THE OBJECTIVITY OF HISTORY*[1]

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Can history be objective? Is history a science or humanistic discipline? What is its subject-matter? These three questions are variations on a single theme—the objectivity of history—which I want to explore. Faced with the welter of claims and counter-claims regarding objectivity in history, there is need to be explicit about one's approach to these claims. My prime endeavor in this paper is to reformulate these questions from my scheme of reference. I want to consider the objectivity of historical knowledge from a framework that does justice both to philosophic and methological issues and to historical knowledge-claims themselves. How this philosophic framework be labeled is immaterial. What is alone important is that it does distinguish philosophy proper from both science and its methodological analysis.[2] Confusion between the three, I believe, frequently generates problems out of whole cloth. The result is that obfuscation of issues present in much recent literature concerning history.

Specifically I want to introduce into our discussion a conception of the *methodology of empirical historiography* paralleling positivist treatment of philosophy of science. Insofar as historiography involves empirical knowledge, we may, I think, profitably consider its methodology in this sense. Consider the historian's use of documents and artifacts, his heavy reliance on common-sense knowledge and those sciences, from archeology to zoology, whose validity he takes for granted. Dates of events are ascertained. Authenticity of artifacts is documented. Chronicles are written. Biographies and narratives of individuals and institutions are composed. Causal explanations or sketches of such explanations are found. In all these tasks the historian can be scientific or proto-scientific.

Like the natural scientist, our empirical historiographer simply builds upon common sense. He has no doubts about the reality of his world. He takes it for granted that events, people, and societies can become the objects of historical inquiry. Rather than a problem, knowledge is a natural fact like breathing or seeing. Though more difficult to acquire, knowledge of the past beyond memory is likewise a fact involving only technical difficulties. As historian, we might say that he is realistic by animal faith, but his realism is not a philosophic position because his language cannot, for want of philosophic terms, even formulate traditional epistemological and metaphysical issues. Such issues are, for him, in this sense unproblematic or noncontroversial. All this I shall take to be obvious for our discussion.

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[2] Elsewhere I have attempted to sketch differences between philosophic and methodological analysis and scientific inquiry by reference to a meaning criterion. See "Levels of analysis," *Philosophy and Phenomenological Research, 11:* 213–220 (1950).

Methodology of historiography, as here conceived, does not seek to undermine or question this animal faith but, rather, to describe analytically the nature and structure of history's concepts, and of the statements, hypotheses, generalizations and causal explanations in which these concepts occur. Hence the methodologist consciously commits himself to the "natural realism" of the historian by ordering the concepts of history with respect to a meaning basis that restricts its primitive or undefined terms to individuals and their overt behavior which is or was immediately observable by anyone present. Theoretical or methodological questions are raised and answered in the only manner available to the empirical historiographer himself. Issues within philosophy proper, however, are not matters of conjecture or controversy since, borrowing as the methodologist does from the unanalyzed notions of common sense, no philosophic question can be formulated.

The methodologist's analyses are, likewise, philosophically neutral in that they do not prejudge one's epistemological or metaphysical position. Conversely, however, it seems to me that a philosophy of history must be able to "square itself" with the results of the methodology of empirical historiography.[3] Any philosophy of history that will not or cannot accomodate itself to these results hardly merits our serious attention.

Judging from the success of positivist methology of science in clarifying relativity and quantum theory, and concepts like probability, confirmation, and explanation, I believe I am justified in here suggesting its extension to historiography and thence to some issues under discussion.

Having talked so much about one of the ways we might talk about historiography, let us revert to one variation on our theme and ask again, Can history be objective? In terms of our framework, this question often merges a philosophic with a methodological question. First, there is the epistemological question, Is knowledge of the past even possible? And second, there is the methodological question, How do selection and evaluation modify the reliability of historical knowledge? When these two questions are merged, we might expect the odd answers of the various strains of historical relativism. The starting point of the historian, says Toynbee (for example), is ". . . the axiom that all historical thought is inevitably relative to the particular circumstances of the thinker's own time and place. This is a Law of Human Nature from which no human genius can be exempt."[4] If history teaches us anything (a question not now at issue), then surely the recent history of the sociology of knowledge has a lesson for us to recall as we consider this axiom.

In another place,[5] I have tried to show that Karl Mannheim's attempts at a

[3] Compare Bergmann's parallel statement, from which I borrow, regarding the obligation of any philosophic position to "square itself" with the results of the methodology of physical science, in his "Frequencies, probabilities, and positivism," *Philosophy and Phenomenological Research, 6:* 26–44 (1945).

[4] Arnold J. Toynbee, *A Study of History, vol. III*, 2nd edition (London: Oxford, 1935), p. 476.

[5] "The Epistemological relevance of Mannheim's sociology of knowledge," *Journal of Philosophy, 40:* 57–72 (1943); "Epistemological relativism and the sociology of knowledge," this journal, *15:* 4–10 (1948).

"sociological theory of knowledge" are doomed to failure. Where they apply, I want to suggest the same strictures of Toynbee's variant of historical relativism. For it is simply a modification of Mannheim's sociologism which, in turn, is a generalization of Marx's "particular conception of ideology" developed in his economic historicism.

"With the emergence of the general formulation of the total conception of ideology," says Mannheim, "the simple theory develops into the sociology of knowledge. What was once the intellectual armament of a party is transformed into a method of research in social and intellectual history generally."[6] ". . . the function of the findings of the sociology of knowledge," we are informed, "lies somewhere . . . between irrelevance to the establishment of truth . . . and entire adequacy for determining truth . . . "[7] Sociology of knowledge, Mannheim insists, has two branches: a theory of the social determination of knowledge and a sociological theory of knowledge that will replace the prevailing idealistic epistemology.

I do not wish to whip a dead horse but, after surveying much of the recent literature, I find that, though occasionally enfeebled, this horse is not quite dead. If we agree with Mannheim that the entire structure of one's thought is ideological, then even science, especially social science and history, becomes necessarily bound to historical social position and is consequently invalid. Such relativism, however disguised, is self-referentially inconsistent. How can one find out about the social determination of knowledge if even the validity of the attempt is historically conditioned? Stark observes that social determination ". . . raises a problem of knowledge. . . which is as significant and as intricate as anything classical epistemology has tackled in the past."[8] Granted. But how did he discover that social classes are circles "closed against each other", from *his* historically determined closed circle? Mannheim himself is not only referring to the familiar personal and social equations but to the *necessity* of significant bias in every situation where "vital interestedness" occurs.

Mannheim's attempted escape from relativism (by syntheses of many perspectives by socially unattached intellectuals) is, though invalid, insightful. Two other attempts that are not so well-known appear in his early essay on historicism. They have, I think, particular relevance for the issue at hand. "To say that the absolute itself," Mannheim writes, "is unfolding in a genetic process, and that it can be grasped only from definite positions within the same process, in categories which are moulded by the unfolding of the material contact of the genetic flux itself—to say this is not tantamount to professing relativism."[9] However valid as claim, this assertion well illustrates Mannheim's philosophical historicism. He is here, obviously, the philosopher, not the empirical scientist. If, following Hegel and Troeltsch, Mannheim has correctly postulated or posited

[6] Karl Mannheim, *Ideology and Utopia* (New York: Harcourt, Brace, 1936), p. 69.

[7] *Ibid.*, p. 256.

[8] W. Stark, "Towards a theory of social knowledge," *Revue Internationale de Philosophie, 4:* 287–308 (1950), p. 290.

[9] Paul Kecskemeti (ed.), *Essays on the Sociology of Knowledge* (New York: Oxford, 1952), p. 310.

(I am not sure which), then historical truth and knowledge will only be "dynamic" approximations to the absolute. But here we have a posit or guess promoted to the status of true philosophic world-view which, on the very account of knowing that such a view offers, it would be impossible to validate.

Mannheim's other attempted escape is put in the question: ". . . what if it can be shown that the accusation of relativism derives from a philosophy which professes an inadequate conception of 'absolute' and 'relative': a philosophy which confronts 'truth' and 'falsehood' in a way which makes sense in the sphere of so-called exact science, but not in history, since in the latter there are aspects of the same subject-matter which can be regarded, not as either true or false, but as essentially dependent on a given perspective or standpoint which can co-exist with others?"[10]

This attempt takes us to our second question regarding objectivity, but first let us briefly summarize, from our framework, what we can at this point conclude about Mannheim's claims and Toynbee's axiom. Inquiry into the definition of truth and knowledge—and, hence, inquiry into *epistemological* relativism—lies beyond the province of history and the sciences themselves. Here the sciences must be dogmatic and the issues concerning the very possibility of knowledge unproblematical. Otherwise, how would the sociologist of knowledge (or historian) ever *know* that (say) the thought of a given society was historically determined? The scientist and empirical historian are properly concerned only with what was held or believed to be true; otherwise, they become easy prey to a genetic fallacy that invalidates even their factual claims. There remains, of course, a substantive sociology of knowledge and history with important functions of their own.

Anyone, moreover, who ascribes a metaphysical theory of truth to science (or to history insofar as it is or relies on science) forgets its testable, confirmable, operational or natural "realism." When pressed for justification of that objectivity which science takes for granted, even the methodologist can only reply by appeal to "brute fact" or, after analysis, with a stipulative answer that makes manifest the latent common sense upon which the scientist and historian build. Methodologists sometimes speak here of the pragmatic vindication of such basic characteristics of common sense and science. But such vindication is only that which the Attorney General's Office might put in the form: "We regret to inform you that we have insufficient information to warrant placing 'objectivity' on our list of subversive characteristics of science, history or common sense."

We have now to consider our second question about objectivity in history, namely, How do selection and evaluation modify the reliability of history? For historical as well as analytical reasons, our consideration of this question will encroach on our remarks about the two other variations on our theme—the subject-matter and nature of history. Pragmatists have long stressed the *presentness* of history. But how can history, presumably about the past, be reliable if it is only history of the present? ". . . all history," Dewey asserts, ". . . is, in an inescapable sense, the history not only of the present but of that which is con-

[10] *Ibid.*, p. 93.

temporaneously judged to be important in the present."[11] "It is idle," observes Mead, "to have recourse to a 'real' past . . .; for that past must be set over against a present within which the emergent appears, and the past, which must then be looked at from the standpoint of the emergent, becomes a different past."[12] Such emphasis has, from our methodological viewpoint, been aptly labeled "historical impressionism". What is significant, however, is that Dewey and Mead are in this context philosophers evidently, as Lovejoy suggests,[13] deducing consequences from the ontological postulate of the internality of all relations, as well as invoking that acute form of the ego-centric predicament that he has tagged the "presenticentric predicment".

The philosopher who merits serious consideration, we have said, must "square" his metaphysic with the results of the methodologist analyzing historical inquiry. As methodologists, let us ask a question or two. Do we find the empirical historiographer building upon a natural belief in the internality of all relations? Do we find ourselves stipulating that all relations are internal? Do we see the empirical historian formulating a principle of historical indeterminacy as Mead implies he must in *his* assertion that every selection alters the past events selected? Hardly. As epistemologists, moreover, we might with Lovejoy[14] warn the pragmatist that his appeal to the presenticentric predicament proves too much since all knowledge rests upon history of the sort he declares impossible. In this regard, pragmatism cannot even "square itself" with itself.

Surely the historian, as we all do, "reconstructs" the perished past. Strong's suggestion, that the historian relies on a "rule of empirical equivalence",[15] is helpful in indicating how both commonsense conceptions of correspondence and coherence function as criteria of knowledge of the past. His rule is applied in a context which properly takes for granted the possibility of our knowledge of the past and is, thus, a rule of reliability. Dewey and Mead, on the other hand, appear to confuse the issue by mixing philosophic with scientific matters. I conclude, therefore, that all history is, in an inescapable sense, the history of the past. Its subject-matter is, in this sense, the irrevocable though recoverable past.

Let us momentarily revert to our second question concerning objectivity in the form which Mannheim suggests. He implied in his question that, unlike the exact sciences, certain aspects of history are neither true nor false but essentially dependent on mutually compatible perspectives. Earlier we tried to specify this alleged characteristic of history by asking, How do evaluations modify the reliability of history? Mannheim himself appears to answer his own question for us. The later Mannheim, according to Merton's interpretation,[16] reinvoked the Rick-

[11] Quoted by Arthur O. Lovejoy, "Present standpoints and past history," *Journal of Philosophy, 36:* 477–489 (1939), p. 478.

[12] George H. Mead, *The Philosophy of the Present* (Chicago: Open Court, 1932), p. 2.

[13] Lovejoy, *op. cit.*, pp. 485–6.

[14] *Ibid.*, p. 487.

[15] Edward W. Strong, "The materials of historical knowledge," Ch. 8, esp. pp. 157–8; p. 171, in Y. H. Krikorian (ed.), *Naturalism and the Human Spirit* (New York: Columbia, 1944).

[16] Robert K. Merton, "Karl Mannheim and the sociology of knowledge," *Journal of Liberal Religion, 2:* 125–147 (1941), p. 144.

ert-Weber concept of *Wertbeziehung* or the value-relatedness of historical facts, when in his essay "The Sociology of Knowledge", he characterized his position as *relationism* rather than relativism. Rickert had held that value-relatedness is the selective principle in historical science. Although in the choice of historical materials and explanations the historian is primarily guided by given values, the validity of his results, Rickert maintained, remains unaffected.[17]

Let us amplify this answer which, as methodologists, we find essentially sound. As in the case of truth, the empirical historian of ideas or societies will (so to speak) "mention" but not "use" common-sense value terms in his accounts. He is properly concerned only with what is believed or held to be right or valuable, and he can surely perform substantial service in distinguishing the factual from the evaluational in ideologies of given societies. Moreover, methodologists can, as Strong urges,[18] discriminate "thesis-rigged" history from warranted history and, I should add, they can identify the value biases of the historian who rigs his history.

But perhaps there is another face to the coin of history—history as "humanistic discipline" (in Panofsky's sense)[19] or even history as "creative imitation" (in Child's sense).[20] As philosophical analysts, we must not, in considering the history of (say) art as a humanistic discipline, confuse the psychology of the historiographer (about which we know little or nothing) with the logical analysis of intellectual and emotional products, and neither of these with present or ultimate limitations of scientific methods. Regarding history as creative imitation, in addition we must recognize that much history is not strictly cognitive, either in intent or result. Like music, art, and fiction, its significance lies predominantly in its satisfaction of man's derived needs. Man can (presumably) be enriched by fact, conjecture, fancy, and the purely expressive and creative. But let us neither mistake this kind or aspect of history for what it is not, nor underemphasize any particular historian's ingenuity or imaginativeness. If stimulation and enrichment is what we crave, however, we can equally well listen to music which, for the most part, raises no questions about subject-matter, objectivity or reliability.

There remains one final remark about the subject-matter of empirical historiography. I want to comment, in terms of our framework, on one of Mandelbaum's recent contentions. If I read him aright, Mandelbaum (to whom we all owe so much) asserts that material philosophy of history, which attempts to interpret the historical process itself, presupposes formal philosophy of history, which examines the objectivity and nature of historical knowledge. In turn, formal philosophy of history makes assumptions drawn from critical social

[17] Compare Felix Kaufmann, *Methodology of the Social Sciences* (New York: Oxford, 1944), p. 211.

[18] See Strong, "Fact and understanding in history," *Journal of Philosophy, 44:* 617–625 (1947), esp. p. 624.

[19] Erwin Panofsky, *Meaning in the Visual Arts* (Garden City, N. Y.: Doubleday Anchor, 1955), esp. the Introduction, "The history of art as a humanistic discipline."

[20] Arthur Child, "History as imitation," *Philosophical Quarterly, 2:* 193–207 (1952), esp. pp. 193–4.

philosophy. In particular, for example, social philosophy considers the problem whether psychological concepts provide an adequate basis for explaining sociological phenomena.[21] Mandelbaum has recently reformulated this issue as likewise one of the major problems facing a critical philosophy of the social sciences by maintaining, against the "reductionists" or "integrationists," that there are " 'societal facts' ["concerning forms of organization present in a society"] which are as ultimate as are those facts which are psychological in character" [namely, "any facts concerning the thoughts and actions of specific human beings"].[22] His avowed practical purpose is, as social philosopher or philosopher of science, to indicate that integration of the social sciences, with respect to concepts and methods, "is a mistaken goal for sociologists and psychologists to pursue."[23]

"While any practicing scientist," declares Mandelbaum, "may (presumably) adopt a position of neutrality on all of the philosophical issues which his discipline raises, a philosophy of science cannot bracket ontological and epistemological problems."[24] The gist of what we said earlier concerning methodology of science and philosophy proper at least suggests an alternative approach. Our methodologist must (so to speak) maintain a neutrality on all philosophic issues since he finds no philosophic terms in science or common sense with which to formulate them.

Matters of proper terminology aside, what worries me is just how, and how much, philosophy proper can legislate for the empirical sciences. We have argued that, judging from its fruitfulness, methodology of science might profitably be extended to empirical historiography. In facing the problem of historical relativism, we tried to show how, from the methodologist's vantage point, an alleged problem dissolves. But we proceeded to sketch the relevant epistemological analysis when we found philosophical arguments compounded with matters concerning scientific fact and the structure of science. It seems to me that we are here faced with a problem of like complexity which cries out for our framework. Much of what Mandelbaum says against the reducibility of societal facts to psychological facts is integral to any analysis, but frequently I find it difficult to keep distinct his factual claims from his philosophical arguments.

I mean to suggest that attempts, like this one, to indicate on mixed grounds the impossibility of integration because of irreducible concepts is perhaps putting the cart before the horse. Such grounds, if epistemological and ontological, stem from a philosophic commitment. And our earlier admonition applies, namely, that a social philosophy or a philosophy of science (in Mandelbaum's sense) will itself stand or fall depending on its ability to "square itself" with the results of the methodology of science. The results are, in the case of the question of reducibility of concepts, laws, or theories, admittedly less determinate than we might wish. They are, moreover, beclouded by advocates who themselves tend to jumble philosophic with methodological issues, and these with questions which

[21] Maurice Mandelbaum, "Some neglected philosophic problems regarding history," *Journal of Philosophy, 49:* 317–329 (1952), p. 328.

[22] "Societal facts," *British Journal of Sociology, 6:* 305–317 (1955), p. 307.

[23] *Ibid.*, p. 306.

[24] "Some neglected philosophic problems regarding history," p. 329.

might, if accurately specified, some day be answered by science. Nevertheless, I wonder what philosophers like Mandelbaum would do if, as in physical theory, genuine—though perchance partial and temporary—reductions were effected in the social sciences.[25]

Perhaps, however, concerning such indeterminate issues I am simply taking my stand with those who, while eschewing scientism, feel far less urge to make science, and hence historical subject-matter, "metaphysically safe". Conversely, as empiricist philosopher, I find that my metaphysic most nearly "squares itself" with both science and the results of its methodological analysis and with that common sense upon which science builds.

[25] For a perspicuous discussion of reduction in science, see Ch. 3, "Configurations and reduction," in Bergmann's recent *Philosophy of Science* (Madison: Wisconsin, 1957).