

Review article

PREINDUSTRIAL CLIOMETRICS

Mark Koyama*

Maristella Botticini and Zvi Eckstein, *The Chosen Few: How Education Shaped Jewish History, 70–1492*, Princeton, NJ: Princeton University Press (2012), 346 pp. ISBN: 9780691144870; £27.95, \$39.50

Peter Temin, *The Roman Market Economy*, Princeton, NJ, and Oxford: Princeton University Press (2013), 299 pp. ISBN: 9780691147680; £24.95, \$35.00.

In the 1960s and 1970s, economic history was transformed by the application of economic theory and econometric techniques. This development – labelled the cliometric revolution – fundamentally changed scholarly opinion on a range of topics from the productivity of slavery in the US to the size and speed of the British industrialisation. This transformation deserves the appellation of a revolution, but even at the time the impact of the cliometric revolution was greatest on those topics in relatively recent history that lent themselves to econometric and statistical analysis. Its impact on other areas within economic history was limited by the fact that the rich historical data that exist for nineteenth-century Britain or the US either did not exist or had not yet been collected for earlier periods of time or other parts of the world. Thus, much of the promise of the application of economics to history was not fully realised. In particular, the transformation that occurred in nineteenth- and twentieth-century economic history did not much trouble historians working in earlier time periods or in countries other than the US and Britain.

In the wake of the cliometric revolution, a major challenge for economic historians has been to demonstrate that the application of economic tools and theory can be similarly fruitful in other areas of history. In this essay I begin by reviewing two recent works in economic history: *The Chosen Few* by Mariastella Botticini and Zvi Eckstein and *The Roman Market Economy* by Peter Temin in order to evaluate how economic historians can tackle topics that do not immediately lend themselves to the standard tools of the economics profession. I use this opportunity to survey recent research in the field. Economics is, I contend, an insightful engine of intellectual inquiry and analysis; moreover, this engine of analysis has been only partially applied to many periods of history, so the marginal benefit from further work in these areas is high.

The Roman Market Economy

Let me begin by defining cliometrics more carefully as the application of economic theory and econometric methods to answer historical questions. In this sense, it differs slightly, I would say,

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from the closely related field of comparative/historical political economy as exemplified by, for example, Daron Acemoglu and James Robinson. These authors use historical examples and historical data as a testing ground for ideas that emerge from political science or economics (see, for instance, Acemoglu and Robinson 2012). Cliometrics, traditionally understood, was focused on answering questions raised within the discipline of history. This distinction is an important one for assessing the significance of the contribution the two books under review make, as all three authors aim at shaping the opinion of historians within their field as well influencing other economists and social scientists.

Several recent books have attempted to understand the origins of economic growth. Among them the most notable contributions by economic historians include Avner Greif's *Institutions and the Path to a Modern Economy* (2006), Greg Clark's *Farewell to Alms* (2007), Jan Luiten van Zanden's *The Long Road to the Industrial Revolution* (2009) and Jean-Laurent Rosenthal and R. Bin Wong's *Before and Beyond Divergence* (2011). The two books under consideration here are focused on slightly narrower topics, but they are still extremely ambitious and wide-ranging in scope.

Peter Temin, author of *The Roman Market Economy*, is a leading authority on the American economy of the nineteenth and twentieth centuries and since the turn of the twenty-first century has written a series of papers on the Roman economy (see Temin 2001, 2004, and 2006) *The Roman Market Economy* is a synthesis of these papers. It amounts to an important work, particularly for ancient historians.

I mean no criticism when I say that perhaps its most significant impact will be on historians and classicists rather than on economists or economic historians. Work in ancient economic history was too long trapped by Karl Polanyi's view that past economies were fundamentally different from modern market economies (Polanyi 1944). This framework, expounded and developed by Moses Finley (1999 [1973]), meant that economic historians of antiquity came to use a different language and a different set of conceptual frameworks from those used by economic historians of even medieval or early modern Europe.¹

The attention ancient historians have already paid to Temin's published articles as well as the interest classicists based at Stanford have shown in *New Institutional Economics* demonstrates that this situation has already started to change and that the paradigm established by Finley has become outmoded.² Ancient historians are increasingly interested in and open to drawing insights from economists and economic historians.³ Nevertheless a gulf in terms of training, techniques and vocabulary remains. Temin's book does an excellent job in bridging this gulf.

As the title indicates, Temin's thesis is that the Roman economy was a market economy. The first task he sets himself is explaining what a market economy is. This is essentially a primer in supply and demand and the theory of comparative advantage (which economists can be forgiven for skipping). He then sets himself the task of showing that Roman prices and prices from antiquity in general are consistent with the conjecture that the ancient economy was a market economy. This is important because ancient historians have often argued that, because the Roman state levied grain from Egypt and north Africa, there was no Roman grain market.

Temin shows that the grain supply cannot have been controlled by government fiat and that even a market economy that is subject to occasional government interventions remains a market economy in the sense that prices are largely determined by changes in supply and

demand. He undertakes a series of simple econometric exercises to show that grain prices are consistent with what we would expect in such a market economy. Using standard linear regression techniques, he establishes that prices were highest where demand was greatest in the city of Rome and were lowest at source in Egypt – that is, the coefficient on distance from Rome is negative and significant in regressions explaining price variation across the empire. Additional, higher frequency data from Babylon shows commodity prices following a random walk – further evidence that markets rather than political administrators were responsible for allocating goods in the ancient world. Temin goes on to assemble various proxies for price inflation and is able to show that they point to a high degree of price stability in the first two centuries of the Roman Empire. This was a society in which the price system functioned.

Economists and economic historians of later periods will need little convincing that the Roman economy was based largely on market exchange. To my mind the existence of the city of Rome with a population of close to a million pointed to the existence of a sophisticated division of labour that was inconceivable without a flourishing market economy. Similarly, Monte Testaccio – the towering pile of broken amphorae close to the river Tiber – testifies to long-distance trade on a vast scale. Certainly it is hard to imagine that a pre-modern state could have organised economic activity on such a scale through administrative fiat.

Perhaps the most interesting chapters are those on how the grain trade and the markets for land, labour and financial services were organised. In the grain market, Temin demonstrates that a range of formal and informal institutions supported long-distance trade. Moral hazard was limited through the use of receipts, the labelling of goods, and guilds which were capable of excluding merchants who were deemed untrustworthy. Social networks also played a crucial role in screening potential business partners. Temin suggests that these institutions were every bit as sophisticated as those employed by merchants in early modern Europe or colonial America.

Contrary to the view of Finley and others who argued that the Roman economy belonged to a distinct category of non-market economies because it was a slave economy, *The Roman Market Economy* presents a wealth of evidence suggesting that there was a labour market in ancient Rome. Wage labour was common, particularly in the cities, and wages varied with the skills required for the job. The Roman system of slavery differed fundamentally from that of the US South, and slaves competed with non-slaves for work in a single labour market. There was a degree of labour mobility, and wages responded to large exogenous shocks such as the plague of the second part of the second century CE. The land market similarly appears to resemble much more a modern land market than feudal or early modern systems of land ownership under which land usage was heavily restricted.

Financial markets, in particular, reached a comparatively high level of development in the Roman Empire. All sophisticated pre-industrial commercial economies ran on credit due to the costs associated with transporting bullion. Rome was no exception. Most loans were no doubt small-scale and informal, and most financial relationships personal. But Temin demonstrates that there was a market for loanable funds in ancient Rome. There were limits on the legal rates of interest, but market rates fluctuated below the legal maximum in response to the scarcity or abundance of money. Long-term partnerships, or *societates*, were used to fund larger ventures. Banks were prominent in financial intermediation, pooling capital and channelling idle funds into productive investment.

The final part of the book seeks to place the Roman economy in the context of recent discussions in economic history on the Great Divergence. Temin performs a growth accounting exercise that allows us to benchmark Roman economic development relative to other pre-industrial societies. By correcting mistakes made by earlier authors, notably Hopkins (1980), he suggests that the available evidence is consistent with viewing the richest parts of the Roman Empire as being as economically developed as the richest parts of Europe in around 1600 CE. Economic historians like to emphasise that sustained or modern economic growth began in 1800. We know this because if we extrapolate modern rates of GDP growth, say of 3 per cent per annum, back in time we soon obtain estimates for pre-industrial GDP per capita that are inconsistent with human survival. But, as Temin rightly points out, this does not mean that there was no growth in the pre-industrial period, just that there was no prolonged or uninterrupted period of sustained and high growth of the kind we have experienced in modern times. There clearly were periods of prosperity and rising living standards, and it seems evident that the Roman Empire corresponds to one such period.

The main driver of economic growth in the Roman period was the expansion of the market and the corresponding growth in the division of labour that followed the establishment of the Pax Romana. In conquering the Mediterranean, the Romans created a single market for grain, olive oil, pottery, wine and other goods. There was technological innovation in the Roman period, and Temin summarises recent research that indicates that the Romans were instrumental in the diffusion of technologies like the waterwheel across western Europe. Nevertheless, the gains associated with the expansion of the market and technological innovation were in a continuous race against the Malthusian forces of a greater population and diminishing returns to production in agriculture. In Temin's telling, the prosperity of the first two centuries of the Roman Empire were largely due to the former two factors temporarily outpacing the latter two.

This explanation is no doubt partly correct; but as it stands it is under-theorised. The claim that Malthusian factors were responsible for the failure of Roman Empire to sustain comparatively high levels of per capita income is difficult to square with the timing of the decline of the Roman economy. A major plague hit the Roman economy in the 160s, and historians estimate that it caused a large decline in the population. In a Malthusian model this fall in population should lead to an increase in real wages and per capita income and to an increase in urbanisation via Engel's Law (Voigtländer and Voth 2009).⁴

However, historians traditionally have dated the decline in the Roman economy in general to precisely this period. The evidence suggests that markets contracted and urbanisation declined in the early third century CE. The best explanation for this decline is the breakdown in the political equilibrium that had maintained the Pax Romana and to the increased military capacity of the barbarian tribes on the imperial frontiers. Political economy considerations were central both to the success of the Roman Empire and economy in the first two centuries of the Principate and to the problems that emerged after the death of Marcus Aurelius in 180 CE, but *The Roman Market Economy* generally avoids a discussion of the political economy of the Roman Empire. In saying this, I should emphasise that although I see it is a shortcoming, the decision to focus on strictly economic issues is an entirely understandable one: the political economy of the Roman Empire is a fitting subject for another book. As it is, *The Roman Market Economy* is an important work and it points a way forward for future research on the economic history of antiquity.

The Chosen Few

Like *The Roman Market Economy*, *The Chosen Few* should be understood as a pioneering attempt to bring to bear the insights of economic theory on a topic that has hitherto attracted only a small amount of interest from economic historians: the history of the Jews in medieval Europe. The result is a fascinating and readable account. Botticini and Eckstein combine the cliometrician's facility with numbers with the historian's ability to both utilise archival evidence and synthesise the secondary literature.

Their account of the history of the Jewish people from Roman times until the end of the Middle Ages is both surprisingly rich and deceptively simple. They explain how an exogenous event – the destruction of the Second Temple – caused Judaism to gradually involve into a literate religion. This was a momentous development. Jews who faced a high cost of becoming educated had an incentive to allow their religious affiliation to lapse or to convert to Christianity or Islam. Jews became on average more literate and more educated than the rest of the surrounding population. This account, which the authors ground in a simple model, generates several predictions. According to the model, Jewish farmers should convert to other religions, and this indeed what occurred during the first millennium. The Jewish population shrank dramatically from a high point of perhaps five million in 70 AC to perhaps as few as one million in 650 CE. The model Botticini and Eckstein build can explain this. Judaism cannot survive in a purely agrarian economy because it is rarely economically rational for subsistence farmers to invest in literacy, but it flourishes in an urban and commercial economy where Jewish religious principles and economic incentives are more closely aligned. As a result, shocks like the fall of the Roman Empire or the Mongol invasion of the Middle East had a disproportionately large effect on Jewish communities and populations.

Botticini and Eckstein use their model to show how the economic recovery in the Middle East in the ninth and tenth centuries benefited literate and educated Jews who possessed a comparative advantage in activities such as trade, medicine and moneylending. Similarly, they are able to demonstrate how the commercial revolution in Europe led to Jews settling across western Europe. Jewish comparative advantage in long-distance trade and moneylending allowed them to prosper, but at the same time made them vulnerable to exploitation and persecution and a source of revenue to rulers.

The way Botticini and Eckstein build their argument is an example of how to do rigorous economic history for the pre-industrial period. Theirs is a quantitative economic history that works despite the absence of abundant or high-quality data. There are no regression tables in *The Chosen Few*, but there is plenty of economic reasoning and a lot of empirical evidence, carefully weighed and considered. In this sense it is an example of an analytical narrative as serious economic history. Much of the evidence Botticini and Eckstein cite to support their arguments comes from the Mishnah commentary – the body of oral law collected and written down during late antiquity and the early Middle Ages – a source that has not previously been used by economic historians. And though specialists will no doubt quarrel with parts of their wide-ranging argument, in general they successfully synthesise a huge historical literature.

Salo Baron, the great historian of medieval Jewish history, developed a critique of a lachrymose conception of Jewish history as continuous suffering and persecution, from the

destruction of the Second Temple to medieval pogroms and the Holocaust (Baron 1975). Botticini and Eckstein are very much in the anti-lachrymose camp. They emphasise and celebrate Jewish religious and economic achievements in the early medieval period. The chapter on Jewish moneylending is excellent in this regard. However, the positive picture they draw of Jewish moneylending is largely derived from Botticini's archival work on Jewish moneylending in fifteenth-century Italy (see especially Botticini 1997, 2000). It is not clear that the conclusions Botticini and Eckstein draw from this example necessarily generalise to England, France or Germany where the state played a much more prominent and problematic role in organising and exploiting Jewish moneylending, a role that often amounted to the king effectively using Jewish moneylenders as unofficial tax collectors.⁵

From the thirteenth century onwards Jews were increasingly subject to persecution and expulsion from western Europe (Anderson, Johnson and Koyama 2013). The rise of popular anti-Semitism and the increasing marginalisation of Jews in the economy of western Europe led a period of economic and culture decline in the early modern period. The rise of nascent nation states at the end of the Middle Ages was accompanied by the expulsion of Jews from most of western Europe. Botticini and Eckstein end their account in 1492 – the year in which the largest Jewish population in western Europe was expelled from Spain – and thus do not provide a detailed account of why this transformation occurred and what consequences it had. A sequel is promised which aims to bring Jewish economic history up to the present.

New horizons

In this article I have stressed the following point: the application of economic concepts and techniques to the pre-industrial period often requires original and imaginative use of both theory and data because we cannot take for granted national income statistics or other high-quality data sources. Pre-industrial cliometric history, when it is done well, pays close attention to the data-gathering process and to the concerns and interests of historians. In this respect it can be distinguished from some of the broader-brush investigations of economists into the past which rely on cross-country growth regressions and prioritise issues of identification and econometric validity.

Both the books reviewed here exemplify these virtues, but they are not the only examples of good work being done on pre-industrial economic history. I conclude this article by pointing out some of the most innovative and interesting work in pre-industrial cliometrics being undertaken by other scholars and by focusing on recent research of my own that seeks to better understand the political economy of medieval and early modern European states.

As we have seen, some of the most interesting work in economic history explores periods or societies which have not previously received detailed attention from economists. Timur Kuran's recent book *The Long Divergence* (2010) has pioneered the study of the economic history of Islam. His former student, Jared Rubin, is currently exploring the relationship between religion and political legitimacy in the Middle East and Europe in the medieval and early modern periods. Rubin argues that it was the greater dependence of political authorities in the Islamic Middle East on religious authority for political legitimacy that made it harder for Islamic institutions to adapt and change in response to new technological and economic opportunities after 1500 (Rubin 2011; Coşgel, Miceli and Rubin 2012). The economic history of China and

Japan are also rightly receiving a considerable amount of scholarly attention; recent innovative work on the political economy and institutions of these two countries includes Ma (2011), Tuan-Hwee (2011) and Tuan-Hwee and Moriguchi (2013).

The two books with which I began this review, *The Roman Market Economy* and *The Chosen Few*, focus on the economics of two different pre-industrial societies: ancient Rome and the Jewish world of the early and high Middle Ages. But perhaps the most exciting area of recent research is the early modern period. Nick Crafts (1985) and Knick Harley (1982) have shown that economic growth during the class Industrial Revolution was significantly slower than previously thought. The implication of this research is that growth in the period before the Industrial Revolution must have been faster, and the roots of economic growth deeper, than had previously been realised. A large body of recent research has consequently tried to locate the secrets of British or European economic success in institutional, technological or demographic changes that occurred in the period before 1800 (see, in addition to work already cited, North and Weingast 1989; Mokyr 2002, 2008; Acemoglu, Johnson and Robinson 2005; Voigtländer and Voth 2006; de Vries 2008; Allen 2009).

Prominent economic historians continue to debate the relative importance of labour-saving technological change (Allen 2009), Enlightenment ideology (Mokyr 2009), or access to cheap coal and other geographical advantages that England enjoyed (Allen 2009; Wrigley 2010). In terms of recent research on institutional change, it is increasingly clear that there was no single institutional change that we can point to as being decisive in the lead-up to the Industrial Revolution.⁶ But it is also clear that there was a gradual improvement in institutional quality in this period (Mokyr and Nye 2007).

Understanding this process is crucial for a better understanding of the growth process in general. The story of the onset of modern economic growth in western Europe poses a series of puzzles, especially from a classical liberal perspective. European states increased in scale and size in the period before 1800. Armies grew dramatically as did civilian bureaucracies and the ability of the state to extract tax revenues from the populace (Brewer 1988; Bonney 1995, 1999). England in the eighteenth century had the highest tax burden per capita in Europe after the Netherlands (Mathias and O'Brien 1976). All European states practised mercantilism and protected domestic trade (Nye 2007). Despite all of this, the growth of the state in this period did not prevent economic growth from getting started, although it likely impeded and obstructed it. How was this possible?

In this respect Ekelund and Tollison (1981) made an important and neglected contribution by highlighting the importance of rent-seeking in impeding growth in pre-modern economies and thus in emphasising the importance of the decline in rent-seeking activities in explaining the economic success of eighteenth century England. This work was neglected because substantive evidence in support of it was always lacking. But this has now begun to change. The research of Bogart and Richardson (2009; 2012) shows how from the mid-eighteenth century onwards Parliament increasingly passed laws which increased the overall efficiency of the economy. My own recent work with Noel Johnson also seeks more broadly to better understand this improvement in political institutions across early modern Europe.

One important factor was that the deadweight loss associated with taxation lessened even as the total tax burden increased. Johnson and Koyama (2012) examine the strategies the rulers of England and France adopted to increase tax revenues and access to credit in the seventeenth century. We provide evidence that the increase in fiscal capacity was accompanied by

investments in standardisation that had positive spillover effects on the growth potential of the English and French economies in the eighteenth century.

Another important dimension along which there was a marked improvement in the quality of European institutions in the early modern period is legal standardisation or rule of law. Rule of law is typically held to be an important precondition for economic growth. But often it is treated as black-box, with little regard as to what it measures or the process whereby it was developed in economically successful societies. In Johnson and Koyama (2011) we investigate the relationship between rule of law and fiscal capacity in seventeenth-century France. Our measure of rule law is the likelihood of bringing a witch to trial. We find that there is a strong negative correlation between taxes collected per capita and witch trials at a regional level and that, as regions increased taxes over time, the number of trials in those regions fell. This provides evidence that the rise of the French state in the early modern period was associated with the imposition of a more standardised legal system and better rule of law.

Religious toleration is another defining feature of modern democratic societies, and a commitment to religious toleration is often seen to be a precondition for scientific innovation and consequently economic growth. In a paper that is closely related to *The Chosen Few*, we therefore explore why European states alternatively protected and exploited or expelled their Jewish minorities in the medieval and early modern period (Anderson, Johnson and Koyama 2013). We use data on European weather to test the hypothesis that negative income shocks made expulsions and persecutions of Jews more likely. We find that a one standard deviation decrease in temperature is associated with a 50–100 per cent increase in the baseline probability of an expulsion taking place in the fifteenth and sixteenth centuries. However, this effect disappeared after 1600, and we view this as consistent with other evidence that suggests that European political institutions became more robust and stable in the period before the onset of sustained economic growth.

These findings are suggestive and provocative. Nevertheless, the process whereby the most successful European states were able to centralise political authority, liberalise their economies and reduce rent-seeking in the period before the Industrial Revolution remains poorly understood. Future research should explore in more detail the mechanisms that are responsible for this transformation. This requires further research in what I have termed pre-industrial cliometrics, where the term ‘cliometrics’ is broadly construed so as not to exclude works in the analytic narrative tradition as well as research that involves the application of econometrics. Only by collecting more evidence and assessing it through the lens of economic theory will we make substantive progress in understanding how the economies of the past functioned and how the modern world economy emerged. Both the books I have considered in this article point the way forward for economic historians.

Notes

1. For an excellent critique of Polanyi's influence in economic history see Hejeebu and McCloskey (2004).
2. See for example Manning and Morris (2005) and the papers contained in Scheidel, Morris and Saller (2008).
3. In addition to the works already cited, see Scheidel (2009, 2010).
4. Engel's Law states that as incomes increase the proportion of income spent on foodstuffs declines. Hence, the increase in per capita income in Europe after 1348 stimulated the production of manufactured goods and luxuries.
5. For England see Lipman (1967), Mundill (1991, 2002) and Koyama (2010). For Germany see Baron (1967a,b).

6. The view that the Glorious Revolution was a decisive institutional change as advanced by North and Weingast (1989) has been cut down to size. See Clark (1996) and Pincus and Robinson (2011).

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