

Introduction

by Donald N. McCloskey

A historical economist applies economic methods (usually simple) to historical facts (not always quantitative). Before 1958 little of this was done. In that year two assistant professors at Harvard, Alfred Conrad and John Meyer, published "The Economics of Slavery in the Antebellum South" and two assistant professors at Purdue, Jonathan Hughes and Stanley Reiter, published "The First 1,945 British Steamships." Historical economics since then has widened into a river. The present bibliography begins with the thin, bright stream before the assistant professors and ends with 1980, as the river reached full flow. By now most serious departments of economics have one or two historical economists, members of a group now numbered in the hundreds, with its own Society, an unusually productive annual conference, and cascading generations of youth and grayheads. The movement begun in the United States is spreading to other countries (many foreign scholars also had contributed to the stream before 1958). Important pieces of history have been decisively reinterpreted. And even economics, after a long run of present-mindedness, has begun to think of olden times.

The bibliography contains about 4,300 items. To use a favorite word of Alexander Gerschenkron's, the number flummoxed me. At the project's beginning, in a footnote to a 1978 application to the National Science Foundation (NSF) for its annual conference on historical economics, I reckoned that a complete bibliography would contain at most 500 items. As someone who earns his living making estimates of upper bounds, I am embarrassed by this one. For most purposes a factor of eight is not a very good degree of approximation.

The 4,300 are closely defined – not out of disdain for the other sorts of quantitative history and for the mildly past-conscious economics that the definition excludes, but merely to make the book usefully coherent, as a starting point for further research and as a reminder for reading lists. The bibliography includes almost no book reviews in book-review sections. It includes a few papers in progress (in progress in 1980, that is; most if not all have been published since then), which seems reasonable in the age of the copying machine and prepublication publication. Four thousand published books and articles over a couple of decades exhibit historical economics as a large supplement to economics and to history.

I have avoided the word "cliometrics." Despite its loony charm (it was coined

by that same assistant professor, the mathematical economist Stanley Reiter), and its appearance in recent dictionaries (misdefined as "quantitative history" *tout court*), the word "cliometrics" has given the field more trouble than pleasure. If the late Simon Kuznets's work is not covered perfectly in this bibliography, for example, it is partly because he stoutly denied being a "cliometrician," and would not give us his *curriculum vitae* as a starting point for our search in his voluminous output. A few Europeans stayed away from the recent, successful Second International Congress of the Cliometric Society, held in Santander, Spain, under the mistaken impression that "cliometrics" means fancy work beyond their ken. On these grounds, too, Douglass North, one of the founders of the field, edges away from the word. Even my own pamphlet for the Economic History Society, which shows how simple is the economics in historical economics and how workaday are its statistical tools, carries forward the confusion, by its title: *Econometric History*. Historical economics, to repeat, is merely economics about history. National income analysis of past economies, loaded with economic thinking at every step, is well within the fence; so is political economy about the past.

The committee of scholars mentioned on the title page assisted us with languages other than English. George Hersh, Jr., was the administrative director of the project while it was in Chicago. Kevin O'Meara devised the computer program well before such programs became common; Professors Nejat Anbarci of the University of Buffalo and Metin Cosgel of the University of Connecticut saw the manuscript into print. I am to blame for the delays, omissions, misconceptions, misspellings, and errors in classification. I am planning to spend a few centuries in purgatory on these counts.

The Economics Program of the National Science Foundation has played a special role in the field and in the bibliography. Much of the work catalogued here was in some way encouraged by the Foundation — in its series of unique conferences since 1961 highlighting new work (called, alas, the "Cliometrics Conferences") or in its research grants to historical economists. Historical economics would not have flourished without the NSF, and economics would have been the less.

It would have been the less in what it lacks the most, although recently it has shown signs of wider reform. Economics needs what historical economics has gotten from its association with history: seriousness about facts, an interest in the long run, and habits of scholarship that make for a cumulative science. Economics will not be as cumulative as its sister discipline evolutionary biology, say, or its cousin from the Scottish Enlightenment geology, until it takes over some of the practices of history. Historical economics, scientifically speaking, is one of the most advanced parts of economics. If other economists learn to read what has gone before, yet keep their foxy love of theory, they will build a science that makes progress.

At a congressional hearing in 1987, Senator William Proxmire, as was his

practice, was torturing the man from the National Science Foundation about silly projects, perhaps worthy of the discovered Golden Fleece. Someone on Proxmire's staff had caught sight of what was supposed to be such a project in the Economics Program, on the industrial revolution in England. (I forget who the alleged culprit was; I was not the one.) Why, asked the senator, would Americans want to know about such a dusty old subject as the British industrial revolution?

Proxmire had a reputation for being open to argument, so I wrote to him, arguing against the line he took. I tried to say why one would care about dusty old economies, even to the point of compiling bibliographies of what has been written about them:

Nothing could be more practical for thinking about our present plight [I wrote him in defense of the study of the British industrial revolution], since Britain's is so similar to ours — seventy years on. The leading industrial power loses its market share and then its nerve and finally its principles: that's a story from today's headlines in America, and from yesterday's in Britain. The only way to get the wisdom our people and our leaders need to face relative decline is through historical example. You and I don't want America to repeat the sixty years of misery that Britain has just passed through. . . .

Whatever you think of the policy issue, I suppose you want economics to be good, because you think it useful. Well, good economics must be historical. The data of economics is necessarily historical, because there's no such thing as future data. And for most questions it doesn't matter how old the data is: economic behavior is economic behavior, whenever it occurs. If you think you can explain industrial growth, you had better be able to explain old industrial growth, too. Explaining the data in the few months before April 1987 is not much of a scientific test.

Old data is often a lot better than new. For one thing, it's often a better experiment. Did you know that a good example of a country with a high ratio of government debt to national income is not the USA now but Britain long ago? How long ago? In 1815. If we want to know how debt affects economic growth, then a good, clean, simple experiment is Britain in the age of the industrial revolution.

And old data is often better because the people with an interest in hiding it are dead. Only for a handful of recent cases can you get the intimate files of price fixers to see what devices they use and how to combat them. For the nineteenth century you can get them for any of the numerous companies whose records have survived.

Dozens of other examples come to mind. Here's an instance close to your own heart. I know you are interested in uncovering waste in government. But we can learn more about waste in the First World War, and still better in the Civil War, than we can about waste in the Pentagon now, because there's no one around to protect the guilty. We don't even need whistleblowers: just step over to the archives of the War Department or the major arms manufacturers, and bring along an economic historian. The way waste develops is not some-

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thing new. I would think that you in particular would want to find out why waste occurs, in order to do something about it, for good. To do it you need political, economic, and social historians, such as Robert Higgs, in his new book on the growth of public spending in America since 1900. If you don't look into waste during America's progressive age or Britain's Napoleonic Wars you're missing a scientific trick. It would be like confining astronomy to the nearer stars.

Anyway, the issue of Britain's industrial revolution is intrinsically important, because it was the granddaddy of . . . all [industrial revolutions]. The biggest question in economics, of course, is what explains modern economic growth, the growth which (we learn from historians) has increased our standard of living over the past two centuries by a factor of ten [or twenty]. It started in Britain. I think you'll agree that if we knew what explains modern economic growth we would have made a discovery more important than an awful lot of drugs that cure or machines that think. In fact, we'd have the money to pay for the drugs and the machines. If we could persuade senators to take the discovery seriously, we could eliminate poverty, worldwide.

A pipe dream? It's no more of a pipe dream than the dream of stable democracy that the people of the 1780s wrote and thought about; or of stable monetary and trade systems that the people of the 1880s wrote and thought about. These programs of research in social science worked (for 200 and 30 years, respectively). Nowadays it's the sober judgment of experts in economics and history that by studying economic growth hard we might be able to find a cure for poverty. We've already discovered many useful things about what does *not* explain modern economic growth, which is irritating but the way research on any tough problem tends to go. You probably agree with me that the case doesn't differ obviously from cancer research or fusion power or elementary particle physics. But on these tens of thousands of times more money has been spent than on explaining modern economic growth.

Sure, it's a longshot. We may discover in the end that modern economic growth is just too hard to understand. But we've made a lot of progress, with sums that would not pay for the spare parts for the latest no-go military tank. . . . It's not in anyone's short-term, practical, applied interest to show that security of contract (say) or elementary education caused economic growth and can cause it again. . . .

So I think you ought to rethink your opposition to research on Britain's industrial revolution. I'd be interested in what you conclude.

Proxmire was a senator of quality. He replied, and agreed, although I do not believe he relented in his pursuit of the National Science Foundation. This bibliography shows that economists are willing to work furiously on the largest scientific problem of the age, the nature and causes of the wealth of nations, and to build a truly historical economics.