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# What is the economy in economic history?

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ABSTRACT

*Any study of economic processes rests on a view about the nature of the economy as its (usually implicit) point of departure. Prior to Adam Smith, the Mercantilist, Physiocratic and Cameralist traditions viewed the economy as being a household writ large – so the metaphor of household choices and management was used to discuss economic policy in particular. Smith dispensed with this, offering the first view of the economy as a self-organizing system, coherent without explicit coordination. Subsequent economic theorizing has refined this concept, in the process dramatically simplifying the view of the economy. The result is an influential neo-classical concept of the economy which largely avoids mention of government, institutions, and business organization. This paper argues that economic historians can overcome this conceptual 'thin-ness' by introducing ideas from institutional studies, business history, the history of work and the history of technology into our understanding of the economic process as a whole.*

## What is the economy in economic history?

Defining 'the economy', and hence the scope of economic history, is no easy matter. After all, economics itself often has no clear conceptualisation of the economy, and sometimes seems to veer between quite incompatible views of what an economy actually is. Before Adam Smith, the mercantilist, physiocratic and cameralist traditions all viewed the economy as being like a household, and frequently used household metaphors to describe the economy (the word is derived from the Greek *oikonomos*, a household). That is, the economy was seen as a kind of agent, led by a ruler (similar to a man as the patriarchal head of a household) who could make decisions about what the economy could and should do. These decisions by the ruler about the economy/household could determine wealth or failure, and as a consequence much of the mercantilist and cameralist writings are actually not about economics at all; they are a legislative discourse, about the appropriate principles of decision-making by government insofar as this affects commercial outcomes.[1] This type of view still exists today, especially among some neo-Schumpeterian economists, or advocates of the developmental state, who think that an economy can be a kind of decision-maker that can make strategic deci-

1 For a discussion of the issues here, see Tribe, Keith, *Strategies of Economic Order*. Cambridge: Cambridge University Press 1990.

sions. Bruno Hildebrand, writing in the nineteenth century, called such thinkers "the oeconomists of the absolutist state", since the policy issues are posed in the context of a powerful directive government.

Against this background, the great contribution of Adam Smith lay not so much in his analysis, as in the new concept of the economy that he developed – the economy as a self-organising system. Of course, Smith did not use the evolutionary concepts of self-organization as an emergent property of the system, but rather the concepts of liberty and equilibrium emerging from the Enlightenment. But the idea of a self-organizing system is nevertheless there. In Smith's work everything is decentralised, there is no leadership or co-ordinating agent, the economy is a system rather than a decision-making household or agent, and yet it all hangs together in a co-ordinated way. It is this vision that has inspired modern neo-classical theory, which is in large part a theory about the extent to which a decentralised economy can remain coherent.[2] The problem however, still largely unresolved, is what kind of system is it? What should we include in the system, and what should we leave out? My argument in this paper is that within the mainstream of economic theory too much has been left out – especially institutions and cultural forms. These institutional and cultural dimensions have certainly not been ignored in economics, but they have been seen either in neo-classical terms (in the early work of Douglass North, for example), or they have been relegated to heterodox sub-disciplines such as evolutionary or institutionalist economics that are rather vigorous at the present time, but are certainly not part of the economics curriculum.[3] Economic history, by contrast, often has much to say on institutional or related issues, but this is rarely integrated into the theoretical underpinnings. The argument here is that the underpinnings remain, on the whole, neo-classical. One of the contributions that economic history can make is to reintroduce institutions, business organizations, culture and complexity to our understanding of the economic system, in a more theoretical rather than simply empirical way.

## The economic history of growth

My aim here is rather limited – it is simply to consider some of the implications of conceptualising the economy (directly or indirectly) in a neo-classical mode, and some of the alternatives. I will limit the discussion further, and approach the question from the point of view of economic growth. The reason for this is that economic growth is arguably the key issue in modern economic history, and the analysis of the overall growth process has been a central – arguably the dominant – concern of much of economic history writing. The general issue has also of course been approached by historians with wider interests – the various contributors to so-called 'world history' are also concerned with the issues of why Europe grew from the eighteenth century and Asia did not. But European and US growth has dominated specifically economic history. At the core of modern economic growth was the growth of industrialised economies, with the development, application

2 For a sustained statement of this view of neo-classical theory, see Hahn, Frank, *Equilibrium and Macroeconomics*. Oxford: Basil Blackwell 1984.

3 For an overview see Hodgson, G.M., *Evolution and Institutions. On Evolutionary Economics and the Evolution of Economics*. Cheltenham: 1999.

and spread of new technologies and new forms of economic organisation. That process began in Britain, and continued in Europe, North America and later in some of the Pacific economies. The rates of change in products and processes, and hence the achievement of productivity growth rates that have profound impacts on real income growth, have fluctuated over the past two centuries, but they have never stopped, and they have revolutionised life in the 'western world'. What emerged around 1750 was indeed 'a new economy' – a world of slow growth was replaced by a world in which real incomes doubled every fifty years or so.

How should the growth process be analysed and understood? Any serious interpretation requires a conceptual and analytical framework. What then, has the tradition been in economic history? And how has 'the economy' been perceived in this?

The tradition of analysis in Europe has tended to be rather loosely formulated and descriptive, with a lot of emphasis on empirical work on sectors or technologies in the industrialization process. In the US, Alexander Gerschenkron had a lasting impact on economic history with his emphasis on institutions and the substitution of new types of institutions in successful developing economies.[4] This view had powerful impacts in development theory, and within economic history itself. But it was never followed through into a consistent theoretical framework that might inform economic history. Rather, from the 1960s, economic historians first in the USA and subsequently in Europe, made more explicit use of theory and modelling, and this introduced a much more neo-classical practice into economic history. They usually resorted to what used to be called the 'grand neoclassical synthesis', that is to some form of integration of neo-classical price-theory with versions of Keynesian macroeconomics, combined with elements drawn from neo-classical approaches to growth.

This broad conceptual approach has had two important and possibly detrimental effects on economic history. First, it has had a conceptual and methodological bias, in that it has prioritised particular research problems and sought to establish techniques of analysis and a view of what counts as an acceptable explanation. A good example of this is the recent debates on the causes of unemployment in the 1930s, which have frequently replicated theoretical debates in economics: a Keynesian deficient-demand hypothesis versus a neo-classical real-wage explanation. Articles on this topic often represent straightforward applications of textbook theory (from the aggregate supply-aggregate demand models) to historical data. It has also shaped arguments concerning the structural change and rates of economic growth on early industrialization (via use of production-function approaches that will be discussed further below).

Turning to techniques, the so-called 'new economic history' of the 1970s was on the one hand an introduction into economic history of statistical and econometric techniques, which were and are the standard tools used by applied economists engaged in empirical testing.[5] However, the introduction of these techniques

4 Gerschenkron, A., *Economic Backwardness in Historical Perspective: A Book of Essays*, Cambridge, Mass.: Harvard University Press 1962.

5 For an overview of early quantitative economic history, see Temin, Peter (ed.), *The New Economic History: Selected Readings*. Harmondsworth: Penguin 1973. For a critique of such work, by a historian also committed to the use of quantitative methods, see Crafts, N.F.R., The new economic history and the Industrial Revolution, *The First Industrial Revolutions*, Mathias, P. and Davis, J.A. (eds), Oxford: Basil Blackwell 1989, 25–43. Examples of modern European work in quantitative

into economic history was more than a technical matter. American 'cliometricians' argued that statistical and econometric techniques were necessary not just to test hypotheses, but also to ensure their rigorous formulation. McCloskey, one of the most influential of modern economic historians, observed that 'Not counting but economic theory, especially the theory of price, is the defining skill of cliometricians, as of other economists. A Cliometrician is an economist applying economic theory (usually simple) to historical facts (not always quantitative) in the interest of history (not economics)'.[6] However, what McCloskey meant by economics was very much neo-classical economics, and in general the hypotheses of quantitative economic history were – at least until quite recently – usually those prompted by a neo-classical approach.

The link with neo-classical economics that began with cliometrics has had an impact on economic history which is wider than the deployment of some concepts and techniques. In a subtle way it has helped to define what the limits and boundaries of the discipline actually are. In so doing it has helped to define what the economy – in economic history – is and is not. Neo-classical theory emphasizes the role of economic actors as rational calculators, as profit and utility maximisers, and stresses the role of competition in bringing about equilibrium positions in economic systems. It is about how the calculating pursuit of private interests leads to various types of economic equilibria. In its strong forms it is a theory of how calculating behaviour by rational agents leads to a general equilibrium, which is also a welfare optimum. In their most rigorous analytic form these models contain only self-interested trading actors, with no government, no taxes, and no institutions. At the welfare maximising equilibrium, wages and returns to capital are equal to marginal products, everyone is on their supply curves, and there is no involuntary unemployment. These views about the nature of the economic process have often had a strong influence on the leading journals of the discipline, and on research strategies of important economic historians.

This approach has often thrown up interesting but contestable hypotheses and suggestions. A good example concerns the understanding of productivity growth in the world economy since 1800. In order to model an economy in which equilibrium is achieved via optimizing behaviour, theoreticians need to make a number of simplifying assumptions, for example, that there is perfect competition (meaning that every agent is a price taker), that there is perfect information, that there are no economies of scale, that labour and capital are perfect substitutes, that there are no externalities in the system. On the basis of these assumptions, economists can define a production function, which makes it possible to measure aggregate inputs and outputs, and to estimate the ways in which the productivity of inputs changes over time. The most common form is the so-called Cobb-Douglas production function, which formed the basis of Robert Solow's famous work on long-run growth in the US economy.[7]

economic history can be found in Crafts, N.F.R., Dimsdale, N. and Engerman, S., *Essays in Quantitative Economic History.* Oxford: Oxford University Press 1991; and Foreman-Peck, J. (ed.), *New Perspectives on the Late Victorian Economy. Essays in quantitative economic history, 1860–1914.* Cambridge: Cambridge University Press 1991.

6 McCloskey, D.N., The Achievements of the Cliometric School, *Enterprise and Trade in Victorian Britain, Essays in Historical Economics,* London: Allen and Unwin 1981, 5.

7 Solow, R.M., Technical Change and the Aggregate Production Function, *Review of Economics and Statistics,* vol. 39, 1957, 312–20.

The Cobb-Douglas production function, and occasionally its functional equivalents, has been used by economic historians to make international comparisons of productivity growth several hundred years back, as well as to examine key propositions about growth at different periods within individual countries. For example, an important part of Angus Maddison's comparative work on long-run growth rates is based on calculations and comparisons of total factor productivity growth for different countries across a period of more than a century and a half.[8] This general approach has been widely used – not just by economic historians, but also in the growth accounting literature – to analyse relative performance among countries.

Nevertheless, this programme of research often neglects to offer any serious discussion of the historical plausibility of the background assumptions about the economy, which underlie the approach as a whole. Some key assumptions are first, that labour and capital markets are perfectly competitive (and that labour and capital are paid strictly according to their marginal productivity); second, that the economy exhibits constant returns to scale (meaning that a doubling of inputs leads to a doubling of outputs and that there are no increasing returns to scale); and third, that there is perfect information and no externalities, no taxes and usually no government. These are just the most visible of the underlying assumptions; there are others, and they add up to the wider assumption that the economy is a fully flexible, de-institutionalised set of interacting rational agents, each of whom is fully informed. To make the long-run productivity comparisons, it is assumed not only that the economy can be thought of in these terms, but also that it is in a permanent competitive equilibrium. These assumptions are rarely defended directly, possibly because it is difficult to see how they can form the basis of a descriptively relevant theory of the economy. This tends to be recognized in many areas of mainstream economics, where there are developments that fundamentally challenge the core neo-classical assumptions, such as the analysis of agents with asymmetric information. Defence of this approach tends to rest on the idea pioneered by Milton Friedman, that the essence of science is prediction, and that successful predictions can be generated by acting 'as if' these assumptions were realistic. Certainly successful predictions can be found, but this leaves us with the wider question of whether occasional predictive success really implies underlying explanatory power in the theory.

Be that as it may, many economic historians have simply applied production functions, in more or less rigorous and careful ways, to historical data. Their results supply most of the historical perspective on international productivity growth, and long-run growth studies using this kind of approach have been carried out for many of the presently advanced economies. Yet such neo-classical economic history embodies a number of worrying features. For example, how valid are the productivity calculations when the underlying assumptions no longer hold, and how sensitive are they to these assumptions? It cannot be claimed that these underlying assumptions have themselves been subjected to empirical tests; in general, economic history tends to suggest that limited information, serious uncertainty, and non-substitutability of labour and capital (except in the long run) are per-

8 See for example, Maddison, Angus, *Phases of Capitalist Development*. Oxford: Oxford University Press 1982; and *Dynamic Forces in Capitalist Development. A Long-Run Comparative View*. Oxford: Oxford University Press 1991.

sistent features of economic life. It is worrying that historians have not used empirical evidence to explore whether such underlying assumptions are adequate as a starting point for an account of the historical development of an economy, even though they know, for example, that the drive to reap scale economies was one of the most important features of the investment process in the late nineteenth century and early twentieth century, and appreciate that factor markets have been segmented and imperfect for long periods of time. Neo-classical economists do, however, recognise and insist that understanding of macro-economic phenomena – movements in overall output, productivity, employment, and so on – must be based on rigorous micro-economic foundations, meaning a theory, which accounts for the behaviour of individual actors in the economy. The problem is not so much to question this approach, but rather to ask, what should these micro-economic foundations look like? The textbook approach in economics simply assumes that economic actors have access to large (or even unlimited) amounts of information, and that they are rational maximisers of profits or utility in the context of a given range of technologies, products and prices. The great difficulty with cliometrics is that it simply imports this approach into economic history. I do not wish to suggest that economic history is overwhelmingly dominated by cliometrics, but rather that its underlying concepts are both more influential and more questionable than is commonly supposed.

## An Institutional and Cultural Economic History?

The central problem with the neo-classically-oriented approach sketched above is that it tends to ignore the institutional and cultural frameworks of economic behaviour that often fascinate historians. An alternative and possibly superior approach to micro-economic foundations might be historical: to look at how agents have actually been shaped, at how firms have evolved, at how they operate in a reality characterised by uncertainty, the presence of institutions, and considerable diversity among agents. In other words, economic historians could give more emphasis to the broader institutional development in all its variety as it unfolded over time. Such an approach is clearly needed because the qualitative historical evidence often seems to contradict more or less sharply the premises of the basic technique, which has been used to map the growth process, for country after country and for the world economy as a whole. This means, of course, that a serious economic history must involve a broader concept of what the economy is than is found in much economic history founded on neo-classical economic theory.

## The innovating economy

Economic and technological historians are increasingly turning to the cultural and institutional contexts for invention and innovation.[9] Organizational innovation and the continuous emergence and use of new technologies are clearly central to

9 Some examples can be found in Berg, Maxine and Bruland, Kristine (eds), *Technological Revolutions in Europe. Historical Perspectives.* Cheltenham: Edward Elgar 1998.

the processes of growth and change which characterize the industrial and post-industrial eras. But how should we conceptualise and understand innovation itself? In this area, much has changed in recent years. The technological determinism of early histories of industrialization, the old certainties over the benefits of technological change, and the traditional enquiries into the economic theory behind invention, technological diffusion and retardation have all been called into question. Most now acknowledge the need for a political economy of technological change, in the sense of an account, which extends the parameters of analysis into institutional and social contexts.[10] There is increasing recognition also of the need for greater emphasis on the varieties of experience across countries and regions at different points in time. It is important to develop and extend such approaches, for although economists have tried to endogenize technological change, that is to make its explanation a part of the theory of economic growth rather than an outside unexplained factor, their accounts of innovation are sketchy at best. They are certainly not conceptually rich enough to understand either the springs of invention or the complexity of the processes of innovation and diffusion.

## Institutions

If different cultures of technology and the learning economy come to the forefront in the analysis of invention and innovation, then politics, institutions and the law are also important factors behind the dynamic of technological change. How should this be approached? One way, commonly used by economists and economic historians, is via the introduction of institutions into the framework of economic and historical analysis, in recent years deriving in large part from the work and approach of Douglass North.

The issues here have been focused by the award in 1993 of the Nobel Prize in Economics to North and Robert Fogel. In a sense, the careers of Fogel and North exemplify some of the issues I have been discussing. Both, in very different ways, produced major work, which consisted of attempts to use explicitly neo-classical economic frameworks for the analysis of historical material. Fogel's work on slavery explored the operation of the slave system in the USA in terms of rational economic calculation in a profit-seeking framework, and his work on railroads explored the economic impacts of railroads in terms of the costs of available alternatives (a very neo-classical approach, in its way, and also a very illuminating one).[11] North's work is focused on institutions, but his theory of why institutions emerge is based on neo-classical theory. His first book (with Lance Davis), *Institutional Change and American Economic Growth*, was in his own words, 'consistent with, and built upon, the basic assumptions of neo-classical theory'. As with neo-classical theory generally, the approach was rather static, and Davis and North clearly stated at the end of the work that their 'model is not dynamic, and we

10 See Mokyr, Joel, Technological inertia in economic history, *Journal of Economic History*, vol. 52, 1992; and *The Gifts of Athena. Historical Origins of the Knowledge Economy*. Princeton: Princeton University Press 2002.

11 Fogel, R.W., *Without Consent or Contract: The Rise and Fall of American Slavery*. New York: Norton 1989; and *Railroads and American Economic Growth: Essays in Econometric History*. Baltimore, Johns Hopkins University Press 1964.

know very little about the path of one comparative static equilibrium to another'.[12] In his later works, such as *Structure and Change in Economic History*,[13] North has been more critical of neo-classical theory as a descriptive starting point, but he has nonetheless seen economic history in terms of widening the scope and bringing greater complexity to the neo-classical approach. As he has argued,

> 'Neo-classical theory has made economics the pre-eminent social science by providing a disciplined, logical analytical framework. To abandon neo-classical theory is to abandon economics as a science. The challenge is to widen its horizons to come to grips with these issues [of change]. The economic historian is uniquely qualified to meet that challenge.'[14]

In effect, North sees the task of economic historians as being to fill out the neo-classical model with evidence, and furthermore to extend that framework to the analysis of political behaviour, the history of science, and the history of population. It should not be surprising that these views are currently a subject of debate among economic historians, since the award of a Noble Prize is a kind of imprimatur on an approach or a body of work.

North argues that institutions should be seen in terms of cultural norms, written rules and unwritten codes of conduct, which provide the framework within which organizations such as firms function. There are lower transaction costs when institutions are efficient, generated by savings on acquiring information or by the reduction of losses due to imperfect monitoring. Thus the historical development of the culture and institutions will effectively determine the outcome of any economic activity in a community. North accepts that economic growth is 'path-dependent'; that is, once a development path is set on a particular course, the network externalities, the learning process of organizations, and the historically derived perception of issues reinforce this course.

The question for economic historians of growth is whether the institutional aspects of growth are best modelled via a neo-classical framework that sees institutions as optimal cost-reducing adaptations. This is a particularly important for understanding growth, since institutions and cultural frameworks not only shaped growth but also set the terms for the diffusion of technologies across regional and national boundaries. In a rather macro way, institutions are central to the types of "world history" exemplified by the work of Kenneth Pomeranz.[15] But they matter in more specific ways too. Institutions and cultures, as revealed for example in the different patent systems of Britain, France and the United States, either prompted or slowed down the speed of these information flows.[16] The development of

12 Davis, L.E. and North, D.C., *Institutional Change and American Economic Growth*. Cambridge: Cambridge University Press 1971.

13 North, D.C., *Structure and Change in Economic History*. New York: Norton 1981.

14 North, D.C., Structure and Performance: The Task of Economic History, *Journal of Economic Literature*, vol. 16, 1978: 3, 963.

15 Pomeranz, K., *The Great Divergence: China, Europe and the Making of the Modern World Economy*. Princeton: Princeton University Press 2000.

16 See for example. Kahn, Zorina and Sokoloff, K.L., Patent Institutions, Industrial Organization and Early Technological Change: Britain and the United States, 1790–1850, *Technological Revolutions in Europe. Historical Perspectives*, Berg, M. and Bruland, K. (eds), Cheltenham: Edward Elgar 1998, 292–315.

new technologies on the Continent during early industrialization is partly a story of precocious and aborted developments, and partly one of international transfers of technology, mainly from Britain.[17] In both cases the contexts for successes and failures can be found in the cultures of technology, in the differences in learning processes, and in political and institutional constraints. The economy of Europe during the eighteenth century functioned, as Sidney Pollard has argued, as an economy of regions – technological experimentation was widespread across export processing and finishing trades, the textile industries and ironworks and mining ventures, not just in North Europe, but in southern Europe as well.[18] The technological high noon of Holland was based in its finishing trades of the seventeenth and eighteenth centuries. The Dutch took charge of and applied up to-date techniques in the bleaching of German, French and Flemish linen, and prepared Italian silk. Holland was a front-runner in the development of textile machinery, copper stills, presses, saws, shipbuilding innovations, and in scientific and consumer novelties such as microscopes, fire fighting engines, street lighting and new methods of processing food. We really do not possess a good comparative institutional framework for looking at these different developments across countries.

## Problems for the development of economic history

What kinds of problems and approaches are appropriate if we seek to go beyond the neo-classical frameworks criticised above? It seems to me that there are two big issues arising from the discussion above. The first is whether economic history should see itself as being based on a deductive theory, which begins with certain a priori axioms about human behaviour. The alternative is some form of inductive approach, which does not reject theory, but sees it in terms of a dynamic interaction between theory and evidence. A related issue is whether we should try to explain the economic growth of the past two centuries purely in terms of economic processes. Let me turn to this point in slightly more detail.

Moses Abramowitz, one of the greatest of growth economists, has argued that one of the problems of growth economics is that it tries to explain growth in terms of various 'sources', which are seen in isolation from each other, and as separable in their effects. Thus the effects of changing educational qualifications in the workforce, changing technologies, changing investment patterns, and so on, are estimated separately. Some key factors, such as changing political context, are left out of the calculations entirely. His argument is that it would be more appropriate to explore growth in terms of interdependence and joint action of a range of phenomena.[19]

I agree with Abramowitz's position, but I think we should go further, and raise questions about the real scope of economic history, and about what kinds of his-

17 See for example Bruland, K., Skills, Learning and the International Diffusion of Technology: a Perspective on Scandinavian Industrialization, *Technological Revolutions in Europe. Historical Perspectives*, Berg, M. and Bruland, K. (eds), Cheltenham: Edward Elgar 1998, 161–188.

18 Pollard, Sidney, *Peaceful Conquest: The Industrialization of Europe, 1760–1970*. Oxford: Oxford University Press 1981.

19 Abramowitz, Moses, The Search for the Sources of Growth: Areas of Ignorance, Old and New, *Journal of Economic History*, vol. 53, 1993:2, 217–43.

tory are relevant to the understanding of growth and change. My view is that, largely as a result of its preoccupation with economic theory, economic history has unnecessarily narrowed the range of fields, which are seen as relevant to the growth process. As an example of this, let me simply point out that there are a number of very active research fields, which are closely relevant to the long-run dynamic of the economy, but whose practitioners rarely publish – or even try to publish – in economic history journals. These fields include:

*1. The history of work and labour*

Perhaps the key issue within this field concerns both the nature and significance of the restructuring and reorganization of work processes, which accompanied early industrialization. One of the central problems in more orthodox accounts of early industrial growth concerns the basic sources of growth in output after 1750. In his famous *Take-off*, W.W. Rostow followed rather orthodox economic theory by suggesting that a key element in the growth process was a sharp rise in the investment rate, causing the capital-labour ratio to rise and therefore productivity to grow.[20] Subsequent empirical research showed that this had not in fact happened. There was no significant rise in the investment rate in Britain until well into the nineteenth century, and there is therefore a serious problem in explaining why output had risen so much earlier. There are various possible explanations, but one of the most persuasive, in my view, is the substantial change in the organization of work, which occurred in many economic activities from the late eighteenth century. The changes essentially took the form of changes, which altered the degree of control over the intensity and duration of work; increasing intensity and duration had important productivity effects even in the absence of investment or technical change.[21] The issues here were discussed rather fully by Marx, and reintroduced into modern history by the publication of Harry Braverman's *Labor and Monopoly Capital*.[22] Braverman's powerful book has given a new impetus to studies of labour processes, and there is now a substantial body of work on the links between changing work organization, technological change, investment dynamics, and so on. Raphael Samuel showed in great detail some years ago that very large parts of the growing Victorian economy in Britain were characterised by handicraft production and hand tool technologies, and that mechanization and steam power simply could not account for the growth of these industries.[23] Very little of this work has been seen as part of economic history proper, even though – as I have suggested – it throws strong light on some major unresolved issues in economic history. The issues here also link with another historical field relevant to economic history, namely women's history. A central issue in the rise of the modern economy has been a changing gender division of labour, which has been continuing from at least the early 19th century to the present day; indeed the role of

20 Rostow, Walt W., *The Economics of Take-off into Sustained Growth*. London: Macmillan Press 1963.

21 See for example Bruland, K., The transformation of work, *The First Industrial Revolutions*, Mathias, P. and Davis, J.P. (eds), Oxford: Basil Blackwell 1989, 154–170.

22 Braverman, Harry, *Labor and Monopoly Capital*, New York: Monthly Review Press 1974.

23 Samuel, R., Workshop of the World: Steam power and hand technology in mid-Victorian Britain, *History Workshop*, vol. 3, 1977, 6–72.

women in the labour force has now become a central issue in understanding present patterns of employment and unemployment as a whole. Apart from Claudia Goldin's extraordinary series of econometric studies of the demand for and supply of women's labour in the USA, this also has been a field conspicuous by its absence in the main journals of economic history.[24]

### *2. Business history, in particular the history of company organization*

The development of the advanced economies has been characterised by major changes in the size, the management, the ownership structure, and the internal organization of firms. This is in fact an area where historians have made substantial contributions to the development of theory, in particular to the understanding of the development of vertical integration. Within economics, the theory of the firm is well known to be one of the weakest areas, and in fact most of the really interesting work on the evolution of company structures is to be found within historical studies. The great contribution here has been from Alfred Chandler, whose research over the past thirty years has opened up many new directions.[25] But there are many other business historians who have focused on specific company histories. In my view the most interesting of these have explored the links between changing company organizations and major processes of technological change: David Hounshell and John Smith's *Science and Corporate Strategy*, for example, has analysed the role of company research and development in the business strategy of the DuPont corporation.[26] If we take the view that long-run economic growth is dependent on the development of large-scale production and advanced technologies, then we should not ignore the basic organizational unit of the capitalist economy, which is not the industry, but the industrial company. William Lazonick has forcefully pointed out that neo-classical theories of how resources are allocated are fundamentally misleading, since they tend to assume that resources are allocated by some kind of 'market mechanism'.[27] However, in the industrial economy, markets do not allocate resources: it is decisions by company management that allocate resources to investment, training, and technology creation. This implies that business organization as a field of research ought to be integral to the study of economic history, not rather peripheral, as it has been over the past decades.

### *3. The history of technology*

The histories of science and technology have been among the most dynamic research areas in historical studies in recent years. In a number of cases work in such fields has thrown new light on economic growth. Von Tunzelmann's work

24 See http://www.economics.harvard.edu/~goldin/papers.html

25 Chandler, Alfred D., *Strategy and Structure: Chapters in the History of American Industrial Enterprise.* Cambridge, MA: M.I.T. Press 1962; *The Visible Hand: the American Revolution in American Business.* Cambridge, MA: Harvard Belknap 1977; *Scale and Scope: the Dynamics of Industrial Capitalism.* Cambridge, MA: Harvard Belknap 1990.

26 Hounshell, David A. and Smith, John K., *Science and Corporate Strategy: DuPont R&D, 1902–1980.* Cambridge: Cambridge University Press 1988.

27 Lazonick, W., *Business Organization and the Myth of the Market Economy.* Cambridge: Cambridge University Press 1991.

on the steam engine, for example, has provided important quantitative insights into the impact of steam engine technology, which was at one time argued to be a key contributor to economic growth. He showed that the impacts were considerably smaller and considerably later than most economic historians have assumed.[28] Generally, work in the history of technology has basically fallen into two categories. On the one hand there have been studies concerned with the process of technological change as a whole, usually looking across a whole technology or range of technologies, and exploring the role of new technologies in the growth of industries or economies, such as Svante Lindqvist's *Technology on Trial*, which analysed the early use of steam power technology in Sweden; some of my own work falls into this field.[29] On the other hand, there are studies of the development of specific technologies, such as Thomas Hughes's *Networks of Power*, which is a history of the managerial and economic aspects of the technology of electrical power production and distribution systems.[30]

This general field is not only large and active, it is clearly addressing a set of problems, which ought to be of central concern for economic history. One of the surprising features of modern economic history is that, in seeking to explain the growth record of the past two centuries, it has obviously had to take account of the comprehensive processes of technological advance that also characterise this period. Unfortunately, there were and are a number of economic historians who simply ascribed the whole process of growth to an exogenous process of technological invention and innovation. Relatively few economic historians have tried to explain the trajectory of technological change as such; like economists, they have often treated it as 'manna from heaven', rather than actually explaining its genesis and consequences. It is very noticeable that the exciting growth of the history of technology as a field has simply not been visible within the journals of economic history – it has entirely occurred within specialist journals such as *Technology and Culture*, and as far as economic history is concerned, the world of technological change remains a black box. Similar points can be made about the history of science. Again, this field divides into two parts; one part dealing with what might be called 'internalist' accounts of the development and explanatory problems of particular sciences or problems, and another dealing with what its practitioners tend to call the 'political economy of science'.[31] The latter deals with issues related not only to the intellectual development of the science, but also to issues of funding, political control and economic use. Once again, this type of material does not appear in the economic history literature, although the links between the scientific and industrial revolutions are very much an issue in economic history.

28 von Tunzelmann, Nick, *Steam Power and British Industrialization to 1860*. Oxford: Clarendon Press 1978.

29 Lindqvist, Svante, *Technology on Trial. The Introduction of Steam Power Technology to Sweden, 1715–1736*, Uppsala Studies in the History of Science, 1. Stockholm: Almqvist & Wicksell International 1984; Bruland, K., *British Technology and European Industrialization. The Norwegian textile industry in the mid nineteenth century*. Cambridge: Cambridge University Press 1989, 2003.

30 Hughes, Thomas P., *Networks of Power. Electrification in Western Society, 1880–1930*. Baltimore: Johns Hopkins University Press 1983.

31 See for example Friedman, Robert M., *Appropriating the Weather. Vilhelm Bjerknes and the Construction of a Modern Meteorology*. Ithaca: Cornell University Press 1988.

## Conclusion

There are really two arguments being advanced here. The first is that a significant number of economic historians has tended to accept an essentially neo-classical conception of the economic process that – because of its underlying assumptions about the nature of the economy – limits our ability to understand the central economic phenomenon of Europe and the US, which is economic growth. This does not of course affect all economic history – it is a phenomenon associated primarily with quantitative studies. The second argument is that we have substantial bodies of work from quite different theoretical backgrounds that can widen the focus – business history, the history of technology, the history of institutions etc. These non-neo-classical approaches rest on superior conceptualizations of the economic process, and it is important to extend them into the quantitative analyses of growth. However, these other approaches should be used not simply to add complexity to an underlying neo-classical framework, but to change the framework as a whole. There is no reason why historical work cannot be used to develop new concepts of the economy itself, perhaps in some of the directions suggested by evolutionary or institutional economics, but without being dependent on these theories. A historically-relevant and historically-informed view of the economic process, as well as being worthwhile in its own right by enriching economic history, would of course also be an important contribution to the renewal of economics.