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Economic history: "an isthmus joining two great continents"?

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Economic History: 'An Isthmus Joining Two Great Continents'?

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Economic History:

‘An Isthmus Joining Two Great Continents’?¹

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¹ This is a slightly revised and extended version, with references added, of an invited address to the 4th Annual Meeting of the Associazione per la Storia Economica at l'Università degli Studi di Modena e Reggio Emilia on June 8, 2019. My thanks to the organizers for the invitation and to Guido Alfani, Bruce Campbell, Giovanni Federico, Deirdre McCloskey, Breandán Mac Suibhne, David Madden, Anna Missiaia, Paul Sharp, Peter Solar, and Judy Stephenson for very helpful suggestions.

Economic History:

‘An Isthmus Joining Two Great Continents’?

ABSTRACT: This paper offers (yet another) reflection on the history and current status of economic history. No other sub-discipline of economics or history has tried so hard to be loved as economic history. That love is unrequited, because economic history’s problem is existential: it is an inherently interdisciplinary field. Economists and historians are interested in only small parts of what economic history should embrace. Some examples are given of how narrow views of the past impoverish research. Not all is gloom and doom, however. The controversies economic history provokes and the insights it provides touch on issues that resonate and that will continue to do so.

Keywords: Economic History, Cliometrics

JEL codes: No

Introduction

The subtitle comes from the eminent economic historian T. H. Ashton (1946, p. 83), who in turn got it from James Boswell, the eighteenth century *literatus* and friend of Samuel Johnson. The original context was far removed from economic history. Having organized a meeting between Johnson and the exiled Corsican patriot General Pasquale Paoli in London in 1769, Boswell likened himself to ‘an isthmus which links two great continents’. But whereas in that case the two Great Continents held each other ‘in great veneration’, basking in mutual compliments, the relationship between the two Great Continents joining economic history has been more fraught. The latest appraisals of that relationship range from the rather triumphalist tone of Bob Margo’s ‘The Integration of Economic History into Economics’—which some might read as ‘absorption by’ or even ‘being swallowed up by’—to the bleak pessimism of Stefano Fenoaltea’s ‘Spleen: The Failures of the Cliometric School’ (Fenoaltea 2019).

Half a century ago, one eminent practitioner (Cochrane, 1969, p. 1562) described old-school economic history before the ‘new economic history’ as ‘narration buttressed by occasional measurement’. The story always came first. With luck, researching the story got the creative juices flowing; conclusions and generalizations would follow in due course. Those generalizations were based on a blend of intuition, common sense, and measurement, but they were rarely informed by economic theory. But to be fair: the above depiction of ‘narration buttressed by occasional measurement’ sells the best of the old economic history short. Not only did the stories usually result in some useful generalizations that are

sometimes testable; they also inspired later readers, by providing much-needed information or, indeed, hypotheses to be tested. Enhancing the narrative by adding a bit of theoretical rigour and better estimation techniques was one of the ‘new’ economic history’s contributions to the old. Broadberry *et al.*’s *British Economic Growth 1270-1870* (2015), with its clear links to Phyllis Deane and W. A. Cole’s classic *British Economic Growth 1688-1959* (Deane and Cole, 1967), is a good example of standing on the shoulders of giants. And in an earlier paean to the same book Knick Harley, noting that it ‘largely defined the territory of British economic history for a generation’, asked readers to keep in mind ‘how much Nick Crafts’ and my own work on the Industrial Revolution rests on Deane and Cole’ (Harley, 2001).

With cliometrics it seemed mostly like the other way around: the question or the hypothesis came first and then the database was found or constructed to test it. Alfred Conrad and John Meyer, occasional economic historians, and Robert Fogel, arguably the founding father of cliometrics, both started with against-the-grain hypotheses. For the former in 1956, it was whether U.S. slavery was profitable; for the latter a few years later, it was the axiom that railways were indispensable for US economic growth in the second half of the nineteenth century. From the inductive of the old to the deductive of the new, you might say: and, yes, in an address aimed at gaining converts to the fold in England in 1966 Fogel described the ‘fundamental methodological feature’ of the cliometric revolution as ‘its attempt to cast all explanations of past economic development in the form of valid hypothetico-deductive models’; in other words, claims that could be tested and verified or

falsified with appropriate data.²

Still, it is important to stress that what drove Fogel first and foremost was the quest for ‘explanations of past economic development’. He was interested in history for history’s sake, though insistent on the role of economic theory and statistical methods as building blocks in the construction of that history. But that was in the 1960s. Then in 1976 came D. N. McCloskey’s ‘Does the Past have Useful Economics?’, which dwelt on the uses of economic history as a new variety of applied economics. McCloskey, never a fan of presentism but willing to take the presentism of economists as a given, strove to persuade them that history was ‘a storehouse of economic facts tested by skepticism, a collection of experiments straining the power of economics in every direction, a fount of economic ideas, a guide to policy, and a school for social scientists’. This attitude to economic history was also reflected in the (unhappy, in my view) decision in 1991 to call what should have been the European Economic History Society the European Historical Economics Society.³

Gaining the respect of economists at the cost of reducing economic history to a testing ground for their hypotheses would have been a Faustian bargain for the ‘new’ economic historians. But the economists were not listening anyway. Nor should they have been, lamented Robert

² But Fogel, it is worth remembering, did not confine himself to testing falsifiable propositions. In his last major work on slavery, his answer to the question ‘What if there had been no Civil War?’, was not, as C. Vann Woodward (‘The Paradox of Slavery’, *New York Times*, 5 November 1989) noted, based on economic modelling but on historical research, and it was this that persuaded him that the Civil War and associated carnage was worth it. And his later work on heights and nutrition, while highly quantitative and informed by economics, was also largely inductive in spirit.

³ See the late Gunnar Persson’s history of the society at: <http://www.ehes.org/history.html>.

Solow a decade later (1985), if all economics had to learn from economic history was ‘the bad habits it has taught to economic history’. Solow wanted economic history to ‘offer the economist a sense of the variety and flexibility of social arrangements and thus, in particular, a shot at understanding a little better the interaction of economic behaviour and other social institutions’. On this Solow would have agreed with fellow Nobel laureate Douglass North who argued that the purpose of economic history was ‘to analyze the parameters held constant by the economist’.⁴

Solow probably did not have his wife in mind when he uttered those words although he liked to say that she was brighter than he was and she was, indeed, an economic historian. Her Harvard thesis, which she wrote after she had reared their family, was on the Irish land question, and in the acknowledgements she wrote that ‘it was only my husband’s monumental indifference to the Irish land question that gave me the courage to begin’ (Solow, 1971, p. v). Later in a footnote to her well-known paper on the Eric Williams thesis, she wrote: ‘I would like to thank my sons, Andrew R. and John L. Solow, for assistance in producing the mathematical results, and my husband, Robert M. Solow, for assistance in producing the sons’ (Solow, 1985, p. 99). Later in their lives Bob accompanied Bobbie, as she was known to friends, on trips to Ireland in search of locations associated with the English novelist Anthony Trollope, for whom Bobbie had an enduring passion and about whose Irish novels she was writing a book that she never quite finished; and it was Bob who did the driving while Bobbie did the research.

⁴ Compare ‘History also loosens the shackles of our preconceptions, since comparing the past and present calls into question the exceptional nature of what we are living’ (Antipa and Bignon, 2018).

Bobbie Solow is a shining example of the economic historian who also loved the past for its own sake. But as Ran Abramitsky, one of the best young cliometricians around today, has recently noted with a tinge of regret: ‘The typical modern economist does not share this view that history is interesting for its own sake. Most economists care about the past only to the extent it sheds light on the present.’ Anything else is ‘antiquarianism’. Contrast that with Ashton’s lyricism in his inaugural lecture at the LSE more than seventy years ago (1946, p. 82):

Interest in history, so it seems to me, arises out of the simple delight we all take in watching things grow—whether it be babies, or puppies, or delphiniums or social institutions. That in itself would make the study worthwhile.

It would take a courageous and foolish economic historian to make that claim before his economics colleagues nowadays. But as a *pensionato*, I can afford to be with Ashton on this and believe that economic historians should accept and celebrate the past for its own sake as different and mysterious and exciting.

The non-random reflections on that follow what is right and wrong with our field are very much in this spirit. I begin with some further thoughts on economic history’s links to those Two Great Continents. I then discuss some recent work in the field that highlights ways in which our discipline has been evolving. I end with some self-referential thoughts on deductive and inductive approaches to research in our field.

1 Lone Wolves and Team Players?

Traditionally, as authors, historians have ploughed a lone furrow

and they continue to do so. Economists formerly used to do so too, but since the 1950s or so they have tended increasingly to work in pairs and in groups. The explanation for this shift, which has been widely documented, is hardly that economists have become friendlier or more sociable than historians. But if it is simply a reaction to the challenge of increasing complexity and thus the need for greater specialization (Jones 2009), must that mean that economics is becoming more complex while history, like poetry or painting, is not?

Although Fogel long ago described ‘large-scale, collaborative research [as] a hallmark of cliometric work’ (Fogel and Elton, 1983, p. 61), the pattern of co-authorship in economic history, as Seltzer and Hamermesh (2018) and Cioni, Federico, and Vasta (2019) have recently documented, remains in-between. Figures 1a and 1b, where economic history is wedged between economics and history, illustrate. In the 2010s, whereas most papers in *Past & Present*, representing the best of history, were single-authored, about two in five papers in the *Journal of Economic History* and *Economic History Review* were still single-authored, whereas the proportion in the *Quarterly Journal of Economics* and the *Journal of Political Economy* was about 15 per cent.

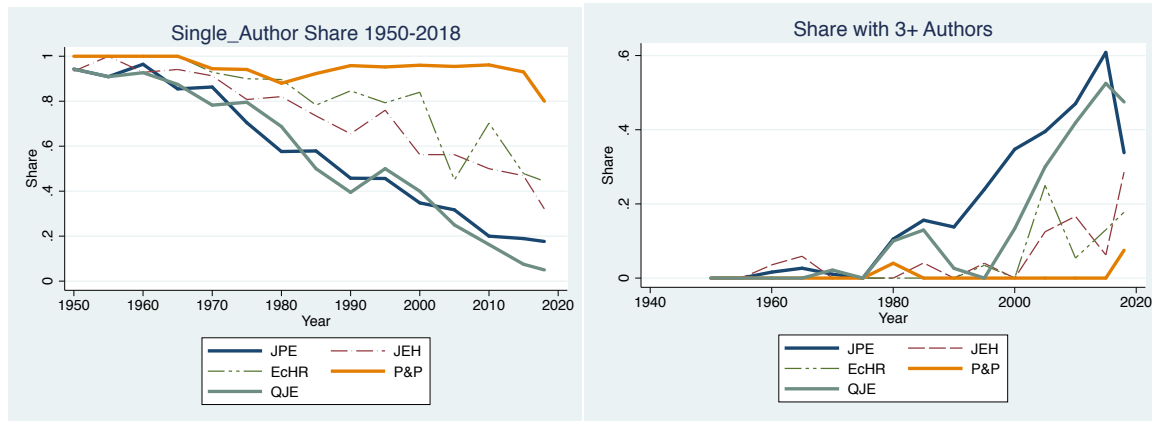


Figure 1. Authorship and Co-Authorship

To some extent, this understates the role of single authorship in economic history, since monographs, which are more likely to be single authored, still represent a significant share of publications in economic history. Again, Cioni *et al.* refer to this. In economics departments books don't count for tenure anymore; indeed, a friend who also happens to be a top-notch economic historian remarked to me once that he would not have obtained tenure on the basis of the research—mostly in books—for which he is best known. But the books included in Table 1 below are their authors' most-cited publication in nearly all cases. Journal articles (single- or multi-authored) feature far less.

There is a generational shift at work here, as Robert Margo (2018) has noted. In the early days of cliometrics there still was an expectation that its practitioners should meet professional norms in history such as 'publish[ing] books as well as articles, perhaps learn a foreign language or two, visit the archives regularly, and so on'. But despite the occasional swallow like Ran Abramitsky's *The Mystery of the Kibbutz* (2018), Leah Boustan's *Competition in the Promised Land* (2016), and Thomas Piketty's

Le Capital au XXI^e siècle (2013)—the English translation of which topped the *New York Times* bestseller list—the practice of writing a book is fading fast since the focus of the new elite among economic historians is on top economics journals rather than field journals and monographs.

It would be nice to think that the historian and the economist offered complementary approaches to economic history, involving complementary skills and the resultant comparative advantage and mutually beneficial trade. Broadberry *et al.*, again, is a shining example of such synergy but, alas, it is an outlier; not only do economists and historians not write together, they do not cite each other. Panel A of Table 2 shows the citation patterns by discipline of seven well-known cliometric papers published in top three economics journals, based on the first three hundred citations in *Google Scholar*. These papers share a common theme; they are about ‘persistence’ or ‘deep origins’ [on which more later]. ‘*EconHist*’ includes citations in the main economic history field journals and books. Clearly, ‘persistence’ papers get little cited in economic history outlets and have virtually zero impact on historical scholarship. These are papers written by economists for economists. Panel B describes a dozen of the best-known works in economic history which appeared either as monographs or as articles in field journals. There at least, the isthmus between the two Continents counts for something.

<i>Table 1. Journal Papers and Single-authored Books: Google Scholar Citations</i>			
Name	Title	Citations	Most cited article
Douglass North	<i>Structure and Change</i> [1981]	10,337	5,827
Joel Mokyr	<i>The Lever of Riches</i> [1992]	4,156	309
Avner Greif	<i>Institutions ...</i> [2006]	3,266	2,950
Barry Eichengreen	<i>Golden Fetters</i> [1996]	2,446	2,157
Claudia Goldin	<i>Understanding the Gender Gap</i> [1992]	2,278	1,438
Peter Lindert	<i>Growing Public</i> [2004]	2,135	690
Gregory Clark	<i>Farewell to Alms</i> [2005]	2,052	487
Gary Libecap	<i>Contracting ... Property Rights</i> [1983]	1,746	561
Robert Fogel	<i>Railways and Econ Growth</i> [1964]	1,738	1,774
Robert Allen	<i>The British Indl Revolution</i> [2009]	1,375	1,374
Nicholas Crafts	<i>British Econ Growth during IR</i> [1983]	1,221	292
Deirdre McCloskey	<i>Bourgeois Virtues</i> [2010]	1,214	291
Gavin Wright	<i>Old South, New South</i> [1986]	1,108	782
Larry Neal	<i>Rise of Financial Capitalism</i> [1993]	1,009	136
Peter Temin	<i>Did Monetary Forces ...</i> [1976]	972	658
Note: citations as of 27 May 2019			

Table 2. Citation Patterns: ‘Persistence’ Papers and Some Others					
<i>Paper/Book</i>	<i>Econ</i>	<i>SS/Science</i>	<i>EconHist</i>	<i>History</i>	<i>Other</i>
<i>PANEL A</i>					
Acemoglu et al. <i>AER</i> 2001	262	32	1	3	1
Banerjee/Iyer <i>AER</i> 2005	235	39	17	8	1
Becker/Woessman <i>QJE</i> 2009	229	26	32	6	4
Nunn/Wantchekon <i>AER</i> 2011	243	40	10	3	2
Nunn/Qian <i>QJE</i> 2011	210	18	39	7	16
Alesina et al. <i>QJE</i> 2013	246	37	3	3	9
Dell <i>Econometrica</i> 2016	220	30	26	2	0
<i>PANEL B</i>					
Fogel <i>Railroads</i> 1964	149	46	80	18	1
Fogel/Engerman <i>T/C</i> 1974	85	82	75	45	5
North/Weingast <i>JEH</i> 1989	172	92	29	8	0
Greif <i>JEH</i> 1989	196	61	36	2	2
Nelson/Wright <i>JEL</i> 1992	233	6	40	1	1
Mokyr <i>Lever of Riches</i> 1992	218	9	41	13	1
Goldin <i>Gender Gap</i> 1992	148	101	17	21	3
Allen <i>EEH</i> 2001	49	22	209	14	1
O’Rourke and JGW <i>EREH</i> 2002	123	30	95	30	5
Clark <i>Farewell to Alms</i> 2005	133	79	70	11	5
Finlay & O’Rourke <i>Power</i> 2009	130	49	79	31	2
Mokyr <i>Enlightened Econ</i> 2009	128	33	112	22	3
Note: based on top 300 citations on GS, April 2019					

The tensions and misunderstandings between our two Great Continents go beyond the methodological. Historians will argue that,

being more archive-oriented, they are more careful with their raw material than cliometricians. They pride themselves on their ‘hand collected’ data, as if they deserved some kind of ‘organic’ certificate for doing so, just as cliometricians pride themselves on devising better ways of automating data collection. In truth, both methods have their place. Ongoing research involving Martha Bailey, Ran Abramitsky, and others is debating how the quality of data generated by automated linking methods compares with hand-collected data (typically collected nowadays by a research assistant) (Bailey *et al.*, 2019; Abramitsky *et al.*, 2019). Both options are prone to error and bias. But it is probably fair to say that historians are fussier about the data than economists, if data is the correct word.⁵

Collecting the data, which can be pretty boring, sometimes leads to new ideas about how to use them—on which more below. It also leads to a better feel for the data’s frailties. Anybody who works on, say, the measurement of agricultural output in the past will come away humbled by the challenge—and sceptical of strong claims made about productivity change or calorific consumption. As Oskar Morgenstern put it long ago (1963), ‘*Qui numerare incipit errare incipit*’. But the issue is not the occasional error *per se*: it is about the acceptable degree of error and what confidence intervals to place on findings based on the data. A few years ago McCloskey wisely cautioned that we need to get ‘beyond counting something without knowing what the something is’.

⁵ As Deirdre McCloskey has written about Fogel: “ ‘data’ were never what Bob collected. The word means in Latin ‘things given’. In none of his work did Bob rely on what other people had given him. The word *data* embodies an attitude to facts fatal to serious science. We need a new word, *capta*, which means ‘things seized’ ” [available at: <https://www.deirdremccloskey.com/editorials/fogel.php>].

One of exciting features of the new economic history in the early days is that it produced a lot of revisionism: Fogel on the railways, McCloskey on Victorian Britain and on the gains from trade, Crafts and Harley on productivity growth during the Industrial Revolution. And the interesting thing in all those cases is that the revisionism was about talking down established claims. Nowadays there is more of a tendency to hype or sensationalize effects as 'big'. Such results are more likely to attract attention.

2 *Long-term Impact of Foetal Distress*

My first two case studies concern the long-term impact of famines and epidemics. They bear mostly on economists' tendency to oversell their results.

2.1 *Famines*

Epidemiologists have been invoking famines in research on the foetal origins hypothesis (FOH) since the 1960s, beginning with the classic study of the Dutch *Hongerwinter* (hunger winter) of 1944-45 by the South Africa-born epidemiologists Zena Stein and Ezra Susser. Only much later, in the 2000s, did economists become interested in famines as a source for testing the FOH. By now their research has extended to famines as far apart as in Greece and China, Bangladesh and Leningrad, Ethiopia and Ukraine. These studies report a wide range of outcomes, with recent work suggesting that the foetal origins effect has been somewhat oversold.

Susser and Stein used data on Dutch military recruits born before, during, and after the Hongerwinter of 1944-45 to test the FOH, and their marker for foetal origins impact was cognitive development. Somewhat disappointingly, they could not confirm the hypothesis, concluding that ‘poor prenatal nutrition cannot be considered a factor in the social distribution of mental competence among surviving adults in industrial societies’⁶, an outcome which they described as ‘negative’. Much later Zena Stein would relate how many colleagues were ‘furious with us because it would have been much more satisfactory (in terms of social justice) to have found that food did matter’ (Willcox and Stein, 2003; see too Davey Smith and Susser, 2002, p. 35). But later they acknowledged that their finding that 18-year old army recruits who had been *in utero* during the famine were more intelligent than both recruits from non-famine areas and recruits born just before and after the famine in the affected area was attributable to selection bias. This was because more resilient and better-resourced households, whose children were on average brighter, experienced less of a fertility decline during the famine than the remainder. So what Stein and Susser initially deemed a disappointing outcome was due to the famine’s impact on the social composition of the famine birth cohort. In short, selection trumped scarring.⁷

⁶ Stein, Susser, Saenger, and Marolla, 1975, p. 236; compare Stein *et al.*, 1972, p. 708. For an excellent recent study of the Hongerwinter see de Zwarte, 2018.

⁷ Subsequent research on the Hongerwinter has spawned a confusing profusion of competing and conflicting claims by rival medical researchers, many of whom ignored the issue of selection. The quest for research funding has led to non-replicable experiments and a tendency to hype implausible outcomes. One 2009 study ‘explore[d]’ the impact of foetal origins on sexual differentiation, on the basis that animal experiments have found that ‘underfeeding of the mother can result in feminization of the male offspring’, but could find ‘no effect of prenatal exposure to

A tempered reading of the results of research so far highlights both the limitations of famines as natural experiments and the robustness of a small number of findings. There is a good deal of evidence linking the Hongerwinter to increasing incidence of obesity, diabetes, heart disease, and schizophrenia. However, a 2001 study of the long-term impact of the Leningrad blockade, which compared outcomes in Leningrad and the Netherlands and failed to find any link between maternal malnutrition and ‘glucose intolerance, dyslipidaemia, hypertension, or cardiovascular disease in adulthood’ in the former, concluded rather lamely that ‘intrauterine malnutrition may be of greater impact when postnatal nutrition is sufficient’ (Stanner and Yudkin, 2001, p. 292). And a recent meta-analysis of studies of the impact of the Chinese Great Leap Forward famine which ‘raises questions about the design and analysis of current studies’, finds that the data ‘show no long-term health effects except for schizophrenia’ (Li and Lumey, 2017).

Economists have also broadened the original question to devoting particular attention to the impact of health insults *in utero* and early childhood on economic well-being in adulthood. Two of the earliest economic studies to use a famine to test the FOH produced quite different results. The first, by Klemp and Weisdorf (2012), used the Cambridge Group’s 26-parish database to test the FOH against what they dubbed ‘the great English famine of the 1720s’. This study is weakened by

famine on sexual orientation and gender identity’. Seemingly disappointed, the authors added that ‘the small sample size of participants with non-exclusively heterosexual identification (possibly due to underreporting of homosexuality) may have reduced our power to detect any differences’ (de Rooij *et al.* 2009). Another ‘fishing expedition’ found that ‘exposure to severe wartime conditions in utero was not associated with the prevalence of IBS [irritable bowel syndrome] in adulthood’, but that early-life exposure to severe wartime conditions was (Klooker *et al.* 2009).

its insistence on highlighting the implausible outcome that this ‘great famine’ reduced the life expectancy of the most exposed group, ‘children born to English Midlands families of a lower economic rank’, by over twelve years. No other study of the FOH has detected such a huge effect. The result is all the more astounding given that the famine in question was far from ‘great’—in fact, it was a very minor one. True, excess mortality was high for several years in succession (1727-31), resulting in the deaths of up to 0.2 million or 0.4 per cent of the entire population, but most of those deaths were not due to famine but to diseases, most likely including a variant of influenza (Healey, 2008). Contrast this van der Berg *et al.* (2010) on the impact of the Dutch potato famine of the 1840s: they found that the life expectancy at age 50 of men exposed to severe famine at birth was significantly shorter (4.2 years) than that of men not so exposed, but that there was no difference at ages below fifty. As for women van der Berg *et al.* could detect no significant differences in either total or residual life expectancies. Klemp and Weisdorf’s twelve years also bears comparing with the results of a recent study by Doblhammer *et al.* (2013) of the Great Finnish Famine of 1867-68, which finds that fetal exposure reduced male life expectancy by one year, while the outcome for females is ‘less conclusive’ (2013, p. 318). These are relatively modest effects, hard to reconcile with the rather extravagant claims made by Klemp and Weisdorf.

2.2 *The Influenza Pandemic of 1918-1919*

One of the most cited studies of the foetal origins hypothesis by an economist also suffers from the same weakness of implausibly strong

results. This is Douglas Almond's study of the long-run consequences the influenza pandemic of 1918-19. The pandemic resulted in 50-100 million deaths worldwide, or more than the sum of fatalities, military and civilian, during the Great War. The pandemic struck in two main waves, of which the second in late 1918 was by far the more lethal. The disease struck suddenly, and perversely, healthy people in their twenties and thirties were the most vulnerable, the product of immunological overreaction or 'a cytokine storm'. The symptoms included shivering, severe headaches, pain in the legs and kidneys, followed by high fever accompanied by a hacking cough. A minority developed infections that led rapidly to pneumonia and death; however, most seemed to recover fully in a week or two. It was once believed that there was a link between the flu pandemic and the epidemic of *encephalitis lethargica* or 'sleeping sickness', which preceded influenza outbreak by a year and recurred in waves until 1926, but nowadays epidemiologists are sceptical of that link (Foley, 2009; Hoffman and Vilensky, 2017).

In his much cited-study (Google Scholar citations=969 in May 2019) of the impact of the flu on U.S. children *in utero* Almond found that the offspring of exposed—and that does not imply *infected*—mothers were 'up to 15 percent less likely to graduate from high school', 15 per cent more likely to be poor, and 20 per cent more likely to suffer from some disability. Males among them earned 5-9 per cent less. These are truly sensational results, given that even the minority of women in the cohort who were attacked recovered quickly and without any apparent long-term scarring and, indeed, that the pandemic had been largely forgotten until the influenza scares of recent times (Beiner *et al.*, 2009).

Almond's findings came under attack from Ryan Brown and Duncan

Thomas (2018) who argued that his results were vulnerable to selection bias; specifically, the parents of children born during the epidemic were less likely to be literate and white and more likely to be of lower socioeconomic status. Controlling for socioeconomic characteristics, Brown and Thomas found little evidence that children born in 1919 fared worse than surrounding birth cohorts. However, a recent study by Beach, Ferrie, and Saavedra (2018) shows that by inferring cohorts from reported age [0 in 1920, 10 in 1930] in the census Brown and Gordon exaggerate the impact of selection; that is because poorer parents were more likely to ‘age heap’, i.e. opt for ages ending in zero. Linking 1920 and 1930 microdata to pin down parental characteristics more accurately, Beach *et al.* report that

the absence of the pandemic, the high school graduation rate of the 1919 birth cohort would have been about 2 percentage points higher ... As for biological outcomes, we find no consistent evidence that in utero exposure to the pandemic affected heights, weights, or BMI – the only proxies for health that are available in the enlistment records.

Beach *et al.* headline their results as confirming those of Almond, because they identify a statistically significant effect. However, the *size* of the effects they identify are far closer to Brown and Duncan’s position. The headline binary outcome--‘fetal shock or selection’--attracts more attention to the statistical significance than the small size of the effect.

Almond’s study has spawned several others. Bengtsson and Helgertz’s 2019 study of the long-run impact of the pandemic in Sweden is one of the finest. While Bengtsson and Helgertz find negative but modest

impacts on morbidity (as reflected in hospitalization rates, 3.6 and 2.9 per cent for males and females, respectively) and mortality (3.8 and 1.2 per cent), they find that fetal exposure was associated with *higher* incomes and *better* socio-economic outcomes for males, outcomes which ‘somewhat difficult to reconcile with the *a priori* expectations’. Neelsen and Stratmann (2014) have studied the long-run outcome in Switzerland. They find that the impact on male educational outcomes was much smaller than Almond did (2.2 per cent for vocational or higher education degree completion in Switzerland versus 13-15 per cent reductions in the high school completion rate) and they find ‘little evidence for adverse education and labor market effects for females’. Again, the implied damage was much less than claimed by Almond. On the other hand, a study of the impact of the epidemic in Taiwan by Lin and Liu in the *Journal of Health Economics* (2014) reports bigger long-term damage to adult heights and educational attainment than Almond did. They do not explain why that is so.

We, economic historians, love papers like Almond’s because they are clever and because they produce results that make us say ‘wow’. But most new useful knowledge is incremental and less sensational.

3 *Real and Unreal Wages:*

My next two case studies concern labour markets and show the tensions between the approaches of economists and historians. Both relate to insensitivity to what certain historical data mean. The first concerns secular trends in wages, the second their levels during the British Industrial Revolution.

Although real wages were and continue to be our most popular measure of living standards in the past, they are by no means an ideal measure. John Hatcher and Judy Stephenson (2018) are only the latest in a long line of historians of medieval and early modern England to caution against over-interpreting real wage data. Hatcher cites in particular Jeremy Boulton, who over two decades ago worried aloud about ‘the fragility of the best known series, that constructed by Phelps Brown and Hopkins from data compiled by Thorold Rogers’, and about ‘the weight placed upon it by later authors’ (1996, p. 268); and Donald Woodward who, while conceding that wages in early modern England were sensitive to market forces, concluded that ‘Any attempt to establish a meaningful series of real income founders on our ignorance of the number of days worked in the year’ (as cited in Hatcher, 2018, p. 27).

Generalizations based on wage data hugely influence our understanding of both the more distant and more recent past. Thus, Greg Clark’s wage data have led him to claim that in England ‘living standards for farm workers were about the same in 1200 as in 1800’, while Bob Allen’s calculations prompt the finding that English wages were ‘high’ relative to elsewhere on the eve of the Industrial Revolution (Clark, 2007, p. 99; Allen, 2009). Allen’s finding is disputed by Judy Stephenson (2018), whose researches on the London building industry concludes that the earnings of unskilled men between the mid-17th and late-18th century were only about half what Allen claims.

Conflicting models of the Industrial Revolution also largely revolve around competing data on real wages and GDP per capita. On the one hand, Bob Allen claims that between 1780 and 1840 British GDP per capita and real wages rose by 46 and 12 per cent, respectively, a divergence that

underpins what he has dubbed ‘Engels’ pause’, whereby the lion’s share of the gains from economic development accrued to capitalists. On the other hand, Gregory Clark’s data have real wages rising by 32 per cent between the 1770s and the 1830s, or at exactly the same rate as GDP per head as estimated by Broadberry *et al.* (2015, p. 242-3). Whence Clark’s claim that ‘wage earners and foreign customers, not entrepreneurs, were the overwhelming beneficiaries of Industrial Revolution innovation’ (Clark, 2014: 20). Given the claims and counter-claims some attention to margins of error seems necessary, and caution too in making strong claims on the basis of the data.

3.1 *By the Day or by the Year?*

One concern about historical wage evidence has to do with its relationship to earnings, which, as Woodward emphasized, depends on how many days were worked. In medieval England landowners relied on three kinds of labour to work their demesnes. *First*, there was the unfree labour associated with serfdom. Such labour had already been commuted to monetary payments in some places long before the Black Death. *Second*, there were paid employees (described as *famuli* in manorial documents) on fixed term contracts, typically annual, who took care of animals, drove vehicles, looked after the dairy, and repaired buildings, fences, and equipment. They tended to include a ‘*permanent nucleus of ploughmen throughout the year*’ (emphasis added) who were responsible for keeping draught animals and ploughs in order, as well as functionaries such as beadles and woodwards [sic!] (Postan, 1954, p. 4). *Third*, there were seasonal workers who helped with the mowing and threshing and

winnowing. As serfdom gave way to free labour, employers relied on a combination of temporary and permanent workers. Changes over time in their respective shares cannot be determined with precision. Annual contracts seem to have accounted for almost half of the farm labour force in the Middle Ages, but their share had dwindled markedly by the early nineteenth century (Snell, 1985; Humphries and Weisdorf, 2019).

In a series of recent papers Jacob Weisdorf and co-authors (Allen and Weisdorf 2011; Humphries and Weisdorf, 2015; 2019) exploit data on the annual payments made to *famuli* (f) and the daily wages paid to seasonal workers (w), to infer movements in labour intensity since the middle ages. Claiming that '*income from casual and annual work was roughly identical*' (emphasis added) Humphries and Weisdorf [HW] (2019) calculate:

$$\text{days worked per year} = f/w$$

Allen and Weisdorf (2011) claim that this formula yields an estimate that 'can be used as a proxy for the actual working year among farming day labourers from the late middle ages through the Industrial Revolution', but do not dwell on the implication that the work year in the late middle ages and in the early modern era was much shorter than during the industrial revolution, with agricultural labourers before the late seventeenth century spending less than two hundred days a year on the farm. This claim, elaborated in Humphries and Weissdorf (2019), implies that the 'Industrious Revolution' in England began much earlier than originally proposed by Jan de Vries (1994); by their reckoning days worked per annum rose almost uninterruptedly from the mid-fourteenth

century on. This is a striking finding, and it has won the support of such leading scholars in the field as Steve Broadberry and Bruce Campbell.⁸

However, the finding is only as strong as the underlying data. And what pass for daily wages have been heavily criticized, particularly by John Hatcher. Indeed, the labour of the *famulus* and the casual worker were far from being the perfect or close substitutes asserted by Humphries and Weissdorf: they were quite distinct categories serving quite different purposes. The wages paid to workers hired for specific tasks were higher than those paid to live-in servants because the nature and the constancy of the work performed by those workers differed. There was a Smithian trade-off: the former earned more per day, but their employment was temporary and seasonal, whereas the latter were paid a lower daily equivalent across the year. This trade-off may well have changed over time. In addition, the work day was much longer in harvest time than the annual average and presumably this was reflected in the harvest wage; moreover, casual employment suited those workers who were also smallholders and who devoted a considerable part of the work-year to their own livestock and crops.

Direct evidence on the length and evolution over time of the work-year is sparse: hence the attraction of the approach taken by Weissdorf and his co-authors. But medieval specialists typically opt for a work-year of 250-260 days for the fourteenth-century *famulus* (Karakacili, 2004; Claridge and Langdon, 2015). And referring to the first half of the fifteenth century, when according to Humphries and Weissdorf the work-year ‘was

⁸ Broadberry et al. (2015, p. 263-5) accept that a secular increase in the work-year ‘seems to fit the British case well’. See too Campbell (2016, p. 380).

as short as 110-120 days’, Liu (2012, pp. 194, 266) argues that ‘[O]verall, on a conservative level, at least 250 workdays were taken on by an ordinary ploughman for the year; and 300 days do not seem unreasonable’.

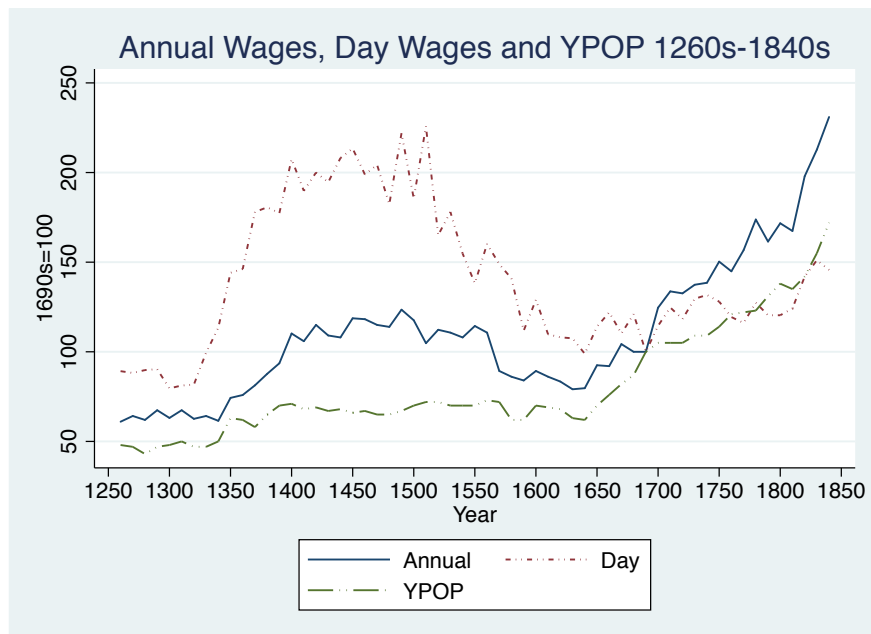


Figure 2a. Annual and Day Wages and YPOP 1260s-1840s

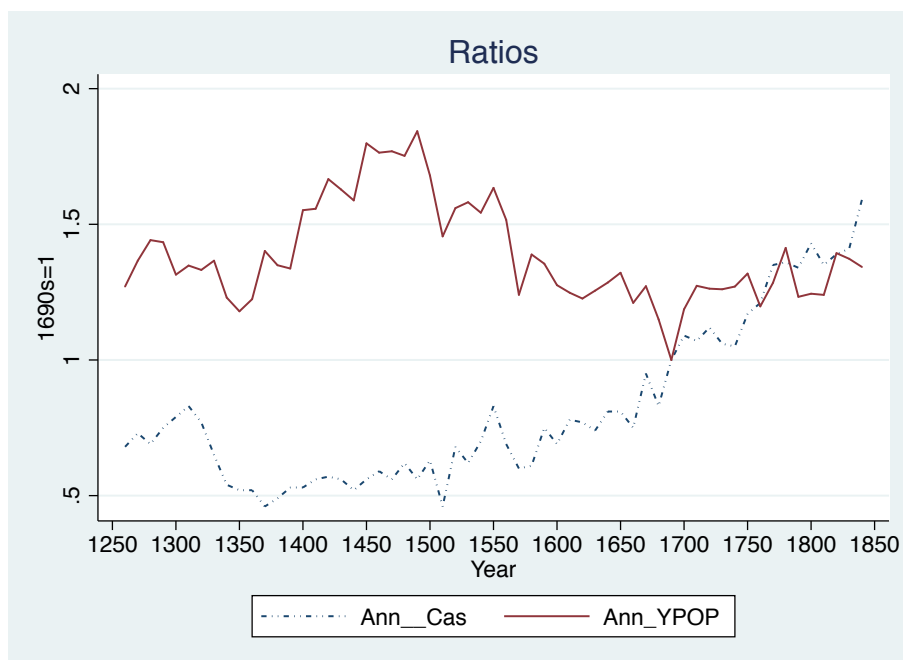


Figure 2b. Ratios

If the historians are not convinced, shouldn't the economically inclined economic historian be wary? Consider the larger implications of Humphries and Weissdorf's results. Figure 2a describes the computed real day and annual wages and GDP per capita as reported them.

Implications of the first series are that real day wages were much higher just after the Black Death than in the wake of the Industrial Revolution, and that they halved between c. 1500 and the early seventeenth century. Second, note that the annual wage and GDP per capita series are quite similar, though derived from quite different sources; indeed, Humphries and Weissdorf (2019) themselves note that the latter 'fits markedly better with per capita GDP compared to earnings inferred from day wages'.

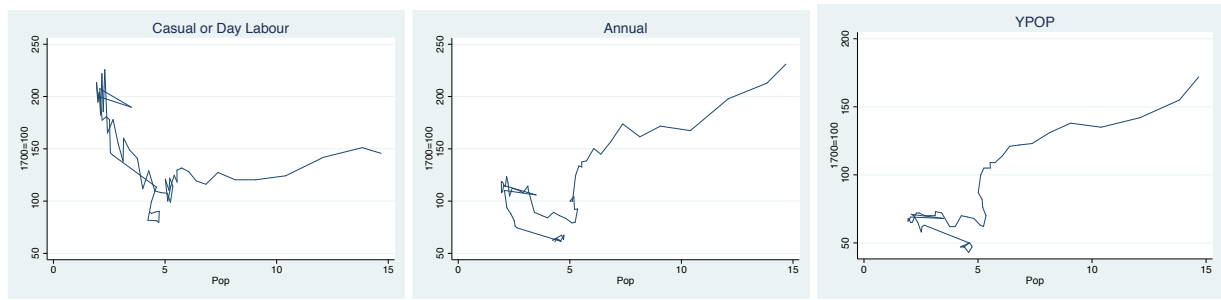
The ratios described in Figure 2b are those of annual wages to day wages [broken line] and of annual wages to GDP per capita [solid line, YPOP], respectively. The rise in the former from the late medieval period on tracks the precocious 'industrious revolution' claimed by Humphries and Weissdorf. The secular pattern implied by the latter is much more plausible and accords with theory: a rise in wages in the wake of the Black Death, when the ratio of labour to land was low, and a decline from the late fifteenth century on, when population pressure was beginning to impact again. Only in the eighteenth century did the reward to labour show signs of recovering, albeit very modestly. This interpretation of the data rejects the HW 'industrious revolution' and also the implication that labourers in the wake of the Black Death were better off than their remote eighteenth-century descendants. It suggests that real wages rose more or

less in line with GDP in the very long run, but with swings associated with population pressure before c. 1700.

Figure 3, also taken from Humphries and Weissdorf (2019), contrasts the relationship between population and their two measures of the real wage. Figure 3a mirrors Clark [2005], tracing an unchanging demand for labour before the mid-seventeenth century. Figure 3b, based on computations of the income of contract servants, reveals a picture similar to Figure 3c, where Malthusian pressures are less evident, and where a rise in living standards after the mid-seventeenth century is implied.⁹

One is left regretting that Humphries and Weissdorf, having assembled a really impressive database on farm servants on annual contracts over the centuries, did not jettison their daily series altogether, instead of seeking to replace the chimera of a fifteenth-century Golden Age for labourers by their own equally implausible ‘leisurely medieval Golden Age and an early modern Industrious Revolution’. And one hopes that Broadberry *et al.* will come to see that annual wage data, warts and all, buttress their case for secular amelioration. An unintended but interesting outcome of Humphries and Weissdorf (2019) is the implied secular fall in the premium paid to casual labour. Investigating and explaining that finding is a topic deserving separate study.

⁹ Humphries and Weissdorf’s annual wage series comprises payments in cash and in kind. They monetize payments in kind by assuming that they covered the costs of food and board, which they approximate by drawing on Bob Allen’s ‘respectability’ consumption basket. A clear drawback of this approach is that the quality of the basket varied over time. More work is required here, but at least the annual series finesses the days worked issue.



[a] Casual, day

[b] Annual

[c] YPOP

Figure 3. Wages, YPOP, and Population, 1260s-1840s

3.2 *'Cheaper by the day but dearer by the grate':*

Why the delay? Surely, the hardest task would seem to have been the original creative acts that produced coke smelting, the mule, and the steam engine. In view of the enormous economic superiority of these innovations, one would expect the rest to have followed automatically.

David Landes (1969, p. 126)

Let us now turn to wages on the eve of the Industrial Revolution. It is not my intention to question the claim associated most closely with Robert Allen that wages in England were 'high' relative to neighbouring economies; few disagree with this stylized fact. The issue is how to interpret it. The history of migration between England and France offers insight.

The revocation of the Edict of Nantes in 1685 prompted the inflow of 40-50,000 French Huguenots of all ages to England, many of the adult

males being skilled artisans in the luxury trades. Less attention has been paid to the much smaller reverse flow of skilled labour, which persisted for most of the eighteenth and nineteenth centuries. Dating from John Law's 1718 scheme to entice British technology and skill across the Channel, this migration reveals about both the beginnings of English technological leadership and the cost of skilled labour in England. Law's success in luring a few hundred artisans and manufacturers, mainly in metalworking and woolen textiles, prompted English legislation in 1719 that would make such migration illegal for over a century. The outflow of skilled workers and manufacturers, for the most part now clandestine, would resume in the mid-eighteenth century. The numbers involved remained small, though Harris's guesstimate of no more than a thousand between 1710 and 1800, derived from an unpublished French dissertation, seems on the low side (Harris, 1998, p. 552; Linant de Bellefonds, 1971, p. 87-98).

Still, the French demand for English artisanal skills spanned several sectors. For successful technological transfer from England to France to occur, the intangible aspects of artisan capital required the physical presence of English artisans. English workers embodied the desired technology. The lack of artisanal know-how delayed the production of crucible steel and the spread of Henry Cort's puddling process (developed in 1783/4) on the Continent France for over half a century. Continental ironmasters hoping to learn how to puddle by on-the-spot observation failed to master "the really decisive 'knacks' of the trade". Similarly, the diffusion of Watt's steam engine in France was delayed by lack of suitably skilled mechanics until after the peace of 1815. Harris (1998, p. 218-22, 591fn22) has claimed that the French obsession with file-cutting

machinery may have sprung from their lacking the appropriate hand skills. As for the new cotton and woolen textile machinery, science and mathematics were all very well, but ‘in the end it was imported English skills in manufacture, assembly and operation which were necessary for its success’. ‘The arts never pass by writing from one country to another, eye and practice can alone train men in these activities’ (Trudaine de Montigny (1752) as cited in Bradley, 2010); continental tariffs and state subsidies were poor substitutes for those ‘arts’ (Mathias, 1975; Tann and Breckin, 1978; Fremdling, 1991, pp. 532-7; Bradley, 2010; see too Fremdling, 2000; Becchia, 1991; Harris, 1998, p. 561).

The Industrial Revolution increased the demand for English artisans on the Continent, so after 1815 the outflow of workers, both legal and clandestine, resumed, raising concerns in England (Bensimon, 2011). Most were technicians who installed, maintained, and managed new equipment. With the exceptions of railway navvies, most travelled singly or in small groups.¹⁰

Prohibition did not prevent English artisans from seeking well-paid employment in France and elsewhere, but the aggregate numbers remained modest. In 1824 a witness appearing before the Select Committee on Artisans and Machinery estimated the stock of English workers in France at 15-20,000 but a more reliable source argued on the basis of information from the French police that there were only 1,300 or

¹⁰ Those who worked for Aaron Manby (1776-1850), owner of a large engineering works in Charenton near Paris, who employed 200-250 Englishmen there in the mid-1820s, were exceptional in this respect; so were those employed at the Crawshay works at Fourchambault in the Nivernais, which housed an expatriate Welsh community of 150 adults and a similar number of children c. 1830, and also those who worked in the lace factories dotted around Calais and its hinterland.

1,400 English artisans in France at that point. In 1831 the authorities reckoned that France contained about 12,500 British-born residents in all. A 1844 report proposed a total of 66,000, but this included many who were not in the labour force. The 1851 census counted 378,000 born abroad (1.1 per cent of the total) of whom 20,357 were English.

The stock of expatriate residents at any point in time was small. But this underestimates the significance of skill transfers because most of the migrants were temporary. The short distances involved made temporary migrations practical. Artisans who moved temporarily tended to be legal; they often travelled within France, working for a series of employers, before returning home. Some of the mechanics, such as those employed by the engineering firm of John Hall & Son of Dartford, paid repeated visits to France.

The one-way flow of artisanal skills was complemented by a reverse one-way flow of 'industrial tourists', often intent on stealing trade secrets or luring away English craftsmen. That flow too was small in size, although it intensified in the wake of the peace-treaties of 1814-5 (Harris, 1998; Jones, 2009; Bradley, 2010; Bertucci, 2013).

Wages were higher in England than anywhere else in Europe before and during the Industrial Revolution. Why, then, was the flow of artisans between England and France uphill rather than downhill? The answer is that French industrialists were prepared to pay British artisans more because they were 'far more skillful' and smarter than their French counterparts. They were better trained; they had more stamina; they were physically stronger and could operate twice as many spindles or

mechanical looms as the average French worker; the latter, according to some, were constrained by an 'inferior intelligence' (Stearns, 1965, p. 55).

For these reasons, skilled labour cost less in England. As noted in evidence to the Select Committee on Machinery in 1824, 'the English workmen, from their better methods, do more work and better than the French ... and though their wages are higher, yet their work does not cost more money in France than when done by Frenchmen, though their wages are lower'.¹¹

The gap persisted into the 1840s. An English machinist resident in Belgium, Grenville Withers, confirmed this:

We must not calculate, because the Frenchman receives three francs for a day's labour, and we pay 4s. in England, that the French labour is cheaper than ours. I think, on the contrary, that an Englishman does a great deal more in proportion for 4s. than a Frenchman for three francs. I have known Englishmen, week after week and month after month, to get £3, £4, and £5 per week, working at the same price as Frenchmen, who do not get more than 18 or 20 francs' (1st Report Export of Machinery, Q1075).

¹¹ John Martineau, who manufactured machines in London, testified that a 'first rate smith in London would receive from 6s 6d to 7s per day. The same workman would receive from 10 to 11 francs in Paris', while a typical French blacksmith would make in France 4 francs a day, against the 7-8 francs an average English smith might make in Paris. Several witnesses offered corroborative evidence; according to one Mr. Alexander, another engineer, 'I will take an English engineer for an example, or an iron-founder, or a smith, or a turner; either of these, if good workmen, obtains ten or twelve francs a day; while the natives will not get more than five or six francs'.

And there is much more like that too.¹² Naturally, in France—and on the continent more generally—the aim was always that skills be passed on to local craftsmen. Cockerill’s establishment at Seraing was described by Withers as ‘a wonderful nursery for machinery’, with ‘artisans in machine-making becoming more dexterous every day’ (Withers, 1st Report, Q793). The new ironworks set up at Terrenoire near St. Étienne by Louis de Gallois in 1818 required the presence of ‘as many as 80’ English workers in the early 1820s, who were paid twice as much as their French co-workers. It took the latter several years to catch up, although by 1828 the English pay advantage had been cut to one-third (Belhoste and Woronoff 2017). But as the Continentals acquired one set of skills, the frontier in England was pushed out further by innovations in machinery production, the steamship and the railway. These required further waves of English artisans who, in turn, were paid a premium to move. For example, although the original group of English workers employed by de Gallois were eventually replaced by French workers, the site at Terrenoire that specialized in the production of iron rails in the late 1830s employed ‘une importante main d’œuvre anglaise d’ouvriers spécialisés’ (Périnaud, 2014).

Evidence that British railway construction workers or ‘navvies’ working in France, of whom there were thousands in the 1830s and 1840s,

¹² Thomas Ashton, a prominent Cheshire cotton manufacturer, who visited France and Belgium to gather evidence for the 1841 Select Committee on Machinery, reported that ‘people in this country are of quite a superior class, as workmen, to any I have seen abroad. It costs foreigners more than it does us to produce the same quantity of work; they pay more for the working of the piece and the spinning of the yarn than we do’. In reply to the comment that continental labour ‘to use a country phrase, [was] cheaper by the day but dearer by the grate’, Ashton replied that ‘So far as I have seen, the labour is quite as low in England, or lower, than in any part of the continent’ (1st Report, QQ309-10). See also QQ2473, 2565-2566.

could earn twice as much as French *terrassiers*, corroborates. It was often claimed that superior strength allowed them to work harder, using ‘tools that modern art has suggested, and which none but the most expert could wield’, while their French counterparts, subsisting on an inferior diet¹³, made do with wooden implements. And so ‘*un atelier anglais, à nombre égal remuait plus du double de mètres cubes de terre qu’un atelier français*’ (cited in Drummond, 2013, pp. 61, 63; see too Nougarede de Fayet, 1847, vol. II, pp. 126-7).

There is ongoing debate about how ‘high’ English wages mattered for the Industrial Revolution. The new conventional wisdom is that the competitive disadvantage resulting from higher wages led English industrialists to introduce labour-saving techniques (Allen, 2009). But what if English workers were paid more simply because their productivity gave them an edge over workers in France and further afield, a claim that can be traced back at least as far as Arthur Young? He noted that ‘labour is generally in reality the cheapest where nominally it is the dearest’ (Young, 1929, p. 311).¹⁴ Actually, the lower productivity of workers in French agriculture went a long way toward explaining why their wages lagged behind and, indeed, the marked regional variation in time rates in French agriculture in the mid-nineteenth century was in part a product of

¹³ This prompts the question of why French entrepreneurs didn’t feed their workers better. Here the problem may be that the strength and stamina of British workers was formed when they were children and adolescents. I am grateful to Peter Solar for this point.

¹⁴ On a more flippant note Samuel Johnson, comparing the three years in which he contracted to complete his *Dictionary* to the forty years it took the *Académie Française*’s forty scholars to complete theirs, boasted: “This is the proportion. Let me see; forty times forty is sixteen hundred. As three to sixteen hundred, so is the proportion of an Englishman to a Frenchman.” I grateful to Deirdre McCloskey for this.

the variation in how long it took workers to harvest a hectare of wheat or barley (Kelly, Mokyr, and Ó Gráda, 2014).

In sum, English workers were paid more because they were worth more. English wages were high, but English labour was not dear.

4 *If You Persist ...*

Benedict Anderson's *Imagined Communities* (1983) and Eric Hobsbawm and Terence Ranger's *Invention of Tradition* (1983) remind us that various phenomena usually associated with the distant past—ranging from nations to landscape features and from Scottish tartans to traditional music—often have their origins in relatively recent times. But in economic history the trend has been in the opposite direction, whereby we are told that recent events and outcomes can have ‘deep origins’ in the dim distant past. This section returns to the persistence literature already encountered above, which links present-day outcomes to events in the past, stretching back for centuries and even millennia.

Back in 1966, Fogel claimed that the impact of cliometrics in the U.S. was mainly due to ‘the novelty of its substantive findings’. The same could be said of the mushrooming literature on ‘persistence’, or on the long-lasting impact of institutions on economic activity. That literature can be traced back to La Porta *et al.* (1997) on legal traditions, Engerman and Sokoloff (1997) on the long-term impact of slavery and other forms of forced labour and, most importantly, to Acemoglu *et al.* (2001) on the impact of European colonization on economic development. By now the persistence approach has generated an enormous literature, much of it in the top economics journals. It has also spawned a 3-volume summary of

that literature by Michalopoulos and Papaioannou (2017); and a programme at the University of Munich aimed at ‘arriving at quantitatively resilient insights into the significance of historical events for present economic development processes and their mechanisms with the assistance of new, mostly quasi-experimental identification methods’.¹⁵

The idea that the regional spread of pogroms across Germany around the time of the Black Death, over six centuries ago, foretold the rise of and support for the Nazi party in the 1920s and 1930s, or that the Spanish Inquisition casts its shadow over attitudes in Spain today are intriguing ones. More farfetched, perhaps, but equally arresting is the possibility that proximity to trade networks fourteen centuries ago predicts adherence to Islam across countries and ethnic groups in the Old World today. An influential paper by Alberto Alesina, Paola Giuliano, and Nathan Nunn argues for a link between the diffusion of the plough two millennia ago and the status of women today (Voigtländer and Voth, 2012; Drelichman *et al.*, 2019; Michalopoulos *et al.*, 2017; Alesina *et al.*, 2011).

All intriguing possibilities, but how is one to know? Historians have been suspicious of such juxtapositions, which they would regard as links that can never be proven, and therefore essentially beyond history. They would see ‘deep origins’ or ‘deeply rooted factors’ as synonyms for links lacking any empirical foundation in historical research. Yet there is a burgeoning literature which claims to prove that these links matter, not with new historical evidence, but with econometric modelling. The

¹⁵ ‘Mechanisms of persistence in economic history’ [https://www.en.cas.uni-muenchen.de/research_focus/finished/persistence_history/index.html].

literature's appeal springs from its use of cutting-edge estimation techniques to buttress quirky and clever findings. That is what gets it into the top economics journals. Its findings are catchy, inviting headline summaries such as 'The hidden histories that shape the way we live now' (*The Financial Times*, 6 March 2010), 'Antisemitism: How the Origins of History's Oldest Hatred Still Hold Sway Today' (*The Independent*, 24 March 2018), or 'The Root of Inequality? It's Down to Whether you Ploughed or Hoed...' (*The Observer*, 31 July 2011). Worries about the lack of hard historical evidence are banished by huge t -statistics. The persistence literature's most famous contributions have had a significant impact, judging by citations. In this race Acemoglu *et al.* (2001) lead by a proverbial mile with twelve thousand citations on Google Scholar by May 2019, but several persistence papers have been cited over a thousand times on Google Scholar.

The notion, which the persistence literature epitomizes, that the past pervades the present and constrains and conditions how we react and behave today, is disquieting. It amounts to a cliometric form of predestination: much of what we do and what we choose is heavily determined by factors beyond human agency. There is the implication that the lack of trust, or the subservient status of women, or racism have become part of our DNA, hidden perhaps but ready to re-emerge 'epigenetically' as in Germany in the 1920s, or whenever.

So it may come as a relief to find that all is not well with 'persistence'. At first sight the models look good: they nearly always produce astoundingly high t -statistics. The trouble is, as Morgan Kelly (2019) has shown, that the underlying regressions are spatial regressions and that spatial data tend to be auto-correlated, just as time-series data

tend to be serially correlated. The standard Moran test statistic, analogous to the Durbin-Watson in time-series, suggests that the degree of spatial autocorrelation between dependent and explanatory variables in persistence papers tends to be very high. That means that the estimated significance levels are much higher than the true ones. In all but five of the twenty-seven of the best-known persistence papers, all published in Top 4 economics journals, closely scrutinized by Kelly the Moran statistic is large, and usually huge, producing empirical p values that are several orders of magnitude different from the nominal ones reported. The outcome is reminiscent of Granger and Newbold (1974) on spurious regressions in time series. Kelly's critique, which is purely statistical and does not engage with the 'history' (or lack of it)¹⁶ in the papers comes at a time when persistence papers are being produced in such quantity that the novelty is gone. And the release of the paper created quite a storm; within a few days it had over ten thousand reads on Researchgate. Whether the Emperor is stark naked remains to be seen but, for sure, he appears far less well-dressed than previously.

Fogel's old sparring partner, the business historian Fritz Redlich, lost the argument about counterfactuals at the dawn of cliometrics. But his claim that much econometric history is based on unverifiable hypothetical models and that the new work produces not history but 'quasi-history' rings resoundingly true half a century later when it comes to the persistence literature.

¹⁶ However, for detailed historical critiques of two of the most-cited persistence studies see Albouy (2012) and Arroyo Abad and Maurer (2019).

And here is something else worth pondering. At a time when the cliometric elite is focusing more and more on Top 5 economics journals rather than on field journals, it is good to note that no economic history journal, as far as I can see, has so far published one of these persistence papers. Is that because no referee sympathetic to the way historians do economic history would allow them through? Perhaps this is something those of us who value the history element should cheer?

5 *De Me Fabula Narratur:*

Mulling over this invited lecture prompted thoughts about some of my own work and on the deductive/inductive distinction mentioned above. And it turns out that often it was the data or the story that came first, not the idea. For example, a tip-off in the 1990s about the then newly-discovered archives of the Emigrant Industrial Savings Bank in New York, which had been founded in 1850 by Irish philanthropists to help their poorer countrymen, led first to some analysis of savings behavior and immigrant acculturation in the antebellum era. But greater familiarity with the data and its context led to an interest in panics and, in particular—because it seemed that the data could answer the question—‘who panics during panics?’ That led to enjoyable and productive collaborations with the economist Morgan Kelly (the first of many) and Eugene White. The point is that the question would never have arisen but for the earlier exploratory work. And a recent return to work on the Emigrant Savings Bank, building on a more comprehensive database compiled by the historian Tyler Anbinder, has resulted in publications focusing on migration as a coping mechanism and on the economic status

of Irish famine emigrants in New York.¹⁷

Let me develop the point with some reflections on my research with Kelly. By now I have written more papers with him than with anybody else. Here too, sometimes the idea or the hypothesis came first, but more often than not the story or the data came first and prompted the question. I remember about a decade ago showing Morgan a scatter plot I had prepared for a class depicting Dutch summer and winter weather over a millennium and naively remarking, ‘where is the Little Ice Age?’ That led to Morgan’s discovery that lots of different weather series generated spurious cycles when converted to moving averages: the so-called Slutsky effect. And further work culminated in two papers, one more ‘historical’, the other highly quantitative, which cast some doubt on the importance and, indeed, the very existence of the Little Ice Age (Kelly and Ó Gráda, 2013; 2014b). The former gets cited a bit, the latter, much more heavy duty and really Morgan’s work, hardly at all. Perhaps Morgan’s celebrity in the wake of ‘The Errors of Persistence’ will draw attention to this sleeper!

Our work on shipping speeds in the era of sail had a similar starting point. Morgan came across a fabulous series of charts produced in connection with *CLIWOC*, an EU-funded project on historical climatological research.¹⁸ We reckoned that the data could be used to calculate ship speeds directly—rather than indirectly as heretofore. And that culminated eventually in a paper in the *Economic History Review*—and serendipitously—in another on safety at sea, with Peter M. Solar. The

¹⁷ Ó Gráda, 2000; Kelly and Ó Gráda, 2000; Ó Gráda and White, 2003; Ó Gráda, 2019; Anbinder, Ó Gráda and Wegge, 2019.

¹⁸ The data are available at: <http://webs.ucm.es/info/cliwoc/>

first showed that what is often still regarded as the stagnant technology of the sailing ship generated modest increases in speed in the decades before the advent of steam and the second shows that, contrary to contemporary perceptions, long distance travel by sea was becoming safer. Both of these papers chimed with the notion that technological change in England preceded the Industrial Revolution and was more pervasive than credited in the literature (Kelly and Ó Gráda, 2019; Kelly, Ó Gráda, and Solar, 2019).

Those papers began with a database. However, another paper making that same point, began with an idea. While reading a paper by Charles Foster and Eric Jones (2011), Morgan came across this: ‘Adam Smith noted that a watch costing £20 in the mid-seventeenth century was 95 per cent cheaper by 1776 (i.e. cost only £1 in 1776) and was better made too. There is only one patent and this does not seem to have affected horology.’ But Smith offered no data other than the £20 and the £1. So we set out to find some and found what we wanted in the Old Bailey Online database of London court trials. These included thousands of observations controlling for date and watch quality. And we concluded that Smith was broadly right.¹⁹

Morgan and I also wrote a set of papers on medieval and early modern English demography. The first, which sought to measure the short-run response of mortality to harvest shocks in the Middle Ages, began with the discovery of a database from the massive Winchester estate in the south of England, which noted the fines imposed on serfs when they took over the holdings of a parent or near relative. Of course

¹⁹ The data are available at: <https://www.oldbaileyonline.org/>.

such data are a second best to what we don't have: direct measures in the form of parish register or death count data. But we found that variations in deaths were strongly affected by food prices.

That led to a curiosity about the preventive check in the middle ages, but on that occasion the question preceded the data. To construct time series on medieval marriages we used the annual number of *merchets*—fines paid by an unfree peasant for the lord's permission for a daughter to marry—from a range of local sources in the pre-Black Death era. And we found a strong connection between changes in wheat prices and subsequent changes in the number of *merchets* paid, but with the twist that for less wealthy tenants, higher wheat prices deter marriages; whereas for larger *merchets* the relationship was the reverse. So a pair of papers on the same topic with different origins; one serendipitous, the other prompted by Malthusian economics (Kelly and Ó Gráda, 2012; 2014a).

In other work with colleagues in Ireland, the data also preceded the question. Alan Fernihough, now of Queens University Belfast, used individual-level data in the 1901 and 1911 Irish population censuses in his University College Dublin PhD dissertation to estimate, *inter alia*, the child quality-quantity trade-off in early twentieth-century Ireland (Fernihough, 2017). Fernihough and I chatted about what other questions might be answered by the data that could not be answered otherwise. Joining forces with a friend and ex-colleague, the late Brendan Walsh, we came up with the idea of investigating interfaith (i.e. Catholic-Protestant) marriages in Ireland roughly a century ago. Such mixed marriages were disapproved of by all faith communities but they still occurred. A limitation was that the census data allowed us to analyze only those

where both parties clung to their original faith. Then we were able to describe not only temporal and geographical patterns but also to address issues such as what determined the religion of the children and whether marrying out meant marrying up or down. Another example is the database on medieval cereal yields produced by another old friend Bruce Campbell, now professor emeritus at Queens University Belfast. Campbell and I came up with a list of issues on which the database might shed light, including the frequency of famines in medieval and early modern England, a reappraisal of Gregory King's Law, and the attenuation of variations in harvest yields over time. A study of the demography of plague in London between the 1560s and the 1660s grew out of a huge database on London burials compiled by fellow Dubliner Neil Cummins. Another effort began with a map in André-Michel Guerry's *Essai sur la Statistique Morale de la France* of the regional origins of prostitutes in early nineteenth-century France and ended with a paper employing a gravity model to measure the changing impact of distance in pre-railway France.²⁰

Much of my research over the years has been single-authored. Would it have been better done with co-authors? I can think of cases where that would certainly be true, but the answer is more complicated than that. For the most part the solo and joint efforts are orthogonal. My books, idiosyncracies and all, stand as solo efforts, but the papers I described above would not have been written without co-authors. And the same goes for earlier work with David Dickson, Tim Guinnane, Joel Mokyr, Kevin O'Rourke, the late Brendan Walsh, and others and, with

²⁰ Fernihough, Walsh, and Ó Gráda, 2015; Campbell and Ó Gráda, 2011; Cummins, Kelly, and Ó Gráda, 2016; Kelly and Ó Gráda, 2019.

Italy in mind, joint work with Guido Alfani, Matteo Gomellini, and Giovanni Vecchi.²¹ Comparative advantage usually looms large. All have added immeasurably to the enjoyment and the quality of what I do. It helps too that they are all deeply interested in the past for its own sake.

6 *Conclusion:*

Economic history has been talking to itself and about itself for a over half a century now, insecure about its status, worrying about its relevance, fearful for its future. Whence the torrent of articles all the way from Douglass North's 'The State of Economic History' in 1965 to the discordant state-of-the-discipline statements from Bob Margo and Stefano Fenoaltea. No other sub-discipline of economics has tried so hard to be loved as economic history. It has been in therapy for a long time but there is no sign of a cure. That is probably because from an academic perspective, its problem is existential: economic history is an inherently interdisciplinary field, like geography, demography, and the history of economic thought, difficult to straitjacket into departments of history or economics.

Not all is gloom and doom, however. The market for economic history may be too narrow to support separate academic departments (with the significant exceptions of the London School of Economics and Lund proving the rule), but it is big enough to support four or five decent journals, international conferences, and annual events such as that of the Associazione per la Storia Economica in Modena in 2019 and regular

²¹ Alfani and Ó Gráda 2017; Gomelli and Ó Gráda, 2011; 2019; Gomelli, Ó Gráda, and Vecchi, 2017.

seminars in many places. The quality of most of what is published in its top journals remains very high. It still attracts talented scholars. The controversies it provokes and the insights it provides touch on issues ranging from economic inequality to climate change; from the role of institutions to that of culture; from the economic costs of war to the functioning of markets; from the causes and consequences of migration to those of hunger and famines; from changing attitudes to work and leisure to the roots of economic growth; and much more. Economic history is therefore unlikely to suffer the fate of its disciplinary cousin, the history of economic thought.²²

So much for the relevance of our discipline. But a remark in that inaugural address by Ashton seventy years ago was prescient and is a good one to conclude on. Thinking, perhaps, of economists in his own day who invoked a version of the past to support tariff protection or inflation as solutions to unemployment, he noted that ‘The interest of the economist in the past arises, as often as not, out of a wish to test his conclusions in a series of different environments.’ But he added the sensible rider that ‘the interest of the [economic] historian is wider than that’ (Ashton, 1946, p. 93). Let us try to keep it that way.

²² The current state of the history of economic thought is much more parlous. According to Milton Friedman, when in 1969 George Stigler published his well-known ‘Does the Past Have Useful Economics’, it was ‘in the doldrums’. Three decades later Friedman credited Stigler, a close friend, with keeping the subject alive and ‘enhancing its attractiveness’, so much so that ‘by the end of his career, the field was flourishing’ (Stigler, 1969; Friedman, 1999, p. 10). Friedman greatly exaggerated—twice. But nobody, alas, would claim that the history of economic thought is flourishing nowadays. In the United States it is now virtually moribund as an academic field.

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