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## « **Clometrics** »

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# Cliometrics<sup>1</sup>

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**Abstract:** Cliometrics has been defined and summarized in numerous scholarly articles. They all pretty much start with the obvious, that cliometrics is the application of economic theory and quantitative techniques to study history; and then move on to the origin of the name, the joining of Clio (the muse of history), with metrics ("to measure," or "the art of measurement"), allegedly coined by economist Stanley Reiter while collaborating with economic historians Lance Davis and Jonathan Hughes.

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## *Introduction*

Cliometrics has been defined and summarized in numerous scholarly articles.<sup>2</sup> They all pretty much start with the obvious, that cliometrics is the application of economic theory and quantitative techniques to study history; and then move on to the origin of the name, the joining of *Clio* (the muse of history), with *metrics* (“to measure,” or “the art of measurement”), allegedly coined by economist Stanley Reiter while collaborating with economic historians Lance Davis and Jonathan Hughes.<sup>3</sup>

Cliometrics has had a profound impact on economic history.<sup>4</sup> It is responsible for transforming the discipline from a primarily narrative to a mathematical approach. This transformation has combined theory with quantitative methods, new and revised databases, and innovative techniques, expanding our knowledge of the process of economic growth.

The culmination of this metamorphosis occurred in 1993 when *Clio* pioneers Robert Fogel and Douglass North received the Nobel Prize in Economic Science. The committee recognized them for having renewed research in economic history by applying economic theory and quantitative methods in order to explain economic and institutional change, making it “possible to question and to reassess earlier results, which . . . not only increased our knowledge of the past, but . . . contributed to the elimination of irrelevant theories,”<sup>5</sup> showing that the traditional understanding of economic growth and development needed modification.

The rise of cliometrics has fostered much debate about the proper balance between economics and history. In 1971 Gavin Wright surveyed the development of cliometrics, arguing that it was more about the use of economic theory than the use of econometrics in the study of history.<sup>6</sup> Deirdre McCloskey agreed, defining a cliometrician as an economist who applied economic theory to history. She claimed that it was not the sophistication of the model, but the use of a model at all that differentiated this “new” economic history from the old.<sup>7</sup> A decade earlier Thomas Cochrane portrayed the difference between the traditional practice of economic history and the new cliometric movement as a “controversy over types of models: the old say that realistic models usually have to be too highly generalized or too complex to allow the assumption of mathematical relationships; the new are primarily interested in applying operative models to economic data.”<sup>8</sup>

Contrary to the perceived divergence of economists and historians, the skills of a cliometrician include, and indeed require, those of both the economist and the historian.<sup>9</sup> Well

before the dawning of cliometrics, Edwin Gay, in his inaugural presidential address to the Economic History Association, preached that economic historians needed to wed the skills of economists with those of historians in order to accomplish their task. He believed such a union was essential, but difficult to accomplish.<sup>10</sup> That has not changed over the past three quarters of a century. What has changed is the degree to which those economic skills have become more formalized and technically demanding.

But Joseph Schumpeter warns us not to forget history amidst the attention paid to the technical and mathematically attractive precision of models. He argued that “scientific” economists must command the techniques of statistics, theory, and history. And, he declared, history is “by far the most important.”<sup>11</sup>

Today the use of cliometric tools to study economic history is a foregone conclusion, though the Cliometric Society itself certainly has not displaced other economic history groups. In fact, the Cliometric Society is one of the smaller societies devoted to the craft of studying economic history. It still sponsors an annual conference that is unique in its construct, sponsors sessions at several other conferences around the world, and publishes a newsletter and maintains a membership numbering a few hundred. But it is the cliometric approach to economic history that has overtaken the discipline. Cliometrics is much more than just an academic society. It is a new approach to the study of economics and history.

#### *The origin of cliometrics*

In December of 1960 the “Purdue Conference on the Application of Economic Theory and Quantitative Techniques to Problems of History” was held on the campus of Purdue University.<sup>12</sup> It is recognized as the first meeting of what is now known as the Cliometric Society.<sup>13</sup> While it was the first formal meeting of a group of like-minded applicants of economic theory and quantitative methods to the study of economic history, it was not the first time such a concept had been broached, practiced, or even mentioned in the literature.<sup>14</sup> Cliometrics was a long time in coming, but when it arrived, it eventually overran the approach to the discipline of economic history, leading to a bifurcation of the economists and historians who practice the art, and the blurring of the distinction between cliometricians (i.e. economic historians) and theorists who use historical data. Indeed, one of the great criticisms of the cliometric movement is the wedge that it has driven between the practitioners of economic

history in history and economics departments (Boldizzoni 2011)<sup>15</sup> due to its focus on quantitative measures and neoclassical theory.<sup>16</sup>

The clash between cliometricians and historians today is not all that different from the clash between economists and historians that began in the 19<sup>th</sup> century. Carl Menger (1884) compared historians to foreign conquerors, complaining that they were forcing their terminology and methods on economists. Half a century later, Ashton (1946) accused those who objected to the idea that economic theory should be applied to history of not truly understanding the nature of economics.

The origin of cliometrics can be found in the origin of economic history, which originated largely as a revolt against classical theory and in its early years it shunned the use of statistical techniques. By the 1920s the attitude toward theory and statistics began to soften. Cliometrics is the continuation of this theoretical-quantitative tradition now nearly a century old, and fortified by advances in economic theory, the melding of economics with approaches from other disciplines, and the growth of computing power. The latter has had profound impacts on the ability to analyze and disseminate data, and puts cliometrics in a good position to exploit the new “big data.”

#### *The economic history discipline*

Economic history as a formal discipline dates only to the late 19<sup>th</sup> century. Before economic history there were political economics departments and history departments, and neither was a natural home for economic history. Political economics departments tended not to focus on history. And the general approach by scholars trained in history departments in the 19<sup>th</sup> century was to consider economic factors as only one cause of change, and not always necessarily the most important one.<sup>17</sup>

The earliest form of economic history was narration fortified with the occasional bit of quantitative data. When formal economic history began to evolve in Germany and England in the late nineteenth century however, leading scholars such as Gustav Schmoller in Germany and Sir John Clapham in England 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 for more than half a century. Data were

only occasionally collected, and when they were, they were seldom analyzed or used to test mathematical propositions, and economic models were practically unknown.

By the 1870s political economy had devolved into a methodological debate about whether economics should be inductive (develop theories providing evidence of the truth) or deductive (gather facts leading to a certain conclusion). Economic history emerged as a distinct discipline during the course of the revolt against the deductive theories of classical economics. 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. They held that history was the key source of knowledge about humans and human organizations, and because it was culture and time specific, it could not be generalized over time or space, hence general theories were useless. Their view was that economics was best approached from the vantage point of empirical and historical analysis, not abstract theory and deduction. The historical school was a reaction against abstract theory, and it was highly critical of the method, fundamental assumptions, and results thereof.

At the dawn of the 20<sup>th</sup> century it appeared that the attempt of the historical school to replace deductive theory with inductive theory had failed. In fact, the economics discipline was moving toward a more deductive approach. The movement to turn economics into a science, which grew out of the rising stature of the natural sciences, gave way to a new understanding that for economics to take its place at the pinnacle of the social sciences, it needed to formalize and rely more on mathematical models.<sup>18</sup> This ushered in a waning of the historical movement, and a low point for the influence of the discipline.

After the First World War, economists became less theoretical and more statistical in their approach. The creation of the National Bureau of Economic Research (NBER) was a result of this new emphasis. This movement brought economists and historians a bit closer together. As an added benefit, it forced historians of all stripes to be less tolerant of loose, unsupported generalizations. The culmination was the creation of the first dedicated economic history society. The Economic History Society was organized in the UK in 1926, followed in 1927 by the first dedicated economic history journal, the *Economic History Review*.<sup>19</sup>

American academics were from an early day interested in data. The *American Statistical Association* was launched in 1839, its membership consisting of individuals who paid serious attention to compiling time series data. 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 censuses had flourished from 1790 onward, with attention to economic measurements increasing after 1850.

Harvard was the incubator of economic history in the US, and became the first university in the world to create a chair in economic history when they hired William J. Ashley in 1892. Ashley (1927) argued for a course in economic history alongside the general economic theory (i.e. political economy) course. Later in his career he promoted statistics, which he felt would become an integral part of every important economics department.

Wesley C. Mitchell 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. Together with Edwin Gay, the successor to Ashley as Harvard's chair in economic history, Mitchell helped found the NBER to stimulate the collection and interpretation of historical statistics.<sup>20</sup> His vision was to improve society through the use of expert analysis and statistical investigation.

He combined his historical approach to understanding business cycles, which he saw as a global phenomenon, with an urgent call for more data collection from around the world. The NBER was central to this data collection effort and served as a sort of haven for statistical economists. The mission of the NBER was to gather empirical information about the American economy in order to create a robust foundation for theoretical generalizations. The NBER ultimately served as a catalyst for the change in emphasis from narrative to quantitative studies in economic history. Over time, economic history presented itself as empirical and multidisciplinary. Empirical in that it dealt with the facts of the past. The facts could be quantitative, as the NBER emphasized, or qualitative. It was also empirical in that economic historians saw history as a laboratory where they could test economic hypotheses.

By 1941 Gay felt that the work of the historical economists had not been able to displace the "theoretical school," but did modify it. He called for the reunification of economic history and theory, noting that the economic historians knew a great deal about the long trends of productive energies and social pressures leading to economic growth, which could be combined with the tools of the theorist to lend greater insight to the growth process. Far from incompatible, he felt that true philosophical objectives and the careful assembling of data were complementary. Others argued that there was too much work in economic history that was a haphazard gathering

of facts with little appraisal of whether they actually shed any light on economic development. This group argued for the employment of the most efficient theoretical tools at the disposal of the economic historian.<sup>21</sup> Even as the EHA was in its infancy, formed in part as a defense against the encroaching “mathematicization” of the discipline, the seeds of the cliometric movement were being sown. It would be the next generation of economic historians who propelled that movement forward.

### *The New Economic History Movement*

New Economic History, or Cliometrics, is a move from the historical, descriptive approach of *describing* a historical event, toward the use of economic theory to *analyze* an event. The first practitioners of the art of cliometrics “proposed that economic historians use the techniques and insights of modern economic theory to frame the questions asked of history, to influence the hypotheses advanced about the past, and to suggest the nature and type of data to be collected from the archives.”<sup>22</sup> Further, they advocated for the rigorous testing of any hypotheses advanced against the alternatives, particularly those found in the “old” economic history. This would require the collection of data and its analysis using econometric techniques.

Arguing against those who cliometricians would later label “old” economic historians, Simon Kuznets claimed that little would be gained from a study of the past unless it was systematic and quantitative. In his view, this was the only way to weigh the relative effects of factors and events. The reason for the scarcity of quantitative work in economic history was due to the extraordinary effort necessary before the computer to sift and classify quantitative information, and the relatively recent development of statistical theory and techniques capable of handling these problems.

After WWII, with the American economy booming, economists gained cachet. Economics with its rigorous models, tested from an abundance of numerical data by use of advanced, mathematically expressed formulae, came to be regarded as the paradigm of the social sciences.

At the same time as this increasingly technical focus, economists were increasingly interested in the determinants of economic growth and what they saw as the widening gap between developed and underdeveloped regions of the world. They saw the study of economic history as a source of insight into the issues of economic growth and economic development, and the new quantitative methods as the ideal tools for analysis.

The generation of economists who were trained in the post-war decades found ways to mesh mathematics and economics, although the idea that economics should appropriate ideas from mathematics was itself contested, especially by economic historians. By the 1960s the battle was over and the results were clear: economics was a “science,” constructing, testing, and applying technically sophisticated models. Econometrics was on the rise and economic historians were divided between those who abhorred it, and those who embraced it. The former faded in influence and their followers retreated to history departments.

The “new” economic history can be dated to the 1957 joint meeting of the EHA (founded in 1940 by “old” economic historians like Edwin Gay) and the Conference on Research in Income and Wealth (under the guidance of the NBER). In particular, two joint papers by Alfred Conrad and John Meyer (1957 and 1958) constituted the manifesto for the new era. The first paper, on methodology, explained what scientific method was really all about and how it applied to economic historians. The second paper has been called one of the most influential in the evolution of economic history.<sup>23</sup> It added enormous force to the methodological prescription by claiming to follow it in an analysis of the profitability of slavery on the eve of the Civil War. The analytical method, the data, the economic and accounting framework, and the choice of slavery as a subject, were to have vast consequences for the next generation of economic historians.<sup>24</sup>

The formal organization of the Cliometric Society would not occur until 1983, under the guidance of Sam Williamson and Deirdre (then Donald) McCloskey. But that 1957 conference begat the smaller gathering at Purdue University in 1960 that has since become the annual conference of the Cliometric Society. The conference preceded the society proper by nearly a quarter century. At the celebration of its golden anniversary in 2011, the Cliometric Society named its first class of Fellows, bestowing the recognition of lifetime achievement on economic historians for the first time.<sup>25</sup>

De Rouvray (2004b) argues that the timing of the cliometric movement corresponded to the success of Kuznets’s quantitative growth studies, a reflection of the infatuation economists had developed for the national accounting approach. This predisposed them to view the past through this same lens and altered their definition of historical evidence.

Kuznets may have inspired the cliometric movement, but it was Robert Fogel who reunified economics and history. He used the latest techniques of modern economics and gathered reams of historical data to reinterpret American economic growth in sectors as diverse

as railroads, slavery, and nutrition. Rather than conjecture about the causes of growth, he carefully measured them. He pioneered the use of large-scale cross-sectional and longitudinal data sets harvested from original sources to examine policy issues.

The cliometric revolution pitted young turks, outsiders, “theorists” as they were called by the old timers, against those “old” economic historians who were more likely to be historians and less likely to rely on quantitative methods. They accused the newcomers of bringing economic theory to history without a proper understanding of the facts (a familiar battle cry).<sup>26</sup> The old guard claimed that realistic models had to be too highly generalized or too complex to allow the assumption of mathematical relationships. The “new” economic historians, however, were primarily interested in applying operative models to economic data. There was a difference in method between new and old economic historians that could not be ignored. The models preferred by the new economic historians were quantitative and mathematical, while those used by “sociological economic historians” tended to be narrative.

The schism was not just about methodology, but also orthodoxy. Cliometricians were using their new tools to overturn 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).

#### *Major Contributions of Clio*

Clio’s moment in the spotlight, or fifteen minutes of fame, as Sam Williamson (1994) coined it, came at the 1964 AEA meetings. William Parker organized a session on “Economic History: It’s Contribution to Economic Education, Research, and Policy,” featuring papers by Douglass North (1965), Robert Fogel (1965), Barry Supple (1965), Richard Easterlin (1965), Robert Gallman (1965), and Rondo Cameron (1965), with comments by Evsey Domar and R. A. Gordon (1965). The session drew a crowd estimated at 200, generated lively discussion, and put cliometrics in a national spotlight that it had never previously experienced.

Fogel (1964b) highlighted the changes in economic history that justified its being “new.” It was not a change in subject, they still remained interested in the description and explanation of economic growth. It was the approach to measurement and theory that was new. Economic history always had a quantitative dimension. But much of the past work had been limited to the simple organization of data contained in government and business records. While continuing this

pursuit, the new economic history placed its primary emphasis on reconstructing measurements and organizing primary data in a manner allowing them to obtain measurements that were never before possible. It thus followed that the most critical issue in the work of the new economic historians was the logical and empirical validity of the theories on which their measurements were based.

The new economic historians made use of the whole gamut of economic theory and statistical models, and the measurements they obtained yielded considerably more precise information than previously available. The perfect example of this was Fogel's railroad study, which will be discussed in greater detail later.

In general, the main achievement of cliometrics has been the establishment of a solid set of economic analyses of historical evolution by means of measurement and theory, and, following the path blazed by Douglass North, to recognize the limits of neoclassical theory and bring into economic models the important role of institutions. Indeed, this latter focus ultimately spawned a new branch of economics altogether, the new institutional economics. Nothing can now replace rigorous statistical and econometric analysis based on systematically ordered data. More specifically, the contributions of cliometrics can be placed in four general categories: new techniques, new data sets, revisions of previously held beliefs, and new approaches.

#### *New Techniques*

Technique is what many people think of when they hear the term cliometrics. Certainly the advancement of econometric theory and computing power have contributed greatly to the techniques used by cliometricians. In the early issues of the *Journal of Economic History* the appearance of an equation was rare. As the "new" economic history took hold under the editorship of Douglass North and William Parker in the early 1960s, equations and the occasional OLS regression began to appear regularly. Today, that occasional OLS regression has been replaced by the latest econometric advancement. It is now a rare article in the *JEH* that does not rely on the latest econometric advances as part of its analysis. However, technique is not merely mathematical sophistication at its utmost. One of the earliest and still useful techniques available to the cliometrician is the counterfactual.

Counterfactual analysis is the idea of determining the impact of an event or factor by considering what would have happened in its absence. Fogel was not the first to use this form of identifying opportunity costs,<sup>27</sup> but he was the most extensive user of it and became famous for

his use of the technique in his landmark railroad study. He attempted to estimate how much less developed the America economy would have been had there been no railroads. Although historians were familiar with counterfactual arguments, the idea of an explicit counterfactual measurement was still a foreign notion in the early 1960s.

The concept of social savings is itself an important research tool, but Fogel greatly advanced its significance by defining it operationally, so that his calculations could be tested against alternative estimates and possible alternative definitions.

The most frequent criticism of Fogel's work was the counterfactual itself. Historians were wont to ask questions about hypothetical history, preferring to focus on events that actually occurred. As Edward Kirkland complained, "Readers are bound to be distracted when they wake up in a world neither they nor any other American, except Fogel, ever made. . . The development he describes is not "what actually happened in the past."<sup>28</sup> Rather, he opens "a new branch of literature, quite unlike what has hitherto passed as historical knowledge and somewhat more analogous to science fiction."<sup>29</sup>

Despite their early rejections of the concept, some historians came to embrace the counterfactual as well. In Robert Cowley's *What If? Eminent Historians Imagine What Might Have Been*, several historians used the counterfactual approach in a series of essays to demonstrate how small changes in events could have possibly altered the course of history. These ruminations followed the spirit of the counterfactual model, if not the precision of the Fogel version. Their focus was on asking how history might have been changed if singular events had been different.

Niall Ferguson distinguishes between two types of counterfactual models: "those which are essentially products of imagination but (generally) lack an empirical basis; and those designed to test hypotheses by (supposedly) empirical means."<sup>30</sup> He defends the second, which most closely aligns with the clio use of the art of counterfactual history.

Other techniques popularized by cliometricians include age heaping models and the use of church book registries. Age heaping can be applied to approximate the basic numerical skills and hence basic education of a population, and its impact on a variety of variables, including the impact of numeracy on long-run growth (Acemoglu, *et al* 2001, 2002), the role of religion in human capital formation (Becker and Woessmann 2009), gender inequalities (De

Moor and Van Zanden 2010, Manzel and Baten 2009) and labor market outcomes (Charette and Meng 1998).<sup>31</sup>

Fogel defined the methodological features of cliometrics. He considered it fundamental that cliometrics should stress measurement while recognizing the existence of close links between measurement and theory. Indeed, unless it is accompanied by statistical and/or econometric processing and systematic quantitative analysis, measurement is just another form of narrative history. It is true that it replaces words with figures, but it does not bring in any new factors. In contrast, cliometrics is innovative when it is used to attempt to model all the explanations of past economic development. In other words, the main characteristic of cliometrics is the use of hypothetico-deductive models that call on the closest econometric techniques with the aim of establishing the interaction between variables in a given situation in mathematical form.

#### *Compilation of data sets*

It is the lack of relevant data more than the lack of relevant theory that is often the greater problem in research. In this way, cliometricians have made some of the greatest contributions to the fields of economics and history by discovering and compiling new data sets that can then be used by future researchers to better understand the evolution and growth of economies over time.

The accumulation of the data is in itself monumental in many respects, but its usefulness has been expanded by the rapid growth of computing power. The ability to handle “big data” is not a cliometric issue by itself, but the construction of significant, important historical data sets, which can then be analyzed using the latest econometric techniques and computer programs, is very much a contribution of cliometrics.

The marriage of cliometrics and big data is a natural one, and has been exploited by economic historians in new and creative ways. The work of James Feigenbaum (2015) is one recent example. He uses new automated linking methods to manage mammoth volumes of census data. In less obvious ways, large-scale qualitative databases are now being used to analyze text,<sup>32</sup> and GIS mapping allows geographic data to be “quantified.” On a broader level is the Integrated Public Use Microdata Series (IPUMS), which provides census and survey data from around the globe in easy to use formats for a broad range of research on economic, social, and health research topics. IPUMS USA collects, preserves and harmonizes U.S. census

microdata and provides easy and free access to the data, which includes all available census data and 21<sup>st</sup> century American Community Surveys.

The collection of data has been cataloged at sites such as EH.net, MeasuringWorth.com and the Global Price and Income History Group, just to name a few. EH.net hosts a large and growing variety of databases, including the historical labor statistics project series, a collection of detailed data on American labor markets; the U.S. Government Bond Trading Database, which describes a large data set of US government bond trades; rates of return to UK home and overseas investments; Confederate note prices; developing country export statistics; U.S. securities prices, U.S. customs house data, national bank notes, U.S. public debt, French wheat prices in the 19<sup>th</sup> century, and 17<sup>th</sup> and 18<sup>th</sup> century New England probate samples. The Global Price and Income History Group has gathered vast quantities of data on prices and incomes for the period prior to 1950 from around the world. Measuring Worth includes series for real and nominal GDP for the US (since 1790), UK (since 1300), Japan (since 1879), China (since 1952), wages, price indices, daily closing values of the Dow Jones since 1885, interest rates, and exchange rates. And this is only a partial list.

### *Revisions*

Revisionist history is not a complimentary term, but the revision of misunderstandings in history is certainly both important and necessary, not just for the reason of setting the record straight, but helping us understand how economies grow and why some lag behind. Developing a clear understanding of the causes of economic growth is among the most important tasks of an economic historian. Cliometrics has overturned some accepted wisdoms and in the process caused hard feelings, resentment, and controversy. However, they have also pushed forward the frontier of our understanding of economic growth and development.

Among the notable “revisions” made by cliometricians were the findings of Conrad and Meyer (1958), Yasuba (1961) and Sutch (1965), who used capital theory models to determine that slavery was indeed a profitable investment. Easterlin (1961) used revised GNP figures to show that income in the antebellum South grew at a faster rate than previously believed, and Fogel (1964a) showed that the railroad was not the determinant of American economic development that it was believed to have been.

In *Time on the Cross: The Economics of American Negro Slavery* (1974), Fogel and Stanley Engerman treated slavery as an institution and examined its role in the economic

development of the United States. They showed that the established opinion that slavery was an ineffective, unprofitable, and pre-capitalist organization was incorrect. They argued that slavery did not fall to pieces due to its economic weakness but collapsed because of political decisions, and that in spite of its inhumanity it had been economically efficient. This research was understandably highly controversial both within and outside of the field of economics. It attracted considerable attention and generated volumes of research in an attempt to either refute or refine its findings.

Careers have been made attempting to solve the mystery of how and why the Industrial Revolution began when and where it did. Greg Clark (2014, 2015) is among a small group of economic historians who have begun to explore an alternative explanation to the standard institution and market based stories by focusing on demographics, in particular the idea that the economically successful in a society will likely be the demographically successful.<sup>33</sup> More recently, Voitländer and Voth (2013) argue that the Black Death gave rise to a European marriage pattern that in turn set in motion a process that led to the Industrial Revolution, a bold claim that leads to a dramatic revision of the economic history of western Europe.

Another early example, somewhat overshadowed by the railroad and slavery controversies, was Peter Temin's (1964) work on the steel industry. He concluded that it was supply side factors that explained the increased use of coke in pig iron production on the eve of the Civil War. This stood in contrast to the widely accepted theory of Louis Hunter (1929), which had stood for nearly four decades prior to Temin's work.

And then there is Douglass North. In his 1968 ocean shipping article, he famously argued that institutions, not technology, were responsible for the increase in the productivity of ocean shipping from the 17<sup>th</sup> to the 19<sup>th</sup> century. The decrease in piracy and quicker turnaround times in port contributed more to productivity gains than did the heretofore credited technological advances. This revision would ultimately lead North to pioneer an entirely new branch of economics focusing on institutions.<sup>34</sup>

### *New approaches*

Finally, cliometrics has spawned entirely new approaches to the study of economics. Among the more prominent are the aforementioned institutional economics, and anthropometrics, which counts Robert Fogel among its earliest practitioners.

Anthropometrics is the study of patterns in human body size over time.<sup>35</sup> The field has its roots in the natural sciences but came into vogue as a field of study in the social sciences in the 1970s. The work of Emmanuel Le Roy Ladurie (1969) is considered to be the first modern historical application of anthropometrics. Economists took to the field as a means of measuring changes in the standard of living. Cliometricians have used anthropometrics to contribute to research in mortality trends (Fogel 1986, Floud and Harris 1997), slavery (Engerman 1976, Steckel 1979, Margo and Steckel 1982), and the outcomes of industrialization and economic development (Floud and Wachter 1982, Steckel and Floud, 1997, Haines 2004). The genesis of much of this research in the United States was an NBER study on American and European mortality trends coordinated by Robert Fogel in the 1980s. Since then the scope of the field has grown to include countries around the world.

The influence of institutional economics grew throughout the 1980s, spreading across disciplines from economics to anthropology, law, management, political science, psychology, and sociology, among others. The growing interest in the topic, and the frustration its practitioners had in getting their research published and onto conference programs, eventually led to the establishment of the *Journal of Institutional and Theoretical Economics (JITE)* in 1986 and the creation of the International Society for New Institutional Economics (ISNIE) in 1997.<sup>36</sup> The impact of the new institutional economics on the discipline can be summed up simply enough in the number of ISNIE founders who have been awarded Nobel prizes.<sup>37</sup>

Demography is another topic that has been subject to an increased focus by cliometricians, in large part due to the ability to create and analyze large data bases. Federal and state censuses have long been available as sources of big data, but only relatively recently has technology made them accessible for serious research. Among the more notable work in the area is that done by Joe Ferrie. One of his earliest contributions was a sample of males linked from Federal censuses of 1850 to 1870.<sup>38</sup> This has created longitudinal datasets allowing scholars to track the economic and geographic mobility of individuals and families over time. When combined with 20<sup>th</sup> century data compiled from the National Longitudinal Surveys (NLS) and

the Panel Study of Income Dynamics (PSID), Ferrie's data set provides a historical benchmark, and the linked samples provide information on occupation, wealth, family structure, and location for individuals across time.

The construction of longitudinal population databases is not confined to the United States. Pfister and Fertig (2010) created an aggregative reconstruction of the population of Germany from the sixteenth to the mid-eighteenth century. Their estimates of population size and an annual series of crude birth, marriage and death rates were built on partial censuses, parish registers, and the protostatistical material on population size and vital events that states began to collect in the mid-18<sup>th</sup> century. Without modern computing power it would have taken an army of scholars a lifetime just to compile the data, let alone make use of the results.

A project that exemplifies new data sets, new techniques, and new approaches is the Longitudinal, Intergenerational Family Electronic Microdata (LIFE-M). LIFE-M is a large-scale public database that extends from the late 19<sup>th</sup> to the 21<sup>st</sup> century. It uses vital records as a basis for linking with census data from 1880 to 1940, providing birth to death coverage of individuals identified in the census. The combination of birth, death, and marriage records with data across censuses will eventually produce a four generation database, including for the first time substantial numbers of women and minorities, when it is completed.<sup>39</sup>

#### *The Contributions of Fogel and North*

In 1993 Robert Fogel shared the Nobel Prize in Economics with Douglass North for having renewed research in economic history. He was born in New York City in 1926 to Russian immigrant parents, whose reverence for learning encouraged his academic pursuits. He earned degrees at Cornell (BA 1948), Columbia (MA 1960), and Johns Hopkins (PhD 1963). His interest in economic history was precipitated during his undergraduate days at Cornell by the widespread pessimism about the future of the economy in the first years after WWII. As he pursued his post-graduate studies he became aware of how little was actually known about the large processes of economic growth, and he began to focus on more discrete issues, such as the nature and magnitude of the contribution of particular technologies to growth. In order to answer such questions he set out to master the most advanced analytical and statistical methods that were then taught in the economics department. It was only later that he would discover that the training program he had worked out for himself was unorthodox for an economic historian.

While at Columbia he studied with George Stigler (who he would later join on the faculty at Chicago, and succeed as the Walgreen Chair in 1981) and Carter Goodrich, who encouraged him to pursue his doctoral work with Simon Kuznets at Johns Hopkins. Fogel presented a thesis proposal to Kuznets in early 1959 entitled “Notes on the Influence of the Railroads on American Economic Growth, 1830-1890.” Within it he outlined seventeen proposed subjects regarding the railroad, including economies of scale and the population growth attributable to railroads, capital formation in railroads, and comparisons of social savings in other countries. His resulting book, which followed his *JEH* paper, covered only two of the seventeen proposed subjects. He was, even as a graduate student, building a project that would endure for half a century. His self-established fifty-year rule required thinking on a grand scale. During the height of the controversy over his work on railroads he was asked if he thought his work was important. He responded that it would be if it still mattered in fifty years. And if it was to matter fifty years later, it would have to be grand.<sup>40</sup>

Fogel’s breakthrough work was *Railroads and American Economic Growth* (1964a). At the time of its publication, economists believed they had established that modern economic growth was due to certain important industries having played a vital role in development. Fogel set out to measure this impact, which he did with extraordinary precision. He constructed a counterfactual to highlight the contributions of the railways to the growth of the American economy. The result was not what economists or historians expected. He famously found that the railroad was not absolutely necessary in explaining economic development and that its effect on the growth of GNP was minimal. Few books on the subject of economic history have made such an impression as Fogel’s. His use of counterfactual arguments and cost-benefit analysis made him an innovator of economic historical methodology, but not universally loved. Fritz Redlich (1965), for example, accused him of “fictitious quasi-history” for his emphasis on the counterfactual. He acknowledged the value of counterfactual analysis, but thought it was social science research, not historical.<sup>41</sup>

This approach formed his major works on slavery and demography as well.<sup>42</sup> Herein was the difference between the “old” economic history and the “new:” The use of newly created data series and cutting edge techniques - made more useful, applicable, powerful, and easy to replicate and reconsider with the growth of computing power, to focus on a problem with laser precision.

As a testament to the staying power of *Railroads*, Diebolt and Hauptert (2018) looked at the citation rate over the fifty year period since its publication and compared it with other works in economic history. They found that *Railroads* was not a passing phenomenon. Its relevancy has remained strong, and in fact, as measured by citations, has gotten stronger over the past decade. The general trend of top cited books over time is a more gradual increase in citations over the first two decades after their publication, followed by a leveling off. Fogel's citation record is more varied, but shows strong growth over the last decade.

Fogel left Johns Hopkins with a research strategy that would occupy him for the rest of his life. He was determined to measure the impact of key scientific and technological innovations on the course of economic growth. His groundbreaking work was due in part to the plunging cost of data processing, made possible by rapid advances in computer software, which made it feasible to work with ever-larger data sets. He believed that “the major obstacle to the resolution of most of the issues in history and economics . . . is the absence of data rather than the absence of analytical ingenuity or credible theories.”<sup>43</sup>

Before Fogel showed what a small impact railroads had on the economy, using new economic history techniques in a classic example of its power to overturn previously held beliefs, it was commonly understood that the railroad was a key factor in economic growth. Joseph Schumpeter and Walt Rostow “had earlier, and with general agreement, asserted that modern economic growth was due to certain important discoveries having played a vital role in development. Fogel tested this hypothesis with extraordinary exactitude, and rejected it. . . . His use of counterfactual arguments and cost-benefit analysis made him an innovator of economic historical methodology.”<sup>44</sup>

After he first estimated his social savings of the railroad, Fogel got an unexpected result. His social savings was so low, he was convinced he had made an error. In trying to find where he made a mistake, he gradually convinced himself that he was right.<sup>45</sup> He did not view his work on railroads as an attempt to provoke controversy, but rather as a very careful, detailed study of the way in which a major innovation increased overall productivity. It was in attempting to answer the “how much?” question that he discovered, quite to his surprise, that the answer was “not much.”<sup>46</sup>

### *Fogel and Railroads*

The publication of *Railroads* “represented a very major milestone – it was as if we now had proof that we had left the bumpy and unpaved dirt road of the first few years and could see ahead a straight and well-paved highway into the future.”<sup>47</sup> Fogel's railroad study generated an entire subdiscipline of parallel studies and, more importantly, provided a methodological foundation for the systematic study of economic history and long-term economic growth.

*Railroads* showed how well economic history could benefit from the careful application of theory and econometrics. The work immediately generated substantial controversy, and even today some quibbling over minor details occurs. However, time has failed to overturn Fogel's major conclusions: that per capita income growth would have been set back only a few months had the railroads never been invented, and there was no other industry that was likely to have been more important than the railroads. Since its publication, the great majority of economic history has been written by scholars employing those basic economic and econometric tools.

Perhaps the most famous claim from *Railroads* was that its most important implication was “that no single innovation was vital for economic growth during the nineteenth century.”<sup>48</sup> While it may not have been indispensable, it still may have been the single most important cause. The size (less than 5% of GDP in 1890, or about three month's retardation of economic growth to that date) is the issue that Fogel set out to measure. His original goal was to measure just how big the contribution of the railroad was, and he concluded that it was not nearly as big as conventional belief held it to be. It wasn't the conclusion that makes this work a landmark, but the process by which he came to it. The application of econometrics and theory, careful attention to methodology, and the consideration of opportunity cost: what would have happened without the railroad, is what sets this work apart as the gateway to cliometrics.

*Railroads* was reviewed more than 20 times in the two years after its publication. Even when it was not received positively, its impact on the field of economic history was acknowledged.

The initial reviews of *Railroads* were largely positive. One reviewer called it “one of the best examples yet of the ‘new quantitative economic history,’”<sup>49</sup> and another thought it deserved “serious consideration for both its methods and its conclusions.”<sup>50</sup> Further praise cited a “sophisticated quantitative approach [that] has added a significant new dimension to economic history,”<sup>51</sup> and called Fogel's performance “an impressive one. . . . He is the new economic

historian *par excellence* and his success as an innovator is best revealed by the rapidity with which academic entrepreneurs have adopted his techniques to their own problems.”<sup>52</sup>

In his review in the *Journal of the American Statistical Association*, William Whitney predicted that Fogel’s methodology pointed “the direction which economic historians will increasingly follow in the future . . . his vision opens historical research to a wide range of new analytical techniques.”<sup>53</sup> Affirming this thirty five years later, Lance Davis pointed out that *Railroads* served as a watershed in the practice of economic history. Since its publication “almost all economic history has been written by scholars who have either been trained in economics or who have found it necessary to acquire (either formally or informally) those basic economic and econometric skills.”<sup>54</sup>

Whether critical or fawning, Fogel’s contemporaries generally agreed that he demonstrated remarkable “possibilities in using statistical inference and economic theory to answer significant historical questions,”<sup>55</sup> and that the question was not so much whether one agreed with Fogel’s conclusions, but “the competence and thoroughness of his analytical framework that commands attention.”<sup>56</sup> *Railroads* was a rare book that reached “genuinely important conclusions on a genuinely important subject by applying novel methods to largely unused sources . . . a book which future economic historians may well remember as the book of its year, if not of its decade,”<sup>57</sup> and is likely to “leave a permanent mark on economic history and economic historians.”<sup>58</sup> Indeed, it is more widely cited today than it was in the years immediately following its publication.

Fogel was also credited for his substantial traditional historical work. “The most impressive aspect of the book . . . is not its liberal application of methods often used by non-historical economists, but its traditional scholarship. Fogel’s techniques are less striking than his use of imagination and a detailed knowledge of, and scrupulous regard for, the facts. . . . In this sense, the break between the old and the new schools of economic history is not sharp at all.”<sup>59</sup> Another reviewer called it “probably the most impressive example of the union of theory, statistical technique and antiquarian digging.”<sup>60</sup>

Marvin Goodstein correctly predicted that Fogel’s impact would forever change economic history, but warned that the “integration between it and economics will still remain somewhat less than complete until economists more frequently ask not what they can do for economic history, but what it can do for them.”<sup>61</sup>

Econometrics and statistical analysis, familiar techniques to the economist, were foreign, unknown, and intimidating to many historians. As a result, this became a 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.”<sup>62</sup> Others acknowledged the heroic efforts at data accumulation and the technical analysis thereof, but criticized the absence of any consideration of other important beneficiaries of the growth of the railroad, such as the development of better capital markets, or the increased mobility of the populace, technological advances in the iron industry, and the international migration of labor to America.<sup>63</sup>

Still others took umbrage at the tone of Fogel’s arguments. “Here is another entry into the polemics between the new and the old economic historians. The good guys call themselves . . . ‘Cliometricians’ and dismiss the bad guys as mere narrative historians. . . . This volume is a new manifesto which, if I get the message, threatens: Retool, rethink, conform, or be plowed under.”<sup>64</sup> Fogel’s “exercise is without any doubt a brilliant tour de force, but it is more likely to widen than to narrow the area of communication between conventional historians and those who, like Professor Fogel, believe that in history as in economics it is only the methods of econometrics that offer real promise of enlightenment.”<sup>65</sup> Others were blunter: “There seems to be some danger in over-enthusiasm for a new “ism” – cliometricism!”<sup>66</sup>

Writing in 1966, Louis Hacker lauded the accomplishments of Fogel and North, but cautioned that their methods were not substitutes for traditional (i.e. orthodox literary) economic history, but rather complementary to it, and they best not forget that if peace and cooperation between the economics and history disciplines was to be maintained. More than fifty years later, this wedge that cliometrics has driven between economists and historians remains.

### ***Douglass North***

Douglass North had a rich and varied education, attending schools in the United States, Canada, and Switzerland before enrolling at the University of California – Berkeley (BA 1942). While in high school he developed a passion – and obvious talent – for photography. He won several international awards and even after deciding on an academic career, continued to pursue

it as a hobby. His intention to enter law school was derailed by World War II. He served in the Merchant Marines, which afforded him much time to read, which helped to convince him that economics, not law, was to be his calling. After the war he returned to Berkeley and completed his PhD in 1952, writing on the history of the U.S. life insurance industry under M.M. Knight.

North could be considered the grandfather of cliometrics because it was two of his students, Lance Davis and Jonathan Hughes, who organized the first meetings of the society at Purdue in 1960. North was in attendance at those inaugural meetings, and became an early and enthusiastic practitioner of this new form of economic history. He and his colleague, Morris D. Morris, trained an impressive group of economic historians who went on to become noted cliometricians.<sup>67</sup>

Like Fogel, Douglass North made his initial impact with research on the American economy. However, whereas Fogel disputed the importance of one sector of the economy in explaining economic growth, North focused on the impact that individual sectors could have in explaining economic outcomes. He sought to explain the causes of growth in the antebellum American economy. Starting with an export based model he had previously formulated, he showed how one sector (the cotton industry) could stimulate development in other branches, ultimately leading to specialization and interregional trade.

Perhaps the most influential book to come from the new economic history is North's *Economic Growth of the United States, 1790-1860* (1961). What it lacked in thorough empirical research, it more than made up for in the way it clearly demonstrated how an economic model, theoretically sophisticated yet nonmathematical, could be employed to explain the organization and evolution of the various regions of the American economy over several decades.

When his research focus shifted from American to European economic history in the late 1960s, he became convinced that the tools of neo-classical economic theory were not up to the task of explaining the kind of fundamental societal change that had characterized European economies since medieval times. His search for a suitable framework that would provide new tools of analysis led to his interest in the new institutional economics. The result was the publication of his signature book, *Structure and Change in Economic History*, in 1981.

In *Structure and Change* he abandoned the notion that institutions were efficient and attempted to explain why "inefficient" rules would tend to exist and perpetuate. This was tied to

a very simple and still neoclassical theory of the state that could explain why the state could produce rules that did not encourage economic growth.

The next step in his research occurred when he left the University of Washington after 33 years on the faculty and accepted the Luce Professorship of Law and Liberty in the Department of Economics at Washington University in St. Louis in 1983. It was there that North began working with political scientists and economists who were attempting to develop new models of political economy.

In a number of books, beginning with *Institutional Change and American Economic Growth* (1971, with Lance Davis), North demonstrated the importance of the role played by institutions (including property rights) on economic development. In *Institutions, Institutional Change and Economic Performance* (1990), he posed the fundamental question of why some countries are rich and others poor. His conclusion was that institutions are a major determinant in the profitability and feasibility of economic activity. The greater the institutional uncertainty, the greater the transaction costs and the greater the drag on economic growth and development. These views were a novel approach in both the history and development fields. Typical economic growth models focused on technological change and capital accumulation, assuming zero transactions costs and ignoring institutions altogether. He maintained that new institutions arise when groups in society see a possibility of profiting that is impossible under prevailing institutional conditions. If external factors make an increase in income possible, but institutional factors prevent it, then new institutional arrangements are likely to develop. Other pioneering work emphasizing the importance of institutions included R. C. O. Matthews (1986) and Oliver Williamson (1985).

The development of a political-economic framework to explore long-run institutional change occupied him for the next decade and led to the publication of *Institutions, Institutional Change and Economic Performance* in 1990. In that book he began to puzzle seriously about the rationality postulate, and became convinced that the missing link was the explanation for why ideologies can shape the choices people make and direct the way economies evolve through long periods of time. Understanding ideologies requires an understanding of the way in which the mind acquires learning and makes choices. This was the focus of his research for the remainder of his life.

Perhaps the most important link between North and cliometrics was his role as a missionary for the new economic history. In his role as co-editor of the *JEH* “he was able to ensure that the field’s most prestigious journal was hospitable to articles and reviews that made self-conscious use of neoclassical economic theory and/or econometric methodology.”<sup>68</sup> While editor, North published three papers (1963, 1965, 1968) praising and evaluating the new economic history. He also published *Growth and Welfare in the American Past* in 1966. It is a collection of essays that use simple economic theory and data to challenge views that were widely held at the time. It was important as the first, and for a long time only, textbook on new economic history suitable for undergraduates. The book was simply a set of examples of “accepted” historical facts that could be called into question with just the simplest application of economic theory. The ideas were largely undeveloped, laying out a gold mine for future research.

### ***The North Editorial Years***

Douglass North and William Parker were appointed editors of the *JEH* in 1961, and would hold the position for six years. During this time the rise of the “new” economic history was at the forefront of the discipline, and the *JEH* was its highest profile venue. As early practitioners of cliometrics, North and Parker viewed the approach favorably, and as editors were in a position to broaden its reach. They launched the publication of clio influenced articles on a path that would lead the approach to dominance in the journal, as well as the field.

All was not smooth in this transition from the “old” to the “new.” Midway through their six years at the helm, North and Parker were called before the Board of Trustees and forced to defend themselves against charges of incompetence. “Old guard” economic historians on the board denounced the articles they were publishing in the *JEH*. In their successful defense, North and Parker pointed out that over the previous year the acceptance rate of articles submitted by historians was actually higher than that for economists.<sup>69</sup> While the momentum in the discipline was inevitably toward the new economic history methodology, it was not to the exclusion of the older, more traditional approach.

North saw the 1960s as a period of massive transition in the economic history field, one that was not to everyone’s liking, but was inevitable. The landscape was changing, and he and Parker were mere pawns in a bigger game. The journal was getting new economic history submissions like Fogel’s on the one hand, and articles from the “old guard” on the other, and the

journal published both. However, the movement was toward an increase in the cliometric approach, and inevitably, the purely narrative style of economic history began to fade. North felt that by the end of their tenure, the question as to whether the new economic history as a method was accepted had been answered in the affirmative. It was an acceptable and accepted part of economic history. The only real question was what proportion of the field it should be.<sup>70</sup>

North believed the true test of a scholar's contribution was not its popularity, but its staying power and ability to enliven the field. In order to test this theory Diebolt and Hauptert (2018) looked at the impact that North and Parker had on the impact and direction of cliometrics in their role as editors of the *JEH*. They constructed a measure of cliometric research and concluded that through the published research in the *JEH*, North and Parker were critical in promoting a shift in the belief that economic history should adopt the cliometric approach, recognising the existence of close links between measurement and theory.

#### *The shortcomings of Clio*

Clio has not had an unchecked history. Its growing popularity has led to a rift between economists who practice cliometrics and historians who practice economic history with little or no reliance on formal models. Historians argue that formal models miss the context of the problem and their proponents have become too enamored with statistical significance at the cost of contextual relevance. Boldizzoni attacked cliometrics, focusing his sharpest criticism on the quantification of history at the perceived expense of its humanity.<sup>71</sup>

On the other side, cliometrics has lost some of its significance with economists, who see it as another application of economic theory, albeit using historical data. While applied economics is not seen as a bad thing, cliometrics is not seen as anything special. Rather, it is often perceived as the application of theory and the latest quantitative techniques to old data instead of contemporary data. In that world view, a cliometrician is just a theorist with a more limited repertoire. As a result, cliometrics shoulders some of the blame for the demise of economic history positions in many economics departments.

As early as 1986 William Parker foreshadowed this problem when he observed that what was lost in the move to theory and econometric emphasis was the humane interest of the old British political economy and social welfare and the idealistic German historical economist's concern for the whole society.<sup>72</sup> At the same time, Alex Field (1987) cited problems from another flank. Whereas the "new" economic historians had to fight to prove their technical skills

belonged in the study of history, by the late 1980s there were no more “old” economic historians left to challenge. Instead, the challenge came from the other side, where economic theorists questioned what value cliometricians added to departments strapped for resources. Most economists possess the same or even more sophisticated technical skills, which can be applied to any data set, contemporary or historical.

Even within the cliometric camp there were those who cautioned against the over reliance on technique. In the early days of the cliometric movement Jonathan Hughes (1966) warned that cliometrics is unkind to those who confuse ends and means in the pursuit of historical understanding. And Lance Davis (1968), though praising the new economic history for its contributions to both economics and history, criticized indiscriminate uses of theory applied to history. He argued that the greatest failure of the new economic history was the rush by some to apply any theory, even if irrelevant, to a historical issue or data set without understanding the context of the historical situation. And North (1965) warned that too much of the new economic history was dull and unimaginative because there was too much emphasis on econometric techniques as a substitute for theory and imagination.

### *Conclusion*

So in the end, what is cliometrics and what is its place in the history and lexicon of economic history? Is it history with an economic (technical) approach? Or is it economics with a long run view of the world? Or has cliometrics become a subset of economic theory? The answer, not surprisingly, is all of the above.

### *A branch of history*

For many authors —and many of its protagonists— cliometrics appears to be a branch of history. Using economic tools, techniques and theories, it provides answers to historical, rather than contemporary economic debates.

This inductive view is intimately linked with the German historical school, despite the use of more sophisticated techniques. It could be said that the two disciplines became closer, but probably within the frame of ‘inductive’ economics. On top of that, despite those early interests in building a kind of historically (i.e. inductively) grounded development economics, cliometrics mainly tried to provide answers to *historiographical* questions — and therefore spoke more to the historian than to the standard economist. Econometric techniques may be used, but they do have a historical vocation - that of shedding light on historical questions.

*An auxiliary discipline of economics*

Some recent work in cliometrics reveals the possibility of a cliometrics that could also be an auxiliary discipline of *economics* per se. As such, it should be part of the toolkit and competencies of all economists. However, as the term auxiliary discipline indicates, it could only fulfil its proper role for economics if it remains slightly outside the realm of standard neoclassical economics. It must meld the newest econometric techniques and economic theory with the institutional and factual culture characterising the old economic history.

History is indeed always a discipline of synthesis. It should also be the case for cliometrics. If not, if cliometrics were to be deprived of all its historical dimensions, it would simply be economics applied to the past, and would thus cease to exist as a separate branch of study. To be helpful for the economics profession at large, its main contribution should be to mobilise all the relevant information that can be gathered from history, including cultural and institutional development, in order to enrich and challenge economic theories.

*A full-fledged field of economic theory?*

Last, but not least, cliometrics could one day be more than just an ancillary discipline of economics and instead become a full-fledged field of economic theory. In this guise cliometrics is the science of the emergence of institutional and organisational structures. Economic history could use the old techniques of the discipline coupled with the state of the art arsenal of econometrics in order to reveal stylised facts about the efficiency of various institutional arrangements as well as on the causes and consequences of institutional change. This could spur the development of a true theory of institutional change, i.e. one that is at the same time general (e.g. serving the needs of policy makers) and theoretically sound, while solidly grounded in empirical regularities as put forward by a joint economic and historical analysis. This analysis of *institutional morphogenesis* would be the true theoretical part of a cliometric science that would emancipate itself from its apparently purely empirical fate — to become the playground of long-run econometricians. The economist's desire for generality and mathematical precision does not encourage contextualisation. However, neo-institutionalist economists like North warn us to seriously consider institutional and cultural contexts.

Fogel defined the methodological features of cliometrics. He considers it fundamental that cliometrics should lay stress on measurements and that it should recognise the existence of close links between measurement and theory. Indeed, unless it is accompanied by statistical

and/or econometric processing and systematic quantitative analysis, measurement is just another form of narrative history. It is true that it replaces words by figures but it does not bring in any new factors. In contrast, cliometrics is innovative when it is used to attempt to formulate all the explanations of past economic development in terms of valid hypothetico-deductive models.

Nevertheless, in spite of the disappointments resulting from some of its more extreme demonstrations, cliometrics also has its successes, together with continuous theoretical progress. The risk would obviously be that of allowing economic theory to neglect a whole body of empirical documentation that can enrich our knowledge of the reality of economic life. Conversely, theory can help to bring out certain constants and only mastery of theory makes it possible to distinguish between the regular and the irregular, between the foreseeable and the unforeseeable.<sup>73</sup>

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<sup>1</sup> This work draws on Diebolt (2012, 2016), Diebolt and Hauptert (2016, 2018), and Hauptert (2016, 2017)

<sup>2</sup> See for example Engerman 1996, Floud 1991, Lyons, Cain and Williamson 2008, Williamson 1991 and 1994, and Williamson and Whaples 2003.

<sup>3</sup> Williamson and Whaples 2003 p 446

<sup>4</sup> The most recent articles published in *Cliometrica* serve as an excellent sample of what is new in economic history research. They exploit new data and databases, with newly developed methods and with newly developed hypotheses, models and theories in economics, history and statistics. For example, see Annaert et al. (2015), Bell et al. (2016), Beltrán (2015), Buelens et al. (2016), Cappelli (2016), Cifarelli et al. (2016), Clark (2015), Dao (2016), Dobado-González et al. (2015), Drelichman et al. (2015), Du Plessis et al. (2015), Edvinsson (2015), Gerlach et al. (2016), Gianfreda et al. (2015), Herranz-Loncán et al.

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(2016), Jaremski et al. (2016), Keay (2015), Martín-Retortillo et al. (2015), McDonald (2015), Ogasawara et al. (2015), Palma (2016), Prados de la Escosura (2016), Pritchett et al. (2016), Saariisa et al. (2016), Sanchis et al. (2015), Standaert et al. (2016).

<sup>5</sup> The Royal Swedish Academy of Sciences, 12 October 1993

<sup>6</sup> Wright 1971

<sup>7</sup> McCloskey 1978

<sup>8</sup> Cochran 1969

<sup>9</sup> Perhaps more than anyone, Deirdre McCloskey has been responsible for holding all economists, not just economic historians, accountable for moving the frontiers of knowledge forward and not simply using the latest techniques to measure something because it can be measured. For example, see McCloskey 1985.

<sup>10</sup> Gay 1941

<sup>11</sup> Schumpeter 1954, p 12

<sup>12</sup> A selection of the papers presented in these early meetings was published by Purdue University in 1967.

<sup>13</sup> The Cliometric Society was formally organized in 1983 by Sam Williamson and Deirdre (nee Donald) McCloskey.

<sup>14</sup> The first use of the term in print: “the logical structure necessary to make historical reconstructions from the surviving debris of past economic life essentially involves ideas of history, economics and statistics . . . has been labeled “Cliometrics.” (Davis, Hughes, and Reiter 1960, p 540)

<sup>15</sup> For earlier laments about the encroachment of theory and mathematics on the study of history, see Braudel 1949 and Polanyi 1944.

<sup>16</sup> Perhaps more than anyone, D.N. McCloskey has been responsible for holding all economists, not just economic historians, accountable for moving the frontiers of knowledge forward and not simply using the latest techniques to measure something because it can be measured. For example, see McCloskey 1978, 1985, 1987, 2006.

<sup>17</sup> Cole (1968)

<sup>18</sup> For a history of the mathematical movement in economics see Weintraub 2002

<sup>19</sup> See Barker (1977), Berg (1992), and Harte (2001) for a history of the Economic History Society

<sup>20</sup> Mitchell served as research director at the NBER from its founding in February of 1921 until 1945.

<sup>21</sup> Johnson 1941, Cochran 1943

<sup>22</sup> Engerman *et al*, 1994, p 71

<sup>23</sup> Parker (1980)

<sup>24</sup> The meeting produced a volume edited by Parker (1960), which included path breaking works by Robert Gallman, Marvin Towne and Wayne Rasmussen, Douglass North, and Stanley Lebergott, among others.

<sup>25</sup> There are currently more than 40 Fellows of the Cliometric Society. The full list can be found on the society website at <http://cliometrics.org/fellows.htm>

<sup>26</sup> Goldin (1995)

<sup>27</sup> Before Fogel the concept was proposed by Meyer and Conrad (1957) and Fritz Machlup (1952).

<sup>28</sup> Kirkland 1949

<sup>29</sup> Erickson 1966, p 107

<sup>30</sup> Ferguson 1999, p 18

<sup>31</sup> Tollnek and Baten (2016) provide an exhaustive overview of age-heaping models and their applications.

<sup>32</sup> For example, see Gentzkow, *et al* 2014

<sup>33</sup> See also Boberg-Fazlic *et al* 2011, and Lee and Feng 1999

<sup>34</sup> See Galiani and Sened (2014) for a discussion of North’s role in the new institutional economics movement.

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- <sup>35</sup> Among the seminal articles in historical anthropometrics are Le Roy Ladurie 1979, Fogel et al 1983, Fogel 1986, and Komlos 1987
- <sup>36</sup> In 2016 ISNIE changed its name to the Society for Institutional and Organizational Economics
- <sup>37</sup> Ronald Coase 1991, Douglass North 1993, Oliver Williamson 2009
- <sup>38</sup> Ferrie 1996
- <sup>39</sup> Bailey, et al 2017
- <sup>40</sup> Engerman *et al* 1994 p 25
- <sup>41</sup> For a different view of counterfactuals see Engerman (1980)
- <sup>42</sup> See Fogel and Engerman 1974 and Fogel 2000, for example.
- <sup>43</sup> Engerman *et al* 1994, p 29
- <sup>44</sup> Engerman *et al* 1994, p xi
- <sup>45</sup> Lyons *et al* 2008, p 334
- <sup>46</sup> Lyons *et al* 2008, p 335
- <sup>47</sup> Davis, Project 2000
- <sup>48</sup> Fogel 1964a, p 234
- <sup>49</sup> Meyer 1966, p 87
- <sup>50</sup> Riegel 1965, p 636
- <sup>51</sup> Saul 1966, p 66
- <sup>52</sup> Madden 1965, p 612
- <sup>53</sup> Whitney 1966, p 276
- <sup>54</sup> Davis, Project 2000
- <sup>55</sup> Rothstein 1965, p 131
- <sup>56</sup> Meyer 1966, p 88
- <sup>57</sup> Gould 1966, p 474
- <sup>58</sup> Williamson 1965, p 110
- <sup>59</sup> Williamson 1965, p 111
- <sup>60</sup> Hilton 1966, p 237
- <sup>61</sup> Goodstein 1965, p 91
- <sup>62</sup> Hacker 1966, p 175
- <sup>63</sup> Madden 1965 and McClelland 1968
- <sup>64</sup> Kirkland 1967, pp 1493-94
- <sup>65</sup> Erickson 1966, p 107
- <sup>66</sup> Mitchell 1965 p 603
- <sup>67</sup> Besides Davis and Hughes, other North proteges included Clio Fellows Roger Ransom, Richard Sutch, Price Fishback, John Wallis, and Lee Alston.
- <sup>68</sup> Engerman *et al* 1994 p 73
- <sup>69</sup> EHA archives, folders 18-27, BoT meeting minutes, Sept 4, 1963
- <sup>70</sup> Lyons *et al* 2008, p 201
- <sup>71</sup> Boldizzoni, 2011.
- <sup>72</sup> Parker, 1986.
- <sup>73</sup> Diebolt and Parent (2011) and Diebolt and Demeulemeester (2012)