"New" and Traditional Approaches to Economic History and Their Interdependence

Author(s): Fritz Redlich

Source: *The Journal of Economic History*, Dec., 1965, Vol. 25, No. 4 (Dec., 1965), pp. 480-495

Published by: Cambridge University Press on behalf of the Economic History Association

Stable URL: https://www.jstor.org/stable/2116124

"New" and Traditional Approaches to Economic History and Their Interdependence

THE purpose of this paper is to analyze the various new or current approaches to economic history. In so doing I reluctantly use the now widely accepted terms "new" economic, or "econometric," history considered by some authors as synonymous. In fact, however, there is both a broader and a narrower application of the phrase "new economic history." In the broader sense, the term embraces the work of the various authors who have in common as their aim theoretically underpinned quantitative economic history; in the narrower, it refers only to what I shall call the "model builders." Moreover the terminology seems to be in process of change.[1]

I

The new approaches, to use a neutral term, are both the phenomenon of a generation and a matter of *Weltanschauung*. When I speak here of generations I mean, of course, historical generations, which in distinction from biological ones can also be called groups of coevals. Men born and maturing at the same time are, under the pressure of the particular historical situation in which they grow up, molded into a community of problems. In the present case, men like Parker, North, Conrad, Fogel, Davis—all born within eight years of one another—belong to such a group. But already there is a younger generation coming along which proceeds farther on the road blazed by the former.

Secondly, the new approaches are based on a particular *Weltanschauung*. By this term I mean a certain articulate outlook on the world which is in itself consistent but neither provable nor disprovable. Research in this area has been badly neglected since

EDITORIAL NOTE. This paper was prepared at the invitation of the Program Chairman for a session at the annual meetings. The session had to be canceled after the paper had been completed, and it is the feeling of the editors that it should therefore be included in the Tasks issue.

[1] Papers and Proceedings of the Seventy-seventh Annual Meeting of the American Economic Association, Chicago, December 1964. The *American Economic Review,* LV (1965), 90, 91, 98.

the death of Wilhelm Dilthey, the great Berlin philosopher of history. In his time, the late nineteenth and early twentieth centuries, one could still distinguish between three *Weltanschauungen:* positivism; the *Weltanschauung* of freedom; and that of harmony, the latter having been held by Leibniz, by Adam Smith, and (deteriorating almost into a cartoon) by Bastiat. Two world wars have shown it to be untenable, and it has died out. Today one has to distinguish between two: positivism and antipositivism "according to their peculiar combination of ontological and epistemological components."[2]

The new approaches to economic history are definitely positivistic, in that for positivism nothing matters unless it can be counted, measured, or weighed. But the age-old empiricism also roots in positivism. Consequently the lines are strangely drawn. The new approaches, while positivistic at their roots, are antiempiricistic through their reliance on economic theory. Thereby they come closer to analytical economic history as practiced by some antipositivistic traditionalists than to the empiricistic approach to which they are related on the basis of the underlying *Weltanschauung.*[3]

II

Two elements are constitutive for the new approaches to economic history: one is the overruling interest in quantification, the other is the use of economic theory manifested by the reliance on hypotheses and figments, as will call for our attention later. The first element, quantification, is by no means new. Fogel pointed out in his Boston address of 1963 that the effort "to discover and present numerical information relating to historical processes" is not new; and Hughes's brilliant bibliographical article in the JOURNAL OF ECONOMIC HISTORY bears out that contention.[4] Yet between December 1963 and December 1964 Fogel changed his emphasis:

[2] Don Martindale, "Limits of and Alternatives to Functionalism in Sociology," in The American Academy of Political and Social Science Monograph No. 5, *Functionalism in the Social Sciences* (Philadelphia: The American Academy, 1965), pp. 149 ff.

[3] See also the illuminating remarks on the role of figures in the Enlightenment and in bourgeois society in Max Horkheimer and Theodor W. Adorno, *Dialektik der Aufklärung* (Amsterdam: Querido, 1947), 17, 18. I stress that the new economic history is not empiricistic, since I have found the statement that it "is coming close to what a modern empiricist might demand of it." See George G. S. Murphy, "The 'New' History," in *Explorations in Entrepreneurial History,* 2d ser., II, No. 2 (1965), 132.

[4] Robert William Fogel, "A Provisional View of the 'New Economic History,'" in *American Economic Review,* LIV (1964), 377; J. R. T. Hughes, "Measuring British Economic Growth," in JOURNAL OF ECONOMIC HISTORY, XXIV (1964), 60 ff.

now "the novel element in the work of the new economic historians is their *approach* [italics mine] to measurement and theory." Formerly, so Fogel goes on, one simply located and classified numerical information; now, however, the emphasis is on "reconstructing measurements . . . no longer extant," on combining primary data with measurements never made before, and on indirect measuring where direct measuring is impossible. It does not appear to me that these aims are held in common by the exponents of all the various new approaches.[5]

It is agreed that the quantitative approach to economic history is not new; it has been pointed out that Fogel's *specific approach* is not shared by all pertinent members of the young generation; finally, reliance on economic theory, where it was sufficiently developed to be useful, characterizes also the work of older analytical economic historians, for example in price, monetary, and banking histories and some other fields. Yet actually there is something new in what goes under the name of "new economic history" in the term's broader connotation, so that one must look for those elements which really distinguish between the traditional and the new approaches. In so doing I again disagree with Fogel.

The matter is rather complicated, as we will see, but one difference between the traditional and the new approaches can be characterized as follows: the old approach deals essentially with institutions, and with processes only to the extent that the latter took place within institutions. (Incidentally, the introduction of the *process* into economic history, or into historical economics if you prefer, was the work of Gustav Schmoller.) The new approach, in contrast, often goes directly at macroeconomic processes, thus disregarding institutions. If one wanted to put it pointedly, though by oversimplifying the problem, he could say: Traditional economic history deals primarily with the development of economic institutions and secondarily with processes taking place therein. The new approaches tend to deal primarily and directly with economic processes while more or less neglecting economic institutions.

This switch in approach became possible only in about the last twenty years when a genuinely dynamic theory, that is, a theory of economic development, rose beside the traditional static theory, largely useless for the economic historian, whose subject matter is

[5] *American Economic Review*, LV (1965), 92.

dynamic. Now, in consequence of new theoretical goals essentially akin to historical ones, more economists than before are taking an interest in economic history. Now, through that increased interest, quantification and reliance on theory are growing in this field; now, these scholars are gaining a greater influence on its development. Thus, the trend toward quantification with the help of refined mathematical and analytical methods is not due only to the prevailing *Weltanschauung* and to the bias of a younger group of coevals. To the extent that its exponents started in economics it is largely due to their training; for economics, according to Milton Friedman, presumably speaking for many of his colleagues, is "a disguised branch of mathematics."[6] The economists devoting themselves to historical topics apply what they have learned. The trouble is only that the mathematical and the historical bents, except for a few extraordinary men like Schumpeter, do usually not go together, a difficulty which can only be partly overcome by appropriate training; and this means more than being steeped in economics and then taking a few courses in history for a year or so. Anyway, the key to the new development in economic history is, besides the evolution of refined economic analysis, the emergence of a new economic theory, the theory of economic development.

The quantitative approach to economic history has been recognized as nothing essentially new, and the application of refined mathematical and analytical economic methods would have come automatically with progress in these fields. Yet the genuinely new elements in the recent movement should not be underestimated. Beside the component already pointed out before, the younger researchers' neglect of institutions, there is another equally important one. The "new" economic historians actually *specialize,* that is on the "purely 'economic' problems of economic history," to quote from a letter of J. R. T. Hughes. Finally the introduction of modern methods of economic analysis into the quantitative approach to economic history has brought in its wake another fresh element, never used before in this area, namely, statistical inference. That is, "quantitative evidence may be used to verify a qualitative historical hypothesis." Conrad and Meyer, whose book containing the quotation will be analyzed in more detail later, refer as an example to an article by Michael C. Lovell. He infers from certain

6 *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), p. 12.

time series that the Bank of England became a lender of last resort in the crises of the eighteenth century. The qualitative hypothesis that the Bank of England came to stand for a central bank as early as that century was tested by statistical means against the actual numerical data and thus verified.[7]

The last statement has already brought us to another characteristic feature of the new approaches to economic history, but one which is only partly new—namely the use of hypotheses and figments. Test hypotheses have been used extensively by traditional analytical historians; but historians in general have shied away from the use of figments as leading to conjectural history which is generally held in disrepute. But we shall need to change our opinion to some extent, since the application of figments may provide useful tools for analytical economic history.

Hypotheses and figments are in fact two very different things, but it is a common error to confound them. Hypotheses are based on assumptions which are held to have a counterpart in reality, while figments are assumptions having no such counterparts or at least known to be irrealistic. While hypotheses reflect and are derived from reality, figments are mere "as if" constructs, without parallels in reality. A hypothesis cries for verification or, if one prefers the more modern way of thinking, for falsification. Figments as mental constructs are neither verifiable nor falsifiable. They demand justification by their usefulness. With the help of hypotheses we gain knowledge, with that of a figment we obtain a tool for the acquisition of knowledge. Scholarly work based on figments is comparable with industrial roundabout production since, as we shall see later, it leads to models.[8] To be sure, what in line with the German philosophical (that is, the Herbarth-Lotze-Vaihinger) tradition I have called "figment," has recently appeared in America under the name of "counterfactual or subjunctive conditional."[9]

7 As to the book of Conrad and Meyer, see p. 488 and, for the citation, footnote 14. The article by Lovell, "The Role of the Bank of England as Lender of Last Resort in the Crises of the Eighteenth Century," is in *Explorations in Entrepreneurial History*, X (1957/58), 8 ff.

8 My thinking is based on the famous book by Hans Vaihinger, *Die Philosophie des Als Ob*. I have used the third edition (Leipzig: F. Meiner, 1918). An English translation by C. K. Ogden appeared under the title, *The Philosophy of 'As-if'* (London: K. Paul, Trench, Trubner & Co., 1924). See especially the Introduction, ch. iii, and Part IA, ch. ii. The English author translates the German word *Fiktion* as fiction. I prefer the term figment.

9 Stuart Hampshire, "Subjunctive Conditionals" in [Oxford] *Analysis*, IX (Oct.

III

If we make the use or nonuse of hypotheses or figments, respectively, the criterion, we are able to distinguish between economic historians and the builders of historical models, and we can point to essential characteristics of the various new approaches which, on the other hand, are held together by a common aim: the extensive use of modern economic analysis, concentration on the purely economic aspects of economic history, and quantification along with refined mathematics.

If with such ends in mind the researcher works with the help of a hypothesis, the outcome tends toward analytical quantitative economic history, and Douglass North's achievement is representative of the approach. On the first page of his much-discussed book, *The Economic Growth of the United States, 1790-1860,*[10] he has been articulate about the hypotheses underlying his presentation: "The gist of the argument is that the timing and pace of an economy's development has been determined by (1) the success of its export sector, and (2) the characteristics of the export industry and the disposition of the income received from the export sector." This is clearly hypothetical as opposed to fictitious, nothing is postulated that is unrealistic. North assumes what he considers correct. Using the established method of the analytical economic historian or for that matter any social scientist, he proceeds to prove his hypotheses on the basis of available material, yet emphasizing quantification. What is new in the book is neither the method nor the far-going quantification, but the subject matter. As he himself states, he emphasizes economic growth as a total phenomenon whereas it has been traditional "to provide separate treatments of the various sectors in the economy . . . with only superficial linkages between them." It should be understood that I do not analyze or criticize the content of the book but deal with the *method* he uses. It is essentially related to the historical quantitative tradition. The result is history beyond any doubt.

Yet regardless of the fact that North starts from hypotheses and not from a figment, there is a fictitious element implied in his work, as in that of all scholars who identify economic development with

1948), 9 ff. Thence Conrad and Meyer have taken the term into their book already mentioned and to be cited in footnote 14.

[10] *The Economic Growth of the United States, 1790-1860* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1961), p. vi.

some set of figures, whatever their character. He states in the preface of his book[11] that due to the preoccupation with description and institutional change, there is "no comprehensive, integrated analysis of United States development." Economic historians have, according to North, "only incidentally discussed the process of economic growth." The content of this statement is granted to be correct, in fact entirely correct. But if North thinks that he has filled the gap, I should have to object as a matter of principle. His integrated discussion of American economic development was made possible by his bringing every phenomenon on a quantitative level; and while he gained integration thereby, he lost touch with reality. Figures are not identical with any process whatsoever. Figures are quantitative symbols which stand for something, in this case for the *result* of a process. By lining those symbols up in the form of a time series, we incorrectly create the impression that they actually represent a process, whereas they merely act as yardsticks, measuring a process. Or to express it differently, they represent a process only by the introduction of a figment, an as-if construct, and so furnish a good example of how useful figments can be in research and why their use is a widely accepted scholarly convention.

We now turn to those scholars who *base* their research on figments and begin with Fogel's work.[12] Here the fictitious character of the assumptions is beyond any doubt. Fogel investigates what would have happened to American economic development if there had not been any railroads. Now, as every schoolchild knows, there were railroads. That is, Fogel investigates what would have happened in the event that something else had happened which could not have happened. I emphasize now the phrase, which *could not* have happened. Technological development follows its own logic. Once the atmospheric engine had been developed into an efficient steam engine and the steam engine had successfully been put into boats, making steamers out of them, it was only a question of when the steam engine would be put on wheels, particularly as the railroad minus locomotive had existed for a long time. Having started from a figment, Fogel did not produce history, he produced what my friend Arthur Johnson has called quasi history, which in professional terms must be characterized as a historical model. Based on

[11] *Ibid.*, p. vi.

[12] Robert William Fogel, *Railroads and American Economic Growth: Essays in Econometric History* (Baltimore: Johns Hopkins Press, 1964).

a figment it does not cry for verification—which is, of course, impossible—but for justification. Now some young friends of mine, who are by no means "new" economic historians, tell me how useful as an eye-opener Fogel's book is in teaching when used, of course, beside the standard presentations on railroad history by Chandler, the Hidys, Jenks, Overton, and others. So it is actually justified, but this does not make Fogel's product history.

Previously I have described Fogel's program of "reconstructing measurements no longer extant," of constructing yardsticks, and of measuring indirectly what cannot be measured directly. This program implies figments all the way through, because for the execution one must rely on the dependence of some figures on others, dependences which econometric theory establishes. This by necessity must be done in disregard of the historical situation in which a theoretically established interdependence of magnitudes may not come into play, because other causal factors impinge. What in theory is entirely correct becomes fictitious in the application, the result being a model rather than something related to reality. But we are not yet at the end.

There are additional elements which characterize Fogel's presentation as an as-if construct, a historical model. He himself states that he has omitted "many conspicuous aspects" because they were "too important" and "too complex" to be treated in this context. He himself knows that the validity of his findings is "not absolute" and nevertheless thinks that he has corrected unwarranted assumptions. This claim is epistemologically untenable, since one *cannot correct anything* by using a figment. As a matter of fact, one can thus only point to tendencies, a term introduced into economics by John Stuart Mill, if I am correctly informed.[13]

But I am afraid that I must criticize Fogel also for not having asked the most fruitful question. Everyone who knows French economic history in the eighteenth and English in eighteenth and early nineteenth centuries knows that then there was economic development under way without the railroad. The fruitful question, if one wished to work in this area with figments, seems to be: At what point would economic development, under way by 1800, have become an arrested development for lack of adequate transportation? The stop would have come rather late in England, because

[13] Introduction, pp. vii, viii.

there ore and coal were available in close proximity and because of the easy access to the sea, providing a kind of unpaid-for road or canals system, if you please. The United States development would have come to a halt much earlier, or several national economies might have developed.

So much for Fogel's work. The most representative model builders besides Fogel are Alfred H. Conrad and John R. Meyer, and they are less extreme. These coauthors have made it easy for the analyst, in that they have published an elaborate methodological introduction to their collection of papers, its title being "Economic Theory, Statistical Inference, and Economic History."[14] They have elucidated the difficult subject with unusual clarity, and, while I would have used other language and while I have objections to a good many details, I can accompany them almost all the way through.[15] Specifically, I agree with them that there can be generalization in historiography to a point and that hypotheses can be tested by historical research. As to the former, if generalization is the goal, I prefer comparative history as a means toward it rather than working with hypotheses. And as to the latter, the authors themselves have indicated the difficulties involved (pp. 24 ff.), while I myself think of qualitative testing as standing on the same level as quantitative.

Now let us see how the methodological prescriptions are applied in actual research and take as representative the paper "The Economics of Slavery in the Antebellum South," first published in the *Journal of Political Economy*, Vol. LXVI (1958). We begin by comparing the basis of the article with those of the books by North and Fogel. The former, as we remember, begins with hypotheses, the latter with a figment. Conrad and Meyer (p. 45) start from a hypothesis; according to them, American Negro slavery was characterized by two production functions—namely, that of southern staples ("inputs of Negro slaves and the materials required to

[14] *The Economics of Slavery and Other Studies in Econometric History* (Chicago: Aldine, 1964).

[15] I wish to register my objection to one minor point only: namely, their juxtaposition of the view that everything in history is unique, with extreme determinism. They are not sufficiently familiar with European history teaching and writing to know that since the turn of the last century outstanding historians have taken a stand which represents the middle ground. These are Jacob Burkhard in Basel, Otto Hintze in Berlin (whose student the author was), and the now-living Theodor Schieder of Cologne. One could also point to Karl Lamprecht in Leipzig, who had an approach of his own.

maintain them to the production of the southern staple crops, particularly cotton") and that of slave labor. This is what they "postulate." Analyzing the execution of this program, one finds that parts II and III of the paper use different methods. In Part II about a dozen additional assumptions are heaped on the basic one. As a matter of fact, the authors were forced to do this. Their primary quantitative material was not only shaky but also was not collected for their specific purposes. They had to make it suitable and this could only be done by introducing more assumptions. Tables 10 and 11 are each based on three of these; and as to Table 9, their number is "untold."[16] If one looks at the assumptions as such, one finds that some are hypothetical, others fictitious, such as the life expectancy of slaves and the fertility of field wenches. These are mere guesses. Under these circumstances, that is, because of the large number of assumptions and the mixture of hypothetical and fictitious ones, I must contend that the whole Part II of the paper is fictitious even though the basic assumption is hypothetical. Things look different in Part III; the figures are much better, the assumptions (estimates) very few and not basic. This part looks much more like history than Part II, and is in fact historically interesting and useful.

Nevertheless I am reluctant to consider the paper as a whole a work of history. In the original discussion over it, one speaker blamed the authors for disregarding the interrelations of economic and other social factors. The authors agreed, but the objection is in fact crucial from our point of view. Both the isolating theorist and the narrative historian (who on principle refuses to ask and answer the question, Why? and, as a true empiricist, asks only: What has happened and when?) are in one boat. They can—to be sure, for different reasons—legitimately present one side of a development only. The analytical historian cannot afford to do so; for then his presentation becomes a one-sided exaggeration—that is, a model.

Yet during the discussion the authors made another important statement: namely, that the purpose of their paper was to test "hypotheses about the profitability [from the economist's point of view] of slavery and the efficiency of the slave labor market." This program poses another question: Is this actually a historical ques-

16 "Various hypothesized conditions"; p. 61.

tion? An economic question, as this undoubtedly is, does not become *ipso facto* a historical one simply because the problem investigated is of yesterday or before yesterday. An economic problem becomes one of economic history only when a specifically historical element is added. As history is essentially change over time, the economic problem must be seen as changing in order to be a subject for the historian. Otherwise, if the test is unspoiled by figments, it may stand at best on the level of a historical case study.

At the time when Conrad and Meyer were working on their paper, they were aware that they were constructing a model. On page 67 they speak of "our model," on page 73 of the "validity of the model"; on page 82 we read "our model of the antebellum southern economy"; and when they first presented their paper, one discussant accepted their own characterization of the work and cited it (*ibid.*, 93). If they had stuck to that perfectly sound description, I would have had no fault to find. Historical models have been used earlier; we have been articulate about them ever since the days of Max Weber, and their refinement by mathematical means or otherwise deserves praise. But at some point, the authors changed their minds and they are now trying to sell their work as "history." And this and this alone is the reason for my protest. A model is never a piece of history, because it is conjectural or subjunctive or, in Max Weber's language used for all ideal types, a distortion of reality. This position I can not abandon even if a qualifying adjective, be it "new" or "econometric," is added. If we were to accept it as history, the piece would be bad history. It is based entirely on secondary sources without any source criticism whatsoever. In their methodological paper (p. 18), they stress the necessity of hunting for quantitative material which they consider "likely to be found." In fact, as experts in southern history have told and even shown me, it can be found in their case. If the authors wanted to write "history" they were by professional standards obliged to search for primary material; if they wanted to build a model, they were free from that obligation. As "history," most professional historians, in contrast to economists with an interest in history, will reject the piece. As a historical model it might be very useful, as only those can decide who work with the tool, for a tool it is and not a consumers' good.

I have tried to show that the result of the scholarly endeavors of both Conrad and Meyer and of Fogel is not history but quasi history. One can express this also in other terms, as has been done. These men do not write history but produce historical models. Now when one distinguishes between models by reduction and models by construction, the latter also called nonempirical models, one recognizes that there is a difference between Conrad and Meyer's Part II and Fogel's book, on the one hand, and the formers' Part III, on the other. The first named are models by construction, the last named one by reduction in that it abstracts only from the non-economic determining factors. But models they are, so that we are cautioned against reifying them, against misplacing concreteness of them, to use the expressions of Morris Cohen and T. N. Whitehead. I am afraid the architects of the models in question and their admirers are not free from these errors. As far as I can see, the applause comes from economists rather than historians, from men never exposed to methodology and often not even to history.

IV

The so-called new approach to economic history has revealed itself as consisting of different movements. One, as represented for example by the data-processing jobs of Davis and Hughes or by Fishlow's paper, "The Trustee Savings Banks 1817-1861,"[17] is close to the tradition of quantitative economic history but uses much more refined mathematical tools. The second, exemplified by North, is based both on quantification and on clearly defined hypotheses which are tested. The product is economic history. The third, typified by Fogel as well as Conrad and Meyer, again emphasizes quantification but is simultaneously based on figments. The result is quasi-economic history, or as-if economic history, if you please, or more exactly a historical model.

A final question remains to be asked and answered: namely, What is the relationship of the traditional economic history to the new approaches? What makes it a little difficult to answer is the fact that, like the "new" economic history in the wider sense of the term, the traditional economic history is by no means uniform. Some is microeconomic history; some corresponds to macroeconomics. One branch emphasizes figures, another institutions. Some is empiricistic, some analytical. Or, focusing on the method applied, we can

17 Journal of Economic History, XXI (1961), 26 ff.

distinguish between a branch using the positivistic from another using the hermeneutic, that is, the interpretative "understanding" method.

Under these circumstances it is quite impossible to confront the two approaches and characterize them with a few concise words. Yet it is not far from the truth to say that the best traditional-yet-modern economic history, beside emphasizing the study of institutions, is well aware of the role of noneconomic factors and of imponderables which the "new" economic historians specializing in the purely economic aspect of the field disregard. To the extent that traditional economic history stresses institutions, noneconomic factors, and imponderables, it might draw for theoretical support on sociology and anthropology rather than on economics; whereas the "new" economic historians rely exclusively on the latter. T. C. Cochran's interest in the cultural element in economic development, or mine in the personal element therein, simply does not interest the exponents of the new approaches. But I am sure nothing is further from their thought than a scientific ostrich policy. Their *interests* lie in a different direction.

Once new approaches have come into existence and are here to stay, the traditional and the "new" economic history become interdependent, but interdependent in different ways since they are not uniform phenomena. To the degree that the new approach simply implies quantification with refined mathematical methods, it will probably be the successor of the traditional quantitative approach to economic history. To the extent that the "new" approach is quantitative and analytical, as in the case of North's work, the old-line empiricists will take the result and the old-line analytical economic historian will take it also, once he becomes convinced that the underlying hypothesis is tenable and the analysis correct. More interesting is the question how the traditional analytical economic history will be related to the "new" analytical economic history as practiced by Conrad and Meyer or Fogel.

It has been pointed out earlier that on the basis of figments one arrives at quasi history or models. These the traditional analytical economic historian will use as he will use any other suitable model —namely, as a flashlight. He will *compare* his empirical material with the model and, if he sees something which he otherwise would not have seen, he will recognize the model's usefulness and be grateful to the model builder. If it does not help him, he will reject it as unsuitable in the particular case, although this will not exclude the possibility of its being valuable for other research purposes. Yet there are also specific interdependences of the traditional and the "new" approaches. A new approach may be based on the work of the traditionalists or, vice versa, it may make the latter's research possible. Let me give an example for each case.

Historians have long known that early English savings banks were founded with a view to helping the poor. But there we were. We did not know whether or not that goal was reached, until Fishlow came and subjected the material to modern statistical analysis.[18] This showed clearly that in fact the British savings banks originally served the lower middle classes rather than the poor. This finding will be adopted in any presentation of savings banking by traditional economic historians.

Now the other case: in a painstaking study by L. E. Davis and J. R. T. Hughes, "A Dollar-Sterling Exchange 1803-1895,"[19] the authors, members of the Purdue faculty, have shown by applying very refined mathematical analysis that "the exchange rate stability commonly associated with the pre-1914 gold standard was a characteristic of the dollar-sterling exchange only after the early 1870's," a conclusion which no economic historian can afford to disregard. But right here, where the "new" economic historians stopped, the traditionalist would start with his research. I myself was tempted to ask a few years ago: Why did the gold-point mechanism work after the 1870's and not earlier? My answer would have been sought in a study of the development of communication and of business administration, and I am sure it is to be found in these areas. This example shows very well where the shortcomings of both the traditional and the new approaches lie which, in turn, result from different goals. The "new" men emphasize measuring, and comprehending by measuring; the old nonempiricistic economic historian stresses comprehending and "understanding." They are certainly now interdependent.[20]

[18] *Ibid.*

[19] *Economic History Review*, XIII (1960), 52 ff.

[20] Although speaking only on a related problem, R. L. Basman, in his article, "The Role of the Economic Historian in Predictive Testing of Proffered 'Economic Laws,'" in *Explorations in Entrepreneurial History*, 2d ser., II (1965), 159 ff., points in the same direction. He seems to plead for the use of realistic assumptions to be provided by professional historians. I wonder if he means to take a stand against the model builders and the use of figments as the basis of historical research.

I have found in some pieces by leading exponents of the "new" approach remarks which indicate their consciousness of the fact that they are standing on the shoulders of traditional economic historians. In their programmatic statements they were not always fortunate, so that these can be misconstrued.[21] A still younger group now coming out of their seminars with the arrogance which is the privilege of youth seem to believe that their approach will take the place of the traditional one, that they will rout economic history as practiced by the old fogies. They will in time mature and learn, or so it can be hoped.

But there comes a point where I stop. A few years ago Bulletin 64 of the Social Science Research Council seemingly wanted to merge history into the social sciences. I do not think that the attempt was a success. Now Fogel wants to "reunify" economic history with economic theory, presumably a *societas leonina* in which economic history would cease to exist, after all traditional economic historians have committed hara-kiri. First, Fogel's concept of reunification is out of order. He does not realize that economic history, created by historians, goes back independently to the eighteenth century, as does modern economics, both being coeval, and that what he presents as the history of a separation of economic history from economic theory is neither factually correct nor correctly interpreted.[22] Moreover a merger, as Fogel plans it, would mean that economic history is dropped in favor of model building, as the economist learns in class. I would not expect many genuine historians to be willing to take that road, and it seems to me that resistance would come even from some of the exponents of the "new" approaches of different persuasion. At all events, since as before a sizable portion of American economic historians will come from history rather than from economics, there is no fear that model building will outgrow historiography or that measuring in economic history will entirely replace the understanding variety.[23] On the other hand I feel strongly that the day of empiricistic historiography is gone and I wholeheartedly agree with Rondo Cameron that the

[21] See, for example, *American Economic Review*, LV (1965), 92 ff.
[22] *Ibid.*, pp. 94, 95.
[23] A high-class discussion of the methods of measuring as against those of comprehending and understanding took place at a meeting of the Verein für Sozialpolitik. See its *Schriften*, new ser., No. 25, *Diagnose und Prognose als Wirtschaftsliches Methodenproblem* (Berlin: Duncker and Humblot, 1962). Especially pertinent here is p. 178, a remark by Georg Weippert.

choice is not between theory or no theory in economic history but between consciously and professionally formulated theory and unconscious amateurish theorizing, because the historian is unavoidably guided by some ideas somehow absorbed.[24]

FRITZ REDLICH, *Harvard University*, retired

[24] *American Economic Review*, LV (1965), 112.