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THE AUTONOMY OF HISTORICAL UNDERSTANDING

LOUIS O. MINK

I

Classical philosophy of history, which claimed to disclose the secret of human progress or to discover the overarching meaning of universal history, was consumed in the holocaust of two world wars, but it has latterly arisen from its own ashes in the guise of the theory of historical knowledge. It would be misleading, however, to refer to a renaissance of interest in the philosophy of history. It is plain that there are two revivals, not one, and that they suffer from a lack of mutual communication even more remarkable — because *prima facie* less justified — than the general breakdown of a community of discourse in a world of increasing intellectual specialization. Yet although each of these revivals can be fairly clearly identified and characterized, and although each is represented by a large and growing literature, neither seems to take account of the other or even to be curious about its existence and direction. Moreover, one is represented entirely by professional historians and the other entirely by professional philosophers. It would be surprising if there were not differences of problems and points of view between two groups, each with much training in and constant exposure to the subtler nuances of guildsmanship. But even so it is surprising to find no community of interest among at least sub-groups; and the fact seems to be that there is an absence of either agreement or controversy between philosophers and historians who devote some thought to problems of historical knowledge.

One symptom of the absence of discourse is the extraordinary difference, in writing by representatives of the philosophical revival and of the historical revival, between footnotes and bibliographies. The latter invariably cover both revivals with at least quantitative fairness; the former infallibly reveal the hermetic limits of each. For example, the latest report of the Social Science Research Council's Committee on Historiography¹ contains an extensive bibliography with many pages devoted to the category of "Philosophical Discussions"; and most, although not all, of the books and articles comprising the recent debate among philosophers on the logic of historical explanation are

1. *Generalization in the Writing of History*, ed. Louis Gottschalk (Chicago, 1963).

listed under this category. Yet — and despite the fact that Hans Meyerhoff, as philosophical consultant to the committee, explained that to philosophers “generalization, explanation and causation form a syndrome from a logical point of view”² — in the text of the twelve articles comprising the report there is but *one* passing reference (a single sentence, in fact) to any of the problems or arguments which appear in the philosophical literature.

On the other hand, this book is the third of a series of reports which among historians have been centers of discussion and controversy on the nature of historical knowledge and inquiry: *Theory and Practice in Historical Study*, published in 1946 as *Bulletin 54* of the Social Science Research Council, and *The Social Sciences in Historical Study*, published in 1954 by the same Council as *Bulletin 64*. While these two earlier reports can hardly be said to be the Old Testament and the New Testament of contemporary historiography, they have been the agencies by which theoretical and methodological questions have been most forcibly brought to the attention of the historical guild. Yet, to choose only one example, while both reports are dutifully listed in the bibliography of Gardiner’s well-known anthology³ there is just *one* passing reference to either of them in the text of the articles by contemporary philosophers printed or reprinted in Part II of that anthology. And this is not an isolated instance. While there are numerous references by the philosophers represented to historical works (usually as sources of examples for analysis), there are almost no references to what historians have written on historiography — although, again, many such items are included in the bibliography.

That the work of guild-historians and guild-philosophers is cited in each other’s bibliographies indicates that neither group is ignorant of the other’s efforts; but the absence of each in the discussions of the other suggests that they do not speak to the same questions. It is understandable that this should be so, although the reason for it is seldom remarked: what the philosophers are really discussing is logical theory, and historical inference interests them not because it is historical but because it is *inference*. Historians suspect this, and to them it seems, as one has said, that “what philosophers seem to be interested in are the remains rather than the views of historians.”⁴ On the other hand, what the historians are really discussing is whether history is a discipline or just an aggregation of parts of a number of other disciplines, and they are not interested in the logic of argument in general but in the *differentiae* of *historical* arguments. Philosophers are not unaware of this, but they have no intellectual or existential stake in the self-definition of the historical guild.

Yet although these differences arise because the organization of research

2. *Ibid.*, vii, n.

3. *Theories of History*, ed. Patrick Gardiner (Glencoe, Illinois, 1959).

4. Leonard Krieger, “Comments on Historical Explanation,” *Philosophy and History*, ed. Sidney Hook (New York, 1963), 136.

increasingly reflects the fragmentation of the university into academic departments by lines as sharp as they are arbitrary, it does not follow that the problems of history and the problems of philosophy are not alternative ways of formulating common intellectual problems whose importance transcends the provincial problems of academic guilds. And this, I believe, is demonstrably the case, and makes the lack of communication between historians and philosophers a serious problem rather than an incidentally curious one. Both historians and philosophers are too sophisticated to reveal — even when they are sufficiently sophisticated to recognize — the deepest concerns and creeds which inform their work. Yet these, suggested rather than stated and obliquely expressed rather than explicitly formulated, are involved in and account for the fact that despite the lack of communication between historians and philosophers *some* philosophers are closer to *some* historians than they are to other philosophers, and *some* historians are closer to *some* philosophers than they are to other historians.

The differences are clearest among philosophers, since their main business is to sharpen them. *Prima facie*, almost all of the philosophical literature on philosophy of history in the last decade has dealt with the logic of explanation, and specifically has consisted of defense and criticism of an increasingly sophisticated version of the “covering-law” model of explanation and *a fortiori* of historical explanation.⁵ Yet the gravamen of the issue has rarely been fully evident. Historians, generally but not unanimously, have felt that the covering-law analysis bears little relation to what they understand as their own mode of inquiry, and have applauded the sustained criticism of it by other philosophers, especially by Dray. Other historians have sided with the Hempelians. But only recently have the elective affinities been made fully visible.

One of the latest and most comprehensive defenses of the covering-law model is May Brodbeck’s “Explanation, Prediction, and ‘Imperfect Knowledge.’”⁶ The criticism of deductive explanation, Miss Brodbeck charges, is not only logically defective but obscurantist, because it is in effect an attack on the “scientific study of man and society” which seeks to discover “quantified relations among relevant variables.” (232) There is no such thing as “historical” explanation, she maintains, only the [scientific] explanation of historical events (254); and the claim that there is a uniquely historical mode of explanation results in, if it is not motivated by, the rejection of any “science of man” (272).

On the other hand, a leading critic of the covering-law model has lately made the other side of the underlying issue explicit: “What drives us to the

5. The locus classicus is of course C. G. Hempel’s “The Function of General Laws in History,” which appeared originally in *The Journal of Philosophy* XXXIX (1942), 35-48.

6. *Minnesota Studies in the Philosophy of Science*, Vol. III (Minneapolis, 1963), 231-272.

study of history, as much as anything else," Dray says, "is a humane curiosity: an interest in discovering and imaginatively reconstructing the life of people at other times and places. . . . My chief complaint against acceptance of the covering law doctrine in history is not the difficulty of operating it, in either fully deductive or mutilated form. It is rather that it sets up a kind of *conceptual barrier* to a humanistically oriented historiography."⁷

Juxtaposed, these asides to the reader make it clear that although it is questions of logical theory to which the recent philosophical literature has been addressed, the underlying issue remains the antagonistic confrontation of the scientific culture and the humanistic culture (I do not say: "of science and the humanities") and the claim of each to privileged inquiry into at least some areas closed in principle to the other. Each side, moreover, charges the other with being the aggressor. Miss Brodbeck presumably will not deny historians the right to indulge in "imaginative reconstruction" if it pleases them, but is outraged by the defense of such activity as yielding *historical* explanation because such a defense precludes *scientific* investigation of the same events. Dray does not claim that any events are closed in principle to "covering-law" explanations, but argues against the imperialistic claim that no alternative modes of explanation are logically defensible.

Now the distinction between universally quantified and statistical hypotheses, to mention only one point which has emerged in the refinement of the covering-law model, may well be immediately relevant only to logical theory; but the underlying issue is one of general intellectual importance, because it concerns man's understanding of his own past and of himself in the light of that past. And the attempt of historians to place themselves in relation to contemporary social science (identified primarily in terms of its commitment to a scientific methodology to which the "covering-law" doctrine belongs) reflects an increasing intellectual and practical awareness of the issue. Historians and philosophers, in fact, seem to have been unwittingly converging on a common point from opposite poles.

Philosophers have always been logicians, and the main problems of modern philosophy since the seventeenth century have arisen in the analysis of the logic of scientific theory, especially physics. Historians, on the other hand, have been researchers and stylists, and the main problems of modern history since the early nineteenth century have been discovery and synthesis of "facts." Philosophers and historians have in general been content to identify themselves with the roles outlined for them by Aristotle: philosophy as the concern with the universal, history as the concern with the particular. But in recent years there have been revolutions in both fields against the received doctrines: philosophers (or some of them) have been attempting to break the spell of the idea

7. "The Historical Explanation of Actions Reconsidered," *Philosophy and History*, 132-33.

of a *single* logical model of knowledge with formal properties applicable to inquiry into any subject-matter. Historians, on the other hand, have been trying to break the spell of the "school of the unique." *Bulletin 64* of the Social Science Research Council attempted with no notable success to convince historians that they should *generalize* more, e.g., by formulating testable general hypotheses. The succeeding Committee on Historiography adopted the subtler strategy of convincing historians that they cannot and in fact do not avoid generalization in any case, for example, because they necessarily use concepts, like "revolution," which are classificatory or "labelling" generalizations. The conclusion of this volume of essays by historians of otherwise divergent views is that, as the editor summarizes the consensus, "the historian willy-nilly uses generalizations at different levels and of different kinds."⁸

The upshot of all this is that any distinction resembling Windelband's classification of "nomothetic," or theoretical-explanatory, and "idiographic," or particularizing, sciences has been rejected from both sides as untenable in the special case of historical inquiry. Some philosophers have advanced reasons for denying the philosophical doctrine that history must be nomothetic if it is to be a science; and most historians have proved susceptible to the arguments that it cannot be merely idiographic if it is to be a discipline. Philosophers and historians, so to speak, have backed into each other, although they are still facing in different directions. The question at issue is whether historians will back all the way into the received philosophical doctrine which has increasingly become dogma in the social sciences as it has increasingly inspired heresy among philosophers themselves.

II

The gist of the received philosophical doctrine is that history is not yet a science but by the explicit adoption of scientific methods and criteria may become one. Bemused by the contradictions and ambiguities of "common sense" and ordinary language, history as we know it is in the chrysalid stage of *proto-science*; and the apparent differences between historical and scientific methods can be reconciled theoretically by analysis and practically by increased methodological sophistication on the part of historians.

Common to all arguments that there is no irreducible difference between historical and scientific method is an acceptance of something like these propositions: (1) No valid distinction can be made between *Naturwissenschaften* and *Geisteswissenschaften* in terms of a difference in the logical and conceptual structure of the explanations which they provide; there is a single logical mode of explanation. (This is the principle of the methodological unity of science.) (2) The explanation of any phenomenon involves its subsumption under general principles which are not *ad hoc* and for which discon-

8. *Generalization in the Writing of History*, 208.

firming instances are not known. Specifically, the explanation of a phenomenon requires showing the statement describing it to be a deductive consequence of a set of general laws ("covering laws") together with particular statements describing initial conditions. (This is the principle of explanation by hypothetico-deductive theory.) (3) The hypothetico-deductive method is not a method of discovery, and creative imagination finds its place in the formulation of new concepts and hypotheses; nevertheless, the *test* of such original formulations is whether they can be ordered in theory, deductively formulated and empirically confirmed. (This is the principle of empirical correspondence.) (4) Hypotheses cannot be confirmed as isolated statements; experimental or observational confirmation is always of a set of hypotheses, many of which may not be explicitly stated, as the results of an experiment in the behavior of neutrons may confirm a hypothesis in nuclear physics but also laws of electromagnetism involved in the construction of the experimental apparatus. (This is the principle of theoretical coherence.) The latter principle makes possible the division of labor among scientists: a single scientist may (and must) use as auxiliary hypotheses the results of the research of many other investigators without repeating their work.

These four principles jointly indicate an emphasis on conceptual theory at some variance with the traditional concern of historians with interpretative narrative; and each principle denies some feature of ordinary historical practice. But it is clearly not obligatory on a logical analysis of knowledge to justify everything which *claims* to be knowledge: its purpose would be defeated if it did not have normative efficacy, that is, the capacity to distinguish between valid and invalid claims. Moreover, it would indeed block the path of inquiry to argue *a priori* that there could be or should be no attempt to develop a methodology of history in accordance with these principles — an attempt which, if successful, would convert history into a true general sociology. But the question is not whether there can be a scientific explanation of what are called "historical" phenomena; it is rather whether historical knowledge can be analyzed *without remainder* on such principles.

Supporters of the proto-science view are of course quite aware that most historians have, to speak mildly, something less than the shock of self-recognition when confronted with a rigorous exposition of scientific method. But, it may be argued, at every point at which a doubt is raised, it can be shown that the doubting historian in fact tacitly agrees in principle without knowing it, just as a scientist might have difficulty in giving an exact description of a method which he nevertheless *uses* correctly and easily. It is not necessarily an embarrassment to the proto-science view when historians refer to "historical insight" or when they deny that they seek historical laws or even make use of general non-historical laws. If history be proto-science, astute analysis can reveal to historians that their habits of work and processes of inference are

more complex than they realize, so that greater logical sophistication will enable them to enjoy and extend the fruits of the scientific habit while avoiding the lapses which tempt the uninitiated.

Now the primary assumption of the proto-science view is that there can be no legitimate mode of understanding not analyzable by an explicit methodology. If it can be shown that history is autonomous, and not proto-science, it must be done, I think, not by showing that there is some fact or set of facts which can be explained "historically" but not "scientifically," nor even by providing alternative models of scientific explanation, but by a critique of this assumption. One may accept the thesis that there is no fact incapable of being scientifically explained, and yet hold without inconsistency that there are other ways of understanding the same facts (although I would not concede, except for purposes of argument, that there is no problem about the meaning of "same facts," since what will *count as a fact* is in part prescribed by the adoption of an explicit methodology). Whether historical understanding is a first approximation to scientific explanation, or whether it can support the claim to autonomy, I propose to examine by reference to six features of historical practice and description which are generally, although of course not universally, accepted by historians. The proto-science view can to a considerable degree account for these features as evidences of methodological immaturity. The question is whether they can be *justified* by any alternative account incompatible with the proto-science view.

III

(1) *Historians generally claim that they can give at least partial explanations of past events; but they do not ordinarily undertake to predict the future, even at the level of incompleteness and generality at which they "explain" the past.* Nor does it, by and large, seem to them an oddity that the past should be explainable but the future unpredictable. The reasons for dissociating explanation and prediction are not often articulated by historians, but probably in any given case include one or more of the following: (a) explanation and prediction are *logically* different, and success in one is not intrinsically connected with success in the other; (b) the past consists of settled fact while the future — at least the future of human decisions and their consequences — is as yet indeterminate; (c) historical explanation requires a certain "perspective," which the historian can achieve only after a connected sequence of events (such as a revolution or a reign) has been completed; (d) too much contemporary evidence (classified data, memoirs, diaries, letters, confidential reports, etc.) is not available to the historian at the time when it is relevant for prediction, although most such material will be available to future historians at a time when it is useful for explanation.

To the proto-science view of history none of these can be expanded into

cogent reasons, because there is, in this view, neither a logical nor a practical (so far as inquiry alone is concerned) difference between explanation and prediction. Either to explain or to predict an event is to show that the statement describing its occurrence follows logically from a set of general laws together with (true) statements of initial conditions. If the event is past, this process yields an explanation; if the event is future, it yields a prediction. And every prediction which conforms to this logical pattern and is confirmed in experience becomes an explanation as soon as the event has occurred. The difference between explanation and prediction may be important for one's practical interests, since one may wish to prepare for the event or to avoid the prediction by changing the conditions; but the *logical* structure is identical in both. No one would argue that we can predict eclipses but that we cannot really "explain" them until after they have occurred. Why should historical explanations be different?

It therefore seems (still runs the proto-science view) that an historical explanation of an event is unwarranted precisely to the extent that the historian *would have been unable* to predict the event before its occurrence, assuming his present professional equipment and knowledge of antecedent facts. Nothing in the lapse of time will convert a logically inadequate prediction into a logically satisfactory explanation. But even though historical explanations are poor creatures indeed by the standards of the physical sciences, the historian need not despair to the extent of seeking refuge in appeals to "perspective" or "insight." He has in fact, without realizing it, been using this logical model of explanation to provide what Hempel has called an "explanation sketch." Compared with an ideal explanation, his statements of initial conditions are factually incomplete, imperfectly analyzed, and imprecise. His general laws, probably not explicitly acknowledged, tend to be common-sense generalizations about human behavior not yet clarified and confirmed by reconstruction in psychological, sociological, or economic theory. But these lacks point the directions which historical inquiry should take: the development of analytical techniques, the quantification of data, and the search for causal and statistical laws. Even with cumulative success in these enterprises, history may fall short of completely accurate specific predictions — but, of course, so does much of science. History *may* be able, like the natural sciences, given specific conditions to predict within a reasonable margin of error the changes in a relatively isolated system.

The force of this proto-science view, however, lies not so much in the proposal that historians should be satisfied with nothing less than accurate predictions as in the argument that they should be dissatisfied with present explanations. Successful prediction is a *test* of theory (although it is not even that in such sciences as geology and cosmogony), but the *condition* of theory is its logical structure which makes prediction possible as more than an in-

formed guess. With a logical model of explanation at hand, the reasons for dissociating explanation and prediction disappear (and all other reasons are in principle disposed of): (a) The belief that explanation and prediction are “logically” different is clearly refuted in the case of scientific knowledge, and one cannot argue that they differ in historical even though not in scientific knowledge without begging the question at issue; (b) The argument that human actions are explainable but not predictable is either ruled out in any particular inquiry (as in psychology) or else outflanked by the admission of statistical macro-laws (as in demography); (c) The appeal to “historical perspective” can be explained in terms of the *ex post facto* selection by historians of common-sense generalizations about human behavior which plausibly fit the completed data. (Men sometimes sacrifice themselves for a cause, and sometimes betray a cause in their own interests; so a historian may “explain” a given action by either of these incomplete generalizations *after* the action is completed and its circumstances known); (d) And the unavailability of contemporary evidence is at best a practical difficulty but not a theoretical objection; the same difficulty obtains, say, in meteorology.

Now to the extent that historians seek to explain events as instances of a general theory, the proto-science view is, I believe, unobjectionable. But it must clearly beg the question if it undertakes to convince them that they *should* accept only theoretical explanations as satisfying the desire to understand. And if one entertains the possibility that historians mean *something* by the claim that the past is explainable but the future unpredictable, to accept the proto-scientific view rules out the possibility of discovering what this meaning is. It may be that historians use other modes of explanation than the theoretical mode; and only an *a priori* argument could rule out this possibility.

It is to describe one such mode that W. H. Walsh has revived Whewell’s term “colligation,” by which Walsh intends to describe “the procedure of explaining an event by tracing its intrinsic relations to other events and locating it in its historical context.”⁹ Explanation by colligation, he suggests, is appropriate in cases where a purpose or policy has found expression in a series of actions each “intrinsically” related to the others in the series. This does not exclude the possibility that many historical explanations, or parts of every historical explanation, also make use of “explanation of a quasi-scientific type, involving the application of general principles to particular cases.” But even so it is questionable whether the facts of historical practice correctly described as “colligative” pose an insuperable difficulty for the proto-scientific view, unless the latter is committed additionally to a mechanistic or behavioristic theory of human behavior which cannot admit purposes as causes. If it is possible in principle to formulate generalizations to the effect that men

9. W. H. Walsh, *Introduction to Philosophy of History* (London, 1951), 59-64.

seeking certain goals under certain conditions will act in specific and predictable ways, then colligation would turn out to be not a mode of explanation but merely a description of very incomplete approximations to the ideal model of explanation proposed in the proto-science interpretation.

Nevertheless, the description of explanations as “colligative” does point to something else; it expresses what historians seem to mean by “perspective,” and the insistence on historical perspective seems to me more than a mere recommendation of the attitude of objectivity or an excuse to avoid making predictions. It is at least in part a claim that for the historical understanding of an event one must know its consequences as well as its antecedents; that the historian must look before *and* after (and not pine for what is not); that in *some* sense we may understand a particular event by locating it correctly in a narrative sequence as well as by classifying it as an instance of a law.

Not infrequently we ask “And then what happened?” not merely out of curiosity but in order to understand what we have already been told. Suppose that one were watching a pantomime of an exceptionally tall man preparing for the night in the upper berth of a Pullman — but without knowing the subject of the pantomime. The mimicry of gesture, at first utterly cryptic, may gradually become intelligible, but a necessary condition of its intelligibility is the significant *order* of represented action. Whether the downward motion of a hand represents the closing of a shade or the drawing of a vertical line depends on its position in a sequence of actions, and a sequence in which the subsequent actions are as significant as those antecedent. It would seem gratuitous to deny that understanding may result from a narrative answer to the question, “What happened then?” And one may call such an answer a *sequential* explanation without implying that it is the *only* explanation possible of a specific fact or set of facts, or that it is a satisfactory answer to the different question, “Why did it happen?” Moreover, the state of understanding produced by a sequential explanation is clearly complicated, and may include, perhaps in all occurrences, common-sense generalizations and implicit theories. But I am concerned only to argue that the latter in some cases presuppose and do not in any reconstruction *replace* the limited understanding afforded by a sequential explanation. If this is so, it indicates that “historical perspective” is not merely an honorific name for an explanation sketch but refers to a kind of understanding which can account for the difference between “hindsight” and “foresight” on grounds which do not necessarily deny the logical identity of scientific explanation and prediction. And it preserves, one might add, the possibility of genetic explanation.

(2) *Historians may often prove false a “hypothesis” about an historical event or period without concluding that it is false in any other case or as such.* Thus it

may be found that the distribution of votes in a parliamentary division on a specific issue is not explicable in terms of the economic classes and interests of its members, and yet no inference may be drawn that the hypothesis of economic determinism, however stated, is thereby disconfirmed.

Now the proto-scientific view can of course account for this fact as follows: although it is true for logical reasons that a hypothesis is disconfirmed by a single negative instance (assuming that it is not one of the auxiliary hypotheses which has really been falsified), the case is no different in history from that in science, because at least two senses of "hypothesis" must be distinguished. In a trivial sense, any particular statement whose truth has not been established can be called a hypothesis. A chemist may entertain of an unknown the "hypothesis" that it is an acid, and quickly test it with a piece of litmus paper. This would be analogous to a historian's guess, "This revolution failed because it lacked leadership." In neither case would the falsification of the hypothesis preclude the employment of a similar hypothesis in another case. But such a particular hypothesis must be distinguished from generalized hypotheses such as "Acids turn litmus paper red," or "Revolutions without strong leadership invariably fail," either of which *would* be falsified by a negative instance. There is no reason to believe, therefore, that the role of hypotheses or the logic of confirmation is subject to special qualifications when applied to historical explanation.

The difficulty with this account, however, is that even generalized hypotheses are not abandoned by historians when disconfirmed, nor can their survival be dismissed as unfortunately ideological. It is highly doubtful whether any historians regard the "economic interpretation of history" as a hypothesis, or theory, to be accepted only so long as some single bristlingly irrefragable fact (perhaps from the Fourth Ming Dynasty) remains undiscovered. The reply to this by the proto-science view, of course, is that the vagueness of terms and the absence of measurability in historical hypotheses make it impossible to decide empirically whether or not they are confirmed by any given data. (How shall we define "leadership" and measure relative strengths of leadership when investigating revolution?) Moreover, historians are often rough-and-ready in their use of terms. A leading defender of Frederick Jackson Turner's "frontier hypothesis," for example, observes of Turner's critics that they refused to recognize "that Turner was advancing a hypothesis rather than attempting to prove a theory."¹⁰ To a logician this is puzzling: a theory *is* a set of hypotheses, and it is logically impossible to entertain a hypothesis except in connection with a theory to be proved or disproved.

Yet such a curious usage may reveal that historians mean something quite different by the term "hypothesis" or that hypotheses function quite differently

10. Ray Allen Billington, *The American Frontier* (Washington, D.C., 1958), 4.

in historical inquiry than in scientific inquiry — including social science. It may not be a logical gaffe but a significant symptom that Turner's "frontier hypothesis" has never been precisely formulated and yet has been a supremely fertile source of suggestions of specific inquiries to be undertaken.

One might say that for the proto-science view of history, a hypothesis is in the first instance a *candidate*, regarded with interest and some hopeful expectation but without approval until it passes its examinations and is admitted (on condition of continued good behavior) as a law. But historians seem generally to regard generalized hypotheses not as potential laws but as *guides* whose services they employ. This Virgilian function of hypotheses is of course not absent in natural science; witness the commonly accepted criterion of "fertility" for good hypotheses. But the historian seems to use hypotheses as *suggestively* rather than *deductively* fertile. For him, a hypothesis is not a tentative law but a rule for asking questions, a rule for delimiting the scope of inquiry, and a rule for determining the relevance of evidence. So it is in science too, but in an incidental rather than in an essential way. For the scientist, the hypothesis is the target; for the historian, a signpost. In history, the informal rule-function of hypotheses is not, as it is in science, preserved in the results obtained; hypotheses are left behind, so to speak, like the wives of some great men. In Kantian language, one might say that hypotheses as rules for inquiry are constitutive in science, regulative in history.

This distinction would be otiose, however, if it served merely as an indirect re-statement of the familiar view that science seeks the universal, history the particular. The latter distinction is at best one of emphasis and degree; and so would be the distinction between the scientific use of data to confirm hypotheses and the historical use of hypotheses to lead to data *unless the distinction points to different kinds of understanding* rather than merely to different stages of the same pattern of inquiry. Nor would it be enlightening to point out that the use even of false hypotheses (as for example racist theories) may lead historians to stumble onto areas of evidence which accidentally turn out to be significant in other contexts; for the history of natural science is not wanting either in examples of false theories which led to happy discoveries. The question is whether the historian seeks a distinctively different *mode of understanding* in the attainment of which generalized hypotheses are means but not ends. Most of the recent literature on historical explanation has sought to justify the ways in which general statements which are not universally quantified statements enter into historical thought and writing; hence they do not have the logical form of laws, and cannot serve as premises for rigorously deductive explanation.¹¹ But suggestions that models of histori-

11. There is a comprehensive review of such suggestions in William Dray's "The Historical Explanation of Actions Reconsidered," *Philosophy and History*, 105-33.

cal explanation weaker than the classic Hempelian model are more adequately descriptive of actual historical practice still share with the proto-science view the assumption that historical understanding can be analyzed into explicit methodology. They call attention to the ubiquity in history (although not *only* in history) of “limited” or “guarded” generalizations, but they do not even claim to account for *interpretative* hypotheses such as the “frontier hypothesis” or the Pirenne thesis. And it is these which historians generally believe in some way distinguish history as *interpretative narrative* from chronology on the one hand and “science” on the other. It is perhaps for this reason that historians have on the whole been unabashed by the notion of *empathy* which philosophers have been so eager to relegate to that limbo of philosophical fictions already populated by final causes, entelechies, dormitive properties, and mental substances.

(3) *Historians very often testify that they find it useful or necessary to “relive” or “recreate” in imagination the events which they investigate; and they have not been reluctant to call this activity “insight” or “intuition.”* Dilthey, Croce and Collingwood, among others, seized on this fact as the defining difference of historical inquiry, arguing that human actions are always the expressions of states of mind and that the distinctive act of historical understanding is the re-experiencing by the historian—and by his readers if he is as adept at writing as he is in research—of these states of mind.

The proto-science interpretation of this claim is quite explicit. Of course, its supporters say, *Nacherleben* or “psychological empathy” may be a useful *heuristic* device — as the use of pictorial models by natural scientists has heuristic value — but like all heuristic devices it must be regarded as an initial stage in investigation, not as its aim or as a test of its conclusions. Thus, according to Edgar Zilsel, “when a city is bombed it is plausible that intimidation and defeatism of the population result. But it is plausible as well that the determination to resist increases. . . . Which process takes place cannot be decided by psychological empathy but by statistical observation only. . . . The method of ‘understanding’ (‘insight’) . . . is not sufficient when investigating historical laws.”¹²

Now it should not be necessary to observe that a historian trying to understand what took place in Guernica or Rotterdam is hardly likely to consider himself to be “investigating historical laws.” Certainly the “method of understanding” has, to my knowledge, *never* been recommended as a substitute for scientific method in the discovery of laws but has always been intended to describe a kind of reflective activity in the understanding of events. A working

12. Edgar Zilsel, “Physics and the Problem of Historico-sociological Laws,” *Philosophy of Science* VIII (1941), 567-79.

historian may well object to Zilsel's example: "Of course, the bombing of a city may lead in general to defeatism or to stiffened morale. But I am interested in *this* city, at *this* time, in *these* circumstances, and the more I learn about this case, the more clearly can I understand why one result rather than the other obtained. What I come to understand is the way in which all of the events — the contamination of the water supply, the mayor's speech, the death of a child — are interrelated as constituents of the total event." Such a reply is relevant at least in its denial that the aim of inquiry is to discover what variables are required for the formulation of a general law connecting "bombing" with "intimidation" and "determination."

Criticisms of the "method of understanding" seem invariably based on the unwarranted assumption that it is intended to verify general laws or to establish particular facts for which there is no independent evidence; and the instances of "understanding" considered in such criticisms are invariably anonymous examples wrenched loose from the rich and detailed context with which the historian works. No doubt this is an inescapable error, since one can hardly quote a written history in full to illustrate a point. But the error is also in part due to the fact that the proto-science rejection of empathy is based on the assumption that to understand an event is equivalent to seeing it as an instance of a law, and this is the very question at issue.

What is required, evidently, is an elucidation of the concept of "understanding," and one which clarifies its connection with other related concepts. Now the proto-science view provides an admirably clear conceptual map, in which the concept of *understanding* is identified with the concept of *explanation*, and the concept of explanation is identified, so far as its formal properties are concerned, with the concept of *prediction*, through a logical model which specifies the relation of both to other concepts such as "law," "deducibility," etc. Most philosophical criticism of the proto-science view has been devoted to prying loose the concept of explanation from that of prediction; but we might also try to see to what degree, and by what arguments, we can loosen the tie that binds the concept of *understanding* to that *explanation*.

The key to an alternate account of understanding is perhaps in the term "context." The minimal description of historical practice is that the historian deals with complex events in terms of the interrelationship of their constituent events (leaving open entirely the question whether there are "unit events" in history). Even supposing that all of the facts of the case are established, there is still the problem of comprehending them in an act of judgment which manages to hold them together rather than reviewing them *seriatim*. This is something like, in fact, the sense in which one can *think* of a family as a group of related persons rather than as a set of persons plus their individual relations of kinship. It is in the latter way alone that one can *describe* a family, but this is an accidental consequence of the fact that language is discursive and that

one must name one after the other the constituents of a group which one can think of (as one can see them) simultaneously. It is this elementary fact, I suspect, which has led critics of the "method of understanding" to center attention on the accidental features of the way in which it is described and illustrated rather than on the act of judgment to which it refers. Nevertheless, it is important to recognize that it is not in any proper sense a "method"; it is neither a technique of proof nor an organon of discovery but a type of reflective judgment. And it is a kind of judgment, if I am not mistaken, for which the proto-science view cannot account — and does not wish to.

(4) *Historians generally do not adopt one another's significant conclusions unless convinced by their own thorough inspection of the argument*; unlike scientists in general, they must read one another's books instead of merely noting their results. Except for monographic studies, historical "abstracts" (like philosophical abstracts) will tell an historian only whether he should study the argument abstracted. To the proto-scientific view, this merely confirms the belief that in the absence of conceptual theory no two historians (perhaps no historian at two different times) mean the same thing by such central terms as "feudalism." At best the lack of method can lead to irresolvable disagreements about the weight and adequacy of evidence; at worst it is an invitation to mistake arbitrary and *a priori* "interpretations" for evidenced conclusions. It is no wonder, then, that history is not a cumulative science in which an investigator can use the results of others' research without repeating for himself the entire process of their research (always excepting monographic results). Thus the second Committee on Historiography of the Social Science Research Council, having learned from discussions with other social scientists that "the most portentous development in modern social science is pioneering work in providing a more coherent basis for cumulative theory," urged that "to give impetus to the process of cumulative analysis is, in the opinion of the Committee, one of the most important ends to be achieved by the use of social science methods in history."¹³

Now cumulative knowledge rests on the possibility of what might be called "detachable conclusions," and there is no question that the use of detachable conclusions affords enormous economy and efficiency in the administration of scientific research. A scientist will normally repeat another scientist's work only if he suspects the possibility of error; a historian may repeat another's work if he is convinced that it has afforded significant results. And despite the lack of rigorously defined method, there may be very considerable agreement among historians who have examined the same materials. The proto-scientific view, of course, interprets such agreement as indicating that tacit hypotheses and principles of inference are shared by the parties to

13. *Bulletin* 64, 136, 138.

the agreement; and it proposes that these can be articulated in an explicitly formulated methodology. But if the account of historical judgment so far adumbrated is tenable, there are other reasons for this agreement in the absence of such a method.

One might plausibly argue that the historian is dealing with types of evidence to which quantitative weights cannot be assigned; or that, even if such assignment were possible, the total weight of evidence is not a function of the weights of individual items of evidence taken separately (there is, I think, at least a question whether in history or in law there can be an explicit logic of corroborative as distinguished from demonstrative evidence); or that there is a different sort of relation between evidence and hypothesis in historical and some other arguments from that which one finds in science. But to expand any of these possibilities would be in effect to accept the gambit of the proto-science view: that the problem is one of the logic of confirmation. The major point of difficulty in attempting to transform history into a cumulative science, I believe, is not one of the *logic of evidence* but one of the *meaning of conclusions*.

Detachable conclusions are possible in science because — and only because — of its theoretical structure. The division of labor in research requires that concepts have a uniformity of meaning, and the methodological problem of definition therefore becomes central. But despite the fact that an historian may “summarize” conclusions in his final chapter, it seems clear that these are seldom or never detachable conclusions; not merely their validity but their meaning refers backward to the ordering of evidence in the total argument. The significant conclusions, one might say, are ingredient in the argument itself, not merely in the sense that they are scattered through the text but in the sense that they are *represented by the narrative order itself*. As ingredient conclusions they are *exhibited* rather than *demonstrated*. Articulated as separate statements in a grand finale, they are not conclusions but reminders to the reader (and to the historian himself) of the topography of events to which the entire narrative has given order. In this one respect at least, history is akin to poetry in its reliance on ingredient rather than detachable conclusions. It was suggested above that the conclusions of historical inquiry dispense with the hypotheses which served as rules to guide inquiry whereas the conclusions of scientific inquiry contain its hypotheses; we might expand this suggestion and say that scientific conclusions dispense with their evidence while historical conclusions contain it. This sounds odd if the comparison is made only in terms of the logic of evidence; it is less odd if we are thinking of the rules of meaning.

Is a historian cheating because he never defines the term “capitalism”? Yes and no. Yes, if the aim of his study is to discover socio-economic laws about the development of economic institutions; no, if his aim is to understand the interrelationships of particular events, ideas, and institutions in their compli-

cated development. It is not uncommon to find historians, bemused by methodology, defining terms and then using these terms in senses not allowed by the definitions. But most often it is no matter unless one is looking for illustrations for a logic text rather than for historical understanding. The actual meanings are provided by the total context; they may be consistently or inconsistently used, but one's judgment about this has exactly the same grounds as the judgment whether or not the argument exhibits significant conclusions.

Even historians who have been attracted to the proto-science view claim that history has a distinctive aim. The second Committee on Historiography reminded itself that the "analysis of interrelations goes on in all social science, but the attempt to make a general synthesis of all major factors at work in a given conjuncture of events is peculiar to historical studies. . . . the conscientious historian, even when engaged upon monographic research, never permits himself to forget the final goal, namely, comprehensive synthesis."¹⁴ In view of the total impression left by this report, it seems necessary to observe that although the "analysis of interrelations" by the use of hypothetico-deductive method may *enable* synthesis, it will not of itself *produce* it, and it may possibly distract the historian from his habit of seeing things together to a program of explaining things separately. Because there is no logical method of testing the synoptic judgment which is his real aim, he may take refuge in a piecemeal series of "testable" statements.

What the historian does in arriving at synoptic judgment, I suggest, is much like what everyone does in interpreting the meaning of a statement. The meaning of an actual utterance can be analyzed as a function of the meaning of its individual terms plus its syntax plus emphasis, all interpreted in a particular context of discourse. Analogously, the historian tries to understand a complex process as a function of its component events plus their interrelationships (including causal relationships) plus their importance, all interpreted in a larger context of change. It is primarily the syntax of events, of course, in which he is interested. One might even say that the aim of historical knowledge is to discover the *grammar* of events, whereas in the proto-science view it is, so to speak, the *logic* of events alone that could deserve the name of knowledge. The figure can be extended even further: as the meaning of a statement may be questionable because of semantic ambiguity, syntactical ambiguity, amphiboly, or because it has been taken out of context, so there may be historical disagreement about the correct description of individual events, of their interrelations, of their relative importance, or of the significance of the process as a part of its larger history. Those who hold the proto-science view of history tend, as a principle of methodology, to reject the possibility of formulating exact rules for the unequivocal determination of meaning of all statements

14. *Bulletin* 64, 87.

under all conditions. The desire to approach this goal has led in fact to the proposal that an artificially constructed "language of science" be substituted for the hopeless complexities, ambiguities, and nuances of living language. But unfortunately the historian cannot solve his problem by constructing an artificial past, as the logician solves his by constructing an ideal language. The danger is rather that the historian may become obsessed with compulsive neatness and try to do so.

(5) *Historians generally agree that there are resemblances among complex events (e.g., revolutions), but also insist that no two such events are identical; and they often add that their aim is to understand an event as "unique" rather than as typical. This is in the first instance a description of historical practice, but has also been the basis of attempts, notably by Windelband and Rickert, to define history as knowledge of the concrete and the particular rather than the abstract and general. But the proto-science view can cogently argue that this will not serve to distinguish history from science, because no two physical events are exactly identical either, and, moreover, a physical fact is just as concrete and particular as an historical fact. There is no reason, according to the proto-science view, why the historian's impressionistic awareness of resemblance cannot be replaced by precise analytic techniques, and no more ultimate significance in the fact that no two revolutions are exactly alike than in the fact that the spectrographs of no two stars are identical. But here again, I suspect, historians have been right in what they have denied, although mistaken in what they have affirmed. No doubt too great emphasis has been given to the irreducible uniqueness of events; but while this is an error if taken as a theory about events, it can be defended as obliquely revealing a distinctive characteristic of the historical judgment. There is reason to believe that the recognition of resemblances is a kind of terminal judgment, which is not replaceable by an analysis of factors, as an explicit methodology would require. When one observes that two people "look alike," one does not ordinarily compare a series of physiognomic details and then infer a similarity; the recognition of resemblance is immediate and total. Of course it is possible to carry out an analysis of the resemblance noted, comparing cephalic indices, lengths of noses, and so on. But the question is not whether the immediate recognition of resemblance can in every case be supported by something like a graphic chart of similar characteristics, but whether the latter can substitute for the former as a description of the fact of resemblance. I think that we should no more accept a set of comparative indices as the fact of resemblance than we should fall in love with a Bertillon identification. One could never predict by looking at sets of physical measurements whether the person they describe resemble each other if one were not antecedently capable of recognizing resemblance without such analytical tools.*

This is not merely an observation that the whole is greater than the sum of its parts, nor a rejection in general of the possibility of exhaustive analysis. But analytic comparisons do seem plainly inadequate as a *substitute* for the immediate recognition of resemblances, and this inadequacy is at least a clue to the nature of historical understanding. The suggestion is that the distinctive characteristic of historical understanding consists of comprehending a complex event by "seeing things together" in a total and synoptic judgment which cannot be replaced by any analytic technique.

Now it is in the effort to attain this judgment and not as a method of "confirmation" that empathy or the so-called "method of understanding" is serviceable. It is misleading to say that in order to understand Caesar's decision to cross the Rubicon I must somehow "become" Caesar or "re-live" his decision. Obviously I can imagine myself as Caesar no matter how little I know about him, and such imagination is worthless from the standpoint of knowledge, although it has produced some interestingly imaginative dramas. But the more I know about the facts of the case, the more necessary it becomes to use something like empathy in order to convert an indigestible heap of data into a synoptic judgment by which I can "see together" all these facts in a single act of understanding. Otherwise, if I am asked what I have learned, I can only point mutely to my filing cabinet. On the other hand, it would be misleading to suggest that historical inquiry consists of the indefatigable collection of facts and *then* a grand swoop of synthesis. The complicated connections between facts and inference and the ways in which inference informs the process of "factual research" have often enough been pointed out. What is here called "synoptic judgment" is, I think, both a characterization of the type of historical thought in the process of research and also a description of its final aim.

The proto-science view also fails to recognize that practical experience is necessary both to attain and to understand such synoptic judgment. It is often observed that Caucasians have great difficulty in telling apart strangers of another race: "All Chinese look alike." Similarly those with little knowledge of history are likely to see resemblances which the working historian considers insignificant, and to fail to see resemblances which he finds important — as Mussolini saw rather too much resemblance between the Roman Empire and the Fascist State, and not enough between himself and Renaissance *condottiere*.

Why then, if synoptic judgment is ingredient in historical skill and an aim of historical understanding, has it been so little noted and so much misunderstood? First, I suspect, because the historian must set forth *in sequence* a narrative which, if I am correct, he understands or tries to understand *as a whole*. Thus the substance of essentially non-discursive understanding may be obscured for his reader, or even to himself, by the compelling problems of dis-

cursive style. Second, an historian is apt to “summarize his conclusions,” thus giving the impression that the latter, like the detachable conclusions of science, are inferred from the evidence, rather than being indicators which point to the way in which the evidence has been ordered. Hence if an historian lists characteristics in which the presidencies of Jackson and the second Roosevelt were alike, the proto-scientific interpreter will at once ask whether functional dependencies can be discovered between these characteristics *in general*: what, for example, is the connection, if any, between the fact that a statesman’s (*any* statesman’s) power rests on mass support and the character of his fiscal policies? Such a question is of course perfectly legitimate in its own right, but a generalization about popular democracy and economic policy is not necessarily the aim of historical inquiry. The synoptic judgment may be the satisfaction of its own aim, not a way of authenticating data in the service of sociology, economics, or political theory. Finally, the primary reason for misunderstanding is the tendency to divert attention from historical *judgment* as a reflective act to historical *explanation* as a series of statements, some of which are offered as reasons for others. The latter is of course a partially correct description of a written history, as of any argumentative discourse. But it is clear that if there *were* such a thing as synoptic judgment, achieved by some historians and communicated in some histories, no analysis of a history as a series of statements would be adequate to rendering the sense of such judgment. Nor would an explicit methodology serve to produce such judgment. The logic of confirmation is appropriate to the testing of detachable conclusions, but ingredient meanings require a theory of judgment. Otherwise one is in the position of denying that a chair will stand alone because four one-legged stools will not stand unsupported.

But two surprising and connected consequences result from accepting the view that historical knowledge involves a distinctive kind of judgment rather than that it deals with an autonomous subject matter or employs a unique “method.” One is that temporal order is not of the essence of historical judgment; the other is that “historical” or synoptic judgment is not limited to the understanding of past events. Philosophical idealists such as Collingwood and Oakeshott have sensed the importance in historical inquiry of what here has been called “synoptic judgment”; but they seem to have concluded that because a synoptic judgment is a single and self-contained act of understanding which does not *contain* temporal sequence, it therefore cannot significantly *refer to* such sequence. As a result, such apparently paradoxical conclusions as “All history is contemporary history” are misleading, but not entirely wrong: misleading because they obscure the distinction between knowing-history and history-as-known, but not wrong insofar as they direct attention to the act of “seeing things together.” That events occur sequentially in time means not that the historian must “re-live” them — by reproducing a deter-

minate serial order in his own thought — to understand them, but that he must in an act of judgment hold together in thought events which no one could experience together.

But this is the same type of synoptic judgment by which a critic “sees together” the complex of metaphor in a poem, by which the clinical psychologist “sees together” the responses and history of a patient, or by which the leader of a group “sees together” the mutually involved abilities, interests, and purposes of its members. Without aesthetic theory the critic may become bizarre; without psychological theory the clinician would be irresponsible; without political theory the leader is likely to become merely the foremost member of a clique. But it is not theory which elucidates the poem, cures the patient, or finds effective expression for the will of the group. Success in any of these enterprises depends at least as much on the ability to make synoptic judgments as on the correctness of theory. The proto-scientific view is quite correct in pointing out that judgments about concrete cases are tests of a theory; its failure is its inability to account for the fact that theory may lead to judgment and yet not be a part of it.

(6) *Historians generally assume that they have a potentially universal audience, especially for the “comprehensive syntheses” at which they aim.* With certain exceptions, such as economic history and history of science, written history has not ordinarily supposed special information or training on the part of its readers. Of course, historians write for one another in the sense that they seek to meet professional standards of competence; but few historians have abandoned the hope of educating a general audience *directly* in the knowledge produced by their inquiries rather than of serving the lot of us indirectly by applying the results of their labors — as does, say, a biologist or an econometrician. Traditionally, historians have believed that whatever utility may be ascribed to historical knowledge accrues from its intellectual *possession*, not from its conversion into techniques, as mathematical principles can be converted into slide-rules. The phrase “applied history” is at best a metaphor. But historians have not without reason believed that they are in a pre-eminent way the stewards of the funded wisdom of the race, with a responsibility to communicate it widely. The very name of their discipline meant originally “to inquire, to know, to *tell what one has learned.*”

Yet the historian has a peculiar problem of organizing (not merely “selecting,” as is commonly said) what he has to tell, and not merely because he cannot reproduce exactly in the order of narrative the sequences and simultaneities of the events described. His problem becomes intelligible, however, if it is seen as an attempt to communicate his experience of seeing-things-together in the necessarily narrative style of one-thing-after-another. (It hardly needs saying that even if we possessed a time machine which enabled us to

sit before a screen and directly review the past in its minutest details, we should still need the act of historical understanding to make intelligible this bewildering panorama.) Under such circumstances, historical inquiry would no longer be necessary to search out and authenticate data of use to social scientists; but for exactly the same reason social science would still be unable to dispense with imaginative schemata of historical change which it could not produce from its own resources. Given the historian's problem, it is not surprising that he finds himself attracted to the possibility of theoretical understanding, which is also a way of seeing-things-together — things, that is, which are instances of the same law or realizations of the same model. But this is a different way, and to adopt it as a logic of inquiry is not a solution of the historian's peculiar problem but a denial that the latter exists at all.

Thus it is proper for the historian to "tell what he has learned," not what he has demonstrated. His readers, including most of his colleagues, may be at his mercy so far as the authenticity of his "facts" is concerned, but he stands before the bar of his audience so far as his judgment is concerned. His judgment is, so to speak, perfectly transparent; one looks through his ostensible conclusions to the total argument which is their meaning. The proto-scientific view correctly objects that "plausibility" is no criterion of truth. The feeling of plausibility may "authenticate," as in better historical novels, narratives of events which never occurred; and it may very well rule out as implausible (like the Irishman who exclaimed, "It's a lie!" when he saw his first swordfish) events of whose occurrence the evidence leaves no reasonable doubt. But plausibility is subject to degrees, and it becomes a better criterion as we learn more of the circumstances of the event. It is perhaps implausible that an emperor should abdicate and enter a monastery; it is more plausible when we know more, not about the statistical behavior of emperors in general, but about Charles V. It is plausible that Hitler should have launched an invasion of England in 1940; it is less plausible when we learn more about Hitler's strategic plans, his own estimate of German and British capabilities and his beliefs about British morale.

In the way it functions, historical judgment seems very like Aristotle's *phronesis*, a kind of sagacity or "practical wisdom." Like the latter, it is concerned with ultimate particulars and is an "intellectual" virtue, "possessing truth by affirmation or denial"; and like the latter, it can be a way of grasping a practical rule or principle which cannot be explicitly exhibited as the basis for a mechanical method. "Therefore we ought to attend to the undemonstrated sayings and opinions of . . . people of practical wisdom not less than to demonstrations; for because experience has given them an eye they see aright."¹⁵

15. *Nichomachean Ethics* 1143^b 11-13.

So historians may be wiser than they can say, but only if we hear what they have to tell, and not insist that they hand out piecemeal, like the slips in fortune cookies, tested hypotheses as “what history teaches.” Moreover, there is some danger that out of respect for “cumulative analysis” and wistfulness for the rewards of group research historians may try to imitate the success of the sciences in finding within the division of labor useful jobs for second-rate as well as first-rate abilities. So long as the historian seeks synoptic judgment, difficult to attain and more difficult to communicate but distinctive of his inquiry, there is no substitute for the alembic of his mind, and no efficiency in detachable conclusions.

III

A common theme runs through all six of the characteristics of historiography on which I have tried at least to focus attention: the idea of “historical synthesis” or “interpretative history”; and it is the special character of this as a mode of understanding which a theory of historical knowledge must recognize if the methodological autonomy of history is to be justified or preserved. Both the proto-science interpretation and the abundant criticisms of that interpretation have discreetly skirted the difficulties of this idea by limiting their attention to the explanation of discrete events, and the result has been to polarize the available accounts of explanation. On the one hand, the proto-science view finds historical explanations highly imperfect but implicitly deductive: the fall of a kingdom is more complex than a track in a Wilson cloud chamber, and the theory available for its explanation much thinner, but they differ in degree only, not in kind. On the other hand, critics of the proto-science view tend to regard historical explanations as common-sense explanations which just happen to be about interesting facts more remote than yesterday. As Scriven says of historical explanations: “There they are, simple and unadorned, logically no different from those [explanations] in common-sense talk about people and physical objects . . .”¹⁶ The assumption is that historical thinking is really nothing but common sense and a lot of factual research; one “explains” Caesar’s actions as one would those of one’s friends, and is historical only in finding out what Caesar’s actions in fact were.

The proto-science view does not claim to provide an explicit model for everything which might be advanced in some context as an “explanation.” Its power is prescriptive rather than descriptive, and it can legitimately argue that the concept of explanation can no more be clarified by regarding every putative explanation as in order as it stands than the concept of force can be made usefully precise by a definition which accounts for every appearance of the term in ordinary usage as well as for its theoretical meaning in physical

16. Michael Scriven, “Truisms as the Grounds for Historical Explanations,” in *Theories of History*, 458.

inquiry. Moreover, there is no *a priori* reason to deny that *every* event is subject in principle to hypothetico-deductive explanation *if* that model of explanation has the prescriptive right to determine what shall *count* as an event. Historians should realize that this is the nerve of the issue between the philosophical proponents of the proto-science view and its philosophical critics, and that it is essentially identical with the issue over behavioral psychology and sociology: can an instance of intentional *action* be analyzed without remainder into an instance of operationally describable *behavior*? This is not an empirical question but a conceptual issue: the dispute is over what *counts* as an event. Both parties to the dispute, however, agree that no events are characteristically and peculiarly *historical* in any sense other than that they are past.

I have tried, therefore, to raise a different problem, and to ask whether “history” differs from “science” *not* because it deals with different kinds of events and *not* because it uses models of explanation which differ from — or may include but go beyond — the received model of explanation in the natural sciences, but because it cultivates the specialized habit of understanding which converts congeries of events into concatenations, and emphasizes and increases the scope of synoptic judgment in our reflection on experience.

Now synoptic judgment is not a *substitute* for a methodology, any more than “empathy” is a substitute for evidence; and acknowledging it leaves open the questions whether “interpretative syntheses” can be logically compared, whether there are general grounds for preferring one to another, and whether there are criteria of historical objectivity and truth. So far it is only an attempt to identify what distinguishes sophisticated historical thinking from both the everyday explanations of common sense and the theoretical explanations of natural science. But the problems which remain in the elucidation of interpretative syntheses are problems of moment, because it is in such synthesis that history can stand apart, if at all, from the lure of the abstract and the tyranny of the particular, from both the computer and the data-retrieval system. And it may be in the understanding of synthesis that historians and philosophers can reach a synthesis of understanding.

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