

11 Commerce and industry

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Introduction

In 1982, economic historian Patrick O'Brien published a paper that is still relevant, even if we must assume that O'Brien would nowadays position himself slightly differently in the debate. In this paper, O'Brien challenged the view that the economic development and success of Western European countries, culminating in industrialisation and the growth of empire of the nineteenth century, were the direct result of the exploitation of non-Europeans during the preceding centuries. Without denying that the relations between world regions had intensified between 1500 and 1800, O'Brien set out to prove that the claim that rents from the exploitation of what was then called the 'periphery' had caused this momentous transformation of the balance of economic and political power in the world, was incorrect. That claim, O'Brien argued, 'foundered on the numbers' (O'Brien 1982, 16). Figures concerning the composition of international trade and national income demonstrated that the share of trade between the core and the periphery was simply too small to act as a game-changer.

O'Brien's paper is significant for this chapter for two reasons. The first is that it took a position in a debate that has become relevant again in light of the rise of 'global history', a new field to which O'Brien himself has made important contributions. His position in 1982 was that exogenous factors were relatively unimportant, compared to endogenous factors, when it comes to explaining the performance of the European economy in the run-up to the Industrial Revolution. The second significant element was O'Brien's insistence on taking numbers seriously. By ignoring them, he claimed, other historians had overlooked evidence that was relevant to their argument – evidence that in fact invalidated that argument. In making this point, O'Brien was taking early modern numbers seriously, something that requires a certain leap of faith. These two themes will structure the contents of the chapter that follows. It will look at the early modern European economy asking what its dynamics were, especially in trade and industry. While doing so, the chapter will use numbers wherever possible, but it will also discuss the problems and pitfalls of those numbers.

Big ideas from the 1970s and 1980s

When O'Brien published his paper, he was challenging one of the most important historical master narratives of his time: that trade was the root cause of the emergence of the 'modern world economy'. This view was firmly embedded in the work of the then dominant French *Annales* school. Not all of the *Annales* historians were convinced of this story; actually, some were generally sceptical about the possibilities of any serious economic progress before the Industrial Revolution. Emmanuel Le Roy Ladurie famously characterised the economy of the early modern era as one of 'immobility' (Le Roy Ladurie 1974). He and several of his colleagues wrote books that reaffirmed the pessimistic prediction by English cleric Thomas Robert Malthus in the early nineteenth century that populations tended to outgrow food supplies and were thus eating up any gains in welfare. This so-called neo-Malthusian school still has its supporters today (Hatcher and Bailey 2001, ch. 2; Clark 2007).

However, the acknowledged leader of the *Annales* school, Fernand Braudel, had different ideas. In 1977 he published a small book, really an essay, titled *Afterthoughts on Capitalism and Material Civilization*. Because his three-volume history of the topic was only published in France two years later, and in English between 1982 and 1984, this was a slightly odd choice of title. The point however, was the same in both: slow changes did occur in all areas of the economies of the Middle Ages and early modern era, but the rise of capitalism was due to international trade, and in this area there was significant change as a result of the so-called discoveries. About the impact of 'capitalism' as such, Braudel was somewhat ambiguous (Braudel 1979, vol. 3). He thought it was important enough for a whole volume in his trilogy, but the other two volumes seemed to imply at the same time that for most Europeans, capitalism was a distant rumble rather than experienced on a daily basis.

The rise of capitalism had already been explored by an American sociologist who was so heavily influenced by Braudel that he dedicated his own book to him, and would ultimately outbid the master by writing a four-volume history of world capitalism. Immanuel Wallerstein's interpretation was analytically more sophisticated than Braudel's, because he proposed that the division between rich and poor countries in the modern world was really a division of labour (Wallerstein 1974, 2011). The origin of this division of labour, however, Wallerstein located in the global trade network as it emerged during the sixteenth century and developed during the seventeenth and eighteenth centuries. In their identification of trade as the root cause of capitalism, Wallerstein and Braudel were united. It was specifically against Wallerstein's interpretation that O'Brien mounted his attack (O'Brien 1982, 18). In doing so, he cunningly used Braudel's vision of the pre-modern economy as one dominated by peasants and local markets to underline his own point that inter-continental trade was, perhaps not culturally but at least economically, of marginal importance. When he wrote his paper, O'Brien had not been able to look at Braudel's latest statements on the subject, which could be read as supporting both positions.

Even though Braudel and Wallerstein were popular and much admired around 1980, sceptics were uncomfortable with their focus on trade. Leftist historians complained that their emphasis on exchange ignored the transformation

in the so-called forces of production (land, labour and capital) that Marx himself had seen as a necessary precondition for the rise of capitalism (Brenner 1977; cf. more recently Bavel 2016). Intuitively, moreover, it seemed difficult to exclude the countryside from any explanation of change in a society where the vast majority of the population worked in agriculture. Rival interpretations therefore emerged that either highlighted changes in agriculture itself (see ‘Introduction’ above), or the emergence of rural industry. In 1972 Franklin Mendels gave a name to those changes in industry by launching the term ‘proto-industry’.

Mendels claimed that he had identified a new phenomenon in the early modern countryside that could explain the success of the Industrial Revolution even better than the invention of the steam engine. To be sure, this phenomenon had already been uncovered by Swiss ethnologist Rudolf Braun, but his 1960 dissertation was only available in German and he had failed to come up with the catchy phrase as well. Proto-industry, as both Braun and Mendels described it, was all about the interaction of family formation and agricultural labour markets. Traditionally, their argument ran, rural population growth had been held in check by the limited availability of farms. Children without the prospect of an independent livelihood were forced to remain bachelors. In the seventeenth century, however, a new source of income became available to them: rural weaving and spinning for merchants who exported to distant markets. These merchants found a willing workforce especially in regions with poor soils, where farmers were struggling. The textile workers used their own homes as workshops, were often forced to buy their own tools and even raw materials, but were ultimately depending on the merchants to sell the fruits of their labour. In these textile districts, the brake on marriage was lifted; indeed, it paid to marry early, because wives and children could all make a contribution to the household’s output. Proto-industry spread like wildfire, creating regions where factory production could subsequently take root because they had a trained workforce that could simply be moved out of their homes and into the factory. This theory of proto-industry seemed to make sense of a lot of disparate facts. The study of proto-industry thus became a cottage industry in its own right, producing a substantial literature, a literature that would inevitably complicate what initially looked like an attractively smooth story (cf. Kriedte, Medick and Schlumbohm 1977).

Quantitative methodologies for the early modern period

Braudel’s work was methodologically old-fashioned, but his collaborators – like Pierre Chaunu, Pierre Goubert, and Emmanuel Le Roy Ladurie – were pioneers of quantitative history (Burke 1990, 53–64; Carrard 1992, 166–81). Building on pre-WWII initiatives, especially a European project compiling series of prices and wages launched by Ernest Labrousse, these scholars published articles and books full of graphs and numbers, and also added extended appendices with long tables, providing annual information about populations, agricultural output and prices, and wealth distributions. Simultaneously, the movement was catching on in other countries. A typical example is Herman Van der Wee’s famous 1963 study of

Antwerp's trade during its sixteenth-century Golden Age. Van der Wee had been trained as a historian and as a social scientist and had done post-doctoral work at the *École Pratique des Hautes Études* in Paris, headquarters of the *Annales* school, and at the London School of Economics. The first volume of his book is entitled 'statistics', and consists of almost four hundred pages of tables, preceded by 170 pages of explanation of the sources and the meaning of the numbers. Volume three consists entirely of fifty graphs. However, from those data the author was able to extract, in volume two, with unprecedented precision the interactions of the long-term endogenous price trends known as the 'secular trend', and the influx of precious metals from the New World.

During the 1970s, American economic historians infused this quantitative type of history with a strong dose of economic theory and methodology. This New Economic History, led by Stanley Engerman and Robert Fogel, was more popular in Economics faculties than it was among historians, many of whom have ignored or even condemned this approach as a-historical (Drukker 2006). However, the results of their successors' work has a direct impact on the subject of this chapter, and we would ignore it at our peril. Much of the New Economic History (NEH) started with the nineteenth century, when the sort of statistical survey data that NEH requires are more or less readily available, thanks to the efforts of Napoleonic bureaucrats and their successors. For the early modern period, these are sadly missing. Any number that you see for this era can be one of three things: a contemporary estimate (likely to be wrong, usually quite substantially so); a detailed reconstruction in one specific location (raising questions about its representativeness); or, finally, an estimate, based on a combination of data of the second type (raising questions about the underlying assumptions of that combination and its elevation to a generalised level). Let us look briefly at an example of each.

Perhaps the most famous contemporary estimate is Gregory King's table of 'ranks and degrees', which offers a breakdown of the English population in 1688 into various ranks and occupations, including commerce and industry (Holmes 1977) (see Excerpt 11.1).

Excerpt 11.1

A population size estimate in an attempt to establish England's National Income in 1696: King, Gregory (1802). *Natural and Political Observations and Conclusions upon the State and Condition of England* [1696]. In George Chalmers, ed., *Estimate of the Comparative Strength of England and of the Losses of Her Trade From Every War Since the Revolution*. London: J. Stockdale.

Whereas the ensuing Treatise depends, chiefly, upon the knowledge of the true number of people in England, and such other circumstances relating thereunto, as have been collected from the assessments on marriages, births and burials, parish registers and other public accounts: We shall,

first, exhibit the calculation of the number of people, as they appear by the said assessments.

Ist, As to the number of people of England

In this calculation we shall consider,

1. The number of inhabited houses;
2. The number of people to each house;
3. The number of transitory people, and vagrants.

The number of houses in the kingdom, as charged, in the books of the hearth office, at Lady-day 1690, were 1,319,215[.]

The kingdom increasing at this time about 9,000 people per annum, as will appear in the ensuing discourse, the increase of houses should be about 2,000 per annum; but, by reason of the present war with France, not much above 1,000 per annum: so that by the year 1695 the increase cannot have been above 6 or 7,000, which makes the present number of houses; that is to say, such as were so charged, in the books of the hearth-office, to be about 1,326,000[.]

But, whereas the chimney money being charged on the tenant or inhabitant, the divided houses stand as so many distinct dwellings, in the accounts of the said hearth-office; and whereas the empty houses, smith shops &c. are included in the said account; all which may very well amount to 1 in 36 or 37 (or near 3 per cent.) which, in the whole, may be about 36,000 houses; it follows that the true number of inhabited houses in England is not above 1,290,000[.]

Which, however, in a round number, we shall call 1,300,000 And shall thus apportion

London and the Bills of Mortality,	105,000 houses
The other cities and market towns,	195,000
The villages and hamlets,	1,000,000[.] [409–410]

According to King's estimates, only some 50,000 households were occupied in commerce and another 60,000 in industry. New research has substantially reduced the number in agriculture to 227,000 and pushed up those in commerce and industry to 128,000 and 257,000 respectively (Lindert and Williamson 1982, 388–89). As a result, England is no longer portrayed as primarily an agricultural economy, but rather one where industry was already the largest sector – well before the Industrial Revolution kicked in. This, obviously, fundamentally changes the story of industrialisation.

Detailed local studies have in many other places helped to obtain much better numbers than we can reasonably expect from contemporaries, who lacked the instruments and reference points to verify their work. Weights and measures, for example, varied widely, not only between countries but also within them, creating all sorts of confusion until, on the Continent at least, Napoleon's armies and bureaucrats imposed uniformity all around (Allen 2012, 32–34). Local archives do, nonetheless, contain documents that help the historian create those numbers themselves from the raw data. Such raw data include various sorts of registers that record all immigrants, contributors to a loan, or taxable households. A problem is that many such registers contain serial information, whereas the historian also would like to have stock numbers: what percentage of the population was migrant, how many worked in the building industry, and so on? Tax registers often have this advantage that they were created at a certain point in time, rather than as a series. Many such registers, moreover, provide additional details, such as occupations of the heads of households, because this was relevant to the assessors. A very famous example of such a rich source is the Florentine Catasto of 1427 (Herlihy and Klapisch-Zuber 1978).

For his study of eighteenth-century Lyon, France's largest industrial centre at the time, Maurice Garden had the good fortune to find such a source: a fiscal register created just before the Revolution, in 1788 (Garden 1970, pt. II). The date meant he did not have to reckon with the upheavals that would soon engulf the city and the country, but there were other issues related to the use of fiscal sources, such as under-reporting and the mismatch between the tax on a specific item (in this case real estate) and what the historian wants to know (household wealth). Still, Garden was able to establish that among the contributing heads of households, a quarter worked in the famous Lyon silk industry, but they were still outnumbered by the artisans in other branches of industry, who added up to just over a third of the population. Unspecified workers, who may have been active in transport as well as in industry, covered another 13.5 per cent. Merchants and shopkeepers on the other hand, constituted a mere 6 per cent of the households but they were taxed significantly higher than the industrial workforce, suggesting also greater wealth (Garden 1970, 191 tab. 11).

In my own study of the world of urban elites in eighteenth-century Holland (Prak 1985), I used probate inventories and tax records to reconstruct the elite's investment portfolios, as is illustrated in Excerpt 11.2.

Excerpt 11.2

A survey of the possessions of Mr Johan van den Bergh (1664–1755), former burgomaster of the city of Leiden, in the context of other probate inventories. Noordam, Dirk Jaap (1994). *Geringde buffels en heren van stand: Het patriciaat van Leiden, 1574–1700*. Hilversum: Verloren, 91, table 15 (sources between 1651–99); Prak, Maarten (1985). *Gezeten*

Burgers: De Elite in E. Hollandse Stad, Leiden 1700–80. Amsterdam: De Bataafsche Leeuw, 276, appendix 4 (sources between 1700–99).

On 15 December 1755 a notary in Leiden drew up a list of the estate of Johan van den Bergh, now preserved in the Leiden Municipal Archive, Notarial Archives 2044. During his lifetime, which lasted unusually long, Van den Bergh had been a councilor of his home town, served as alderman and burgomaster and represented Leiden in the States of Holland and in the Council of State. His assets consisted of the following items:

- *A house valued at fl. [florins/guilders] 16,800;*
- *Four small plots of land, together worth fl. 2,000;*
- *A very large number of government bonds, mainly on the province of Holland, with a total value of fl. 145,876, plus another fl. 15,200 administered by the local poor relief institution;*
- *Loans to four private individuals, including his son-in-law, as well as to a neighbouring village, to the tune of fl. 26,900;*
- *He also owned fl. 7,802 in cash and fl. 19,856 in foreign government bonds, all of them English;*
- *His total estate amounted to fl. 223,504, which made him almost twice as wealthy as his average colleague.*

One probate inventory can inform us about investment opportunities for the rich in eighteenth-century Holland. A whole series of these sources helps us to identify patterns of investment, as well as changes over time. The table below is the result of a compilation of these data for a total of 101 inventories across 150 years.

Investments of Leiden town councilors, 1650–1800

Percentage distribution and average values (in florins) according to probate inventories and estate tax records.

	1651–74	1675–99	1700–19	1720–39	1740–59	1760–79	1780–99
Houses	43.8	25.9	9.6	4.9	5.5	4.3	3.1
Land	14.6	15.7	8.5	10.9	11.4	7.1	7.6
Gov't bonds	25.5	46.6	53.1	68.1	54.1	54.9	70.4
Private lending	14.5	6.3	14.9	6.1	4.0	4.6	0.8

(Continued)

(Cont.)							
	1651-74	1675-99	1700-19	1720-39	1740-59	1760-79	1780-99
East/West India Companies	1.0	4.5	3.0	4.9	0.8	4.0	4.3
Miscellaneous*	1.0	1.0	9.9	5.0	24.1	25.4	13.9
Average value			110,183	113,567	124,664	200,357	195,155
N =	27	16	7	15	15	17	14

* *includes seigniorial rights, commercial investments (never above 6 per cent), and foreign bonds sold on the Amsterdam capital market (rising above 10 per cent from the 1740s).*

Probate inventories were made by notaries to help the inheritors with the division of the estate. These documents provide very detailed lists of the contents of the deceased's house, listing furniture, works of art, as well as underwear, napkins, and so on. On top of that, they provide a description of each item of wealth, allowing historians to gauge the changing size and composition of individual investments. In this case we are dealing with a town whose textile industry experienced a spectacular collapse from the 1670s, reducing in the process the size of Leiden from *c.* 70,000 around 1670 to *c.* 35,000 around 1750. Meanwhile, public debt rose spectacularly as a result of a series of wars between the Dutch, and later also the English, and France, starting in 1672 and stretching to 1713. Against this background it is first of all remarkable to see how the average estate increased against the tide of economic downturn. Secondly, we see a shift away from investment in private loans, often to businesses, towards government bonds. Dutch historians are still debating if this was a cause or the effect of the Dutch Republic's shifting economic fortunes.

The third example concerns attempts to use such local data to produce national figures. Lindert and Williamson follow this course in their improvement of Gregory King's numbers from 1688 (see Appendix 11.1). For their reconstruction of national income for Spain during its centuries of decline, Carlos Álvarez-Nogal and Leandro Prados de la Escosura combined daily wage data series, estimates of the percentage of working people in the population as a whole, another estimate about the number of days worked, and a third estimate of the labour share (as opposed to capital) in the national income. To establish the size of the non-agricultural sector, the authors used data about the percentage of the urban population in the various regions of Spain. Despite significant margins of error, their results are the best reconstruction we currently have of the trends in the Spanish economy (Álvarez-Nogal and Prados de la Escosura 2007).

This latter sort of work is the most speculative, but it has also contributed significantly to the creation of a comprehensive picture of economic developments of the period, at least for several countries, including England, the Netherlands, Italy, and indeed Spain. These reconstructions rely, as in the Spanish case, especially on three elements: urbanisation, wages, and national income.

Levels of urbanisation are seen as reflecting levels of economic development in two ways. On the one hand, more town-dwellers implies more people working in trade and industry, which have higher levels of productivity than agriculture. On the other hand, urbanisation provides a stimulus for raising agricultural productivity and assumes that farmers are producing sufficient surplus to feed not only themselves, but also the urban population. Obviously, this is a gross simplification: not all villagers work in agriculture; food does not have to be produced locally but can be imported. Still, these assumptions seem valid on a general level of abstraction.

As it happens, reliable lists of urban populations have been collected and published by Jan de Vries in 1984, which support a chronology that is broadly familiar. In the late Middle Ages the European urbanisation league table was headed by Italy, followed by the Low Countries. However, Italian urbanisation stagnated, allowing first the Southern Low Countries to take the lead and during the seventeenth century the Northern Low Countries, by then known as the Dutch Republic. Meanwhile, England was climbing to the top of the table very quickly. De Vries's figures were culled from numerous local studies, but are limited to towns that between 1500 and 1800 at one point or another held a population of at least 10,000. Sceptics might argue that this leaves out a substantial proportion, and worse, that the percentage of the urban population in smaller towns varied significantly between countries. This method therefore has several measurement problems.

The second measurement looks at real wages in industry. For this the building industry is often used, because the records are good, and because building technology was relatively homogeneous throughout Europe. The argument behind this standard is that real wages are a reflection of the levels of productivity in the industry, because in the long run employers have to earn enough from the fruits of their workers' labour to pay for workers' wages. They are also a reflection of the levels of productivity in the country more generally, because of competition between employers: very low wages in one sector force down the wages in another because the workers in the poorly paid sector would want to move to the better paid jobs, where over-supply would subsequently allow employers to reduce wages.

Robert Allen has compared wage series from the building industries in various European cities, including not only Italian, Dutch and English figures, but also data from Spain, France, Germany and Poland (Allen 2001, 2003). These series are usually reported in local currencies, so they have to be standardised by conversion to the silver equivalent of the currency. Nominal wages can distort the picture so we want to include the cost of living to arrive at so-called real wages. In the past, this was done by looking at grain prices, reflecting the cost of food,

but Allen has created ‘baskets of goods’ that include a much wider variety of foods, but also fuel, lighting, and linen, and takes account of the quantities that were consumed (Lapeyres index). Allen’s results show that in the first half of the sixteenth century the variations within Europe were relatively small. Three groups could be distinguished nonetheless: Antwerp, Amsterdam and London were the wealthiest towns, followed by several Mediterranean cities. France and Central Europe were the least developed according to this standard. During the early modern period, real wages remained broadly the same in the first group, but declined dramatically in the other two, creating a ‘divergence’ between north-western Europe on one side, and the other regions on the other.

To understand the third type of results, and their limitations, we need to have a sense of the procedures that economic historians have been following to arrive at the figures. This type of work employs a standard known as Gross Domestic Product (GDP), standing for the total size of a national economy. The latter will depend to some extent on the number of inhabitants; economists therefore prefer to use GDP per capita, which gives us essentially the average annual income of every person in that particular country. For the modern era we can reconstruct GDP with the help of the National Accounting methodology. This looks at inputs and outputs in the three sectors of the economy, i.e. agriculture, industry, and services (trade, but also government), and adds them up to a total figure. There are all sorts of issues – for example: how to measure output of public officials – but given the richness of the available data it has proved possible to do this more or less reliably.

Now wind back to the early modern era, where the necessary data are missing. Enter Angus Maddison. Maddison, who died in 2010, was a British economist who, through his work at the OECD, became fascinated by historical National Accounting and the possibilities to uncover long-term patterns of economic growth and divergence. From 1982 Maddison published various series of GDP/capita numbers for individual countries, initially starting around 1800 (Maddison 2003, 2007). Those numbers were regularly updated and refined, but they also became more ambitious in their coverage, ultimately going back to the Roman Empire and taking on board sub-Saharan Africa, where written sources are very scarce before the colonial era. To the untrained eye, Maddison’s numbers all look the same and they are also deceptively precise. How could he know that GDP/capita in Mexico was 755 dollars in 1600? The truth of the matter is that he did not. Many of his numbers are estimates, and in this case even ‘guesstimates’. Basically, what Maddison did was to start from reasonably reliable numbers and then through comparisons work his way backwards. One important benchmark was his use of the World Bank’s minimum income level for anyone to survive on: one dollar a day, or 350–400 dollars a year in 1990. To make his numbers comparable, Maddison used 1990 dollars throughout his tables. The figure for Mexico in 1600 tells us that Maddison had reasons to believe that the average Mexican in 1600 lived substantially above the minimum.

Nothing of what was said in the previous paragraph will have given the ordinary historian much confidence in the use of numbers. And indeed, even economic historians have raised serious concerns. They have pointed out that, for instance, if we would recalculate the numbers with 2003 dollars, major shifts would occur. They have also pointed out that many of Maddison's numbers present national averages whereas there were wide regional varieties within countries. And they have, with some justification, complained that Maddison was not very transparent about his sources or about the methods he used to arrive at his numbers (Deng and O'Brien 2017). Before we dismiss them altogether, however, let's give Maddison's supporters a chance to make their point. Their main line of defence is that his numbers should be read as a provocation, an invitation to all economic historians to produce more reliable estimates. Maddison's intuitions were, his supporters also claim, often pointing in the right direction. But more importantly: his provocations have worked. Efforts are underway for Japan, China and India, but for several European countries we now have sophisticated figures that go back well beyond the early modern era, into the High Middle Ages (Zanden and Ma 2017; Inklaar et al. 2018).

The most detailed and best-documented of these series was produced for England (from *c.* 1700 Great Britain) by a group of five scholars headed by Stephen Broadberry (Broadberry et al. 2015, ch. 4) (see Appendix 11.2). More or less simultaneously another series was published by van Zanden and van Leeuwen about Holland and later the Netherlands (Zanden and van Leeuwen 2012). These two publications contain the most reliable figures to date about the contribution of industry and services to the national income and their shares in the workforce (see the percentages in Tables 11.1a and 11.1b; a third study on central and northern Italy lacks sectoral breakdowns: Malanima 2010).

What can we learn from these numbers? Unsurprisingly, the share of agriculture declined in England as well as in Holland, although this decline was not as steep as one might have expected. Even by 1800 between a quarter (Netherlands) and a third (Britain) of the workforce in the two most advanced

Table 11.1a Sectoral shares in GDP and labour-force in England and Britain (from 1700), 1522–1801

	<i>GDP agriculture</i>	<i>Labour agriculture</i>	<i>GDP industry</i>	<i>Labour industry</i>	<i>GDP services</i>	<i>Labour services</i>
1522	40	58	39	23	22	19
1600	41		36		23	
1700	27	39	42	34	32	27
1759	30	37	35	34	35	29
1801	31	32	33	36	36	32

Source: (Broadberry et al. 2015, 194–95) (tables 5.01 and 5.02)

Table 11.1b Sectoral shares in GDP and labour-force in Holland (1510) and the Netherlands (1807)

	<i>GDP agriculture</i>	<i>Labour agriculture</i>	<i>GDP industry</i>	<i>Labour industry</i>	<i>GDP services</i>	<i>Labour services</i>
1510	27	39	35	38	40	22
1807	18	23	31	42	51	35

Source: (Zanden and van Leeuwen 2012, 125) (table 3)

economies of Europe was employed in the primary sector. Still, industry and trade together were bigger, at least in terms of GDP, even in the early sixteenth century, demonstrating that both economies were clearly moving away from the traditional reliance on food production. By the early nineteenth century the service sector had become the largest in terms of GDP in the Netherlands, while surprisingly for what was reputedly a late-industrialiser, the industrial workforce was larger than either agriculture or services. In Britain, both GDP and the workforce were divided remarkably equally between the three economic sectors, each claiming about one third of both GDP and the workforce.

The new data have revised Maddison’s figures, especially for the sixteenth and seventeenth centuries, by an order of magnitude of 25 per cent (cf. tables 10.01 and 10.02 in Broadberry et al. 2015, 373, 375). Italy, England and Holland were more affluent than Maddison assumed; only the revised figures for Spain more or less confirm Maddison’s original estimates for this period (see Table 11.2). These revisions reflect the more optimistic interpretation of the European economies before the Industrial Revolution that emerged in recent decades. This general impression is built on a great amount of detailed work that has been done on regional and local developments, as well as on investigations of

Table 11.2 GDP/capita in Europe, 1500–1800 (in 1990 dollars)

	<i>England UK*</i>	<i>Holland Neth’s**</i>	<i>Italy</i>	<i>Spain</i>
1500	1,114	1,483	1,403	889
1570	1,143	1,783	1,337	990
1600	1,123	2,372	1,244	944
1650	1,110	2,171	1,271	820
1700	1,563	2,403	1,350	880
1750	1,710	2,440	1,403	910
1800	2,080	1,752	1,244	962

Source: (Broadberry et al. 2015, 375–76) (table 10.02)

* UK from 1750

** Netherlands from 1800

individual branches of the economy. In other words, these numbers are the result of a collaboration between the ‘lumpers’ and the ‘splitters’ as they were memorably distinguished by J.H. Hexter (1979, 242–43).

Widening horizons and the expansion of trade

O’Brien’s 1982 article raised a question that is, even 35 years later, still difficult to tackle with reliable figures: was trade with the colonies marginal compared to trade within Europe? (Cf. Chapter 10: Expansion, space and people) We are not even thinking about domestic trade, because that is still more difficult to measure. In most places, ongoing trade statistics date only from the eighteenth century, when states began to keep records of the taxes they imposed on imports and exports. For earlier centuries we mostly have to make do with incidental observations, and of course the inevitable estimates, which use indirect sources of information. These can nonetheless give us a clue as to the value of trade and its geographical features.

Ulrich Pfister has analysed the Hamburg harbour records from the eighteenth century (Pfister 2015, 2017). Hamburg was at the time the port for a huge hinterland as far as Saxony, but also a major industrial centre in its own right, boasting over 200 sugar refineries. Between 1733 and 1798 complete records have been preserved for 36 years, allowing us to get a representative picture of Hamburg’s international trade (Weber 2015). Nonetheless, we have to acknowledge that some imports were exempt, notably those from the Dutch Republic, that prices were self-reported by merchants and therefore possibly unreliable, and that commodities were categorised inconsistently by clerks. Nonetheless we can safely say that among imports colonial goods made up almost half of the total value (it had been only a quarter in 1678), and over 70 per cent in the final decades of the eighteenth century. Within Europe, France was Hamburg’s most important trading partner. Hamburg thus constitutes a confirmation of the importance of colonial trade, although we cannot rule out that inclusion of the Dutch trade would alter the balance significantly.

For Holland, de Vries and van der Woude provide numbers that underline the precociousness of the Dutch Golden Age (de Vries and van der Woude 1997, 406, table 9.6). Employment estimates suggest that in 1610 Dutch European trade was five times as big as the trade with the New World, Africa and Asia combined: 19,500 versus 4,000 sailors. By 1680 this gap had shrunk to two times: 20,500 versus 10,500. Obviously, the value of the colonial cargoes was on average higher, possibly much higher, than those on European ships, but then again a sailor on a European ship could make many more trips during the time of a single journey to East Asia. As a matter of fact, de Vries and van der Woude have estimated that in the 1650s, from the total value of international trade of the Dutch Republic, 90 per cent came from exchanges within Europe. In the 1770s the absolute value had declined and colonial trade had become more important, but it was still just below a quarter of all Dutch international trade (de Vries and van der Woude 1997, 499, table 10.13). This is

especially significant because the Dutch Republic was a small country with a large colonial empire and commercial network. The fact that European trade remained dominant in the Dutch Republic underlines how easy it is to overestimate the importance of colonial trade.

Still, we cannot rule out the possibility that this colonial trade created a crucial impetus for other economic changes (O'Rourke et al. 2010; Palma 2016). This has indeed been the point of Jan de Vries' 'industrious revolution' thesis (de Vries 2008). As this is discussed at greater length elsewhere in this book we can just sum up his ideas here. (Cf. Chapter 7: Material cultures) On the basis of probate inventories in particular, de Vries argued that patterns of private consumption changed massively, by and large beginning in the seventeenth-century Dutch Republic. Colonial products like sugar, tobacco, coffee and tea were key to this change. They in turn required new industrial products, like pipes for smoking tobacco, and pots and cups for brewing and drinking coffee and tea. None of these could be produced at home; they had to be acquired through the market. To be able to do so, households had to increase their cash incomes, and this could only be achieved by working longer hours and involving women and children as well as the men. The resulting increase in consumption opportunities then encouraged other entrepreneurs to develop new industrial and service products, even agricultural products like cut flowers.

The growth of colonial trade was not necessarily at the expense of European trade. Indeed, an absolute expansion of trade during the early modern period has been well documented. In England total output in trade and transport increased fourfold between *c.* 1500 and *c.* 1600, fourfold again in the next century, and three times during the eighteenth century. In total the output around 1800 was forty times larger than it had been in 1500 (Broadberry et al. 2015, 164 table 4.07). Because the population had increased only fourfold, this means that trade and transport were growing ten times faster than the population. English trade may have been growing unusually fast, but substantial gains were made almost everywhere. How could this have happened? History textbooks suggest that it was due to the bright ideas of a handful of adventurers, like Magellan and Columbus, but their 'discoveries' cannot explain the growth of intra-European trade. Economic historians, moreover, are suspicious of explanations that rely on the acts of a handful of individuals, and want to understand how those 'discoveries' caught on. They have offered three alternative explanations: one technological, another organisational, and the third institutional.

Ship designs had improved dramatically in the late Middle Ages. This included the use of three masts, and a combination of triangular and square sails in full rigging. In subsequent centuries, additional improvements were made, but more incrementally. As a result, sailing qualities improved and cargo space increased (Unger 2011). Other technological improvements, concerning maps, clocks, sextants and compasses, helped ship crews to better locate themselves, and their destination, in the vast open space of the oceans, allowing them to cut crossing times (Davids 2008, 97–101).

A second, and probably more important source of growth in the transport sector was in the various aspects of its organisation (Lucassen and Unger 2011,

23–29; also Grafe, Neal and Unger 2010). By this we mean the way in which the activities on board and in harbours were set up. On board, for example, ships benefitted from the better knowledge of sea routes. With the thickening web of commercial contacts, ship crews were more likely to sail the same routes on numerous occasions, reducing search time for the best ways of tackling problems of location, avoiding obstacles, and so on. Improved harbour routines reduced the turn-around time. Ports invested in quays and docks, allowing easier access. Porter services were regulated, saving time and money for individual ships (Lucassen 2011). Sailing in convoy, under the protection of one or two fully armed ships, reduced the necessity to arm every individual ship, creating more space for cargo.

Whereas there seems to be broad consensus about the contribution of technology and organisation, the institutional explanation has been contentious. There are two issues at stake. One is the impact of institutions on trade: was it positive or negative? The other is the evolution of these institutions: were they basically the same in 1800 as they had been at the end of the Middle Ages? Sheilagh Ogilvie published an important book in 2011 that took up strong positions in both debates. She argued that merchant guilds, which originated in medieval Europe, were still widely active in the seventeenth and eighteenth centuries. In earlier work on guilds she had acknowledged that this was less true in the Low Countries and in England, but it was, she argued precisely for this reason that those two regions were able to develop more precociously than, for example, Central Europe (Ogilvie 1997, 412, 420, 436–37, 449, 475). Because – and this was her contribution to the other debate – merchant guilds were generally bad for the development of trade. Their main objective was to create rents, i.e. non-economic income streams from higher prices, procured through the exclusion of newcomers in the markets. In other words, by restricting competition, the members of the merchants guilds created profits for themselves (Ogilvie 2011, ch. 3).

To understand this second claim, we have to get a sense of the problems that international merchants faced during our period. These had to do especially with insecurity. Some of the insecurity was inevitable: ships sank and cargoes were lost; winter could set in early and cut off some harbours from the rest of the world; diseases at times wiped out entire crews. But many hazards were man-made. Due to poor communication international merchants had to trust their agents in far-away places. Busy trade routes were interrupted by pirates. Governments could order the confiscation of ‘enemy’ property. The point of commercial institutions was to shield merchants from some of these hazards. According to Avner Greif, who studied commercial institutions in the Mediterranean region during the Middle Ages, merchant guilds were a form of collective action that on the one hand allowed merchants to share risks between members, but also gave them leverage over cheating business partners, competitors and government agents. Basically, Greif argued, international trade would have come to a more or less complete standstill if it had not been for institutions that made risks manageable (Greif 2006, ch. 4).

Oscar Gelderblom studied commercial institutions in the Low Countries and broadly underwrote Greif's reasoning, but saw a much bigger role for municipal institutions (Gelderblom 2013). In the Low Countries, many towns were trying to lure merchants to their communities, because of the economic benefits emanating from international trade. One way in which towns could make themselves attractive was to offer protection against risks, as well as institutional mechanisms to settle conflicts. This bundle of institutions, Gelderblom concluded, helped international trade to expand and flourish. But as it did so, markets could increasingly take care of these issues by themselves. He found that in seventeenth-century Amsterdam merchant guilds had become insignificant. Public institutions, as well as the quality of information and the frequency of commercial interactions, had improved to such levels that merchants no longer felt the need for collective action.

This looks like a confirmation of Ogilvie's claim that the Low Countries were exceptionally early with their reliance on governments and markets and that the rest of continental Europe was still working in the traditional groove, with the Hansa and merchant guilds covering international trade. Unfortunately, Ogilvie has muddied the waters of her argument by including as merchant guilds the chartered companies that emerged in the seventeenth century for extra-European trade, such as the English and Dutch East India Companies. More than half of the examples from the seventeenth and eighteenth centuries in her book relate to such companies, which operated as a single business rather than the consortium of independent merchants of the guilds. Chartered companies were especially active in the Dutch Republic and England, the two countries that Ogilvie identified in earlier work as prospering because of their weak guilds. When we discount these companies, the number of examples she provides of 'genuine' merchant guilds from the seventeenth and eighteenth centuries looks distinctly smaller than the examples from the Middle Ages, suggesting that Gelderblom's point about the decline of merchant organisations is basically correct.

Another institution with a significant impact on international trade was the state. Various states aggressively sought to expand their commercial networks, a policy known as mercantilism. The Dutch in the seventeenth century and the British in the eighteenth are notorious examples (Findlay and O'Rourke 2007, ch. 4–5; O'Brien 2014). They used their navies to open up new areas for trade, especially outside Europe, subsidised trade ventures, and provided protection on the high seas if they could. States also used import duties to restrict access to their domestic markets for foreign competitors in favour of home-grown businesses, and gave formal permission to pirates to attack enemy shipping. There is a sense that all this made a difference, but how significant the impact of the state really was, is difficult to establish beyond the anecdotal.

To sum up: even though the trade between Europe and other continents had become a significant portion of international trade by 1800, our current figures suggest that trade within Europe remained more significant, not only in volume but also in value. The growth of trade was probably more important to the over-all growth of the early modern European economy than either agriculture

or industry. Progress in the sector was mainly due to greater efficiency in the organisation of trade, rather than technology, although this contributed as well. Overall, European trade relied less on institutions for collective action such as merchant guilds in this period. Governments, on the other hand, remained important actors, as providers of military protection and sponsors of overseas trade companies in particular.

Industry and the Industrial Revolution

The Industrial Revolution of the eighteenth century is the most important watershed in Europe's economic history since the emergence of agriculture some 12,000 years ago. There is no serious dispute about the fact that the first steam engines for industrial use were developed and installed in England. Why this happened is, however, as fiercely debated now as it was fifty years ago. This debate has been changed by new data and by new perspectives. Perhaps the most important of those new perspectives has been the comparison between developments in China, or Asia more generally, and Europe (more specifically England). This debate has come to be known under the title of the book that launched it: the 'Great Divergence'. In this book, Kenneth Pomeranz, an American expert of Chinese history, claimed that the most advanced regions of Europe and China were basically on an economic par in 1700, and that the Industrial Revolution was to an important extent the result of colonial exploitation, and of sheer good fortune (Pomeranz 2000, ch. 5–6). England had easily accessible coal reserves, encouraging it to develop the pumping technology to exploit them, whereas China's coal was difficult to extract, forcing it to rely on other energy sources, forsaking the possibility to develop steam technology.

In the context of the book for which this essay is written, it makes sense to disregard the Chinese side of the comparison and focus on Europe. Several issues have been raised from the European side. One is that Pomeranz was too pessimistic about Europe's level of prosperity (Allen et al. 2011). More fundamentally, perhaps, various authors have claimed that the Industrial Revolution was not a shock that more or less came as a surprise in the decades around 1700, but was rather the tail-end of a long process of economic development that included expanding markets, improved technology and supportive institutions – all more developed on the European side than in China. While this debate has led some scholars to develop an optimistic picture of the trajectory of the European economy (Zanden 2009, ch. 8), others have highlighted its limitations, underlining that many efficient technologies remained under-utilised because of poor institutions, for example (Epstein 2000, ch. 8). Still others have pointed to the unsystematic character of technological development in this period and how innovations were routinely opposed by the powers-that-be (Mokyr 2002, ch. 6). What are we to make of those contradicting opinions: was early modern Europe's industrial glass half full or half empty?

For a long time, the history of the Industrial Revolution was dominated by the emergence of the new steam technology, and how it transformed industry.

The new quantitative economic history changed that. It tried to look at the overall impact of steam on the transformation of English industry, in much the same way that O'Brien challenged the assumption that Europe's prosperity was built on the exploitation of the periphery. His verdict that it 'foundered on the numbers' applied equally to the Industrial Revolution. In 1976 Nick Crafts demonstrated that in terms of growth, the Industrial Revolution only became a significant factor in the British economy by the middle of the nineteenth century (Crafts 1976; also, 1985; Harley 1982). That verdict has now been generally accepted (Broadberry et al. 2015, 202).

Equally importantly, two historians of the Dutch Golden Age pointed out that in the Netherlands substantial growth had already been achieved during the seventeenth century, without the help of steam technology. De Vries and van der Woude (1997, 693) argued that the Netherlands could therefore be labelled the 'first modern economy', a prize that had traditionally been awarded to England (see Appendix 11.3). They argued that Holland had all the trappings of a modern economy, including unhampered market exchanges, levels of agricultural productivity that could sustain a significant division of labour, a state supportive of enterprise, and access to complex technologies and institutional arrangements that could sustain a consumer culture. Their critics (e.g. Zanden 2002), however, pointed out that Dutch growth figures never reached 'modern' levels of 2 per cent and more, rates that Britain did manage year-on-year from the 1830s, after half a century of 1.5 per cent annual growth (Broadberry et al. 2015, 199 table 5.03). Still, de Vries and van der Woude (1997) have managed to overturn the automatic assumption that serious economic growth was only possible with the invention of the steam engine. Others too have been more willing to embrace the idea that some pre-modern regions experienced significant economic growth well before industrialisation set in, and managed to sustain that growth over longer periods. Renaissance Italy and the late medieval Southern Low Countries have been singled out as examples, besides the Dutch Golden Age of the seventeenth century (Zanden 2001, 84–85; 2009, ch. 8; Persson 2010, ch. 4; see also de Vries 2001).

The quantitative history of European industry before the Industrial Revolution is still largely *terra incognita*. However, the best numbers we have – and these are tentative – seem to confirm the idea that already before the Industrial Revolution major changes were underway. These numbers concern the production of wrought iron in a handful of countries. Comprehensive figures on the textile industry would have been more helpful, because it was by far the most important branch of industry at the time. However, with iron being an intermediate, used by other industries, its trajectory is indicative of what happened in those other branches of industry as well. The numbers in Table 11.3 suggest a clear increase in the per capita output of iron between 1500 and 1800.

The presumed role of proto-industry (as outlined above) has been undermined by numerous local studies. In a survey published in 1996, Sheila Ogilvie and Markus Cerman listed a range of objections. The demographic dimension of the theory, plausible as it sounded, had not been borne out by

Table 11.3 Estimated output of wrought iron in Europe (in 1,000 tonnes)

	<i>Great Britain</i>	<i>Sweden</i>	<i>France</i>	<i>Germany</i>	<i>Europe, kg./capita</i>
1500	1	5	12	5	0.65
1700	17	7	-	-	1.60
1700	24	28	25	30	2.00
1740–50	27	40	-	-	1.5–1.90
1790	80	50	140	50	2.20

Source: (Zanden 2001, 83) table 4.5

detailed research. The predicted depression of living standards was found in some places, but in others living standards had risen. Proletarianisation was primarily caused by changes in agriculture, not proto-industry. The transition to factories and mechanisation happened in some proto-industrial regions, but by no means everywhere. This was the crux of the matter: if proto-industry was not the obvious predecessor of industrialisation, what good was the theory then?

As a result, the interest in proto-industry has declined markedly, but Jan de Vries has suggested that it might be saved from the dustbin of historical theories by recasting it as part of a broader restructuring of Europe's economies in the context of the 'crisis of the seventeenth century' (Parker 2013). De Vries' point is that the ruralisation of industry can be understood as a part of the same general process as the internationalisation of trade, or the rise of new consumer goods: something was stirring in these eighteenth-century economies (Gutmann 1988; de Vries 1984, 238–41, 1974, ch. 3; 2009, 191–93).

With proto-industry more or less discarded as an explanation for industrialisation, historians have been forced to look again at the steam engine. Where did it come from? Robert Allen (2009) has argued that its invention in England was due to a combination of two unique circumstances: high wages and easy coal. The former made it attractive for industrial entrepreneurs to invest in labour-saving technologies. Coal was crucial for two reasons: steam engines were first developed to drain coal mines, but they also required coal as fuel – and the energy consumption of the early steam engines was notoriously inefficient.

However, although wages and coal may have been necessary conditions, it is not obvious that they are also sufficient explanations. After all, the steam engine is a complicated piece of technology. Joel Mokyr and Margaret Jacob have therefore looked for the intellectual origin of the Industrial Revolution and found it in Newtonian science (Jacob 1997; Mokyr 2017, ch. 8). The century preceding the Industrial Revolution was, after all, one of momentous changes and improvements in Western science, which became more systematic and more mathematical, culminating in Newton's ground-breaking contributions. (cf. Chapter 12: Science and reason). To test its importance, Allen has subjected

a cohort of 79 ‘important inventors’ to a systematic analysis, showing that about half of all British inventors had ‘Enlightenment connections’ of the sort favoured by Jacob and Mokyr, while for the other half no such connection could be established. Allen’s results also show that the single most numerous social background of these inventors was the world of the crafts, suggesting a connection, not only with science, but equally importantly with practical skills (Allen 2009, ch. 10).

These explanations connect the Industrial Revolution to the debate about the role of guilds, and institutions more generally. Traditionally, and starting with the founding father of Economics, Adam Smith himself, guilds were seen as an obstacle to economic progress. This thesis seemed to be confirmed by the emergence of the Industrial Revolution in England (where the economy was least incorporated, or alternatively, guilds had been on the decline from the seventeenth century). Moreover, the Industrial Revolution started in the English countryside, where guilds had always been weak, in contrast to Württemberg, for example, whose merchant guilds imposed a very restrictive regime on a textile industry that failed to produce the sort of breakthrough technology associated with the Industrial Revolution (Ogilvie 1997, 2019).

This view has clashed with a more optimistic reading of the guilds’ history. From the 1980s, revisionist historians have argued that these institutions were as a matter of fact much more ‘flexible’ than Smith and his followers had assumed (Epstein and Prak 2008). Indeed, it was argued, this flexibility had allowed them to survive major transformations of the economy since their creation in the thirteenth century. Their demise at the end of the eighteenth century had not been the result of economic development, but of political decisions in the wake of the French Revolution (Haupt 2002). One particular contribution of the guilds to economic development, S.R. Epstein has argued, was their role in the production of human capital, where they helped smooth potential conflicts in the area of apprenticeship. The problem here was one of timing. In the early stages of apprenticeship, it was the apprentice who benefited from the master’s instruction. Later, the master would benefit from the free labour of a by now skilful apprentice. However, the apprentice might be tempted to leave prematurely, negotiate a wage for his labour services, and deprive his former master of his side of the bargain. The role of the guilds, Epstein claimed, was to tie both sides to the agreement, ensure that apprentices were trained properly and that masters would reap the benefits of their educational efforts (Epstein 1998).

Quantitative history has contributed to three elements of this debate. First, simple counting of the number of guilds has demonstrated that guilds were compatible with economic growth. The Dutch Republic’s Golden Age, for example, was accompanied by the establishment of an unprecedented number of guilds (De Munck, Lourens and Lucassen 2006). This does not, however, imply that they were also the cause of that growth, because a comparison of guild establishments in the Northern and Southern Low Countries and Italy has demonstrated that there was no strong correlation between the number of guilds and good times or bad times (Laborda-Peman 2017, ch. 1). Secondly, local histories of apprenticeship are making it increasingly clear that the guilds’ role in overseeing

apprenticeships was not always very strong, and was often supplemented or even taken over by private contracts between masters and apprentices or their parents and guardians (Prak and Wallis 2020). Thirdly, there is now strong evidence that in most towns and cities guilds were accommodating large numbers of immigrants in their ranks. The image of the closed guilds as it emerges from their regulations needs to be revised in view of their actual admission policies. These discriminated against women, but much less against migrants and other groups without previous ties to the guild community (Prak et al. forthcoming).

A very influential alternative institutional explanation was provided by Douglass North and various co-authors (North and Weingast 1989; North 1990, 138–40; North, Wallis and Weingast 2009, 213–19). North saw the pre-modern state as ‘predatory’, because it was not held in check by voters and their representatives. Predatory states infringed property rights, for example by levying taxes that hit ordinary people but exempted the rich. In England, this problem had been overcome with the Glorious Revolution and the Bill of Rights (1689), which gave Parliament the final say in financial decisions. This not only made property secure, but also gave investors the confidence to support new technologies and industries. Although North’s ideas about institutions are still influential, very few historians now believe this particular story about the Glorious Revolution. As Gregory Clark has shown, confirming most historians’ intuitions, property rights were reasonably secure in England (and on the Continent) long before the end of the seventeenth century (Clark 1996). If 1689 contributed something significant to the Industrial Revolution, it had to do with the new procedures worked out in Parliament, which benefited infrastructural projects (Bogart 2011).

Looming over these debates is the question of the nature of the Industrial Revolution. Whereas fifty years ago it was portrayed as a radical transformation concentrated in the span of a couple of decades, economic historians have extended the whole concept in both directions: a run-in of several centuries, as well as a long consolidation in the wake of the great inventions of the eighteenth century that created the foundations for the industrialised economies of the nineteenth and twentieth centuries. This implies several shifts of emphasis in the study of pre-modern industry. Firstly, from the transformations of the eighteenth century (steam engine, machines, factories) to micro-inventions and improvements of earlier centuries. This research is still in its infancy. Secondly, from technology to science and education more broadly, an area where important progress has been made. And thirdly, a shift from the various branches of industry as such, to the wider institutional environment in which they were operating. In this area strong claims have been put on the table, but the evidence is still haphazard and therefore inconclusive.

Conclusion

How has our understanding of trade and industry changed in the 35 years since O’Brien published the paper that launched our investigation? In the 1980s and 1990s the debate was dominated by the quest for the roots of ‘capitalism’.

Braudel and Wallerstein claimed to have found those roots in the development of global trade from the sixteenth century; Mendels and his followers pinpointed the rise of cottage industry; while Robert Brenner saw agriculture as the key sector (Aston and Philpin 1985). Since the 1990s capitalism has gone out of fashion, and then made a come-back. This has, so far, not led to a new paradigm, however. Instead, the debate has been dominated by attempts to produce more reliable numbers for key dimensions of the early modern economy and has looked for explanations in the institutional framework of that economy. The search for better quantitative indicators has produced some remarkable results, but is ongoing. Many territories of Europe await further investigation, and margins of error in present series suggest that improvements can still be made.

However, the even bigger challenge is in the interpretation. If we accept the proposition that at least some European economies had been able to grow significantly before the Industrial Revolution, what set those economies apart from the rest? If institutional explanations are the key, we have to acknowledge their dual character: institutions create agency through coordination mechanisms, and they create incentives through policies that make certain activities attractive. Of course they can also do the exact opposite: suppress agency and discourage activities. An explanation that sets ‘Europe’ as a whole against the rest of the world is not working; there are too many divergences within Europe. It also seems increasingly unlikely that we will be able to identify one single factor that set the ‘successful’ regions apart (Grafe and Prak 2018). Probably, a combination of institutional solutions worked best in an era when states and markets were growing but still relatively ‘thin’.

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Appendices

Appendix 11.1

Lindert, Peter H. and Williamson, Jeffrey G. (1982). Revising England's social tables 1688–1812. *Explorations in Economic History* 19, 385–408.

In order to improve our head counts for 1688, one of the present authors [Lindert] analyzed 26 local censuses taken between 1676 and 1705 which give occupations, as well as burial records from 41 parishes covering the period 1685–1714. Using regression analysis, these parish returns were extended to other places, the resulting predictions summed over all of England and Wales. Carefully comparing King with the new estimates and with other independent clues, we can reach a compromise set of 'best guesses' [...]. For the top of society, our regression-based counts by themselves are insufficient grounds for revising King. They are based on too few titled and skilled persons for sharp estimates. Fortunately, however, we have King's notations and Holmes' penetrating critique guide our modification of King's tables, and these support the hypothesis that King undercounted titled persons and overcounted professionals. Starting with 'Temporal Lords', the 40-odd lay holders of Irish and Scottish peerage whose main estates and residences were in England should be added. King's 12,000 figure for Gentlemen should also be raised to 15,000, based on recent findings of various scholars.

Within the professions, Harley challenged King's figures for the clergy, to which King gave unconvincing replies. We know that there were over 11,000 Church of England parishes in England and Wales as of 1801, and there were probably about 9000 in 1688. Vacancies among these seats were almost surely outweighed by the numbers of higher-ranked Church officials plus dissenting clergy. We have accordingly added 2000 lesser clergymen to King's figure [...]. King also appears to have exaggerated numbers in the Law, Sciences, and

Liberal Arts, judging from surviving records for London and market towns. We have accordingly scaled these numbers down from 26,000 to a more plausible level of 15,960, implied by accepting his overall total for all titled, professional, and skilled persons.

King was literally at sea on commercial classes. Merchants appear in his table only as a mere 10,000 ‘Merchants & Traders by Sea’, and he allowed for only 40,000 shopkeepers and traders. The archives strongly disagree. Guided by regression estimates inflated to national levels, the local archives suggest 26,321 merchants on land and sea and 101,704 shopkeepers and tradesmen, for a total of 128,025 in commerce (plus or minus a wide range of error). Just before his death King virtually agreed with this massive revision, postulating 24,500 merchants and a total of 151,000 persons in all commercial occupations for 1710–1711. Accordingly, we stand by the revisions [...]. For the remaining civilian middle and lower classes, King’s estimates and ours clash dramatically. The regression estimates repeat what we have already noted above from direct inspection of raw data: King must have underestimated the industrial and building trades, and overstated the numbers in agriculture, common labor, and poverty. His figure of 60,000 artisans in the Observations has to be one of his shakiest, as his own notes confirm. Was it really 60,000, or his own alternative guesses of 70,000 (p. 270 of Burns Journal) or 100,000 (p. 65 of Burns Journal)? All of his guesses for artisans seem too low to us, and we prefer the regression estimates instead. We shall also abandon his implausibly high estimates for freeholders and farmers in favor of the lower totals suggested by the archives and regression.

To square these occupational revisions with King’s plausible number of total families requires a reduction in the residual, the numbers in ‘poverty’. The regression estimates based on local archival data suggest the same revision. Indeed, so do the figures King himself assumed for poor relief expenditures and the numbers receiving that relief. King spread Davenant’s estimated £665,000 in poor rates over anywhere from 900,000 to 1,300,000 recipients of alms. The implied average relief per recipient of less than 13 shillings is at odds with much higher estimates reported in parish lists from London, Warwickshire, and Essex. We infer that King overstated the number of cottagers, paupers, and vagrants, and offer a downward revision [...]. [387–390]

Appendix 11.2

Broadberry, Stephen, Campbell, Bruce M.S., Klein, Alexander, Overton, Mark and van Leeuwen, Bas (2015). *British Economic Growth 1270–1870*. Cambridge: Cambridge University Press.

Estimation of GDP per head for contemporary economies is an intrinsically inexact process; for historical economies, with their less complete and more problematic datasets, precision is even more elusive. That is why here the data, methods and assumptions involved at each step in the estimation process have

been made explicit. Improvement is certainly possible with more and better production series for a wider cross-section of activities, but that is a task for the future. Although further systematic work in the archives is likely to repay dividends, certain statistical lacunae are bound to endure due to the paucity or absence of relevant historical information. Bridging the documentary discontinuity between the late-medieval and early modern periods will always present a challenge and obtaining direct evidence of many aspects of service-sector activity before 1700 may never be possible. Methodological resourcefulness will always be required if these gaps and discontinuities are to be overcome. Fortunately, the National Accounting approach offers a number of well-established ways of developing proxy measures of activities not directly recorded. Without resort to such measures the current estimates could not have been made.

With so many assumptions, qualifications and uncertainties, can historical national income estimates ever be convincing? Ultimately, their credibility hinges upon whether or not they can be falsified. That is why every effort has been made to ensure that those advanced here are free from internal contradictions and inconsistencies, especially where the outputs of one sector or sub-sector comprise the inputs of another. In addition, Part II of this book, 'Analysing economic growth', subjects these estimates to a number of tests. One of the most obvious is a comparison with the independent chronology of the returns to labour offered by real wage rates, especially the well-known rate indices of building and agricultural labourers. Real wage rates are often treated as surrogate measures of living standards and GDP per head, although, as Angeles has highlighted, they were also influenced by changes in the factor returns to land and capital as also by the market supply of labour per head (in terms of hours worked per day and days worked per year). Nevertheless, there ought to be some correspondence between real wage rates and real GDP per head; hence any significant divergences between them, such as occurred in the fifteenth and again in the late eighteenth centuries, need to be explicable. Reconciling the somewhat contrasting chronologies of these two measures of economic well-being is the subject of Chapter 6.

Two further cross-checks considered in Chapters 7 and 8 are whether agriculture and net imports together delivered enough food to feed the populations and whether total estimated national income, when disaggregated, was sufficient to meet the income requirements of all socio-economic groups. [...]

A final test of the credibility of these estimates of GDP per head for England 1270–1700 and Great Britain 1700–1870 is whether they make sense when compared with those now available for a number of other pre-industrial economies in both Europe and Asia. This helps establish whether they are of the right relative order of magnitude and clarifies when Britain overtook other economies. As Chapter 10 demonstrates, in Western Europe the key comparisons are with Italy, the leading economy of the twelfth and thirteenth centuries and still far ahead of all but the Flemish economy in the early Renaissance, and with Holland, which grew faster and became richer than any other European economy during the sixteenth and early seventeenth centuries. [214–15, 217]

Appendix 11.3

De Vries, Jan and van der Woude, Ad (1997). *The First Modern Economy: Success, Failure and Perseverance of the Dutch Economy, 1500–1815*. Cambridge: Cambridge University Press.

A ‘modern economy’ need not be one with the outward attributes of a twentieth-century industrial economy; rather, it should incorporate the generic features that make those outward signs possible. Foremost among those features are:

- markets, for both commodities and the factors of production (land, labor, and capital), that are reasonably free and pervasive;
- agricultural productivity adequate to support a complex social and occupational structure that makes possible a far-reaching division of labor;
- a state which in its policy making and enforcement is attentive to property rights, to freedom of movement and contract, and at the same time is not indifferent to the material conditions of life of most inhabitants; and
- a level of technology and organization capable of sustained development and of supporting a material culture of sufficient variety to sustain market-oriented consumer behaviour.

[...]

Although certain other European polities may have shared all these features for a time, the United Provinces can lay claim to being the first modern economy by virtue of continuity (it has been a modern economy ever since) and by virtue of its leadership in establishing the conditions for economic modernity over much of Europe. It became not only the commercial entrepôt for Europe; it also achieved Europe’s highest overall level of total factor productivity for the better part of the seventeenth and eighteenth centuries. That is, it became the first what Angus Maddison calls a ‘lead country’, operating nearest to the technological frontier and doing most to define that frontier, until it was dislodged from that position by Great Britain, by his reckoning, toward the end of the eighteenth century. Britain, in turn, ceded this place at the technological frontier to the United States toward the end of the nineteenth century. [693–94]