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Recommendations for Reading (and Re-Reading): Guinnane on Undergraduate Courses in European Economic History

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Teaching courses in European economic history always presents a challenge. There are a number of textbooks available, some quite good, but none is likely to cover all the topics and perspective the instructor has in mind. In the U.S., at least, there is always the "level" problem: a reading that will challenge and inform students at one kind of institution might bore students at another. I have never found the magic solution, and every year I shift around my readings. This is partly because the topics change -- it is easier to interest U.S. students in international trade today than it was 10 years ago -- but also because new, good works are published, and through the magic of search engines I learn about papers I had never known, but should have.

The following readings won't make up a course. But they might help, and my use of them might stimulate instructors to think of other readings. People who are just putting together a course and who will not be overwhelmed might consult the astonishing website of John Munro, who has years of experience at the University of Toronto: **http://www.economics.utoronto.ca/munro5/**

1. E.A. Wrigley, "Urban Growth and Agricultural Change: England and the Continent in the Early Modern Period," *Journal of Interdisciplinary History* (1985).

This paper is a classic in the research literature. When it was written we knew less about the details of farming and output in the relevant countries than we do now, so some of what Wrigley concludes is now disputed. I use the paper because it establishes some very important general facts at the level undergraduate students need to know, but also because it clearly and elegantly demonstrates an important element of the economic historian's craft. Often we do not have the information we want. An important skill for the economic historians is to be able to think of what information might be available, and how to relate it to the question at hand. Here Wrigley uses urbanization as a proxy for agricultural productivity. A more important skill is to be able to identify the weaknesses in the approach, and figure out ways to limit their impact or at least place reasonable bounds on their impact. Using urbanization as a proxy for agricultural productivity at first blush assumes away international trade in food, rural industry, and rural/urban differences in the demand for food. Wrigley is well aware that there was international trade, that some people who lived in rural areas were not producing food, and that family size and composition might differ between town and country. Rather than ignore these issues (which is one bad, and common instinct) or to throw up his hands and say that we cannot study the issue because we lack perfect sources (another bad, common instinct, and frequently invoked to deny the possibility of quantitative economic history), Wrigley carefully works through ways to assess the plausible impact of each problem.

I ask my students to do a few problem sets in my undergraduate courses; otherwise, I find, they say “marginal this” or “convergence that” but don’t really understand how the models are used to understand the history. Wrigley’s paper forms the basis for a wide-ranging problem set. I make up some facts similar to Wrigley’s and then pose a debate between two economic historians, both of whom think they have some crucial factoid relevant to the argument. The students have to figure out how to use the information, if at all. The reports I get back are that students genuinely enjoy this problem set. Unlike others!

2. Rollin S. Atwood, “Localization of the Cotton Industry,” *Economic Geography* (1928).

This is a well-written, useful paper in its own right. But it first appeared on my reading list because students kept asking about things they were learning in other courses. Many undergraduate economics courses today will discuss the “new economic geography” and the phenomenon of Silicon Valleys etc. We all know that the British cotton textile industry was strongly localized in Lancashire, and there have been several very fine studies of the process that led to this localization. Atwood’s paper has the considerable virtue of being short and to the point, and thus working well in an undergraduate course. He stresses a simple narrative of the industry’s growth and concentration in Lancashire, and his causal discussions focus on physical geographic matters, such as the location of streams for water power. The paper works well as the springboard for a lecture that draws on the more recent “new economic geography.”

For a readable discussion of why firms might want to locate in the same area as most of their competitors there is nothing better than Alfred Marshall’s *Principles of Political Economy*. (The eighth edition is in print.) I cribbed my lectures on this point largely from this work.

In the past I have sometimes paired the Atwood paper with John Brown’s "Market Organization, Protection, and Vertical Integration: German Cotton Textiles before 1914," *Journal of Economic History* (1992). Brown argues that the different industrial organization of cotton textiles in Britain and in Germany can be explained by location. British firms could count on thick local markets for their inputs and outputs, and thus did not need to integrate vertically. German producers, who were spread out all over Germany, had to worry about both input and output markets, and as a result were more likely to integrate vertically. Brown’s paper nicely leads to two further points. First, he is turning the usual Anglo-German comparison on its head; in his story the Germans vertically integrated not because they could (that is, had better capital markets, managers, etc. than the British) but because _they had to_. Second, his paper raises the issues of the “other” Coase paper (on the boundaries of the firm).[1] Students today unfortunately do not often know that paper, although we talk about its ideas all the time (what is “outsourcing” other than adjusting the boundary between the firm and the market?).[2]

3. N.F.R. Crafts, “Industrial Revolution in England and France: Some Thoughts on the Question ‘Why was England First’?” *Economic History Review* (1977).

Many of us are drawn to economic history by big questions, such as why Britain’s industrial revolution was first. But these questions do not figure prominently in our scholarly discussions. There are good reasons for that, reasons that are amply demonstrated in the books that do try to address such questions. As a keen student of the Three Little Pigs,[3] my own instincts are not to try to build a house unless I have bricks. For most interesting questions we are only starting to assemble the bricks.

But students find that approach frustrating at some level, with good reason. We introduce them to something new and exciting, and then tell them that certain topics are only admissible in sophomore bull sessions. Crafts' paper works extremely well, I find, because he is both addressing the big question and also warning us against there being a good answer. The paper falls analytically into two parts. The first, which is almost epistemological, notes that the style of argument we like to use in economic history comes apart in the face of an event that can only occur once, by definition. We could not run a regression predicting which country would industrialize first; any variable that identified Britain would be perfectly classified with the dependent variable. Based on what happened we cannot really claim what was most likely to happen.

The second part of the paper takes up the idea of the error term in those hypothetical regressions. Crafts appeals to an interpretation where the error term reflects shocks that cannot be predicted ex ante, such as the series of terrible harvests France experienced in the 1760s. He then notes that France, and not Britain, seemed to be closer to industrialization in the early eighteenth century, and that one can plausibly argue that had it not been for those shocks, the first industrial revolution would have taken place in France. This provocative conclusion suggests a whole range of issues that would need to be re-thought. If we take Crafts' argument seriously, and I do, then we would today be trying to understand why the French system of government was so much better for economic growth than the English.

The paper opens up a large range of questions, but does so in a way that makes it easy to encourage students to think about them in a structured way. Thus they see why the big questions are often appealing just because they are so slippery. Is anything either "all deterministic" or "all chance"? (Rostow's reply to Crafts demolished the strawman that all historical events are just accidents. Crafts never said that. Getting students to understand this distinction is not difficult.) It is easy to construct examples of models where the R-square is .9, and the mean values of the shocks imply France's industrialization first. This angle forces students to think about how we interpret likelihood in the social sciences; sometimes we insist on the actual outcome as being the most likely, while path-dependency arguments often stress the critical role of a single realization of random variable.

How do we reconcile this kind of argument with the extremely long tradition of identifying X in France as bad for economic growth, and Y in England as good for economic growth? Is this tradition -- as Crafts suggests -- just based on the inability to appreciate the underlying explanatory problem? (I always challenge the students to prove that industrialization was not caused by having a monarchy taken over by a German aristocratic family who would have an heir named Charles in 2000. The wonderful thing about this example is that _somebody, somewhere_ has probably argued just that.)

I sometimes pair the Crafts paper with Patrick O'Brien, "Path Dependency, or Why Britain Became an Industrialized and Urbanized Economy Long before France," *Economic History Review* (1996). O'Brien's paper is not only very good, it poses a completely different kind of argument. To Crafts, there is enough chance in economic history that sometimes the outcome could not be predicted in advance. O'Brien on the other hand makes a very strong path-dependence argument, one that starts with the Black Death.

4. Barry Supple, "Fear of Failing: Economic History and the Decline of Britain," *Economic History Review* (1994).

When I was a graduate student, lectures on European economic history always included a section called "British decline" or "relative British decline" or such. The underlying issue, of course, is the more rapid growth of several economies (such as Germany and the U.S.) in the late nineteenth century. How economic historians view this topic depends a lot on when they wrote and the perspective they wrote from. To some, this is nothing more than the convergence implied in at least some version of the neoclassical growth model attributed to Solow. (Others, of course, have tried to use the elaborate "endogenous growth" models to get a better understanding of why countries such as Germany overtook Britain, rather than converged on Britain.) Others see in this issue a fertile ground for discussion of how institutions can be a source of rigidity or even path-dependence.

For an American of my generation the issue has a curious resonance at several different levels. I remember the year Honda Civics first appeared in the U.S.. Many in the industrial Midwest thought this was the end of western civilization, or at least of U.S. dominance. Small, cheap, high-quality cars -- and Japanese! Throughout the 1970s and into the 1980s, the Zeitgeist as well as scholarly writing assumed the U.S. would go the way of Britain in the nineteenth century (well, back to that in a minute). Then along came the 1990s, and scholars un-learned most of what they knew about the ability of one economy to enjoy permanent world leadership. I suspect we are about to re-learn it, at some cost.

Supple's paper is not so much an economic history of decline as a discussion of the phenomenon of _worrying_ about decline. His paper turns on the paradox those Japanese cars should have brought to mind in the U.S.: their existence in the U.S. might signal problems for the U.S. auto industry (as of course they did) but the presence of these cars was one more indicator of the unprecedented affluence of American society. Supple notes that Edwardian Britain was in a funk, but a funk that any earlier generation would have envied, as would most of those in the countries to which Britain had "lost out."

The challenge of teaching students this material, at least in a society that for good or ill is not much interested in yesterday, is to get them to think about the underlying economics (is it _conditional_ convergence? does this example support one or another endogenous growth model?) while at the same time appreciating the historical irony and the historical weakness of the "horse-race" approach to economic history. Britain is now wealthier in per-capita terms than Germany. Japan was the economic basket case of the 1990s. Supple's paper is a very thoughtful and thought-provoking discussion that works well alongside lectures on growth accounting and discussion of more specific features of the alleged decline.

5. Kevin O'Rourke and Jeffrey G. Williamson, *Globalization and History: The Evolution of a Nineteenth-century Atlantic Economy*, MIT Press (1999).

Kevin O'Rourke and Jeff Williamson did the profession a great service when they took some of their earlier scholarly work and re-packaged it into a form that can be used for a wide variety of courses. The product is hard to classify because it could appeal to so many different audiences. I have recommended it to professional trade economists, to graduate students in history, and to my brother. This book would not be out of place on a graduate reading list. Students in selective undergraduate programs should not have any trouble with it, either, so long as the instructor talks them through parts of it, and it is so clear that a gifted teacher might be able to use it for less selective institutions.

The central theme in this work is the rise of the "Atlantic Economy" in the nineteenth century: its causes, its implications for the European economies, and the political reaction to it. One might quarrel with the approach or conclusions of any of the chapters -- I have, although the more I use the book the more I understand why O'Rourke and Williamson took the approaches they did. But I know of no other work that so neatly and clearly introduces a set of central concepts, organizes some empirical material around it, and then gives the student a sense of what the historical record says. I find this especially useful because I would like to discuss more trade issues in my courses, but many of our undergraduate students never take a course in international trade and don't learn basic models, such as Heckscher-Ohlin (H-O), in other courses. O'Rourke and Williamson have a real gift for sort of telling the reader what the H-O model is without demanding that they push around curves, and then using that basic understanding to get a grip on, for example, why trade increased Irish wages. "Sort of" here is an accolade: they, of course, are masters of this framework, and use it to good effect in their scholarship. In the book they take a different approach, which is to draw on the central insights (relative factor prices converge) without demanding that the reader understand the workings of the model in the abstract.

Some of the chapters in the book also deal with political economy, that is, how policy was formed or changed in reaction to a particular flow of goods or people. These chapters also work very well in an undergraduate course, partly because the issue can be framed very cleanly (who stood to benefit from the Corn Laws?) and because the issues are at once remote and topical. Students in my courses could read about the political reaction to migration to the US in the late nineteenth and early twentieth centuries, and then watch Congressmen on TV braying about the same issue today.

6. J.M. Keynes, *The Economic Consequences of the Peace* (1920) and Etienne Mantoux, *The Carthaginian Peace: or, The Economic Consequences of Mr. Keynes* (1946).

Many students come to my courses from survey courses in European history or from courses in International Relations. There they have "learned" that the Versailles Treaty was obviously unfair and self-defeating, and that it played a direct role in the problems of the German economy in the 1920s and 1930s, and in the rise of Hitler. Some might have been taught that Keynes prophesized as much in 1919. These two readings are not going to settle those questions, but they are wonderful at forcing students to re-think what seemed obvious, which is excellent in itself.

(To be clear: the research literature on the economic performance of Germany and other countries in the 1920s and 1930s is vast and tendentious. But students have to come to grips with it. The point of these two readings is to force them to re-think some of what seemed obvious to them in other courses. It cannot be a substitute for dealing with the nuts-and-bolts of hyperinflation, debt, unemployment, etc.)

These readings work best in a seminar setting or with a small-ish lecture class where it is possible to ask for very close reading. If the students read all of both works, then this will take a fair bit of time. But it is worth it. Start them with Keynes, he of the wonderful if angry English. Then watch what happens when they read Mantoux (make sure they know enough about Mantoux to know why _he_ was so angry). The first time Mantoux points out one of Keynes' rookie errors (like when Keynes switches between pre-war and post-war exchange rates, using whichever supports his argument) the students will squirm a bit. They've been taught to think that Keynes was semi-divine. Ask them to speculate on what accounts for these problems -- was Keynes deliberating playing games? Was he working so fast that he made an honest mistake? Was he so caught up in the passion of his position that he let his feelings overwhelm his reason? Where precisely is the line between propaganda and passionate argument?

Now push them a bit, using Mantoux's argument. Was *The Economic Consequences of the Peace* really as important as Mantoux claims? Did Keynes foretell the future or did he help to make that future happen by convincing many people that no other future was possible? If things had turned out differently, would we just view the *Economic Consequences* as a kooky rant written by a brilliant man in a moment of weakness? At the end, bring them back to the central historiographical point. Their international relations professors will have taught them that if the powerful had listened to the smart in 1919, the Versailles Treaty would have been ripped up and the world spared a lot of horror. Really?

A final observation: I have tried, in the past, to include my own work on the undergraduate syllabus. But I have concluded that it is a bad practice. (Although I am always tempted -- at least _someone_ would read the stuff!) Undergraduates are simply too cowed to be comfortable doing anything other than heaping mindless praise on their professors' work. Welcome as this might be -- it sure beats mindless criticism! -- shutting down discussion seems to defeat the purpose of undergraduate education. Graduate students, on the other hand, lack such inhibitions ...