Malthus in the Bedroom: Birth Spacing as a Preventive Check Mechanism in Pre-Modern England*

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Abstract

We question the received wisdom that birth limitation was absent among historical populations and that it did not emerge before the fertility transition of the late nineteenth century. Using duration, panel and instrumental variable models on a rich set of Anglican parish data we find a statistically significant, negative effect of living standards (real wages and wheat prices) on birth intervals in the three centuries leading up to England's fertility transition. While the effect could be driven by biology in the case of the poor, the presence of an effect also among more affluent families suggests that birth spacing was used as birth control mechanism in pre-modern England. Our findings thus support the Malthusian *preventive-check* hypothesis and motivate England's historical leadership as a low population-pressure, high-wage economy.

Keywords: Spacing, birth intervals, fertility limitation, natural fertility, preventive check

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1 Introduction

The timing of the industrial revolution and the onset of the demographic transition strongly influenced the growth pattern of modern economies (Galor 2005). Unified growth theory (Galor and Weil, 2000; Galor and Moav, 2002; Hansen and Prescott, 2002; Jones, 2001) provides a framework which explains the long-run transition from Malthusian stagnation to modern economic growth. England was the first country to make the transition from a Malthusian economy to one of sustained economic growth, and its world leadership during the eighteenth century is often attributed to its fertility restrictions (Voitgländer and Voth 2009, 2011). The argument made is that Malthusian *preventive checks* (i.e. birth limitations in periods of economic hardship) kept the population pressure low, allowing higher incomes per capita (Wrigley and Schofield 1981).

Scholars have long believed that late age at first marriage of women was the main preventive check mechanism operating in England prior to its demographic transition of the late nineteenth century (Voigtländer and Voth 2011). Indeed, early work by Wilson concludes that pre-industrial England was absent of any marital birth limitation, classifying it as a *natural fertility* society (Wilson 1984). Later, statistically more advanced studies have found little or no effect of living standards—measured in terms of real wages and food prices—on aggregate birth rates.[1] This fits well with the conclusion reached by the European Fertility Project, holding that marital birth limitation was invented during the European fertility decline of the late nineteenth century, achieved by the diffusion of knowledge about contraceptives, such as *coitus interruptus*, sexual abstention, and extended breastfeeding (Ansley et al. 1986).

Recent works, however, especially in the field of historical demography, have shown that marital birth limitation was practiced in the Low Countries, Germany, and Sweden from the late eighteenth century onwards (Bengtsson and Dribe, 2006; Dribe and Scalone, 2010; Van Bavel, 2004a; Van Bavel and Kok, 2004). Yet, with the exception of Wrigley's study of the parish of Colyton (Wrigley, 1966) and Wilson's subsequent

[1] Bailey and Chambers (1993); Crafts and Mills (2009); Kelly and O Grada (2012); Lee and Anderson (2001).

analysis of 13 English parishes (Wilson, 1984), no attempts have been made to analyze fertility restriction at the household level before the English fertility transition exploring the English parish records, despite England's key role in the long-term economic development of the west.

Equally surprising, the numerous attempts to document a short-term response of marriage rates and birth rates to living standards in the aggregate offer very little evidence in favor of Malthusian preventive checks operating in England before 1800 (Bailey and Chambers, 1993; Crafts and Mills, 2009; Lee, 1981; Lee and Anderson, 2001; Weir, 1984). This lack of evidence may be grounded in two key issues. First, the use of aggregate data tend to average out the fertility response of different socioeconomic groups, making it difficult to study the impact of living standards on marriage and birth decisions among the people most prone to economic distress, e.g. the poor. Second, the crude birth rates, as well as the crude marriage rates, are incomplete proxies for marriage and birth decisions within the family, in that crude vital rates fail to fully reflect the demographic composition of the population. In fact, the vital rates do not capture entirely the household's spacing behavior, the study of which requires access to the demographic statistics at the family level.

In this paper we investigate marital birth limitation in pre-transition England, casting serious doubts on the notion that England was a natural fertility society. We show that the length of the birth interval functioned as a *preventive check* mechanism among English couples whose response to falling living standards manifested itself in an extension of the time-span between the births of their offspring. More generally, this is the first study to provide a comprehensive picture, at the micro-level, of the relationships between English living standards and the patterns of family planning (including marriage, starting, spacing, and stopping) before its demographic transition.

We use family reconstitution data from Anglican parish registers to investigate the effect of living standards on the timing of family births in the three centuries leading up to England's fertility decline in the late nineteenth century. Equipped with a variety of statistical tools (i.e. duration, panel and instrumental variable models) we attempt to advance the research frontier along several dimensions. First, we exploit a smaller but substantially richer sample of the data previously used to study effects at the aggregate level.[2] Second, the nature of our data (family reconstitutions) allows us to control for a wide range of family characteristics, including the location, education and fecundity of the spouses. Third, information about the husband's occupation enables us to isolate the families most vulnerable to economic hardship: the poor. Finally, vital dates in the data permit the use of duration analysis, meaning that we can study the influence of living standards on the *timing* of events.

Malthus conjectured that periods of economic difficulty were met by delayed marriages (Malthus 1789), a hypothesis which we are the first to test directly. Still, delayed marriage was not the only precautionary action a couple could take to reduce their births. Historical families were relatively large (averaging 6-7 children) and the decision about the timing of a birth could be made repeatedly throughout the marriage. This makes the spacing of family births a potential preventive check mechanism. For completeness, we also investigate the influence of living standards on the timing of the first- and last-born (known in demography as 'starting' and 'stopping' behaviour).

Our findings cast severe doubt on the widespread belief that pre-industrial England was a *natural fertility* society. Virtually all our statistical specifications demonstrate a negative effect of living standards (real wages and wheat prices) on the spacing of family births in the three centuries leading up to England's fertility transition, supporting the notion that economic hardship led to longer birth intervals. Importantly, the effect is prevalent among the poor (labourers and servants) as well as among their more affluent counterparts (farmers, traders, merchant, and gentry), with the poorest groups displaying the largest effects (as expected).

We argue that the increased spacing between family offspring resulted from actions taken by the couples and, thus, was not *only* due to a biological effect (i.e. starvation causing infertility). This is substantiated by two main findings: (*i*) a negative relationship between living standards and birth intervals is present across different socio-economic groups and not only among the poor; and (*ii*) the fact that the effect remains significant even when we exclude years of severe economic depression or extreme climatic conditions (the latter causing failed harvests and hunger).

[2] Wilson (1984) uses a sub-sample of our data, but a rather less advanced statistical strategy.

Consistent with findings by Clark and Hamilton (2006) and Boberg-Fazlic et al. (2011), demonstrating that the rich had more offspring than the poor, our results imply that this was achieved through relatively shorter birth intervals among the rich. Also, our investigation of the behaviour of different socio-economic groups reveals that farmers, as expected, behave opposite to other occupational groups, *increasing* their birth spacing intervals in response to higher wheat prices. This finding further rationalizes the notion of behavioural effects over biological explanations.

Our analyses confirm the Malthusian paradigm that lower living standards led to delayed marriage and late first conceptions (i.e. postponed 'starting'). Moreover, the fact that living standards have *no* effect on the waiting time from a couple's marriage date to their first conception verifies the premonition that marriage in the past marked the onset of unprotected sex (Wrigley et al. 1997). We also conclude that the timing of the last delivery ('stopping') is unaffected by living standards. However, the finding that the rich stopped sooner than the poor is further evidence in favour of birth limitation taking place centuries before the demographic transition of the end of the nineteenth century.

In addition to the use of duration models, we also analyze the data in a panel setting which enables us to account for heterogeneity at the family level. Moreover, the possible existence of unobserved time-varying variables, correlated with both real wages and birth intervals, raises concerns that our estimates could be biased. To address this issue, we adopt an instrumental variable approach, identifying exogenous variation in real wages using monthly air temperatures. Weather conditions (as captured by air temperature) influence crop yields and thus wheat prices and real wages. The identifying assumption is that the monthly air temperatures have no direct effect on birth intervals. The instrumental variable estimates confirm the negative effect of living standards on the spacing of births.

The rest of the paper is organized as follows: First, we describe the key features of the data and the problems related to their use (Section 2). In Section 3 we analyse fertility behaviour by estimating duration models. In Section 4 we adopt a panel structure and address the issue of causality using an instrumental variable approach. In Section 5 we present some robustness checks to confirm that the adjustment of birth intervals is not only a biological mechanism. Section 6 concludes.

2 Data

The analysis below is conducted using three pieces of data: real wages, wheat prices and demographic statistics. Starting with the latter, the demographic data used to compute the timing of our events come from Anglican parish registers (English church books). Collected over the past 40 years by the *Cambridge Group for History of Population and Social Structure* the full data comprise a total of 404 parish records. Documented by Wrigley and Schofield (1981) this sample provides yearly birth-, death-, and marriage-rates covering the period 1541-1871. Counting the number of events per 1,000 persons, these rates have been previously used to test the Malthusian *preventive check* hypothesis.

Meanwhile, inspired by Louis Henry's family reconstruction of French parish data, the Cambridge Group selected 26 out of their 404 English parishes and used the ecclesiastical events to reconstitute over 80,000 families, comprising nearly 280,000 individuals. The 26 parishes (forming what we call the *Reconstitution data*) were chosen for their remarkable quality and because they appeared to be representative of the entire country. The sampled parishes range from market towns to remote pastoral villages, including proto-industrial and urban communities along with agricultural ones (Figure 1). The data is documented in detail by Wrigley *et al*. (1997).

[Figure 1: The 26 parishes]

In a descriptive analysis of the parish of Colyton (one of the 26 reconstituted parishes) Wrigley found evidence pointing towards deliberate birth limitation taking place in the period around 1700. This was obtained, he argued, through late marriages, extended birth intervals, and low stopping ages (Wrigley, 1966). However, after adding another 12 parishes to the sample (totalling 13 of the 26 parishes) Wilson revised Wrigley's conclusion, stating that 'while the existence of family limitation in pre-industrial England cannot be ruled out, it is highly unlikely that it was of any significance in determining the overall pattern of marital fertility' (Wilson, 1984,

p. 240).[3] Below we extend the work of Wrigley (1966) and Wilson (1984) by including all 26 parishes in our sample. Moreover, by means of modern econometric techniques we are able to deal more substantially with any geographical and family heterogeneity present in the data.

Family reconstitution data offer many more details (and hence covariates) compared to the (aggregate) birth- and marriage rates used in the recent studies of birth patterns.[4] Indeed, every family in the Reconstitution data is build up around a marriage, providing information about the birth (baptism) dates and death (burial) dates of the spouses, as well as the gender and birth and death dates of their offspring.

The church typically noted down baptism dates rather than birth dates. We generate a birth date variable using the actual birth dates when available. To obtain the date of conception, which we will use in the analysis, we subtract 280 days from the birth date variable.[5] Moreover, in order to assess the quality of the birth dates, Figure 2 and Figure 3 illustrate the distribution of births by month and year, respectively. The distribution by month does not show any significant heaping. However, Figure 3 indicates some heaping, especially in the months of January and December. The spike on the 25th of December can be explained by the preference of families to baptize their children on Christmas Day. The spike on the 1st of January is possibly related to missing (unreadable) dates, imputed by the transcribers as the first date of the year. Note that, since England switched from the Julian to the Gregorian calendar during our period of study, we have converted all dates into the Gregorian calendar.[6] The spike on the 11th of January is thus due to the same reason as the spike on the 1st of January. Hence, in the analysis below we use controls for the following dates: 25th of December, 1st of January, and 11th of January.

The data also provides ample information about the socio-economic background of the family, as well as the education and fecundability of the couple. For instance, the

[3]Reviews and critics of the Wrigley and Schofield (1983) study are also included in a special issue of the *Journal of Interdisciplinary History* published in 1985.

[4]Bailey and Chambers (1993); Crafts and Mills (2009); Kelly and O Grada (2012); Lee and Anderson (2001).

[5] The traditional definition of a full-term pregnancy is 40 weeks. Our results are not sensitive to a different definition.

[6] Britain adopted the gregorian calendar in 1752, by which time it was necessary to correct by 11 days.

clergy frequently reported the occupation of the spouses (though far more often for men than for the women). The occupations were noted down at the time of marriage and burial, as well as the baptisms or burials of offspring. Using wills records from historical England, Clark and Cummins (2010) have constructed seven socio-economic groups, ordered according to the wealth information found in the wills. The occupational titles thus permit a classification of our families according to their wealth or income potential. From the poorest to the richest these are: labourers, husbandmen, craftsmen, traders, farmers, merchant, and gentry.[7] We use the earliest known occupation of the husbands to classify our sampled families (and a binary variable if occupation is missing). Educational information comes from the spouses' signatures on their wedding certificates (as opposed to leaving a mark) revealing in this way their literacy status. This is a widely used indicator of human capital before public schooling became prevalent (Clark 2008). Finally, as is standard in historical demography (e.g. Wrigley et al. 1997), the fecundability of our couples is inferred from the time-interval between their marriage and their first birth.[8]

While family reconstitution data provide an invaluable source of information, they are also subject to a set of restrictions.[9] A natural limitation is that any event happening outside of the parish of origin is not recorded in the parish register and, therefore, does not show up in the reconstitution. It is reasonable to assume that migrating and non-migrating families did not differ systematically with respect to their fertility response to changes in living standards. However, we performed several robustness checks to ensure that our estimates are not biased because of selective migration. Indeed, we can show that when constraining the sample to families that are *completed* (that is, we can observe them through to the end of the wife's reproductive period) we obtain qualitatively the same results.[10]

[7] We are grateful to Greg Clark for providing us with the mapping procedure.

[8] Fecundability is the probability that conception will occur in a given population of couples during a specific time period.

[9] See Ruggles (1999) for a more in-depth explanation of possible sources of error in the English family reconstitution and in the analysis performed by Wrigley et al. (1997).

[10] A family with completed fertility is defined as a marriage in which both the wife and the husband survived (at least) until the wife reached the age of 50 years. It therefore consists of a couple that exhausted their reproductive lifetime in the parish of origin.

A related issue is that some couples may have migrated only temporarily. If these couples had children before and after the migration period, an unusually large birth interval may occur, since we cannot detect any children born and baptized elsewhere. Likewise, a miscarriage early into the pregnancy was not recorded in the parish registers, but can nevertheless create an extended birth interval.[11] The issue of migration will bias our results to the extent that migration patterns and spacing behavior are correlated. Miscarriages, on the other hand, touches the problem related to separating the actions taken to limit fertility from a biological reaction (temporary infertility) caused by malnutrition or poor health conditions. We will address these issues performing different robustness checks.

Note, finally, that in the duration models we will take into account the problem of right-censoring due to the death of a spouse or the wife reaching age 50, after which we assume conception is no longer possible due to sterility.

Outcome Variables

As a first step, we will investigate the effect of real wages on the hazards of five different demographic events: (*i*) marriage, (*ii*) starting, (*iii*) first birth, (*iv*) spacing, and (*v*) stopping. In the 'marriage', 'starting' and 'stopping' analyses, every wife (i.e. every couple) is included once, and the events scrutinized concern the points in time at which she married, conceived her first child, and conceived her last child, respectively. We assume that the wife becomes at 'risk' of entering these events from age 15. In the case of the 'first birth' variable, the event analysed is the conception of the first child, conditional on the wife being married. This analysis, therefore, includes only couples that conceived their first child while married (excluding this way prenuptially conceived births). Finally, in the analysis of the 'spacing' variable, the event analysed is the conception of a child, conditional on having given birth to a child of lower order. All five outcome variables are regressed on real wages (sources and methodology detailed

[11]On the contrary, the data suggest that stillborn children are present in the parish register as we have about 2700 observations for which the date of birth coincides with date of death. This is consistent with the parents' willingness to baptize the stillborn children to avoid them the purgatory.

below) as well as a set of family-background covariates, including the socio-economic rank, literacy status, and fecundity of the couple.

[Table 1: Summary Statistics]

The summary statistics are reported in Table 1. Average age at marriage of wives is 23.7 years and the average age at starting is 25. Thus, the time interval between marriage and the first birth is slightly over one year. The average length of a birth interval is 929 days (roughly 2.5 years) with a standard deviation of 475 days. Twin births (less than 2 percent of all births) are considered as single events, whereas the relatively few cases (n=986) in which the birth intervals are less than 40 gestational weeks (stemming either from preterm births, transcription or data errors or delayed baptisms) are removed from the sample.[12]

The most common occupations in the data are labourers, husbandmen and craftsmen. For roughly fifty per cent of the sample we do not have information about the parental occupation. Information about the literacy status of women is available only after 1750. About 33 per cent of the brides was able to sign with a proper signature.

[Figure 4: time series of spacing and real wages]

Living Standards

Our key explanatory variable is living standard, measured by the level of the real wage. Following the recent literature, the real wages used come from Clark (2007). The real wage series is constructed by dividing the nominal wage rate of unskilled (agricultural) labourers by the cost-of-living index.[13] It should be noted that the wage series combine wage observations from all across England, as documented by Clark (2007).

[12]Their inclusion has no impact on our qualitative conclusions.

[13]Gregory Clark kindly provided the annnaul data. A related real wages series constructed by Allen, which has rather less variation in the nominal wages than Clark's, provides results quantitatively similar to those obtained by using the Clark series. Allen's data is available at http://www.nuffield.ox.ac.uk/users/allen/data/labweb.xls.

We also use two alternative measures of living standards. First, since wheat was a main staple in historical England, we use yearly data on wheat prices, again provided by Clark (2007), to proxy the living standards. In addition, we use a national series of the crude death rates provided by Wrigley (1997) to account for famine and disease. The descriptive statistics of these series are presented in Table 2.

Figures 4 and Figure 5 show the relationship between average birth intervals and real wages. Figure 4 illustrates the evolution of the two time-series for the whole period 1540-1850, whereas Figure 5 shows the average birth intervals when we subdivide the standardized real wages in percentiles. In fact, the latter shows a cross-sectional gradient in birth intervals: higher levels of the real wage are associated with shorter spacing. We obtain a similar picture when looking at average birth intervals by occupational group (Figure 6): more affluent social groups (merchants and gentry) are associated with shorter birth intervals.

[Figure 4: Spacing and real wage, time-series]

[Figure 5: Spacing by real wages in percentage groups]

[Figure 6: Spacing by occupation]

3 Duration Analyses

In this section, we explore the effect of living standards on the five variables defined above: ‘marriage’, ‘starting’, ‘first birth’, ‘spacing,’ and ‘stopping’. We use the Cox Proportional Hazard (CPH) model (Cox 1972) and estimate the effects of time-varying covariates on the hazard function. The CPH model with time-varying covariates is specified as follows:

$$h(t) = h_0(t)\exp\{\beta_1 x_1 + \cdots + \beta_k x_k + g(t)(\gamma_1 z_1)\} \qquad (1).$$

The term $h_0(t)$ is the baseline hazard function; $(x_1, \ldots, x_k)$ are socio-economic and demographic covariates; and *z* the (time-varying) real wage. Estimates are stratified by parish and quarter centuries, with each stratum having its own baseline hazard $h_0(t)$.

Durations are measured at the individual level, whereas the real wages are measured annually at the national level. Therefore, we cluster the standard errors by the year of the respective demographic outcome, namely the marriage year and the conception year of the first ('starting'), successive ('spacing'), or last offspring ('stopping').

The last birth intervals in the sample (spanning the time from the penultimate to the final delivery) are significantly longer (on average) than previous ones (see Table 3). Although this could be attributed to fertility decreasing with age (Baird et al., 2005), demographers have argued that longer spacing to the last birth captures a failed attempt to end the wife's childbearing period (Van Bavel, 2004b; Okun, 1995; Knodel, 1987; Anderton, 1989). For this reason we include two versions of the 'spacing' model. In the first version, we include all birth intervals, while in the second version we exclude the last. Note also that in the stopping analysis, we are only able to consider *completed* marriages.[14]

The Effect of Real Wages on Fertility Outcomes

Table 4 reports the results of the duration models for the full period, 1540-1850, with the living standards measured by real wages. To ease the interpretations, the real wages are standardized with a mean of zero and a standard deviation of one The coefficients are reported as semi-elasticities, with a *positive* coefficient indicating a *higher* 'risk' that the event occurs (broadly speaking, a higher probability of marriage or conception), and vice versa.

Table 4 shows that the real wage has a significant, positive impact on the risk of 'marriage' and 'starting' (Columns 1 and 2). It follows that a one-standard deviation increase in the real wage increases the probabilities of marriage as well as first conception by roughly 52 per cent. The former effect—falling real wages delay the marriage—is first-hand evidence that Malthusian *preventive checks* operated at the family level in historical England. The latter effect—falling real wages delay the first conception—could potentially be a biological effect (lower real wages causing undernourishment and hence infertility). Yet, when fitting the model for 'first birth' we

[14] A family with completed fertility is defined as a marriage in which both the wife and the husband survived (at least) until the wife reached the age of 50 years. It therefore consists of a couple that exhausted their reproductive lifetime in the parish of origin.

find no significant effect of changes in the real wage, suggesting, *ceteris paribus*, that a couple's fecundability (as measured by the time from the marriage to the first birth) is *not* influenced by real wages (Column 3). Since the magnitude of the two effects on 'marriage' and 'starting' are almost identical, and because the real wage has no significant effect on fecundability, it suggests that the timing of the first conception ('starting') resides among the decision variables of the couple and is not biology-driven.

[Table 4: Starting, stopping and spacing]

In a world without access to modern contraceptives, and with marital births continuing throughout most (if not all) of the reproductive periods of women, Malthus emphasized the idea that couples would mainly attempt to act prudently *prior* to their marriage. Yet we know from the fertility decline of the nineteenth century that parental prudency *within* marriage is perfectly feasible by means of withdrawal, abstention, or extended breastfeeding (Ansley et al. 1987). Unlike the conclusion reached by the European Fertility Project, these methods may indeed have been practised even *before* the nineteenth century, and, hence, could well have contributed to England's low population-pressure, high-wage regime. Indeed, the coefficients of Columns (4) and (5) lend strong support to the idea of within-marriage preventive checks, with the real wage exercising a significant, negative impact on the spacing of consecutive births. Column (4) reports the effect of real wages on *any* birth interval (i.e. including the last birth interval) while Column (5) shows the effect on the birth spacing excluding the last interval. The latter effect implies that a one-standard deviation reduction in the real wage increases the risk of a birth by 18 per cent. To eliminate the bias of a failed attempt to stop childbearing, in the following analysis the 'spacing' variable excludes the last birth spacing interval.

To make sure that the effect on spacing is not a spurious finding, we can perform a placebo test shifting the real wage series forward by 3, 5, and 10 years, respectively. It follows that the effect of the real wages on the birth intervals is small and highly insignificant in all the cases (Table 5).

[Table 5: Placebo test]

Turning to the question of 'stopping' (Table 4, Column 6), there is no significant effect of the real wage on the risk of a last conception. However, the 'stopping' interval (from when the wife turns 15 to her last conception) comprises up to 35 years, so a lacking effect is maybe not surprising. [15]

Occupational Groups

Our covariates can help to shed light on the role of socio-economic rank for fertility patterns in the past. The reference group in the specifications of Table 4 is people whose occupation is 'labourer'. We find that the lower socio-economic ranks (labourers and husbandmen) on average had longer birth intervals but also that they stopped later than their more affluent counterparts, such as farmers, merchants, and gentry (Table 4, Columns 4 to 6). This result—that the hazard of a next birth increases with family affluence—has already been noted in Figure 6, which shows average spacing by occupational group.

In order to test whether the effect of the real wage on spacing differs across the various socio-economic groups, we sub-divide the sampled families into poor (labourers and husbandmen) and rich (the remaining categories). Table 6 reports the results when estimating the model for each group. As expected, the point estimates suggest that the hazard of a next birth is larger among the poor (Column 1) than among the rich (Column 2). Yet, the fact that both groups respond significantly and similarly to changes in living standards provides additional evidence that the effect cannot be only driven by a biological mechanism. Dribe and Scalone (2010) reach a similar conclusion in their investigation of German data, 1766-1863.

[Table 6: Spacing by economic status (rich and poor)]

[15] We have experimented with different starting points of the risk of 'stopping' (i.e. from when the wife turned 25, 30 and 35 etc) but these specifications also did not generate any significant effect.

The reason the rich had more offspring than the poor, as recently demonstrated by Clark and Hamilton (2006) and Boberg-Fazlic et al. (2011), can be partly due to their shorter birth intervals (Table 4, Columns 4 and 5). Early 'stopping' among the rich, i.e. presumably before the end of their reproductive period (inferred from the fact that the poor are able to continue), seems to suggest that families of higher socio-economic rank had a target number of offspring (Table 4, Column 6).[16]

Other Covariates

Among the remaining covariates, it is interesting to note that female literacy is related to shorter birth intervals and early stopping (Table 4), even after controlling for affluence. Also, the couple's fecundity—measured by the time-interval from the marriage to the first conception—significantly reduces the spacing of the couple's later birth intervals, i.e. low-fecundity couples face a lower hazard of subsequent births. Couples with prenuptially conceived children show also a lower hazard of subsequent births.[17]

Also in line with our expectations, child mortality during infancy (ages 0-1) or in early childhood (ages 1-3), substantially raises the hazard of a next birth, indicating an attempt to immediately replace a deceased children. We have also included the annual crude death rate (at the national level) to account for shocks like famines or wars which might have impacted the fertility of the households. We find that periods of high mortality significantly reduces the hazard of a next birth and hence extends the spacing of births. This is consistent with the idea that famines and diseases had a negative impact on women's fertility. Yet, this supports the assertion that the effect of real wages on spacing reflects a choice rather than a biological effect, the latter being captured by the crude death rate.

Finally, we can see that birth order has a significant, negative effect on the hazard of a next birth, meaning that birth intervals increase with the birth order of the child.[18] This

[16] See Van Bavel (2004b).

[17] The variable "Prenuptially conceived" is a binary variable which takes on value one if the difference between the marriage date and the date of the first born is less than 40 weeks, the average length of the gestation period.

[18] We have experimented to see if there is any effect of child gender on the birth intervals, but that was never the case.

is perfectly consistent with the fact that female fecundity declines with age (Baird et al., 2005).

Wheat Prices

The conclusions reached above regarding the effect of living standards on birth spacing remain intact when measuring living standards by wheat prices rather than real wages. Using the same econometric approach as above, we find that rising wheat prices significantly reduce the hazard of a next birth, hence increasing the birth spacing intervals (Table 7). The fact that the rich have shorter birth spacing intervals than the poor is repeated in the present specification, i.e. the higher the socio-economic rank, the higher is the hazard of a next birth.

Note that the interaction terms between the wheat price and the occupational categories reveal an interesting result: the category 'farmers' responds to higher wheat prices by *expanding* their birth intervals. This result suggests that farmers (unlike other groups) benefitted from higher wheat prices, and that they adjusted their spacing strategy accordingly—a strong sign of deliberate birth regulation within marriage. The remaining covariates (not displayed in the sake of space) confirm the findings in Table 4 above when using real wages.

[Table 9: Wheat prices and spacing]

Sub-Periods

Does the effect of living standards on birth intervals change over time? Using our preferred measure for living standard, the real wages, Table 8 shows the results when we divide the full period into 50-year sub-periods. With the exception of the last period 1800-1850, the effect of the real wages on spacing is always significant. The largest effects occur between 1600 and 1800. Among the few studies finding evidence of preventive checks using aggregate data, Kelly and O'Grada (2012) also conclude that the real wage coefficients are largest between 1600 and 1800. The reason for this is likely to be found in Figure 4 above: the periods up until 1600 and after 1800 are characterized by relatively high real wages compared to the middle period. These conclusions show precisely how the English resorted to the use of *preventive checks* when times were economically unfavourable.

Looking at the different socio-economic groups, it is interesting to note that up until 1650, only the middle and upper classes (traders, farmers, merchants, and gentry) were significantly different from the very poor (the labourers) in terms of spacing. But, as time passed, also the lower socio-economic groups (craftsmen and husbandmen) began to differ significantly, indicating that these groups became gradually more affluent relative to the very poor in the run up to 1850.

[Table 8: Duration with sub-periods]

4 Panel Analysis

We can estimate the effect of living standards on spacing using also a panel structure, which allows us to deal more directly with family heterogeneity. This comes at a cost, in that we are unable to include covariates which remain constant over time (such as occupational and educational information of the family).

We estimate a model with family-fixed effects defined as follows:

$$spacing_{ijt} = q_t + a_i + \beta_1 realwage_{j,t-\tau} + X_{ijt}g + \varepsilon_{ijt} \qquad (2).$$

The variable *spacing* is the birth interval (in days) for family *i* of a childbirth *j* in year *t*; *q* denotes a time-varying intercept; *a* include unobserved family fixed effects; *realwage* is the real wage in year t-τ for childbirth *j* (common to all families); and finally X is a vector of other covariates, including wife's age at each of her births, child birth order, and child mortality.[19]

Due to the time interval between conception and birth, we do not expect the real wage in year *t* to impact on the birth in year *t*. The descriptive statistics show that the

[19] Similar to the duration analysis, we exclude the last birth interval from the analysis. The inclusion of the last birth interval does not change qualitatively our results.

average birth interval is roughly 2.5 years (Table 1). So the effect of living standards is likely to happen in the two years preceding the year of the birth. Thus, if sibling *n* is born in year *t*, we will estimate the effect of the average real wages of time t+1 and t+2 on the spacing between siblings *n* and n+1. For reasons of tractability standard errors are clustered by the year of the firstborn, grouping this way all families that had their first delivery in the same year.[20]

Panel Results

Table 9 reports the estimates of equation (**Fehler! Verweisquelle konnte nicht gefunden werden.**) for the entire period (Column 1) and by sub-periods (Columns 2-7). Overall, the panel analysis provides the same results as the duration model: higher living standards reduce the birth spacing intervals. Note that the coefficients now express the change (in days) in the length of the birth interval. It thus follows that an increase of one standard-deviation in the real wage decreases the average birth interval by 64 days (Column 1). Again, we find that child mortality drastically reduces the subsequent birth interval; that higher birth order increases birth spacing; and finally that the crude birth rate has a positive effect on spacing, suggesting once more that famine and disease had a negative impact on the couple's fertility.

Looking at the sub-periods (Columns 2-7), the pattern of the duration analysis is largely repeated: the effects are only significant in the middling period (here between 1650 and 1800) and insignificant (but still with the expected sign) before and after.

[Table 9: Panel results]

The causal effect of real wages on spacing

The existence of an omitted time-varying variable which is correlated with both real wages and birth spacing may bias our estimates and, therefore, question the causality of the effect. To overcome this possible bias we adopt an instrumental variable approach. That is, we identify exogenous variation in real wages using variation in monthly air

[20] We cannot cluster the standard errors by birth year as the panels (i.e. the families) are not nested within the clusters.

temperature in the relevant years. The line of reasoning is that the air temperature (especially during certain seasons) affects the harvest outcome, which in turn influences food prices and, through the consumer price index, the real wage. The exclusion restriction is that the temperature affects the birth intervals only *indirectly*, i.e. through prices and wages.

For every year starting in 1659 we have monthly temperature readings for England, provided by the Hadley Centre Central England Temperature dataset (see Manley (1953, 1974) and Parker et al. (1992)). The data offer the longest available series of monthly temperatures based on instrumental observations, and is widely used in climatology. We use the average monthly temperature by season (spring, summer, autumn, and winter) for the relevant year to identify variation in real wages.[21] Since our real wages are averages of the two years preceding childbirth, we use average seasonal temperatures of the same two years.

The instrumental variable estimates are shown in Table 10 (column 2), with corresponding standard panel estimates for comparison (column 1). The first thing to note is the strong partial correlation of the seasonal average temperatures with the real wages (Column 2, upper panel). The first stage F-statistic is reassuringly high (bottom of Table 10). We find that an increase of the real wage by one-standard deviation causes a reduction of the birth spacing interval by about two months. The instrumental variable estimate is remarkably similar to the standard fixed effect estimate, suggesting an absence of omitted variable bias.[22]

Average temperatures are a plausible source of variation also for wheat prices. Hence, we can adopt the same instrumental variable approach when using wheat prices as an indicator of the standard of living. The results are presented in Table 11. Also in this case the first stage estimates (upper panel) show a strong correlation between average seasonal temperatures and wheat prices. The instrumental variable estimate (Column 2) is larger compared to the standard panel estimate (Column 1). In this case, an increase of the wheat price by one-standard deviation causes a delay of the next childbirth by roughly 30 days.

[21] Using monthly temperatures instead of averages by season does not change our results.
[22] Reverse causality should also not be an issue in our models.

[Tables 10 and 11]

5 Robustness Checks

In the previous section we have shown that the negative effect of living standards on birth spacing has a causal interpretation. Throughout the paper we have also provided evidence suggesting that the effect is the result of behaviour rather than biology (undernourishment causing amenorrhea and hence infertility).[23] We can stress this point further by excluding from the sample those years in which the living standards were exceptionally low, i.e. years in which the biological mechanism may have manifested itself, such as during the great famine of 1727-28.[24]

To this end we re-estimate equation (2) excluding the years in which (*i*) the real wages are below the 10th percentile; (*ii*) the wheat prices are above the 90th percentile; and (*iii*) the crude death rates are above the 90th percentile. To make sure we exclude the peaks of extreme low living standards, we focus, moreover, on the period 1600-1800, characterized by the absence of long-term trends (see Figure 4).[25] As can be seen in Table 12, the effects on birth spacing remain significant and negative, even after the exclusion of years of very low living standards.

[Table 12: Spacing behaviour - robustness checks]

Alternatively, we can compare the effect of real wages on spacing in 'good' and 'bad' years, for 'poor' and 'rich' families, respectively. The 'good' years are those in which real wages are above the long-run median (vice versa for 'bad' years). The 'poor' families are labourers and husbandmen, while 'rich' families include the rest, i.e. craftsmen, traders, farmers, merchant, and gentry. The results are reported in Table 13. Both rich and poor families adjusted their spacing behaviour during bad years (columns

[23] Amenorrhea is the absence of a menstrual period among woman in their reproductive age (ages 15 to 50) and has been demonstraed to occur from physical stress, eating disorders and weight loss.
[24] See Klemp and Weisdorf (2012).
[25] This is also the period during which we find the strongest preventive checks. Using the full time-period, however, does not change the direction of our results.

1 and 3). In those years, a decrease in the real wage by one-standard deviation increases the birth spacing interval by 86 days for the rich and 102 days for the poor. We cannot entirely rule out that this was a biological mechanism in the case of the poor. However, because the rich were unlikely to suffer from starvation, even during bad years, the delay strongly indicates a behavioural mechanism for this group. When turning to the good years, the coefficient for the rich group becomes insignificant (Column 2), while even during prosperous years, the poor still respond to falling real wages by significantly increasing their birth spacing (Column 4).

[Table 13: Spacing behaviour of rich and poor in good and bad periods]

6 Conclusion

Britain was the first nation ever to escape the Malthusian trap and enter into the current regime of modern economic growth. The relatively late age at marriage, as well as the high share of unmarried people, has long been attributed to the key reason for Britain's low population-pressure, high-wage economy, and its early transition to sustained economic growth (Voth and Voigtländer 2010).

It has also long been thought that within-marriage birth limitation behaviour was absent in pre-industrial England, and that it emerged at the end of the nineteenth century, when the fertility transition swept across Western Europe. Previous research investigating the short-term response of post-marital family planning to changing living standard has been unsuccessful in demonstrating that Malthusian *preventive checks* operated in pre-industrial England. Taking the question of *preventive checks* to the bedroom, we provide ample evidence that such checks existed in the three centuries leading up to England's fertility transition.

Specifically, we find that falling real wages had not only increased the age at marriage among women (as is generally assumed to have been the case) but also that it extended the time-interval within family births. The *preventive checks* are especially strong between 1600 and 1800, a period characterized by relatively low and stagnant real wages, and they seem to vanish when wages rise. In terms of magnitude, we find that an increase in the real wage by one-standard deviation decreased the birth spacing interval by roughly two months during the seventh- and eighteenth centuries.

Our results are robust to different estimation methods, including duration and panel models. By means of instrumenting changes in living standards by variation in monthly air temperatures, we also find that the effect has a causal interpretation, from living standards to the spacing of births. Although we cannot entirely rule out the possibility that a biological mechanism was at play, with undernourishment leading to infertility and hence extended birth spacing among the poor, the fact that falling real wages exercise a negative effect on the spacing of births also among the rich makes it likely that delayed births signifies economically rational behaviour. Alternative specifications and several robustness checks support this assertion.

The presence of *preventive checks* in pre-industrial England, both in the form of late age at marriage and of flexible birth intervals, helps understanding England's leading position as a low population-pressure, high-wage economy, and therefore its primacy in the transition from a Malthusian to a post-Malthusian regime.

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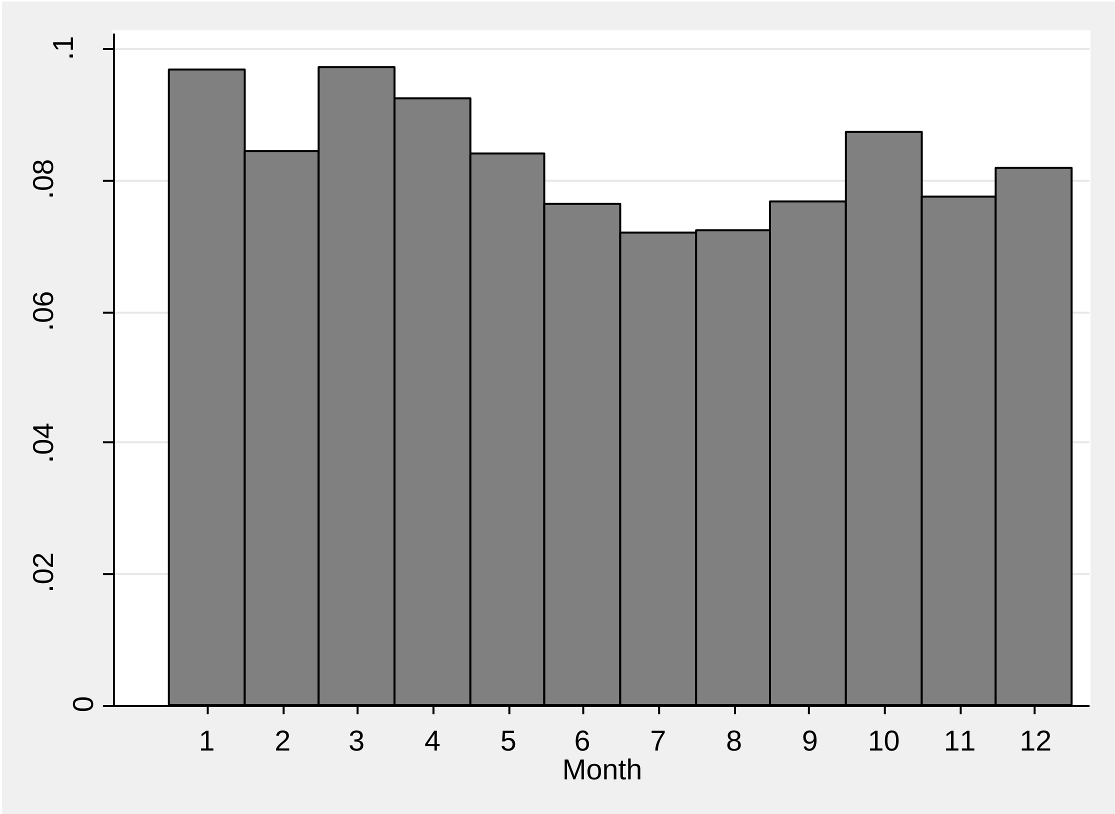
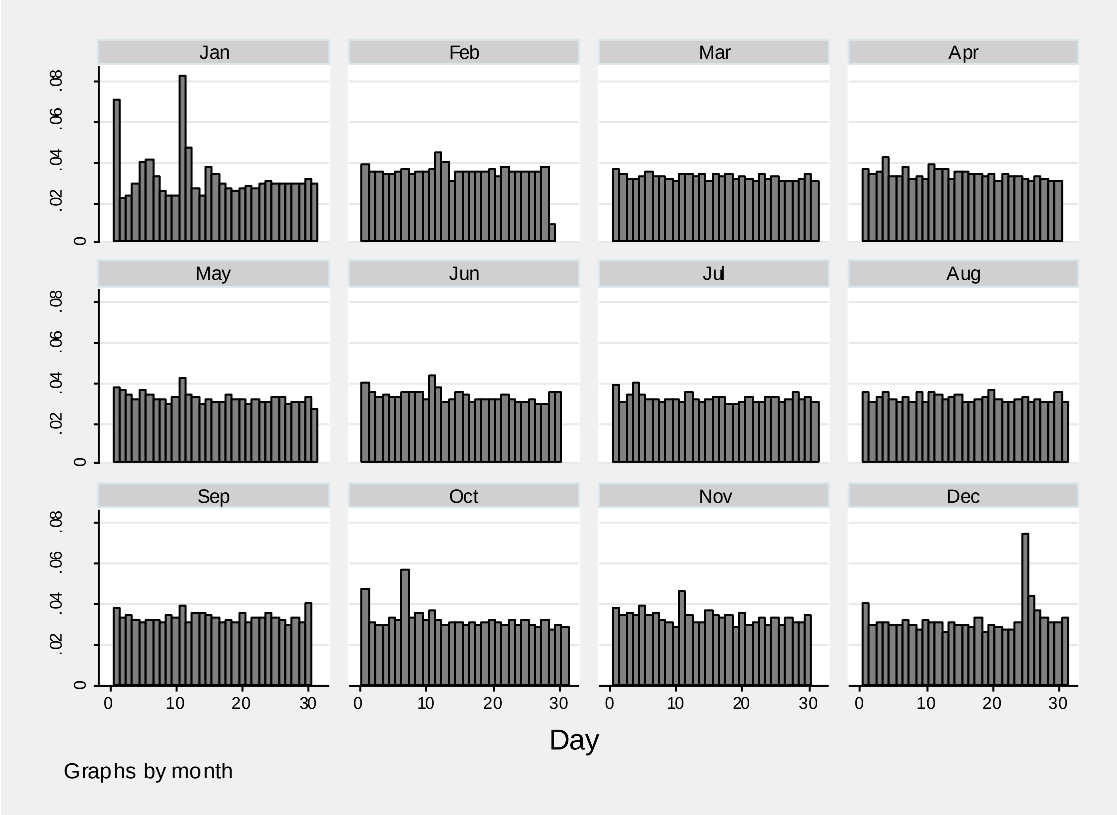
Figure 1: The 26 parishes of the reconstitution data

Figure 2: Distribution of births by month



Source: Cambridge reconstitution data

Figure 3: Distribution of births within the twelve months of the year

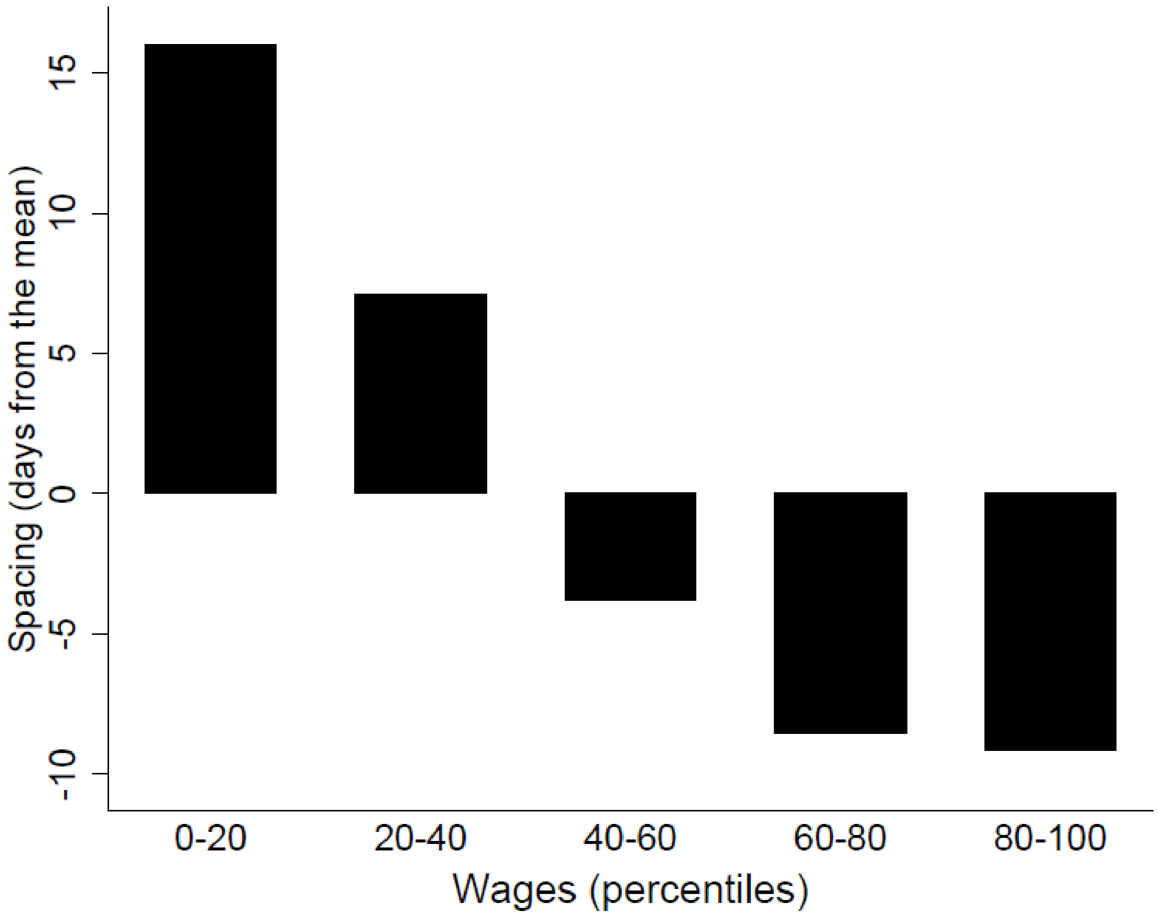


Source: Cambridge reconstitution data

Figure 4: Real wages and average spacing, 1540-1850

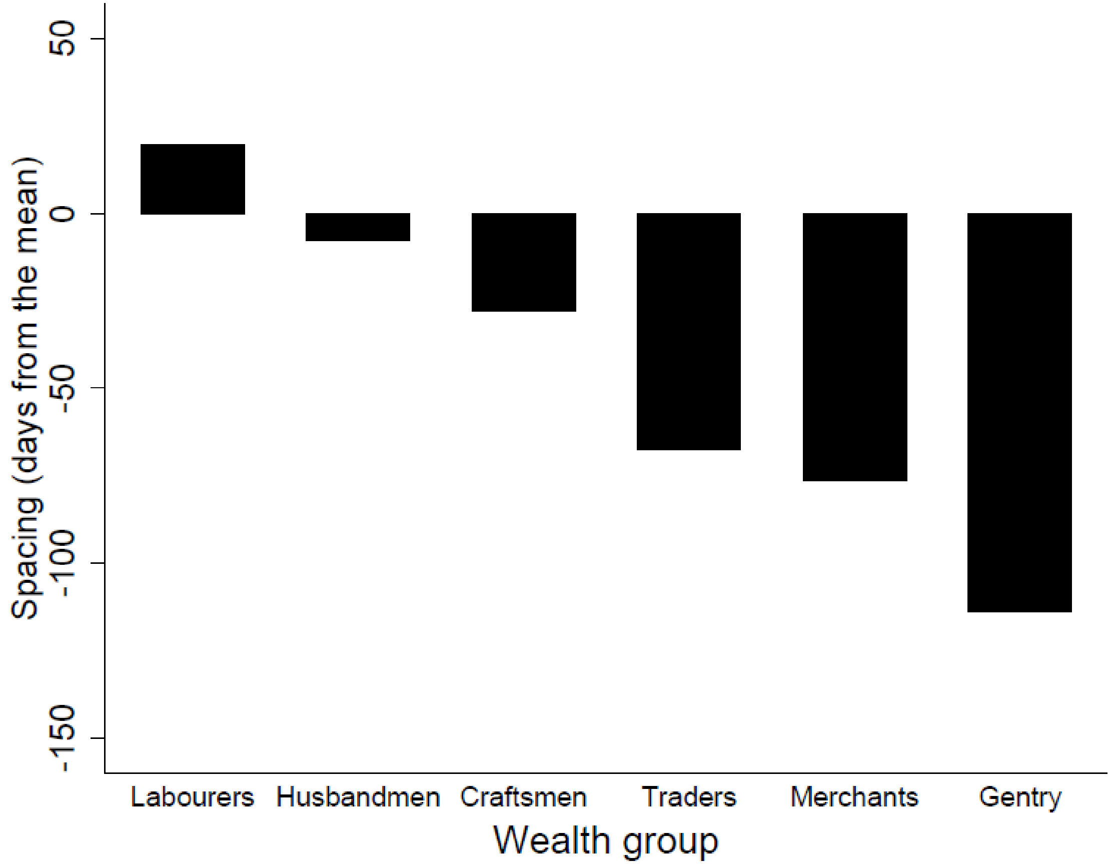
Source: Birth data are from the Cambridge reconstitution data. Real wages are from Clark (2007).

Figure 5: Average spacing by real wage percentiles



Source: Cambridge reconstitution data.

Figure 6: Average spacing by occupational group



Source: Cambridge reconstitution data.

Table 1: Summary Statistics

Variable	*Mean*	*SD*	*Min*	*Max*	*N*
Spacing (days)	929.238	475.058	260.000	4368.000	191892
Mother's age at marriage	23.669	4.275	15.001	46.667	62515
Mother’s age at starting	24.972	4.510	15.110	47.606	71556
Time to first birth (years)	1.194	1.131	-0.077	11.975	116220
Prenuptially conceived (share)	0.215	0.411	0	1	191892
Mother’s age at stopping	38.411	5.858	16.794	49.993	71556
Labourers	0.153	0.360	0.000	1.000	191892
Husbandmen	0.085	0.279	0.000	1.000	191892
Craftsmen	0.101	0.301	0.000	1.000	191892
Traders	0.047	0.212	0.000	1.000	191892
Farmers	0.030	0.171	0.000	1.000	191892
Merchant	0.057	0.232	0.000	1.000	191892
Gentry	0.015	0.122	0.000	1.000	191892
Occupation unknown	0.511	0.500	0.000	1.000	191892
Mother's age when born	30.014	5.875	15.110	48.997	71556
Mother literacy	0.334	0.472	0.000	1.000	36126
Mother literacy unknown	0.812	0.391	0.000	1.000	191892
Birth order	3.082	2.137	1.000	19.000	191892
Household size	6.175	2.703	2.000	21.000	191892
Child mortality (0-1 year)	0.138	0.345	0.000	1.000	191892
Child mortality (1-3 years)	0.057	0.231	0.000	1.000	191892
Child mortality unknown	0.593	0.491	0.000	1.000	191892

Source: Cambridge reconstitution data.

Table 2: Summary statistics of aggregate variables

Variable	*Mean*	*SD*	*Min*	*Max*
Real wage	0.199	0.064	0.078	0.418
Wheat price	2.892	2.625	0.222	14.837
Crude death rate	26.633	4.479	19.200	53.900
Mean temperature	9.214	0.659	6.840	10.82

Source: Real wages and wheat prices are from Clark (2007). Crude death rates are from Wrigley (1997). Mean temperatures from Manley (1953).

Table 3: Average birth intervals within family

Period	*First interval*	*Second last interval*	*Last interval*
1540-1699	830.4	936.0	1066.3
1700-1749	803.3	926.4	1076.6
1750-1799	798.2	922.9	1053.0
1800-1850	805.9	916.4	1005.3

Source: Cambridge reconstitution data.

Table 4: Duration models

Dependent variable: Spacing in days	*(1) Marriage*	*(2) Starting*	*(3) Time to first birth*	*(4) Spacing*	*(5) Spacing (w/o)*	*(6) Stopping*
Real wage	0.419*	0.423*	0.017	0.057*	0.166***	0.050
	(0.237)	(0.250)	(0.144)	(0.032)	(0.022)	(0.244)
Husbandmen	-0.031	-0.035	0.089***	0.060***	0.073***	0.222***
	(0.032)	(0.032)	(0.027)	(0.013)	(0.016)	(0.079)
Craftsmen	-0.076***	-0.076***	0.089***	0.071***	0.086***	0.111
	(0.027)	(0.028)	(0.025)	(0.014)	(0.016)	(0.069)
Traders	-0.039	-0.048	0.080**	0.151***	0.185***	0.180*
	(0.042)	(0.035)	(0.034)	(0.019)	(0.022)	(0.103)
Farmers	-0.042	-0.071*	-0.041	0.142***	0.222***	0.233**
	(0.045)	(0.039)	(0.044)	(0.020)	(0.023)	(0.101)
Merchant	-0.013	-0.037	0.074**	0.164***	0.205***	0.222**
	(0.039)	(0.038)	(0.035)	(0.021)	(0.022)	(0.094)
Gentry	0.130	0.082	-0.087	0.169***	0.303***	0.832***
	(0.086)	(0.070)	(0.063)	(0.034)	(0.037)	(0.225)
Occupation unknown	-0.105***	-0.124***	-0.080***	-0.101***	0.069***	0.298***
	(0.024)	(0.025)	(0.022)	(0.015)	(0.013)	(0.068)
Mother literacy	-0.004	-0.011	-0.004	0.026*	0.068***	0.212***
	(0.023)	(0.022)	(0.023)	(0.015)	(0.015)	(0.073)
Mother literacy unknown	-0.121***	-0.300***	-0.227***	-0.017	-0.008	0.109
	(0.037)	(0.026)	(0.045)	(0.030)	(0.021)	(0.083)
Time to first birth (years)				-0.098***	-0.052***	0.011
				(0.003)	(0.004)	(0.014)
Prenuptially conceived				-0.020**	-0.017*	0.020
				(0.008)	(0.009)	(0.042)
Child mortality (0-1)				0.460***	0.737***	-0.048
				(0.013)	(0.015)	(0.062)
Child mortality (1-2)				0.200***	0.162***	-0.152*
				(0.016)	(0.017)	(0.090)
Child mortality unknown				-0.011	0.029***	-0.084*
				(0.010)	(0.009)	(0.043)
Crude death rate		-0.023	-0.008	-0.007***	-0.007***	-0.014
		(0.022)	(0.012)	(0.002)	(0.002)	(0.020)
Birth order				-0.094***	-0.011***	
				(0.002)	(0.002)	
Mother's age at marriage	No	No	Yes	Yes	Yes	Yes
Observations	214939	262618	58619	351815	225312	93781
Subjects	20040	22621	28100	142009	85147	3795

Note: Cox proportional hazard model with time-varying real wages. Real wages are standardized. In Column 5 we do not consider the last closed birth interval. Coefficients (semi-elasticities) reported. Estimates are stratified by parish and quarter century. Standard errors are clustered by the year of the demographic outcome. *** p<0.01, ** p<0.05, * p<0.10.
Source: Own estimates.

Table 5: Placebo test on duration models

	Shift 3 years	*Shift 5 years*	*Shift 10 years*
Dependent variable: Spacing in days	*(1)*	*(2)*	*(3)*
Real wage	-0.008	-0.006	-0.011
	(0.025)	(0.023)	(0.023)
Controls	Yes	Yes	Yes
Observations	225312	225312	225312
Subjects	85147	85147	85147

Note: Cox proportional hazard model with time-varying real wages. Real wages are standardized. Coefficients (semi-elasticities) reported. Estimates are stratified by parish and quarter century. Standard errors are clustered by the year of the demographic outcome. *** p<0.01, ** p<0.05, * p<0.10.
Source: Own estimates.

Table 6: Spacing by economic status

Dependent variable: Spacing in days	*Poor* *(1)*	*Well-off* *(2)*
Real wage	0.231***	0.146***
	(0.036)	(0.036)
Mother literacy	0.060***	0.094***
	(0.023)	(0.023)
Mother literacy unknown	-0.054	0.045
	(0.034)	(0.029)
Time to first birth (years)	-0.045***	-0.065***
	(0.008)	(0.008)
Prenuptially conceived	-0.008	-0.018
	(0.018)	(0.019)
Child mortality (0-1)	0.764***	0.651***
	(0.028)	(0.029)
Child mortality (1-3)	0.156***	0.144***
	(0.031)	(0.033)
Child mortality unknown	0.044**	0.027
	(0.017)	(0.017)
Birth order	-0.011***	-0.016***
	(0.004)	(0.004)
Crude death rate	-0.005	-0.004
	(0.004)	(0.003)
Mother's age at marriage	Yes	Yes
Observations	62128	54945
Subjects	23346	21762

Note: Cox proportional hazard model with time-varying real wages. Real wages are standardized. Coefficients (semi-elasticities) reported. Estimates are stratified by parish and quarter century. Standard errors are clustered by the year of the demographic outcome. *** p<0.01, ** p<0.05, * p<0.10.
Source: Own estimates.

Table 7: Wheat prices and Spacing

Dependent variable: Spacing in days	*Main effect* (1)	*Interaction terms* (2)
Wheat price	-0.059***	-0.056***
	(0.009)	(0.010)
Husbandmen	0.073***	0.081***
	(0.016)	(0.025)
Craftsmen	0.086***	0.111***
	(0.016)	(0.023)
Traders	0.184***	0.177***
	(0.022)	(0.032)
Farmers	0.223***	0.159***
	(0.023)	(0.033)
Merchant	0.206***	0.243***
	(0.022)	(0.038)
Gentry	0.304***	0.336***
	(0.037)	(0.054)
Occupation unknown	0.069***	0.070***
	(0.013)	(0.020)
Husbandmen x wheat		-0.007
		(0.018)
Craftsmen x wheat		-0.021
		(0.017)
Traders x wheat		0.009
		(0.023)
Farmers x wheat		0.058***
		(0.018)
Merchant x wheat		-0.038
		(0.027)
Gentry x wheat		-0.042
		(0.050)
Occupation unknown x wheat		0.000
		(0.011)
Control variables	Yes	Yes
Observations	225312	225312
Subjects	85147	85147

Note: Cox proportional hazard model with time-varying wheat prices. Wheat prices are standardized. Coefficients (semi-elasticities) reported. Estimates are stratified by parish and quarter century. Standard errors are clustered by the year of the demographic outcome. *** p<0.01, ** p<0.05, * p<0.10.
Source: Own estimates.

Table 8: Spacing by sub-periods

Dependent variable: Spacing in days	*1540-1599*	*1600-1649*	*1650-99*	*1700-1749*	*1750-1799*	*1800-1850*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Real wage	0.089*	0.134**	0.164***	0.126***	0.205***	0.102
	(0.048)	(0.058)	(0.035)	(0.046)	(0.061)	(0.093)
Husbandmen	0.076	0.058	0.122***	0.071*	0.053*	0.098***
	(0.084)	(0.047)	(0.046)	(0.041)	(0.029)	(0.033)
Craftsmen	0.059	0.062	0.160***	0.110***	0.099***	0.051*
	(0.085)	(0.049)	(0.046)	(0.037)	(0.029)	(0.030)
Traders	0.106	0.146**	0.201***	0.217***	0.200***	0.220***
	(0.125)	(0.060)	(0.063)	(0.046)	(0.040)	(0.049)
Farmers	0.297***	0.161***	0.182**	0.075	0.236***	0.284***
	(0.092)	(0.062)	(0.092)	(0.069)	(0.038)	(0.043)
Merchant	0.200**	0.344***	0.424***	0.280***	0.144***	0.118**
	(0.097)	(0.062)	(0.071)	(0.054)	(0.034)	(0.058)
Gentry	0.509***	0.392***	0.351***	0.141	0.189***	0.360**
	(0.140)	(0.087)	(0.084)	(0.121)	(0.056)	(0.141)
Occupation unknown	0.088	0.084*	0.098**	0.040	0.041*	0.112***
	(0.080)	(0.044)	(0.041)	(0.036)	(0.022)	(0.024)
Mother literacy	0.075	-0.897*	0.889**	0.160	0.088***	0.044**
	(0.459)	(0.495)	(0.419)	(0.197)	(0.020)	(0.021)
Mother literacy unknown	-0.048	-0.297	0.020	0.028	-0.003	0.006
	(0.270)	(0.275)	(0.292)	(0.164)	(0.026)	(0.031)
Time to first birth (years)	-0.028**	-0.048***	-0.059***	-0.059***	-0.052***	-0.057***
	(0.012)	(0.009)	(0.011)	(0.011)	(0.007)	(0.008)
Prenuptially conceived	0.077**	-0.024	0.012	0.021	-0.054***	-0.041*
	(0.033)	(0.020)	(0.026)	(0.025)	(0.017)	(0.021)
Child mortality (0-1)	0.736***	0.887***	0.786***	0.686***	0.676***	0.698***
	(0.047)	(0.048)	(0.036)	(0.035)	(0.029)	(0.040)
Child mortality (1-3)	0.232***	0.152***	0.193***	0.088**	0.186***	0.174***
	(0.077)	(0.037)	(0.041)	(0.038)	(0.028)	(0.049)
Child mortality unknown	0.016	-0.013	0.046**	0.022	0.028*	0.065***
	(0.031)	(0.024)	(0.020)	(0.021)	(0.015)	(0.025)
Birth order	-0.014*	-0.009	-0.012*	-0.014***	-0.012***	-0.004
	(0.008)	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)
Crude death rate	-0.007*	-0.004	0.000	-0.013***	-0.001	0.010
	(0.004)	(0.003)	(0.003)	(0.004)	(0.005)	(0.010)

Note: Cox proportional hazard model with time-varying real wages. Real wages are standardized. Coefficients (semi-elasticities) reported. Estimates are stratified by parish and quarter century. Standard errors are clustered by the year of the demographic outcome. *** p<0.01, ** p<0.05, * p<0.10.

Source: Own estimates.

Table 9: The effect of real wages on spacing - panel estimates

Dep.var.: Spacing in days	*(1) 1540-1850*	*(2) 1540-1599*	*(3) 1600-1649*	*(4) 1650-1699*	*(5) 1700-1749*	*(6) 1750-1799*	*(7) 1800-1850*
Real wage (std)	-64.126***	-27.168	-35.573	-78.246***	-72.725***	-55.746**	-8.267
	(8.019)	(22.341)	(22.835)	(17.850)	(18.251)	(22.013)	(27.148)
Mother's age at birth (25-29)	43.472***	30.365	79.369***	25.309	70.914***	47.210***	25.162**
	(5.636)	(35.604)	(18.956)	(22.591)	(16.021)	(9.150)	(9.589)
Mother's age at birth (30-34)	47.636***	75.827*	110.799***	19.472	93.832***	43.452***	16.561
	(7.511)	(44.847)	(25.213)	(30.062)	(19.775)	(14.490)	(15.919)
Mother's age at birth (35-39)	8.324	27.643	105.947***	-50.586	20.947	9.479	-6.024
	(9.515)	(74.619)	(32.811)	(33.202)	(27.208)	(18.797)	(20.829)
Mother's age at birth (40-44)	-57.091***	-93.572	6.881	-154.033**	16.423	-42.897	-100.874***
	(16.482)	(194.640)	(50.149)	(69.445)	(44.414)	(27.271)	(32.967)
Mother's age at birth (45-)	-187.490***			-197.532	-95.466	-148.778**	-261.388**
	(49.170)			(229.311)	(98.412)	(71.255)	(107.355)
Child mortality (0-1 year)	-213.512***	-242.346***	-257.507***	-210.156***	-197.103***	-189.908***	-200.605***
	(4.157)	(11.930)	(9.119)	(9.419)	(8.557)	(7.867)	(10.509)
Child mortality (1-3 years)	-35.326***	-47.744***	-32.298***	-32.177***	-24.582**	-43.637***	-32.837**
	(4.446)	(17.465)	(10.850)	(11.619)	(9.370)	(7.152)	(13.864)
Child mortality unknown	-9.506***	-14.577*	-6.959	1.263	-1.936	-6.619	-26.846***
	(2.782)	(8.550)	(7.123)	(6.534)	(5.952)	(5.283)	(8.490)
Birth order	230.793***	273.763***	238.086***	238.977***	214.826***	229.958***	251.988***
	(3.606)	(12.276)	(10.931)	(10.032)	(8.236)	(7.086)	(10.065)
Crude death rate	2.781***	2.464	1.567	3.040*	4.068***	4.321*	-6.574*
	(0.663)	(1.738)	(1.336)	(1.550)	(1.201)	(2.434)	(3.687)
Time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	137032	12290	20047	21859	25364	33368	24069
Number of groups	41866	4406	7045	7703	8591	10410	7392

Note: Family fixed effects. Robust standard errors in parenthesis. Standard errors are clustered by the year of the first childbirth. *** p<0.01, ** p<0.05, * p<0.10.
Source: Own estimates.

Table 10: The causal effect of real wages on spacing - Instrumental variable estimates

	Panel *(1)*	*Panel IV* *(2)*
Dependent variable: Real wages (standardized)		First stage
Average spring temperature		0.043***
		(0.008)
Average summer temperature		0.043***
		(0.012)
Average fall temperature		-0.013
		(0.010)
Average winter temperature		0.059***
		(0.006)
Dependent variable: Spacing in days		Second stage
Real wage (std)	-59.802***	-63.609***
	(9.183)	(23.326)
Mother's age at birth (25-29)	41.031***	41.053***
	(6.004)	(6.007)
Mother's age at birth (30-34)	41.524***	41.568***
	(8.191)	(8.211)
Mother's age at birth (35-39)	0.401	0.461
	(10.715)	(10.755)
Mother's age at birth (40-44)	-62.042***	-61.941***
	(17.832)	(17.895)
Mother's age at birth (45-)	-186.628***	-186.453***
	(49.659)	(49.629)
Child mortality (0-1 year)	-196.578***	-196.564***
	(4.530)	(4.519)
Child mortality (1-3 years)	-33.438***	-33.431***
	(5.220)	(5.209)
Child mortality unknown	-7.393**	-7.389**
	(3.250)	(3.244)
Birth order	225.244***	225.231***
	(3.859)	(3.857)
Crude death rate	2.928***	2.877***
	(0.863)	(0.897)
Time trend	Yes	Yes
Observations	102026	102026
Number of groups	30626	30626
1st stage F-stat		54

Note: Family fixed effects. Robust standard errors in parenthesis. Standard errors are clustered by the year of the first childbirth. Real wages are instrumented with average seasonal air temperatures. *** p<0.01, ** p<0.05, * p<0.10.
Source: Own estimates.

Table 11: The causal effect of wheat prices on spacing - instrumental variable estimates

	Panel	*Panel IV*
	(1)	*(2)*
Dependent variable: Wheat prices (standardized)		First stage
Average spring temperature		-0.076***
		(0.021)
Average summer temperature		-0.155***
		(0.026)
Average fall temperature		0.077***
		(0.021)
Average winter temperature		-0.091***
		(0.013)
Dependent variable: Spacing in days		Second stage
Wheat price (std)	19.490***	30.500**
	(3.739)	(11.960)
Mother's age at birth (25-29)	40.102***	39.777***
	(6.024)	(5.941)
Mother's age at birth (30-34)	39.719***	39.086***
	(8.263)	(8.133)
Mother's age at birth (35-39)	-2.299	-3.285
	(10.873)	(10.685)
Mother's age at birth (40-44)	-65.303***	-66.245***
	(17.960)	(17.668)
Mother's age at birth (45-)	-189.997***	-190.345***
	(49.160)	(48.594)
Child mortality (0-1 year)	-196.655***	-196.568***
	(4.518)	(4.504)
Child mortality (1-2 years)	-33.515***	-33.501***
	(5.210)	(5.196)
Child mortality unknown	-7.385**	-7.341**
	(3.248)	(3.241)
Birth order	225.288***	225.199***
	(3.854)	(3.846)
Crude death rate	3.391***	3.200***

	(0.862)	(0.862)
Time trend	Yes	Yes
Observations	102026	102026
Number of groups	22831	22831
1st stage F-stat		50

Note: Family fixed effects. Robust standard errors in parenthesis. Standard errors are clustered by the year of the first childbirth. Wheat prices are instrumented with average seasonal air temperatures. *** p<0.01, ** p<0.05, * p<0.10.
Source: Own estimates.

Table 12: Spacing behavior excluding selected years

Dependent variable: Spacing in days	*Excluding years of low wages*	*Excluding years of high wheat prices*	*Excluding years of high mortality rates*
	(1)	*(2)*	*(3)*
Real wage (std)	-57.086***	-74.491***	-69.235***
	(12.198)	(9.678)	(9.422)
Mother's age at birth	Yes	Yes	Yes
Child mortality	Yes	Yes	Yes
Birth order	Yes	Yes	Yes
Time trend	Yes	Yes	Yes
Crude death rate	Yes	Yes	Yes
Observations	88456	88251	98017
Number of groups	30101	30182	31076

Note: Family fixed effects. Robust standard errors in parenthesis. Standard errors are clustered by the year of the first childbirth. *** p<0.01, ** p<0.05, * p<0.10.
Source: Own estimates.

Table 13: Spacing behavior of rich and poor in good and bad periods

Dependent variable:	*Rich*		*Poor*	
Spacing in days	*Bad years*	*Good years*	*Bad years*	*Good years*
	(1)	*(2)*	*(3)*	*(4)*
Real wage (std)	-85.723*	39.136	-101.932***	-102.387*
	(47.239)	(80.399)	(33.596)	(57.105)
Mother's age at birth	Yes	Yes	Yes	Yes
Child mortality	Yes	Yes	Yes	Yes
Birth order	Yes	Yes	Yes	Yes
Time trend	Yes	Yes	Yes	Yes
Crude death rate	Yes	Yes	Yes	Yes
Observations	5202	3433	12102	8293
Number of groups	1983	1597	5106	3996

Note: Family fixed effects. Robust standard errors in parenthesis. Standard errors are clustered by the year of the first childbirth. Real wages are instrumented with average seasonal air temperatures. *** p<0.01, ** p<0.05, * p<0.10. Rich are merchants and gentry; poor are laborers, husbandmen, craftsmen, traders, and farmers. Good (bad) years are those in which the real wage is above (below) the long-run median.
Source: Own estimates.