



The Maddison Project

Book review of Broadberry, Campbell, Klein,
Overton, and van Leeuwen, *British Economic
Growth, 1270-1870*

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British Economic Growth, 1270-1870, by Stephen Broadberry, Bruce M. S. Campbell, Alexander Klein, Mark Overton, Bas van Leeuwen. Cambridge University Press (461 pages)

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Introduction

This book is the culmination of an outstanding effort by the authors (henceforth BCKOvL) to reconstruct British historical national accounts, chiefly Gross Domestic Product (GDP), from the output side, over the very long-run.¹ By dividing this measure by population estimates, they arrive at a measure of how “wealthy” the average British inhabitant was at different moments in the past.² Such a measure permits, in principle, comparisons not only across time but also with developing countries today. The real per capita GDP estimates which are the main outcome of the first half of the book illustrate the enormous improvement in real income and consequently standards of living which have taken place over time, but that this was the case was already well-known, at least among economic historians. The most original picture that comes out is that the emergence of sustained growth was much more gradual than it was previously thought, and that it started around the mid-seventeenth century. The data also emphasizes the historical nonlinearity of the long-term growth process.

Of course, real per capita GDP is certainly not a measure without problems if interpreted as a measure of standards of living for the “typical” (e.g. the median) individual in the past, among other reasons because it ignores distributional issues and changes in the quality and variety of goods over time are hard to control for. Furthermore, by comparison with modern economies, measuring GDP for past economies also evidently presents special difficulties such as the fact that non-market production was comparatively more important. These are caveats to keep in mind, but do not serve as excuses not to engage in quantitative measurement. As long as the underlying assumptions are clear, having numbers is usually best than relying on narrative or circumstantial evidence alone. Furthermore, England’s special position as the first country to industrialize certainly warrants it special attention, despite the fact that truly understanding the process of modern economic growth requires a comparative approach where the experience of countries that failed to develop is taken into consideration.

The second half of the book goes beyond this task and provides a magisterial overview of what we currently know about consumption practices, distribution, labor productivity, and compar-

¹ Their estimates in fact correspond to English GDP until 1700 and British GDP afterwards.

² To be precise, wealth is a stock, while GDP measures income, hence it is a flow. However, because the latter largely reflects past accumulation, it is usually safe to say that countries with higher GDP per capita are richer.

tive income levels relative to other countries in this period. It is truly difficult to do justice to this landmark publication in a short review.

Methodologies for the reconstruction of historical national accounts

Is it even possible to reconstruct GDP for economies so far back in the past, one might ask? The fact that the notion of GDP itself is a 20th century concept does not in itself present difficulties, as it is not hard to agree that different economies in the past produced more output at the national level than others. Other anachronisms may be more difficult to deal with. For one, there is the matter of choice of borders – using constant (usually, modern) borders is the most common practice in the field of economic history, and one which the authors follow when possible, though they are in fact forced to switch from England to Britain in 1700. But finding the right data is always a challenge in economic history, which makes only more laudable the impressive collection effort undertaken here.

This book is not the first attempt at estimating British GDP over the long run. Work by Deane and Cole (1967) and by other economic historians had provided a few benchmarks for part of the early modern period, while Gregory Clark (2010) has decadal data from 1209 onwards. However, given its construction method, the BCKOvL data is undeniably the best, and it significantly changes our picture about the long-term performance of the British economy. Nonetheless, the authors often rely on Clark's extensive price and wage data, which emphasizes the fact that apart from the methodological and interpretative disagreements, this research is the result of a collective effort for which Clark's commendable data-collection efforts has played an important part.

Nonetheless, the main reasons why the BCKOvL GDP estimates are superior to those of Clark are as follows. Clark's method of demand estimation requires the usage of real wages as proxy for (part of) households' income. Real wages are in turn constructed by dividing the nominal wage by a cost-of-living index, but the nominal wage more commonly available corresponds to the day wage. This procedure, while useful, suffers from the major disadvantage that it is difficult to estimate what annual income a typical household might have received at different points in time, because it is difficult to know how many days (or more generally, hours), people were working per year. For any given distribution of prices, a carpenter working 150 days per year would have been able to afford 25% less goods and services than one earning the same day wage but working 200 days per year (assuming that he would buy different total volumes of the same goods but on constant proportions between different goods). Clark assumes a constant number of 300 days (50 weeks), which surely overestimates the late medieval working time and accordingly underestimates early modern growth. BCKOvL provide a competent overview of the issues at stake in chapter 6. They might have additionally added that Clark's land rents series, another component of income affecting demand, is equally unrepresentative of agricultural rents for most of the early modern period, and especially before 1750, being substantially upwardly biased as a consequence of the inclusion of "numerous urban fringe properties ... together with housing" (Ormrod 2013).

BCKOvL's production-side estimates do not suffer from these shortcomings. For all these reasons, the macroeconomic data presented in this book is undeniably more representative than that of Clark. However, the authors are in fact forced to retreat to the demand-based estimation of agricultural output for the "statistical Dark Age" epoch of 1492-1553, when manorial data disappears while probate inventory or modern farm accounts data is not yet available in sufficient quantity. The authors have been careful to compare the model's prediction for that period with those immediately before and after as an external-validity cross-check, and show that it performs remarkably well (p. 123). This is reassuring both with respect to the precision of the available estimates for other countries for which this method has been used, and to the possibility of reduced representativeness of surviving manorial records of the second half of the fifteenth century, and probate inventories of the first half of the sixteenth.

At the same time, it is intriguing that the estimates are so close in light of the fact that the demand function estimation method uses real wages as a proxy for income. Given the authors' well-justified continued insistence elsewhere in the book that the working time increased during the early modern period, it is particularly puzzling that the model underestimates income levels for the period after 1650, a matter which would have deserved to be better explained in the book.

The data

In addition to their own data, the authors rely on a tremendous wealth of secondary source information produced by generations of economic historians, the equivalent of which is simply not available for other continental countries, with the notable exception of the Netherlands.

For agriculture, the authors use manorial accounts data for the medieval period, probate and Church commissioners' inventories for the early modern period, and modern farm accounts for the period from 1720 onwards. This often builds on previous work by the authors themselves (Campbell for the medieval period, Overton for the early modern period). As for industry, an index is built using data for wool and woolen cloth, iron, and tin production; output of leather and food processing is derived indirectly using data from the main inputs, available from the agricultural sector reconstruction. For the construction sector, it is also the case that while for cathedral-building detailed data is available, housebuilding needs to be derived indirectly from data on urbanization and total population. Similarly, book production was inferred from titles available at the British Library. A previous industrial index was available from Crafts and Harley (1992) for the period after 1700, which the authors rely on and improve.

Finally, with regards to the service sector, the Deane and Cole (1967) methodology is followed, with some improvements. This sector is divided into several subsectors. The first is commerce, both domestic – relying on the market size of the agricultural and industrial outputs they previously calculated, adjusted for changes in percentage marketed over time – and international, relying on tax records. Commerce also included freight transport, which the authors estimate using merchant shipping tonnage data, and financial services (for which the velocity of money is used). Finally, housing and domestic services were estimated indirectly by assuming propor-

tionality with population, and government activity was estimated using government revenue data from exchequer accounts.

In all of the data for both the industry and service sectors, the authors rely on the work of previous scholars. Most of the time this works, though at one time a questionable choice was made: the velocity of money, the inverse of which is used to estimate the size of financial intermediation, is taken from (a conference version of) Mayhew (2013). This implied a considerable number of interpolations, but most importantly, Mayhew himself drew heavily on the BCKOvL data when he produced velocity estimates. So because of this numerator there is here an important element of circularity. (Mayhew's 1750 estimate also overlooks the fact that after 1700 the BCKOvL data corresponds to Britain rather than England alone). It is surely the case that this did not affect the results very much, however.

Results

What are the main new conclusions that result from this impressive exercise? It was already known among economic historians that following the Black Death and related plagues in the fourteenth century, the English population did not respond as it did elsewhere in the continent, failing to grow sufficiently (hence keeping incomes high), which contradicts what a simple-minded Malthusian model would suggest. This is confirmed by the new data presented here.

BCKOvL persuasively show that despite stagnation or even some slight decline (in per capita terms) during the sixteenth century, the English economy never went back to the pre-Black Death income levels, and that it indeed grew substantially during the early modern period, especially after 1650. This position stands in sharp contrast to that of Clark (2007), who argues that the English economy was trapped at an approximately constant, (non-physiological) "subsistence" level until it finally broke away already during the nineteenth century.

The fundamental disagreement between BCKOvL and Clark can be summarized by saying that the latter authors show that the English economy grew substantially, and underwent considerable structural change, between the 1380s and the early nineteenth century³, while Clark argues that they did not. The classic view of the industrial revolution, as having been a fast "take-off" occurring between 1760 and the early nineteenth century, had already been questioned by evidence showing that fast technological change had at that time been limited to a small set of industries. Consequently, growth and structural change were slower than previously thought during the 1760-1830 period (Crafts and Harley 1992). As we have reasonably secure estimates for the nineteenth century, this in turn implies that the early modern economy must have been richer than was previously thought. But was this higher level a result of growth during the early modern period (as argued by BCKOvL), or was the economy already at that level during post-Black Death medieval period (as argued by Clark)? It must be said from the outset that Clark's view is contradicted by most narrative evidence we have available, namely that which has been put forward by historians of material culture (Clark dismisses this sort of evidence as

³ See also Shaw-Taylor and Wrigley (2014)

the outcome of changing representativeness of surviving probate inventories over time). The BCKOvL position also represents a partial return to the once-prevailing consensus of Maddison (1999, 2003).

Nonetheless, despite BCKOvL's rather reasonable results it is striking to notice that some of their results are unexpected or even contradict well-known narratives about English history. Here is a short summary of the seven-century history of England/GB as told by their data:

1. Initial, low per capita real GDP until the Black Death
2. Discontinuous rise in incomes immediately following the Black Death, followed by some non-monotonous but positive trend p.c. real GDP growth until the last quarter of the fourteenth century
3. No per capita real GDP growth, 1380s-1650s, though with structural change after 1522
4. "Fast" growth and continued structural change, 1650s-1710s
5. Slower but positive growth under ongoing but more limited structural change, 1710s-1820s
6. Modern economic growth afterwards

Two aspects are here especially striking. First, while it is true that income per capita approximately quadrupled between 1270 and 1870, as far as this variable is concerned "not much" happened during a long period, approximately 1380 to 1650. The authors write that "From the mid-fifteenth century the changes in the structure of the economy ... and the growth of real GDP ... proceeded more or less in continuously and in tandem" (p. 203). But they are here referring to aggregate GDP – that is, was there extensive growth during this period, but no intensive (per capita) growth. Furthermore, it is unusual to think of structural change without per capita income growth, as happened during 1520s-1650s.⁴ (Nevertheless, this is possible to justify, and my feeling is that this is indeed roughly correct.)

Second, premodern growth was fastest in the period after the civil war, but preceding the Glorious Revolution and indeed real per capita growth was faster than during the "classical" period of the industrial revolution, 1760-1820. The fact that the beginning of fast growth proceeds 1688 makes the citation of North and Weingast 1989 in p. 211 seem misplaced, but more importantly opens important questions which in all fairness mostly lie beyond the scope of this text: was growth after 1650 caused by changes in institutions or fiscal capacity associated with the civil wars, as suggested, for instance by Patrick O'Brien? was the fast growth at this time mainly catch-up growth or the begging of something new altogether? could have it been sustained without the Dutch invasion?

⁴ See Broadberry et al (2013) for a detailed discussion of the measurement of structural change.

International comparisons

In one of the last chapters of the book the authors provide a set of international comparisons in Geary-Khamis “international” dollars of 1990 (pp. 374-5).⁵ Unlike using market exchange rates, in principle this allows for comparison of income levels across space and time, for instance comparing how richer (or poorer) was an “average” person living in medieval England (GK \$1090 of 1990) with one in, say, Nigeria today (GK \$1876 in 2010; Maddison project 2013). In practice, however, the devil is in the details, and these matters are more complicated than they sound. No proper PPP’s exist before the twentieth century, which means that backward projection using volume indices could easily lead to greatly compounded errors. As we move back in time, modern baskets become less and less representative, leading to severe index number problems. Hence the usage of GK dollars for economies with very different relative prices and patters of consumption easily leads to arbitrary conclusions (Prados de la Escosura 2000, Deaton and Heston 2010; see, however, Bolt et al 2015).

That this is the case is known for a long time, in theory at least, though routinely ignored in applied work, often for the lack of usable alternatives.⁶ Consequently, it would be unfair to the authors to place too much criticism at their usage of the GK international 1990 dollars method, which is standard in the literature and needed to be included. At the same time, I would have preferred a more frank and through discussion of its caveats, and in particular, it would have been useful to also show the results under the main presently available alternative, the shortcut or indirect method of Prados de la Escosura (2000), especially in light of the fact that they lead to a considerably different picture of comparative income levels for early modern Europe. Specifically, the early modern “little divergence” in incomes defended by Broadberry (2015), Van Zanden (2009), or Allen (2001), largely disappears (table 1).

⁵ Henceforth referred to as GK dollars. For a critical discussion concerning their construction method, see Prados de la Escosura (2000, pp. 3-4) and Deaton and Heston (2010).

⁶ See de Jong (2015) for a helpful summary and literature review of the issue of measuring living standard over the long run.

| | England | Holland | Germany | France | Italy | Spain | Sweden | Portugal |
|------|---------|---------|---------|--------|-------|-------|--------|----------|
| 1500 | 39 | 37 | 49 | 50 | 68 | 50 | - | 58 |
| 1550 | 39 | 37 | - | - | 64 | 54 | 35 | 30 |
| 1600 | 37 | 68 | 34 | 50 | 60 | 53 | 36 | 44 |
| 1650 | 34 | 69 | - | - | 62 | 41 | - | 51 |
| 1700 | 55 | 54 | 40 | 54 | 65 | 48 | 53 | 45 |
| 1750 | 61 | 60 | 45 | 55 | 68 | 46 | 41 | 59 |
| 1800 | 75 | 67 | 42 | 56 | 60 | 54 | 40 | 50 |
| 1850 | 100 | 79 | 61 | 78 | 66 | 64 | 52 | 46 |

Table 1. Output per capita in Europe (GB 1850=100), using the 1850 benchmarks of Prados de la Escosura (2000, p. 24), and assuming Italy’s relative level was constant 1850-1860, as in Álvarez Nogal and Prados de la Escosura (2013). England refers to England alone until 1700 and GB from that date onward, Holland refers to Holland until 1800 and the Netherlands for 1850, while Italy refers to North and Central Italy only. Data sources: For England/GB, Broadberry et al (2015), for Holland, van Zanden and van Leuween (2012), for Germany, Pfister (2011), for France, Álvarez-Nogal and Prados de la Escosura (2013, p. 23), for Italy, Malanima (2011), for Spain, Álvarez-Nogal and Prados de la Escosura (2013), for Sweden, Schön and Krantz (2012), and for Portugal, Palma and Reis (2016).

My feeling is that the method of Prados de la Escosura provides a more accurate picture of relative income levels during the early modern period than the authors’ preferred GK 1990 international dollars method, because the latter mitigates index-number problems by relying on current-price comparisons (even if derived indirectly) instead of backward projections from very remote PPP’s. As we move further away from the twentieth century benchmarks, deviations can plausibly become arbitrarily large, as it is hard to know if errors are compounding over time. In order to figure out which set of estimates is closer to the truth, we would need a more systematic collection of prices that allow us to construct less remote PPP’s to which to anchor surrounding volume indices.

Doing so is far from trivial, however, because of lack of product standardization. Even for very basic staples such as wheat ensuring constant quality is harder than it sounds, and in any case people did not really consume wheat but bread. Not only is the price for bread generally less widely available from primary sources, but quality varies more widely as well, given that it incorporates the costs of energy and ovens (capital), and can vary widely in terms of other inputs such as salt or yeast. For other goods such as clothing, controlling for quality is close to impossible over both space and time, given that sufficiently detailed information to construct hedonic price indexes using regression techniques is rarely available from historical sources.

Finally, people in different economies reacted to different environmental conditions to determine the kinds of goods were consumed in equilibrium; the Mediterranean south consumed olive oil and wine to provide for the same basic needs as were filled by butter and beer in Northwestern Europe, but under what proportions? Should we count calories, specific nutritional needs, or some other criteria? Before we hurry to a precise answer, we should take into account that neither peasants nor anyone else in those societies at any rate would have had such knowledge, even if empirical knowledge might have been argued to evolve jointly with social norms determining approximately “optimal” functionally-equivalent consumption patterns conditional on income. Until these difficulties can be surpassed, the current-price PPPs method of Prados de la Escosura remains a worthy alternative to GK international dollars of 1990, and it is worth considering what it adds to the table.

Final assessment

In economics, generations of researchers have struggled to understand the process of long-term economic growth, and in particular the reasons for the emergence of modern economic growth. Much of this work has been theoretical, but it should be evident that the “why?” cannot be answered without considering exactly the facts of “when” and “how”. This book, together with the related research of Broadberry et al (2013), represents the synthesis and culmination of the work by generations of economic historians of Britain, and a significant research effort of the authors in their own right. Together with Deane and Cole (1967), Crafts (1985), and Crafts and Harley (1992), it is one of the most seminal contributions made in the last decades toward establishing the facts that may one day permit answering that which is perhaps the most important question of all social science: why are the societies we live in so much richer than was the case in the past?

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