understand the occurrence of "some contingency which we find puzzling: we do not know why or how it happened. Here the inquiry into causes brings to light previously unknown factors." (23) Explanatory causal inquiries, then, are generated by ignorance of fact, they are undertaken to fill the gaps in one's understanding. More specifically, though, Hart and Honoré maintain that requests for causal explanations are prompted by some "particular contingency the occurrence of which is puzzling because it is a departure from the normal, ordinary, or reasonably expected course of events." (23)[5] When such situations confront us the condition we regard as "the cause" will be something that interferes with or "makes a difference" in the course of events that would normally have taken place. In short, the cause is an abnormal condition. On the other hand, conditions that are regarded as "normal," and therefore classified as "mere conditions" will be those "which are present as part of the usual state or mode of operation of the thing under inquiry." (32-33) However, in order to fully appreciate the way in which the contrast between abnormal and normal conditions functions in particular cases, Hart and Honoré insist upon the importance of recognizing that "what is normal and what is abnormal is relative to the context of any inquiry in two ways." (33)

*This section is a revised version of my paper "The Method of Difference and Species of Singular Causal Judgments in History," in La Philosophie de l'histoire et practique historienne d'aujord'hui/ Philosophy of History and Contemporary Historiography, eds. David Carr et. al., Ottawa: University of Ottawa Press, 1982, pp. 347-359.

The first type of relativity to context Hart and Honoré speak of we may refer to as relativity to context of occurrence. In cases that exemplify this type of relativity to context we find that a certain type of factor which is a "mere condition" in some sets of circumstances may be cited as "the cause" in others. On the other hand, what is typical of the second type of relativity to context is that two or more people cite different conditions of a single event as its cause. Let me call this relativity to point of view.

As an example of how the first type of relativity to context works, Hart and Honoré invite us to consider an occasion on which a fire destroys a building. Here, "mere conditions" will be factors such as the oxygen in the air, the presence of combustible material or the dryness of the building." (32) The reason why we reject all of these conditions as the cause of the fire is that they "are present alike both in the case where such accidents occur and in the normal cases where they do not." (32) Upon learning that an arsonist set aflame a pile of newspapers in the basement of the building our desire to have the occurrence of the fire explained would have been satisfied, for citing the arsonist's action would serve to distinguish the occasion on which the fire occurred from the sum of previous occasions where conditions in the building were more or less the same as on the occasion when the fire did occur, yet no fires occurred. On the other hand,

> if a fire breaks out in a laboratory or in a factory, where special precautions are taken to exclude oxygen during part of an experiment or manufacturing process, since the success of this depends on safety from fire, there would be no absurdity at all in such a case in saying that the presence of oxygen was the cause of the fire. The exclusion of oxygen in such a case, and not its presence, is part of the normal functioning of the laboratory and the factory, and hence a mere condition: so the presence of oxygen in such a case is not a feature common to both the disaster and normal functioning. (33)

What makes for the relativity to context of occurrence is the fact that while regularities are part and parcel of the natural world, human interventions in the normal course of natural events may themselves establish regularities, i.e. conventions, thereby creating new standards of normality. For example, with the invention of the umbrella its use has become conventional such that failure to bring an umbrella with you when it is raining may be cited as the cause of your being all wet when you arrive at work. However, not only may the use of technological inventions establish new standards of normality, so too may the conduct of people who enter into agreements or contracts. And what is true of individual people holds also for communities and nation-states. Apart from any moral significance such agreements may have, behavior founded upon treaties, alliances, and acknowledged spheres of interest may constitute standards of normality in a purely statistical sense. Henceforth, deviations from such patterns of behavior will count as departures from the normal and so, as causes of whatever consequences may follow in the wake of these deviations. This aspect of the distinction between abnormal and normal conditions will be dealt with at greater length when we examine, in a later section, the debate between A. J. P. Taylor and Hugh Trevor-Roper on the cause of the Second World War.

The foregoing indicates the manner in which causal judgments are arrived at when there is a relativity to context of occurrence. In going on now to consider an example Hart and Honoré give to illustrate the operation of relativity to point of view we will find that the very same procedure is employed to establish singular causal judgments in these contexts. Thus, Hart and Honoré ask us to imagine a man who has had an ulcerated stomach for some time. This man usually has no

digestion problems but one evening after supper he comes down with a severe attack of indigestion. In searching for an explanation of his attack, his wife decides that the parsnips he ate must be the cause of his present condition. Unsure of her diagnostic abilities, however, she calls a doctor in. The doctor, of course, disagrees with the wife's causal judgment. According to him the ulcerated condition of his patient's stomach was the cause of his indigestion, not his eating parsnips. Ostensibly the wife and doctor are asking the same question about this man's attack of indigestion, but their presuppositions or background assumptions lead them to select different conditions of that attack as its cause: the wife is contrasting her husband's present condition with what normally happens after he has a meal whereas the doctor is contrasting his patient's present condition with what normally happens after healthy men and women eat meals which include parsnips.

The liberty which the wife and doctor exercise in choosing their respective standards of normality may, in some minds, confirm Mill's view that the condition cited as the cause is selected arbitrarily. But it is doubtful that Mill had anything of the sort in mind when he made that claim, for as we saw, he apparently thought that this arbitrariness reflected only the "ordinances of language." Nevertheless it would be dishonest to refuse to acknowledge the element of choice that ultimately determines which condition is elevated to the status of "the cause." But equally important is the fact that the respective standards of normality chosen by the wife and doctor do represent statistical norms. As such, it would be a mistake to argue that causal judgments typical of this type of relativity to context are completely

subjective in nature, which is what Mill may have been implying in calling them "arbitrary."

Having sifted through a number of examples that serve to illustrate how the contrast between abnormal and normal conditions functions in the two kinds of relativity to context, we are now in a position to see just how singular causal judgments based on the cause-conditions distinction are to be viewed as a species of the Method of Difference. To begin, I find it astonishing that after writers like Ducasse and Hart and Honoré have identified "the cause" as something which "makes a difference" nobody has yet suggested that it is the Method of Difference that constitutes the justifying grounds for many singular causal judgments. Further, given Mill's analysis of the gun-shot example we introduced earlier it is perhaps even more astonishing that he did not recognize this fact and correct his views on the arbitrariness of such judgments. Be that as it may, before I can establish that Hart and Honoré's analysis of the cause-conditions distinction does indeed conform to the requirements of the Method of Difference let me present Mill's formal statement of this method:

> If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance save one in common, that one occurring only in the former; the circumstance in which alone the two instances differ, is the effect, or the cause, or a necessary part of the cause, of the phenomenon. (SL 225)

Now when Hart and Honoré tell us that a "'mere condition' is one that is present alike in the case where the effect occurs and in normal cases where it does not," in my view we can replace their talk about "cases" and "conditions" with Mill's terminology of "instances" and "circumstances," without any loss of meaning. The only objection I can foresee to the comparison I have drawn has to do with the fact that Mill

speaks of comparing one instance (wherein the phenomenon occurs) with another instance (in which the phenomenon does not occur) whereas Hart and Honoré suggest a comparison between one case (which contains the abnormal condition) and normal cases. To counter this objection we need only point out that what Hart and Honoré refer to as "normal cases" are periods of a certain duration wherein the effect does not occur, e.g. previous occasions on which experiments were conducted in the laboratory and no fires occurred, or what is typical of most individuals in whom the effect (indigestion) does not occur. Since these time periods can be analyzed into smaller units (i.e. days, weeks, months) and what is typical of a certain group can be reduced to what happens to each individual member of that group over a period of time, talk about "normal cases" is really but an elliptical way of referring to what has been observed to occur on a number of previous occasions. As such, we can either speak of the result of each observation considered apart from the others or we can summarize the results of our observation when these are considered together.[7] When we do the latter we can express our summary as an instance.

One consequence of adopting this procedure is that the instance in which the effect occurs will often be of a much shorter duration than the instance with which it is being compared or contrasted. Moreover, use of the summative approach is quite apparent in Hart and Honoré's discussion of what they call the "provision of opportunity cases." In these, as we shall see shortly, it is the frequency of a certain type of harm occurring in a specific manner that constitutes the basis for singular causal judgments. While it is the frequency as such that constitutes the comparison instance, there can be no doubt that it can be reduced to the isolated occasions which are its components.

Prior to discussing these cases it is worthwhile to mention that there have been a number of writers who have attempted to present concise analyses of cases like the ones we have been examining. One such writer, Raymond Martin, has produced what he calls a "consensus account" which is said to incorporate what all of these writers accept. On this account a causal factor is correctly regarded as "the cause" of a certain result "only if it differentiates between the actual situation which included the occurrence of that result (hereafter, the ef-fect-sit-uation) and certain actual or hypothetical situations with which the ef-sit is being compared (hereafter the com-parison-sit-uations)."[8]

This account explains, in a preliminary way, the difference in the causal judgments made in the four examples presented to this point. In the case of the fire in the building the arsonist's action is part of the ef-sit but not the com-sit, whereas in the laboratory the presence of oxygen is part of the ef-sit but not the com-sit. Again, for the wife of the man who suffered indigestion, parsnips are part of the ef-sit but not the com-sit, whereas for the doctor, ulcers are part of the ef-sit but not the com-sit.

While the examples hitherto discussed do conform to Martin's "consensus account" when we compare an ef-sit with an actual com-sit, none of these bear upon the contrast between an ef-sit and a hypothetical com-sit. To indicate how this contrast works I want to introduce a historical example that probably goes beyond anything Martin had in mind when he drew this contrast, nevertheless it does make his point. In addition, it serves to indicate in what way the methods employed by the "new economic historians" do not constitute a departure from the means utilized by "traditional" historians to establish their causal judgments. The example I have in mind is taken

from Robert Fogel's well-known work, Railroads and American Economic Growth. In this work Fogel attempts to determine the net contribution of the railroad to American economic growth in the year 1890. To determine this amount he employs a standard to measure what he calls "social saving," this being

> the difference between the actual level of national income in 1890 and that level of national income that would have prevailed if the economy had made the most efficient possible adjustment to the absence of....the interregional railroad.[9]

Peter McClelland, in a critique of the work of Fogel and of another economic historian, Albert Fishlow, indicates just how Fogel's definition of "social saving" involves certain assumptions about what was not, but might have been. According to McClelland, if one wants to know how much of a difference railroads made to aggregate American economic growth one must "mentally remove factor X (railroads) from the economy, speculate on the total impact of that removal on aggregate growth, and then compare those hypothetical results with the growth that was actually achieved."[10]

This is in fact what Fogel has tried to do. In order to succeed in the task he set for himself he must assume that in the absence of railroads either no new modes of transportation would have been developed or the existent modes would have been extended and improved. Here Fogel chooses to make an assumption that provides the foundation for his entire analytical framework, he assumes that an extended system of canals would have replaced the existent network of railroads and that these would have been used in the most efficient way possible. While I am quite aware that Fogel has been criticized for biting off more than any economic historian can chew,[11] it does appear that the results he obtained (correct or not) depended upon a contrast between an ef-sit

(actual economic growth with railroads) and a hypothetical com-sit (an economy wherein there exists an extended system of canals which are used with optimum efficiency).

One thing that is brought out by Fogel's explanation of American economic growth in 1890 is the importance of quantifying the elements contained in the ef-sit and the com-sit. Neither Hart and Honoré nor Martin mentions this in their accounts of singular causal judgments, though it is easy to see that what Hart and Honoré call "normal cases" are, as I have already suggested, the sum of previous occasions on which certain types of conditions prevailed. However, the importance of quantification is perhaps even more apparent in the provision of opportunity cases, for in these the basis for making singular causal judgments is the existence of statistical asymmetries. While Hart and Honoré fail to generalize on this aspect of their theory of causation, their treatment of such cases makes it clear that for a causal condition to differentiate between an ef-sit and an actual com-sit it need not be the case that that condition has never appeared in the com-sit, although such a situation is the optimal one when making such a contrast. So long as the number or times that that condition has appeared in the com-sit is small, it cannot be said to be typical of that com-sit and thus, will not suffice as the basis of reasonable expectation.

The central notion in the provision of opportunity cases is that of an action or omission being a "deviation from a standard practice or expected procedure." (56) As Hart and Honoré correctly suggest, we would want to distinguish a case where a man's storing of firewood in the basement of his house provided a pyromaniac with an opportunity to burn the house down from a case in which a friend, entrusted with the

keys to a house while its owners were away on vacation, left the house unlocked and a thief entered and stole some furniture. In general, the chances of a pyromaniac setting aflame the firewood one has stored in his basement is low, whereas the chances of a thief entering an unlocked house and stealing furniture is much greater. Our assessment of the likelihood of these types of harm occurring depends upon our observation of the frequency with which such harm, produced in the relevant manner, has occurred in the past. It is these observations which form the basis of our causal judgments in such cases, for where the frequency of a certain type of harm occurring as the result of providing a certain type opportunity is low, we do not call the provision of an opportunity of this type the cause of the subsequent harm, whereas when the frequency is high, the reverse is true. But it is very important that we recognize that these observations are themselves relative to some context of occurrence, be it a country, city, or neighborhood. If it were the case that a great majority of the adolescents in a particular neighborhood were pyromaniacs then it would not be improper to say that the fire caused by one of them was the consequence of the householder's storing of wood in his basement. Similarly, if you lived in a country where stealing seldom occurred we would not hesitate to say that the loss of furniture was not a consequence of your friend's leaving your house unlocked.

(2) Judgments of Causal Importance

We have now reached the juncture where we can begin to compare singular causal judgments based on the cause-conditions distinction with judgments of causal importance. As we have already seen, singular causal judgments in the provision of opportunity cases depend upon the

existence of statistical asymmetries. This, I want to claim, is also what characterizes Ernest Nagel's analysis of four types of judgments of causal importance one finds in his The Structure of Science.[12] A fifth type of judgment of causal importance analyzed by Nagel involves a rather straightforward application of the Method of Difference.

I suspect that many people are of the opinion that drawing a distinction between the cause of an event and its other conditions is really the same thing as saying that one condition of an event is causally more important than another. While there may be some justification for this view it probably rests upon a confusion between a "subjective" and an "objective" sense of importance. We have already noted that in the relativity to point of view cases what is cited as "the cause" of an event is, in one sense, "subjective." Though not "arbitrary" it does depend upon one's choice of a standard of normality. On the other hand, those who have presented analyses of judgments of causal importance have done so with the aim of showing that such judgments are based upon "objective" grounds, i.e. they are intersubjectively verifiable. Morton White, who is the only writer I know of who has endeavored to distinguish the two types of causal judgments at issue,[13] points out that even when one can, on "objective" grounds, say that one causal factor was more important than another in bringing about a certain event, there may still be a relativity of causes when the cause-conditions distinction is applied to the same case. Consequently, we could conceivably have a situation where a factor that is deemed more important than another in bringing about an event may, from one point of view, be regarded as "the cause" of that event, but also, from another point of view, be regarded as a "mere condition" of that event.

In order to make this point White examines Nagel's first sense of "more important." The example Nagel offers to illustrate this sense reads as follows:

> Suppose that a strong dislike of foreigners and an acute need for additional economic markets are both necessary conditions for the adoption of an imperialistic foreign policy by an industrialized nation: and suppose that xenophobia in the country varies little if at all during relatively short periods whereas the need for foreign markets steadily increases. In this first sense of "more important', the need for additional economic markets is a more important cause of imperialism than is dislike for foreigners. Accordingly, if a certain country is found to have embarked upon a policy of imperialist aggression at a specified time, and if investigation shows that before this event there had been no marked change in xenophobic attitudes of its citizens but that recurrent overproduction in a number of its industries had produced a growing demand for new markets, a historian might claim that of these two factors the latter had been the more important in bringing about the adoption of the imperialist policy.[14]

Now White does not wish to deny that historians sometimes do rank factors in the way suggested above, but what he thinks important to notice is that the less important cause, xenophobia, may, from a certain point of view, be deemed "the cause" of the adoption of the imperialist foreign policy adopted by Ruritania (a country of White's invention). This is possible because a historian, like the doctor who cited the presence of ulcers as the cause of his patient's indigestion, may ask "Why did Ruritania, an industrialized nation with an acute need for economic markets, adopt an imperialist foreign policy when other industrialized nations, also having an acute need for economic markets, did not?" And he may answer, "because of the presence of xenophobia in Ruritania and its absence in those other industrialized nations."

What this shows is that the cause-conditions distinction cuts across the distinction between more and less important factors. Another way of showing that the two types of causal judgments are essentially different is to look at those judgments of causal importance that relate to types of event rather than to particular events. Nagel's fourth and fifth senses of "more important" are of this kind. In his fourth sense it is said that "negligence would be a more important cause of automobile accidents than would mechanical failures."[15] Such a judgment is based upon these considerations:

> that automobile accidents happen either because of the negligence of motorists or because of mechanical failures in relevant parts of automobiles, and also that the frequency with which such mechanical failures cause accidents is very much less than the frequency with which negligence is responsible for them.[16]

Given this sense of "more important" it is rather obvious that the cause-conditions distinction is presupposed by any such judgment. What I mean by presupposed is that in order to possess the relevant statistical data on which to base such judgments one would already have had to apply the cause-conditions distinction when considering each automobile accident. In other words, one would have had to decide in the particular case whether it was driver negligence, mechanical failure, or some other condition that was "the cause" of the accident. Thus, unless one has distinguished cause from conditions in each particular case one would never possess the data on which to formulate a judgment of causal importance in Nagel's fourth sense.

Our previous discussion has indicated two ways in which the distinction between cause and conditions and that between more and less important causes are different: the former distinction both cuts across and is sometimes presupposed by the latter one. Now that I have alluded

to some of the differences it is time to demonstrate what are the common grounds of these two distinctions. First I shall deal with certain aspects of those types of judgments of causal importance that are related to types of events (Nagel's fourth and fifth senses) and then with those that are related to particular events (Nagel's second and third senses).

Earlier I remarked that singular causal judgments in the provision of opportunity cases depend upon the existence of statistical asymmetries. Moreover, I suggested that this was also true of several types of judgment of causal importance analyzed by Nagel. What I now want to show is that the statistical data that form the bases for such judgments also serve to characterize the "instances" in the Method of Difference which, as I previously argued, often forms the grounds for the distinction between cause and conditions. Like Hart and Honoré's "normal cases," "instances" are really only the sum total of what Nagel has called "the recorded data of observation."[17] This claim is based on a passage in The Structure of Science where Nagel is trying to show how it is possible to do what Mill thought impossible, i.e. to perform controlled experiments when we cannot manipulate the factors we suspect of having had a causal influence on some phenomena, viz.:

> Since by hypothesis the relevant factors cannot be overtly manipulated in these investigations, the control must be achieved in some other manner.... this control is achieved if sufficient information can yield symbolic constructions in which some of the factors are represented to be constant (and hence without influence upon any alterations in the phenomenon under study), in contrast to the correlations between the recorded data on variations in the other factors and the recorded phenomenon. Accordingly, the subjects manipulated in these investigations are the recorded (or symbolically represented) data of observation on relevant factors rather than the factors themselves.[18]

Here, as I suggested in my discussion of Collingwood's controllability criterion, we have a sense of "control" that does not involve the direct manipulation of conditions, but rather, the manipulation of data that represent these conditions. This would seem to confirm Hart and Honoré's criticism of Collingwood's controllability criterion, that the "willingness of common sense to classify a factor as a cause does not seem to depend directly upon ability to use it or interfere with it as a method of control." (34) However, it is unlikely that Hart and Honoré themselves appreciated the role played by the notion of "control" in either prospective experiments or, as is the subject of discussion in the above passage, retrospective experiments.

Regardless of the limitations of Collingwood's and Hart and Honoré's views on the scope of the notion of "control," by employing the procedure outlined by Nagel it is sometimes possible to determine which of several factors was a more important cause of some phenomenon. The basis for my claim that this procedure does constitute a species of the Method of Difference is twofold, (1) that causal judgments resulting from the application of the procedure depend upon there being variations against a background of constancy and, (2) that the recorded data of observation from which these variations are derived constitute the "instances" that need to be contrasted in order to determine which of two or more factors contributes most to the occurrence of some phenomenon. Bearing this in mind I propose to give an account of a judgment of causal importance that has been the subject of some debate between R. G. Frey and Michael Martin,[19] this being, "inadequate housing is a more important cause of violence among working-class adult males in Liverpool than is insufficient income."

If a historian or a social scientist has access to government files from which he can compute the total number of violent crimes committed in Liverpool, the social status of the criminal (working class, unemployed, etc.), the level of income and kind of dwelling lived in, this historian or social scientist would then be in a position to conduct a demographic survey employing all of these statistics. Given access to the kind of information I have specified the purpose of this survey might be to answer the following question: Of the total number of violent crimes committed by working-class adult males in Liverpool, was the incidence of such crimes greater in neighborhoods where such people had insufficient income but received adequate housing, or in neighborhoods where such people had sufficient income but lacked adequate housing? Now supposing it was discovered that the incidence of violent crimes among working-class adult males in Liverpool is 30% lower when they have insufficient income but receive adequate housing (perhaps through government subsidization) than when they have sufficient income but lack adequate housing. Given these findings it would not be inappropriate to conclude that "inadequate housing is a more important cause of violence among working-class adult males in Liverpool than is insufficient income." The same analysis applies to an example Nagel offers to illustrate his fifth sense of more important, i.e. "broken homes are a more important determinant of juvenile delinquency than is poverty."[20] What this judgment of causal importance implies is that the relative frequency of delinquents coming from broken homes which are not poverty-ridden is greater than among delinquents whose parents are poor but live together amicably.

In the two examples analyzed above it was the frequency of the occurrence of some type of event that was the subject of the judgment

of causal importance. Such judgments, as we saw, are based upon the relative frequency of occurrence of the determinants of that type of event. It has been argued, however, that although judgments of causal importance pertaining to types of events may be meaningful and sometimes objectively warranted, the frequency claims on which they are based cannot be applied to judgments of causal importance that relate to particular events. Raymond Martin appears to have effectively argued this point against Nagel. Martin contends that

> It would be curious, if not downright wrong, to claim that driver negligence was a more important cause of some particular accident than was mechanical failure merely because both were a cause of the accident in question and the frequency with which driver negligence has been a cause of automobile accidents is greater than the frequency with which mechanical failure has been a cause of automobile accidents.[21]

While I do think Martin is right when he insists that Nagel's analysis of the example where driver negligence is said to be a more important cause of automobile accidents than is mechanical failure cannot be applied to any particular automobile accident, the conclusion he draws, that "the weighting aspect of many CCEs (comparative causal explanations) may not imply any frequency claim,"[22] indicates a certain caution on his part. This caution is entirely justified for, although Martin is also right in taking Nagel to task for trying to apply this analysis of more important to the particular historical example he chose to illustrate it, that is, that "Austria and Germany's fear of Pan-Slavism was a more fundamental reason for the outbreak of the First World War in 1914 than was the assassination of the Archduke Franz Ferdinand at Sarejevo," this judgment of causal importance is also based on a frequency claim, albeit one that is different than the one involved in the automobile case.

What I want to suggest is that Nagel's account of the judgment of causal importance relating to the outbreak of the First World War is more appropriately viewed in terms of his third, rather than fourth sense of "more important." As Nagel rightly suggests, this third sense of more important "is perhaps most frequently intended when A is said to be more important than B."[23]

Nagel's explication of this third sense of more important is directly concerned with the "likelihood" of one or another condition occurring at a certain time and place. An example of this sort that is formally the same as the one given by Nagel, though easier to grasp than his, has been proposed by Sten Nilson. Writes Nilson,

> An open powder keg having been placed on a quay under a gangway, a cigarette butt is thrown into the keg by someone passing over the gangway. The result is a violent explosion. If no more than the above is known about the facts of the case, all we can say is that the explosion occurred both because the dry powder was placed here and because the burning cigarette was thrown here.
>
> However, should an investigation succeed in establishing some further facts, then we will also be in a position to say something about the relative importance of the two causal factors. Should he find that burning matches, cigarettes, and cigars, were being constantly thrown down from the gangway; in other words, an explosion was very likely to occur even if the particular cigarette in question had never been thrown.[24]

In this case, then, the presence of the keg would be deemed a more important cause of the explosion than was the thrown cigarette. Likewise in Nagel's First World War example. There Austria and Germany's long-standing fear of Pan-Slavism is held to be a more important cause of the outbreak of the war than is the assassination of the Archduke because, it is assumed, had not the Archduke been assassinated then some other "precipitating" event would have "ignited" the situation.

This illustrates the sense in which historians treat "deep-seated" causes as more important than "precipitating" ones in bringing about particular events. It is important to notice, however, that such judgments of causal importance are based upon the assessment of probabilities (i.e. likelihood), which in turn are based upon a comparison of the relative frequency of occurrence of two or more conditions. As such, Martin's criticism of Nagel is justified only to the extent that Nagel wrongly identified the First World War example as a case that could be analyzed in terms of his fourth sense of more important when in fact it is appropriately analyzed in terms of his third sense. Despite his faux pas, Nagel's third and fourth senses of more important are based on frequency claims, though these are different types of frequency claims. Accordingly, Martin is mistaken in thinking that frequency claims do not apply to judgments of causal importance that pertain to particular events.

Apart from Nagel's sixth sense of more important, which has nothing whatsoever to do with causes, the only sense of more important we have not yet touched upon is his second sense. This sense of more important bears examination for two reasons. First, because an example corresponding to this sense is used by Martin to show that not every judgment of causal importance implies a frequency claim and, second, because a proper analysis of this sense of more important clearly depends upon an appreciation of the role played by the Method of Difference. In order to facilitate our discussion of this second sense of more important it will help if we first juxtapose Nagel's own example illustrating this sense with the one Martin has devised.

> Nagel: suppose that an adequate supply of coal as well as a trained labor force is indispensable for

> industrial productivity; but suppose that a 10 percent increase in the trained labor force yields a considerably larger volume of goods than is obtained by a 10 percent increase in the coal supply. Accordingly,....the availability of a trained labor force would be a more important determinant of industrial productivity than the availability of coal.[25]

> Martin: I've gained so much weight since the last time I saw you partly because I'm getting less exercise now than before, but mostly because I'm eating much more.[26]

In both of these examples the particular effect to be explained is some magnitude. Strangely, then, this second sense of more important appears to be something of a hybrid, for like the first and third senses and unlike the fourth and fifth senses it pertains to the occurrence of particular events, but like the fourth and fifth senses and unlike the first and third senses that which is in need of explanation is some magnitude. In Nagel's example this magnitude is the volume of goods produced, in Martin's it is the number of pounds gained. Still, the important question to answer is: On what basis is the claim being made that one contributory cause is more important than another in determining that magnitude?

I would begin to answer this question by suggesting that Martin is right in seeing no frequency claim involved in cases of this sort. However, what Martin fails to point out when analyzing his example is that the basis for claiming that eating more was a more important cause of the person's weight gain than was his exercising less is the Method of Difference applied on successive occasions. To see that this is indeed the case let us look at the model devised by Martin to account for the weighting aspect in his example:

> A was a more important cause of P than was B, if
>
> (1) A and B were each a necessary cause of P, and

(2) There is some appropriate state, ϕ, such that:
(a) A, B, and P are each deviations from ϕ, and
(b) on the occasion in question, had A occurred as it did and B occurred as in ϕ, a result would have occurred more similar to P than had B occurred as it did and A occurred as in ϕ.[27]

Applying this model to Martin's example, A is eating more, B is exercising less, and P is the weight gain. What he refers to as "some appropriate state ϕ" is really only a fixed level of food consumption and exercise. Note that the deviation from ϕ represented by A is an increase while that represented by B is a decrease. Now, the basis for the judgment of causal importance rests ultimately on (b). This, however, is really only a description of how the Method of Difference works. We first hold constant the person's level of exercise and increase his food consumption, then calculate the gain in weight (first application of the Method). Secondly, we hold constant his food consumption and allow his exercise level to decrease, once more calculating his weight gain (second application of the Method). Again, if the person gained more weight during the first test than during the second one it would be justified to claim that eating more was a more important cause of his weight gain than was exercising less.

The very same procedure is involved in Nagel's example. In order to say that a 10 percent increase in the trained labor force was a more important cause of industrial productivity than was a 10 percent increase in the coal supply one would first have to hold constant the number of trained laborers, increase the coal supply by 10 percent, and then calculate the increase in the volume of goods produced. Secondly, you would hold constant the amount of coal and increase the number of trained laborers by 10 percent, again calculating the increase in the volume of goods produced. If, as Nagel suggests, we find that a

10 percent increase in the number of trained laborers yields a considerably larger volume of goods than does a similar increase in the coal supply then the judgment of causal importance is warranted.

In each of these two examples the procedure employed in order to ascertain which of two factors was a more important determinant of some particular result was the Method of Difference.[28] I conclude, then, that this Method does constitute the justifying ground for this type of judgement of casusal importance and, as such, is what I have called a species of the Method of Difference.

(3) Causes, Conditions, and the Method of Difference in History

In view of the fact that many of the judgments of causal importance discussed in the last section were related to changes in the magnitude of some particular event or type of event I suspect that the reader has been left with the impression that I think all historians who make causal judgments based on the Method of Difference must be engaged in statistical research. Clearly, economic historians almost always utilize statistical indices of one sort or another while social historians, especially recently, are making greater use of statistical data in order to formulate and test causal hypotheses.[29] Despite the prominence of these quantitative studies in the last two decades there have always been historians who have employed the Method of Difference to support their causal judgments about the occurrence of particular events even when they do not explicitly cite statistical data. Henri Pirenne's explanation of the collapse of what he calls the "Mediterranean Commonwealth" is a good example of this, for while Pirenne utilizes the Method of Difference to support his singular causal judgment we find no tables or graphs representing statistical data in Medieval Cities.

In turning to Pirenne's explanation of the collapse of the Mediterranean Commonwealth it should be pointed out that in addition to writers like Raymond Martin who have attempted to simplify Hart and Honoré's account of the contrast between abnormal and normal conditions, there have also been a few philosophers who have sought to apply their account to the causal judgments historians make. In Foundations of Historical Knowledge, Morton White, who was the first philosopher to develop and apply Hart and Honoré's doctrine of "Abnormalism" (as he calls it) to the sphere of history, offers us an account of how this doctrine applies to Pirenne's explanation of the collapse of the Mediterranean Commonwealth in the last quarter of the seventh and first quarter of the eighth century. The following is White's synopsis of this explanation:

> Pirenne's explanation of the collapse of what he calls the "Mediterranean Commonwealth" is one in which the Moslem invasion in the seventh and eighth centuries is said to be the cause. He thinks of this collapse as an unusual or abnormal event in the history of Europe, and for that reason thinks it demands explanation. Therefore, the opening chapter of Pirenne's Medieval Cities is devoted to showing the continuity in European life from ancient times down to the eighth century, when the overthrow occurred. Culturally, politically, and economically, he argues, an uninterrupted unity persisted on the shores of the Mediterranean, which was the center of gravity of European thought and culture. Moreover, one of Pirenne's most important efforts is to show that the invasion of the barbaric tribes, which had, of course, preceded the Moslem invasion, did not destroy the civilization of antiquity. But what survived the Germans, he maintains, did not survive the Moslems.[30]

In a formal sense, then, Pirenne's manner of selecting the cause of the collapse of the Mediterranean Commonwealth is analogous to the considerations brought to bear in the example where the wife sought to discover the cause of her husband's attack of indigestion. As White

points out, "the husband is the analogue of the Mediterranean Commonwealth, his indigestion is the analogue of the collapse of the Commonwealth, and his eating parsnips is the analogue of the Moslem invasion. The indigestion and the collapse are unusual events and their causes are also unusual events."[31]

In Pirenne's view, then, it was the Moslem invasions, not the Germanic ones, that constitute the abnormal factor which explains the collapse of the Commonwealth. However, based on the evidence available to him, Pirenne not only offers us his specific causal conclusion, he also takes into account certain states of affairs before and after the Moslem invasions. In this respect he attempts to establish continuity and change in the Roman Empire's commercial relations, the standard of its monetary system, and the location of its largest cities. He finds, for example, that subsequent to the Germanic invasions trade continued to flourish in the Empire: "Both manufactured and natural products were still excessively dealt in; textiles from Constantinople, Edessa, Antioch and Alexandria; wines and spices from Syria; papyrus from Egypt; wheat from Egypt, Africa, and Spain; wines from Gaul and Italy."[32]

The Moslem invasions changed all this, once in full swing they reduced the flow of these articles to a trickle. Similar changes are noted in the monetary policy of the Empire and in its migration patterns. After the Germanic invasions and the Empire's loss of political autonomy gold continued to be the standard of the monetary system and all the largest cities in the Empire were strung along the shores of the Mediterranean. With the advent of the Moslems, Pirenne notes, silver replaced gold as the standard of the money system and large numbers of people were deflected northward, such that the largest

cities in the post-invasion period were located north of the Loire River, whereas in previous centuries they had all been situated on the shores of the Mediterranean. It is this contrast between long periods of stability and sudden change that leads Pirenne to claim that it was the Moslem invasions that, in effect, "made the difference."

While it must be acknowledged that scarcely a single aspect of Pirenne's explanation of the collapse of the Mediterranean Commonwealth has escaped historical criticism,[33] I am not concerned here with the truth or falsity of his conclusion but with the structure of his explanation. Schematically, we might sketch the structure of this explanation as follows: T_1, T_2 and T_3 represent, respectively, the periods A.D. 284-395 (from the beginning of the reign of Diocletian to the end of the reign of Theodosius), 378-639 (from the battle of Adrianople where the Visigoths defeated the Romans to the establishment of the Merovingian kingdom of Franks in Gaul), and 637-713 (from the Islamic overthrow of the Persian Empire to the seizure of Constantinople). Also, let X stand for the Germanic invasions, Y for the Moslem invasions, A, B, and C represent, respectively, active trade in spices, wines, papyrus, and cloth; gold coins as standard currency; and the largest cities present on the shores of the Mediterranean. Finally, let a and p designate the absence or presence of a given factor or state of affairs:[34]

I	X	A	B	C
T_1	a	p	p	p
T_2	p	p	p	p

II	Y	A	B	C
T_2	a	p	p	p
T_3	p	a	a	a

This representation of the structure of Pirenne's explanation shows, I believe, that his causal conclusion is based upon the Method of Difference. In I this Method allows Pirenne to claim that the Germanic invasions made no difference in the nature of the commercial relations, monetary policy, or in the location of the largest cities in the Empire. In II, on the other hand, we see that the Moslem invasions made all the difference, the presence of the Moslems radically altered the states of affairs represented by A, B, and C. Note, finally, that even though Pirenne's causal judgment does presuppose a contrast between continuity and discontinuity occurring within periods of a specified duration, which does imply a numerical comparison, the conditions he contrasts with one another in order to arrive at that judgment are not represented statistically.

(C) Summary and Conclusion

In this chapter I have focused my attention on part of Hart and Honoré's contribution to the analysis of singular causal judgments. As we have seen, they have not only responded to Mill's challenge to find principles guiding the selection of causes, but have done so in a very thorough and systematic way. Their remarks concerning factors that "make a difference" and the way they subsume this notion under what they take to be an even broader notion, i.e. abnormality, provides the background against which they analyze the great number of examples of singular causal judgments they present.

As far as applying their analysis of these judgments to the ones historians make I, like Morton White and William Dray, believe that their analysis can be readily applied to the kinds of causal judgments historians make. However, I have insisted throughout that the

abnormality criterion is but one aspect of the idea that a condition that makes the difference is the cause of an event. In this regard the respective importance I attach to the two notions--making a difference and abnormality--runs counter to Hart and Honoré's view of this relation. The reasons why I attribute so much importance to the idea of difference have been articulated. To summarize them: (1) Hart and Honoré's conception of abnormality aligns perfectly with Mill's statement of the Method of Difference, (2) historical examples analyzed in terms of abnormalism (e.g. Morton White's analysis of Pirenne's explanation of the collapse of the Mediterranean Commonwealth) can be reformulated more precisely in terms of the Method of Difference, and (3) judgments of causal importance--examples of which were provided by R. G. Frey, Ernest Nagel, and Richard Martin--also are formulated on the basis of the Method of Difference.

In view of the increased scope and simplicity of my analysis of the kinds of causal judgments made by historians I think that this analysis is an improvement on the one offered by Hart and Honoré.

Having made a point of what I take to be my own distinctive contribution to the analysis of singular causal judgments I should point out that there are certain aspects of the abnormality criterion that were ignored in this chapter. The reason for this was stated at the outset: Hart and Honoré draw a broad distinction between two types of contexts in which singular causal judgments are made, explanatory and attributive. Thus far I have dealt only with those judgments that occur in explanatory contexts. In the next chapter I want to examine in some detail the relationship between these types of causal inquiry, especially those in which the normal/abnormal distinction plays a fundamental role. What we will find, I suggest, is that while Hart and

Honoré's characterization of the explanatory-attributive contrast is not entirely acceptable, the distinction can (with certain modifications) be used as a way of distinguishing objective singular causal judgments from subjective ones. What I mean and will explain more fully in the next chapter is that the correlative terms normal and abnormal can be interpreted in various senses. In the present chapter normal has been interpreted in a statistical sense; something is normal if it is in accord with what usually or ordinarily happens, abnormal if it departs from these states of affairs. The Method of Difference, as I have presented it, codifies this relationship.

When normal and abnormal are understood in the statistical sense I think that the causal judgments based upon these assessments are objective. However, there are other senses in which these terms can and commonly are understood, such that when these determine the content of the 'normal-abnormal' contrast the resulting causal judgments may be subjective in character, i.e. they depend upon a prior value judgment having been made by the inquirer. Thus, by distinguishing different senses of normal and abnormal we will be in a much better position to see why it is that singular causal judgments are not all alike and how this ambiguity can account for some of the disputes historians engage in about the cause of a given event.

CHAPTER FOUR: HART AND HONORÉ--ATTRIBUTIVE CAUSAL INQUIRIES

In the last chapter a number of different aspects of Hart and Honoré's theory of causal explanation were presented. Foremost among these was the distinction between normal and abnormal conditions. While it was noted how this distinction could account for many singular causal judgments made in everyday situations, the law, and history, I tried to show how it could be reformulated in terms of the Method of Difference. In addition, a number of different kinds of causal explanations that utilize this method were reviewed.

The fact that I have placed the abnormality criterion under the same umbrella of explanatory strategies as experimental results and judgments of causal importance would therefore seem to indicate that I hold a rather pluralistic view of causal explanation. This is indeed the view I hold, but it does conflict with that of Hart and Honoré in several important ways. In this chapter, then, I will present the two basic components of their theory of causal explanation (i. e. their understanding of 'explanatory' and 'attributive' contexts of inquiry) and, beginning with a critical review of their conception of explanatory inquiries, assess the strengths and weaknesses of the theory of causal explanation they have offered.

(A) Limitations of Hart and Honoré's Conception of Causal Explanation

It will be recalled that Hart and Honoré characterized explanatory causal inquiries as ones generated by "some contingency we find puzzling: we do not know why or how it happened. Here the inquiry into causes brings to light previously unknown factors." (23) Although I suggested earlier that this view of causal explanation could explain the automobile examples discussed in section B of Chapter II, it seems

to me that it is much too narrow to account for many types of causal inquiry that are explanatory: Not only would Fogel's counterfactual model-building and Martin's judgments of causal importance fail to rank as explanatory, so too would Hart and Honore's prime example of relativity to point of view.

The fundamental problem with their conception of explanatory inquiries is that they place too much emphasis on bringing to light "previously unknown factors." This emphasis makes it appear that all causal explanations are, like Sherlock Holmes' investigations, attempts to identify factors that were unknown to the investigator when he commenced his inquiry. But this view is much too restrictive. Though I would agree with Hart and Honore that for anything to count as a causal explanation there must be some initial lack of knowledge on the part of the investigator, this need not imply that some factor is unknown to him. Take, for example, Fogel's explanation of the contribution of the railroad to American economic growth in 1890. The railroad, as a factor, was surely not unknown to Fogel when he began his inquiry. Nor was there any doubt that the railroad did contribute to American economic growth. What he was uncertain about was the amount of this contribution. I see no reason why answers to questions of the form: "How much did this factor contribute to that result?" should be barred from claiming the title of explanation. The very same considerations apply to what I have called judgments of causal importance and what Raymond Martin refers to as "Comparative Causal Explanations." With these as well, there are no unknown factors to uncover, the problem is to determine which of two factors was more important in bringing about a certain result. In both types of inquiry, then, there is some-

thing that is unknown to the inquirer, but it is not the identity of some factor.

If we now go on to reconsider Hart and Honoré's example of the man who suffered an attack of indigestion I think we will find that their point about bringing to light "previously unknown factors" becomes even more suspect as supplying the intension of the term "explanation." In that example the wife claimed that it was the parsnips he ate that was the cause of her husband's indigestion while the doctor said the cause was the ulcerated condition of his stomach. The wife knew right from the start that her husband had stomach ulcers and also that he had eaten parsnips. So where is the unknown factor? On the other hand we are led to believe that the doctor initially knew absolutely nothing about his patient except that he suffered a severe attack of indigestion. In this case, eating parsnips and having an ulcerated stomach are both factors that were previously unknown to the doctor. Still, he had no difficulty in specifying which of these two factors was the cause of the indigestion. Consequently, there being one or more factors that are initially unknown to the inquirer cannot be what entitles his or causal judgment to be deemed explanatory.

The fact that Hart and Honoré's conception of what is to count as a causal explanation is too narrow is the first indication that their distinction between explanatory and attributive causal inquiries is not entirely satisfactory. But in order to arrive at an understanding of the way these inquiries do and do not differ, we need to examine in greater detail their views on attributive inquiries.

(B) The Nature of Attributive Inquiries

According to Hart and Honoré attributive causal inquiries begin where explanatory ones end, that is, all the facts are known and the

only question that remains is which of several factors is the cause of some harm. What precipitates the kind of difficulties that arise in attributive legal inquiries is that

> among the conditions required to account for the harm which has occurred, there is found in addition to the defendant's action a factor (usually a human action or some striking natural phenomenon) which itself has some of the characteristics by which common-sense distinguishes cause from mere conditions; so that there seems as much reason to attribute harm to this third factor as to the defendant's action. (23)

The part of this paragraph that follows the semicolon seems most important to me. What Hart and Honore appear to be describing is a situation in which there is a cause competition. Typically, the sort of case they have in mind is one in which a person dies in a hospital after having been run down by a car, then, while being transported to the hospital in a stretcher, is hit on the head by a falling tile. (127) Concerning legal cases like this they write:

> In all these instances we assume that the wrongful act and the third factor were each a necessary condition of the harm, but in each instance a causal problem is raised by the presence of the third factor: the law must decide whether or not the third factor negatives causal connexion. (127)

I think it is safe to say that this type of cause competition is endemic to the law. There will always be cases in which two or more causal factors contribute to some outcome, but where one of these must, for legal purposes, be designated the cause, and the others conditions. With this part of Hart and Honore's analysis of attributive legal inquiries I have no quarrel. But it seems to me that they mistakenly assume that the competitive aspect of attributive legal inquiries is what differentiates these from explanatory inquiries as such. If this is indeed what they are assuming then we have another reason to suspect

the distinction they draw between explanatory and attributive causal inquiries. To see why this is so we need to look more closely at other types of causal competitions.

(1) Cause Competition

Cause competition, as I conceive it, can take two forms. First, a single author may identify a number of conditions that were necessary for some event and then ask himself or herself: Which of these was the cause of that event? Here, as Hart and Honore have suggested, there may be two or more conditions that possess some characteristic in terms of which we ordinarily distinguish cause from conditions. The competition then consists in eliminating all but one of such conditions as the cause.

A second, and very prevalent way in which cause competition occurs is when one person cites one condition of an event as its cause and another person, disputing that claim, accords the title of cause to another condition of that event. This form of cause competition is reflected in all manner of causal controversies in everyday life, law, science, and history. Such debates, however, need not have anything to do with attributing responsibility for harm, though of course they sometimes do. In order to appreciate the general character of this form of cause competition, which should enable us to see the way in which it resembles the particular form attributive inquiries take in the law, I want to present and offer an analysis of one example of a cause competition that does not involve the notions of responsibility or harm at all, and then a second example that involves the notion of responsibility but not of harm.

(a) An example of a non-legal cause competition can be taken from Elizabeth Eisenstein's two volume work, The Printing Press as an Agent of Change.[2] One of Eisenstein's objectives in this work is to challenge prevailing causal explanations of the unprecedented rise of science in the 16th and 17th centuries. Eisenstein notes that many historians of science have pointed to the "stagnation" that marked the scientific enterprise in the Middle Ages. To explain this phenomenon these historians have suggested that it was caused by a lack of interest in natural phenomena, that it was due to inattention to mathematical order, and to unconcern with precise measurement. Eisensteim suggests that all of these explanations are ultimately unsatisfactory. Instead, she claims that the absence of progress should be attributed to the lack of a form of technology that could reproduce tables, charts, diagrams, and maps without distortion and error.

Prior to the invention of the printing press, she suggests, an inordinate amount of time and energy was devoted to the presentation and reconstitution of manuscripts that were in danger of being lost forever. "During the interval that elapsed between Ptolemy and Regiomontanus," she writes,

> new evidence was less likely to accumulate 'gradually' than to get corrupted or get lost and have to be retrieved or restored. It is not so much 'stagnation' or even a slow rate of advance that needs explaining when dealing with conditions before printing, it is rather the manner in which a process of loss and corruption was temporarily arrested, and even on occasion reversed. (464-65)

Against the view that the deficiencies and inconsistencies to be found in medieval science are attributable to "a prolonged 'lack of interest' in natural phenomena" (469), Eisenstein claims that "Present evidence suggests that medieval natural philosophers were not lacking in

curiosity. They were, however, lacking some essential investigative tools." She then goes on to add, "In view of the new equipment printing provided, it seems worth thinking longer about the new implementation of old aims before assuming that the significant change came from a shift in attitudes and goals." (469-70) On another front she insists that

> Belief in mathematical order, 'a habit of definite exact thought', and concern with precise measurement need not be seen as late medieval novelties ....They may be regarded as long prevailing attitudes held by different groups." (465-66)

Eisenstein's thesis, that science could not advance rapidly "until the process of textual transmission was transformed and data collection was placed on a new basis" (464-65) is of interest here because it is meant to replace rival explanations of the rise of science. Of course Eisenstein does not wish to deny that interest in natural phenomena and "'a habit of definite exact thought'" were necessary conditions for that rise. What she does wish to dispute, however, is the claim that either of these conditions constitutes the cause of that rise. Of crucial importance is the manner in which she reduces these two conditions to the status of mere conditions. She does this by pointing out that they were common to both those who failed and those who succeeded in extending scientific knowledge. As such, they could not have made the difference. In a moment I will indicate how Eisenstein used the Method of Difference as a basis for her causal judgment. Before doing so, however, I want to show how the very same strategy is at work in a book by a historian who is interested in attributing responsibility for the final outcome of the Congress of Vienna, a context that more nearly resembles what Hart and Honoré have called attributive inquiries.

(b) In the ninth chapter of A World Revisited,[3] then Harvard historian Henry Kissinger seeks to explain the nature of the complicated cluster of problems that were eventually resolved at the Congress of Vienna. Without going into the details of his general views on the problems besetting international settlements, I would like to focus on his explanation of the terms of the settlement and, in particular, the role played by the French plenipotenciary, Talleyrand.

Kissinger contends that there arose a diplomatic stalemate between the status quo powers, Britain and Austria, and the acquisitive powers, Russia and Prussia. This stalemate could only be broken by adding an additional weight to one side or the other. France, whose exploits under Napoleon was the raison d'etre for the Congress, turned out to be that additional weight. Being the only uncommitted major power, the former enemy emerged as the key to the settlement. "Thus," writes Kissinger,

> grew up a myth about Talleyrand's role at the Congress of Vienna, of the diabolical wit who appeared on the scene and broke up a condition of hostile powers, who then regrouped them into a pattern of his liking by invoking the magic word 'legitimacy' and who emerged, finally, as the arbiter of Europe. (147-48)

Kissinger contends that such a view of Talleyrand's importance has been held by many historians, two of the more prominent being Harold Nicolson and Crane Brinton.[4] He insists, however, that such a view of Talleyrand's role at the Congress is a myth which "gained currency because, Talleyrand, whose monarch had not come to Vienna, was obliged to write voluminous reports, and, in order to cement his domestic position, the former Foreign Minister of Napoleon tended to emphasize his indispensability." (148)

Another, and more important aspect of Kissinger's attempt to downplay the importance of Talleyrand's contribution to the final settlement reached at Vienna is his view that

> arguments not dissimilar to Talleyrand's, at least with reference to Russia's acquisitiveness, had been used six months previously by Napoleon without effect, because nobody trusted him. The real transformation in the situation had been wrought, not by Talleyrand's memoranda, but by the Bourbon Restoration and the Treaty of Paris. Talleyrand could be effective because these acts had ended a revolutionary situation and inaugurated a 'legitimate' era. He could be successful, not because he invented the concept of legitimacy, but because it was there for him to exploit. (148)

According to Kissinger, then, the Bourbon Restoration and the Treaty of Paris provided the opportunity for France's plenipotenciary to assert his influence; in the absence of these two events Talleyrand could not have been effective. This claim is supported by the fact that Napoleon, who had "presented arguments not dissimilar to Talleyrand's," failed to convince the other European powers to include France in a general European settlement. Thus, it could not have been these arguments that made the difference.

Now it seems to me that Kissinger's explanation of Talleyrand's role at the Congress of Vienna is quite similar to Eisenstein's explanation of the rise of science in the 16th and 17th centuries. Both proceed by challenging previous claims that some condition was the cause of some result and the way they do so is the same, that is, they argue that the condition previously cited as the cause was present on the occasion when the result occurred as well as on previous occasions when the result did not occur. Consequently, such a condition could not have "made the difference."

Causal explanations such as those given by Kissinger and Eisenstein can, in my view, be construed as the simplest form of the Method of Difference. Represented symbolically they appear as follows:

A B occurs with a b

B occurs with b

Therefore, A is the cause of a

Applying this schema to Eisenstein's account, A would be the invention of the printing press, B would be the presence of an interest in natural phenomena or, alternatively, "a habit of definite exact thought," a would be the rapid advance of science and b would be some circumstance common to both the centuries comprising the Middle Ages and the 16th and 17th centuries.

Turning to Kissinger's account of Talleyrand's role in the settlement reached at Vienna, A would be the combined effect of the Bourbon Restoration and the Treaty of Paris on the attitudes of European powers towards France, B would be the arguments presented by Napoleon and Talleyrand, a would be entry into the negotiations at Vienna, and b would be some circumstance common to both the period prior to and subsequent to the Bourbon Restoration and the Treaty of Paris.

To sum up, the Kissinger and Eisenstein examples illustrate what I take to be one very prevalent type of cause competition. Although neither example involves the notion of responsibility for harm, they seem to possess the feature in terms of which Hart and Honore originally characterized attributive causal inquiries, i.e. there are two or more conditions that possess some characteristic in terms of which we ordinarily distinguish cause from mere conditions. It is in virtue of this common feature that I base my claim that attributive causal

inquiries are but a special case of cause competition. Finally, since cause competitions frequently occur in explanatory causal inquiries, e.g. the wife-doctor example, I do not think that Hart and Honoré's initial characterization of attributive inquiries serves to distinguish these from explanatory ones.

Now that an important point of resemblance between explanatory and attributive causal inquiries has been examined we can turn to those features of attributive inquiries that do set them apart from explanatory ones.

(C) Special Features of Attributive Inquiries

The discussion of the last section has shown that the competitive aspect of causal inquiry is not what distinguishes explanatory from attributive ones. When we once again consult Hart and Honoré on this problem we find that they propose two further respects in which these types of causal inquiry differ. In my view one of these proposals is entirely sound, the other, however, is mistaken. Here I will start off on a positive note and discuss their sound proposal.

At one point in the chapter of their book that is entitled "Causation and Responsibility," Hart and Honoré remark that in attributive legal cases "we are only in a position to say that he (the defendant) has caused harm when we have decided that he is responsible." (62) What I find interesting about this remark is that virtually the same point was made by J. L. Mackie in an article that appeared prior to the publication of Hart and Honoré's book, though Mackie's point is meant to pertain to situations outside the law. So what we get, initially, is a common view that applies to legal and extra-legal attributive inquiries. However, the big payoff comes when we consider

Mackie's analysis of the particular example he develops, for then we see that the inadequacy of Hart and Honore's final proposal rests upon a confusion of the idea of vagueness and that of ambiguity.

In "Responsibility and Language" Mackie offers the same observation as Hart and Honore subsequently made; i.e. in order to determine what a person "did," in the sense of caused to happen, we must have already determined what he was responsible for. Mackie describes this situation as a "curious inversion of the utilitarian account of moral judgment" because "according to utilitarianism, you determine whether one act is right or wrong by looking at its results; but we have found that you cannot determine what events are to count as its results without first deciding whether it is right or wrong." (145)

In the law, deciding whether an act is right or wrong usually involves a decision as to whether a given legal rule imposes a responsibility on a given person or persons. A case presented by Robert Keeton makes this point clear. In this case

> the defendent, proprietor of a restaurant, placed a large unlabelled can of rat poison beside cans of flour on a shelf near a stove in a restaurant kitchen. The victim, while in the kitchen making a delivery to the restaurant, was killed by the explosion of the poison. Assume that the defendant's handling of the rat poison was negligent because of the risk that someone would be poisoned but that the defendant had no reason to know of the risk that the poison would explode if left in a hot place.[5]

In discussing this example Joel Feinberg correctly points out that the defendant's conduct was negligent with respect to its having "created a risk of poisoning, but the harm it caused was not within the ambit of that risk. The risk of explosion was not negligently created."[6] Hence, the respect in which the defendant's act was wrongful, i.e. what he was

under a legal obligation to refrain from doing, was causally irrelevant to the victim's death.

In another case of this type Hart and Honore report that

> a plaintiff claiming compensation for loss of sheep washed overboard in a rough sea for want of sheep pens prescribed by statute, is told that his claim fails because the loss which 'we assume to have been caused by' the defendant's breach of statutory duty is 'of such a nature as was not contemplated at all by the statute', that 'there was no purpose direct or indirect to protect against such damage....the Act is directed against the possibility of sheep or cattle being exposed to disease....(88)

These last two legal cases make it clear that it is the purpose for which a legal rule was created that ultimately decides the causal issue. For this reason Hart and Honore are entirely correct when they characterize attributive legal inquiries as ones in which

> after it is clearly understood how some harm happened the courts have, because of the form of legal rules, to determine whether such harm can be attributed to the defendant's action as its consequence, or whether he can properly be said to have caused it. (23)

Having acquired an understanding of how attributive legal inquiries operate let us now consider what occurs in attributive inquiries outside the law. Towards this end it will be useful to look at a story that Mackie relates. The story reads like this:

> In Sydney some time ago a motor cyclist was exceeding the speed limit; a traffic policeman, also on a motor cycle, chased him, and soon they were both traveling, according to reports, at 70 m.p.h. Then an unobservant citizen stepped off a bus into the policeman's path; in the crash that resulted the other man was killed at once; the policeman died the next day. (143)

This story provides a good example of what Hart and Honore have called "attributive inquiries" for though we understand how the accident occurred, we are not so sure who caused it, that is, whether the accident

can be treated as the consequence of the action of the cyclist, the policeman, or the pedestrian. As in the legal examples already presented, the solution to the causal problem rests upon a determination of what these people were responsible for. In Mackie's view, the answer we chose "will depend on what we take to be the normal, proper, or expected course of events; the person that we hold responsible is the one who steps outside this expected pattern." (144)

At first glance there may appear to be little difference between the conditions set out by Mackie to explain how one would go about assigning responsibility for the accident and Hart and Honore's description of the abnormality criterion, i.e. the cause is a departure from the "normal, ordinary, or reasonably expected course of events." Yet it is not difficult to see that what is normal may not be what ordinarily happens, that what can reasonably be expected may not be normal, and that what is normal, ordinary, or reasonably expected may not be what is proper. Far from being synonymous, these terms are notoriously ambiguous, the intensions of any two of these terms may, in one case, exhibit considerable overlap, in another they may have nothing at all in common except that they can both be invoked as standards against which abnormal conditions are identified. As such, we can expect to find many situations in which people will offer different and perhaps incommensurable singular causal judgments because they are adopting different senses of "normal."

This, it seems to me, constitutes a principal feature of attributive causal inquiries. So long as the cause of an event is identified with an abnormal condition and what is an abnormal condition is determined by one or another sense of "normal" that is adopted, we have a situation where singular causal judgments will all be formally alike

yet differ with respect to content. To see why this is so consider Mackie's discussion of the grounds upon which each of the three persons involved in the accident might be said to have caused it.

If, in the first instance, the cyclist is judged to have been responsible for the accident the following assumptions would be made:

> that it is normal and proper for traffic police to pursue, relentlessly and with all the means in their power, those who break the speed limit, and that it is normal and proper for people to step off busses without taking precautions against motor cycles passing at 70 m.p.h., but that it is not normal and proper for cyclists (other than police in pursuit of a criminal) to break the speed limit. (144)

Secondly, if the policeman was deemed responsible for the accident the grounds for this judgment would be that

> the cyclist's conduct, though illegal, was yet normal and expected, including his increase in speed when chased, whereas it was not normal, not 'reasonable', for the policeman to go to such lengths to catch the speedster. (144)

Although it would be unlikely that anyone would want to hold the pedestrian responsible for his own death and that of the policeman, Mackie does present us with a scenario wherein just such a judgment would seem appropriate. Here we must assume that

> both law-breaking and the all-out pursuit of criminals form two parts of a normal social activity, as together constituting something like a game, but a very important game, with which the rest of society should not interfere. (Thus) we might regard the man who stepped off the bus as the intruder into this field of normal activity, and so as being responsible for the accident. (144)

I think that the account Mackie gives of the grounds upon which each of the three parties in the accident could be said to have caused makes it clear that singular causal judgments that result from a contrast between normal and abnormal conditions will vary according to senses

of "normal" that are adopted. This is why I believe that Hart and Honoré are mistaken when they claim that attributive inquiries are unique in that they "are generated less by ignorance of fact than by the vagueness or indeterminacy of the very concept of causal connexion which we are endeavoring to apply in a particular case." (23)

In my view, vagueness is not the problem with attributive inquiries, but the fact that clear yet distinct senses of normal can be appealed to as the basis for making singular causal judgments. Once we recognize that the term "normal" can have different meanings it becomes easier to see that as a theory of causal explanation, abnormalism is operative in quite diverse contexts of inquiry. In explanatory contexts it assumes the shape of the Method of Difference, in attributive ones, where standards of normality are a function of people's responsibilities, it expresses a relationship between appropriate action and negligence.

In light of what my analysis of abnormalism has revealed the idea that some singular causal judgments are objective and others subjective gains considerable support. So long as attributive causal judgments require that a person's responsibilities must be ascertained before we can say what he "did" (and responsibilities can be incurred as well as assumed), there will always be a value judgment that guides the selection of cause. On the other hand, we had ample opportunity to see that the Method of Difference provides an objective criterion for many singular causal judgments and judgments of causal importance.

(D) Attributive Causal Inquiries in History

The distinction between explanatory and attributive causal inquiries would not merit the attention I have given it were it not for the

fact that historians present their readers with singular causal judgments of both kinds. We have already seen how Pirenne, Kissinger, and Eisenstein use the Method of Difference to arrive at their causal judgments, but we have not yet looked at the attributive causal judgments offered by historians. Towards this end I want to explore in some detail a single example that shows how two historians, both acknowledging that a contrast between normal and abnormal conditions can determine what is the cause of an event, can cite different conditions as the cause simply because they do not interpret "normal" and "abnormal" in the same way. The case study I am about to present concerns the controversy between A. J. P. Taylor and Hugh Trevor-Roper over the cause of the Second World War.

In what follows my concern with their rival explanations of the cause of the war will be limited to an examination of the way that the cause-conditions distinction is both accepted and applied by these authors. Moreover, in this section I will deal only with the way in which they contrast normal with abnormal conditions. Towards this end it will prove useful to draw upon W. H. Dray's recent article, "Concepts of Causation in A. J. P. Taylor's Account of the Origins of the Second World War."[8] With respect to Taylor and his critics, Dray attempts to show "that at least four (and perhaps five) different models or paradigms of causal thinking can be discerned at points in their work" where the drawing of the cause-conditions distinction is at issue. However, since we are concerned only with the application of the abnormality criterion in this section we will only consider Dray's second paradigm, which corresponds to this criterion.

According to Dray the basic idea underlying the second paradigm of causal thinking to be discerned in Taylor's The Origins of the

Second World War[9] is that of "causes as conditions which disrupt, or interfere with, or intrude upon, a settled state of affairs: or conversely, which restrict or impede ongoing processes or movements." (161-62) In short, causes, in contrast with mere conditions, are to be understood as abnormal interventions. In light of our previous discussion of senses of normal and abnormal we would do well to remember that "a settled state of affairs" or "an ongoing process" may themselves be regarded as either normal or abnormal. As such, what conditions are cited as causes will depend upon these prior judgments of normality and abnormality. Should states of affairs that embody justice and fairness be treated as "normal" and those deficient in these properties as "abnormal," then a given state of affairs may be abnormal and attempts to disrupt it, normal. Henceforth resistance to attempts to disrupt the established order will be deemed "abnormal" and so, as the cause of whatever harm results from the encounter between the forces of change and those of stability.

The same remarks apply to Dray's characterization of causes as conditions which "restrict or impede ongoing processes." While it is true that attempts to restrict or impede ongoing processes are often viewed as causes, such judgments depend upon the assumption that these are "normal" or "natural." Should they be regarded as abnormal or unnatural, then attempts to restrict or impede them may be deemed normal while attempts to keep them going, abnormal.

In the context of the historical debate between Taylor and Trevor-Roper the contrast between normal and abnormal conditions is most obvious in Trevor-Roper's critique of Taylor's causal judgment. Writes Trevor-Roper,

> All we have to do is to ask ourselves, at what point do we make our calculation of reality? This then provides us with a datum. Mr. Taylor takes as his datum the Spring of 1918. At that time Germany was victorious in the West and triumphant in the East. This, he implies, was the "natural" situation; the Allied victory later in 1918 was artificial--or at least it was made artificial (or, in his words, deprived of "moral validity") by the failure of the Allies to carve up Germany before making peace. This omission left Germany still potentially the greatest power in Europe, naturally tending to revert to the "real" position of January 1918.[10]

If I am right, Trevor-Roper's characterization of Taylor's position reveals that Taylor was operating with two contrasts between normal and abnormal conditions. On the one hand he seems to regard the Allied victory in 1918 as an abnormal condition setting the stage for future developments because it upset the "natural" situation. On the other hand, Taylor appears to treat "the failure of the Allies to carve up Germany before making peace" as an abnormal condition in its own right, one that invited or even encouraged German leaders to try to recoup the losses incurred as a result of their defeat in the First World War.

The latter abnormality, that is, the omission on the part of the Allies to divide after they had conquered, seems to be regarded by Taylor in a purely prudential sense. What he appears to be saying is this: given that the Allies intended to prevent German expansion in the future ("the armistice was more than a cessation of fighting. Its terms were carefully framed to ensure that Germany could not renew the war." p. 46), and given that the Germans could be expected to try to acquire Lebensraum, despite their defeat, the Allies ought to have taken appropriate steps to insure that opportunities were not provided that would enable the Germans to realize their aspirations. Since the Allies did not carve up Germany before making peace, and this, according

to Taylor, encouraged German leaders to try to recoup their losses, such an omission was a blunder, a mistake that proved costly.

To call an act or omission abnormal in a prudential sense is to say that it falls below a certain standard of reasonable care. Analogous cases make this point clear. If a policeman were to apprehend a murderer at the scene of the crime and attempt to transport the criminal to the station-house without having first confiscated his gun, such an omission, should the criminal shoot and kill the policeman with that weapon, may plausibly be regarded as the cause of the policeman's death. Here we might be tempted to say that the policeman "brought it upon himself." The same conclusion would apply if, instead of having been apprehended at the scene of the crime, the criminal had escaped but had left his wallet beside the victim, thereby enabling police to identify, locate, and arrest him.

In both of these cases the agents omitted to take adequate precautions while pursuing their objectives. What this appears to show is that omissions can sometimes be regarded as abnormal conditions independently of whether one approves or disapproves of the objective in pursuit of which the agent committed his faux pas. Now Taylor believes himself to have provided his readers with just such an objective account of what were normal and what were abnormal political conditions in the early part of the 1920's. Yet the very fact that he speaks of the Allies' omission as a failure may lead us to think otherwise. After all, not all omissions are failures. If there is a blizzard on the island of Bermuda and the local economy is crippled as a result, such a result could hardly be attributed to the Bermuda government's not having previously acquired Canadian snow-removal equipment. Because blizzards rarely, if ever occur in that part of the

world, the occurrence of such an event could not reasonably have been expected, and as a consequence, the government cannot be said to have failed to acquire snow-removal equipment. To suggest otherwise would be to apply an unreasonably high standard of care to that government.

At one level the debate between Taylor and Trevor-Roper appears to center on just this issue; given the Allies' desire to prevent German expansion in the future, did the Versailles Treaty constitute a reasonable standard of care? From Taylor's point of view it did not, from Trevor-Roper's it did. Taylor therefore judges the Allies' omission as a failure, Trevor-Roper does not.

But upon what basis are we to judge whether or not the Treaty of Versailles represented an adequate standard of care against the prospect of future German expansionism? Although Trevor-Roper never explicitly responds to this question his attitude seems to be that since the German government signed the treaty, any "normal" German leader would, in the future, comply with its provisions. As such, the treaty was adequate as a basis for deterring future German expansion. Taylor, on the other hand, eschews this narrowly legalistic approach. For him, what could reasonably be expected from the Germans in the future can only be determined by consideration of the state of affairs that obtained in the Post-Versailles period. Thus, he proposes that the basis for this determination be sought in the breakdown of the system of collective security that was one of the consequences of the First World War, as well as in the will and capacity of the Germans to exploit this breakdown to their own advantage. Towards this end he finds that (1) ever since the Franco-Prussian war there had been a noted tendency for Germans to seek expansion of their territory, (2) although the Allies defeated Germany in the First World War, their victory was not

achieved until Germany had inflicted great damage upon Russia. The German victory over Russia, along with the demise of the Austro-Hungarian Empire left a vacuum in eastern Europe which, in turn, created a marked imbalance in the previously existing Balance of Power, (3) with Russia sidelined for years to come, Germany had a larger population than any other Great Power on the continent (e.g. France) as well as the capability and resources (coal and steel) to rearm quickly, and (4) while Britain and France had defeated Germany in battle, they too were exhausted by the war effort and were willing to settle for an armistice with Germany, rather than pursuing their struggle to the point where they could have exacted an 'unconditional surrender' from Germany. (Summary of pp. 44-48--Penguin 1961)

What all of this amounts to, according to Taylor, is that the objective situation in Europe after the First World War favored the resumption of German expansionism, even after Germany's defeat. It could, therefore, have been expected by the victorious Allies. Having made his "calculation of reality" Taylor can argue that the Versailles settlement was not adequate as a precaution against future German expansion, only the dismemberment of Germany would have been sufficient to achieve this end. In fact, not only does he argue that the treaty was inadequate as a precautionary measure, but also that it exascerbated the existent problem (the "German Problem") and was, in this light, counterproductive.[11]

Given Taylor's claim that his "calculation of reality" is anchored in an assessment of the objective situation that obtained in the period immediately after the First World War it is interesting to note that Trevor-Roper accuses Taylor of having appealed to a state of affairs existing at the time when he was writing his book, some fifteen years

after the Second World War, as his basis for determining whether the treaty of Versailles was an adequate precautionary measure. In the final portion of his critique of Taylor's book Trevor-Roper claims that Taylor has endorsed the view that since Germany had been partitioned after the Second World War and no new agression was to be seen, had the Allies done so following the First World War the second war need never have been fought. What made the difference between German foreign policy after the first and after the second World War was whether Germany had been partitioned or not. Personally, I have not found Taylor arguing in this way in his book, though I can understand why Trevor-Roper wishes to impute an argument of this kind to Taylor. For, to adopt this argument one must assume a ceteris paribus clause, and of course Trevor-Roper adamantly maintains that other things were not equal, most notably the presence in Berlin of a German leader who suffered from megalomania.

While Dray does not treat "the failure of the Allies to carve up Germany before making peace" as an abmormal condition in its own right, he does consider another of Taylor's arguments that turns on the idea that providing someone with an opportunity to do harm may rank as an abnormal condition, and so, as the cause of subsequent harm. "Taylor," writes Dray,

> views European statesmen of the thirties, other than Hitler, as having had, by virtue of their positions and powers, the task of identifying and neutralizing such threats to peace as Hitler represented. It should have been clear to them, Taylor apparently believes, that Hitler would take advantage of any opportunities he found for harm in this direction. These opportunities, therefore, ought not to have been provided. Since they were provided, and were in fact exploited, the cause of the harm that resulted must be traced to those who made it possible. (167)

This argument is a good example of what Hart and Honoré took to be a distinctive feature of attributive inquiries, i.e. that in order to say what a person has caused one must first determine what he was responsible for. Clearly Taylor is attributing certain responsibilities to "European statesmen, of the thirties, other than Hitler." It is in virtue of their having these responsibilities that their actions are considered prime candidates for causal status.

Dray expresses uncertainty as to whether this line of reasoning can be brought under his third paradigm of causal thinking. Although we have not dealt with this paradigm as yet (see pages 121-142, Chapter Five), it seems to me that it can easily be accommodated under the second paradigm.[13] The argument Taylor is advancing here is the very same one he offered with respect to the Allies failure to carve up Germany before making peace. What he is claiming is this: Given the Allies' interest in preventing German expansion, and given that Hitler could be expected to pursue an expansionist foreign policy, it was a prudential error on the part of the Allies to have provided Hitler with such opportunities for expansion that he would welcome and be able to capitalize upon. Consequently, the provision of such opportunities is regarded by Taylor as abnormal insofar as it falls below an adequate standard of care.

The second contrast between normal and abnormal conditions that is exhibited in Trevor-Roper's characterization of Taylor's position has to do with the state of affairs taken as a "datum." Taylor takes the spring of 1918 as his datum, Trevor-Roper, the Allied victory later that year and the Versailles settlement. In each case the "datum" that is chosen constitutes a standard of normality by which future developments are gauged. Since Taylor takes the point in time when

Germany was "victorious in the West and triumphant in the East" as the "natural" or normal state of affairs, the Allied victory in 1918 is considered abnormal.[14] For Trevor-Roper, on the other hand, since the Allied victory and the Treaty of Versailles were "normal," Hitler's disrupting the state of affairs created by these two events was abnormal. Clearly, when the choice of standard of normality differs, so too will the causal judgments arrived at on the basis of these standards. But why is it that Taylor and Trevor-Roper cannot agree on a single "datum"? Dray has suggested that it is because they are making value judgments, such that

> what often lies behind the judgments of normality that lie behind the judgments of causality made by Taylor and many of his critics appears to be a fundamental political value judgment: a conception of what it was legitimate, politically speaking, to expect from Germans, post-Versailles Europeans, or great powers generally, in the twenties and thirties. (163)

Even if we agree with Dray that a conception of what it was legitimate to expect from these groups of people does lie behind the judgments of normality made by Taylor and his critics, we may still ask, whose conception? Is it the historian's own conception or is it that of some individual or group of individuals who were contemporary with the events he is writing about? With respect to the causal disagreement between Taylor and his critics this question is difficult to answer, for they were not only contemporary observers of German expansionism under Hitler but were also active participants in the war effort. Taylor, for example, claims to have been an anti-appeaser from the day Hitler assumed power. Yet many of his critics, e.g. Trevor-Roper, contend that his Origins presents an argument in favor of appeasement. Taylor himself claimed to be utterly detached from any source of

personal or national bias. In fact, at the close of the first chapter of his book he informs the reader that he will attempt "to tell the story as it may appear to some future historian, working from the records."[15] The extent to which he succeeded in this attempt is indicated, perhaps, by the great amount of criticism levelled at his use of the term "morality." In particular, critics protested his description of the Versailles Treaty as "lacking in moral validity from the start," and so, in his "Second Thoughts," Taylor qualified what he intended by the use of these terms. In so doing he explicitly responds to the question I have posed above. Wrote Taylor,

> I ought perhaps to have warned the reader that I do not come to history as a judge; and that when I speak of morality I refer to the moral feelings at the time I am writing about. I make no moral judgments of my own. Thus when I write that 'the peace of Versailles lacked moral validity from the start', I mean only that the Germans did not regard it as a 'fair' settlement and that many people in Allied countries, I think most people, soon agreed with them.[16]

The problem that Taylor's response to his critics poses for us can perhaps be restated as follows: can a historian present his readers with his <u>own</u> judgment about the cause of the war (in contrast with mere conditions) without either <u>endorsing</u> a political value judgment made by one or more contemporary observers (as opposed to other such judgments made by other observers) or supplying his own political value judgment, which was not held by any contemporary observers? Taylor seems to think that he can. As long as a historian only <u>reports</u> that different people had different conceptions as to what could reasonably be expected, e.g. from the Germans, he can, by carefully analyzing the evidence, conduct a kind of public opinion poll to establish which conception was most prevalent, taking this as his

standard of normality. In this way the historian's choice of standard of normality would have an objective basis, which is only to say that it is a factual claim that admits of confirmation or falsification. This, it seems to me, is how Taylor would like his remarks to be understood.

Dray acknowledges that Taylor's remarks serve to repudiate "in its fully normative sense" his earlier claim that the Treaty of Versailles "lacked moral validity from the start." However, Dray does not explain the distinction he draws between a fully normative and a less than fully normative sense of a claim. One would think that a claim either is or is not normative, i.e. such claims do not admit of differences in degree. Nevertheless, Dray sees quite a different conclusion following from Taylor's response to his critics than the one I have presented. According to Dray, if Taylor consistently applied his own precept in depicting the moral feelings at the time he was writing about, "the most he could consistently aim at would be a collection of causal diagnoses from various standpoints: German, British, Polish, and so on, whereas, in fact, he offers us his own diagnosis (as he should)." (168)

Even if one accepts the perspectivist approach to historical causation Dray has in mind, it does not follow that the causal diagnoses from various standpoints will necessarily differ. In claiming that large numbers of people in all Allied countries came to share the German view of the "morality" of the Versailles Treaty, Taylor seems to be making just this point. As such, he thinks that quantifying these moral feelings provides him with an objective standard of normality, one that is not based on his own political value judgment.

Still, it is one thing to say that Germany was treated unfairly at Versailles, quite another to propose what ought to be done about it. If so many people felt that Germany had been unjustly treated one might expect that German grievances would be redressed peacefully. And, indeed, such attempts were made. The Dawes and Young Plans scaled down the amount of reparations to be paid and, at one point, President Wilson secured a one year moratorium on such payments. Territorial concessions were made as well. In the latter part of the 1930's first Austria and then half of Czechoslovakia came under German control, with the acquiescence of the British government. And so, according to Taylor, it seemed that the "slave treaty" was in the process of being undone and Germany was reestablishing her "natural weight." There were still the problems of Danzig and the Corridor, but these too could be settled without any great war having to be fought. At least this is what Chamberlain's government thought. But like so many other people who felt that the German grievances were justified and that Versailles had to be undone, Chamberlain misunderstood what this "undoing" implied. According to Taylor,

> Many people, including many Germans, said that Germany merely wanted to reverse the verdict of the First World War. This is correct. But they misunderstood what was implied. They thought that it meant only undoing the consequences of defeat--no more reparations, the recovery of European territory and the colonies lost by the Treaty of Versailles. It meant much more than this: not only that things should be arranged as though Germany had not been defeated, but that they should be arranged as though she had won.[17]

And what would the Germans have arranged if they had won the First World War? Taylor, citing the work of Fritz Fischer, claims that "it was a Europe indistinguishable from Hitler's empire at its greatest

extent, including even a Poland and a Ukraine cleared of their native inhabitants." What this means is that Hitler would not have been satisfied with the restoration of the position Germany enjoyed at the height of her success before she was defeated in the First World War. The point at which Germany was "victorious in the West and triumphant in the East" was not the "natural" situation, more territory would be required to reach that state of affairs.

The popular conception of what was to be expected from Germany and what needed to be done to fulfill those expectations explains the course of European diplomacy up to the point when Germany occupied Czechoslovakia. Then the moral feelings of the populace and of Allied leaders changed. Enough was enough. As compensation for previous wrongs the concessions already made to Germany balanced the scales of justice. Moreover, while these concessions were part of the solution to the problem of creating a new European order in the aftermath of the First World War, any further concessions would only aggravate this problem. From this time on resistance to German demands was "normal," appeasement "abnormal."

Like the policeman in the story Mackie relates, Hitler was thought to have been reasonable in pursuing German interests up to a certain point, such pursuits being "normal." But like the policeman again, once he persisted in this pursuit beyond that point his actions became abnormal. Being excessive, his demands are treated as the cause of the war that included Britain, France, Russia, and the United States. Accordingly, if Taylor's survey of the moral feelings shared by many people subsequent to Versailles provides an objective account of what were normal conditions up to Germany's seizure of Czechoslovakia, the

very same kind of account can be given of what were normal and abnormal conditions after that seizure, an account that would treat Hitler's actions as "abnormal," resistance to those actions, "normal."

Given that a majority of people may come to decisively reject a situation or state of affairs they formerly accepted, should we subscribe to the view that a historian's judgments of normality/abnormality can be value-neutral so long as he or she only applies this distinction to the moral feelings people express?

I think the answer must be yes. Moral feelings, like many other things, can be classified and counted. Should one type of moral feeling be shared by a majority of people then we have a consensus, and I see no reason to deny that a consensus represents a norm. If this is what is meant by claiming that a historian's judgment of normality/abnormality can be value-neutral then we have a rather innocuous claim. What is bothersome in Taylor's case, however, is that he appears willing to maintain this view only as long as it serves to support his argument that Hitler was not the cause of the war. When this view appears to undercut his argument he then feels free to abandon it.

If, as Taylor claims, so many people were committed to the view that certain provisions of the Versailles Treaty must be revised or abandoned (this being the norm), then surely he must support the view that once people corrected their misunderstanding of Hitler's desire to "revise the verdict of Versailles," resistance to new territorial demands became normal, pursuit of these demands abnormal. Yet we find that until the very end Taylor regards the German invasion of Poland as normal. Consequently it appears to me that Taylor is willing to adopt a public consensus as his standard of normality only so long

as he endorses the view it expresses. When his own views diverge from the consensus then he no longer seems willing to accept that consensus as a standard of normality. This indicates to me that Taylor's account of the origins of the Second World War is non-objective in a most important way: he refuses to consistently apply the same principle or mode of argument when this is warranted. In his case, then, Dray may be quite right when he suggested that it is the historian's own value judgment that determines which conditions are given causal status. However, I do not think that all historians are as inconsistent as Taylor. I therefore believe that historians can give objective accounts of normal and abnormal conditions so long as there really exists a consensus view.

(E) Summary and Conclusion

We began this chapter by noting precisely why Hart and Honoré's conception of causal explanation is too narrow. It was then argued that since one of the features they took to be distinctive of attributive causal inquiries was also present in many explanatory ones (i.e. cause competition), this feature cannot be what distinguishes the two types of causal inquiries. Next I considered the view that it was "the vagueness or indeterminacy of the concept of causal connection" that accounts for the distinctive aspects of attributive inquiries but found this view to be mistaken. Developing a specific point made by Mackie and Hart and Honoré I then proceeded to argue that what distinguishes explanatory from attributive causal inquiries is that in the latter but not in the former it must be determined what a person was responsible for before we can say what he has caused. Moreover, I pointed out that in legal contexts responsibilities are determinable

by consulting legal statutes but that in non-legal contexts the basis for the ascription of responsibilities will vary according to the sense of "normal" adopted.

When applying these findings to the historical debate between A. J. P. Taylor and Hugh Trevor-Roper I suggested that Taylor was operating with two distinct contrasts between normal and abnormal conditions. On the one hand he argued that the Allies caused the war because they were negligent in not taking precautions to insure the impotence of Germany; on the other hand he argued that Allied attempts to block the reversal of the verdict of Versailles were abnormal and so the cause of the war. At the same time I indicated in what way a historian's causal judgment could be value-neutral though I added that I did not believe that Taylor's second contrast between normal and abnormal conditions fulfills these requirements.

If Taylor's causal judgments provide good examples of attributive causal inquiries historians undertake (and I think they do), then it would appear that some of these judgments require prior value judgments to be made by the investigator, others do not. In other words, we are not faced with the exclusive option: either value-laden or value-neutral, many examples of each kind can be found in the works of historians.

There now remains but one important aspect of Hart and Honoré's analysis of singular causal judgments that has yet to be discussed. In Chapter Three I presented their views on how the abnormality criterion functions in explanatory inquiries; in the present chapter I considered its role in attributive inquiries. Although I have been critical of some of the claims made by Hart and Honoré I have tried

to build constructively upon the genuine insights they offer us. One of these insights that has yet to be presented is their view that voluntary actions are on par with abnormal conditions insofar as they too function as conditions which negative causal connections between certain consequences and one or more antecedent circumstances. Accordingly, Hart and Honoré's analysis of this type of causal connection and its application to historical accounts will be the subject of the next chapter.

CHAPTER FIVE - HART AND HONORÉ: VOLUNTARY ACTIONS

For a variety of reasons the distinction between fully voluntary and less than fully voluntary actions plays a predominant role in legal inquiries. The need to adjust penalties to specific categories of crime and to define criminal activity in terms of human knowledge, capacities, and intentionality are but a couple of these. The law, however, is not the source of the many fine distinctions by which we differentiate various degrees to which actions can be voluntary. According to Hart and Honoré the idea that voluntary actions are causes has its roots in certain common-sense notions. Just as they argued that the abnormality criterion is based on the common-sense view that causes are conditions that "make a difference," they argue in a similar way that something that "makes" or forces another is at the root of the notion that voluntary actions are causes. And like the idea of difference again, the notions of making and forcing undergo subtle transformations when they are utilized by lawyers and historians. In this chapter I will examine some of these transformations and the affect they have upon the way the cause-conditions distinction is drawn in legal and historical inquiries.

The purpose of evaluating Hart and Honoré's analysis of the voluntariness criterion is, once again, to see whether historians employ this criterion in the same way or to the same extent as lawyers and judges do. A good way to begin to answer these questions is to look at the way Hart and Honoré formulate the voluntary intervention principle, a principle they give wide scope to in the determination of legal causes.

(A) The Voluntary Intervention Principle

Under the heading 'Voluntary Human Conduct' Hart and Honoré formally state the voluntary intervention principle, viz., "the free, deliberate and informed act or omission of a human being, intended to produce the consequence which is in fact produced, negatives causal connection." (129) Some examples they use to illustrate the application of this principle are as follows: If A, wishing to kill B, pushes him off a very tall building, but C, also wishing to kill B, shoots him between the eyes as B was falling to his death, the law would regard C's action as the cause of B's death. Here, to use a phrase adopted by Joel Feinberg, we might want to say that C's action got B "off the causal hook." Again, suppose that A wishes to kill B and decides that the best way to go about doing this is to place a lethal dose of poison in B's drink. If things go as A planned and B imbibes his drink and dies shortly thereafter, A has caused B's death. However, if B, knowing what A was up to when he spotted him pouring some powder into his drink, decides to consume the beverage anyway, A can no longer be considered the cause of B's death, for B's fully voluntary action has negatived this causal connection.

On the other hand antecedent physical conditions of some event are often passed over in the attempt to establish the cause of that event. Thus,

> If unusual quantities of arsenic are found in a dead man's body this is up to a point an explanation of his death and so the cause of it: but we usually press for a further and more satisfactory explanation and may find that someone deliberately put arsenic in the victim's food. This is a fuller explanation in terms of human agency; and of course we speak of the poisoner's action as the cause of his death; though we do not withdraw the title of cause from the presence of

> arsenic in the body--this is now thought of as an ancillary, the 'mere way' in which the poison produced the effect. (39)

What this example purports to show is that once we come upon a free and deliberate action intended to produce the consequence that does in fact occur, we have, "something which has a special finality at the level of common-sense." (39-40) Though we may seek an explanation of the free and deliberate action that is regarded as the cause of the consequence at hand (why did he do it?), we do not regard the agent's motives as the cause of this consequence. Based on these observations Hart and Honoré offer us the following generalization concerning the role of voluntary human actions in common-sense and the law:

> A deliberate human act is therefore most often a barrier and a goal in tracing back causes in such inquiries: it is something through which we do not trace the cause of a later event and something to which we do trace the cause through intervening causes of other kinds. (41)

Three questions that need to be asked about this generalization and their statement of the voluntary intervention principle are (1) How do Hart and Honoré conceive of "voluntary human actions" or "free and deliberate actions"?, (2) Are there any exceptions to the voluntary intervention principle?, and (3) To what extent does their conception of the role played by voluntary action in the law apply to the attempts of historians to discover causes?

(B) Free Action and the Threshold Phenomenon

Hart and Honoré's view of "voluntary action" is said to depend upon a conception of a human agent "as being most free when he is placed in circumstances which give him a fair opportunity to exercise normal mental and physical powers and he does exercise them without pressure from others." (131) On this conception the degree of freedom

possessed by a human agent will depend upon the "circumstances" in which he is placed. In identifying this conception of freedom with the voluntary character of human action, Hart and Honoré offer an analysis of those circumstances, the presence of which on a given occasion serves to reduce the voluntariness of an action. "In common speech, and in much legal usage," they maintain,

> a human action is said not to be voluntary or not fully voluntary if some one or more of a quite varied range of circumstances are present: if it is done 'unintentionally' (i.e. by mistake or accident); or 'involuntarily' (i.e. where normal muscular control is absent); 'unconsciously', or under various types of pressure exerted by other human beings (coercion or duress); or even under the pressure of legal or moral obligation, or as a choice of the lesser of two evils....(38)

Evidently, both freedom and voluntariness are scaler, or extensive properties. An agent can be more or less free, his actions can be more or less voluntary. Accordingly, there must be a whole spectrum of human behavior ranging from fully voluntary actions to sheer physical movement devoid of any conscious purpose. One would think, then, that this spectrum could be divided up into discrete units and these units assigned some arithmetic value, thereby providing a standard for measuring the voluntariness of a given instance of human behavior. In fact, all legal systems, in adjudicating claims for and against plaintiffs and defendants do seem to presuppose the existence of some such standard of measurement. Although none has at its disposal an exact standard of measurement, all seem to acknowledge some "threshold" beyond which human behavior loses "enough" of its voluntary character to to either diminish or totally eradicate an individual's legal responsibility. This threshold phenomenon accounts for the special way in which causal language is applied to human action in the law, as well as

to certain common-sense views concerning the relation between causing and human agency. When the circumstances in which a person finds himself push him beyond that threshold, his behavior is caused, when circumstances do not deprive him of a fair opportunity to exercise his normal mental and physical powers we cannot speak of his behavior as caused. Thus, Hart and Honoré argue that "whatsoever the metaphysics of the matter may be, a (free and deliberate) human action is never regarded as itself caused."[1]

The view espoused here, we may note, is in direct conflict with Collingwood's understanding of the relationship between action and causation. We will recall that Collingwood's definition of sense I of cause stated that "that which is 'caused' is the free and deliberate act of a conscious and responsible agent, and causing him to do it means affording him a motive for doing it." In addition, for 'causing', we may substitute 'making', 'inducing', 'persuading', 'urging', 'forcing', 'compelling'....Hart and Honoré affirm that we can substitute making, forcing, or compelling for 'causing', but deny that we can do the same for urging, persuading, or advising.[2]

The concept of "cause" that Hart and Honoré are appealing to here is one which they attribute to common-sense. This concept is bound up with the idea that causes "make" things happen or "force" them to occur. The application of this notion in the sphere of interpersonal transactions is said to be an extension of our rudimentary techniques for making something happen, e.g. by pushing, pulling, pounding, and so on. In all of these instances the maker must exert "force" if he is to succeed in producing the effect he desires, e.g. by rubbing sticks together to make a fire. The transference of the notion of "force" to circumstances in which one person makes or compels another

to do something provide paradigm cases of "forcing causes" in the sphere of interpersonal transactions, for when one person exerts a certain kind and degree of "pressure" on another it is thought that he thereby removes or severely impairs the capacity of the second agent to act otherwise, i.e. freely. When this is not the case, when no external pressure is exerted on a person's capacity to choose (e.g. when one person urges or advises another on a certain course of action), we do not say that the first person caused the second person's action.

The most poignant cases of human behavior that are "not voluntary" are those where one person or thing imparts movement to another's body against his will. In these cases the second agent moves, but he does not act. To cite just one example of this sort that Hart and Honoré relate,

> when plaster from defendant's ceiling fell on plaintiff's head and she then ran forward and fell down a flight of stairs, sustaining further injuries, it was held that she could recover for all her injuries since her 'dazed mental condition was the direct result of the blow on her head. The fall down the stairs was the direct result of her dazed mental condition'. (135)

In this case no motive was offered to the agent to consider and act upon, she did not <u>choose</u> to stumble down a flight of stairs. A similar type of case where the consequences of an individual's behavior will be traced <u>through</u> that behavior to some prior voluntary action can be seen in contexts where one person administers hypnosis or electric shock to another person. While the consequences of the latter individual's behavior, e.g. if that person should murder someone while in a hypnotic state, is causally connected with his mental condition, we do not cite that mental condition as the cause of the murder, though we do cite the earlier free and deliberate action as the cause, for once again the individual who wielded the murder weapon 'had no choice'.

In many other contexts where an agent can be said to have exercised some degree of choice his action may still be deemed less than "fully" voluntary, such that, as Hart and Honoré concede, "his act is in one sense free," but

> if the choice is made under pressure from the prior wrongful act, or is not a fair choice because the alternative is serious harm, or may be said not to be a 'real' choice because the alternative is neglecting a duty, the decision to avoid the pressure, harm or breach of duty is not treated as free either in ordinary life or in the law. (134)

Quite clearly, Hart and Honoré have drawn a distinction between two senses of "free." These correspond to the two senses of "free" that I spoke of at the close of Chapter I. So long as a person has some degree of choice over how his body is to move or not move he is "free" in one sense. But in another sense, which is that sense of the term endorsed by common-sense and the law, more than a choice is required if some item of behavior is to be called "free," it must be a "fair choice." Under the headings of 'Self-Preservation', 'Preservation of Property', 'Safeguarding Others' Rights and Privileges', 'Safeguarding Interests', 'Legal Obligations', 'Moral Obligations', and 'Unreflective Acts', Hart and Honoré consider a variety of legal cases in which an agent's conduct is considered unfree because, though he had a choice, circumstances present at the time severely narrowed the range of alternative courses of action open to him, such that the adoption of any of these alternative modes of action would, in all likelihood, result in some degree of harm befalling that agent.

Typical of cases in which the action of one person severely narrows the area of choice of another person is where "A sets a house on fire (intentionally or unintentionally); B to save himself has to jump from a height involving certain injury, or to save a child rushes in and is

seriously burned." In this case, continue the authors,

> B's movements are not involuntary; the 'necessity' of his actions are of a different order. His action is the outcome of a choice between two evils forced on him by A's action. In such cases, when B's injuries are thought of as the consequence of the fire, the implicit judgment is made that his action was the lesser of the two evils and in this sense a 'reasonable' one which he was obliged to make to avoid a greater evil. This is often paradoxically, though understandably, described by saying that here the agent 'had no choice' but to do what he did. Such judgments involve a comparison of the importance of the respective interests sacrificed and preserved, and the final assertion that A's action was the cause of the injuries rests on evaluations about which men may differ. (72)

For present purposes we can set aside considerations of the comparative nature of the significance attributed to the sacrifice, preservation, and enhancement of interests. Once we have examined the influence side of the "domination-influence" spectrum and come to respond to the third question posed at the outset of this section, we shall return to these considerations.

In contrast to the sort of cases we have been dealing with thus far, when one person only urges, persuades, points out certain facts or offers advice to another person, the second person's action, should he act upon the words spoken by the first agent, is held to be fully voluntary, and so does negative causal connection between some harmful consequence of his action and the other person's earlier act or omission. Perhaps the most bizarre case cited by Hart and Honoré to illustrate their thesis was heard by a Rhodesian court. In this case the defendant

> had lost his daughter and accused his mother of killing her. His mother did not deny the charge but promised to commit suicide. Eight days later, his mother still not having committed suicide, he

> fetched a rope, tied a noose to the end and said to her: 'I have already fixed the rope. Get up and hang yourself', which she did. The Southern Rhodesian court held that accused had not committed murder or attempted murder, since although he persuaded his mother to kill herself, he had used neither physical compulsion nor threats. (295)

The line dividing "dominance" from "influence" (which together comprise our voluntary-not voluntary spectrum) is somewhat blurred by the fact that, though the two are conceptually distinct, the sort of results one can expect to receive if, for instance, one acts in accordance with the intentions of a person who offers a bribe in contrast with acting in accordance with the intention of one who makes a threat, can be described in identical terms. Thus, if I want a certain person banned from a social club of which I am a member and, for the first time, offer the head of the membership committee $500 on the condition that he see to it that that person is refused entry, my offer is a bribe. On the other hand, if the head of the membership committee had expected to receive $500 from me because I had already promised it to him or had been giving him that sum at regular intervals in the past, then the payment of $500, which is now made conditional upon his making sure that the person I had designated be excluded from the club, would be a threat.

Here, if we are to follow the lead of Hart and Honoré, should my contact accept my offer of $500 when he had no previous expectation of receiving that sum from me, his action would be described as fully voluntary and I could not be said to have caused it. On the other hand, had he been in the habit of receiving such a sum from me in the past my present offer could be said to have caused him to act in such a way as to deprive the candidate of admission into the club, for now his act would be said to have been less than fully voluntary.

It seems to me that there are many problems that must be confronted by any theory that purports to distinguish causes from conditions in terms of varying degrees of voluntariness. With respect to Hart and Honoré's theory, they tend to overlook the fact that what is considered by one person to be an inducement to act in a certain way may fail to be so for another person. What is even more important is the fact that contrary to what they claim we do sometimes regard one voluntary action as the cause of another. D. L. Lloyd Thomas presents one example of this kind, viz.,

> Suppose I am a rude host who wants one of my guests to go home. I know that the guest, a woman, has divorced her husband. I say, while she is listening, "Any woman who would divorce her husband is a bitch'. The woman takes offense as planned, and leaves in a huff.[3]

Here the rude host did not demand that his guest leave nor even suggest that she do so. In order to achieve his end the host employed a strategy of indirection. Nonetheless, I cannot see that there is any conceptual impropriety in saying that the host 'caused' his guest to leave or that her leaving was an intended consequence of his remarks, in spite of the fact that her action was a highly voluntary one.

In the different realms of social life an individual often has a number of ways to cause other people to do what he wants. Outside of institutional contexts where there exist established orders of authority relations (e.g. superior-subordinate), the best way to 'manipulate' people according to one's designs is seldom to issue demands and utter commands. Even where there does exist a relationship of authority, say parent-child, it is oftentimes more efficacious for the authority figure to refrain from using that authority and instead, to adopt more congenial though indirect methods for attaining his ends. Thus, a mother

who wishes her child to come into the house after the child has been playing outside for several hours may achieve her objective by raising her voice and demanding that the child come inside or she may choose to just pick the child up and carry him into the house. Should she adopt the first strategy, and should the child acquiesce to her emphatically stated wish, we might say that the child's action was less than fully voluntary and that the mother caused him to come inside. Had she opted for the second strategy his movement from outside to inside the house would be completely nonvoluntary because involuntary. Here too the mother caused him to come into the house. But suppose there is a third option. The mother knows that her child has a strong liking for fruit juices and decides to use his proness to respond to such a stimulus to achieve her end. Now she may just say to him, 'there's a glass of apple juice on the kitchen table for you', whereupon the child comes running into the house. Here the child's action is fully voluntary (no threats, demands, attempts to persuade, etc.) yet there seems to be nothing wrong with saying that the mother caused the child to come into the house or that the child's coming into the house was an intended consequence of her action.

(C) Triggering Causes and the Expectability Principle

Up to this point I have been trying to show that common-sense does endorse the view that some voluntary actions are caused. In disputing Hart and Honoré's claim to the contrary I have not meant to deny that the legal conception of voluntary actions rests on common-sense assumptions nor that the voluntary intervention principle is somehow out of place in the law. But I do think that they have evaded two important considerations. First, although common-sense may endorse the voluntary

intervention principle and presuppose the domination-influence spectrum of human behavior, it also endorses another model of causal thinking, namely one which takes triggering or precipitating events as causes.[4] In this respect recall our earlier example about an earthquake causing a people to migrate. Here we have a physical event causing human action. A second point that has escaped Hart and Honoré's attention is this: although both the law and common-sense accept the voluntary intervention principle as a means for adjudicating causal disputes, this principle must be seen as subordinate to another principle, this being "the more expectable human behavior is, whether voluntary or not, the less likely it is to 'negative causal connection'."[5]

The expectability principle, as we may refer to it, explains the willingness of common-sense to cite the remarks of the rude host and of the anxious mother as the cause of the other person's voluntary actions. In our first example the rude host was confident that his guest would be insulted by his remark and that this would prompt her to leave. Because the expectation that she would choose to depart the party was reasonably high, her free and deliberate act does not negative the causal connection between the host's remark and her departure. Likewise, because the mother could predict with a good deal of assurance that her son would respond to the prospects of drinking a glass of apple juice, his action of running into the house can be said to have been caused by her remark.

The importance of the expectability principle can be readily seen if we respond to the second question posed at the outset; are there any exceptions to the voluntary intervention principle? There is, indeed, one important class of exceptions that Hart and Honoré acknowledge, cases where one person provides an opportunity for another to do harm.

In these cases, as we have already seen, the voluntary action of, e.g. a thief, will be passed over in the search for the cause of some harm because it could reasonably be expected, whereas the omission on the part of a friend to lock the doors of the house will be cited as the cause of the subsequent harm. Viewed in this way, cases of this kind do not constitute exceptions to the voluntary intervention principle but rather bringing that principle under the expectability one as a special case. With this I think enough has now been said to indicate just how voluntary human actions can be spoken of as being caused.

(D) Limits on the Use of the Voluntariness Criterion in History

Having responded to the first two questions posed earlier we may now take up the third one, viz., to what extent do free and deliberate human acts constitute a barrier and a goal in tracing back causes in historical inquiries?

Insofar as any response to this question requires one to generalize about the role played by voluntary actions in historical works, regardless of time and place where these were written, such a response must be made with caution. Naturally one wants to know what types of historical accounts there are, whether one or more of these types accords a special place to voluntary actions, and what percentage of those who have written historical works have chosen to write a type of account wherein voluntary actions function more or less in the way they do in legal inquiries. Without pretending to give a precise answer to any of these empirical questions it appears to me that the answer is: to a much lesser extent than such acts serve in that capacity in the law.

Sometimes historians do have as the goal of their causal explanations the discovery of one or more voluntary actions to which certain consequences can be traced. To this extent their causal thinking will sometimes resemble what goes on in a courtroom when causal disputes must be resolved. But more often than not historians are not concerned with voluntary human actions in the same way as judges and lawyers are. There are several reasons that explain the divergence between the focus of the historian's and the lawyer's interests. Here I will mention only two of them.

In the first place, while the concept of causal connection most prominent in the legal cases discussed by Hart and Honoré is that of a human action causing certain consequences, Collingwood was surely correct in pointing out that historians are very often interested in determining the causes of voluntary human actions, i.e. determining the relevant causa quod and causa ut. As such, voluntary actions constitute a goal and barrier less often in historical accounts than in legal inquiries simply because in the former but not in the latter these are taken as the starting point (what requires explanation) rather than the terminus (what does the explaining).

Secondly, the fundamental notion in the legal cases analyzed by Hart and Honoré is that of an act being the cause of harm. But as Morton White has correctly suggested, "the historian is not as preoccupied as the lawyer is with finding the cause of harm nor with attributing that harm to deliberate acts." (146) A casual acquaintance with the diverse types of historical accounts is enough to reveal that even when voluntary actions are cited as the causes of certain consequences, these consequences need not be viewed as harmful nor must their causes be regarded as wrongful. The notion of harm caused by

voluntary human actions is seldom encountered in histories of art, technology, science, philosophy, etc. A historian of technology, for example, may trace certain changes in economic structure, working conditions, public health, demographic patterns, and political organization in 19th century Europe to the invention of the standardized screw by Henry Maudslay, for this made possible the assembly line. Similarly, an author writing a history of disease in human populations may cite the emergence of markets in 13th century Europe as the cause of the epidemics that were to devastate large numbers of people in that part of the world, for these attracted large numbers of rodents and enabled them to proliferate, these being hosts to the parasites which carried the infectious agents.

In such histories what appears to be a minor technological improvement or an innocuous modification in trading practices may provide the "spark" that sets off a dramatic explosion.[6] The task of the historian, then, is to trace the plethora of consequences attributable to that spark. This often requires the historian to reconstruct causal chains or, to use a more felicitous metaphor, causal webs. For example, Reay Tannahil, author of Food in History, writes that

> Finer-quality glass, coupled with advances in the science of optics, made it possible to manufacture more accurate microscopes, which in turn, stimulated research in biology and chemistry and helped found the science of bacteriology. The repercussions, not only in medicine and public health, but in food hygiene and preservation, were to be almost incalculable.[7]

Historical explanations such as this make it clear that, as much as anything else, it is the nature of the historian's central subject that determines the scope of his interest, and that this is what circumscribes the role played by voluntary human actions in such contexts

of explanation. Whereas voluntary actions constitute a goal in causal inquiries in the law because they are generally regarded as proximate causes of a specific harm for which restitution or compensation is sought by the victim or his benefactors (hence the legal maxim, "intended consequences cannot be too remote"), in many historical contexts, though the voluntary actions of certain individuals may have provided the spark that set off an explosion, e.g. those of inventors or merchants, these are not treated as the cause of harm, nor are they accorded the attention attracted by wrongful actions in the law.

Nowhere is the explanatory role of the actions of individual human agents less prominent than in quantitative histories that deal, e.g. with changes in the structure of institutions over some period of time. In one such historical work, The Making of the Modern Family, the author, Edward Shorter, claims to be interested in "the representative experience of the average person." Employing this statistical notion of a person, Shorter attempts to offer his readers some understanding of the representative experience of married and unmarried mothers, urban and rural fathers, 18th and 19th century children, and so on, never once mentioning the names of the individuals who constitute these groups. The anonymity of the individuals in Shorter's work does not deter him from seeking out causal connections, however. But when he presents his readers with a causal explanation of some phenomenon he is not interested in depicting it as the harmful consequence of some individual's wrongful action; the sort of causal connection he is interested in discovering concerns the functional relationship between changes in two or more factors over a period of time. For example, Shorter notes that over the period 1850-1940 "the rate of out-of-wedlock conceptions--at least those leading to illegitimate births--

plummeted....This precipitous drop in illegitimate fertility extended to virtually every province of Europe, save Bulgaria." Given this information Shorter asks, "did premarital pregnancies fall off because people had sex before marriage less often in these years or because unmarried couples began to practice contraception?" Noting a corresponding decline in the rate of marital fertility, Shorter concludes that "the simultaneity in the timing of the marital and nonmarital fertility downslides is so close as to suggest that contraception caused the drop in nonmarital conceptions as well." (italics mine)[8]

Causal accounts like Shorter's represent only one type of causal inquiry historians engage in. In contrast with the legal preoccupation with determining responsibility for harm, historians are interested in explaining a wide range of events, processes, and states of affairs. It is this diversity of interest, I believe, that accounts for the limited scope enjoyed by the voluntariness criterion in historical works.

(E) The Application of the Voluntariness Criterion in Historical Works

Now that we have played down the importance of the explanatory role played by voluntary actions in historical accounts we may consider cases in which such actions do serve as a goal and barrier for historians. As in the type of legal cases examined by Hart and Honoré, we would expect that in historical cases of this kind there will be some event or series of events that is regarded as harmful, that this can be attributed to a human agent, and that there is an accused (defendant) and an accuser (plaintiff). Where, then, shall we find historical examples wherein voluntary action is treated in such a way? Clearly, it is in those historical accounts that deal with such events as the

origins of wars and revolutions, for in these death, destruction, and the loss of property are those consequences for which the causes are sought, much as they are in the law. Let us then turn back to the conflict between Taylor and his critics over the origins of the Second World War and see if we can find a mode of argument wherein the attempt to determine the degree of voluntariness of the actions of the prominent figures has become the goal of the historian's causal explanation.

Once again we will find it profitable to consult Dray's paper on the dispute between Taylor and his critics. Dray draws attention to the fact that on a number of occasions Taylor represents Hitler as being placed in situations where he "had" to act. Much like the man in Hart and Honoré's example who could jump out of the burning house and sustain one kind of injury or try to rescue a child in another room and sustain another kind of injury, Hitler, according to Taylor, was constantly pressured to undertake actions he had neither planned nor wished to undertake. In Dray's words, Hitler is represented "not only as failing to take the initiative himself, but as being forced continually to respond to the initiatives of others. He is maneuvered, incited, and even driven to act by events and situations not of his own making." (164) Neither the occupation of Austria or of Prague, nor the invasion of Poland were actions that Hitler undertook voluntarily, these were things that he was "forced" to do because of the pressure exerted on him by others, e.g. Von Schuschnigg, Chamberlain, and Beck.

The kind of argument Taylor is advancing to get Hitler off the causal hook is one which his critics have also proposed, albeit with the aim of placing him on that same hook. This argument falls under Dray's third paradigm of causal thinking, a paradigm which has "its

origin in the apparently innocuous idea that causes, since they are what make things happen, must be 'forcing' or 'active' conditions." (163) I have already argued that Hart and Honoré overstate the importance of this "apparently innocuous idea." Dray, however, has not done the same. What he had done is to take the solid part of Hart and Honoré's analysis of circumstances that are acknowledged to reduce the voluntariness of actions in the law and apply this analysis to the causal disagreement between Taylor and his critics. Along these lines he shows that the causal judgments made by these historians depend on a prior assessment of the degree of voluntariness of certain actions performed by the various leaders whose countries became involved in the Second World War. In other words, the causal conclusions reached by these historians presuppose that such an assessment has been made. Like Hart and Honoré, Dray maintains that the issue of whether Hitler was "forced" to act has nothing to do with "his psychological powers of resistance, or his degree of bodily control," but rather with

> the sorts of reasons there were for doing what Hitler did--the degree of justification he had for claiming that, in view of the situations confronting him, vital interests were in danger which it would be reasonable and proper to defend. In other words, it is a matter of the moral nature of the choices that were open to him--so that....behind the activity/passivity judgments that lie behind the causal judgments lie value judgments, indeed moral judgments, about human life and action. (166)

The historian who asks whether Hitler was justified in sending German troops into Austria and Poland must be prepared to answer the question: how vital were the interests being threatened? In itself this question should alert us to the fact that there are different kinds of interests, some of which are more dear, hence more important than others. For example, few would deny that the preservation of one's physical existence

is a more important interest than the preservation or enhancement of one's property. Nevertheless, there are circumstances in which people are prepared to sacrifice their lives to preserve or extend their property, still others in which people are ready to sacrifice their livelihood or social standing in order to preserve the rights of others which they perceive as being threatened. If altruistic acts as well as purely self-interested ones can both be regarded as "forced," how, should there arise a conflict between such actions, can we adjudicate a dispute over which action was the cause of some harm that occurred?

It is with reference to the question just posed that we must view the causal disagreement between Taylor and his critics. For in a certain sense the causal issue does not turn on the question of whether Hitler alone was "forced" to act as he did, simply because all parties concerned were forced to act by the demands and diplomatic maneuvers of the others. The causal issue can be decided only when the question which leaders were least forced to act has been answered. Critics of Taylor may allow that Hitler was to some degree "forced" to take certain measures and that, to this extent, his actions were less than fully voluntary. Yet they may point out that the Polish minister Beck was even more constrained to act in the manner in which he did than was Hitler. After all, it will be argued, the interest Hitler perceived as threatened was the extension of German territory whereas the interest Beck perceived as threatened, and which he was trying to preserve, was the very existence of Poland as a sovereign state. Threatened by Germany to the west and Russia to the east, Beck could argue that he "had no choice" but to refuse to negotiate with Hitler over Danzig and the Corridor or to permit Russian troops onto Polish soil in the event of German aggression.

If, in the end, the cause of the war is to be sought in the actions of those leaders who provided the active element, or in the actions of that leader whose actions provided the most active element, then we can well understand why historians will disagree about who caused the war, for, as Hart and Honoré have claimed about such conflicts in the law, causal decisions of this sort in history also presuppose "a comparison of the importance of the respective interests sacrificed and preserved," and as in the law again, the outcome of such a comparison is something about which historians may and will differ.

(F) Summary and Conclusion

In this chapter I have examined a number of issues arising from Hart and Honoré's analysis of the voluntariness criterion for distinguishing causes from conditions. I began by looking at their presentation of the voluntary intervention principle and its role in legal inquiries. Next I considered the way in which the "threshold phenomenon" accounts for the way causal language gets used in everyday affairs, the law, and in historical accounts of the past. At the same time I pointed out that, contrary to what Hart and Honoré claim, common-sense does sometimes view voluntary actions as being caused. After considering several examples that illustrate this point I concluded that the voluntary intervention principle should be seen as a special case of the expectability one.

Next I addressed the question: To what extent do historians treat voluntary actions in the same way as lawyers and judges do, i.e. as constituting a barrier and goal in tracing causes backwards in time? My response to this question was: to a much lesser extent than Hart and Honoré appear to have thought. The basis of this conclusion is

simply that historians are not as preoccupied with discovering the cause of harm as jurists are. Notwithstanding the limited scope of the voluntariness criterion, I then went on to show that when the focal point for drawing a distinction between cause and conditions is the degree of voluntariness displayed by certain actions, historians will adduce evidence and formulate arguments in much the same way as lawyers and judges do. Our re-examination of the Taylor-Trevor-Roper debate from the voluntariness angle aptly illustrated this point.

Finally, I concurred with the view shared by Hart and Honoré and Gray that in the final analysis singular causal judgments based on an assessment of the degree of voluntariness exhibited by certain actions are subjective, that is, which condition is elevated to causal status is determined by the values of the one who renders the causal judgment. In this respect I find that these causal judgments are on par with those attributive causal judgments based on the normal/abnormal distinction, for in these cases as well it is often the investigator's values that circumscribe the field of causal conditions and ultimately determine the cause.

CHAPTER SIX: MAURICE MANDELBAUM'S ATTACK ON THE CAUSE-CONDITIONS DISTINCTION

In the last chapter a number of important aspects of Hart and Honoré's theory of causation were examined and I proposed that several adjustments needed to be made in order to make it acceptable. But despite my criticism of the way they draw the explanatory-attributive distinction and the refinements I made to their conception of the abnormality criterion I would have to say that, on balance, theirs is a powerful theory of causation. Not only does it adequately account for causal judgments made in courts of law and in everyday situations, it also explains many kinds of singular causal judgments offered by historians. In this respect I think that Morton White and William Dray were on the right track when they sought to analyze the causal judgments of Henri Pirenne and A. J. P. Taylor in terms of their theory. However, Hart and Honoré's theory has not been without its critics. Most prominent among them has been Maurice Mandelbaum.

In his book The Anatomy of Historical Knowledge[1], Mandelbaum develops a theory of causation which, he claims, is both incompatible with yet superior to the one offered by Hart and Honoré. He argues that a number of general features of their theory are unacceptable and that it cannot adequately explain the bulk of causal judgments made by historians. For example, he claims they are wrong to always identify the cause of an event with a temporally antecedent occurrence; moreover, he feels they are mistaken when they claim that causal explanations are context-dependent. For my own part I think that many of Mandelbaum's claims are misdirected while others that he makes can be handled as long as the refinements I have made to Hart and Honoré's theory are

accepted. In this chapter, then, I will present Mandelbaum's own theory of causation and a number of his arguments that are directed against Hart and Honoré's views. In doing so I hope to defend those aspects of Hart and Honoré's theory that have been undeservedly criticized and, at the same time, show how the broader conception of causal explanation that I have offered serves to undercut several principal aspects of Mandelbaum's own conception of causal explanation.

(A) Mandelbaum's Theory of Causation

Each of the three theories of causation I am dealing with in this thesis (that of Collingwood, Hart and Honoré, and Mandelbaum) are founded on the idea that there are certain primitive, common-sense conceptions of the cause-effect relationship which, if properly understood, can provide a framework for analyzing much more sophisticated patterns of causal explanation in the law, science, and history.

Collingwood held that our use of cause-effect terminology "conveys an idea not only of one thing's leading to another but of one thing's forcing another to happen or exist; an idea of power or compulsion or constraint" (EM 309). When he then goes on to ask, à la Hume: From what impression is this idea derived?, he responds,

> from impressions received in our social life, in the practical relations of man to man; specifically, from the impression of causing some other man to do something when, by argument or command or threat or the like, we place him in a situation in which he can only carry out his intentions by doing that thing; and conversely, from the impression of being caused to do something (EM 309).

In a section of their book entitled "Cause and Effect: The Central Notion," Hart and Honoré adopt a similar strategy to explain our use of causal concepts. However, they seek to trace our use of these concepts to experiences where, by the use of our bodies, we bring about

intended changes in animate and inanimate objects and their environment. Such experiences are expressed by simple transitive verbs such as push, pull, bend, twist, etc. They point out that the activities expressed by these verbs "consist of an initial immediate bodily manipulation of the thing affected and often takes little time" (CL 26). From these quite basic experiences, however, people have learned that they can sometimes produce secondary changes, not only in the object actually manipulated, but in other objects as well. It is in such instances, claim Hart and Honoré, that we use the correlative terms cause and effect rather than simple transitive verbs. Thus, we cause a mountain to crumble by igniting dynamite placed at strategic points beneath its surface, a river to stop flowing by erecting a dam, things to get hot by placing them on fires. The root notion in all of these cases is that of a human action which "is an interference in the natural course of events which makes a difference in the way these develop" (CL 27). To this Hart and Honoré add:

> Analogies with the interference by human beings with the natural course of events in part control, even in cases where there is literally no human intervention, what is to be identified as the cause of some occurrence; the cause, though not a literal intervention, is a difference to the normal course which accounts for the difference in the outcome (CL 27).

When we turn to Mandelbaum's theory of causation we find much the same strategy at work. Mandelbaum fastens onto what he feels is a somewhat neglected aspect of common-sense conceptions of the cause-effect relationship and makes this a paradigm for analyzing the nature of causal explanations in science and in history. This aspect refers to an experience that Hume maintained was impossible, i.e. the perception of causation. When we can be said to perceive causation, suggests Mandelbaum,

"the connection between cause and effect lies in the fact that both are seen as aspects of a single ongoing process; of which the effect is viewed as its end point or result; the cause of this is the process itself" (AHK 57).

Several situations in which an eye-witness could be said to perceive causation are presented by Mandelbaum. In one such situation we see a man pushing an item of furniture in his living room. In another we see a teacher at the blackboard writing a sentence. Mandelbaum points out that in these situations (and countless others like them), the cause is not complete until the effect occurs--the coming-to-rest of the piece of furniture and the completion of the sentence. The causes of these two effects, Mandelbaum insists, cannot be seen as separate prior events, instead they comprise all that is included in the act of pushing or of writing. What is more, when we can be said to perceive causation it is not between two distinct events that we experience causal connection but within different phases of a continuous happening.

The visual perception of spatial and temporal continuity forms the basis of Mandelbaum's theory of causation. What he finds so striking about these experiences is the unity of what he likes to call "single ongoing processes." This is reflected in a contrast he draws between Mill's conception of the causal relation and his own. For Mill, the cause of a given phenomenon consists of "a compound of separate elements that produce a new and distinct occurrence when they come together"; the conception of cause that Mandelbaum advocates "takes these elements as components within a single process, terminating in that particular state of affairs we regard as the effect of the process" (AHK 95).

Interestingly, this contrast runs parallel to a distinction that Mandelbaum draws between historical accounts that have an "explanatory structure" and those that have a "sequential structure." "In an explanatory account of a particular occurrence," he writes, "the events with which the historian deals may be extremely diverse, not belonging together except insofar as each happened to contribute to the particular outcome the historian is investigating" (AHK 27). As such, "an explanatory account of a particular effect draws upon factors each of which has its own separate history: the historian will not be following any one continuous series of events" (AHK 27).

On the other hand historical accounts having a sequential structure will trace "one continuous series of events." In other words, the elements included in these accounts will be "components within a single process" rather than "separate elements." For example, a historian faced with the problem of explaining the decline in the popularity of Louis XIV would attempt to "trace out the elements that entered into the process that led from a point at which the King was highly esteemed to the view held of him at his death" (AHK 111). Similarly, when a historian seeks to explain the rise of the city of London as a financial center he "appeals to changes included within that rise which together account for it" (AHK 111).

Based on this process conception of the causal relationship Mandelbaum draws the following two conclusions: (1) that the cause is not an event which occurs prior to the occurrence of the effect, and (2) that cause and effect cannot be seen as separate events. These two conclusions provide the basis for two of Mandelbaum's more important criticisms of Hart and Honoré's theory of causation. For this reason I will now turn my attention to the way in which he regards the

recognition of these two considerations as sufficient to undermine their analysis of the causal relation.

(B) Temporality and Causal Sequences

One of Mandelbaum's more sustained criticisms of the cause-conditions distinction is that it makes it appear that all causal sequences exhibit a given effect as the result of some temporally antecedent cause. In his first book, The Problem of Historical Knowledge,[2] Mandelbaum contrasts what he calls the "popular view" of causation (which employs the cause-conditions distinction) with that of the "scientific" conception of causation. There he maintained that, unlike the popular view of the causal relationship, the scientific one "does not make the causal relationship one of temporal sequence." (227) His own view is that there cannot be any temporal hiatus between cause and effect, as the popular notion of causation would have us believe. "In order to be a determining condition of any other event," he writes, "a given event must at least be partially contemporaneous with it, that is, their durations must overlap." To this he adds: "To give up this view would be to return to the popular view of causation, where the cause produces the effect at a definite point in time, and we would thus fall prey to the Humean analysis which derives much of its strength from the fact that the cause had been considered as separate in time from the effect." (228-229)

In The Anatomy of Historical Knowledge Mandelbaum reiterates his earlier position, supplementing it with a host of examples intended to support his view and undermine the analysis of causation offered in Causation in the Law. Although Mandelbaum is prepared to acknowledge that "there are many cases in which what is taken to be the cause of an

event is some specific event that was precedent to its effect" (AHK 52), he still insists that

> the primary source of many oversimplifications and distortions has been the conventional that when we speak of the cause-effect relationship we always have in mind a temporal sequential relationship in which some specific prior event is the cause of a subsequent event. The pervasiveness of this view is probably due to the influence of Hume's analysis....(52)

Obviously Mandelbaum's target is the same in both of his books. But in his current work he identifies this target with Hart and Honoré's analysis of the causal relationship, claiming that "even Hart and Honoré take this conventional view for granted and treat it as if it were clearly adequate in all situations, whether in science or in everyday life." (52) The question I wish to raise here is whether or not Mandelbaum's charge is justified; are Hart and Honoré committed to the view that causal sequences always manifest a prior event as the cause of a subsequent one?

Unfortunately, Hart and Honoré are themselves silent on this issue, though what they do have to say about the temporal aspects of causal inquiries may incline us to accept Mandelbaum's characterization of their position. Insofar as their main concern was to analyze the use of causal concepts in the law, the occasions on which they address themselves to the temporal dimension of the causal relationship are restricted to a discussion of a problem that pervades legal inquiries, this being: How far back shall we trace a cause through a number of other conditions, equally necessary for the given occurrence? The very way this question gets posed makes it clear that a distinction between the cause and other necessary conditions is accepted. Hart and Honoré's

solution to the problem this question raises consists of two criteria they find operative when, in everyday life, history, and the law we seek to limit the backward movement of the inquiry: abnormality and voluntariness.

Preoccupation with the above-mentioned question certainly does bring the temporal nature of causal inquiries into the forefront. But does this justify Mandelbaum's claim that, in Hart and Honoré's view, the condition denominated the cause will always be an event that is prior to the occurrence of the effect? I suggest that it does not.

One piece of circumstantial evidence I shall introduce to support my claim concerns Mandelbaum's identification of Hart and Honoré's analysis of causation with important aspects of Hume's. It is clear from the passages previously cited that Mandelbaum believes that Hume's account of the causal relationship suffered serious defects, the most notable being that he treated cause and effect as discrete events separated in time. As we have also seen, Mandelbaum believes that the pernicious influence of Hume's account has left its mark on the work of Hart and Honoré. If this is so, if Hart and Honore really do take Hume's account for granted, how do we explain the fact that they are critical of Hume's event-ontology, saying that, in analyzing the causal relationship Hume "was mislead by his terminology of events"? (15) This hardly suggests that Hart and Honoré uncritically accepted Hume's analysis of the causal relationship. Still, this does not establish that they did not accept the view that cause and effect are always events separated in time.

In order to see that they do indeed reject the view Mandelbaum attributes to them we need only consider an example Mandelbaum uses

to show that many of the ordinary, everyday causal explanations we offer do not exhibit the cause as some event antecedent to the effect, and then compare this with one of Hart and Honoré's own examples. Thus, at one point in his argument Mandelbaum asks us to consider a situation where we seek to explain to a child why his toy car sinks when placed in a swimming pool whereas his sister's rubber ball floats. Here, it is suggested, our explanation would consist of an appeal to the fact that "the car is heavier." What this example shows is that "the cause is not some antecedent event distinct from the effect: It is in terms of the properties of the object itself that we seek to explain how it behaves." (73)

Undoubtedly Mandelbaum's analysis of this simple case is adequate and the point he wishes to make is well taken. The question remains, though, does Hart and Honoré's account of the causal relationship preclude cases of this sort, where clearly the cause is not a distinct antecedent event? In fact, it does not. One need only consider their treatment of the example in which a man suffers an attack of indigestion to see that this is the case. In that example the wife of the man cites his eating parsnips as the cause of his attack. Here it is a distinct antecedent event that is cited as the cause. On the other hand, the doctor who was called in to examine this man declared that his ulcers were the cause of his attack of indigestion. Here it is a property or standing condition of the object (man) that is regarded as the cause, just as it was a property of the child's toy car that explained its sinking. Clearly, then, Mandelbaum's charge cannot be sustained, for the account of the causal relationship Hart and Honoré provide us with can accommodate cases in which the cause is not

a distinct event temporally preceding the effect.

(C) The Perception of Causation and Causal Explanations

Another of Mandelbaum's more important criticisms of Hart and Honoré's analysis is that it fails in the very area in which it claimed success. That is, Mandelbaum feels that their analysis is not in accord with many of our ordinary, common-sense causal explanations. And if their analysis fails here, it is suggested, it must certainly fail when it is extended to account for the operation of causal concepts in history.

According to Mandelbaum the view of the causal relationship endorsed by Hart and Honoré does not conform to many of our ordinary causal explanations for

> we do not always regard the cause and its effect as two distinct and discontinuous events. In such cases a cause and its effect appear as related aspects of a single event whose continuity we directly perceive, rather than as different events. (AHK 52-53)

Earlier we outlined several situations in which an observer could be said to perceive causation. Whether or not we agree with Mandelbaum that an eye-witness can perceive causation in these or any other situations (and there are some contemporary philosophers who would argue that, strictly speaking, this cannot be done, e.g. J. L. Mackie and G. von Wright), we should not fail to notice that in addition to affirming the possibility of perceiving causation Mandelbaum is also advancing a particular thesis about causal explanations. Briefly, he appears to be suggesting that many of our ordinary causal explanations are nothing more than descriptions which mirror the way we see an event unfold. I shall return to this suggestion in a moment, but first I would like to point out that even if Mandelbaum is right to

insist that when we perceive causation the cause is not a distinct prior event, this does not mean that it does not make sense to separate "the cause" from "the conditions" when we seek to explain that event, ex post facto. Mandelbaum anticipates this objection. He agrees that we can segment the successive stages of a process when we seek to explain it, but as the following passage shows, he does not think much of this practice:

> this form of cinematographical analysis, in which continuous processes are treated as if they were composed of a series of discontinuous independent happenings, does not reproduce what we perceive when we perceive causal connections (AHK 57).

One insuperable difficulty this view presents is that even if we allow that our descriptions sometimes reproduce what we see when we are eye-witnesses to certain events, and thus could be construed as causal explanations of these, how can we make this a requirement for our explanations of events we have not witnessed, as Mandelbaum appears to suggest we must? The only plausible way to argue this point would be to formulate the problem counterfactually; one would have to argue that causal explanations correspond to what an observer would see if he were appropriately located. However plausible this argument might be, it is not one that Mandelbaum can adopt for, as we will see very soon, he says quite explicitly that counterfactuals have no place in causal explanations of any kind.

One thing that needs to be said is that even if we recognize the possibility of perceiving causation in ordinary contexts and allow that Mandelbaum has pointed to a deficiency in Hart and Honoré's analysis of common-sense views of causation, it remains an open question whether his theory of causal explanation in history fares any better than theirs.

To even raise this question is to suppose that the two accounts are in conflict, when in fact this may not be the case. It is one thing to maintain that the requirement of being in a position to directly perceive causation is not normally fulfilled by historians, quite another to suggest that regarding an event as the end point of an ongoing process is incompatible with separating the cause from the other conditions necessary for its occurrence when we seek to explain it. Though we affirm the former, we may reject the latter suggestion and try to show that the two activities are compatible.

Consider, for example, the following singular causal explanation. Some historians would say that the cause of the outbreak of the First World War was the assassination of the Austrian Archduke at Sarajevo in July 1914. This statement of the cause does not include the other "conditions" necessary for that occurrence, nor does it indicate the process leading from the assassination to the Austrian declaration of war. However, it would be highly unlikely that a historian who made such a statement at the beginning of his work would not intend to trace the intermediary stages connecting the assassination with the outbreak of hostilities; or, if the statement appears at the end of his account we would expect that such a process would have been reconstructed in the earlier portions of that account. Thus, a historian might suggest that the assassination caused the Austrian ultimatum to Serbia, and that the ultimatum prompted Russia to mobilize its troops. This in turn fortified Serbian resistance to the Austrian threat. And when the Serbian government refused to comply with all the conditions of the ultimatum, the Austrian declaration of war of the Serbs followed.

Here the explanation of the outbreak of the First World War reconstructs the pivotal stages in a decision-making process initiated by the assassination and terminating in the outbreak of hostilities. Clearly, providing an account of the process which connects the initial and terminal points of a single happening is one way to establish a singular causal explanation. However, it is not the only way, as Mandelbaum seems to think. After considering those cases which appear to be most favorable to the analysis of Hart and Honore, he writes:

> to explain the difference between cases in which the effect follows and those in which it does not, we must trace the processes intervening between the event that is called the cause and that which we regard as its effect. In tracing these processes we quickly discover that what appeared as two distinct events from our ordinary common-sense point of view were in fact simply parts of one continuous process. (AHK 69)

In my opinion it is a serious error on Mandelbaum's part to insist that the only way to establish authoritative causal conclusions is to trace single continuous processes. If his claim were true we would have no justification for our causal beliefs arrived at on the basis of inductive reasoning; our confidence in causal conclusions based on simple enumeration as well as those achieved by the application of more rigorous experimental techniques would be misplaced. Not only would this undermine much scientific inquiry, it would also mean that many historical explanations would have to be discarded. In this regard let us briefly reconsider Pirenne's singular causal explanation of the collapse of the Mediterranean Commonwealth in the last quarter of the seventh century and the first quarter of the eighth century.

Pirenne's explanation is one in which apparently similar causes produce divergent effects. This is made clear from what Pirenne says in the synoptic account he offers at the end of the first chapter of Medieval Cities. The following passage summarizes what he believes himself to have established earlier in that chapter:

> On all its shores (the Mediterranean), for centuries, social life, in its fundamental characteristics, had been the same; religion, the same; customs and ideas, the same or very nearly so. The invasion of the barbarians from the North had modified nothing essential in that situation. But now, all of a sudden, the very lands where civilization had been born were torn away; the Cult of the Prophet was substituted for the Christian Faith, Moslem law for Roman law, the Arab tongue for the Greek and Latin tongue....The Mediterranean had been a Roman lake; it now became for the most part, a Moslem lake. From this time on it separated, instead of united, the East and West of Europe. The tie which was still binding the Byzantine Empire to the Germanic kingdoms of the West was broken.[3]

In Pirenne's view it was the Moslem invasions, not the Germanic ones, that constitute the abnormal factor which explains the collapse of the Mediterranean Commonwealth. But as we pointed out earler, Pirenne not only offers us his specific causal conclusion, but also describes certain states of affairs characteristic of life in those lands included in the Commonwealth, both before and after the Moslem invasions, i.e. continuity and change in the Empire's commercial relations, the standard of its monetary system, and the location of its largest cities. Rather than tracing the process "intervening between the event called the cause and that which we regard as its effect," Pirenne employed the Method of Difference to establish his causal conclusion. Thus, while Pirenne, or anyone else who uses such a method for establishing a causal hypothesis, must assume that there is a single ongoing process

that connects the initial and terminal points of some event, he need not trace that process in order for his causal conclusion to be considered warranted.

The importance of this last point cannot be underestimated. Mandelbaum thinks that all causal explanations must involve tracing the process that led to some result, but this is far from the truth. If we think back to the examples I gave of causal explanations that were based upon the Method of Difference we can appreciate how futile it is to insist that causal explanation must consist of a description of processes.

It seems to me that the reason Mandelbaum insists that satisfactory causal explanations must consist of a description of processes is that he endorses the ideal of explanatory closure, that is, he believes that causal explanations must include all the necessary conditions of the event to be explained. Despite claims to the contrary, I think this is evident when he asserts that an adequate description of a causal process must include all of the elements that are the components of that process. In the next section, then, I want to examine his views on the subject of explanatory closure more extensively. What we will find, I suggest, is that his view of the manner in which closure is to be achieved is objectionable and, moreover, that it conflicts with the role he attributes to pragmatic considerations.

(D) Explanatory Closure and Causal Explanations

In The Discussion of Human Affairs Charles Beard argued that

> A Baptist sermon in Atlanta, if we seek to explain it, takes us back through the Protestant Reformation to Galilee--and far beyond in the dim origins

> of civilization. We can, if we choose, stop at any point along the line of relations, but that is an arbitrary act of will and does violence to the quest for truth in the matter.[4]

What Beard must be understood to be saying here is that because every causal condition of a particular event has its own causal conditions, that event is never truly explained unless all of the elements in the whole regressive chain of causal conditions are themselves explained. Beard's counsel of despair, which some would claim is the result of accepting something like Hegel's notion of the internality of all relations, has met with stiff resistance from those who would maintain the objectivity of historical knowledge. Being exponents of this view, both Nagel and Mandelbaum ask whether violence to the truth is actually being done by stopping at some point in the regressive series of causal conditions when we are trying to explain some event. They both deny that this is the case, and for exactly the same reason. The problem as they see it, is that any event can be described in a variety of ways. Such descriptions will often depend upon the inquirer's interests, knowledge, and purposes. But once an inquirer describes an event in a particular way and asks a specific causal question under that description, we need not assume that a correct response to his causal question will also answer any other causal questions that the very same event may provoke. Nagel states this view quite forcefully when he asks,

> precisely what is it in connection with the Baptist sermon in Atlanta for which an explanation is to be sought? Is it why a given individual delivered a sermon at a stated time and occasion, or why he chose a particular text and theme, or why that occasion happened to arise, or why Baptists flourish in Atlanta, or why the Baptists developed as a Protestant sect, or why the Protestant Reformation occurred,....[5]

Quite clearly, a correct answer to any one of these questions does not answer all the rest. In Nagel's words, the conclusion we then must draw is that "once the event to be accounted for is made reasonably definite, it is self-contradictory to maintain that a historian's explanation of the event is objectively warranted only if he first completes a series of explanations each term of which is an explanation for the data assumed in the previous one" (SS 579).[6]

Mandelbaum draws much the same conclusion as Nagel when he claims that "While each answer to a specific causal question may raise further causal questions, this does not mean that the initial question was not adequately answered" (AHK 79). Although I find this pluralistic conception of question-asking and question-answering wholly unobjectionable, in another passage Mandelbaum asserts something very much like this while, in my view, adding something extra to it. This additional element, I wish to maintain, is not warranted. "In explaining an event," writes Mandelbaum,

> we come to a natural stopping point when we specify the elements in the process that terminated in that particular event; we are not committed to offering further explanations as to why each of these elements itself occurred. This is no less true on the view I have been defending than it is on the conventional view (AHK 90).

The problem with this is that on the conventional view, or at least on Hart and Honoré's view, the regressive chain of causal conditions is stopped by applying the criteria of abnormality and/or voluntariness. But Mandelbaum proposes no such criteria to halt the backward movement of such inquiries. We are never told how we are to recognize a "natural stopping point" nor do we ever find out how many "elements" must be specified before a given event is explained. I think that Louis Mink has expressed a related, though different concern about

Mandelbaum's process conception of causation when he writes that

> Mandelbaumian causality makes it impossible to impute particular responsibility for an occurrence. A Mandelbaumian murderer might well plead that it is irrational to single him out from the 'whole process' eventuating in his death ('people don't kill people; processes kill people'). (220)

One of the major misgivings Mink has about Mandelbaum's idea of causal processes is that he inflates the notion of a process to such a degree that it seems to preclude ever finding the beginning of an event. I, too, share this view. It seems to me that it is more profitable to try to get clear about what criteria are used to specify the beginning of an event than it is to talk idly about "natural stopping points." The virtue of the voluntariness and abnormality criteria is that they very often define the goals of causal inquiry. Accordingly, they enable us to determine when the goal of a particular inquiry has been met. Without some such criteria I cannot see where or when a causal inquiry would terminate. In their absence there would be no "stopping points," let alone "natural stopping points."

In another, though related assault on Hart and Honoré's view of causal explanation Mandelbaum contends that they said something wrong; or at least misleading, when they claimed that "explanations are context-determined." Instead of using this expression, Mandelbaum recommends that we say that "answers to questions that are asked or implied are context-determined" (AHK 79). The virtue of this formulation is that

> we need not then speak as if the same occurrence may have two correct but different explanations; we can say, more accurately, that the two inquirers were puzzled by the same occurrence for different reasons. (AHK 67)

With this part of Mandelbaum's claim I think we can wholly concur. As we saw when considering Hart and Honoré's example of the man who suffered an attack of indigestion, while the wife and doctor were asking the same causal question (Why did this man suffer an attack of indigestion?) the presuppositions of their questions turned out to be quite different. When the wife asks why her husband suffered an attack of indigestion she is presupposing that he normally gets by without it, but when the doctor asks the same question he presupposes that other men usually get by without it. As such, the presuppositions of their questions (which were the same) were different. So far so good. But what worries me is the conclusion Mandelbaum draws from these observations, i.e. "that neither had grasped the correct explanation, but the failure of each was due to a different reason: Each lacked a different piece of the correct explanation" (AHK 67).

One point that surely needs to be made here is that in no sense did the wife or doctor lack any piece of relevant information about this man's indigestion that the other possessed. In making her causal judgment the wife was well aware of her husband's ulcers, likewise the doctor knew of his having eaten parsnips. Their problem was, given both of these causal conditions, which one was the abnormal one? It is in response to this question that their causal judgments differ and not with respect to any information that one possessed and the other lacked.

The idea of explanatory closure becomes even more apparent when Mandelbaum elaborates upon the manner in which singular causal judgments are deficient. As we have already seen, he wants to allow for different causal questions being asked of some one event, without a

correct response to any of these questions being regarded as deficient simply because it does not answer all other causal questions that may be asked of that event. Yet he seems to be saying just that when he speaks of the "failure of each." A closer reading of the paragraphs that precede the passage cited above reveals that Mandelbaum should not want to say that singular causal judgments of the sort that the wife and doctor offer are incorrect, but that they are incomplete in that they only partially explain the event to which their judgments refer (cf. Hempel's notion of an "explanation sketch"). This is apparent when Mandelbaum writes that "such explanations serve useful purposes and are often satisfactory as answers to particular causal questions. However, even though they may have adequate pragmatic justification, they ought not to be treated as if they were sufficient as explanations of the occurrences that, in a shorthand manner, they are used to explain" (AHK 66).

Evidently Mandelbaum is trying to have it both ways. On the one hand he maintains that "it is precisely this pragmatic dimension that keeps us from an endless quest in our search for causes" (AHK 79), yet once this pragmatic dimension comes into play it is then claimed that this renders our explanations insufficient or, to use my expression, incomplete. Thus, while Mandelbaum disparages the ideal of explanatory closure his account of a correct explanation obviously requires it. In order to fully appreciate the inconsistencies in Mandelbaum's conception of causal explanation we will need to examine in some detail his views on the nomothetic-idiographic distinction, for I believe that the basis for his claim that singular causal explanations are incomplete is that such judgments betray a nomothetic interest.

(E) The Nomothetic-Idiographic Distinction

The nomothetic-idiographic distinction, a distinction originally drawn by Windelband, is one which Mandelbaum explicitly accepts. Indeed, this is an understanement, for not only does he accept it, he invokes it in a particular form at one stage of his argument against the legitimacy of the cause-conditions distinction. The deployment of the distinction, in the particular form he gives to it, I believe to be decisive in his campaign against the viability of the latter distinction. I want, then, to examine just how he construes the nature and role of the distinction. In so doing I hope to show that Mandelbaum has proposed a false dichotomy between "giving a causal explanation of a specific event" and "formulating a law concerning all events of a given type." This errant proposal, I believe, is what leads Mandelbaum to the conclusion that in the end it is impossible for historians to distinguish the cause from other conditions necessary for the occurrence of an event.

To begin, the nomothetic-idiographic distinction has usually been understood to refer to the aims of various kinds of studies or to the interests of those who conduct such studies. More particularly, a nomothetic interest is said to refer to an interest in the general, whereas an idiographic interest refers to an interest in the particular. One who was motivated by a nomothetic interest would ask why certain things happen generally, e.g. Why do the stars shine? Why do earthquakes occur? On the other hand, those having idiographic interests would be concerned with particular happenings, e.g. Why did this fire occur? What led to his death? While it was originally thought that one could distinguish the activities of those who were engaged in studying physical phenomena from those who were concerned with human

phenomena along these lines, it is now generally agreed that this cannot be done, for much of the work done by sociologists, psychologists, and economists is guided by a nomothetic interest. Nevertheless, some writers have sought to show that the aims of historians can be contrasted with those of scientists, whether the latter be concerned with physical phenomena or human ones. Some who maintain this position do not even deem it essential to their argument that scientists should never be concerned with particular occurrences nor that historians need never concern themselves with generalizations of various kinds. What they stress, however, is that the primary concern of scientists is to establish generalizations and that their interest in particular occurrences is only incidental to that aim, whereas historians are primarily concerned with establishing what has occurred at a particular time and place and their use of generalizations is subordinate to that end. As the reader may suspect, Mandelbaum is a proponent of this latter view.

The particular form in which Mandelbaum presents the nomothetic-idiographic distinction involves a contrast between two distinct activities, analyzing the cause of a particular occurrence and formulating a law connecting events of a certain type. "To give a causal analysis," writes Mandelbaum,

> is to trace an ongoing process that terminated in the specific effect we wish to explain: This involves describing a particular set of interconnected occurrences. In formulating a law, on the other hand, one is concerned not with a particular effect, but with an effect of a specified type; the object is to show on what factor or factors an effect of this type always depends. (AHK 97-98)

Though a scientist must have recourse to particular instances when he formulates and tests his generalizations, and while historians rely on

generalizations (mostly generalizations about human nature) when the links in their descriptive accounts appear opaque, such activities should be taken as ancillary to the main purpose of their respective inquiries. Earlier Mandelbaum had endeavored to firm up the contrast alluded to in the passage cited above by offering a definition of a cause sine qua non while, at the same time, suggesting that the search for such causes is characteristic of those guided by a nomothetic interest, but not those who seek to explain particular occurrences. Thus, he maintains that

> the causa sine qua non be taken as referring to whatever condition is necessary for an event of a given type to occur. On the other hand, to explain the occurrence of some particular event one must discover the entire cause. Thus, the causa sine qua non of an event would be some factor that must be present in the entire cause of a specific event in order that any event of that type should occur. (AHK 81)

It is easy enough to see what Mandelbaum has in mind when he speaks of a causa sine qua non. It is, of course, exactly the same thing Hart and Honoré had in mind when they discussed this issue. To recall an example used earlier, a sine qua non of any occurrence of fire is the presence of oxygen. What Mandelbaum has in mind by discovering the "entire cause" of some particular event, as I've already suggested, is much less clear. To make it clear he invites us to consider a physiological example, the disease of tuberculosis. After providing the reader with the account of the cause of this disease that one finds in the Encyclopedia Britannica, Mandelbaum draws our attention to the fact that

> the effect, a case of tuberculosis, is regarded as the end result of a process beginning with the bacterial invasion. However, what is important is not merely the invasion but also the tissue reaction to

> it; the actual end result will be dependent upon those factors that, together, constitute the body's reaction to the invasion and the presence or absence of secondary infection. In speaking of any particular case of tuberculosis, we must therefore take into account not only the initial invasion, but the whole complex process that terminates in the spread of the infection or its containment. So long as we are attempting to explain what occurred in a particular case, it would be false to regard the bacilli as responsible for the effect. The actual outcome will have depended on an interplay of the various factors present in the case; no one among them is entitled to be isolated from the others and designated as 'the cause' that brought about the effect. It is only if one considers tuberculosis as a type of disease that one speaks of the tubercle bacilli as 'the cause' of that disease; but one is not then using the term 'cause' to refer to some event that was sufficient, under the circumstances, to produce a given effect, but to some factor that is a necessary feature present in all instances of the disease. In short, instead of giving a causal explanation of a specific event, one is formulating a law concerning all events of a given type. (AHK 86)

I now want to bring together what Mandelbaum has been saying in the passages I have cited in this section by challenging what he claims in the last two sentences of his lengthy discourse on explaining the cause of a single occurrence of tuberculosis. To begin, I would agree with Mandelbaum when he argues that if one were to assert that the invasion of the tubercle bacillus was "the cause" of this particular occurrence of the disease then one would be viewing that occurrence as an event of a given type. What I disapprove of is the conclusion he draws from this, that is, that one would then be "formulating a law connecting all events of a given type." This conclusion he arrives at by noting that the tubercle bacillus is a causa sine qua non for any occurrence of that disease. He then assumes that since those with nomothetic interests are concerned with discovering such causes and with connecting them with events of certain types, that therefore when

anyone cites a causa sine qua non as "the cause" of a particular occurrence he must be engaged in formulating or applying a law concerning all events of a given type. This conclusion simply does not follow.

Suppose that in some region of the world there has not been a case of tuberculosis in some twenty years. Given that context, should someone come down with this disease, I submit that it would not at all be inappropriate to cite the presence of the tubercle bacillus as "the cause" of this particular occurrence of the disease. On the other hand, if this occurrence of the disease happened in some part of the world where the inhabitants were exposed daily to this bacillus, but few came down with the disease because they had built up an immunity to its toxic effects, then the patient's susceptibility to the effects of the bacillus, or whatever factor may have been responsible for that susceptibility could be deemed the cause of this occurrence of the disease. Here I fully acknowledge that such singular causal explanations depend upon a contrast with what has happened in other cases and, as such, that one is contrasting the cause with events of a given type when one makes such judgments. However, when we cite the presence of the tubercle bacillus as the cause of a particular instance of tuberculosis, even though the presence of that bacillus is a sine qua non of the disease that takes its name, it is not on this score that it is designated as the cause of that occurrence. It just may turn out that what is regarded as the abnormal condition, and so, the cause of the event, may also be a sine qua non of any event of that type. As I suggested at the outset, Mandelbaum has proposed a false dichotomy; his distinction between tracing causal processes and formulating laws that connect events of various types does not exhaust the

field of causal explanation by any means.

Ironically, Mandelbaum himself offers an example that serves to undermine his thesis. It does so because he misinterprets the significance of the situation it depicts. Thus, he writes that

> in examining the problem of whether one can in fact distinguish between a cause and the accompanying conditions let us first take as an example the fact that one sometimes explains an exceptionally good harvest in terms of that year's prevailing weather. Although 'the cause' is taken to be the weather, it is nonetheless true that a whole set of other conditions must have been fulfilled for the harvest to have been good. Since we assume that farmers will have the skill and opportunity needed to plant and to tend their crops, and we also assume that the crops will grow when properly planted and tended, we regard the weather as the most variable of the factors relevant to the success of a crop in a particular year. Therefore it is the weather we single out as being responsible for the harvest. (italics mine; AHK 67-68).

What Mandelbaum fails to appreciate is that in making a comparison of the extent to which several factors have varied over a number of years, we sometimes find that one of these has varied greatly in a particular year whereas the others have changed little, if at all. Under these circumstances factors that have been relatively stable from year to year can be seen as "normal" conditions, conversely, a factor that represents a marked departure (great variation) from its status in previous years may be construed as an "abnormal" condition.

The logic here is that of the experimental design. Some factors are held constant while one factor is allowed to vary. In Mandelbaum's example it was reported that when the weather varied in the way it did while the other conditions for producing a crop remained relatively unchanged, an exceptionally good harvest took place. Thus, one change (the weather) accounts for another (the increase in crop yield).

What this indicates is that the singular causal judgments made by historians and other people sometimes constitute the results of experiments they have conducted, i.e. what I earlier referred to as retrospective or ex post facto experiments. In offering such judgments we are neither formulating laws nor tracing processes, nevertheless we are giving causal explanations that require a distinction to be drawn between cause and conditions.

(F) Beyond Causal Processes: Counterfactuals and Causal Explanation

Had this thesis been concerned only with the role played by the cause-conditions distinction in causal explanation my discussion of Mandelbaum's views on causal explanation would have terminated at the end of the last section. But as I have already maintained that causal judgments based upon counterfactual reasoning are sometimes bona fide causal explanations of actual events I feel compelled to defend this view against Mandelbaum's expressed opposition to it. Like many authors before him, Mandelbaum contends that "to say what might have happened if any number of things had happened other than what actually occurred is surely not to give a causal explanation of the particular case at hand." (AHK 91)

This view is shared by many historians who tend to see the use of counterfactual reasoning in causal inquiries dealing with the past as an unwarranted intrusion into their profession. To them, counterfactual reasoning is synonymous with wishful thinking, and this surely cannot serve as a foundation for determining what occurred in the past.

Among philosophers both Nagel and Mandelbaum find it necessary to take this view seriously. While Nagel rejects it out of hand and warns of its pernicious effects, Mandelbaum wholeheartedly subscribes

to it. Their respective positions vis-à-vis the role of counterfactual reasoning in historical research are so diametrically opposed that the passage in which Nagel portrays the view he wants to criticize could have been found in The Anatomy of Historical Knowledge. "According to one influential school of thought," writes Nagel,

> the historian's job is to discover what actually happened and to ascertain by what continuous transformations one period of human life grew out of a preceding one: in consequence, although it is fitting that a poet or a moralist concern himself with what might have been, it is inappropriate for a serious student of the past to do so. (SS 588)

Now I would not deny that the view expressed in this passage may serve a useful purpose in reminding historians to pay close attention to facts, but it does more harm than good when it is interpreted to mean that no kind of counterfactual reasoning can be part of a causal explanation. Yet this is exactly what Mandelbaum means when he suggests the following:

> In raising these hypothetical questions (about what might have happened had....) one is not offering a causal explanation of what actually did occur: One is not identifying anything that can rightly be termed a causal condition of the actual event one is attempting to explain. (AHK 91)

This passage reveals that Mandelbaum is making two distinct claims. On the one hand he is denying that responses to hypothetical questions constitute causal explanations, on the other hand he is maintaining that such responses do not serve to isolate any causal condition of the event one is trying to explain. By briefly considering the controversy among economic historians over the status of counterfactuals I think it can be easily shown that both of these claims are false.

In recent years the role of counterfactuals in historical research has been the subject of a heated debate among economic historians. Many say that some form of counterfactual reasoning is always a part of causal explanations, others contend that it is never a part of them. Characteristic of those who would adopt the first view is Fritz Redlich. While judging the work of the "new" economic historians (e.g. that of Robert Fogel), Redlich writes:

> It appears to me that there is no reason whatsoever to exempt economic and social history from the rule valid for history as a whole, namely, that historical research deals with the past that was and not with the past that might have been.... The result of such investigations (that seek to respond to counterfactual questions) is for me 'as if' history, quasi-history, fictitious history ----that is, not really history at all.

Although Mandelbaum says nothing specifically about the controversy over the role of counterfactuals in economic history, from what we have already seen him say about this issue it is clear that he would fully endorse Redlich's view. Stated simply, they both maintain that what did not happen cannot explain actual occurrences.

Among the "new" economic historians Robert Fogel stands out as one writer who has sought to defend the use of counterfactuals in causal explanations of the past. He argues that historians "do not really have the freedom to avoid counterfactual conditional statements." If they could avoid these statements then they must forever refrain from asserting that any historical agent made a mistake. For when we ask ourselves what we mean when we say that someone made a mistake we find that "we mean that the course of action that he followed was inferior to some other course that might have followed. Once we admit the possibility that people pursue paths of action that are inferior

to alternative ones," concludes Fogel, "we are assuming the existence of counterfactual conditional patterns of behavior."[9]

In arguing that the difference between the old and the new economic history rests not in the use of counterfactuals but in their explicit use by the new economic historians, Lance Davis draws the same conclusion as did Fogel. To make his point Davis invites us to consider the work of Professor Harry Scheiber, a critic of the new economic history. Writes Davis:

> In a recent article he has described settlement in the northwestern corner of Ohio.[10] To the extent that his work is limited to that description, he has no need for a contrapositive argument. But to the extent that his work is so limited, so is his contribution. In fact, the importance of Scheiber's work lies in his analysis of the causes of the particular pattern of settlement. Scheiber argues that settlement was much slower than one would have expected and that the explanation for the delay lay not, as others have implied, in the physical characteristics of the area, but in the particular land policy that led to large purchases by 'speculators'. Scheiber arguments are compelling, and I, for one, am convinced; but his argument by its very nature is a contrapositive one. Scheiber implicitly (by using terms like 'slower') compares a situation that was (the actual distribution of land) with one that never was (a land distribution policy disigned to get land quickly into the hands of the farmer).[11]

It seems clear to me that the conclusion Davis has drawn from his analysis of Scheiber's work is thoroughly sound. However, an important point that Davis fails to underscore is that when Scheiber says that "settlement was much slower than one would have expected," this expectation is not an arbitrary whim, but is based on a survey of the rate of settlement that occurred in other territories that possessed many of the features of the land situated in the northwestern corner of Ohio. What "made the difference" between the rate of settlement in

these areas and in the northwest corner of Ohio was the lack of a "land distribution policy that enabled farmers to acquire lands quickly." In the terminology used by Hart and Honore, the absence of such a policy would rank as an abnormal condition.

The kind of counterfactual statement that Davis discerned in Scheiber's work has been given concrete expression by another economic historian, Alexander Gerschenkron. According to him "certain well-accepted modes of thinking in comparitive history have counterfactual aspects." Thus, he contends that

> We may argue that in country M a certain factor A_1 appearing in conjunction with factors A_2, A_3.... A_n must have had a decisive causal impact on producing effect B because in country N, where effect B did not obtain, all other factors (A_2 to A_n) were present, but A was not. Here too we are asking the same counterfactual question: What would have happened if a certain factor were absent, that is say if a certain event or series of events had not occurred?[12]

The mode of counterfactual reasoning that Gerschenkron has outlined is none other than the Method of Difference. To say, as Mandelbaum has, that when historians employ this method in their research they cannot be "identifying anything that can rightly be termed a causal condition of the actual event," is far from being true. In Gerschenkron's model A_1 is certainly a causal condition of B, it is, in fact, its cause. Consequently, no matter how wide or narrow one takes the scope of comparative history to be, the very existence of this form of historical inquiry attests to the inadequacy of Mandelbaum's view that responses to hypothetical questions do not serve to isolate any causal condition of the event one is trying to explain. It is equally futile, I submit, to deny that such inquiries may yield bona fide causal explanations.

(G) Summary and Conclusion

For the most part this chapter has been concerned with Mandelbaum's criticism of Hart and Honore's account of singular causal judgments. Of course Mandelbaum is not only critical of their account in a destructive sense, he also develops a view of the causal relation that is supposed to be superior to the one presented by Hart and Honore. We began by considering this view.

Upon examination the salient feature of Mandelbaum's view of the causal relation appears to be the visual perception of occurrences that manifest temporal and spatial continuity. When we perceive causation, he claims, the cause cannot be seen as being separate from the effect, both are parts of an ongoing process. The point of causal explanations, he maintains, is to trace the succession of occurrences that lead from cause to effect.

Given this conception of causal processes and causal explanation Mandelbaum confronts Hart and Honore's account of singular causal judgments. In the first instance he argues that they endorse the view that causal sequences always manifest a prior event as the cause of a subsequent one, a view which he finds untenable. As I have shown, this claim is false for Hart and Honore do discuss cases in which the cause is not a temporally antecedent event.

Secondly, I pointed out that even if we were to accept Mandelbaum's idea of perceiving causation it is still quite possible to separate cause from causations when giving a retrospective account of the event. In other words, causal explanations need not mirror the flow of events in causal processes. I think that our reconsideration of the Pirenne explanation served to make this point.

Next I considered Mandelbaum's attempt to specify a criterion for demarcating the beginning of a causal process. His argument that there are natural stopping points is supposed to present us with such a criterion. However, when we ask how we are to identify a natural stopping point, his response turns out to be quite unenlightening, viz., we reach a natural stopping point once "we specify the elements in the process that terminated in that particular event."

Another of Mandelbaum's criticisms of the cause-conditions distinction is presented in the form of an exclusive option: a bona fide causal explanation must involve an attempt to either formulate laws that connect various types of events or trace a process that terminates in a specific effect. Here I argued that singular causal judgments do not fall into either category, nonetheless they can quite properly be viewed as a distinct type of causal explanation.

Together, these four arguments constitute the brunt of Mandelbaum's attack on Hart and Honoré's account of singular causal judgments. Although I have already pointed out deficiencies in their account of these judgments I still think that his wholesale rejection of it is way out of line; at least their analysis of the abnormality criterion does demonstrate one of the ways in which the Method of Difference functions in causal explanations.

The issue of the role of counterfactuals in historical research (which has no bearing on Hart and Honoré's views) was the final one I considered in this chapter. As I pointed out earlier when examining Fogel's explanation of the importance of the railroad and Scheiber's explanation of the delay in settlement, responding to counterfactual questions need not be an exercise in wishful thinking. Contrary to what

Mandelbaum believes these kinds of causal-explanations make it clear that responses to such questions can serve to identify conditions of an event that actually occurred.

In conclusion, it seems to me that the sheer number of mistaken or exaggerated claims made by Mandelbaum does little to enhance the plausibility of his own account of causal processes and their relation to causal explanation. Thus, despite the fact that I found it necessary to revise certain aspects of Hart and Honoré's analysis of singular causal judgments, I still feel that their account of these judgments can withstand Mandelbaum's objections and that this account is a better representation of the historian's use of causal concepts than is Mandelbaum's.

CONCLUSION.

In the preceding pages I have surveyed a number of views concerning the nature of singular causal judgments. Beginning with J. S. Mill's recognition that these judgments present a problem for philosophers I then went on to present a historical overview of the major responses to Mill's position on this problem, most notably those of Ducasse, Collingwood, and Hart and Honoré. In so doing I found that singular causal judgments present many aspects and are often based on quite different grounds. Accordingly, my reply to the question: Are these judgments objective or subjective?, was necessarily equivocal.

By now it should be clear that I believe that singular causal judgments are objective when they are based on the Method of Difference. In this respect I indicated how the drawing of the cause-conditions distinction in everyday situations can be based on that method; moreover, I explained the manner in which it is used in scientific experimentation and in historical inquiry. In addition, I gave a detailed account of the way in which this method provides the basis for judgments of causal importance, whether these occur in everyday situations or in the social sciences.

What makes these kinds of causal judgments objective is their reliance upon a control subject or group of subjects. As I pointed out, the nature of a control group can vary quite a lot; entities, states, and processes can all serve as controls. Viewed in this way Hart and Honoré's "normal cases" are controls, so too are the actual and hypothetical "com-sit's" that Raymond Martin spoke of. Once it is seen that achieving control need not consist of direct physical

manipulation, it then becomes easier to appreciate how many causal explanations are based on this notion, whether these explanations are offered in everyday situations, historical accounts, or scientific reports.

Between singular causal judgments that utilize controls and those which are groundless (as Mill claimed all such judgments were), there exists a variety of these judgments that are based on other considerations, e.g. the adoption of different senses of "normal" and the estimation of degrees of voluntariness. These judgments, I maintained, while they also contrast cause with conditions, are subjective. That is, what is called the cause will be ultimately determined by the inquirer's own values.

The result of applying this analysis of singular causal judgments to the sphere of history is clear: Those causal explanations presented by Pirenne, Eisenstein, Kissinger, Scheiber, and Fogel are objective. The ones offered by Taylor and Trevor-Roper are subjective. This is not to say that objective causal judgments are immune from criticism or that they cannot be shown to be inaccurate. What it does mean is that they conform to the basic requirement of experimentation, i.e. there being two or more subjects that are alike in relevant respects, with one of these being exposed to the (suspected) cause while the other is left unexposed. It is this claim that I would like to count as my own response to Mill's challenge to find a principle for distinguishing cause from conditions.

FOOTNOTES

Introduction

[1]J. S. Mill, System of Logic (New York 1857), p. 199. Hereafter all page references to this text will appear in parentheses after quoted passages.

[2]Ibid., p. 199.

[3]Two other contemporary philosophers have drawn a distinction between productive and explanatory causes. In The Cement of the Universe, J. L. Mackie speaks of such a distinction, claiming that productive causes are events whereas explanatory causes are facts. (See pp. 260-265) Joel Feinberg has also drawn attention to a distinction "between two different perspectives from which causal judgments can be made--the productive and the explanatory." "Causing Voluntary Actions," in Doing and Deserving (Princeton 1970), p. 158. In a footnote in his Rejoinder to criticism of this paper Feinberg maintains that "Difficult as this distinction is to characterize in the abstract, it is intuitively clear, as is illustrated by the story of the straw that broke the camel's back." The story goes as follows: Suppose that Omar's daily routine is to take his camel to ten different stations in the marketplace. At each stop a merchant loads one hundred straws, one at a time, on the camel's back. By the end of the day, then, there are one thousand straws on the camel's back, a weighty but not quite at the back's breaking point; and Omar then leads the camel to a warehouse to be unloaded. One day, just as the tenth merchant puts the last straw on the load, the camel sags to the ground, his back fractured. A subsequent inquest discloses that early in the day, at the third stop, a mischievous urchin, unnoticed by anyone, slipped one extra straw on the load. Now the question arises: which straw caused the camel's back to break? Both the urchin's straw and the last straw (let us suppose) were necessary conditions of the break, and, depending on what we include in the causal background, both can be regarded as sufficient. From the productive point of view, clearly the last straw was the cause, since its arrival at the top of the pile brought the burden above the breaking point. Still Omar does not understand why the camel's back broke until he learns of the urchin's straw, and the point of a causal explanation is to foster understanding--Doing and Deserving, p. 182.

[4]Mill, p. 197.

[5]A. J. P. Taylor, English History, 1914-1945 (New York 1967), p. 453.

[6]Robert Fogel, Railroads and American Economic Growth (Baltimore 1964), p. 20.

Chapter One

[1]David Hume, A Treatise of Human Nature (Everyman edition), p. 168.

[2]Ibid., p. 168.

[3]Ibid., p. 168.

[4]A very good recent account of these techniques is provided by Ronald Giere in Understanding Scientific Reasoning (New York 1979). An older, though still an excellent treatment of this subject is Ernest Greenwood's Experimental Sociology (New York 1945).

[5]This summarizes the account of Pasteur's experiment given by Paul De Kruif in Microbe Hunters (New York 1926), pp. 159-161.

[6]This summarizes the account of Pasteur's experiment given by H. T. Pledge in Science Since 1500 (New York 1959), p. 165.

[7]I should add that one can find instances when the causal judgments rendered by historians will be based on the Method of Agreement or on the Method Concommitant Variation. However, I am not convinced that either of these two Methods is essentially different than the Method of Difference. As far as I can tell the Method of Agreement is the Method of Difference without any explicit reference to a comparison group or subject while the Method of Concommitant Variation is the Method of Difference employed successively to the qualitatively same though quantitatively different cause and effect-factors.

[8]C. J. Ducasse, Causation and the Types of Necessity (New York 1924), p. 19.

[9]Ibid., p. 19.

[10]Ibid., p. 19.

[11]Ibid. pp. 19-20.

[12]Ibid., pp. 85-86.

[13]Samuel Gorovitz, "Causal Judgments and Causal Explanations," Journal of Philosophy, Vol. 62, No. 23 (December 1965), p. 695.

Chapter Two

[1]R. G. Collingwood, The Idea of History (Oxford 1971, p. 215. Hereafter all page numbers of passages cited from this text will appear in parentheses after the quoted passage.

[2]R. G. Collingwood, Essay on Metaphysics (Oxford 1972), p. 47. Again, page numbers of all passages cited from this text will appear in parentheses after the quoted passage.

[3]This example was first discussed by Ernest Nagel in The Structure of Science (New York 1961), p. 552 ff. and then by Michael Scriven in "Causes, Connections, and Conditions in History," in Philosophical Analysis and History, ed. by W. H. Dray (New York 1966), p. 246 ff.

[4]Alan Donagan, The Later Philosophy of R. G. Collingwood (Oxford 1970). p. 193.

[5]W. H. Dray, "Singular Hypotheticals and Historical Explantation," in Sociological Theory: Inquiries and Paradigms, ed. by L. Gross (New York 1967), p. 195-96.

[6]This schema for action-explanations closely resembles that produced by Rex Martin, Historical Explanation: Re-enactment and Practical Inference (Ithaca 1977), pp. 77-78. Essentially there are two differences between my schema and Martin's. What goes under the heading of items (4) and (5) in Martin's account I have condensed into item (4) in my account, and, secondly, I take seriously Donagan's claim that an agent may have both a causa quod and causa ut in his mind yet fail to "put them together." Thus item (5) in my schema is not included in Martin's.

[7]Lawrence Davis, Theory of Action (Englewood Cliffs, N. J. 1979), p. 59.

[8]Ibid., p. 59.

[9]When we come to examine Hart and Honore's conception of a "voluntary action" in Chapter V we w ll find that they contest this claim.

[10]W. H. Dray, "Concepts of Causation in A. J. P. Taylor's Account of the Origins of the Second World War," in History and Theory, Vol. 17, 2, p. 164.

[11]Ibid., p. 164.

[12]A rather lengthy example of the kind of causal relationship at issue is to be found in Edward Shorter's book, The Making of the Modern Family (Toronto 1976). In this work Shorter trys to explain what he regards as "the central fact in the history of courtship over the last two centuries," namely, "the enormous increase in sexual activity before marriage." p. 81. Shorter contends that the history of sex before marriage in Western Europe and North America can be divided into four major periods: 1550-1650, 1750-1850, 1850-1940, and 1940-1970. Assuming that the number of illegitimate births and premarital pregnancies afford the most reliable data for determining the incidence of sex before marriage, he compiled statistics on these two indices and transposed them onto a graph. What this graph shows is that in the first period there was a brief, but relatively unimportant rise and fall in out-of-wedlock pregnancies. During the second period, however, there was an enormous rise in illegitimacy and premarital pregnancies. Towards the end of the eighteenth century "the number of out-of-wedlock pregnancies began to skyrocket in

virtually every community we know about, often reaching three or four times the previous levels" (pp. 82-83). Then, towards the middle of the nineteenth century there was a levelling off of the rate of bastardy and premarital pregnancies. In the third period this rate declined sharply, only to soar to new heights during the fourth period.

What is interesting about Shorter's observation of fluctuations in sexual activity before marriage is the manner in which he goes about trying to explain the transition from the second to the third periods. In this respect he poses a question: "Did premarital pregnancies fall off because people had sex before marriage less often in these years (Victorian repression and all that) or because unmarried couples began to practice contraception?" To this question he replies: "While many scholars choose the former explanation, the simultaneous decline in marital fertility suggests to me it was the latter. If people had fewer and fewer children within marriage, it was probably not because they were making love less often, but because they were practicing birth control. The simultaneity in the timing of the marital and nonmarital fertility downslides is so close as to suggest that contraception caused the drop in nonmarital conceptions as well" (p. 83).

[13]Arthur Lower, Colony to Nation (Toronto 1946), pp. 48-49. The affinities as well as the differences between F. J. Turner's "frontier hypothesis" and Lower's explanation of the characteristics of early Canadian social relations is discussed by Carl Berger in The Writing of Canadian History (Oxford 1976).

[14]William Ogburn, "How Technology Causes Social Change," in Technology and Social Change, (New York 1957), ed. by W. Ogburn et. al., p. 15.

[15]Ernest Nagel, The Structure of Science (New York 1966), pp. 452-453. Hereafter page numbers of all passages cited from this text will appear in parentheses after the quoted passage.

Chapter Three

[1]H. L. A. Hart and A. M. Honoré, "Causation in the Law," Law Quarterly Review, Vol. 72, January 1956, pp. 58-90, 260-81, 398-417.

[2]H. L. A. Hart and A. M. Honoré, Causation in the Law (Oxford 1959). Hereafter page numbers pertaining to passages cited from this work will appear in parentheses following the quoted passages.

[3]Collingwood alludes to this aspect of the causal relation in the Essay on Metaphysics, pp. 309-11. For a more detailed examination of this view of the causal relation see Douglas Gasking, "Causation and Recipes," Mind, Vol. 64, October 1955, pp. 479-87.

[4]Since I am trying to show that the grounds upon which the cause-conditions distinction is sometimes drawn corresponds to the conditions of a controlled experiment it may be argued that it is not only the Method of Difference that is involved in these inquiries, but also the Method of Agreement. Nicholas Rescher has argued that "any experiment in which a control group is used would (if completely successful) yield a conclusion by the Joint Method of Agreement and Difference"--Introduction of Logic (New York: St. Martin's, 1964) p. 304.

[5]Although Hart and Honoré were apparently unaware of R. M. MacIver's Social Causation at the time they were writing, MacIver's discussion of the notion of "cause as Precipitant" suggests the same view, viz., "we do not usually raise the question why so long as things pursue what we regard as their normal or typical course. It is the exception; the deviation, the interference, the abnormality, that stimulates our curiosity and seems to call for explanation." Social Causation (New York: Peter Smith, 1942), pp. 72-73.

[6]Authors of logic texts that contain a chapter on Mill's Methods invariably point out that Mill's formulation of this Method is too strong. They claim that it is impossible to take all circumstances of a phenomenon into account, suggesting that the Methods can only be used in conjunction with the hypothesis that the circumstances mentioned are the only relevant ones. On this point see M. Cohen and E. Nagel, An Introduction to Logic and Scientific Method (New York 1934); A. Frye and A. Levi, Rational Belief (New York 1958); and I. Copi, Introduction to Logic (New York 1978).

[7]The point I am making about "an instance" and "normal cases" is analogous to a problem students often have in comprehending what the Method of Difference applies to when they are dealing with experiments where there are a large number of subjects in both the experimental and control groups. Does it apply to the groups or to the individual subjects in those groups? Many students tend to think that the Method applies collectively rather than distributively. On the other hand Copi, when providing an answer to an example of a controlled experiment he offers, shows that he favors a distributive account, pointing out that "there are as many uses of the Method of Difference here as there are 'matching' pairs of chickens in the two groups"--Introduction to Logic (New York 1978), p. 567.

[8]Raymond Martin, "Singular Causal Explanations," Theory and Decision, Vol. 2, No. 3, March 1972, p. 166.

[9]Robert Fogel, Railroads and American Economic Growth (Baltimore: John Hopkins University, 1964), p. 26.

[10]Peter McClelland, "Railroads, American Growth, and the New Economic History," Journal of Economic History, Vol. 27, No. 1, 1968, p. 102.

[11]McClelland makes this charge in his paper. Ibid., p. 121.

[12]Ernest Nagel, The Structure of Science (New York: Harcourt, Brace, and World, 1961), pp. 582-86.

[13]Morton White, Foundations of Historical Knowledge (New York: Harper and Row, 1965), pp. 129-32.

[14]Nagel, op. cit., p. 584.

[15]Ibid., pp. 585-86.

[16]Ibid., pp. 585-86.

[17]Ibid., p. 458.

[18]Ibid., p. 458.

[19]See the ongoing debate between M. Martin and R. G. Frey in the journal Philosophy of the Social Sciences. M. Martin, "Causal Importance and Objectivity," 4, 1974, pp. 159-68. R. G. Frey, "Judgments of Causal Importance in the Scoial Sciences," 6, 1976, pp. 245-48. M. Martin, "Judgments of Contributory Causes and Objectivity," 8, 1978, pp. 173-75. R. G. Frey, "Contributory Causation and the ," Objectivity of the Social Sciences,", 8, 1978, pp. 175-79. M. Martin "Contributory Causes Again," 8, 1978, pp. 180-81. R. G. Frey, "Contributory Causation and Objectivity: A Final Installment," 8, 1978, pp. 182-83. I find it odd that neither Martin nor Frey even so much as mentions Nagel's examination of senses of more important and less important causes in The Structure of Science.

[20]Nagel, op. cit., p. 586.

[21]Raymond Martin, "On Weighting Causes," American Philosophical Quarterly, Vol. 9, No. 4, October 1972, p. 292.

[22]Ibid., p. 292.

[23]Nagel, op. cit., p. 585.

[24]Sten Nilson, "On the Logic of Historical Explanation," Theoria, Vol. 36, No. 2, 1970, p. 75. Also see Sten Nilson, "Two Additional Remarks on the Logic of Historical Explanation." Theoria, Vol. 15, 1974, pp. 1-8. And on the same problem see Michael Hammond, "Weighting Causes in Historical Explanation," Theoria, Vol. 43, No. 2, 1977. It is worth mentioning that though they are dealing with the same problem, there is not a single cross-reference to be found in the aforementioned debate between M. Martin and R. G. Frey and the debate over weighting causes in Theoria.

[25]Nagel, op. cit., pp. 584-85.

[26]R. Martin, op. cit., p. 292.

[27]Ibid., pp. 292-93.

[28]Due to the fact that these examples center around changes in the magnitude of some result, these changes being correlated with changes in two or more factors, it might be thought that the Method applied here is that of Concomitant Variation rather than of Difference. In my view they amount to the same thing in cases of this sort.

[29]See footnote 12 in Chapter 2, Section 1, pertaining to Edward Shorter's The Making of the Modern Family.

[30]Morton White, Foundations of Historical Knowledge (New York: Harper and Row, 1965), pp. 116-17.

[31]Ibid., p. 116.

[32]Henri Pirenne, Medieval Cities (Princeton: Princeton University Press, 1925), p. 14.

[33]See A. F. Havighurst (ed.), The Pirenne Thesis: Analysis, Criticism, and Revision (Boston: D. C. Heath, 1958).

[34]The form in which the Method of Difference is presented below has been adapted from J. L. Mackie's discussion of Mill's Methods in the Appendix of The Cement of the Universe (Oxford: Clarendon, 1974), pp. 303-04.

Chapter Four

[1]A form of cause competition that resembles, but is distinguishable from the second type listed below occurs when one person suggests that a condition cited as the cause of some event by another person is not even a condition of that event, let alone its cause.

[2]Elizabeth Eisenstein, The Printing Press as an Agent of Change (2 Vols.; New York: Cambridge University Press, 1979). Hereafter page numbers of all passages cited from this work will appear in parentheses following the quoted passages. All references are from Volume 2.

[3]Henry Kissinger, A World Restored (New York: Grosset and Dunlap, 1964). Hereafter page numbers of all passages cited from this work will appear in parentheses following the quoted passages.

[4]See Harold Nicolson, The Congress of Vienna (London: Constable and Co., 1946).

[5]Robert Keeton, Legal Cause in the Law of Torts (Columbus: Ohio State University Press, 1963), p. 3.

[6]Joel Feinberg, "Sua Culpa," in Doing and Deserving (Princeton: University Press, 1970), p. 196.

[7]J. L. Mackie, "Responsibility and Language," Australasian Journal of Philosophy, Vol. 33, 1955, pp. 143-59. Hereafter the page numbers of all passages cited from this work will appear in parentheses following the quoted passages.

[8]W. H. Dray, "Concepts of Causation in A. J. P. Taylor's Account of the Origins of the Second World War," History and Theory, Vol. 17, No. 2, 1978. Hereafter page numbers of all passages cited from this article will appear in parentheses following the quoted passages.

[9]A. J. P. Taylor, The Origins of the Second World War (Middlesex, England: Penguin, 1964). This edition contains Taylor's "Second Thoughts." Hereafter page numbers of all passages cited from this work will appear in parentheses following the quoted passages.

[10]Hugh Trevor-Roper, "A. J. P. Taylor, Hitler, and the War," Encounter, Vol. 17, July 1961, pp. 89-90.

[11]Taylor argues that Article 231 of the treaty, which ascribed sole responsibility for the First World War to Germany was never accepted by the German people or their leaders. Moreover, the requirement that Germany pay reparations to the Allies, a sum that would have required two generations to pay in full, was a constant source of grievance for the German people.

[12]See the corollary Trevor-Roper draws from the argument he imputes to Taylor: "in the new circumstances this created, 'realism' consists in allowing the new great power which has replaced Germany in Europe to assert its 'natural weight.' Mr. Kruschev, we should recognize, has no more ambitions of world-conquest than Hitler. He is a traditional Russian statesman of limited aims, and the 'moral line' consists in letting him have his way more completely than we let Hitler has his: in other words, unilateral disarmament." op. cit., p. 96.

[13]This is not to say that the same result cannot sometimes be achieved by applying both the abnormality and the voluntariness criteria. As Hart and Honoré point out, a voluntary action may, in certain circumstances, be an abnormal condition.

[14]Actually, Taylor claims to be presenting Hitler's conception of what were normal and abnormal conditions. He asserts that Hitler "wanted the Allies to accept the verdict of March 1918; to abandon the artificial undoing of this verdict after November 1918; and to acknowledge that Germany had been victorious in the East." Origins, p. 99.

[15]Ibid., p. 40.

[16]"Second Thoughts," op. cit., p. 7.

[17]A. J. P. Taylor, "War Origins Again," in The Origins of the Second World War, ed. E. M. Robertson (London, 1971), p. 139.

[18]Ibid., p. 140.

Chapter Five

[1]H. L. A. Hart and A. M. Honoré, "Causation in the Law," Law Quarterly Review, 72, (1956), p. 80.

[2]The authors offer two arguments to support this claim. One is a linguistic argument concerning circumstances in which it is natural or unnatural to say that one person caused another to act. This argument is treated at length below. The second argument we shall not consider in depth, though some have thought it more important than the first. This second argument they offer in support of their view that there are "radical differences which separate 'He induced me to do it' from 'His blow caused the victim's death'." (49) Their argument is that the relationship between the actions of two human agents "though often and intelligibly called causal connexion.... do not depend upon 'regular connexion' or sequence as the causal relations between physical events do." (48) The argument from generalization, as we may call it, has been challenged by a number of writers. In The Cement of the Universe, J. L. Mackie writes that "our general knowledge of human purposes facilitates interpersonal causal interpretations of what we observe, and this is strictly analogous to what we can say about physical cases. Once we see just how general knowledge of different sorts may or may not come in, we find no systematic difference between physical and interpersonal types of cases which would justify us in speaking of two causal concepts." (p. 123) Again, in The Logic of Explanation in Psychoanalysis, R. Sherwood contends that "There is a sense in which it is true to say that singular reason statements are not covertly general. But in this same sense, it seems clear that singular causal statements likewise do not imply any general statements. On the other hand, there is also a sense in which it is true that singular causal statements do imply a general statement. But in this second sense, it is also true that singular reason statements likewise imply such statements." (pp. 158-59)

[3]D. A. Lloyd-Thomas, "Consequences," Analysis (October 1967), p. 139.

[4]See R. M. MacIver, Social Causation (New York 1942), pp. 172-73.

[5]Joel Feinberg, "Causing Voluntary Actions," op. cit., p. 166.

[6]The most conspicuous cases of this sort in the history of all forms of life, including human history, occur when some extraneous agent comes into contact with a foreign population for the first time. Thus, Rene Dubos, the distinguished comparative pathologist, has written that "the most dramatic examples of pestilence have been provided by the introduction of pathogenic agents into populations which have not had recent exposure to them. Thus, it is widely recognized that increase in international trade brought plague to the Roman world

of the Justinian era and again to Europe during the Renaissance." Mirage of Health (New York 1960), p. 156. Historians now accept the view that it was the White Man's diseases, rather than his alcohol or guns, that accounts for the enormous casualties suffered in a short span of time by the North American Indians.

[7]Reay Tannahill, Food in History (New York 1973), p. 347.

[8]E. Shorter, The Making of the Modern Family (New York 1975), p. 83.

Chapter Six

[1]Maurice Mandelbaum, The Anatomy of Historical Knowledge (Baltimore 1977). Hereafter page numbers for all passages cited from this work will appear in parentheses at the end of the quotation.

[2]Maurice Mandelbaum, The Problem of Historical Knowledge (New York 1938), p. 227.

[3]Henri Pirenne, Medieval Cities (Princeton, 1925), pp. 24-25.

[4]Charles A. Beard, The Discussion of Human Affairs (New York, 1936), p. 36.

[5]Ernest Nagel, The Structure of Science (New York, 1961), p. 579.

[6]Ibid., p. 579.

[7]Louis Mink, Review of The Anatomy of Historical Knowledge, History and Theory (Vol. 17, No. 2), p. 220. 1978.

[8]Fritz Redlich, "Potentialities and Pitfalls in Economic History," in The New Economic History: Recent Papers on Methodology, ed. by R. L. Andreano (New York, 1970), p. 91.

[9]Robert Fogel, "Comment," Ibid., pp. 129-30.

[10]Harry Scheiber, "State Policy and the Public Domain: The Ohio Canal Lands," Journal of Economic History (Vol. 25, No. 1 1965), pp. 86-113.

[11]Lance Davis, "And It Will Never Be Literature: The New Economic History: A Critique," op. cit., p. 76.

[12]Alexander Gerschenkron, "Some Methodological Problems in Economic History: Postscript," in Continuity in History and Other Essays (Cambridge, Mass. 1968), p. 53.

APPENDIX

WHAT'S THE PROBLEM WITH CAUSAL OVERDETERMINATION?

By Martin Schatz

In a recent paper[1] R. G. Frey has argued that a contributory theory of causation is superior to a conditional one in that the former 'easily accommodates talk of part causes in cases of causal overdetermination whereas conditional analyses apparently produce counterintuitive results' (p. 111). I think that Frey's claim is false, and can be shown to be false by making explicit the assumptions underlying his argument and by giving a detailed account of the example of causal overdetermination he has devised.

Frey allows that in causally nonoverdetermined cases the results of applying one or the other theory of causation will be more or less the same. Thus, 'if it takes 10 units of mercury dumped into a river to pollute it and kill the fish, and if factory A dumps 5.3 units and factory B dumps 4.7 units, and if the river is thereby polluted, it may well be tempting to label both of these contributory causes necessary conditions (in the circumstances) of the pollution' (p. 111). The situation is otherwise, we are told, when we consider cases of causal overdetermination, for in such cases talk of necessary and sufficient conditions becomes impossible. This is said to be obvious if we suppose that 'it had been five factories which simultaneously dumped 20 units of mercury into the river' (p. 111). On the basis of this description of what occurred Frey draws two conclusions:

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[1] R. G. Frey, "Causal Responsibility and Contributory Causation', Philosophy and Phenomenological Research, 39, 1978, 106-19.

> (1) it would not be true of any factory's act of dumping that it was a necessary condition for the river's pollution, since, obviously, even if that factory had not dumped its mercury, the river would still have been polluted.
>
> (2) Nor do we get anywhere if we attempt to regard each of the five acts of dumping as part of a sufficient condition for pollution, since, plainly, any single one of the acts on its own more than suffices to pollute the river/ (P. 111).

What I want to suggest here is that in whatever sense Frey is justified in claiming that 'each factory's act is a part cause of and that all five acts of dumping are the whole cause of the river's pollution' (p. 111), in that sense we can, and indeed must, regard each factory's contribution as a necessary condition and as part of a sufficient condition for 'the river's pollution.'

I

As Frey's five-factory example makes clear, the problem of causal overdetermination arises when some effect has occurred as the result of the operation of several causal factors, each one of these being sufficient to produce the effect by itself. When we are presented with the task of analyzing such cases there seem to be three considerations that may lead us to abandon talk about causally relevant factors being necessary and/or part of sufficient conditions for some effect. These are (1) the effect is described very broadly, (2) the effect cannot be broken down into parts, and (3) we are uncertain about the amount that each factor contributed to the production of the effect.

In the example of causal overdetermination with which Frey has presented us neither the second nor third consideration applies. Since we know that each factory simultaneously dumped 20 units of mercury

into the river and that the presence of 10 units is sufficient for the river to be declared 'polluted', we can determine the magnitude of contribution of each of the five factories. However, the first consideration listed above does play a crucial role in Frey's argument. He fails to recognize that a causally relevant factor which we would hesitate to call a necessary condition, or part of a sufficient condition for an effect described in broad terms, may have to be treated as such when the effect in question is described with greater precision. In what follows I shall show in what way recognition of this consideration serves to undermine Frey's argument.

II

I have already said that we can determine the magnitude of contribution of each of the five factories in Frey's example. In order to make this determination, however, we need to assume that the cause of the effect is some minimally sufficient condition, that is, the cause must be 'sufficient in the circumstances'. Having made this assumption we can then say that if it takes 10 units of mercury to pollute the river, then each of the five factories which simultaneously dumped 20 units of the toxic substance into the river contributed 2 units (or 10% of its total contribution) to the production of the effect. Here, what is left over once 10 units of the mercury are present in the water (5 times 18 units equals 90 units) will be regarded as surplus.[2] Notice that this approach to the determination of the magnitude of contribution of each of the factories makes sense only because we assume that there is a definite answer

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[2]Or, if we interpret the words 'the river's pollution' as referring to the extent of pollution, this surplus will figure into our calculation of the total amount contributed by each factory to that state of affairs.

to the question, when did the river begin to be polluted or, at what point in time did the river become polluted? If we did not accept the validity of this question it would be ridiculous to try to assess the contribution of some factor to the occurrence of the effect. Corresponding to this assumption, I want to suggest, is another one to the effect that each act of dumping took time, that is, we must assume that there exists some time interval between the beginning of each act and its completion. No doubt the interval separating the precise instant when the first unit of mercury entered the water and the precise instant when the last unit did so will be quite small. Nevertheless, we must assume the existence of such an interval if we are to specify the exact point in time when the river became polluted. Moreover, however small this interval may be, we can divide it into sub-intervals.

III

Once we make explicit these assumptions our account of Frey's five-factory example would be as follows: If five factories simultaneously dump 20 units of mercury into a river at T, and it was not until T_{10} that the last unit entered the river, we can say that at T_1 10 units of mercury were present in the river, at T_2 20 units, ...at T_{10} 100 units. Consequently, for there to have been 10 units of mercury in the river at T_1 each factory must have contributed 2 units by that time, which is of course 10% of its total contribution. Once we view this case of alleged overdetermination in this way we can see where Frey has gone wrong. For although it may be true that 'any single one of the acts on its own more than suffices to pollute the river', the conclusion drawn from this premiss, that we cannot

'regard each of the five acts of dumping as part of a sufficient condition for pollution' (p. 111) is a non sequitur. For if it were the case that at time T only one factory dumped 20 units of mercury into the river, the river would become polluted only at T_5, whereas if all five do so at T the river would have become polluted earlier (at T_1). Thus, although all five acts of dumping considered together will be sufficient for 'the river's pollution', as will any single act of dumping, they are not sufficient for the same temporally dateable effect.

The same strategy can be employed to show that each act of dumping was necessary for the effect to have occurred when it did, that is, that each act of dumping was 'necessary in the circumstances'. If we eliminate any one of the five acts of dumping it may be the case that 'the river would still have been polluted', but not at the precise point in time at which it did become polluted when all five factories simultaneously dumped their 20 units of mercury. Remember, if all five factories dump their portion of mercury into the river, the river would become polluted at T_1. On the other hand, if only four of the factories simultaneously dumped their 20 units of mercury there would only be 8 units of mercury present in the river by T_1. Hence, in the absence of any one of the five acts of dumping the effect would have occurred later than it did when all five factories dumped their 20 units of mercury into the river.

IV

The way that I have developed Frey's example of causal overdetermination shows that a conditional analysis need not produce counterintuitive results when applied to cases of causal overdetermination.

As long as the effect can be broken down into parts, the magnitude of contribution of each of the factors determined, and the effect given a precise temporal description, we can always speak of a given factor as both necessary and part of a sufficient condition for the effect. I conclude, then, that Frey has failed to establish his argument against the application of conditional analyses in cases of causal overdetermination.

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ABSTRACT

The subject of this thesis is the nature of the cause-conditions distinction and the role it plays in the causal explanations given by historians. In order to clarify these issues a historical overview of the claims of those authors who have addressed these is presented. This survey begins with J. S. Mill's stated view that the cause-conditions distinction is an arbitrary one and his challenge to others to find an objective basis for drawing this distinction.

Against this background proposals made by C. J. Ducasse, R. G. Collingwood, and H. L. A. Hart and A. M. Honoré were examined in great detail, for these authors claimed to have identified objective principles for drawing the cause-conditions distinctions. The most important result of this analytical survey is the observation that many singular causal judgments that distinguish cause from conditions are based on the Method of Difference, a method which was first codified by Mill. Ironically, then, Mill himself provided an objective basis for drawing this distinction. In addition to providing the basis for many singular causal judgments it was also seen that this method supplies the warrant for judgments of causal importance; i.e. judgments that state that one contributory cause was more important than another in bringing about a certain result.

Given this conception of the cause-conditions distinction and judgments of causal importance a number of examples taken from political history, social history, economic history, and the history of science were shown to conform to the requirements of the Method of Difference. Despite the fact that many of the causal judgments

historians make have an objective basis there are still many others that are subjective in character, that is, what is singled out as the cause of an event ultimately depends on the inquirer's own values. Historical examples that illustrate this point are also examined. Accordingly, this thesis supports the view that some singular causal judgments are objectively based, others are not.