0650
NEW ECONOMIC HISTORY AND LAW AND ECONOMICS

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Abstract

This chapter reviews the contributions of the new economic history in light of their relationship to the study of law and economics. After reviewing the progress of the subdiscipline over the past century, and comparing the approaches of new and old economic historians, the chapter provides an overview of some of the most important controversies in the field and offers a bibliography with references to the relevant literature.
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1. Introduction

The purpose of this chapter is to give readers a broad overview of the new economic history and its relationship to the study of law and economics. The emphasis is not primarily on contributions specifically concerned with law and institutional structure as explananda per se - such matters are treated elsewhere in these volumes. Rather the focus is on what the field of the new economic history is, what its relationship is to the 'old economic history', and how its methodology and concerns relate to those of the economics discipline on the one hand and the study of law and economics on the other.

To understand the relationship between law and economics and the new economic history, it is important to understand what the new economic history is (or was, since it is hardly new anymore) and what it reacted against. Economic history began its life as a specialized social science subdiscipline with close links to legal and institutional scholarship, and considerable shared interest in documenting rule variation and its independent causal influence on economic life. Receptivity to that methodological stance waned during the heyday of the new economic history, although there is evidence in recent years that it is beginning to wax again.

The new economic history was born in the United States in the late 1950s and early 1960s, and has subsequently come to have increasing influence in Europe and the rest of the world as well. Prior to the late 1950s scholarship in the area of economic history had a strong institutional, legal, and arguably statistical focus, but was largely agnostic when it came to the desirability of applying formal economic theory to historical inquiry. Attitudes toward theory changed with the advent of the new economic history, and with these came differences in how old and new economic historians defined their tasks. A strand of continuity over the last century has been a subject of concern in the history of growth and development. In this context, economic history is distinguishable from the history of economic thought, a branch of intellectual history.

2. The Legacy of the German Historical School

The first professor of economic history in the United States, William J. Ashley, assumed his position at Harvard University in 1892, seven years after the founding of the American Economic Association and eight years after the establishment of the American Historical Association. The initial memberships of both of these associations were heavily influenced by ideals and scholarship developing within German universities, which led the world in doctoral instruction at the end of the nineteenth century. Indeed, up until the 1880s it was impossible to obtain a PhD in the United States; the first American programs were established at Johns Hopkins University and Clark University at the end of the century.

The German tradition of economic scholarship, associated with such scholars as Wilhelm Roscher, Gustav von Schmoller, Karl Knies and Werner Sombart, emphasized inductive generalizations reached through the careful study of primary materials, and defined itself in opposition to the more abstract deductive approach popularized in England by David Ricardo and others. German economic scholarship was dynamic and developmental, in that it sought to delineate and understand how economic categories changed and evolved over time, leading to various ‘stage’ theories, or at least typologies of economic progress. The ambitions of German historical economists were hardly modest: they looked forward to a victory over the English approach as complete as had been Adam Smith’s over Mercantilist doctrine (Gay, 1941, p. 10).

This ambition was overreaching, a conclusion announced as early as 1901 by Thorstein Veblen and affirmed consistently by scholars thereafter (Veblen, 1901). Neither the extensive description of legal and institutional environments nor the development of stage theories of economic development would halt the elaboration, development and growing influence of partial and general equilibrium analysis known generically as neoclassical economics. But the failure of the historical school to supplant economic theory led many economists to dismiss its concerns, in particular the emphasis on description of the institutional and cultural context within which economic activity takes place as a starting point for static economic analysis, and the focus on the *process* of economic growth and development - the how and why of institutional change over time and its consequences - as of limited relevance to economic inquiry. Specialized scholars in economic history rejected, and continue by and large to reject, this view. But the drift away from the German tradition within US economics departments left a major unanswered question: what was to be the role of legal and institutional scholarship and more particularly what were economic historians supposed to do within economics departments? This question has been finessed during the twentieth century, and remains with us today. In England and Europe, on the other hand, it was largely resolved politically, although not intellectually, through the establishment of separate departments of economic history.

The finessing began with the first US appointment in the field. When Ashley migrated from England to assume the first American chair in economic history, he took pains to distance himself from the German tradition by rejecting any ambition to supplant economic theory. In exchange, he asked that the study of economic history in economics departments ‘be let alone’. Thus was a truce declared. He proposed an historic division of labor within economics departments, with economists concerned with static analysis within existing institutional structures, and economic historians assuming the tasks not only of documenting the historical trajectories of development, but also of describing institutional structure, documenting its changes across time and space and considering its impact on economic growth. The position of economic history *vis à vis* historical scholarship was reaffirmed by Ashley’s successor at Harvard, Edwin Gay, in his presidential lecture inaugurating the Economic History Association in 1940. Gay, a student of Schmoller, who had in turn been a student of Roscher, candidly reiterated the view that ‘the full hopes of the historical economists have not been realized and are not realizable’ (Gay, 1941, p. 13).

But the rejection of the ‘full hopes’ of the German school did not mean for Ashley or for Gay that economic historians in the United States rejected wholesale its concerns or methods. The most important area of continuity between the German and English traditions in Europe and indeed between the old and new economic history in the United States has been a subject concerned with the process of economic growth and development. A key difference between old and new economic history, we shall see, has been the role allotted to legal and institutional structure as independent factors accounting for variations in economic performance over time. For old economic historians that role was much larger, which is why, for them, delineation of structure was the starting point (although not, in principle, the ending point) for analysis.

One can appreciate the old economic history perspective on the tasks of economic history in this excerpt from a paper published in 1913 by Guy Callender in the *American Historical Review.* Callender, then professor of economic history at Yale, began by contrasting the lack of interest in economic history he experienced that year at the annual meeting of the American Economic Association with that displayed by historians at their conclave. He noted that relative interest levels in the two disciplines appeared to have reversed over the previous two decades and went on to reaffirm the indebtedness of economic history to the German historical tradition, as well as the primacy of institutional analysis as the starting point for economic inquiry: 'economic history owes its existence to [the historical school] … Economic science ought to be primarily a theory of development and not merely an explanation of the way in which human beings produce wealth and share it as income under a given set of social conditions'.

Callender's vision of economic history as a fundamentally empirical science, concerned with growth and development, recognizing that institutions have consequences for economic performance and may change over time in ways we should try and understand, is a statement of the tasks of economic history that continues to have relevance today. But only parts of it would have found favor at the height of the new economic history revolution. Although new economic historians were as concerned as was Callender with understanding growth and development, institutional variation was *not* the first thing they turned to in accounting for it.

A second arguable area of continuity among old and new economic history has been an emphasis on the use of statistics to measure economically important magnitudes. Sir John Clapham, in his inaugural lecture for the first chair in economic history at Cambridge University in England proclaimed it 'the obvious business of an economic historian to be a measurer above other historians' (cited in McCloskey, 1987, p. 41; from Clapham 1930, p. 68), and most new economic historians would embrace the sentiment. The continuity is arguable, however, because old economic historians' interest in statistics was often limited to documenting the historical record.

New economic historians have been interested in doing more than just clarifying the record per se: they have yearned to verify conjectures, to 'test hypotheses'. New economic historians have been more likely to say, 'well, if this is true, then that must be true. Let's go look at the data and see if it is.' They have also been armed with more powerful statistical techniques - in particular multiple regression - and much cheaper computational tools than were available to their predecessors. And they have approached with enthusiasm the task of teasing out relationships and possible causal connections from the experiments provided by history in what of course has had to remain a nonexperimental science.

Another reason the continuity is arguable is that in the first half of the twentieth century the most dedicated analyzers of quantitative historical data were not economic historians per se but rather students of business cycles associated with the National Bureau of Economic Research. Wesley Mitchell, Arthur Burns and Simon Kuznets were certainly sympathetic to the study of economic history, but it was not their primary professional identification. This anomaly led Carter Goodrich to wonder, on the eve (or morning?) of the revolution in 1960, whether economic history [was] 'one subject or two?' He commented that 'the study of business cycles and conventional economic history are for the most part carried on in separate compartments' and that 'twice during the present century … a vigorous body of research concerned with economic changes over time [has] developed largely in isolation from conventional economic history'. (The first was the study of business cycles, the second the resurgence in the 1950s in interest in economic growth.) 'In each case the new work was quantitative in method, and the result was the phenomenon of two separate bodies of scholarship - the one written in prose and calling itself economic history, the other written mainly in figures and calling itself by another name' (Goodrich, 1960, p. 531).

Goodrich concluded by asking plaintively whether 'If these are tasks that can be done better by scholars operating under another banner, is there any remaining purpose for us to serve?' The partial isolation of the field from work relevant to its main concerns was something that new economic historians wished to remedy, and it is one area in which there has been partial success.

A final area of continuity between the German and English/American traditions was in the commitment to describing and communicating to students the important and essential features of legal and institutional environments, even as this commitment received less and less affirmation from mainstream economists. Here one finds the weakest link between old and new economic history - a point of obvious relevance to scholars in the law and economics area. Both old and new historians have sought to understand and explain growth. Old economic historians, and institutional economists such as John R. Commons, were interested in institutions, not so much in and of themselves, as for the insight they might offer into variations in economic performance. Nevertheless, for old economic historians, one started with the institutions, and in that sense they were primary (Redlich, 1965, p. 482).

3. The Committee for Research in Economic History and the Founding of the Economic History Association

It is fair to say that throughout the first four decades of the twentieth century, American scholarship proceeded very much in the shadow of Europe. The outbreak of the World War Two in Europe created conditions under which the modern American university would blossom, both because of the interruptions of contacts and scholarly materials from overseas and because of the exodus of scholars (and scholars to be) from the war-torn continent. The latter migration, which brought to the United States such luminaries as Alexander Gerschenkron and Simon Kuznets, enriched economic history in the United States as it did other scholarly fields, and the increased difficulty of maintaining scholarly contact with Europe helped lead to the establishment in 1940 of a US based Economic History Association (EHA).

Although an Economic History Society (EHS) had existed in England since 1926, and been supported by American scholars, there were concerns that the EHS was partial to European topics and thus intermittent pressures for a new association on the western side of the Atlantic. The war catalyzed these concerns, and the establishment of the EHA along with its journal, the *Journal of Economic History*, announced and ratified at meetings of both the American Economics and the American Historical Associations in 1940, fleshed out the thesis against which the antithesis of the new economic history would be opposed (Lamoreaux, 1998).

Two other developments should be noted in order fully to appreciate the historical setting in the United States in the late 1950s and early 1960s, within which the new economic history strode brashly forward. The first was the establishment, also in 1940, with Rockefeller Foundation support, of the Committee on Research in Economic History, administered for the first 10 years through the Social Science Research Council, and incorporated separately thereafter. (The other, dealt with subsequently, was the rapid growth after 1957 of the American university system, the consequence of demographic trends and the launching of Sputnik.) The purpose of the Rockefeller grant was quite simply 'to develop the field of economic history.' To do this the committee supported studies of the role of government and its relationship to economic development, of entrepreneurship within the private sector, of the banking system and its role in mobilizing capital and facilitating interregional and intersectoral intermediation, and of historical statistics (Cole, 1953, p. 79).

The latter initiative is probably least well known yet illustrates how interest in statistical data does represent at least a weak strand of continuity between the concerns of the old and new economic history. CREH funding partially supported the preparation of the first edition of *Historical Statistics of the United States*, published by the Federal government's Bureau of the Census in 1949 (Cole, 1953). This publication and its successors both reflect and have served as a starting point for much research in US economic history, providing authoritative statistical series extending far beyond those available in current editions of the *Statistical Abstract of the United States* or *Economic Report of the President*. A second edition of *Historical Statistics* appeared in 1960, with data through 1957, and a Bicentennial edition in 1975, with data through 1970. A new 'millennial' edition is now under preparation by a consortium of economic scholars in universities (because of budget cutbacks the United States Census Bureau has been unwilling to fund a revision) and is scheduled to be published in both print and electronic editions by Cambridge University Press in the year 2000.

The other CREH initiatives, particularly those in the areas of institutional structure, strengthened 'old economic history' traditions that new economic historians would enthusiastically reject. The committee's grants in the institutional area reflected a view that governments of American states had, in the early history of the republic, played an influential role in fostering economic development and that the increased federal role evident in the New Deal should be understood as a change in the locus of regulatory influence and government action to improve economic performance, not wholesale abandonment of a laissez faire past. CREH funding supported the well-known works by Oscar and Mary Handlin on the role of government in Massachusetts (Handlin and Handlin, 1969), as well as Louis Hartz's 1955 treatise on government in Pennsylvania, *The Liberal Tradition in America*. Less familiar are CREH funded studies of Georgia (Heath, 1954) and Missouri (Primm, 1954).

The intellectual framework underlying these grants was the 'old economic history' belief in the need for an intellectual division of labor between scholars who would describe the institutional setting within which economic activity (and the processes that led to its change over time) took place, and those who would model and study behavior within such settings. The position of economic historians within economics departments had traditionally reflected this division of labor, but it was and continued to be an awkward division because the intellectual tools needed to do the former well were different from those required to excel at the latter. Analyzing economic behavior within an institutional framework required tools of great analytical sophistication whereas studies of an evolving institutional environment required sensitivity to context, nuance and skills in the interpretation of documentary materials, all more likely to be found in humanities as opposed to mathematics or statistics departments.

It was this divergence of traditions and methodologies, particularly with respect to the use of formal economic theory in historical analysis, that the new economic historians would ultimately try to bridge. Fogel (1965, p. 95) argued in 1965 that 'the new economic history represents a reunification of economic history with economic theory and thus brings to an end the century old split between these two branches of economics'. The new economic historians possessed stronger training in statistical methods and econometrics than did their predecessors. But the emphasis on the use of statistics for measurement was not, as has been noted, entirely new. In that sense the term Cliometrics (from Clio, the muse of history), sometimes used synonymously for the new economic history, is misleading to the extent that it focuses on a less fundamental methodological initiative.

The more fundamental departure involved the selfconscious use of formal economic theory to model historical questions and, in so doing, identify empirically measurable magnitudes of interest and what might reasonably be inferred from them (McCloskey, 1978, p. 15). But the precise extent to which the revolution represented a rejection or abandonment of the historical division of labor announced by Ashley was always unclear. From the standpoint of law and economics - and the future of historical economics - the big and unanswered question remained: what was to be the role of legal and other institutions in the study of economic history, or, for that matter, economics? New economic historians dealt with the issue, with one or two notable exceptions, largely by ignoring it.

It was perhaps not coincidental that neither the Handlins nor Hartz had doctorates in economics or resided in economics departments (Oscar Handlin was in History and Hartz in the Government department, both at Harvard). Indeed, by the late 1940s, it was rare to find individuals within economics departments who had the background or interest to conduct such studies. The studies of the state's role in economic development were conducted by scholars outside of economics, scholars frankly more attuned to the historical analysis of the particular than were most economists by this time.

A second main CREH initiative, however, addressed private action within the governmental structures that Hartz and the Handlins were examining. This led to the establishment at Harvard in 1948 of a Center for Entrepreneurial History, an effort reflecting a Schumpeterian view that the driving economic forces within the private sector (circumscribed by legal, cultural and institutional structures) was the entrepreneur. Philosophically, this represented an extension, in a sense, of the 'great man' tradition of political and diplomatic history to economic history. The actual role played by Schumpeter in this center is unclear. He commented extensively on Cole's 1946 proposal for such a center in an essay published in part in the *Journal of Economic History* in 1947 as 'The Creative Response in Economic History' and in full in a recent edited collection of papers by Richard Swedberg (Swedberg, 1991, pp. 406-428). But Schumpeter spent most of the last years of his life (he died in 1950) at work on his monumental *History of Economic Analysis.*

In spite of Schumpeter's presence and legacy in the department, this initiative did not find more fertile intellectual ground within the Harvard department than had the state institutional studies. The methods used to study entrepreneurs were not those favored by the developing 'science' of economics, and most found it hard to see how a doctorate in the field was of much help in pursuing such studies. Finding little to assist them analytically or theoretically within economic theory, entrepreneurial historians turned to other disciplines, such as social psychology, for inspiration (McClelland et al., 1953). Thus the two main CREH initiatives had relatively little influence on scholars interested in historical economics who also had some systematic exposure to formal economics.

Establishment of the Research Center in Entrepreneurial History did, however, lead to the establishment of a second American journal in the area, *Explorations in Entrepreneurial History*, a journal that would be renamed *Explorations in Economic History* in 1969, presumably to distance itself somewhat from its intellectual origins and broaden its appeal. (A third American outlet, an annual, *Research in Economic History*, began publication in 1979.)

The final factors reinforcing the growth of the new economic history were US demographic trends and the launching in October of 1957 of Sputnik. The Soviet scientific and propaganda triumph led to a great deal of national soul searching and a substantial increase of US funding for scientific, including social scientific research. Although the baby boom in the US peaked in that year, the enormous number of children born after World War Two and their eventual demands for higher education, contributed, along with Sputnik, to a golden age for American academicians between 1957 and 1969. These were years of great expansion in the capacity of the American University system as undergraduate enrollments soared and faculty shortages developed. New and expanded graduate training programs designed to address these shortages served in the short run only to worsen them as they added their own demand for new faculty to an already undersupplied pool (the phenomenon can be understood in economic terms as a capital stock adjustment phenomenon). The real income of scholars and funds available for research increased over these years until the expanded graduate programs eventually flooded the faculty market as the 1970s began, and the macroeconomic legacy of the Vietnam war led to more moderate levels of support for scientific research (Field, 1987a).

4. The New Economic History - Goals and Agenda

The new economic history defined itself and made its greatest impact during these golden years. What the insurgents promised was an extension of the revolution in method then sweeping the economics profession as a whole, a revolution that emphasized much greater use of analytical, mathematical and statistical methods, to economic history. The old intellectual guideposts were vigorously attacked. In particular, two pillars of the CREH initiatives and the traditional economic history - (1) the emphasis on description of institutional or legal structures as the first step in understanding the process of development and (2) the study of entrepreneurial behavior within those structures - was, if not rejected, at least very substantially reduced in the attention devoted to it.

First, the causal importance of entrepreneurial choice, vision, and action was downplayed. As William Parker put it, in discussing how rational businesspeople had responded to the changes in prices resulting from technological change, capital accumulation, demographic growth and the expansion of natural resources, 'there is not much room here for good and bad entrepreneurs' (Parker, 1971, p. 6). As a consequence of such attitudes, students of entrepreneurial history felt themselves increasingly less welcome within the Economic History Association (Lamoreaux, 1998).

Gone also was much reverence for studies of evolving institutional structures, such as those by Hartz and the Handlins, with their implicit assumption that politics and public policy choices mattered because they influenced the environment within which economic activity took place. Fishlow and Fogel (1971, p. 18) argued that the shift to an explicit concern with long run economic growth and development meant an 'inevitable shift of focus from earlier writing in which the description of the institutional structure was the central objective'. In principle the concern of old economic historians with institutions was instrumental - the ultimate purpose was, or should have been, to shed light on economic performance. In practice, Fishlow and Fogel's implicit criticism is probably fair: scholars did end up focussing centrally on description of the institutional structures, spending less time articulating clearly what their impact was on performance. In any event, instead of entrepreneurs and institutions, much greater emphasis was now placed on technological and demographic change and their consequences, worked out and understood within the analytical framework provided by formal economic theory. Such a framework could, of course, be used to explore the consequences of variation in rule structures, but such issues were less frequently explored.

Why did the new economic history give law and institutions so little intellectual shelf space? Some scholars were influenced by residual Marxian views that the forces of production (technology) ultimately influenced the relations of production (institutions, culture). Thus it was deemed less important to study the evolution of institutions per se - since ultimately they were epiphenomenal. It was far better to go right to the source: the technological and demographic prime movers of history. Ironically, these residual Marxian tendencies often dovetailed conveniently with a conservative approach to public policy issues, in the sense that at one level, institutions did not matter all that much and therefore it was not worth spending a great deal of time studying them or exploring the consequences of changes or variation in them (Field, 1991, pp. 2-4).

In some instances this view was supported by careless interpretations of the 'Coase theorem' (Field, 1991, pp. 10-16). Coase, of course, never developed his analysis as a theorem - this language, common in economic discourse, itself reflected the encroachment within economics departments of the culture and language of mathematics departments - but his articles did suggest that, *assuming there were no transactions costs*, the initial assignment of economic rights should not affect the ultimate economic disposition of a resource. Coase's analysis was microeconomic, and it is not clear he ever intended it to apply at the level of macroinstitutional structure. He admitted that the institution of slavery probably did affect the disposition of resources - for example, black labor in the New World - but took this to be the exception, rather than the rule. For older economic historians, and those interested in general in the consequences of institutional change and variation, the impact of an institution such as slavery on the regional and sectoral distribution of inputs is, of course, the rule rather than the exception. Moreover, transactions costs do exist, and consequently some institutional rules are superior - more efficient - than others, in the sense that they economize on transactions costs or allow risk to be borne by parties who are better situated to do so. That indeed, is the insight that underlies much recent law and economic literature, and represents a productive way in which law and economics scholars have tried to make endogenous the 'choice' of legal rules.

Coase's position on the role of legal institutions as exogenous determinants of performance remains, however, ambiguous. In a 1997 retrospective, the law and economics scholar William M. Landes wrote that he (Landes)

> was genuinely interested in explaining legal rules and doctrine from an economic perspective. Coase was not. He believed that knowledge of law and legal institutions was valuable because it helped one understand how explicit markets truly worked ... (Landes, 1997)

That sentiment, of course, would place Coase squarely in the intellectual traditions represented by old economic history.

For whatever reasons, new economic historians were, on balance, less interested in legal, political and constitutional issues, particularly at the macro level. Either such issues were ignored, on the grounds that for the regions and time periods under investigation such conditions were relatively stable, or had variations whose effects were swamped by variations in other (technological or demographic) variables; or attempts were made to explain change with reference to technological and demographic models which did not in turn make reference to institutions as givens within the explanatory model. One of the most ambitious attempts along these lines was that by North and Thomas, which attempted to account for eight centuries of European development in 158 pages, trying to appeal only to technological and demographic variables, and avoid reference to 'ad hoc' political or institutional conditions or changes as causal factors in their own right (North and Thomas, 1973; Field, 1981).

Why this aversion to explicitly recognizing institutional factors as both consequential and in part exogenous? Probably because there was a sense that to do so was to allow the camel's nose under the tent, to take the first step down the slippery slope of adhocery whereby the methodology and concerns of the old economic history would reintroduce themselves. If one analyzes the North and Thomas argument carefully, however, one finds that they are in fact, and scarcely surprisingly, forced to violate their methodological strictures in order to tell their story. In subsequent works North appears to acknowledge this, by explicitly discussing the impact on behavior of such factors as ideology (North, 1981, 1990) but the underlying methodologically individualist research program persists.

Having rejected or drastically downplayed the old economic history's concerns with entrepreneurship and institutions, the revolutionaries levied a more general methodological criticism. Rightly or wrongly, they stigmatized their older colleagues as having been satisfied with general and vague qualitative statements about causal factors in economic growth. Cliometricians insisted that explanatory hypotheses be explicitly set forth and that scholars go beyond the simple qualitative statement that something mattered, to exploring exactly how much it mattered. For example, Robert Fogel and Albert Fishlow rejected the argument by W.W. Rostow and others that the railroad was 'critical' or 'indispensable' for nineteenth century American economic development on the grounds that one could not attach economic magnitudes to these qualitative evaluations (Fogel, 1964; Fishlow, 1965a). They explored the indispensability thesis about American railroads using a 'counterfactual methodology' by asking how much lower would have been GNP if the railroad had not been developed, in one case in 1859, in the other in 1890. The difference between GNP as it was in actuality and what it would have been in the absence of the railroad represents the 'social savings' of the innovation.

Historians objected strongly to the counterfactual methodology but, as noted, the charge that previous students of historical economics had been uninterested in measurement is unfair. What was true was that more conscious theorizing now made it possible to tease out new and intriguing inferences from quantitative data, and helped direct researchers towards particular (and sometimes different) magnitudes. In retrospect, the indictment of old economic historians as uninterested in measurement and prone to set forth casual, qualitative and empirically unsubstantiated causal hypotheses could with rather more justification have been levied at many of the mathematical theorists then coming to dominate economic departments.

Is this entirely coincidental? If one takes the *methodenstreit* of the early 1960s at face value, it concerns how theoretically self conscious economic history should be. But perhaps old economic historians were merely stalking horses in a struggle as much within economics itself as to how empirically and historically focused the larger discipline would be. This is a speculative hypothesis, and the evidence supporting it inconclusive. McCloskey, writing in 1976, did not think this was so - (s)he felt that the attack on old economic history - certainly a far weaker adversary than those gradually importing math department values into economics departments, represented a political error, a failure by 'economic imperialists' to insure sufficient levels of domestic support (McCloskey, 1976, p. 438). It was easier to beat up on the old economic historians, but perhaps not wiser in the longer run. McCloskey also studied the percentage of column inches devoted to economic history in the main general interest economic journals in the United States (*American Economic Review*, *Quarterly Journal of Economics*, *Journal of Political Economy*) and found a significant downward shift between the periods 1925-44 and 1945-74; there is little evidence that this trend has been reversed in the last quarter century. Callender, of course, was already bemoaning this neglect in 1913.

Certainly Douglass North's comment in 1965 displays little ambivalence about who the heroes were:

> In summary, it is my conviction that we need to sweep out the door a good deal of the old economic history, to improve the quality of the new economic history, and it is incumbent upon economists to cast a skeptical eye upon the research produced by their economic history colleagues to see that it lives up to standards which they would expect in other areas of economics. (North, 1965, p. 91)

The irony is that comments such as those of North have, on balance, fostered neither hostility toward economic history nor a critical perspective on its practice. Rather they run the danger of feeding indifference. Such indifference impoverishes economic analysis as a whole, but its prevalence is not entirely the fault of mainstream economists. Both economics and economic history might well have been healthier had a different course been pursued by the revolutionaries, had they not embraced so uncritically the potential of neoclassical economic theory to illuminate historical inquiry. As McCloskey noted, 'The days are passing when the social sciences bridged the two cultures, literary and scientific, and economics burned the bridge long ago' (McCloskey, 1976, p. 439).

In general, economic theorists have actually been less enthusiastic about the power of their tools to illuminate historical inquiry (in some cases the skepticism seems to spill over to contemporary data as well) than have been new economic historians. Simon Kuznets summarized discussion of a variety of methods papers at the 1957 EHA annual meetings and noted, somewhat surprisingly, that

> Three theorists on a panel were 'rather skeptical of the value of greater integration of economic theory and economic history' …. On the other hand, at least some of the panel discussants and Gerschenkron from the floor, appeared to feel a greater need for reliance (in economic history) on economic theory... it did seem as if almost all economic theorists participating in the discussion were doubtful of the value of theory in work on economic history, while at least some economic historians felt that it [was] needed. (Kuznets, 1957, p. 550)

One detects a similar reticence on the part of theorists at a 1984 AEA session to propagandize too highly for the value of their tools in historical inquiry (Solow, 1985, p. 328).

5. Economic History, Douglass North and the Nobel Prize

The Nobel prize in economics was first awarded in 1969, and a review of recipients with work touching on economic history, law and economics, and their relationship provides perspective on the recent evolution of these disciplines. Simon Kuznets was the first of five honorees with such connections. Although Kuznets, who received the award in 1971, did not consider himself primarily a new economic historian, he trained many of its pioneers (such as Robert Fogel and Stanley Engerman) and his development of national income accounting reflected the new economic history's passion for measurement. Kuznets' work laid the foundation for modern empirical macroeconomics and macroeconomic history, even though, as noted by Goodrich above, the National Bureau tradition proceeded largely independently from economic history.

Five years later (1976) Milton Friedman received the award. From the standpoint of economic history, his greatest contribution has been in the area of monetary history, in particular the monumental 1963 work coauthored with Anna Schwartz, *A Monetary History of the United States* (Friedman and Schwartz, 1963). Friedman's work, and the reaction to it, played an important role in seating the study of cyclical macroeconomic phenomena squarely within the purview of economic history. Part of the success here, however, is attributable to a shift toward a more contemporary focus on policy issues by the National Bureau under the leadership of Martin Feldstein, with the consequence that historical studies of business cycles no longer play as central a role in the Bureau's agenda.

Fifteen years later, in 1991, Ronald Coase won the prize. Coase is a pivotal figure in the development of industrial organization as well as law and economics, and his contributions are discussed in detail elsewhere in these volumes. To the extent that economic historians have developed a more sophisticated approach to economic and legal institutions, his ideas have been important. Yet for reasons having to do with the sociology of the profession, his overall influence on economic history has been curiously weak. This is because scholars who start out self-identifying as economic historians, but become interested and proficient in a 'Coasian' type of analysis, generally begin publishing in law reviews or law and economics journals. There is little bias against an historical perspective in law; indeed one might argue that such a perspective is essential, and thus the barriers to exit in this direction are weak.

Finally, in 1993, two pioneers of the new economic history, Robert Fogel and Douglass North, received the prize. Fogel was a central player in two of the defining intellectual debates of the Cliometrics revolution, the study of how indispensable were the American railroads and, in a book coauthored with Stanley Engerman in 1974, *Time on the Cross*, the economics of slavery. This latter work, which attempted to turn upside down much received wisdom about how efficient the slave plantation system had been, and how well slaves were housed, fed, clothed and treated, engendered enormous controversy within the profession. In the last two decades, Fogel has pioneered in the use of historical demographic data to study variations and changes in the standard of living.

Douglass North made important empirical contributions to the economic history of the antebellum period, in particular his study of the US as an export-led economy from 1790 to 1860 (North, 1961). But his greatest professional success has been in trying to fill the gap left by the new economic history's attack on the older legal/institutional tradition. For many scholars outside of the profession, North *is* the new economic history. But there is a real question as to how central North's institutional work is to the new economic history or what exactly law and economics scholars should draw from it. How to reintegrate institutions with historical analysis without reembracing the 'errors' of the old economic history? As noted, in his 1973 work with Robert Paul Thomas, instead of making the study of the institutional framework the starting point of analysis, North struggled bravely to avoid appeals to ad hoc explanations, and instead to develop endogenous theories of institutions: that is, August 17, 1999 theories that would explain within a general (not historical or case-specific) framework, how and why institutions varied over time and space. The problem with such an approach is this. If we take the fundamental task of economic history to be understanding and explaining economic performance over time, then an interest in institutions - by old or new economic historians - is ultimately relevant only to the degree that it can shed light on such performance. To the extent that institutions are made endogenous - determined within the context of a general theory by more fundamental factors such as technology or demography, they cannot play an independent role in accounting for variation in performance.

These are the dimensions of the intellectual box that North - and scholars in many other disciplines - have struggled mightily to escape from. In a 1971 book coauthored with Lance Davis, Davis and North were willing to take the initial constitutional setup as given, but then went on to describe how private groups motivated by self interest seized profitable opportunities to make institutional innovations. Ex post one can certainly find many cases where such a perspective makes good narrative history. But whether Davis and North actually succeed in developing a general theory, an ‘explanatory framework’ that leads to refutable hypotheses, remains questionable. This was the view of Olmstead and Goldberg (1975), who also pointed out that many of the factors (such as public opinion) that North and Davis took as exogenous environmental factors, could in fact also be affected by expenditures of resources by private groups.

In the previously discussed *The Rise of the Western World*, coauthored with Robert Paul Thomas (North and Thomas, 1973), North tried to make endogenous the entire institutional structure of Western Europe, accounting for the breakdown of feudalism and the rise of modern economic and political institutions with reference only to more fundamental factors, such as demographic change and, to a lesser degree, new technology. The key component of their analysis is the labor to land ratio, whose rise, caused by population growth, is supposed to account for the demise of feudalism. But feudalism in western Europe broke down both as population rose, prior to the Black Death, and when population fell, after the plague. Moreover, falling population appears to have been associated with a recrudescence of feudalism in Eastern Europe, and it was precisely the scarcity of labor that Evsey Domar has used to explain the initiation and persistence of coerced labor regimes, both in Eastern Europe, and in the American South (Domar, 1970). Looked at objectively, one extracts from the work a theory that says that rising labor to land ratios lead to breakdowns of coerced labor regimes, except when they do not.

In later work, North (1981, 1990) has made valiant attempts to respond to such criticisms, but as one reads his contributions, one cannot help but continue to ask how much of the ‘explanation’ represents ex post rationalization for what has happened. One continues to be troubled by the paucity of testable hypotheses, as compared, for example, with his earlier empirical studies of the US as a (cotton) export led economy prior to the Civil War (North, 1961). That theory generated predictions that other scholars such as Fishlow (1965b) could then test (in a way not favorable to the North hypothesis) but in the process our understanding of antebellum economic history advanced.

Northian institutional economic history tends to be neither quantitatively empirical nor, because of its sweep, informed by detailed knowledge of law in its particulars. In contrast, scholars in the law and economics area *must* possess such understanding. In the United States, a minimum familiarity with the law of property, contracts, civil procedure and torts (at least at the level it is taught in a typical first year law school curriculum) as well as some Constitutional Law is in general a precondition for success in the field. Particularly in countries with an Anglo Saxon tradition, legal research and historical archival research have important similarities: the role of precedent (stare decisis) means that history matters (although in legal research the 'archives' are more likely to be computer searchable).

Legal scholarship, in general, has a solid grounding in the history and historiography of the law, as reflected for example in the enormous footnote apparatus of a typical law review article. Empirical economics and economic history, in general, have a firm grounding in the statistical record. Northian institutional economic history, written almost exclusively from secondary sources, often lacks much of either. Armchair economic theorists, and North, have a fondness for 'stylized facts' but stylized facts are sometimes simply wrong, and someone has to be responsible for trying to ensure that the generalizations are reasonable. It falls to economic (and legal) historians to be at least as concerned with accurately characterizing the stylized facts as explaining them.

North, as we have seen, has been a continuing proponent of revitalizing economic history through the introduction of the perspectives of economic theory, and the urge to generalize in his work often militates against detailed immersion in the particulars of a topic. But importing even a little bit of the attitude toward data of theoretical economists into historical, let alone legal or institutional scholarship, creates real problems. In an effort which has been relatively exceptional in recent years, however, Wallis and North (1986) did try empirically to estimate the size of the transactions economy: that part of economic activity devoted to negotiating and transactions among individuals, as opposed simply to producing the goods or nontransactions services (like haircuts).

In reflecting on North's contributions, one is struck by the extent to which the interface between law and economics and economic history might benefit from more contributions such as McCurdy (1978). This article, although published in the *Journal of Economic History*, displays the legal historian's knowledge of constitutional and statutory law and attention to detail sometimes lacking in North's grand excursions. I mention McCurdy's article first because it is relatively obscure and second because it deals with private actions to achieve institutional change - in this case those of American corporations such as Singer to eliminate state created obstacles to interstate trade, and in so doing help forge a national market. Such actions in response to opportunities for gain are of course the main subject of Davis and North.

When all is said and done, however, North deserves credit for the indefatigable way in which he has worked successfully to reinject a concern with legal and institutional arrangement back in to the new economic history. What can law and economics scholars learn from North's sweeping forays into institutional and economic history? Probably not too much about law and economics. But his work can open up for them a better understanding of the broad macroeconomic concerns about growth and development that are second nature to practicing economic historians and remain at the core of the discipline. Interested readers should consult North's numerous publications (see bibliography) as well as surveys such as Libecap, (1978, 1986).

Returning to Fogel, the publication of *Time on the Cross* in 1974 engendered enormous publicity and an equally massive intellectual reaction (see David et al., 1976). Looking back from the perspective of the late 1990s, the controversy over slavery in the 1970s, which continued a conversation begun by Conrad and Meyer in 1958, was in a very real sense the beginning of the end of the new economic history as a revolutionary movement (Conrad and Meyer, 1958). There have been few contributions in the last two decades that have engendered anywhere near as much heat, with the possible exception of relatively recent work on technological lock-in (Arthur 1994; David, 1985).

This reduction in heat has not been entirely regrettable. Some of the criticisms of the old economic history evidenced more bluster than substance, and when the dust settled and these critiques were subject to more dispassionate analysis, were found to have weak intellectual foundations. Although there were a few more displays of revolutionary rhetoric in the late 1970s, the field began to exhibit more intellectual maturity. The sense of superiority that economic historians trained as economists maintained *vis à vis* their colleagues trained as historians began to weaken, and a rapprochement of sorts took place at least with those historians who remained active in the Economic History Association because of their interest in the application of social science methods to history.

Although mathematical symbols and econometric results continue to appear frequently within the pages of economic history articles, text and narrative hold their own as important parts of the exercise in persuasion. And so an uneasy methodological truce has been reestablished, with economic history in the late twentieth century perhaps now poised to provide object lessons both to historians and to economists about how to conduct empirically based, analytically sophisticated inquiries into social phenomena.

But the fundamental issue of the role of legal and institutional analysis within economics and the role of economic historians in addressing such phenomena remains unresolved. The finesses simply have not been successful. Perhaps the scholar who came closest to striking the right balance in recent decades was Jonathan Hughes. Hughes, although a new economic historian, was always something of a maverick, publishing entrepreneurial history when it was no longer fashionable, and, when it was no longer fashionable, continuing to treat the institutional environment as consequential and at least partially exogenous, and thus deserving of detailed attention in its own right (Hughes, 1966, 1977). His approach to institutions can also be appreciated by reading the first sections of his textbook (Hughes, 1990).

6. What have been the Contributions of the New Economic History in Recent Decades?

The best current overview of the contributions of the new economic history to the economic history in the United States is Atack and Pasell (1994). This is the second edition of a volume that appeared first in 1979 as Lee and Passell. The second edition is more comprehensive and balanced; the first, for example, devoted almost a third of its text to slavery, reflecting the extent to which that topic dominated discussion in the 1970s. For a survey of the state of the art in economic history circa 1972, readers should consult Davis, Easterlin and Parker, 1972. For a textbook on American topics with a somewhat more chronological approach, see Hughes (1990). Although this essay focuses largely on American topics, readers interested in what the new economic history has accomplished for the historiography of British growth should consult Floud and McCloskey (1994). For a modern survey of the role of technological change in economic development, see Mokyr (1990). For membership lists, online book reviews, sample syllabi, discussion groups, and more, consult the website maintained by the Cliometrics society at http://cs.muohio.edu. The Cliometrics association also publishes a newsletter, which in recent years has printed interviews with new economic history pioneers.

Identifying the significant post-slavery controversy developments in US economic history is difficult. Southern agriculture has continued to figure, with important books by Ransom and Sutch (1977) and Wright (1978, 1986). Ransom and Sutch (1977) and Wright (1986) both focus on the post bellum period, as do Alston and Ferrie (1985). The greater interest in post Civil War topics was reinforced by the publication in 1977 of Alfred Chandler's *The Visible Hand*. The book was influential not only because of its masterful reinterpretation of the dependence of the rise of the modern business enterprise on new communication and transportation technologies (the telegraph and the railroad), not only for the stimulus it has given to the study of post Civil War topics, but also because it was significant in reintegrating the sophisticated study of business history into the agenda of economic history (Field, 1987a; Temin, 1991; Lamoreaux and Raff, 1995; Lamoreaux, Raff and Temin, 1998).

The 1980s also saw an emphasis on the systematic analysis of height by age data to shed new light on variations in standards of living across time and space. This has been part of a broader increase in the interests of new economic historians in historical demography (Steckel, 1995). Another notable development has been increased interest in the phenomenon of technological lock-in - the idea that economies might be subject to technological hysterisis and thus path-dependent. The argument is that accidents of the past can matter, determining everything from the shape of a dollar bill to our dependence on internal rather than external combustion (steam) engines for road transport (David, 1985; Arthur, 1994). While this literature has developed largely out of an interest in technological trajectories, it has great potential relevance for the study of legal structure and institutions. Indeed, since there is broader variation in legal structures at moments in time than there is in technological practice, lock-in effects might be more significant in the analysis of law than in technology per se. This is largely an unexplored area (Field, 1991), however, and the empirical significance of technological lock-in remains under debate (Liebowitz and Margolis, 1990), although it has figured heavily in the 1998 Department of Justice antitrust proceeedings against the Microsoft corporation.

Other major trends in the 1980s and 1990s have included an emphasis on what might be called applied labor economics using older data - with analysis applied to everything from the traffic in indentured servants in the colonial period (Galenson, 1981) to the earnings of women (Goldin, 1990). But macroeconomics has not been entirely eclipsed, particularly in studies of the Great Depression. Its history remains in an unsettled state, but not for want of attention, with a new international perspective (Eichengreen, 1992; Temin, 1989), which emphasizes the deflationary consequences of failure to abandon gold as well as new work by Field (1992) and Romer (1992) exploring internal reasons why the depression in the United States was so long and a number of contributions focusing on the role of financial structure (see Calomiris, 1993 for a review of recent research on financial aspects of the downturn).

In other work Romer has questioned the conventional wisdom that the post-World War Two period has in fact been less volatile than prior periods. Romer's methodology was thought provoking, but the contribution is notable also because it signals the degree to which, whether on the microeconomic or macroeconomic side, the great unexplored frontier for economic historians lies in the post-World War Two period. It is symptomatic that the Atack and Passell survey stops in 1940, and the postwar sections in standard economic history texts tend to be relatively weak. When the new economic history began in the late 1950s, the end of the World War Two was barely a decade in the past, and consequently it was hard to think of the postwar period in historical terms. More than half a century has now elapsed since 1945, a half century that will benefit in the future from the detailed attentions of economic historians. Such work will likely cause us increasingly to rethink interpretations of prior periods.

7. Conclusion

Scholars in law and economics should read economic history not so much because of what it will teach them about law, but because of what it can teach them about economies. In spite of all the methodological writing about the differences between old and new economic history, one constant throughout the century during which the subdiscipline of economic history has had a distinct existence has been an empirical concern with the process of economic growth and development. This theme unifies the practitioners of the German historical school, with their tastes for inductively developing stage theories of growth, through the old economic history up through and continuing with the new economic history. The general understanding of the trajectories of growth and development, and the creative use of the framework of economic theory to account for these are valuable complements to the knowledge base typically possessed by the law and economics scholar. So too is the consistent approach to data and measurement - particularly because the study of law and economics, with the possible exception of studies of criminal law, is not in general empirically quantitative.

The contributions of the new economic history to legal analysis per se are less clear, with the possible exception of recent work on lock in. First of all, law and economics scholars will already be familiar with much of it, because individuals who may have begun in economic history have mastered the body of knowledge possessed by law and economic scholars and have consequently published in *Journal of Law and Economics* or various Law Reviews. A search of the Econ Lit database (published for the American Economic Association by Silver Platter) for economic history and law brings up over a thouand citations, but most of them are *not* in economic history journals.

Law and economics researchers interested in institutional and legal structure per se may actually find more of interest in the writings of old economic historians. But for economic historians, the interest in law and institutions has, at least in principle, always been instrumental - the payoff comes in understanding how variation in such structures may affect performance. Naturally, time in the classroom and time in the library is limited and old economic historians can be faulted for sometimes appearing to lose steam after describing the changing institutional structure, perhaps assuming that the connections with economic performance were obvious. They are not. On the other hand, new economic historians have often ignored the institutional structure or tried to make it endogenous. It is in understanding the links between legal and institutional structure and performance that lie the greatest challenges to scholars in law and economics and economic history.

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