HOW LONG WAS THE WORKING DAY IN LONDON IN THE 1750S? EVIDENCE FROM THE COURTROOM

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Little is known about the length of the working year in pre-industrial times. This paper develops a new method for analysing patterns of time-use in the past. Witnesses' accounts in court records, it will be argued, reflect the actual behaviour of a group that is representative of the population at large. This new technique is applied to London during the middle of the eighteenth century. Results are compared with evidence from other eighteenth-century sources. These findings have important implications for our understanding of the Industrial Revolution. Our estimate of the number of working days helps to resolve some apparent contradictions between wage and income measures.

Time is all we have to spend. All of our actions take time, and any sensible definition of well-being has to take into account how we use our time. Research in the social sciences has long acknowledged the central importance of time use. Sociologists have incorporated evaluations of time budgets into the methodology of quality of life surveys.[1] Economists, dissatisfied with the usefulness of national income accounts as indicators of living standards, have made attempts to incorporate data on time-use.[2] At the same time, historians have had surprisingly little to say about how time was actually used in the more distant past.[3] This is particularly true of the one period during which working practices probably underwent the most dramatic transformation - the Industrial Revolution.[4] As Mokyr remarks:[5] '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 paper is organised as follows. I first give a brief overview of the current state of historical knowledge. Conflicting assessments are compared. I then introduce a new method for analysing time-use in the past in section 2. After discussing the representativeness of our sample, results are presented in section 4. The reliability of witnesses' testimonials is examined in the section 5. The conclusion summarizes the argument and draws out some implications.

1 A Golden Age?

It is part of conventional wisdom about the Industrial Revolution that workers toiled for longer hours in 1830 than in 1760.[6] The most prominent statement was made by E.P. Thompson, and it is convenient to take his contribution as a starting point for a discussion of working time during the eighteenth and early nineteenth century. In 'Time, Work-Discipline, and Industrial Capitalism' he assembles evidence from biographical and literary sources. The cornerstone of traditional work practices before the advent of the industrial revolution was 'St Monday', the habit of taking the first day of the week off to recover from the weekend. Thompson cites satirical rhymes describing the practice as well as the glaring indictments by moralists and producers.[7] The latter were particularly scornful of the heavy drinking that

[1]Campbell, Converse and Rodgers 1976.
[2]Kendrick 1979.
[3]Notable exceptions also include Thompson (1967) and Atack and Bateman 1992.
[4]Thompson 1967.
[5]Mokyr 1985, p. 32.
[6]Reid 1976; Briggs 1965, p. 98; Freudenberger 1974, p. 314ff.; Jones 1974, p. 116f; Pollard 1978, p. 162.
[7]Thompson 1967, p. 74-76.

often took place on St. Monday.[8] The slack often continued until Tuesday; it was during the later half of the week that hours (and intensity) of work mounted so that the agreed amount of produce could be handed over to the employer or customer. The weekly cycle was less pronounced for children and women, who pursued their tasks already at the beginning of the week, but who nonetheless shared in the rush towards its end. Stark contrasts between idleness and grinding toil, according to Thompson, also characterised the working year. Agricultural employment was strongly seasonal, and the long hours during the harvest or the ploughing season alternated with periods of low pressure at other times of the year. The essence of 'pre-industrial' working patterns, according to Thompson, was the ability of men and women to set their own pace; hence work was irregular, varied, and often interrupted by socialising and play.[9]

In this perspective, discipline and regularity were the result of a need to co-ordinate the schedules of workers.[10] Where onehanded clocks had once sufficed, watches with minute and second handles became widespread.[11] What transformed the self-determined and leisurely pace into the stringency of later schedules was the division of labour and the growing capital-intensity of manufacturing in the 'dark satanic mills'. These, according to Thompson, were necessary corollaries; but they do not explain why working hours per day in nineteenth century cotton mills reached fourteen or even sixteen hours.[12] In the final analysis, exploitation by capital owners is the driving force for these excesses. Thompson describes the employers' attempts to regulate and discipline the workforce through the use of time as a disciplinary device. Watches were taken from workers when they entered the factory so that manipulations of 'official' clocks went unnoticed and the capitalist's time could rule.[13]

This view has since been criticised. Hobsbawm opined that workers, whether preindustrial or not, 'did not dislike work'.[14] On a more substantial level, Clark has recently argued that factory workers preferred discipline and higher pay over alternative modes of employment. If one accepts the basic tenets of economic theory about individual decision-making, then, according to Clark, one could argue that factory workers effectively hired capital owners to overcome their own bounded rationality - externally imposed discipline assured that myopic time-preferences could not be exercised.[15]

Freudenberger and Cummins agree with the thrust of Thompson's argument, but they argue for a different set of causes underlying low annual labour input. In their perspective, the supply of nutrients before the 1750s was so severely constrained that workers could not have toiled for any large number of days in the year. Therefore, effort-saving customs like old holidays and St. Monday persisted. When the nutritional constraint was lifted, additional hours could be supplied.[16] Fogel has recently presented estimates that imply that the bottom 10 percent of the population only had enough energy for 6 hours of light work per day.[17] Their work is, however, not without critics since direct evidence on nutrients consumed is

[8]Thompson 1967, p. 76.
[9]Thompson himself is sceptical about the use of the term, cf. Thompson 1967, p. 79f.
[10]Thompson 1967, p. 70. The most important statement on time and social coordination of activities is by Elias 1984. For a recent economic argument, cf. Weiss 1991.
[11]Thompson 1967, p. 56, 89; Landes (1983, p. 97) argues that, depending on labour conditions, the spread of clocks could also have a liberating effect, separating the workers own time from that of their employers.
[12]Thompson 1967, p. 81f.
[13]Thompson 1967, p. 81f. Karl Marx also discussed the question at some length: Cf. Marx 1867 [1983], p. 249ff.
[14]Hobsbawm 1965, p. 100.
[15]Clark 1994, p. 160f.
[16]Freudenberger and Cummins 1976, p. 8ff.
[17]Fogel 1993, p. 11f.

scarce.[18] Further, since a large part of digested calories is required for basal metabolism, small changes in estimates of mean calorie intake yield very different results for the amount of energy available for work.[19]

The image of pre-industrial work presented by Thompson is also subject to debate. McKendrick has criticised the view that workers before the factory age worked short hours at a leisurely pace and without strict discipline as a 'prelapsarian myth of the golden past'. From his point of view, premodern labour schedules were as exacting as factory work, but not as well paid.[20] The factual foundation of Thompson's description is indeed weak. Literary sources and the writings of Protestant moralists provide colourful images, to be sure. Yet it is difficult to assess how accurate they are. First, it is important to note that Thompson does not seem to describe practices from a clearly-defined period in history. His latest source dates from 1903,[21] while the earliest example of premodern attitudes to work and time comes from Chaucer's *Canterbury Tales*.[22] Thompson himself seems to circumscribe the era in question when he argues that 'the years between 1300 and 1650 saw ... important changes in the apprehension of time', yet then goes on to cite some complaints about that pillar of 'preindustrial' labour practices, 'St. Monday', from as late as the 1870s.[23] Second, Thompson's reliance on literature and moral tracts means that he adduces very little direct evidence on labour practices. Research after the publication of Thompson's path-breaking article has attempted to fill some of these gaps.

Rule examined contemporary regulations; his results do not lead either to an outright rejection of Thompson's hypotheses nor to their full corroboration. Rule finds little evidence of short hours before industrialisation; for breeches-makers, carpet weavers, coopers, saddlers, woolcombers and shoemakers, 14 hour days (including mealtimes) were often prescribed. Even longer working days prevailed among pin makers, calico printers, book-binders and glovers, with 16 hour days being the maximum recorded.[24] Rule's source for this information is, however, not the actual number of hours worked, but the labour regulations documented in Campbell's *London Tradesman*. However, the reliability of this information is questionable; to know that certain hours of work were prescribed is not the same as demonstrating that people actually worked. When the tailors argued for a reduction of their regular hours, they claimed that in 'most handicraft trades', 12 hours were common.[25] This is appreciably shorter than the median of 14 hours recorded by Campbell.[26] If 1 to 1½ hours for meals in each case are taken into account, the difference amounts to approximately 20% - not exactly eloquent testimony to the reliability of either contemporary comments or prescriptive sources. In the case of tailors, administrative sources used by Rule show a trend towards shorter hours. Both the length of the working day and its tendency to shorten in some cases seem to contradict Thompson; yet there is also some evidence corroborating his views. Miners worked around 6 to 8 hours a day on five to six days of the week during the later half of the eighteenth century. During the following fifty years, this gave way to 12 hours shifts.[27] The driving force behind the intensification of work practices was, according to Rule, 'the

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[18]Riley 1994.

[19]If Fogel's estimate of mean calorie intake errs by as little as 13 percent on the low side, no binding energy constraint can be demonstrated. Cf. Voth 1996, section 13.

[20]McKendrick 1974, p. 163.

[21]Thompson 1967, p. 75.

[22]Thompson 1967, p. 56.

[23]Thompson 1967, p. 74.

[24]Rule 1981, p. 58ff. The minimum was 12 hours. Cf. Campbell 1747, p. 331ff.

[25]Rule 1986, p. 132.

[26]Campbell 1747, p. 331ff. N=182, mode=14, mean=13.86.

[27]Rule 1981, p. 59.

demands of capitalising industry'.[28] Higher capital/labour ratios meant that longer working hours reduced capital costs per unit of output. Rule also sides with Thompson when he describes the prevalent practice of St. Monday as well as the numerous holidays observed by workmen before the factory age. He does so on the basis of the same literary sources as well as biographical material.[29] The discussion of holy days also treats a longer period than the entire eighteenth century as a monolithic entity, juxtaposing complaints about the cost of absenteeism in 1802 with work practices of Cornish miners during the early decades of the eighteenth century.[30]

The overall impression is therefore one of a gradual move towards the discipline that Thompson painted in the darkest hues. Two qualifications apply. First, irregularity rather than a low workload seem to have been characteristic of premodern work practices - the longest days in traditional trades apparently approached the hours of the cotton mills. Second, the evidence gathered by Rule shows a striking degree of geographical variation. Since observations from the same location are rare, the trends over time identified by him may simply be due to large regional differences.

That, during the nineteenth century, individual areas experienced different patterns of labour and leisure is borne out by Hopkins's and Reid's work.[31] Reid shows on the basis of parliamentary papers and the child employment commissions' [CEC] reports that St. Monday prevailed in Birmingham until the 1860s. The evidence of contemporaries is, however, contradictory, as Reid himself notes. In parliamentary papers, information on the matter is scarce and scattered, and the CEC reports hardly relate to a representative sample of industry. Further, Reid concedes that only the better paid could afford to take a day off at the beginning of the week.[32] He then goes on to show that, nonetheless, large numbers visited Edgbaston Botanical Gardens on Mondays.[33] The reason why the custom prevailed longer in Birmingham than appears to have been the case elsewhere is the late arrival of steam power - lower capital/labour ratios allowed the persistence of premodern customs. Only during the 1860s, when large scale manufacturing appeared and the Saturday half-holiday became popular, did St. Monday disappear. This is also the conclusion of Hopkins, who examined the Black Country and Birmingham.[34] In the Black Country, neither mines nor ironworks seem to have extended working hours during the first half of the nineteenth century. Also, the building trades in general show little tendency towards longer hours.[35]

What emerges from these studies is a potentially high degree of divergence both at the local level and for different occupations. Yet occasional comments in reports of the child employment commission are anything but a reliable indicator of actual time-use in most professions. The same is true of the minute fraction of the population visiting pleasure gardens or the occasional factory book examined by Thompson, which, by virtue of its survival to the present day, could be regarded as untypical.[36] As the comment by Mokyr cited in the introduction makes strikingly clear, there is little that can be said with confidence about the length of the working year before the middle of the eighteenth century.

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[28]Rule 1981, p. 59. Interestingly, there is no positive association between capital requirements and working hours for the trades listed by Campbell.
[29]Rule 1981, p. 56.
[30]Rule 1981, p. 56f; Rule 1986, p. 217.
[31]Hopkins 1982, Reid 1976.
[32]Reid 1976, p. 78ff.
[33]We do not know how many of the up to 41,639 visitors per year (or 801 on an average Monday, less than 1 percent of the population) came from the working classes.
[34]Hopkins 1982, p. 62.
[35]Thomis 1974.
[36]Thompson 1967, p. 81.

This paper therefore sets itself a much more humble task. Before we can discuss the changes in working hours during the Industrial Revolution, and the causes underlying any shifts, we need to establish actual patterns of time-use, and to do so on a broad empirical basis. The next section introduces a method that allows us to take a first step towards that goal. In section 4, it is then applied to the case of London during the 1750s and early 1760s.

2 Crime and Random Hour Recall

The conclusion from a survey of the literature was that a shortage of sources has made it difficult to ascertain patterns of time-use in the past. Yet there exists one source that contains information on the timing of activities in abundance - witnesses accounts. A typical example reads like this:[37]

Thomas Sibley: I am a baker, I had been *making my dough, on the 19th of April, about twelve at night*, I heard a noise of breaking, I looked to the prosecutor's cellar door, and saw one half of the door broke, soon after I saw the prisoner come up out of the cellar, on which I asked him what business he had there ...

Witnesses heard before a court typically describe their profession, their main activity, and the day and hour when they performed it. Further, the frequent practice of subjecting witnesses to cross-examinations allows us to assess the trustworthiness of witnesses. For example, in one case, the availability of precise knowledge about the time of certain activities aroused the suspicion of the court:

Q. How come you to be so exact as to the time?
A. I can look out at the clock, and see what a clock it is at any time.
Q. Can you take upon you to say you looked at the clock that time?
A. I looked at the clock at six o'clock at night.
Q. Did you see the clock at seven?
A. No, I could not, then it was candle-light, I heard it strike seven; he was then coming from the Hay-market.[38]

A whole range of acoustic and visual information was available in urban centres. Access to knowledge about the time of day was therefore not restricted to those owning a watch. Further, watch-ownership was no longer as socially exclusive as it had been in earlier centuries - we find individuals of low social standing appearing before the court because their watches have been stolen. In 1760 already, we encounter an individual who is too poor to afford a bed of his own, but nonetheless owns a watch.[39] At about the same time, two bricklayers' labourers lose their watches.[40] As early as 1750, we find a metal watch valued at a mere 10s.[41] According to the wage data compiled by Schwarz for the London building trades, bricklayers would have had to work little more than three days for such a watch; bricklayers' labourers a little less than five.[42]

[37] Old Bailey Sessions Papers, Case No. 211, 1753 [my italics].
[38] Old Bailey Sessions Papers, Case No. 154, 1759.
[39] Old Bailey Sessions Papers, Case No. 165, 1760.
[40] Old Bailey Sessions Papers, Case No. 75, 1759 and Case No. 68, 1758.
[41] Old Bailey Sessions Papers, Case No. 3, 1749. There is no information on the quality of the watch.
[42] Cf. Schwarz 1985, appendix I, p. 37.

Corroborating evidence comes from the Essex pauper inventories collected by King.[43] In this socially disadvantaged group, 20 percent owned watches or clocks before 1769. The inventories produced between 1770 and 1812 show watch or clock ownership among 38 percent of paupers.[44] Consequently, the price of time-pieces hardly acted as a constraint on watch-ownership.

3 The representativeness of the sample

We already noted that the cost of watches and the availability of information on the time of day was unlikely to bias our sample in favour of the richer strata of society. Yet a more rigorous test is clearly necessary.

In one respect, the 1,005 witnesses that appeared before the court are clearly not a random sample of the population. 76.5 percent are male, whereas only 23.5 are female.

The skewed distribution of observations indicates that witnesses are not a completely random sample of the population. Many pressures may have played a role in shaping this peculiar asymmetry: Men may have been present more often in public places, where the majority of crime occurred. Also, it is likely that men were - rightly or wrongly - seen as better witnesses, being less likely than women to succumb to the pressures of cross-examination. This danger loomed large in the considerations of contemporaries: Sir John Hawkins, a Middlesex magistrate, believed that many prosecutions were abandoned because of the danger that witnesses 'may be entangled or made to contradict themselves, or each other, in a cross examination, by the prisoner's council.'[45]

How adequately do the witnesses reflect the composition of the population at large? We do not have many independent indicators with which witnesses' characteristics can be compared. One variable that is available, however, is the social composition of the (male) London labour force in 1800. Since our data refers to the period 1749-63, the data can only be compared to a limited extent. Since changes in social stratification cannot be expected to be very rapid, it may nonetheless be excused as a first indicator of the representativeness of our witnesses. Table 1 compares the Schwarz's estimates with our data from the Old Bailey Sessions Papers.

Table 1: Composition of the Male Labour Force

	Schwarz (1800) lower bound	*upper bound*	*Old Bailey (1749-1763)*
upper income	2	3	1.4
middle income+shopkeepers	25	29	27.6
self-employed	5	6	2.8
artisans	23.8	21.7	14.3
semi- and unskilled	(44.2)	(40.3)	53.9
sum	100	100	100

[43]King 1996.
[44]King 1996, table 3.
[45]Beattie 1986, p. 375.

The composition of our 1749/63 sample does not match the composition of the labour force perfectly. There seems to be some bias in favour of the semi- and unskilled, which again argues against the hypothesis that only the wealthy had access to accurate time-keeping. Artisans are underrepresented.

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.[46] Table 2 reports results for both Pearson and Spearman tests:

Table 2: Correlations - Percentage of Male Labour Force

	lower bound	*upper bound*	*Old Bailey (1749-60)*
Pearson			
lower bound	1		
upper bound	0.987	1	
Old Bailey (1749-60)	0.962	0.96	1
Spearman			
lower bound	1		
upper bound	1	1	
Old Bailey (1749-60)	1	1	1

The correlation between the population shares from Schwarz and the witnesses in the Old Bailey Sessions Papers is always 0.9 or above. This result is independent of whether we use cardinal or ordinal measurement. In the context of historical studies, these coefficients indicate a high degree of similarity.[47] We can therefore conclude that, if we use social class as our standard of comparison, no significant difference between our sample and the population can be found. This should not be confused, however, with positive proof that witnesses are representative of the (male working) population at large.

4 How good was the memory of eighteenth-century witnesses?

Witnesses' accounts differ from the data gathered by random hour recall in one important respect - the recall period is markedly longer. This section briefly demonstrates that this does not affect the quality of our data adversely, and offers an explanation why little memory decay should be expected.

Sociologists have conducted extensive experiments to assess how the quality of replies is associated with the time elapsing between event and interview. Patterns for events on weekdays and on the weekend differ sharply. During the week, between 24 and 48 hours after the day under consideration, there is a marked decline in response quality, with between

[46]Nicholas and Johnson 1995, p. 10.
[47]Nicholas and Johnson (1995, p. 10) regard values above 0.7 as sufficient.

10% and 20% fewer activities being reported. Thereafter, there is no clear evidence of further deterioration; surprisingly, the data show a weak recovery after 4-7 days (figure 1). For Friday, Saturday and Sunday, there is no association between the length of recall period and the number of activities reported.[48]

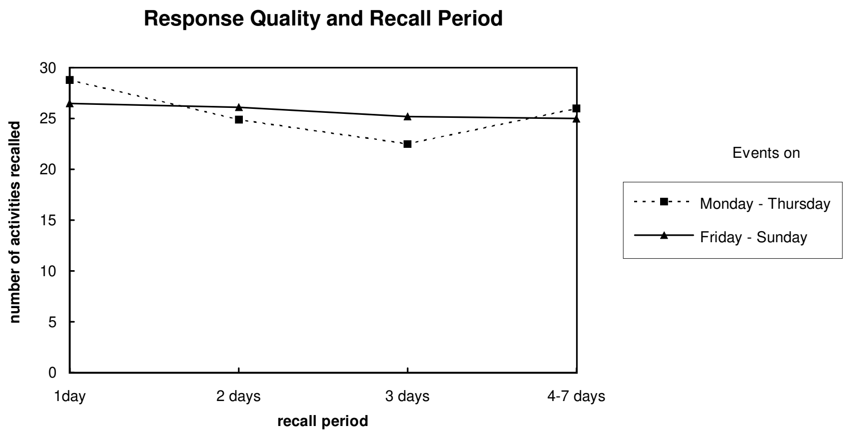

Figure 1

An interesting experiment concerns memory decay and the context of events. Juster uses data on the activity patterns of couples. Each partner was asked to report at various times of the day if the other partner was present. Obviously, with perfect memory, the two reports should be identical. This is only the case for about 80% of all activities. On average, data from later hours of the day is more reliable. His interpretation is as follows:

... match rates would be expected to be relatively low early in the day, and to improve throughout the day as the cumulative probability of a salient event increased. Thus the match rate data are consistent with two propositions: that recall diaries get to be pretty reliable once a salient event has occurred that provides a stimulus to respondents' memory; and second, that if there are enough salient events in a day there will not be very much diary time characterized by unacceptably large memory errors.[49]

It appears likely that the very slow decay of data quality concerning weekend activities also reflects the importance of salient events - weekdays are less likely to contain interesting occasions than the weekend. Since witnessing a crime can be seen as a very salient event, we have some *a priori* reason to believe that memory errors should be few and far between - even after longer periods.

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 50 days had approximately passed since the last session, we would expect that the average witness's memory had to bridge 25 days. This is the minimum we would expect if the timing of court sessions were the only constraining factor. If the average figure was appreciably longer, this could be interpreted as indicative of either longer legal procedures (establishing evidence etc.) or of a substantial backlog of cases before the court.

[48]Juster 1986, table 1, p. 394, p. 395. Some earlier authors are more sceptical. Cf. Kalton 1985, p. 95.
[49]Juster 1985, p. 82.

The average lag between crime and trial in 1749-63 was 45.6 days. This statistic is influenced strongly by outliers. The extreme is marked by one case in which someone was tried six and a half years after he had allegedly committed a crime.[50] If we use the median as a measure of central tendency, thus avoiding the distorting effects of outliers, the result is 30. As the mode in figure 2 demonstrates, the most frequent delay between crime and trial was between 10 and 20 days.[51] Variability was not large - the interquartile range is a mere 31 days.

Did the quality of witnesses' accounts vary with the length of the recall period? Witnesses very often mention the day of the week when they observed a crime. In addition, the court records would note the date of the crime. From the latter, the day of the week can also be calculated. One way of assessing the performance of our witnesses' memories is to check if disagreement between these two types of information is systematically related to the lag between crime and trial. If this is indeed the case, then the longer recall period in our historical data has caused a deterioration in data quality.

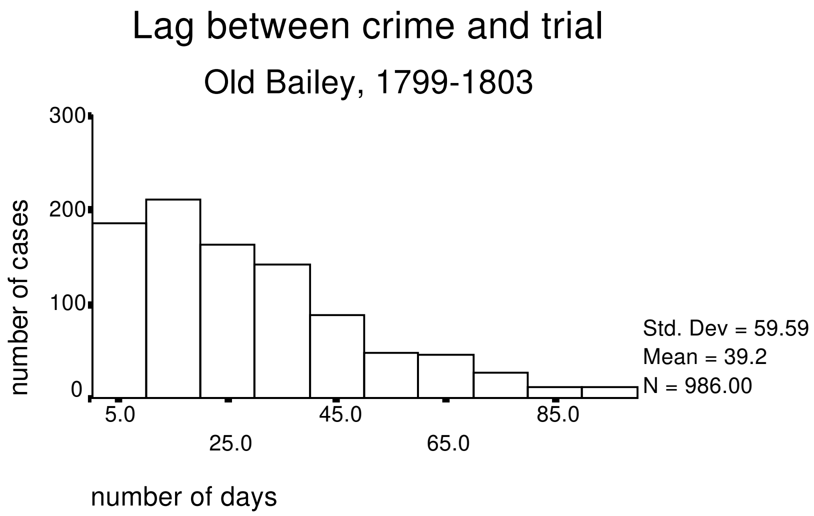

Figure 2

To examine this further, I coded all cases in which there was disagreement between the recorded and the calculated day of the week as 1, and 0 otherwise. This dummy was then used as the dependent variable in a logit regression, using the lag between crime and trial as the predictor variable:

$$C = -1.42 + 0.0044\ LAG \qquad (1)$$
$$\quad (42.1) \quad (1.6)$$

Model $\chi^2 = 1.54$

[C=control variable, 0 if the recorded and inferred day agree, 1 otherwise. LAG=number of days between the crime and the trial; Wald-statistic in parentheses]

[50]Old Bailey Session Papers, Case No. 268, 2.7.1755 (crime committed on 11.1.1749)
[51]Values higher than 100 are included in the last interval.

The exogenous variable is insignificant even at the 80% level, and the model χ^2 indicates that our regression doesn't fit the data very well. The number of mistakes concerning the day of the week in witnesses' reports was not influenced by the recall period.

The second indicator of response quality is the precision of witnesses' statements. During data collection, I recorded if the witnesses indicated the time to the nearest hour, half hour etc. If we use precision to the nearest hour as the cut-off point (coding everything more precise as 1, 0 otherwise), then we can repeat the exercise with a second control variable. The result is as follows:

$$C = -0.68 - 0.0044\ LAG \qquad (2)$$

$$(52.2)\ (1.34)$$

Model $\chi^2 = 2.01$

[C=control variable, 0 if the recorded and inferred day agree, 0 otherwise. LAG=number of days between the crime and the trial; Wald-statistic in parentheses)

Again, the effect of the lag between crime and trial has only a minute influence; as the Wald statistic demonstrates, it is also insignificant. These two tests combined strongly reject the hypothesis that the relatively long lag between an event and a witnesses' testimonial has a detrimental effect on data quality.

5 When Londoners worked

Work appears to have started early in London in the mid-eighteenth century. The average start of work was at 6:50 a.m. There are 44 observations on the time of starting work in our dataset. Nine concern women, 35 refer to men. The confidence interval is rather large - from 5:48 a.m. to 7:51 a.m.. With 15 (out of 44, or 34 percent) of observations only being precise to within one hour, the imprecision of witnesses' statements compounds the problem. The average time of starting for those giving imprecise information was 6:10 a.m.; without these observations, the sample mean would have been 7:11 a.m. The lower and upper bounds consequently have to be adjusted by an additional 7 minutes (0.34´20) if we again assume that it is unlikely for all 15 witnesses to have erred simultaneously on the high or low side by more than 20 minutes.

Before 6 a.m., a little more than a quarter of the individuals who gave evidence at the Old Bailey were already at their workplaces. The cumulative proportion shows a marked increase at 6 a.m., rising to almost 50%. Thereafter, it increases slowly but steadily, until the last witnesses, a stockbroker, starts work at 10 a.m on 30 May 1759.[52]

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

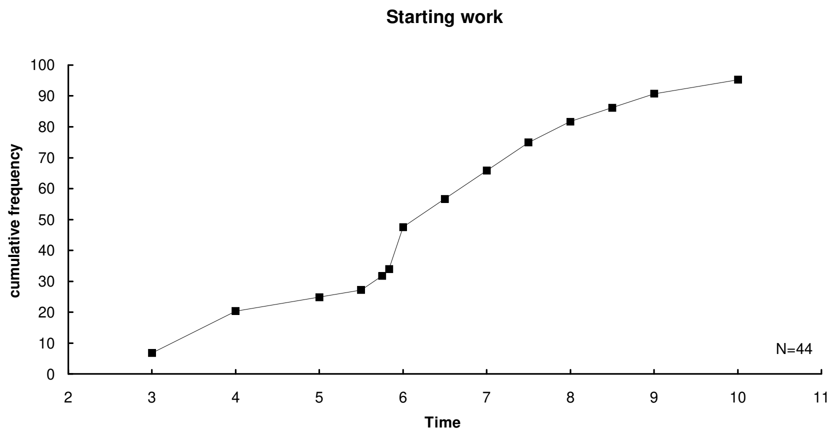

Figure 3

Fifty percent of all witnesses started work between 5:45 a.m and 7:30 a.m. Campbell's *London Tradesman* from 1747 contains a long list of London trades including their 'hours of working'.[53] The average starting time for the 182 professions contained in his work is 6:08 a.m. This does not agree perfectly with our estimate; it is nonetheless easily within the 95% confidence interval. The slight tendency towards later hours in our sample is probably due to differences in sample composition - Campbell restricts himself to artisans, whereas our sample also contains occasional labourers and others who were more likely to start work later in the day. At least in this regard, the data from the Old Bailey and from Campbell's treatise mutually reinforce each other.

On average, work finished at 6:50 p.m. during the 1750s. The estimate of the mean is not very precise: the 95 percent confidence interval extends from 5:44 p.m. to 7:57 p.m. To this, we have to add 7 minutes on either side because of the 35 percent of all observations which were only accurate to within one hour. This yields an estimate of 5:37 p.m. to 8:04 p.m. - clearly a more than narrow range. Yet this is only partly due to the small number of observations. As described above, large differences existed between occupational groups, thereby widening further the confidence interval. In the calculation of the average time of stopping work, I also included the many unskilled labourers who were employed on an occasional basis and often finished their daily work during the early afternoon. Skilled craftsmen, apprentices and masters worked until 7 p.m. or 8 p.m. This finding is in line with eighteenth century regulations: Campbell records an average of almost precisely 8 p.m.[54] - close to the median in our sample (7:30 p.m.).

During no interval do more individuals stop work than between 6 p.m. and 7 p.m., when the cumulative frequency plot shows a sharp rise from 27% to 53%. Thereafter, quite a few individuals carry on working, like the smith who, giving evidence in 1751, said that he 'was coming home from work...' on 6 May at 10:15 p.m.,[55] or the chairman who finished working at 11 p.m.[56]

[53]Campbell 1747, p. 330ff.
[54]Cf. for example Campbell 1747, p. 330ff.
[55]Old Bailey Sessions Papers, Case No. 367, 1750. We cannot rule out the possibility that he spent quite some time on his way back, e.g. by going to a public house. Yet he very clearly felt that the interval was not large enough not to define his coming back as a return from work.
[56]Old Bailey Sessions Papers, Case No. 436, 1754.

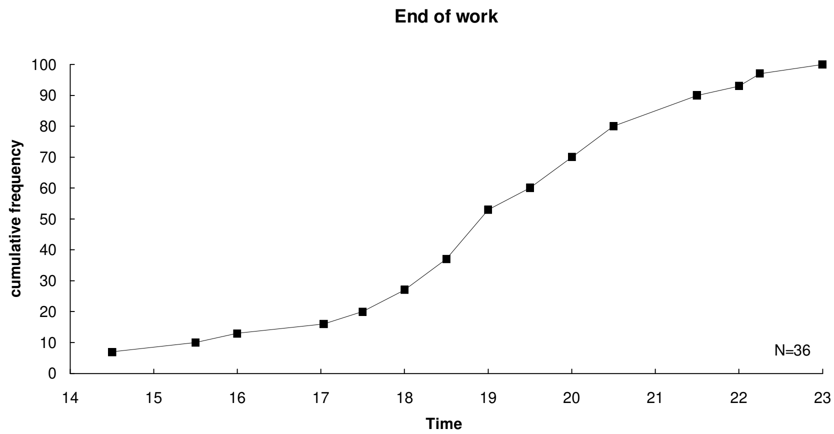

Figure 4

Are those starting and stopping work sufficiently similar for us to compare the two means? I use χ^2-tests on the basis of both sector and gender to test for this possibility. If we use the sector in which a witness is occupied, the Pearson statistic is 5.19, which suggests that we cannot reject the null hypothesis (no significant difference) at confidence levels as low as 65%.[57] This result is reinforced if the exercise is repeated on the basis of gender. Of the 44 witnesses beginning work, 36 were male. For those stopping work, the proportion is somewhat larger - 32 out of 36. The absolute difference is small. It is also statistically insignificant. A χ^2-test yields a statistic of 2.1, which leaves a probability of 35% that both groups are identical with regard to gender composition.

Having established that the two groups are comparable, we can now calculate the duration of work. The difference between starting and stopping work suggests a working day of exactly 12 hours length.[58] From this, we have to deduct one to one and a half hours for breakfast and lunch, which gives 10.5 to 11 hours of work per day.[59] Given that we have taken irregular employment into account, this also corroborates the less quantitative historical literature on the subject.[60]

Next, we have to consider time-use during the week. The custom of Saint Monday - taking an additional holy day at the beginning of the week - has attracted much attention in the literature. E.P. Thompson viewed it as a cornerstone of traditional time-use, and Reid, for example, has argued that it was still being observed in the nineteenth century.[61] Clearly, no day will be used for rest by everyone. As a first step, it may therefore be sensible to count as 'days off' those that (a) show a marked reduction in the number of people at work and (b) have a probability of observing people in work that is similar to a Sunday. Differences between individual days are pronounced. While the number of observations from Monday to Saturday fluctuated between 131 and 167 cases, Sunday registered a low of 89 cases. The variation is not random - the χ^2-test yields a statistic of 27.9. With six degrees of freedom, the null hypothesis is rejected at the 99.9 percent level of significance.[62]

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[57]Witnesses were assigned to five categories: agriculture, trade, manufacturing, services, and other.

[58]The difference amounts to thirteen hours if we compare medians.

[59]Breakfast was taken after the start of work, thereby interrupting work during the early morning. On average, the nine witnesses who reported this activity ate their early morning meal at 8.50 a.m.

[60]Daily working hours for fully employed artisans ranged from 12 to 14 hours. Cf. for example George 1925, p. 205ff.

[61]Thompson 1967, p. 64f; Reid 1976, p. 76ff.

[62]During the week from Monday to Saturday, there is no indication of a non-random pattern (χ^2-statistic 5.6).

The highly uneven incidence of crime during the week is reflected by equally large variations in the probability of observing individuals in paid work. Table 3 gives the number of individuals working by day of the week, the total number of cases per day, as well as the resulting percentage.

Table 3: Paid Work by Day of the Week

Day of the week	*Number engaged in work*	*Number of cases*	*Percentage*	*Deviation from average (%)*
Monday	27	167	16.2	-23.17
Tuesday	28	131	21.4	1.49
Wednesday	38	159	23.9	13.35
Thursday	35	146	24.0	13.82
Friday	36	150	24.0	13.82
Saturday	36	140	25.7	21.88
Sunday	11	89	12.4	-41.19

On weekdays, an average of approximately 23% of our witnesses were engaged in paid work. Wednesday, Thursday, Friday and Saturday registered a slightly higher ratio; Mondays and Sundays both show less than 20% of those observed as working.

The variation in the number of those recorded as working is not random. A χ^2-test gives a result of 19.7, which is sufficient to reject the null hypothesis of random variation at the 99 percent level of confidence. We have now established that the weekly pattern of work was non-random, and that some days were markedly different from others. This, however, is not sufficient to conclude that, for example, Monday was statistically different from all the other days of the week. I proceed as follows. First, Mann-Whitney U-tests are performed, comparing each individual day with the remainder of the week. The Mann-Whitney U-statistic is calculated as follows: First, the data from both samples is combined, and then ranked from smallest to largest. The first test-statistic is then calculated by examining each A value (from sample A) and counting the number of B values which precede it. The second test statistic, the number of times a B value precedes an A value, is then given by the formula:[63]

$$U2=(n1*n2)-U1 \qquad (3)$$

[U1, 2 - first and second test statistic, n1, 2 - sample sizes]

The *smaller* of the two values becomes the Mann-Whitney test statistic U. On the basis of these two non-parametric techniques, we are able to establish if the overall pattern is random, and if individual days diverge significantly. In a third step, the magnitude of this divergence is assessed by running logit regressions on the incidence of work. In principle, the regressions could take the place of the Mann-Whitney U-test. However, only the latter is a non-parametric technique, and since we cannot be assured that the observations in our data set are

[63]Norusis 1988, p. 137ff.

normally distributed, it seems prudent to proceed from the more general technique to the one that makes more stringent demands on the data.

Table 4 presents results from twenty-one Mann-Whitney U-tests. The incidence of work, coded as a dummy variable, was tested for systematic differences by means of a grouping variable, which took the value of one for the weekday under consideration, zero otherwise.

Table 4: Mann-Whitney U-Tests for Work on Different Days of the Week

		Mo	*Tu*	*Wed*	*Thu*	*Fri*	*Sat*	*Sun*
w1	U	65020	56772	65032	60610	61950	57320	36428
	Z	-1.9*	-0.08	-0.77	-0.75	-0.77	-1.3	-2.2**
	Prob.	0.06	0.95	0.44	0.45	0.44	0.2	0.03
	Mean Rank	-32.61	-1.3	13.67	13.89	14.12	23.92	-40.3
w2	U	63840	55805	64392	57950	63550	58420	36368
	Z	-2.12**	-0.46	-0.93	-1.7*	-0.08	-0.71	-2**
	Prob.	0.03	0.65	0.35	0.09	0.94	0.48	0.04
	Mean Rank	-41.09	9.77	18.45	35.22	-1.6	14.78	-51.5
w3	U	62650	54978	65522	60670	60950	56720	36638
	Z	-2.4**	-0.7	-0.5	-0.6	-0.99	-1.3	-1.7*
	Prob.	0.02	0.46	0.64	0.55	0.32	0.2	0.08
	Mean Rank	-49.75	17.05	10.01	13.41	-21.96	28.91	-48.1

Note: *, ** indicates significance at the 90 and 95 percent levels, respectively.
w1, w2, w3: see text.

Three different definitions of work were employed. W1 denotes all those case where witnesses engaged in normal work activities during the day;[64] w2 also includes those cases where witnesses started or stopped work on a specific day. W3 is the most comprehensive of the definitions employed - it includes all cases in w2 plus work in shops, which has been excluded in w1 and w2 because there are *a priori* reasons to suspect that shopkeepers kept different hours.[65]

Only Monday and Sunday showed strong, statistically significant and consistent differences on the basis of the Mann-Whitney U-test.[66] As the mean ranks indicate, the probability of finding people at work on these days was much lower; this result is robust to changes in the definition of work. All other days do not differ from the rest of the week. The one exception is Thursday under definition w2, which indicates a higher likelihood of work.

[64]In table 3 above, w1 was used.
[65]There are obvious incentives for shopkeepers to open when work has ceased for the majority of the labour force.
[66]Note that, if we compared Monday only with other days of the week, constrasts would be even more pronounced - the presence of Sundays in our sample increases total variance.

Since this is, however, not a persistent feature, we cannot conclude on the basis of these tests that Thursdays registered markedly more work than, say, Tuesdays.

To clarify this question, I use logit models, regressing a dummy variable for work / no work on a dummy for the day of the week.[67] The use of a logit regression is necessary since the dependent variable is dichotomous.[68] The relationship can be stated as

$$\zeta = \frac{1}{1+e^{-(\beta+\gamma x)}} \tag{4}$$

where ζ is the probability of the activity an individual is involved in (in this case, paid work), β and γ are coefficients estimated from the data, x is the exogenous variable, and e is the base of the natural logarithm. After taking the logarithm of both sides and arithmetic manipulation, we obtain equation (3), which can easily be estimated under OLS:

$$\log \frac{\zeta}{1-\zeta} = \beta + \gamma x + \varepsilon \tag{5}$$

Results were as follows:

Table 5: Logit Regressions

(dep. var.: individuals engaged in work - w1)

Weekday	*B*	*Wald*	*ΔOdds Ratio*	*Significance*
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

Note: *, ** indicates significance at the 90 and 95 percent levels, respectively.
The Wald-test has a χ^2 distribution; significance levels according to Hauck and Donner 1977, p. 851ff.

Sunday, Monday and Tuesday show a negative divergence from the average. For Mondays and Sundays, the difference is statistically significant. In both cases, the odds of observing individuals in work are almost halved. Wednesdays, Thursdays and Fridays again show small increases, but the significance levels are very low. The only other day that gives a significant coefficient is Saturday, which shows a large (54%) increase in the odds of finding individuals in work. The results from logit regressions reinforce findings obtained through the Mann-Whitney U-tests. In one instance, however, the results are different. Saturdays showed a large and positive mean difference in table 4, yet the divergence was insignificant at the customary rejection levels. Since the assumptions underlying the application of logit regressions are

[67]Hardy 1993.
[68]Demaris 1992.

more exacting than those for Mann-Whitney U-tests, the probability of a significant divergence rises from 80% to 96.7%. This demonstrates that our finding of a significant divergence on the basis of logit regressions is sensitive to the test employed.[69]

How large are the differences? The dependent variable in our regressions is the log odds ratio (the ratio of the probability of an event occurring and the probability of it not occurring). From equation (3), the odds ratio itself can then be written as:

$$\frac{\zeta}{1-\zeta} = e^{\beta+\gamma x} \qquad (6)$$

Consequently, e^{γ} gives the change in the odds ratio. For a Monday, for example, this means that the odds of observing people in work are 0.59 of what they are on other days of the week.[70] On Sundays, the odds were reduced to 0.52, while Saturdays showed an increase by 0.54.

Table 6: Logit Regressions

(dep. var.: individuals engaged in work - w2)

Weekday	*B*	*Wald*	*ΔOdds Ratio*	*Significance*
Sunday	-0.51	3.57	0.6*	0.059
Monday	-0.49	6.1	0.61**	0.014
Tuesday	0.01	0.23	1.02	0.93
Wednesday	0.21	1.27	1.23	0.26
Thursday	0.33	3.0	1.38*	0.08
Friday	-0.087	0.2	0.92	0.66
Saturday	0.29	2.3	1.34**	0.017

Note: *, ** indicates significance at the 90 and 95 percent levels, respectively.

How are our results affected when the alternative definitions of work are used as dependent variables instead? Tables 6 and 7 present the results for w2 and w3.

[69]Below, I show that this finding is also sensitive to the definition of work employed. This suggests that we should not place too much emphasis on the increased work activity on Saturdays.

[70]For the methodological background, cf. Hardy 1993, p. 9.

Table 7: Logit Regressions

(dep. var.: individuals engaged in work - w3)

Weekday	*B*	*Wald*	*ΔOdds Ratio*	*Significance*
Sunday	-0.4	3.02	0.67*	0.08
Monday	-0.41	5.52	0.66**	0.019
Tuesday	0.14	0.54	1.15	0.46
Wednesday	0.08	0.22	1.08	0.64
Thursday	0.11	0.36	1.11	0.55
Friday	0.18	1.0	1.19	0.32
Saturday	0.23	1.63	1.26	0.2

Note: *, ** indicates significance at the 90 and 95 percent levels, respectively.

Our main finding is not sensitive to the definition of work. In all three tables, Mondays are days of less work. The same is true as well of Sundays, although this is hardly a surprise. Saturday retains its significance under definition w2 - normal work including starting and stopping, but excluding shopkeeping. If the broadest definition is used, the significance again disappears (although the size of the effect is still large). These results strongly suggest that Sundays and Mondays were days of rest, and that the pace of work increased during the week. This is in line with many suggestions in the historical literature (cf. section 1). The high point on Saturday probably affected many, but it was not a universal phenomenon.

Even after the disappearance of old Catholic holy days, there was no shortage of festive occasions in the English calendar. Freudenberger and Cummins have argued that, during the middle of the eighteenth century, work ceased on up to 46 days in the year plus Sundays, Easter and Whitsun.[71]

The data set from the 1750s and early 1760s allows a direct test of the hypothesis that work ceased on the 46 holy days listed by Millan. I apply the same two-step procedure used earlier. First, the Mann-Whitney U-test is used.

Table 8: Mann-Whitney U-tests for Work on Holy Days

	w1	*w2*	*w3*
U	51555	52385	54888
Z	-2.4**	-1.83*	-0.75
Probability	0.016	0.067	0.45
Mean Rank	-47	-39.67	18.3

Note: *, ** indicates significance at the 90 and 95 percent levels, respectively.

Work was markedly less frequent on holy days. This is true (at the 95% confidence level) for all normal work activities and, at the 90% level, for w1 plus starting and stopping of work (w2). If, however, shopkeeping is included also, the result is reversed. Work is more frequent now, but the difference is not significant. Clearly, this suggests that shopkeeping did not follow the same temporal pattern as, say, making watches or driving sheep. Before we can be

[71]Freudenberger and Cummins 1976, p. 6 based on Millan 1749, p. 15.

assured that such wider conclusions can be drawn, let us examine the effect of using different definitions of holy days.

Holy days are not a homogenous group. The most obvious distinction that needs to be drawn is between political and non-political holy days. The former were much less frequent than the latter - in Millan's list from 1749, there are 15 political holy days (like the 5th of November, the anniversary of the gunpowder plot, and the birthdays of various princes) alongside 30 religious festivals.[72] Table 9 presents the results.

Table 9: Mann-Whitney U-Tests for Work on Political and Religious Holy Days

	political 'holy days'			*religious holy days*		
	w1	*w2*	*w3*	*w1*	*w2*	*w3*
U	10493	11993	11230	46454	45452	48682
Z	-1.71*	-0.23	-0.83	-1.89*	-2.15**	-0.69
Probability	0.087	0.82	0.4	0.058	0.032	0.49
Mean Rank	-72.09	10.56	41.8	-39.32	-49.32	17.11

Note: *, ** indicates significance at the 90 and 95 percent levels, respectively.

If we use the most restrictive definition of work, then both political and religious holy days show a marked reduction in work activities. In the case of church festivals, this finding is robust even if starting and stopping is included, but the same cannot be said of work (w2) on political holy days. It should be borne in mind that the introduction of multiple subdivisions reduces sample sizes rapidly. Both groups of holy days indicate that work - including that carried out in shops - is *more* frequent on holy days, but the divergence is not observed with sufficient consistency to be statistically significant.

Next, we need to establish the magnitude of effects. I have run logit regressions to establish how the chances of finding individuals in work were influenced by Millan's holy days.

[72]The total is actually 46, since the 2nd of September, the day of the commemoration of the Great Fire in London, is probably best categorized as neither a religious nor political festival.

Table 10: Logit Regressions - Work on Holy Days

Dependent variable	*w1*	*w2*	*w3*
Holy days			
B	-0.63	-0.4	0.14
Wald	5.6**	3.3*	0.56
Probability	0.018	0.068	0.45
Change in Odds Ratio	0.53	0.67	1.15
Political 'holy days'			
B	-1.18	0.098	0.33
Wald	2.7*	0.05	0.68
Probability	0.09	0.82	0.41
Change in Odds Ratio	0.31	1.1	1.4
Religious holy days			
B	-0.52	-0.51	0.14
Wald	3.5*	4.5**	0.47
Probability	0.06	0.033	0.49
Change in Odds Ratio	0.59	0.6	1.15

Note: *, ** indicates significance at the 90 and 95 percent levels, respectively.

The same combinations of dependent and explanatory variables lead to significant coefficients. Using all holy days as the regressor, we find a striking reduction in the odds of observing individuals in work. This effect is significant at the 95% confidence level. If starting and stopping of work is included, the magnitude of the effect is reduced somewhat, and the coefficient is significantly different from zero with more than 90% probability. Again, political festivals give the weakest support to the hypothesis that work was less frequent. Only when the strictest definition of paid employment is applied do we find a significant coefficient on the independent variable. Activity patterns on religious holy days are more favourable to the Freudenberger and Cummins hypothesis that old festivals restricted the length of the working year. Just as for all holy days, the first two definitions of work yield a strongly negative effect of religious festivals.

In general, shopkeepers were distinctly less likely to close down on these days. Political holy days also show a much weaker association with a reduction in the incidence of paid work. Where statistically significant effects exist, they are also large in magnitude. The change in the odds ratios is very similar to the one observed on Sundays and St Mondays. This strongly suggests that neither St Monday nor Millan's holy days were 'normal working days'.

How can we infer the length of the working year from these estimates? There are three alternative approaches:

1. In the 'naive' case, we simply assume that all work stopped on Mondays and holy days in 1750. We do not adjust for differences between groups, and for differences between different types of holy days. This is equivalent to assuming that the people that we

observe in work on these days in 1750 rest at a different time. This provides a lower bound on the length of the working year.

2. Alternatively, one could argue that those encountered at work on 'St Monday', old holy days and Sunday in 1750 did not catch up on lost leisure at some other time. Reductions in the odds of observing witnesses at work indicate the number of people not working. Also, shopkeepers do not rest on holy days. Our estimate for the length of the working year in 1750 would therefore be correspondingly higher.

3. Finally, we can restrict the holy day effect to religious ones alone, since the evidence for a reduced incidence of work on political holy days is sensitive to the definition of work.

Table 11 compares the results from these three approaches.

Table 11: Alternative Estimates of the Number of Working Days, 1749-63

		Monday	*Old holy days*	*Easter, Christmas, Sunday*	*Sum*
assumption (1)	365	- 52.14	- 46	- 59.14	207.7
assumption (2)	365	- 36.5	- 29.4§	- 41.4	257.7
assumption (3)	365	-36.5	-24.8§	-41.4	262.3

Note: § Adjusted for the number of shopkeepers (Schwarz 1992, p. 57).

The range of estimates is not small - our lower bound estimate (assumption 1) is 20.8 percent lower than the upper bound calculated under assumption 3. This should not detract from the fact that the data has allowed us to evaluate various hypotheses in the historical literature using everyday patterns of time-use referring to over 1,000 individuals. Section 6 will demonstrate that, despite the range of our estimates, these results can help clarify conflicting evidence.

6 Implications

The time-use data has further implications for the history of income. Lindert and Williamson recently re-examined 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.[73] Estimates of mean weekly income appear unconvincing when compared with daily wage rates from other sources, they argue. Table 12 gives an overview of their results:

[73]Lindert and Williamson 1982, p. 395f.

Table 12: Length of the Working Week

	Weekly income (1)	Daily wage (2)	Implied number of working days [col. 1/ col. 2] (3)
Observations	108	22+	4.9
	60	13	4.6
	135	33	4.1
	99	20	4.95
Average (Lindert and Williamson)			4.64
Using both observations	108	24	5.4
	108	20	4.5
Alternative average			4.79

Note: + average taken from range 20 - 24 d./day.

Dividing the former by the latter implies a working week of only 4.64 to 4.79 days. Lindert and Williamson's results are 4.9, 4.6, 4.1 and 4.95, giving an average of 4.64. One of their sources for daily wage rates (building labourers) actually gives a range of 20-24 d./day. They use the average of 22 d., which combined with weekly earnings of 108 d. implies a workweek of 4.91 days.[74] Alternatively, we can use the upper and lower bound estimates of the daily wage rate, and replace their midpoint observation with these. This raises the average implied number of working days slightly, but both methods can be defended since my revision implies that we give double weight to one observation.

Lindert and Williamson deem their figure of 4.64 working days per week much too low since they believe that there is overwhelming evidence for a six-day working week at this time (or more than 25% more than the implied figure). They cite Bienefeld as a source.[75] 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.[76] Second, they do not take account of the large number of public and religious festivals still prevailing at this date. Converting scenarios 1, 2 and 3 derived in section 4 by dividing the number of working days per year by 52 suggests an average of 4.66 days /week. This is 0.13 days less than the revised calculation in table 12. Estimate 1 is markedly lower than the figure implied by Massie by a mere 0.79. For estimates 2 and 3, the difference is very small - 0.17 and 0.25 (equivalent to 3.4 and 5.3 percent). Our finding of a comparatively short working week in 1749/63 therefore resolves

[74]Lindert and Williamson 1982, p. 395.
[75]Lindert and Williamson 1982, p. 398f. They also believe that the implied figures are much too low because secondary breadwinners also contributed to family earnings.
[76]Bienefeld 1972, p. 36ff.

the inconsistency in favour of Massie *and* it vindicates the accuracy of the contemporary wage assessments used by Lindert and Williamson.

The value of these calculations is twofold. While 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 labour input help to resolve some of the puzzles posed by previously conflicting evidence about weekly income, real wages, and working time. 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 11 are also of interest for the historiography of the Industrial Revolution. They indirectly lend further support to the revisionist's view that output growth in the period after 1750 was less spectacular than was previously thought.[77] The working week in London during the 1750s was clearly shorter than five days, as this paper has argued. If we accept that later periods registered higher workloads, then living standards before the Industrial Revolution compared to later periods were commensurately higher.[78] The underlying assumption for such an assessment is that full income in past periods should be calculated as realized income plus the income foregone while engaged in leisure activities. Nordhaus and Tobin were among the first to calculate full income as the sum of wages earned plus the value of consumption.[79] In addition to ascertaining the length of the working year at later stages, we would have to assess if leisure during the middle of the eighteenth century was voluntary - only then could changes in living standards be revised in the way I have outlined. Further, it will be necessary to decide if leisure should be evaluated at the wage rate of some base year, or at the wage rate of some future year.[80] Note, however, that this problem will affect individual periods in different ways - since productivity and consumption only grew slowly before 1800, these difficulties will be more acute for the first half of the nineteenth century than for earlier periods.[81] Thus, the new method introduced in this paper will eventually allow us to evaluate everyday patterns of labour and leisure in later years. It will then be possible to determine if, as Crafts has suggested, one item '... neglected by national-income accounting may dominate conventional consumption in this period...'.[82]

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[77]Crafts 1985, p. 26-34.
[78]This is commonly accepted, cf. Tranter 1981, p. 220f; Crafts 1985, p. 110; Voth 1996, table 11.1, p. 175. Matthews, Feinstein and Oddling-Smee (1982) estimate that, in 1856, annual labour input was equivalent to 3185 hours. This contrasts with 2566 to 2688 hours, if our mean estimate in table 11 is multiplied by 10.5 and 11 hours, respectively. The underlying assumption would have to be that idleness was voluntary.
[79]Nordhaus and Tobin 1972.
[80]Cf. Crafts 1985, p. 114.
[81]Crafts and Harley 1992, table 5, p. 718. Crafts and Harley 1995, p. 142f.
[82]Crafts 1985, p. 114.

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