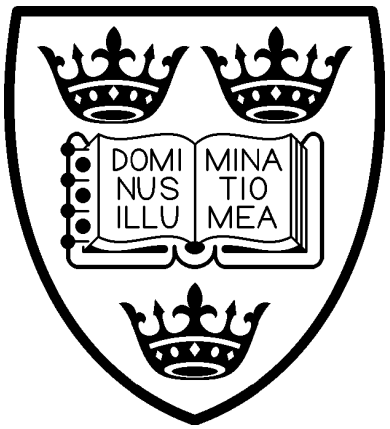

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Spinning their Wheels: A Reply to Jane Humphries and Benjamin Schneider

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Abstract

Jane Humphries and Benjamin Schneider have assembled several large data bases of spinners' production and wages that they believe disprove my view that high wages led to mechanization in eighteenth century England. This paper examines their data and shows that they have little value in understanding the incentives to mechanize. They collected thousands of observations of the earnings of women, but they do not know how many hours the spinners worked, so the data fail to establish whether their wage per hour (the relevant variable) was high or low. Another large sample of evidence concerned the production per day of spinners, but this information was mainly derived from schools and charity programs whose participants were selected because they were unproductive–so valid inferences about the productivity of women in general cannot be derived from these data. In addition, I present new evidence that substantiates my earlier estimates of productivity and earnings. The High Wage Hypothesis is unimpaired by the critique of Humphries and Schneider.

Keywords: industrial revolution, technical change, induced innovation**. JEL codes:** N13, N22, N63, O31.

A decade ago I advanced the thesis that the Industrial Revolution was triggered in Britain by the country's unusually high wages and cheap energy prices (Allen 2009a, 2009b). This price structure stands out when Britain is compared to other countries in Europe and Asia (Allen 2001, Allen, Bassino, Ma, Moll-Murata, and van Zanden 2011). English factor prices made it profitable to use–and, therefore, to invent–machine technology that raised labour productivity by increasing the use of capital and energy per worker. This technology was not profitable where wages were lower relative to the price of capital, and that is the immediate reason the Industrial Revolution was British. This argument was developed in detail for the spinning jenny, which was the first machine to be used commercially to spin cotton yarn (Allen 2009). This unusual price structure was attributable to the trade boom caused by Britain's imperial policies (Allen 2003, 2009).

My analysis of technical change and the history of wages has been modified and extended since its original formulation. In 2011 I respecified the model of the spinning jenny without any major changes in the results (Allen 2011). In 2015, I added a time series dimension to the analysis by showing that the profitability of using machinery in spinning increased sharply in the late seventeenth century. This was one reason that mechanization did not happen earlier (Allen 2015). It is this conclusion that Jane Humphries and Benjamin Schneider dispute and to which I respond here.

I have also reinforced the high wage thesis by showing how the perfection of the power loom was driven by the wage of handloom weavers relative to the cost of machinery (Allen 2018). More recently, I have shown that the technological pattern of the Industrial Revolution repeats on a global scale by using economy-wide data for seventeen countries to show that the leading economies have invented new technology that raised output per worker and the capital-labour ratio concurrently (Allen 2012). Acemoglu and Restrepo (2017) have captured (and magnificently extended) this view of technical change in their model of machine invention and labour displacement. Finally, the specification of the cost of subsistence, which is the usual deflator in many studies of real wage history, is being put on a sounder basis (Allen 2017).

The high wage explanation has been disputed by some historians for several reasons. I will review these in the conclusion. Here I concentrate on the recent critique by Humphries and Schneider. In considering the issues, it is worth remembering that the 'high wage' is relevant for answering two questions. The first is the issue of welfare, and much of the Humphries-Schneider paper demonstrates how little women earned spinning. I completely agree. However, I have also demonstrated that, despite the meagerness of their earnings, they made an important contribution to family income. Female spinners were the first example of mass technological unemployment, and the loss of the spinning income was a major cause of rural poverty in the late eighteenth century (Allen 2015, 2017).

The second way in which 'high wages' matter is the incentive they give to adopt and invent labour saving technology. Humphries and Schneider claim that the low earnings that they document were a cause of mechanization, but in this regard they are unconvincing. To support their view, Humphries and Schneider (2018, p. 28) cite Ure's *Philosophy of Manufactures* to the effect that 'the constant aim and tendency of every improvement in machinery [is]. . . to diminish the costs by substituting the industry of women and children for that of men.' The capital costs of a business usually rose when a machine process was adopted, so it was only profitable if there was a labour cost saving that more than offset the increase in capital costs. Ure wrote in 1835 when the big change in textile technology was the diffusion of the power loom. In this case, Ure (and Humphries and Schneider) are right. One reason the power loom cut costs was because it

allowed the substitution of low paid women for high paid male hand loom weavers, as I have pointed out (Allen 2018). However, the situation was different with mechanical spinning. In this case, there was no substitution of female for male labour since women (not men) did the spinning on hand wheels. Indeed, the most widely used machine in nineteenth century cotton spinning was the mule, which was operated by men, so it involved the substitution of men for women. The first spinning machine in wide use was the spinning jenny, which was operated by women, so it's adoption implied no change in the gender balance. The cost savings that offset the cost of the spinning jenny was the savings in female labour per pound of yarn spun multiplied by the woman's wage. Clearly, the lower the woman's wage, the less was the cost saving from mechanizing and the greater the likelihood that the machine would not pay. Cheap female labour was a disincentive to mechanization when it was a case of saving female labour, as it was with the spinning jenny.

The Humphries-Schneider data

Having dismissed its relevance for the overarching question of the origins of machine spinning, I will now consider Humphries' and Schneider's critique in more detail. They are to be commended for compiling a large sample of earnings and productivity records relating to spinning in the early modern period. While this evidence throws light on the income of domestic spinners (at least after 1730 when their sample is large enough to give reasonable results), it turns out to be irrelevant to the question of the choice of technique in spinning and the origins of mechanization..

I developed my view of the importance of high wages for the adoption of the spinning jenny by comparing the cost of the jenny to the savings in labour costs. To follow the Humphries-Schneider critique, it is important to recall the values I assumed for key parameters. I used evidence to support an assumption that a 'sturdy, hardworking' young spinner could spin one pound of low count yarn per day and earn 8d per day for that work in the first two-thirds of the eighteenth century. The 'pound-a-day' productivity figure was used by a variety of English contemporaries (Humphries and Schneider 2018, p. 9), and it is also found in French sources (Evrard 1947, p. 349), but Humphries and Schneider are dismissive of it. The average earnings of spinners were, of course, lower earlier as shown in Figure 1.

Contrary to the claim of Humphries and Schneider, I never contended that the average woman spun so much nor earned so much. Indeed, I assumed in both my analysis of the profitability of spinning machinery and of family income that the typical spinner only worked 40% of full time and earned only 40% of the full time earnings (3.2 d per day in much of the eighteenth century) [1] I relied on contemporary estimates of earnings to parameterize the models but recognized that contemporaries often referred to very wide ranges when they discussed 'typical' earnings. The discussion was more certain when they focused on good, full-time workers, rather than the wide range of earnings implied by the varying time worked by part-timers of varying ages and abilities.. So I worked with 'full-time' data where possible and deduced 'typical' earnings by multiplying full-time earnings by 40%.

[1]"I assume that women worked 40 per cent of full time." (Allen 2009, p. 214) "A spinner was assumed to work 120 days per year (40% of full time)." Allen (2015, p. 18).

The Humphries-Schneider data are impressive in their volume–thousands of observations–but they suffer from fatal defects when choice of technique is the question. I focus on their two largest classes of evidence.[2] One substantial class of records are those of poor law and other philanthropic schemes as well as spinning schools. These are all drawn from the middle of the eighteenth century. The data set also includes the records of a single commercial business that operated in 1628-30. Sample selection problems limit the utility of the inferences that might be drawn from this information for all but the commercial enterprise. Pupils were in a spinning school precisely because they did not know how to spin, so their performance is not indicative of the capacities of experienced spinners. Moreover, the women who received charity in any form were not a random sample of rural women. They were selected according to criteria, which were typically aimed at separating the 'deserving' from the 'undeserving' poor and restricting assistance to the former. The 'undeserving' poor were those who could support themselves; the 'deserving' poor could not–that is, their productivity was low. The Humphries-Schneider sample is (a) not representative of women in general and (b) biased toward low productivity workers.

Humphries and Schneider deal the issue of selection bias by claiming that the schools and work houses were run like commercial businesses. Maybe so, but the issue is not how they were managed, but rather the capabilities of the people in them. Since these workers were selected because they needed public assistance or instruction in the craft, the results based on these data are not generalizable and understate the performance of typical spinners in society at large. Indeed, the results of the charity schemes were far inferior to those of the single commercial business in the sample. Output per worker in the business was 10.48 lbs per week–almost two pounds per day!–not the miserable 1-3 lbs per week in the charity schemes. Humphries and Schneider (2018, p. 12) try to dismiss the high performance of the commercial enterprise by claiming the product was 'inferior yarn' and 'low grade work'. Perhaps so, but the labour productivity of the business was twice the conventional value of one pound per day and eight times the productivity of the charity programs. Without strong evidence of the quantitative implications of 'inferior quality' (not provided), the Humphries-Schneider dismissal is special pleading. The only plausible conclusion is that the business employed a better quality work force.

A second substantial class of records used extensively by Humphries and Schneider are those of individuals or businesses that show how much yarn a spinner spun and how much she earned in the interval between receiving the raw material and delivering the spun yarn. The problem with these data is that the number of hours that the spinner worked in that period are not known. Humphries' and Schneiders' Figures 2, 3, and 5 show scatter diagrams of the data, and the variance at each date is enormous. This is also clear from their Appendix Table S2 where decennial averages are reported as well as the ranges of the raw data. Four wages in wool spinning in the 1580s are reported, and they run from .71 d per day to 10.84 d with a mean of

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[2]The two classes I do not discuss are wage assessments, which are found only for the sixteenth and seventeenth centuries, and observations of contemporaries. The latter are hard to assess without a case-by-case examination to ascertain whether the wage reported is that of a full-time or a part-time worker. In many cases, it is impossible to say.

4.33 d. This is not an isolated example. In the 1750s, the range was .2 d per day to 16 d. per day. Why are the differences so great? Surely it reflects, at least in part, differences in time spent on the job. Humphries and Schneider cannot transform these data to a full time equivalent (FTE) basis, and that greatly diminishes their utility.

Humphries and Schneider are aware of the full-time/part-time problem, but their attempts to resolve it are inconclusive. Thus, they note that Eden (whom they earlier dismissed) reckoned that single women spinning full-time could make 6 lbs per week, while married women spinning part-time could produce 2.5 lbs. They compare the production per day of workers whose marital status they could determine and find that the difference in productivity differed by only 25%. "If our sample included a large share of part-time spinners, we would expect to see a much larger difference in the productivity figures for single and married women. The absence of such a disparity indicates a high prevalence of full-time working." Or a high prevalence of part-time work. It is impossible to tell, although the low levels of productivity suggest the predominance of part-time work.

What we would really like is output or earnings per hour worked, but early modern sources do not record that information. The best we can get is output or earnings per full day worked. To understand what the Humphries-Schneider data have to tell us, we need to compare either earnings or output per day in their data set to full time earnings or output. Figure 1 plots average earnings per day in the Humphries-Schneider (JHBS) data set and two full time earnings series. One is my estimates of the earnings of full-time spinners. This is the series that Humphries and Schneider claim is misleading. The other series is that developed by Humphries and Weisdorf (2015) in their recent paper on the earnings of women in early modern England. Humphries and Weisdorf present two series, one for women hired on annual service contracts, and the second for casual day labour. Figure 1 shows the overall trend as the average of the two series.

Several significant features stand out. The first is the agreement between my series of the earnings of full time spinners and the Humphries-Weisdorf (JHJW) series of the earnings of women. Correspondence is close at the beginning in the 1580s and during the eighteenth century up to the 1780s when spinners' earnings dropped in the face of machine competition. The main difference between these two series is that mine lags behind theirs in the inflation of the seventeenth century. The second observation is that the Humphries-Schneider series is much more erratic than the other two. The value of the JHBS series for the 1580s is implausibly greater than those of the other series, and this is confirmed as the value of the JHBS series is halved in the next decade! In the eighteenth century, there is a big upward jump in the 1710s and 1720s when the JHBS series exceeds both of the others followed by an equally pronounced crash in the 1730s. It is difficult to believe that the earnings of spinners were actually on this roller coaster, especially in view of the sedate movements of the other series. The sample sizes for the decades before 1730 are very small. For wool, which is far and away the most represented fibre, there are 60 observations between 1570 and 1730 for an average of 3.75 per decade. In contrast, there are 749 observations for the 1730s.[3] In view of the high volatility of the individual observations, one would expect the mean of a sample of 3 or 4 observations to

[3]See Appendix S2 of the Humphries and Schneider paper.

also have a very high variance, and that is surely the reason why the JHBS series shows such great volatility pre-1730. The third observation is that the Humphries-Schneider series is far below that of the other two in much of the seventeenth century and after 1730 when the JHBS samples are large enough to give stable results.[4]

Why are the Humphries-Schneider earnings so much below my series of spinner's earnings and the Humphries-Weisdorf series of women's earnings? Humphries and Schneider propose one explanation, namely, that the spinners were dispersed in villages where the manufacturers who hired them had the monopsony power to depress their wages. No evidence is offered in support of this possibility, and it is difficult to credit. Indeed, it is at odds with Defoe's (1724, p. 83) observation that yarn manufacturers and farmers competed against each other to hire young women. Details on the relations between merchants and spinners is scarce, but the Cottswolds is an example to consider. The Witney blanket industry was an important employer. In the eighteenth century, 60-80 blanket manufacturers in Witney engaged women in Cottswold villages to spin the yarn. About 7000 packs of wool were processed in a year (Allen 2017, pp. 15-18). If each woman spun 100 lbs per year (.4 lbs per day for 250 days), 16,800 spinners would have been employed. In addition, Cottswold wool was used in the cloth industry of the West of England. Yarn was purchased by weavers in local markets and from yarn merchants (Mann 1971, pp. 260-6). There were many thousands of spinners in the Cottswolds and many buyers of wool yarn. This looks more like a competitive labour market than a monopsony.

There is a much more likely explanation for the low earnings of spinners, namely, that most of them were only working part-time. In my previous work, as noted earlier, I estimated the earnings of the modal spinner as 40% of full-time earnings. Figure 1 also plots my estimate of typical part-time earnings. Before 1730, when the Humphries-Schneider samples are tiny, my estimates are below theirs and avoid their peculiar spikes. After 1730, when the JHBS samples are large, mine is in good agreement with theirs (Allen 2017, pp. 15-18).

Three conclusions follow: First, before 1730, the Humphries-Schneider data set is too small to provide credible results. Second, after 1730, but not before, the Humphries-Schneider series probably does indicate the typical earnings of spinners. Third, again post-1730, these results confirm rather than contradict my high wage model since my model predicts the earnings which they find.

The incentive of mechanized cotton spinning

I will now examine the incentives to mechanize spinning more closely. My previous analyses of this question were highly aggregated. I was trying to unearth the drivers of mechanization over a two century period. I made no distinction between the various fibres on the grounds that all were eventually mechanized. I postulated a stylized production process in

[4]Humphries' and Scheider's (2018, p. 2) claim that "spinners' remuneration shows trends similar to those found in recent evidence on women's day wages" is hard to fathom in view of Figure 1. Figure 9 on p. 24 shows the same erratic movements discussed here.

which a full time spinner could produce 1 pound of yarn per day for all fibres. A similar cost structure in all regions of the country were also assumed. Engagement with the evidence of Humphries and Schneider and other contributions to the wider debate requires a more disaggregated approach. This has much to commend it. While all fibers were eventually spun with machinery, cotton was nonetheless the first to be mechanized. The usual explanation for the early focus on cotton is that it presented fewer technical problems than other fibres. Not surprisingly, it also looks as though productivity levels in spinning varied among the fibres. So we must distinguish between them.

I focus on cotton. Humphries and Schneider contest my assumptions that a woman could spin one pound of cotton per day and earn 8 pence in the middle of the eighteenth century if she worked full time; however, their data have little bearing on the question for two reasons. First, almost all of their information is about other fibres. Second, they did not resolve the question of sample selectivity and the full-time, part-time issue just discussed.

We need different sources of information to examine the economics of cotton spinning. I consider two sources. One is well known, while the other is not. The well known source is Guest's *Compendious History of the Cotton Manufacture* (1823, p. 10). Guest presents costs for spinning cotton weft and weaving cloth in 1760. In his example, a merchant supplied a weaver with the warp and raw cotton for the weft. The weaver subcontracted for picking, carding, and spinning the weft. Most yarn was very coarse by later standards: Cloth was woven with counts 5, 6, 8, 11, 13, and 16. Guest's example pertains to 13 count weft. (By contrast, in the 1840s the average count in Britain was about 40.) Guest chose this, presumably, because it was a common grade, an inference supported by the next source of evidence we consider. In the event, spinning cost 9 d per pound and the cleaning and carding together came to a further 9 d.

Guest does not tell us how long it took to do these tasks. One way to find out is to divide the cost by a woman's wage rate. Defoe (1724, p. 83) made it clear that women shifted between spinning and annual service in response to changes in earnings. "The Farmers' Wives can get no Dairy-Maids...truly the Wenches answer, they won't go into Service at 12d. or 18d. a week, while they can get 7s to 8s a Week by Spinning."[5] We follow Defoe's lead and use the implicit daily wage of women hired on annual contracts as our measure of the value of their labour. This wage has the additional advantage that it was indubitably a return for full-time, full-year work. Humphries and Weisdorf (2015, p. 432) report the implicit average daily earnings of women hired as servants in the 1750s and 1760s as 8.275 pence per day. Dividing Guest's costs by this wage implies that 1.09 days of labour were required to spin one pound of cotton and that a total of 2.18 days were required for all the tasks in spinning including preparation and carding. These days are shown in Table 1.

Can we believe these labour requirements? Fortunately, we can cross check them with another source. In the early days of scientific management, Carroll Wright, the US Commissioner of Labor, organized a large scale data collection project to compare the time required to produce many important products by hand and by machine methods. "Briefly stated, this report is designed to bring into comparison the operations necessary in producing an article

[5]Defoe here understates the earnings of servants in the 1720s as Humphries and Weisdorf (2015) show in their recent study of women's earnings.

by the old-fashioned hand process and by the most modern machine methods, showing the time consumed by the workmen and the cost of their labor for each operation under the two systems." (Wright 1899, Vol. I, p. 11). Considerable effort was taken to secure accurate information.

> Having secured the data for the machine production, much greater difficulty was often experienced in securing reliable data for the hand production of the same article. In certain cases it was ascertained that the article, though produced quite largely by machinery in the more densely populated sections, was still made by the primitive method in some rural community. More often it became necessary to hunt for employers or workmen, long since retired from active life, who had once been engaged in the making of the article in question by the old-fashioned hand methods, and draw from them the needed facts. In such cases other employers or workmen were usually hunted out and the data submitted to them for verification, and, in addition, it may be stated that with scarcely an exception the facts gathered, after being arranged in the tabular form under which they are presented, were submitted to the original persons furnishing them, with request to verify or change as they deemed best. This was done for greater security, although each agent had months before, when he finished gathering the facts relating to an article, submitted his results to the person furnishing information, and together they had examined them and made any changes necessary. (Wright 1899, Vol. I, p. 12)

The products in the US study included six types of cotton cloth, cotton thread, and a small bundle of cotton yarn. Since the stages of production were distinguished and hours measured for each, the production of the yarn in the fabrics and thread can be readily identified. The cloths had cotton warps and wefts, and all yarn was spun by hand. The type of labourer doing the work is identified, and, in the case of yarn, the worker was always described as a 'house wife'. There is no indication that children were working, and this is consistent with Guest's description of English domestic practice in which both the spinning and the carding were done by women rather than children. In addition, the USA study reports the equipment used at each stage of the process: the carding was done with cards and the spinning with a spinning wheel. This was traditional practice.

It would be very helpful if the count of the yarn produced in the USA study were the same as the count in Guest's account, and, indeed, they were. In the American examples of yarn and thread, the count of the yarn is reported (12 and 6 respectively). In the cloth examples, we can work out the count from the construction of the cloth (the number of ends, that is warp threads, per inch by the number of picks, that is weft threads, per inch). In all cases, the number of ends and picks were equal or nearly so. These cloths were 'balanced' constructions. In the case of a balanced cloth, the relationship between construction and count is given by the formula count = 11 * square route of ends or picks.[6] Using the average of the ends and picks for each

[6]This was the formula used for Indian hand woven cotton cloth in the 1950s (Amalsad 1961).

cloth, this formula implies that the average count of the cotton yarn used in the six cloths in the American study was 13. This is the same as in Guest's example. I presume this was not a coincidence: A 40 x 40 cloth woven with 13 count yarn must have been a common construction–and thus worthy of study.

Wright's report distinguishes four stages in the production of yarn–cleaning the raw cotton, carding, spinning, and reeling (generally the warp yarn and occasionally the weft). All operations were performed in all examples, except for the case of thread where reeling was not undertaken. The hours of labour to perform each operation were recorded. I took the weight of the yarn produced to have equaled the weight of the cloth since the loss of material in weaving (unlike spinning) was negligible. From this information, hours of labour per pound of yarn can be calculated. I assume that a full day equaled 12 hours of work–the common standard.[7] Table 1 shows the implied number of days of labour required for each pound of cotton yarn.

Table 1 shows remarkable agreement in the overall labour requirements from the various sources. Guests' costs imply a total requirement of 2.18 days. My previous estimate came to 2.07 days. Carroll Wright's (1899) figures imply the same total. The breakdowns differ in detail: with respect to spinning itself, Wright's figures imply a requirement of 1.15 days per pound and Guest's costs imply 1.09 days. These compare to my assumption, based on contemporary generalizations, of 1 day per pound. This oft repeated contemporary figure does not look like a bad rule of thumb.

The new evidence on productivity and earnings figures discussed in this section is consistent with my previous analysis and are considerably higher than the figures presented by Humphries and Schneider. Their figures do indicate how much many individuals produced and earned. Their figures are lower than mine because they are the records of part time workers or, in some cases, workers in school or Poor Law Workfare programs.

Conclusions regarding Humphries and Schneider

The Humphries-Weisdorf wage series applied to Guest's cost data and the independent evidence of Carroll Wright's investigation corroborate the technological parameters used in my model of the profitability of the spinning jenny. The Humphries-Schneider data on the average incomes earned by spinners from 1730 onwards are consistent with my model since they are implied by my model. The samples underlying their evidence for earlier years are too small to

[7]For instance, Austin (1840) generally converts hours to days at this rate. Voth (2012) has attempted to measure the length of the working day from witness testimony in court cases. He found that the average difference between starting and stopping times in the mid-eighteenth century was 13 hours and 3 minutes (Voth 2012, pp. 164-5). His graphs indicate the variances in both start and stop times were very large, so the variance of the difference was also large. In other words, his data are consistent with much longer and much shorter working days. In addition, breakfast and lunch were consumed between the start and stop times. Voth has no information on how much time was devoted to eating, so his 'best guess estimate' that people worked 11 hours per day has a substantial conjectural element. The information he provides is consistent with the standard norm of 12 hours of work. My conclusions are not much affected by assuming an 11 hour day.

give reliable results in view of the high variance of the underlying data and that explains their otherwise inexplicably wild fluctuations. The invocation of Andrew Ure by Humphries and Schneider to claim that the employers of mechanical spinning were aiming to take advantage of low wage female labour confounds very different historical and technological contexts and does not stand up to theoretical scrutiny. The Humphries-Schneider view that low wages led to mechanization would warrant careful consideration if the Industrial Revolution had happened in India. But it did not.

While Humphries and Schneider have provided us with abundant evidence that many hand spinners earned very little money, their research supports rather than contradicts the high wage explanation of the Industrial Revolution since that is what my model has always implied.

Other Challanges to the High Wage Hypothesis

Humphries and Schneider are not the first to dispute the view that real wages were high in eighteenth century England and help explain the Industrial Revolution. Challenges relate to the specification of the model of the spinning jenny, the measurement of nominal wages, and the procedure to deflate nominal wages to obtain real incomes.

Gragnolati, Moschella, and Pugliese (2011) focussed on model specification. They argued that my comparison of the profitability of using the spinning jenny in England and France was incorrect since I modelled the impact of the jenny as a reduction in employment with output remaining constant, while a more realistic scenario would have kept employment constant with output increasing. In their view, this new scenario made the spinning jenny profitable in both countries. This scenario is, indeed, more realistic, and I reformulated the model, focussing on the situation when a spinner buys a jenny to operate herself, and showed that it was a profitable investment in England but not in France once account is taken of the related processing steps like carding and reeling. Indeed, I have used this reformulated model in all subsequent analysis of the jenny, e.g. Allen (2015).

Several commentators besides Humphries and Schneider have disputed various nominal wage series. Geloso (2018) has pointed out that the Strasbourg unskilled wage series for 1702-64 is low in comparison to that of comparable towns, and workers may have received food, which has not been taken into account. This is a perceptive point, but its implications are limited. The most important use I make of the Strasbourg evidence is in calculating the ratio of the wage to the user cost of capital. If the Strasbourg wage in this calculation is raised to that of neighbouring towns, the wage-capital cost ratio does rise but only by a small degree. The reason for this somewhat surprising result is that the wage is also an argument in the formula for the user cost of capital–building workers have to build the machines and the mills that house them–so the denominator of the ratio increases as well as the numerator, although to a lesser extend.

Malanima (2013) raised two issues regarding nominal wages that led him to conclude that Britain overtook Italy far later than I found. One was a small and not consequential upward adjustment to de Maddalena's wage series, which I also used. The other, far more important, revision was to reject London as the comparator for the North and Central Italian cities and to substitute southern English towns instead. Since their wages were lower than London's until the end of the eighteenth century, this shift improved the comparative standing of Northern and

Central Italy. Malanima's rationale for rejecting comparison with London it that it was growing exceptionally rapidly in the early modern period. But this is surely special pleading. In the middle ages, the Italian cities were in the first division, but they had dropped into the second division by the seventeenth century. To judge their performance by comparison with other second division cities is to downplay their fall from pre-eminence.

Stephenson (2017) argued that the London wage series that I and other historians have used to measure London wages were rates charged to institutions by contractors, and that these rates were higher than the rates paid to their employees. She found the records of one building contractor in London that showed wages he paid his workers, and she inferred from those records that London workers earned as much as 30% less than existing wage series show–a claim she repeats in her rejoinder to my comment (Stephenson 2018). This claim, however, is not born out by the data for two reasons. First, my craftsman's wage series exactly matches the higher mode of her bimodal craftsman's wage sample. So no adjustment there. This mode corresponds to 'masters' in the contemporary usage, while the lower mode corresponds to the earnings of so-called 'journeymen,' who made 15% less. Averaging across the two skill levels implies an adjustment of 8% (not 20-30%)—if we take all of this seriously. (Reasoning along the same lines implies a 17% adjustment for labourers.)

But, second, we should not take these calculations too seriously, for most building workers were employed on piece-rate contracts. These contracts incentivized them to work harder and faster in order to earn more money. So long as the extra earnings were less than the increase in labour productivity, costs fell, in which case, both workers and employers gained financially. That is why piece rate contracts were widespread. The implication is that workers actually earned more than the 'adjusted' wages just discussed and may, indeed, have earned more than the wage series I have used for London.

Stephenson has also 'adjusted' up the wage rates of continental countries to eliminate any British premium, but for this she relies on other historians and exaggerates their analyses. Thus, citing Geloso, she raises the Strasbourg wage series uniformly for 300 hundred years, although his critique concerns data only for the eighteenth century. Similarly, her adjustment of the Italian wage series is not warranted by Mocarelli (2004), her authority, who is much more circumspect in his discussion. Casual treatment of sources leads to mistaken conclusions.

Finally, some commentators have disputed the respectability and subsistence baskets used as deflators. In 2001, I developed the respectability basket, which was intended to reflect the spending pattern of an agricultural labourer in southern England in the late eighteenth century, However, it was always problematic since most workers in central and southern Europe could not afford it in the eighteenth century (Allen 2001). When investigations were extended to Asia, the situation became even more problematic, and the bare bones subsistence basket was developed as an alternative that provided spartan nutrition at minimal cost (Allen, Bassino, Ma, Moll-Murata, and van Zanden 2011). Humphries (2013) correctly pointed out that the original respectability basket provided insufficient calories, and that the calorie contend of the baskets was increased to 2100 per person in later versions of the baskets.

Lopez Losa and Piquero Zarauz, have critiqued the subsistence basket from another point of view. Oats were the main carbohydrate source in the subsistence baskets for England. In comparing England and Spain,Lopez Losa and Piquero Zarauz note that there is no counterpart to oats in Spain, so, instead, they design a basket with wheat bread since that was consumed in

both London and Madrid. The use of these baskets sharply lowers that measured real wage advantage of Northern Europe vis-a-vis Spain.

While Lopez Losa and Piquero Zarauz's Spanish basket is arguably a subsistence basket for Spain since no cheaper carbohydrate was available there, the English basket is manifestly not a subsistence basket in England since it cost far more than the oat basket. The reason for this difference is that Spain is too arid to grow spring crops like oats, which were the cheap carbohydrate in Northern Europe. It is important to remember that before the Black Death, most lower class English people subsisted on rye, barley, or oat bread, and a pottage made of oats and other spring grains–the English bare bones subsistence basket was not far from the norm.[8] The fact that by the eighteenth century, agricultural labourers in southern England were eating white bread rather than oatmeal attests to the economic growth that had taken place and the high wage economy that had emerged. This achievement is obscured by the procedure of Lopez Losa and Piquero Zarauz.

This observation raises a larger issue: An assessment of the high wage hypothesis cannot be complete without locating the discussion in the wider context of the performance of the English economy before and during the Industrial Revolution. Many developments occurred that make sense if real wages were rising and reached a high level. Otherwise, they do not, and it is incumbent on opponents of the high wage hypothesis to account for them. I consider five.

Economic development

The English economy was one of the most successful in the early modern period, featuring high rates of structural transformation, urbanization, proto-industrialization and growth in per capita income and agricultural and industrial output. This economic dynamism transformed the Malthusian relationship between wages and population: traditionally, when the population increased the real wage decreased and conversely. After 1600, the population and real wage increased in step with each other (North and Thomas 1973, pp. 116-9, Clark 2005, p. 312, Allen 2017, p. 27) This rising trend in wages led inevitably to a high wage economy in the eighteenth century.

Improving diet

As noted, British diets improved between the middle ages and the nineteenth century. Furthermore, the geographical pattern of improvement coincides with the geographical pattern of wage increases. In the middle ages, the labouring population, as noted, and the poor ate coarse bread and a potage of oats and legumes. The well-to-do ate wheat bread. After the Black Death, the consumption of wheat, meat, and ale increased among labourers (Dyer 1988). Between Gregory King's account of 1688 and the Essex miller Charles Smith's analysis of 1764, the share of wheat in English bread consumption rose from 20% to 62.5%. By mid-eighteenth century, wheat bread and was the norm for all classes across southern England (de Vries 2008, p. 167, Collins 1975, pp. 98-9). This increase coincided with the rise in real wages in southern English towns as their wage levels rose towards London levels. In the nineteenth century, wheat bread displaced oats and barley in northern England, as wages their also converged upwards to the

[8]Campbell (2000, pp. 238-47), Dyer (1988, 1989, pp. 151-160). Campbell, Galloway, Keene, and Murphy (1993, p. 42) estimated that about 40% of the calories produced from grain in England around 1300 came from oats and spring grain mixtures.

London level (Collins 1975).

Greater stature

In the eighteenth century, the tallest men in the world were white men in the future USA at 172-3 cm and English men at 169-72 depending on issues of measurement. The English were followed by other northern European, who were, in turn, taller than central and southern Europeans. The latter populations had experienced pronounced falls in average height as real wages dropped in the eighteenth century. Emigrants from South China were 163 cm, while Japanese soldiers were 157 cm. Indigenous natives and peasants in Latin America averaged 153-9 cm. The regions with the short heights were the regions with low real wages and conversely (Allen 2015, pp. 11-12)..

consumer revolution

England had a consumer revolution in the eighteenth century, in which the consumption of housewares, books, mirrors, and other amenities increased. The upper and middle classes led the way, but the working class also participated in the consumer revolution (Brewer and Porter 1993, de Vries 2008). Clothing consumption, for instance, was markedly higher compared to the middle ages (Dyer 1989, pp. 175-7, Styles 2007). Adam Smith (1776, pp 821-2) famously discussed the English labourer's need of a linen shirt and claimed it indicated that 'subsistence' was a social rather than a biological phenomenon. The alternative explanation is that eighteenth century England was a high wage economy in which farm workers were able to afford more and better clothing–a view, as it happens, that Adam Smith also endorsed.

contemporary commentators I am, of course, not the first to propose that England was a high wage economy. This idea was a staple of eighteenth century political economy. Adam Smith (1776, pp. 74-5, 91, 187, 206) thought the Dutch and the English had the highest real wages in the world. Scotland trailed England, and France was even further behind. All of the European countries were ahead of China and India, however. Malthus (1798, chap 7) agreed:

> The labourers of the South of England are so accustomed to eat fine wheaten bread that they will suffer themselves to be half starved before they will submit to live like the Scotch peasants. They might perhaps in time, by the constant operation of the hard law of necessity, be reduced to live even like the Lower Chinese, and the country would then, with the same quantity of food, support a greater population.

Arthur Young (`1793, Vol. II, pp. 316-7) compared wages to prices in England and France and concluded that "All those classes" in France that support themselves by "labour and are the most numerous in society, are 76 per cent. less at their ease (if I may use these expressions), and worse fed, worse clothed, and worse supported both in sickness and in health, than the same classes in England." Modern research has extended the range of these comparisons both temporally and geographically but not substantively altered the conclusion.

Days of labour per pound of Cotton Yarn

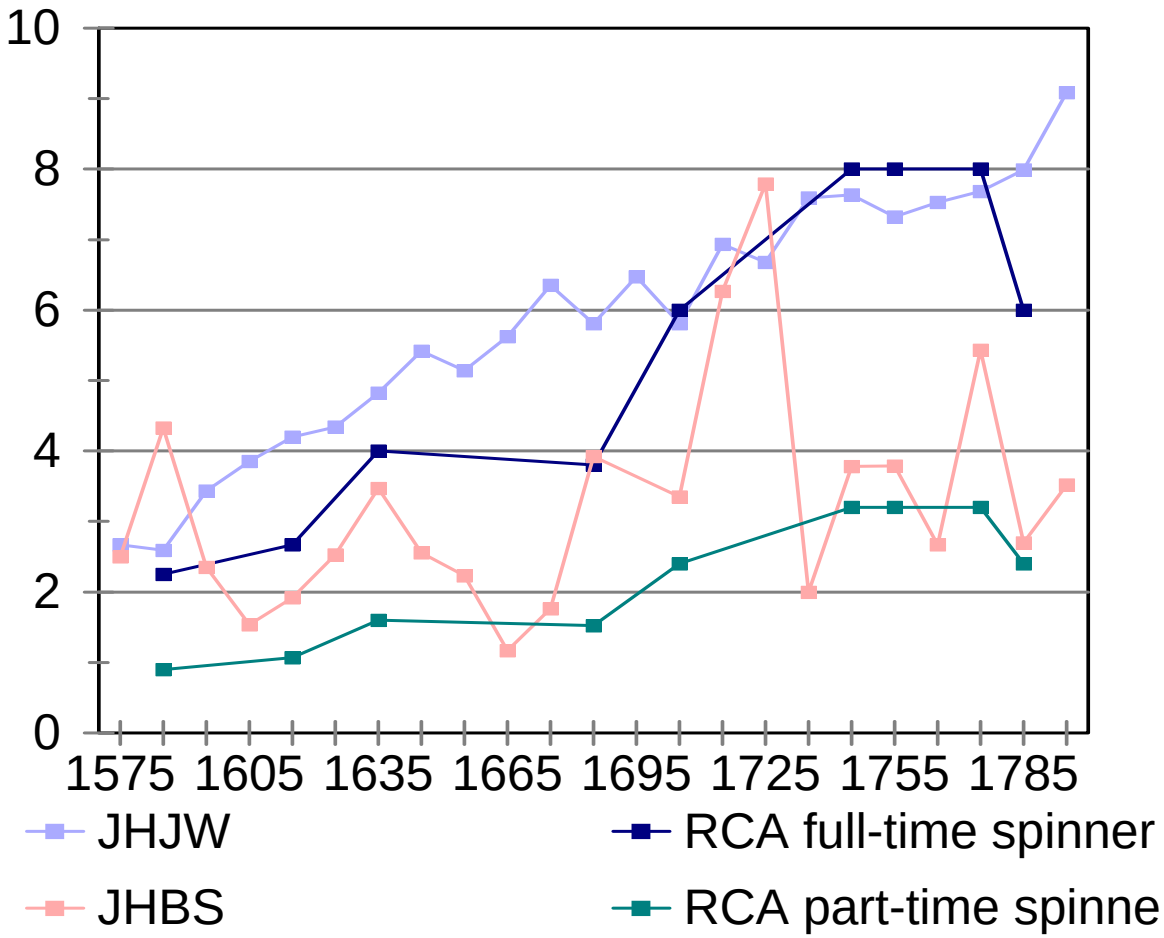
	Guest 1760	Allen 2009	US Commission of Labor
preparatory		0.20	0.03
carding		0.80	.81
spinning	1.09	1.00	1.15
reeling		0.07	.08
non-spinning tasks	1.09		
total	2.18	2.07	2.07

Sources:

Guest (1823, p. 10)
Allen (2009, p. 185).
Wright (1899, Vol. II, pp. 956-77) The figures reported here are weighted averages of the hours per pound of product (cloth, yarn, or thread) where the weights are also pounds of product.

Figure 1

Spinners' earnings (pence per day)



Source:
JHJW–Humphries and Weisdorf (2015, pp. 431-2) average of casual and annual means.
JHBS–Humphries and Schneider, (2018, Appendix Table S2.).
RCA full time spinner–spreadsheet 'London, wages:S' The figure of 12 pence per day for 1760-4 has been excluded from the calculations.
RCA part-time spinner–40% of earnings of full-time spinner.

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