

REVIEW ARTICLE

A harvest gathered in: some implications of *British economic growth 1270–1870* for agricultural history*

by R. W. Hoyle

British economic growth 1270–1870 is the most important work on British economic history to have appeared in recent years or is likely to appear in the near future. Its relevance to agricultural historians may not be immediately apparent, and yet it is inevitable given the nature of the pre-industrial economy that the earlier chapters of the book should serve as a new account of English agriculture, indeed the fullest and most up-to-date account of English developments available. This account comes to striking conclusions and it is important that these are both widely known and appropriately tested in the future.

Whilst *British economic growth* puts our understanding of the British experience on a new footing, much of what it contains had been anticipated in earlier writings.¹ It offers a comprehensive account of change over 600 years, looking back to Domesday on occasion. (The date of 1270 merely reflects the commencement of the continuous series of statistical data employed by the authors.) This is a work of economic history in the tradition of national accounting whose founding father may be taken to be Angus Maddison.² (The intellectual genealogy of the book is described in its preface: no more needs to be added here.) The account of agriculture is merely a building block on the way to the larger ambition. This review is in essence a discussion of the three chapters of the book which discuss agriculture, with some reference to material drawn from elsewhere in the book. We are reading the book selectively for a purpose for which it was not intended.

* This is a selective review of parts of Stephen Broadberry, Bruce M. S. Campbell, Alexander Klein, Mark Overton and Bas van Leeuwen, *British Economic Growth, 1270–1870* [hereafter *BEcG*] (2015). An earlier version was presented by the invitation of Professor Gerard Beaur at Rural History 2017 in a session convened by him. I am grateful to a number of colleagues and the *Review's* referees for reading drafts of the paper. Bruce Campbell and Mark Overton read a late version of the review and suggested a number of corrections which I was happy to accept.

¹ It is interesting to see just how much of the argument of these chapters is anticipated in Overton's paper of 1996, 'Re-establishing the English Agricultural Revolution', *AgHR* 44 (1996), pp. 1–20.

² Maddison (1926–2010) published a series of works leading to *The contours of the world economy 1–2030 AD: essays in macro-economic history* (2007).

Not all economic historians are convinced by the approach that this work represents or find it useful. Those on the historical wing of the discipline might well argue that individual figures for this or that should be up by a little, or down by a little; but small adjustments in the underlying data will probably do nothing to change the overall story that these chapters tell, which cannot be challenged by historical (as opposed to economic) methods.

The book draws on the work of five historians, all of them good friends of the British Agricultural History Society. The first is Bruce Campbell who has increasingly taken a quantitative approach to the middle ages, and in recent works has not shrunk back from describing the whole of the British Isles at the end of the thirteenth century in quantitative terms.³ Campbell brings to the party two big datasets, the first gathered from the extents contained in the inquisitions post mortem made after the death of tenants in chief, and the other his database of crop yields drawn from manorial accounts, which in turn draws on the work of earlier historians who pioneered the collection of data on demesnes.⁴ The second is Mark Overton who developed techniques to obtain quantitative information on yields from probate inventories in the late 1970s. His work on Norfolk inventories is familiar: this discussion also draws on unpublished datasets gathered subsequently.⁵ The other historians whose data is drawn on here are John Beckett and Michael Turner who worked on eighteenth-century and later farm accounts with the aid of their research assistant, Bethanie Afton.⁶

This review falls into six main sections. Sections I to IV attempt to summarize the major conclusions of the book as they bear on agriculture. In sections V and VI we offer a commentary on aspects of *British economic growth*. In the final section we consider where British agricultural history might usefully go next.

I

We have had sound population figures from c.1540 for most of forty years: it is only now that we have equivalent figures for yields. But where the *Population history of England and Wales* was careful to publish annual figures supported by the workings which underpinned them, *BEcH* presents its results in the form of graphs which are reproduced far too small to read easily.⁷ Annual data is generally not provided in the book, and this is regrettable. In a sense this may acknowledge the insecurity of data for single years and the reluctance of the authors to allow people to make year-by-year comparisons, and yet this is what many readers will wish to do.

³ B. M. S. Campbell, 'Benchmarking medieval economic development: England, Wales, Scotland and Ireland circa 1290, *EcHR* 61 (2008), pp. 896–945; id., 'The population geography of Great Britain, c.1290: a provisional reconstruction', in C. Briggs, P. M. Kitson and S. J. Thompson, *Population, welfare and economic change in Britain, 1290–1832* (2014), pp. 43–78.

⁴ Campbell's IPM data underpinned his *English seigniorial agriculture, 1250–1450* (2000) and B. M. S. Campbell and Ken Bartley, *England on the eve of the Black Death. An atlas of lay lordship, land and*

wealth, 1300–49 (2006). For the crop yield data, www.cropyields.ac.uk/project.php.

⁵ *BEcG*, pp. 84 and 85 for the counties included, and Figure 3.01 for the numbers of inventories drawn on.

⁶ M. E. Turner, J. V. Beckett and B. Afton, *Farm production in England, 1700–1914* (2001).

⁷ The figures and tables (but apparently not the data files) can be downloaded from the Cambridge University Press website: go to the book's webpage and then follow the tab for 'resources'.

Having said that there are generally accepted annual figures for population from 1540 onwards, there are no such figures for the previous 450 years. There has been much debate over the size of the English population both at the time of Domesday and c.1300 (when population is taken to have reached its medieval peak before falling back before the Black Death). The literature on this issue is now substantial: much of it depends on putting fingers in the air, and the reading of local and often insecure materials. The most recent estimates are those published by Gregory Clark in 2007. He approached the question in two ways, by extrapolating from those few communities which had runs of figures for tenants or persons paying tithing pennies and by modelling the marginal product of labour. Between them, these two methods gave a best estimate of 4.92 million for the decade 1260–69, 5.53 million for 1300–09 and a peak of 5.98 million for 1310–19. By the decade of the Black Death the figure had dropped back significantly to 4.86 million. Clark recognized that these figures were high compared to those of other historians.⁸ In 2000 Campbell had proposed a maximum of 4.5 million people. As recently as 2008, he gave a best estimate for c.1290 of 4.0 million which he justified by saying that it was compatible with what was known about the area of England known to be under cultivation and the agricultural technology available at the time.⁹ Clark's estimates were therefore treated with some scepticism, but the objection to them was not his methodology so much as doubts about how these people could be fed. Hence the question which underpins these estimates is that of the total landed area available for cultivation, and again Clark adopted a much higher figure than had been proposed by Campbell. This allowed him to reconcile his high population estimate with a high estimate of the area under cultivation (although it must be said that the latter rested more on assertion than careful calculation).¹⁰

The new population estimates by Broadberry *et al.* are given in their Table 1.06. They settle on a Domesday population estimate of 1.71 million people: for 1290 they suggest 4.75 million. This is calculated by using the methodology pioneered by H. E. Hallam of comparing tenant numbers at Domesday with those recorded in later manorial documents. The new estimate for 1290 sits almost exactly mid-way between the small Campbell and large Clark estimates previously published, but is better founded than a simple 'splitting the difference' between these two authorities would be! Nonetheless it represents something of a change of mind by Campbell, prompted by a reassessment (to which we will turn in a moment) of the total possible cultivable area in the nineteenth century.¹¹ Calculating forwards using the number of males recorded in frankpledge lists gives a population in 1450, at the nadir of medieval population, of 1.9 million. Having established best estimates of English population, estimates are then produced of Scottish and Welsh population from 1700 onwards, to enable a succession of British population estimates to be produced.¹²

What will be equally interesting to many historians is that Broadberry *et al.* proceed to distribute population c.1086, 1290, 1377 and 1600 by county, giving both shares of population but also absolute figures and growth rates (Tables 1.08 and 1.09). (One wonders why 1524–25

⁸ Clark, 'The long march of history', pp. 118–22

⁹ Campbell, *Seigniorial agriculture*, pp. 399–400; 'Benchmarking', p. 925.

¹⁰ Clark, 'The long march of history', pp. 124–5.

¹¹ Campbell, 'Population geography', p. 51.

¹² BEcG, Section 1.6, Table 1.10: annual population figures can be found in Appendix 5.3.

was also not treated in this way given the amount of material available through the work of John Sheail and others.¹³) This allows us to see where population was growing fastest. For instance, population in Yorkshire had an annual population growth rate of 0.19 per cent between 1377 and 1600, but in the East Riding population shrank at 0.6 per cent per annum, in the North Riding it grew at 0.15 per cent per annum and in the West Riding it grew at 0.37 per cent per annum (Table 1.09). This serves as an index of industrialization.

II

Chapter 2 considers agricultural land use. It proceeds carefully, methodically, incrementally. It begins by estimating the landed area of England in the mid-nineteenth century, drawing estimates from the tithe files (1836 and later) and the earliest agricultural censuses. That said, some of the figures are hard to reconcile. The mid-nineteenth century is seen as representing a maximum. The total area of England in 1871 is 32,328,331 acres. Of this, only about three quarters (72.7 per cent) or about 24 million acres is seen as being suitable for agriculture and this is divided roughly equally between arable (including fallows and temporary grass) and permanent grass. The country's potential arable came 'at most' to 15 million acres, equivalent to 46 per cent of the national land area. An extrapolation based on the slightly earlier tithe files suggests that the 13.9 million acres or 43 per cent of the national landed area was under grain, fallows, clovers and temporary grasses.

We then work backwards from the mid-nineteenth century figure towards the end of the thirteenth century. The area under cultivation in 1290 is unlikely to have been greater than that of c.1850. But the question is posed, could it have been smaller? There are a number of factors which lead the authors to conclude that it could not have been as large. From the 13.9 million acres, they deduct 1.39 million acres which had been drained between 1290 and 1871 (Table 2.04). In addition, and following Phillips, they hold that some 4.0 or 4.5 million acres were underdrained between 1845 and 1890, with most of this being undertaken between 1840 and 1870 in the north and west of England, 'where it allowed much land to be ploughed for the first time'.¹⁴ We then have an estimated half a million acres which had been arable in 1290 but was used as grass in 1870. The calculation is therefore 13.9 million acres, minus 1.4 million acres for land drained (makes 12.5 million acres), minus 4.5 million acres for land underdrained (makes 8.0 million) plus 0.5 million acres for land down to grass in 1870, but arable at earlier dates, makes 8.5 million acres. But then we are told that 'the maximum arable acreage of 1290 is therefore unlikely to have been more than 12.5m to 13.0 million acres',¹⁵ and one wonders quite how have arrived here. One suggestion must be that the area underdrained is finally treated as potential arable in 1290 despite the comment that underdraining allowed at least

¹³ John Sheail, *The regional distribution of wealth in England as indicated in the 1524/5 lay subsidy returns* (2 vols, List and Index Society 28–9, 1998) which presents much of the raw material. See for instance an early paper of Bruce Campbell's, 'The population of early Tudor England: a re-evaluation of the 1522 muster returns and 1524 and 1525 lay subsidies', *J. Historical*

Geography 7 (1981), pp. 145–54.

¹⁴ BEcG, p. 56. A. D. M. Phillips, *The underdraining of farmland in England during the nineteenth century* (1989). Phillips gives an estimate of 4.5m acres underdrained (p. 242). On p. 65 of BEcG this is misreported as 4.0 m acres.

¹⁵ BEcG, p. 65.

some land to be ploughed for the first time. Underdrainage seems to be first introduced as a factor and then discounted as a factor, then mentioned again all in the space of a few pages. Other factors are considered but for want of certainty about how they should be estimated are finally disregarded as being self-cancelling. We return to this vexed question later, but in the end, the figure adopted by Broadberry *et al.* for the extent of arable land in 1290 is 12,772,192 acres (Table 2.07, which is rounded to 12,750,000 acres in Table 2.08) which is 39.4 per cent of the total cultivated area in the mid-nineteenth century.

The authors argue that this is very much an upper-bound estimate and find it hard to see that any more land than this could have been under cultivation in 1290. They also argue that the implication of these figures is that the area under arable cultivation in 1290 was 1.4 million acres more than that cultivated in 1801 although 1801 hasn't been mentioned before, and the scale of growth in arable acreage between 1801, the tithe returns (1836 and later) and the 1870 agricultural census is only stated two pages later (p. 73). Here it is estimated that the total arable acreage in 1801 was about 11.35 million acres which was 2.5 million acres less than in 1836, so it was not wartime conditions at the turn of the eighteenth century which produced the peak in arable cultivation, but High Farming and investment in drainage a generation later.

It is not easy to unpick these figures: the authors, perhaps realizing this, once again turn to denouncing Gregory Clark for offering much higher and 'idiosyncratic' figures of 15.7 million acres c.1300 and even 20 million acres. Again, it is argued that it is hard to see how such large areas could have come under the plough given contemporary restraints, 'environmental, technological and institutional' (pp. 71–2).

Table 2.07 allocates the 12.75 million acres of arable county by county in 1290 by taking into account the location of the drainage schemes identified earlier (in Table 2.04), the density of deserted medieval villages (as a proxy for conversion of arable to pasture) and the effects of urbanization. It is never quite explained how this table was compiled. It gives population per square mile and seems to factor this into the calculation, then the percentage of the surface area likely to have been arable in 1290 on a scale between 12.5 per cent (Westmorland) and 60 or 62.5 per cent (Norfolk and Huntingdonshire) and then the estimated arable acreage in 1290.

We need to note one further point though. Table 2.08 gives the regional distributions of arable c.1290 by eight provinces. It also gives estimated total population, population per square mile, estimated arable acreage, arable as a percentage of the total area and arable acres per head. The assumption is made that there is very little interregional trade. Now arable as a percentage of the total area falls from 58.8 per cent in the Eastern Counties to 25.0 per cent in the south west and 1.4 per cent in the north west. The arable acres per head of population lies very sweetly between 2.4 and 2.8, with an outlier of 3.1, and a national average of 2.7 arable acres per head. Now these figures bear thinking about, but they are *too uniform*. They imply a uniformity of agricultural practice and yield. Indeed one has a chicken-and-egg question as to which came first, the estimates of acreage or the estimates of arable acreage per head.¹⁶

¹⁶ It needs to be stressed that these are figures for total cultivated area in each province divided by estimated population and are not therefore estimates of holding size.

TABLE 1. The changing availability of arable land per head, 1270–1871

	<i>Total arable (m acres)</i>	<i>% fallow</i>	<i>Total sown (m. acres)</i>	<i>Arable acres per head</i>	<i>Sown acres per head</i>
1270	12.52	41.0	7.40	2.87	1.70
1290	12.75	38.8	8.19	2.69	1.73
1300	12.72	35.8	8.16	2.69	1.73
1380	9.64	40.4	5.75	3.95	2.26
1420	8.75	40.3	5.21	4.29	2.55
1450	8.44	40.4	5.03	4.42	2.63
1500	8.50	38.1	5.26	3.86	2.39
1600	8.87	24.4	6.72	2.16	1.63
1650	9.63	19.5	7.74	1.81	1.45
1700	9.56	20.0	7.64	1.84	1.47
1750	10.51	15.1	8.92	1.74	1.48
1801	11.35	11.2	10.08	1.27	1.12
1836	13.87	9.4	12.57	0.95	0.86
1871	13.83	3.5	13.35	0.64	0.62

Source: BEcG, p. 74 (Table 2.10) (without the notes to the table as printed there).

Having established the likely acreage under arable in the mid-nineteenth century and at the end of the thirteenth, estimates are made for intervening datum points and so we get Table 2.10 which gives us the total arable acreage, the percentage of that acreage used as fallow in the datum years, the total sown acreage, the arable average per head and the sown acres per head. The table is reproduced here as Table 1. The problem it presents, as the notes to the original make clear, is that some years are based on hard evidence where others draw on interpolations from population figures. On the other hand, there is much of interest in the table, chiefly the progressive decline of fallow to a point where it was unimportant by 1871. This means, of course, that the *sown* acres per head shifts from being about two thirds of the arable acres per head (as one would expect under a three-field system) to just about parity, because all the arable, in every given year, was under a crop of some sort. Here we see the beginnings of a thesis that the agricultural revolution was about the more efficient use of fallows, which in turn involved the adoption of plants little used (if not actually unknown) to medieval peasants. This is further discussed at the end of the chapter (pp. 77–8). It is admitted that the area under fallow has to be inferred. The important observation is made that the incentive to reduce the area of fallow especially increased when the area under arable per head was falling. Population growth encouraged farmers to make more positive use of their fallows. This is no more than a generalization of Campbell's observations about high medieval Norfolk agriculture made a generation ago to a national scale and a longer time span. Likewise Overton has long argued for the importance of the better utilization of fallows. The result is that the decline in the amount of fallow was more important in increasing the area under crops than the overall

extension of the landed area, so that by 1750 the sown area exceeded the sown area in 1300 even though around 2.25 million fewer acres were in arable use. This is a factor expounded at greater length in chapter three. Farmers found more rewarding things to do with their fallow than leave it growing weeds, so it was increasingly planted with grasses, turnips, potatoes and other roots, all of which were beneficial to the land, served to feed larger numbers of animals and produce a profit to the farmer. The result was that the increased area sown came largely from what we might call interior space; making better use of what was already there rather than extending the total cultivated area.

III

Chapter three contains the meat of the book for our limited purposes. The methodology can be explained as a series of steps. First the sown acreage per crop is estimated using the information on crop shares found in the three foundational datasets. This is then weighted by regions using 1290 as a guide to late medieval practice and the 1836–70 data as the basis for the post-medieval share. This gives figures for the total sown areas of the major crops, 1270–1871 (Table 3.03). Yields are then calculated from the existing databases and applied to the estimated acreages already established.

The result is that we get national average gross crop yields per acre (Figure 3.02) for the five major crops. These are reduced to averages for every fifth decade in Table 3.06. Total arable output for every tenth year is then calculated and presented in Table 3.07.

If the basis of these calculations seems reasonable enough, then the authors admit that they are less confident about their estimates of livestock farming. The sources are poorer and the problems more inscrutable. The source information comes from the three databases which are weighted regionally (with the estimates of the area not under arable) to achieve a national total.

The problem is that we have little direct evidence as to the degree to which individual counties were involved in forms of pastoral agriculture between the high medieval peak (when we have demesne accounts) and the nineteenth century. Regional weights are therefore established for 1300 and 1870. At the end of this process we have Table 3.11 which gives us estimated livestock numbers at half century intervals from the 1270s through to the 1860s. These show a rough doubling of animals between c.1750 and the mid-nineteenth century. So, the number of milk cattle is estimated at about 600,000 in c.1300 and had returned to this approximate level (570,000) by c.1750, having fallen to as few as 240,000 c.1700, a reduction of a third from 1650.¹⁷ For beef animals the comparable estimates are 540,000 in c.1300, then 520,000 in 1750 and 1.11 million in c.1860. The authors estimate that there were more sheep in England in 1300 (15.22 million) than in 1750 (13.58 million) but about 22.75 million in the 1860s. Estimates of the quantity of animal products produced are given in Table 3.13. Next estimates are given of the number of working animals although in the case of horses this appears to be

¹⁷ The orthodox literature would lead one to suppose a late seventeenth-century expansion of the dairy sector. One would therefore expect the number of cattle to be larger in 1700 than 1650 and not a third smaller. This figure may be a quirk of the inventory sample.

TABLE 2. Real annual agricultural output growth, 1270s–1860s (constant 1700 prices)

<i>Decades</i>	<i>Arable sector (%)</i>	<i>Livestock sector (%)</i>	<i>Total agriculture (%)</i>
1270s–1300s	0.12	0.40	0.25
1300s–1340s	–0.19	–0.04	–0.12
1340s–1400s	–0.87	–0.42	–0.66
1400s–1450s	–0.37	0.03	–0.16
1450s–1470s	–0.38	0.00	–0.16
1470s–1550s	0.72	0.18	0.45
1550s–1600s	0.63	0.72	0.69
1600s–1650s	0.34	–0.04	0.21
1650s–1700s	0.24	0.47	0.33
1700s–1750s	0.29	0.90	0.55
1750s–1800s	0.52	1.31	0.93
1800s–1830s	0.98	0.63	0.77
1830s–1860s	0.58	1.08	0.85
1270s–1340s	–0.05	0.14	0.03
1270s–1700s	0.08	0.13	0.11
1270s–1860s	0.21	0.37	0.29
1700s–1860s	0.55	1.02	0.77

Source: BEcG, p. 115 (Table 3.16) (without the notes to the table as printed there).

a calculation of the number of animals working on farms only. This matters as these numbers are then used to come up with figures for the production of hay. Finally we arrive at Table 3.15, ‘Total outputs of milk, meat, wool, hides and hay, 1270–1860s’, presented as ten-year averages centred on century ends and half centuries.

In the next stage, product is valued by multiplying output in both sectors of the economy by prices and in Figure 3.04 we arrive at the total arable, livestock and agricultural output 1270–1870 (which is also a good example of a figure reproduced much too small to read). In Table 3.16, annual real agricultural output growth for the two sectors and the total is presented. There is much of interest in this table and it is reproduced here as Table 2. It even contains some support for an eighteenth-century agricultural revolution.

Output is shown to have been growing between 1270 and 1300, but the contraction of the arable sector starts well before the Black Death, much earlier than that of the livestock sector. The contraction of arable persists for 120 years after the Black Death and whilst the livestock sector shrinks between the 1340s and the 1400s, it does so at a much slower rate than the arable. Overall, the agricultural sector shrinks at 0.66 per cent per annum between the 1340s and 1400s. It continues to do so after 1400, but at a much slower rate, and most of the contraction takes place in the arable sector. In short, production is switching from grain to wool and meat, just as we would expect.

Between the 1470s and the 1550s, the arable sector grows strongly (0.72 per cent per annum), much more so than the livestock sector (0.18 per annum). But the growth in the latter exceeds the arable sector in the late sixteenth century, but then contracts slightly between 1600 and 1640. After 1650 though, it is normally the livestock sector which grows faster than the arable sector, in the first half of the eighteenth century at three times the rate (0.29 per cent per annum against 0.09 per annum). This can be seen in Table 3.18 when the relative shares of the two sectors of the economy are presented. (In Table 3.17 the data is divided into the major crops and outputs.) In 1600 the division (in current prices) was 58.1 per cent arable to 41.9 per cent pasture. In 1800 it was 48.5 per cent to 51.5 per cent. By the 1860s it was 40:60. Taken over longer periods, the livestock economy consistently outpaced the arable: so taking the 1270s to the 1700s, the rates of growth were respectively 0.08 per cent per annum and 0.13 per annum; and for the 1700s to 1860s they were 0.55 and 1.02. A final point might be made about this table. The period of fastest growth in the arable economy was in the first years of the nineteenth century when a rate of 0.98 per cent per annum was achieved. This might seem surprising given that the period is interrupted by a period of post-war depression. The period exhibiting the fastest growth in the livestock sector is the late eighteenth century, the classic period of the adoption of 'roots' as winter fodder.

Finally a calculation to span the gap between the end of the manorial sources and the beginning of the inventory data is made using price inelasticity. Historians will generally not go there, but a model is constructed to estimate production over the period from 1300 and 1700. This gives a decent match with the known output data but again the graph is presented too small to be really useful and the actual data is not given. By eye though it suggests a falling output per head over the early sixteenth century, which is surely a point of some interest. It hints at a drift towards a cottage subsistence economy in the first half of the century which was reversed in the second.

This allows the establishment of a continuous data series rather than data series which end in the late fifteenth century and pick up again in the late sixteenth. Appendix 5.3 summarizes the annual data computed in the first half of the book, including that for the output of industry and services (which are not considered in this essay), population, and the calculation of GDP, all indexed on 1700. Unfortunately annual data for the two agricultural sectors is not published here. The data are reproduced here in Figure 1 as both annual data and a seven-year moving average. Where the line for production lies above or to the left of the population line, we can infer episodes of market oversupply, low prices, rising living standards and within agriculture, a shift to pastoral production from arable production. And because this is a measure of production by the price of the commodities produced, it conceals the fact that the supply of grain in the late eighteenth and early nineteenth century was increasingly reliant on imports because English/British agriculture concentrated on high value and perishable crops (meat, butter, cheese) and the country increasingly sourced its bread grains and animal provender from abroad.

There then follow six pages of discussion of the findings. These are the pages which historians and their students will use, and we defer discussion of them until later, turning first to report other findings of the book.

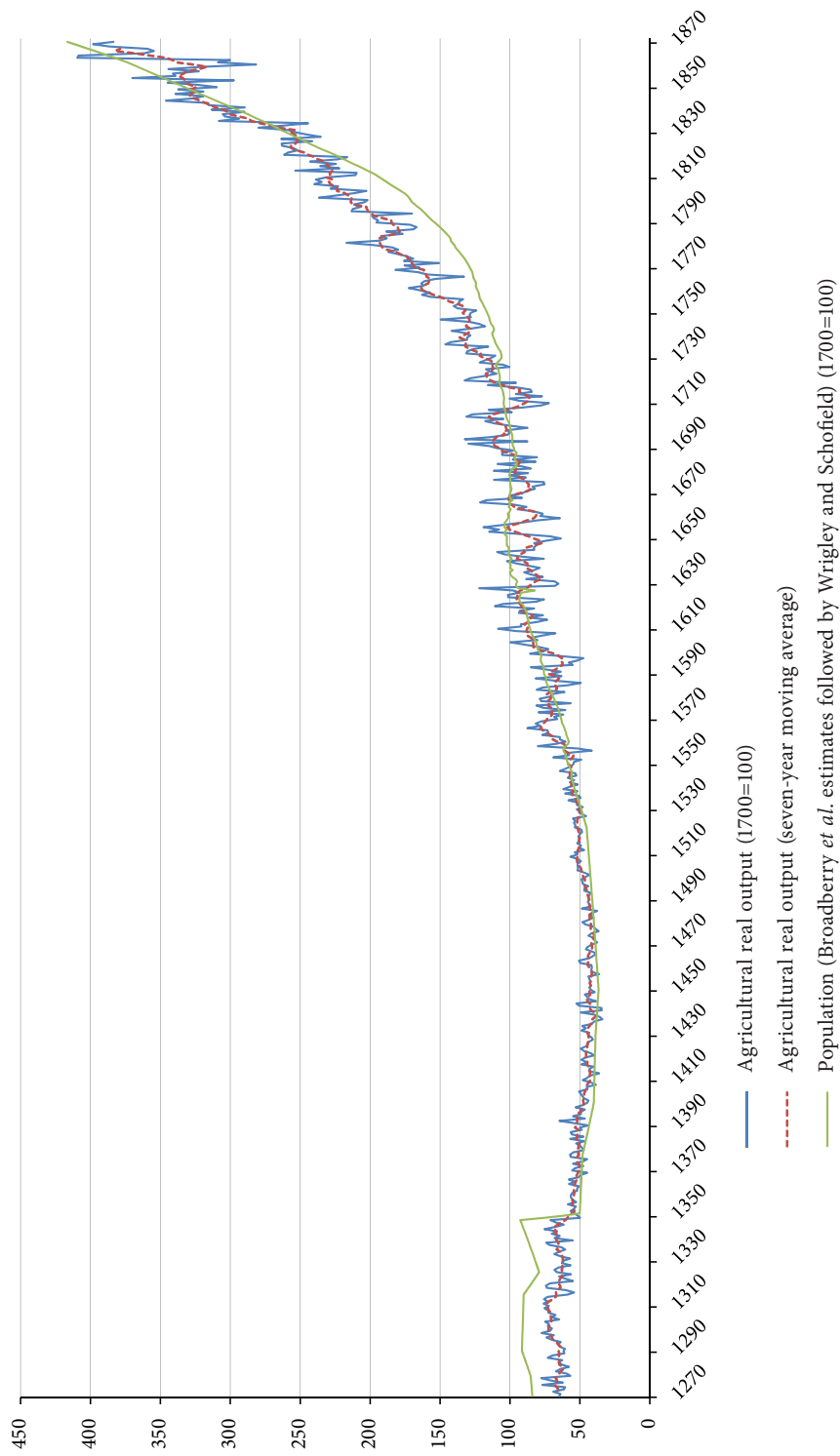


FIGURE 1. Agricultural real output as estimated by Broadberry *et al.* v population
Source: data from Broadberry *et al.*, *British economic growth*, Appendix 53. (Available in colour on www.bahs.org.uk.)

IV

A further area in which the book offers new estimates of interest to readers of the *Review* is the proportion of society working in agriculture, and conversely the proportion of population who depend on foodstuffs acquired through markets. It has to be said that understanding here has moved on since Broadberry *et al.*'s book was finished. Table 9.08 summarizes their findings and places them alongside those published some years ago by Shaw-Taylor (2009 and 2010) and Clark (2013). The direction of all recent work, this included, has been to reduce the share of the population engaged in agriculture. The figure given in the book for c.1380, derived from those poll taxes that give occupational detail, is 67.0 per cent. That for 1522, drawn, one assumes, from the military surveys of that year, is little different, but the implication is that one person in every three was being fed by the other two. The later figures in the first column of Table 9.08 are derived from the classic political arithmeticians: they suggest little change in the male labour force between 1688 and 1759, but a substantial fall between 1759 and 1801 (from 43.0 per cent to 35.7 per cent) and a further fall between 1820 and 1851 to about 27.2 per cent. By this time each person engaged in agriculture was feeding three others, more or less. Estimates for female employment are also given: these loosely mirror those for male employment.

It seems likely that these figures for sectoral employment will be superseded in the next year or two by the work of Sir Tony Wrigley, Leigh Shaw-Taylor, and others in Cambridge. An early publication of some of this data gives figures which differ significantly from those computed by Broadberry *et al.* and those taken from Shaw-Taylor's earlier publications. So, in their Table 9.08, following King and Massie, Broadberry *et al.* offer a figure for male involvement in agriculture of 46.0 per cent in 1688 and 43.0 per cent in 1759 respectively. Shaw Taylor's figures cited here are 43.0 and 44.0 per cent in 1710 and 1755, close enough; but in 2014 Shaw-Taylor and Wrigley offered an estimate of 49.8 per cent for c.1710.¹⁸ This is plainly a treacherous field where judgement has to be suspended until the full publication of the data being gathered in Cambridge, but we are talking of two thirds of men being engaged in agriculture perhaps as late as 1520 but one third by the beginning of the nineteenth century. This is in itself evidence of the efficiency of English agriculture but more on this issue is found in Table 9.09. The labour force involved in agriculture was 90 per cent bigger in 1851 than 1700: but the output of each of those individuals was 75 per cent greater in 1851 than 1700. Table 9.10 presents growth rates in labour productivity. Between 1381 and 1522 the growth rate was marginal (0.02 per cent per annum); between 1522 and 1759 it was present but hardly spectacular (0.13 per cent per annum), but between 1700–59 it reached over half a per cent per annum (0.57). In 1759–1801 it continued at 0.41 per cent per annum. In 1801–51, at a time of increased mechanization, it dropped back to early modern levels (0.10 per cent per annum).

As it seems likely that these figures will be subject to revision, it is perhaps not worth worrying too much about them, but the lack of labour-force productivity gains in an age of agricultural mechanization is striking. One would like to know just how far the

¹⁸ Leigh Shaw-Taylor and Sir Tony Wrigley, 'Occupational structure and population change', in Roderick Floud *et al.*, (eds), *Cambridge Economic History of modern Britain, 1700–1870* (sec. edn, 2014), I, Table 2.2.

eighteenth-century gains reflect the orientation of farming towards a pastoral economy but no analysis by sector is attempted here and, indeed, it might never be possible to offer anything very plausible.

V

There are a number of areas where we need to record reservations. First, there is a basic question about the book: is it a study of England or Britain? Table 1.11 gives figures for English population to 1700 and then suddenly switches to figures for Great Britain. The data in Appendix 5.3 is for England to 1700 and for Great Britain for 1700–1870. There is no good reason why estimates for Scotland and Wales cannot be factored in at earlier dates – Campbell, after all, has done the spadework for c.1290 and estimates for c.1500 are not beyond the bounds of possibility. The data on output is entirely based on English data. When we come to estimates of output per head in Figure 3.05, we are not told whether the heads are English heads or British heads but the notes to Table 3.21 (‘Summary trends in agricultural output and productivity, 1270s–1860s’) suggest that the data presented there is entirely English. So far as can be told, chapters two and three are based exclusively on English data and offer the best available account of long-term trends in English agriculture. But over-all, the book talks about England before 1700 and Britain afterwards, and yet it is not plain what the extra-English data is included. This is nowhere explained or justified, an omission which seems to me to be a serious oversight, albeit one which might be justified by the lack of readily available data. (Neither Scotland nor Wales appears in the index, which tells its own story.) One therefore has to assume that there is no distinctively Scottish data in the book except for the population estimates, and that what claims to be Britain is simply England scaled up with an allowance for the Welsh contribution and then further by the estimates of Scottish population in chapter 1. It then becomes an interesting question as to what a properly researched Scottish contribution to the data would look like, and how it would affect the *British* data.

Secondly, we have already suggested that aspects of the calculation of the area cultivated in 1290 is implausible and that some further thought is needed. If it is too large, then the area per capita is also too large and must be smaller. Let us offer an alternative calculation starting with the estimate of 13.9 million acres for the extent of arable in England in the mid-nineteenth century. We can agree with Broadberry *et al.* that virtually all of the land drained in the Fens and Sedgemoor, and from the coastal marshes of the east coast, was unusable as arable before its recovery, so we subtract 1.4 million acres, which makes 12.5 million. This figure also includes inland lakes (Martin Mear in Lancashire for instance) and tracts of boggy ground (the Weald Moors in Shropshire or Wallingfen in East Yorkshire). Much of this may have gone through a second stage of drainage in the nineteenth century. Then there is then the issue of nineteenth-century tile underdrainage. Phillips seems to regard the process of underdrainage as being about increasing productivity rather than changing usage and for this reason underdrainage should be discounted in any assessment of the extent of the arable area, whilst acknowledged as an important element in the improvement of productivity (as we argue subsequently).

There is then the reclamation of woodland, the enclosure of lowland heaths, the relative uplands of the Downs and so on. In short, how much land was used as common in c.1300 but

as arable in 1850? If we go for a low estimate of half a million acres, then we are talking about a national arable acreage of 12.0 million in 1300: if we see this movement as equivalent in scale to the draining of the Fens, then we are talking of a national arable acreage of 11.0 million in 1300. On the other side of the table though we need to consider how much land was cultivated as arable in c.1300 but was still laid down as improved pasture in the mid-nineteenth century, even after half a century of incentives to plough. One suspects that by 1850 there was relatively little of this, but it was surely a factor before the end of the eighteenth century. Wharram Percy, for instance, was largely ploughed by 1836.¹⁹ Hence this element should also be discounted in any calculation of the size of the arable in 1300 worked back from c.1850.

It may be seen how intractable the problems are but also how important it is to achieve a satisfactory resolution of them. There may be finer calculations to be made through intensive local analysis, but it is striking that at the moment we don't even have figures, at the county level, which distinguish the area of open field and the area of common enclosed by parliamentary enclosure. The relevance of this to the present discussion is that land which was common when enclosed by act in the later eighteenth century and later cannot, by definition, have been used as arable in c.1290. For immediate purposes, the figure adopted by Broadberry *et al.* of 12.75 million acres of arable in c.1290 seems to be on the high side: one of 12 million might be nearer the mark. If we take the figures for population c.1290 as being fairly secure, the average acreage per head cannot be higher than 2.4 acres and is almost certainly lower.

Third, there is a further factor which needs to be considered here. This is the assumption that the productivity of demesne agriculture was much the same as that of peasant agriculture. That there is a question to be posed is acknowledged and a counter argument (that demesnes had better land) is advanced. This in itself is a point of some interest. If demesnes had the best land, then it suggests that they were ring-fenced rather than having their land distributed through the open fields in the fashion of their peasant neighbours. Now if the former applies, there then are important implications for labour productivity as a demesne's labourers would not have had to trudge through the open fields from strip to strip, nor carry manure long distances out and produce back. In fact there seem to be as many demesnes which were consolidated as demesnes which were scattered, but where demesnes were enclosed, whether or not they had secured the best land, labour inputs must have been on a lower level than those where the land was in open fields.²⁰

More importantly there is a growing body of material (much of which has appeared since the book was published) which suggests that demesnes may not have been either a leading or even an equal sector in the medieval agrarian economy, but may have trailed behind.²¹ They may have been short of manure: they were certainly short of animal traction and they may have skimmed on labour inputs. Peasants on the other hand may have used much more spade husbandry

¹⁹ S. Wrathmell (ed.), *A history of Wharram Percy and its neighbours* (Wharram, a study of settlement on the Yorkshire Wolds, XIII, 2012), Fig. 116 (p. 361).

²⁰ David Hall, *The open fields of England* (2014), pp. 95–105, a reference I owe to Professor Christopher Dyer.

²¹ David Stone, *Decision-making in medieval*

agriculture (2005); H. J. P. La Poutre, 'Fertilization by manure: a manor model comparing English demesne and peasant land, c.1300', *AgHR* 65 (2017), pp. 20–48; Janken Myrdal and Alexandra Sapoznik, 'Technology, labour and productivity potential in peasant agriculture: England, c.1000 to 1348', *AgHR* 65 (2017), pp. 194–212.

than has been assumed and have put more time into soil preparation, weeding and the close management of crops. All of this suggests that productivity per acre in peasant agriculture was higher. Broadberry *et al.* acknowledge that 'care must ... be taken in moving from data on demesnes to inferences about the development of English agriculture as a whole', but finally they conclude that, 'Since the direction of the adjustment is unclear and would anyhow be quite small, it has been assumed here that the yields obtained on demesnes are representative of those within agriculture as a whole'.²² The demesne sector was never more than 25 per cent of the total area sown c.1300. If peasants – and this is a very big if – were able to achieve higher yields, albeit from using techniques which may have been more demanding of labour, then it goes some way to reconciling the lower acreage per capita which we suggest follows for 1290.

A final point might be made. High productivity in English agriculture was achieved by drawing greater levels of resource from first within and then without the British Isles. The trade in store cattle from Scotland (and to a degree Ireland) might be read as simply an internal transfer within Britain but it was this that underpinned the Norfolk rotation with its heavy demand for cattle. Towards the end of the period English agriculture drew heavily on American nitrates – guano – and again this needs to be acknowledged. In any classic calculation of GDP it would be placed on the debit side of the calculation as a cost. Of course, it was the use of Scots cattle and American nitrates which allowed fallow to be reduced, in the one case by allowing higher levels of stocking which made more manure available and in the other by reducing the reliance on manure as fertilizer. They have a place in any comprehensive account of the agricultural revolution. But do the authors still believe in one?

VI

With this question in mind, let us now turn to the chief conclusions of Broadberry *et al.* as they concern us here. Their figures lead them to adopt a Boserupian approach, that demand led to agricultural improvement and increased productivity. In a key comment:

Historians have sometimes speculated whether in earlier centuries at times of acute population pressure an inability to reconcile these inherently competitive land uses may have resulted in over-stocking of pastures and/or under-manuring of fields, thereby jeopardising the fragile ecological equilibria upon which the sustainability of production depended. It would be surprising if, at the micro-levels of individual farms and localities, this had not occasionally happened. Never the less, at a macro-level and much as Ester Boserup would have expected, population growth clearly drove up yields of both crops and livestock, whilst population decline led crop yields at least to fall. It was in the mid-fifteenth century when population pressure was weakest and land most abundant, that grain yields sank to their lowest recorded level. Thereafter yields steadily improved as the population grew and the arable area expanded. The impressive gains in agricultural output achieved between 1450 and 1850 arose more from intensifying and rationalising the use of existing resources than by bringing more land into production.²³

²² *BEcG*, pp. 82, 90–1.

²³ *BEcG*, p. 128. The references to secondary sources are omitted.

Now important consequences follow from this assessment. Malthus is demoted. The operation of Malthusian positive checks in the thirteenth century is acknowledged but not thereafter (see the discussion on p. 273). But there is no room in this analysis for the positive check in the form of famine in the sense that Malthus envisaged. That famines happened is not to be questioned, but this work fits with a growing consensus that famines are produced by short runs of aberrant weather rather than by populations exceeding the capacity of agriculture to support them.²⁴

The key event in this account is the Black Death and the plagues which followed it. This completely altered the equilibrium between population and resources, and increased income per head by forcing rises in real wages. Pre-Black Death arrangements never reappeared. When population began to grow again, real wages began to fall but GDP per head remained stable as the new surplus population was accommodated in towns and drew its livelihood from industry rather than having to be accommodated in the countryside. What may be significant here (and I think this is not a point Broadberry *et al.* make), is that industrial pursuits allowed for the employment of women and children. Household income held steady, and the old view that working people were tipped into poverty, based on a reading of daily wage rates, now seems less rather than more likely. Where, however, there was no employment for women and children, falling wage rates for agricultural labourers produced real destitution by the end of the eighteenth century and even a basic standard of living had to be subsidized through parochial rates, especially in high price years. In short then, the labour-intensive organic expansion of textile production between the mid-sixteenth and the mid-eighteenth century enabled living standards to be maintained. The machine-intensive period of expansion which followed destroyed living standards.

So whilst after the mid-fourteenth century there were no ceilings on productivity which, when reached, produced a reaction through the operation of the positive check, ideas of Malthusian stagnation of the sort that have been so influential are also rejected. Broadberry *et al.* recognize the operation of the preventive check though the European marriage pattern, and that access to marriage was dependent on ability to earn wages. But they do not concede that the limits of agricultural technology acted as a bar on population growth. Of course, what differed over time was the range of the technologies available to farmers, especially the range of crops.

If the idea of Malthusian ceilings is rejected, then so too is the idea of agricultural revolution. Instead of seeing productivity increasing in a series of spurts as institutional restraints – notably open fields and unproductive commons – were swept away:

The essential point [is] that progress was incremental from the fifteenth to the nineteenth centuries and rarely more than evolutionary in pace, since agricultural output never grew at more than one per cent per year until the nineteenth century. Over several centuries, however, this was sufficient to effect a transformation.²⁵

Here we might comment that the idea of a persisting growth rate of less than one per cent

²⁴ See here the recent volume edited by Guido Alfani and Cormac Ó Gráda, *Famine in European history* (2017).

²⁵ BEcG, p. 129.

almost marks the reappearance of Malthus's arithmetical increase in production, which, of course, he read in terms of the 'improvement of barren lands'.²⁶

Returning to *British economic growth*, the writer continues by offering a list of the usual suspects that together form the traditional ingredients of agricultural change. We now have a continuous conveyor belt of improvement. Demand produced increased production: the removal of restraints and the introduction of new techniques followed behind. Again, this fits with recent work which sees open fields as being less of a bar to development than was once assumed. In fact one could go further and suggest that not only demand but also oversupply was the stimulus to change. The problem for most of the 100 years after 1650, and probably before and to a degree after, was one of glut, of low grain prices, which forced subsidized exports to maintain the farm-gate price but also encouraged farmers to withdraw from grain-growing and concentrate on dairying and fattening. There is no evidence in this period for demand beginning to outstrip production and forcing another round of innovation. On the contrary: what we can perhaps see is demand making innovation worthwhile and oversupply making it necessary. We can posit that innovation takes place when there is confidence that it will produce a return. The mindset of the individual farmer lies far beyond the range of this book, but one can all too easily see how, if farmers saw an opportunity to make money by producing more, or producing differently, they took it. It is all very well arguing that institutional restraints held back agriculture: but capitalism is rather good at dissolving barriers to the making of money and one is bound to suggest that if some enclosure was delayed even into the early nineteenth century, it is because the profits of seeing it through had not previously been compelling enough.

Finally one is left wondering whether the rejection of the idea of revolution in place of continuous incremental change does not throw the baby out with the bathwater. Commentators at the end of the eighteenth century were in no doubt that in their lifetime there had been momentous changes in agricultural practice.²⁷ Of course what they saw came not from the perspective of national accounting but their own experience within predominantly local horizons. They recalled changes in the palate of crops grown, the adoption of new machinery (and improvements in traditional machinery) and they had witnessed rapid landscape change. They also believed that there was much more to be done.

The problem with a methodology that looks at national levels of output (or GDP) is that the steady tread of improvements disguises local leaps forwards. Let us suppose that the national production of peas increases by 20 per cent. What this probably means is that in some areas it increases by 50 per cent: in others the production of peas stops completely as

²⁶ Malthus, in his *Essay*, took the doubling of agricultural productivity in 25 years as his example, although he doubted it could be achieved, and he saw a further doubling in the following 25 years as impossible. (Of course, one per cent growth per annum falls far short of a doubling in 25 years.) He did though acknowledge the possibility of technological change, for he saw the outcome of continued productivity growth as labour-intensive cultivation ('In a few centuries it would make

every acre of land in the island like a garden'): his view was therefore deeply pessimistic. In general though, Malthus was uninterested in, or ignorant of, agriculture, perhaps because he had no statistics to play with. T. R. Malthus, *An essay on the principle of population*, ed. Donald Winch (1992), pp. 18–19.

²⁷ See for instance essays like J. E. Hamilton, 'On the improvement of agriculture in Great Britain and Ireland', *Annals of Agriculture* 13 (1790), pp. 74–9.

the efficiencies achieved by the areas undergoing revolution make the production of peas in less efficient or advantaged areas uneconomic. This sort of behaviour can be seen very plainly in the Industrial Revolution in textiles where the development of very efficient factory production in a limited number of geographical locations made production in others – except for high-value, bespoke products – a hopeless proposition. Revolution in some localities was matched by disengagement in others: there was an element of geographical relocation. We might suspect that exactly the same went on agriculture. This is surely what underpins the division into arable and pastoral areas which is very marked from the mid-seventeenth century onwards but was hardly a new phenomenon even then. If the production of a cereal crop increased by, say, 10 per cent, it was not that it increased by 10 per cent everywhere. It increased by 20 or 30 per cent in some places – places which may be said to have gone through a revolution – while production ceased in others and farmers looked for alternative products to grow.²⁸

An approach which eschews ideas of revolution is open to the sort of criticism which Berg and Hudson made quarter of a century ago of the economic interpretation of the Industrial Revolution by Crafts and others.²⁹ In terms of output, the year-on-year change found by them was not impressive and it could plausibly be argued that there had been no revolution. In terms of the regional experience, the forms of production and the employment and the social experience of labour, there was indeed a revolution between 1780 and 1850. People no longer worked in a domestic environment or in small workshops: they worked in factories under an industrial discipline. Now one would not wish to press the analogy with Berg and Hudson's critique too far, but one could argue that ideas of revolution are finally a matter of perspective. Reading economic growth as a national experience suggests only gradual and incremental change. Treating it as the sum of many local and regional experiences surely finds examples of rapid, accelerated *regional* change if only as some areas played catch-up with what had been achieved – and normalized – elsewhere, or looked for other specialities to pursue. It is certainly far too soon to say that there was no agricultural revolution and until we decide where we stand on this issue, the question will hopefully provoke thousands of undergraduate essays.

VII

The question now becomes one of where British agricultural historians should direct their research effort over the next few years. Of course, this is a study of productivity which draws on work undertaken at a time when studies of productivity still commanded the heights. In a sense *British economic growth* draws to a close a phase of English agricultural history which began in the 1970s when Campbell and Overton started their doctoral work. The hot money has long since moved elsewhere, into social aspects of rural history, cultural histories of the

²⁸ For this reason there is a doubt in my mind about how far inventory data from regions which continued in arable production may be taken to reflect *national* trends in arable production. The present book may

escape this problem by weighting production by county.

²⁹ Maxine Berg and Pat Hudson, 'Rehabilitating the industrial revolution', *EcHR* 45 (1992), pp. 24–50. (Capitalization as in the original.)

countryside and environmental history, and now there is a new concern with technology through an interest in the treatment of animals over time.

Within the area that the book considers, there is a plain need for more evidence at certain moments, for instance in the gap between the end of the demesne accounts and the beginning of the probate inventory materials. More inventories could be collected for the later sixteenth century to narrow the gap, and the book is clear that the inventory sample could usefully be widened to more counties, but one suspects that it would require a big correction to the existing inventory data to materially alter the results reported by Broadberry *et al.*: the point is more and better data would build confidence in their results rather than serve to overturn them.

British economic growth shows that there is a real question about when England achieved its maximum cultivated area in the late eighteenth and first half of the nineteenth century. Clarity here would be helpful in underpinning future calculations of the likely cultivated area c.1290. It would certainly be worth looking at how we can secure better estimates of the area of arable, permanent enclosed pasture and common in England between the later middle ages and the onset of parliamentary enclosure in the mid-eighteenth century. As a process which transformed landscapes and allowed for the extension of the arable area by enclosing commons (and the conversion of open field arable to common), enclosure will continue to hold its fascination for agricultural historians.

Another point which needs to be made is that whilst *British economic growth* places a great deal of emphasis on fallows, it ought to say more about underdrainage. Apart from Phillips's work on the later phases of underdrainage, this is an orphan subject which awaits its historian. A number of writers have shown that underdrainage using hollow or bush drains is a significant factor in both increasing the area available for cultivation but also raising the productivity of parcels of ground prone to poor drainage or water-logging. For Allen drainage was one of the uncontested advantages brought by enclosure. Using data from Vancouver's *General View of Cambridgeshire*, Allen calculated an increase in yield of 20 per cent for wheat, 18 per cent for barley and 27 per cent for oats following underdraining.³⁰ This was specific to certain soil types: it should not be taken to be a general effect.

Wade Martins and Williamson produced evidence that drainage by hollow drains was familiar on the heavier soils of Essex and Hertfordshire by the end of the 1720s and suggested that it was adopted on the East Anglian clays after the 1760s.³¹ There was a great deal of interest in underdraining in the 1790s: the Board of Agriculture and the circle of Arthur Young took up a drainer called Joseph Elkington and a book on the principles of drainage as developed by him appeared under the Board's auspices in 1797.³²

The problem is that this early drainage using hollow drains is largely historically invisible. The practice, whose use continued throughout the nineteenth century, was to dig a trench

³⁰ R. C. Allen, *Enclosure and the yeoman. The agricultural development of the south Midlands, 1450–1850* (1992), pp. 138–9.

³¹ Susanna Wade Martins and Tom Williamson, *Roots of change. Farming and the landscape in East Anglia, c.1700–1870* (1999), p. 65 and see pp. 61–7 *passim*.

³² John Johnstone, *An account of the most approved*

mode of draining land according to the system practised by Mr Joseph Elkington drawn up for consideration of the Board of Agriculture and internal improvement (1797); Chris Holland, Anne Langley and Adam Moore, *Joseph Elkington: Warwickshire's land drainage pioneer* (2006).

and fill the lower part of it with vegetable matter – gorse, hedge cuttings, coppice timber even – and then backfill the trench. Water would then flow through the vegetable matter, but as this decayed, the drain would collapse and the efficiency of the drain would diminish. The work of drainage therefore needed to be repeated every ten, twelve or fifteen years. The decision as to whether to make or to repair hollow drains and the costs they incurred largely fell on the farmer himself. Tile drains and pipe drains had a long life-expectancy and were therefore improvements which had something of the character of fixtures. The introduction of machine-made tiles and drains substantially reduced their cost. As they were expected to last, it was possible to borrow on their security, both in the sense that tenants might pay interest to landlords for their investment but that from 1846 landowners were able to access government-backed loans for underdrainage. It is the records created by government loans that Phillips exploited, but this stage in the history of underdrainage stands in relation to the longer history in much the same way as parliamentary enclosure stands to enclosure: it is a phase backed by readily accessible records. Unlike parliamentary enclosure though, drainage could take place several times. Tile drains could supersede hollow drains and doubtless often did.

Contemporaries were impressed by the efficiency of drainage. In 1802 the vicar of Mitton in the West Riding of Yorkshire reported that ‘The face of this country is of late very much altered by draining the lands’.³³ It is not hard to find other examples of such enthusiasm. We therefore need to take underdraining seriously. It may be argued that underdrainage by hollow or bush drains merely introduces another unquantifiable element into the equation. Moreover, there was probably a regional element to it – the benefits would be felt in some localities and not others according to soil type. It was a technique which farmers could adopt when the prospects for agriculture looked good whilst drains could be allowed to decay when agriculture was in the doldrums. As such, it is a technology which conforms to a Boserupian approach, something which is far from original or innovative, but which can be picked up when the time is right (and put down when it ceases to be useful). The adoption of underdrainage is a factor which needs to be included in any assessment of agricultural change in the half century before tile drains became usual.

There is too the whole question of weather, and the extent to which eighteenth- and nineteenth-century gains in agricultural productivity can be attributed to changes in the prevailing climate rather than being the outcome of changes contrived by people, be it new crops, new strains of crops or new technologies. Did it, in effect, become easier to farm? Did changing climate force the abandonment of some arable land in the later middle ages but permit its return to arable half a millennium later? Did the great expansion of cereal growing into improbable places (the north-west coast of Ireland for instance) become possible because the weather became more clement and the growing season longer? Weather barely features in this book, which, given Campbell’s prominence as an advocate of weather as an exogenous factor to medieval society, is perhaps a bit odd. In general agricultural historians don’t really

³³ M. E. Turner (ed.), *Home Office acreage returns, Staffordshire to Yorkshire* (List and Index Society 195, 1983), p. 177. The vicar also made some interesting comments on the move away from bare fallows to rotations which lacked a fallow year.

know what to do with weather as a variable, and yet we would all acknowledge that it is not unchanging, even if the exact pattern and timing of change is elusive.

In many respects though *British economic growth* makes the point that agricultural history in England is now a mature discipline. Here we have a work which sums and interprets the work of the last generation, perhaps producing surprising results along the way. But the book also shows the gaps in our knowledge. We have already commented on the uncertainties about the area under cultivation. The account of pastoral productivity is much less well founded than that of arable productivity. But where work is needed most is on areas external to England, on Scotland for one, and on the imports and exports between England and its immediate neighbours. Scotland lacks some of the sources which English historians fall back on – the 1801 crop returns, the tithe files and tithe awards – but it has successive generations of the *Statistical Survey*, which offer the possibility of constructing series of data extending back into the third quarter of the eighteenth century. Price data is also available from the mid-sixteenth century in the form of the fiars.³⁴ Accounts of Scottish economic history will never be as fine-grained as those of England, but this is no reason not to try to assemble more, and better, Scottish data. It may be expedient to make the assumption that Scotland is simply the younger sibling of England, but it is finally not good enough to be persuasive.

Overall though, *British economic growth* is the gift that the generation now moving into retirement makes to us. Like many a birthday or Christmas present, it may not be entirely welcome, but we should take care that it is not simply put away to gather dust. The chapters on agriculture should be central to our teaching, our research and our discourse.³⁵

³⁴ A. J. S. Gibson and T. C. Smout, *Prices, food and wages in Scotland, 1550–1780* (1995).

³⁵ Since this review was submitted for publication, Professor Clark has published a discussion of the same chapters of the book ('Growth or stagnation? Farming in England, 1200–1800', *EcHR* 71 [2018], pp. 55–81) to

which Campbell and Overton are going to reply in a further article in *EcHR*. They also tell me that they intend to refine their estimates of the area under cultivation. I have chosen not to revise this review article in the light of Professor Clark's comments.