

# Does the Past Have Useful Economics?

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*It is not mere convention that impels me to thank the many colleagues who have commented on earlier drafts of this essay. I have received from them in writing the equivalent of over 100 typed pages, and many hours of conversation as well. This itself measures the vigor of historical economics, one theme here, but their contribution to the product is immeasurable. I would like to thank, therefore, the seminars in economic history at Chicago and at Northwestern; and R. Cameron, M. Edelstein, S. L. Engerman, R. W. Fogel, R. Gallman, H. Gemery, C. D. Goldin, G. Gunderson, G. Hawke, R. Higgs, G. Hueckel, J. R. T. Hughes, H. G. Johnson, E. L. Jones, A. Kahan, C. P. Kindleberger, A. Leijonhufvud, P. Lindert, P. McClelland, M. McInnis, J. Mokyr, L. D. Neal, A. Olmstead, D. Perkins, J. D. Reid, N. Rosenberg, W. W. Rostow, A. J. Schwartz, B. Solow, G. Walton, D. Whitehead, and J. G. Williamson. And I would like to apologize to George Stigler for inverting for my own purposes the title of his fine essay, "Does Economics Have a Useful Past?" [107, 1969], and for ignoring the useful lemma illustrated there (p. 226): "there are not ten good reasons for anything."*

THE ANSWER, of course, is "yes," and at one time the very question would have seemed impertinent. Smith, Marx, Mill, Marshall, Keynes, Heckscher, Schumpeter, and Viner, to name a few, were nourished by historical study and nourished it in turn. Gazing down from Valhalla it would seem to them bizarre that their heirs would study economics with the history left out, stopping their desultory search for facts in time series at the last 25 years and in cross sections at the latest tape from the Bureau of the Census, passing by the experiments of history with little regard for their place in a nonexperimental science, distrusting old facts as er-

ror-ridden intrusions from another structure, abandoning historical perspectives on their political economy, and basing their theory and policy on stylized nonfacts about economic development, fairy tales remembered from their youth.

Yet this is what has happened. It began in the 1940's, in some respects earlier, as young American economists bemused by revolutions in the substance and method of economics neglected the reading of history in favor of macroeconomics, mathematics, and statistics. The low opportunity cost of such specialization reinforced it: American economic history by that time was, albeit with a few brilliant

exceptions, neither good economics nor good history, a dim echo of the American institutionalists and through them of the German historical school. It is not surprising that immature scholars undervalued history then, still less surprising in a decade in which economists were having difficulty understanding macroeconomic policy even in the short run, mathematical maximization even under narrow constraints, and statistical inference even with a simple structure. What is more surprising is that the reading and using of history was not taken up again in the early 1950's, as economists rediscovered economic growth, surprise which turns to astonishment when the neglect of history persisted into the 1970's, as they rediscovered property rights, inheritance, educational investment, social class, income distribution, and other pieces of history in economics. And what is most astonishing in this—what must make Schumpeter, say, turn back in disgust to his horn of mead and his dialogue with Marx and Smith on the historical dynamics of capitalism—is that in the late 1950's a throng of historical economists equipped with Lagrangean multipliers and Durbin-Watson statistics poured out of Purdue, Harvard, Washington, Columbia, Johns Hopkins, and a widening array of new factories at home and abroad to reshape economic history into a form suited to the tastes of their col-

<sup>1</sup> This difference, treating each of the fifty years as an observation, is significant at the .00003 level. Because the distribution for the earlier period was bimodal, and plainly therefore non-normal, I used the Mann-Whitney U test. The underlying evidence is available on request. Briefly, for 1925–1963 the number of pages in articles on history in the three journals was calculated from all their appearances in the various history classifications in the *Index of Economic Journals* [1, 1961–1965]. Its definition of "history" is "articles concerned primarily with a period 20 years or more earlier than the beginning date of the volume" [1, 1961, p. xi]. The definition imparts a downward bias to the number of pages recorded as history in earlier years, because the volumes of the *Index* for those years cover a wider span: to qualify as history an article in 1939 had to concern events 34 years before; one in 1949, 29 years before; one in

leagues in economics, yet their colleagues did not buy. The identification problem is solved: it was the demand curve, not the supply curve, that moved back. At the same price measured in an hour of reading or a month of writing, economists nowadays demand less economic history.

The result is apparent in those periodical declarations of what it is that real economists do, the general interest journals of the profession. From 1925 to 1944 the pages of the *American Economic Review*, the *Quarterly Journal of Economics*, and the *Journal of Political Economy* taken together contained 6.5 percent (as weighted by size) of articles on economic history; from 1945 to 1974 they contained 3.3 percent (similarly weighted).<sup>1</sup> For the three journals taken separately the drift away from history was as shown in TABLE I, and more detailed statistics tell a similar story. The *JPE* was for a long time the most historical of the journals, reflecting perhaps the reluctance with which Chicago embraced the view that economics is applied mathematics and statistics and that in the long run we are all dead. From 1929 to 1944, with a remarkable devotion to intellectuality in the face of world depression and war, the *JPE* devoted 11 percent of its pages to economic history, many of them the products of Earl Hamilton and John Nef. By 1970–74, however, the heirs of Jacob Viner and Paul Douglas devoted 2.8 percent of the pages of their journal to

1963, 23 years; and one in 1974, only 20. A correction would accentuate the postwar fall. For 1964–74 the number of pages in articles on history was calculated from the journals themselves, classifying doubtful cases as history. The total number of pages available for all subjects was calculated from the journals, including supplements sent to subscribers and excluding advertisements, administrative matter, and book reviews in book review sections (not classified in the *Index*). The *AER* after 1969 is not defined to include this *Journal*; if one were to adopt the alternative definition, the recent evaporation of history from the journals would be somewhat more pronounced. The percentages divide the pages of history by total pages, weighting journals in the total by their number of total pages. Unweighted averages of the three behave in much the same way.

TABLE I

SUMMARY OF THE PERCENT OF PAGES DEVOTED TO ECONOMIC HISTORY, 1925-1974

	AER	OJE	JPE
1925-44	4.4	5.4	9.9
1945-74	2.2	3.3	5.4
Level at which the differences are significant (Mann-Whitney test)	.011	.021	.020

it. They were merely joining the trend. The journals of 1935-39, when economists were obsessed (properly) with last year's unemployment and trade statistics, contained proportionately 2.7 times more economic history than the journals of 1970-74, when economists were instead obsessed (again properly) with the origins in the very long run of growth, discrimination, legal change, and the historic evils of capitalism.

These contrasts will surprise no one familiar with the literature of economics over the last fifty years, confirmed as they are in other ways.<sup>2</sup> To be sure, specialized journals of economic history drew off historical articles from the three major journals, as did specialized journals in other fields. The composition of the general journals is nonetheless a measure of what those who write for, edit, and referee them believe is of general interest to economists. They believe history to be of small and diminishing interest. That the general journals have little economic his-

tory in view of the existence of specialized journals in economic history read exclusively by economic historians does not contradict the observation that economists are increasingly ahistorical. It restates it. Indeed, even if one were to suppose that economists read at random in all the 200-odd journals indexed in this *Journal*, the probability of them stumbling on an article on economic history in 1973-74 would have been .028, and the number of encounters with history in the journals, like the number of deaths from horse-kicks in the cavalry, would approximate a Poisson distribution.<sup>3</sup> Of course, economists in other fields do not in fact consult specialized journals of economic history. The drift of economic history into specialized journals and of economists out of reading these journals is doubtless attributable to the widening gap in method, closing only comparatively recently, between economics and history. In 1926 the editors of the *JPE* believed, no doubt correctly, that their subscribers would read B. J. Hovde, "French Socialism and the

definition of "history" in the index (an article appearing 1924 would have to be concerned chiefly with events before 1866, 58 years before, to qualify as history). A. W. Coats' assertion that economic history has occupied "an increasing share of the total periodical literature" [12, 1971, p. 32] is based on Bronfenbrenner's figures.

<sup>3</sup> The figure is the share of column inches of history titles in the index of the *Journal*. In 1973-74 these included all articles in history, not merely those fitting into Class 5 of the *Index*. Highly specialized journals of economic history, such as *Agricultural History*, are not indexed in the *Journal*; they were in the *Index*.

Triple Entente, 1893-1914," the first of three long articles cast in narrative. What is most significant, though, and least quantifiable, is the drift in economics away from using history, as distinct from merely reading it. Whether or not an economist specialized in, say, international trade reads for himself a seminal article on distributed lags in *Econometrica* or on portfolio analysis in the *Journal of Finance*, he will use their results, distilled in survey articles and in textbooks. The same cannot be said at present for economic history. Does the past have useful economics? The average American economist nowadays answers, "No."

The exceptions are notable, in more ways than one. It will come as a surprise to many economists that among others Armen Alchian, E. Cary Brown, Richard Caves, Donald Gordon, Reuben Kessel, Marc Nerlove, Mancur Olson, Albert Rees, Stanley Reiter, and Arnold Zellner, none of whom do their main work in history, have in fact made contributions to it.<sup>4</sup> Turning away for a moment from the subject here, American economics and its relations with American history, it might be noted that in Britain such traditions of a serious amateur interest in economic history are strong: Mark Blaug, A. K. Cairncross, J. R. Hicks, R. C. O. Matthews, E. H. Phelps-Brown, R. S. Sayers, Brinley Thomas, and John Vaizey, for example, are well-known in Britain as economists dealing with contemporary problems of policy and theory, yet all of them have contributed to British economic history at a high level. The postwar officers of the American Economic Association, members of the older generation trained to place history as Schumpeter did with theory and statistics at the foundation of eco-

nomics science, can provide a comparable list. Among recent vice-presidents, Moses Abramovitz, Evsey Domar, Charles Kindleberger, W. Arthur Lewis, and Robert Triffin show no signs of forsaking history. Nor do the words and works of postwar presidents reflect the dominant opinion of their constituents that economic history is a frill, useless to the hard, important business of formalizing another economic idea, of refining the techniques for exploiting a given set of statistics, or of deflecting current policy from a third into a second best configuration. In his presidential address to the Association in 1970, Wassily Leontief scolded those who had elected him for ignoring empirical work in favor of ever more mechanical theory and scholastic econometrics [66, 1971, p. 3]:

Devising a new statistical procedure, however tenuous, that makes it possible to squeeze out one more unknown parameter from a given set of data, is judged a greater scientific achievement than the successful search for additional information that would permit us to measure the magnitude of the same parameter in a less ingenious, but more reliable way.

He applauded agricultural economists for a long tradition in another style, and might as well have applauded historical economists for a younger tradition in the same style [66, 1971, p. 5]:

An exceptional example of a healthy balance between theoretical and empirical analysis and of the readiness of professional economists to cooperate with experts in the neighboring disciplines is offered by Agricultural Economics as it developed in this country over the last fifty years.

One of the agricultural economists whom Leontief undoubtedly had in mind, Theodore W. Schultz, himself a past president of the Association, regretted in 1974 that he himself had not studied economic history more diligently in his youth and argued that "there is a strong tendency on the part of virtually all economists to undervalue the history of the economy of

<sup>4</sup> [60, Kessel and Alchian, 1959; 8, Brown, 1956; 10, Caves, 1971; 11, Chambers and Gordon, 1966; 77, Nerlove, 1965; 81, Olson, 1963; 90, Rees, 1961; 54, Hughes and Reiter, 1958; 137, Zellner and Murphy, 1959.] It was Reiter who invented the word "cliometrics," a joke that caught on.

<sup>2</sup> Martin Bronfenbrenner would not agree [7, 1966]. He measured the column inches of Class 5 (Economic History) relative to the total in the *Index* volumes, arriving at:

1896-1924	1.21%	1950-54	1.44%
1925-39	1.49	1955-59	1.64
1940-49	1.49	1960-63	1.47

This he described as an "upturn . . . with some slight decline since 1960" [7, 1966, p. 544]. In this calculation, however, he neglects the articles classified as History but appearing in other parts of the index (such as 9.51, Security and Money Markets, History), which are, ignoring the double-counting, 80 percent of the total; and he does not notice the sharp downward bias in the earlier volumes imparted by the

both high and low income countries. I doubt the wisdom of this tendency to concentrate on the immediate present" [101, 1974, p. 12]. Another postwar president, Milton Friedman, in collaboration with Anna J. Schwartz, carried a high valuation of economic history to the point of making a seminal contribution to it, as in a less extended way did Paul Douglas, John Kenneth Galbraith, Robert Aaron Gordon, and J. H. Williams. And still others, such as Schumpeter, Harold Innis, and Simon Kuznets, valued economic history to the point of devoting sustained effort over long careers to its enrichment.

It is apparent, however, that this older generation of American economists did not persuade many of the younger that history is essential to economics. Those they did persuade—the "new" economic historians or "cliometricians"—ignored the task of persuading their doubting colleagues and directed their rhetorical energies instead towards non-economists, chiefly historians. This choice of audience had the advantage of imparting emotional cohesion to the cliometricians, filling them with the enthusiasm and energy of convinced imperialists. The result was a series of conquests beginning, as I have said, in the late 1950's and widening further with each year that sharply revised American economic history and has begun recently to revise other economic histories as well. Being intellectual imperialists, however, the cliometricians forgot, as many imperialists do, that foreign adventures require domestic support, and by neglecting to solicit it, they lost it. Were other economists so disregarding of their self-interests they would court a similar fate? For thirty years after the first stirrings in the 1930's, mathematical and statistical economists pointed out to everyone who would listen that one or another piece of economics is essentially mathematical or essentially statistical until at last no one remained to be convinced. Historical economists could

have pointed out with equal force that one or another piece, in some cases the same piece claimed by their more aggressive colleagues, is essentially historical. But they seldom did. Socialized inside economics as it developed after the War, they were apologetic and deferential towards their colleagues, to the point at times of imitating their colleagues' low standards of factual accuracy and wider social relevance along with their high standards of logical cogency and statistical grace. Lacking the self-confidence of the mathematical or statistical economists, the new historical economists have neglected the task of persuading others of the worth of history in economics.

### I. The Value of Economic History

It is not because it is difficult to do that the task has been neglected. The lines of argument are opened with little effort. For the professional economic historian the worth of economic history is that of general history, to which it contributes, and it is because he puts a high value on history, economic or not, that he chooses to study it. This justification suffices for him and for any economist who believes that history, whether or not it is directly useful in testing economic laws or framing economic policy, is collective memory fruitful of wisdom. At the least pragmatic level, indeed, the worth of economic history is that of intellectual activity generally, and nothing should be easier than convincing professional intellectuals that such activity is worthwhile. G. M. Trevelyan put the point gracefully [117, 1942, pp. viii, x]:

Disinterested intellectual curiosity is the lifeblood of real civilization. . . . There is nothing that more divides civilised from semi-savage man than to be conscious of our forefathers as they really were, and bit by bit to reconstruct the mosaic of the long-forgotten past. To weigh the stars, or to make ships sail in the air or below the sea, is not a more astonishing and

ennobling performance on the part of the human race in these latter days, than to know the course of events that had long been forgotten, and the true nature of men and women who were here before us.

One can admire historically important and economically perceptive histories of Southern slaves, nineteenth-century businessmen, or medieval peasants in the same way that one admires a mathematically beautiful and elegantly proven theorem in the theory of optimal control, whether or not the histories or the theorem have any practical use.

In this respect, indeed, by their attachment to the ivory tower, historical economists have much in common with mathematical economists. Further, though in their fascination with markets both activities are recognizably economic, both practitioners are likely to be met with a glassy stare and a change of subject when they speak of probate records or fixed point theorems to their colleagues in the coffee room. There remains, to be sure, one conspicuous point of asymmetry: forty years of investment in mathematizing economics and of disinvestment in historicizing economics has made it less acceptable among economists to admit ignorance of mathematics than to admit ignorance of history. The days are passing when the social sciences bridged the two cultures, literary and scientific, and economics burned the bridge long ago. Comfortable ignorance, to be sure, is not a monopoly of economists. A culture is a definition of barbarians, a definition of which people one may safely ignore; an intellectual culture is a definition of which classes of knowledge one may safely ignore. A social historian dealing habitually with inherently quantitative issues would be deeply ashamed to admit that he is ignorant of the languages, literature, or political history of the societies he studies; yet admits cheerfully, with no apparent resolve to amend his ignorance, that his mathematical and

statistical sophistication is that of a ten year old child. It is meritorious in such circles to be innocent of numbers, as to be free from some mental defect. Economists have not usually carried the parallel attitude so far. It is true, nonetheless, that an applied economist dealing habitually with inherently historical issues would be ashamed to admit that he is ignorant of differential equations or identifiability, yet admits with no sense of loss that he is entirely ignorant of what occurred in the economy he studies before 1929 or 1948 or 1970.

What, then, do economists lose by their increasing inclination to define their intellectual culture to involve ignorance of the past? Why, even if they choose not to heed the lofty call of disinterested intellectual curiosity, should economists read and write economic history?

### II. The Pragmatic Value of Economic History

#### A. More Economic Facts

The pragmatic answers are straightforward, the first and most obvious being that history provides the economist with more information with which to put his propositions in jeopardy. The volume of information available will come as a surprise to most economists, consumers as they are. The National Bureau of Economic Research is unusual in this, and its half-century of tillage of the past, harvested as data in thousands of regressions by economists otherwise uninterested in history, amply nourished the new historical economists of the last fifteen years. During the 1950's and 1960's many of them served an apprenticeship in economic observation, to change the metaphor, at the Bureau's social observatory in New York, contributing heavily to the two catalogues of historical objects produced in the late 1950's and the early 1960's (edited by W. N. Par-

ker [82, 1960] and D. S. Brady [6, 1966]).<sup>5</sup> The publication in 1960 of another work in which the historians at the NBER had a hand, together with the Bureau of the Census and the Social Science Research Council [118, 1960], can mark the beginning of the Keplerian stage of the new economic history. The National Bureau's interests were more nomothetic than historical—an interest in quantitative history for the light it could cast on regularities and (eventually) predictabilities of the economic system rather than for the light it could cast on history itself—but it would be churlish as well as inaccurate to discount for that reason the role Moses Abramovitz, Arthur Burns, Solomon Fabricant, Raymond Goldsmith, and John Kendrick among many others played in encouraging historical economics. In a discipline increasingly bored by history the Bureau was from the beginning, as Wesley Clair Mitchell put it in 1927, committed to the notion that [74, 1927, p. x]:

Business cycles consist of exceedingly complex interactions among a considerable number of economic processes, that to gain insight into the interactions one must combine historical studies with quantitative and qualitative analysis, that the phenomena are peculiar to a certain form of economic organization, and that understanding of this scheme of institutions is prerequisite to an understanding of cyclical fluctuations.

Thirty-six years later the commitment to history lived on in the ambition of Milton Friedman and Anna J. Schwartz to write an "analytical narrative" as "a prologue and background for a statistical analysis of the secular and cyclical behavior of money

<sup>5</sup> The earlier volume contains most of the papers delivered to the joint meeting of the Conference on Income and Wealth and the American Economic History Association at Williamstown in 1957, a meeting that celebrated the marriage between the NBER and the new economic history. Recently, it is sad to report, the marriage has been drifting towards separation.

in the United States" [33, 1963, pp. xxi-xxii].

This governing idea of the Bureau—that one could in empirical work go beyond consuming historical facts to producing them, embedding the output in its appropriate historical milieu—was seized on and expanded by the young historical economists of the 1950's and 1960's. It occurred to them that the statistics most economists are content to receive from clean-looking columns of reference books could in fact be constructed for much earlier times than had been thought possible and could be brought to bear after their construction on important historical issues. Brimming from their other courses in graduate school with the new mathematical, statistical, and computational techniques that flowed into the curriculum in the 1950's, they had the tools with which to reshape the historical object. To use symbolically the names of three men whose influence was more than symbolic, the students of Alexander Gerschenkron, Simon Kuznets, and Douglass North were quick learners and saw that if the masters could push measures of American income or Italian industrial output or the American balance of payments back to 1869 or 1881 or 1790, they could too, and more. Robert Gallman, a student of Kuznets, laboriously reconstructed first American commodity output then GNP back to the 1830's [34, 1960; 35, 1966]; he later joined with William Parker, a student of A. P. Usher at Harvard and of Gerschenkron, Usher's successor, in a large-scale sampling of the hand-written manuscripts of the 1860 agricultural census. Richard Easterlin, another student of Kuznets, reconstructed income by state back to 1840, then turned, by way of the long swing, to the analysis of American population back to the middle of the nineteenth century [21, 1960; 22, 1961; 23, 1968]. Alfred Conrad, Paul David, Albert Fishlow, John Meyer, Goran Ohlin, Henry Ro-

sovsky, and Peter Temin, all students of Gerschenkron, made Harvard for a time in the late 1950's and early 1960's a center of research in the new economic history by exploring with economists' eyes the voluminous quantitative records, hitherto neglected, of slavery, agricultural machinery, railways, schooling, and iron and steel in nineteenth-century America, agriculture and governmental finance in nineteenth-century Japan, and population in medieval Europe.<sup>6</sup> At about the same time, another example of simultaneous discovery so common when an idea's time has come, similar centers had sprung up at Rochester (where two students of Kuznets, Robert Fogel and Stanley Engerman, were exploring the records of American railways, slavery, and agriculture in the nineteenth century) and at Purdue (where Jonathan Hughes and Lance Davis, students of North, together with Edward Ames, Nathan Rosenberg, and a startlingly large number of other economists were reinterpreting the record of finance, business cycles, and technological change from the twentieth century to the fourteenth).<sup>7</sup> From 1960 on, these groups gathered annually at a conference at Purdue, transferred to Wisconsin after 1969.<sup>8</sup> Elsewhere Gary Walton [121, 1967] and other students of North joined with North himself in a reconstruction of ocean shipping rates back to the seventeenth cen-

<sup>6</sup> The monument to this work is Part One of Rosovsky, ed. [96, 1966]. Conrad and Meyer's work is collected in [14, 1964].

<sup>7</sup> The products of the Purdue school (*floruit* 1958-1966) are gathered in *Purdue Faculty Papers in Economic History* [83, 1967].

<sup>8</sup> The role of foundation financing for this and other projects in cliometrics was critical. The Ford Foundation supported the Purdue meetings for a time, and the Rockefeller Foundation supported a generation of Gerschenkron's students at Harvard. A brief gap in the middle 1960's was filled after 1968 by the National Science Foundation, which has continued since then to encourage cliometrics. When some future historian of economic thought applies refined measures to this history, he will find, I think, that the intellectual marginal product of these grants was extraordinarily high.

tury [78, 1968]; Matthew Simon, also working with the fact-makers at Columbia and the NBER in the 1950's, developed balance of payments accounts for 1861-1900 [104, 1960]. Stanley Lebergott examined anew the record of American labor back to 1800 [65, 1964]. Gary Walton in collaboration with James Shepherd [102, 1972], another student of North, constructed trade accounts for the American colonies, and still another student of North, and of R. P. Thomas, Terry Anderson [2, 1972], constructed income and population statistics of New England in the seventeenth century. Roger Weiss estimated the supply of money in the American colonies [122, 1970; 123, 1974]. So it went, and goes.

To some degree these waves of fact originated inside economics. Yet once transferred to specialized historical economists such work developed a momentum of its own. To take a recent example, the successes of Friedman and Schwartz with the American monetary statistics for 1867 to 1960 inspired historical work on earlier American statistics, then on British, and now on other countries [111, Temin, 1969; 103, Sheppard, 1971]. The historical study of productivity change is another recent example of transferred momentum.<sup>9</sup> The early work by Abramovitz and Solow was, like that of Friedman and Schwartz, nomothetic rather than historical. In the hands of historical economists, however, it gave impetus to the construction of historical series on the quantities and prices of inputs and outputs useful far beyond their initial purpose. Whether or not the theories tested by such economic studies

<sup>9</sup> It is not widely appreciated outside British economic history that the "residual" (in its price dual form) was invented in the 1920's for a historical study of British and American industry by G. T. Jones in his posthumous book *Increasing Returns: A Study of the Relation Between the Size and Efficiency of Industries, with Special Reference to the History of Selected British and American Industries, 1850-1910* [57, 1933, p. 33]. As we shall see again below, reading history has even its theoretical rewards.

survive the next twist in intellectual fashion—theories of business cycles, consumption, investment behavior, growth, money, or productivity change—the urge to implement them historically continues to generate new and lasting facts.

It will seem strange to economists exposed only to older writing on history or to no writing on history at all to assert that history is a rich mine of statistical information. Badly educated economists believe there are “no data” before the year in which the reference book nearest to hand begins its series on income or wages or exports, and twenty years ago most historians, even economic historians, would have agreed with them. Some still do, dropping with relief the task of measurement before 1900 as soon as they hit on one or another specious reason for doing so: that perfect accuracy is not attainable (estimates have errors), that no individual possesses the attributes of the average individual (distributions have variances), or that statistics dehumanize history (sets are defined for limited characteristics of the objects included). The economist should be aware that the case against statistics in history rests on such pitiable foundations, however pleasing it may be for him to suppose that the historian possesses special tools of insight superior to the spirit-killing tools of his own trade. The computer and the resulting advance in quantitative history, led by the new economic historians, have in any case given statistical agnosticism in history a quaint look.

The historical facts available for the economist's work, in truth, are voluminous beyond the wildest dreams of intellectual avarice, extending back in diminishing volume to the Middle Ages. They require only work and imagination. No Ministry of Agriculture in the thirteenth century collected statistics on English agricultural output for the benefit of twentieth-century students of agricultural economics. Yet medievalists realized long ago

that the annual account of the bailiff to his lord could yield such statistics for the lands farmed by the lord himself; and they have realized more recently that all the land, farmed by the lord or by the peasants, paid tithes to the Church, itself a literate and methodical bureaucracy with a strong self-interest in examining and preserving the records of the tithe from year to year, from which output can be estimated.<sup>10</sup>

A large investment, of course, is necessary to put such collections of facts into usable form, and relative to the size of the investment economic historians, for all their energy, have just begun. A case in point is the astoundingly large collection of genealogical records held for baptizing the dead by the Mormon Church in Salt Lake City, records yielding detailed family histories for many generations.<sup>11</sup> But the student of inherited and acquired human capital could find material for his work that is less difficult than this to handle in the historical record were he to look into it. Surveys, for example, are not a recent invention. The history of Europe and its offshoots from 1086 to the present is littered with them. To take a comparatively recent example, in 1909 the United States Immigration Commission collected questionnaires from over half a million wage earners, some 300,000 of them foreign born, and from 14,000 families totaling 60,000 people, asking them about their occupation; wage income, employment, property income, earnings of the household, housing, rent paid, children, schooling, literacy, languages, money remitted abroad, money when first landed, and many other matters. The Commission

<sup>10</sup> Skeptics will find a look at J. Z. Titow convincing on the wealth of data derivable from bailiffs' accounts [116, 1972]. There does not appear to be a use of the tithe in the English literature, but it has become a commonplace in the French, as in J. Goy and E. Le Roy Ladurie [41, 1972].

<sup>11</sup> See the article by Clayne Pope and Larry Wimmer on these records, forthcoming in *Historical Methods Newsletter*.

surveyed employers on a comparably large scale. The results were published in 42 volumes, which still await the curiosity of economists interested in the accumulation of human capital, the life cycle of income, the participation of women in the labor force, migration, and discrimination.<sup>12</sup> Whenever men write down accounts of their own or others' economic activities the economist has more observations for his science. Economic historians realize that men have been doing this for a long time.

### B. Better Economic Facts

The inspiration for reconstructing the statistics of the past has not come from economics alone, with the result that economic historians can present to economists new classes of facts, richer in many dimensions than modern facts. The very deadness of the men and companies of the nineteenth century and before opens to view records closed to an economist who insists that his subjects be alive or recently deceased. Only a successful antitrust suit pries loose the records of General Electric's conspiracies in restraint of trade, yet the student of industrial organization could if he wished turn to business historians for information on the costs and benefits of collusion that would bring statistical life to his speculations on their magnitudes. The Department of Commerce, the SEC, and the self-interest of the companies expose to public view some scraps of information about the costs, profits, and investments of industrial firms; yet the student of investment and finance could turn to work such as Paul F. McGouldrick's

*New England Textiles in the Nineteenth Century: Profits and Investment* [70, 1968] for much richer information.<sup>13</sup> Even for firms that have come now under close and inquisitive government regulation, such as banks, old records, once confidential and therefore candid and complete, are better than new [80, Olmstead, 1974].

Demographic history, long practiced outside of economic history but now influencing it heavily, provides still more examples of the virtues of the dead as objects of economic study. The very records of death, probate inventories and wills, are rich sources of facts (see A. H. Jones [56, 1972]). So too are counts of the once living. The 100-year rule of disclosure in, say, Britain makes it possible to do for the 1871 census of population what is impossible for the 1971 census, to scrutinize samples or, if one wishes, the entire population of all coal-mining towns (with their startlingly high fertility) or of all industrial villages (with their startlingly wide variation in family structure).<sup>14</sup> The critical item that is missing in any modern sample from the census is the person's name, for without his name one is unable to link the record of the census to other records. To appreciate the significance of this fact, one has only to reflect that men and governments are more methodical in their record-keeping and more bold—one might say impertinent—in their curiosity today than they were once, and when some future economic historian is able to trace people by

<sup>13</sup> Gavin Wright described this impressive study as “the most ‘vertically integrated’ study of econometric history to date. McCouldrick has performed every ‘stage of production’ himself, from the basic source work with a sample of Waltham-Lowell type textile firms (1836–86) to sorting out the various conceptual problems involved in measuring the capital stock, output, capacity, etc., and finally, to regression analysis of dividend and investment behavior” [132, 1971, p. 440].

<sup>14</sup> See E. A. Wrigley [135, 1972], especially the essay by Michael Anderson on the use of the British census manuscripts for the study of family structure.

<sup>12</sup> See R. Higgs [50, 1971] for a brief description and use of the *Report*. Higgs used the published volumes, but the manuscript questionnaires, if they have survived, would be still more revealing. The *Report*, incidentally, is a good example of the need for historical sophistication in interpreting historical statistics. It was a nativist and racist document, in the candid style of the age of the Big Stick and the White Man's Burden.

their name (or social security number) through all the records of families, businesses, the IRS, the credit bureaus, the schools, the hospitals, and the courts, our knowledge of economic behavior will, to put it mildly, increase. It has occurred to historical demographers that we need not wait until the twenty-first century (and if we wait we are liable in fact to be disappointed, for the cheapening of travel and the spread of the telephone—sans tap and tape—has impoverished the written record). If it is important for certain issues in labor economics, for example, to have collections of economic biographies of people, the historian stands ready to supply them in detail. The work of the Cambridge Group for the History of Population and Social Structure, building on the work of French historical demographers after the War, has developed two centuries of family histories in Britain from "nominal record linkage," to use the jargon, applied to birth, death, and marriage registers back to the sixteenth century.<sup>15</sup> In what is perhaps the most ambitious project of this sort to date, scholars at the University of Montreal are reconstituting the entire population of Quebec from the beginning of the colony to the French and Indian War, recording every notice of every person in the remarkably complete records of French Canada and linking them. As the age of economists and calculators dawns, statistics such as these can be linked with a widening array of records on income, property holdings, business, education, and the like to provide life histories much superior to the recent samples worked over so lovingly in any current issue of the *Journal of Political Economy* or the *American Economic Review*.

<sup>15</sup> A selection from the work of the Cambridge Group is worth including in any reading list for the new labor economics. For a nontechnical summary, see E. A. Wrigley [134, 1969], and for a recent example in detail of such work, T. P. R. Laslett [64, 1972].

The census is, of course, a survey on a massive scale, and when the manuscripts are open—i.e., when the census is old—there are few limits on economic curiosity. The Parker-Gallman work mentioned earlier, for example, matched the manuscripts of the American census of agriculture in 1860 with those of the census of population—matching that cannot be done on recent, closed censuses, that is, without the name of the respondent—and produced a full profile of those involved in farming enterprises. Because the 1860 census inquired into the wealth of those it surveyed, it is possible to examine the determinants of the distribution of wealth in 1860 at a level of detail unattainable with modern records, and Lee Soltow is currently exploiting these possibilities [106, 1975]. Roger Ransom and Richard Sutch were able to extract from the manuscript census of 1880 intimate details on a random sample of 5,283 farms in the South and to confront the issue of racial discrimination more directly than is typically possible with modern data [89, forth.]. By comparison with such rich and varied facts, the economist's usual store looks pitifully thin.

Nor are the errors in these facts larger than those in modern facts. It is naive on two counts to believe that historical statistics have larger errors, naive both in overestimating the quality of modern statistics and in underestimating the quality of historical statistics. When pressed an economist will usually admit that his "data" on, say, prices in the American economy over the last twenty years are in error to some large and unknown degree because the quality of the goods in question has improved, because the list prices correspond poorly with the transaction prices, because the definition and relevance of the sample is in doubt, or because the price index used corresponds poorly with the conceptually correct definition. He will

admit, too, that these errors introduce biases of unknown sign into his multivariate regressions containing prices as an independent variable. He will run the regressions anyway, comforting himself with the mistaken reflections that his data are as good as one can get and that his estimates are in any case consistent.

Confronted on both sides by skepticism, from his colleagues in history that statistical demonstrations in history are persuasive and from his colleagues in economics that historical statistics are reliable, the historical economist cannot take this line. He has in fact developed an art of creative self-doubt that is practiced in some other fields of economics and might be with profit practiced more widely. The habit of testing the sensitivity of one's argument to possible errors in its data or possible mistakes in its analytical assumptions is widespread among scientists and historians, but is not among economists. Many, of course, understand the frailty of "data" and act on this understanding. The tradition of the National Bureau and of the more careful empiricists outside it of publishing a full description of how data were made and where they might be wrong, in the hope (so often vain) that users will read it, fits well with historiographic traditions: in his preface to Albert Fishlow's *American Railroads and the Transformation of the Ante-Bellum Economy*, Alexander Gerschenkron drew special attention to "the statistical appendixes in which the author offers a full insight into his laboratory and without which no real appreciation of the importance of the study and the validity of its interpretative results is possible" [27, Fishlow, 1965, p. viii]. Yet it is rare for the major journals in general economics to publish factual revisionism such as Robert J. Gordon's "\$45 Billion of U.S. Private Investment Has Been Misaid," perhaps because it is rare for economists

to write it [39, 1969].<sup>16</sup> Zvi Griliches put his finger on the reason many economists are uninterested in the sources of data and their errors [42, 1974, p. 973]:

Much of the problem, I think, arises because of the separation in economics between data producers and data analyzers. By and large, we do not produce our own data and, hence, do not feel responsible for it.

For economic historians, required to collect their own materials and imbued with the historian's rather than the economist's attitude toward their handling, the buck stops here. Robert Fogel's *Railroads and American Economic Growth* [28, 1964] is perhaps the fullest example to date of this attitude.<sup>17</sup> Combining the traditions of creative self-doubt in economic history and in project evaluation, its 260 pages are directed at producing essentially one number, the benefit half of a cost-benefit study of nineteenth-century investment in American railways. Fogel began this research believing that he

<sup>16</sup> That George Jaszi of the U.S. Department of Commerce was able to argue in a comment that Gordon had discovered nothing new makes the other point: details of data, even important details, are not interesting to economists [55, 1970]. In his reply to Jaszi, Gordon asserts that "the economics profession and particularly production function investigators had remained ignorant of government owned, privately operated capital" [40, 1970, p. 945] before his article. This appears to be correct.

<sup>17</sup> Having drawn on Griliches's thinking here, it would be impolite to add that the studies on which he comments make no attempt to remove errors by remeasurement and embark instead, to repeat Leontief's acerbic remark quoted earlier, on "devising a new statistical procedure, however tenuous, that makes it possible to squeeze out one more unknown parameter from a given set of data" [66, 1971, p. 3].

<sup>18</sup> Fogel's calculations were for 1890. Fishlow's *American Railroads* is a similar study for the early nineteenth century [27, 1965]. Together they constitute a brilliant reinterpretation of the role of transportation in American growth, for which they were awarded in 1971 the Schumpeter Prize. The account of Fogel's experience derives from conversations with him.



would confirm the assumption of the indispensibility of the railways underlying earlier treatments (by Schumpeter and Rostow, for example), but found to his surprise that the facts cast doubt on it. To test this doubt, therefore, he directed his energies to estimating an upper bound on the contribution of railroads to national income and found it low; therefore, he concluded that railroads were far from indispensable for American economic growth. Historical facts are often better for economists' purposes than recent facts: they are often more detailed, voluminous, and accurate, and what errors they contain are treated with respect.

But there is, of course, another sense in which they are "better," for history performs experiments: history provides the economist not only with more rich and accurate facts but also with more variable facts. A macabre example of the point is T. W. Schultz's use of Indian statistics of agricultural output and population during the influenza epidemic of 1918-19 to argue that the marginal product of labor was positive and roughly equal to the going wage: output fell as the working population did, and labor therefore was not "surplus," contrary to the assumption of much work on economic development, particularly Indian economic development [100, 1964, pp. 63-70]. An equally dismal experiment, the Great Depression, will remain for a long time to come the great testing ground for theories of aggregate economics, as monetarists, fiscalists, and others have on occasion realized. The appreciation of the pitfalls of monetary policy was much increased by the argument of Friedman and Schwartz that, far from having little impact, it was powerfully mishandled in the 1930's; and the appreciation of the potential of fiscal policy was much increased by the argument of E. Cary Brown that, far from failing, it was not in fact tried [8, 1956] (see also L. C. Peppers [85, 1973]).

That history has performed the very experiment he wishes had been performed must occur from time to time to every economist. He must realize, too, that economics is like astronomy an observational science, taking its data and its controls, alas, as it finds them. Yet he fixes his telescope (during his infrequent trips to the observatory) on the sun, moon, and nearer planets alone, for two reasons: first, he believes that these objects close to home are the only ones that provide insight into how the home planet behaves; and, second, he believes that to look beyond the near solar system, not to speak of the galaxy, is to look into another structure, where familiar laws (e.g., there are six planets, stars are little points fixed to a sphere, and light moves in straight lines) might not apply. The belief that history is irrelevant to public policy will be examined below and will prove to be incorrect. The incorrectness of the belief that history might come from a different structure than the quarterly national income figures since the War and is therefore to be ignored is plain enough. To those who adopt the argument in order to limit the amount of empirical work they have to do, one can only sigh and turn back to scholars who take scholarship seriously. These innocents will always believe that "empirical work" is a conflation of the appendix to the *Economic Report of the President* and Johnston's *Econometric Methods*. Even serious and sophisticated economic scholars are prone to adopt the assumption that the past has a different structure without testing it. The cliometricians have been forced to test the assumption at every turn, facing as they do scholars in both economics and in history who adopt it as a matter of course. Indeed, if the findings of the new economic history of the last fifteen years or so had to be put in one sentence, it would be this: in the eighteenth and nineteenth centuries men sought profit in as clear-headed and com-

petitive a way as an economist dreaming of auctioneers and perfect markets might wish. Following Lenin and Veblen, of course, one is free to assert that the atomistic competition of the age of Smith and Mill is dead, that simple models of competitive behavior might apply to the nineteenth century but not to the twentieth. This variant assertion, however, merely reinforces the point, for it has never been tested, at any rate not in a way that would convince someone who did not believe it to begin with, despite its large role in the political economy of the last fifty years. Even if one could show that for a particular experiment (the effect of government spending on employment, say) the environment of the nineteenth century was so different from that of the 1970's that little could be learned about the present structure from the comparison, it would remain true that structures continue to change, as the often discouraging and sometimes comical results of large scale econometric models suggest. History, like the study of other countries and cultures, is an education in structural change. A familiar example of the more usual practice is that of dropping the War years from regressions, as intrusions from another structure. Wars, however, recur, and it behooves the economic scientist, even if his interests in science extend only to its uses for today's public policy, to understand how war changes the way economies behave (see, for example, D. F. Gordon and G. M. Walton [38, 1974] and Mancur Olson [81, 1963]). Paul David put a similar point in the following way [15, 1975, p. 14]:

An equation that fits the data well for half the available run of time-series observations and not for the rest is, for the ordinary applied economist, a failure; he will have to resist the impulse to discard the recalcitrant data in presenting his results. By contrast . . . the economic historian may hail the half-failed regression equation as nothing less than a tri-

umph—in the sense that by uncovering the occurrence of a change in economic structure, it signals him to set to work to learn what happened in history.

Limiting one's field of vision to close objects, in any case, is as peculiar in economics as it would be in astronomy. Examples of historical experiments larger, clearer, and more decisive than most that could be framed on the basis of recent experience can be generated at will. The migrations from one country or another in the last twenty years that have alarmed modern governments are dwarfed by the migrations of the nineteenth century.<sup>19</sup> The same can be said of the migrations of capital: if one wishes to measure the effects of foreign investment on the sending or receiving country, the British, French, Argentinian and Canadian experiences of the late nineteenth century are the best available cases in point.<sup>20</sup> From 1870 to 1913 Britain sent one-third of her savings abroad. If one wishes to measure the effects, burdensome or otherwise, of government debt, the British experience with the debt from the Napoleonic War or the American experience with the debt from the Civil War are the clearest experiments, taking place as they did before the Internal (or Inland) Revenue codes among other disturbing influences reached their present chaotic state (for the American case see J. C. Williamson [130, 1974]). In the 1820's the British government debt was on the order of two and a half times national income, about the same as the

<sup>19</sup> There is a voluminous literature by historical economists on these: B. Thomas [114, 1954], R. A. Easterlin [22, 1961], and many more, among them P. Hill [52, 1970], L. Neal and P. Uselding [75, 1972], and A. C. Kelley [58, 1965].

<sup>20</sup> See Michael Edelstein [24, 1974] and works cited there. The seminal work on the case of a receiving country is Jacob Viner [120, 1924]. The other books from the Taussig school of international finance published at Harvard in the 1920's and 1930's were also richly historical: J. H. Williams [128, 1920], H. D. White [126, 1933], and W. F. Beach [4, 1935]. Taussig himself as a young man wrote history [101, 1888].

present ratio in the United States.<sup>21</sup> If one wishes to measure the impact of legal changes, the experiences in the nineteenth century and before with laws of incorporation, school attendance, child labor, and the like are large and varied experiments [109, Sylla, 1969; 63, Landes and Solmon, 1972; 124, West, 1975; 99, Sanderson, 1974]. So too, if one wishes to measure the impact of floating exchange rates, are the experiences of the United States in the 1860's and 1870's, of Britain from 1914 to 1925, or of China in the 1930's. In a time of free banking, as in the United States before the Civil War, one can examine the consequences of free entry [92, Rockoff, 1974]; in a time of war and unfettered capital markets, as during the Civil War, one can examine the responsiveness of expectations to events [94, Roll, 1972]; in a time of massive new investment in public hygiene, as in American cities after the Civil War, one can examine the value of health [71, Meeker, 1972; 72, 1974]. History is society's laboratory.

### C. Better Economic Theory

The products of this laboratory affect economic ideas in ways that few economists recognize. The headline of today's newspaper, to be sure, has an effect, the

more so as money for research follows the headline with a short lag. But historical findings, true or false, underlie the reaction to the headline. To pick some influential historical findings that have recently been shown to be false by cliometricians, the finding that the increase in the capital stock per man left much of the increase in income per man unexplained set off in the late 1950's an intellectual explosion in models of growth with technological change. The historical finding that the rate of savings was constant over a long period set off in the early 1950's a somewhat smaller explosion in the theory of the consumption function. The historical finding that the share of labor in income has been constant set off in the 1930's still another in the theory of the production function. The influence of economic theory on the writing of history is apparent in most pieces of new economic history, but the influence of economic history on the writing of theory is apparent only in the seminal pieces, to be forgotten in the sequel. The high ratio of historical reserves to theoretical deposits in the work of Robert Solow, Milton Friedman, or Paul Douglas is not maintained in the work of their intellectual customers, with the result that the intellectual money supply is a large multiple of the factual base and subject to violent fluctuations. Rondo Cameron put the point well:

In analogous discussions concerning the role of theory in historical research the argument is frequently made (perhaps because it is valid) that the historian will inevitably be guided by some *a priori* ideas. It is desirable, therefore, that these ideas be made explicit and systematized if possible. The choice, in other words, is not between theory and no theory, but explicit, consciously formulated theory and implicit, unconscious theorizing. Much the same can be said for the use of history by theorists. Even the most scornful ahistorical economist makes some use of history: his own experience, the experience of his generation, or the loose historical generalizations which abound in the folklore of even highly sophisticated societies. [9, 1965, p. 112; cf. 74, Mitchell, 1927, p. 59.]

The obvious case in point is the theory of economic growth, in which a particular set of historical conventions dominate the argument. These conventions—described by Nicholas Kaldor in 1958 as “stylized facts,” a defensive usage that has been adopted widely—were developed in the 1950's, once intellectual putty but now clay, before new economic historians had begun in earnest to announce the unstylized facts. It is at least uncertain that their work will confirm the constancy of the capital/output ratio, of the rate of profit, or of the rate of growth of output per man and of the capital stock. As Robert Solow remarks at the conclusion of a brief inquiry into the factual relevance of these elements in the steady state of economic growth, “the steady state is not a bad place for the theory of growth to start, but may be a dangerous place for it to end” [105, 1970, p. 7]. From the historical work by economists over the last two decades or so, ignored by growth theorists, it would seem so. During the second half of the nineteenth century, for example, the capital/output ratio in America rose by a factor of two, while falling by a third in Britain; during the first half of the twentieth century, the ratio in America fell 22 percent, while remaining roughly constant in Britain.<sup>22</sup> It may be that a fuller definition

<sup>22</sup> For the United Kingdom this is the ratio in 1855, 1900, and 1958 of fixed reproducible capital net of depreciation to net domestic product (see C. H. Feinstein Tables 43 and 20, with an allowance for capital consumption based on Table 1 [25, 1972]); for the United States the ratios in 1844–53, 1894–1903, and 1958 of depreciable capital to net national product (Gallman's Table 2.9 in Davis *et al.* [18, 1972]). The two books from which these figures come, incidentally, illustrate the role of social observatories in the encouragement of new economic history: Feinstein's is one of a series published under the auspices of the National Institute of Economic and Social Research (the British equivalent of the NBER) and the Cambridge Department of Applied Economics (an interpretive volume by Feinstein and R. C. O. Matthews is to follow); eight of the twelve authors of the Davis *et al.* book have worked at the NBER, and the book itself amounts to an interpretive summary of the long inquiry by them and others (notably Simon Kuznets) at the Bureau into trends in American economic growth.

of “capital” to include acquired human skills and a fuller definition of “output” to include production in the household would yield different results. Economic historians, facing long periods of history in which the relation of the narrow to the full definitions have changed radically, are forced routinely to consider refinements of this sort. Whether refined or not the facts accumulated by them for the study of economic growth warrant a second look. This is perhaps most clear in the matter of technological change, the chief jewel and the chief embarrassment of the modern theory of growth. As R. R. Nelson and S. G. Winter have recently emphasized [76, 1974], historians of technology such as Paul David, Peter Temin, and Nathan Rosenberg have much to tell the theorists, but the theorists' minds are fixed on other things (see, *e.g.*, Rosenberg [95, 1972] or David [15, 1975]).

The sins of pseudo-history are not, of course, confined to mathematical theorists of economic growth. There is nothing in words as distinct from equations, however frequent the appeals in the words to the alleged experience of history, that protects looser theorizers from the error of irrelevancy. Ricardo's notion of rising land rents, Marx's of immiserization of the industrial proletariat, Lenin's of the profits from imperialism, Dennis Robertson's of foreign trade as an engine of growth, Harold Innis's of staple products as centers of growth, W. A. Lewis's of development with unlimited supplies of labor, or W. W. Rostow's of a take-off induced by great inventions and a sharp rise in the savings rate, to name a few, have not fared well in confrontation with historical fact.<sup>23</sup> This

<sup>23</sup> See, in order, P. Lindert [68, 1974]; R. M. Hartwell [46, 1970]; R. P. Thomas [115, 1968]; I. B. Kravis [62, 1970]; E. J. Chambers and D. F. Gordon [11, 1966]; A. C. Kelley, J. C. Williamson, and R. J. Cheatham [59, 1972]; and W. W. Rostow, ed. [97, 1963]. The work by Fishlow and Fogel on the American railway in the nineteenth century was in part motivated by Rostow's large claims for it as the critical innovation in American growth.

<sup>21</sup> One must include gross social security wealth, gross of the present value of social security taxes (as estimated by Martin Feldstein [26, 1974, p. 915, col. 3]). In 1971 GNP was on the order of \$1,000 billion, the gross government debt \$400 billion, and Feldstein's estimate of gross social security wealth \$2,000 billion, for a ratio of about 2.4. In 1821 the GNP of the United Kingdom was on the order of £340 millions (based on the P. Deane and W. A. Cole [19, 1962] estimate for Great Britain increased by an estimate of Irish income at two-thirds the British level *per capita*) and the funded and unfunded government debt £840 million [73, Mitchell, 1962, pp. 8, 366, 402], for a ratio of about 2.5. The ratio of payments of interest to GNP in the U.K. in 1821 was 9 percent, about the same as interest, income security, and veterans benefits and services to GNP in the U.S.A. in 1971, namely, 8 percent [73, Mitchell, 1962, p. 396; 119, *Economic Report of the President*, 1975, p. 325]. These comparisons could be refined to include local governments and non-interest-bearing debt.



is not to say that theorists should forsake their blackboards or their typewriters for the nearest archive. An occasional trip to the library might help. And they should be doubtful of their own unassisted ability to summarize historical experience in a few stylized facts.

The contribution of history to theory is not confined to a supply of factual grist for the theorists' mill. The use of theory in economic history illuminates the theory and tests it, and in this respect economic history is no different from other applied economics. An application of input-output analysis to the measurement of effective protection in nineteenth-century America tests the usefulness of this tool in the same way as does an application to the measurement of effective protection in present-day Pakistan [127, Whitney, 1968; 43, Guisinger, 1970]. An agricultural economist, at least, would be comfortable with the use of simple models of supply and demand to explore the growth of the American shipbuilding, cotton textile, or iron industries, and would not be surprised that use deepens them.<sup>24</sup> Nor would the student of international trade, aggregate economics, or labor markets find anything strange in applications of models of two sector general equilibrium to the American economy before and after the Civil War or to the British economy during the Napoleonic War,<sup>25</sup> of money and prices to the British, and American business cycle in the early nineteenth century [113, Temin, 1974], or of marginal

productivity to slavery or post-bellum sharecropping.<sup>26</sup> He might be a little surprised that such remote issues can be reached with tools perfected in the middle of the twentieth century and might, too, admire the skill with which issues long cut off from economic thinking are brought back to it. But on the whole he would perceive good economic history to be simply good applied economics.

It should be pointed out, parenthetically, that in an important sense his perception would be wrong, for good economic history must also be good history. It is this requirement that puts economic history at the highest levels on a par of difficulty with, say, econometrics at the highest levels, which requires a mastery of statistics, or mathematical economics at the highest levels, which requires a mastery of mathematics.<sup>27</sup> True, some new economic historians believe that economic history consists of the application of production theory or econometrics to a more or less vague notion of what happened in history, just as other economists believe that economic thinking consists of the application of Lagrangean multipliers or optimal control theory to a more or less vague notion of what is to be maximized. But the best new economic historians are historians as well as economists, just as the best economists are social scientists as well as applied mathematicians.

Even at the lower levels of historical as distinct from economic sophistication,

<sup>24</sup> C. D. Goldin [36, 1973; 37, 1976] and Fogel and Engerman [31, 1974] are recent examples of a large literature on slavery deriving from the early work of Conrad and Meyer [13, 1958]. J. D. Reid, Jr. [91, 1973] is an example of an equally large literature on sharecropping by cliometricians, among them Robert Higgs [51, 1974], Stephen DeCanio [20, 1974], and Roger Ransom and Richard Sutch [89, forth.].

<sup>27</sup> Any economic historian has had the experience of colleagues announcing to him that they, too, are economic historians. It usually develops that they have run a regression back to 1929. The effect is similar to that of an economist who uses arithmetic announcing to his colleagues in mathematical economics that he, too, is a mathematical economist.

however, reforming the economic history of the late 1950's into good applied economics, exhibiting in the reformation the power of modern economic theory, was a remarkable achievement, comparable with the reformations over the last decade in the economics of politics, property rights, labor markets, and the household. For a time the new economic historians, like the new labor economists and the rest, devoted themselves to this task of intellectual arbitrage. But the theoretical rewards of economic history are greater. Any extension of economics to new subjects sets new questions with which existing theory cannot deal, and for which new theory must be created. Economic historians have been bold in this. Their theoretical boldness arises in part from the recalcitrance of the world: when the scholar's chief purpose is to understand a piece of behavior, historical or current, rather than to test a familiar economic idea (still less to develop its logic), he takes his insights from wherever he can get them, whether or not they bear the *imprimatur* of an economic bishop.<sup>28</sup> It arises, too, from the unusually close contact that historical economists have with another discipline, history. They have internalized the intellectual values of historians more than sociological economists have internalized those of sociologists or legal economists those of lawyers, and in consequence are peculiarly inclined to face questions for which economics has no ready answer. A case in point is the question of why political and social revolutions occur, a question that even most political scientists and sociologists, contrary to what one might ex-

pect, carefully avoid. It is impossible for a historian who wishes to write coherent history to avoid the question, even if he wished to, for revolutions, such as the American Revolution and the Civil War, are the stuff of change and change the stuff of history.<sup>29</sup> For this reason a good deal of the new economic history in America has centered around the causes of the Revolution and the Civil War, approaching the causes (as comparative advantage dictates) with the characteristically economic assumption of rational and informed self-interest. The new economic history made a contribution, albeit a modest one, to the understanding of the American Revolution by measuring the economic burden of the Navigation Acts and finding it small (see P. D. McClelland [69, 1969] and works cited there); it made a contribution to the understanding of the Civil War by measuring the economic burden on the South of the tariff or of the restrictions on the expansion of slavery and finding these also to be small [87, Pope, 1972; 84, Passell and Wright, 1972]. If one believes that economic interests determine political behavior, then, one can look to new economic historians for measurements of these interests. If one does not believe it, one can look to new economic historians for whatever economic measure is to the point: by showing, for example, that slavery was not economically moribund on the eve of the Civil War, the new economic historians were able to reject the theme of many historians sympathetic with the South that military intervention

<sup>29</sup> Coherence was sacrificed for the gains from specialization in the collective volume by Davis, Easterlin, Parker, and others [18, 1972], in so many other ways such a fine summary of the work of the cliometricians. Its focus on the economic revolution (its subtitle was "An Economist's History of the United States") required it to by-pass the contributions of new economic history to political history. The Revolution, Jackson and the Second Bank, the tariff, slavery, the Civil War, and the free coinage of silver occupy, according to the index, 20 pages in total, smaller than the single entry "Canals."

<sup>24</sup> See among many others C. K. Harley [45, 1973]; R. B. Zevin [138, 1971]; and R. W. Fogel and S. L. Engerman [29, 1969], reprinted in Fogel and Engerman [30, 1971]. This book [30, 1971], incidentally, is a good selection of work on America by new economic historians, as is P. Temin [112, 1973].

<sup>25</sup> See C. Pope [87, 1972]; P. Passell and G. Wright [84, 1972]; P. Passell and M. Schmundt [83, 1971]; G. Hueckel [53, 1973]; and, the most ambitious work to date along these lines, J. C. Williamson [130, 1974]. Cliometricians have been among the few economists to use non-linear general equilibrium models empirically.

<sup>28</sup> A good example is J. G. Williamson, especially Chapter V [129, 1964]. After using without success the usual theories to explain the American balance of payments in the nineteenth century, he developed finally what is now known as the monetary theory, anticipating by several years its first theoretical statement. P. B. Whale, beginning with a similar historical problem, had done the same in 1937 [125, 1937].

to abolish slavery was unnecessary.<sup>30</sup> In any case, the application of economics to politics raises the theoretical issue, neglected by most economists (namely, most economists to the left of Milton Friedman and to the right of Paul Sweezy), of bringing politics into economic models.<sup>31</sup>

The new economic history has turned increasingly in the past few years to issues such as this, central to the development of economics as a social science. The deepening of the study of American slavery, for example, notably by Fogel and Engerman in their recent book [31, 1974], has opened the issue of the role of coercion in economic society. Outside the growing band of Marxist economists, who are like their colleagues on the right unusually historically-minded, the limit of thinking on the matter in economics has been an occasional remark on command compared with market economies, assuming in the background that market economies use little coercion beyond taxes, enforcement of contract, and the criminal law. The assumption has never been appropriate for that quarter of the population under the age of responsibility, and in a slave society, of course, it is still less appropriate. Fogel and Engerman were able to show, however, that Southern slaveowners, capitalistic as they were, used market mechanisms as well as the whip to manipulate their slaves. In "Slavery: The Progressive Institution?" a long review of the book, two other economic historians, Paul David

and Peter Temin, argued that the economic theory to deal with such mixed systems of enticement and coercion does not exist [16, 1974, esp. pp. 778-83]. It may not, and this is the challenge to theory.

The challenges arise from the wide perspective forced on the economic historian by his subject. Obviously, one cannot study the long swing, if one wishes to, without long swings in income [61, Klotz and Neal, 1973]; one cannot study the long-run determinants of city size without long runs of city sizes [108, Swanson and Williamson, 1974]. But the point goes deeper than this. An economist whose attention is riveted on the present cannot be expected to ask why the institutions of the labor and capital market change, as Lance Davis and Douglass North did in *Institutional Change and American Economic Growth* [17, 1971], still less to ask why fundamental social arrangements rise and decay, as North and Robert Thomas did in *The Rise of the Western World* [79, 1973]. At a more modest level, few economists outside of agricultural economics and economic history have given serious attention to measuring (as distinct from theorizing about) managerial ability or, in more elaborate language, entrepreneurship, that phantom of the theory of the firm. The measurement was forced on economists studying agriculture by the insistence of government planners who were not economists that farmers are irrational; it was forced on economists studying the Victorian economy by the insistence of historians who were not economists that British businessmen in the late nineteenth century were irrational as well [98, Sandberg, 1974]. And even agricultural economists, on the whole exceptional among economists for their long historical perspective, cannot be expected to ask why the peculiarities of peasant land tenure have survived in many countries for centuries and why they were dissolved in land reform.

The Icelandic poet Einar Benediktsson put it this way: "To the past you must look/ If originality you wish to build;/ Without the teaching of the past/ You see not what is new."<sup>32</sup>

#### D. Better Economic Policy

Few intellectual activities are more mischievous when done poorly than economics or history. The power of fallacious economic reasoning or fallacious historical example to damage society is obvious: the pseudo-economics of mercantilism has been reducing trade and protecting vested interests for many centuries; the pseudo-history of the Aryan "race" lent dignity to German fascism. The combination of bad economics and bad history in bad economic history is pernicious. To be sure, the makers of economic policy have ample opportunity for falling into error without the excuse of economic history poorly grasped. Yet, to specialize Keynes's frequently quoted remarks on the subject—frequently quoted, perhaps, because they are correct—the ideas of economic historians, both when they are right and when they are wrong, are more powerful than is commonly understood. Madmen in authority, who hear voices in the air, are distilling their frenzy from an understanding of the economic events of a few years back. Practical men, who believe themselves to be quite exempt from any historical influences, are usually the slaves of historical example.

The industrial revolution, it is said, came to Britain suddenly and simply around 1760 in a wave of gadgets, justifying policies for growth that equip illiterate peasants with computers. Foreign trade, it is said, was an engine of economic growth in Britain (and, lately, Japan), justifying a policy of impoverishing one's citizens in the pursuit of exports. Floating exchange rates, it is said, added to the chaos

<sup>32</sup> I owe this quotation to Jon Sigurdsson of the Icelandic Economic Development Institute.

of the international economy in the 1930's, justifying the sacrifice of employment to the maintenance of \$4.86, \$2.80, \$2.40, or (most recently) \$2.00 to the pound sterling. Railways, it is said, were crucial to industrialization in the nineteenth century, justifying policies in nonindustrial countries in the twentieth of shoring up railways with subsidies and of eliminating trucking competition. Industrialization, it is said, brutalized the working class, justifying among most educated people a deep suspicion of capitalism. Labor unions, it is said, were responsible for a good part of the increase in wages since 1900, justifying government protection of extortionate plumbers, electricians, and butchers. The competitive supply of professional services in the nineteenth century, it is said, grievously injured consumers, justifying official cartels of doctors and undertakers. Business monopoly, it is said, has spread greatly during the last century, justifying public hostility towards big business. The payment of competitive interest on demand or time deposits, it is said, created instability in the banking system, justifying laws to forbid it. Air pollution, it is said, is worse now than it was once, justifying draconic policies to combat it. Fossil fuel, it is said, is being used at a faster rate relative to proven reserves now than fifty years ago, justifying national goals of subsidizing new fuels and abandoning international trade in oil. Whether these are good or bad policies, to the extent that their public propaganda and their private inspiration rest on false historical premises—and most of them to a large extent do—their rationale is full of doubt.

One could add cases in point without limit, but two of the more important will suffice. The muddle of exchange rates in the 1920's and 1930's led to the development of the elasticities approach to the balance of payments, which to this day dominates theory and policy. The ap-

<sup>30</sup> This was explicit in Conrad and Meyer [13, 1958], although it was not put on a firm base until Y. Yasuba [136, 1961]. Cf. C. Gunderson [44, 1974].

<sup>31</sup> Violent revolution and civil war are not the only political events to attract the attention of new economic historians: see J. Pincus [86, 1972] on the causes of early nineteenth-century tariffs; E. P. LeVeen [67, 1971] on the British suppression of the slave trade; R. B. Freeman [32, 1972]—an example of fine historical work by a non-historian—on the rise of educational discrimination in the South; R. Higgs [49, 1971, chap. IV] and J. Bowman and R. H. Keehn [5, 1974] on agrarian protest in the late nineteenth century; and G. Wright [133, 1974] on the political economy of New Deal spending.

proach has been under attack now for several years on logical ground, but the development of an alternative will depend on a reinterpretation of past experience with exchange rates.<sup>33</sup> The muddle of employment in the 1930's and the interpretation of the muddle by Keynes and others led to the postwar policy of full employment and to a concentration on fiscal methods to achieve it. Their interpretation bears rethinking. As Hugh Rockoff remarked in a recent survey of the American experience with free entry to banking, "One purpose of history is to broaden our conception of the possible" [93, 1975, p. 176]. The apprehension of true history as well as the correction of false contributes to public policy because an economist whose memory is limited to the recent past has a narrow conception of the possible. We may in our praise and criticism of present governments be willing or unwilling slaves of historical example, but slaves we are.

#### E. Better Economists

In the light of all this, it is not surprising that Smith and Marshall, Schumpeter and Keynes were deeply historical in their thinking. An economist, least of all a cliometrician, cannot argue that there are no substitutes for history in the production of important economics, no more than he can argue that there was no substitute for the railway in American economic growth. Some important economics have been written by historical illiterates, although it must be admitted that cases are difficult to find. The work of Edgeworth as distilled in modern textbooks, for example, seems a likely candidate until one reads the work itself and stumbles over tags from Herodotus. In much of the work

of J. R. Hicks it is not obvious that history plays a part, yet he lectured on medieval history in one of his early academic appointments, has been by his own account a lifelong reader of the *Economic History Review*, and published in 1969 *A Theory of Economic History* [48, 1969, p. v] (see also [47, 1953]). History is a stimulus to the economic imagination, defining and stretching the limits of economic craft. An economist learns from his other studies how to see, to label, and to repair the pieces of the economic building. From history he learns whence the building came, how its neighbors were built, and why a building in one place was and will be built differently from one in another. The wider questions that face economics are historical. If history is useful to an economist's work, it is still more useful to his education.

It would be unreasonable to propose in the style of the German historical school that history dominate the education of economists, that abstractions of maximization be abandoned in favor of the concreteness (or, more commonly in practice, the verbal abstractions) of history. The reaction to this unreasonable proposal, indeed, explains some of the drift towards present-mindedness in modern economics. Yet, as the English economic historian T. S. Ashton said [3, (1946) 1971, p. 177]:

The whole discussion as to whether deduction or induction is the proper method to use in the social sciences is, of course, juvenile: it is as though we were to debate whether it were better to hop on the right foot or on the left. Sensible men with two feet know that they are likely to make better progress if they walk on both.

An economist hopping along without a historical leg, unless he is a decathlon athlete, has a narrow perspective on the present, shallow economic ideas, little appreciation for the strengths and weaknesses of economic data, and small ability to apply economics to large issues. If we interrogate our students, we will find that

<sup>33</sup> Presently much of the effort of the International Trade Workshop at the University of Chicago is devoted to applying the "monetary" approach to the experience of England, France, and Japan before the Second World War.

they believe economic research to consist chiefly of a passing acquaintance with the latest pronouncement of the Council of Economic Advisors, the latest assumption relaxed in an economic model, and the latest revision in the local canned regression program. One does not have to look beyond their teachers to find where they acquired this peculiar set of notions.

For fifteen years or so cliometricians have been explaining to their colleagues in history the wonderful usefulness of economics. It is time they began explaining to their colleagues in economics the wonderful usefulness of history. Wonderfully useful it is, a storehouse of economic facts tested by skepticism, a collection of experiments straining the power of economics in every direction, a fount of economic ideas, a guide to policy, and a school for social scientists. It is no accident that some of the best minds in economics value it highly. What a pity, then, that the rest have drifted away. Does the past have useful economics? Of course it does.

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