

Economic Method and Economic History: An Historian's View

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The objectives of this paper are modest. It is not intended as a contribution, original or otherwise, to the contemporary debate on methodology. Rather, it is hoped merely that the personal observations of an historian, who has for two decades taught economic history within an Economics Department, may be of some interest to participants in the debate – perhaps reassuring them (if this is necessary) that their concerns do reach a wider audience.

The paper is presented as a personal history-the evolution, if you like, of my own approach to my adopted discipline in the context of the intellectual currents to which it (and myself) have been subject.

I

Reared in the History Department of a British redbrick university in the early 1960s, my exposure to theory or method in undergraduate studies was approximately nil. If we undergraduates read Collingwood, Butterfield or Carr it was of our own volition (I remember a visiting lecturer asserting that students should be discouraged from reading journal articles – this may seem at a tangent to my theme but serves to make a general point on the British mode of approach).

H.A.L. Fisher's dictum that there are no patterns to history was not consciously taught but seemed to hold sway. Attempts to argue otherwise, as in Toynbee's (1934-61) *A Study of History*, were denigrated. The evaluation of the validity of themes in the literature we were required to read seemed to us to hinge largely on the elegance of exposition (American historians were not greatly admired. Further, in the absence of an alternative, we tended to fall back on an inchoated Marxism to provide an intellectual structure. We might have no conception of Marxist economics but did like Marx's historical schema. Our teachers could sneer at the crudity of this stages, and at the contortions of this followers who tried to find a transition from feudalism to capitalism which accorded with historical reality, but, for me at least, along with class conflict, it did provide some sort of basic methodological device with which to approach history.)

The vagueness and subjectivity of approach in historical study was infuriating to the young historian – the purpose of our study was hardly discussed or even mentioned. Moving into economic history during the late 1960s, first as a graduate student, then as tutor and lecturer, brought a breath of fresh air, especially as my own transfer coincided roughly with the advent of a methodological transformation in that discipline (which, perhaps misleadingly, then seemed, in Britain at least, as untheoretical in approach as conventional history).

This methodological transformation, stemming, naturally enough, from the United States, had two broad aspects. The first was the explicit use of economic theory, culminating in the use of econometrics for historical investigation (this was, somewhat grandiosely, entitled cliometrics by its practitioners). The second was the extension of the use of economic theory, particularly in the form of property rights theory, outside of its traditional domain. These innovations were commonly based on the use of a "scientific" approach to history – essentially a Popperian form of scientific method.

'Bliss it was to be alive in that dawn.' A lack of training in the basic tools of econometrics left me a neutral in the debates on cliometrics, but the explicit use of theory and the "intellectual imperialism" of economics could be welcomed unreservedly. I shared the (implicit or explicit) view of protagonists that order could be imposed on history, that the latter could, through the new methodology, be made to yield up its secrets. At a more mundane level, an economic approach provided the means of organising research (and later teaching) material (it did no harm that this accorded with an existing tendency to a vulgar form of economic determinism.)

In this respect, intellectual imperialism was more valuable than cliometrics. Fishlow and Fogel on American railroads left me cold; it was the message of Steven Cheung's *Theory of Share Tenancy* (1969-recommended by a Chicago postgraduate) which caught the attention. The

notion of institutional arrangements as devices designed to minimise risk and uncertainty – rather than as merely functions of class exploitation – offered a whole new perspective on the English land system, my own area of research at the time.

There is no doubt that the structure of the nineteenth century English land system was shaped by the dominance of the landed classes. The puzzle was/is that the system was also economically efficient and allowed for substantial welfare gains on the part of the two subordinate classes in the course of the century. Property right theory, with its stress on the comparability of institutions in allocative terms, where property rights were clearly specified, helped to resolve what, viewed either in Marxist or untheoretical mode, was otherwise a paradox.

More generally, the two themes came together in the major debate of the time: the efficiency or profitability of slavery. On the one hand, protagonists of the new economic history drew on the intellectual imperialism of human capital and property rights theory. On the other they combined their explicit theory with the use of mathematical and statistical techniques (aided by the computer and massive financial resources to generate the necessary data) to produce what were, in the light of traditional historiography, iconoclastic results.

Such irreverence had enormous appeal. And more seriously, what could be better from the serious historian's point of view. The subject was one of abiding interest and relevance and, despite its emotive nature, the researchers were concerned with what history should be about – not passing moral judgements on what was good and bad in the past, but on the why of what existed. The fascination of history is the exploration of cause and effect. The debates on slavery seemingly epitomised the new economic history at its best – in throwing up new approaches, firmly grounded in research and scientific theory, to the explanation of well-known phenomena.

The debate on slavery, and with it the public fame of new economic history, reached its apogee with the publication of Fogel Engerman's *Time on the Cross* (1974) – then to decline in a welter of hubris (a personal view; Fogel (1983) still regards the enterprise as a success.) My own response ran parallel to the general trend – with admiration gradually faltering under a weight of doubt. What seemed

initially an elegant argument for high profitability arising from the restriction inherent in slavery on the ability to choose between work and leisure did not stand up sufficiently to critical review.

The initial furore over *Time on the Cross* was largely technical in nature (a credit to the commitment of cliometricians to the academic search for truth?). However, the episode also brought into focus some of the costs associated with the nature of the new economic history in general – including fundamental doubts as to the validity of its methodology. (Bear in mind that this is a personalised account; doubts had been expressed before but it was now that I began to take them seriously.) Perhaps Henry Woo's survey of methodological errors commonly committed by economists can serve as a useful categorising device here.

Henry adverts to the 'unhelpfulness' in economics of such methodological "sins" as 'native eclecticism', 'substantivism', and 'importationism' (I'm still not sure whether his category of 'integrationism' is in fact a sin in his eyes.) As to the first, I would have to concede *mea culpa* – as I think would virtually all economic historians, new and traditional. In extenuation, I would plead that 'native eclecticism' is not a heinous sin. In fact it is fun – and can be illuminating (a nice example is the work of Douglass North, prepared as he is to use bits of economic theory to 'explain' everything from the beginnings of agriculture to the nature of ideology. Part of the attraction is that his 'explanations', however wrong, make excellent straw men for tutorial discussion). In fact, economists and economic historians can perhaps assuage their guilt by reading McCloskey's (1983 and 1986) good-humored work on 'The Rhetoric of Economics'.

As for 'substantivism', in so far as I understand Henry, I don't think the new economic historians could be accused of this – the reverse in fact. The accusation would not be that their substantive enquiries dictate their choice of method but that – and perhaps this can be related to 'importationism' – the reverse is true. It was (and is) the claim of the new economic historians to be scientific in their mode of enquiry which was initially exciting, then profoundly disturbing.

The claim is importationist in so far as the associated methodology of falsificationism was borrowed from the philosophy of science – but its origin is of course less of a worry than its

implications. Two concerns can be addressed very briefly here. The first relates less to the claim (see for example Fogel, 1983) that 'lawlike statements' could be generated by the new economic history than that their method assumed that economic models and theories, often of great complexity or in mathematical and form, could be applied, ad hominem, to any historical question. Further, that the method was capable of supplying definitive answers, of resolving (presumably for ever) historical debates.

After an initial bedazzlement at the audacity of such claims, (speaking personally) doubts grew. This was in part due to personal reflection (I have never been able fully to appreciate the usefulness of falsificationism in economics, where a poor result can be dealt with simply by invoking *ceteris paribus*, by theoretical modification or the development of ancillary hypotheses; see also Klant, 1990.) It was also due to contending intellectual influences – and the fact that, as with *Time on the Cross*, new economic historians did not achieve their stated objective of producing definitive answers.

The reactions against claims that economics can be scientific has of course increased immeasurably since the 1970s (I wonder if the Hong Kong Institute of Economic Science should change its name?). It is (hopefully) significant that a notable contribution has come from a leading new economic historian in Donald McCloskey, confessing past signs (not all of them though!) and stressing the damage done to civilised discourse in the discipline by recourse to a borrowed and inapposite methodology. This raises the second cause of concern over the new economic history.

The language in which the claims of the new methodology were made were discourteous in the extreme. What now seems the arrogance of its objectives was matched by an arrogance in assertion which went hand-in-hand with a thinly or unveiled contempt for past scholarship. (I forget the source but remember one utterly dismissive comment on Sir John Clapham, one of the greatest British economic historians – because of the depth and rigor of his scholarship – of the twentieth century.) Such contempt is seldom stridently explicit today but is still implicitly demonstrated in modern works which ignore completely past scholarship. And this is the rub. Such discourtesy is wrong for more serious reasons merely being bad manners. A contempt for what has been written before

argues a contempt for history itself, an unwillingness to join in what is a continuous and continuing endeavor to find "truth".

Even today, too much of what a leading Australian economic historian, G.D. Snooks (1990b) describes as 'economicists' history' rather than economic history is produced. History may appear a useful laboratory for the testing of theory but merely 'illustrating the operation of simple models' makes little or no contribution to the better understanding of complex reality – the task of economists, economic historians and historians alike.

I find the common complaint of econometricians in my own Department significant in this context. End-users of their work, they report, want simple straightforward results, preferably in the form of a number or a set of numbers. But such results, they emphasise, are meaningless in themselves, making sense only within the context of a qualitative assessment which itself requires a thorough knowledge of all aspects of a subject. In this simple truth, it seems to me, lies the key to the continuing usefulness of the new economic history.

In fact, in recent years, the position of the new economic history has altered, largely in subtle rather than dramatic fashion, but still unmistakably. Its protagonists are now inclined to be integrationist in strategy, less imperialist in ambition. Something of the old aggression remains, notably in the works which receive most attention (the short run gains from aggressive assertion overcome consideration of the longer run losses consequent on bad history?). Overall, though, practitioners now show a greater awareness of the limits to the usefulness of their method, a greater desire to work with traditionalists. In addition to McCloskey, that other major figure, Robert Fogel, has recently been reported as saying that he now sees the 'gathering of new evidence' as a more significant gain from the new economic history than the more explicit use of theory (quoted in Snooks, 1990c). A further pointer can be found in the sudden popularity of the concept of 'path dependency' (Snooks, 1990c), something which new economic historians seem to find highly significant but which, for traditional historians, is a truism.

The shifts in approach from McCloskey and Fogel are significant because of the undoubtedly major contribution they have made to economic history. Methodological progress is also more

generally apparent in the contribution that new economic historians are making to what is, in my opinion at least, the most intriguing question posed in the discipline today. This lies in the investigation of the roots and dynamics of long run economic growth.

II

A conscious preoccupation with economic growth since the Second World War has helped to secure the pre-eminence of economics among the social sciences during the same time period. Not unnaturally, it has given direction to research and teaching in economic history as well. Important gains in understanding in both disciplines have been made in this era – one result of which is the present focus on very long run economic growth.

Two points can be made to illustrate the last assertion. The first lies in the recognition that economic growth is not a mechanistic process, explicable in purely economic terms. The abandonment of Harrod-Domar style models in economics has been matched by the growth of a widespread consciousness of the inadequacies of static neo-classical theory in economic history. As Douglass North noted in 1972, capital accumulation, with other economic factors, are not causes of growth – ‘they *are* growth’ (North and Thomas, 1972).

Second, and closely interdependent with the last proposition is the realisation that economic growth is not and never has been the direct result of deliberate economic policy. The collapse of the Eastern Bloc ‘command systems’ is the most spectacular symbol of this proposition – of the failure of the use of state power in finding short cuts to economic development. The failure of Keynesian prescriptions, on which the hegemony of economists as a source of public advice was built, could be seen as some sort of equivalent in the West (although an attendant paradoxical feature has been the degree to which economists have maintained and even increased their importance).

The trend has been matched in economic history by a new awareness of the evolutionary nature of economic development. The notion of abrupt transformations, of economic revolutions as such, is now largely discredited. The two best examples lie in the economic history of Japan, where the substantial historical foundations of the modern achievement are now better appreciated (Jansen & Rozman, 1986), and the British Industrial Revolution – where the term Revolution is now seen as an anomaly,

retained essentially for period identification than because of any intrinsic value (Cannadine, 1984; Crafts, 1985).

Such progress in historiography, with a focus on evolution combined with an appreciation of the importance of non-economic variables to the growth process, has naturally led to an extension of the time horizons of economic historians. As noted above, Douglass North was one of the first of the new economic historians to translate a dissatisfaction with existing methodology into a search for the springs of long run growth. He has been followed by a range of economic historians, new and traditional, pursuing a medley of themes and using a mixture of methodological approaches.

A small sample will give some idea of the trend. North, himself, in *Structure and Change in Economic History* (1981), begins with the origins of agriculture and of the state and moves easily up to nineteenth century British and American history. Others, notably Elvin (1973) and Jones (1981 and 1988), have explored the same theme through a comparison (in Elvin’s case, implicit) of the two leading regions (in terms of economic performance) of the past two millenia. Still others, including Rosenberg (1986) and Mokyr (1990), have made innovation, as qualitatively the most important component in the process of growth, their focus. On a parochial note, Australian economic historians have shown a new interest in and made a substantial contribution to Australian prehistory – very long run growth, taking in some 50,000 years (Blainey, 1984; Dingle, 1988; Butlin, 1989).

The scope of such studies extends far beyond the traditionally accepted bounds of economic history although the theoretical apparatus is definitely economic in nature. Beyond these two generalisations, diversity reigns. North is the most eclectic (and the most ambitious) in his method; Jones the least explicitly theoretical. But the diversity of approach highlights Henry Woo’s key point on why method matters: because ‘any particular choice of method affects the definition, selection and omission of facts.’ The study of economic growth over the long run, of all possible historical enquiries, repels eclecticism and demands methodological consistency.

This is of course easy to say; it is more difficult to prescribe what the approach should be. Nevertheless, and despite the diversity of existing method, the works adverted to contain

sufficient of a consensus to act as a pointer to what could emerge as a viable method.

All would agree that there is an economic dynamic at the core of process of long run growth. Further, all the writers mentioned relate this, implicitly at least, to the gains made from exchange: they accept that the movement from simplicity to complexity derives from a growth in the division of labor based on increasing specialisation and (although it also transcends) comparative advantage. Interdependent with this trend is the use of more and better capital (technological progress plus capital accumulation) and institutional innovation in the search for transaction cost savings. The problem for all, in the face of variability in the record of growth over time and space, lies in identifying the causes of such variability.

As noted above, there is also a reasonable degree of consensus on the evolutionary nature of the process – the obvious analogies with the biological theoretical prototype have been explored to great effect by Mokyr (1990). The consensus extends to the non-unilineal nature of growth – and indeed the diversity of growth paths (with none, not even the Western European, being seen as being in any sense optimal).

Finally, there is also general agreement as to the importance of non-economic variables to explanation of the differences between growth paths. However the consensus breaks down here – to an extent at least. Most perceive the key variables involved to lie in what North calls institutional structures. More importantly, there is considerable variety in approaches to these, with a range of opinion on how far standard economic theory can be usefully extended outside of its usual domain.

Arguing as a historian and as a teacher who attempts to use the studies of long run growth (rather than being directly involved), it seems to me that what is needed is less an all-embracing theory as such than a methodological approach which highlights the interdependence of economic and non-economic trends. The approach which conforms most closely to this, to my knowledge, is the Austrian. Its attractions can be set out briefly here.*

1. The Austrian use a subjective rather than an objective approach to human action. This acknowledges human diversity and avoids the reductionism associated with orthodox assumptions of 'economic man.'
2. Austrian theory is *a priori* in nature but was

developed, rather like evolutionary theory in biology, in a conscious search for a better understanding of the real world. It acknowledges the cumulative nature of history, is not deterministic but stresses rather that growth is the unintended consequence of a myriad of actions.

3. The Austrian approach to growth is predicated on the interdependence of three thematic trends: the development of exchange relations, of an integrated capital structure and of a system of rules. These evolve out of the changing needs of people interacting – in so far as they are not stunted or constrained by dictates from political authority. This last is perhaps the crucial constraint on systems (notably China) which have evolved towards a certain degree of complexity over the past millenium.

The beauty of this approach is that it provides a common method towards the analysis of any growth path – without compromising the unique historical identity of the latter. The Austrian approach is evolutionary in the best sense of the word: it is neither determinist nor teleological. Economic logic provides a unifying framework or structure but proper historical investigation is still a prime necessity. Under such an approach the best features from the disciplines can be combined to maximum effect – not just in the study of very long run growth but in any area of economic history.

III

It will be evident from the above that my views on the gains possible from the new economic history have been modified substantially in the past few years. In fact I have come to have a new respect for my original history teachers (whatever their technical defects), whose implicit message was to pay the utmost respect to the evidence and to maintain an open mind on its explanation. In so far as the new economic historians violated these precepts then they produced bad history.

As can be reemphasised, this is not to deny the usefulness or importance of theory or the benefits to be gained by borrowing from other disciplines. For the historian, however (and I have a sneaking feeling that this point also applies to economics), theory should never be accorded too predominant a role. It should be used to illuminate the complex processes of historical reality; the latter should not, indeed it

cannot be made to accommodate the limitations of theory.

Historiography (the writing of history), is as much an evolutionary process as is history itself. It has its blind alleys, dead ends and separate paths but, ultimately, it also has a progressive side. There are no final explanations to the important historical questions but, with each historian, or generations of historians, building on the work of previous scholars, advances are made. The new economic history had a stimulating effect on the wider discipline but, if it does not acknowledge the continuing relevance of traditional method, it will prove a blind alley. The indications are, at least from the leading exponents like Fogel, that this acknowledgement is increasingly forthcoming.

It may be presumptuous but I think that there are lessons in my story for economists interested in method. The evolutionary analogy can also be applied to the history of economic thought. There has been progress in economics – even if its nature is often obscured by the clouds of controversy which has attended intellectual debate. In recent times, falsificationism has proved a blind alley while there seem to be growing reservations concerning usefulness of econometrics. However the ultimate test lies in the illumination of reality – and here, surely, economics, at its best, still enjoys a rightful primacy in the social sciences.

Note

- * My knowledge of method, in either history or economics, is rudimentary at most. What I have learnt of the Austrian comes less from an extensive reading of Hayek and Mises than from communication and debate with my colleague, Sudha Shenoy. My apologies to her for any mistakes or misunderstandings in what is set out here on their methodology.

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