

# **Beyond the Old and the New: Economic History in the United States<sup>1</sup>**

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There have never been separate departments of economic history in the United States. Instead, economic historians have always been divided in varying proportions among economics and history departments, with the occasional appointment in sociology or political science. This lack of an independent disciplinary home has had both positive and negative consequences for the field. On the minus side, practitioners have invariably found themselves in the position of step-children in their parent disciplines, often losing out in the competition for attention and resources to their mainstream brethren. On the plus side, economic historians have never been isolated from intellectual developments in their home disciplines. As a result, the field has constantly been reinvigorated by borrowings from economics, history, and the other disciplines within which economic historians reside.

This reinvigoration has worked best when ideas stimulated by developments in one discipline were tested in a broader interdisciplinary context. After the so-called Cliometric Revolution of the 1960s, however, economists came to dominate the practice of economic history in the United States. Historians largely abandoned the field and interdisciplinary conversation for the most part came to a halt. This essay tells the story of that transformation and its effect on the practice of economic history. In recent years there has been a resurgence of interest in the field among historians, and the essay ends with a discussion of the potential that this development brings for renewed cross-disciplinary fertilization.

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<sup>1</sup> This essay draws on but substantially revises and updates Lamoreaux (1998). I have benefited from comments from the editors Francesco Boldizonni and Pat Hudson, as well as from Timothy Guinnane, Paul Rhode, Ariel Ron, and Francesca Trivellato.

## **Early (Inter) Disciplinary Foundations**

Economic history had its formal beginnings in the United States in the late nineteenth century at the very moment when the structure of the country's newly emerging universities was solidifying around formal academic departments (Rudolph 1965:399-402; Veysey 1965: 320-24). Harvard's economics faculty awarded the nation's first chair in economic history to British scholar William J. Ashley in 1892, just a few years before its members formally constituted themselves a department (Mason and Lamont 1982: 396-99 Cole 1968: 558-60). Other chairs in the field were established around the same time by economics departments preoccupied with forming their professional identities and distinguishing what they did from other units of the university (Mitch 2011: 240-41). An important part of the process of department building was the differentiation of the various social sciences from history, the main subject around which teaching in old-style colleges had been organized, and so the new social sciences staked out claims to historical study by their practitioners, even though historians elsewhere might be engaged in very similar research (Ross 1991; Mitch 2011; Adcock 2003).

In economics, the role of history in the process of identity formation was more fraught than in the other social sciences because the dominance of classical economics was then under assault by scholars like Richard T. Ely who had been trained in the German school of historical economics. The latter deplored the classical theorists' deductive methods, arguing that economists could only generate useful knowledge if they proceeded inductively and inferred the laws and principles of economic life through careful study of the past.<sup>2</sup> The spread to the United States of marginalist ideas beginning in the late 1880s eased the strain between the old guard and their historicist challengers and paved the way for a productive division of labor between

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<sup>2</sup> See the essays by Pat Hudson and Jan-Otmar Hesse in this volume.

economic historians and economists more generally (Mason and Lamont 1982). Uniquely equipped to probe the stages through which economies had evolved over the centuries, economic historians were “expected to play an important part in that reconstruction of economic science which was then going on” (Callender 1913: 80). As Ashley announced upon his arrival at Harvard, economists now willingly acknowledged that “economic conclusions are *relative* to given conditions,” that those conditions change over time, and that understanding such changes requires consideration of a range of factors besides purely economic ones (Ashley 1893:118).

Although the first chairs in economic history were all in economics programs, research in the subject was simultaneously expanding within history departments. Like economics, the field of history was undergoing professionalization during the late nineteenth century. Many of the new breed of academic historians were trained in Germany, where they absorbed research techniques similar to those advocated by historical economists (Higham 1965). Comparing historical articles published in economics journals such as the *Journal of Political Economy* (*JPE*) or the *Quarterly Journal of Economics* (*QJE*) with those appearing in the *American Historical Review* (*AHR*) or other historical journals in the early twentieth century, one is struck by how little there was to differentiate the work methodologically. Searches of JSTOR show, moreover, that authors who published historical articles in economics journals often simultaneously disseminated their research in history and other social-science publications, as well as by writing books. Even the assumptions about human behavior that historians made were generally the same as those made by economists. For example, the so-called “Progressive” historians who dominated the study of U.S. history analyzed the economic underpinnings of major historical developments in ways that economists found appealing. One of the most famous products of this school, Charles A. Beard’s 1913 book arguing that the Constitution was

shaped by the economic interests of the founding fathers, received favorable reviews in leading economics journals (see Levermore 1914; Wright 1914).

Perhaps because so little writing in economic history was geared toward generating broad theoretical insights, the field's relationship to the larger discipline of economics soon became strained. Guy Callender complained in 1913 that economists had lost interest in history to such an extent that "topics in economic history found no place upon their programme" (Callender 1913: 81). Although the *QJE* and especially the *JPE* continued to publish historical pieces, articles on economic history were strikingly absent from the American Economic Association's new flagship journal, the *American Economic Review (AER)*, which began publication in 1911. There were also few presentations on economic history at the association's annual meetings until economic historians began to assert themselves in 1926 by organizing what became a regular round table on the field.

The years following World War I were a time of institution building in economic history. Most of the new organizations founded during the 1920s aimed to bolster the relationship between economic history and economics. In 1920 Ashley's successor at Harvard, Edwin Gay, joined with Wesley Mitchell, a historically minded economist at Columbia, to found the National Bureau of Economic Research (NBER) on the principle that research in economic history, particularly the careful collection of long-term quantitative data sets, provided a vital foundation for economic policy making. Other new organizations created at this time, such as the Commission on Recent Economic Changes, the Commission on Recent Social Trends, and the Social Science Research Council, had similar motivations (Heaton 1952; Heaton 1965: 467-69; Cole 1968: 573-83s).

Writing in 1931, British economic historian J. H. Clapham faulted earlier scholarship in the field for its tendency to generalize on the basis of scanty data or even worse the statements of self-interested participants. These criticisms fell on fertile ground in the United States, where economic historians in economics departments increasingly called upon their colleagues to make more systematic use of quantitative methods (see, for example, Usher 1932). As one economist later put it, “the outstanding characteristic” of economic history writing in the 1930s was scholars’ attention to “such questions as How much? How many? How quickly? Or How representative?” (Heaton 1942). The result was the birth during the interwar period of a new style of economic history that intentionally put research in the field in service of the economics profession, or, as Chester Wright (1938) put it, in service of raising the standard of living. Using historical data, for example, Elizabeth Gilboy (1934) derived supply and demand curves for commodities such as tea and coffee, and scholars such as Mitchell (1927), Edwin Wilson (1934), and Clarence Long (1939) analyzed the structure of the business cycle.

In their zeal for quantification, some economists were already challenging the conventional wisdom of historians in ways that foreshadowed the later Cliometric Revolution. For example, C. M. Thompson (1927) criticized the idea that the antebellum South was overspecialized in cotton, anticipating findings by Albert Fishlow (1964) and Ralph Anderson and Robert Gallman (1977) by reporting that the southern United States produced more than enough food for the region’s sustenance and that large cotton plantations were also large producers of foodstuffs. Similarly, Carter Goodrich and Sol Davison (1935 and 1936) contested Frederick Jackson Turner’s famous thesis that the frontier functioned as a “safety valve” for urban workers by presenting evidence on the high costs of migration and on the small number of industrial laborers who actually moved west.

Economic historians in economics departments also began to call for more use of economic theory in historical research. According to Gay, for example, German historical economics had failed to generate a body of inductive theory that could replace the deductive insights of mainstream theorists, but in response to its challenge “economics ha[d] increased the range and depth of its contemporary observation; its use of the deductive method ha[d] become more guarded, its analysis more subtle....” In Gay’s opinion, these changes had made possible a new cooperative relationship between economic history and economics, characterized by “more community of training, interest and awareness of interdependence” between the two bodies of scholarship. For the relationship to work, however, Gay argued that economic history had to be informed by, as well as to inform, economic theory (Gay 1941: 14-15).

Although economic historians trained in economics departments were increasingly oriented toward their parent discipline by the 1930s, they maintained ties with practitioners in history and other disciplines. And more than ties: most contemporary writing by economists on historical topics continued to be qualitative and descriptive and to differ little in terms of sources and methods from writing on similar topics by historians.<sup>3</sup> When American economic historians determined to organize their own professional society in 1941, therefore, it was an interdisciplinary effort. A group of historians had proposed to form an Industrial History Society in 1939, but that organization never came to fruition. Instead, a small coterie of economists led by Anne Bezanson, Arthur Cole, Earl Hamilton, and Herbert Heaton swept the historians into a new Economic History Association (EHA), ratified at joint meetings with both the AHA and the AEA in 1940. At the outset, the EHA had 361 members and supported its own scholarly publication, the *Journal of Economic History* (*JEH*) (Heaton 1941: 107-9; Heaton 1965: 470-72).

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<sup>3</sup> There were substantial differences across economics journals, however. 60 percent of the 38 regular articles on historical topics published in the *JPE* during the 1930s did not include any tables or figures, whereas almost all of the 17 articles in the *QJE* did. The *AER* still published very little economic history.

The formation of the EHA was accompanied by a burst of related organization-building led by many of the same individuals. An important result was the creation of the Committee for Research in Economic History (CREH) in the winter of 1940-41 with support from the Rockefeller Foundation (Sass 1986: 54-59; Cole 1970). The Committee sponsored a number of important studies and also became the locus of planning for a new Research Center in Entrepreneurial History, which was founded, again with Rockefeller seed money, at Harvard in 1948. Although the scholars who organized the EHA and the CREH were mainly economists, both organizations attracted an interdisciplinary mix of participants. The early issues of the *JEH* published an eclectic range of scholarship. For example, Volume 5's regular issues included one article by an economist, five by historians, one by a sociologist, and one by a geographer. The same volume's "Tasks" issue (papers presented at the EHA's annual meeting) contained two papers by economists, two by historians, and two by sociologists (including a paper by C. Wright Mills on "The American Business Elite"). One of the CREH's major accomplishments was to fund a series of studies of the role of government in the American economy before the Civil War. Commissioned in the wake of the New Deal, these studies aimed to show that government had played an active role in the U.S. economy through much of its history.<sup>4</sup> This project produced classic works by historians Oscar Handlin and Mary Flug Handlin (1947) and political scientist Louis Hartz (1948), among others.

The group that assembled at the Research Center in Entrepreneurial History was even more interdisciplinary. The starting point for their thinking was Joseph Schumpeter's concept of entrepreneurship as a creative act that in discontinuous fashion shifted outward the economy's production possibility frontier (Schumpeter 1934). Entrepreneurship was important to study, they believed, because this kind of creativity was the key to greater economic well-being.

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<sup>4</sup> See the Preface to the 1969 Revised Edition of Handlin and Handlin 1947. See also Cole 1970: 729.

Schumpeter himself was unable to explain why some societies at some times produced disproportionate numbers of entrepreneurs, and neoclassical price theory also appeared to lack answers to such questions. After an active search for a usable theory (traceable through the pages of the center's in-house organ, "Explorations in Entrepreneurial History"), they turned to Parsonian sociology. Some of the most important historians with the center, such as David Landes, Thomas Cochran, and Alfred D. Chandler, Jr., consistently employed concepts and addressed debates at the heart of this sociological literature, even when they did not make extensive use of its rather arcane vocabulary and categories of analysis (Sass 1986: 107-223; Galambos 1969).

As the star-studded list of scholars associated with these ventures suggests, the interdisciplinary collaboration sparked during this period of organization-building was extraordinarily fruitful. But it would not last. The Rockefeller Foundation moved on to fund other activities, and the CREH and its spinoff Research Center in Entrepreneurial History faded out of existence when their initial grants expired (Cole 1970; Sass 1986: 243-49). For several decades the EHA continued to attract members from history as well as economics and to publish work by scholars trained in both disciplines (and occasionally other fields as well), but the Cliometric Revolution of the 1960s would disrupt the interdisciplinary mix, and economists would increasingly dominate both the association and its journal.

### **The Cliometric Revolution**

Spurred by the Great Depression, the Second World War, and the Cold War that followed, the discipline of economics underwent major changes over the middle third of the twentieth century, including the invention of national income accounting, the development of

econometrics, and the spread of new computer technology (Morgan 2003). Economists were also increasingly preoccupied during this period with understanding how newly independent nations might develop vibrant free-market economies (Easterly 2001), and as a result interest in the history of countries that had successfully negotiated the transition to sustained economic growth was on the rise. The Cliometric Revolution would emerge out of a combination of this renewed interest in history and the energetic iconoclasm of proponents of the new quantitative methods.

The basic elements of the revolution were already apparent in 1960 at a conference organized by the International Economic Association around W. W. Rostow's book, *The Stages of Economic Growth* (1960). At stake was Rostow's theory that the U.S. and Western European economies had "taken off" by means of large-scale investments in leading sectors like the railroad. The most biting critiques were offered by two historically minded American economists who between them trained most of the leaders of the Cliometric Revolution: Simon Kuznets (1963), a development economist famous for his pioneering work in national income accounting,<sup>5</sup> and Alexander Gerschenkron (1963), a specialist in Soviet planning who was developing his own, more historically contingent theory of economic growth.<sup>6</sup> Although there was undoubtedly a political subtext to the debate (Rostow subtitled his book *A Non-Communist Manifesto*), the critique was fundamentally methodological. Rostow had provided only the most casual theoretical and empirical support for his stage theory of economic development, and his critics bore in on both weaknesses. Another attendee at the conference, Douglass North, himself

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<sup>5</sup> See "Simon Kuznets, The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel, 1971," [http://www.nobelprize.org/nobel\\_prizes/economic-sciences/laureates/1971/kuznets-facts.html](http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1971/kuznets-facts.html), accessed 15 December 2014.

<sup>6</sup> See Albert Fishlow's retrospective review of Gerschenkron's *Economic Backwardness in Historical Perspective*," [http://eh.net/book\\_reviews/economic-backwardness-in-historical-perspective-a-book-of-essays/](http://eh.net/book_reviews/economic-backwardness-in-historical-perspective-a-book-of-essays/), accessed 15 December 2014.

one of the leaders of the Cliometric Revolution, put forward an alternative, export-led theory of economic development (North 1961 and 1963a). When North published his book the next year, it was subjected to similar critical scrutiny (see, for example, Fishlow 1964). Cliometrics' negative and reactive essence was on display in both episodes: practitioners used basic economic theory to subject scholarly works to quantitative tests of consistency with the evidence and generally found them wanting, regardless of their interpretative slant.<sup>7</sup>

During the post-Sputnik expansion of higher education, funds were suddenly available to support initiatives in the quantitative social sciences. The same year as the Rostow conference a group of entrepreneurial young economists at Purdue, including Lance Davis and Jonathan R. T. Hughes, secured backing for a series of conferences to promote this "new economic history" (Davis 1990: 9-10). The Purdue meetings became famous for their feisty criticism and camaraderie, and they helped to form participants into a cohesive group. According to Hughes, "the intellectual atmosphere was intense" but also fun (Hughes 1985: 3; Hughes 1991: 24). Robert Fogel, a Kuznets student who would later share the Nobel Prize in economics with North, was still writing his dissertation when he attended the 1960 meeting and got caught up in the "tremendous excitement and exhilaration" (Fogel 1990: 6). Robert Gallman went to the first Purdue meeting thinking of himself "as a development economist of a Kuznetsian variety." The conference converted him to economic history and transformed his career (Gallman 1992: 5-6).

Although the cliometricians made their mark by challenging existing scholarship, the older generations' response was initially positive. John Meyer recalled the reaction to his and Alfred Conrad's controversial paper (1958) critiquing the traditional view that slavery was unprofitable as "quite open-minded" (Meyer 1995: 4). Gallman agreed, noting that there was no

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<sup>7</sup> Most of the first generation of cliometricians were trained by Kuznets, Gerschenkron, and North (Williamson 1994: 115).

general division between “cliometricians and traditionalists,” and that several of the latter gave “thoughtful and friendly reviews” of cliometric papers (Gallman 1992: 4). Indeed, some senior economic historians saw the work as an extension of their own commitment to quantification, “welcomed the new departures with open arms, and minds,” and rewarded the young cliometricians with leadership positions (Hughes 1985: 2). Cole offered the directorship of the Research Center in Entrepreneurial History to North in 1954 and three years later chose Meyer as acting editor of *Explorations in Entrepreneurial History* (the journal that grew out of the Center’s in-house publication). Frederic Lane put North on the council that replaced the CREH in 1959, and the board of trustees of the EHA chose North and William Parker to be coeditors of the *JEH* in 1960 (Cole 1970; Sass 1986: 245; Williamson 1994: 116).

Over time, however, the reaction turned more negative. In part the problem was the increasingly pointed attacks that cliometricians mounted against earlier work. The “young turks,” as Claudia Goldin (1995) called them, built interest in their achievements to a large extent by denigrating their predecessors. In a 1963 communication published in the *American Economic Review*, for example, North proclaimed that “a revolution is taking place in economic history in the United States” and went on to justify in sweeping terms the overthrow of the old regime. “Even a cursory examination of accepted ‘truths’ of U.S. economic history suggests,” he asserted, “that many of them are inconsistent with elementary economic analysis and have never been subjected to—and would not survive—testing with statistical data” (North 1963b: 128-29). Two years later he expounded on the “deficiencies of economic history” as previously practiced. “Many writings in economic history are loaded with statements which have economic implications and imply causal relationships which ... run counter to basic economic propositions” supported by only “a mishmash of quotations and oddly assorted statistics” (North 1965: 87).

There was, in fact, considerable “low hanging fruit” in the form of untested or inadequately documented quantitative statements in the literature. As Hughes recalled, “In those early years of Cliometrics it seemed like you could hardly miss. Pick any topic in Economic History. Did it make sense as theory? If not, why not? Were there data available? If so, BINGO” (Hughes 1985: 1-2). Fogel showed that Eugene Genovese’s claim that plantation slavery hampered southern industrialization depended on a number of assumptions that Genovese never tested, for example that the relevant industries were characterized by economies of scale (Genovese 1962; Fogel 1967: 285-89). Peter Temin challenged historians’ assertion that Andrew Jackson’s destruction of the Second Bank of the United States was the root cause of the rapid inflation of the 1830s. He pointed out that such an argument implicitly assumed that bank reserves fell after Jackson’s veto, but when he looked at banks’ balance sheets (readily available in government publications), he found that historians had too readily taken the charges of Jackson’s critics at face value (Schlesinger 1945; Hammond 1957; Temin 1969).

The older generation’s antipathy to cliometrics focused in particular on Fogel’s use of counterfactual analysis to dispute the notion that railroads were “indispensable” to American economic development. Fritz Redlich ranted to Gallman about “that madman Fogel” who “plans to build canals across the Appalachian Mountains” (Gallman 1992: 5), and after North and Parker published Fogel’s article in the *JEH* in 1962, several EHA trustees moved to get them fired (North 1993: 11). The fuss about counterfactual history was itself, however, largely a proxy war for a more fundamental disagreement about the importance of entrepreneurial innovation to economic growth. Fogel promoted the neoclassical view that technological change was induced by movements in relative prices that signaled opportunities for profit. In the absence of the railroad, he argued, not only was it likely that the canal system would have

expanded to meet the demand for low-cost transportation services in the U.S., but the automobile could well have been developed earlier: “The axiom of indispensability proceeds on the implicit and unverified assumption that the success of railroads did not choke off the search for other solutions to the problem of overland transportation” (Fogel 1964: 14-15).

Other leading cliometricians shared Fogel’s view that technological innovation was largely a response to demand-side stimuli. Meyer “tended to be very skeptical of the importance of any intangible, such as entrepreneurship” and committed “some of that skepticism to paper,” despite his position as acting editor of *Explorations in Entrepreneurial History* (Meyer 1995: 22). North explicitly downgraded the role of the entrepreneur in his *Economic Growth of the United States*, arguing instead that technological innovations were “a nearly automatic response to successful expansion of industries in an acquisitive society under competitive market conditions.” Although the cotton gin was “unquestionably the most significant invention during the years between 1790 and 1860,” he thought there was little to be gained from studying Eli Whitney. The gin was the product of a “concerted search” for a solution to a pressing economic problem. If Whitney had not invented it, someone else would have (North 1961: 8, 52).

It was precisely this reliance on neoclassical price theory and the methodology of comparative statics that the economic historians who had been associated with the Harvard Center decried. Fogel’s social savings calculation compared the cost of shipping various goods by railroad and the cheapest alternative means of transport at a given point in time. He later acknowledged that the calculation ignored possible dynamic consequences of the railroad, “such as the effect of transportation improvements on the spatial location of economic activity, induced changes in the industrial mix of products . . . , induced changes in the aggregate savings rate, and possible effects on either the rate of technological change in various industries or on the overall

supply of inputs” (Fogel 1979: 5). Yet these were sorts of changes that the entrepreneurial historians thought should be the focus of attention. Chandler notably argued that Fogel’s calculation underestimated the magnitude of the railroad’s most important contribution to economic growth: the greater speed and regularity of transportation compared to canals, which made possible the integration of mass distribution and mass production in large, managerially directed enterprises (Chandler 1977).

What cliometricians like Fogel had done in effect was to move beyond Gay and completely upend the original division of labor between economics and economic history. At a time when economic theory was becoming more uniformly neoclassical (Morgan 2003), they pushed consciously and deliberately to expand its domain by emphasizing the explanatory value of the price signals and market processes that this type of theory was so well suited to analyze. For the most part, they used the neoclassical toolkit destructively—to critique the work of “traditional” economic historians, as well as to demolish each other’s contributions. At the height of the Cliometric Revolution, however, some attempted to go further and formulate a positive theory of historical change. Thus Lance Davis and Douglass North co-authored a book-length study (1971) promoting the utility of a simple model in which rational actors organized to secure institutional change whenever the benefits of the change outweighed the costs of obtaining it.

Cliometricians succeeded in attracting the attention of the economics profession, advancing their own careers and bringing new practitioners into the field. As late as 1959, the EHA’s individual members numbered 476, just 32 percent more than at the organization’s inception. By 1965, however, the rolls had swelled to more than 800 (Heaton 1965: 472). Most of the new members were economists, and the association took on an increasingly cliometric

tone. Robert Whaples has analyzed trends in the content of the *JEH* and found a dramatic increase in the proportion of cliometric-type articles—up from about 10 percent in 1956-60 to more than 40 percent in 1966-70 to more than 70 percent in 1971-75 (Whaples 1991: 293). At the same time, cliometricians gained control of *Explorations in Entrepreneurial History* in 1969, renamed it *Explorations in Economic History*, and redefined the journal's target audience to be economic historians trained in economics (Rosenberg, Williamson, and Rothstein 1970: 3; Neal 1999: 9-10).

The movement of economists into the EHA in turn induced many historians to switch their allegiance to a new organization called the Business History Conference (BHC). The BHC had its origin in a series of meetings that brought together economic and business historians critical of the atheoretical type of business history that N. S. B. Gras was promoting at the Harvard Business School. The group met sporadically between 1954 and 1958 and then yearly thereafter, and in 1971 transformed itself into a full-fledged professional association. Although many of the BHC's original members were economists, during the 1970s the organization increasingly provided an intellectual home to historians unhappy with cliometricians' dominance of the EHA (Lamoreaux, Raff, and Temin 1997: 61).

### **Bottom-Up History and the Cultural Turn**

Meanwhile, trends in the historical profession during the 1960s and 1970s, particularly the growth of "bottom up" and "New Left" history, were fostering a basic distrust of cliometric work. Historians who wrote about the "underside" of history started from the premise that economic development had dire consequences for the bulk of the laboring population. They had little sympathy for the idea, espoused by many cliometricians, that market forces operated for the

general good. Nonetheless, the view of human behavior that underpinned much of this scholarship was not fundamentally at odds with the cliometricians' assumption that human beings rationally pursued their economic self-interest (Sewell 2005: 22-80). New Left historians such as William Appleman Williams (1959 and 1969) and Gabriel Kolko (1963 and 1965) saw American foreign policy and government regulation as straight-forward expressions of businesses' economic interests. Similarly, much of the bottom-up history of the period aimed to restore rational agency to those whom historians had ignored or treated as helpless victims (Thernstrom 1964; Dublin 1979; Prude 1983).

The growth of cultural history had a more profound effect on the relationship between the historical profession and economic history because it effectively redefined historical studies, in the words of one commentator, "as the investigation of the contextually situated production and transmission of meaning" (Toews 1987: 882). It is beyond the scope of this essay to analyze the sources of this trend or survey its scope. Suffice it to say that some of the new cultural historians made sophisticated use of poststructural literary theory and the work of such thinkers as Michel Foucault, whereas others engaged in largely descriptive investigations of cultural practices. Virtually all, however, rejected the idea that human behavior could be reduced to the model of economic rationality at the heart of neoclassical theory. That model might apply to "capitalists," but not to most ordinary historical subjects. Thus, Michael Merrill, Christopher Clark, and James Henretta drew on ideas from the French *Annales* tradition to argue that the *mentalité* of early American farmers was not capitalist—that farmers put other values before profit maximization, such as insuring that their children would be able to earn a "competence" or maintaining an ethic of mutuality with members of their community (Merrill 1976; Henretta 1978; Clark 1979). Similarly, historians of American labor drew on E. P. Thompson and other

British scholars to highlight the cultural traditions that bolstered resistance to industrialization (Montgomery 1968; Foner 1976; Sewell 2005: 22-80). Others rejected the simple model of homo economicus for everyone, claiming that capitalists too were imbedded in larger cultural systems that shaped their behavior, including their business decisions. Businessmen, for example, often discriminated in employment practices against women and minorities in ways that cannot be explained away as economically rational (Kessler-Harris 1990 and 1991; Kwolek-Folland 1994).

Within the larger historical profession, there was a general move in the 1980s and 1990s away from topics related to economic and business history, and as a result, job opportunities in these subfields largely dried up. Many historians in the Business History Conference responded by embracing the shift to cultural history, eschewing not only economic history but also the types of business history, such as Chandler's studies of the managerial enterprise, that seemed to abstract business behavior from its larger cultural context. Indeed, critiquing "internalist" studies of the Chandlerian variety became a veritable industry (Rosen 2013). As an alternative, William Becker advised business historians to learn from what "those engaged in, broadly speaking, cultural studies" had to offer (Becker 1996: 4). Kenneth Lipartito rejected the "current practice" of explaining business behavior in "functional" terms and instead advocated an approach centered on the concept of business culture, which he defined as the "set of limiting and organizing concepts that determine what is real or rational for management, principles that are often tacit or unconscious" (Lipartito 1995: 2). Historian of technology Thomas J. Misa (1996: 55-56) likewise urged business historians to turn away from the "structural-functionalist approach pioneered by Alfred Chandler" and, following the lead of sociologists of knowledge, explore the tension-filled cultural systems that shaped business decision making. There was simultaneously a concerted effort to move the study of gender and race into the center of

business history. A search of the print archive of *Business and Economic History*, which published papers presented at the BHC's annual meeting, shows no papers with titles relating to women or gender before 1979, six from 1979 to 1989, and fifteen in the 1990s. Similarly, there was only one paper relating to African Americans or race before 1990 and then five in the 1990s.<sup>8</sup>

### **Contemporary Developments in Economic History**

The movement of historians from the EHA into the BHC and the cultural turn accelerated at about the same time that controversy erupted over *Time on the Cross* (1974), the analysis of antebellum southern slavery offered by Fogel and his co-author Stanley Engerman. Fogel and Engerman began the book with a list of the conventional historical interpretations that their theoretically informed, quantitatively grounded research would overturn. In place of that received wisdom, they offered a relatively benign view of the institution of slavery in which rational, profit-maximizing plantation owners took care of their human property, understood that keeping families together was important for reproduction, used positive more than negative incentives to motivate their hands, and organized their work forces to capture economies of scale.

This kinder, gentler portrait of slavery rankled many historians, especially since Fogel and Engerman had reached beyond the kinds of questions that econometric techniques were most useful for resolving, such as whether the cultivation of cotton using slave labor was profitable or whether large plantations benefited from economies of scale, to consider the incentives that plantation owners used to drive their hands and whether enslaved workers internalized the

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<sup>8</sup> Not all papers presented at the annual meetings appeared in *Business and Economic History*. Publication was voluntary, and there was in some years an element of selection.

Protestant work ethic. To address these more subtle issues of motivation required different kinds of sources and different sets of skills. Although historians hesitated to assess Fogel and Engerman's econometric work (they let other cliometricians do that), they felt no compunction about examining their handling of textual sources, and they found much to criticize in the way Fogel and Engerman read and analyzed plantation documents. For example, Herbert Gutman (1975) offered a devastating critique of Fogel and Engerman's claim that masters made relatively limited use of physical punishments such as whipping.

As cliometricians jumped into the debate and highlighted even more glaring faults, Gutman and others fretted that Fogel and Engerman's sloppiness would discourage historians from using quantitative techniques—that “the egregious errors” that critics had uncovered would provide “a perfect foil for those skeptical of efforts to employ techniques and methods of the social sciences in the reconstruction of the past” (Yetman 1975: 202). Whether it was indeed the errors that turned historians away is difficult to assess. Whatever push came from this debate may well have been swamped by the powerful pull of the cultural turn in the larger discipline. There is no doubt, however, that historians stopped following the criticism of Fogel and Engerman's work and stopped paying attention to research in economic history more generally. As a result, their views of the literature remained frozen in time, and most subsequent critics have written about the field as if there was little or no change in approach or method since the 1970s (see, for example Adelman and Levy 2014). If they had not stopped paying attention, however, they would have seen that what was at stake in the debate over *Time on the Cross* was greater than tendentious research methods. At the heart of the controversy was a more fundamental argument over the appropriateness of the simple neoclassical models that Fogel and Engerman had used for studying the past.

Even before the debate over *Time on the Cross*, some leading economic historians had moved significantly away from basic neoclassical theory. North dismissively dubbed the earliest of these apostates the “Harvard Wing,” because a number of them had studied with Gerschenkron at that institution (Sutch 1994: 77-79). By the mid-1970s, however, North himself had joined their ranks, declaring in his 1974 address as president of the Economic History Association: “Neo-classical economic theory has two major shortcomings for the economic historian. One, it was not designed to explain long-run economic change; and two, even within the context of the question it was designed to answer, it provides quite limited answers since it is immediately relevant to a world of perfect markets” (North 1974: 2). From the mid-seventies on, North worked to transcend these theoretical limits by developing his own, more general theory of institutional change (see North 1981, 1990, 2005; and North, Wallis, and Weingast 2009).

Parker had fretted in his own presidential address that economic historians, in their eagerness to showcase the explanatory power of market forces, were “in danger of producing simply a kind of hymn to what really happened” (Parker 1971: 6, 7). Not finding enough intellectual sustenance in neoclassical theory, he read more widely—in German historical economics, *Annales*-school history, sociology, and anthropology. As he later mused, “The response to opportunity is a problem of human organization—a political problem rather [than] an economic one.... It is about power and contrivance and how individuals control one another mutually.” Parker strove to look “past the ‘opportunity’ part to this other element, where culture, society, and a collection of individual personalities all come into the structure of explanation, piled on top of one another in layers” (Parker 1991: 21).

The debate over *Time on the Cross* accelerated the move beyond basic neoclassical principles. At the simplest level, critics observed that the evidence on the brutal treatment of

slaves was not consistent with the idea that rational, profit-maximizing slave owners took good care of their human property. However, a more profound revision emerged from scrutiny of Fogel and Engerman's claim that large slave plantations were more productive than small, slaveless farms. Plantations only appeared more efficient, the critics argued, because they grew relatively more cotton, which had a higher value in world markets than other crops (David and Temin 1976 and 1979; Wright 1976 and 1979). At existing market prices, it would have been profit maximizing for free farmers to grow as much cotton as could they pick, but instead they chose to grow relatively more corn. To explain this behavior Gavin Wright (1978) developed a "safety-first" model in which small farmers were primarily concerned with their ability to feed their families and preserve their status as independent landowners. If historians had been paying attention, they might have noticed that Wright's model was analogous in important ways to the concept of competency that historians were developing around the same time to explain the *mentalité* of early-nineteenth-century northern farmers (Henretta 1978; Merrill 1976; Clark 1979).

By the 1970s mainstream economists were developing new fields of research such as information economics, game theory, and mechanism design that recognized that information was imperfect and costly, that human beings were only boundedly rational, that they behaved strategically, that they differed in their access to information, that models might yield multiple equilibria, and that the outcome of any economic process might therefore be a matter of history.<sup>9</sup> Stimulated by these developments many of the cliometricians who had criticized *Time on the Cross* pushed the field of economic history in new directions in the years that followed. Temin, for example, sought to understand why popular support for federal pharmaceutical regulation

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<sup>9</sup> The change is apparent in the list of recipients of the Nobel Prize in Economic ([http://www.nobelprize.org/nobel\\_prizes/economic-sciences/laureates/](http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/)), but see especially the work of George Akerlof, Joseph Stiglitz, Eric Maskin, Oliver Williamson, Peter Diamond, and Jean Tirole.

was so concentrated temporally and turned to anthropology for a model of how personality type might interact with change in the larger society to produce this pattern (Temin 1980).

Collaborating with historian Louis Galambos (1987), he puzzled over the AT&T executives who settled the antitrust suit against the company by making what was (with hindsight) the wrongheaded choice to spin off Bell Labs and Western Electric (that is, the company's research and development capabilities) into a separate company. He joined with Daniel Raff and Naomi Lamoreaux to explore how some of the new theory might be useful for writing business history (see Temin 1991; Lamoreaux and Raff 1995; Lamoreaux, Raff, and Temin 1999 and 2003). He also, in his presidential address to the Economic History Association, called for economic historians to take up the study of culture (Temin 1997a).

David, pondering the awkward organization of the QWERTY typewriter keyboard, seized upon the concept of path dependence and argued that societies find it difficult to adopt alternative technologies or ways of organizing economic activity, even when it would clearly be more efficient to do so (David 1985). He and Gavin Wright challenged the simple notion that economic actors respond in a straightforward neoclassical way to factor endowments. The U.S. rise to industrial leadership in the early twentieth century may have been resource-based, they showed, but the U.S. was not particularly well-endowed with mineral resources. At the heart of the country's economic success, in their view, was a set of institutions and cultural beliefs that encouraged individuals and companies to search for minerals, provided them with the necessary expertise, and supported and even subsidized their efforts (David and Wright 1997). Wright went on to devote much of his career to documenting the role that racism played in maintaining apartheid-like institutions in the South, making the case that change had to be imposed by the

federal government even though whites as well as blacks stood to benefit from an end to segregation (Wright 1986 and 2013).

This is not to say that economic historians abandoned neoclassical theory or their cliometric roots. Most economic historians draw eclectically on a mix of theoretical traditions and still consider the standard model of perfect competition to be a valuable part of their toolkit. Thus Temin used a clever one-factor (so-called Ricardian) trade model to defend the idea that there was an industrial revolution in Britain (Temin 1997b), and Engerman and Kenneth Sokoloff (a Fogel student) traced present-day variation in levels of economic development in the Americas to the factor endowments that Europeans encountered at the time of settlement (2012). Although these contributions underscore the continued power of simple theoretical concepts in economic history, they also show how far the field has moved since the Cliometric Revolution. Temin used his model to challenge the essentially neoclassical arguments of N. F. R. Crafts and C. Knick Harley (Harley 1982, Crafts 1985, Crafts and Harley 1992) that there was no significant break that could be called an industrial revolution in Britain in the late eighteenth century, and his critique has provided reinforcement to economic historians, such as Joel Mokyr (a student of Parker's), who has argued that enlightenment culture was an important driver of the industrial revolution (Mokyr 2009). Moreover, Engerman and Sokoloff did not consider their factor-endowments story to be alternative to an explanation based on institutions. Rather their goal was to propose a mechanism for understanding why institutions in some parts of the Americas were much less conducive to economic development than those elsewhere. Where initial factor endowments produced high levels of inequality, Engerman and Sokoloff hypothesized, they enabled the wealthy to establish institutions that perpetuated their advantages—institutions that

insured that the bulk of the population would remain poor and uneducated and thus inhibit technological change and productivity growth.

### **Promising Signs and Ongoing Concerns**

Over the last couple of decades there have been scattered but promising signs of renewed interdisciplinary conversation. Some of this rapprochement has been a byproduct of continued institution building. In California, for example, several prominent economic historians took advantage of an effort by the University of California to foster intellectual exchange among the system's various campuses to organize the All-UC Group in Economic History. Formed originally in 1972, the group is still going strong. It has attracted participants from other schools in the region and made California the center of economic history in the United States. Although its initial leadership came mainly from economists, the group's successful navigation of the politics of the UC system required that its composition be interdisciplinary as well inter-campus, and the group has provided funds and a supportive intellectual environment for training graduate students in history, as well as in economics.<sup>10</sup> Other exemplary interdisciplinary initiatives include the Early Modern Group in Social Science History founded by Jean-Laurent Rosenthal at Caltech<sup>11</sup> and the Joint Centre for History and Economics founded by Emma Rothschild at Harvard and Cambridge.<sup>12</sup>

Interdisciplinary conversation is difficult to sustain under any circumstances, but it is especially difficult when practitioners from the different disciplines have not been talking to each other for decades. Lack of interaction makes it easier to stereotype the "other" and react with

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<sup>10</sup> For lists of participants and past conferences, see the group's website: <http://allucgroup.iga.ucdavis.edu/>.

<sup>11</sup> [http://people.hss.caltech.edu/~jlr/jlr\\_files/ECHIEV.htm](http://people.hss.caltech.edu/~jlr/jlr_files/ECHIEV.htm).

<sup>12</sup> <http://www.fas.harvard.edu/~histecon/index.html>; <http://www.histproj.org/>.

hostility to those who dare to cross disciplinary boundaries. The attack on Kenneth Pomeranz's *Great Divergence* by Philip Huang and others is a good example. Although Pomeranz is a historian by training who has done substantial archival work in Chinese sources, the historians who slammed his study cast him as overly influenced by neoclassical (and even classical) theory and, like the stereotypical economist, dependent on secondary sources for information about Chinese history (Huang 2002). Historian R. Bin Wong, Pomeranz's intellectual partner in developing a new view of Chinese economic history, was subjected to similar treatment for his book *China Transformed* (1997). Both scholars, however, benefited from the serious and supportive reception their ideas received from the economic historians in the All-UC group and from economic historians more generally.<sup>13</sup> Their claim that the wealthiest parts of China had levels of economic development similar to those of the wealthiest parts of Europe in the eighteenth century has revolutionized the study of both Chinese history and world economic development. It has stimulated new empirical challenges (see, for example, Broadberry and Gupta 2006 and Allen 2009). It has also called attention to complementary work by scholars such as historian Madeleine Zelin, whose studies of salt producers in Zigong and business organizational forms are characterized by deep archival research and the creative use of economic theory (Zelin 2005 and 2009).

Beyond encouraging unproductive stereotyping, the lack of interdisciplinary conversation can adversely affect the quality of scholarship of both historians and economists. Historians in the U.S. are now flocking to study topics in the history of capitalism that are closely related to work in economic history. The phenomenon began during the early- to mid-2000s, when a number of scholars started dissertations that became the books that ultimately staked out the field

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<sup>13</sup> See, for example, Pomeranz's CV for the list of presentations he made of this work at All-UC conferences and at economic history workshops at UC campuses and elsewhere. <https://history.uchicago.edu/sites/history.uchicago.edu/files/uploads/PomeranzCV.pdf>, accessed 26 April 2015.

(Mihm 2007; Hamilton 2008; Morton 2009; Hyman 2011; Ott 2011), and when Sven Beckert inaugurated his conferences at Harvard on the history of capitalism. But it took on the trappings of a movement once the financial crisis of 2008 rekindled historians' interest in the economy. The scholars in the forefront of this movement are trained in cultural history and make little use of quantitative sources. To the extent, therefore, that they focus on what people were saying was happening, rather than what we can see occurring from the sources, they are likely to repeat the errors of the past. A good example is Edward Baptist's *The Half Has Never Been Told* (2014), an important effort to place the violence of slavery squarely at the center of the history of American capitalism. Baptist makes much of the growth of productivity in cotton picking that cliometricians Alan Olmstead and Paul Rhode (2008) have documented for the first half of the nineteenth century, but he dismisses their contention that the increase owed to the development of new varieties of cotton that were easy to pick and instead attributes it primarily to coercion. Undoubtedly, both biological innovation and violence were behind the rise in productivity, but the relative contributions matter and cannot be established, as Baptist tries to do, by a few quotations deprecating the new seeds types. Another lapse is Baptist's claim—and also Beckert's in *Empire of Cotton* (2014)—that slave-based cotton cultivation drove U.S. economic development and the world economy in the antebellum period, a claim that ignores the abundant evidence to the contrary amassed by economic historians since the 1930s.

Economists also stand to benefit from renewed interdisciplinary conversation. There has been a resurgence of interest in history among mainstream economists as a byproduct of the search for novel “instrumental” variables. Economists have been preoccupied in recent years with the technical problem of how to establish a causal relationship between one variable and another. One way of making the case for causation is to find a third variable (an instrument) that

is plausibly not be a function of either of the variables of interest and only affects outcomes through the posited causal channel. Variables from the distant past are obvious candidates for instruments and their successful use (see Acemoglu, Johnson, and Robinson 2001) has stimulated a number of such studies. Perhaps inevitably, economists came to see the instruments themselves as explanations, and the channels they were supposed to help identify faded into the background (Rodrik, Subramanian, and Trebbi 2004). The result has been a flurry of “historical” studies in which history itself plays little role. Instead, economists emphasize the persistent effects of bygone institutional or cultural patterns and implicitly treat everything that happened in the interim as if it were of little consequence (see, for examples, Nunn 2008; Nunn and Wantchekon 2011; Alesina, Giuliano, and Nunn 2013).

Although some economic historians have jumped on the persistence-studies bandwagon, others have checked to see if the observed statistical relationships are stable over time, as the method implies they must be, and have uncovered considerable evidence that they are not (see, for examples, Musacchio 2008; and Frankema and Van Waijenburg 2012). These findings in turn have resulted in increased interest in the study of cultural change, long the domain of historians, and in political economy, a subject increasingly attracting the attention of the new historians of capitalism. It would therefore seem to be a propitious time to bridge the gulf that has opened up between economists and historians and encourage not only interdisciplinary conversation but collaboration. Historians generally do not have the theoretical or quantitative skills to tackle many of the questions of political economy in which they are interested or to move beyond hermeneutics and develop a systematic understanding of cultural change. Economists, in turn, are often handicapped by an overly stylized view of cultural practices and the workings of social and political institutions and could benefit not only from historians’

deeper understandings but also from consideration of models developed in the other social sciences.

Interdisciplinary exchange, though obviously valuable, will not be easy to accomplish and undoubtedly will be accompanied by intellectual fireworks. But that should not be a reason to avoid it. I have already mentioned the important contributions that the debate over Pomeranz's and Wong's books stimulated. Economist Avner Grief's work on medieval commerce sparked a similarly acrimonious response (Greif 1989; Edward and Ogilvie 2012; Greif 2012). Although the debate has been contentious, it has brought to the fore important new studies of medieval and early modern trade by Francesca Trivellato (2009) and Jessica Goldberg (2012). Marrying cultural and economic analysis in a sophisticated way, these books provide tangible evidence of the gains to be derived from renewed interdisciplinary conversation.<sup>14</sup>

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<sup>14</sup> Both Trivellato and Goldberg participate in the interdisciplinary early modern group that Rosenthal convenes at Caltech.

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