M. M. Postan. Fact and Relevance: Essays on Historical Method. Review

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nects the nervous activities with which our mental events are correlated and the spatio-temporal events which occur in the rest of the universe. There is likewise no contradiction with regard to the possibility of our performing what we call voluntary activities. These activities are not initiated by the mental states that we call will, but by their neurophysiological correlates. Their determinism is therefore again exclusively that of physics. (p. 115)

Since Dr. Rosenblueth does call his view “dualistic” (pp. 114–115), and since he never elaborates on that of which the mental and the physical are each “aspects,” I take it his view is really epiphenomenalism.¹ The mere fact that I cannot be sure what it is, illustrates my point that scientists philosophizing on a problem to which scientific knowledge is relevant are unlikely to have acquired the perspicuity necessary to see through all the issues involved.

But I shall close my review on a remark Rosenblueth would perhaps appreciate, since it is not philosophical but scientific. If indeed there are no brain-mind interactions, i.e. no cases of mental events causing neurophysiological changes in the organism, then why did mental events evolve as accompaniments of neurophysiological activities in man and certain other higher animals? There would be no selective pressure for their occurrence, to a chimpanzee any more than to a planarian, if they were devoid of biological effects. That mental events should be gratuitous to survival is possible, but that they should be discriminately gratuitous is improbable. A truly scientific account of the mental would have to explain this disparity. How one could do that without invoking mental causality, hence interactionism, is not clear to me; and how could one expand the laws of physics to accommodate mental events is not either. But perhaps Dr. Rosenblueth’s scientific successors will find time to enlighten us. Roland Puccetti, Dalhousie University.


This book is a collection of fourteen essays by the distinguished Cambridge economic historian, M. M. Postan. The essays range all the way from his inaugural lecture to book reviews and cover a period of about thirty years. He tells us in the preface that these essays have been “selected and brought together on the strength of their common theme” (p. ix) viz. the interrelation of history and the social sciences. This collection, according to Postan, reasserts a point of view—a point of view he thinks needs reasserting in the 1970’s—which he labels “positivistic” or “scientific.” By this Postan seems to mean the point of view that maintains that the methods and findings of science—in particular the social sciences—are relevant to the study of history and that history is not a humanistic study.

There is no doubt that such a point of view does need to be reaffirmed and that Postan’s essays are urbane and contain polished arguments for the relevance of social science to history. His essays are by and large rather superficial and naive from a philosophical point of view, however. Judging from them it seems doubtful that Postan has read anything in contemporary analytic philosophy of history. Yet his handling of many of the problems he deals with could have benefited from this sort of philosophical literacy.

Postan argues that the difference between historical study and scientific study is the result of different mental attitudes to the world and is not inherent in the subject matter.

What makes the material fact a fit object for scientific study is that men are prepared to treat it as an instance of a generic series. What makes a social phenomenon an historical event is that men ask about it individual or, so to speak, biographical questions. But there is no reason why the process should not be reversed: why we should not ask generic questions about historical events or should not write individual biographies of physical objects. (p. 13)

Postan further argues: “The possibility of social science, and of social generalization in history, turns on whether the historian like the physicist is willing to subordinate his study of the

¹ Just to complicate things, the publisher’s blurb provides this description of Rosenblueth’s theory: “Brain and mind function is parallel—on separate levels of reality that are in constant relation to each other but that do not interact.” But surely he did not advocate parallelism: the laws of physics would then have nothing to do with mental correlates of neurophysiological phenomena.
unique to his search for the general” (p. 18). But in any case historians' preoccupation with the unique and concrete is “very largely fictitious” (p. 18). First, historians like scientists abstract from the concrete historical situation; they merely abstract less than scientists (p. 31). Second, even when the degree of abstraction of historians seems very little, appearances may be deceptive. For example, Weber’s real interest was with “general correlations of religious ideas and economic development” although he seemed to be concerned only with the Puritan Ethic in 17th and 18th century Europe (p. 19).

Indeed, it is this study of the general in the particular that is characteristic of good historical research and that at the same time distinguishes historical study from scientific study. Historical research should be made, to use Postan’s expression, “Microcosmic—capable of reflecting worlds larger than themselves” (p. 32). The historian should not try to formulate explicitly general laws or theories. His work must “appear” to be concerned only with the concrete; he must write “as if” his subjects are individual and unique. Yet the study must implicitly “reach out of the individual and the concrete to the general and universal” (p. 62).

Following Postan let us call this approach to historical research “the micro-cosmic approach.” What is the relation of Postan’s microcosmic approach to the covering law controversy in contemporary philosophy of history? It would seem at first blush that Postan is squarely in Hempel’s camp as against people like Dray. Yet although it is likely that Postan’s sympathies are more with Hempel than with Dray, as I understand him his position is rather different from Hempel’s. Postan at one place suggests that “no laws except perfect ones bear being put into words” (p. 19) and that the laws implicit in historical study are incapable of being stated and are only “implied” (p. 19). This suggests something different from Hempel’s view. Hempel thinks that some laws used in history can be articulated and he certainly thinks that rough approximations of all laws used in history can be articulated in, e.g. explanation sketches. Postan seems to deny this.

Postan recommends that historians write history so that the general truth is implied in their work: the general truth he has in mind is some social scientific theory of generalization. Hempel is not giving advice about how historical research should proceed nor is he recommending the use of exemplification of social science and history. Still another difference between Hempel and Postan is that: according to Hempel general laws are implicit in historical study in the sense that such laws are presupposed. But this does not mean that such laws are implicit in the sense that historians are trying to prove or illustrate some general point in their concrete works. Postan argues however, for example, that Mcllwain’s “avowed subject” is a certain phase in legal and constitutional development in the middle ages; however his actual concern is the general transformation of custom into law (pp. 19-20). Hempel might consistently argue that Mcllwain’s avowed subject was the same as his actual subject but still maintain that general laws are presupposed. In sum, the microcosmic approach receives little support from the covering law view of historical explanation.

What can we say about Postan’s microcosmic position in its own right? There are at least two serious problems with Postan’s thesis. First, it is difficult to understand how one can know what general truth is implied—or indeed even that there are general truths implied—in historical research which proceeds “as if” it is concerned with concrete historical reality unless one can articulate, at least in a rough way, what these general truths are. Although Postan is critical of antirationalism in historical study his views on the unstatability of the general laws implicit in history leave him open to similar critical comment. For how could one know there are general truths which cannot be articulated except by intuition—an approach that Postan condemns as antirational (p. 1). To be sure, he does not speak of intuition in regard to his own position but his position would seem to force him into the intuitionist camp. Second, although for certain purposes it is good methodological advice to write history so as to exemplify some general thesis in the social sciences, this surely should not be the only model of historical research. It is unclear whether Postan thinks that it should be or whether he thinks it should be the basic model of historical research in economics. However, even in the latter case the thesis is dubious. Postan seems to assume at times that unless the historian has some general thesis to push under the guise of a study of some concrete historical period, history will be a mindless accumulation of facts—the work of what he calls an antiquarian (p. 25). But of course this is not so. Narrative history, for example, is often neither the sort of microcosmic history advocated by Postan nor the antiquarian history he criticizes.

Postan is, I believe, opposed to the pursuit of narrative economic history. He advocates instead problem oriented historical research: the historian approaches history with some particular question to answer rather than with the idea of giving a general narrative account of some period. Now whether a narrative approach should be rejected in economic history is unclear and Postan does not give very compelling arguments for rejecting it. In any case there is no necessary relation between the problem oriented approach Postan prefers and the microcosmic approach he advocates. The problem that sets the stage for historical research could be very particular and specific with no particular theoretical origin or motivation. Surely this could be true even in economic history.

Postan advocates that in history—or at least in economic history—social science should set the problems that history answers. But even this particular problem oriented approach to history is independent of the microcosmic approach. For if social science does set the problems that history answers, it does not follow that historical study should exemplify some general truths of social science under the guise of studying some concrete social event. The question that social science posed for history might be a very particular one, e.g. a question about particular business conditions in the predepression era of the 1920's.

It would seem to be possible for history to be concerned with a question suggested by an economic theory without its being written to exemplify that theory. For one thing a historian may investigate whether certain business conditions obtained in the 1920's because of some relevant economic theory and yet not write a history in which this general theory is implied or suggested to his readers. On the other hand, a historian may write a history of business conditions in the 1920's and explicitly state the theory which suggested his investigation. Both of these possibilities are apparently ruled out by Postan's microcosmic approach. In sum, a problem oriented approach to economic history—even when the problems are set by social science—is independent of the microcosmic approach. This point seems to be blurred in Postan's thought.

Although we have criticized at some length Postan's view of microcosmic history it is difficult to know how seriously to take his remarks advocating this approach. For when he occasionally gets down to saying in some detail what economic history should be doing with respect to the social sciences the microcosmic approach seems irrelevant. For example Postan argues in his inaugural lecture that the contribution of economic history to economic theory should be to complete and fill out the conditions under which the very abstract and formal theories of economics apply; to specify in detail the "other things being equal" clause used in economic theorizing (p. 27). However, there is nothing in this interesting recommendation to suggest that economic history must reflect the general in the particular, i.e. that it should be microcosmic.

In another essay he argues that history should not be so preoccupied with "linked historical sequences" (p. 37), that is in tracing one historical event to some earlier event causally relevant to the later event. Postan suggests that historians pay more attention to functional relations of one phenomenon to other phenomena occurring at the same time in historical periods; historians should show how one phenomenon "fitted into other phenomena of the same age" (p. 47). This approach Postan argues "will do more for our understanding of mankind than the spinning of facts with their antecedents into evolutionary yarns" (p. 42). This functional approach to history Postan borrows from the functional approach in social science, in particular social anthropology. Postan seems unaware however of the criticisms of functionalism by philosophers and social scientists. In the light of their criticisms whether his suggestion is a wise one is unclear. But in any case it seems to have nothing to do with his microcosmic approach.

I have suggested that Postan might have profited from some knowledge of contemporary analytic philosophy of history. However, it is also true that philosophers of history could profit from reading Postan. Too many contemporary philosophers of history seem to have too narrow a view of what historical writing is; this is perhaps largely due to a steady diet of one kind of historical writing—roughly general and narrative histories—where theory is scarce and social science insights are uncommon. Postan's experience as a historian comes from an entirely different tradition: economic history. Here theory is everywhere and social science finds easy application. Philosophers of history might well profit from considering the logic of this sort of

history; it may be rather different from the sort they are used to. Michael Martin, Boston University.


Many recent works which ostensibly treat of the methodology of social research dwell on issues in the philosophy of science which seem largely irrelevant to the practical requirements of theory construction in sociology, political science, anthropology, and many aspects of psychology. Researchers in these disciplines had better repair to a work which introduces mathematical and conceptual techniques drawn largely from econometrics if they desire practical light as opposed to philosophical heat. Blalock's book is just such a work. It presents a reasonably clear and simple introduction to those techniques originally developed in econometrics which are likely to be of service in the other social sciences. Not only are the techniques introduced but they are usefully applied to a range of examples, in a way which enables a theorist, (with very little more training than Blalock's book, itself, provides) to begin to apply the suggested methods.

Of course the very features which make the volume so useful to the social scientist make it much less philosophically contentious. There are few philosophical claims or assumptions in Theory Construction and consequently fewer objections for this reviewer to draw up. If these are dwelt upon, it should not be supposed that the book is mistaken in any uneliminable way.

Blalock begins by trying to draw up some precepts for determining which propositions of an informal account of a theory ought to be selected as the axioms of a more formal presentation. These nonformal theories are alleged to be presented in terms of covariances of factors without any special care to indicate the causal ordering of the factors cited. Some popular works on the methodology of the social sciences advance the view that the proper choice of axioms here is dependent on which choice results in the simplest theory. But this answer turns out to be unhelpful since no one has yet presented an adequate measure of simplicity upon which such a choice can be made. What is required is a more practical, if less general, answer. Blalock's answer is to select as axioms those propositions in which the factors cited seem to have "direct causal links." This answer is practical, for theorists always seem to have intuitions about the causal links involved; it seems an admissible method so long as the possibility of testing proposed axioms is open. It remains for the philosopher to ferret out special difficulties about this proposal, but I daresay, following it will often result in a relatively simpler theory than many other applicable precepts.

The immediate problem about the proposal is that in Blalock's view axioms are not testable, and this, if true, militates strongly against the proposed precept:

Our axioms should be causal assertions that strictly speaking will be untestable because of the fact that it will never be possible to control for all "relevant" variables ... we can never be sure that [covariations] have not been produced by some extraneous factor. But if our axioms contain such causal assertions, and if we make certain additional assumptions concerning the operation of extraneous factors, we shall then be in a position to derive from our axioms testable theorems about covariances and temporal sequences. (p. 11)

Of course, this passage indicates that the real culprit of Blalock's difficulty is an idiosyncratic use of 'testable'. It sounds as though the only sort of testability Blalock recognizes is direct testability. For in this passage he claims that causal propositions are not testable, and then goes on to stipulate the circumstances under which observable consequences can be deduced from them, that is, circumstances under which causal propositions are testable (albeit, "indirectly")). Moreover, his demands on testability are far too strong. There is hardly any general proposition at all for which we can be sure that the observations that confirm it "have not been produced by some extraneous factor." In any case, we always need to "make certain additional assumptions concerning the operation of extraneous factors" whether we test "axioms" or "theorems." So it will turn out that on Blalock's use of 'testable' no general proposition is testable, since none is "directly" testable, each requires (different) auxiliary assumptions. But this is tantamount to saying that Blalock's difficulty is simply that he has misunderstood 'testability' to mean something akin to 'strict verifiability'.

Theory Construction goes on to canvass various causal chain schemata into which nonformally