

« The Role of Cliometrics in History and Economics »

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The Role of Cliometrics in History and Economics

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Summary

How did cliometrics in particular, and economic history in general, arrive at this crossroads, where it is at once considered to be a dying discipline and one that is spreading through the economics discipline as a whole? To understand the current status and future prospects of economic history, it is necessary to understand its past.

Keywords

Cliometrics, economic history, Robert Fogel, Douglass North, economic growth, econometrics, interdisciplinary economic history, new economic history, multidisciplinary, methodology, quantitative.

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Introduction

In 2019 Diebolt and Hauptert (2019a), in a response to the question of whether economic history had been assimilated by the economics discipline, argued that rather than assimilation, economic history resembled a ninja, and had infiltrated the discipline. That view of the current status of economic history is not universally shared. Abramitzky (2015 p 1242) bemoaned the fact that the typical economist only cares about the past “to the extent that it sheds light on the present.” More recently, Stefano Fenoaltea (2018) mourned what he saw as the loss of respect for the field of cliometrics.

Abramitzky and Fenoaltea represent contemporary scholars who identified dark shadows encroaching upon economic historians. Aitken (1960), Redlich (1965), and Hacker (1966) were critics of the shortcomings of clio. And Solow (1985) and Boldizonni (2011) were damning critics, predicting its imminent demise, exhorting its shortcomings, and criticizing its practitioners for derailing the discipline of economic history. Even one of its founders, Douglass North (1965) held a critical view of cliometrics at one point. And in the larger picture, economic history was

written off almost as soon as it asserted itself as a field (Marshall 1897, Veblen 1901), and frequently criticized throughout the 20th century (Gras 1920, 1931, 1962).

But amidst this foreboding of doom and neglect on the part of economic historians, there is much to celebrate. Some indicators suggest economic history is being more widely published and cited (Diebolt and Hauptert 2018) and economic historians have integrated themselves into the wider discipline where they are “appointed to prestigious offices . . . serve on the editorial boards of mainstream and field journals . . . [and] win major economics prizes.” (Margo 2018 p 378).

How did cliometrics in particular, and economic history in general, arrive at this crossroads, where it is at once considered to be a dying discipline and one that is spreading through the economics discipline as a whole? To understand the current status and future prospects of economic history, it will be necessary to understand its past. Economic historians have shown great interest in studying their roots. William Ashley (1893) was the earliest to discuss the evolution of the discipline. Among other notable contributors are Clapham (1931), Cameron (1976), Kadish 1989, and Mitch (2011).

Economic history in Europe

The origins of economic history are not shrouded in the distant mists of time. Rather, the field originated in the late 19th century, largely along two strands: one in Germany and the other in the United Kingdom. The two would eventually merge in the person of Edwin Francis Gay in the United States. Gay was not only a significant figure in the early days of economic history in the US, but he can legitimately lay claim to being the grandfather of Cliometrics.

While the formal study of economic history as a discipline began in the late 18th century, there are examples of earlier works regarded as historical treatments of economic issues. Among the earliest recognized works of what could be considered economic history are two 17th century treatises (Temple 1672, Evelyn 1674). Each addressed concerns over contemporary international political and economic rivalries.

The first economist to argue for historical context in the approach to addressing economic issues was Richard Jones, who taught political economy at Haileybury in the 1850s. This was an unusual calling, as political economy and history were separate subjects, taught in separate departments, and neither was particularly welcoming to the other’s discipline. Political economics departments tended to focus on contemporary issues with little interest in historical antecedents. History departments, while they were more likely to consider economic factors in their work, did not consider them particularly important. (Cole 1968)

The Germans were the first to formally organize economic history as an academic discipline. In part, this was the result of German interest in establishing the most appropriate economic policies to be followed by the developing states in the mid to late 19th century. The UK followed at the end of the century in reaction to the rising social concern over the poverty of the urban industrial working class.

The seminal event for economic history in Germany was the publication of Wilhem Roscher's *Grundriss* (1843), which altered the approach to economics. Roscher was a historical economist who focused on the origins and impact of historical economic activities and institutions. Along with Friedrich List, Bruno Hildebrand and Karl Knies, and later followed by Roscher's most prestigious student, Gustav Schmoller, these historical economists published much of their economic history research relating to England, though little of it was ever translated and published in English.

In England, a similar movement was taking place, led by Arnold Toynbee, who sought to develop economic history independent of political economy. Toynbee believed that economic theory was relative, not universal, and that history could be used to show how, for example, free trade and competition were not always beneficial. By the 1870s political economy had devolved into a methodological debate over the merits of inductive (the development of theories that would provide evidence of the truth) or deductive (the gathering of facts, which would then lead to a certain conclusion) reasoning. This debate helped pave the way for historical economics. The thorough investigation of the origin of social problems in turn fostered an interest in the origins of other economic-based issues. Ultimately it was the revolt against the deductive theories of classical economics that laid the groundwork for the emergence of economic history as a distinct discipline.

Led by Roscher and carried on by Schmoller in Germany, and Toynbee and Ashley in England, the original aim of the historical school was to replace what they believed to be the unrealistic theories of deductive economics with theories developed inductively through the study of history. This line of thought held that history was the key source of knowledge about humanity and its organizations, and because it was culture and time specific, it was relative, and could not be generalized over time or space. Their view was that economics was best approached from the vantage point of empirical and historical analysis, not the deductive approach favored by political economists wielding their abstract theories.

Before Schmoller, the historical economists had focused their work more on the field of history than economics. The distinguishing characteristic of Schmoller's work was that it aimed to account for the evolution and variation of institutions in so far as they affected the economic aspects of life. Though trained in the historical school, Schmoller's emphasis was on economics, which is what set him apart from his peers. For this reason, he is often identified as the first true economic historian.

In the 1880s the historical school of economics developed a schism that would be further exacerbated nearly a century later by the rise of cliometrics. The more conservative branch, the historical economists who followed in the line of the original historical school, abandoned the use of theory altogether. While the work they did was valuable, Veblen (1901) argued that because it was completely lacking in economic theory, it was not actually economics at all – and thus not economic history. The other branch was represented by Schmoller, and was the wellspring of the first generation of economic historians.

Schmoller eschewed the use of formal models in the social sciences. He believed there were too many social interactions to consider to be adequately captured by a general model. He was, however, partial to statistics, which he considered an invaluable companion for historical research, but only for those variables that could be accurately measured. He always questioned the source and interpretation of the data in relation to other cognate facts and theories. However, the fact that he was willing to go this far is what distinguished him from his mentor and the elder historical scholars.

The earliest form of economic history was based more in the historical approach. It tended to be narration fortified with occasional quantitative data. As the discipline began to take shape in the later 19th century, its leading practitioners sought to develop it independent of standard economic theory. Clapham (1929) argued that the central problems of economic theory, though stated in terms of a particular historical phase, were in essence independent of history. With few exceptions this general view permeated the writing of economic history into the 1920s. Data were only occasionally collected, and when they were, they were seldom used to test economic theory. They were typically either used anecdotally, or served in the role of summary statistics. Economic models were practically unknown.

As the interest in economic history grew, so did the need to teach it, which led to the creation of exams, which in turn necessitated teachers of the subject. The adoption of economic history for examination in the History Tripos at Cambridge in 1875 led to the publication of the first English language textbook in the subject by William Cunningham in 1882. The History Tripos at Cambridge produced its first fully-fledged, formally trained, economic historian, John Harold Clapham, in 1898. (Tribe 2000)

Cunningham made two seminal contributions to economic history. The most obvious was his lifelong effort to advance the subject through the further work on his textbook (1882), which had five editions and grew to three volumes. Additionally, he undertook a vigorous campaign to achieve public and scholarly recognition for the approach of economic history (Cunningham 1892).

The opening of the London School of Economics (LSE) in 1895 marks the unofficial beginning of economic history in the UK. The LSE was founded in opposition to the tenets of orthodox economics. As a result, economic history was an important presence from the beginning. The first Director of the LSE was an Oxford educated mathematician-turned-economist named W.A.S. Hewins. In 1901 it became the first British university to offer a degree in economics, with economic history as one possible course of study.

While the founding of the LSE and its courses in economic history was a cornerstone in the evolution of economic history, the discipline was suffering growing pains. At about the same time that the LSE was awarding its first degree in economics, the economics discipline itself was moving more firmly in the direction of deductive scholarship. It appeared that the attempt of the historical school to replace it with inductive theory had failed. The movement toward deductive reasoning was an effort to make economics more scientific. This was a reaction to the

rising stature of the natural sciences, and led to a waning of the historical movement, representing a historical low point for the discipline.

Events during the First World War contributed to a growing interest in statistics at the expense of theory among economists. Chief among those was the creation of the National Bureau of Economic Research (discussed in greater detail below). This shift in focus began to close the gap between historians and economists. As an added benefit, it forced historians of all stripes to be less tolerant of research supported by loose, unsupported generalizations. In the UK the culmination was the creation of the *Economic History Society*, in 1926. It was the first formal organization of economic historians, and was followed in 1927 by the debut of the *Economic History Review* (EHR), the first academic journal dedicated to research in economic history. (Barker 1977)

Economic History in America (Hauptert 2016)

American economists were always more tolerant than their European contemporaries of the use of statistics in their work. The *American Statistical Association* (ASA) was launched in 1839, its membership consisting of individuals who paid serious attention to compiling time series data. Published collections of such data had already appeared well before the formation of the ASA (Pitkin 1816, Seybert 1818). By the late nineteenth century numerous state and local historical societies as well as the *American Antiquarian Society* (founded in 1884), could boast of vigorous data accumulation efforts. The federal government had conducted a decennial census since 1790, with attention to economic measurements increasing after 1850. Among the earliest American publications combining data and history were Freeman Hunt, *Lives of American Merchants* (1858), James L. Bishop, *History of American Manufactures from 1608 to 1860* (1861), and Thomas P. Kettell, *One Hundred Years' Progress of the United States* (1870).

Before the EHR and its American cousin, the *Journal of Economic History*, there was no specialized outlet for the publication of research in economic history. However, general economics journals did occasionally publish research in the field. Among the earliest with a historical bent were Charles F. Dunbar's "Economic Science in America," published in the *North American Review* in 1876, and a study of early US transportation and banking, which appeared in the *Quarterly Journal of Economics* (Callender 1903).

Harvard was the epicenter of economic history in the United States in the late 19th and early 20th century. Dunbar was a professor of political economy and founder of the Harvard economics department. With his colleague Frank W. Taussig, who authored *Tariff History of the United States* in 1888, he taught courses titled "Financial History of the United States," and "The Tariff History of the Country." J. Lawrence Laughlin, who would eventually leave Harvard to found the University of Chicago economics department, taught a course on the history of political economy. Over the course of the 1890s, Dunbar also taught "Economic history of Europe and America since the Seven Years' War," and Taussig taught "The history of tariff legislation." (Mason 1982)

Harvard, and with it the United States, made its first serious statement in support of economic history in 1892, when Dunbar and Taussig hired William J. Ashley as the world's first chair of economic history. Ashley's reputation as an economic historian was made with the publication of his history of the English woolen industry (1887) and what is considered by many to be his greatest work, *An Introduction to English Economic History and Theory* (1885, followed by a second volume in 1893).

Ashley studied under both Arnold Toynbee, the Oxford based scholar responsible for popularizing the term "industrial revolution," and Schmoller. In 1885 he left Oxford to accept the position of Professor of Political Economy and Constitutional History at the University of Toronto. He argued for a course in economic history alongside the general economic theory (i.e. political economy) course (Ashley 1927). Later in his career he was a proponent of the study of statistics, which he felt would become an integral part of every important economics department.

Ashley remained at Harvard for a decade, returning to England in 1901. Edwin F. Gay was chosen to assume his chair in economic history. The appointment of Gay to replace Ashley represented a sort of melding of the German and English roots of economic history. Ashley, though trained in the UK, was strongly influenced by German scholarship. For his part, Gay had spent more than a decade studying the social sciences in various European universities. While in Berlin in 1893, he attended Schmoller's economic history seminar and became a convert. His contribution to economic history rests less on his research than his teaching and administrative skills. He would eventually become the first dean of the Harvard business school and the first president of the Economic History Association, as well as a founder of the *National Bureau of Economic Research* (NBER). But before that, he began to make his mark by producing graduate students grounded in the standards and techniques of the German methodological principle of sticking to the facts, of telling history as it really was. Gay used a multidisciplinary approach in the classroom, similar to what he had learned from Schmoller, who was famous for his insistence that the big picture always had to be kept in mind. To account for the complexity, Gay taught his students that hypotheses had to reflect economic, social, political, international, and psychological approaches. (Hamilton 1947, Heaton 1965a)

Ashley and Gay were both proponents of the German concept of stages in economic growth, particularly the notion of a "take-off." Toynbee (1884) had espoused this concept as well, but he called it the "industrial revolution," coining a term that would develop a life of its own.

Gay mentored a noteworthy assemblage of doctoral students, including Chester Wright, Norman S. B. Gras, Abbott Usher, Julius Klein, and Earl J. Hamilton, all of whom manifested in one way or another their perception of economic history as an adjunct of economic theories. Wright (1941) attempted to carry the relationship the farthest in his *Economic History of the United States*. Gras (1962) went to great lengths to extend the Germanic scheme of economic stages to study the history of capitalism.

While the first decades of the 20th century saw economic history spread widely across departments, it was still not very influential within the larger economics discipline. Many leading institutions in the US created chairs in economic history, but without a domestic society or dedicated journal, the field had difficulty making inroads. It didn't help that economic historians rejected the growing fascination within the discipline with the scientific method and its potential applications to economics, exemplified by the theoretical approach espoused by Alfred Marshall (1890). In the US this manifested itself in the growth of economic forecasting. As Friedman (2014) details, this eventually led to the creation of the NBER.

Forecasting, the NBER and the Roots of Cliometrics

Wesley C. Mitchell, a Columbia University economist, believed that economic theories were not immutable laws, but rather that they depended on context and evolved over time. He was interested in developing the field of economics into one that took into account what human beings actually did, a precursor to the behavioral economics approach popularized in the 21st century. In this approach he was greatly influenced by Thorstein Veblen, his mentor at the University of Chicago. Mitchell's *Business Cycles* (1913) was an assemblage of business data and his comments on the various series, which seemed to forecast a new theory of business cycle movements.

During WWI, Gay and Mitchell headed the government's Central Bureau of Planning and Statistics, responsible for the gathering and reporting of statistical data to aid in the war effort. It was during this service that Gay became convinced of the need for better economic statistics. After the war, the pair were instrumental in the creation of the NBER to stimulate the collection and interpretation of historical statistics.

Mitchell served as research director of the NBER from its founding in February of 1921 until 1945. He gathered tremendous amounts of empirical economic data in order to draw inductive generalizations from it. Building on his work on business cycles he envisioned the use of expert analysis and statistical investigation as a tool to improve society. He believed the dissemination of scientific, objective data coupled with a better understanding of business cycles would allow government and business leaders to successfully enact countercyclical policy that could tame the business cycle.

While the gathering of statistics by the NBER was very much in the interest and to the benefit of economic historians, the resulting expansion and increased proficiency in the use of statistical materials diverted attention and resources from economic history. The field also began to lose graduate students to "applied" fields such as international finance, statistics, and business cycle analysis. Enrollment in economic history courses held steady, since it was a required course in the graduate programs of the leading universities, but writing it as a field declined. Norman Gras (1931) painted a gloomy picture of the state of economic history as being neglected by universities, who regarded it as a very special subject, but one suffering a lack of intellectual resilience.

The NBER ultimately served as a catalyst for the change in emphasis from narrative to quantitative studies in economic history, serving as a precursor to the eventual cliometric movement. Mitchell and Simon Kuznets led a group of scholars at the NBER in the production of a series of quantitative descriptions of the growth and development of the American economy, using data the NBER had gathered to measure growth as far back as the 1870s. The culmination of this quantitative approach to descriptive economic history was the *Historical Statistics of the United States* (1960).

Over time economic history presented itself as empirical and interdisciplinary. The empirical focus stemmed from the NBER approach to collecting historical data. But data could also be qualitative, as the German school believed was the proper focus of economic historians. It was also empirical in that economic historians viewed history as a laboratory where they could test economic theory.

The Birth of the Economic History Association (Hauptert 2016)

The formation of a U.S. economic history society was spurred in part by fear (Aitken 1975, Clough 1970, Cole 1968, de Rouvray 2004a, Heaton 1941, 1965b). Edwin Gay's students, who dominated the American field of economic history, had learned the empirical, inductive approach to economic history, and when it was threatened with extinction by the growing mathematical approach, they sought a refuge. They feared that journals would no longer publish their work, nor conferences accept their papers, which would have sent the discipline, along with their careers, into a death spiral. Earl Hamilton, a Gay protégé, was the first to attempt a rescue. In 1937 he tried to rally colleagues to create a US based economic history association. The endeavor failed, in part due to the concern that it would cannibalize the UK *Economic History Society* and result in two weak sisters.

Ultimately, it was the outbreak of WWII that led to the creation of a US economic history association. The war was expected to cause a decrease in scientific exchanges between the U.S. and Europe, which spurred American economic historians to action, and in December of 1940 the *Economic History Association* (EHA) was born. Edwin Gay was named the first president, and E. A. J. Johnson was named editor of the society's new publication, the *Journal of Economic History*, which debuted in 1941

Gay was the obvious choice for president, but was initially reluctant to take the position. He had previously held the presidencies of the *American Economic Association* and the *Agricultural History Society*, in addition to his administrative positions with the NBER and as dean of the Harvard Business School. However, he had recently retired to California for the express purpose of reducing his administrative responsibilities in order to concentrate on research.

At the time, Gay (1941) felt that the attempt by historical economists to displace the "theoretical school," had failed. The use of the deductive method had been fortified with an increased range and depth of contemporary observations, and its perspective had expanded to become less individualistic and more social. In his presidential address, he called for the reunification of economic history with economic theory, noting that the economic historians

knew a great deal about the long trends of productive energies and social pressures that impacted economic growth, which could be combined with the tools of the theorist to lend more robust insight into the growth process. Far from incompatible, he felt that true philosophical objectives and the careful assembling and analysis of data were complementary.

E. A. J. Johnson (1941) agreed with the approach that Gay proposed. While he acknowledged that the number of tools available to economic historians was increasing, he felt there was still too much work in economic history that was little more than a haphazard gathering of facts with little consideration for their applicability to understanding economic growth. He argued that the discipline could best be advanced by employing the most efficient theoretical tools at the disposal of the economic historian, citing Leo Rogin's (1931) work on farm productivity as an example.

Thomas Cochran (1943) believed that the use of limited and modest economic hypotheses, such as monopolistic competition or location theory, would be more useful tools for practical research than the sweeping assumptions of the historian. He felt that specific limited propositions could be the first steps in applying a logical technique to the data of social science for the purpose of stating and testing hypotheses.

Even in its infancy the EHA was already sowing the seeds of the cliometric movement. It would be the next generation of economic historians who would reap the harvest.

The New Economic History

After WWII, with the American economy booming, the field of economics rose in status. Featuring rigorous models, tested using an abundance of quantitative data with advanced, mathematically expressed formulae, economics was regarded as the paradigm of the social sciences. William Parker (1986) quipped that if economics was the queen of the social sciences, then economic theory was the queen of economics, and econometrics was its handmaiden.

Concurrent with the increased technical focus in the economics discipline, economic historians took greater interest in the determinants of economic growth and the widening income gap between developed and underdeveloped regions of the world. They felt their insights, coupled with new data, techniques and tools, could shed light on the mysteries of uneven global economic development.

Norman Gras (1962) had a different take on economic history. He believed that it was showing its old age and ceding its status to the rising field of business history. He cited the decline of economic history in Germany as evidence of its waning influence. The burgeoning cliometric movement forestalled any imminent decline, but future events would prove Gras correct. The rise of the cliometric movement in the US was not mirrored in Europe, and when it did finally begin to make headway, it did so in the UK before arriving on the continent. Cliometrics did not dominate the European scene as early or as completely as it did in North America. Tilly (2001), Grantham (1997), Crouzet and Lescent-Gille (1998), and Floud (2001) document the history of cliometrics in Europe.

As we have seen, the roots of cliometrics, originally known as the “new” economic history, are deep. But its coming-out party can be dated to the 1957 joint meeting of the *EHA* and the NBER Conference on Research in Income and Wealth. If a specific moment were to be cited as the dawning of cliometric research, it would be a pair of papers coauthored by Alfred Conrad and John Meyer (1957 and 1958) that were delivered at the conference. The first paper, was methodological, and explained how the scientific method applied to economic history. Parker (1980) cites the second paper as one of the most influential in the evolution of economic history. It demonstrated the power of this new methodology by using it to calculate the profitability of slavery on the eve of the Civil war. The analytical method, the data, and the economic framework, showed what this new method, this “new” economic history, was capable of producing, and how it could upend previously held beliefs. Each would prove to be both influential and controversial for the next generation of economic historians.

Arguing against those who cliometricians would later label “old” economic historians, Kuznets (1966) claimed that little would be gained from a study of the past unless it was systematic and quantitative, because that was the only way to gauge the relative effects of factors and events. His student, Robert Fogel (1965) agreed, crediting his mentor as the primary inspiration for the work of the new economic history.

Kuznets may have inspired the cliometric movement, but it was Robert Fogel who reunified economics and history. He paired the latest techniques of modern economics with large-scale cross-sectional and longitudinal data sets harvested from original sources to reinterpret American economic growth in sectors as diverse as railroads, slavery, and nutrition. Rather than conjecture about the causes of growth, he set out to measure them with great care and precision, opening new ways to the past (McCloskey 1992).

Fogel’s breakthrough work was *Railroads and American Economic Growth* (1964a). At the time of its publication, economists believed that modern economic growth was due to the “take off” of certain important industries that propelled economic growth. Fogel set out to measure the impact of the railroad, which was generally credited for the growth of the American economy. He constructed a counterfactual to measure, with extraordinary precision, the contributions of the railways to the growth of the American economy. Counterfactual analysis is the idea of determining the impact of an event or factor by considering what would have happened in its absence. Contrary to popular belief, he found that the railroad was not an indispensable ingredient in America’s economic growth.

Fogel’s use of counterfactual arguments and cost-benefit analysis made him an innovator of economic historical methodology, but not universally loved. Fritz Redlich (1965) accused him of “fictitious quasi-history” for his emphasis on the counterfactual. He acknowledged the value of counterfactual analysis, but thought it was a tool well suited for social science research, but not history. Others simultaneously acknowledged the heroic efforts at data accumulation and the technical analysis Fogel applied to his railroad study, but criticized the absence of any consideration of other important beneficiaries of the growth of the railroad, such as the development of capital markets, the increased mobility of the populace, technological advances

in the iron industry, and the migration of foreign labor to America (Madden 1965, McClelland 1968). Harry Scheiber probably spoke for many when he concluded that *Railroads and American Economic Growth* “is wrong, but brilliantly so.” (Scheiber 1966, p 278)

Fogel was not the first to use counterfactual analysis, the concept had previously been used by Fritz Machlup (1952). But Fogel raised it to a new level with his exacting measurement and analytical approach. This approach formed his major works on slavery and demography as well (Fogel 2000, Fogel and Engerman 1974). Fogel recognized early in his career that to answer such questions much greater use had to be made of quantitative evidence. He mastered the most advanced analytical and statistical methods and successfully employed them in his research.

Fogel was the embodiment of the difference between the “old” economic history and the “new:” He applied cutting edge techniques to newly created data series. Those techniques were made more powerful and easy to replicate and reconsider thanks to the growth of computing power.

Like Fogel, North also focused on quantification early in his career, measuring the impact of decreased transoceanic shipping costs. His surprising finding was not that shipping costs decreased, which was widely recognized at the time. Rather, it was his explanation that the reduced cost was not a function of technological innovation, but institutional changes, such as a decrease in piracy and faster turnaround times in port, that were the primary source of the decreased costs. North would spend most of the rest of his long and successful career focusing on institutions.

The generation of economic historians who were trained during the 1960s embraced the cliometric approach, using econometrics to test economic theories applied to historical settings. The love affair was not unanimous though. Cochran (1969) characterized the disagreement as one about the choice of models. Those preferred by the new economic historians were quantitative and mathematical, while those used by the “old guard” tended to be narrative. The latter accused the newcomers of bringing economic theory to history without a proper understanding of the facts, claiming that realistic historical models had to be too highly generalized or too complex to allow the assumption of mathematical relationships (Redlich 1965, Aitken 1960).

Historians and “old school” economic historians had neither the skills necessary to exploit the new tools the cliometricians brought to the academy, nor shared their narrow analytical focus. Critics chided Fogel for his “counterfactual” approach, dismissing the idea that any such exercise could be carried out with any meaningful result (Erickson 1966, Kirkland 1967). They were critical of the techniques Fogel and other cliometricians used (Hacker 1966, McClelland 1968), and they chastised Fogel in particular, and the cliometrics movement at large, for the condescending attitude of its disciples toward the old economic historians (Kirkland 1967).

Econometrics and statistical analysis, familiar techniques to the economist, were foreign, unknown, and intimidating to many historians, thus they became easy targets, and the focus for criticism. “Economic analysis, using the tools of econometrics, is not enough and by itself

capable of explaining causatively the process and structure of change and development. Political, social and legal historians, examining institutions, and social philosophers and sociologists, theorizing about them, have much to contribute. The hunt for invariant law in history – to explain the past, manipulate the present, and predict the course of the future – has all the dangers of a fall into a deep and possibly bottomless pit.” (Hacker 1966, p 175)

There was a difference in method between new and old economic historians that could not be ignored. But the schism was not just about methodology. It was also about orthodoxy. Cliometricians were using their new tools to “rewrite history” by overturning some long-held beliefs. Among the accepted wisdom they overturned was that railroads were indispensable to economic growth (Fogel, 1964a), they were built ahead of demand (Fishlow, 1965), that President Jackson caused the financial panics of the 1830s (Temin, 1969), and that slavery was unprofitable (Conrad and Meyer, 1958).

Andreano (1970) collected a series of articles originally published in *Explorations in Entrepreneurial History, Second Series* that he felt reflected dialogue that had been taking place during the 1960s between economists and historians on the methodology of the “new” economic history. But the first attempts to bring together a body of work representative of the “new” economic history were the publication of *The Reinterpretation of American Economic History* (Fogel and Engerman 1971), and *American Economic Growth: An Economist’s History of the United States* (Davis et al 1972).

Cliometrics was clearly not universally embraced, but its potential could not be ignored. Hughes and Reiter compared the computational effort it took them in their steamships paper (1958) to that of Newmarch (1857), who compiled more than 13,000 individual pieces of information and then performed a mere three arithmetical calculations, but all by hand. His efforts represented a lifetime of work, while the steamship paper was but one of many “big data” projects that Hughes and his colleagues at Purdue could explore with the power of new techniques and technology. For example, they cited four additional data-processing studies in economic history carried out at Purdue in the late 1950s that had developed entirely new statistical series, and could not have been conducted without the latest technology or mathematical models: Lance Davis’s textile studies (1957, 1958 and 1960), and the Davis and Hughes exchange rate study (1960).

Cliometrics began as a small movement with a band of devoted and talented disciples. It required a broader stage to take over the discipline, and the *Journal of Economic History* provided just such a stage when Douglass North and William Parker took over as editors in the 1960s. Robert Whaples (1991) found that from 1956-60 ten percent of all articles published in the JEH articles were “cliometric.” Over the next twenty years, six of which were under the North-Parker editorship, that percentage climbed steadily to 72%.

By the end of the 1960s the battle was over and the results were clear: economics was a “science,” constructing, testing, and applying technically sophisticated models, and the new economic historians fully embraced it. The awarding of the 1993 Nobel Prize in Economics to Robert Fogel and Douglass North put the final stamp of approval on the evolution of economic

history from the “old” to the “new.” The “old guard” faded in influence and their followers retreated to history departments.

***The Curse of Clio* (Diebolt and Hauptert 2019a, 2019b)**

Critics of the new economic history criticized them for throwing in their lot with the econometricians. Cliometricians turned to the collection and accumulation of historical data and used the latest econometric techniques to test their hypotheses about economic activity. While this brought economic history into the mainstream of economics as it was developing, in some ways it also contributed to the demise of economic history positions and courses. To non-historians it appears that economic history is little more than the application of economic theory to historical data.

As noted, cliometrics has had its share of detractors, and they continue to make their objections known. In its early days, the conflict was between the “old” and “new” practitioners of economic history. But even within the clio family there were words of caution, lest the movement get carried away. No less a figure than Douglass North (1965) cautioned early on, when the movement was just in its ascendancy, that too much emphasis was being put on econometric technique as a substitute for theory and imagination. He criticized the resulting work as dull and unimaginative. Jonathan Hughes (1966) warned that cliometrics would be unkind to those who confused the means with the end in the pursuit of historical understanding. And Lance Davis (1968), though praising the new economic history for its contributions to both disciplines, argued that its greatest failure lay in the rush to apply theory, even if irrelevant, to a historical issue, or even worse, to a handy data set without understanding its historical context.

More recently, its ascension to the dominant form of economic history has led to a rift between economists who practice cliometrics and historians who practice economic history without the use of formal models. These “traditional” historians, who closely resemble the “old guard” in their methodology, argue that cliometricians fail to see the forest for the trees, as they have become too enamored of statistical significance at the cost of contextual relevance. Boldizzoni (2011) attacked cliometricians for what he perceived as their singular focus on the quantification of history to the exclusion of its humanity. Alex Field (1987) cited problems from economic theorists, who questioned the value cliometricians brought to departments facing declining resources. They felt they could do without a specialist in economic history when anybody could apply theory to old data. Economic historians appeared to be theorists with a more limited focus. William Parker (1986) foreshadowed this problem when he observed that what was lost with the adoption of theory and econometrics was the humane interest of the old British political economy and social welfare and the concern for the whole society, the big picture of the idealistic German historical economists.

Before cliometrics, it was the fate of economic history that was bemoaned, so the slings and arrows hurled at the cliometrician are not unprecedented. Economic history, no matter the form in which it is practiced, has been written off many times before. Norman Gras was an early

and persistent doomsayer. He wondered whether economic history was falling into disfavor because “historical economics has become discredited, or because the statistical method as applied to historical data has failed.” (Gras 1920 p 222) Ten years later he gloomily summarized the state of economic history as being neglected by universities who regarded it as a special subject, but one suffering a lack of intellectual resilience. (Gras 1931)

A generation later, Hugh Aitken, questioning the veracity of the new economic history movement, warned that “economic theory today, in most of its branches, neither draws on economic history for its data nor goes to economic history for empirical verification. Economic history, for its part, commonly uses only the crudest of the tools in the economist’s tool-box, and displays almost complete indifference to the refinements in analytical methods that occupy the theorist’s working time.” (1960 p 87) According to Robert Solow, things had not improved much over the next quarter century. He expressed an equal degree of pessimism, lamenting that the recent work in economic history left him with “the sinking feeling that a lot of it . . . gives back to the theorist the same routine gruel that the economic theorist gives to the historian. Why should I believe, when it is applied to thin eighteenth-century data, something that carries no conviction when it is done with more ample twentieth-century data?” (1985 p 330)

The criticism continued into the 21st century, and the refrain was familiar, that the cleft created by cliometrics between history and economics put “the field of economic history . . . in deep trouble . . . from both history and economics, it is in dire straits in each of these disciplines.” (Coclanis and Carlton 2001, p 93) Paul Hohenberg, concurred, warning that “our discipline is not exactly prospering . . . Why [it] is struggling is no secret: the underlying disciplines of economics and history have diverged sharply.” (2008 p 340)

Economists and economic historians alike have been predicting the demise of economic history for a century now, yet it is still here. While its extinction appears to have been staved off, the fact that it has not yet succumbed does not mean it is immortal. It is resilient, but does face some significant challenges, despite the fact that it may be more widespread now than ever.

The disappearing economic history course

While economic history is gaining wider support both within and outside of the economics discipline, there is some concern that the tools of their trade are not being taken seriously. Economic history is more than the mere application of theory to historical data. Much more than that, it is the empirical extension of the theory that all economic graduate students are taught. It is the addition of history, institutions, demography, geography, politics, and religion to economic models in the on-going effort to understand how and why economies grow. These dimensions are often missing from theoretical growth models, but have proven critical to understanding how economies work. And if they are not being taught to graduate students, the fear is that they will not be carried on. (Diebolt and Hauptert 2019b)

Recent scholarship has highlighted the decline in the ranks of economic history positions and course requirements at leading PhD granting institutions. Two examples will suffice to illustrate the problem.

Graduate education at MIT had long been based on a sort of three-legged stool approach, consisting of theory, econometrics, and economic history. Today the three legs are still there, but they are micro theory, macro theory, and econometrics. (Temin 2016) Economic history has not only been eliminated as a requirement for graduate education, it has ceased to exist altogether. The 2018-19 MIT course catalog listed 46 courses in the graduate curriculum. Among them were six statistics and econometrics courses, four on micro theory, and none in economic history.

A 2005 survey (Diebolt and Hauptert 2019b) indicated that 7.1% of self-identified economic historians had earned their PhD at the University of Chicago, second to Harvard, which had produced 7.4% of economic historians. Like MIT, by 2018 the University of Chicago no longer required a field course in economic history at the graduate level, though it did offer one as an elective. The decline of economic history at Chicago has been dated to the early 1980s, when the required economic history course was eliminated. A decade later the percentage of graduate students writing in the field had decreased, and the economic historians on the faculty shifted their research into other fields. (Mitch 2011)

MIT and Chicago are not the only examples of disappearing economic history courses at leading doctoral programs. By 2020, fewer than half of the top 50 graduate programs offered any economic history course, only 11 offered it as a field of emphasis, and eight required it as part of the core curriculum. (Diebolt and Hauptert 2019b) These developments stand in stark contrast to the proclamation once made by Rondo Cameron that “the vast majority of professional economists are trained in graduate schools that require their students to take course work or examinations in economic history.” (Cameron 1965 p 112) The view that economic history is a useful tool, and that the research of its practitioners offers value and insight into the most pressing issues of the day has not translated into the belief that it is important to pass along its skills and tools in graduate programs.

The disappearance of economic history as a field of study is due to the attitude of some economists that it is only useful if it can shed light on present conditions. “This is unfortunate and we can (and should) keep arguing that this is a narrow view of social science.” (Abramitzky 2015, p 1242) We ignore history at the risk of missing its many important contributions. Economic history “accustoms the student to the inductive method of thinking; to the practice of generalization, and to the realization of the multiplicity of causation; and all this in an environment not of assumption but of recorded fact. It is precisely as a half-way house between the abstract and the concrete that . . . [it] is useful.” (Ashley 1927, p 9)

Whither the future?

The use of history as a crucible to study economic theory has advanced the frontiers of our knowledge and understanding of how, why, and when economic change occurs. Cliometrics spawned two now common approaches to study economic history. Douglass North (1990, and with Davis 1971) pioneered the new institutional economics, carried on quite successfully by Daron Acemoglu (2015 and et al 2001, 2005, 2008a, 2008b), among others. Robert Fogel (1986) was an early and influential practitioner of anthropometrics. He was also at the forefront of the gathering of historical primary datasets that were essential for the large-scale data-driven studies he pioneered in the era before “big data” became ubiquitous. North and Fogel were recognized for their contributions to the study of economic history, especially cliometrics, with the Nobel Prize in Economic Science in 1993.

There are several promising and innovative applications of cliometric techniques that have evolved as new datasets, new techniques, new approaches, and enhanced computer power have been discovered. Counted among them are experimental economics, geocoding, text recognition software, and linked census data. Novel innovations and approaches, applied to newly created and digitized data, suggest a promising future for economic historians.

Cliometricians have borrowed experimental economic techniques to use history as a laboratory in which to perform natural experiments. Such historical experiments allow economists to get around the hurdles that impede empirical economic experiments. Jeremiah Dittmar’s (2011) study of the impact of the printing press on economic development was an early example. Other studies have explored historical questions with more traditional experimental economics techniques (Wilson, et al 2012). Cantoni and Yuchtman (2020) provide an overview of the intersection of economic history and experimental economics.

Advances in computing technology have added many techniques to the economic historian’s toolbox. One popular example is geographic information systems (GIS), which makes possible the analysis and display of data along spatial dimensions. This allows for the combination of standard economic history issues of growth and development with the social and environmental aspects of geography. Jeremy Atack (2013, 2019) and Donaldson and Hornbeck (2016) used GIS to study the American railroad industry. Kasakoff et al (2013) and Hedfalk et al (2017, 2018) used geo-coding for historical demographic analysis, and St-Hilaire et al (2010) and Lan and Longley (2019) applied it to census studies. Goldberg et al (2007), Cura et al (2018), Weinberg (2018), and Ploeckl (2019), all provide overviews of the wide range of uses of spatial modeling to analyze historical data. The technique is popular and powerful enough that the London School of Economics has created a new program of study in economic history and geography.

Economic historians of all stripes have stood out from their peers in the discovery and creation of databases, especially of the long run variety. These databases, which can contain millions of observations, make it possible to study the evolution of the economy over time. As well, they have shed new light on previously held beliefs, which has caused us to reevaluate what we thought we knew. Slavery was indeed profitable, the railroad was not the driver of economic growth, and the American south grew faster than we thought.

The construction of these datasets is an admirable contribution to our ability to understand economic growth. In combination with the rapid growth in computing power and more sophisticated theoretical models, the significance of these data sets has been compounded, especially when they can be linked over time and across other data sets to create the historical equivalent of “big data.” The ability to handle “big data” is not itself an economic issue, but the construction of significant long run data sets, which can then be used to test economic theories using the latest econometric techniques, is very much a contribution of cliometrics. (Diebolt and Hauptert 2019d)

Economic historians have exploited the marriage of the latest analytical techniques with large historical data sets in creative ways. Abramitzky, et al (forthcoming) test various automated methods for linking the digitized versions of the complete count US census records to create large scale longitudinal datasets linking individuals over time. They identify several reliable methods that allow for the exploitation of these enormous databases that will continue to grow as future censuses are released. Bailey, et al (2019) use automated methods to combine census data with marriage and birth records to create a multigenerational social database in their Longitudinal, Intergenerational Family Electronic Microdata (LIFE-M) project. Since 1992 the University of Minnesota’s Integrated Public Use Microdata Series (IPUMS) has made social and economic data integrated over time from more than 100 countries publicly available for historical research. They have created one of the largest accessible archives of census microdata in the world, containing over a billion records. (Ruggles, et al 2020)

Computing power has also been harnessed to study non-numeric data. Large-scale qualitative databases can be used to analyze text. Wehrheim (2019) displays the usefulness of topic modelling to analyze the contents of some 2700 articles in the *Journal of Economic History*. Topic modelling infers the content from large databases of text. Borowiecki (2017) uses linguistic analysis software to construct a well-being index to study the emotional well-being of 18th and 19th century composers and its effect on their music. The increasing availability of digitized qualitative data sources hints at the limitless possibilities for future research.

Economic history is a multidisciplinary field that uses tools from and spreads its influence to the other social sciences as well as other sub fields of economics. More than 70 years ago John Nef argued that “any attempt to separate the economic side from the rest of life leads to a narrow view of history,” (Nef 1944 p 16) likening the work of economic historians to that of a hunting ground for other social scientists. Half a century later, Christine Romer argued that “the field of economic history is no longer a separate, and perhaps marginal, subfield of economics, but rather, is an integral part of the entire discipline.” (1994 p 49)

Economic historians have always had, and continue to have an impact far beyond their own discipline. Diebolt and Hauptert (2019a) demonstrate this by looking at the wide range of non-economic history journals citing the research of economic historians and the increasing number of journals that publish their work. Their findings are in line with those of earlier studies (McCloskey 1976, Abramitzky 2015) citing the large and persistent percentage of citations and publications of economic history articles in the wider economics literature.

Cliometricians have made an indelible mark on the field of economic history narrowly considered, as well as the larger economics discipline. It is not their use of theory or cutting edge econometrics which has set them apart. Indeed, as Margo (2018) has shown, economic historians have lagged behind other sub fields of economics in their adoption of new techniques. Rather, it is the combination of theory, econometrics, and the use of time as a variable that has been the calling card of the cliometric approach. The inclusion of time makes it possible to study the growth and evolution of nations, institutions, and markets over the long term. The attention that economic historians give to noneconomic factors, such as legal and political systems, religion, and culture, is what distinguishes them from economic theorists. The longer time span that economic historians consider, allows for the study of how institutions evolve, and their impact on economic growth.

Economic history provides more and better economic facts, and does so over a longer period of time and greater variety of institutional settings than any other field of economic study can provide. The practical value of historical scholarship is not necessarily in its direct or immediate application. It is, rather, an indispensable part of the combined labor of the social sciences.

Conclusion

John Nef long ago recognized the unique challenge faced by economic history when he prescribed a solution to what he saw as its declining relevance. "What economic history should become is an instrument for reducing rather than for increasing the number of compartments into which scholarship is now divided." (1944 p 15) Seventy years later this sentiment was echoed by William Collins. Speculating on the future of economic history, Collins predicted "that the boundaries of economic history, which have always been permeable, will grow less distinct." (2015 p 1232) To paraphrase Deirdre McCloskey (1976), the past does indeed have useful economics, and economic historians deliver that message.

If the Great Recession of 2008, and the Covid-19 pandemic of 2020 have taught us anything, it is that we need to listen to people who know and understand what's happening in our economy. These are usually the same people who understand our history, for "those who cannot remember the past are condemned to repeat it." (Santayana 1905 p 284) Economic history crosses the boundaries of academic fields of study more frequently than any other field of economics. It is used to study issues, institutions, and entities across geographic borders and over the expanse of time. Cliometrics may have superseded traditional economic history in its practice, but at its core, it answers the same questions, faces the same hurdles, and offers the same advantages as it has since its inception. Its persistence over more a century and a half is testament to its value. While the ability and willingness of its practitioners to adopt the best tools and viewpoints from across the economics discipline has created growing pains, this is the very thing that has kept it relevant. Despite predictions of doom, criticism from all sides, and periods of decreased demand from economics departments, economic history has prevailed. It does not look like it did at its inception, and it may not look the same a generation into the future, but it will still be with us.

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