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Abstract

We question the received wisdom that birth limitation was absent among historical populations before the fertility transition of the late nineteenth-century. Using duration and panel models on family-level data, we find a causal, negative short-run effect of living standards on birth spacing in the three centuries preceding England's fertility transition. While the effect could be driven by biology in the case of the poor, a significant effect among the rich suggests that spacing worked as a control mechanism in pre-modern England. Our findings support the Malthusian preventive check hypothesis and rationalize England's historical leadership as a low population-pressure, high-wage economy.

JEL-Code: J110, J130, N330.

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 (i.e. fertility decline) strongly influenced the growth pattern of modern economies (Galor, 2011, 2005). Unified growth theory (Galor and Weil, 2000; Galor and Moav, 2002; Hansen and Prescott, 2002; Jones, 2001) provides a framework that 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 (Voigtländer and Voth, 2009; Voigtländer and Voth, 2011). The argument 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, 1989).

Scholars have long believed that late age at first marriage of women was the main preventive check mechanism operating in England prior to its late nineteenth-century demographic transition (Voigtländer and Voth, 2011). Indeed, early work by Wilson concludes that marital birth limitation was absent in pre-industrial England, classifying it as a *natural fertility* society (Wilson, 1984). Later, more statistically 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 that marital birth limitation was invented during the European fertility decline of the late nineteenth century and implemented by the diffusion of knowledge about contraceptives, such as *coitus interruptus*, sexual abstention, and extended breastfeeding (Coale, 1986).

However, recent studies, 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 and Kok, 2004; Van Bavel, 2004). But despite England's key role in the long-term economic development of the west, with the exception of Wrigley's study of the parish of Colyton (Wrigley, 1966) and Wilson's subsequent analysis of 13 English parishes (Wilson, 1984), no attempts have been made to analyze fertility restriction at the household level using data from English parish records.

It is equally surprising that 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 that Malthusian preventive checks were operating in England

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

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 tends to average out the fertility response of different socio-economic groups, making it difficult to study the impact of living standards for the marital and reproductive behavior of those most prone to economic distress, i.e. 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 as 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 birth 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 about 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 was a prolongment 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 to England's fertility decline in the late nineteenth century. Equipped with a variety of econometric 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 occupations of the husbands 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, 1798), a hypothesis which we are the first to test directly. However, delaying marriage was not the only precautionary action a couple could take to re-

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

duce births. Historical families were relatively large (averaging 6-7 children) and the decision regarding 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").

Virtually all our econometric 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 (laborers, servants, and husbandmen) 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 childbirths resulted from actions taken by the couples and, thus, was *not only* due to a biological effect (i.e. infertility caused by famine or malnutrition). This is substantiated by two main findings: (*i*) a negative relationship between living standards and birth intervals exists across the entire socio-economic spectrum and not only among the poor; and (*ii*) the negative effect remains large and significant even when we exclude the years of severe economic depression (causing failed harvests and food shortages).

Consistent with the findings of 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. In addition, our investigation into the behavior of different socio-economic groups reveals that, as expected, farmers responded differently to other occupational groups, showing *reduced* birth intervals in response to higher wheat prices. This finding further rationalizes the notion of behavioral effects (in addition to biological explanations).

Our analysis confirm the Malthusian hypothesis that lower living standards led to delayed marriages and later 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 presumption that, in the past, English marriage 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 earlier than the poor is further evidence of the existence of birth limitation in the centuries prior to the demographic transition of the late 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 the air temperature, have an impact upon crop yields and thus wheat prices and real wages. The identifying assumption is that the monthly air temperatures have only an effect on the birth intervals through the real wages and wheat prices. The instrumental variable estimates confirm the negative effect of living standards on the spacing of births.

The remainder of the paper is structured as follows: In Section 2 we describe the key features of the data and the potential problems related to their use. In Section 3 we analyze fertility patterns 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 main pieces of data: real wages, wheat prices and demographic statistics. Beginning 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 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 rural villages, including proto-industrial, urban and agricultural communities. The data is documented in detail by Wrigley et al. (1997).

In a descriptive analysis of the parish of Colyton (one of the 26 reconstituted parishes) Wrigley found evidence pointing towards deliberate birth limitation occurring around 1700. This was attained, he argued, through late marriages, extended birth intervals, and low stopping ages (Wrigley, 1966). However, after adding a further 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 more advanced econometric techniques we are able to deal more substantially with geographical and family heterogeneity present in the data.

Family reconstitution data offer more information (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 built around a marriage, providing information about the birth (baptism) dates and death (burial) dates of the spouses, as well as the gender, birth, and death dates of their offspring.

Typically, the church recorded baptism dates rather than birth dates. We generate a birth date variable using the actual birth dates where 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 1 and Figure 2 illustrate the distribution of births by month and day of the month, respectively. The distribution by month does not show any significant heaping. However, Figure 2 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. It should be noted that since England switched from the Julian to the Gregorian calendar during our period of study, we

[3] Reviews and criticisms 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.

have converted all dates into the Gregorian calendar.[6] The spike on the 11th of January is thus due to the same reason for the spike on the 1st January. Hence, in the analysis below we use controls for the following dates: 25th December, 1st January, and 11th January.

[Figure 1: distribution of births by month]

[Figure 2: distribution of births by day of the month]

The data also provide ample information about the socio-economic background of the family, as well as the education and fecundability of the couple. For example, the clergy frequently reported the occupation of the spouses (albeit far more frequently for men than for women). The occupations were recorded at the time of marriage and burial, as well as at the baptisms or burials of the offspring. Using will 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: laborers, husbandmen, craftsmen, traders, farmers, merchants, and the gentry.[7] We use the earliest known occupation of the husbands to classify our sampled families (and a binary variable if the occupation is missing). Educational information comes from the spouses' signature on their wedding certificates (as opposed to leaving a cursory mark) which reveals their literacy status. This is a widely used indicator of human capital for the time 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 ecclesiastical event occurring outside of the parish of origin is not recorded in the parish register and, therefore, does not appear 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,

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

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

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, when we can observe them through to the end of the wife's reproductive period) we obtain qualitatively the same results.[10]

A related issue is that some couples may only have temporarily migrated. Thus, 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. Similarly, 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, touch upon the problems involving separating the actions taken to limit fertility from biological reactions (such as temporary infertility) caused by malnutrition or poor health conditions. We will address these issues by performing various 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 the age of 50, after which we assume sterility has set in and conception is no longer possible.

2.1 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" analysis, every wife (i.e. every couple) is included once, and the events examined involve 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 encountering these events from the age of 15. In the case of the "first birth" variable, the event analyzed 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 (thus excluding prenuptially conceived births). Finally, in the analysis of the "spacing" variable, the event analyzed is the conception of a child, conditional on having given birth to a child of lower order. Each of the five outcome variables are regressed on real wages (the sources and methodology are described below), as well as a set

[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.

[11] However, 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' desire to baptize the stillborn children to save them from purgatory.

of family-background covariates including the couple's socio-economic rank, literacy status, and fecundity.

[Table 1: Summary Statistics]

The summary statistics are reported in Table 1. The 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 laborers, husbandmen, and craftsmen. For roughly fifty percent of the sample we have no information about the parental occupation. Information about the literacy status of women is available only after 1750. About 33 per cent of the brides were able to sign their names.

2.2 Living Standards

Our key explanatory variable is living standards, 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 rural laborers by the cost-of-living index.[13] It should be noted that the wage series combine wage observations from throughout England, as documented by Clark (2007).

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 et al. (1997), to account for famine and disease. The descriptive statistics of these series are presented in Table 2.

[Tables 2 and 3]

Figures 3 and 4 illustrate the relationship between average birth intervals and real wages. Figure 3 reveals the evolution of the two time-series for the entire period

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

[13] Gregory Clark kindly provided the annual data. A related real wages series constructed by Allen, which has less variation in the nominal wages than Clark's, provides results that are 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.

of 1540–1850, whereas Figure 4 shows the average birth intervals when we subdivide the standardized real wages in percentiles. In fact, the latter figure 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 5): more affluent social groups (traders, merchants and gentry) are associated with shorter birth intervals.

[Figure 3: Spacing and real wage, time-series]

[Figure 4: Spacing by real wages in percentage groups]

[Figure 5: Spacing by occupation]

3 Duration Analysis

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:

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

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 the previous intervals (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, 2004; 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] This way we will know that the last birth recorded was indeed the final delivery of the couple, and not the last birth record before the couple moved to an (unobserved) parish where they continued to have children.

3.1 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). A one-standard deviation increase in the real wage increases the probabilities of marriage, as well as first conception, by roughly 52 percent. 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 attributed to a biological effect (i.e. lower real wages resulting in undernourishment and hence infertility). Yet, when fitting the model for "first birth" we find no significant effect of changes in the real wage, suggesting that, *ceteris paribus*, 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 appears that the timing of the first conception ("starting") lies within the decision variables of the couple and is not biology-driven.

[Table 4: Marriage, starting, first birth, stopping, and spacing]

During a time without access to modern contraceptives, and with marital births continuing throughout most (if not all) of a woman's reproductive period, Malthus emphasized that couples would largely seek to act prudently *prior* to marriage. Yet we know from the fertility decline of the nineteenth century that parental prudency *within* marriage was also perfectly feasible by means of withdrawal, abstention, or extended breastfeeding (Coale and Watkins, 1986). Contrary to the conclusion

[14] See footnote 10.

reached by the European Fertility Project, these methods may indeed have been practised even *before* the nineteenth century, and, hence, may well have contributed to England's low population-pressure, high-wage regime. In fact, 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 (including the last birth interval) while Column (5) shows the effect on the birth spacing excluding the last interval (see above). The latter (most relevant) effect implies that a one-standard deviation reduction in the real wage increases the risk of a birth by 18 percent. To eliminate the bias of a failed attempt to stop childbearing, in the following analysis the "spacing" variable excludes the last birth interval.

To ensure 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 final conception) can comprise some 35 years, so a lacking effect is, perhaps, unsurprising.[15]

3.2 Occupational Groups

Our covariates can help shed light on the bearing of socio-economic rank for fertility patterns in the past. The reference group in the specifications of Table 4 are those whose occupation is "laborer". We find that the lower socio-economic ranks (laborers and husbandmen) had on average 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 further birth increases with family affluence — has already been noted in Figure 5, which demonstrates average spacing by occupational group.

In order to establish whether the effect of the real wage on spacing differs across the various socio-economic groups, we sub-divide the sampled families into 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.

(laborers and husbandmen) and rich (craftsmen, traders, farmers, merchants, and gentry). Table 6 reports the results when estimating the model for each group. As expected, the point estimates suggest that the risk of a further birth is higher among the poor (Column 1) than among the rich (Column 2) when the living standard (real wage) increases. Nevertheless, 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) reached a similar conclusion in their investigation of German data from 1766–1863.

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

The fact that 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 ascribed 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 - as 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]

3.3 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. A couple's fecundity — measured by the time-interval from the marriage to the first conception — also 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 also demonstrate a lower propensity for 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 child. We have also included the annual crude death rate (at the national level) to account for situations such as famines or war, which might have impacted upon the fertility of the households. We find that periods of high mortality significantly reduce the hazard of a next birth and hence extend the spacing of births. This is consistent with the idea that famines and diseases had a negative impact on women's fertility. However, it supports the

[16]See Van Bavel (2004).

[17]The variable "Prenuptially conceived" is a binary variable which takes on a value of 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.

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

3.4 Wheat Prices

The conclusions made above regarding the effect of living standards on birth spacing remain valid 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 the hazard of a next birth.

Note that the interaction terms between the wheat price and the occupational categories reveal an interesting result: the "farmers" category responds to higher wheat prices by *expanding* their birth intervals. This suggests that farmers (unlike the other groups) benefitted from higher wheat prices, and that they adjusted their spacing strategy accordingly — a clear 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 7: Wheat prices and spacing]

3.5 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 the largest between 1600 and 1800. The reason for this is likely to be found in Figure 3 (above): the periods between 1600 and 1800 are characterized by relatively low real wages when compared to the periods before

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

and after. These conclusions show clearly how the English resorted to the use of preventive checks mainly during times of economic hardship.

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) differed significantly from the very poor (the laborers) in terms of spacing. But, as time passed, the lower socio-economic groups (craftsmen and husbandmen) also 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 also estimate the effect of living standards on spacing using 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 that remain constant over time (such as the occupational and educational information of the family).

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

$$\text{spacing}_{ijt} = q_t + a_i + \beta_1 \text{realwage}_{j,t-\tau} + X_{ijt} g + \varepsilon_{ijt}. \qquad (2)$$

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 includes unobserved family fixed effects; realwage is the real wage in year $t - \tau$ for childbirth j (common to all families); and finally X is a vector of other covariates, including the 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 average birth interval is roughly 2.5 years (Table 1). So the effect of living standards is likely to occur 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, thus grouping all families that had their first delivery in the same year.[20]

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

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

4.1 Panel Results

Table 9 reports the estimates of equation 2 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 a 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]

4.2 The causal effect of real wages on spacing

The existence of an omitted time-varying variable correlated with both real wages and birth spacing may bias our estimates and, therefore, question the causality of the effect. To overcome this potential bias we adopt an instrumental variable approach. That is, we identify exogenous variations in real wages using variation in monthly air 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 after 1659 we have monthly temperature readings for England, provided by the Hadley Centre Central England Temperature dataset (Manley, 1953, 1974) and Parker et al. (1992). The dataset offers 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.

[21] Using monthly temperatures instead of averages by season does not change our results.

The instrumental variable estimates are shown in Table 10 (column 2), with corresponding standard panel estimates for comparison (column 1). The first aspect 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 also a plausible source of variation 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. In this case the first stage estimates (upper panel) also show a strong correlation between average seasonal temperatures and wheat prices. The instrumental variable estimate (Column 2) is larger when 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. The quantitative conclusions from the analysis above thus remain intact.

[Tables 10 and 11: Panel IV]

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 behavior rather than biology (i.e. 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 10$^{\text{th}}$ percentile; (*ii*) the wheat prices are above the 90$^{\text{th}}$ percentile; and (*iii*) the crude death rates are above the 90$^{\text{th}}$ percentile. Moreover, to ensure that we exclude the peaks of extremely low living standards, we focus on the period 1600–1800, characterized by the absence of long-term trends (see Figure

[22] Reverse causality should also not be an issue in our models.

[23] Amenorrhea is the temporary absence of menstruation among otherwise fertile women of average reproductive age (15 to 50) and has been demonstrated to result from physical stress, malnutrition, eating disorders and extreme weight losses.

[24] See Klemp and Weisdorf (2012).

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 behavior - robustness checks]

Alternatively, we can compare the effect of real wages on spacing in "good" and "bad" years, for "poor" and "very 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 laborers and husbandmen, while the "very rich" families include only merchants and gentry.[26] The results are reported in Table 13, looking again at the period 1600 to 1800. Both very rich and poor families adjusted their spacing behavior during bad years (columns 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 very 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 very rich were unlikely to suffer from starvation, even during bad years, the delay strongly indicates a behavioral mechanism for this group. When turning to the good years, the coefficient for the very 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 behavior of very rich and poor in good and bad periods]

6 Conclusion

Britain was the first nation 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 as the main reason for Britain's low population-pressure, high-wage economy, and its early transition to sustained economic growth (Voigtländer and Voth, 2011).

It has also long been thought that within-marriage birth limitation behavior was absent in pre-industrial England, and that it only emerged at the end of the nineteenth century, when the fertility transition swept across Western Europe. Previous research investigating the short-term response of aggregate demographic variables (i.e. crude marriage and birth rates) to changing living standard has been largely unsuccessful in demonstrating that this kind of Malthusian preventive check operated

[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.

[26]Including also craftsmen, farmers, and traders among the rich (see Table 6) provides virtually the same results.

in pre-industrial England. Moving the issue 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 not only increased the age at first marriage among women (as is generally assumed to have been the case) but also that this extended the time-interval between family births. The preventive checks are especially strong between 1600 and 1800, a period characterized by relatively low and stagnant real wages, but 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 seventeenth and eighteenth centuries.

Our results are robust to different estimation methods, including duration and panel models. Instrumenting changes in living standards by variation in monthly air temperatures, we also find that the effect has a causal interpretation. 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 exercised a negative effect on the spacing of births among the rich makes it likely that delayed births signifies economically rational behavior. 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 extended birth intervals, helps explain England's leading position as a low population-pressure, high-wage economy, and hence its primacy in the transition from a Malthusian to a post-Malthusian regime.

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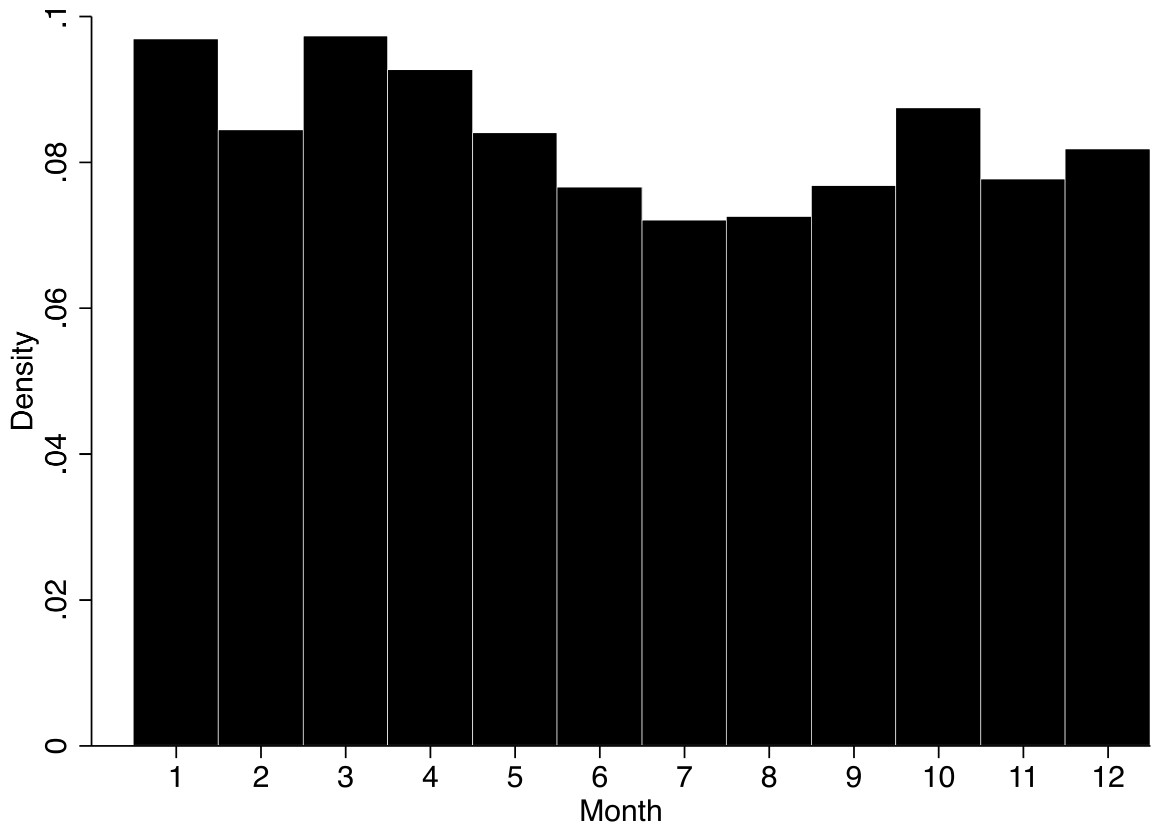

Figure 1: Distribution of births by months.

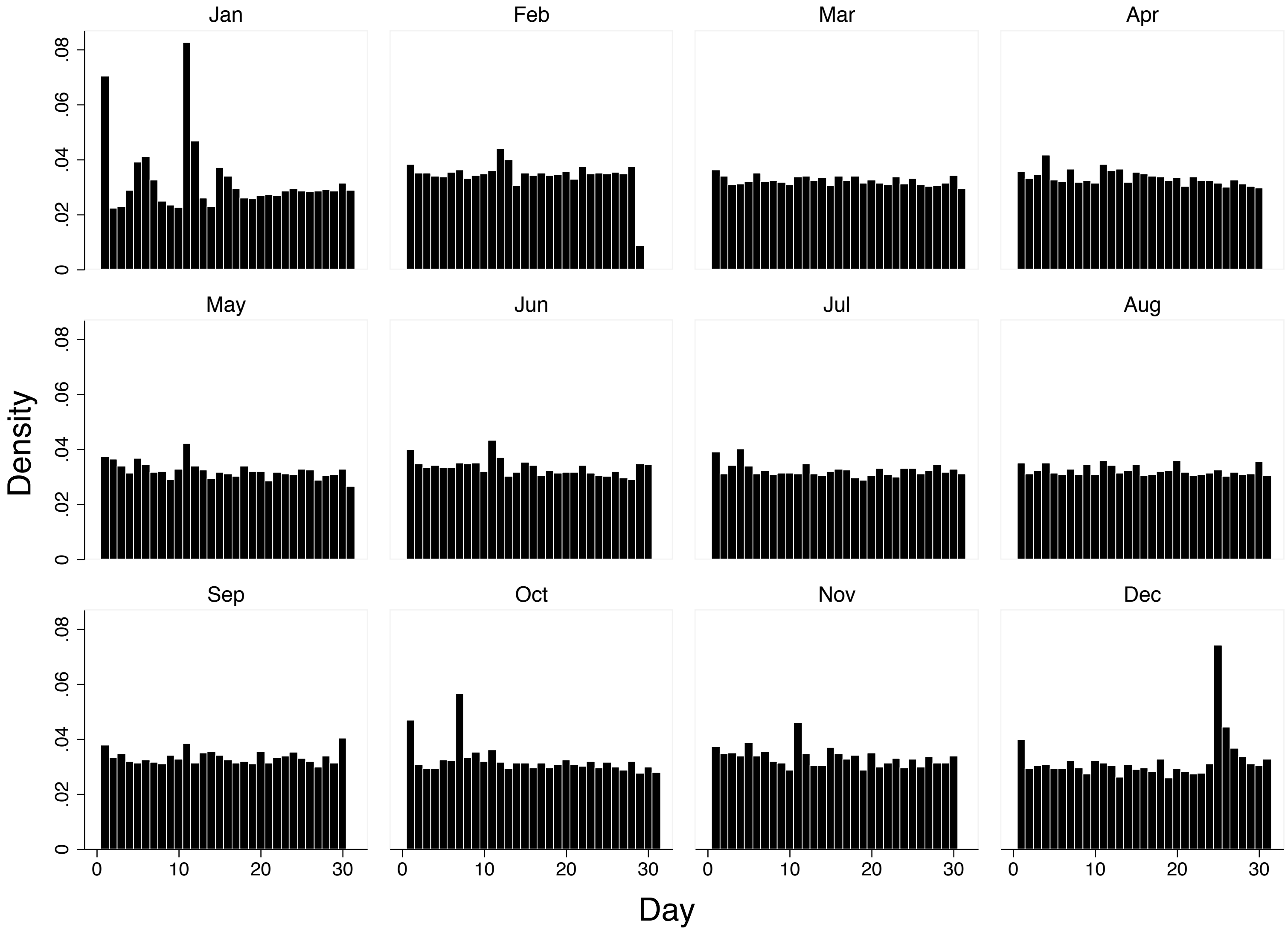

Figure 2: Distribution of births within the twelve months of the year.

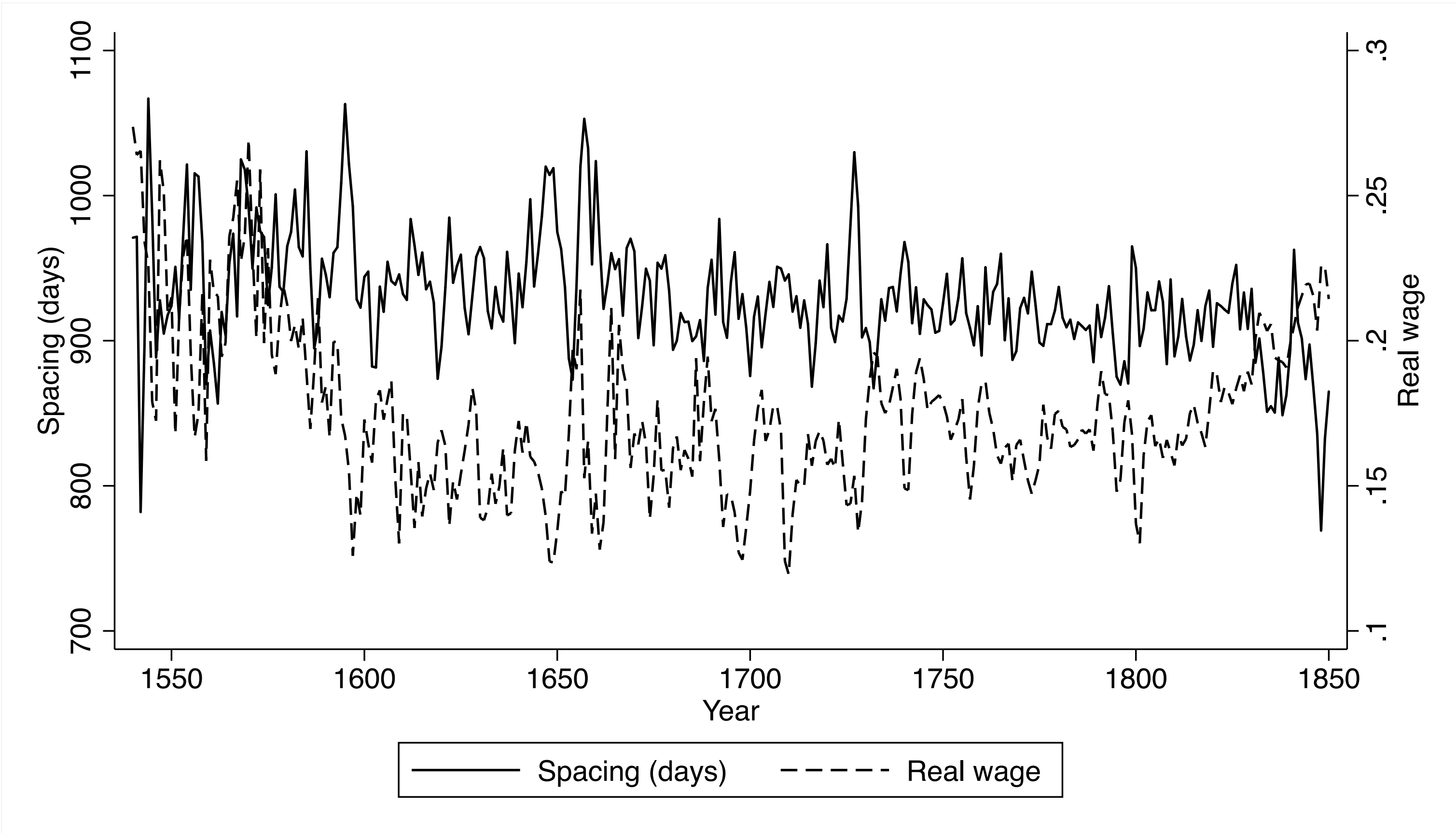

Figure 3: Real wages and average spacing, 1540–1850.

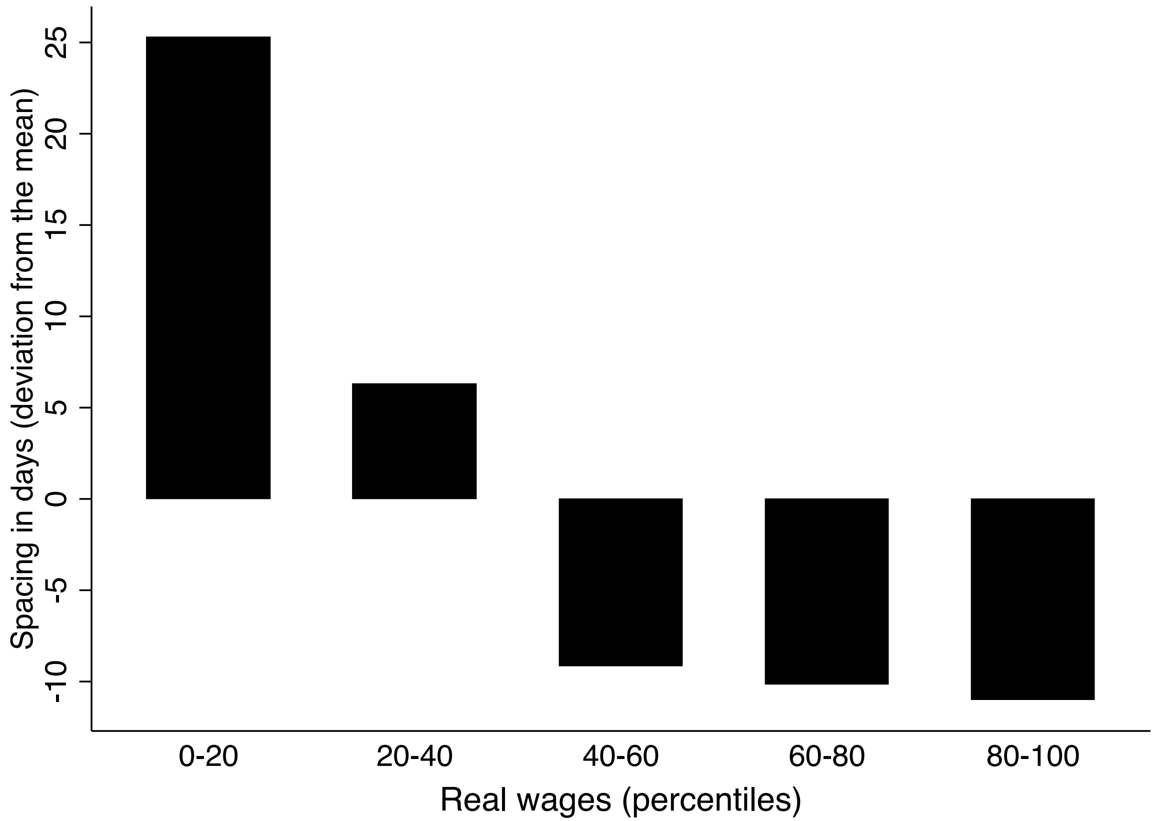

Figure 4: Average spacing by real wage percentiles.

Figure 5: Average spacing by occupational group.

Table 1: Summary statistics

Variable	*Mean*	*SD*	*Min*	*Max*	*N*
Spacing (days)	929.238	475.058	260	4,368	191,892
Mother's age at marriage (years)	23.669	4.275	15.001	46.667	62,515
Mother's age at starting (years)	24.972	4.510	15.110	47.606	71,556
Time to first birth (years)	1.194	1.131	-0.077	11.975	116,220
Prenuptially conceived	0.215	0.411	0	1	191,892
Mother's age at stopping (years)	38.411	5.858	16.794	49.993	71,556
Labourers	0.153	0.360	0	1	191,892
Husbandmen	0.085	0.279	0	1	191,892
Craftsmen	0.101	0.301	0	1	191,892
Traders	0.047	0.212	0	1	191,892
Farmers	0.030	0.171	0	1	191,892
Merchant	0.057	0.232	0	1	191,892
Gentry	0.015	0.122	0	1	191,892
Occupation unknown	0.511	0.500	0	1	191,892
Mother's age when born (years)	30.014	5.875	15.110	48.997	71,556
Mother literate	0.334	0.472	0	1	36,126
Mother's literacy unknown	0.812	0.391	0	1	191,892
Birth order	3.082	2.137	1	19	191,892
Household size	6.175	2.703	2	21	191,892
Child mortality (0-1 year)	0.138	0.345	0	1	191,892
Child mortality (1-3 years)	0.057	0.231	0	1	191,892
Child mortality unknown	0.593	0.491	0	1	191,892

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 (per 1000 people) are from Wrigley (1997). Mean temperatures (in degrees Celsius) from Manley (1953).

Table 3: Average birth intervals (days) within family

Period	*First interval*	*Second last interval*	*Last interval*
1540–1699	830.4	936.0	1,066.3
1700–1749	803.3	926.4	1,076.6
1750–1799	798.2	922.9	1,053.0
1800–1850	805.9	916.4	1,005.3

Source: Cambridge reconstitution data.

Table 4: Duration models

Dependent variable: Spacing in days	*Marriage (1)*	*Starting (2)*	*Time to first birth (3)*	*Spacing (4)*	*Spacing (w/o) (5)*	*Stopping (6)*
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)
Wealth group:						
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)
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 literate	-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's 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 at age (years):						
0–1				0.460***	0.737***	-0.048
				(0.013)	(0.015)	(0.062)
1–3				0.200***	0.162***	-0.152*
				(0.016)	(0.017)	(0.090)
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	214,939	262,618	58,619	351,815	225,312	93,781
Subjects	20,040	22,621	28,100	142,009	85,147	3,795

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. Laborers are the reference wealth group. *** p<0.01, ** p<0.05, * p<0.10. Source: Own estimates.

Table 5: Placebo test on duration models

Dependent variable: *Spacing in days*	*Shift 3 years* *(1)*	*Shift 5 years* *(2)*	*Shift 10 years* *(3)*
Real wage	-0.008	-0.006	-0.011
	(0.025)	(0.023)	(0.023)
Controls	Yes	Yes	Yes
Observations	225,312	225,312	225,312
Subjects	85,147	85,147	85,147

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)*	*Rich* *(2)*
Real wage	0.231*** (0.036)	0.146*** (0.036)
Mother literate	0.060*** (0.023)	0.094*** (0.023)
Mother's literacy unknown	-0.054 (0.034)	0.045 (0.029)
Time to first birth (years)	-0.045*** (0.008)	-0.065*** (0.008)
Prenuptially conceived	-0.008 (0.018)	-0.018 (0.019)
Child mortality at age (years):		
0–1	0.764*** (0.028)	0.651*** (0.029)
1–3	0.156*** (0.031)	0.144*** (0.033)
Unknown	0.044** (0.017)	0.027 (0.017)
Birth order	-0.011*** (0.004)	-0.016*** (0.004)
Crude death rate	-0.005 (0.004)	-0.004 (0.003)
Mother's age at marriage	Yes	Yes
Observations	62,128	54,945
Subjects	23,346	21,762

Note: Cox proportional hazard model with time-varying real wages. Real wages are standardized. Poor are laborers and husbandmen; rich are craftsmen, traders, farmers, merchants, and gentry. 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)
Wealth group:		
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)
Unknown	0.069***	0.070***
	(0.013)	(0.020)
Interaction terms:		
Husbandmen × wheat price		-0.007
		(0.018)
Craftsmen × wheat price		-0.021
		(0.017)
Traders × wheat price		0.009
		(0.023)
Farmers × wheat price		0.058***
		(0.018)
Merchant × wheat price		-0.038
		(0.027)
Gentry × wheat price		-0.042
		(0.050)
Unknown × wheat price		0.000
		(0.011)
Control variables	Yes	Yes
Observations	225,312	225,312
Subjects	85,147	85,147

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. Laborers are the reference wealth group. *** 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* *(1)*	*1600–1649* *(2)*	*1650–1699* *(3)*	*1700–1749* *(4)*	*1750–1799* *(5)*	*1800–1850* *(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)
Wealth group:						
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)
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 literate	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's 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 at age (years):						
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)
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)
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)
Observations	17,746	30,979	27,928	41,204	64,647	42,781
Subjects	6,503	11,484	10,374	15,518	24,858	16,400

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. Laborers are the reference wealth group. *** p<0.01, ** p<0.05, * p<0.10. Source: Own estimates.

Table 9: Panel estimates by sub-periods

Dependent variable: Spacing in days	*1540–1850* (1)	*1540–1599* (2)	*1600–1649* (3)	*1650–1699* (4)	*1700–1749* (5)	*1750–1799* (6)	*1800–1850* (7)
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 (years):							
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)
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)
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)
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)
45–	-187.490***			-197.532	-95.466	-148.778**	-261.388**
	(49.170)			(229.311)	(98.412)	(71.255)	(107.355)
Child mortality at age (years):							
0–1	-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)
1–3	-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)
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	137,032	12,290	20,047	21,859	25,364	33,368	24,069
Number of groups	41,866	4,406	7,045	7,703	8,591	10,410	7,392

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 temperature:		
Spring		0.043*** (0.008)
Summer		0.043*** (0.012)
Autumn		-0.013 (0.010)
Winter		0.059*** (0.006)
Dependent variable: *Spacing in days*		*Second stage*
Real wage (std)	-59.802*** (9.183)	-63.609*** (23.326)
Mother's age at birth (years):		
25–29	41.031*** (6.004)	41.053*** (6.007)
30–34	41.524*** (8.191)	41.568*** (8.211)
35–39	0.401 (10.715)	0.461 (10.755)
40–44	-62.042*** (17.832)	-61.941*** (17.895)
45–	-186.628*** (49.659)	-186.453*** (49.629)
Child mortality at age (years):		
0–1	-196.578*** (4.530)	-196.564*** (4.519)
1–3	-33.438*** (5.220)	-33.431*** (5.209)
Unknown	-7.393** (3.250)	-7.389** (3.244)
Birth order	225.244*** (3.859)	225.231*** (3.857)
Crude death rate	2.928*** (0.863)	2.877*** (0.897)
Time trend	Yes	Yes
Observations	102,026	102,026
Number of groups	30,626	30,626
1^{st} stage F		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 *(1)*	*Panel IV* *(2)*
Dependent variable: *Wheat prices (standardized)*		*First stage*
Average temperature:		
Spring		-0.076***
		(0.021)
Summer		-0.155***
		(0.026)
Autumn		0.077***
		(0.021)
Winter		-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 (years):		
25–29	40.102***	39.777***
	(6.024)	(5.941)
30–34	39.719***	39.086***
	(8.263)	(8.133)
35–39	-2.299	-3.285
	(10.873)	(10.685)
40–44	-65.303***	-66.245***
	(17.960)	(17.668)
45–	-189.997***	-190.345***
	(49.160)	(48.594)
Child mortality at age (years):		
0–1	-196.655***	-196.568***
	(4.518)	(4.504)
1–2	-33.515***	-33.501***
	(5.210)	(5.196)
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	102,026	102,026
Number of groups	22,831	22,831
1^{st} stage F		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

Dependent variable: Spacing in days	*Excluding years of low wages (1)*	*Excluding years of high wheat prices (2)*	*Excluding years of high mortality rates (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	88,456	88,251	98,017
Number of groups	30,101	30,182	31,076

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 very rich and poor in good and bad years

Dependent variable:	*Very 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	5,202	3,433	12,102	8,293
Number of groups	1,983	1,597	5,106	3,996

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. Very rich are merchants and gentry; poor are laborers and husbandmen. Good (bad) years are those in which the real wage is above (below) the long-run median. Source: Own estimates.