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The Integration of Economic History into Economics

By

Robert A. Margo*

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*Professor of Economics, Boston University; and Research Associate, National Bureau of Economic Research. I am grateful to Ran Abramitzky, Jeremy Atack, Leah Boustan, Lee Breckenridge, William Collins, Martin Fiszbein, Kevin Lang, David Mitch, Claudia Rei, Paul Rhode, John Wallis, and Robert Whaples for helpful comments. This paper was written for the session "Cliometrics in Historical Perspective: In Remembrance of Robert Fogel and Douglass North," to be held at the ASSA Meetings in Chicago IL, in January 2017.

1.0 Introduction

In departments of economics in the United States today, the academic field of economic history is far more economics than history. Freshly minted economic historians with PhDs in economics write dissertations that are collections of essays with a principal "job market paper" just like other fields of economics, rather than books-in-waiting as in history. Many of the freshly-minted will have earned their stripes in economics departments where there is a research group composed of multiple economic historians and, perhaps, fellow travelers; dedicated research seminars; and routine doctoral production – again, similar to other fields of economics.[1] Faculty mentors in these departments counsel their PhD 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). A budding economic historian will "introduce" herself to the profession by presenting a paper at a specialist meeting – the annual convention of the Economic History Association (EHA) or the Cliometrics Conference – but also at general economics meetings, such as those held by the Allied Social Science Association. Once the degree is on the horizon, the 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 market (Abramitzky 2015).

[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.

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 fields of economics along with economists who earn fame and fortune in these other fields 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 United States mentioned in this and the previous paragraph 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 very closely related to Abramitzky (2015).[3] I shall offer two types of evidence. The first derives from text processing of digitized journal articles using Google Scholar (GS). I use GS to search through past issues of the *Journal of Economic History* (JEH) and the *AER* for instances of econometric language. I compare these instances with measures of the

[2] "Visible" means publishing in economics journals, whether or not the topic is historical. I present evidence this has become quite common among scholars receiving their PhDs after the year 2000. Examples of economic historians who received their PhDs 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 (corporate finance), and Nathan Nunn (trade). Examples of prominent 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, 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.

"population at risk" – basically, articles and notes/comments published in regular and conference proceedings of both journals that plausibly could have used econometrics.

I find that, for a "bread-and-butter" econometrics method like regression, economics moved well ahead of economic history starting in the 1950s, leading to a quite large gap in usage in the 1960s. By the 1970s the gap begins to decline steadily but it took about two decades for full convergence. For more exotic techniques that, eventually, became common much later than regression – I examine "logit or probit" and "maximum likelihood" – there is also an initial gap in favor of economics but once convergence starts it occurs more quickly than in the case of regression. The timing was such such that 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 are the early publication histories by PhD cohort of economic historians with doctorates in economics. By "early" I mean the first ten years post-PhD, and by "publication history" I mean the classification of published research into various categories – books, and articles in economics journals (e.g. the *AER*) vs. economic history journals (e.g. the *JEH*) versus history (e.g. *Agricultural History*) or other (e.g. *Demography*). I consider two non-random samples of economic historians who received PhDs in economics, both arranged by decade of PhD cohort. Sample #1 consists of "prominent" economic historians, where "prominent" refers to past EHA presidents, former (or current) editors of the JEH or EEH, and scholars who obtained tenured at "top-10" economics departments or equivalent business

schools.[4] 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 reflect the tastes and political agenda of the chooser. Thus, while there is some overlap between samples #1 and #2, it is much less than perfect -- hence, sample #2 is a bit more representative of the economic history profession than sample #1.

I find a long-run increase in the fraction of articles published in economics journals, including the "top-five" (e.g. *AER*) economics journals. Total journal productivity is roughly constant across cohorts, so the increase in economics publishing is offset by a decrease in publishing in economic history and other outlets, along with a decrease in the probability of publishing a monograph. Further analysis shows that, while these trends are present in the 1980s and 1990s PhD cohorts (relative to the 1970s), there is a structural break for those obtaining their doctorates after 2000.

To explain the integration of economic history into economics, I offer a simple analytic framework. The components of the framework are initial conditions, labor market structure/incentives, and selection.

The initial conditions are those in effect at the dawn of cliometrics. Then, as now, economic historians in the United States sit either departments of history or economics, not in departments of economic history. In the aftermath of World War Two the discipline of

[4] These categories overlap. For example, Jeremy Atack, who received his PhD in the 1970s, served as EHA president and JEH editor. The joke in economics is that there are more than 10 "top-10" departments – but not that many more. Scholars in sample #1 who are included solely on this criteria, with the department/school in which tenure was first obtained are Martha Bailey (Michigan), Hoyt Bleakley (Michigan), Leah Boustan (UCLA), Maristella Botticini (BU), Dora Costa (MIT), Joseph Ferrie (Northwestern), Timothy Guinnane (Yale), Richard Hornbeck (Chicago Booth), Nathan Nunn (Harvard), and Christina Romer (UC-Berkeley).

economics was growing rapidly and there was an emerging demand within economics for evidence on the historical development of rich countries like the United States, both to serve as grist for policy advice to developing countries and as "stylized facts" 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.

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 outcome of interactions between senior faculty inside and outside the field, reflecting their respective beliefs about what is meritorious, and their relative bargaining power. After a shock to the system a new equilibrium might emerge quickly, or the transition might be protracted.

The final component is selection. While economics had already begun to turn analytical and quantitative relative to history even before World War Two, disciplinary differences in PhD training in the early years of the cliometrics revolution were nowhere near as dramatic as they would become. History, too, had a brief fling with social science methods that lasted well into

the 1970s. In principle and in fact, therefore, two individuals could enter PhD programs in economics or history in, say, the mid-1970s; come of the other end as economic historians, one with a PhD in history and the other, a PhD in economics; and, conceivably, have similar career trajectories.[5] But history turned sharply away from quantitative methods in particular and social science more generally in the 1980s. Today, economics PhD programs are very selective for particular traits, such as aptitude for advanced mathematics and computer 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 incentive, to seek employment outside of economics.

Although the framework can broadly explain my date on the integration of economic history into economics, the structural break in publication histories for post-2000 PhDs is surprising. There were two "transformative" impulses that broadly characterize the first generation of cliometricians. Some wished to use economic tools to transform history, making it more theoretically "rigorous" and quantitatively grounded. Others wish to use history to transform economics, on the belief that economics was ignoring crucial factors in economic growth and development. The first impulse is commonly associated with Robert Fogel, and the second with Douglass North. I shall try to persuade readers that the effects were to slow the pace at which economic history integrated into economics. Both impulses remained strong

[5] Well-known examples include Michael Bernstein (currently serving as Provost at Tulane, but formerly a tenured faculty member at UC-San Diego); Philip Hoffman and Naomi Lamoreaux, both former presidents of the Economic History Association.

through the 1980s and 1990s but waned after 2000, and thus may explain the structural break in publication histories, at least in part.

The integration of economic history into economics has brought tangible benefits to economic historians with PhDs in economics – an active job market with the relatively high salaries and good working conditions that come with an economics doctorate. Can the profession expect this to continue for the foreseeable future? Taking a cue from Romer (1994) I suggest that one possible conclusion 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. I argue that the emergence of a new "history of capitalism" in academic history is a case in point. In a nutshell, there are costs to integration as well as benefits.

2.0 Background and Empirical Analysis

I offer two types of evidence to support my contention that, over time, economic history has integrated into economics. The first type of evidence derives from text processing of digitized files of the *JEH* and the *AER*. The second derives from analysis of the early publication histories of different PhD cohorts of economic historians. Before turning to the empirical analysis, I set the stage by reviewing the history of economic history up to the dawn 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 (Lamoreaux 2016). Economic historians worked either in economics or history departments but arguably were sufficiently small in number that it never made economic sense to establish separate departments of economic history in the United States. 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 towards 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 towards 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 1919 for the purpose of developing a solid statistical basis for understanding

the growth of 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. Digesting the contents 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. 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 economic history 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.

Usage of Econometric Language

I track instances of the use of econometric terminology, comparing the *JEH* – one of the flagship journals in the field – with the *AER* -- one of the leading general interest journals. My analysis provides a way of measuring when articles in economic history began to "look like" those in economics in terms of econometric practice.

I search for instances of econometric language using computerized text processing of digitized files of both journals using Google Scholar (GS) as the search engine. The "advanced"

features of GS allow me to search for a specific word or phrase in a specific journal during a specific period of time. GS will then return a list of "items" in which the word or phrase appeared. Items are returned only once, even if the word appears multiple times in an item. An item consists of any self-contained file that has been digitized. In the case of an academic journal, this could be an article, or note, or it could be a book review, a report by the journal editor, and so on. By imposing certain screens and also scanning the list of results, I can eliminate items (for example, advertisements) that are irrelevant to the analysis. I search for four econometric words or phrases: "regression", "logit", "probit", and "maximum likelihood". Obviously, this does not cover the gamut of possibilities but through trial and error I determined that looking for additional words or phrases – for example "t-statistic" or "R^2" or "instrumental variables" did not change my substantive findings.

To scale instances of econometric language I need to determine the "population at risk" – items in which the language might be put to use for a substantive research purpose.[6] By its very nature economic history is an empirical discipline, so for the JEH this arguably is straightforward – the population at risk is the number of articles, notes, and comments that appear in regular issues or the so-called "Tasks" issue.[7] For the AER the problem is more complex, because pure theory always has been and, presumably, always will be embedded in the DNA of economics; and, therefore, some fraction of articles in the AER will not be empirical.

[6]For example, a book review might refer to the use of econometrics in the monograph but it is extremely rare for a reviewer to run a regression and report the results as a substantive part of the review; therefore, such reviews are not part of the population at risk.

[7]It turns out that GS is not well suited to do this automatically; instead, I determine the JEH population at risk by manually inspecting each issue of the journal, and (literally) counting the number of articles, etc. The *JEH* ceased publication of the "Tasks" issue in 1997, substituting instead a regular issue in June but retaining the presidential address and summaries of doctoral dissertations and associated commentary delivered at the preceding annual meeting.

As a plausible way to distinguish the empirical from the opposite, I use GS to count items in the AER in which the world "table" appears – the idea being that, if there is a table, it is reasonable to presume that the paper could be empirical.[8]

Once I have the counts for the numerator – items containing an econometric word or phrase – and the denominator – the population at risk, I compute the ratio. At present my empirical analysis covers the period 1948 to 2010. The results are displayed in Figures 1-3.

Figure 1 shows the proportion of items in which the word "regression" appears. Multiple regression, or ordinary least squares, is the most bread-and-butter of econometric techniques. Its use in academic economics pre-dates World War Two, but such uses were highly unusual because computing coefficients and test-statistics by hand or by mechanical means then available was time consuming and error-ridden, even for small samples. This changed with the invention of the mainframe computer and the development of reliable statistical software.

In the immediate aftermath of World War Two, "regression" is uncommon in the AER in the population at risk and barely noticeable in the JEH. However, a steep upward trend emerges in the AER in the 1950s, continuing in the 1960s before reaching a plateau ca. 1970. An upward trend in the JEH does not emerge until the late 1960s, and when it does it is less steep than in the AER such that it takes about three decades for the JEH to catch up.

[8] I thank Leah Boustan for suggesting this approach to using GS to automate the measurement of the AER population at risk.

Regression, in other words becomes common in economics before the cliometrics revolution had taken hold and diffuses more quickly than in economic history, even after the revolution.

Like all fields of economics, econometrics is a work in progress – new techniques are constantly being manufactured out of whole cloth and others coming from statistics proper reconfigured for use in economics. Ex-post, some econometric techniques become ubiquitous while others remain esoteric or shelf-bound. It may be informative, therefore, to compare the relative frequencies of use of what were, initially, more exotic techniques that did not really become operationally feasible until well after World War Two but which then proved popular enough to be routinely taught to PhD students. I do this in Figure 2 for the instances of the words "logit" or "probit"; and in Figure 3, for instances of the phrase "maximum likelihood". The idea is to see if such techniques appear in the *JEH* more or less simultaneously as in the *AER*, or with a lag; and then to track integration, if any.

The patterns in Figures 2 and 3 are somewhat fuzzier than in the case of regression but a fair reading is that, just like regression, there was an initial gap; however, once the gap began to narrow it did so more quickly. In the case of logit and probit (Figure 2), the technique appears first in the AER in the late 1960s and not in the JEH until about a decade later, at which point there is a clear gap in favor of economics. Once logit and probit begin to be used in economic history, in the 1980s, the take-up is more rapid than in economics such that, by the 1990s, usage rates are similar. The same is broadly true of maximum likelihood, although the gap in favor of economics is more persistent until ca. 2000.

To summarize, the evidence in Figures 1-3 suggests that, when regression became realistically accessible as an empirical technique, it diffused fairly quickly in economics, creating a large gap with economic history practice in the 1960s, when the cliometrics revolution was beginning to take hold. As we all know, regression techniques did become ubiquitous in economic history, but the pace of adoption was slower than initially in economics. Because regression use in economics reached a plateau fairly quickly after it became popular, economic history did catch up, roughly ca. 1990. More exotic techniques that took longer to become feasible also were taken up earlier in economics, but once take-up began in economic history, the gap closed more quickly than in the case of regression.

Early Publication Histories

I examine the early publication histories of successive PhD cohorts of economic historians who received their PhDs in economics. By "early" I mean the first decade post-PhD – so, for example, if an individual received her PhD in 1985, the relevant period is 1985 to 1995 – and by "publication history" I mean the distribution of publications by publication type.[9] These types are monographs and refereed articles. I group the articles into those published in economics journals, economic history journals, and "other". An example of an economics journals is the *AER* or the *Journal of Human Resources*. An example of an economic history journal is *Explorations in Economic History* (EEH) or the *JEH*. An example of "other" is Historical Methods or Agricultural History.

[9] There is some censoring in the data, because not all individuals receiving their PhDs 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).

For the population at risk I select two samples. The first, as previously noted, is a sample of "prominent" economic historians, where prominence refers to having been a president of the EHA, a *JEH* (or *EEH*) editor, or having been awarded tenure at "top-10" economics departments or the business school equivalent. In its present form sample #1 is numerically balanced by having six scholars represented in each PhD cohort-decade.

The second sample 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 PhDs 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 comments on the dissertations 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 criterion 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.

In the present draft, data on publication histories are taken from on-line CVs, typically those posted at department or personal websites. These are very convenient to use and (presumably) accurate. The downside side is that, when individuals retire (or die), the on-line CV usually disappears from the web.[10] This is not an issue for sample #1 but it is somewhat for sample #2 for the dissertation sessions of the 1960s and early 1970s. For sessions held from the mid-1970s to the present, I am able to find on-line CVs for conveners.

Table 1 shows sample means, grouped by PhD decade, for sample #1 (Panel A) and sample #2 (Panel B). The columns show the fraction publishing at least one monograph; the average number of monographs conditional on publishing at least one; the total number of refereed articles; the proportion of articles published in economics journal; the percent of total articles published in "top-five" economics journals; the percent of articles published in economic history journals; and the percent published in "other" journals.[11]

The general patterns are fairly clear. Taking sample #1 first, total journal productivity is roughly constant across PhD cohorts, but there is a long-term downward trend in the proportion publishing at least one monograph in the first decade post-PhD. There are also downward trends in the proportion of total articles published in economic history and in "other" journals, and corresponding increases in the proportion published in economics

[10]In future drafts of the paper I plan to supplement the on-line CVs with data extracted from EconLit and similar sources; however, my experiments thus far suggest it is difficult and time-consuming to generate complete publication histories this way. I also intend to explore archived webpages (the "Wayback" machine).

[11] The "top-five" economics journals are those generally agreed upon -- the AER, JPE, QJE, Econometrica, and The Review of Economics Studies. The results are somewhat sensitive, however, to the exclusion and/or substitution of *The Review of Economics and Statistics* (RESTAT) which published some cliometrics articles in the 1970s and 1980s. Even if RESTAT is included, however, the sharp break evident for post-2000 PhDs in publication histories would still be present.

journals, including top-five. The trends are modest, however, for the pre-2000 cohorts – that is, there is a structural break for scholars obtaining the PhD after the turn of the century. [12]

Measured by the total number of journal articles sample #2 is somewhat less productive than sample #1, particularly among scholars who obtained their PhDs in the 1990s. Otherwise, the trends in publication histories are broadly similar across the two samples, as is the occurrence of a structural break post-2000.

Sample sizes are too small for a detailed regression analysis, but one might wonder if the patterns are affected by differences across PhD granting institutions. Accordingly I estimated regressions for which the dependent variables are the same as in the column titles in Table 1; the right hand side variables are either dummies for decade of PhD or linear time trends, plus a full set of PhD-granting institution dummies. The results (not shown) do not affect my substantive conclusions about shifts in publication histories across cohorts.

3.0 Integrating Economic History into Economics: An Explanatory Framework

I sketch 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 part of the framework pertains to labor market structure, so I start with this.

[12] It might be argued that the post-2000 cohorts benefited from the introduction by the AEA of the AEJ journals, which function somewhere between a top-five and a top field journal. An individual receiving her PhD in 2000 by definition could not publish in an AEJ during the first decade post-PhD. Using sample #1, if I assume every paper published in an AEJ by a post-2000 PhD 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 19.1 percent for the post-2000 PhD cohort, still far below the percent so published by the pre-2000 PhD cohorts. In other words, the structural break is robust to the introduction of the AEJ jounrals.

Labor Market Structure

There is an "overlapping generations" structure to the academic labor market. 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 Economics, "Early Modern Europe" a field in History. "Economic history" can be a field either in economics or history (or both) but is not itself a discipline (see below).

Scholars live for three periods. In period #1, a person enters a disciplinary PhD program and devotes all of her time to human capital investment, eventually obtaining a PhD 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 it is negative, she takes an outside option. Upon receiving tenure, she spends part of her time training PhD 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 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 PhD cohort.[13] These standards refer to content – 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 PhDs 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 consisting mainly of journal articles, 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.

Crucially, by making the standards depend on PhD cohort of period #3 faculty I am introducing a lag structure in faculty opinion that potentially will feed back on the behavior of

[13]See Poelmanns 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 PhD 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.

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 PhDs in N – 1 and are evaluated by period #3 scholars who received their PhDs in period N – 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. Instead, "economic history" is a subject that exists in both economics and history departments; however, an economic historian hired in economics still has to have a PhD in economics (and vice versa). Initially, 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 in lieu of some number of articles in economics journals that would otherwise be expected of candidates in economics.[14]

Second, I assume that, PhD 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

[14] 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 is good overall for the discipline.

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 PhD 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 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 PhDs 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.

4.0 Discussion and Speculation

The broad elements of Figures 1-3 and Table 1 appear consistent with my model. Econometric usage in economic history eventually converges with that in economics. Over time, successive PhD generations of economic historians who were employed in economics departments shifted their early publication portfolios towards economics publication outlets and away from economic history outlets and book publishing.

Subtler features of the data, however, raise some puzzles. As the model predicts, regression methods diffused more quickly than in economic history. However, more specialized methods diffused more rapidly in economic history once diffusion started. Although some shift towards publishing in economics outlets is present by PhD cohorts of the 1980s the differences

between the 1970s cohorts and those of the 1980s – and even the 1990s -- is not that large, whereas they are far greater for the post-2000 PhD cohorts – that is, there is a structural break in the publication histories.

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 "economic" economic history. The diffusion of regression methods in economic history is a case in point.[15] 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 the 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 through 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. Typically, results are briefly discussed in the text and specific details, if any, reported in a footnote. Econometrics is <u>never</u> 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

[15] 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 – often, 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.

no longer novel and the presentation of results could come out from the cold. When a more exotic technique – logit/probit – proved worthy, it appears in economic history with a lag but then the gap narrows with economics more quickly than in the case of regression.

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 PhD cohorts in economic history also had started to turn towards 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 PhD 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. The intellectual holes were wide and deep, the academic equivalent of shooting fish in a barrel.[16] Relatively early in the 1960s the JEH came under the sway of cliometrics through the appointment of like-minded editors and editorial board members. New journals of "quantitative history" were established such as the *Journal of Interdisciplinary History*,

[16] As Fogel reminisced about the early years of his graduate teaching, "I challenged [PhD] 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[.]" Quotation from Williamson and Lyons (2013, p. 350).

Historical Methods, and *Social Science History*. For papers thought to be too technical for the JEH or an interdisciplinary outlet, along came *Explorations in Economic History* in 1969.

With the benefit of hindsight it is far from obvious that the goal of subjecting as much of history 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 outlets favored – monographs and "interdisciplinary" journals – had less prestige value than economics journals. History, too, began to part with cliometrics by the mid-to-late 1970s, in part because of the rancorous debate over Fogel and Engerman (1974) but also simply because economics was becoming ever more technical. Demand for one of the scholarly activities privileged by the early cliometricians – "reinterpreting" (see Fogel and Engerman 1971) the pre-existing literature -- began to dry up.

The impulse in early cliometrics towards 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 towards 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.

The impulse that economic history should reform economics may have had a similar effect in moderating the speed of integration. 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. In the end North was right, because economics eventually did buy into the idea that institutions matter. But North was not inclined to provide the necessary technical tools for institutional analysis himself and neither were his students. 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). 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, and perhaps, even the 1980s and 1990s. Book publication was universal, and the vast majority of journal articles appeared in economic history or "other" journals – roughly six of every seven – than in economics journals. The 1980s and 1990s cohorts had begun to move towards economics outlets, but the changes were relatively modest on average.

Why, then, does the structural break occur? One simple explanation is that the battles that engaged the early cliometricians no longer had sway over younger scholars due to cohort succession. Another is that various second or third generation cliometricians gradually stepped in, taking over from Fogel, North, and the other early cliometricians, serving as role models for integration. For example, Claudia Goldin's early articles and first book (Goldin 1976) were squarely in the Fogelian tradition but her second book (Goldin 1990) and the articles associated with it addressed a far 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 (e.g. Romer 1986) provided spectacular examples of how a large and very important field, macroeconomics, could go wildly astray by collectively forgetting the historical details underlying the sources of macroeconomic data. Goldin was a PhD student of Robert Fogel's. Her PhD students, among those of other second and third generation cliometricians who participated in this "quiet revolution" of sorts, are some of the post-2000 PhD economic historians who, as Table 1 shows, shifted their publication sharply toward economics, accelerating the pace of integration.[17]

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 not rooted in the "lessons" from the past achievements of developed countries but rather how "natural experiments" in the distant past could explain contemporary

[17] For example, Leah Boustan, a PhD student of Goldin's, is in the post-2000 part of the "stars" sample. Of the 13 articles Boustan published in the first ten years after her PhD, 11 appeared in economics journals.

differences in per capita income (Acemoglu, Johnson, and Robinson 2001; Diamond and Robinson, 2011). The formal theory of economic growth was revitalized with the development of so-called "endogenous growth models" which, as in the 1950s and 1960s, created a burst of enthusiasm for economic history in the growth field (e.g. Galor and Weil 2000). Macroeconomists discovered 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 (e.g. Greenwood, Seshadri, and Vandenbroucke 2005). The vast majority of economists who participated in these lines of research were not trained as economic historians but their collective willingness to pursue such topics was a signal that historical evidence and argument remained important to a broad swath of economics. But that broad swath of economics was accustomed to publishing and reading articles in economics journals, not in economic history journals. This, too, was a signal, to which the post-2000 PhD generation of economic historians appears to have responded loudly and clearly.

In addition, there is the relative profitability of an economics career to be considered. Although (much) more research needs to be done on long run trends of salaries in economics relative to other fields, it appears that when cliometrics came into being differences in salaries and other aspects of working conditions between economics and history were not large. But, by the end of the twentieth century and certainly today, starting salaries for economic historians are far higher in economics departments, and jobs much more plentiful, than in history. In addition, today there are significant economic returns in economics to publishing in "top-five" journals like the *American Economic Review* or the *Quarterly Journal of Economics*;

there are analogous, if smaller, returns to publishing in the American Economic Association's field journals (for example, the *American Economic Journal: Applied Economics*). To some extent, the post-2000 PhD generation of economic historians is simply responding to these incentives by choosing to publish a (much) larger fraction of their total production of articles outside of economic history outlets.

Speculation: The End of Economic History?

One can speculate about the future of economic history, assuming that integration of economic history into economics continues apace. I am not the first to engage in such an exercise – see, in particular, Romer (1994) who, in the aftermath of the awarding of Nobels to Fogel and North, provocatively queried whether there might be an "end" to economic history.

Raising the question is, in effect, pointing out the endogeneity of "fields" in an academic discipline. 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. However, fields are not set in stone. Economic history may be a case in point.

In my framework, economic history is demanded in economics because historical evidence is valued in economics. The model does not specify why but in the real world, not all historical evidence is created equal. As McCloskey (1976) famously quipped, the past does have useful economics, but not everything that economic historians do is useful as economics.

Moreover, the useful bits are not necessarily supplied by individuals who self-identify as economic historians.

One end game has economic history disappearing as a separate field in economics but historical content is still part of economics. Under this scenario, historical evidence and argument relevant to fields like labor or public finance would be developed primarily by specialists within these fields. There would be no generalist keepers of the historical flame but, rather, 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 knowledge developed in each of the fields would be integrated into PhD field courses, but generalist economic history courses almost certainly pass from the economics curriculum.[18] It seems obvious that, were this to come to pass, many topics that have been part and parcel of such course would no longer be taught in economics, although specific aspects might still retain sufficient interest for instruction.[19] The same would be true of economics research.

[18] This would happen in the model because no faculty in economics would be sufficiently knowledgeable to teach them. Instead, relevant historical topics in health would be covered in courses in health economics.

[19] The possibility that specific historical topics might still be sufficiently in demand might also provide incentives for covering the more general historical context. For example, if public finance economists thought that the New Deal experience provided useful evidence for understanding the local effects of government expenditures, students might still learn more broadly about the Great Depression.

Thinking this way calls attention to an important and, arguably, negative externality associated with the integration of economic history into economics. The loss of courses and research topics from the economics curriculum is one thing but this is not necessarily socially undesirable compared with having the same topics re-emerge elsewhere in the academy. A current case in point is the so-called "History of Capitalism". On paper, historians of capitalism are history PhDs who are interested in some of the same topics that have long captivated cliometricians of the United States, such as the economics of slavery. However, historians of capitalism conduct their business eschewing economic methods and largely ignoring past scholarship in cliometrics, even when the latter is directly on point. Economic historians 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 – except that books by historians of capitalism have been nominated for prestigious national awards, such as the Pulitzer Prize (Olmstead and Rhode 2016; Hilt, forthcoming).

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. The more integrated economic history becomes in economics, the less likely it that economists will engage productively with these other demands.

5.0 Concluding Remarks

In higher education in the United States today there are only a few academic disciplines for which it can be said that there is a robust demand for new PhDs. Economics is one of these disciplines. New PhDs in economics have opportunities not only in universities but also the private sector and in government. Not only are there more jobs, but pay and working conditions are far better than in the humanities or natural sciences, on average. At present, PhD economists who self-identify as economic historians can have their cake, and eat it, too. There may be a future in which economic history is no longer a separate field in economics, but it would be very surprising, indeed – and, I would argue, very unlikely --- if historical evidence was not part of the conversation of academic economics at some level.

We can have our 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.

I largely refrain from engaging in normative analysis – that is, whether the integration of economic history into economics is desirable on social welfare grounds. A simple revealed preference argument suffices for those economic historians who followed the path of integration. I believe the integration is desirable for economic history and economics more generally. 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 in all

fields of economics, so we cannot avoid the use of observational data. By definition, all observational data in economics sits in a particular historical context. There may be excellent reasons to downplay or outright ignore the historical context in any particular analysis, as is the norm in much of empirical economics. But this is not necessarily the case; putting the context front and center is the essence of economic history, its fundamental contribution to economics *per se*.

That said, there are social costs to integration. As economic history integrates, economic historians burrow deeper into the other field(s) of economics in which their topic fits. At some point, scholarly identity may shift to the other field – lock, stock, and barrel. Such trends are accentuated as economics becomes ever more technical, because technique has a return in economics. The "big picture" disappears from view or is taken up elsewhere, and not always for the better. Individuals benefit from integration but economic historians with PhDs in economics lose influence in the broader conversation.

As an intellectual endeavor cliometrics is almost 60 years of age and economics far older. The history of economic thought is largely intellectual history. Intellectual history has its charms but by its very nature does not have much to say about the historical evolution of the "industrial organization" of economics – its nuts and bolts, how it works. There is much about this evolution that is obscure or poorly understood. Economists have a comparative advantage in doing the economic history of economics, as I hope this paper has demonstrated.

Figure 1

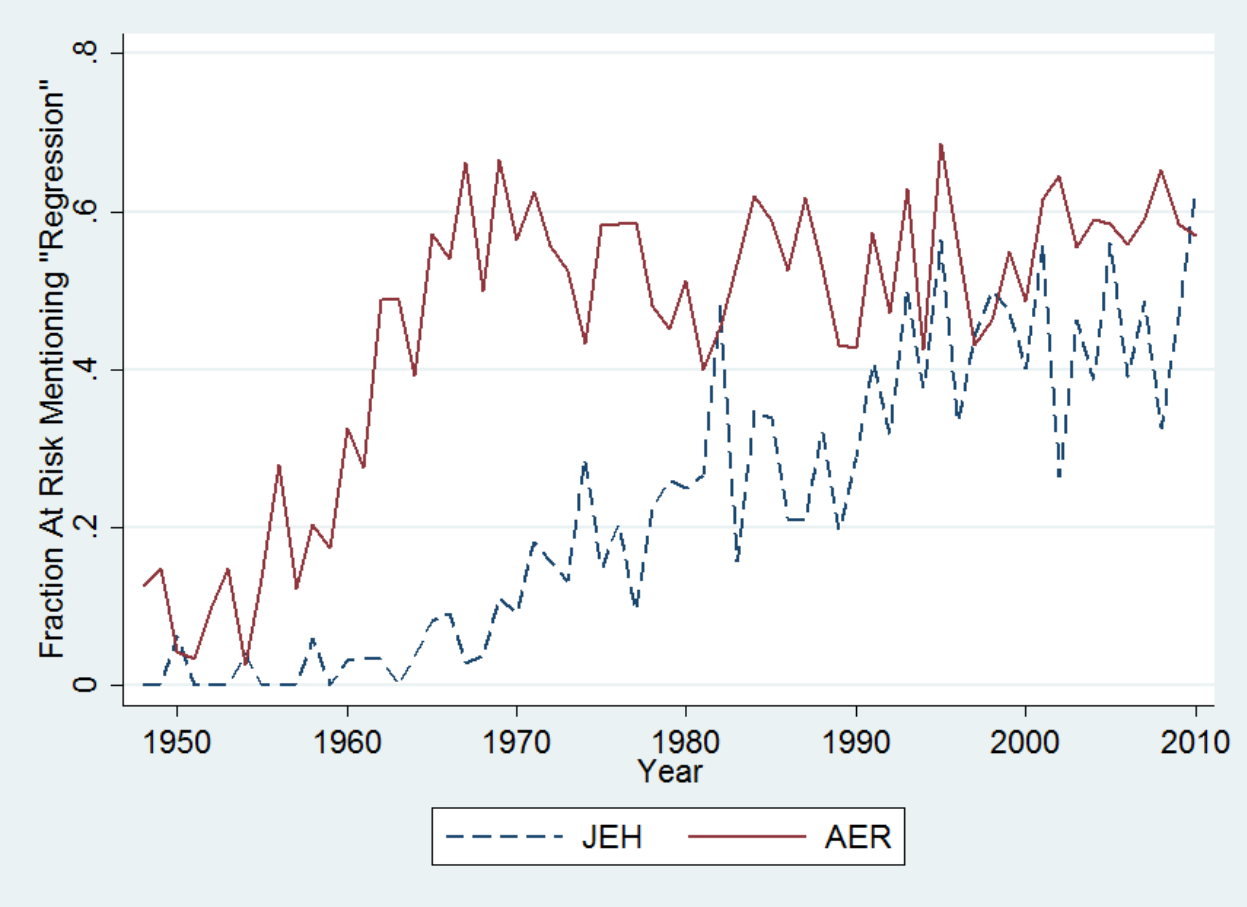

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Figure 2

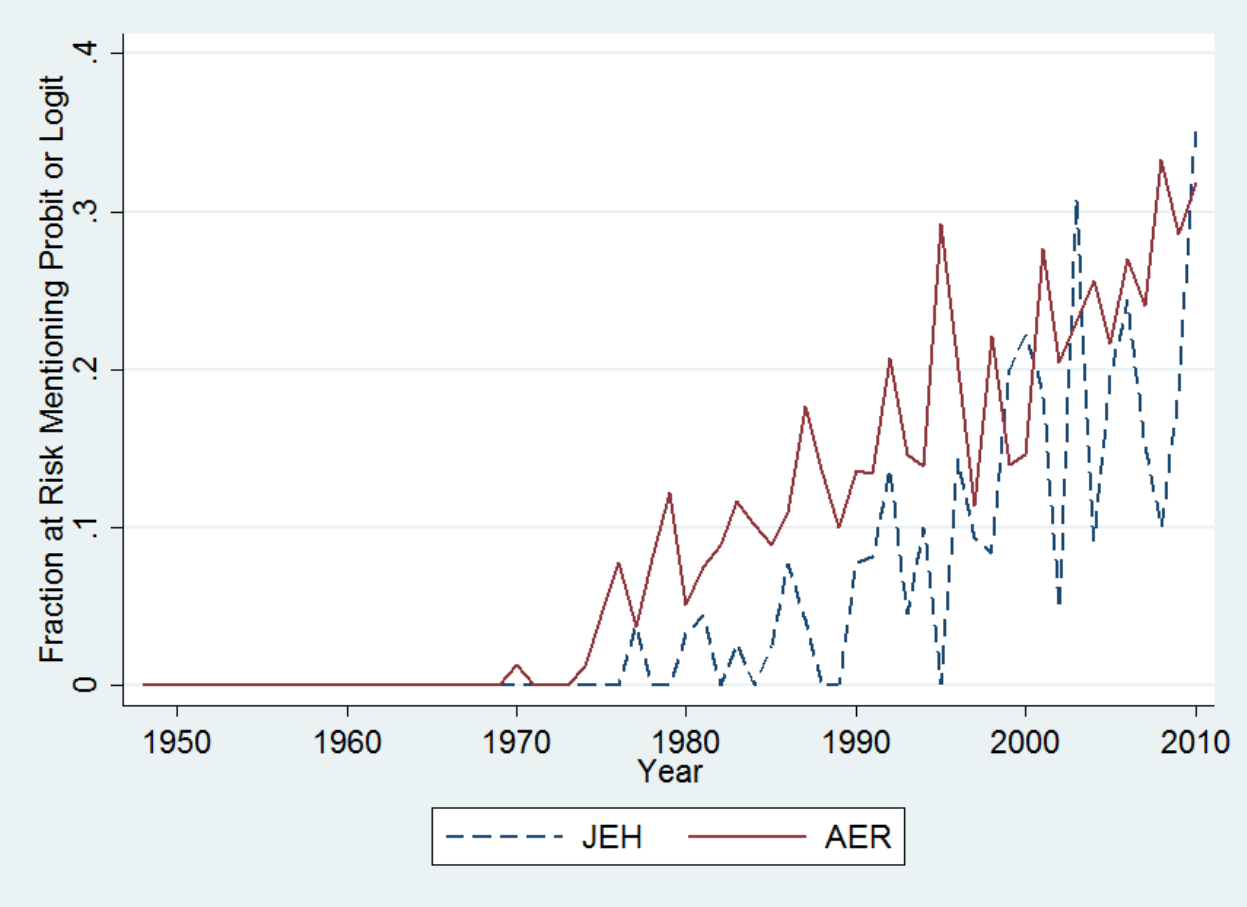

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Figure 3

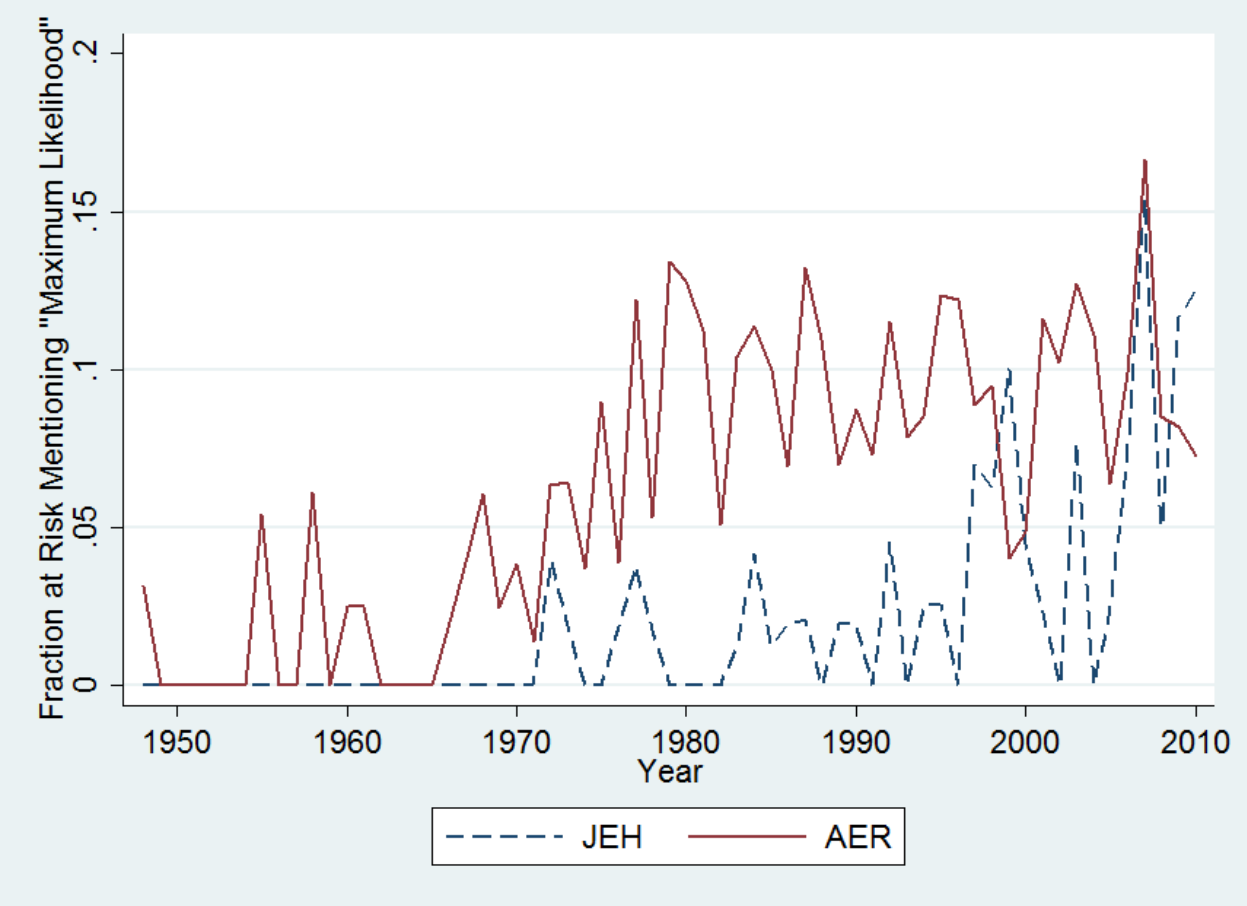

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Table 1

Early Publication Histories: Economic Historians with PhDs in Economics

Panel A: Sample #1

PhD Decade	P(Book)>0	Mean Number of Books if >0	Mean Number of Refereed Articles	Percent in Economics Journals	Percent in Top-Five Economics Journals	Percent in Economic History Journals	Percent Other
1970-1979	1.00	1.16	14.3	16.3%	8.2%	61.7%	22.2%
1980-1989	0.83	1.40	14.0	35.7	14.3	45.2	19.1
1990-1999	0.67	1.33	13.0	35.9	15.4	44.8	19.2
2000-2009	0.17	1.00	13.1	89.9	28.2	7.6	2.5

Source: compiled from on-line CVs. There are six individuals in each PhD cohort. 1970-79: Lee Alston, Jeremy Atack, Gary Libecap, Claudia Goldin, Joel Mokyr, Richard Steckel. 1980-89: Price Fishback, Timothy Guinnane, Robert Margo, Christina Romer, Jean-Laurent Rosenthal, Eugene White. 1990-1999: Maristella Botticini, William Collins, Dora Costa, Joseph Ferrie, Paul Rhode, Hans-Joachim Voth. 2000-2009: Ran Abramitzky, Martha Bailey, Hoyt Bleakley, Leah Boustan, Richard Hornbeck, Nathan Nunn. Top-Five: *American Economic Review*, *Econometrica, Journal of Political Economy*, *Quarterly Journal of Economics*, *Review of Economic Studies*. Papers published prior to receipt of PhD are included.

Panel B: Conveners Sample

PhD Decade	P(Book)>0	Mean Number of Books if >0	Mean Number of Refereed Articles	Percent in Economics Journals	Percent in Top-Five Economics Journals	Percent in Economic History Journals	Percent Other
1970-1979	1.00	1.25	12.5	4.0%	0.0%	76.0%	20.0%
1980-1989	0.67	1.70	12.7	27.6	8.4	56.2	16.2
1990-1999	0.36	1.25	8.5	26.4	8.2	62.7	10.9
2000-2009	0.22	1.50	10.3	58.1	13.9	38.7	3.2

Source: compiled from on-line CVs. Sample size is 52; 4 in row #1 (1970s), 16 in row #2 (1980s), 13 in row #3 (1990s; and 9 in row #4 (2000s). Calculations in Panel B proceed in the same manner as in Panel (e.g. definition of top-five).

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