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Malthusian progress in pre-industrial England

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Jacob L. Weisdorf, *Malthusian progress*, Lund studies in economic history 56 (Lund: Lund University 2011). 179 p.

Jacob Weisdorf's thesis is an impressive piece of work. It consists of eight stand-alone chapters, all co-authored and all (bar one) published in field journals. While each essay is self-contained – there are virtually no cross-references – all except one relate to issues linked to the interaction between population, economic growth and living standards in pre-industrial England. They are distinctive contributions to an active research field. There is much to admire – and there are also things to argue about. While it is unusual and admirable to have published so much of the dissertation's content in advance, the *imprimatur* of journal referees and editors does not entail infallibility.

The chapters in the dissertation are mostly clever, short, one-idea papers that speak to the moment, as reflected in the bibliography. In their style, they are more akin to a *vin nouveau* than a ten-year malt.

At first glance the title of both the dissertation and of chapter 1, "Malthusian progress", seems like an oxymoron, and one that T. R. Malthus himself would have trouble accepting. But Weisdorf is not concerned with the history of economic thought: what is meant here is a scenario in which the standard positive and preventive checks are present, but in which appropriate shifts in the birth and death schedules are compatible with sustained gains in the equilibrium wage. Some mild productivity change is also envisaged. Is such a scenario valid for pre-industrial England? Although some historians would deny it – notably Gregory Clark of *Farewell to alms* fame – Weisdorf sides with those economic historians, like Stephen Broadberry and Karl-Gunnar Persson, who argue for slow progress.

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Chapter 2 (co-authored with Bob Allen) describes one route to higher living standards, the so-called Industrious Revolution, whereby from the fifteenth century on most succeeding generations of English urban workers (though, by this reckoning, not English farm labourers) worked more days per year than their predecessors, and so increased purchasing power, even though real wages per day worked did not necessarily rise.

The chapter's outcome is derived from comparing the number of days it would have taken to earn enough money to pay for a given basket of goods and services with estimates of the actual number of days worked. The result is interesting, and recalls well-known works (not cited) on the behavior of New York taxi-drivers in the 1990s.¹ However, the case is not entirely proven. For one thing, the allowance for housing in the hypothetical basket seems far too low, given what is known about lodging costs and money wages. Second, its evidential base for days worked in the early modern period is very thin. Third, the eminent medievalist John Hatcher has recently vigorously criticized the real wage series that underpins the Allen-Weisdorf outcome; and if Hatcher's suggested revisions are correct, the reality is more complex than described here.²

Chapter 3 (co-authored with Paul Sharp) uses the same approach to assess living conditions in London and Paris on the eve of the French Revolution. It compares the number of days' work it would have taken Parisian and London workers to buy a modest basket of goods and services daily. A mild caveat is that its use of an English basket biases the outcome against the French for index number reasons. Still, its finding that life was much harder for Parisian workers is plausible, although hardly novel. The authors might have marshaled a great deal of other evidence in order to buttress their case: what is known about labour productivity, life expectancy, literacy, height, and marital fertility all corroborate. Be that as it may, political historians, even Marxist ones, would balk at the rather link made between harsh living conditions and the outbreak of the French Revolution. Surely the sense of injustice born of misery was one ingredient among several.

Weisdorf and Sharp note that not merely were wage levels lower in France; they were also much more variable. This could have been important, although the provenance of the underlying data (wholesale versus retail, market-determined versus institutional) needs greater scrutiny.

1. C. Camerer, L. Babcock, G. Loewenstein & R. Thaler, "Labor supply of New York City cabdrivers: one day at a time", *Quarterly journal of economics* 112:2 (1997), pp. 407-441; Henry S. Farber, "Reference-dependent preferences and labor supply: the case of New York City taxi drivers", *American economic review* 98:3 (2008), pp. 1069-1082.

2. John Hatcher, "Unreal wages: long-run living standards and the 'Golden Age' of the fifteenth century", in B. Dodds & C. Liddy (eds) *Commercial activity, markets and entrepreneurs in the middle ages* (Woodbridge, Suffolk 2011) pp. 1-24.

Chapter 4 (co-authored with Paul Sharp and Holger Strulik) begins as a non-controversial theoretical contribution to how income might evolve under a regime of "Malthusian progress", but ends in an attempted estimation of the model that is interesting but that, in the end, remains unpersuasive. The reduced form of the model comes to $WAGE = \sigma + \delta DEATH + \gamma INDUSTRY$, where $\delta = \gamma = 1$. This is estimated by running an estimate of the wage level on a mortality index and a productivity index, but the data proxying both explanatory variables are of highly dubious quality. Weisdorf's mortality proxy relies on the assumption that "child mortality is more sensitive to economic fluctuations than is adult mortality..." (p. 73), but this runs counter to everything we know from the historic-demographic literature.³ Second, a glance at the productivity series used (p. 74) suggests that it is very problematic. It turns out to be based on various wage series, so the estimated regression in effect is running wages on wages. In several other places (e.g. pp. 113, 157f.) the dissertation shows an economic historian's reverence for quality data, but not here.

Chapter 5 is the only purely theoretical essay in the dissertation and also the only one that is somewhat divorced from its over-riding themes. It proposes an interesting lock-in mechanism to explain the historically irreversible shift from hunting-gathering to farming that constituted the Neolithic revolution. The same model might account for irreversibility in the shift in historical times from pastoral to tillage agriculture.

Chapters 6–8 rely on a common database. This is the 26-parish dataset compiled by the Cambridge Group for the Study of Population and Social Structure (CAMPOP) between the 1960s and the 1990s, which uses the family reconstitution methodology pioneered by the French demographer Louis Henry. The reliability of the results depends on the quality of the underlying data, and Weisdorf claims that neither under-registration nor migration is a serious problem for his analysis. While a more explicit and sterner defense of the data's quality and representativeness would not have gone amiss, it seems likely that the under-registration of births at least was less serious than previously thought. One worries a bit more about unregistered deaths, particularly in times of economic stress. These chapters offer ingenious and topical applications of a dataset never fully exploited by its creators, and are thus worthwhile and important.

Chapter 6 (co-authored with Nina Boberg-Fazlik and the ubiquitous Paul Sharp) follows the lead of economic historian Gregory Clark in studying marital fertility and reproductive success by socioeconomic group c. 1650–1800. Its findings corroborate Clark in that they argue for the "survival of the

3. E.g. Cormac Ó Gráda, *Famine: a short history* (Princeton, N.J. 2009).

fittest”, that is the greater reproductive success of the better-off. This is an interesting and well-crafted contribution to a currently active literature. However, the reliability of the outcome depends in part at least on the assumption that live children were present in the household until age fifteen. But did children stay at home till they were fifteen? (p. 107). The English rural historian Keith Snell notes that rural apprentices were bound at the age of 14,1 years on average between 1700 and 1815.⁴ Since apprentices were more likely to be poor, this means that the estimate of reproductive success by socio-economic group is biased towards the rich. How much this mattered depends in turn on the variance of the mean age and the relative importance of apprenticeship.

Chapter 7 (co-authored with Marc Klemp) exploits data on the demographic crisis of the later 1720s to test the fetal origins hypothesis, that is the claim that malnutrition and health insults in utero can have adverse consequences for health in adulthood. Here the dependent variable is the expected life span at various ages of those born during, before, and after 1727–1730. The paper differentiates between working-class and other households, and between those living in the north and south of England. It finds that the costs for the famine cohort were very high, and highest (12 years) for northerners from a working-class background.

An interesting conundrum deserving further attention is the extent to which the mortality in 1727–1730 was due more to an outbreak of influenza than the sheer shortage of food. Significantly, the peak in cereal prices in the late 1720s was rather modest, which may explain why this crisis has tended to hide below the econometric radar. There is some contemporary evidence supporting the notion of an influenza epidemic. The symptoms described by Dublin physician John Rutty – a cough, soreness of the breast, and some pain of the head and back, and a slight fever – bespeak flu, as does the account from the parish of Deane (in Lancashire) that most victims died “of agues, pluraisy, etc, tho a fever came ye first [and] in some respects ye disorder resembled ye Plague”.

Selection bias is the original sin of much research on the fetal origins hypothesis.⁵ The trouble is that parents who gave birth during famines were likely to be systematically different to parents giving birth before and after. During famines the very poorest women either suffered from famine amenorrhoea or practiced prudential restraint, while mothers who gave birth after famines were likely to be materially better off than they had been before the

4. K.D.M. Snell, *Annals of the labouring poor: social change and agrarian England 1660–1900* (Cambridge, UK 1985), pp. 236, 323–31.

5. Cf. Carlos Bozzoli, Angus Deaton & Climent Quintana-Domeque, “Adult height and childhood disease”, *Demography* 46:4 (2009), pp. 647–669.

disaster. Thus one is almost never comparing like with like. Weisdorf and Klemp do not address this difficulty adequately, although it should be noted too in support of their case that selection almost always biases the result against the finding evidence of adverse consequences.

The demographic impact reported here – a twelve-year “loss” to the children of manual workers in the Midlands – is truly striking. There is nothing like this in the literature. A recent study of the Dutch hungerwinter of 1944–1945 reported a life expectancy loss of four months for those aged fifty years, while a recent study of the Chinese Great Leap famine by Shige Song (2011) finds no effect.⁶ What makes the outcome in this chapter even more astounding – and therefore worth further scrutiny – is that this was a very small famine indeed by world-historical standards.

Chapter 8 (also co-authored with Marc Klemp) exploits the 26-parish dataset to analyze the quality–quantity trade off in children that features prominently in the analysis of economists Gary Becker and Oded Galor.⁷ This promising chapter again was prompted by and engages with a lively ongoing literature. Its motivation is Galor’s emphasis on the importance of human capital for the Industrial Revolution. Its measure of human capital is literacy, which is fair enough, although one must bear in mind the objection that much of Western Europe (including England) may have been oversupplied with human capital by this definition. The Portuguese economic historian Jaime Reis (2005) has suggested that a great deal of literacy was surplus to “economic” requirements and represented consumption rather than investment.⁸

Overall, the dissertation shows an ability to produce a brand of economic history that casts new light on a range of issues, both current and historical, and that is likely to be read by other practitioners. Jacob Weisdorf (and his co-authors) have produced a work that is coherent, and that shows the necessary knack for asking pertinent questions. Weisdorf has also amply demonstrated the skills and intuitions and imagination necessary to provide interesting and plausible, if not always totally convincing or definitive, answers to them.

6. Shige Song, “Assessing the intergenerational effect of prenatal exposure to acute malnutrition on infant mortality: evidence from the 1959–1961 Great Leap Forward famine in China”, unpublished paper presented at the 2011 Population Association of America Meeting.

7. Gary Becker & H. Gregg Lewis, “On the interaction between the quantity and quality of Children”, *Journal of political economy* 81 (1973), pp. S279–S288; Oded Galor & Omer Moav, “Natural selection and the origin of economic growth”, *Quarterly journal of economics* 117 (2002) pp. 1133–1191.

8. Jaime Reis, “Economic growth, human capital, and consumption in western Europe before 1800”, in Robert C. Allen, Tommy Bengtsson & Martin Dribe (eds) *Living standards in the past: new perspectives on well-being in Asia and Europe* (Oxford 2005) pp. 195–225.