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Economic History as a Progressive Science

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Abstract

This essay examines the relationship between Austrian economics and economic history. It notes their different origins as scholarly fields, and divergent trajectories over the course of the twentieth century, before discussing recent examples of cross-fertilization and pointing to areas of shared interest and other complementarities. I argue that methodological differences have been one barrier to dialogue between these two fields, but suggest that these methodological differences have diminished in recent years.

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Introduction

Austrian economics and economic history—as a subfield within economics—arose at roughly the same time, in the late nineteenth century. The Austrian tradition inaugurated by Carl Menger was theoretical and abstract, shaped as it was in its early decades by disputes with the German Historical School. Economic history, in contrast, can trace its origins as an academic discipline back to the very same German Historical School. So that an observer writing circa 1930 would see few natural connections between Austrian economics and economic history (any more than one might see connections between the Walrasian tradition and economic history).

This changed in the post-World War Two period, for two different, though related, reasons. Both Austrian economics and economic history were first eclipsed by, on the one hand, the rise of Samuelsonian neoclassical economics as the dominant paradigm within Anglo-American economics, and then, second, by the emergence of numerous new subfields such as econometrics, macroeconomics, game theory, and later experimental and behavioral economics, many of which were highly mathematical or otherwise influenced by the natural sciences.

The fate of Austrian economics as an academic discipline is usually thought to have reached its nadir in the early 1970s prior to the 1974 South Royalton Conference, which is seen to mark its modern revival. Economic history also declined in prominence after 1945. Donald McCloskey (1976, 436) noted that most economists in the mid-1970s “believe[d] history to be of small and diminishing interest”. The number of economic history papers published in top economics journals reached its lowest level in percentage terms between 1975 and 1984 (Ran Abramitzky 2015, 1242). Since this time, economic history has returned to prominence as a thriving, though small, subfield within modern economics.

Austrian economics has also experienced a (modest) return to prominence. This has been driven in part by a move by scholars within the Austrian tradition of economics away from pure theory towards applied work, and much of this applied work has been historical (see Peter Boettke, Christopher Coyne, and Peter Leeson 2013). Scholars within the Austrian tradition have embraced insights from public choice economics, from new institutional economics, and from law and economics. This has created opportunities for cross-fertilization with adjacent fields, including economic history. These opportunities are greatest for those economic historians who under the influenced of Douglass North, have turned their attention to the study of institutions in economic history.[1]

Given these important and promising developments, it is opportune to ask: What is the relationship between Austrian economics and economic history?[2] And what can be done to strengthen cross-fertilization between these two subfields? This paper hopes to address these questions. It starts with a brief historical sketch of the development of each field, followed by insight into the questions, methods, and approaches most commonly used in economic history. Finally, I consider questions of methodology asking the following question: to what extent are Austrian economics and economic history complementary fields? I outline some points of mutual sympathy and point out some key differences.

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[1] There are numerous examples of this cross-fertilization. For example, *The Review of Austrian Economics* edited by Boettke published a symposium on Douglas W. Allen’s *The Institutional Revolution* (2011), which featured an essay by leading economic historians like Joel Mokyr (Mokyr and José-Antonio Espín-Sánchez 2013). I recently published a review essay on Sheilagh Ogilvie’s *The European Guilds* (2019) in that journal (Koyama 2020a).

[2] This question is also asked by Nicola Tynan’s essay in this volume which also considers points of convergence between economic historians and Austrian economics.

Schools of Thought?

In a paper about the relationship between Austrian economics and economic history, the following caveat is necessary. I find the concept of schools of thought in modern economics unhelpful. Schools of thought were relevant in the nineteenth and early twentieth centuries. The Swedish economists working around Stockholm in the 1920s and 1930s—Bertil Ohlin, Gustav Cassel, and Gunnar Myrdal—really did share a distinctive perspective on macroeconomic questions, one which developed in relative isolation from economists working in Cambridge, the London School of Economics, or in North America. Similarly, between the 1870s and the 1920s, Austrian economists did develop a distinctive and singular approach based on a deep understanding of the subjective nature of value and costs. Today, though, the idea of a distinctive and self-contained school of thought hardly makes sense.

More useful, in my eyes, than the concept of a school is the idea of a tradition, a body of ideas that shares commonalities and complementarities and hence are most usefully viewed together. And it is in this light that this essay will consider the relationship between economic history and the Austrian economic tradition.

The emergence of the Austrian School of Economics in the late nineteenth century is a well-known story that I do not wish to rehash. What is relevant here is that within German-language scholarship, the Austrian tradition of Menger, Böhm Bawerk and Wieser was a body of theoretical insights—marginalism, opportunity cost, round-aboutness and capital theory, the regression theorem etc.—in contra-distinction to the explicitly atheoretical German Historical School of Wilhelm Roscher, Karl Knies, Bruno Hildebrand, Gustav Schmoller, and Werner Sombart.[3] The Historical School advanced a vision of economics as an inductive discipline, more akin to history, and perhaps to the modern disciplines of human geography and business studies. They rejected the use of models or the concept of economic laws.

It is equally important to note that the origins of economic scholarship in Germany go back to the cameralists of the seventeenth and eighteenth centuries. The first German economist qua-economist was Friedrich List, who wrote in explicit opposition to Adam Smith, David Ricardo and classical political economy. The Austrian economists were the first German-language scholars to entirely reject this perspective and to view economics as a progressive science, that is a body of cumulative knowledge and insights. Austrian economics in the 1870s through to the 1930s was a cumulative body of knowledge and insights that was largely theoretical. The Austrian tradition is thus, through various channels, an ancestor of modern neoclassical economics. Its influence, though largely unacknowledged today, can be discerned in the development of consumer theory, business cycle theory, and game theory.

Appreciating this point can help shed light on why there is little “Austrian” economic history, notwithstanding the fact that Mises and Friedrich Hayek were both favorable to the application of economic theory to history. Traditionally, Austrian economists have predominantly seen their contributions as contributions to economic theory.[4] Modern Austrian economics was initially revived in the 1970s as a body of theoretical insights (for example, Gerard O’Driscoll and Mario Rizzo 1986; Israel Kirzner 1973). Since then there has been the development of applied theoretical insights (into non-market decision making, the economics of anarchy, and the various forms of spontaneous orders) and the application of these insights

[3] This distinction is sometimes overdrawn—after all Max Weber was both a “member” of the Historical School and influenced by and sympathetic to Austrian economists including Ludwig von Mises—but it is nevertheless meaningful.

[4] Hayek did engage with economic historians, most notably in *Capitalism and the Historians* (Hayek 1954). In retrospect, however, one can see that opportunities from cross-fertilization were not fully grasped. See Vincent Geloso (2020) for a discussion of some of these missed opportunities, particularly those stemming from his collection of novel macroeconomic data.

to historical and contemporary issues (see for instance, Leeson 2014; David Skarbek 2012; Edward Stringham 2015).[5]

Economic history developed very differently as a field of economics. William Ashley was the first holder of a professorship in economic history in the English-speaking world (at Harvard in 1892). While Ashley's undergraduate education had been influenced by Arnold Toynbee—who made famous the term "Industrial Revolution" but died at 30 and hence did not make other major contributions to the field—his graduate education was in Germany, where his vision of economic history as a subject was shaped by the German Historical School.[6] Ashley went on to teach at the University of Birmingham and to found the Economic History Society. Economic history, as it was largely practiced by Ashley, and his intellectual successors such as Sir John Clapham and Richard Tawney, was largely atheoretical, a sub-discipline of history rather than of economics.

It was only in the 1950s that economic history came to be reformulated as a social science and specifically a positive social science characterized by the application of economic theory to history. This cliometric revolution—associated with the work of Robert Fogel, North, and William Parker—saw the introduction of formal economic theory and statistical testing.[7] Reformulated as cliometrics, this new economic history was also self-consciously a progressive science, specifically modeled on the applications of the techniques and approaches of the natural sciences to the social sciences and to history. The cliometric revolution in economic history, however, developed orthogonally to Austrian economics. North, though later deeply influenced by the insights of Hayek, attributes many of his ideas to his immersion in the arguments of Marxian historians and later the writings of Polanyi (see North 1977). It was only after his PhD and as an assistant professor that he learned neoclassical economic theory (see North 1993). Fogel, who would win the Nobel Prize for Economic History along with North, was trained as an economist by George Stigler and hence inherited the insights of Chicago price theory.

In general then, the leading economic historians of the late twentieth century were not exposed to the writings of Mises or Hayek—see, for example, the following remarks by Deirdre McCloskey:

> In college I had a roommate, a brilliant electrical engineer, who would break from solving second-order differential equations by reading Ludwig von Mises' Austrian classic *Human Action*. But I was the official economics major, so I supposed that what my teachers were telling me in classes about Keynesian economics and social engineering was the Real Thing. My roommate's Misesian hobby was obviously "conservative" nonsense. (McCloskey 2010a, 45)

McCloskey obviously changed her mind. She now identifies as an Austrian economist. But this still leaves unanswered the broader question: why has Austrian economics not been more influential in economic history? And how might this change? What are the barriers to

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[5] Indeed, this turn towards historical analysis can be seen as originating with the work of Don Lavoie (for example, Lavoie 1985).

[6] While the Historical School was largely purged from mainstream economic thought in the interwar and postwar periods, its wider influence remained strong. The legacy of the Historical School is most evident, via the influence of Karl Polanyi (1944), in the work of modern historians, particularly those associated with the New History of Capitalism literature (see Gareth Dale 2010). Polanyi's popularity among historians largely stems from his opposition to market liberalism and remains untarnished by the many criticisms levied at his actual knowledge and understanding of either history or economics.

[7] For a discussion of the impact of Fogel and North on economic history see Claude Diebolt and Michael Haupert (2018).

further cross-fertilization? To make progress towards answering these questions it is important to have a firmer definition of economic history.

What is Economic History?

Is economic history defined by its setting or by the questions that it addresses? According to the former view, any paper set in the past is, by definition, a work of economic history. The former view is widely held, but it is the latter view that captures the self-image of most economic historians. Economic history is not simply defined by topic. It is also connotes a distinct approach to history, one shaped by economic theory.

The Questions Asked by Economic Historians

For example, one important historical question is the profitability of slavery on the eve of the American Civil War. At the time when the initial cliometric investigations into the profitability of slavery began, historians like U.B. Philips and Eugene Genovese held that slavery was unprofitable and incompatible with a modern economy. This argument had important consequences since it lent itself to the position that the Civil War was unnecessary to eliminate slavery; over time slavery would itself naturally disappear of its own accord. This view was also appealing to some economists as it was consistent with the suppositions of Adam Smith who argued that slavery was likely to be inefficient compared to wage labor.

The first paper to investigate this question quantitively was Alfred Conrad and John Meyer (1958). Conrad and Meyer calculated the rate of return on slaves and compared them with other capital assets. With various caveats, they concluded that the returns to slave capital were at least equal to those earned on other forms of capital.

A large body of scholarship, notably Fogel and Stanley Engerman (1974), built and expanded on these insights. This scholarship established that not only were the returns on slaves comparable or slightly higher than those on other assets, but also that the number of slaves was growing over time, and that the price of slaves also suggested that slave owners expected slavery to continue. This decisively answered the older claims of historians who had viewed slavery as unprofitable and in decline before the Civil War.[8]

Another important historical question was the role of the railroad in American economic growth. Conventional histories of American economic growth viewed the railroad as the decisive breakthrough technology that made possible America's rise to economic leadership in the late nineteenth century. But the evidence they assembled to make this point was largely speculative and descriptive. Fogel (1964) sought a quantitative answer to this question. Fogel's approach was narrowly neoclassical. He estimated the cost of inputs and outputs in a world in which entrepreneurs had to rely on the next-best alternative transportation technology to railroads. He found that the additional value of railroads to the American economy circa 1890 was marginal. Fogel's (1964) conclusion was based on a standard competitive model in which the marginal product of inputs was always equal to marginal cost. Subsequent research using more sophisticated methods has overturned Fogel's original argument and, to a degree, reinstated the views of non-cliometric historians, but the new estimates are on a far sounder footing than were the guesses of pre-cliometric historians (David Donaldson and Richard Hornbeck 2016; Hornbeck and Martin Rotemberg 2019).

Another important historical question concerns the origin of the British Industrial Revolution. This question has proven harder to tackle with cliometric methods. But the application of cliometric techniques, particularly in the form of better measurement and the use of growth account techniques—has still improved our understanding of this period and of

[8] See Eric Hilt (2020) for a survey of how the conclusions of *Time on the Cross* have weathered in the 45 years since its publication.

economic growth in general. The primary contribution of the first generation of cliometrically-inclined scholars was to quantify the pace of industrialization and to provide estimates of productivity growth (Nicholas Crafts 1985; Crafts and Knick Harley 1992). While previous economic historians had viewed the Industrial Revolution as a period of "take-off" (for example, Walt Rostow 1960), Crafts and Harley found that growth was slower than had been thought previously and more narrowly confined to a few sectors of the economy.

Many insights of cliometricians into the British industrialization were summarized in Mokyr (1999) and in the first two editions of the *Cambridge Economic History of Modern Britain* (Roderick Floud and Paul Johnson 2004; Floud and McCloskey 1981). But these contributions fell short of explaining the Industrial Revolution. More recently, Robert Allen (2009) offered a simple, economic, explanation of the British Industrial Revolution based on relative factor prices and biased technological change. Allen hypothesizes that macro-inventions like the spinning jenny occurred in response to the high wages that had to be paid workers in England and to the relative cheapness of capital and energy. However, this argument has received considerable push back. The current scholarly consensus continues to place considerable importance on factors that cannot be quantified econometrically such as the Enlightenment or the rise of bourgeois values (McCloskey 2016; Mokyr 2009). Indeed, McCloskey's formulation of the origins of economic growth is explicitly Austrian in flavor.

That these positions do not yet command consent among economic historians reflects the field's high evidentiary standard. Allen's factor price hypothesis initially commanded considerable assent. And it has required scholars to assemble new datasets in order to dispute its validity (Allen 2019, 2020; Jane Humphries and Benjamin Schneider 2019; Humphries and Jacob Weisdorf 2019; Judy Stephenson 2018). Similarly, there is presently no consensus on the role played by the Enlightenment or culture more generally. The most intriguing quantitative evidence in favor of the Enlightenment-based argument that Mokyr favors comes from France and not England. Mara Squicciarini and Nico Voigtländer (2015) study the relationship between subscribing to Diderot's *Encyclopèdie* and city growth and productivity in eighteenth- and nineteenth-century France. Nonetheless, much more work needs to be done to substantiate the role of either human capital or a change in values or rhetoric.

It is important to note that these questions are first and foremost historical questions. Economic history remains a form of history, dedicated to understanding the past. It is this emphasis on historical questions that distinguishes economic history proper from other historical-inclined social scientific scholarship conducted by economists or political scientists who use history in order to test more general social scientific theories. For example, Daron Acemoglu, Simon Johnson and James Robinson (2001) were interested in the hypothesis that institutions are the ultimate cause of long-run economic development. They leveraged historical data on settler mortality in an attempt to isolate the impact of historical institutions on economic growth. This is best understood as a contribution to development economics rather than economic history proper. Other papers by Acemoglu and Robinson and their coauthors are, however, primarily concerned with addressing historical questions (for example, Acemoglu, Johnson and Robinson 2005; Acemoglu, Davide Cantoni, Johnson and Robinson 2011).

As Cantoni and Noam Yuchtman (2021, 213-214) note, for some historically-inclined economists:

> historical variation provides a laboratory in which there exist unique opportunities to test hypotheses that cannot be tested using naturally-occurring contemporary variation or experimental methods … historical natural experiments have allowed scholars to test for causal drivers of plausibly fundamental factors—for example, political institutions and culture—while preserving the causal, experimental

language that has become central to empirical work in (micro) economic development research.

Such work is important and highly influential but not necessarily economic history, defined narrowly as I doing here. A similar caveat applies to the historical research conducted by scholars in the Austrian tradition. Leeson (2007) is interested in the circumstances under which self-governance is possible. This research does make a contribution to historical knowledge, but it is not its primary goal.

Theory and Economic History

Beyond the types of questions that economic historians consider, another key distinguishing feature of economic history, at least as it is practiced in North America, is reliance on the methods of modern economics. Here it is important to note that as economics has become a broader field in recent years so has economic history become more eclectic in both themes and methods. The papers published in the leading economic history journals increasingly concern global economic history (and not just the economic history of North America or Britain) and they remain open to a range of approaches. Nonetheless, it is fair to say that the majority of papers in the two leading economic history journals, *The Journal of Economic History* and *Explorations in Economic History* use regression analysis and occasionally formal models.[9] The approach they employ is broadly speaking positivist.[10] That is, economic theory is used to generate hypotheses that are then "tested" using historical evidence. This was not always the case. Is fair to characterize economic history in the first half of the twentieth century as quite divorced from economic theory. This is evident simply from considering some of the seminal works of economic history written in this period. Reading Abbott Payson Usher's *The History of the Grain Trade in France, 1400-1700* (1913), Clapham's trilogy on the economic history of Britain after the Industrial Revolution (1926; 1932; 1938), Raymond de Roover's "The Medici Bank" (1946), Eli Hecksher's *Mercantilism* (1955a; 1955b), or Warren Scoville's *The Persecution of Huguenots and French Economic Development, 1680-1720* (1960), one has the sense that the authors only felt the need to rely on the economics of Adam Smith or John Stuart Mill; and with a few exceptions, they were innocent of, and largely uninterested in the developments taking place contemporaneously in modern economics.[11] It is telling, therefore, that when John Hicks wrote his *Theory of Economic History* (1969) it contained almost no references to modern economic theory, to which Hicks was himself a distinguished contributor.

This changed with the cliometric revolution. The cliometricians were dissatisfied with the inability of traditional economic history to resolve long-standing controversies and questions. They sought to refashion economic history as the application of economic theory to historical questions. Fogel, in particular, saw cliometrics as a variant of scientific history. Scientific

[9] The other leading economic history journal, *The Economic History Review*, is decidedly less wedded to the methods of modern economics and frequently publishes papers by historians that are largely qualitative in nature.

[10] I use the term "broadly speaking" advisedly. Economists frame their research in positivist terms though, in practice, no scientist adheres fully to the rigorous strictures of positivism, as defined by philosophers of science (see, for discussions, Lawrence Boland 1991); actual science is always messier than that. Thus, the role of economic theory is not always prediction. Good scientific theories often offer taxonomy and explanation rather than prediction. See Dani Rodrik (2015).

[11] The only exception to this is some interest by economic historians in Keynesian economics. But the major developments in economic theory that occurred in the 1930s-1950s passed by the older generation of economic historians.

history for Fogel was not the misguided quest for “historical laws”, rather it was the employment of social scientific theories to guide historical research.[12]

In practice, the cliometric revolution entailed the application of economics as it existed, at the time of the cliometric revolution, to questions in economic history (see Avner Greif 1997). The framework was neoclassical, and largely based on the assumptions of exogenously given and fixed preferences, competitive markets, and equilibrium behavior.

The cliometric revolution was extremely successful in transforming economic history as an academic discipline. But it also ran into criticism. Naomi Lamoreaux (2015) discusses the limitations of early cliometric work on the adoption of reapers in American agriculture. She discusses how studies based on the neoclassical assumption that each farm was an independent economic unit making its own cost-benefit decision over whether to adopt the new technology were overturned by studies that showed, using historical records, that farmers jointly purchased reapers or purchased them with the intention of providing harvest services to other neighboring farmers.

Another shortcoming was that cliometrics rested on neoclassical economics as it was constituted in the 1950s and 1960s and that this framework was ill-suited for studying many of the topics that had traditionally been of interest to economic historians “such as the nature of nonmarket institutions, culture, entrepreneurship, technological and organizational innovation, politics, social factors, distributional conflicts, and the historical process through which economies grew and declined” (Greif 1997, 401). Not coincidentally, many of these topics, particularly entrepreneurship and the emphasis on processes have also been of interest to modern Austrian economists.

The economic history of the United States was particularly well suited to the application of standard economic tools. The relative abundance of data and the federal character of American government means that the tools of modern causal inference can be readily applied. A different set of tools may be more appropriate for other times and places. A healthy development since the 1990s, therefore, has been the expansion of economic history beyond cliometrics as it was initially narrowly construed.

For many areas within economic history, research involves time spent accumulating data. This is the case with building historical national accounts. Here Angus Maddison was the pioneer (for example, Maddison 1983, 2003) and there is much work still to be done in extending backwards and refining his estimates of GDP and population (see Stephen Broadberry, Hanhui Guan, and David Daokui Li 2018; Roger Fouquet and Broadberry 2015).[13] Other scholars have similarly worked on collecting and improving our knowledge of prices and wages in order to estimate premodern living standards. Estimates of real wages occupied the attention of some early economic historians like William Beveridge and Thorold Rogers (Beveridge 1939; Rogers 1884). But they have been expanded and refined considerably by subsequent scholarship (for example, Allen 2001, 2003a; Gregory Clark 2005; Humphries and Weisdorf 2019). Reconstructed GDP estimates are often reliant on economic theory. Real wages require the construction of representative consumption baskets and the choice of Paasche or Laspeyres price indexes. Understanding how they are constructed is critical to properly appreciating their value and their limitations. Our knowledge of living standards in the past rest on the construction of such social scientific “facts”.

[12] Scientific history, as defined by Fogel, has the following characteristics: first, the application of social scientific (economic) theory to history, not simply as a loose analogy or reference point, but as a set of hypotheses to be tested; second, a preference for questions that can be addressed quantitatively; and thirdly, the use of the scientific-empirical model of proof as opposed to the “legal” standard used in traditional history (Fogel and Geoffrey Elton 1983).

[13] The Maddison dataset generally has been extended and updated through the work of numerous researchers.

Formal models have also been fruitfully employed in economic history. Avner Greif (2006) pioneered the use of game theory in economic history in his study of medieval trading networks and advocated for the use of analytical narratives as an alternative approach to doing economic history (see Robert Bates, Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry Weingast 1998). Analytic narratives combine rigorous, sometimes formalized, theorizing from economics and political science with the narrative form usually used by historians (see Koyama 2018).

The theory in question does not have to be mathematical nor do the tests have to be econometric. Greif's classic papers on the Maghribi traders relied on qualitative evidence (Greif 1989, 1993, 2006). To understand how private prosecution associations functioned in Industrial Revolution England, Koyama (2012, 2014) draws on theoretical insights from James Buchanan (1965) and Harold Demsetz (1970) in conjunction with detailed archival records. These records substantiate theoretical predictions concerning the bundling of private with public goods and the use of price discrimination to ensure that membership was as widely dispersed as possible.

The value of formal models is sometimes contested, especially by heterodox scholars. One use of formal models is to construct explicit counterfactuals. Traditional historians have long deplored counterfactuals. Nonetheless, every causal argument made by a historian contains an implicit or explicit counterfactual. Explicit counterfactuals have the benefit of being transparent.[14] I have already mentioned the most influential counterfactual exercise in economic history, Fogel's (1964) study of the impact of the railroad on the American economy, but there are many other examples. To buttress their analysis of the British Industrial Revolution, Crafts and Harley (2000) construct a computable general equilibrium model of the English economy to assess their claim that productivity growth was confined to a small number of sectors such as cotton textiles and iron. This model allows them to counterfactually close the British economy to trade or turn off productivity growth in certain sectors to see how different the economy would have looked in 1840 as a result.

Formal models can also be used to generate counter-intuitive predictions or provide auxiliary tests of the model that would not have been obvious to the research without the model. For example, in a recent paper Desiree Desierto and Koyama (2024) examine the rise and fall of sumptuary legislation—laws restricting dress based on class or status—in medieval and early modern Europe. The puzzle they address is why before 1200 there were few attempts to legislate dress based on class but after 1200 these laws proliferated before disappearing in the seventeenth and eighteenth centuries. To address this puzzle, a model is required. Desierto and Koyama (2024) introduce a model based on the hypothesis that individuals care about relative, and not absolute, status-good consumption. They derive negative utility from the consumption of status goods, i.e. clothing, of the competing class, and therefore allocate their income so as to maximize "status distance"—the difference between their status-good competition and those of the competing class. This model generates a non-monotonic relationship between sumptuary laws and income per capita.

To test the predictions of the model, Desierto and Koyama (2024) compile a unique new dataset of sumptuary laws at the country- and city-level for all Europe. To ascertain whether there is a causal relationship from income to sumptuary legislation they study the relationship between plague and sumptuary legislation in Italian city states. As plague shocks raised wages and per capita income in subsequent decades, a positive relationship between the plague and sumptuary legislation corroborates a key prediction of the model.

Chiu Yu Ko, Koyama and Tuan-Hwee Sng (2018) focus on the unique role of the Eurasian steppe in shaping different patterns of state formation at either end of Eurasia.

[14] I discuss this point in detail in Koyama (2020b) where I review Walter Scheidel's extensive and systematic use of counterfactuals in global history (2019).

Drawing on a large historical literature, they argue that the external threat of invasion that China faced from the steppe was more severe than any of the threats Europe faced, and that it came from a single direction. Hence a single state is more likely to emerge in China, than in Europe where it faces a severe multi-tasking problem in responding to threats from multiple directions.

This argument can be stated verbally. But its full implications emerge only from a formal model. This model generates a range of auxiliary predictions concerning the location of capital city, frequency of internal and external wars, levels of taxation, and population growth. One can then see whether these predictions are in accordance with the historical evidence. Ko et al. (2018) do so by showing that, within China, nomadic invasions are correlated with subsequent political unifications.

In summary, while the scope and range of the first generation of cliometric research was fairly narrow, this is no longer the case. Economic history, as it is practiced today, is open to a range of approaches from relatively standard applied micro-styled research to more descriptive studies and analytic narratives that combine formal models with qualitative historical evidence.

Austrian Economics and Economic History

To what extent is the approach I have outlined above consistent or compatible with Austrian economics?

Boettke (2010) defines modern Austrian economics in terms of ten substantive propositions. The first proposition is that only individuals choose. This is the premise behind methodological individualism and is common to other areas of economics and rational choice political science. The second is that the market is about exchange (as opposed to being defined in terms of specific outcomes). The third is that the facts of the social science are subjective. The fourth to seventh propositions concern microeconomics. The fourth proposition is that utility and costs are subjective. The fifth proposition is Hayek's (1945) insight into the price system as information aggregation system. Six, Austrian economists claim that private property is necessary for economic calculation. Seven, Austrian economists view the market as a process—a process characterized and driven by entrepreneurial discovery. Austrian economics is also characterized by three macroeconomic propositions: eight, money is non-neutral; nine, the capital structure of an economy is heterogenous; and finally ten, social institutions are the result of human action rather than human design.

Few of these propositions would seem to be objectionable to most economic historians. And some of these propositions have already inspired important historical research. Consider the economic history of the Soviet Union which is a thriving research area.[15] Research that neglects the Socialist Calculation Debate and simply treats the Soviet Union as another developing economy (for example, Allen 2003b) will fail to appreciate the full costs of the Soviet system or why it eventually failed. The most promising research into socialist economies such as Mark Harrison (2011) and Leonard Kukić (2018, 2020) takes seriously the fundamental problem of misallocation and traces out its consequences historically.[16]

Many economic historians study institutions as emergent phenomena shaped less by the goals and objectives of their founders than by the incentives facing those who enforce the rules. Examples include my study of the usury prohibition as the product of rent-seeking by merchants, rules and the Church (Koyama 2010), Timur Kuran's analysis of the long-term consequences of the Islamic waqf (2010), or Ogilvie's study of the merchant and craft guilds

[15] See, for instance the work of Boettke on the Soviet Union as a rent-seeking society (Gary Anderson and Boettke 1997), and on War Communism (Boettke 1990, 2001).

[16] Also see the papers referenced by Tynan in her essay in this volume.

(Ogilvie 2011, 2019). While these approaches are not explicitly Austrian, they are consistent with an Austrian approach.[17] Nicola Tynan in her essay in this volume also discusses several points of intersection between economic history and Austrian economics.

Another example of applying Austrian economics to address an historical question is Rosolino Candela and Geloso (2018). Candela and Geloso (2018) draw on the Austrian theory of interventionism to explain why private lighthouses came to be regulated and then nationalized over the course of English history, making it possible for twentieth-century economists to claim that they represented a good that could not be provided by the market. Important historical work has also been conducted in the area of free banking and monetary economics (Tyler Goodspeed 2016; Lawrence White 1990). Whether this work is considered Austrian or not, it is consistent with Austrian views on the importance of money.

What then do I think is the limiting factor holding back Austrian influences on modern economic history? One possible barrier is methodological.

Does Austrian economics have a methodological position and is this at odds with how economic historians typically conduct research? This appears to be the case to the extent that Austrian economics is associated with the methodological stance explicated by von Mises (1957) and forcefully restated by Murray Rothbard (1997). According to this viewpoint, one cannot meaningfully "test" economic theory.[18] Nor can history be used to adjudicate between different economic theories. Rather economic theory is to be deployed to understand history:

> To the economic historian, economic law is neither confirmed nor tested by historical facts; instead, the law, where relevant, is applied to help explain the facts. The facts thereby illustrate the workings of the law. (Rothbard 1976, 36)

No-one will deny the usefulness of economic theory so-applied. Nonetheless, the emphasis on the application of theory will strike most modern economists as a highly restrictive and limiting vision of the usefulness of economic theory.

My concern with the methodological strictures above is that they miss the important ways in which economic theory and evidence interact. The first and most basic is that economic theory is not a complete or settled body of thought. Theories sometimes clash and conflict. And it is crucial to use evidence to distinguish between them on the basis of their predictions. In a progressive science one often learns the most when different papers disagree. In the research detailed below I demonstrate how economic theory can be used as an engine of analysis and how quantitative and qualitative evidence can be brought to test that theory.

Elsewhere, Mises offered a more eclectic vision of the relationship between economics and history:

> It [economics] does not strictly separate in its treatises and monographs pure science from the application of its theorems to the solution of concrete historical and political problems. It adopts for the organized presentation of its results a form in which aprioristic theory and the interpretation of historical phenomena are intertwined. (von Mises 1949, 1996, 66)

[17] The outcomes described in these works are not intended but the result of action rather than human design. Furthermore, the approach used by Koyama, Kuran and Ogivlie is largely reliant on the use of qualitative historical evidence.

[18] As Rothbard puts it, "not only that economic theory does not need to be 'tested' by historical fact but also that it cannot be so tested. For a fact to be usable for testing theories, it must be a simple fact, homogeneous with other facts in accessible and repeatable classes" (Rothbard 1976, 32). As a reader on this paper pointed out, this can be regarded as "Rothbard's shadow".

Hayek's writings also lend support to the possibility of a richer interrelationship between economics and history. And modern Austrian economists have sought to move away from the kind of methodological straitjacket implied by the Rothbardian reading of Mises here (see Leeson and Boettke 2006).

A second factor is that Austrian economists were skeptical of the use of econometrics. Mises's critical statements on econometrics date to the era of Cowles Commission and the idea of using econometrics for planning and prediction.[19] What is clearer now than it was then is that econometrics is simply a tool and not tied per se to any specific approach to public policy. Nor does one need to embrace the atheoretical approach espoused by some econometricians to appreciate the importance of causal inference. Indeed, Austrian economists in the past, such as Roger Garrison (1993), have criticized mainstream economics and econometrics for neglecting causality and for elevating purely statistical concepts of causation such as Granger causality.[20] It will be evident to anyone who has followed the path of economic scholarship in the past 25 years that this criticism, though once valid, cannot be sustained today and that important progress has been made in understanding causal relationships precisely through the application of mathematical ideas.

To see the value of empirics in distinguishing between different social scientific theories consider the debate over the causes of the spike in witchcraft trials that occurred in Early Modern Europe. There are many explanations within the literature, including those that emphasize the importance of bad weather (Wolfgang Behringer 1995), weak state capacity (Brian Levack 1996; Alfred Soman 1992), disease (Eric Ross 1995), or religious tensions (Gary Waite 2009). In a recent paper Leeson and Jacob Russ (2017) argue that the European witch trials can be viewed as a form of non-price competition between Protestant and Catholic communities. This hypothesis can explain why witch trials were concentrated in the period following the Reformation and died down after the European Wars of Religion ended and why the majority of witch trials occurred in the religiously contested borderlands of the Holy Roman Empire.

Leeson and Russ (2017) test their hypothesis econometrically using a dataset of European witch trials. They measure religious contestation using data on battles (termed "confessional battles"). Going further, Leeson and Russ (2017) evaluate their argument against alternative explanations of the European witch trials. Specifically, they seek to show that these other hypotheses such as bad weather, negative income shocks, and weak government cannot explain their findings.

The benefit of this approach is transparency. The assumptions are clear and the methods common. As the coauthor of one of the main papers advancing weak government as an important cause of the proliferation and intensity (but not their initial occurrence) of witch trials (Noel Johnson and Koyama 2014), one concern is that the measure of religious contestation Leeson and Russ use (confessional battles) itself is a measure of weak state capacity. Countries which consolidate state power earlier such as Spain were able to achieve a degree of religious homogeneity and hence did not experience "confessional" battles on their soil (see Johnson and Koyama 2019). Thus the strong correlation between witch trials and confessional conflicts—many of which occurred on the borders of the fractured and religiously fragmented Holy Roman Empire—is consistent with the importance of both state capacity and religious contestation.

[19] Mises discusses the shortcoming of quantitative economics in Chapter 16 of *Human Action* (1949, 1996).

[20] Thus Garrison (1993, 106) writes: "While mathematical economists may not deny that the ultimate cause is to be found in the actions of market participants, they proceed untroubled by the fact that mathematics is inherently silent on the issue of cause and effect".

This is not to say that quantitively-inclined economic historians always agree on the evidential standard; long-running disputes do exist (and sometimes take decades to be resolved!). But the commitment to empiricism and hypothesis testing provides a fruitful path along which our understanding of the relevant issue can grow. One ongoing debate about the apparent paradox that heights declined in mid-nineteenth century America is currently being advanced in precisely this fashion.[21]

Second, we are often interested in the magnitude of effects. The question often is not does a minimum wage law, for instance, increase (or decrease) unemployment? But by *how much* does it increase (or decrease) unemployment? How do the effects differ in the long-run from in the short-run (see Jonathan Meer and Jeremy West 2016)? As McCloskey has written many times, what makes economic history scientific is its emphasis on measurement and ascertaining magnitudes.[22]

Consider the debate over the importance of coal to the British Industrial Revolution. Several historians, notably Kenneth Pomeranz (2000) and Tony Wrigley (2010), have claimed that the location of easy to access coal deposits was critical for the Industrial Revolution. Absent such deposits, they claim, sustained economic growth may never have gotten started. Economic historians like Mokyr (1990, 2009) have expressed skepticism towards this argument, but to refute it one needs to show quantitatively that coal was unlikely to have been the decisive factor. Clark and David Jacks (2007) do just this. They find that the supply of coal was highly elastic. This implies that coal production expanded as demand for coal increased with industrialization. This implies that expansion of coal output could have occurred in earlier decades had there been demand and that in the absence of coal, industrialization would still have occurred. Coal would simply have been imported from Ireland, France, or elsewhere in Northern Europe.

Similarly the debate over trade, colonies, slavery and industrialization is one where magnitudes matter. Pomeranz (2000) argues that colonial empires gave European countries a decisive edge in industrializing and that absent these “ghost acres”, European economies would have been trapped in a Malthusian crisis after 1750. The main counterargument has been made by economic historians like McCloskey (2010b), who note that, prior to 1820, international trade was too small as a proportion of GDP to have made a decisive impact on European economic growth. Harley (2004) calculates how much GDP would have been lost had Britain been autarkic. He estimates it to be only around 6 percent of GDP. Counterclaims rest on theories of increasing returns and dynamic linkages between industries (for example, Ronald Findlay and Kevin O’Rourke 2007) These arguments are theoretically plausible, but are regarded as speculative precisely because it is difficult to quantify their importance.

The most credible evidence for the role of slavery in the British Industrial Revolution exploits the information on individual slave holders compensated by the Abolition of Slavery Act of 1833. Linking the location of slave holders, Stephan Heblich, Stephen Redding, and Hans-Joachim Voth (2022) establish a causal link between slave holdings and subsequent proxies for industrialization. The authors quantitatively substantiate claims that slave

[21] See the discussion in Eric Schneider (2020). Briefly, the antebellum heights puzzle posed by John Komlos (1998) was that during a period of rapid economic growth heights appeared to fall (based on the records of army recruits). This suggests that periods of growth can see other aspects of wellbeing worsening. Howard Bodenhorn, Timothy Guinnane, and Thomas Mroz (2017) argue, however, that this finding relies on selected samples—and specifically that army recruits might be more negatively-selected in periods of great economic prosperity. Schneider (2020) shows that this can be analyzed as a case of collider bias. This is fruitful as it suggests which strategies are appropriate for addressing the problem (modeling the selection of recruits, for example) and which are not (more controls or even instrumental variables for nutritional status).

[22] For example, simply accurately accounting for the contribution of slavery to GDP in the US on the eve of the Civil War (see Paul Rhode 2024).

contributed to Britain's early industrialization. Nonetheless, questions remain. And an important topic for subsequent scholarship will likely concern the magnitude of these effects.

Even in non-econometric studies magnitudes matter. Consider the question: why was Europe fragmented and China unified? Scholars such as Jean Baechler (1975), Jared Diamond (1997), John Hall (1985), Eric Jones (1981, 2003), and Nathan Rosenberg and L.E. Birdzell (1986) argue that one reason why economic growth began in Europe rather than in China was the fact that China was a unified empire for much of its history while Europe was fragmented into numerous competing states. But why was Europe fragmented for much of its history while China tended to be ruled by a single centralized empire?

Numerous explanations exist (including the role of the steppe mentioned above). Jesus Fernandez-Villaverde, Koyama, Sng, and Lin Youhong (2023) consider one answer to this question: the fractured-land hypothesis advanced by Diamond (1997, 1998). This hypothesis proposes that “fractured-land” such as mountain barriers, dense forests, and rugged terrain impeded the formation of large empires in Europe compared to other parts of Eurasia.

Diamond and other authors stated this hypothesis verbally. But one requires a model to make quantitative statements about the relative importance of fractured-land. Fernandez-Villaverde et al. (2023) build such a model of state formation. In this model, Eurasia is divided into fine grid-cells that begin the simulation as independent polities. Over time, as polities come into conflict with one another, the outcome is decided by their geographical characteristics and underlying agricultural productivity or initial population density.

This model of state formation can generate some striking patterns that resemble what we observe historically: Europe remains fragmented and polycentric whereas one state always unifies China. There is an intermediate level of state formation in other parts of Eurasia. These results suggest that the presence of a core area of high land productivity in China and the lack thereof in Europe was critical. Compared to a purely qualitative analysis, the added value of a formal model is that it generates probability distributions over historical outcomes. This allows us to gauge the relative importance of structure versus contingency in shaping observed degrees of political unification.

Thirdly, empirical evidence provides information about what mechanisms were relevant. For example, scholars interpreted Weber’s classic *The Protestant Ethnic and the Spirit of Capitalism* (1930) as arguing that Protestantism was a driver of economic growth. The main channel they hypothesized was the existence of a Protestant work ethic. A scholar convinced of the plausibility of this hypothesis could easily apply it and find evidence in support of it. After all, Protestant countries like Norway and the Netherlands are on average richer than Catholic countries. But this consistency check is a low bar.

Credible evidence on the Weber hypothesis awaited a seminal paper by Sascha Becker and Ludger Woessmann (2009). They examine the relationship between Protestantism and prosperity in the context of nineteenth-century Prussia—the setting that motivated Weber’s initial claims. More Protestant parts of Prussia were indeed more prosperous. They were also more likely to be literate. The question was whether this relationship is merely a correlation or whether it reflects an underlying causal relationship. To address this, they proposed distance to Wittenberg—the city where the Reformation began—as a source of exogenous variation in the likelihood of individuals becoming Protestant. Distance to Wittenberg is associated with higher levels of literacy. As distance from Wittenberg does not predict pre-Reformation differences in education and economic development, Becker and Woessmann (2009) claim that the Reformation affected human capital acquisition, and not vice versa. This empirical study provides novel insights into which mechanisms were relevant. Becker and Woessmann (2009) find that the economic differences between Protestant and Catholic areas are fully explained by differences in literacy. This suggests that there is little role for an independent effect associated with other aspects of Protestantism, such as a Protestant work ethic.[23]

Taken together, this discussion suggests that economic theory and empirical evidence have a richer relationship than is implied by a narrow reading of von Mises (1957) or by Rothbard (1997). Empirical evidence informs one's understanding of which economic models are appropriate. Empirical evidence also sheds light on the size of the different effects predicted by theory. And finally, empirical evidence is critical for understanding the relevant mechanisms at work. None of this is incompatible with the historical and empirical work I see being done today by scholars in the Austrian tradition.

Concluding Comments

The title of this essay suggests its main thesis: economic history has been a comparatively successful subfield in economics because it is (or aspires to be) a progressive science, that is, a body of research that aims to add to cumulative knowledge. In this regard, economic history is different from many other areas within the academic discipline of history which explicitly repudiate positivism. It also distinguishes it from many self-consciously heterodox variants of economics which focus their attention on criticism and methodological issues rather than the ordinary science involved in "doing economics".

My reading of the Austrian tradition is that during its most fruitful period between 1870-1930 it was also a progressive science in the sense that I have defined it. Similarly, the most productive work within the Austrian tradition since the 1980s has been achieved by scholars aiming to build a progressive research program.

In this essay, I suggested several points of engagement and cross-fertilization between scholars in the Austrian tradition and those working in economic history. To do this, I provided an outline of how economic history is conducted—the questions asked, the methods employed, and the evidence considered. I further described how theory and empirics are interrelated in much economic history research. One major barrier to greater cross-fertilization appeared to be methodological. But this barrier has become less relevant over time. And I look forward to seeing more economic history research inspired by Austrian themes and more work by Austrian economists motivated by a serious engagement with economic history.

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[23] Again it is indicative of a progressive science, that more recent research has suggested that nationalism can account for observed differences in incomes and literacy in late-nineteenth century Germany (Felix Kersting, Iris Wohnsiedler, and Nikolaus Wolf 2020).

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