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# Economic History Matters


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# Economic History Matters

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**Abstract**

This paper considers the future of economic history in the context of its relationship with economics. It is argued that there are strong synergies between the two disciplines and that awareness of the economic past is an important resource for today's economists. Examples are given that illustrate these points. It is clear that the past has useful economics but the potential value of economic history to economics will only be realized if economic historians are fluent in economics and organize the presentation of their research findings with a view to addressing questions that matter from a policy perspective.

**Keywords**: analytic narratives; economic growth; institutions; technological change

**JEL Classification**: A12; N01; O49

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## 1. Introduction

Economic history is under threat but still has a great deal to offer. This is as true today as it has been throughout my career. In this short essay, I aim both to show why the study of economic history is more valuable than is often recognized and also to propose a survival strategy for the discipline. In so doing, I shall concentrate on the relationship of economic history with economics. This is, of course, not the only academic subject with which economic history can and should interact fruitfully but it is vitally important and it is the area on which I am best qualified to write.

However, the general line of argument that I wish to make is applicable more generally, as follows. First, economic history is a small discipline. Like the citizens of a small country, its practitioners are well advised to be fluent in the language of their big neighbours including economics and history. When we have a conversation with these other disciplines, it should be on the basis that we have things to teach as well as to learn. Second, economic historians study matters of great policy relevance for today's world. It is important that opinion-formers and policymakers know this and are made aware of the results of our research and their significance for the big issues. This is not only a duty but also a way to justify and to obtain research funding. Third, this implies that the weight of effort should tilt somewhat more towards policy-menu-driven questions and a bit away from a preoccupation with the pursuit of research motivated by engaging with the historiography. We need to be willing and able to discuss the lessons of history and debunk the myths.

## 2. Why do economics and economic history need each other?

In an early survey article McCloskey (1976) listed 5 reasons why the study of economic history was useful for economists, namely, that it would produce more economic facts from a wider range of experience, better facts from the availability of materials that are no longer confidential, better economic theory based on identification of regularities to be explained, better economic policymaking through knowing the lessons of the past, adding up to the production of better economists. Many years ago, at a time when cliometrics was much less widely diffused than is now the case, I suggested a parallel list of the contributions that serious understanding of economics might make to economic history, namely, more facts from basing measurement on appropriate economic concepts, better facts from using econometric techniques, better hypotheses drawing on the questions suggested by economics, better interpretations of the data through paying attention to model specification and robustness, adding up to better historians (Crafts, 1987).

It is straightforward to give examples of these propositions. One obvious example of 'more facts' for economists relates to measurement of living standards where putting together historical estimates of the Human Development Index (HDI) as in Crafts (2002) highlights the progress made by developing countries during the twentieth century as was subsequently recognized by Kenny (2005). Better facts abound in demographic history as epitomized by the work of the Cambridge Group for the History of Population and Social Structure on English parish registers (Wrigley et al., 1997) which is a rich source indeed for those seeking to investigate the claims of unified growth theory (Galor, 2004). Better economic theory in terms of growth economics has been aided by the realization thanks to decades of research on historical national accounts compiled by Maddison (2003) that the predictions of the Solow growth model are not matched by the historical experience of growth; the discovery of 'divergence big time' (Pritchett, 1997) epitomizes this. Better policy can be aided by understanding that 'lessons' drawn from present-day experience may be contingent rather than universal, as is underlined by the apparently different relationships between protectionism and growth before and after World War II (Clemens and Williamson, 2004).

For historians, more facts would again include historical national accounts noting that estimates of income levels can now be extended far back in time (Broadberry, 2011) and better facts using econometric techniques would naturally include estimates of real wages over the long-run based on samples drawn from various primary sources processed by regression analysis (Clark, 2005). Better hypotheses come from asking interesting questions and specifying the ceteris paribus well; here an excellent example turns on the role of relative factor prices in the development and diffusion of new technologies (Allen, 2009; Magee, 2004) along the lines now highlighted in models of directed technological change and inappropriate technology (Acemoglu, 1998; Acemoglu and Zilibotti, 2001). Powerful examples of better interpretations come from the economics of catch-up growth; as Harrison (2003) pointed out, contrary to some claims in the literature, growth in the Soviet Union after World War II was not surprisingly good taking into account the low initial level of income and much faster growth in western European countries which started out at a similar level. Similarly, the argument made by Temin (2002) and other sceptics of British decline that there was no policy-induced growth failure during the Golden Age of growth does not seem plausible once it is realized that Britain was overtaken in terms of (PPP-adjusted) income and productivity levels by many European countries during those years (Crafts, 2011).

These examples are taken from the key area of growth economics in which research has made great strides in directions which are conducive to a fruitful dialogue.[1] This has brought themes which have always been central in economic history into mainstream economics while at the same time giving economic historians sharper analytic tools. That said, this is also an example of an opportunity, still to be seized, for the latter to set out more forcefully what they can bring to the party.

Nevertheless, successful exploitation of these synergies often requires the professional skills of the sister discipline. An excellent example of this is the lessons to be drawn from the Bengal famine of 1943/44, the locus classicus of the reorientation of famine studies from a Malthusian to a distributionist perspective by Sen (1981). Sen’s interpretation that the famine was due to hoarding and speculation rather than a food-availability decline matches the claims of the official inquiry published in May 1945. O’Grada (2008) interrogates the sources as a well-trained historian would and shows that officials really believed that food shortage was the basic problem but at a time of war needed to say the opposite since it undermined demands to divert shipping and food supplies from the war effort and that the report was a whitewash whose conclusions are inconsistent with the available quantitative evidence.

Equally, convincing use of quantitative techniques can depend on fully understanding the economic theory on which they are based. A good case in point is using estimates of total factor productivity (TFP) growth based on conventional growth accounting assumptions, i.e., by imposing a Cobb-Douglas production function, to infer the rate of technological progress. This will not be reliable either in the case of biased technological progress and a low elasticity of substitution as in the

[1] These include models of endogenous and directed technological change, recognizing the importance of institutions and serious analysis of catch-up growth. The danger that Solow highlighted a quarter century ago, namely that “economic theory learns nothing from economic history, and that economic history is as much corrupted as enriched by economic theory” (1985, p. 328) seems substantially reduced in this context at least.

United States in the nineteenth century (Abramovitz and David, 2001) or where there are fixed factors of production, high adjustment costs and economies of scale as in early postwar European manufacturing (Crafts and Mills, 2005). Since it turns out that the deviations from the standard assumptions vary over time, inter-temporal comparisons of TFP growth are not necessarily a good guide to changes in the rate of technological progress but many incautious users of the methodology seem unaware of this.

## 3. What is distinctive about economic historians' expertise?

If it is accepted that awareness of economic history is useful for economists, is it also the case that specialist economic historians add significant value over and above the efforts of an economist's DIY economic history? Obviously, I believe that the answer is yes and that we should make sure that this is known to economists, opinion-formers, and policymakers. At the most basic level, economic historians know a great deal about what has and has not worked in the past and why. In some cases this simply entails a deeper knowledge of experiences which at a superficial level are already conventional wisdom but widely-held beliefs are not always right and sometimes need to be dispelled.

It is straightforward to illustrate these points with examples based on famous episodes. For example, it is well-known that the Soviet command economy ultimately failed to deliver a satisfactory growth performance. This has been well documented by economic historians (Allen, 2003; Ofer, 1987) and it is clear that this reflects poor TFP growth reflecting incentive-structure problems that stifled innovation (Berliner, 1976). But, in addition, archival work has developed real expertise in the way in which planning was carried out and the insuperable problems that it encountered (Gregory and Harrison, 2005). Similarly, it is generally accepted that the Marshall Plan was good for early postwar European growth and new 'Marshall Plans' are frequently demanded. However, it is probably not generally realized that the Marshall Plan worked as a structural adjustment program rather than through donation of large amounts of unconditional aid (De Long and Eichengreen, 1993) and that it shares much in common with the proposals that comprised the original Washington Consensus (Crafts, 2011).

Recent events have once again shown the importance of scholarship on the American Great Depression of the 1930s. This is, of course, commonly attributed to the 1929 Wall Street Crash. Economic historians would disagree but would say that financial crisis in the form of bank failures was the heart of the matter. Over time, a massive amount of detail has been provided that allows us to understand why this crisis occurred, why it had such devastating effects on the level of economic activity, what could have been done to prevent it, or to respond more effectively once it was under way (Calomiris and Mason, 2003; Calomiris and Wilson, 2004; Mitchener, 2007; Richardson and Troost, 2009). Some of the lessons were heeded in the recent past, notably, in terms of the policy actions taken in 2008/9 once the current crisis had erupted. Others relating to the regulation of financial systems and the design of preventive measures appear unfortunately to have been forgotten even though financial historians are well aware of them (Mishkin, 1991).

A classic example of a myth that seemingly refuses to go away but is important continually to refute is that the British Industrial Revolution and transition to modern economic growth was based on the gains from imperialistic exploitation in general and African slavery in particular (Frank, 1998; Inikori, 2002). Mainstream economic historians have repeatedly shown that the numbers do not add up to sustain these claims – the trade volumes, markets and profits were too small (Harley, 2004). The key to the industrial revolution was technological change not relaxing a savings constraint for which in any case, as O'Brien put it, "the periphery was peripheral" (1982, p. 18). It is quite reasonable to see commercial expansion and the open economy setting as an influence on incentive structures that encouraged the development of new technologies through input price ratios and increasing market size (Allen, 2009; Findlay and O'Rourke, 2007) but also to recognize that the innovative response depended on a favourable supply-side environment which owed nothing to imperialist adventures (Mokyr, 2009). Ironically, the gains from British innovations went in large measure to consumers in the rest of the world through rapidly falling prices for exports; for example, Leunig and Voth (2011) estimated that annual foreign consumer-surplus gains in cotton textiles alone were equivalent to 2-3 per cent of British national income in the 1820s and 1830s – greater than any supernormal profits from intercontinental trade and colonization of resources.

These examples could be multiplied but it is more useful to focus on several (related) areas where economic historians have special expertise, distinctive arguments to make, and in which a long-run perspective is valuable, namely, technological change, institutions, and catch-up growth. In each case, I shall highlight some examples with no pretence at providing a comprehensive survey.

Technological change is central to the study of economic history both in terms of its consequences and also as something to be explained both with regard to invention and diffusion. As such, the notions of factor-saving bias and of the appropriateness of adopting new technology in different cost and demand conditions have always been key concerns, as is evidenced by the so-called Habakkuk debate (David, 1975) and the cause celebre of alleged late-Victorian British failure (McCloskey and Sandberg, 1971). This provides a framework for understanding the 'race' between education and technology in the United States in the twentieth century and its implications for wage differentials (Goldin and Katz, 2008) and for understanding why technological change which is developed in the rich west offers relatively little to poor countries with lower capital to labour ratios (Allen, 2012). The bottom line is that economic historians have much to contribute to discussions of directed technical change and its implications for the distribution of income both within and between countries.

It is generally accepted that capitalism has been successful in generating new technologies. It is much less widely understood to whom the gains from technological progress accrue. The clear message from economic history is that typically in the medium term the users get nearly all the benefits and that this works through price falls brought about by innovation. This can be seen for major inventions like railways (Mitchell et al., 2011), cars (Raff and Tratjenberg, 1997), electric motors (Edquist, 2010) and, of course, computers (Nordhaus, 2007). An analysis of the American experience in the second half of the twentieth century found that on average 98 per cent of the total gains went to the users (Nordhaus, 2004). A key implication is that benefits from technological progress are transmitted to other countries; for example, falling prices of information and communications technologies (ICT) have major effects on growth in non-producer countries through accumulation of ICT capital (Oulton, 2010). The focus of supply-side policies should be on facilitating efficient diffusion of new technologies rather than promoting domestic R & D.

Given that the benefits of technological progress largely accrue to the users, it is not surprising that economic historians have devoted much effort to thinking about implications for living standards.

Here, a long-run view promotes an emphasis on the importance of new goods and impacts on health and leisure and the need to think in terms of utility-based real national income. As De Long (2000) pointed out so effectively with his depiction of the limitations of J. P. Morgan's choice set, conventional calculations of real GDP do not take proper account of the consumer surplus gains from the vast increase in the range of goods since then. Equally, major gains from better technology have come through longer life and shorter work years which using standard valuation techniques have added at least 50 per cent to the growth in living standards indicated by real wages for the United Kingdom over the twentieth century (Crafts, 2007). Awareness of these benefits from technological progress should be central to the thinking of policymakers concerned with 'well-being'.

The claim that institutions and, in particular, property rights, matter is, of course, characteristic of economic historians' work on economic growth, most famously identified with North (1990), and is now stressed by many economists, especially since imaginative use of instruments has persuaded them that the relationship is causal (Acemoglu and Johnson, 2005). The economic history literature is replete with detailed examples which are a major resource for development economics. For example, McMillan et al. (1989) demonstrate that the incentive effects of the shift from communal decision making to individual responsibility in Chinese agriculture after 1978 had a massive effect on effort and productivity, Haber (1998) shows that institutional reforms in the Brazilian capital market has major effects on investment and TFP by allowing easier monitoring of managers and facilitating the supply of external finance with very positive effects on the performance of the cotton textile industry, and David and Wright (1997) note that, although flawed in some respects, the property rights regime was a major reason why the United States was so successful in finding and exploiting its mineral resources in the nineteenth century.

Nevertheless, there are aspects of work in economic history that have not yet been taken on board by economists. First, the powerful argument in the tradition of Gerschenkron (1962) that at an early stage of development the institutions (and policies) which are appropriate may differ with a greater reliance on hierarchy and less on the market and a formal legal system leading to different optimal boundaries for the firm and a premium on mitigating capital market failures. This leads to greater understanding of the developmental state and sympathy for 'unorthodox' institutions as, for example, in China (Crafts, 2003).[2]

Second, the new institutional economic history stresses both the persistence of institutions and also the absence of any automatic tendency for good to replace bad; much weight is placed on path dependence (North, 2005). Moreover, informal as well as formal institutions matter but they are not readily amenable to top-down reform. Economic historians would certainly endorse the point made by Acemoglu and Robinson (2006) that de jure does not necessarily equate to de facto change, as in the post-bellum South. This implies that both that quite far distant history may cast a long shadow over the present (Nunn, 2009) and that institutional quality is a subtler concept and is harder to measure than has been believed by the growth regressions industry; historical understanding is an advantage.

[2] Proactive developmental states may have motives of which many would disapprove, for example, military ambitions, as Gerschenkron himself was well aware. For example, the military objectives of Soviet planning have been made very clear by recent scholarship (Kontorovich and Wein, 2009).

In a very influential paper, Abramovitz (1986) emphasized that catch-up growth by follower countries was by no means automatic but depended on 'social capability', i.e., having incentive structures based on institutions and policies that were conducive to the necessary investment and innovation effectively to assimilate imported technology both in terms of speed of its diffusion and realization of its productivity potential. As modern growth economics has now recognized (Aghion and Howitt, 2006), the institutions and policy choices that can deliver social capability in a far-from-frontier economy differ in many ways from what is appropriate for a close-to-frontier economy. Similarly, as new technologies come along, institutions and policies may need to be reformed. The constraints of the historical legacy are important in this context. It will be desirable to reform and for institutions and policies to evolve as countries progress through a process of catch-up. Understanding the complexities of this evolution is an area where historical case studies have a major advantage over applied econometrics.

If continual reform is needed to achieve full catch-up, it is very possible that countries find catch-up easy to start but difficult to complete. Japan, where catch-up of the United States stalled 20 years ago, epitomizes this problem (Chen, 2008). Idiosyncratic features of the Japanese economy such as lifetime employment, the main bank system, business groups, industrial policy, and the absence of competition in non-tradables were no longer advantageous in the 1990s but were hard to reform (Ito, 1996). This suggests that predictions of catch-up and convergence of China with the United States - which is taken to be a done deal by the general public - might be viewed sceptically by economic historians. Similarly, the projections of future catch-up by the so-called BRICs economies, popularized by Goldman Sachs (Wilson and Purushothaman, 2003), have a mechanistic flavour which abstracts from the political economy of development. Exploring the sustainability of catch-up growth processes in the light of historical experience is an area where economic historians have important insights to offer.

Furthermore, mainstream economists may be over-optimistic about the prospects for catch-up and convergence in poor countries. Historical experience suggests that the neoclassical prediction of future convergence of incomes appears to be too optimistic even though it can be argued that, now that it is understood which institutions and policies are conducive to growth, in a globalized world rapid catch-up growth financed by capital inflows should be much easier to achieve (Lucas, 2000). It is noticeable that recent catch-up growth in the transition economies of eastern and central Europe has not matched Western Europe in the Golden Age, that growth performance has reflected the extent of institutional reform and liberalization (Fidrmuc, 2003), and that this has been highly correlated with years under communism and rents from natural resource exports (Beck and Laeven, 2006). Once again history (and an understanding of history) matters.

## 4. Some areas where economic history could achieve greater impact

The previous section has demonstrated that economic history has distinctive contributions to make in the area of long-run economic performance which can add to economists' analyses of economic growth and to development policymaking. To capitalize on this potential and to promote the value of economic history to a wider audience, I suggested in the introduction that it would be desirable to put more effort into writing about topics that are relevant to economists and policymakers. How might we go about this and what might we have to offer? What follows is simply a starting point, focusing entirely on growth, and obviously is not anything other than suggestive.[3]

The most obvious way to make more impact with these communities is to write more papers that explicitly address a question in which they are interested and organize the presentation of research findings accordingly. In terms of the type of contribution which would be valuable, an obvious further possibility is to produce in-depth country case studies of growth over the long-run in the form of analytic narratives informed by growth economics but complementing theoretical ideas with understanding of institutional change and the political economy of the evolution of policy. As Rodrik (2003) observed in his introduction to a book of essays that made a start in this direction, there are surprisingly few such papers in the literature – and it should be noted fewer still with the benefit of serious economic history inputs.

Where might we offer more? The list could be long but how about the following as points of departure. First, economists have become persuaded that differences in TFP are responsible for a large part of the variance in labour productivity across the world (Hall and Jones, 1999) without really understanding why. Economic historians have a great deal potentially to say about this issue as was recognized, for example, by Maddison (1987) who sought to explain Solow's residual rather than treat as 'the measure of our ignorance'. Maddison's methods were crude but his agenda was spot on. Second, the applied econometrics literature has concluded that on average aid has not increased growth in developing countries (Doucaouliagos and Paldam, 2011) yet economic historians studying episodes like the Marshall Plan believe the opposite. There is a big opportunity for economic historians to provide more detailed and nuanced studies of the impact of aid on growth in recipient countries with a view to clarifying in what circumstances positive outcomes might be expected. Third, economists continue their post mortem on the Washington Consensus. Did the disappointing outcomes of reforms come from fundamental conceptual flaws, or failures in implementation, or because the program was incomplete (Birdsall et al., 2010)? Given that reforms with a similar philosophy have been successful in other contexts such as Franco's Spain (Prados de la Escosura et al., 2011) and that trade liberalization that reduced capital goods prices in the late twentieth century raised growth (Estevadeordal and Taylor, 2008), there is something of a puzzle to be solved and the 'local knowledge' of economic historians can be a key ingredient in answering these questions.

35 years ago McCloskey concluded his survey as follows: "Does the past have useful economics? Of course it does" (1976, p. 455). As we have seen that is still true but, all the same, it is vital to keep demonstrating this to the audiences that matter and communicating it in their language.

[3] Economic historians who work in other areas, for example, business history, financial history, labour history could easily generate similar lists.

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