A STRANGE CAREER:
THE HISTORICAL STUDY OF ECONOMIC LIFE

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ABSTRACT

This article attempts to account for professional historians’ relative neglect of the history of economic life over the past thirty years, looking mainly at the American case. This neglect seems paradoxical, considering the remarkable transformations that have taken place in world capitalism during this same period. I trace the neglect to the capture of the once interdisciplinary field of economic history by mathematically inclined economists and to the roughly simultaneous turn of historians from social to cultural history. I conclude by suggesting some topics in the history of economic life that seem both timely and exciting. I also suggest some intellectual resources that other disciplines, particularly economic sociology and economic history, could offer should historians decide to tackle the history of economic life once again.

Keywords: economic life, economic history, social history, cultural history, economic sociology, macro-dynamics of capitalism.

I do not believe it is possible to make useful predictions about the next fifty years of historical scholarship. Instead, I attempt something far more modest: to identify an area of inquiry that I feel has been neglected by historians in recent decades—the study of economic life—but that I expect to figure much more centrally in historical scholarship over the next decade or two. I try to assess the historical causes of this neglect, which I regard as highly paradoxical, and to suggest how it might be overcome. The history of economic life—by which I simply mean the history of human participation in the production, exchange, and consumption of goods—has, in my opinion, had a strange and unfortunate career in the United States over the decades since the end of World War II.

Economic life was initially studied primarily by economic history—a new, robust, and promisingly interdisciplinary field in the middle years of the twentieth century. The emergence of economic history as a distinct field of study was marked by the appearance of specialized journals: the Economic History Review in Britain in 1927; the Annales d’histoire économique et sociale in France in 1929; and the Journal of Economic History in the United States in 1941. In the postwar decades, economic history thrived in America in both history and economics departments. But over the course of the 1970s and 1980s, the field came to be increasingly dominated by economists. This development was over-determined: on the one hand, mathematically inclined economic historians, based in economics departments,
developed new and intellectually powerful methods (dubbed “the new economic history”) that proved intimidating to most history-based economic historians, who normally lacked the mathematical training either to use or to criticize them effectively. Meanwhile, over the same years, social historians, who in the 1960s and 1970s had been deeply interested in economic aspects of social change, and who were close allies of economic historians in an effort to develop more quantitative and social-scientific forms of historical inquiry, increasingly turned to the new cultural history in the 1980s and in the process lost interest in both quantification and the economy. Indeed, cultural history was marked by an intense reaction against economic determinism, against quantification, and against the positivist epistemological outlook that had previously united social and economic historians. The result has been the development of an economic history almost entirely dominated by the concerns of professional economists and a shrinking-back of work on the history of economic life carried out by historians.

This has, in my opinion, left the history profession in an increasingly untenable position. Precisely in the years since 1980, when globalization, deindustrialization, repeated financial crises, and soaring economic inequality should have made obvious the need for a deeper historical understanding of modern capitalism’s dynamism and perversity, historians have largely abandoned the historical study of economic life while economists turned economic history into a branch of mathematical development economics. My goal in this article is to understand how this situation came about and to make some suggestions about how it might be remedied.

I. THE RISE OF “THE NEW ECONOMIC HISTORY”

In the postwar United States, specialists in economic history could be found in both history and economics departments. By the 1960s and 1970s, most major PhD-producing history departments had economic historians on their faculties: there were, for example, David Landes at Berkeley and then Harvard; Jan de Vries at Berkeley; Robert Reynolds and Domenico Sella at Wisconsin; Rondo Cameron at Wisconsin and then Emory; David Herlihy at Wisconsin and Harvard (later at Brown); Shepard Clough at Columbia; Jerome Blum at Princeton; Sylvia Thrupp, Jacob Price, and Albert Feuerwerker at Michigan; Robert Brenner at UCLA; Arcadius Kahan and John Coatsworth at Chicago; Frederic Lane, Louis Galambos, and Richard Goldthwaite at Johns Hopkins; and Robert Lopez and William Parker at Yale. For reasons mysterious to me, most prominent historians of the United States economy seem to have taught in economics departments in these years (Parker and Galambos were exceptions). This was true, for example, of Robert Fogel at Rochester, Chicago, and Harvard; Jeffrey Williamson at Wisconsin and then Harvard; Stanley Engerman at Rochester; Albert Fishlow at Berkeley; Peter Temin at MIT; Lance Davis at Cal Tech; Richard Easterlin at Penn; Harold Williamson at Northwestern; Douglass North at the University of Washington and then Washington University; Robert Gallman at North Carolina; Gavin Wright at Michigan and then Stanford; and Paul David at Stanford. But economic historians working on other regions of the world also taught in economics departments: for
example, Carlo Cipolla (Europe) at Berkeley, Henry Rosovsky (Japan) at Berkeley and then Harvard, and Donald McCloskey (Europe) at Chicago and then Iowa (now Deirdre McCloskey at the University of Illinois, Chicago).

But in the early twenty-first century, economic historians have become relatively rare in leading PhD-producing history departments. According to my survey of history department websites, seven of the top sixteen departments, as ranked by the latest US News survey, currently have no specialist in economic history and only four (Yale, Michigan, UCLA, and Northwestern) have two or more such specialists. UCLA, with seven, is the only history department with more than two.1 Especially in light of the fact that the faculties of many of these departments have grown substantially since the 1970s, these figures indicate a major erosion of economic history within the history profession.2 For all intents and purposes, the field has decamped into economics departments.

This change in the departmental location of economic historians traces a change in the nature of economic history as an intellectual field. Most of the articles published in the first years of the Journal of Economic History (henceforth JEH) would not have been out of place in general historical journals of the time. The review’s second volume, in 1942, included articles on classical economic history topics, such as “Early English Woodland and Waste” or “British-American Rivalry in the Chilean Trade, 1817–1820.” But there were also studies that might more readily be classified as social or political or intellectual history, such as “Stephen Girard, Promoter of the Second Bank of the United States”; “Land Hunger and Nationalism in the Ukraine, 1905–1917”; “Indentured Servants: New Light on Some of America’s ‘First’ Families”; or “Keynes and Proudhon.” Ten years later, in 1952, nearly all of the nineteen articles published in the JEH were recognizable as economic history, although there were still two devoted to the history of economic thought. This indicates that the Economic History Association and its journal had been successful in helping to consolidate a distinct field of study.

But even in the early 1950s, articles in the JEH had relatively little specialized scholarly apparatus. With the notable exception of an article by the future Nobel laureate Simon Kuznets, few made any reference to economic theory or made much use of statistical techniques.4 To be more exact, of the twenty-five articles published in the JEH from 1951 through 195, only eight made use of numerical tables or graphs to present data, and in most of these cases the tables and graphs

1. These figures are based on my survey of information on the departments’ websites. The departments with no specialists in economic history were Princeton, Chicago, Johns Hopkins, Penn, North Carolina (Chapel Hill), Cornell, and Wisconsin. There is only one economic historian each at Berkeley, Stanford, Harvard, Columbia, and Duke.

2. Among the top sixteen departments, only Johns Hopkins, Northwestern, and Duke have rosters of less than forty full-time tenure-track historians. Yale, Michigan, and UCLA—perhaps not coincidentally three of the four departments with two or more economic historians—all have rosters of seventy or more.

3. The authors and page numbers of these articles are, in order: H. Neilson, 54-62; Dorothy Burne Goebel, 190-202; Kenneth L. Brown, 125-148; H. R. Weinstein, 24-35; Abbot Anderson Smith, 40-53; and Dudley Dillard, 63-76.

4. Kuznets’s article, “National Income Estimates for the United States Prior to 1870,” demonstrated that the estimates of U.S. national income for the first seven decades of the nineteenth century that had previously been used by most economists were statistically incoherent and therefore essentially valueless. JEH 12 (1952), 115-130.
gave only descriptive statistics rather than engaging in statistical manipulation of the data. With the exception of Kuznets, the authors never went beyond indexing prices or stating quantities as percentages. Typical was an article by Robert T. Patterson, whose only table indicated the absolute dollar value of bonds authorized and sold at the outbreak of the U.S. Civil War. Many of the articles published in this period made quantitative claims of some sort, but these were rarely based on rigorous statistical methods or mathematical economic reasoning.

One might say that the epistemological object for a majority of economic historians in the 1950s was not “the economy” but what one might call “forms of economic life.” Hence there were many articles in the early years of economic history journals on particular firms, industries, entrepreneurs, policies, or institutions. Often such studies asked, at least in passing, what the firms or institutions had to do with the growth or stagnation of the national economies of which they were a part, but the question of growth was usually not the central concern. Economic life had its particular features—pursuit of profit, regulation of markets, hedging against risk, sustaining adequate consumption levels, organizing work effectively, and so on. But the history of economic life was regarded by most of its practitioners as but one aspect of the overall history of the human adventure. The great French medieval economic historian Marc Bloch put this point trenchantly in 1929 in the first volume of the *Annales d’histoire économique et sociale*. In a critical review of studies of medieval economic history, he stressed that “intimate links united economic activity to other forms of human life. To deny or to silence them under the pretext of specialization in research would be to falsify, by means of an unacceptable abstraction, the entire tableau of the past.” In the hands of a great historian like Bloch, the history of economic life opened out striking vistas onto the larger social and cultural structures of society. But, as Bloch would have been the first to admit, economic history as the study of forms of life also underwrote many banal or merely competent works as well, including most of those published in the *Journal of Economic History*. Indeed, economic history as the history of economic life tended to be relatively vague about causal relations: a thorough description of the situation faced by the entrepreneur, the industry, or the policy-maker and of the actions to which this situation gave rise was the goal of most *JEH* articles in this period.

The “New Economic History” or “Cliometrics” took aim precisely at the lack of explanatory rigor in earlier forms of economic history. The young scholars who formed this movement had been trained in economics departments that were increasingly stressing mathematical forms of theory, statistical methods, and econometrics. The overriding, one might say all-consuming, issue for the new economic historians was the explanation of economic growth, conceptualized quite specifically as a rise in national income per capita. This field had been pioneered in

the 1940s by Simon Kuznets, who developed detailed econometric methods of national income accounting, which he applied initially to the United States.\(^8\) The question of economic growth also came to the fore at this time as a result of post-war efforts to spur industrialization in the colonial and postcolonial countries of Asia, Africa, and Latin America.\(^9\) In the work of the new economic historians, economic growth rather than forms of economic life became the prime issue worthy of historical research—one is tempted to say the only issue. Consequently, rather than economic institutions and economic actors, the theoretical abstraction known as “the national economy” became the key unit of study.

The research that launched the new economic history explored the contribution of railroads to American economic development. Robert Fogel published his initial findings on this question in the *JEH* in 1962, and in 1964 he published *Railroads and American Economic Growth*.\(^10\) In the following year Albert Fishlow published his own book on the topic, which disputed some of Fogel’s key findings.\(^11\) In his book, Fogel, who signaled his positivist epistemological commitments by beginning the first chapter with an epigraph from Carl Hempel, argued that contrary to the claims of previous economic historians, railroads were by no means indispensable to the rapid economic development of the United States in the nineteenth century.\(^12\) He constructed an ingenious “counterfactual” estimate of how much economic growth would have been lost had no railroads been built. Canal expansion, he showed, would have made up much of the difference. All told, the advantage of railroads over canals and wagons would have amounted to less than 5% of the actual U.S. gross national product as of 1890.\(^13\) Fishlow, whose study measured indirect effects of railroads that Fogel had omitted (such as stimulation of other industries) concluded that railroads were more important than Fogel had claimed, but still put their contribution at only a little more than ten percent of GNP.\(^14\)

What was most significant, however, was not who had the right estimate, but the terms in which the debate was carried out. Methodologically and epistemologically, Fishlow and Fogel were in essential agreement. Indeed, they co-wrote a key article in the 1971 issue of the *JEH* that celebrated the new economic history.

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9. Kuznets himself contributed to this interest in development of what were then called the “under-developed” countries. See *Economic Growth: Brazil, India, and Japan*, ed. Simon Kuznets, Wilbert E. Moore, and Joseph E. Spengler (Durham, NC: Duke University Press, 1955).


12. The epigraph begins with this statement: “In history as anywhere else in empirical science, the explanation of a phenomenon consists in subsuming it under general empirical laws….” Fogel, *Railroads*, 1.


tory’s contributions. “The common thread running through the New Economic History” they said in this article, “is a commitment to the efficacy of theory in specifying useful counterfactuals, and to quantitative methods in implementing them.” The implementation of counterfactual propositions and their comparison to the actual facts required the generation of masses of new data. As Fishlow and Fogel made clear, “The data categories—national income, capital stock, etc.” are inventions of the modern economist, not the concerns of past government officials, and are therefore not readily available in printed sources. The categories of data being sought are defined by economic theory, by “economic models of production and income determination,” like those pioneered by Simon Kuznets. As Fishlow and Fogel put it, “because such data are not directly available, it is necessary to resort to the art of synthetic reconstruction.” This synthetic reconstruction “is in itself based upon application of theoretical relationships to limited information.” In other words, by mathematically manipulating the equations used in preparing contemporary national income accounts, cliometricians were able to generate estimates of key unstated quantities. Although such theoretically specified quantities (for example, productivity or net capital expenditure) cannot be measured directly, algebraic manipulation of the appropriate mathematical formulae reveal what sorts of existing data (for example, figures on prices, imports and exports, rents, insurance rates, or the quantities of different types of goods produced) can be plugged into the equations to generate valid estimates of the theoretically specified quantities. The procedures, in short, are based on multiple levels of mathematical abstraction. Neither the independent nor the dependent variables, nor, for that matter, the questions being asked about them, would exist without specification by economic and econometric theory and by the mathematical equations and the statistical estimation techniques of modern economics.

In their joint article, Fishlow and Fogel were quite explicit about how the new economic history was transforming the field.

Two principal factors have shaped the directions, and thereby the contributions, of the New Economic History. The first has been an unabashedly developmental interest. Cliometricians have been concerned with rates of growth rather than levels, with economic aggregates rather than persons, and with production rather than distribution. This has meant an inevitable shift of focus from earlier writing in which the description of the institutional structure was the central objective, and in which much attention was bestowed upon distributional problems reflected in the farmers’ discontent, the rise of unions, business concentration and government regulation. A parallel list of topics covered by the new economic historians would read agricultural productivity, investment in human capital, industrial supply and demand responses, and government promotion of social overhead investment.

This account makes it clear that the new economic history had a much more abstract epistemological object than the old economic history: not persons, institutions, and social movements, but growth rates, economic aggregates, productivity, and investment. This was, precisely, a move from economic history as the study

16. Ibid., 18.
of forms of economic life to economic history as the historical study of the determinants of national economic growth.

The new economic historians held the intellectual high ground in the debates of the 1960s and 1970s. They had more trenchant questions, more powerful methods, and highly original research strategies. Moreover, their willingness to amass and evaluate mountains of new empirical data made them veritable heroes of quantitative history—a movement of historical research that extended far beyond economic history in those years. It is not hard to see why they succeeded in taking over the field.

A look at articles published in the *JEH* shows that their victory within the American discipline of economic history was astonishingly swift. Table 1 traces the rise of the new economic history by counting the percentage of articles in the *JEH* making use of various characteristic methodological apparatuses: tables and graphs presenting data, theoretical diagrams (of the sort familiar from the supply and demand diagrams in elementary economics courses), and mathematical equations.

### Table 1:
Percent of Articles in the *Journal of Economic History*
containing Different Methodological Apparatuses

<table>
<thead>
<tr>
<th>Year</th>
<th>None</th>
<th>Statistical Tables</th>
<th>Statistical Graphs</th>
<th>Theoretical Diagrams</th>
<th>Mathematical Equations</th>
<th>No. of Articles</th>
</tr>
</thead>
<tbody>
<tr>
<td>1951–53</td>
<td>68%</td>
<td>32%</td>
<td>12%</td>
<td>0%</td>
<td>0%</td>
<td>25</td>
</tr>
<tr>
<td>1965–66</td>
<td>24%</td>
<td>72%</td>
<td>20%</td>
<td>4%</td>
<td>8%</td>
<td>25</td>
</tr>
<tr>
<td>1970–71</td>
<td>23%</td>
<td>69%</td>
<td>6%</td>
<td>4%</td>
<td>51%</td>
<td>35</td>
</tr>
<tr>
<td>1975–76</td>
<td>15%</td>
<td>76%</td>
<td>29%</td>
<td>15%</td>
<td>50%</td>
<td>34</td>
</tr>
<tr>
<td>2008</td>
<td>3%</td>
<td>90%</td>
<td>76%</td>
<td>14%</td>
<td>62%</td>
<td>29</td>
</tr>
</tbody>
</table>

Papers presented to the annual meeting, which were published in the journal in the 1950s, 1960s, and 1970s, were not included in these counts. Because these were shorter and more informal presentations, their inclusion would have reduced the incidence of methodological apparatus.

Let us follow convention and take the publication of Fogel’s and Fishlow’s books as marking the beginning point of the new economic history as a scholarly movement. Table 1 shows that already in the mid-1960s, when these books had just been released, research practices in the field had changed markedly since the early 1950s. Fully three-quarters of the articles in the *JEH* volumes of 1965 and 1966, as against only a third in the early 1950s, used some form of quantitative methodological apparatus, mostly statistical tables of some description. Thus, even before the new economic history movement got its name, economic historians had already begun to adopt more rigorous quantitative techniques. In 1970–1971, the percentage of articles using some quantitative apparatus was ac-
ually no higher than it had been a half-decade earlier. But on closer inspection the change over that half-decade was profound. In the mid-1960s only one article in ten contained a mathematical equation; by the early 1970s, fully half did. It is the use of mathematical notation that most clearly indicates acceptance of the scholarly norms of the new economic history. Any diligent historian could arrange her or his findings into a tabular form, but using equations indicates a working knowledge of modern, mathematically based economic theory that only rare working historians could have mustered. To include equations in an article was a badge of membership in the new intellectual regime.17

I read Table 1 as indicating that the new economic history was well entrenched in the journal and the association as early as the beginning of the 1970s. The figures for the mid-1970s show that this position was yet further strengthened. Although the proportion of articles using mathematics had remained constant, only one article in seven now lacked quantitative apparatus, and the proportions using both statistical graphs and theoretical diagrams rose. By 2008, which I include to indicate the shape of the field in the present, only one of the twenty-nine articles lacked quantitative apparatus altogether, sixty-two percent used mathematics, ninety percent used statistical tables, and three quarters—a big increase—used graphs.18 If we take this as the portrait of a field entirely won over to economic history as a branch of economics, we can see that most of the way had already been traveled as early as 1970 and 1971. It is therefore understandable that nearly all the distinguished economic historians trained after the early 1970s were educated and made their careers in economics departments. Economic history was by then well on its way to becoming the study of economic growth in the past by means of methods developed and employed in economics departments.

It is indisputable that the triumph of the new economic history increased the rigor and intellectual power of the field of economic history. Although this increase in power was based on mathematical abstraction from the concrete experience of economic life in the past, economic historians were by no means uninterested in collecting empirical data. Quite the contrary, they greatly expanded the collection of data, often exploiting new and unsuspected sources of quantifiable evidence with tremendous ingenuity and assiduity. But the questions they were interested in answering ceased to be primarily about forms of economic life. Although one might study the cotton textile industry or railroads, what was important to economic historians about such topics was not, for example, how work experiences or clothing fashions were changed by the advent of mechanized cotton production, or how the development of street railroads enabled new forms of urban life, but rather what effects technological change, rising capital investment, decreased turnover time, and labor productivity in the cotton or railroad industry had on the growth of GDP per capita.

17. One could, of course, be a card-carrying econometrician and not include an equation in one’s article. Neither Kuznets in his 1952 JEH article nor Fogel in his 1962 JEH article included equations, but this was probably so as not to intimidate their expected readers, which, in those years, would have included historians.
18. My guess is that the increase in the use of graphs is a consequence of the development of digital graphing software, which makes high-quality diagrams much easier to prepare.
II. THE RISE AND DECLINE OF THE NEW SOCIAL HISTORY

It is important to recognize that there was both gain and loss in the transition to the new economic history. But initially the gains were much more obvious than the losses. This was largely because history as a whole was being transformed in the 1960s and 1970s by the rise of what was called “the new social history” and by the application of quantitative techniques to many fields of historical study. The new economic history may have been unique in its extensive use of mathematics, but in these years it was very common for historians to borrow both theory and methods from a whole range of quantitative social sciences—especially sociology, political science, demography, and geography—in addition to economics. This tendency is nicely illustrated by the foundation of the Social Science History Association, which held its first annual meeting and began publishing its journal, *Social Science History*, in 1976. According to the editors’ foreword in the journal’s first issue, the SSHA was to bring together “historians seeking a more rigorous and consciously theoretical orientation” and “social scientists interested in longitudinal analysis.” The journal would publish “research that attempts generalizations of some breadth verified by systematic examination of the relevant evidence and supported by quantitative analysis when appropriate.” Although the SSHA carefully avoided language that might exclude historians using methods other than quantification or adhering to non-positivist epistemologies, this statement of purpose made clear the founders’ commitment to the Hempelian program we have already encountered among the new economic historians.

Within this vibrant and growing community of interdisciplinary historians and social scientists, the study of economic life actually figured quite prominently, even though economic historians had turned almost entirely to the question of economic growth. Most of this work on economic life was actually carried out by scholars who identified themselves as social historians. A major priority of the new social historians was to trace out the effects of economic change on society; studies of the social effects of industrialization were particularly prominent. But even social histories of pre-industrial societies—of peasants, lords, merchants, artisans, religious congregations, cities, regions, or professional groups—nearly always paid close attention to the economic forces and economic experiences that social historians typically saw as underlying the social structures, social institutions, or social movements they were studying. Indeed, in the 1960s and 1970s, social historians themselves carried out the sort of research that once would have been published in economic history journals—about guild structures, food prices, business practices, wage patterns, or technological innovations—often supplying far more extensive quantitative evidence on these issues than would have been typical in articles in the *JEH* in the 1940s or 1950s.

19. “Editor’s Foreword,” *Social Science History* 1 (1976), i-ii.
20. One of the leading figures in the new economic history, Robert Fogel, served on the journal’s editorial board.
One reason for this close attention to economic life was a widespread assumption that economic structures and changes determined other aspects of social life. One certainly didn’t have to be a Marxist in the 1960s and 1970s to be an economic determinist. Few social historians would have disagreed with the British Marxist historian Eric Hobsbawm in 1971 when he attempted to sketch a “common working model” for social historical research:

One starts with the material and historical environment, goes on to the forces and techniques of production (demography coming somewhere in between), the structure of the consequent economy—divisions of labor, exchange, accumulation, distribution of the surplus and so forth—and the social relations arising from these. These might be followed by the institutions and the image of society and its functioning which underlie them... The practice is thus to work outwards and upwards from the process of social production in its specific setting.\(^{21}\)

This statement aptly sketches the working assumptions not only of the British Marxist historians but also of French social historians of the Annales School or of American new social historians. As long as there was a consensus that one should work “outwards and upwards” from forces and relations of production to politics, thought, and social institutions, social historians could be counted on to engage in serious study of economic life.

But this interest in economic life faded out in the 1980s and 1990s as social history was increasingly displaced by what Lynn Hunt celebrated in 1989 as “the new cultural history.”\(^{22}\) Because I have written about this transition at some length elsewhere, I will be very brief here.\(^{23}\) Influenced especially by new work in cultural anthropology and literary studies, historians—including historians who had themselves been practitioners of the new social history—became increasingly skeptical of the economic determinism that underlay Hobsbawm’s working model. Rather than assuming that social or political movements somehow reflected economic changes or economic interests, they began to focus on the symbols, discourses, and rituals that specified the movements’ meaning. This new emphasis required methods very different from those championed by the new social history. Linguistic theories and interpretive methods replaced causal analysis and statistics; hermeneutics and postmodernism replaced positivism; Clifford Geertz and Michel Foucault replaced Karl Marx, Max Weber, and W. W. Rostow. Indeed, cultural historians increasingly dismissed assumptions about the primacy of economic forces out of hand as crude and naïve. Rather than industrialization, exploitation, class, and the pursuit of economic interests, the key issues became the relationship between knowledge and power and the cultural construction of difference, particularly differences in gender and race.

I was an enthusiastic participant in this cultural turn, which, in my opinion, gave rise to much groundbreaking and powerful historical research. I think that cultural historians were right to reject the default economic determinism of the

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new social history and its preference for supposedly “hard” quantitative data over “soft” or “impressionistic” evidence. But these positive changes nevertheless had negative effects on the historical study of economic life. Although cultural approaches to economic life could and did yield important insights, most cultural historians seemed to regard economic questions as tainted by their association with positivism and economic determinism. 24 Already marginalized in economic history, studies of economic life also dwindled in the output of historians after the mid-1980s. By the turn of the millennium, they had become rare.

One might, of course, argue that the history of economic life had been overvalued in the 1960s and 1970s and that the swing to cultural history was therefore a useful correction, even if the pendulum may have swung too far. But in my opinion, real historical developments in the years following 1973 made this otherwise understandable pendulum swing most inopportune. In precisely the period when social history was being displaced by cultural history, the world capitalist economy underwent a set of fundamental changes. The extended economic crisis of the 1970s brought an end to the long postwar boom, and the world economy that emerged from this crisis in the 1980s was very different from that of the postwar “golden years.” Overall growth rates fell sharply. The Bretton Woods fixed-exchange-rate currency regime collapsed in the early 1970s, and currencies began to float on the open market. Keynesianism, which had seemed incapable of dealing effectively with the “stagflation” of the 1970s, was replaced by monetarism and a revived laissez-faire. The wealthy nations experienced extensive deindustrialization as manufacturing moved increasingly to lower-wage countries. Labor movements declined nearly everywhere and income distributions became more unequal. The rise of new information technologies encouraged both greater dispersion of production and the development of free-wheeling, worldwide financial markets. The moderated capitalism of the postwar years, carefully managed by welfare-oriented nation-states, was replaced by an aggressive, footloose, market-driven, hyper-competitive, cost-cutting global capitalism that was beyond the control of any nation-state and that was loudly championed by a neoliberal economic ideology. In other words, at the very time when historians were losing interest in studies of economic life, transformations in the world capitalist economy should have made it clear that economic change was profoundly reshaping history in the present. 25 In retrospect, it seems astounding that these massive economic transformations had no discernible effects on the research programs of historians—that, indeed, historians were turning away from economic topics at the very moment when economic realities became particularly puzzling, dramatic, and consequential. 26

26. This declining interest in the history of economic life was by no means a purely American
A paucity of work on economic life is a serious weakness of the history profession in the present, although it seems likely that the collapse of world financial institutions in the autumn of 2008 and the consequent “Great Recession” may be sufficient to regain historians’ attention. But if historians do decide once again that economic life is passionately interesting, what issues are likely to be investigated? And what intellectual resources are currently available for such investigations?

It should go without saying that merely returning to the sort of work done thirty years ago by new social historians is neither promising nor likely. Current economic life and the politics that surrounds it suggest a different set of questions from those that inspired historians in the 1960s and 1970s. As organized labor declines and industrial jobs dwindle in the advanced capitalist countries under the joint force of automation and the migration of production to low-wage countries, studies of the industrial working class have lost much of their political appeal. The worldwide advance of industrialized agriculture has had much the same effect on peasant studies. Although oppression and grinding poverty still afflict producers of agricultural and industrial goods the world over, the apparent political weakness of farm and factory workers in the contemporary world—by comparison with the much more insurrectionary period stretching from 1917 to the 1970s—seems to have limited historians’ interest in the experiences of the exploited, which was one of the most important topics of the new social history. Although I regret the waning of historians’ interests in the struggles of the exploited, I can understand why current economic and political trends suggest a very different portfolio of topics.

Meanwhile, other social-science disciplines have been much more responsive to the challenges posed by recent economic transformations. The most obvious case is the burgeoning interdisciplinary field of economic sociology or socioeconomics, which has developed mainly in departments of sociology and political science. This work tends to be highly critical of the neoliberal economic assumptions that have become dominant both in politics and in the economics profession since the 1970s. The economic sociologists insist on what, borrowing from Karl Polanyi, they call the “socially embedded” character of firms and markets. Socioeconomics strives to demonstrate that the working of actual markets and firms cannot be explained by the purely self-interested actions of rational economic actors of the sort assumed by neoclassical economics. Rather, it shows that firms and the markets in which they participate are actually governed as much by social ties, cultural assumptions, and political processes—what this literature typically calls “institutions”—as by purely “rational” economic action. Although much of
the work in this vein amounts to a form of social, political, and cultural history of modern economic life, it has been written by sociologists and political scientists rather than by historians. It is therefore hardly surprising that most economic sociology focuses on the relatively recent past, particularly the period since World War II; it is, however, admirably international and comparative in scope.

It is curious that historians have been essentially unaware of this vigorous intellectual movement; one would expect historians to be highly sympathetic to work on the social and cultural history of the economic institutions—such as corporations, markets, and regulatory agencies—that so dominate modern life. Indeed, historians might profitably extend the range of such studies back into the centuries prior to the twentieth.

A second source of new thinking about the history of economic life comes, ironically, from economic historians. In the years since the triumph of the new economic history the field has made something of a turn back toward the broader study of economic life, but in new ways backed by economic theory and mathematical reasoning. Articles published in the economic history journals are still written almost exclusively by economists, and most of them still assess the contribution of some historical development to economic growth. However, the range of developments regarded as capable of having a major effect on economic growth has grown considerably since the triumph of the new economic history


30. The ninety-seven authors of the forty-six articles published in the JEH in 2008 and 2009 include only one who taught in a history department, along with two in political science, two in statistics, and one each in law, sociology, international studies, and oriental studies. All the rest taught in economics departments (fifty), business schools (thirteen), departments of economic history (twelve, all Europeans), or were economists employed in the research departments of banks (seven) or academic research institutes (six).
in the 1970s. In the ensuing years, important economic theorists and econometricians, including a number of Nobel laureates, have, for example, explored the role of human capital formation, of nutrition and health, and of a variety of institutions in fostering economic growth.\(^3\)

The influence of the economics of institutions is a particularly telling case. Oliver Williamson and Douglass North, who were major figures in the development of contemporary institutional economics, were both concerned—one might even say obsessed—to work out how institutions such as private property rights, capital markets, contract law, insurance, or the corporate form of enterprise increased economic efficiency by lowering transaction costs. But the analytical framework they developed could also be applied to explain why institutions failed to develop efficient forms—otherwise, how could one explain the differences between efficiency-enhancing and efficiency-blocking institutions? The analytical apparatus of institutional economics therefore gave economic historians certified theoretical and methodological tools that they could use to study the history of institutions of all kinds—and they have proceeded to do so.\(^3\) Meanwhile Gary Becker’s theory of family consumption and labor-allocation decisions was not focused on the contribution of family organization to rates of economic growth; his goal was to expand the economist’s approach to decision making to human organizations of all sorts.\(^3\) At the same time, new means of gauging the nutritional status of past populations gave economists surprising insights into the history of human well-being.\(^4\) And economic theories about human capital formation made it possible for innovative studies of the history of education, and for clever attempts to estimate premodern numeracy, to count as legitimate pieces of economic research.\(^5\)


\(^4\) For a fascinating application of Becker’s household allocation equations, see Joel Mokyr, “Why ‘More Work for Mother?’ Knowledge and Household Behavior, 1870–1945,” *JEH* 60 (1990), 1-41.

The consequence is that economic history journals now offer a wide range of insights into, and methods for studying, the history of economic life more broadly. To fully absorb what the economic historians have to offer will require historians to brush up on their algebra and statistics. But on returning to economic historical journals after having ceased to read them some three decades ago, I have found the recent yield encouragingly rich.

There are, then, new tools on offer from adjacent disciplines. At the same time, recent trends in the world economy have also brought into focus a new set of topics about economic life, many of which can be attacked using tools that historians already know how to use. I will briefly discuss what seem to me likely candidates for historical work in the present and near future, indicating a few examples of studies already accomplished, and signaling, where possible, important work by scholars outside the history profession that might serve as a spur or inspiration for historians.

**Consumption**

In the last few decades modern consumption regimes have spread all over the world and consumption has become the chief driver of U.S. economic performance. The issue of consumption is actually one of the few economic topics that has been adequately studied by historians over the past few decades. In part this is because consumption has long been a flagrantly obvious feature of modern life, at least in America. Consumption has been studied more by historians than other flagrantly obvious facts of economic life (for instance, the dominance of contemporary societies by business corporations or finance) because consumption is a profoundly symbolic and expressive form of behavior that lends itself easily to the research agendas of cultural history. Some of the best recent work attempts not only to read consumption as a text or to indicate its political valences but also to show how changing consumption patterns reacted back onto production and onto the larger structures of the economy. Maxine Berg demonstrates that the flow of Asian commodities into early modern Britain, especially Indian calicoes and Chinese and Japanese porcelains, not only was instrumental in the burgeoning of new forms of consumption, but stimulated British entrepreneurs to produce domestically manufactured substitute goods, including Manchester calicoes and Staffordshire pottery, which eventually dwarfed the original Asian items in economic significance. Jan de Vries has recently published a book that creatively combines the “new home economics” of Gary Becker with studies of the changing culture of consumption and with quantitative studies of personal possessions (based on inventories after death) in early modern Britain, France, Holland, and America. He argues that consumption was a central component of what he calls an “industrious revolution” in the long eighteenth century—a new ordering of the family economy in which both rural and urban labor (especially that of women

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172-201; Brian A’Hearn, Jörg Baten, and Dorothee Crayen, “Quantifying Quantitative Literacy: Age Heaping and the History of Human Capital,” *JEH* 69 (2009), 783-808.

and children) was applied more intensively to the production of goods for sale in the market in order to maximize income for the purchase of other market-supplied goods, such as textiles, crockery, furniture, clocks, and tableware. He argues that this largely voluntary increase of labor intensity was a crucial factor in stimulating economic growth in the eighteenth and early nineteenth centuries. The de Vries book is particularly notable for using the tools of modern professional economic history—with its equations, diagrams, and statistical tables—in such a way that they illuminate changing forms of economic life.  

Globalization

The prominence of talk about globalization in the contemporary public sphere has certainly gained historians’ attention. The topic of globalization has stimulated a wide range of new research—including work like Berg’s on European fascination with consumer goods imported from other continents. There is also much current work on migration from the global south to both Europe and the United States, although most of this work focuses more on problems of cultural identities and political responses rather than on the economic aspects of migration. Awareness of globalization in the present surely also explains the massive contemporary development of studies of all facets of European and American colonialism. Again, the cultural and political effects of colonialism, both in the colonies and in the metropoles, tend to dominate this research. But some recent studies explicitly interpret such political and cultural effects through the lens of the worldwide expansion of capitalism that was part and parcel of colonial domination. Rising awareness of contemporary globalization is surely responsible for the prominence of historical research on Asian and Indian Ocean economic history. The pioneering work of historical sociologists on the history of global economic dynamics should also be noted. Although much remains to be done on the history of economic globalization, this is an area in which some high-quality exemplars are already in place.

The Environment

Environmental history arose out of the dawning environmental movement of the late 1960s and early 1970s; the journal Environmental History dates from 1976,  


38. Two recent books that do this for the Indian case are Manu Goswami, Producing India: From Political Economy to National Space (Chicago: University of Chicago Press, 2004), and Andrew Sartori, Bengal in Global Concept History: Culturalism in the Age of Capital (Chicago: University of Chicago Press, 2008).


the year that Social Science History was founded.41 Given the continued risks posed by modern economic growth to the world’s natural environment, the field continues to have a strong political and moral relevance. As J. R. McNeill observes in his excellent review article on environmental history, most American work on this topic deals with the politics of conservationism or changing attitudes toward the environment, but the work he categorizes as “material environmental history” normally has an important economic component.42 Work like that of William Cronon, Donald Worster, and Joel Tarr focuses powerfully on how the pursuit of profit and public efforts to regulate or remediate its deleterious environmental effects have transformed rural and urban landscapes and economies.43 Once again, there is much work still to be done, but there are some excellent models.

If good historical research into economic facets of consumption, globalization, and the environment is already under way, historians have made only rare efforts to understand two other problems that contemporary economic life seems clearly to pose: debt and credit relations, and the macro-dynamics of capitalism. Happily, if historians do take up these questions, they will be able to learn from very good work by scholars in other fields.

**Debt, Credit, and Finance**

In 2010, in the wake of the second-worst financial crisis in the history of world capitalism (exceeded only by the Great Depression of the 1930s), the history of credit and debt is hard to ignore. To be sure, Niall Ferguson has recently published a popular survey of the role of finance in world history,44 and the history of finance is a longstanding topic in economic history.45 Some of this work is highly technical and intended only for an audience of other economists, but I am impressed by the conscientious efforts made by some historians of finance to make their findings accessible to non-specialists.46 Beyond the undoubted importance of money, banking, and financial institutions in the history of the economy and the state, there is also the question of how debt and credit relations have affected the daily life and

41. Called the Environmental Review at its founding, it became the Environmental History Review from 1990 to 1995 before assuming its present title.
culture of capitalist society. My experience as a teacher of graduate students at the University of Chicago tells me that this question is beginning to find its historians. Over the last few years I have worked with three students writing dissertations on the cultural history of debt–credit relations: on the changing social and moral meanings of bankruptcy in nineteenth-century France; on popular and intellectual ideas about finance in Germany and Britain in the 1920s and 1930s; and on how people made sense of the increasing commodification of risk in nineteenth- and early twentieth-century America.47 Historical sociologists have also made important contributions to the study of credit and finance, although their work tends to focus on economic institutions per se rather than on the cultural effects of changing modes of finance. Neil Fligstein’s study of changing paradigms of corporate management in the twentieth-century United States demonstrates that financial ideas and tools had already come to dominate the internal governance of firms by the mid-1960s; Greta Krippner has shown that financial activity came to account for the lion’s share of all corporate profits in the U.S. economy over the past three decades; and Giovanni Arrighi has argued that financialization analogous to what Fligstein and Krippner document has in fact recurred periodically over the long course of the history of world capitalism.48

The Macro-Dynamics of Capitalism

The recent financialization of the American economy since the late 1970s is but one aspect of a thoroughgoing, neoliberal transformation of the world capitalist economy over the past three decades—one I have sketched out briefly above. The neoliberal turn reversed the tendencies of a long period of increasing state involvement in the management of the economy, one that reached back as far as World War I and that was consolidated in the advanced industrial economies after World War II with the development of Keynesian welfare states. Hobsbawm has labeled this period of managed capitalism stretching from the late 1940s to the early 1970s a “Golden Age” because it witnessed the most rapid sustained economic growth in the history of world capitalism and because it ushered ordinary European, North American, and Japanese citizens into an era of high wages, relative material equality, economic security, high mass consumption, and seemingly endless material progress.49 In retrospect, it seems clear that a form of world capitalism that had brought great prosperity for a quarter of a century went into a


terminal crisis in the 1970s, a crisis marked by stagflation, deindustrialization, and general uncertainty about the appropriate direction of economic policy.

The capitalism that replaced this state-managed form in the 1980s and 1990s worked by very different rules. Industries were increasingly “deregulated,” which meant that the fates of all firms were increasingly determined by markets, especially equity and bond markets; states privatized the industrial and financial enterprises they had previously run; financial markets moved increasingly “off-shore,” thereby largely escaping state controls; firms internationalized, taking advantage of advancing information and communications technologies to distribute the various stages of production to different areas of the globe; and states not only found it ever more difficult to regulate far-flung corporate enterprises, but were themselves increasingly subjected to market forces by currency speculators. The Keynesian welfare state was increasingly replaced by what Bob Jessup has called a Schumpeterian workfare state—one characterized by tax cuts for the wealthy, an attack on labor unions, and an ideology that promotes private enterprise and entrepreneurialism.50 In the 1980s and 1990s, the ideology and policies of neoliberalism were spread to the rest of the world by the International Monetary Fund and the World Bank, which became what Joseph Stiglitz has called “market fundamentalist” institutions. They used their financial leverage to force indebted countries in the global south to dismantle developmentalist states and open themselves up to international capital; they also subjected post-communist countries of eastern Europe to what was called “shock therapy”—the painful immediate imposition of “free” markets rather than a managed transition from socialist to capitalist economic forms.51

This dramatic and difficult transition from one framework of world capitalism to another poses important issues for historical reflection and research. The new economic sociology has, to be sure, responded to one aspect of this transition by challenging the revived claims of neoclassical economics with its counter-claim of “social embeddedness.” But work on social embeddedness fails to address the dynamics of the overall world capitalist system. Economic sociology’s conceptual apparatus is organized around comparing responses to economic stimuli that, according to neoclassical theory, should lead to strongly converging behavior on the part of different firms, markets, or industries, but that often fails to do so because these firms, markets, and industries are embedded in different social settings. But a comparative method of this sort cannot easily grasp changes in the world capitalist system as a whole. To put it provocatively, if markets and firms are embedded in state and society, society and the state themselves appear to be embedded in the macro-structures and the unpredictable rhythms of world capitalism. This implies that an adequate historical sociology—or history—of economic life must also find some way of making sense of the historical dynamics of world capitalism itself.

This way of putting the issue rests on assumptions that are certainly not shared by all who work on the history of economic life. Most basically, it assumes that

world capitalism can be spoken of as a specific historical object. Economic historians generally do not use the term “capitalism,” or they use it as a synonym for an economic system based on free-market exchange. They certainly do not see capitalism as a specific historical formation with any particular dynamic—beyond, at best, a tendency to produce periodic business cycles. Analytically, they treat the kinds of developments that produce welfare-Keynesianism and then neoliberalism as exogenous to their object of study—what they regard as the economy proper.

The notion of capitalism as an evolving totality with its own dynamics arises above all from the Marxist tradition. Marx, writing relatively early in the capitalist era, was interested principally in what he saw as a dynamic that would lead, via increasingly violent economic crises, to the inevitable overthrow of capitalism by a proletarian revolution. Later Marxists, such as Rudolph Hilferding or Vladimir Lenin, distinguished new phases within the capitalist era—finance capitalism or imperialism—subsequent to the liberal phase that Marx had known. More recent Marxists, less convinced of the imminent transcendence of capitalism by socialism than Marx or Lenin, but also less convinced than the intervening generation of critical theorists like Adorno, Horkheimer, and Marcuse that capitalism is permanently frozen in a bureaucratic-technological form, have come to see capitalism as a self-transforming system characterized by a whole succession of macro-institutional forms. World-system theorists, beginning with Immanuel Wallerstein, have proposed long cycles or long centuries of capitalist history punctuated by a succession of international political-economic hegemonies over the capitalist world economy—from Spanish, to Dutch, to British, to American, to, perhaps, in the not-too-distant future, Chinese. They regard great power rivalries, warfare, and political upheavals as part and parcel of the development of the capitalist world-system, both as causes and as effects.52 Others—those in the “French regulation school,” as well as David Harvey, Bob Jessop, Fredric Jameson, and Robert Brenner—have limited their compass more strictly to the transition from the state-centered political economy of the “Golden Age” to the neoliberal political economy of the past thirty years.53


Working on the macro-dynamics of capitalism, of course, poses a number of difficult challenges for historians. Much of the work that has been done thus far treats only the period since World War II, which might make this topic seem irrelevant to the large majority of historians who work on earlier periods. Yet the political-economic transition of recent decades also poses much longer-term issues—about the historical genesis and transformation of different forms of capitalist governance, about the relations between world politics and world economic power, about the relationship between economic doctrines and the changing shape of the capitalist economy, about the specific historical temporalities of capitalism as a system, about the origins and evolution of world capitalism as an epistemological object capable of having a history. Both Immanuel Wallerstein and Giovanni Arrighi—by training a sociologist and an economist—have posed historical questions about the evolution of capitalism that reach back to the sixteenth century and earlier. It is paradoxical that the wrenching changes of the past thirty or forty years have largely failed to generate research by historians on this range of problems.54

To undertake historical work on the macro-dynamics of capitalism would, of course, require us to abandon the current taboo in historical thought on arguments that might be construed as economic determinism. As I have already indicated, I regarded and still regard as a positive step the rejection of the pervasive background assumption of economic determinism that was once so widespread in social history. But rather than asking precisely what sort of determinations, constraints, or opportunities economic developments, configurations, or interests might have placed on historical actors in different circumstances, cultural historians have effectively made the citing of any economic causalities taboo—that is, shameful and unthinkable. Recent history should have demonstrated that this attitude to economic determinations and constraints is counter-productive, that transformations of economic life are indeed having powerful, in some sense determinative, effects on our own contemporary history, and hence that analogous transformations might well have had such effects in the past as well. I am convinced that we historians can construct a more powerful and meaningful history by re-embracing the study of economic life. Among other things, we should use history’s analytical tools—including notably those of cultural history—to investigate the constraints, the compulsions, the enables, and the long-term dynamics that have contributed to the shaping of capitalist societies for the past four centuries.

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54. See note 51.