Time and Work in Eighteenth-Century London

HANS-JOACHIM VOTH

Witnesses' accounts are used to analyze changes in working hours between 1750 and 1800. Two findings stand out. The article demonstrates that the information contained in witnesses' accounts allows us to reconstruct historical time-budgets and provides extensive tests of the new method. Estimates of annual labor input in 1749/63 and 1799/1803 are presented. It emerges that the number of annual working hours changed rapidly between the middle and the end of the eighteenth century. These findings have important implications for the issue of total factor productivity during the Industrial Revolution.

According to conventional wisdom workers during the Industrial Revolution toiled longer in 1850 than they had a century earlier.[1] In his pathbreaking article "Time, Work-Discipline, and Industrial Capitalism," E. P. Thompson, the most prominent proponent of this view, argued that "Saint Monday" (the practice of taking Monday off to recover from the weekend) was universally observed until the beginning of the nineteenth century.[2] Once it began to disappear under the impact of the factory system, total workloads began to rise rapidly. In addition to the increase in labor input, work discipline increased sharply.[3] Preindustrial work was characterized by irregularity. The allegedly slow pace of work on Tuesdays and Wednesdays is said to have gathered pace gradually during the course of the week, culminating in a frenetic rush at the end of the week to complete work. The Industrial Revolution thus transformed work patterns that were irregular and often proceeded at a leisurely pace into the iron discipline of nineteenth-century cotton mills.

The importance of holy days in England before and during the Industrial Revolution has been a matter of discussion for some time.[4] Herman

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[1]Reid, "Decline"; Briggs, "Work," p. 98; Freudenberger, "Arbeitsjahr," pp 314–16; Jones, *Agriculture*, pp. 116–17; and Pollard, "Labour," p. 162.

[2]Thompson, "Time."

[3]Ibid., pp. 74–76.

[4]Rule, *Experience* and *Labouring Classes*.

Freudenberger and Gaylord Cummins added another aspect to the issue of labor intensification when they argued that the observance of holy days was sharply reduced during the eighteenth century.[5] The basis of their contention is a list of holy days contained in a handbook published by J. Millan in 1749.[6] He gives 46 fixed days on which work at the Exchequer and other government offices ceased. Later, during the second half of the century, the observance of these holy days is said to have vanished slowly. Consequently, Freudenberger and Cummins argue, annual labor input possibly increased from less than 3,000 to more than 4,000 hours per adult male between 1750 and 1800.[7] The cause of this rise in labor input was the increased availability of food. As nutrition became more plentiful, people had less of an incentive to save on energy by maximizing the number of days of idleness. Thus, old feastdays gradually began to fall into disuse. More recently, Jan de Vries has argued that working hours must have been rising rapidly in early modern Europe since the increased standards of consumption cannot be explained by the course of real wages. An "Industrious Revolution," giving rise to a maximum 307-day working year, must have been responsible for much of the wealth found in probate inventories.[8]

Unfortunately, the empirical basis for these views is weak. Thompson largely relied on literary sources. As many critics have argued, these are difficult to interpret and are unrepresentative as well.[9] Freudenberger and Cummins point to holy days mentioned in contemporary calendars. However, knowing that a day was officially recorded as a holy day is not the same as determining that it was a day off. Even De Vries's elegant argument relies on indirect evidence of an increase in working time. Reasonably accurate estimates only become available from the 1850s.[10] The verdict in the profession is unanimous. N.F.R. Crafts, commenting on the substantial body of literature that suggests an increase in the number of working hours per year observed that "[m]easurement of this supposition has never been adequately accomplished."[11] Joel Mokyr concurs:[12] "We simply do not know with any precision how many hours were worked in Britain before the Industrial Revolution, in either agricultural or non-agricultural occupations." The following section describes a method that is designed to fill this void in the historical record.

[5]Freundenberger and Cummins, "Health."
[6]Millan, *Coins*, p. 15.
[7]Freudenberger and Cummins, "Health," p. 6.
[8]De Vries, "Between Purchasing Power," pp. 107–114 .
[9]Rule, *Experience*; Hopkins, "Working Hours"; and Reid, "Decline."
[10]Matthews et al., *British Economic Growth*; and Maddison, *Dynamic Forces.*
[11]Crafts, *British Economic Growth*, p. 82.
[12]Mokyr, "Industrial Revolution," p. 32.

COUGH PILLS AND THE LAW: DATA AND METHOD

The "Proceedings of the Sessions of the Peace, and Oyer and Terminer for the City of London and County of Middlesex" are a colorful source for modern historians. They came into existence as a precursor of the modern "yellow press". Interest in sex and crime has always been buoyant, and it was in the second half of the seventeenth century that entrepreneurs began to print reports about the proceedings at the Old Bailey in order to satisfy this demand. During the 1720s, the publication as a whole became much more respectable—even if it still contained advertisements for anything from cough pills to remedies against syphilis. During the 1720s, verbatim reporting was introduced.[13] For our purposes, the reports from the Old Bailey become truly useful after 1748. It was in this year that Thomas Gurney began to take down the proceedings in shorthand. He and his son continued to act as scribes for the next 35 years. Although the publisher changed with considerable frequency, the reports from the courtroom maintained a high degree of precision and detail. [14]

Data collection was carried out for two periods: from 1749 to 1763 and from 1799 to 1803.[15] A total of 7,650 court cases were evaluated, leading to a little over 2,000 observations.[16] In the majority of cases, a lack of information either on the time of the crime or the witness led to the exclusion of a case from the dataset. For obvious reasons, information from the accused was not included. The scarcity of sufficient information was more pronounced for the earlier period, when data collection had to be carried out on records from 14 years to collect a dataset of sufficient size. In 62 cases, witnesses' accounts were ruled not to be admissible evidence before the court and were consequently excluded—even if the lie did not pertain to time-use information.[17] It is likely that some inaccuracies, even gross misrepresentations, went unnoticed before the court. In so far as they relate to time-use, this is not necessarily a grave problem: the witness was obviously able to invent a probable, possibly even a typical activity pattern. There is also little evidence that witnesses attempted to create an ideal image of social respectability before the court. Those called to give evidence showed few inhibitions, relating freely that they "went awhoring" or gave someone

[13]Harris, "Introduction," pp. 10–11.

[14]Ibid., pp. 11–12.

[15]When a trial was held in 1800 for a crime committed in 1799, these observations were also entered. The same applies to 1749/50.

[16]The number of occasions when a single trial led to more than one entry was small.

[17]A typical example reads like this: "The jury declared they believed but very little of what Tindal had sworn; and not a word that Woolf, Trueman, and Pretyman had sworn: And desiring that the three last might be committed for perjury, they were committed accordingly." City of London, *Old Bailey Sessions Papers*, Case No. 73, 1756.

"a good licking."[18]

Crimes are committed on all days of the week, during all seasons of the year. All hours of the day are present in the sample. We can thus replicate a method for measuring time-use that modern-day sociologists favor: random-hour recall.[19] In modern surveys, individuals participating in the study are asked to provide a thorough description of their activities for a randomly chosen hour of an earlier day. Very much the same occurs in front of a court when witnesses are asked to testify. Witnesses very often not only mention their occupation and sex (and, in a substantially lower number of cases, age and address), but also report what they were doing at the time of the crime, at the time when they last saw the victim, or when they observed the perpetrator trying to escape.[20]

The parallel between court records and sociological surveys using random-hour recall is not exact. We shall treat the various potential objections to this approach after a brief overview of the main results.

IN THE SWEAT OF THEIR BROWS: TIME USE 1750 TO 1800

The average witness during the 1750s rose shortly after 6:00 A.M. A total of 59 individuals gave evidence before the court about their time of rising in the morning. The earliest riser in the sample is a publican who got up at 2:00 A.M. on July 4, 1756, to go "a mowing."[21] No individual rose later than a domestic servant, who, on Sunday, March 14, 1759, remained in bed until 10:30 A.M. These extremes were highly unusual. Half of the sample rose between 5:00 and 7:00 A.M. Given the wide dispersion in the sample as well as the limited sample size, the 95 percent confidence interval is quite wide, extending from 5:41 A.M. to 6:39 A.M.[22]

[18] *Old Bailey Sessions Papers*, Case No. 101, 1752.

[19] There are three principal techniques: electronic pagers, which emit an acoustic signal at random intervals, diaries, and the random hour method. These methods are not all equally useful: "With an unlimited budget, one would pick the random-hour method; budget limitations argue for the diary." Juster and Stafford, "Allocation," p. 484.

[20] Furthermore, this information also fulfills another requirement established by time-use research: "The only way in which reliable data on time allocation have been obtained is [from] a sample of individuals in a population and organized in such a way as to provide a probability sample of all types of days and of the different seasons of the year." Ibid., p. 473.

[21] *Old Bailey Sessions Papers*, Case No. 300. 1756.

[22] In addition, a further problem arises. Some statements by witnesses are not very precise. Although most give the exact time of rising in the morning, 25 percent are only precise to within one hour. The overall impact, however, is quite limited—we have to widen the confidence interval by another five minutes on both sides. The mean for the relatively imprecise observations is 6:38 A.M. Without these observations, the overall mean would have been 6 A.M. Let us assume that all of these individuals had been much closer to the lower bound of the range than to the upper bound: every time a witnesses claimed to have risen between 3:00 A.M. and 4:00 A.M. , he or she would have left bed at 3:10 A.M. (instead of the 3:30 A.M. that we assigned). Every single one of our observations in this category would then have introduced an error of 20 minutes into the calculation of the mean. It seems inherently unlikely that they would have all erred on the same side. Even if this had been the case, the effect on

Work during the 1750s began shortly before 7:00 A.M. On average, the 44 witnesses started work at 6:50 A.M. Before 6:00 A.M., only one-quarter of the individuals who gave evidence were already at their workplaces. The vast majority of witnesses started work between 6:00 and 7:00 A.M. Such an early start to the working day was not everyone's lot; in 1759, we find a stockbroker who began work at 10:00 A.M.[23] Work stopped at 6:50 P.M. on average. This average also includes the many unskilled laborers who were employed on an occasional basis and often finished their daily work during the early afternoon. Skilled craftsmen, apprentices, and masters often worked until 7:00 P.M. or 8:00 P.M.[24]

On average, the witnesses giving evidence before the Old Bailey went to bed at around 11:00 P.M. The statistical average is 10:50 P.M., and we can be 95 percent certain that the mean for the underlying population was between 10:30 and 11:10 P.M.

Fifty years later, we find 34 individuals reporting their time of rising in the morning 5:56 A.M. on average. Given the wide confidence interval, we cannot claim that witnesses rose much earlier than their ancestors during the middle of the eighteenth century. Work began at half past six now (6:33 A.M. on average), a little earlier than in the first sample. Also, 44 witnesses reported their time of stopping work before the court. The average time is 7:07 P.M., but because of the large variation and the relatively small sample, the 95 percent confidence interval extends from 6:30 to 7:44 P.M. Londoners in the sample were not only early risers, they also went to bed rather late. Unsurprisingly, the latest bedtimes seem to have been the result of important social events: On December 24, 1800, a journeyman tailor was being entertained and danced at his master's house until he finally went home at 4:00 A.M.

Mondays and Holy Days

The witnesses giving evidence before the Old Bailey during the 1750s were very likely to take Sunday and Monday off and to work on Saturdays. I regressed a dummy variable indicating if a person worked on a given day of the week.[25] The use of a logit regression is necessary since the dependent

our estimate of the overall mean is nonetheless small. If the less accurate statements were all off by 20 minutes in the same direction, then a maximum bias of five minutes would have been introduced (0.25 × 20). If such a systematic form of imprecision existed, we would have to revise the average to 6:05 A.M. Similarly, if every single witness in this category had erred on the high side, the upper bound would be 6:15 A.M. The overall confidence interval therefore has to be widened by five minutes on either side. Compared to the error bands arising from the statistical properties of our data, the maximum inaccuracy introduced by using midpoint estimates is small.

[23] *Old Bailey Sessions Papers*, Case No. 317, 1759.

[24] This is very similar to the figures given in contemporary accounts of working hours. Compare Campbell, *London Tradesman*, pp. 331–40.

[25] Hardy, *Regression*.

TABLE 1
LOGIT REGRESSIONS
(Dependent Variable: Individuals Engaged in Work = 1)

Weekday	B	Wald	Odds Ratio	Significance
1749–1763				
Sunday	-0.66	4.24	0.52**	0.039
Monday	-0.51	5.14	0.59**	0.023
Tuesday	-0.11	0.23	0.89	0.62
Wednesday	0.23	1.32	1.26	0.25
Thursday	0.15	0.55	1.17	0.46
Friday	0.07	1.07	1.07	0.74
Saturday	0.43	4.53	1.54**	0.033
1799–1803				
Sunday	-0.64	23.1	0.53**	0.04
Monday	-0.21	0.99	0.81	0.32
Tuesday	0.38	3.7	1.45*	0.055
Wednesday	0.12	0.33	1.13	0.56
Thursday	-0.11	0.28	0.89	0.59
Friday	0.19	0.89	1.22	0.35
Saturday	0.27	1.95	1.31	0.16

* = significant at the 90-percent level.
** = sgnificant at the 95-percent level.
Notes: The Wald-test has a χ^2 distribution; significance levels are according to Hauck and Donner, "Wald's Test," pp. 851–53. The odds ratio measures the change in the odds and is defined as e^B.
Source: See text.

variable is dichotomous.[26] Results are reported in Table 1. Three days of the week are significantly different from all others: Saturday, Sunday and Monday. Sunday, and Monday are very clearly days of rest, showing large reductions in the probability of finding people at work. Saturdays record an above-average incidence of work.[27]

Repeating the exercise for the beginning of the nineteenth century yields different results. Sunday is still clearly a day of rest, but the prominent position of both Mondays and Saturdays has vanished. There is still a slight reduction in the probability of observing witnesses at work on a Monday, but it is not significant at any of the customary confidence levels. Surprisingly, Tuesdays now appear to record a slightly higher incidence of work, whereas Saturdays no longer show an unusual incidence of work.

Similar changes can be observed in the case of old religious and political holy days. I examined whether the witnesses were less likely to work on feast days (as recorded in a contemporary calendar by Millan).[28]

On holy days during the 1750s, we observe a strong and significant reduction in the probability of witnesses working. This goes for both political and religious holy days, with the effect being a little more pro-

[26]Demaris, *Logit Modeling*.

[27]The definition of work used was rather restrictive. I only used information on those witnesses who reported being at work, and not those starting or stopping work. Results are not sensitive to such questions of definition. Additional results are available from the author.

[28]Millan, *Coins*.

TABLE 2
LOGIT REGRESSIONS: WORK ON HOLY DAYS
(Dependent Variable: Individuals Engaged in Work = 1)

Explanatory Variable	1749–1763	1799–1803
Holy days		
B	-0.63	0.29
Wald	5.6**	2.26
Probability	0.018	0.13
Change in odds ratio	0.53	1.34
Political "holy days"		
B	-1.18	-0.01
Wald	2.7*	0.0003
Probability	0.09	0.99
Change in odds ratio	0.31	0.99
Religious holy days		
B	-0.52	0.23
Wald	3.5*	0.93
Probability	0.06	0.33
Change in odds ratio	0.59	1.3

*= significance at the 90 percent level
** = significance at the 95 percent level
Source: See text.

nounced for political festivals. Fifty years later, there is a slight tendency for witnesses to work more often on holy days, but the effect cannot be estimated with great accuracy. Only in the case of political feast days is there a reduction of the probability of observing witnesses in paid work, but it is very small and not significant according to the Wald-statistic (Table 2).

Change over Time

The basic structure of life remained largely unchanged during the second half of the eighteenth century. The timing of main activities during the day shows barely any differences. Hours of sleep were shorter towards the beginning of the nineteenth century than during the middle of the eighteenth century, but the difference is not significant. Although sleep averaged 7 hours and 27 minutes for 1750 to 1763, this figure had fallen to 6 hours and 35 minutes in 1800 to 1803. It must be stressed that the difference is not statistically significant at the customary 90 percent and 95 percent levels. Of the 52 minute difference between the averages, 24 were caused by people rising earlier, whereas 28 minutes of rest were lost due to later bedtimes.

Hours of work during the day were also largely static. Whereas people in the *Old Bailey Sessions Papers* on average started work at 6:45 A.M. during the 1750s and early 1760s, the respective figure for 1800 to 1803 is 6:33 A.M. The difference is equally small between the times of stopping work. Work activities ended at 6:48 P.M. in the 1750s; fifty years later, the average working day extended to 7:06 P.M. Again, these differences are not statistically significant. Unless changes in the duration of meals were dramatic, the best guess estimate for daily working hours for both periods is eleven hours.[29] Note that the estimate for daily working hours is in close agreement with the data published in Campbell's *London Tradesman* from 1747. Campbell's guide, which describes in some detail the various professions found in mid-eighteenth century London, their work-practices and economic situation, also contains a long list of London trades' "hours of working."[30] The average starting time for the 182 professions contained in his work is 6:08 A.M. This does not agree perfectly with the estimate; it is nonetheless easily within the 95 percent confidence interval. The slight tendency towards later hours in the sample is probably due to differences in sample composition: Campbell restricts himself to artisans whereas our sample also contains occasional laborers and others who were more likely to start work later in the day.

In marked contrast to the unchanging pattern of daily life, time allocation both during the week and during the year exhibits radical change. The dataset allows us to test both the Thompson and the Freudenberger-Cummins hypothesis rigorously and on a large empirical basis. As discussed above, in the 1750s the probability of observing an individual at work is sharply reduced on Mondays. Indeed, Monday was virtually identical with Sunday in this regard. This strongly suggests that during the middle of the eighteenth century Monday was a day off. Witnesses' time-use in the period 1800 to 1803 was quite different. Although the probability of observing individuals engaged in work activities on a Monday is again smaller than on average, logistic regressions demonstrate that this effect is not statistically significant. With respect to patterns of paid work, Monday does not differ from other days of the week. On the basis of the findings inferred from the probability of observing individuals engaged in work, there is no conclusive evidence to suggest that workers enjoyed an extended weekend through the custom of Saint Monday as late as the period 1800 to 1803, let alone that the practice was widely observed until the middle of the nineteenth century. It therefore seems sensible to conclude that Saint Monday declined rapidly during the second half of the eighteenth century and that it had all but disappeared by the turn of the century.

A similarly large change occurred on public and religious holidays. The dataset was used to test the Freudenberger-Cummins interpretation empirically. As the preceding section demonstrated, the probability of observing people in paid employment on holidays was sharply reduced. The impact was large, suggesting that work was as rare on a holy day as on a Sunday (or

[29]For both periods, I checked if those starting work came from the same occupations as those stopping work. Although this is an imperfect test for sample composition, it is the only one that can readily be performed. χ^2-tests fail to reject the null of no significant difference in both cases.

[30]Campbell, *London Tradesman*, pp. 331–40.

on Saint Monday). The same is not true in the period 1800 to 1803. Here, the change in the odds ratio from logit models suggests an (insignificant) positive effect. Holy days no longer influenced everyday patterns of labor and leisure in London at the turn of the century.

How long, then, was the working year during the eighteenth century? I estimated that the average working day was 11 hours long, and that, in the 1750s, Sundays and Mondays as well as the 53 holy days (46 listed by Millan plus seven on Christmas, Easter, and Whitsun) were days off.[31] This leaves 208 working days per year. If the conclusions about changing time-budgets during the second half of the eighteenth century are correct, this implies that there were 2,288 hours of work per year.[32] This result represents a lower bound. We assume that, since the probability of observing individuals on Mondays, Sundays, and holy days is sharply reduced, these are not "normal working days." Yet the changes in the odds ratio only show a reduction of roughly 40 to 50 percent on these days compared with all the others. These other days, however, contain (if we are interested in Mondays, say), Sundays and weekdays which were holy days. Consequently, the relative reduction in the probability is understated. Compared to the average working day, it is more accurate to assume that Mondays, Sundays, and holy days registered a 70 percent lower probability of observing individuals in paid work.[33] It seems likely that the remaining 30 percent simply point to individuals who are not employed in professions keeping "normal hours," such as innkeepers, coach drivers, or chairmen. Treating the remaining 30 percent as if they were still engaged in normal work activities gives an upper-bound estimate for working hours in the year (equivalent to 2,631 hours).

For the period 1800 to 1803, the calculation is more straightforward. There is little evidence to suggest that Saint Monday was still the occasion of much absenteeism. Holy days no longer influenced work activities. Work ceased on 52 Sundays in the year, plus seven days at Christmas, Easter and Whitsun. This implies a working year of 306 days; combined with the 11-hour working day, this suggests 3,366 hours of work per year. If we again

[31]The difference between starting and stopping work was exactly 12 hours. Based on the timing of lunch and breakfast, I deducted 1.5 hours for mealtimes.

[32]This allows two days for Christmas and four days for Easter. Anecdotal evidence on working patterns during the eighteenth century has always stressed the importance of fluctuating short-term employment (for example, on the docks). Compare Schwarz, *London*, pp. 106–09. Since those employed short-term are included in my estimates of the time when work started and stopped, this factor has been taken into consideration. The underlying assumption is that occasional laborers were as likely to appear as witnesses (given their share in the total labor force) as member of other professions.

[33]The change in the log-odds ratio for these days is roughly 0.5. For a Monday, this reduction applies vis-à-vis an "average day" containing Sundays and holy days. They present approximately 25 percent of the year. Since on these days, too, the chance of observing an individual in paid work is only 0.5 of what it is on all the other days, the probability for Monday compared to average working days is closer to 30 percent (1-[98/365]) × (0.5).

TABLE 3
WORKING HOURS PER YEAR, 1760 AND 1800

	1760	1800	Difference
Lower bound	2,288	3,366	1,078
Upper bound	2,631	3,538	907

Source: See the text.

assume that the 70 percent lower probability of observing individuals on Sundays indicates that 30 percent of the population regularly worked on this day, then the upper bound estimate for 1800 to 1803 becomes 3,538 hours per year. The difference between both upper bound calculations is 907 hours; for the two lower bounds, the difference is 1,078 hours per year. The extent of the upward movement is therefore not very sensitive to assumptions about residual work on Mondays, holidays, and Sundays. The change between 1760 and 1800 in the upper bound scenario is 118 percent of the change in the lower bound scenario. Change over time is therefore much easier to infer from the data than absolute levels.

So far, I have ignored changes in the occupational composition of the labor force. Where we have evidence on agricultural employment, it shows markedly higher probabilities of employment on Sundays, Mondays, and holy days. The probability was roughly 0.6 of the average. The first question therefore has to be whether it is credible that the working year in agriculture was even longer than in the other professions. If the answer is yes, then we will have to adjust the change in annual labor input downwards. The percentage of the labor force employed in agriculture declined during the second half of the eighteenth century. Therefore, the shift out of one of the most labor-intensive sectors would have exerted a diminishing influence on the upward movement of working hours. If we believe that the working year in agriculture was roughly equivalent to that in other professions, then no further adjustments are needed.

Indirect evidence supports the notion that working hours were particularly long in agriculture. In England, output per agriculturist was not very far below the level attained in other sectors. By 1800, the sectoral productivity gap had almost disappeared.[34] The comparatively small difference in productivity, and the ability of English agriculture to feed a rapidly growing population while employing an almost constant number of men, both lend indirect support to the hypothesis that labor input per member of the agricultural workforce was high.[35] This argument has recently been enforced and put on a more convincing basis by Greg Clark and Y. van der Werf, who find that, on English farms, the average length of the working year was largely

[34]Crafts, *British Economic Growth*, table 3.6, pp. 62–63.
[35]For a dissenting view, compare Clark, "Revolution."

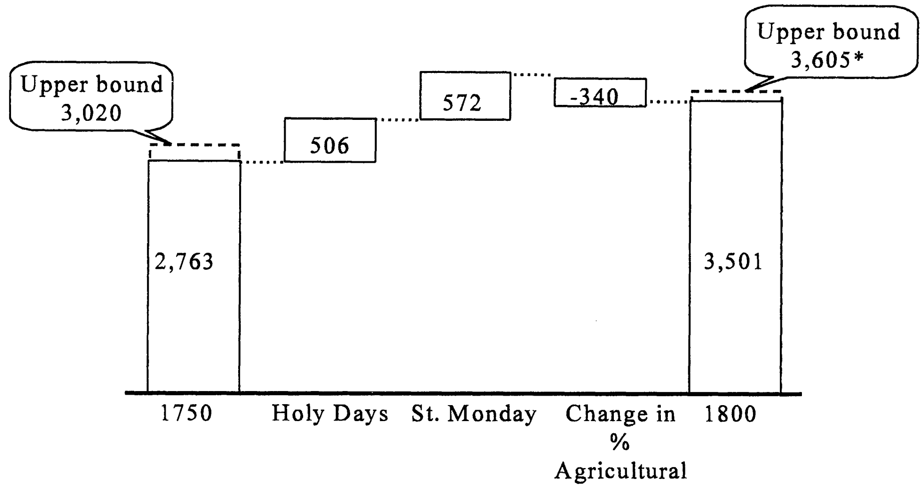

FIGURE 1
WORKING HOURS IN ENGLAND, 1750–1800: FACTORS OF CHANGE

*Denotes factors of change in upper-bound scenario: add 354 due to holy days; add 400 days due to St. Monday; and minus 170 days due to change in the percentage agriculture.
Source: See the text.

unchanged between 1300 and 1850—around 300 days.[36]

N. F. R. Crafts's figures suggest a decline of 7.5 percent in the agricultural share of the labor force. In revising the previous estimates, we therefore have to take into account two additional factors: first, agriculture's special work rhythm raises the estimated labor input for 1760. Second, the shift out of the primary sector acts as a countervailing force to the increase in the overall length of the working year. If we assume that outside the primary sector, Sundays, Mondays and holy days were "days of idleness" and that 60 percent of the agricultural labor force worked on these days (during both periods), then the reallocation of workers reduced the rise in annual labor input by 340 hours/year. Combined with the lower bound estimates, we arrive at an average working year of 3,501 hours (Figure 1).[37] If we assume that 30 percent of the total labor force worked on the (extended) weekend and 60 percent did so in agriculture, the movement into the secondary and tertiary sectors would only have diminished labor input by 170 hours per year.[38] The result is an estimated working year of 3,605 hours (Figure 1).[39]

[36]Clark and van der Werf, "Industrious Revolution?"

[37]Due to the new assumption about the working year in agriculture, the lower bound is now 2,763 hours per year for the earlier period.

[38]Since our reduction by 170 hours per year is the smaller of the two (negative) adjustments we have to make, it is sensible to combine it with the upper bounds.

[39]Incidentally, this figure lies in the same range as Phelps Brown's educated guess (3,500–3,750). Compare Phelps Brown and Browne, "Labor Hours," p. 487.

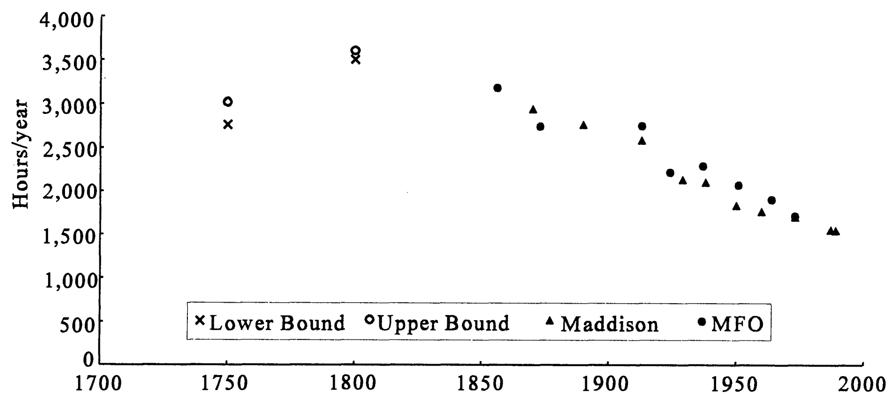

FIGURE 2
WORKING HOURS IN ENGLAND, 1750–1989

Source: Maddison, *Dynamic Forces,* table C.9, p. 270; and Matthews et al., *British Economic Growth*, table 3.11, p. 64. The Maddison series is augmented in 1870 with the figure inferred from Bienefeld, *Working Hours in British Industry*, p. 111.

The upper-bound estimate is therefore only 3 percent higher than the lower-bound estimate; the increase in annual workloads amounted to 585 to 738 hours. Labor input grew by 20 to 27 percent; the elimination of holy days and of Saint Monday alone would have boosted the length of the working year by 25 to 39 percent. The reduction caused by the reallocation of labor was equivalent to 6 to 12 percent of the starting level.[40]

How did working time change in the long run? At the present time, there are data on the changing number of working hours in the year for little more than the last century.[41] Although it must be emphasized that the precision of the estimates presented here is considerably lower than the accuracy of more recent ones, and that this data largely refers to London, we can nevertheless now provide a rough outline of the course of working hours since the Industrial Revolution. Figure 2 gives an overview.

Developments over the long run lend empirical support to suggestions in the literature that changes in labor input described an inverse U. The length of the working year in 1750 was similar to the second half of the nineteenth century. In 1800 both upper and lower bound estimates are higher than any observed since 1850. Around 1750 annual labor input reached levels equivalent to those in the 1850s to 1870s. The speed of change was also high. If

[40]Note that, because of our assumptions about the length of the working year in agriculture, the starting levels are different from the ones used in Table 3.

[41]Differences are largely due to assumptions about vacations, sick leave and so forth, but the empirical basis of the MFO series appears to be more reliable. Compare Huberman and Lewchuk, "Glory Days?" pp. 6–8.

the calculations are approximately correct, then the development between 1750 and 1800 was dramatic. The rise in annual labor input per person over 50 years (+585 to +738) is roughly as large as the reduction in working hours between 1870 and 1938 (−717).[42] These findings are more or less independent of the data used for the period after 1850; long-run trends in working hours in the Maddison and the MFO series are broadly similar. Although these changes took place in less than 50 years in the eighteenth century, the decline of working hours by the same order of magnitude required almost 70 years.

FACT OR FICTION? TESTING THE NEW METHOD

We have established the timing of activities as well as changes in time use between the middle and the end of the eighteenth century using a new and as yet untested method. There are, however, numerous sources of potential bias, and it is important to demonstrate that none of these affects the accuracy of the results.

Hours and Days—Sample Selection Bias

Let us assume for a moment that every day in the year showed exactly the same pattern of time-use. If earning a living required, say, an average of 144 minutes per day, then 10 percent of the witnesses in the sample should have reported that they were engaged in work-related activities. There is one difficulty: sleep. Only during waking hours are witnesses likely to observe activities. We therefore have to make an assumption about the likely duration of sleep, that is the length of sleep inferred from the timing of going to sleep and waking up. In the sample of activities, we then also exclude the sleep-related ones. In the sample from 1760, we have to deduct the 7 hours and 20 minutes inferred from the difference between going to bed and rising in the morning from the 24 hours of the day. Only during the remainder could witnesses observe patterns of time-use. Given that 56 percent of the recorded activities in 1760 were work-related, this implies that 9.4 hours were devoted to work.

This is far more than the 7.8 hours we inferred by comparing the time of starting and of stopping work. Note, however, that the estimate based on the distribution of activities is not completely independent of the duration-based estimates; we still use the estimate for hours of sleep, and the results are strongly influenced by assumptions about hours of sleep (Table 4). Perhaps more importantly, the direction and speed of the rise in annual labor input is quite independent of the assumptions made about hours of sleep. The dif-

[42] This was calculated from the Maddison series. The difference would be even more pronounced if we use the series without adjustments for agriculture.

TABLE 4
HOURS OF WORK: SENSITIVITY TO ASSUMPTIONS ABOUT SLEEP

Statistic	Hours of Sleep	Year	Duration Estimate	Control Estimate	Difference	Index Duration	Index Control	Difference
	8	1760	7.8	8.9	-1.1	100.0	100.0	
		1800	9.7	10.8	-1.1	124.4	121.3	3.1
Mean	7.27	1760	7.8	9.4	-1.6	100.0	100.0	
	6.35	1800	9.7	11.9	-2.2	124.5	126.6	-2.1
Upper bounds	8.3	1760	7.8	8.8	-1.0	100.0	100.0	
	7.23	1800	9.7	11.3	-1.6	124.4	128.9	-4.5
Lower bounds	6.35	1760	7.8	9.9	-2.1	100.0	100.0	
	5.68	1800	9.7	12.4	-2.7	124.5	125.3	-0.8

Source: See the text.

ference of the percentage change between 1760 and 1800 implied by the two methods is never larger than 5 percent. Independent of the assumptions about sleep, there appears to be a slight tendency for the duration-based method to underestimate the number of working hours or for the frequency-sample method to overstate them. There is no way to ascertain which method is correct. However, since there is some reason to believe that there is a reporting bias in favor of outdoor activities, it is likely that the frequency method overstates work activities (outside the home) systematically.

There is an alternative explanation of why we find a systematic difference between the estimates of working hours in Table 4. Since the beginning and end of meals was not clearly distinguished by witnesses, I resorted to observations on the interval during which these activities were reported. For the final calculation, 90 minutes were deducted from the interval between starting and stopping work in order to account for meals. This cavalier approach can possibly be improved by using the direct evidence on the number of individuals engaged in eating during waking hours. In the 1750s, 2.4 percent of witnesses claimed to have had breakfast. Assuming eight hours of sleep for simplicity, this implies 23 minutes spent on the first meal of the day. Dinner (that is, lunch) was reported as the prime activity at the time of the crime by 3.7 percent of witnesses, which is equivalent to 35 minutes. For 1800 the respective figures are 13 and 50 minutes. If we augment the calculation of working hours (based on the time of starting and stopping) with these figures, this suggests 8 hours and 21 minutes in 1760 and 10 hours 7 minutes in 1800.[43] The difference between the two methods is reduced to a mere 37 minutes in 1760 and 43 minutes in 1800. The frequency-based method now suggests an increase in annual labor input by 20.8 percent, whereas the duration-based approach gives 21.2 percent.

[43]I have reverted to assuming 8 hours of sleep. The justification is that the two methods should be kept as independent as possible if one is to serve as a test of the other.

The Representativeness of Witnesses and Changes in Sample Composition

How representative of London's population are the witnesses? Since we cannot test this aspect directly, I shall follow the standard procedure of choosing an additional characteristic that is recorded for witnesses and also known for London's population.[44] Hard data on London in 1800 are not abundant. L. D. Schwarz has nonetheless estimated shares in the male working population according to socioeconomic status. He concludes that only 2 to 3 percent of London's adult male population belonged to the upper-income group (over £200 p.a.). The middling sort constituted another 16 to 21 percent. The remainder he calls "the working population". Schwarz also provides a more detailed (and more tentative) breakdown of this residual.[45]

If we can show that witnesses testifying before the Old Bailey came from a similar background, it would be much more likely that they are a representative sample of the population as a whole. Definitions of socioeconomic class are not always clear-cut, and not all of the witnesses provide sufficient information about themselves to allocate them to a particular group. I follow Schwarz's definition that the middling classes consisted of "anyone below an aristocrat or very rich merchant or banker, but above a journeyman worker or small-scale employer in one of the less prestigious trades." Small shopkeepers are not included in this group, according to Schwarz; they contribute another 9 to 10 percent to the male working population.[46] In the *Old Bailey Sessions Papers*, I was unable to distinguish between the "middling sort" and shopkeepers in this way. It therefore seemed more appropriate to combine these two categories for purposes of comparison. In 1800, 793 of the male witnesses gave an occupational description that allows us to allocate them to one of Schwarz's groups. Table 5 gives the composition of the sample as well as upper and lower bounds from Schwarz. The distributions are remarkably similar. For the upper-income group as well as for the self-employed and artisans, the figures are almost identical. Yet the estimate from the *Old Bailey Sessions Papers* for the combined middle income and shopkeeper group is below even the lower bound given by Schwarz, and there seem to be too many witnesses in the semiskilled and unskilled group. How do we assess the importance of the similarities and differences? Chi-squared tests fail to reject the null hypothesis of no significant difference. Another technique commonly used to explore the relationship between observed sample characteristics and the control group is simple correlation analysis.[47] The correlation between the population shares from Schwarz and

[44] A good example of this technique can be found in Johnson and Nicholas, "Health," pp. 10–14.

[45] Schwarz, *London*, p. 57.

[46] Schwarz also analyzes the female working population. Since proportions can not be derived from his description, the analysis is not extended to women.

[47] Johnson and Nicholas, "Health and Welfare," p. 10.

TABLE 5
COMPOSITION OF THE MALE LABOR FORCE
(percentage)

Category	Schwarz: Lower Bound	Schwarz: Upper Bound	Old Bailey: 1800
Upper income	2	3	1.6
Middle income and shopkeepers	25	29	20.1
Self-employed	5	6	4.8
Artisans	23.8	21.7	20.7
Semiskilled and unskilled	-44.2	-40.3	52.8
Sum	100	100	100

Notes and sources: Upper and lower bounds are from Schwarz, *London*, p. 57. The semiskilled and unskilled category was then derived as a residual.

the witnesses in the *Old Bailey Sessions Papers* is always 0.9 or above—a high degree of similarity. We can therefore conclude that, if we use social class as the standard of comparison, no significant difference between the sample and the population can be found. However, this should not be confused with positive proof that witnesses are representative of the (male working) population at large.

Ideally, we would want to apply the same tests to the sample from the 1750s and early 1760s. Unfortunately, there are no sufficiently detailed and reliable estimates of labor force composition for the earlier period. Instead, we can examine the proposition that shifts in sample composition between the two benchmark years bias the results. The most striking finding in the empirical section was the increase in the number of working days per year. It could be argued that the more intensive working year is not due to any changes in actual working practices in each socioeconomic group. Rather, it could reflect changes in the number of witnesses coming from individual groups. If, say, the semiskilled and unskilled worked appreciably longer than the rest of the population, and their share in the total number of witnesses rose between 1750 and 1800, then one of the main findings might have been caused by a statistical illusion.[48] Such a shift in selection bias might even be expected as watch ownership spread from the top of the social hierarchy to the lower ranks. Table 6 compares sample composition in 1760 and 1800. The share of the semiskilled and unskilled remained virtually unchanged between the middle and the end of the eighteenth century, slipping by a little more than 1 percent. This is eloquent testimony against the idea that a "trickling down" of watch ownership biased the results.

The main change in Table 6 is that the number of artisans (not self-

[48]Strictly speaking this would only be true if the witnesses are not a representative sample of the population. If they are, then the rise in labor-input would be to due shifts in labor force composition. Society's "great day" would still have changed, but for very different reasons.

TABLE 6
SAMPLE COMPOSITION IN 1760 AND 1800

Category	Old Bailey: 1760	Old Bailey: 1800
Upper income	1.4	1.6
Middle income and shopkeepers	27.6	20.1
Self-employed	2.8	4.8
Artisans	14.3	20.7
Semiskilled and unskilled	54	52.8
Sum	100	100

Source: See the text.

employed) rises from 14 to 20 percent, whereas those in the middle-income range plus shopkeepers slip from 27 to 20 percent. Is the magnitude of these differences sufficient to explain a rise of aggregate labor input by at least 20 percent? Employed artisans would have had to work more than six times longer than the population at large to be responsible for this kind of shift. Since this is obviously absurd, we can safely conclude that the main result is not caused by a shift in sample composition.[49]

The Uneven Distribution of Crimes

Crimes were not committed with equal frequency throughout the day. Hence, the number of observations provided by witnesses differs from hour to hour, and it is theoretically possible that this imparts a bias to the calculations. For example, there may be as many people starting work at, say, 6:00 A.M. when crime is rare, as at 8:00 A.M., when it is becoming more common.

We can explore the consequences of such a possible bias in more detail by adopting a simple reweighting scheme. For each one-hour interval, we know the number of statements by all witnesses. In 1800, for example, there was an average of 40.9 observations during any one-hour period.[50] For the interval from 4:00 P.M. to 5:00 P.M., however, we have 50 statements; consequently, we would reweight any time-use information by a factor of 0.82. In the majority of cases, the difference between the reweighted and the original estimates is minute. Witnesses rose at 6:10 A.M. in 1760 if we use the naive method, and at 6:17 A.M. when we correct for the fluctuating incidence of crime. In the few cases where the difference is larger, the standard error bands of the original and the reweighted estimate overlap. We

[49]The same logic can be applied to sectoral shifts among the witnesses. Trade and services, for example, were famous for long working hours, but the increase in the number of individuals in these categories (4.3 percent between 1749–1763 and 1799–1803) is not large enough to drive the observed increase in working hours. Details of this and the previous calculation are available from the author.

[50]There were 19 exact descriptions of an individual's activity for which the day but not the time were recorded.

TABLE 7
REWEIGHTED AND ORIGINAL TIME OF FOUR MAIN ACTIVITIES, 1760 AND 1800

Year	Rising in the Morning	Going to Bed	Starting Work	Stopping Work
1760				
Original	6:10	22:50	6:50	18:50
Reweighted	6:17	23:27	7:38	18:35
1800				
Original	5:53	23:21	6:33	19:06
Reweighted	5:34	23:58	6:10	18:52

Source: See the text.

can therefore conclude that estimates of the main structure of daily life are not biased by the timing of crime (Table 7).

Memory Decay and Recall Period

How long was the interval between the crime and the court trial? Both dates are given in the *Old Bailey Sessions Papers*, so we can easily reconstruct the time period over which witnesses had to recall their activities. The number of sessions at the Old Bailey varied from year to year, but six to eight were common between the middle and the end of the eighteenth century. Since approximately 50 days had passed since the last session, we would expect that the average witness's memory had to bridge 25 days. In addition to this minimum period, legal procedures (establishing evidence and so forth) or a backlog of cases before the court could lengthen the period between crime and trial.

The average lag in the period 1749 to 1763 was 45.6 days (median 30); from 1799 to 1803, it had been reduced to 39.2 days (median 25).[51] Compared with modern sociological studies, where recall periods of a few days normally prevail, these are long intervals. Are recall period and data quality in any way related? There is one immediate indication of faulty reporting in the verbatim reports: if the day of the week mentioned by the witness and the date (which implies a certain weekday) do not agree.[52] This was true in a number of cases, as the empirical sections demonstrated. If we can show that the lag between crime and hearing has no appreciable influence on the quality of recollections in this regard, then there is even less reason for concern about the length of the recall period. To test this possibility, I assigned

[51]The lag length for the two samples is not identical, but there is no significant difference – the confidence intervals overlap. This provides further indirect evidence that the two samples were not generated by vastly different judicial procedures.

[52]Implicit in this method is that witnesses (and not scribes at the court and so forth) are responsible for errors. This approach would be invalidated if the errors of witnesses varied inversely with the scribes' errors, depending on lag length. Such a possibility is, however, purely speculative.

the value 0 whenever there was agreement between the two days, and 1 otherwise. We would now expect the probability of this new variable being equal to 1 to vary with the lag between trial and crime if witnesses' reports in general become less accurate over time. The results from logit regressions are as follows:

1749–1763:

$$C = -1.42 + 0.0044\,LAG$$
$$\quad (42.1)\quad (1.6)$$
Model $\chi^2 = 1.54$

1799–1803:

$$C = -2.97 - 0.0039\,LAG$$
$$\quad (112.7)\ (0.4)$$
Model $\chi^2 = 0.569$

where *C* is the control variable, which is zero if the recorded and inferred day agree, and one otherwise; and *LAG* is the number of days between the crime and the trial. (Wald-statistics are in parentheses).

The χ^2-statistics show that the models do not explain variation in the data adequately, and the Wald statistic on the delay between crime and court session is insignificant. Even if the estimated coefficient for 1749 to 1763 were significantly different from 0, the effect would be very small. For the period 1799 to 1803, the coefficient on LAG is even wrongly signed, which implies that, the longer the recall period, the less likely mistakes were.

Hence, there is no evidence that links the recall period to data quality. Witnesses were sometimes unable to give all the details we would want to know for a variety of reasons, but forgetfulness because of an extended recall period was probably not one of them.

Work on a Cheshire Canal

So far, I have largely examined issues of internal consistency. I have tested the possibility of witnesses' accounts contradicting themselves, at least on the issue of time-use, of unobserved shifts influencing our results, and of inconsistencies arising from potential sampling biases. The results have been encouraging. Yet what is really at issue is how representative the judicial evidence from a London court is. Are shifts in time-use found among those testifying before the Old Bailey indicative of patterns elsewhere? I use new data from an additional source to examine this question.

The evidence comes from the day wage book (repairs) from the Burnton and Western Canal in Cheshire in 1801.[53] Payments to carpenters, sawyers and yard laborers are documented. Their work was classified as "extra labor." This implies that they were not regarded as a regular part of the company's labor force. During the year 1801, however, the individuals named in the wage book do not change very much. What fluctuates in the

[53]P.R.O. (Kew) Rail 883–189.

course of the year is the number of them that the company employed. Consequently, there was a more or less stable group of men available for work on the canal. The company employed their services as it saw fit, but it rarely turned to outsiders. The workers whose wages are documented may have been a reserve army of labor, but its composition was very stable.

The wage book is not an ideal source for our purposes. Peculiarities of labor demand on the canal may have made employment patterns highly atypical. However, the possibility that work on the canal was timed in an unusual way should only concern us if the wage-book data and witnesses' accounts contradict each other. If they do not, it appears highly unlikely that both the *Old Bailey Sessions Papers* and the canal wage book recorded the same aberrant work patterns: the former pertains to 1,000 individuals in virtually all professions. A second possible objection is that the fluctuating type of employment may have induced workers to seek work elsewhere, leaving us with an understatement of annual working days. Since we find a strong upward movement of labor input and a very long working year in absolute terms, this would only be a problem if the number of hours worked on the canal is much lower than implied by the Old Bailey witnesses. Finally, there is no information on the number of hours worked per day. Occasionally, laborers receive more than a day's wage, which implies that they worked longer than normal, but there is no indication either of these regular hours nor the exact amount of overtime. For our purposes, the absence of information on hours of work is not as unfortunate as may be supposed—the main finding concerns weekly and annual patterns of labor and leisure.

During 1801, a total of 5,924 man-days were worked on the canal. The maximum number of workers employed on any one day was 42; the smallest observed value is zero. On average, 16 men are employed for repair work and the like. Work on the Burnton and Western Canal in 1801 was strongly seasonal. Because the degree of seasonality is broadly comparable in both samples, we can argue that the pattern of work captured is similar.[54]

We are also interested in the days when work stopped, and if the weekly and annual patterns in Cheshire is similar to the London one. There are only 25 days on which nobody worked. All of them are Sundays; no other day saw everyone refraining from working. During the rest of the week, the number of men at work is fairly constant. Table 8 compares the data from the Old Bailey with the weekly pattern of work on the canal.[55] In 1800 there

[54]Agreement between the two series is not always perfect; the trough during the summer months, for example, seems to be more acute in the Old Bailey data than on the canal. Overall, similarity between the two datasets is not small. Although the more sensitive Pearson correlation coefficient only suggests a value of 0.35, the Spearman rank correlation coefficient is 0.96—far higher than values that are generally regarded as acceptable in the literature (compare Johnson and Nicholas, "Health," pp. 10–12).

[55]Note that the Old Bailey data from 1800 in table 10 refers to most narrow definition of work; levels for broader definitions of work are higher, but the weekly pattern is broadly similar.

TABLE 8
WORK ON THE CANAL

Day of the Week	(1) Old Bailey: 1800 Count	(2) Percentage of Overall Total	(3) Canal Count	(4) Percentage of Overall Total
Sunday	35	6.2	199	3.4
Monday	79	13.4	974	16.4
Tuesday	85	14.7	976	16.5
Wednesday	99	17.1	945	16.0
Thursday	85	14.7	910	15.4
Friday	90	15.5	958	16.2
Saturday	106	18.4	962	16.2

Source: P.R.O. (Kew) Rail 883–189. See the text.

are slightly more observations on Sunday, but the difference is small. On the canal, the days of the working week register almost identical manning levels. The variation is somewhat higher in the witnesses' accounts - as is only to be expected since there is more than one profession in the sample. In both datasets, Sunday appears to be a day of rest, and Monday shows no significant divergence from other working days. The (Pearson) correlation coefficient between the two relative frequencies (columns 2 and 4) is 0.91, and the Spearman rank correlation coefficient has a value of 0.93. As regards the weekly cycle of work and rest, the evidence from the Burnton and Western Canal in 1801 does not contradict the data from the Old Bailey in the years 1799 to 1803.

We have thus demonstrated that one source of growing labor input that we inferred from the Old Bailey, the decline of St Monday, was also present on the canal. Is this also true for the second cause of the lengthening working year, the disappearance of holy days? In deciding whether a day was normally used for work or not, it will be convenient to define a certain number of men in employment that clearly marks a working day. However, the same number of men at work may have been high during the summer and very low in the autumn. I will consequently focus on the relative difference between the number of men at work on a specific day and the seven-day moving average. If we decide that 50 percent of the moving average is a reasonable cut-off point, then 44 days were used for rest. All but three of these are Sundays. The result is not very sensitive to the cut-off point we use. At 30 percent, it is 41; at 70 percent, it is 48. This implies that not even every Sunday was a day off. The consequence of moving to a higher threshold is simply to add additional Sundays; there are still only three other non-Sundays.

Clearly, none of the traditional holidays persisted, at least on the canal in Cheshire. The *Old Bailey Sessions Papers* allow us to observe a large number of individuals, but each only over a very short period. The nature of the data in the wage book is exactly the reverse: the number of individuals is comparatively small (about one-sixtieth of the number in the Old Bailey reports), but we are able to track each one over the course of an entire year. Also, the two datasets come from different geographical areas. This lends some support to our procedure of treating London developments as representative of England as a whole. Both methods agree on the main points: St Monday and old holy days held no importance any more in 1800, and the weekly and annual cycles of work and rest are remarkably similar. Unfortunately, we cannot repeat the experiment with data from the same source for 1760. Our findings would be fully corroborated if there were evidence from another independent source of traditional practices still persisting in 1760.

IMPLICATIONS

For our period, evidence on real wages on the one hand and on patterns of consumption on the other present a conundrum. Schwarz finds a rapid fall in London real wages between the middle and the end of the eighteenth century.[56] Peter Lindert and Jeffrey Williamson also find a reduction in real wages, but of a much smaller magnitude.[57] At the same time, calculations of consumption per head of population show a small gain between 1760 and 1801. N. F. R. Crafts, using his new output figures, suggests that consumption rose by almost exactly ten percent between 1760 and 1800.[58] Also, as has been noted elsewhere, probate inventories record a rising stock of consumer goods being passed on from one generation to the next.[59] Can the new estimates for labor input help to resolve the puzzle?

Consumption per capita net of saving will equal total wages earned by the labor force, divided by the size of the population.[60] As a first approximation, changes in income per head of population should then be the sum of changes in days worked per member of the labor force, the labor force participation ratio, and the real wage. We can now combine the new estimates for labor input with some of the daily real wage indices in the literature to examine if there is still evidence of conflicting trends. Table 9 gives the results. I have calculated the implied change in consumption per capita between 1760 and 1800, using both the Schwarz and the Lindert and Williamson series.[61]

[56]Schwarz, "Standard," pp. 28–29.

[57]Lindert and Williamson, "Revising England's Social Tables," table 5, p. 13.

[58]Crafts, *British Economic Growth*, table 5.2, p. 95.

[59]King, "Pauper Inventories". For general trends, compare DeVries, "Purchasing Power".

[60]This only applies, of course, if we disregard consumption financed by profits or income from private wealth. Since I am inferring rates of change over time, my results will only be biased if income from these sources did not fluctuate in parallel with the wage bill.

[61]I used their real wage for all blue collar workers; Lindert and Williamson, "Revising," table 5, p. 13. Unfortunately, the much-improved series in Feinstein ("Conjectures") is only available from 1770. If his figures for changes in real wages between 1770–1772 and 1798–1802 are used, we find implied increases in consumption of between 14 and 21 percent.

TABLE 9
OBSERVED AND IMPLIED CHANGE OF CONSUMPTION, 1760–1800

Statistic	Upper Bounds for Labor Input 1760	Upper Bounds for Labor Input 1800	Upper Bounds for Labor Input Percentage Change	Lower Bounds for Labor Input 1760	Lower Bounds for Labor Input 1800	Lower Bounds for Labor Input Percentage Change
A. Schwarz Wage Series						
Labor input (hours/year)	2,763.00	3,501.00	+27	3,020.00	3,605.00	+19
Labor-force participation ratio	46.5	44.9	-4	46.5	44.9	-4
Wages, London	117.5	82.3	-30	117.5	82.3	-30
C per capita, implied (1760=100)	100.0	85.6	-16	100.0	80.7	-19
C per capita, actual (1760=100)	100.0	110.1	+10	100.0	110.1	+10
C implied as a percentage of actual C			77.7			73.2
B. Lindert and Williamson wage series						
Labor input (hours/year)	2,763.0	3,501.0	27.0	3,020.0	3,605.0	+19
Labor-force participation ratio	46.5	44.9	(4.0)	46.5	44.9	-4
Wages of all blue collar workers	56.3	51.7	(8.0)	56.3	51.7	-8
C per capita, implied (1760=100)	100.0	112.3	12.0	100.0	105.5	+5
C per capita, actual (1760=100)	100.0	110.1	10.0	100.0	110.1	+10
C implied as a percentage of actual C			102.0			95.8

Note: Labor-force participation ratios are not available from standard sources. I regressed the labor-force participation ratio on the share of the population aged 15 to 59. For the period 1801–1879, the labor-force participation ratio rose by 0.8 percent for every 1 percent increase in the share of the population of working age (t-statistic 5.4, $R^2 = 0.8$). On the basis of this relationship, the Wrigley and Schofield figures on population structure were used to extrapolate backwards.
Sources: London wages are from Schwarz, "Standard of Living" ; wages of blue collar workers are from Lindert and Williamson, "Revising England's Social Tables"; actual change in per capita consumption is from Crafts, *British Economic Growth.*

If the Schwarz series is used, the rise in annual labor input is insufficient in either case to compensate for the fall in real wages and the declining labor force participation ratio.[62] However, without the rise in labor input, we would have expected consumption p.c. to fall by 32 percent because of falling wages and the rising dependency burden. Because of the increase in working hours, the implicit change in per capita consumption is only -16 percent, a sizable reduction of the puzzle. The Lindert and Williamson series, combined with my upper bound estimate of changes in labor input, allows us to resolve the puzzle almost completely. It implies a rise in per capita consumption by 12 percent versus the 10 percent calculated by Crafts.

[62]This need not imply that it is less accurate than the Lindert and Williamson series; trends in London may very well have diverged from national ones.

In this case, even the lower bound estimate for time-use tips the scales in favor of growing standards of consumption: the calculated change per capita is 5 percent. These results demonstrate that the implied trend in consumption is most sensitive to the real wage index used. More working hours go some way towards resolving the paradox noted above; yet for the final result to be positive, we have to believe that the Lindert and Williamson series is superior to Schwarz's. This cannot be tested directly by the evidence assembled here.

The time-use data has further implications for the history of income. Lindert and Williamson recently reexamined Massie's social tables for England in 1759. In addition to revising his estimates for occupational composition, they argue that his guesses of family income at this time are too low.[63] Estimates of mean weekly income appear unconvincing when compared with daily wage rates from other sources. Dividing the former by the latter implies a working week of only 4.79 days.[64] Lindert and Williamson deem this figure much too low since they believe that there is overwhelming evidence for a six-day working week at this time (or more than 25 percent more than the implied figure), citing Bienefeld as a source. First, it is important to note that Bienefeld was anything but firm on the matter, merely stating that the six-day week was generally regarded as the norm.[65] Second, they do not take account of the large number of public and religious festivals still prevailing at this date. Converting scenarios A and B above suggests 4.83 and 5.27 working days per week. Scenario A therefore only diverges from Massie's figure by 0.8 percent, scenario B by 10 percent. Our finding of a comparatively short working week in 1760 resolves the inconsistency in favor of Massie *and* it vindicates the accuracy of the contemporary wage assessments used by Lindert and Williamson.

The value of these calculations is twofold. Although it must be stressed that our simplifying assumptions diminish the accuracy of the exercise, and the time-use data almost exclusively refers to London, it is nonetheless reassuring that our revised estimates for labor input help to resolve some of the puzzles posed by conflicting evidence on consumption, income, and real wages. This is important if we believe that economic history should strive for a coherent image of the past. By fitting another piece into the puzzle (and connecting two disparate parts), the existing results and our findings reinforce each other. Further, the calculations in Table 9 are also of interest for the historiography of the Industrial Revolution, in that they lend further

[63]Lindert and Williamson, "Revising," pp. 395–96.

[64]Their results are 4.9, 4.6, 4.1, and 4.95, giving an average of 4.64. Since one of their sources for daily wage rates (building laborers) actually gives a range of 20–24 pence, I calculated an additional observation from the lower bound (equivalent to 5.4 days).

[65]Bienefeld, *Working Hours*, pp. 36ff.

credence to a cautiously optimistic interpretation of its early years.[66]

Total Factor Productivity

At present, the historiography of the Industrial Revolution seems to diminish the importance of productivity growth by the decade. For 1760 to 1801, research during the past 15 years has halved its importance. For the three decades to 1831, there was a decline from 1.3 percent per annum to 0.35 percent per annum, a fall equivalent to 73 percent.[67] Recent advances in the measurement of capital formation and output growth have greatly increased the accuracy of TFP estimates.[68] The level of sophistication is such that only "declining marginal returns" can be expected from further contributions concerned with output growth and the rate of investment. The same is not necessarily true in the case of labor input, where estimates are normally based on the Wrigley/Schofield data for population growth.[69] Recent work confirms that there was no sudden burst of capital accumulation during a brief period of ten to twenty years, no "take off" in the sense suggested by Walt Rostow. Saving, and consequently, investment, made the largest single contribution to output growth during both periods according to Crafts and Knick Harley. Yet the expansion of capital stock was even slower than initially estimated by Charles H. Feinstein, and it compares unfavorably with growth rates of other industrializing nations at a similar stage of development.[70]

On the basis of our new estimates, we can now argue that $\Delta L/L$ grew at a rate of 1.2 to 1.3 percent per annum.[71] This alone would reduce most estimates of TFP growth to negative values, implying that the economy experienced diseconomies of scale.[72] Yet there is some evidence in modern economic studies that longer working hours have an effect above and beyond additional labor input. A longer working year also increases the availability of capital—tools, machinery, and buildings will go unused for shorter periods. Feldstein uses cross-sectional data on 24 British industries during the postwar period and finds that the return to working hours was much

[66]The welfare implications largely depend on the extent to which the additional labor supplied was voluntary. Compare Voth, "Why Did Working Hours Increase."

[67]It should be noted that per capita output rose chiefly because of technological change if the Crafts and Harley figures are used. Compare Mokyr, "New Economic History," fn. 21, p. 25.

[68]Crafts and Harley, "Output Growth."

[69]Crafts, *British Economic Growth.*

[70]Ibid., p. 73.

[71]Approximately two-thirds of this is caused by a larger population, with the remaining one-third coming from longer working hours.

[72]The population grew very rapidly. The idea of a (mild) Malthusian crisis during the late eighteenth century in England was first formulated by Crafts, *British Economic Growth*, p. 77. It has recently been extended in a more assertive yet less convincing manner: Komlos, *Nutrition*, chap. 5 and "Secular Trend".

larger than the return to the number of workers. Roger Craine, using time-series evidence, estimates elasticities of output with respect to working hours in the range of 1.9 to 2.2. One of the most comprehensive studies by Derek Leslie using panel data also found returns greater than 1.[73] This suggests that the standard TFP formula has to be modified to take differences in the return to labor into account:

$$\text{TFP} = (\Delta Y/Y) - \eta_K(\Delta K/K) - \eta_L(\Delta L/L) - \eta_H(\Delta H/H)$$

where Y equals output, K equals capital, L equals number of workers, and H equals working hours. We can test the sensitivity of our result by using a number of alternative values for η_H to calculate TFP: For all factor inputs and output growth, I use the figures from Crafts and Harley.[74] I also use their assumption that capital and labor both have weights of 0.5.[75] The top half of Table 10 uses the lower bound on the change in annual working hours (from scenario B), equivalent to 0.4 percent per annum. The lower half assumes an annual rate of growth equivalent to 0.5 percent per annum. Modern empirical studies often give elasticities (η_H) between 1.5 and 2. If we assume such values, between 60 and 100 percent of output growth can be explained by the increase in working hours alone, and TFP would have fallen quickly. If the return to increases in working hours is unity, and capital and labor inputs grew at the rates suggested by Crafts and Harley, TFP growth would definitely have been strongly negative (column 2). A longer working year alone would be sufficient to account for 40 to 50 percent of output growth from 1760 to 1801. The efficiency with which the economy combined factors of production would have fallen at a rate of 0.3 percent to 0.4 percent per annum. Interestingly, even if we only assume that the return to working hours is equivalent to the one for men (column 1), then 20 to 25 percent of total output growth could still be attributed to the lengthening working year alone. Independent of our assumptions about the return to working hours, total factor productivity was probably falling between 1760 and 1800.[76]

Note, however, that technology may nonetheless have played an important

[73]Feldstein, "Specification," tables 1, 2, 4–6 and equations 5–8, pp. 379–84; Craine, "On the Service Flow," p. 43; and Leslie, "Productivity," pp. 489–90. Solow and Temin ("Inputs," p. 12) assume that 60 hours per week is a biologically determined upper limit beyond which output will rise no further; Matthews et al. (*British Economic Growth*) argue that the reduction in weekly hours from 65 to 56 between 1856 and 1873 was fully compensated by rising efficiency of the labor force due to shorter hours. There are a number of reasons why the argument about offsetting efficiency gains is of little relevance to our period. First, the starting level in 1760 was not very high—44 to 51 hours a week in the basic scenario, and 53 to 58 hours if we make the adjustment for agriculture. It is not clear if negative returns can already be expected in this range.

[74]Crafts and Harley, "Output Growth."

[75]Ibid., p. 718.

[76]Note that, strictly speaking, the TFP framework assumes constant returns to scale. Compare Nicholas, "British Economic Performance," pp. 576–77.

TABLE 10
GROWTH ESTIMATES FOR ENGLAND, 1760–1801

Statistic	(1)	(2)	(3)	(4)
ηH	0.5	1	1.5	2
$\Delta Y/Y$	1	1	1	1
$\Delta K/K$	1	1	1	1
$\Delta L/L$	0.8	0.8	0.8	0.8
Δ Working hours (lower bound)	0.4	0.4	0.4	0.4
TFP	-0.1	-0.3	-0.5	-0.7
Percentage of output growth explained by working hours alone	20	40	60	80
ηH	0.5	1	1.5	2
$\Delta Y/Y$	1	1	1	1
$\Delta K/K$	1	1	1	1
$\Delta L/L$	0.8	0.8	0.8	0.8
Δ Working hours (upper bound)	0.5	0.5	0.5	0.5
TFP	-0.15	-0.4	-0.65	-0.9
Percentage of output growth explained by working hours alone	25	50	75	100

Source: See the text.

role. Even if the efficiency with which the economy combined factors of production was falling, we assume in our slightly extended Solow framework that there are positive marginal returns to capital, labor, and working hours. That these still existed at a time of spectacular population growth cannot be taken for granted, as Malthus reminds us. It is likely that, in the absence of technological advances, declining marginal returns would have rapidly acted to depress the living standard of the population.

CONCLUSION

The purpose of this article is twofold. It demonstrates the feasibility of a new method for reconstructing time-use in the past, and it has put forward some tentative conclusions for the history of the Industrial Revolution.

At present, the results that have emerged from the *Old Bailey Sessions Papers* cannot be said to provide wholly accurate measurements of working hours. The merit of the new method is that, while still being far from precise, the estimates based on court records present an improvement because they are based not on anecdotal evidence but on the everyday patterns of labor and leisure of more than 2,000 individuals. It is hoped that the method presented here can be readily applied to court records from other areas and other periods, ultimately enabling historians to measure historical time-budgets adequately.

At the same time, the implications are sufficiently large to substantially revise our view of economic development in England from 1750 to 1800. Productivity growth—"ingenuity," in McCloskey's phrase—may have played an even smaller role than is assumed in accounts of the British

Industrial Revolution.[77] Output growth would have largely been driven by additional labor input, and the "Industrious Revolution," as Jan de Vries termed it, was responsible for overcoming the adverse effects of rapid population growth. Abstention seems to have been more important than invention, but it was abstention from leisure—and only partly from consumption—that was at the core of economic growth.[78]

[77] McCloskey, "1780–1860," pp. 267–68.
[78] DeVries, "Industrious Revolution," pp. 255–60.

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