Malthus in the Bedroom:
Birth Spacing as Birth Control in Pre-Transition England[1]

Francesco Cinnirella[2]
Ifo Institute, CESifo, CEPR, and CAGE

Marc Klemp[3]
Brown University and University of Copenhagen

Jacob Weisdorf[4]
University of Southern Denmark, CEPR and CAGE

Abstract

We use duration models on a well-known historical dataset of more than 15,000 families and 60,000 births in England for the period 1540–1850 to show that the sampled families adjusted the timing of their births in accordance with the economic conditions as well as their stock of dependent children. The effects were larger among the lower socioeconomic ranks. Our findings on the existence of parity-dependent as well as parity-independent birth spacing in England support the growing evidence that marital birth control was present in pre-transitional populations.

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

JEL classification: J11, J13, O12, N33

[1] We thank the editor and two anonymous referees for their helpful comments. We also thank Karine van der Beek, Davide Cantoni, Greg Clark, Romola Davenport, Martin Dribe, Alan Fernihough, Georg Fertig, Oded Galor, Bernard Harris, David Jacks, Morgan Kelly, Jan Kok, Joel Mokyr, Tine de Moor, Christine Mumme, Tommy Murphy, Alessandro Nuvolari, Cormac Ó Gráda, Jim Oeppen, Leigh Shaw-Taylor, Alice Reid, Guido Schwerdt, Richard Smith, Uwe Sunde, Tony Wrigley, Jan Luiten van Zanden, as well as conference and seminar participants at Cambridge University, University Catholique de Louvain, Harvard University, University of Münster, University of Munich, University of Rome Tor Vergata, University of Rome La Sapienza, the Munich Center for the Economics of Aging (MEA) and the Ifo Institute, the Pisa FRESH Meeting, the 7th Sound Economic History Workshop, the 8th BETA workshop, the 9th European Historical Economics Society (EHES) Conference, the 9th European Social Science History Conference, the European Society for Population Economics (ESPE), the 16th and 17th World Economic History Congress (WEHC), the European Economic Association (EEA), the CAGE/CEPR Conference 'Long-run Growth: Unified Growth Theory and Economic History', and the Royal Economic Society (RES) for helpful comments and suggestions. We wish to thank the Cambridge Group for making the data available, as well as Gill Newton for her help with clarifying questions and useful suggestions. We also thank Helen Aitchison for English proofreading. Laura Schilz provided excellent research assistance. While conducting this research Jacob Weisdorf was partly funded by the ERC grant 'United we stand' (grant no. 240928) courtesy of Tine de Moor. Francesco Cinnirella acknowledges the financial support of the Pact for Research and Innovation of the Leibniz Association. Marc Klemp acknowledges the financial support of the Carlsberg Foundation and the Danish Research Council reference no. 1327-00245 and 1329-00093.
[2] Corresponding author: Poschingerstr. 5, 81679 Munich, Germany. E-mail: cinnirella@ifo.de.
[3] Øster Farimagsgade 5, building 26, DK-1353 Copenhagen, Denmark. E-mail: marc.klemp@econ.ku.dk.
[4] Campusvej 55, DK-5230 Odense M, Denmark. E-mail: jacobw@sam.sdu.dk.

1 Introduction

The existence of marital birth control before the fertility transition of the nineteenth century is a core question among historical demographers. Early statistical analyses have showed an overall absence of marital birth control in pre-transitional England (Wilson 1984; Wrigley and Schofield 1983; Wrigley et al. 1997) and in other European countries (Knodel 1987; Henry 1961). More recent analyses using alternative methodologies and historical data from other countries and regions have found systematic evidence of parity-independent birth control both among natural-fertility populations and within populations in transition (Anderton and Bean 1985; Crafts 1989; David and Mroz 1989a, 1989b; Bengtsson and Dribe 2006; Dribe and Scalone 2010; Kolk 2011; Amialchuk and Dimitrova 2012).

In this paper we employ a novel empirical strategy, which accounts for heterogeneity between families, on a well-known historical dataset (the *Cambridge Group*'s family reconstitution data) to demonstrate that parity-independent as well as parity-dependent birth controls were practiced among the sampled families in the three centuries that preceded England's historical fertility transition. Previous studies, which have used variation in English vital statistics (e.g., births per thousand women) from a sample of 404 parish registers (Wrigley and Schofield 1989), have found limited evidence of *preventive-check* or *vice* behaviour (e.g. Bailey and Chambers 1993; Lee and Anderson 2001; Nicolini 2007; Crafts and Mills 2009; Kelly and Ó Gráda 2012). Our analysis relies on a sub-sample including 26 of the original 404 parishes (Wrigley et al 1997). The advantage of using this sub-sample, containing the dates of more than 60,000 English births over the extraordinarily long time-period of 1540 to 1850, is that it concerns reconstituted families, enabling us to study patterns of births at the family level. In addition to information about the time elapsed between births within marriage, the sub-sample also provides individual-level data, including the order of births, the wife's age at birth, and the husband's profession.

Our evidence on parity-independent birth control is related to both of the Malthusian concepts of preventive checks and vices. That is, by employing a wide range of duration model specifications, we document the existence of parity-independent birth control in the form of a robust economically and statistically significant negative effect of real wages on the spacing of births among the sampled families. We also find a negative effect of real wages on the time between a woman's 15th birthday on the one hand and her marriage and first child on the other. We do not, however, find any significant effects of real wages on the women's stopping behaviour. Our findings are consistent with previous works using short-term variation in prices and wages to document an inverse relationship between living standards and family birth spacing in other pre-transition populations (e.g. Bengtsson and Dribe 2006; Dribe and Scalone 2010; Amialchuk and Dimitrova 2012). Furthermore, we document that the birth-control effects were prevalent among low- and medium-income families but, as expected, not among families of high income. We also show that the response to real wages in terms of birth spacing increased with the number of surviving children, rejecting the notion that the delay of birth has a purely biological explanation. Our findings are robust to the introduction of potentially confounding factors, including the wife's age at marriage, the wife's age at the beginning of the birth interval, as well as variables capturing episodes of high disease occurrences and undernourishment (such as excessive death rates and temperature variations).

More importantly, we document the existence of parity-dependent birth control. That is, while controlling for the wife's age at the beginning of her birth intervals, we establish that the time to the next birth increased significantly with the number of surviving children. A complicating factor when estimating the effect of parity on the spacing of births in a population concerns the potential heterogeneity between couples in their ability to conceive. More fecund couples tend to have shorter birth intervals, and are therefore, *ceteris paribus*, more likely to reach higher parities (Van Bavel 2004a, 2004b; Van Bavel and Kok 2010). This heterogeneity may create compositional variation causing a selection bias: at higher parities there is a higher representation of relatively more fecund couples. A higher proportion of more fecund couples at higher parities may hide the existence of parity-specific controls and result in the absence of a correlation between parity and birth intervals. Methods that do not account for family heterogeneity in fecundity, such as those relying on the use of age-specific parity progression ratios, will therefore not be able to properly identify the existence of parity-dependent birth control (Van Bavel 2004b). The nature and the size of our dataset allow us to account for family heterogeneity and thus to focus on variation in birth intervals *within* families. Hence, by exploiting differences in spacing within the sampled families and controlling for age-related maternal infertility, we establish that the time to the next birth increased significantly and monotonically with the number of surviving children. This finding suggests the existence of pre-transitional parity-dependent birth control, which is consistent with evidence found among other populations worldwide during the initial stages of the fertility transition (e.g. Anderton and Bean 1985; David and Mroz 1989a, 1989b; Friedlander et al. 1980; Gehrman 2007; Van Bavel 2004a, Van Bavel 2004b; Van Bavel and Kok 2004). Our evidence of parity-independent and parity-dependent birth spacing for England thus provides additional support for the existence of marital birth control in pre-transition populations.

2 Data

The Family Reconstitution Dataset

The original dataset, based on family reconstitutions of 26 English parishes, includes 80,704 families and 272,164 births. We begin by restricting the sample to births that occurred in the period 1540–1850, leaving out the thin tails (1536–1539 and 1851–1889). This reduces the number of families to 80,198. Next we require that the wife had at least two recorded births and that her age was known, between the ages of 15 and 45, at the time of her deliveries. The lower end of the age interval comes naturally: the Church of England did not allow women below the age of 15 to marry, and this is confirmed by the data. The rationale for the upper end is that the sampled women rarely conceived children after the age of 45 due to the menopause. Moreover, the restriction mitigates the potential problem that births recorded as occurring after age 45 may be wrongly recorded.[5] The restrictions listed above reduce the sample size to 18,220 families.

The sample is then further restricted to families for which wife's age at marriage can be observed and dates of births (or baptisms) of all recorded family children exist. Pre-nuptially conceived children are included, but birth intervals shorter than 40 weeks, which either stem from premature births or potential data entry errors, are excluded. These restrictions ultimately leave us with a total of 15,593 families and 62,223 birth events.[6] This baseline sample includes 1208 twin births, which are treated as single events.

[Insert Table 1 here]

The summary statistics are reported in Table 1. The upper part of the table reports the descriptive statistics concerning variation across births, and the lower part gives the descriptive statistics concerning family-specific variables. The mean age at marriage of wives is 24.5 years, and the mean age at the time of the first birth is 25.7 years, resulting in an average time span between marriage and first birth of about 1.2 years. The average birth interval is 924 days (circa 2.5 years) with a standard deviation of 455 days (about 1.2 years). The mean age among the sampled wives at any birth is 29.8 years, and the mean age at the time of their last birth is 35.7 years. The occupational title of the husband is known in slightly more than half of the sampled families, with the most common occupations being labourers, craftsmen, and husbandmen. For this subsample the literacy status (inferred from signatures) is recorded for 40% of all wives (and only after 1750), with 33% of the wives signing their marriage certificate. The literacy rate among husbands, also known in 40% of the

[5] Relaxing either of these two restrictions has no influence on the qualitative nature of our results.
[6] Note that in the analyses that are stratified by family, the data requirements are less strict since time-invariant variables drop out. This means we can exploit a total of 71,165 birth events.

regression sample, is 57%. Since the literacy status of husbands and wives is highly correlated, we use only the literacy status of wives in our analyses below.

Real Wages, Death Rates, and Temperatures

In addition to the demographic data described above, our analysis also uses statistics concerning national real wages, crude death rates, and temperatures. Our key explanatory variable when testing for the existence of preventive checks (i.e. parity-independent birth control) is living standards measured by national real wages. The real wage series employed in the main analysis is provided by Clark (2007). The wages and prices used to compute the real wages in England are a combination of observations from across the entire country, as discussed in Clark (2007).[7] In the duration analyses, the national yearly real wage series is combined with the demographic event data from the year in which the relevant interval started to the year in which the modelled event took place.

The upper panel of Figure 1 illustrates the relationship between average birth intervals and (standardized) real wages in percentiles. The graph demonstrates that periods characterized by higher real wages were associated with shorter birth intervals, suggesting the existence of marital birth control in response to economic conditions. Similarly, in illustrating average spacing of births by occupational groups, the lower panel of Figure 1 shows that more affluent social strata (traders, merchants, and gentry) had comparatively shorter birth intervals, suggesting that marital birth control was widespread among lower socio-economic ranks.

[Insert Figure 1 here]

Any analysis that considers birth spacing as a measure of birth control has to address a number of confounding factors linked to biology. The key suspects are undernourishment and climatic conditions, both of which can influence the ability to conceive (Bongaarts 1980; Lam and Miron 1996). Both of these factors are also likely to be correlated with real wages through food prices. In

[7] The same real-wage series was used in Kelly and Ó Gráda (2012).

further robustness analyses we use two additional series of data, one of crude death rates and one of surface air temperatures, to identify and control for harvest failure and undernourishment.

The longest existing historical series of surface air temperatures was recorded in England. Provided by Manley (1953) and starting in 1659, the series cover a substantial part of our period of investigation. A national series of crude death rates covering the entire period of investigation, 1540 to 1850, is provided by Wrigley and Schofield (1989). The descriptive statistics of the control variables and (standardized) real wage by sub-period are reported in Table 2.

[Insert Table 2 here]

3 Duration Analysis

In order to explore the effects of real wages and parity on marital birth control we estimate the hazard rates of four different events: (*i*) marriage; (*ii*) first conception ("starting"); (*iii*) conception following the previous birth ("spacing"), and (*iv*) last conception ("stopping"), where the date of a conception is set 40 weeks prior to the date of a birth.[8] The unit of observation in the marriage, starting, and stopping analyses is the wife. The outcome variable here measures the time span from when the wife becomes *at risk* until the relevant event occurs.[9] In the marriage, starting, and stopping analyses, where the relevant events are the marriage, the first conception, and the last birth, respectively, the unit of observation is the family (i.e., there is one observation per family), and the family is considered to be at risk from the point in time at which the wife reaches the age of 15. In the spacing analysis, where the relevant event is the conception following the birth of the previous child, the unit of observation is the birth interval (i.e. there are potentially multiple events per

[8] Since the date of conception can potentially be influenced by parents, whereas the interval between conception and birth is presumably outside of parental control, the analyses of birth events focuses on dates of conceptions rather than dates of births.

[9] In the marriage analysis, we focus only on first marriages of both husband and wife and we exclude the cases for which we are sure that it is at least a second marriage. We only include individuals who eventually marry, consistent with the idea of studying the timing of marriage in response to changing economic conditions.

family), and the wife is considered at risk of conceiving her next child at the birth of the previous child. The outcome variable is, therefore, the time span from the date of birth or baptism of one child until the date of conception of the subsequent child. The date of conception is calculated by subtracting nine months from the date of birth or baptism. In our analysis we only consider closed birth intervals.[10]

It is important to note that 90% of our sampled birthdates are inferred from baptism dates. Previous studies have shown that most children were baptised within one month of birth (Midi Berry and Schofield 1971). Yet, a potential problem is that the time elapsed between birth and baptism may have differed systematically over time, across the sampled parishes, and across occupational groups. However, since our estimates are either based on variation within families or stratified by parish and quarter century, such differences are accounted for.

In regressions investigating parity-independent birth spacing, each of the four events is regressed on national real wages for each of the years over the modelled interval.[11] Furthermore, dummy variables indicating the order of surviving births (here denoted "net parity") are included in regressions investigating parity-dependent birth spacing.[12] We control for the income class of the husband based on his occupation; the wife's age at marriage;[13] the wife's age at the beginning of the interval; the wife's literacy status;[14] and a proxy for the couple's fecundity (i.e. capacity to conceive) measured by the time elapsed between marriage and first birth. To capture the possibly non-linear association between fecundity and age, we include a quadratic polynomial of maternal age at the beginning of the birth interval. We also account, in a time-varying fashion, for the death of the previous child before the next conception. Finally, as is common in the literature, we include a

[10] Our results are qualitatively unaffected if we allow for censoring, namely we consider the last birth interval as open (see online appendix, Table A-2).

[11] For example, if a child was born on October 21, 1750, and the successive child was conceived on July 5, 1753, then the relevant real wages for this birth interval are those recorded in the years 1750, 1751, 1752, and 1753.

[12] Net parity is computed as the number of children alive at the start of the interval (see Van Bavel 2004a for a discussion on crude parity and net parity).

[13] Information about the wife's age at marriage is missing in about 75% of all families. We thus include a binary variable for unknown age at marriage of the wife in the regression.

[14] Since maternal and paternal literacy are highly correlated, we include only the wife's literacy.

binary variable for the last birth interval to capture a failed attempt to stop having children (Van Bavel 2004a; Okun 1995; Knodel 1987; Anderton 1989).[15]

We estimate a time-varying Cox Proportional Hazard Model (Cox 1972) specified as follows:

$$h(t) = h_0(t)exp\big(\beta_1 x_1 + \beta_2 x_2 + \cdots + \beta_k x_k + g(t)(\gamma W)\big) \qquad (1).$$

The term $h_o(t)$ is the baseline hazard function where t is time, measured in days; $(x_1 \ldots, x_k)$ are socio-economic and demographic covariates; and W is the standardized (zero mean and unit standard deviation) time-varying (yearly) real wage (Clark 2007). In all our analyses we stratify by parish and quarter century, i.e. each parish and quarter century provide unique baseline hazard functions. With this stratification our analyses account for the heterogeneity between different time periods and locations. The stratification by quarter century furthermore implies that the estimated impact of real wages on birth intervals can be interpreted as a short-term effect. Finally, although demographic events are recorded on specific dates, the real wages are annual averages, and so our standard errors are clustered by the year of the demographic event considered.[16]

Parity-Independent Birth Control

Table 3 reports the estimates of our duration models capturing the effects of the real wage and the control variables on the duration to each of the studied events. Real wages are standardized with a mean of zero and a standard deviation of one and to ease comparison with previous studies we report hazard ratios.

Column 1 of Table 3 establishes that the real wage is positively and significantly correlated with

[15] Note that the binary variable for last birth interval is based on the last birth recorded. Thus, we cannot exclude that the wife migrated to another parish and that she had further births there. However, in Table 9 we address directly the issue of migration.

[16] Whenever we include parity fixed effects we cluster the standard errors by family.

the hazard of marriage. This is *prima facie* evidence of a direct negative effect of living standards on the wife's age at marriage, supporting the Malthusian hypothesis that delayed marriage was a response to hard times, and a sign of the existence of a preventive check mechanism operating among the sampled population in pre-transitional England.

Column 2 focuses on the event of giving birth to the first child within marriage ("starting"). The estimates indicate a positive and statistically significant correlation between the real wage and the time to the first birth. The magnitude of the effects on the events of marriage and starting are very similar: a one-standard deviation increase in the real wage accelerates time to marriage and to first conception by 23% and 25% respectively. This is consistent with the conventional view that marriage historically marked the onset of a family (i.e. to give birth).

[Insert Table 3 here]

The estimates in column 3 present evidence of pre-transitional, parity-independent birth control, establishing that an increase in real wages accelerates the timing of the next conception. The magnitude of the impact of the real wage is also economically significant: a one-standard deviation increase in the real wage accelerates the timing of the next conception by about 10%.[17]

Turning to the stopping specification (column 4), we find no statistically significant impact of real wages on the hazard of the last conception. The effect remains statistically insignificant when considering starting ages other than 15 for the event of stopping, and when we split the sample by fifty-year sub-periods (not reported). These results are perhaps unsurprising: since real wages are largely non-trending across the period under observation, short-term variations in real wages are likely to cancel out over the course of a family lifecycle, leaving little room for wages at any point in time to substantially affect the timing of the last birth.[18]

In summary, our analyses establish that falling living standards captured by lower real wages led

[17] Note that if we exclude childbirths coming from second or higher order marriages we obtain virtually the same results.
[18] For a methodological discussion on the relationship between spacing and stopping you are referred to Anderton (1989) and McDonald and Knodel (1989).

not only to significantly later marriages but also to longer birth intervals within marriage. This documents the existence of parity-independent birth control among the sampled couples. In our robustness analyses we explore the impact of some key confounding factors to rule out the possibility that the spacing effects we observe are positive-checks rather than preventive-checks.

Socio-Economic Factors and Other Covariates

In order to shed light on the role of socio-economic factors in historical birth patterns, we have sub-divided our sampled families into income groups using a categorization proposed by Clark and Cummins (2010). Clark and Cummins have used information about male testators to group male occupations according the amount of wealth left in the will. From poorest to richest these groups are: labourers, husbandmen, craftsmen, traders, farmers, merchants and gentry. Our reference group in the analysis is labourers (i.e. the poorest group in the classification scheme). Concerning the hazard of a marriage, none of the groups differs significantly from labourers, apart from craftsmen that tend to marry later in life than others (Table 3, column 1). When looking at the timing of the first birth, craftsmen, but also farmers, had their firstborns comparatively later in life (column 2).

More interestingly, looking at birth spacing, we find that poorer families had longer birth intervals on average than richer ones: column 3 shows that all six occupational groups included in the model have significantly shorter birth intervals (higher hazard ratios) compared to the reference group (labourers). In particular, we find that the coefficients for the richest groups (traders, farmers, merchants, and gentry) are statistically different from the coefficients of husbandmen and craftsmen, respectively.[19] Therefore, birth intervals appear to decrease with wealth.

The mechanism causing these differences in birth intervals between rich and poor may have to do with differences in breastfeeding practices: while women in poor families would breastfeed their own children, the rich could afford to pay a wet nurse, explaining why the more affluent social

[19] The difference between husbandmen and craftsmen is not statistically significant.

groups display larger hazard of a further birth (Fildes 1987). Differences in the practice of *coitus interruptus* may also explain the different patterns of birth spacing (Santow 1995).

We also find a large impact of a child death on the next conception, with a child death accelerating the timing of the next conception by some 74 per cent. Possible reasons for this effect include the interruption of the breastfeeding period, which shortens *postpartum amenorrhea*, and the attempt to replace the deceased child.

Interestingly, socio-economic differences also apply in the case of stopping. Column 4 establishes that labourers stop later on average than their more affluent counterparts, and that the gentry are more likely to stop earlier. Husbandmen, craftsmen, traders, and merchants also stop significantly earlier than labourers. These results are conditional on the mother's age at marriage, her age at the last birth, and the family size. In fact, we find that a larger family size is associated with a later time of stopping. Therefore, differences in sterility associated with differences in the age at marriage, or family size, cannot explain the differences in stopping practice across occupational groups. The fact that the rich had more surviving offspring than the poor, as demonstrated by Clark and Hamilton (2006) and Boberg-Fazlic et al. (2011), can thus be ascribed to earlier starting and shorter birth intervals. The earlier stopping among the rich (especially the gentry) is consistent with the notion that wealthier families may have had a target number of offspring.[20]

Literacy among wives is also associated with shorter birth intervals and earlier stopping age, even after controlling for socio-economic status. One reason for this could be that literate individuals from the lower socio-economic ranks imitate the fertility patterns of their higher socio-economic counterparts. Moreover, couples of low fecundity, captured by a relatively large time interval from their marriage to their first birth, have, as expected, significantly larger birth intervals than couples of high fecundity. Also, the group of couples that give birth to children within 40 weeks of marriage (which includes couples that conceived their firstborn before marriage) have an overall lower hazard

[20] For a discussion on this see Van Bavel (2004a).

of subsequent births. The latter finding seemingly contradicts the suggestion made by Wrigley et al (1997, p. 422) in their description of the data's prevalence of pre-nuptially conceived births where they note that "It might be expected that such women [giving pre-nuptially conceived birth] would display higher fertility during the balance of their childbearing life than women whose first child was born more than nine months after marriage, since it might be supposed that women of high fecundity, or perhaps with a greater appetite for sexual activity, would have higher fertility and would be more likely to become pregnant before marriage than others".

Lastly, as documented in previous studies, we find that the last birth-interval is significantly larger on average than the previous intervals, consistent with the idea that the last birth was sometimes a failed stopping attempt.

Before we proceed to explore the role of parity in detail, it is useful to take a preliminary look at the variable 'birth order'. The coefficient of birth order (Table 3, column 3) is highly statistically significant and suggests that higher parities are associated with *shorter* spacing. As discussed in the introduction, this finding may arise from a selection bias stemming from the use of variation in birth spacing *across* families rather than *within* them. That is, as we move from lower to higher birth orders, the composition of the sampled families may shift towards a higher share of more fecund couples, and hence couples of shorter-than-average spacing. As the birth order results of Table 3 indicate, the composition effect may lead us to mistakenly conclude that higher parities are associated with shorter spacing of births. However, given the nature of our data, this issue of selection bias can be addressed by accounting for between-family heterogeneity. To shed light on these matters, the next section explores variation in birth spacing across, as well as within, families.

Parity-Dependent Birth Control

This section is devoted to the question of whether or not birth spacing depended on the stock of surviving offspring in a family. More specifically, we test the hypothesis that the timing of a successive birth is independent of the number of children already born (e.g. Henry 1943). We conduct the test in a manner similarly to previous studies (e.g. Bengtsson and Dribe 2006; Van Bavel 2004a) by estimating parity-fixed effects. In particular, we define net parity as the number of children alive at the start of the interval and include in the model a dummy variable for each net parity. Importantly, in order to test for parity-specific birth control appropriately, we also account for between-couple heterogeneity, i.e. the fact that highly fecund couples are able to have shorter birth intervals on average, and hence can reach higher parities, causing a potential selection bias towards shorter spacing at higher parity. We account for this selection bias by stratifying our sampled birth intervals on the family level.

Table 4 presents the estimates from duration models of birth intervals with and without stratifying on the family level. The different estimates reported in columns 1 and 2 of Table 4 illustrate the relevance of stratifying on the family level, indicating also the main reason for why our findings deviate from those of the Cambridge Group (e.g. Wrigley et al. 1997). Column 1 reports the results of using our previous spacing model only augmented with parity-fixed effects. Note that in this specification we stratify the model by parish and quarter century and control for the wife's age at the beginning of the birth interval (in a quadratic fashion) in order to capture age-related variation in maternal fecundity, but we do *not* stratify by family.

It is clear from the findings reported in column 1 that the speed of a successive conception is significantly lower for parity 2+ compared to the reference group (parity 1). It is also clear that the difference between parity 2 and the remaining (higher) parities is statistically the same. This result is consistent with the finding of the Cambridge Group concluding that 'birth interval lengths changed very little between parities 2 and 5' (Wrigley et al 1997, p. 435). The latter finding would therefore support the ‘natural fertility’ hypothesis in that the spacing of births (after parity 1) does not appear to depend on parity.

[Insert Table 4 here]

Column 2, instead, reports the results when we account for heterogeneity among the sampled couples, stratifying by family and quarter century. We stratify also by quarter century to allow the baseline hazard to vary over time.[21] By using variation in birth spacing *within* families, we find that the speed of a successive conception *decreases* monotonically with net parity, meaning that the spacing of births *increases* monotonically with net parity. For example, the coefficient for “Net parity 2” implies that the time to the successive conception after the second sibling is about 53% lower than after the first sibling; the time to the next conception after the third sibling is 73% lower; the time to conception of a further sibling after the sixth child is 95% lower with respect to the spacing between the first two siblings. These effects are significantly different from each other. This can be seen in Figure 2, where we depict the coefficients for net parity with the relative confidence intervals estimated in columns 1 and 2 of Table 4. The figure clearly shows how *not* accounting for family heterogeneity conceals the positive impact of parity on the spacing of family births. It should also be noted that these findings are obtained while accounting for age-related changes in maternal fecundity by controlling for the age (and its square) of the mother at the beginning of the interval.[22] Interestingly, we find that accounting for family heterogeneity the impact of child death on the successive birth interval increases in size. This suggests that unobserved heterogeneity at family level is correlated with child death and estimates not accounting for family heterogeneity provide biased estimates.

[Insert Figure 2 here]

The impact of the real wage on the spacing of births, i.e. the evidence of parity-independent birth

[21] Stratifying by quarter century is not necessary to obtain the new results on parity. As we will show below, the results on parity holds also when analysing sub-periods and including decade fixed effects.

[22] To reduce the impact of age-related maternal infertility, we also estimated a model for a subsample of families with less than 4 children. The coefficients for parity show the same pattern, which strongly suggests the existence of parity-dependent birth spacing.

control observed above, remains highly significant also in the specifications including parity dummies. This leaves an important question: Does the effect of wages on births vary with parity? The underlying hypothesis here is that the decision to postpone a birth during hard times may be exacerbated by the presence of other dependent children. We test this hypothesis by interacting the real wage with the parity-fixed effects. As in column 2 of Table 4, we stratify by family and quarter century. Column 3 reports the results, establishing that not only are higher parities associated with significantly larger birth intervals but the size of the demographic response to changing real wages also rises significantly with parity. The coefficients of the interactions show that the impact of real wages on spacing increases up to parity 3 (the interval between the third and fourth sibling) and then it stabilizes. The coefficients for the interaction terms imply that, if the real wage decreases by one standard deviation, then the time to the next conception is about 7% lower for the third child (parity 2) and 11% lower for the fourth child (parity 3) compared to that of the second child (parity 1, i.e. the reference group).[23] The fact that the real-wage effect varies across parity is strong evidence that the observed birth control was a deliberate decision rather than a biological mechanism.[24]

It is also possible to quantify the effects of the real wage and of parities in terms of time. Let us consider the baseline estimate with parity-fixed effects and stratification by family as in column 2 of Table 4. The birth interval associated with the first parity (first two siblings) is 457 days; the birth interval associated with parity 2 (siblings 2 and 3) is 570 days: a difference of 113 days. This difference increases to 213 days if we consider the birth interval between siblings 3 and 4 with respect to the first interval in the family.[25] As for the real wage, an increase of the real wage by 1.5

[23] The coefficients of the interaction terms are not statistically different from each other. However, by interacting the real wage with the variable *birth order* provides a significant and positive coefficient, indicating that the effect of the real wage on spacing changes across parities.

[24] We have performed a standard test of the proportionality assumption based on the Schoenfeld residuals (online appendix). This test cannot reject the null hypothesis that the real wage satisfies the proportionality assumption both in the parity-independent and in the parity-dependent analysis. The proportionality assumption can also not be rejected for the parity fixed effects.

[25] These are median survival times predicted for a birth interval starting in the year 1710, from a woman aged 30 at the beginning of the birth interval, with the standardized real wage at zero. In order to compute the predicted median survival time we first compute the adjusted survival probabilities for each mother at every observation (i.e., once per year of each birth interval) by raising the estimated stratum-specific baseline survival functions to the power of the linear prediction

standard deviations is associated with the postponement of a conception by about 54 days. [26]

Table 5 establishes that the parity effects are not just a nineteenth-century phenomenon. Indeed, the effects become larger as we move the sample period back towards the sixteenth century. This strongly supports the birth-limitation hypothesis by providing evidence of parity-dependent birth control similar in nature to that observed by Van Bavel (2004a) and others elsewhere in Europe during later periods: the larger the size of the families, the more the couples strive to postpone the next birth.

[Insert Table 5 here]

4 Robustness Checks

This section analyses the robustness of our results. Our main interest concerns fertility behaviour within marriage, so in this section we focus exclusively on the "spacing" analysis outlined above. All of our robustness checks are performed in a model with parity-fixed effects where we stratify births by family and quarter century.

Sub-Periods

Table 5 estimates our model for the following sub-periods: 1540–1599, 1600–1649, 1650–1699, 1700–1749, 1750–1799, and 1800–1850. The impact of the real wage on spacing is always statistically significant, apart from the very early period, 1540–1599. The lack of significance during the sixteenth century is partly due to the lower number of observations (1,357 birth intervals) resulting in a more imprecisely estimated coefficient as reflected by the larger standard error.

The analysis by sub-periods suggests that the response in spacing to changes in real wages was most pronounced in the periods 1600–1649 and 1700–1749. This latter finding is consistent with the

---

$\boldsymbol{x}_i\hat{\beta}$. We then compute the average of the survival function around 0.5 within the interval 0.475–0.525.

[26] These predicted median survival times are adjusted for a 4th surviving child, born in 1710, from a mother who was 30 at the beginning of the birth interval. Note that such fluctuations in the real wage occurred actually in the first decades of the 18th century.

findings of Kelly and Ó Gráda (2012) who observe a rising impact of wages on birth rates in the early eighteenth century. The point estimate for the sub-period during which we have the strongest preventive check (1600–1649) reveals that a one-standard deviation increase in the real wage was associated with a 21% increase in the speed of a successive conception. Again, the coefficients for the parity-fixed effects confirm the prevalence of parity-specific birth control during each of our sub-periods.

Compositional Effects

Since data are unavailable for some parishes for some periods, the composition of parishes in our analysis changes over time. Likewise, the occupational titles of husbands are more frequently reported towards the end of the period under consideration. To the extent that the likelihood of inclusion in the regression sample is correlated with spacing behaviour, the estimated relationships may be biased. While the stratification by quarter century, parish, or family already accounts for potential associations between spacing and time, location, or family, we assess the influence of sample selection by focusing on parishes without attrition, and with full occupational coverage. As a further assessment, we investigate the robustness of our results when controlling for time-fixed effects on a higher resolution than quarter centuries.[27]

In column 1 of Table 6 we estimate our model using a sub-sample containing those 12 parishes with continual coverage across the period 1600–1800.[28] The impact of the real wage on birth intervals remains highly significant and of similar size with respect to the baseline estimate reported in the previous section. In column 2 we include only families where the husbands have an occupational title recorded: our findings are robust to this subsample also.[29]

[27] In the online appendix we show that the geographical coverage of the original sample and of the regression sample does not deviate systematically (see Figures A1 and A2).

[28] The 12 parishes are: Aldenham, Banbury, Birstall, Bottesford, Colyton, Gainsborough, Gedling, Methley, Odiham, Shepshed, Southill, and Terling. The three parishes with the longest birth intervals are Terling, Dawlish, and Hartland. The three parishes with the shortest birth intervals are Lowestoft, Gainsborough, and March.

[29] We have also estimated a model for a sub-sample including the three parishes that have the richest occupational

Along similar lines, while the stratification of birth intervals by quarter century accounts for changes in quarter-century fixed effects, it does not account for potential secular changes affecting both the real wages and the demographic outcomes on a shorter timescale. Column 3 establishes that when accounting for decade-fixed effects instead of quarter-century fixed effects (while still stratifying by family), the estimated impact of the real wage on birth intervals remains highly significant both statistically and economically. Thus, the estimated effect of aggregate wages on the hazard of births within families cannot be attributed to secular variations across quarter centuries. Overall, these specifications indicate that the qualitative results cannot be attributed to sample selection bias.

[Insert Table 6 here]

Migration

Another limitation of the data is the fact that the migration of people in or out of the sampled parishes is not detected. This presents a problem if the decision to migrate is correlated with both real wages and actual birth intervals, or if migrants and non-migrants have different spacing behaviour. While there are of course limits to what we can do to deal with the problem of migration, we address the issues in two ways: we account for *permanent* migration by including a dummy indicating those husbands and wives who had a missing birth or death date, and we account for *temporary* migration by excluding birth intervals lengthy enough to potentially conceal unobserved births. Furthermore, we also follow Ruggles (1999) and restrict our sample to so-called "completed marriages" ensuring that the sampled husbands and wives did not terminate the marriage prematurely by migration or death.

In order to investigate if movers are different from stayers in terms of spacing behaviour, we first exclude immigrants and then emigrants from the baseline sample. We define a couple as an

information (Austrey, Earsdon, Gainsborough). The results are virtually identical. Moreover, Wrigley et al. (1997, p. 43-44) have suggested that the parish of Birstall may be problematic due to its large population size and the fact that its occupational structure is strongly biased towards manufacturing. Our results are, however, unaffected if we exclude the parish of Birstall.

immigrant couple (i.e. coming from an unobserved parish) if the husband and wife both have missing birth/baptism dates but recorded death/burial dates. Furthermore, we define a couple as an emigrant couple (i.e. moving to an unobserved parish) if the husband and wife both have missing death/burial dates but recorded birth/baptism dates. Table 7 shows that it makes virtually no difference to the effect of real wages on spacing whether we exclude immigrant couples (column 1) or emigrant couples (column 2) compared to the baseline estimate (Table 3, column 3). Including dummy variables indicating immigrants and emigrants (not shown) generates similar results.

[Insert Table 7 here]

Low real wages may induce couples to temporarily leave their parish of residence in search of work. If they give birth and baptize a child while living outside of their home-parish, these births will go unobserved in our data and instead appear as an extended birth spacing interval. We address this issue by excluding intervals that are comparatively long. In column 3 of Table 7 we restrict the sample to birth intervals of less than three years, roughly making up the 75th percentile of the sampled intervals. The coefficient on real wages remains highly significant and of the same order of magnitude as the baseline estimate. Column 4 shows the results for an even more restrictive assumption, namely focusing on birth intervals of less than 2.5 years, which is close to the average length of birth intervals. Once more, we find a significant effect of real wages on spacing.

Ruggles (1999) has pointed out that restricting the sample to “completed marriages”, meaning marriages where both spouses survive to the point in time where the wife reaches age 50, is particularly useful to deal with issues of migration. Not only do “completed marriages” ensure that the sampled couples are neither permanent immigrants nor permanent emigrants – because we require their birth and death dates to be known – but they also warrant that the couples are healthy enough to not end reproduction prematurely. Column 5 of Table 7 reports the estimates based on the sub-sample of completed marriages: the coefficient for the real wage is still highly significant and even larger in magnitude than the baseline estimate.[30]

Biological Influences

Is what we observe a biological mechanism rather than deliberate spacing behaviour? Two potentially confounding biological factors are undernourishment and climatic conditions, as measured by air temperatures, both of which have been shown to impact fertility (Bongaarts 1980; Lam and Miron 1996). Since both these factors are also likely to be correlated with real wages (temperatures through crop yields and hence food prices, and undernourishment when real wages are close to subsistence), we need to account for such potentially confounding mechanisms. While we have already established in the baseline analysis that the impact of real wages on spacing is parity-specific, we further address the question of biological influences by accounting for the potential confounding effects of climate and undernourishment.

Accounting for the potential confounding effect of air temperatures (provided by Manley 1953), we find that they are not significantly associated with birth intervals (Table A5, column 1, online appendix). Reassuringly, the effect of the real wage and parity on birth spacing remains highly significant.

To account for the potential biological effects of unobserved correlates of real wages, such as undernourishment, we control for aggregate crude death rates (provided by Wrigley and Schofield 1981). Death rates not only reflect the disease environment, and thus disease-related infertility, but can also capture episodes of crop failure and famines, and the effect thereof on fecundity. We find that higher death rates increase the time to the next conception (Table A5, column 2, online appendix). However, the effect of the real wage on birth spacing remains highly significant. We reach the same conclusions when including both temperatures and death rates in the model (Table

[30] In the online appendix we report additional robustness checks. In Table A1 we show estimates using alternative real wage series. In Table A3 we estimate our parity-fixed effect model constraining the sample on known marriage date and on households for which the original source explicitly mentions it is a first marriage. In Table A4 we estimate our model including the last birth interval accounting for censoring. In all cases the results are virtually unaffected.

A5, column 3, online appendix).

Occupational Group

Our last robustness check concerns the extent to which the real-wage impact on the spacing of births differs across different occupational groups. To this end, we estimate the spacing model separately for each of the socio-economic groups as categorized by Clark and Cummins (2010): labourers, husbandmen, craftsmen, traders, farmers, merchants and gentry. Table 8 shows that the impact of the real wage on spacing is large and highly significant for all occupational groups except for farmers and for the joint category of merchants and gentry, indicating a significant insensitivity to real-wage variation among the wealthier sections of society.[31] These differences in birth spacing among occupational groups are consistent with the findings of Bengtsson and Dribe (2006) observing a response in spacing to food price variations among the landless and semi-landless, but not among noble tenants and freeholders in Southern Sweden. This result is also in line with the findings of Kelly and Ó Gráda (2012) who conclude that higher wheat prices deterred marriages of less wealthy tenants, whereas they had a positive impact on wealthier families. Our findings show that, while parity-independent birth control (the real-wage effect) only pertains to less affluent sections of society, parity-dependent birth control is common across the entire socio-economic spectrum.

[Insert Table 8 here]

5 Conclusion

We set out to reinvestigate the hypothesis that marital birth control in pre-transitional England was absent. To analyse this issue we use a variety of specifications of duration models on a well-known historical dataset with the timing of births as the main outcome variable. A large and robust effect of real wages on birth spacing confirms the existence of parity-independent birth control in pre-

[31] We have merged the merchants and gentry groups because of the low number of observations.

transition populations. By exploiting variation in birth intervals *within* families, which allows to account for family heterogeneity, we establish the existence also of parity-dependent birth control in the sampled population in the three centuries preceding England's fertility transition. Evidence of parity-dependent birth control holds across occupational groups and across centuries. Our findings strongly suggest that couples adjusted the timing of their births in accordance not only with the prevalent economic conditions, but also with their stock of dependent children.

References

AMIALCHUK, A. AND E. DIMITROVA (2012): "Detecting the evolution of deliberate fertility control before the demographic transition in Germany", *Demographic Research* 27, 507-542.

ANDERTON, D.L., AND L.L. BEAN (1985): "Birth spacing and fertility limitation: A behavioral analysis of a nineteenth century frontier population", *Demography* 22, 169-83.

ANDERTON, D. N. (1989): "Comment on Knodel's 'Starting, Stopping, and Spacing during the Early Stages of Fertility Transition'", *Demography*, 26, 467–470.

BAILEY, R. E. AND M. J. CHAMBERS (1993): "Long-term Demographic Interactions in pre-census England", *Journal of the Royal Statistical Society*, 156, 339–362.

BAIRD, D. T., J. COLLINS, J. EGOZCUE, L. H. EVERS, L. GIANAROLI, H. LERIDON, A. SUNDE, A. TEMPLETON, A. VAN STEIRTEGHEM, J. COHEN, P. G. CROSIGNANI, P. DEVROEY, K. DIEDRICH, B. C. FAUSER, L. FRASER, A. GLASIER, I. LIEBAERS, G. MAUTONE, G. PENNEY, AND B. TARLATZIS (2005): "Fertility and ageing", *Human Reproduction Update*, 11, 261–276.

BENGTSSON, T. AND M. DRIBE (2006): "Deliberate control in a natural fertility population: Southern Sweden, 1766-1864", *Demography*, 43, 727–746.

BOBERG-FAZLIC, N., P. SHARP, AND J. WEISDORF (2011): "Survival of the richest? Social status, fertility and social mobility in England 1541-1824", *European Review of Economic History*, 15, 365–392.

BONGAARTS, J. (1980): "Does malnutrition affect fecundity? A summary of evidence", *Science*, 208(4444), 564-569.

CLARK, G. (2005): "The Condition of the Working Class in England, 1209-2004", *Journal of Political Economy*, 113, 1307–1340.

——— (2007): "The Long March of History: Farm Wages, Population and Economic Growth, England 1209–1869", *Economic History Review*, 60, 97–135.

——— (2008): *A Farewell to Alms: A Brief Economic History of the World*, Princeton University Press.

CLARK, G. AND N. CUMMINS (2010): "Malthus to modernity: wealth, status, and fertility in England, 1500–1879", *Journal of Population Economic*, 28, 3–29.

CLARK, G. AND G. HAMILTON (2006): "Survival of the richest in pre-industrial England", *Journal of Economic History*, 66, 707–736.

COALE, A. (1986): "The decline of fertility in Europe since the eighteenth century as a chapter in human demographic history", in *The decline of fertility in Europe*, ed. by A. Coale and S. C. Watkins, Princeton, 1–30.

COALE, A. J., AND TRUSSELL, J. T. (1974): "Model fertility schedules: Variations in the age schedule of childbearing in human populations", *Population Index*, 40(2), 185–258.

COALE, A. J. AND S. C. WATKINS, eds. (1986): *The decline of fertility in Europe*, Princeton: Princeton University Press.

COX, D. R. (1972): "Regression Models and Life-Tables", *Journal of the Royal Statistical Society. Series B (Methodological)*, 34, 187–220.

CRAFTS, N. F. R. (1989): "Duration of Marriage, Fertility and Women's Employment Opportunities in England and Wales in 1911, *Population Studies: A Journal of Demography,* 43(2), 325-335.

CRAFTS, N. F. R. AND T. C. MILLS (2009): "From Malthus to Solow: How did the Malthusian economy really evolve?", *Journal of Macroeconomics*, 31, 68–93.

DAVID, P. A., AND MROZ, T. A. (1989a): "Evidence of Fertility Regulation among Rural French Villagers, 1749–1789: A Sequential Econometric Model of Birth-Spacing Behaviour (Part 1)", *European Journal of Population*, 5(1), 1–26.

DAVID, P. A., AND MROZ, T. A. (1989B): "Evidence of Fertility Regulation among Rural French Villagers, 1749–1789: A Sequential Econometric Model of Birth-Spacing Behaviour (Part 2)", *European Journal of Population*, 5(2), 173–206.

DRIBE, M. AND F. SCALONE (2010): "Detecting Deliberate Fertility Control in Pre-transitional Populations: Evidence from six German villages, 1766-1863", *European Journal of Population/Revue européenne de Démographie*, 26, 411-434.

FILDES, V. A. (1987): *Breast, Bottles, and Babies: A History of Infant Feeding*, Edinburgh University Press.

FRIEDLANDER, D; Z. EISENBACH; AND C. GOLDSCHEIDER (1980): "Family-Size Limitation and Birth Spacing: The Fertility Transition of African and Asian Immigrants in Israel". *Population and Development Review* 6, 581-593.

GEHRMANN, R. (2007): "Birth limitation in rural Germany before the fertility decline". *Historical Social Research-Historische Sozialforschung* 32, 91-110.

HENRY, L. (1953): "Fondements theoriques des mesures de la fecondite naturelle", *Revue de l'Institut International de Statistique*, 21(3), 135-151

HENRY, L. (1961): "Some data on natural fertility", *Eugenics Quarterly* 8, 81–91.

KELLY, M. AND C. Ó Gráda (2012): "The Preventive Check in Medieval and Pre-industrial England", *Journal of Economic History*, 72, 1–21.

KNODEL, J. (1987): "Starting, Stopping, and Spacing During the Early Stages of Fertility Transition: The Experience of German Village Populations in the 18th and 19th Centuries", *Demography*, 24, 143–162.

KOLK, M. (2011): “Deliberate Birth Spacing in Nineteenth Century Northern Sweden”, *European Journal of Population*, 27, 337-359.

LAM, D. A. AND J. A. MIRON (1996): “The Effects of Temperature on Human Fertility”, *Demography*, 33, 291–305.

LEE, R. D. AND M. ANDERSON (2001): “Malthus in state space: macroeconomic-demographic relations in English history, 1540 to 1870”, *Journal of Population Economics*, 15, 195–220.

MCDONALD. P. AND J. KNODEL (1989): “The Impact of Changes in Birth Spacing on Age at Last Birth: A Response to Anderton”, *Demography*, 26(3), 471-472.

MALTHUS, T. R. (1798): *An Essay on the Principle of Population*, London: J. Johnson.

MANLEY, G. (1953): “The mean temperature of central England, 1698–1952”, *Quarterly Journal of the Royal Meteorological Society*, 79, 558–567.

MIDI BERRY, B. AND R. S. SCHOFIELD (1971): “Age at Baptism in Pre-Industrial England”, *Population Studies*, 25, 453-463.

OKUN, B. S. (1995): “Distinguishing stopping behaviour from spacing behaviour with indirect methods”, *Historical Methods*, 28, 85–96.

RUGGLES, S. (1999): “The Limitations of English Family Reconstitution: English Population History from Family Reconstitution 1580-1837”, *Continuity and Change*, 14, 105–130.

SANTOW, G. (1995): “Coitus Interruptus and the Control of Natural Fertility”, *Population Studies*, 49, 19-43.

SZRETER, S. (1996): *Fertility, class and gender in Britain 1860–1940*, Cambridge, UK: Cambridge University Press.

VAN BAVEL, J. (2004a): “Deliberate birth spacing before the fertility transition in Europe: Evidence from nineteenth-century Belgium”, *Population Studies*, 58(1), 95–107.

VAN BAVEL, J. (2004b): “Detecting Stopping and Spacing Behaviour in Historical Fertility Transition: A Critical Review of Methods”, *Population (English edition)*, 59, 117–128.

VAN BAVEL, J. AND J. KOK (2004): “Birth Spacing in the Netherlands: The Effects of Family Composition, Occupation and Religion on Birth Intervals, 1820-1885”, *European Journal of Population*, 20, 119–140.

VAN BAVEL, J. AND J. KOK (2010): “A mixed effects model of birth spacing for pre-transition populations”, *The History of the Family*, 15(2), 125-138.

WILSON, C. (1984): “Natural Fertility in Pre-Industrial England, 1600-1799”, *Population Studies*, 38, 225–240.

WRIGLEY, E. A. (1966): “Family Limitation in Pre-Industrial England”, *The Economic History Review*, 19, 82–109.

WRIGLEY, E. A., R. DAVIES, J. OEPPEN, AND R. SCHOFIELD (1997): *English Population History from Family Reconstitution*, Cambridge: Cambridge University Press.

WRIGLEY, E. A. AND R. S. SCHOFIELD (1981): *The Population History of England 1541–1871*, Cambridge: Edward Arnold Ltd.

——— (1983): "English population history from family reconstitution: summary results 1600-1799", *Population Studies*, 37.

Figure 1: Average spacing by real-wage percentiles and occupational group

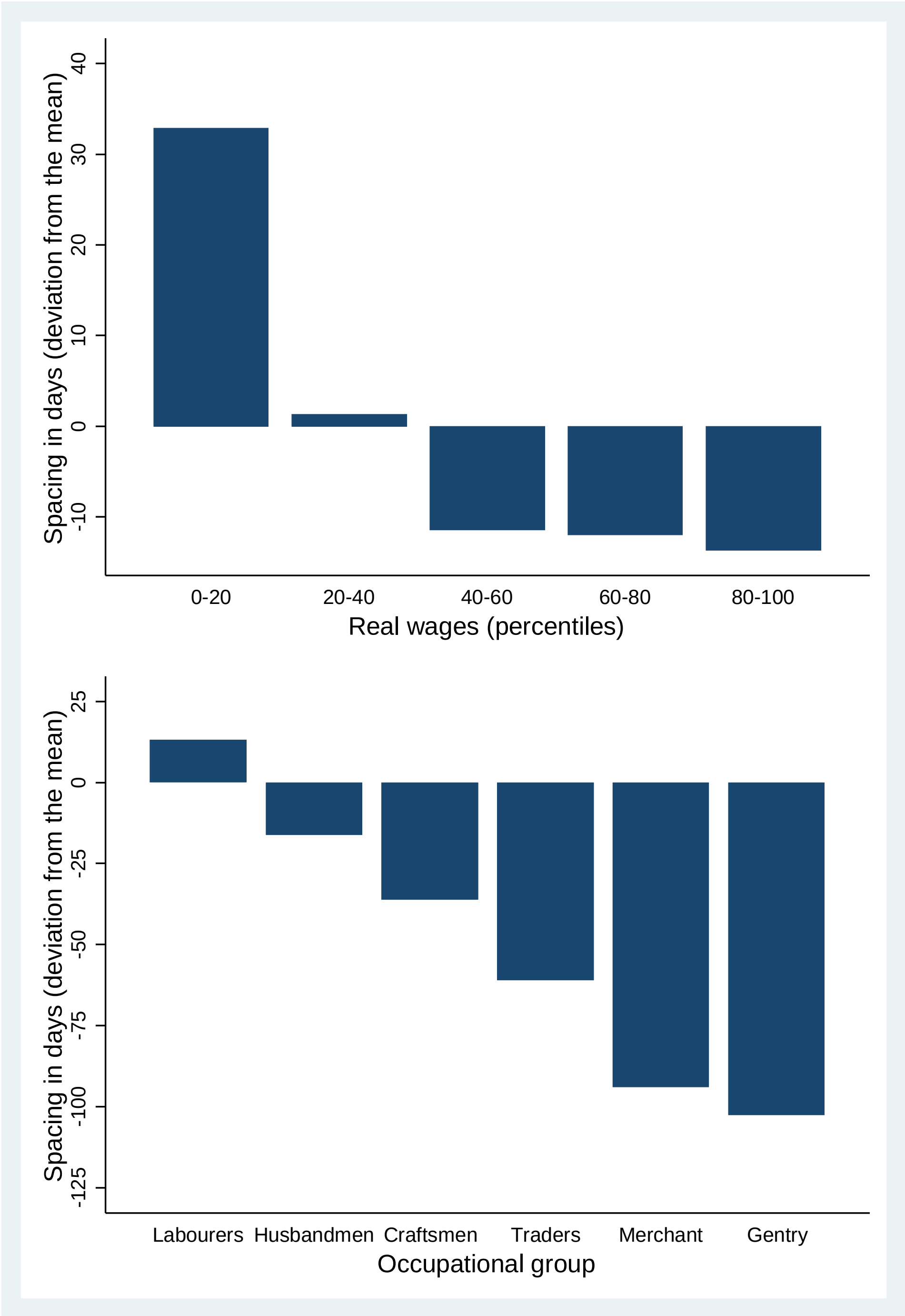


Figure 2: The impact of parity on birth spacing

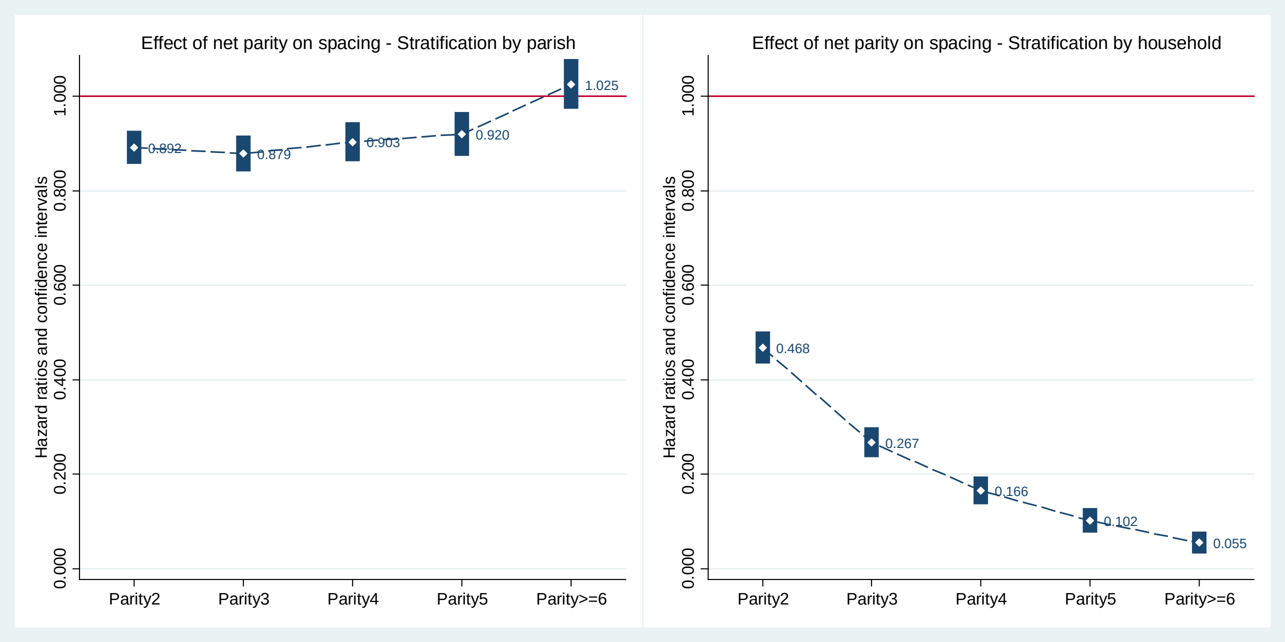


Table 1: Summary statistics

Variable	Mean	Standard deviation	Min	Max	N
Individual					
Spacing (days)	924.5	455.25	281	4368	62223
Mother’s age at beginning of the interval	29.80	5.77	15.28	44.99	62223
Birth order	3.32	2.25	1	19	62223
Child death before next conception	0.16	0.37	0	1	62223
Family specific					
Mother’s age at marriage	24.46	4.81	15.00	44.49	15845
Mother’s age at starting	25.68	4.96	15.28	44.95	15845
Mother’s age at stopping	35.68	6.69	16.79	49.99	15845
Time to first birth (years)	1.23	1.20	-0.08	11.57	15845
Pre-nuptially conceived (dummy)	0.36	0.48	0	1	15845
Labourers	0.19	0.40	0	1	15845
Husbandmen	0.09	0.28	0	1	15845
Craftsmen	0.10	0.31	0	1	15845
Traders	0.04	0.19	0	1	15845
Farmers	0.03	0.17	0	1	15845
Merchants	0.07	0.26	0	1	15845
Gentry	0.01	0.09	0	1	15845
Occupation unknown	0.47	0.50	0	1	15845
Mother’s literacy (dummy)	0.13	0.34	0	1	15845
Mother’s literacy unknown (dummy)	0.60	0.49	0	1	15845
Sibship size	5.02	2.60	2	21	15845

Source: Cambridge Group family reconstitution data.

Table 2: Summary statistics of aggregate variables

Variable	1540-1850 (1)	1540-1600 (2)	1600-1650 (3)	1650-1700 (4)	1700-1750 (5)	1750-1800 (6)	1800-1850 (7)
Real wage	70.83	80.73	60.71	65.74	69.45	67.49	78.57
	(11.70)	(14.16)	(6.19)	(8.61)	(7.60)	(5.54)	(9.44)
Crude death rate	26.93	26.54	25.66	29.36	28.94	27.21	23.64
	(4.47)	(6.23)	(3.92)	(4.07)	(4.11)	(1.69)	(1.65)
Mean temperature	9.04			8.64	9.26	9.07	9.10
	(0.64)			(0.62)	(0.59)	(0.57)	(0.65)

Note: Standard deviations in parenthesis. Source: Real wages from Clark (2007); crude death rates from Wrigley and Schofield (1989); temperatures are from Manley (1953) and are available from 1659.

Table 3: The impact of the real wage on marriage, starting, spacing, and stopping

	Marriage	Starting	Spacing	Stopping
	(1)	(2)	(3)	(4)
Real wage	1.235*	1.252**	1.097***	1.006
	(0.142)	(0.122)	(0.012)	(0.089)
Husbandmen	0.982	0.964	1.082***	1.268***
	(0.030)	(0.030)	(0.019)	(0.112)
Craftsmen	0.923***	0.924***	1.072***	1.167*
	(0.026)	(0.026)	(0.018)	(0.093)
Traders	0.958	0.951	1.169***	1.483***
	(0.041)	(0.034)	(0.029)	(0.164)
Farmers	0.992	0.930*	1.144***	1.068
	(0.046)	(0.037)	(0.027)	(0.139)
Merchants	0.995	0.962	1.187***	1.310**
	(0.041)	(0.037)	(0.024)	(0.151)
Gentry	1.133	1.087	1.181***	1.948***
	(0.098)	(0.077)	(0.056)	(0.448)
Occupation unknown	0.911***	0.882***	1.077***	1.170**
	(0.022)	(0.022)	(0.015)	(0.084)
Mother literacy	0.986	0.990	1.046***	1.297***
	(0.023)	(0.022)	(0.016)	(0.087)
Mother literacy unknown	0.896***	0.738***	0.976	1.207*
	(0.036)	(0.019)	(0.021)	(0.123)
Child death			1.740***	
			(0.023)	
Mother's age			0.899***	0.777***
			(0.007)	(0.035)
Mother's age (squared)			1.001***	1.000
			(0.000)	(0.001)
Time to first birth (years)			0.986***	0.989
			(0.005)	(0.019)
Prenuptially conceived (dummy)			0.970***	1.022
			(0.010)	(0.052)
Last birth interval			0.590***	
			(0.011)	
Birth order			1.062***	
			(0.004)	
Sibshipsize				0.518***
				(0.008)
Age at Marriage FE	No	No	Yes	Yes
Subjects	19845	22622	62223	3795

Note: Cox proportional hazard model with time-varying real wages. Hazard ratios reported. Real wages are standardized with zero mean and unit standard deviation. “Labourers” is the reference group. Mother’s age is measured at the beginning of the interval. Standard errors in parenthesis are clustered by the year of birth. Estimates are stratified by parish and quarter century. * $p< 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Parity-dependent birth spacing

	Parish FE (1)	Family FE (2)	Family FE (3)
Real wage	1.078***	1.128***	1.052**
	(0.008)	(0.014)	(0.024)
Net parity 2	0.892***	0.468***	0.470***
	(0.010)	(0.010)	(0.010)
Net parity 3	0.879***	0.267***	0.269***
	(0.012)	(0.008)	(0.008)
Net parity 4	0.903***	0.166***	0.167***
	(0.013)	(0.007)	(0.007)
Net parity 5	0.920***	0.102***	0.102***
	(0.016)	(0.006)	(0.006)
Net parity 6	1.025	0.055***	0.055***
	(0.019)	(0.004)	(0.004)
Child death	1.733***	2.870***	2.872***
	(0.024)	(0.063)	(0.063)
Last birth interval	0.579***	0.578***	0.576***
	(0.006)	(0.011)	(0.011)
Mother's age	0.934***	1.128***	1.126***
	(0.007)	(0.030)	(0.030)
Mother's age (squared)	1.001***	0.999***	0.999***
	(0.000)	(0.000)	(0.000)
Real wage * net parity 2			1.066***
			(0.025)
Real wage * net parity 3			1.114***
			(0.030)
Real wage * net parity 4			1.097***
			(0.034)
Real wage * net parity 5			1.109***
			(0.038)
Real wage * net parity 6			1.068*
			(0.040)
Quadratic time trend	Yes	Yes	Yes
Subjects	71164	71164	71164

Note: Cox proportional hazard model with time-varying real wages. Hazard ratios reported. Real wages are standardized with zero mean and unit standard deviation. Mother's age is measured at the beginning of the interval. Standard errors in parenthesis are clustered by household. Model in column 1 is stratified by parish. Models in column 2 and 3 are stratified by household. All specifications are stratified also by quarter century. * p< 0.1, ** p < 0.05, *** p < 0.01.

Table 5: The impact of real wages on spacing by sub-period

	1540-1599	1600-1649	1650-1699	1700-1749	1750-1799	1800-1849
	(1)	(2)	(3)	(4)	(5)	(6)
Real wage	1.069	1.207***	1.100***	1.154***	1.096***	1.072**
	(0.078)	(0.047)	(0.033)	(0.030)	(0.024)	(0.029)
Net parity 2	0.336***	0.409***	0.469***	0.481***	0.502***	0.531***
	(0.054)	(0.026)	(0.030)	(0.024)	(0.016)	(0.020)
Net parity 3	0.170***	0.214***	0.244***	0.263***	0.301***	0.334***
	(0.044)	(0.022)	(0.025)	(0.020)	(0.014)	(0.018)
Net parity 4	0.080***	0.123***	0.141***	0.162***	0.197***	0.216***
	(0.028)	(0.017)	(0.020)	(0.017)	(0.012)	(0.016)
Net parity 5	0.056***	0.070***	0.090***	0.096***	0.120***	0.143***
	(0.025)	(0.012)	(0.016)	(0.013)	(0.010)	(0.013)
Net parity 6	0.024***	0.031***	0.045***	0.050***	0.074***	0.076***
	(0.015)	(0.007)	(0.010)	(0.009)	(0.007)	(0.009)
Child death	5.453***	3.924***	3.445***	2.944***	2.418***	2.499***
	(0.971)	(0.271)	(0.238)	(0.147)	(0.083)	(0.107)
Last birth interval	0.698**	0.596***	0.537***	0.536***	0.564***	0.606***
	(0.101)	(0.035)	(0.034)	(0.024)	(0.017)	(0.019)
Mother's age	1.106	1.069	0.991	0.888*	1.110***	1.223***
	(0.212)	(0.086)	(0.090)	(0.057)	(0.045)	(0.056)
Mother's age (squared)	0.995**	1.000	1.000	1.001	0.999	0.998***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Quadratic time trend	Yes	Yes	Yes	Yes	Yes	Yes
Subjects	1357	6793	6184	11708	25340	19731

Note: Cox proportional hazard model with time-varying real wages. Hazard ratios reported. Real wages are standardized with zero mean and unit standard deviation. Mother's age is measured at the beginning of the interval. Standard errors in parenthesis are clustered by household. Estimates are stratified by household. * p< 0.1, ** p < 0.05, *** p < 0.01.

Table 6: Accounting for compositional effects

	W/o parish attrition (1)	Known occupation (2)	Decade FE (3)
Real wage	1.135***	1.129***	1.116***
	(0.017)	(0.019)	(0.013)
Net parity 2	0.465***	0.504***	0.507***
	(0.012)	(0.013)	(0.009)
Net parity 3	0.265***	0.301***	0.303***
	(0.010)	(0.012)	(0.009)
Net parity 4	0.169***	0.197***	0.195***
	(0.009)	(0.011)	(0.007)
Net parity 5	0.106***	0.127***	0.125***
	(0.007)	(0.009)	(0.006)
Net parity 6	0.058***	0.073***	0.068***
	(0.005)	(0.006)	(0.004)
Child death	2.904***	2.638***	2.679***
	(0.077)	(0.074)	(0.055)
Last birth interval	0.586***	0.575***	0.575***
	(0.013)	(0.014)	(0.010)
Mother's age	1.146***	1.136***	1.089***
	(0.037)	(0.039)	(0.026)
Mother's age (squared)	0.999***	0.999***	0.999***
	(0.000)	(0.000)	(0.000)
Decade FE	No	No	Yes
Quadratic time trend	Yes	Yes	Yes
Subjects	47514	40763	71164

Note: Cox proportional hazard model with time-varying real wages. Hazard ratios reported. Real wages are standardized with zero mean and unit standard deviation. Mother’s age is measured at the beginning of the interval. Standard errors in parenthesis are clustered by household. Estimates are stratified by household and quarter century in column 1 and 2; estimates in column 3 are stratified by household.
* p< 0.1, ** p < 0.05, *** p < 0.01.

Table 7: Accounting for migration

	Immigrants (1)	Emigrants (2)	Spacing<3 yrs (3)	Spacing<2.5 yrs (4)	Compl. marr. (5)
Real wage	1.097***	1.075**	1.106***	1.077***	1.144***
	(0.018)	(0.035)	(0.016)	(0.019)	(0.025)
Net parity 2	0.453***	0.499***	0.530***	0.537***	0.501***
	(0.015)	(0.024)	(0.012)	(0.014)	(0.019)
Net parity 3	0.230***	0.298***	0.371***	0.403***	0.292***
	(0.012)	(0.022)	(0.013)	(0.016)	(0.016)
Net parity 4	0.127***	0.187***	0.280***	0.310***	0.188***
	(0.009)	(0.019)	(0.013)	(0.016)	(0.014)
Net parity 5	0.075***	0.125***	0.207***	0.235***	0.120***
	(0.007)	(0.016)	(0.012)	(0.016)	(0.011)
Net parity 6	0.035***	0.063***	0.153***	0.193***	0.073***
	(0.004)	(0.010)	(0.012)	(0.016)	(0.008)
Child death	3.070***	2.860***	3.030***	3.007***	2.746***
	(0.109)	(0.163)	(0.076)	(0.084)	(0.107)
Last birth interval	0.562***	0.595***	0.793***	0.845***	0.505***
	(0.017)	(0.026)	(0.018)	(0.024)	(0.020)
Mother's age		1.178***	1.022	1.043	1.042
		(0.071)	(0.031)	(0.038)	(0.052)
Mother's age (squared)		0.998***	1.000*	1.000	1.000
		(0.001)	(0.000)	(0.000)	(0.000)
Quadratic time trend	Yes	Yes	Yes	Yes	Yes
Subjects	25641	12683	54831	43657	19624

Note: Cox proportional hazard model with time-varying real wages. Hazard ratios reported. Real wages are standardized with zero mean and unit standard deviation. Mother’s age is measured at the beginning of the interval. Standard errors in parenthesis are clustered by household. Estimates are stratified by household and quarter century. * p< 0.1, ** p < 0.05, *** p < 0.01.

Table 8: The impact of the real wage on spacing by occupational group

	Laborers	Husbandmen	Craftsmen	Traders	Farmers	Merchants & Gentry
	(1)	(2)	(3)	(4)	(5)	(6)
Real wage	1.111***	1.126***	1.229***	1.138**	1.071	1.061
	(0.031)	(0.047)	(0.047)	(0.062)	(0.072)	(0.046)
Net parity 2	0.505***	0.498***	0.463***	0.521***	0.492***	0.558***
	(0.023)	(0.035)	(0.027)	(0.045)	(0.055)	(0.037)
Net parity 3	0.311***	0.326***	0.251***	0.280***	0.322***	0.321***
	(0.022)	(0.035)	(0.022)	(0.035)	(0.051)	(0.032)
Net parity 4	0.189***	0.202***	0.180***	0.258***	0.209***	0.200***
	(0.018)	(0.029)	(0.021)	(0.041)	(0.046)	(0.027)
Net parity 5	0.118***	0.122***	0.121***	0.145***	0.146***	0.143***
	(0.014)	(0.023)	(0.018)	(0.030)	(0.037)	(0.023)
Net parity 6	0.068***	0.073***	0.066***	0.085***	0.085***	0.080***
	(0.010)	(0.017)	(0.013)	(0.023)	(0.028)	(0.017)
Child death	2.729***	2.735***	2.589***	2.299***	3.168***	2.579***
	(0.136)	(0.194)	(0.156)	(0.190)	(0.410)	(0.193)
Last birth interval	0.566***	0.613***	0.548***	0.551***	0.472***	0.642***
	(0.023)	(0.039)	(0.030)	(0.046)	(0.052)	(0.040)
Mother's age	1.208***	1.106	1.002	1.198	1.179	1.133
	(0.068)	(0.094)	(0.076)	(0.153)	(0.174)	(0.099)
Mother's age (squared)	0.999**	0.999	0.999	0.999	0.998	1.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Quadratic time trend	Yes	Yes	Yes	Yes	Yes	Yes
Subjects	14648	6493	8046	3310	2355	5911

Note: Cox proportional hazard model with time-varying real wages. Hazard ratios reported. Real wages are standardized with zero mean and unit standard deviation. Mother’s age is measured at the beginning of the interval. Standard errors in parenthesis are clustered by household. Estimates are stratified by household and quarter century. * p< 0.1, ** p < 0.05, *** p < 0.01.

Online Appendix

Alternative wage series

Our baseline analysis uses real wages of farm workers from Clark (2007). Table A1 reports the estimates of our model using alternative real wage series collected by Allen, including the real wages of craftsmen from London and Oxford (columns 2 and 3) as well as the real wages of labourers from London (column 4). For ease of comparison, column 1 reports the baseline estimates using Clark's real wages. Table A1 establishes that our results are robust to the use of different real-wage series: in all our specifications the coefficients for the real wage are not only highly significant but also of similar magnitude.

Table A1: Alternative real wage series

	Baseline	Alternative real wages		
	(1)	(2)	(3)	(4)
Real wage	1.128***			
	(0.014)			
Craftsmen real wage (London)		1.116***		
		(0.021)		
Craftsmen real wage (Oxford)			1.154***	
			(0.031)	
Labourers real wage (London)				1.165***
				(0.026)
Net parity 2	0.468***	0.467***	0.468***	0.468***
	(0.010)	(0.010)	(0.010)	(0.010)
Net parity 3	0.267***	0.266***	0.267***	0.267***
	(0.008)	(0.008)	(0.008)	(0.008)
Net parity 4	0.166***	0.165***	0.165***	0.165***
	(0.007)	(0.007)	(0.007)	(0.007)
Net parity 5	0.102***	0.101***	0.102***	0.102***
	(0.006)	(0.006)	(0.006)	(0.006)
Net parity 6	0.055***	0.055***	0.055***	0.055***
	(0.004)	(0.004)	(0.004)	(0.004)
Control variables	Yes	Yes	Yes	Yes
Subjects	71164	71164	71164	71164

Note: Cox proportional hazard model with time-varying real wages. Hazard ratios reported. The real wage series in columns 2-4 are from Robert Allen and are standardized with zero mean and unit standard deviation. Control variables are: child death, last birth interval, quadratic polynomial of mother age at the beginning of the interval, dummy variables for children born on January 1st, January 11th, and December 25th, and quadratic time trend. Standard errors in parenthesis are clustered by household. Estimates are stratified by household and quarter century. * $p< 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Representativeness of the regression sample

Table A2 reports a cross-tabulation of the parishes and occupational groups included in our sample. There is clearly a large variation across the sampled parishes, both concerning occupational structures and missing information on occupations. In parishes like Austrey and Gainsborough the share of husbands with a recorded occupation was 80%, while Hartland and Methley had no occupational information recorded at all. Also, the practice of recording occupations becomes more common with time.

Table A2: Parishes and occupation

Parish/Class	Labourers	Husbandmen	Craftsmen	Traders	Farmers	Merchants	Gentry	Unknown
Alcester	6%	2%	7%	4%	0%	2%	6%	74%
Aldenham	13%	5%	3%	2%	4%	1%	2%	70%
Ash	26%	5%	6%	3%	9%	1%	1%	48%
Austrey	25%	18%	12%	4%	15%	3%	2%	20%
Banbury	20%	12%	22%	9%	2%	7%	1%	27%
Birstall	2%	3%	4%	1%	2%	17%	1%	69%
Bottesford	19%	12%	9%	6%	9%	2%	0%	43%
Bridford	11%	8%	4%	1%	9%	1%	0%	66%
Colyton	6%	8%	10%	3%	3%	2%	2%	66%
Dawlish	17%	5%	8%	1%	3%	6%	2%	58%
Earsdon	19%	37%	17%	4%	4%	4%	1%	14%
Gainsborough	22%	13%	22%	12%	2%	8%	1%	20%
Gedling	1%	1%	6%	0%	0%	1%	0%	92%
Great Oakley	14%	10%	6%	3%	10%	1%	0%	56%
Hartland	0%	0%	0%	0%	0%	0%	0%	100%
Ipplepen	22%	14%	10%	2%	7%	2%	2%	42%
Lowestoft	9%	14%	7%	3%	1%	9%	1%	56%
March	4%	2%	1%	1%	1%	0%	1%	90%
Methley	0%	0%	0%	0%	0%	0%	0%	100%
Morchard Bishop	18%	7%	6%	2%	7%	1%	0%	59%
Odiham	27%	7%	10%	6%	7%	5%	3%	35%
Reigate	12%	10%	14%	12%	5%	5%	4%	39%
Shepshed	37%	9%	9%	3%	3%	1%	0%	38%
Southill	6%	2%	1%	2%	2%	1%	1%	86%
Terling	32%	8%	12%	7%	3%	3%	2%	33%
Willingham	7%	1%	1%	0%	0%	1%	1%	89%
Total	15%	8%	10%	4%	3%	5%	1%	53%

Source: Cambridge Group family reconstitution data

Figure A1 shows the distribution of births by parish in the original and in the regression sample. The graph indicates that there is no major deviation in the geographical coverage when going from the original to the constrained sample. Here is the list of 26 parishes: 1 Alcester, 2 Aldenham, 3 Ash, 4 Austrey, 5 Banbury, 6 Birstall, 7 Bottesford, 8 Bridford, 9 Colyton, 10 Dawlish, 11 Earsdon, 12 Gainsbro, 13 Gedling, 14 Great Oakley, 15 Hartland, 16 Ipplepen, 17 Lowestoft, 18 March, 19 Methley, 20 Morchard Bishop, 21 Odiham, 22 Reigate, 23 Shepshed, 24 Southill, 25 Terling, 26 Willingham.

Figure A1: The distribution of births by parish in the original sample (blue) and regression sample (red)

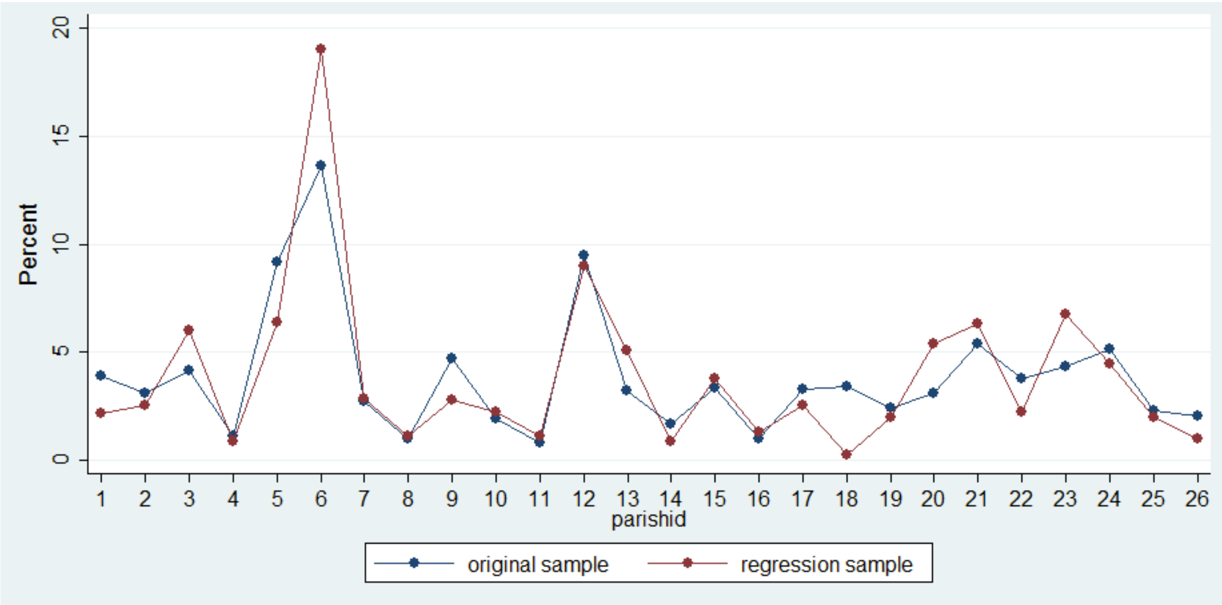
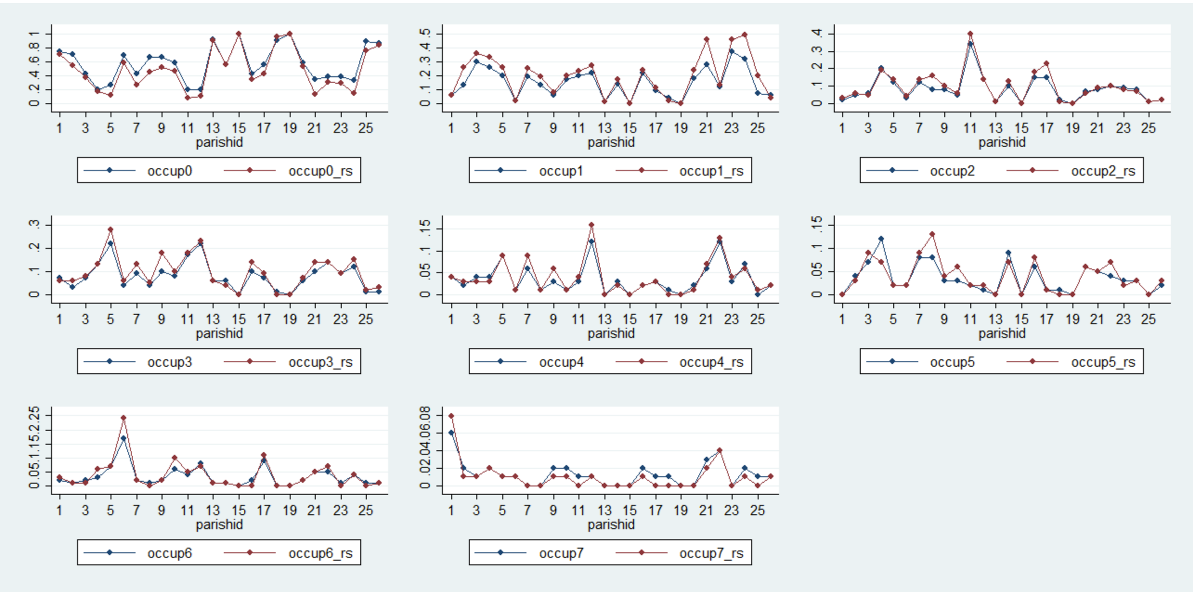


Figure A2 shows how the shares of occupational groups are distributed by parish in the original and in the regression sample. As one can see the occupational patterns of the original sample are pretty well preserved in the regression sample. The legend for the occupational groups is reported here: Occup0 = Unknown occupation; Occup1 = Labourers; Occup2 = Husbandmen; Occup3 = Craftsmen; Occup4 = Traders; Occup5 = Farmers; Occup6 = Merchants; Occup7 = Gentry; “variable_rs” = variable from regression sample.

Figure A2: The shares of each occupational group by parish in the original sample (blue) and in the constrained sample (red)



Additional robustness checks

In the parity-dependent analysis with stratification by household we do not impose the constraint regarding knowledge about the date of marriage, and, therefore, we do not account for pre-nuptial conception. That explains why the number of observations increases from 62,223 (Table 3, column 3) to 71,164 (Table 4). In column 1 of Table A3 we constrain the sample to households with a known marriage date. The impact of the real wage and of parity is unaffected. In column 2 we show estimates restricting the sample to households for which the original source explicitly mentions that it is a first marriage. Also in this case we obtain virtually the same results although we lose about half of the observations.

Table A3: Constraining on known marriage date

	Known marriage date (1)	First marriages (2)
Real wage	1.129***	1.142***
	(0.015)	(0.019)
Net parity 2	0.466***	0.472***
	(0.010)	(0.014)
Net parity 3	0.265***	0.260***
	(0.009)	(0.012)
Net parity 4	0.166***	0.161***
	(0.008)	(0.010)
Net parity 5	0.103***	0.098***
	(0.006)	(0.008)
Net parity 6	0.056***	0.053***
	(0.004)	(0.005)
Control variables	Yes	Yes
Subjects	62223	36000

Note: Cox proportional hazard model with time-varying real wages. Hazard ratios reported. Real wages are standardized with zero mean and unit standard deviation. Control variables are: child death, last birth interval, quadratic polynomial of mother age at the beginning of the interval, dummy variables for children born on January 1st, January 11th, and December 25th, and quadratic time trend. Standard errors in parenthesis are clustered by household. Estimates are stratified by household and quarter century. * p< 0.1, ** p < 0.05, *** p < 0.01.

Table A4 shows estimates using a standard Cox model (without time-varying covariates) including the last interval. In particular, we consider an open interval, *i.e.* we censor the last birth, if we lose track of the mother (column 1). Through censoring we account for the possibility that the mother produced another birth in another parish. As one can see, both the impact of the real wage and of parity does not depend on how we treat the final open birth interval. In Table A5 we control for temperatures and crude death rates to account for potentially confounding biological effect. See discussion in the main text in section 4.

Table A4: Last birth interval (censoring)

	Open interval (1)	Closed interval (2)
Real wage	1.082***	1.064***
	(0.012)	(0.012)
Net parity 2	0.455***	0.479***
	(0.009)	(0.010)
Net parity 3	0.286***	0.281***
	(0.008)	(0.009)
Net parity 4	0.206***	0.179***
	(0.008)	(0.008)
Net parity 5	0.153***	0.113***
	(0.007)	(0.006)
Net parity 6	0.104***	0.064***
	(0.006)	(0.004)
Last birth interval		0.578***
		(0.011)
Control variables	Yes	Yes
Subjects	89163	71073

Note: Cox proportional hazard model. Hazard ratios reported. Real wages are standardized with zero mean and unit standard deviation. Control variables are: child death, quadratic polynomial of mother's age at the beginning of the interval, dummy variables for children born on January 1st, January 11th, and December 25th. Standard errors in parenthesis are clustered by household. Estimates are stratified by household and quarter century. * p< 0.1, ** p < 0.05, *** p < 0.01.

Table A5: Accounting for contemporary temperatures and crude death rates

	(1)	(2)	(3)
Real wage	1.118***	1.105***	1.107***
	(0.015)	(0.015)	(0.015)
Net parity 2	0.480***	0.480***	0.480***
	(0.010)	(0.010)	(0.010)
Net parity 3	0.278***	0.278***	0.278***
	(0.009)	(0.009)	(0.009)
Net parity 4	0.175***	0.174***	0.175***
	(0.008)	(0.008)	(0.008)
Net parity 5	0.108***	0.108***	0.108***
	(0.006)	(0.006)	(0.006)
Net parity 6	0.060***	0.060***	0.060***
	(0.004)	(0.004)	(0.004)
Temperature	0.992		0.992
	(0.011)		(0.011)
Crude death rate		0.991***	0.991***
		(0.003)	(0.003)
Control variables	Yes	Yes	Yes
Subjects	62327	62327	62327

Note: Cox proportional hazard model with time-varying real wages, temperatures, and crude death rates. Hazard ratios reported. Real wages are standardized with zero mean and unit standard deviation. Control variables are: child death, quadratic polynomial of mother's age at the beginning of the interval, dummy variables for children born on January 1st, January 11th, and December 25th, and quadratic time trend. Standard errors in parenthesis are clustered by household. Estimates are stratified by household and quarter century. * p< 0.1, ** p < 0.05, *** p < 0.01.

Test of the proportional-hazard assumption

The formal test based on the Schoenfeld residuals can never reject the proportionality assumption for our variable of interest, the real wage. A high *p-value* indicates a non-rejection of the proportionality assumption. The *p-value* for the real wage based on the specification with stratification by household in Table 4 (column 2) is 0.62. The test for proportionality is more meaningful if we restrict the analysis by sub-period (as in Table 5) to account for secular changes in spacing behaviour which could affect the results of the test. In this case the *p-values* for the real wage are reported in Table A6 below. The impact of the real wage on spacing based on the Cox proportional hazard model satisfies the proportionality assumption.

Table A6: Testing the proportional-hazards assumption

	Period					
	1540-1600 (1)	1600-1650 (2)	1650-1700 (3)	1700-1750 (4)	1750-1800 (5)	1800-1850 (6)
Real wage (*p-value*)	0.300	0.674	0.710	0.674	0.428	0.872

Note: Test of the proportional-hazard assumption based on the Schoenfeld residuals. The test is conducted by subperiod. *P-values* reported.

In Table A7 we report the results of the formal test using the Schoenfeld residuals for the parity-fixed effects and the other control variables. Since the dummies for the parity effects do not vary over time within birth intervals, the test is based on a standard Cox proportional hazard model (without time-varying covariates). To account for secular trends we base the test on the estimates by sub-period as in Table 5. The proportionality assumption cannot be rejected in the vast majority of the cases. Only for the variable child death and from 1700 onwards the proportionality assumption is rejected.

Table A7: Testing the proportionality assumption

	Period					
	1540-1600 (1)	1600-1650 (2)	1650-1700 (3)	1700-1750 (4)	1750-1800 (5)	1800-1850 (6)
Real wage	0.463	0.742	0.964	0.434	0.971	0.583
Net parity 2	0.942	0.893	0.839	0.177	0.408	0.112
Net parity 3	0.769	0.555	0.853	0.245	0.845	0.230
Net parity 4	0.502	0.360	0.926	0.101	0.998	0.276
Net parity 5	0.520	0.460	0.889	0.274	0.915	0.537
Net parity 6	0.745	0.372	0.617	0.199	0.583	0.637
Child death	0.306	0.237	0.288	0.079	0.001	0.002
Last spacing	0.988	0.394	0.331	0.751	0.840	0.924
Mother’s age	0.792	0.608	0.892	0.864	0.400	0.727
Mother’s age sq.	0.941	0.838	0.781	0.606	0.487	0.752

Note: Test of the proportional-hazard assumption based on the Schoenfeld residuals. The test is conducted by subperiod. P-values reported.

The non-rejection of the proportionality assumption for the real wage and the parity effects can also be shown graphically. In Figure A3 we plot the standardized Schoenfeld residuals against time for the variables of interests, namely the real wage and the net-parity fixed effects for the whole period 1540-1850. The line that fits the observations has virtually a zero slope, indicating that the real wage and the parity-fixed effects satisfy the assumption of proportional hazards.

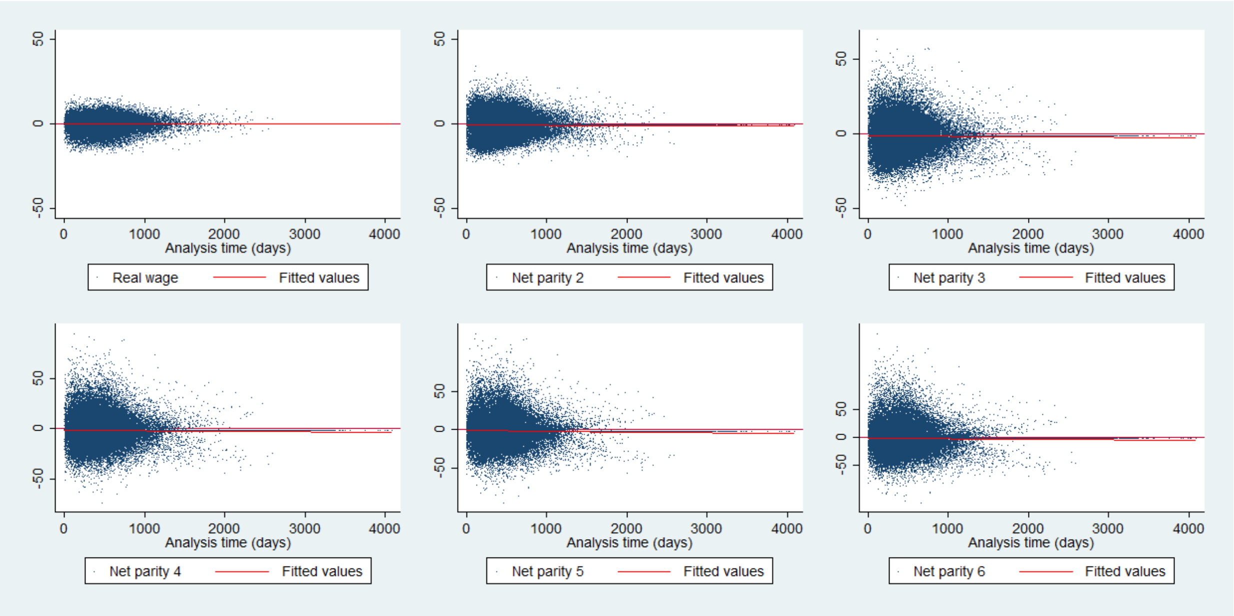


Figure A3: Testing the proportionality assumption