The integration of economic history into economics

Robert A. Margo, [1]✉

Phone 1-617-353-6819
Email margora@bu.edu

[1] Department of Economics, Boston University, 270 Bay State Road, Boston, MA, 02215 USA

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Abstract

In the USA today the academic field of economic history is much closer to economics than it is to history in terms of professional behavior, a stylized fact that I call the "integration of economic history into economics." I document this using two types of evidence—use of econometric language in articles appearing in academic journals of economic history and economics; and publication histories of successive cohorts of Ph.D.s in the first decade since receiving the doctorate. Over time, economic history became more like economics in its use of econometrics and in the likelihood of scholars publishing in economics, as opposed to, say, economic history journals. But the pace of change was slower in economic history than in labor economics, another subfield of economics that underwent profound intellectual change in the 1950s and 1960s, and there was also a structural break evident for post-2000 Ph.D. cohorts. To account for these features of the data, I sketch a simple, "overlapping generations" model of the academic labor market in which junior scholars have to convince senior scholars of the merits of their work in order to gain tenure. I argue that the early cliometricians—most notably, Robert Fogel and Douglass North—conceived of a scholarly identity for economic history that kept the field distinct from economics proper in various ways, until after 2000 when their influence had waned.

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Keywords

Economics
Economic history
Integration
Labor economics
Econometrics
Overlapping generations
Scholarly identity

JEL Classification

A14
N01

1. Introduction

In terms of professional behavior, the academic field of economic history is far more economics than history as it is practiced today in economics departments in the USA. Freshly minted economic historians with Ph.D.s in economics write dissertations that are collections of essays, sometimes unrelated, with a principal job market paper, just like in other fields of economics, rather than books-in-waiting as in history. Many of the freshly minted will have studied for the Ph.D. in departments where there is a research group of multiple economic historians and, perhaps, fellow travelers; dedicated research seminars; and routine doctoral production in the field—again, similar to other fields of economics.[1] Senior faculty mentors in these departments counsel their Ph.D. students to structure the thesis research—for example, which topics to choose, which techniques to use, and so on—with the aim of developing the skills necessary to produce articles on a regular basis for refereed economics journals, ideally the most prestigious general interest outlets such as the *American Economic Review* (AER), *Journal of Political Economy* (JPE), or the *Quarterly Journal of Economics* (QJE). Once the degree is on the horizon, the literally new economic historian will seek employment as an economist—for example, a tenure track position as an assistant professor of economics—for which there is an active and efficient labor market (Abramitzky 2015).

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In addition, compared with just a few decades ago scholarly identity in economic history today falls on a continuum. There are well-known economic historians who

are visible in other subfields of economics along with economists who earn fame and fortune in these other subfields but who conduct research in economic history from time to time.[2] Economic historians are appointed to prestigious offices or committees in the American Economic Association (AEA), serve on the editorial boards of mainstream and field journals in economics, are nominated for and sometimes win major economics prizes, and provide economics expertise as public servants. These and other related features of professional economic history in the contemporary USA are markers of a stylized fact that I call the "integration of economic history into economics."

I am far from the first scholar to document the integration of economic history into economics. There are various ways to do so (see, for example, Whaples 1991, 2002), and some of what I do is related to Abramitzky (2015).[3] In the main I shall offer two types of evidence.[4] The first derives from automated searching of digitized journal articles using Google Scholar (GS) to produce indices of instances of the use of econometric language that range between zero and one (higher values implying more frequent appearance of econometric language in the relevant journal). The searches are programmed such that items (for example, articles) in which multiple words or phrases appear are counted only once and that the resulting indices are comparable across the journals. There are two journals in economic history, the *Journal of Economic History* (JEH) and *Explorations in Economic History* (EEH); two in labor economics, the *Industrial and Labor Relations Review* (ILRR) and the *Journal of Human Resources* (JHR); and one so-called top-five general interest journal, the *American Economic Review* (AER). All five journals exhibit a marked increase over time in use of econometric language, which is no surprise because economics has become more empirical. What is more surprising is that economic history lagged somewhat behind economics in general—the AER—and labor economics—ILRR and the JHR—in particular. In the early 1950s there was little or no econometrics to speak of in the ILRR or the JEH, but there already was a fairly sizeable gap between both and the AER. There was a sharp rise in econometric language use in the AER between the early 1950s and mid-1960s, at which point the upward trend flattened out. The ILRR caught up to the AER by the mid-1970s; and the JHR, which began publishing in the mid-1960s, surpassed the AER in econometric language use very quickly. Notwithstanding the lag just mentioned, it was quite reasonable for the eminent econometrician, Nobel Laureate, and occasional economic historian James Heckman (1997, p. 404) to remark in 1997 that "[c]liometrics has prevailed … [e]conomic history has been integrated into mainstream economics, statistical and econometric tools are widely used in conducting systematic empirical analyses of historical topics[.]"

The second type of data is the early publication histories of economic historians with doctorates in economics, and of labor economists. By "early" I mean the first ten years post-Ph.D., and by "publication history" I mean the classification of published research into various categories related to economics and the scholar's field (see "Appendix 2"). I consider two non-random samples of economic historians and one of labor economists, all arranged by decade of Ph.D. cohort. Sample #1 consists of prominent economic historians, where "prominent" refers to past EHA presidents, JEH or EEH editors, fellows of the Cliometric Society, and scholars who obtained tenure at leading economics departments or equivalent business schools. Sample #2 consists of individuals who convened a dissertation session at the annual meetings of the EHA. Conveners are selected by the EHA president and therefore reflect the preferences of the chooser. Thus, while there is some overlap between samples #1 and #2, it is less than perfect, and sample #2 is somewhat more representative of the economic history profession than sample #1. The labor economists are all prominent, being Fellows of the Society of Labor Economics (SOLE).[5]

Among economic historians, I find a long-run increase in the fraction of articles published in economics journals, especially the top-five (for example, the AER). The increase in economics publishing is offset by a decrease in publishing in economic history and other non-economics academic outlets, along with a decrease in the probability of publishing a monograph. However, while these trends are somewhat present in the 1980s and 1990s Ph.D. cohorts (relative to the 1970s), there is a structural break for those obtaining their doctorates after 2000. Thus, just like the indices of econometric language, the publication histories suggest a lag in the pace at which economic history integrated into economics. The lag is confirmed when the publications data for economic historians are compared with those for SOLE fellows. Regardless of when they received their Ph.D.s, SOLE fellows have always published a larger share of their articles in top-five journals than have economic historians, and a smaller share in labor economics journals than economic historians have in economic history outlets. But the gaps here narrowed over time, particularly for the post-2000 Ph.D. cohorts of economic historians.

I offer a simple analytical framework to account for the integration of economic history into economics. The components are initial conditions, labor market structure/incentives, and selection.

The initial conditions are those in effect ca. the mid-1950s. Then, as now, economic historians in the USA sat either in departments of history or economics, not in

stand-alone departments of economic history.[6] The discipline of economics was growing rapidly, and there was an emerging demand within economics for evidence on the historical development of rich countries like the USA, both to serve as grist for policy advice to developing countries and as the factual basis for growth theory. I see the cliometrics revolution as a supply-side response, one that could only come from scholars trained in economics departments. Since then, the demand in economics for economic history has waxed and waned for various reasons, but it has always been present in one form or another.[7]

The second component is labor market structure and incentives. By "labor market structure" I am referring to the overlapping generations nature of academic labor markets. Senior scholars train junior scholars, and also evaluate junior scholars for tenure and promotion. The incentives are those associated with labor market success—tenure, promotion, fame; and, on occasion, fortune. Junior scholars value success and therefore pay close attention to whatever the incentives happen to be in the discipline in which they are employed when allocating their time, talents, and resources. While I do not model the process explicitly, I assume that the standard used in evaluating junior scholars is an equilibrium of a dynamic game consisting of interactions between senior faculty inside and outside the field, reflecting their respective beliefs about what is meritorious, and their relative bargaining power in the decision process.

The final component is selection. While economics had already begun to turn analytical and quantitative relative to history before World War Two, disciplinary differences in Ph.D. training in the early years of the cliometrics revolution were not as dramatic as they would later become. History, too, had a brief fling with social science methods that lasted well into the 1970s. In principle and in reality, therefore, two individuals could enter Ph.D. programs in economics or history in, say, the mid-1970s; come of the other end as economic historians, one with a Ph.D. in history and the other, a Ph.D. in economics; and, conceivably, have similar career trajectories.[8] But academic history turned sharply away from quantitative methods in particular and social science more generally starting in the mid-1970s while economics continued to become more technical. Today, individuals who matriculate into economics Ph.D. programs are highly selected for particular traits, such as aptitude for advanced mathematics and related quantitative skills that are rewarded in professional economics, not in professional history. The typical young economic historian trained in an economics department today has little interest, and virtually no labor market incentives to develop a professional reputation outside of economics.

Although my framework can broadly explain the integration of economic history into economics, the lag in econometric usage behind labor economics and economics more generally as well as the structural break in publication histories for post-2000 Ph.D.s sit uneasily. Cliometricians are accustomed to celebrating their revolution *per se* but unaccustomed having to explain why the revolution did not happen more quickly.

The integration of economic history into economics can be seen as an interesting example of the evolution of scholarly identity of a subset of scholars within a wider academic discipline. While all subfields of economics share content and style, there are intellectual boundaries, sometimes fluid but typically clear, that mark whether an individual self-identifies primarily as a "labor economist" or a "macroeconomist." For the most part, these boundaries exist entirely within the discipline of economics—the individual in question is an economist specializing in, say, public economics. Economic history is different, however, in that the boundaries cut across two intellectual disciplines, history and economics.[9]

Although the question of identity for economic historians pre-dates the cliometrics revolution, there is no doubt that it came to the fore when the revolution occurred. One initial, and powerful impulse was that, to be successful, cliometricians needed to meet certain professional norms in history as well as economics. To accomplish this, it was necessary to walk the walk and talk the talk—publish books as well as articles, perhaps learn a foreign language or two, visit the archives regularly, and so on. For the purposes of this paper I associate this first impulse primarily with Robert Fogel, but it was widely accepted by the early cliometricians, not just Fogel.

A second impulse was the cliometrics needed to be something more than just history plus regressions and/or empirical economics with older and oft-poorer quality data. Pointedly cliometricians needed to play the role of the gadfly, studying and documenting crucial factors in growth and development that economic theory had failed to (yet) incorporate. This impulse is usually associated with Douglass North, but it, too, had many adherents. Taken together, the two impulses created an intellectual "space" in which the early cliometricians, and their students, could function.

Because cliometrics began to ascend at a time—the 1960s—when American higher education was growing very rapidly, the robust labor market enabled the early cliometricians to gain tenure, promotion, and influence relatively quickly. Tenure being what it is, this influence—and therefore, the two impulses just noted—

remained strong into the 1980s and even the 1990s, but waned after 2000, once the early cliometricians began leaving the scene. This institutional structure, in other words, gives rise to a timing that help explains the lag in econometric language use as well as the structural break in publication histories.

The integration of economic history into economics has brought tangible benefits to economic historians with Ph.D.s in economics employed in the USA—an active job market with the relatively high salaries and good working conditions that come with an economics doctorate. Should we expect integration to continue for the foreseeable future? Taking a cue from Romer (1994) I conclude the paper by speculating that one possible end game of current trends is that economic history might disappear as a separate field in economics. Instead, historical topics for which economists profess an enduring demand would become part and parcel of each field, but other topics not so privileged would disappear from economics scholarship and the economics curriculum, as would generalist courses in economic history taught in economics departments. Some of these topics might covered elsewhere in the academy minus the economics, which could be a worse outcome from a social welfare point of view. I argue that the emergence of the so-called "History of Capitalism" in academic history is a case in point. In a nutshell, there are costs to integration as well as benefits.

2. Background and empirical analysis

I set the stage for my empirical analysis by reviewing the history of economic history in the USA prior to the arrival of cliometrics. As a professional discipline the origins of economic history can be dated to those of academic economics and academic history in the late nineteenth century (Mejia 2015; Lamoreaux 2016). Economic historians worked either in economics or history departments but were sufficiently small in number and similar in outlook and style that it never made economic sense to establish separate departments of economic history in the USA. Whether employed in history or economics, the economic historians of the early twentieth century were largely united by topic and methods. This can be seen easily by inspecting virtually any of the economic history articles from the period that were published from time to time in the main economics journals of the time, such as the *AER*, *JPE*, or *QJE*, which were similar in format and content to articles on economic history topics appearing in, for example, the *American Historical Review* which, like the *AER*, *JPE*, and *QJE*, was founded in the late 1890s.

Beginning in the 1920s, economic analysis began its long march toward the centrality of quantitative and mathematical methods. Founded in 1933, the journal *Econometrica* was intended from the start to be the outlet for such work. As the discipline shifted toward formal models and quantitative analysis, history began to take a back seat in economics, and fewer economic history articles appeared in the principle economics journals, especially the *AER*.

There were some voices in the opposite direction. Economic historians working in economics departments before World War Two advocated for the greater use of historical evidence in economics and, simultaneously, greater use of economic methods in historical analysis. An especially important voice was the National Bureau of Economic Research, which was founded in 1920 for the purpose of developing a solid statistical basis for understanding changes in the American economy. Another was the EHA, established in 1941 as joint venture of the American Historical Association and the AEA.

These developments aside, a snap-shot of the economic history profession at mid-century gives the distinct impression of an intellectual backwater. The topics in play were largely those that had occupied the profession for the preceding half-century. The *JEH*, which had been established in 1941 along with the EHA, was entering its second decade of publication. By the early 1950s regular issues appeared three times a year, with a fourth devoted to the "The Tasks of Economic History" consisting of papers given at the annual EHA meeting and summaries of their discussion. A typical issue had three main articles; a similar number of shorter notes, comments, or review articles; and book reviews—roughly 100 pages in length, including front and back matter. Reading an issue while taking the train from, say, New Haven to Manhattan would have been easy—not so today.

Nonetheless, change was in the air. The economics profession began to grow rapidly after World War Two. Some of this growth can be attributed to rising demand for college teaching of economics, fueled by the GI Bill, and also to an expanded role for economists in government. The trends of greater use of mathematics and statistics, already present before World War Two, accelerated. Research output began to grow, as reflected in a substantial annual increase in articles submitted to flagship journals like the *AER* (Margo 2011).

Of singular importance for economic history was the emergence of a specific intellectual demand from economics for historical evidence on growth. This demand had three fundamental sources. The first was the National Bureau of

Economic Research, previously mentioned. Led by Simon Kuznets, a research group at the NBER was deeply immersed in developing the infrastructure for extending the measurement of national accounts back in time and place. The second source was the Cold War, which created a pressing need in the West to provide policy advice to developing nations lest they fall into the Soviet orbit. The third source was growth theory which originated before the War but which received a large intellectual boost with the publication of Solow's (1956, 1957) fundamental papers. While doctoral programs in economics at the time often included some coursework in economic history the courses were very traditional with little connection to the advances in theory and statistics occurring elsewhere in the discipline.

The time was ripe, therefore, for a revolution to take place, in which the tools of economics were put to use to provide a body of evidence and rigorous argument that would supply the growing demand for historical evidence useful in economics. As I shall argue later, because this demand had its origins in economics, it could only be supplied from within. This was the cliometrics revolution.

2.1. Usage of econometric language

I track instances of the use of econometric language over time in five academic journals—the JEH, EEH, ILRR, JHR, and the AER. The goal of this analysis is to provide a compact way of measuring when economic history journals began to look like labor economics journals in particular and economics in general (for example, the AER), in terms of econometric language use.

I search for instances of econometric language with automated text processing of digitized files using the advanced feature of Google Scholar (GS) as the search engine. The searches are programmed in a multi-step process using Boolean logic so as to yield two integer counts—a numerator and a denominator—for each journal-year pair. The numerator is intended to capture the number of discrete items (for example, articles or comments) in a journal-year pair in which econometric language is used. The denominator is intended to be the population at risk—items in which econometric language in principle could be used.

Ideally, the searches would begin with an exhaustive list of words and phrases that objectively characterize what is meant by "econometric language." To my knowledge, no such list exists. However, in practice it turns out that a surprisingly brief list can be used, because when econometrics is objectively present—for example, there is a regression equation somewhere in the paper—the usage of

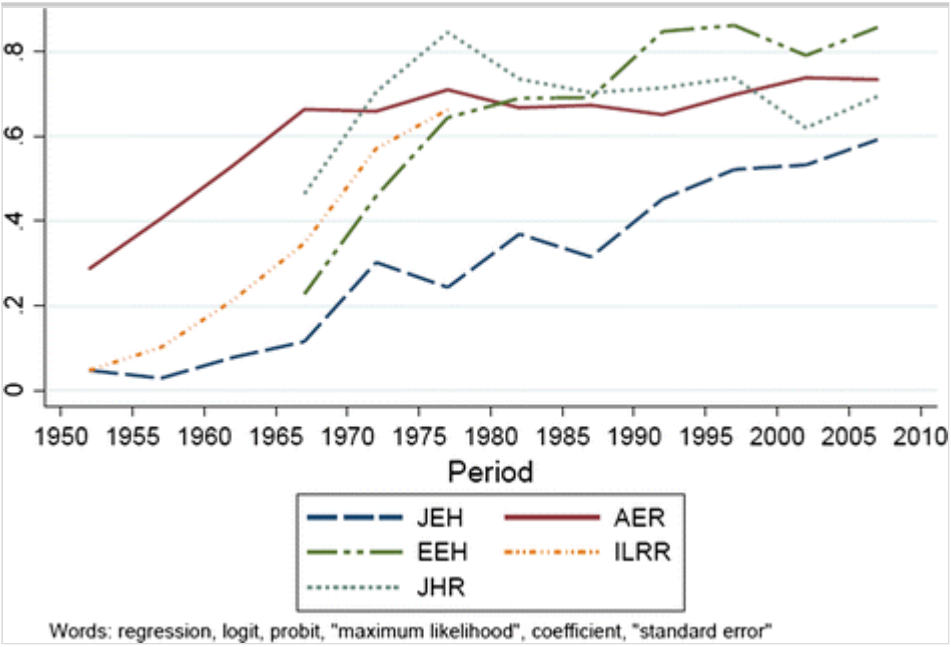
various words and phrases is highly correlated. For example, if the phrase "instrumental variable" appears in a paper, it is highly unlikely that the word "regression" does not also appear, so searching for "regression" sweeps in items in which "instrumental variables" appears. By judiciously choosing a short list of common words and phrases, I have found that the automated text processing will determine the numerator and denominator counts with a very high degree of accuracy. My list consists of "table," "regression," "logit," "probit," "coefficient," "standard error," and "maximum likelihood."

The denominator consists of items in a journal-year paper with one or more of these words or phrases; the numerator is the same, except the word "table" is excluded. Thus, in effect, the population at risk consists of items in a journal-year paper in which the word "table" appears, or one or the other words in my list if "table" is not used. By design, the count in the denominator is greater than or equal to the count in the numerator, so the ratio of the two falls within the closed interval [0, 1]. In order to smooth out fluctuations, I average the data over five-year periods, beginning with 1950–1954 and ending with 2005–2009. The averages are centered on the mid-points of each period (e.g. 1952 for 1950–1954).

Figure 1 graphs the indices by journal. GS is linked to JSTOR. For reasons that are not documented in the program and, therefore, I cannot explain, GS is unable to process items in the JSTOR digitization of ILRR after the mid-1970s. However, this has no effect on my substantive conclusions because, as is clear from the figure, by the mid-1970s the index for the ILRR reaches the level observed for the AER (see below).

Fig. 1

Five journals: percent at risk using econometric words.
Source: see text



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The figure reveals that, in the early 1950s, econometrics was already present in the AER, albeit at much lower frequency than would be the case just a decade or so later. No econometrics to speak of, however, is present in either the JEH or the ILRR. Starting in the early 1950s, econometrics begins to feature more prominently in the AER, and the journal's index rises steeply. A plateau is reached by the mid-1960s, from which point the index continues to rise but more slowly. The presence of a plateau is not surprising—the AER is a general interest journal and, as such, devotes space to economics research which is applied in a broad sense—and thus, the word "table" is present—but for which econometrics is not the relevant toolkit.

The figure also reveals a take-off in the use of econometric language in the ILRR starting in the early 1950s such that the index reaches the level observed in the AER by the mid-1970s. In the case of the JHR, which was established in the mid-1960s, econometrics is well represented from the very start, and the index reaches the AER level almost immediately. The index for the JEH also begins to rise around 1960, but the rate of change is slower than in the ILRR or the JHR and the JEH index always lies below those for the two labor economics journals (or, for that matter, the AER). EEH began publication in the mid-1960s and, when it did, its level of econometric use exceeded that of the JEH, but was below the other journals. EEH caught up to the AER and JHR by the 1980s and remained more "econometric" than the JEH to the end of the sample period.

The comparison between economic history and labor economics is telling because, shortly after WW2, the ILRR was the academic organ of traditional labor economics, which was heavily institutional at the time. Then, labor economics experienced the human capital revolution, the empirical fruits of which seem to have diffused more rapidly than the analogous harvest in cliometrics, judging by my indices.

2.2. Early publication histories

I examine the early publication histories of successive Ph.D. cohorts of economic historians and labor economists.[10] By "early," I mean the first decade post-Ph.D.—so, for example, if an individual received her Ph.D. in 1985, the relevant period is 1985–1995—and by "publication history," I mean the distribution of publications by publication type.[11] These types are monographs and articles in refereed journals. I focus on early publication histories because this is what affects the likelihood of scholarly tenure, which is a central element in my explanatory framework (see below).

For economic history I study two samples and one sample for labor economics. Sample #1 of economic historians and the sample of labor economists are scholars who meet a set of objective criteria for prominence.[12]

Sample #2 of economic historians consists of conveners of the dissertation session at the annual meetings of the EHA. Beginning in the mid-1960s, the EHA regularly sponsors a session at the annual meeting at which new Ph.D.s make brief presentations of their thesis research. Two prizes are awarded, one for the best dissertation in American economic history and similarly, one for the best dissertation on a non-American topic (for example, the British Industrial Revolution). Typically, three dissertations are chosen per category for presentation at the meetings, so six in all. There are two conveners, one for each category, and each of whom selects the winner ("best") in their category. In almost all cases the conveners publish some version of their comments on the dissertations that they presented at the meetings in the *Journal of Economic History* and thus the conveners can be identified by name.

The conveners are selected by the EHA president. There are no explicit selection criteria that must be followed. In fact, however, even a minimal glance at the list of conveners makes it obvious that the choices are made with care. Tastes differ—some presidents favor their own students, while others might showcase someone

whom they or a significant fraction of the field regard as up-and-coming. Because of the latter effect, the list of conveners overlaps somewhat with sample #1.[13]

For the purposes of this paper, publication data are from CVs, as these are very convenient to use, accurate and complete.[14] I obtained most CVs on-line for the economic history samples ~~#1~~ and entirely so for the sample of labor economists. A few economic history CVs ~~for sample #1~~ came from an archive maintained by the Cliometrics Society (sample #1) or by personal correspondence (sample #2). Sample #2 of economic historians (the convener sample) is less complete than it might be because some of the conveners have died and their CVs are no longer available. For the dissertation sessions held from the mid-1970s to the present, the convener sample is much more complete and I believe any survivorship bias is very small.

For each CV I first determine if the author published any monographs during the allotted decade; and, if so, the number.[15] Next, I classify journal articles into categories. For the economic historians there are four categories: top-five economics journals, all other economics journals, economic history journals, and non-economics academic journals.[16] For the labor economists, the journal categories are top-five, labor economics, non-labor economics, and non-economics. Also shown (in brackets) is the total share of articles published in top-five or economics journals, for the two economic history samples; and the total share of articles published in top-five or non-labor economics journals, for the labor economics sample.

Table 1 shows sample statistics and sample sizes by Ph.D. cohort. The general patterns are fairly clear. Taking economic history sample #1 first, there is a downward trend in monograph production that begins in the 1970s cohorts; these cohorts also diverge from their predecessors in shifting article production away from economics journals. However, the 1980s cohorts mean-revert, and the 1990s cohorts increase their shares of articles in top-five and other economics journals. The trends are very modest, however, until we get to post-2000 Ph.D. cohorts, for whom there are discrete jumps, or structural breaks, in the shares of articles published in economics journals, particularly those other than the top-five.[17]

Table 1

Early publication histories: scholars in economic history and labor economics

Ph.D. decade	*P* (Book) > 0	Mean number of books if > 0	Mean number of articles	Percent of articles in top-five economics journals	Percent of articles in other economics journals	Percent of articles in economic history journals	Perce artic non- econ acad journ
Panel A: economic history sample #1: "prominent" economic historians[a]							
1950–1969	0.81	1.23	10.6	10.1%	26.2% [36.3]	50.3%	13.4%
1970–1979	0.76	1.38	14.8	5.2	20.6 [25.8]	53.2	21.0
1980–1989	0.67	1.17	12.2	13.8	26.7 [40.5]	42.2	17.2
1990–1999	0.50	1.33	14.8	14.6	24.8 [39.4]	43.8	16.9
2000–2009	0.22	1.00	14.6	23.1	52.1 [75.2]	19.8	5.0
Ph.D. decade	***P*(Book) > 0**	**Mean number of books if > 0**	**Mean number of journal articles**	**Percent of articles in top-five economics journals**	**Percent of articles in other economics journals**	**Percent of articles in economic history journals**	**Perce artic non- econ acad journ**
Panel B: economic history sample #2: EHA dissertation conveners[b]							
1950–1969	0.67	1.33	7.4	7.5%	16.4% [23.9]	52.2%	23.9%
1970–1979	0.67	1.33	12.6	1.8	24.8 [26.6]	57.7	15.9
1980–1989	0.61	1.09	13.3	6.3	20.8 [27.1]	53.3	19.6
1990–1999	0.31	1.20	10.7	7.2	18.0 [25.2]	48.9	25.9
2000–2009	0.11	1.00	11.2	10.9	47.5 [58.4]	36.6	5.0

See "Appendix 1" for the names of scholars included in Panel A, Panel B and "Appendix 2" for the classification of journals into the various categories. []: sum of percent in top-five and other economics journals

See "Appendix 1" for the list of scholars included in Panel C and "Appendix 2" for the classification of journals into the various categories. []: sum of percent in top-five and other non-labor economics journals

Ph.D. decade	*P* (Book) > 0	Mean number of books if > 0	Mean number of articles	Percent of articles in top-five economics journals	Percent of articles in other economics journals	Percent of articles in economic history journals	Perce artic non- econ acad journ
Ph.D. decade	***P* (Book) > 0**	**Mean number of books if > 0**	**Mean number of articles**	**Percent of articles in top-five economics journals**	**Percent of articles in other economics journals, non-labor**	**Percent of articles in other economics journals, labor**	**Perce artic non- econ acad journ**
Panel C: fellows of the society of labor economics (SOLE)[c]							
1950–1969	0.55	1.4	11.2	42.9%	34.8% [77.7]	13.2%	8.9%
1970–1979	0.19	1.67	16.6	33.8	27.1 [60.9]	24.0	15.0
1980–1989	0.18	1	16.9	34.7	36.5 [71.2]	24.0	4.9
1990–1999	0.09	1	13.6	47.3	36.0 [83.3]	13.3	3.4
See “Appendix 1” for the names of scholars included in Panel A[a], Panel B[b] and “Appendix 2 classification of journals into the various categories. []: sum of percent in top-five and other economics journals							
See “Appendix 1” for the list of scholars included in Panel C[c] and “Appendix 2” for the clas of journals into the various categories. []: sum of percent in top-five and other non-labor ec journals							

Panel B shows the results for the sample of conveners. Except for the 1970s cohorts the conveners sample is less likely than sample #1 to publish in economics journals. However, the trends are the same—a decrease in the frequency of monographs and an increase over time in the share of articles published in economics journals. Importantly, the conveners sample also shows structural breaks for the post-2000 Ph.D. cohorts.[18]

Panel C shows the results for the sample of Fellows of the Society of Labor Economics. There are three patterns worthy of comment. First, book publishing was not just the province of economic historians with Ph.D.s from before 1970; it was also fairly common among SOLE fellows, although not as common as among economic historians. However, book publishing among SOLE Fellows plummets

among the post-1970 cohorts, long before it does so among economic historians. Second, the share of articles published in top-five journals has always been quite high, accounting for between a third to just slightly less than half of all papers for the 1990s cohort. Third, as a corollary, the share of articles appearing in labor or non-economics outlets is always lower for the SOLE Fellows (holding the cohort decade constant) compared with the economic historians. That said, it is noteworthy that, in terms of the overall share of articles appearing in economics outlets, the post-2000 Ph.D. cohort of prominent economic historians (sample #1) looks more similar to the SOLE Fellows than any of the preceding cohorts of economic historians. Overall, the data for SOLE Fellows confirm the relatively slow pace of integration of economic history into economics, until the post-2000 Ph.D. cohorts.

3. Integrating economic history into economics: an explanatory framework

I sketch in words a simple analytical framework to explain the integration of economic history into economics. The framework has several moving parts—initial conditions; labor market structure; and selection. The theory pertains to labor market structure, so I start with this.

3.1. Labor market structure

On the supply side, there are individuals—scholars—who engage in scholarship and teaching; on the demand side, there are university departments that hire scholars. Departments and disciplines are equivalent, so there are departments of economics, of history, and so on. Departments (disciplines) are divided into fields—for example, labor economics is a field in Eeconomics, “Early Modern Europe” a field in Hhistory. Economic history can be a field either in economics or history (or both) but is not itself a discipline (see below).

There is an overlapping generations structure to the academic labor market. Scholars live for three periods. In period #1, a person enters a disciplinary Ph.D. program and devotes all of her time to human capital investment, eventually obtaining a Ph.D. that qualifies her for employment in a disciplinary university department. In period #2, she is hired as an assistant professor and devotes all of her time to research.

At the end of period #2, our assistant professor faces an up or out vote of the period #3 faculty at her institution. If a majority vote in favor, she advances to period #3 faculty—senior status or tenure; if negative, she takes an outside option. Upon

receiving tenure, she spends part of her time training Ph.D. students (period #1 scholars) and part of her time evaluating assistant professors (period #2 scholars) for advancement.

Each senior faculty member in department j has one vote in an up or out tenure decision. In deciding which way to vote the senior faculty member personally evaluates the research of the tenure candidate and also takes into account the opinions of the other senior faculty in the department and also external senior faculty in the same field as the candidate. These external scholars do not vote per se but offer their opinions as to whether the vote should be up or out. Thus, the outcome of the tenure decision depends on a weighted average of the opinions of internal senior faculty in the same field as the tenure candidate (if any), the external senior faculty in the same field whose opinions are solicited, and the opinions of internal senior faculty in other fields.

In evaluating a tenure case, I assume that senior faculty, whether internal or external, apply reference standards that are specific to the discipline, field, and their Ph.D. cohort.[19] These standards refer to content, method—for example, the use of econometrics—and form—journal articles versus books. Because the tenure decision is a disciplinary one, and all of the voting senior faculty have Ph.D.s in the relevant discipline, it is reasonable to hypothesize that, in equilibrium, the disciplinary component of the standards will be prominent and, to a first approximation, broadly similar across fields within a discipline. Thus, for example, candidates in fields in economics would be expected to offer a research portfolio in which multiple journal articles are present, for such has long been the disciplinary norm in economics. Whereas, in history, a tenure candidate would be expected to offer one (or more) books, because books are the scholarly norm in history.

Crucially, by making the standards depend on Ph.D. cohort of period #3 faculty I am introducing a lag structure in faculty opinion that potentially will feed back on the behavior of period #2 faculty and, consequently, tenure outcomes. To keep the time line squarely in mind, at date $T = N$, period #2 scholars up for tenure received their Ph.D.s in $N - 1$ and are evaluated by period #3 scholars who received their Ph.D.s in period $N - 2$.

3.2. Initial conditions, sorting, and the cliometrics revolution

I posit several initial conditions before running a thought experiment. The first initial condition is that there are departments of economics and departments of history, but no departments of economic history. Economic history is a subject that

exists in both types of department; however, an economic historian hired as a new assistant professor in an economics department still has to have a Ph.D. in economics (and vice versa, in a history department). Initially, however, the labor market for economic historians is in equilibrium such that the standards for promotion have a common component, regardless of whether the scholar is employed in history or in economics. As a concrete example, we could suppose that an economic historian in an economics department can present a book for tenure that substitutes for some quantity of articles in economics journals that would otherwise be expected of candidates in economics.[20]

Second, I assume that, Ph.D. training in economics requires investment in certain types of technical skills—for example, mathematics and statistics—which in the initial equilibrium have no special use or value for historians or for economic historians, whether the latter are employed in history or in economics departments. If individuals were identical *ex ante*, there would be a compensating differential for economic historians employed in economics department; instead, I shall assume that individuals differ in their ability or comparative advantage in the aforementioned technical skills. Those with a comparative advantage at learning technical skills sort into Ph.D. economics programs.

Third, a shock occurs at date $T = K$ that creates a new demand for the use of technical skills in economic history in economics departments. We can think of this shock as derived from research undertaken by period #3 faculty in economics departments when they were themselves junior scholars but who are not themselves economic historians—for example, in order to test a new theory of economic growth a long time series of GNP is necessary which, presently, does not exist but which requires technical skills to construct.

To run the model, note that the shock creates an incentive for period #2 economic historians at date $T = K$ to supply what is demanded, but this cannot come from economic historians employed in history departments because of the sorting assumption. The supply will necessarily come from period #2 economic historians employed in economics departments.

However, period #2 economic historians in economics departments will soon be evaluated for tenure. They are evaluated by period #3 economists and economic historians who received their Ph.D.s in period $K - 2$—that is, before the shock. The senior economists like what the "new" economic historians are doing because it uses technical skills to respond affirmatively to the demand instigated by the senior economists in $K - 1$. Senior economic historians, however, place no special value

on the use of technical skills because the skills previously were not used by them. To successfully advance period #2 economic historians in time period K must figure out how to package their research to appeal to two masters—both the senior economic historians and economists who are evaluating their work. The successful ones advance to tenure status, and help train period #1 scholars and evaluate period #2 scholars at $t = K + 1$.

At time $K + 1$ the period #2 scholars will now have to gain the support of period #3 economic historians as well as other senior economists. Period #3 economic historians will expect more technical work in the tenure portfolios they evaluate, because it was expected of them, but they will also expect that the work will appeal to historians because that, too, was expected of them. As the model continues to run, the content and form of the research portfolios of economic historians who are successful getting tenure in economics departments will become closer in content and form to other fields in economics.

The above describes the evolution of the model in response to a demand shock in economic history. Suppose, instead, that a technique demand shock occurs in, say, labor economics, in which the interdisciplinary forces at play in economic history are not present. Period #2 labor economists still have to convince their elders, some of whom may be reluctant—but, presumably, there are relatively fewer of these than in the case of economic history. We expect, therefore, that the demand shock will be absorbed more quickly if it is purely in economics rather than interdisciplinary.

To summarize thus far, the basic prediction of the model is that, as long as economic historians continue to be in demand in economics departments, there will be convergence in the tenure portfolios of period #2 faculty and economic historians employed in economics will increasingly "look like" other economists. This occurs because of the nature of the incentives for advancement and the overlapping generations structure of the labor market. The speed of convergence can be fairly quick, but ultimately depends on the precise nature of the standards employed in evaluating faculty and the relative weight given to the different "interest groups" among period #3 faculty. If, for example, disciplinary standards are strongly favored in the voting, convergence can be very rapid.

As noted above, one of my initial conditions is that economics has a technical component to the skill set, which induces sorting. If the technical component increases over time, differences in the skill sets between economic historians in economics and history departments will widen across cohorts. As a result, the

content, and possibly the form, will diverge, perhaps to the point where there will be little or no overlap, even though the field has the same nominal title in both disciplines.

4. Discussion

In a broad sense Fig. 1 and Table 1 appear consistent with my model. Econometric usage in economic history eventually converges with that in economics. Over time successive Ph.D. generations of economic historians who were employed in economics departments shifted their early publication portfolios toward economics publication outlets and away from economic history and non-economics journals and book publishing. Subtler features of the data, however, raise some puzzles. Econometric language use diffused earlier in economics than in economic history, which is consistent with the framework. However, more specialized methods diffused more rapidly in economic history once diffusion started.

AQ4

My framework reminds us that the first generation of cliometricians had to appeal to more traditional economic historians as well as economists who wished to encounter a more technical economic history. The diffusion of regression methods in economic history is a case in point.[21] To glean further insight into this diffusion, I have read through all of the articles in the *Journal of Economic History* in the 1960s that my text processing indicated the word “regression” was used in the econometrics sense. This is less work than might be imagined, because there are only 16 such appearances in the 1960s JEH, or roughly 1 in every 20 items in the population at risk.

In reading the articles, it is not just that they are infrequent; it is also that the manner in which econometric results are presented and discussed is different from the norm just a decade or so later. Typically, results are briefly discussed in the text and specific details, if any, reported in a footnote. Econometrics is never the main event and it is rarely organic, in the sense that one cannot imagine the same points being made another way. Authors go out of their way to downplay the novelty of regression.

Fast forwarding to the late 1970s and beyond, regression was still less frequent in economic history than in economics proper, but the gap was closing, and concomitantly, it was no longer novel and the presentation of results could come out from the cold. When an exotic technique—logit/probit—proved worthy, it

appears in economic history with a lag, but then the gap narrows with more quickly than in the case of regression.

AQ5

The specific timing here is relevant. The more exotic techniques enter the economics canon well after the onset of cliometrics, unlike regression, which enters before. Once the diffusion of these techniques begins in economic history, it is more rapid than in economics—opposite the pattern for regression. This more rapid diffusion occurs at a time when the publication histories of successive Ph.D. cohorts in economic history also had started to turn toward economics and away from history. That said, my framework suggests steady convergence, not a structural break, as occurred in the publication histories of the post-2000 Ph.D. cohorts.

I have argued that the cliometrics revolution was a supply-side response to a specific demand from economics. However, once the first generation of cliometricians took a careful look at the economic history literature they inherited it proved to be an irresistible target, an end in itself. From the perspective of academic economics, the intellectual holes looked wide and deep, the academic equivalent of shooting fish in a barrel.[22] Relatively early in the 1960s, the JEH came under the sway of cliometrics through the appointment of like-minded editors and editorial board members. As I noted in the Introduction, history, too, developed a fascination with quantitative methods that, for a while at least, suggested the possibility of revolutions similar to cliometrics in other historical subfields, such as social and political history. Accordingly, new journals catering to quantitative history were established such as the *Journal of Interdisciplinary History* (JIH) *Historical Methods* (HM) and *Social Science History* (SSH), all of which attracted submissions from cliometricians and which published cliometric work at the time. This can be seen in Panels A and B of Table 1 by the sharp increase in the proportion of articles appearing in the non-economics category, which includes the three journals just mentioned.[23] With the benefit of hindsight, one can certainly question whether a goal of subjecting as much of historical scholarship as possible to cliometric scrutiny was worth the scholarly effort allocated to it. After the initial novelty wore off, Economics decided it had limited interest in who won which historical debate and why, and the formats and some of the outlets favored—monographs and interdisciplinary journals—had less prestige value than papers in economics journals. History, too, began to lose interest in cliometrics by the mid-to-late 1970s, in part because of the rancorous debate over Fogel and Engerman (1974) but also simply because the articles and books being written by

cliometricians were becoming ever more technical and, therefore, less accessible to professional historians. Demand on both sides of the market, economics and history, for one of the scholarly activities privileged by the early cliometricians —"reinterpreting" (see Fogel and Engerman 1971) the previous, non-quantitative literature—began to decline. In response to this decrease in demand, we would expect to see an eventual decline in the share of articles published in the non-economics category in Panels A and B in Table 1. Such a decline is clearly visible although, like the upward trend in economics publishing, it proceeds at a fairly slow pace until the post-2000 Ph.D. cohorts.

The impulse in early cliometrics toward reforming history is commonly associated with one Nobelist in the field, Robert Fogel. Another impulse was to use economic history to reform economics. This impulse is associated with Douglass North, the other Nobelist in economic history. North's dissertation was traditional business history, but he quickly signed on to the cliometrics revolution, and his work in the late 1950s and early 1960s reflects this. But, by the late 1960s and early 1970s, North had shifted gears toward institutions and institutional change for which he is honored today. North believed that economic theory and econometrics were powerful tools, but the models favored in economics at the time—for example, the Solow growth model—were seriously flawed because they omitted or downplayed factors that North felt were first order. North frequently chided cliometricians for not doing enough to change economics and for becoming too cozy with the status quo (see, for example, North 1997)

The impulse that economic history should reform economics may have had a similar effect in moderating the speed of integration. However, it is not often that disciplinary criticism is successful. A critic may have a worthy target, but not necessarily the means to fix the problem. Economics eventually did buy into the idea that institutions matter and the requisite tools did appear once the theory of dynamic games developed to the point where it could be applied profitably to institutions and institutional change (Acemoglu and Robinson 2006). But these tools came from economics proper, not economic history.

To the extent that the impulses just described caused the first few waves of students of the early cliometricians to integrate more slowly with economics than otherwise, the effects can be seen in the publication histories of the 1970s cohorts. Book publication was very common, and three-quarters of journal articles appeared in economic history or non-economics journals. The 1980s and 1990s cohorts had

begun to move toward economics outlets, but the changes were relatively modest until the structural break of the post-2000 cohorts.

Why, then, does the structural break occur? The simplest explanation is that the battles that engaged the early cliometricians held less sway over younger scholars, once the earlier generation began to leave the professional scene through retirements or deaths—in other words, cohort succession. Another is that various second or third generation cliometricians ascended in reputation and influence and began serving as role models for integration. For example, Claudia Goldin's early articles and first book (Goldin 1976) were squarely in the cliometric tradition, but her second book (Goldin 1990) and the articles associated with it addressed a larger core audience in economics, showing how historical evidence could reshape and advance fundamental topics in labor economics as well as economic history. Christina Romer's various papers on spurious volatility in aggregate time series (see, for example, Romer 1986) provided spectacular examples of how a large and very important field, macroeconomics, could go astray by collectively forgetting the historical details underlying the sources of macroeconomic data.

I suspect, however, that a role model explanation is not the full story behind the structural break. In the early 2000s development economics found new common cause with economic history, one much less rooted in the lessons from the past achievements of developed countries but rather how historical natural experiments could explain contemporary differences in economic development (Acemoglu et al. 2001; Diamond and Robinson 2011). The formal theory of economic growth was revitalized by endogenous growth models which, as in the 1950s and 1960s, created a burst of enthusiasm for historical evidence in the growth field (see, for example, Galor and Weil 2000). Macroeconomists realized that dynamic general equilibrium models could be applied to problems in long-term growth, such as the shift of labor out of agriculture, the demographic transition, and rising female labor force participation (for example, Greenwood et al. 2005).

Acemgolu, Galor, Greenwood and the many other economists who participated in these lines of research were not trained specifically as economic historians, but their collective willingness to pursue such topics was a signal that historical evidence and argument was newly important in an array of subfields in economics. Arguably, this cleared space for more articles on historical topics in the journals specializing in these subfields, many of which had significant audiences because the subfields were large.[24] As Panels A and B of Table 1 show, post-2000 Ph.D.s in economic history seem to have taken notice because collectively they were much

more likely than earlier generations to publish in economics journals; important for the point being made here, while the share of top-five articles did rise for the post-2000 Ph.D. cohorts, the relative increase was even greater for economics journals outside of the top-five.

5. Conclusion and speculation

In higher education in the USA today there are only a few academic disciplines for which it can be said that there is moderately robust demand for new Ph.D.s. Economics is one of these disciplines. New Ph.D.s in economics have opportunities not only in universities but also the private and public sectors. Not only are there more jobs, but pay and working conditions are better on average than in the humanities or natural sciences. At present, Ph.D. economists who self-identify as economic historians can have their cake, and eat it, too, because, over the past several decades, economic history has integrated into economics. This integration was largely inevitable once the initial demand shock took place calling forth the cliometrics revolution. I say “inevitable” because the nature of the shock and the institutional environment created strong incentives for scholars doing economic history to follow the money. I say “largely” because strong incentives, by themselves, do not guarantee conforming behavior. By and large, though, the incentives worked to promote integration.

It is customary in economics to distinguish between positive and normative analysis. I have adopted a positivist mindset in this paper, in part to distinguish it from many previous papers about the cliometrics revolution that emphasized various normative aspects. For those economic historians who followed a path of integration a simple revealed preference argument establishes optimality for the individuals in question. But this is clearly different from whether the integration is, on net, socially beneficial.

Refraining entirely from the normative would be a cop-out because there are intellectual externalities across fields. At the end of the day there are only two types of empirical evidence in economics, experimental and observational. There is too little of the former to be broadly useful so we must have the latter. By definition, all observational data sit in their peculiar historical context and therefore possess a narrative structure—past, present, and future. In terms of conclusions, it may matter little if a contemporary labor economist abstracts from historical context in the analysis of the treatment effects of a specific labor market policy, but this is not necessarily true. By placing the historical context front and center, economic

historians make what I regard to be a critical contribution to economic analysis. Had economic history remained aloof, as it were, instead of integrating, economic historians would have lost influence in the broad intellectual conversation that is economics. Instead, that influence has arguably increased because integration happened.

That said, the normative consequences of the integration of economic history into economics may not be wholly positive. In a well-known article written in the aftermath of the awarding of Nobel Prizes to Fogel and North, Romer (1994) provocatively queried whether there might be an end to economic history. In doing so, Romer was, in effect, pointing out the endogeneity of fields in an academic discipline (see also Stigler 1984). Fields exist for good economic reasons because they are focal points for scholars seeking fellow travelers with associated infrastructure—journals, conferences—where division of labor can occur and intellectual agglomeration economies realized—but they are not set in stone.

Imagine, therefore, a counterfactual academy in which whatever historical evidence and argument that is deemed relevant in economics is developed primarily by specialists whose scholarly identity is wholly subsumed within a particular subfield of economics—public economists whose job it is to study and teach about the history of, say, taxation and government finance; labor economists who study and teach about the evolution of retirement; macroeconomists who look to events of the past like the Great Depression for evidence to evaluate theories about fiscal multipliers; and development economists who have extracted what they believe to the key features of early industrialization, if any, that are deemed relevant for policy advice today. The specialized historical knowledge developed in each of the fields might be passed along across Ph.D. generations, but in the counterfactual academy, generalist economic historians would no longer be employed in economics departments. As a rule, the earliest cliometricians were more generalist than specialist because they were aiming at the big picture—partly, as I have argued, because revisionism for its own sake seemed like a good idea at the time but also in the hope of influencing the broader social conversation about long-run economic development outside of the ivory tower—in policy circles or the general public. In the counterfactual academy, this becomes much harder to accomplish because the historically minded labor economist, say, has few if any incentives to venture outsider her narrow area and therefore, learn about the big picture.[25] A related point is that the generalists during the cliometrics revolution have served as valuable social links, or points of intellectual first contact, between professional

economics and professional history; this seems much less likely to occur in my counterfactual academy.[26]

Although professional historians had started to lose interest in cliometrics beginning in the 1970s, this is not the same as losing interest entirely in the substantive questions of economic history. Many of the economic historians in history departments who felt displaced (or disparaged) by the cliometric revolution found their way to other groups, such as the Business History Conference, where they could continue their work among like-minded colleagues. As the technical gulf between economics and history grew, the labor market incentives for such historians to keep up to speed with cliometric research diminished sharply. This, in turn, can negatively impact the quality of historical scholarship, as evidenced by the current, albeit controversial example of the so-called "History of Capitalism".

On paper, historians of capitalism are professional historians who are interested in some of the same topics that have long captivated cliometricians, such as the economics of American slavery, but they conduct their business generally eschewing economic methods and past scholarship in cliometrics, even when the latter is directly on point. ~~Economic historians~~ Cliometricians who have looked carefully at the recent literature of the history of capitalism find scholarly lapses of fact and interpretation so prolific in number and egregious in content that it is easy to be dismissive of the entire line of research. In the broader scheme of things this might not matter too much—except that historians of capitalism have received prestigious prizes, been awarded tenure in top history departments, and have influenced the presentation of historical narrative and artifacts in museum exhibits directed at the general public (Olmstead and Rhode 2016; Hilt 2017). Like all academic fields, history is subject to whims and fashion, and the history of capitalism may be today's but not tomorrow's fancy. But it is a reminder that it is not only in economics that there is demand for economic history. History retains some such demand, as do other social sciences, as does the general public. Private incentives may keep economists and historians from engaging productively with each other, even when it is socially beneficial for intellectual trade to occur.

As an intellectual endeavor cliometrics has been around for almost six decades and economics is far older. I am aware that it is a bit of a caricature, but there is some truth that, as a field of study, the history of economic thought has traditionally been more about the history of economic ideas rather that the historical evolution of the industrial organization of economics—its nuts and bolts, how it works. Consequently, there is much about the historical evolution that is obscure or poorly

understood. Economic historians may have some comparative advantage in doing the economic history of economics, as I hope this paper suggests.

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6. Appendix 1

This appendix lists the names of scholars included in the two samples of economic historians and the one sample of labor economists analyzed in the paper. To be included in any of these samples, the individual must meet the requirements for inclusion and I must be able to obtain a copy of the CV. With the exception of two scholars (Michael Edelstein and Michael Haines), all individuals in the two economic history samples have Ph.D.s in economics.

6.1. Economic History Sample #1

This sample consists of prominent economic historians. To be considered "prominent," the individual must satisfy at least one of four criteria: (1) has served (or is serving) as President of the Economic History Association (2) editor of the *Journal of Economic History* or *Explorations in Economic History* (3) Fellow of the Cliometrics Society (4) received tenure at a leading economics department or equivalent business school ~~(criteria #4)~~. In the table below, I rank order the criteria as above, listing only the first criteria met, even if the person satisfies more than one.

6.2. Economic History Sample #2, Dissertation Conveners

To be included in sample #2, the individual must have served as the convener of one of the two dissertation sessions held at the annual meetings of the Economic History Association. The last column of Table 2 indicates if an individual in sample #1 also served as a convener. In addition, the following scholars are in sample #2: Brian A'Hearn, Howard Bodenhorn, George Boyer, Loren Brandt, Stephen Broadberry, Joyce Burnette, Louis Cain, Leonard Carlson, Eric Chaney, Mauricio Drelichman, Alan Dye, Greg Clark, Lee Craig, Michael Edelstein, Farley Grubb, Christopher Hanes, Carol Heim, Eric Hilt, Ian Keay, Zorina Khan, Carolyn Moehling, Petra Moser, John Murray, John Nye, Kevin O'Rourke, Joshua Rosenbloom, Carol Shiue, Kenneth Snowden, Melissa Thomasson, Paul Uselding, Marianne Wanamaker, and David Weir.

Table 2

List of scholars in economic history sample #1, by first met criteria of inclusion

Name	Ph.D. cohort	Criteria	Also in sample #2?
Fred Bateman	1950–1969	3	Yes
Lance Davis	1950–1969	1	Yes
Stanley Engerman	1950–1969	1	Yes
Robert Fogel	1950–1969	1	No
Peter Lindert	1950–1969	1	Yes
Larry Neal	1950–1969	1	Yes
Douglass North	1950–1969	1	No
Roger Ransom	1950–1969	1	Yes
Richard Sutch	1950–1960	1	No
Richard Sylla	1950–1969	1	Yes
Peter Temin	1950–1969	1	No
Thomas Weiss	1950–1969	1	No
Jeffrey Williamson	1950–1969	1	No
Gavin Wright	1950–1969	1	Yes
Lee Alston	1970–1979	1	No
Jeremy Atack	1970–1979	1	No
Michael Bordo	1970–1979	1	No

Name	Ph.D. cohort	Criteria	Also in sample #2?
Barry Eichengreen	1970–1979	1	Yes
Alexander Field	1970–1979	3	No
Claudia Goldin	1970–1979	1	Yes
Michael Haines	1970–1979	3	No
Knick Harley	1970–1979	2	Yes
John James	1970–1979	3	No
John Komlos	1970–1979	3	No
Gary Libecap	1970–1979	1	No
Diedre McCloskey	1970–1979	1	No
Joel Mokyr	1970–1979	1	Yes
Cormac O'Grada	1970–1979	1	Yes
Alan Olmstead	1970–1979	1	Yes
Hugh Rockoff	1970–1979	3	No
Richard Steckel	1970–1979	1	No
Ann Carlos	1980–1989	2	No
Price Fishback	1980–1989	3	No
Avner Grief	1980–1989	4 (Stanford)	No
Timothy Guinnane	1980–1989	4 (Yale)	Yes
Robert Margo	1980–1989	1	Yes
Christina Romer	1980–1989	4 (UC-Berkeley)	Yes
Jean-Laurent Rosenthal	1980–1989	2	Yes
John Wallis	1980–1989	3	No
Eugene White	1980–1989	2	Yes
Maristella Botticini	1990–1999	4 (Boston University)	No
William Collins	1990–1999	2	No
Dora Costa	1990–1999	4 (MIT)	No
Joseph Ferrie	1990–1999	4 (Northwestern)	Yes

Name	Ph.D. cohort	Criteria	Also in sample #2?
Paul Rhode	1990–1999	2	Yes
Hans-Joachim Voth	1990–1999	2	Yes
Ran Abramitzky	2000–2009	2	No
Martha Bailey	2000–2009	4 (Michigan)	No
Hoyt Bleakley	2000–2009	4 (Michigan)	Yes
Daniel Bogart	2000–2009	2	Yes
Leah Boustan	2000–2009	4 (Princeton)	Yes
Carola Frydman	2000–2009	4 (Northwestern Kellogg)	No
Richard Hornbeck	2000–2009	4 (Chicago Booth)	No
Kris Mitchener	2000–2009	2	Yes
Nathan Nunn	2000–2909	4 (Harvard)	No

6.3. Labor Economics Sample

This sample consists entirely of Fellows of the Society of Labor Economists. The following are included: Daron Acemoglu, George Akerlof, Joseph Altonji, Joshua Angrist, Orley Ashenfelter, David Autor, Marianne Bertrand, Sandra Black, Rebecca Blank, Richard Blundell, George Borjas, Charles Brown, Kenneth Burdett, David Card, Janet Currie, John DiNardo, Ron Ehrenberg, Henry Farber, Roland Fryer, Victor Fuchs, Claudia Goldin, Ruben Gronau, John Haltiwanger, Dan Hamermesh, Eric Hanushek, James Heckman, Caroline Hoxby, Lawrence Katz, John Kennan, Alan Krueger, Kevin Lang, Edward Lazear, Thomas Lemieux, Shelley Lundberg, Costas Meghir, Robert Michael, Robert Moffit, Enrico Moretti, Richard Murnane, Kevin Murphy, Derek Neal, John Pencavel, Robert Pollak, Canice Prendergast, Mark Rosenzweig, Kathryn Shaw, Robert Shimer, James Smith, Jeffrey Smith, Gary Solon, Christopher Taber, Petra Todd, Robert Topel, Yoram Weiss, Finis Welch, and Robert Willis.

7. Appendix 2

This appendix classifies academic journals that appear at least once on any of the CVs of scholars in the three samples. The journals classified as top-five, all other

economics, economic history, and non-economics academic. In the all other economics category, labor economics journals have “labor” in parentheses after the title.

7.1. Top-five Economics Journals

American Economic Review, Econometrica, Journal of Political Economy, Review of Economic Studies, Quarterly Journal of Economics.

7.2. All Other Economics Journals

Advances in Economic Analysis and Policy; Advances in Macroeconomics; Advances in the Study of Entrepreneurship, Innovation, and Economic Growth; American Economic Journal: Applied Economics; American Economic Journal: Macroeconomics; American Economic Review, Papers and Proceedings; American Journal of Agricultural Economics; American Journal of Economics and Sociology; American Law and Economic Review; Annales de Economia; Annals of Economic and Social Measurement; Annual Review of Economics; Applied Economics; British Journal of Industrial Relations (labor); Brookings Papers on Economic Activity; Canadian Business Economics; Canadian Journal of Economics; Carnegie-Rochester Conference on Public Policy; Central Bank of Ireland Quarterly Bulletin; CESIfo Economic Studies; Contemporary Policy Issues; DICE Report-Journal for Institutional Comparisons; Eastern Economic Journal; Econometric Reviews; Economia Internazionale; Economica; Economics; Economie Appliquée; Economic Record; Economics and Human Biology; Economics and Politics; Economic and Political Weekly; Economic Development and Cultural Change; Economic Journal; Economic Inquiry; Economics Letters; Economic Policy Review; Economic and Social Review; European Economic Review; Explorations in Economics Research; Federal Reserve Bank of St. Louis Review; Fiscal Studies; German Economic Review; Health Economics; Industrial and Labor Relations Review (labor); Indian Economic Review; Industrial Relations (labor); International Economic Review; International Labor Review (labor); International Migration Review (labor); Journal of Applied Econometrics; Journal des Economistes et des Etudes Humane; Journal of Banking and Finance; Journal of Business and Economic Statistics; Journal of Common Market Studies; Journal of Comparative Economics; Journal of Development Economics; Journal of Economic and Social Measurement; Journal of Economic Education; Journal of Econometrics; Journal of Economic Growth; Journal of Economic Integration; Journal of Economic Inequality; Journal of Economic Issues; Journal of Economic Literature; Journal of Economic Perspectives; Journal of Economic Theory; Journal of the

European Economic Association; Journal of the European Economic Association, Papers and Proceedings; Journal of Farm Economics; Journal of Finance; Journal of Financial Economics; Journal of Financial and Quantitative Analysis; Journal of Human Resources (labor); Journal of Institutional and Theoretical Economics; Journal of International Economics; Journal of International Money and Finance; Journal of Japanese and International Economics; Journal of Labor Economics (labor); Journal of Labor Research (labor); Journal of Law and Economics; Journal of Law, Economics, and Organization; Journal of Macroeconomics; Journal of Money, Credit, and Banking; Journal of Monetary Economics; Journal of Population Economics; Journal of Public Economics; Journal of Real Estate Finance and Economics; Journal of Risk and Uncertainty; Journal of the American Real Estate and Urban Economics Association; Journal of Urban Economics; Land Economics; Labour Economics (labor); Malayan Economic Review; Monetary and Economic Studies; Monthly Labor Review (labor); NBER Macroeconomics Annual; National Tax Journal; Networks and Spatial Economics; New England Economic Review; Oxford Bulletin of Economics and Statistics; Oxford Economic Papers; Oxford Review of Economic Policy; Pacific Economic Review; Philippine Economic Review; Public Choice; Public Finance; Public Finance and Management; Proceedings of the Annual Meetings of the Industrial Relations Research Association (labor); Quantitative Economics; Quarterly Review of Economics and Business; Rand Journal of Economics; Regional Science and Urban Economcs; Ricerche Economiche; Research in Labor Economics (labor); Research in Population Economics; Review of Black Political Economy; Review of Development Economics; Review of Economic Dynamics; Review of Economics and Statistics; Review of Financial Studies; Review of Social Economy; Rivista di Politica Economica; Scandinavian Journal of Economics; Taxing and Spending; The Manchester School of Economics and Social Studies; Transportation Research Forum; Transportation Research Record; Southern Economic Journal; Weltwirtschaftliches Archiv; World Bank Economic Review; World Bank Economic Observer

7.3. Economic History Journals

Advances in Agricultural Economic History; Australian Economic History Review; Business and Economic History; Cliometrica; Economy and History; Economic History of Developing Regions; Economic History Review; Essays in Economic and Business History; European Review of Economic History; Explorations in Economic History; Financial History; Financial History Review; Irish Economic and Social History; Journal of Economic History; Journal of European Economic

History; Research in Economic History; Revista di Storia Economica; Yearbook of Economic History.

7.4. Non-Economics Academic Journals

Advances in Strategic Management; Agriculture and Human Values; Agricultural History; Agricultural History Review; American Historical Review; American Journal of Education; American Political Science Review; Annals of Human Biology; Annales; Annales de Demographie Historique; Annales E.S.C.; B.C. Studies; Behavior Genetics; Bulletin of the History of Medicine; Business History Review; Canadian Public Policy; Chicago Policy Review; China Quarterly; Civil War History; Communal Societies; Continuity and Change; Cornell Journal of Law and Public Policy; Demography; Doctrine and Life; Education Next; Educational Researcher; East Central Europe; Environment and History; Family Planning Perspectives; German History; Harvard Educational Review; Health Affairs; Health Policy and Education; Historical Methods; History of Economics Society Bulletin; History of European Ideas; History and Theory; History of Political Economy; Impact on Instructional Improvement; Independent Review; International Journal of Maritime History; International Regional Science Review; Journal of the American Statistical Association; Journal of Conflict Resolution; Journal of Family History; Journal of Interdisciplinary History; Journal of Law Reform; Journal of Modern Physics C; Journal of Research in Education; Journal of Regional Science; Journal of the Scientific Study of Religion; Journal of Social Science; Journal of the Statistical and Social Inquiry Society of Ireland; Industrial and Corporate Change; International Journal of Maritime History; Journal of Educational Measurement; Journal of Legal Studies; Journal of Policy Analysis and Management; Journal of Public Policy and Marketing; Journal of Regional Science; Journal of Research in Crime and Delinquency; Journal of Statistical Planning and Inference; Kyklos; Legislative Studies Quarterly; Management Science; Mariner's Mirror; Mathematical and Computer Modeling; Mathematical Social Sciences; New International Realities; Natural Resources Journal; Pacific Historical Review; Papers and Proceedings of the American Statistical Association; Pennsylvania History; Pennsylvania Magazine of History and Biography; Perspectivas: Análisis de temas críticos para el desarrollo sostenible; Perspectives on Politics; Policy Sciences; Political Methodology; Political Science Quarterly; Population and Development Review; Population Studies; Public Policy; Proceedings of the American Philosophical Society; Proceedings of the Conference on Theoretical Aspects of Rationality and Knowledge; Proceedings of the National Academy of Sciences; Proceedings of the Regional Science Association; Proceedings of the

Royal Irish Academy; Publishing History; Rationality and Society; Regional Studies; Review of Education; Reviews in European History; Rivista Internazionale; Rocky Mountain Social Science Journal; School Research Forum; Science; Sloan Management Review; Social Concept; Social Science History; Social Science Quarterly; Social Studies of Science; Social Research; Sociology of Education; Sociological Methodology; Spine; Studia Hibernica; Teachers College Record; Technology and Culture; The American Statistician; The Old Northwest; Theory and Society.

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[1] Examples include UC-Berkeley, Boston University, Harvard, Michigan, Northwestern, Stanford, UC-Davis, UCLA, Vanderbilt, and Yale. Not all of these have dedicated research workshops in economic history but most do.

[2] Here, “visible” means that an economic historian publishes in an economics journal. Below I present evidence that this has become quite common among scholars receiving their Ph.D.s after the year 2000. Examples of economic historians who received their Ph.D.s after 2000 and who routinely publish in economics journals are Ran Abramitzky (public economics), Martha Bailey (labor/demographic economics), Leah Boustan (urban economics), Carola Frydman (finance), and Nathan Nunn (trade). Among the many examples of well-known economists today who do not consider themselves to be economic historians primarily but who write in economic history from time to time are Daron

Acemoglu, Oded Galor, Robert Gordon, Lawrence Katz, Matthew Kahn, Daniele Paserman, Thomas Piketty, Claudia Olivetti, Valerie Ramey, and James Robinson.

[3] In particular, Abramitzky (2015) also uses GS to show that the percentage of economic history articles appearing in "top-five" economics journals (e.g. *AER*) increased after 2000, which is consistent with the findings of my Table 1. See also Selzer and Hamermesh (2017) who show that economic historians frequently co-author papers, just as in other fields of economics.

[4] In writing this paper I have also been influenced by reading various unpublished documents from the papers of Robert Fogel, held at the Special Collections Research Center in the Regenstein Library at the University of Chicago; and from the papers of Douglass North, held at the Rubinstein Rare Books and Manuscript Collections, at Duke University. The content in the documents shaped the development of the theoretical framework in Sect. 3, as well as my discussion of scholarly identity for economic historians employed in economics departments. The documents that were most influential in my thinking can be found in Boxes 68 and 159 of the Fogel papers and Box 1 of the North papers. I am most grateful to David Mitch for alerting me to their existence.

[5] See "Appendix 1" for the names of the scholars in the various samples. In the case of economic history sample #1, individuals can meet more than one of the four criteria for inclusion. I rank order these criteria and show the top ranked criteria by which scholars qualify for inclusion (for example, if a person served as President of the Economic History Association, this is criteria #1). "Leading" departments or equivalent business schools are those that fall into the top 10–15 in various rankings, for example, RePEc.

[6] Such departments exist in the UK and Europe. While it certainly is of interest to examine whether professional behavior of scholars in economic history departments differs from that in economics or history departments, my interest in this paper lies in the USA where cliometrics originated. There are cliometricians who obtained interdisciplinary Ph.D.s in economic history; important examples include Michael Edelstein and Michael Haines.

[7] An example of waxing and waning is the requirement in some departments that Ph.D. students in economics take a course in economic history. The requirement seems to have been introduced before World War Two. As of the early 1980s, there was such a requirement at Chicago, Harvard, MIT, Stanford, and Yale. The requirement has been abandoned at Chicago and MIT; at Harvard, students must satisfy a distribution requirement that can be met by taking an economic history course; Stanford requires that students take at least one economic history course, as does Yale.

[8] Well-known examples include Philip Hoffman and Naomi Lamoreaux, both former presidents of the Economic History Association.

[9] There are other interdisciplinary examples in economics, the most obvious being law and economics and, to a (much) lesser extent, econometric theory, which overlaps with statistics.

[10] All of the economic historians in sample #1 or #2 received Ph.D.s in economics with two exceptions (Michael Edelstein and Michael Haines) who, as previously noted, received interdisciplinary Ph.D.s in economic history in the 1970s. Because these programs included substantial coursework in economics

and because both Edelstein and Haines have spent their academic careers in economics departments, I keep them in their respective samples (Edelstein, #1; Haines, #2).

[11] There is slight censoring in samples #1 and #2, because not all individuals receiving their Ph.D.s after 2000 have experienced the first full decade of their professional career. The bias is extremely modest, however, because there is only one censored observation in sample #1 (Richard Hornbeck) and two in sample #2 (Eric Chaney and Marianne Wanamaker).

[12] The sample of labor economists cuts off with the 1990s Ph.D. cohort because there are hardly any SOLE Fellows with Ph.D.s post-2000. The labor economics sample could be filled out for the post-2000 cohorts by adding scholars who obtained tenure at top departments, similar to sample #1 of economic historians, but I believe the substantive value of doing so would be very small; that is, it would merely confirm the strong economics "identity" evident in Panel C of Table 1.

[13] See the last column in Table 2 for the overlap individuals in samples #1 and #2. Note that there is one person, Claudia Goldin, who appears in both economic history samples and the labor economics sample.

[14] I have experimented with data extracted from EconLit and similar sources; however, my experience is that it is extremely difficult—and for some scholars, impossible—to generate complete publication histories this way. I have also experimented with using the Wayback machine to retrieve old copies of CVs and found it not to be fruitful.

[15] A monograph can have more than one author but it cannot be an edited volume, which are not counted. I also do not count textbooks.

[16] I only count articles appearing in academic journals (see "Appendix 2"). "Articles" includes full length papers, comments, and notes, except that for the economic historian samples I exclude dissertation summaries and discussant comments (including the comments by the dissertation conveners). These exclusions have no effect on the substantive findings.

[17] The top-five journals are listed in "Appendix 2". Articles appearing in the *American Economic Review Papers and Proceedings* are not considered to be top-five but are included in the count of articles in all other economics journals. It can be argued that the post-2000 cohorts benefited from the introduction by the AEA of the various *American Economic Journals* (e.g. *AEJ: Applied*) which function somewhat like a top-field journal. An individual receiving her Ph.D. in 2000 by definition could not publish in an AEJ during the first decade post-Ph.D. Using sample #1, if I assume every paper published in an AEJ by a post-2000 Ph.D. cohort scholar would have been, instead, published in an economic history journal instead if the AEJs had never been introduced, the proportion of articles published in economic history journals increases to 28.1 percent for the post-2000 Ph.D. cohort, still far below the percent so published by the pre-2000 Ph.D. cohorts. In other words, the structural break is robust to the introduction of the AEJ journals. I am grateful to Ran Abramitzky for raising this issue.

[18] Sample sizes are too small for a detailed regression analysis, but one might wonder if the patterns are affected by differences across Ph.D. granting institutions. Accordingly, I estimated regressions for which the dependent variables are the same as in the column titles in Panels A and B of Table 1; the

right-hand side variables are either dummies for decade of Ph.D. or linear time trends, plus a full set of Ph.D.-granting institution dummies. The results (not shown) do not affect my substantive conclusions about trends in publication histories across cohorts or the post-2000 structural break.

[19] See Poelmans and Rousseau (2016) for evidence that disciplinary standards strongly affect the format and publication outlets chosen by junior scholars in economic history; and Diamond (1980) for evidence from the late 1970s that year of Ph.D. influenced an economic historian's acceptance of cliometrics in the direction (negative, meaning older is less accepting) implied by my framework. Implicit in my argument is that the gatekeeping function of period #3 faculty has value, both to the gatekeepers and to those being evaluated. Alternatively, we can imagine a hierarchy beyond the department that values academic prestige and has sanctions in place that guard against a breakdown of tenure standards.

[20] To consider another pertinent alternative, senior economic historians might convince their senior colleagues in other fields that junior economic historians will promote the discipline of economics by publishing in history outlets, and this advances the discipline overall.

[21] Other factors not explicitly in my model may be relevant. An example is the availability of data for econometric analysis. The early cliometricians had to develop these data from scratch—typically, from archival sources—whereas labor economists, for example, benefited immediately from the availability of household surveys like the public use sample of the 1960 federal census.

[22] As Fogel reminisced about the early years of his graduate teaching, "I challenged [Ph.D.] students to pick any page at random from whatever history book they had at hand. The odds were … that there'd be either an explicit or implicit quantitative statement that needed to be measured. The challenge was often taken up and I was never shown up[.]" The quotation is from Williamson and Lyons (2013, p. 350).

[23] The three journals survive to this day, as does the Social Science History Association (SSHA), which was formed in 1976. SSHA is interdisciplinary by design by having "networks" in the various disciplines, including economics. Economic historians with Ph.D.s in economics continue to participate in the annual SSHA conference and, from time to time, publish in SSH, along with the JIH and HM; however, as shown in Panels A and B Table 1, this is far less common among post-2000 Ph.D.s than earlier cohorts.

[24] In turn, a larger readership raises the likelihood that articles will be cited, to the economic benefit of the author(s).

[25] Consider, for example, textbook writing. Several of the earliest cliometricians and their immediate students wrote textbooks that attempted to interpret traditional accounts of, say, American economic history in light of cliometric research but, as a scholarly activity, this has gone by the wayside as economic historians identify more strongly with a subfield. That said, there clearly is still a demand for scholarly books in economics that deal with the big picture, including economic history, and I see no reason for this to abate.

[26] Points of first contact are important because they can serve as on-the-ground sources of ready information, for example, to professional historians about basic questions in economics—for example, what exactly do economists mean by the unemployment rate—as well as the esoteric—what is the best price available price index, say, for the antebellum USA?