The Anatomy of Historical Knowledge

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Notes

CHAPTER ONE


2. The conflation of these distinct questions is apparent in the first two sentences of Hempel's paper, which read as follows: “It is a rather widely held opinion that history, in contradistinction to the so-called physical sciences, is concerned with the description of particular events of the past rather than with the search for general laws which might govern these events. As a characterization of the type of problem in which some historians are mainly interested, this view probably can not be denied; as a statement of the theoretical function of general laws in scientific historical research, it is certainly unacceptable.” For Hempel's criticism of my claim that the primary aim of the historian lies “not in the formulation of laws of which the particular case is an instance, but in the description of the events in their actual determining relationships to each other,” see the footnote to section 7.4 of his paper, in which the two issues are again identified.
Hempel’s paper was originally published in the *Journal of Philosophy* in 1942; it is reprinted in his *Aspects of Scientific Explanation* (New York: Free Press, 1965).

3. For a general discussion of the problem of explaining cultural traits in terms of diffusion or in terms of independent origins, as well as for a discussion of the phenomenon of convergence among cultural traits, the reader can consult A. A. Goldenweiser, *Anthropology* (New York: Crofts, 1937), chaps. 28, 29.

On the other hand, those who wish a concrete example in which relatively sophisticated generalizations play a part in the actual work of a historian can consult H. R. Trevor-Roper’s essay “The European Witch-craze in the Sixteenth and Seventeenth Centuries,” which was originally published in his *Religion, the Reformation and Social Change* (London: Macmillan, 1967). Whether or not one agrees with his interpretation, Trevor-Roper’s interest was that of a historian but his method included the undisguised use of a number of sociological and psychological hypotheses.

4. I here paraphrase the position of Charles A. Beard in “Written History as an Act of Faith”; a similar position was adopted by Carl L. Becker in “What Are Historical Facts,” and elsewhere. These essays are readily available in Hans Meyerhoff, ed., *The Philosophy of History in Our Time* (Garden City, N.Y.: Doubleday Anchor, 1959). (For the relevant passages, see pp. 140 and 124, respectively.) The same point of view had earlier been espoused by James Harvey Robinson in his essay, “The New History” (1912), where he said: “In its ampest meaning History includes every trace and vestige of everything that man has done or thought since first he appeared on earth” (*apud* Fritz Stern, *The Varieties of History* [New York: Meridian, 1956], p. 258).

R. G. Collingwood adopted a parallel position, defining that which serves as the object of history as “res gestae: actions of human beings that have been done in the past” (*The Idea of History* [Oxford: Clarendon Press, 1948], p. 9). So long as any action, no matter how trivial, can be the object of the kind of inquiry carried on by a historian, Collingwood held that it is not be excluded from the domain of history. This is clear in his *Philosophy of History* (Historical Association Leaflet no. 79, 1930), where he said that the question of who played center forward on a village soccer team is as much a historical question as who won the battle of Cannae.


Croce also uses the terms “special histories” and “general history” (*History, Its Theory and Practice* [New York: Harcourt, Brace, 1923], pt. 1, chap. 8, and app. 2). While there are points of contact between his usage and mine, his conclusions are entirely different from those I draw.

7. For example, Herskovits says, “A culture is the way of life of a people; while a society is the organized aggregate of individuals who follow a given way of life. In still simpler terms, a society is composed of people; the way they behave is their culture” (*Man and His Works* [New York: Knopf, 1948], p. 29). Cf. Kluckhohn, “The Concept of Culture” (1945), reprinted in his essays *Culture and Behavior* [New York: Free Press of Glencoe, 1964], in which note especially p. 21.

8. For example, Clifford Geertz distinguishes culture as a system of ideas from the economic, political, and social relations in a society, all of which are to
some degree informed by that system of ideas (The Interpretation of Cultures [New York: Basic Books, 1973], p. 362.) Such a system of ideas is characterized by him as "an historically transmitted pattern of meanings embodied in symbols, a system of inherited conceptions expressed in symbolic forms by means of which men communicate, perpetuate, and develop their knowledge about and attitudes toward life" (ibid., p. 89). I find this type of definition too restrictive, since I find it essential, as do many anthropologists, to view language, tools, and crafts as aspects of culture, no less than are, say, religious beliefs.

Kroeber and Parsons, in an attempt to clarify the relationship between the terms "culture" and "society" (or "social system"), also tended to identify culture with values and ideas, though they added to their definition a reference to artifacts ("The Concepts of Culture and of Social System," American Sociological Review 28 (1958): 582–83). It is to be noted, however, that this represents a major departure from Kroeber's earlier view as stated in sections 6 and 117 of his Anthropology, new ed., rev. (New York: Harcourt, Brace, 1948).

9. In the opening paragraph of his Primitive Culture (London: J. Murray, 1871), Tylor used the term "culture" to refer to "all capabilities and habits acquired by man as a member of society" and he included in his discussion all artifacts resulting from those capabilities and habits. (Cf. chap. 7, "Growth and Decline of Culture" in his Researches into the Early History of Mankind [London: J. Murray, 1865].) Partly as a result of his evolutionary views regarding societies, he spoke of "culture" and not of diverse, particular "cultures." While not sharing his views on social evolution, I too shall use the term in a generic sense and shall define it as including artifacts as well as ideas and values. In each of these respects, my view is similar to that to be found in Kroeber's Anthropology.

10. I quote the relevant passages from "Das Fach 'Geschichte' und die historischen Wissenschaften," Hamburger Universitätsreden no. 25 (1959):


In contrast, Brunner characterized the special forms of historical study in the following way:

Das primäre, zentrale Objekt der historischen Fachwissenschaften ist eben nicht der Mensch und die menschlichen Gruppen, sondern dessen Werke. Hier werden Institutionen, Rechts- und Wirtschaftsordnungen, religiöse und philosophische Lehrmeinungen, Werke der Kunst und der Literaturen, die Sprachen und vieles andere, zuerst einmal abgehoben von ihren Trägern, als Sinngebilde untersucht, interpretiert und dargestellt. [pp. 25–26]


12. Biographies constitute a special form of historical account, in which the interest is focused on the person who is the subject of the biography. Bio-
graphical studies do not, in most instances, provide an exception to what I have emphasized, since the persons chosen for biographical study are, in general, persons in whom there is interest because of their roles in a particular society or because of their relation to some aspect of its culture. In some cases, however, a person may be the subject of a biographical study simply because of his character, rather than because of his relation to his society or to the culture of his time. Such studies can contribute indirectly to an understanding of the past, but their primary function is likely to be either psychological or (taking the term in its literal and nonvidious sense) hagiographic.

A point of view diametrically opposed to that adopted here is to be found in Frederick A. Olafson’s article “Human Action and Historical Explanation,” in New Essays in Phenomenology (ed. James Edie [Chicago: Quadrangle Books, 1969]). Olafson regards the subject matter of all historical accounts as being human actions, and finds the structure of these accounts to be dictated by the manner in which we analyze such actions.

A similar position is implicit in G. H. von Wright’s Explanation and Understanding (Ithaca: Cornell University Press, 1971), chap. 1 and 4 (especially pp. 137–39). In assessing von Wright’s position, it is to be noted that he confines causal explanation to discovering connections between generic characteristics of events or states of affairs, rather than in an analysis of the sufficient conditions of individual occurrences (cf. pp. 38, 43, and 74).


14. In “Central Subjects and Historical Narratives,” History and Theory 14 (1975): 253–74, David L. Hull holds that one should not regard historical narratives as being concerned with a series of connected events but with historical entities which have unity and continuity over time. Insofar as general history is concerned, I find myself in agreement with much that he says. However, I do not find him convincing when he holds that this is also true of all histories. In fact, his basic contention that biological species are to be considered as unitary continuing entities seems to me open to doubt.

15. For a stimulating discussion of periodization in relation to chronological time, see Kracauer, History chap. 6.

CHAPTER TWO


Neither these articles nor other formulations of a narrativist position by A. R. Louch in History and Theory 8 (1969): 54–70, and by Haskell Fain in Between Philosophy and History (Princeton: Princeton University Press, 1970), have led me to alter the arguments I used. I find support for some of my arguments in an article by C. B. McCullagh in Mind 78 (1969): 256–61. In the balance of this discussion, my criticisms of the narrativist position are retained, but the wider framework within which they are placed may perhaps make them somewhat more acceptable to my critics.

2. To be sure, if an author writes a historical novel, or seeks historical
material as background for his fiction, he will have to engage in preliminary research, and what he writes will have to be compatible with what he has learned. However, his research will not dictate the structure of the story he has chosen to tell; therein lies an essential difference between history and historical fiction.

3. This point is also made by Gordon Leff in *History and Social Theory* (University: University of Alabama Press, 1969), when he says: “The historian methodologically works back from the problem which he has already identified to the circumstances which led up to it” (p. 96) and, again, “Historical explanations begin from what is to how it became such” (p. 100).

The point is also made by Paul K. Conkin in “Causation Revisited,” *History and Theory* 13 (1974): 4. He says of the strength of the implicative relationship in causal attributions: “All the strength is retrospective, but so is the vision of the historian. In his causal analysis, if not in his narration, he usually moves back from a significant but somehow puzzling event (the effect) to search out some of its causes, often guided by a detailed description of the event.”

4. That Young’s aim was to interpret, and not to recount or explain, can be clearly seen in a remark he makes in the introduction to the 1953 edition of *Victorian England: Portrait of an Age* (London: Oxford University Press, 1953): “the real, central theme of History is not what happened, but what people felt about it when it was happening.” Burckhardt’s interest in treating the Renaissance in Italy as a single “geistiges Kontinuum,” which it was his purpose to reveal through treating its various facets, is clear in the very first paragraph of his book.


It is also to be noted that in the concluding section of a paper entitled “On the Nature and Role of Narrative in History,” Dray discusses the fact that such a view is implicit in the claims of Gallie and Danto, and is explicit in Oakeshott; he expresses his own strong sympathy with it (*History and Theory* 10 (1971): 169 ff.).


9. For some examples of how economic theory may be important in explaining particular historical changes, see A. G. L. Shaw, “Economics and History,” *Historical Studies. Australia and New Zealand* 3 (1949): 277–86. For a more radical approach to the problem, the example of Robert W. Fogel and his fellow “cliometricians” is to be noted.

10. A suggestive example of how one’s definition of literature influences the organization of a literary history is to be found in chapter 1, “Prospective,” of Oliver Elton’s *Survey of English Literature, 1730–1780* (New York: Macmillan, 1928). Particularly relevant is the role that actual speech plays in his definition of literature (pp. 2–3).

11. In what follows, I am confining my attention to the problem of selection insofar as it concerns forms of special history; I shall not consider the question of importance as it arises in other contexts. For example, the term is sometimes connected with the problem of weighing causal factors; sometimes it is identified with whatever criteria a historian may use in selecting a subject matter, and whatever principles of inclusion or exclusion he applies in dealing with that subject. For a discussion of the former problem, see the concluding section of Ernest Nagel, “Some Issues in the Logic of Historical Analysis,” which appeared in *Scientific Monthly* in 1952, and is reprinted in Patrick Gardiner, ed., *Theories of History* (Glencoe, Ill.: Free Press, 1959); for a suggestive treatment of the second issue, see W. H. Dray; “On Importance in History,” in *Mind, Science, and History*, which is volume 2 of *Contemporary Philosophic Thought: The International Philosophy Year Conferences at Brockport* (Albany: State University of New York Press, 1970).

12. In chapter 1, I called attention to the parallel between the task of a historian in tracing influences and that of the anthropologist in assessing the diffusion of cultural elements, and I shall not labor the point. The anthropologist, of course, suffers under the disadvantage of not having written history on which to rely, and it is therefore especially difficult for him to determine what may have been the point of origin of a particular cultural element and in what direction it spread. Goldenweiser’s discussion of the problem, which I cited, affords a useful contrast to the manner in which a literary historian is generally able to proceed.

13. The same situation obtains with respect to the works of any individual author: They are not to be viewed as forming a single linear series in which each work grew only out of the work that preceded it. From work to work an author’s experience will have changed, his interest may have been caught by what others were doing, or he may have felt that he had exhausted the vein in which he had been working. These and many other factors may have to be taken into account in order to interpret the relationship between any individual work and the author’s life or his work as a whole.

14. What obtains generally with respect to interpretive disagreements applies equally to debates concerning such issues as “the causes of the Civil War,” which are not in fact conflicts between differing *explanatory* historical accounts, but between rival interpretive accounts (see Appendix B).

CHAPTER THREE

1. While Bertrand Russell was among the most influential figures in standardizing the prevailing view, it was also a view shared by all positivists. In addition, it was accepted by a number of antipositivist American philosophers, such as J. B. Pratt and Morris R. Cohen.

Among those who rejected this view, Emile Meyerson was for a time the most influential. The view was also repeatedly challenged by idealists, some of whom (e.g., A. C. Ewing and Brand Blanshard) claimed that logical necessity is involved in the causal relationship. Among other forms of challenge, mention should be made of Whitehead’s attack on the Humean analysis and C. J. Ducasse’s systematic attempt to defend an alternative position in *Causation and Other Types of Necessity*, University of Washington Publications in the Social Sciences, vol. 1, no. 2 (Seattle, 1924). However, none of these succeeded in leading to any basic revision of the dominant view, though Ducasse’s position has recently become better known and more widely discussed since its reformu-


4. It is to be noted that both J. L. Mackie and K. Marc-Wogau also insist that one should distinguish between the causal explanation of a particular case and generalizations concerning cases of a particular type, but do not find that the differences between explanations in the sciences and elsewhere involve different conceptions of the causal relation. See Mackie, *Cement of the Universe*, pp. 121 and 270–71 for summary statements, and Marc-Wogau, “Historical Explanation,” *Theoria* 28 (1962): 214 and 215–16.

It is also to be noted that attempts to draw a sharp contrast between history and the sciences is mistaken in still another respect: In some natural sciences, such as geology, inquiries into temporal sequences in the past are of great theoretical importance, providing an indispensable complement to those non-temporal, functional generalizations with which scientists are primarily concerned. T. A. Goudge has made this point clear in an article that has important consequences for understanding historical explanation: “Causal Explanations in Natural History,” *British Journal for the Philosophy of Science* 9 (1958): 194–402.

5. In the recent literature of our subject, one of the first to point this out was Michael Scriven. He makes the point especially clearly in “Causes, Connections and Conditions in History,” first presented at a conference at the University of Cincinnati, and later published in W. H. Dray, ed., *Philosophical Analysis and History* (New York: Harper & Row, 1966). See especially pp. 242–43.

6. To avoid misunderstanding, I wish at the very outset to say that when I speak of someone’s “perceiving” a causal connection it is possible that the person is mistaken, and that such a connection does not exist. To find examples of this one need merely recall one's experience in watching a skillful magician.

7. For example, I assume that even an uninstructed spectator, who did not know the rules of football, would see a difference between the behavior of the players between downs and their behavior during a play, recognizing the latter as a single and unified event in a way in which the intervals between downs, or in a time-out period, are not.

One aspect of an experiment performed by Heider and Simmel may be relevant here. They designed a movie that has as its “characters” two triangles, differing in size, and a circle. These three figures moved in and around a rectangle through an aperture that opened and shut. In general, all subjects interpreted the figures in human terms, and interpreted the rectangle as a house. In the present context, what is relevant about this experiment is that, within the temporally continuous film, particular sets of motions of the figures became segregated as belonging together, with the total flow of motions being seen neither as a disconnected sequence nor as a single event; rather, it was seen as a series of episodes punctuating a flow of action. This is not a point brought out by Heider and Simmel, but as an observer of the film I was particularly struck by it, and my present description of the phenomenon conforms to their account of what their first (uninstructed) group of subjects reported (Fritz Heider and Marianne Simmel, “An Experimental Study of Apparent Behavior,” *American Journal of Psychology* 57 [1944]: 243–59).

In this particular paper Heider and Simmel discuss the perception of causation in the film, giving an account very similar to that which has been more fully developed through the Michotte experiments to which I shall later call attention.
8. In this case I must remind the reader that we are here concerned with causal beliefs in everyday life; we are not dealing with the explanations of the motions of the billiard balls that a physicist might give. We shall come to such explanations later. I am here confining myself to what is seen when we say that we saw that a billiard ball moved because it was hit by another.

9. I must repeat that I am not claiming that all cases in which the cause-effect relationship is applied to events in everyday life are of this kind. As we shall see, many conform to the regularity model. I am now concerned only with those in which we believe that we do actually see the connection between cause and effect. Furthermore, I must once again say that I am not now discussing how, if at all, necessity may be involved in causal connections.

10. In each of the preceding illustrations I have been concerned with cases in which the effect is taken as the end point in a process, but we can ask causal questions concerning continuing processes: Why does the top continue to spin, or the engine continue to run? In these cases, too, our causal attributions refer to what is occurring within the process designated as the top's spinning or the engine's running; it is not some prior event.

11. The following general statement made by A. Michotte, the Belgian psychologist, concerning his experiments on the perception of causation, is relevant here: "It is true that by an analytical and abstract approach such as I have adopted above, it is possible, theoretically, to distinguish two successive events, the movement and the contact. But actually there are not two events; there is only one event which develops progressively. As we shall see later, the impact is not really limited to the coming into contact of the two objects; it constitutes a whole process, of which the movement and the contact are both constitutive parts. . . . The whole is one gradual development" (The Perception of Causality [London: Methuen, 1963], pp. 24–25).

Given this statement and the foregoing analysis of cases in which we may be said to perceive causal connections, it should be clear that these cases bear a greater resemblance to the ways in which historians are apt to use the concept of causation than a Humean analysis provides. Thus, the present discussion prepares the way for what is to be said in chapter 5.


13. It is to be noted that Hume himself used the resemblance of successive impressions to explain our belief in the continuing identity of objects. I am here using a similar factor to explain continuity in events.

14. In a quite different connection, Bertrand Russell speaks of similarity of structure as giving us warrant to infer causal relations. Although his defense of this inductive postulate depended upon considerations of probability and not upon perceptual belief, in both cases the influence of our recognition of similarity in structure is a factor that leads us to regard different events as belonging together in a connected series. (See Russell, Human Knowledge, Its Scope and Limits (New York: Simon and Schuster, 1948), pt. 6, chap. 6; pp. 460–64 are particularly relevant.)


16. Analogues to these "explanations" are to be found in many historical works. I find them unsatisfactory, both in history and in everyday life, since the explanation is not couched in terms of actual events, but in terms of what is only a pattern running through them. When such a pattern is particularly striking it is apt to be reified. Instead of being viewed as what results from these events in their interrelationships, it is taken as if it explained them. See chapter 5 for a further discussion of this.

17. If there is no rhythmic pattern, and they bear a fixed spatial relationship to
each other, they will be seen moving as a pair, as if they were rigidly connected. (For example, see Michotte's discussion of the difference between displacement and what we experience as movement, in *Perception of Causality*, pp. 315–16.)

18. This question is, of course, a psychological question. In attempting to suggest a psychological explanation of the origins of our causal beliefs, I am merely following the practice of Hume.

19. In his experiments on "qualitative causation" (as distinct from "mechanical causation") Michotte reached an essentially negative conclusion: There was no perception of causation unless a connection was seen between the movement of one object and the movement (or change of shape) of another. I am not denying this negative conclusion. When we switch on a light we cannot claim to see the linkage between what occurs at the switch and the fact that the light comes on; it is only past experience that binds these two events directly together. Thus, this is not the sort of example with which Michotte's experiments were concerned: He attempted to find cases in which, independent of past experience, there was a direct perception of some factor linking cause to effect.

20. Even from a phenomenological point of view, the two events do not appear as instantaneous. The motion involved when we switch on a light does not appear to us as instantaneous. Even the light's coming on does not, under scrutiny, always appear instantaneous, as those familiar with "gamma movement" will recognize.

21. Another condition that leads us from one specific case to cases of a particular type is when someone asks us to validate our belief that a causal connection did in fact obtain in some particular instance. I am not here dealing with the problem of validation, but only with what is initially involved in our ascriptions of causal relationships. In this connection I might point out that in two passages in which C. G. Hempel criticized my views regarding causation, holding that the causal relation refers to types of cases and not individual instances, his argument hinged on the issue of validating our original causal ascriptions. (See "The Function of General Laws in History," as reprinted in Hempel, *Aspects of Scientific Explanation* [New York: Free Press, 1965], pp. 233 n. and 241 n.)

22. This is not to say that lawyers and historians ordinarily use this form of explanation in dealing with the instances of causation with which they are concerned. As I shall note in Appendix B, Hart and Honoré may have too readily assimilated common-sense views with the manner in which causation is used among lawyers. I shall also argue that they have erred in assuming that the procedures of historians are similar to both.

23. Conversely, of course, the onset of a storm can serve as an indicator that changes in barometric recordings have occurred, but for pragmatic reasons we are rarely interested in tracing this particular relationship. Barometers are generally of interest only because they forewarn us of changes in the weather that we do not yet directly detect, or because they help us evaluate other signs that the weather may change.

24. Such cases bear a close resemblance to the cases in which, as I pointed out in the preceding section, what is given in direct experience appears as having a definite rhythmic pattern, and we feel the presence of some underlying but not directly experienced cause.

25. On the other hand, as I have pointed out, there are also many instances in which a Humean model of explanation seems to serve our purposes adequately in ordinary life.

26. The term "mana" entered anthropological literature through R. H. Codrington's *The Melanesians* (Oxford: Clarendon Press, 1891), and became extended in its application by R. R. Marrett's article on "Mana" in Hastings'
Encyclopedia of Religion and Ethics (Edinburgh: T. & T. Clark, 1908–26), and by the writings of Durkheim, Lowie, and others. Alternative interpretations of the concept and of the phenomena to which it was used to refer were closely tied to alternative theories of magic and religion in primitive societies. At present a somewhat restricted interpretation appears dominant (see E. E. Evans-Pritchard, Theories of Primitive Religion [Oxford: Clarendon Press, 1965], p. 110). For my purposes, however, where the issue is one of causal efficacy and not a theory of religion, and where theoretical issues concerning the role of magic in primitive communities do not arise, it is presumably legitimate to use the term in its more extended sense. For one standard work on mana, see F. R. Lehmann, Mana: Der Begriff des "außerordentlich Wirkungsvollen" bei Sudseevolkern (Leipzig; O. Spamer, 1922).


27. For example, Hume says in An Inquiry Concerning Human Understanding, sec. 7, pt. 2: "One event follows another, but we never can observe any tie between them. They seem conjoined, but never connected" (Selby-Bigge edition [Oxford: Clarendon Press, 1966], p. 74).

28. It is in such cases that Hume speaks of "secret powers." See Inquiry Concerning Human Understanding, sec. 4, pt. 2, pp. 32–34.

CHAPTER FOUR


2. In his well-known article "Causal Relations" (Journal of Philosophy 64 [1967]: 691–703), Donald Davidson was also concerned with causal explanations of particular events, and in this connection he cited a similar example. He was, however, critical of Mill's inclusion of all relevant conditions as elements in the true cause of a particular event. However, that point was not essential to the main thrust of his argument (see p. 692).

3. In this connection it is apposite to cite J. L. Mackie's defense of Mill against the criticisms made by Hart and Honoré. Speaking of what Mill termed "the philosophical view" of causation, in which any distinction between "cause" and "mere conditions" disappears, Mackie says: "Since what we recognize as a cause, rather than a mere condition, commonly depends on what we know—or what we knew first—or what is closely related to our interests, there is much to be said for Mill's refusal to distinguish 'philosophically speaking' between causes and conditions. As an analysis of ordinary language, this would be wrong; but from a theoretical point of view, as an account of causal processes themselves, it would be right" (The Cement of the Universe: A Study of Causation [Oxford: Clarendon Press, 1974], p. 120).

4. Mill offers an account of why we single out some event as the cause of an effect, and why, in contrast, we designate continuing states as being merely attendant conditions. He says of the latter that they 'might therefore have preceded the effect by an indefinite length of duration, for want of the event which was requisite to complete the required concurrence of conditions: while as soon as that event . . . occurs, no other cause is waited for, but the effect begins immediately to take place: and hence the appearance is presented of a more immediate and close connection between the effect and that one antece-
dent, than between the effect and the remaining conditions. But though we may think proper to give the name of cause to that one condition, the fulfillment of which completes the tale, and brings about the effect without further delay; this condition has really no closer relation to the effect than any of the other conditions has. The production of the consequent required that they should all exist immediately previous, though not that they should all begin to exist immediately previous" (J. S. Mill, A System of Logic, bk. 3, chap. 5, sec. 3).

(For this variant of the passage, see Collected Works of J. S. Mill [Toronto: University Press, 1973], 7: 328.)

5. C. J. Ducasse, Nature, Mind, and Death (LaSalle, Ill.: Open Court, 1951), p. 108. This volume contains Ducasse's fullest explanation of his theory of causation, but (as I have pointed out) it can be supplemented by an earlier monograph, Causation and Other Types of Necessity, University of Washington Publications in the Social Sciences, vol. 1, no. 2 (Seattle, 1924), and by his later collection of studies, Truth, Knowledge, and Causation (London: Routledge & Kegan Paul, 1968).

6. This passage comes from the 14th edition, s.v. "tuberculosis." The account of the pathology of tuberculosis given in the 15th edition is more detailed, but is entirely compatible with the point here being made. Equally clear is the account in the American Encyclopedia, where the etiology is discussed in terms of the host-parasite relationship.

7. In a different context (namely, in attacking the notion of the "accidental" in history), Michael Oakeshott makes a similar point (cf. Experience and Its Modes [Cambridge: At the University Press, 1933], p. 140; also, pp. 129 and 142).

Alexander Gerschenkron, the economic historian, distinguishes between what he terms "nonfacts" and "counterfacts" (Continuity in History and Other Essays [Cambridge, Mass.: Harvard University Press, Belknap Press, 1968], pp. 53-54). If I understand him correctly, this parallels the distinction I here wish to draw.


9. The most influential statement of the supposed difficulty has probably been that of Bertrand Russell in his essay "On the Notion of Cause," in Mysticism and Logic (New York: W. W. Norton, 1929), pp. 184-85.

10. It is obvious that in this discussion, and elsewhere, I have been assuming that the notion of what constitutes "a process" is not hopelessly obscure. I do not believe that it is, and I see no reason to think that it raises special metaphysical difficulties that are not paralleled in those cases in which we claim to know what we mean by "an object," "an event," "a state of affairs," etc.

At this point I should also indicate that my present account of the cause-effect relation differs from the manner in which I stated my position in The Problem of Historical Knowledge (New York: Liveright, 1938). There I spoke of "events" and "sub-events" rather than speaking of the end point in a process and what led up to it. In correspondence and conversations (long ago), Hugh Miller, formerly of the University of California at Los Angeles, pointed out to me some difficulties inherent in my earlier formulation. I should like to think that my present way of formulating my views of the relation between cause and effect has overcome those difficulties without giving up what was basic in my earlier position, and without having engendered serious new difficulties.


12. If I understand him correctly, this point would be endorsed by Wesley Salmon, who, in discussing his own and Reichenbach's view of causal relevance, says: "One very basic and important principle concerning causal relevance . . . [is] that it seems to be embedded in continuous processes" ("Theoretica Explanation," in Stephan Körner, ed., *Explanation* [Oxford: Blackwell, 1975], p. 132).


17. For example, in Hempel's illustration we must exclude the possibility that the owner will start his car in order to warm its engine, or that he will add an antifreeze solution as the temperature drops to the freezing point. The sequential aspect of the series of occurrences would surely not be denied by Hempel, even though this aspect does not become apparent in his schematic account of how initial conditions and laws are used in explaining a given effect.


19. To relate this and what follows to more technical discussions of the issue involved, I find myself in agreement with J. L. Mackie when he characterizes the various factors entering into the cause of an effect as being what he terms "inus conditions," and also with what I take to be Michael Scriven's meaning when, in reviewing Ernest Nagel's *Structure of Science*, he says that causes are "contingently sufficient." I shall quote the summary statements given by each.

Mackie says: "In the case described above the complex formula '(ABC or DGH or !KL)' represents a condition which is both necessary and sufficient for *P*: each conjunction, such as 'ABC,' represents a condition which is sufficient but not necessary for *P*. Besides, ABC is a minimal sufficient condition: none of its conjuncts is redundant; no part of it, such as AB, is itself sufficient for *P*. But each single factor, such as A, is neither a necessary nor a sufficient condition for *P*. Yet it is clearly related to *P* in an important way: it is an insufficient but non-redundant part of an unnecessary but sufficient condition: it will be convenient to call this (using the first letters of the italicized words) an inus condition" (*Cement of the Universe*, p. 62).

In holding that causes (by which he means specific conditions) are "contingently sufficient," Scriven says: "They are part of a set of conditions that does not guarantee the outcome, and they are non-redundant in that the rest of this set (which does not include all other conditions present) is not alone sufficient for the outcome" (*Review of Metaphysics* 17 [1964]: 408).

Scriven then introduces five refinements of this account (pp. 408–12).

20. In "Causation" (Monist 47 [1963]: 287–313), Richard Taylor takes a different position. He argues that each of the conditions that, together, are necessary for the production of a given effect must also, when taken individually, be regarded as necessary.
CHAPTER FIVE

1. Experience and Its Modes (Cambridge: At the University Press, 1933), p. 141.


3. Experience and Its Modes, p. 127.


5. Dray concludes an article entitled “On the Nature and Role of Narrative in Historiography,” in History and Theory 10 (1971): 153–71, with a section contrasting Mink with Oakeshott, and with other narrativists; he, too, finds Mink’s position on this point especially congenial.

In the Review of Metaphysics 21 (1967–68): 667–68, Mink reviewed the books of Gallic, White, and Danto, but in that essay he did not develop his own views beyond what was contained in his earlier article.


7. See “Causes, Connections and Conditions in History,” in Dray, Philosophical Analysis and History, pp. 242–43. Scriven, however, does not speak of the part-whole relationship between any one element in the causal conditions and the total effect; rather, he speaks of the cause as “physically identical and only conceptually distinct” from the effect.

8. There are, of course, other biographical studies, such as literary or scientific biographies, in which some facet of culture, rather than the institutional aspects of a particular society, provide the primary context in terms of which the life and work of an individual are viewed. Some biographies, depending upon the career of their subject, may fuse these interests; others may focus on the subject in relation to an interpretive thesis regarding the characteristics of the period in which he lived.

9. In contrast to this, as I pointed out in chapter 2, a special history involves a historical study of some type of cultural product, tracing the connections and the changes in examples of it; it is not the primary task of special histories to deal with the nature and changes of the society or societies that either produced or have preserved these products.


11. I might here point out that any events prior to the particular series of events under investigation are not to be viewed as determining its characteristics, except indirectly. Thus, while they may be part of the cause of its cause, they are not to be included among the causal features responsible for it.

12. This was a relatively common point of view among social psychologists, of whom William McDougall was one. Among social anthropologists one finds it represented in B. Malinowski, A Scientific Theory of Culture (Chapel Hill: University of North Carolina Press, 1944), though not in his earlier works.

In three articles to be cited in n. 30, below, I have argued against a related view, “methodological individualism,” which holds that societal facts are
to be understood and are to be explained in terms of the behavior of individuals.

13. This remark also applies to most attempts to use psychoanalytic theories in historical explanations. While some historians hold that such theories may be fruitfully applied both in biographical studies and in explaining the persistent patterns of action of various political figures, many of the actions with which historians are concerned involve the decisions of individuals concerning whom there is insufficient knowledge to warrant a concrete application of psychoanalytic theory.


What has been said may also remind the reader of Michael Scriven's thesis that the generalizations historians use are "truisms." (Cf. "Truisms as the Grounds for Historical Generalizations" in Patrick Gardiner, ed., *Theories of History* [Glencoe, Ill.: Free Press, 1959], pp. 443–75.) However, Scriven interprets these truisms as relating to what individuals *normally* do, and he therefore calls them "normic generalizations." I do not believe that, as a rule, the generalizations historians use are meant to refer to what is normal behavior, or to what normally happens, in either the statistical or the quasi-normative sense of the term "normal." Therefore, in spite of a superficial resemblance between what I here say concerning the looseness of the generalizations used by historians and the views brought forward by Scriven, I wish to separate my position from his.

15. In part, its importance for discussions of historical methodology lies in its ability to provide an alternative to Carl Hempel's view that because of the looseness of their generalizations historians offer only "explanation sketches," not explanations. (Cf. "The Function of General Laws in History," sec. 5.4, reprinted in *Aspects of Scientific Explanation* [New York: Free Press, 1965], p. 238.)

16. The only work I know that makes a serious attempt to define the parameters of this type of problem and to elicit conclusions is *Size and Democracy* by Robert A. Dahl and Edward R. Tufte (Stanford: Stanford University Press, 1973).


19. One of the most stimulating attempts to provide such a law seems to me to have been Robert Michels' development of Mosca's theories. According to Michels, there is a basic principle of political organization that— in a manner reminiscent of Marx—he called "the iron law of oligarchy" (Political Parties [New York: Free Press, 1966], pt. 6, chap. 2; cf. *First Lectures on Political Sociology* [Minneapolis: University of Minnesota Press, 1949], pp. 141–42). It was his contention that there was inherent in the necessity for political organization in society an inescapable tendency for control to be exercised by a minority group. He phrased this most concisely as follows: "Organization implies the tendency to oligarchy. In every organization, whether it be a political party, a professional union, or any other association of the kind, the aristocratic tendency manifests itself very clearly. The mechanism of the organization, while conferring a solidity of structure, induces serious changes in the organized mass, completely inverting the respective position of the leaders and the led. As a result of organization, every party or professional union becomes divided into a minority of directors and a majority of directed" (Political Parties, p. 32). In *Union Democracy* (Glencoe, Ill.: Free Press, 1956), S. M. Lipset, M. A. Trow,
and J. S. Coleman examined the International Typographical Union in an effort to explain why it, alone among labor unions, provided a conspicuous exception to Michels' iron law of oligarchy. (See their first chapter, "Democracy and Oligarchy in Trade Unions.") Their answer involved certain of the structural features of this union, but their explanation of the development and persistence of these features demanded an appeal to various historical occurrences: The cumulative effects of these occurrences changed the initial conditions present for each subsequent stage in the development of the union, thus negating the applicability of Michels' law. For their brief discussion of this basic theoretical point, see pp. 393–94 and 402–3.

While Michels' formulation and defense of his position was overburdened by a concern with problems of socialism and democracy in modern political life, and was therefore not formulated in terms that make it readily applicable to all forms of society, one can conceive of a more generalized statement of it which could be applied to all forms of organization and not to modern forms of political life alone.

I might add that one conventional generalization I have cited, concerning size of population and direct democracy, can itself be considered only a special case of Michels' "law," as he himself attempted to show in the chapter of Political Parties entitled "The Mechanical and Technical Impossibility of Direct Government by the Masses," from which the above quotation was taken. (See also pt. 5, chap. 1 of that work, on "The Referendum.") However, in his explanation of why the iron law of oligarchy holds, Michels offered two basic principles, only one of which depends upon "tactical and technical necessities"; the other rests on assumptions as to psychological changes that individuals undergo when they assume roles of leadership (Political Parties, pp. 400–401). Only the first of these, and not the psychological assumption, seems to me likely to have universal applicability.


Once this distinction is drawn, so that not all occurrences that may affect various aspects of life in a particular society are regarded as belonging within its own history, the temptation to view the whole human past as constituting a single history should disappear. However, idealists such as Oakeshott will, on metaphysical grounds, of course reject any such distinction between external and internal relations.

As we shall see, general histories that are primarily interpretive in structure fall between special histories and other forms of general history in this respect.

While the term "genre" is usually used only in referring to stylistic types in the arts, I believe that its use can be extended to other fields. One can, for example, say that different types of philosophic problems and different methods of approaching these problems resemble the diversity to be found in different literary genres or in different genres in the plastic arts. So, too, in the sciences there are many sorts of problems to be investigated, and there is also variety in the styles of investigation that different scientists follow. In different periods some types of problems and some styles of investigation may be more dominant than others, just as is true in the arts.

It is said—and probably correctly said—that in a primitive society (that is, in a nonliterate society) tradition is more rigorously followed and innovation is more restrained than in other societies; as a consequence, a greater degree of continuity in the culture of that society is to be expected than would otherwise be the case.

In addition, the greater the contact of a society with other societies, the more opportunity there is for cultural interchange and, therefore, the more are innovations and discontinuities likely to occur.

26. In this connection one may note that there is some evidence that economic factors, rather than the exploitation of new scientific discoveries, play a dominant role in developing major technological innovations. For a defense of this thesis, see Jacob Schmookler, *Invention and Economic Growth* (Cambridge, Mass.: Harvard University Press, 1966), especially chap. 3. In any such study there is, of course, the problem of defining “major technological innovations” in a way that does not prejudge the issue, and Schmookler’s case studies might be challenged on this ground. Nevertheless, his study calls attention to the role played by economic factors—as distinct from scientific advances—in fostering technological change.

27. I say “concrete explanations” since I think it entirely possible that one might be able to set up general principles relating to stylistic change in the arts, and perhaps in other fields of cultural history as well. Such principles might refer to changes in fashion due to factors such as the satiation of taste for a style when that style has been dominant for a time, or because of factors such as a tendency for styles to be vulgarized as their influence spreads. However, such general principles—if any are to be found—would only explain why there is change and would not concretely explain the direction in which the change took place.

28. In *English Literature in the Sixteenth Century*, C. S. Lewis made some very apposite remarks on how the word “Renaissance”—which was originally used by humanists conscious of living in a *renascenza* in which Greek was recovered and there was a revival of classical Latin—became debased by the inclusion of other elements which were quite independent of the new classical learning. As he said, “Where we have a noun we tend to imagine a thing. The word Renaissance helps to impose a factitious unity on all the untidy and heterogeneous events which were going on in those centuries as in any others. Thus the ‘imaginary entity’ creeps in. Renaissance becomes the name for some character or quality supposed to be immanent in all the events, and collects very serious emotional overtones in the process. . . . No one can now use the word Renaissance to mean the recovery of Greek and the classizing of Latin with any assurance that his hearers will understand him. Bad money drives out good” (pp. 55–56).

29. For example, this is the view of Melville Herskovits in his well-known text in social anthropology *Man and His Works* (New York: Knopf, 1948), pp. 21–28. For a philosophic discussion of “methodological individualism” and some of its alternatives, see “Holism and Individualism in History and Social Science,” by W. H. Dray, in the *Encyclopedia of Philosophy*.


our drama, but as the author of *Fulgens and Lucrece*, the first purely secular English play that has survived, he is a significant figure. . . . The discovery of this play in 1919 caused almost as much surprise as the recent discovery of an historical play dating from the great period of Greek drama" (p. 7). But Wilson immediately adds: "No doubt Medwall's play seems to us more original than in fact it was," and he traces a whole series of works of different types that also were popular in character and were both secular and quasi-dramatic. To this he adds the concluding comment, drawn from a remark of Sir Edmund Chambers: "So we build up the past." It is this that an experienced cultural historian is in a position to do, but the novice is not.

**CHAPTER SIX**

1. For both cognitivists and noncognitivists moral judgments are valid only if they are objective. For the distinction between the validity and the truth of moral judgments, see chapter 6 of my book *The Phenomenology of Moral Experience* (Glencoe, Ill.: Free Press, 1955).

2. In the concluding section of "Some Issues in the Logic of Historical Analysis," Ernest Nagel suggested various ways in which historians attempt to estimate the relative importance of two causal factors in a situation. I would be willing to accept each of the five ways Nagel differentiated, but I wish to call attention to the fact that he failed to include the type of assessment I have indicated. Were he to have included this type—but his general conception of causal explanation perhaps made it impossible for him to do so—his final position might have been less discouraging than it was. (His article was originally published in *Scientific American* in 1952; it is reprinted in *Patrick Gardiner*, ed., *Theories of History* [Glencoe, Ill.: Free Press, 1959], pp. 373–85.)

3. In the first three sections of chapter 25 of *The Open Society and Its Enemies* (Princeton: Princeton University Press, 1963), Karl Popper argues that theories have no place in history: There are at best general interpretations, which he also calls quasi-theories. This contention is based on two premises, each of which I take to be false. First, Popper assumes that a genuine theory can be formulated only if one has already established empirical laws, but, as I have indicated, this was not the case with Darwin's evolutionary theory. Second, Popper assumes that the laws upon which theories regarding history would necessarily depend will take the form of saying that, given an occurrence of type a, an occurrence of type b will always follow. However, as I have elsewhere tried to show, sequential laws of this type constitute only one of the kinds of laws that might be used in understanding societal change; yet it is the only type that Popper considers when he rejects the possibility of establishing laws concerning history. (See my article "Societal Laws," *British Journal for the Philosophy of Science* 8 (1957): 211–24; reprinted in W. H. Dray, *Philosophical Analysis and History* [New York: Harper & Row, 1966].)


5. The passage reads as follows:

It is always difficult, and frequently impossible, to bring to the point of inductive testing a system of explanation of a system of interrelated facts. "If Keynes was really to be successful" (remarks Sir Roy Harrod in his *Life of Keynes*) "he should have been able, it is argued, to refute, say, Mr. D. H. Robertson by showing a set of facts which the Keynesian doctrine would fit, while the other would not. Unhappily, the state of economics is not so advanced." Both of two alternative systems of explanation may fit most of the facts (and those it did not
--an adherent might hope--could be explained away with further research or reflection. In complicated disputes, it is not detailed theories, but rival attitudes to the world that are in collision. [Times Literary Supplement (London), 2 May 1975, p. 471]

6. To be sure, if one were to interpret Hegel's philosophy of history as being concerned only with the development of the notion of political freedom, and not with the growth and decline of those great societies whose destinies he traced, this stricture would not hold; however, such an interpretation of what his philosophy of history was about would seem to me untenable.

As to the views of Comte and Spencer regarding historical methodology, see my book History, Man, and Reason (Baltimore: Johns Hopkins Press, 1971), particularly pp. 88–89.

7. In my opinion, there can be relevant “outside” evidence for or against any form of general aesthetic theory: Appeal can be made to historical and psychological facts, as well as to phenomenological investigations, all of which can go far toward resolving such disputes. However, I shall not press the point here.


9. Philosophy of the Inductive Sciences (London: J. W. Parker, 1847), 2: 36. This part of that work was republished as Novum Organum Renovatum, and the passage occurs on p. 60 of the edition published by J. W. Parker in London in 1858.

10. Introduction to the Philosophy of History, p. 23.


13. Morton White’s treatment of colligation in The Foundation of Historical Knowledge (New York: Harper & Row 1965), pp. 252–54 and 257–64, is even more explicitly opposed to the possibility of objective historical knowledge, as I have defined that term.


CHAPTER SEVEN

1. Cf. chap. 2, sec. 3. What was said there holds not only of interpretive accounts of particular periods, my primary concern in that discussion, but holds also of interpretive biographies and interpretive studies of, for example, the works of an individual author. In this connection I might cite a passage from J. A. Passmore, “The Objectivity of History”:

Some little time ago, I wrote a book which purported to be an interpretation of Hume’s philosophy. One reviewer addressed me somewhat as follows: “a possible interpretation, but other interpretations are equally possible.” How is one to reply? . . . What happens is something like this: an interpretation is suggested by certain passages in Hume; that interpretation is then confirmed by passages I had
not previously so much as noticed, which the proposed interpretation serves to illuminate. Or I discover that passages I previously could not understand now make good sense. . . . If a reader is convinced of my interpretation, this will be because he has himself been puzzled by passages in Hume, and my interpretation solves his puzzle for him. [Philosophy 33 (1958): 106–7]

E. D. Hirsh’s bold yet careful study Validity in Interpretation (New Haven: Yale University Press, 1967) should be consulted by anyone concerned with problems in the authentication of interpretations.

2. To reply in advance to an apparent exception that might spring to mind, I remind the reader of what I said in Appendix B concerning the problem of “the cause of the American Civil War”: Affixing of praise or blame presupposes detailed, structured historical knowledge and is not itself an investigation of causes, as I use that term.

3. I have dealt with this at greater length in an article appearing in a Spanish symposium on the philosophy of history. In translation it was entitled “Historia y Universalidad” and it appeared in Revista de la Universidad de Madrid 12 (1963), no. 45.


6. In “Historical Explanation,” pp. 239–41, I criticized Dray in the same way, and developed the point at somewhat greater length. At that time, however, I failed to see—as I now see—that there are cases in which tracing a series of events, such as those constituting a campaign, may provide an adequate explanation of a particular state of affairs. However, I wish to emphasize that in such cases one must not assume (as Dray tends to do) that the relevant events form a single linear series. On this point, see chapter 2, section 1, and also an article, “A Note on History as Narrative” (History and Theory 6 [1967]: 413–19), in which I developed the point in a more precise way. Dray responded to the latter article in History and Theory 8 (1969): 287–94.

7. This is admittedly a crude statement of the factors that might be involved. For example, in speaking of “economic factors” I might be referring to the conditions obtaining either at the source of emigration or at its goal. For a careful study of one case of immigration, see Simon Kuznets, Immigration of Russian Jews to the United States: Background and Structure, Perspectives in American History, vol. 9, 1975 (Cambridge, Mass.: Harvard University, Charles Warren Center for Studies in American History, 1976). In that study (pp. 86–89), Kuznets cites comparable cases that suggest that his findings are generally applicable and are not confined to the case at hand.

8. When laws are mistakenly assumed to state an invariant connection between specific types of occurrences, it is plausible to hold not only that historians do not use laws to explain events, but that no one can cite any example of a well- formulated law that a historian could possibly use. When, however, it is understood that in the natural sciences, too, events are not explained by deducing them directly from a law—that an analysis of the initial conditions is essential before a law can be successfully applied—it will become apparent that there may be a great many generalizations upon which historians can and do rely in explaining events.
9. See Appendix B for my argument that the lawlike connections with which Hart and Honoré, and also Hume, were concerned are fundamentally different from any basic laws characteristic of an advanced science.

10. On phases and component parts as two different but compatible ways of looking at the structure of continuing events, see chapter 5, section 1.

11. A somewhat analogous situation is to be found in the development of the much younger discipline of cultural anthropology, though its theories have tended to shift more rapidly than have those in history, where evidence concerning particular societies has always been more readily available.

When this difference is taken into account, what Clifford Geertz has said concerning his discipline can be used in substantiation of my claim regarding history. He said: "Rather than following a rising curve of cumulative findings, cultural analysis breaks up into a disconnected yet coherent sequence of bolder and bolder sorties. Studies do build on other studies, not in the sense that they take up where the others leave off, but in the sense that, better informed and better conceptualized, they plunge more deeply into the same things. . . . Previously discovered facts are mobilized, previously developed concepts used, previously formulated hypotheses tried out; but the movement is not from already proven theorems to newly proved ones, it is from an awkward fumbling for the most elementary understanding to a supported claim that one has achieved that and surpassed it" (The Interpretation of Culture [New York: Basic Books, 1973], p. 25).

In histories, the new sorties that are most apt to change previous interpretations involve examining the same events on a different scale, or examining other facets of the same events. When this is so, shifts in interpretation do not warrant a denial of objectivity.

APPENDIX A


2. I have examined at least some of them in the third chapter of Philosophy, Science, and Sense-Perception (Baltimore: Johns Hopkins Press, 1964).

3. See the opening paragraph of bk. 1, pt. 3, sec. 4 of the Treatise of Human Nature.

4. A. Michotte, The Perception of Causality (London: Methuen, 1963). In this connection, I have particularly in mind his "entraining" experiments—for example, p. 21, experiment 2.

APPENDIX B


2. Causation in the Law, pp. 8–9.


6. To be fair, I must point out that in distinguishing between the explanatory and the attributive contexts in which causal judgments are made, Hart and Honoré acknowledge that it is sometimes necessary in the law to offer explanatory accounts before attributive judgments can be made (*Causation in the Law*, pp. 22–23). Further, in the same passage they admit that such preliminary inquiries may sometimes be difficult, but they add that “such searches for explanation are not the source of the lawyer's main perplexities.” This suggests that when they speak of “the lawyer” they have in mind primarily the judge, and not the investigative officers of the court. This should suggest how misleading it may be to view historians as if their interests were essentially like those that Hart and Honoré find to be characteristic of “the lawyer.”