Economic History Working Papers No: 391

Endogenous fertility in pre-transition England

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January 2026

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Draft: January 20, 2026

Was family planning a behavioural innovation that initiated the fertility transition (c. 1870) in England? Evidence from a charitable lottery in London that exogenously affected the timing of marriage suggests that married couples practiced birth control long before fertility rates fell. Birth intervals lengthened in response to earlier marriage, offsetting the higher fertility that would be expected in the absence of deliberate control. This suggests marriage timing and contraceptive effort were substitute strategies to achieve fertility outcomes. Family planning was thus present, but large-scale demographic change required shifts in incentives to mobilize this latent capacity.

JEL: J13; N33;

Keywords: Fertility; demographic transition; economic growth

After centuries of high fertility, the mean number of children born to a woman in England declined from roughly five to two for cohorts born between 1830 and 1900 (Guinnane, 2011). A recent estimate suggests that declining fertility accounts for nearly 70 per cent of the annual growth of GDP per capita in the period 1876-1935 via increased human capital investment, savings, female labour force participation, and changes to the population age structure (Madsen, Islam and Tang, 2020). The fertility transition represents a major inflection in the history of human welfare.

Although fertility in England had fluctuated with changes in food availability and mar-

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riage age (Scott and Duncan, 1999), the fertility transition was marked by a sharp decline in the fertility of married couples (Szreter, 1996; Woods, 2000; Guinnane, 2011). Opinion is divided on whether this represented a behavioural 'innovation' (Carlsson, 1966; Alter, 2019). Many regarded birth control as morally offensive or were simply ignorant (Folbre, 2009; Malthus, 1909). For example, Francis Place (b. 1771), a Malthusian who published one of the earliest pamphlets to advocate family planning in England, had fathered 15 children after an early marriage and claimed to have never conceived of birth control until a colleague brought contraceptive sponges to London from France in 1818 (Cook, 2004; Miles, 1988).

It is fitting that Place identified France as the origin of this knowledge transfer, as the French fertility transition inspired an influential theory of fertility decline as a primarily cultural phenomenon (Doepke et al., 2023). Observing that fertility decline began in revolutionary France, not with the industrial revolution in England, and that the timing of fertility decline was only weakly related to economic growth in Europe, the Princeton European Fertility Project concluded that cultural norms and ignorance caused high fertility. The 'natural fertility' hypothesis holds that pre-transition fertility lay 'beyond the calculus of conscious choice' (Coale, 1973; Coale and Treadway, 1986). This hypothesis is regularly evoked to characterise fertility in the past, particularly by scholars whose work is informed by evolutionary theory (e.g. Colejo-Durán et al., 2024; Dillon et al., 2024; McFadden, 2023; Clark, 2007). A survey of the field of evolutionary demography, for example, characterised the view 'that human fertility behaviour is driven by conscious decision-making' as 'a very big assumption' (Sear, 2015).

Recent quasi-experimental evidence supports the natural fertility hypothesis directly. Clark, Cummins and Curtis (2020) found that twin births increased average family size by one, suggesting no compensating changes to fertility and leading the study's authors to conclude that it is possible to regard 'all the variation in family size as exogenous' in pre-transition families (Clark, Cummins and Curtis, 2020). However, while monozygotic twins occur more or less randomly, the same biological processes that make dizygotic twinning more likely also increase overall fecundity (Tong and Short, 1998). It is not possible to distinguish these twin types in historical data. Additionally, twin pregnancies are at greater risk of miscarriage and death, leading to survivorship bias, particularly in developing countries where maternal health is likely poor (Guo and Grummer-Strawn, 1993; Marco-Gracia, 2024). While this study is convincing, without measuring underlying health, fecundity, and zygosity, it is not possible to completely rule out confounding factors.[1]

Further evidence comes from a wide and varied literature documenting the contribution of norms and information to fertility transitions. For instance, secularization and social change roughly coinciding with the French Revolution (c. 1789) were associated with lower fertility in parts of France that had not experienced significant industrialisation (Blanc, 2024; Blanc and Wacziarg, 2020; de la Croix and Perrin, 2018; Perrin, 2022). While evidence of a direct cultural transmission between France and England is speculative (Clark and Cummins, 2015), migrants both carried and transmitted reproductive behaviours during the historical fertility transition (Beach and Hanlon, 2023; Melki et al., 2024). Places more culturally similar to France adopted fertility control sooner, suggesting possible information diffusion along cultural lines (Spolaore and Wacziarg, 2022). For instance, fertility declined faster in French-speaking Wallonia than in Flemish Belgium, and spatial proximity to francophones accelerated Flemish demographic change (Lesthaeghe, 1977; Van Bavel, 2004*b*). Cultural norms and information diffusion also played a role in later fertility transitions in, for example, Germany (Braun, Franke and Öztürk, 2025), North America, South Africa (Beach and Hanlon, 2023), Latin America (Moorthy, Iyer and Moyano, 2025), and Bangladesh (Munshi and Myaux, 2006), and likely contributed to the recent emergence of below-replacement fertility rates in the U.S. (Bailey, 2025; Goldin, 2021).

On the other hand, many studies have linked longer birth spacing to temporary income shocks, suggesting possible fertility control (van Bavel, 2004*a*; Bengtsson and Dribe, 2006; Bengtsson and Quaranta, 2025; Péter Őri and Levente Pakot, 2025; Marco-Gracia, 2019; Cinnirella, Klemp and Weisdorf, 2017). While such unexpected shocks can help

[1] I thank James Fenske for pointing this out.

identify causal effects, they do not directly address the question of endogenous lifetime fertility, which remains of central theoretical interest, although some studies have found longer-term effects as well (Cilliers, Mariotti and Martins, 2024; Sandström and Vikström, 2015). Critics argue the methodology for detecting spacing (Cox proportional hazards) in historical populations may be particularly prone to specification errors and truncation bias (Clark and Cummins, 2019; Cinnirella, Klemp and Weisdorf, 2019; Alter, 2019). Furthermore, because income may also affect birth intervals through health, fecundity, and breastfeeding duration (Oris, Mazzoni and Ramiro-Fariñas, 2024; McFadden, 2023), which are rarely observable in historical datasets, these findings are also vulnerable to omitted variable bias (e.g. Klesment and Lust, 2025; Willführ and Perez, 2025).

This study also considers spacing as a birth-control technique but makes two novel methodological contributions. First, I study an historical lottery to bypass the problem of biological unobservables via treatment randomization. Raine's charity operated a semi-annual lottery for a small group of unmarried women in London between 1758 and 1872; the winner received £100 (equivalent to roughly £71,040-£226,000 today) on condition that she marry within six months.[2] Second, although other studies demonstrate birth control via birth spacing, I demonstrate that birth spacing also responded to marriage timing—the primary mechanism affecting lifetime fertility before the fertility transition (Woods, 2000). Differences of opinion also exist on whether marriage timing responded endogenously to planned fertility or was exogenously affected by labour market conditions(Sarti, 2008; Horrell, Humphries and Weisdorf, 2020; Szreter and Garrett, 2000; Foreman-Peck, 2011). I find that lottery winners married earlier, but this had no impact on their fertility due to compensating change to birth spacing. Because lottery winning is plausibly uncorrelated with unobservable biological determinants of birth spacing, this suggests deliberate control. More broadly, the findings imply that marriage timing and birth spacing may have been substitutes for achieving fertility outcomes, thus linking

[2]Conversion refers to labour earnings relative to 1870 and 1760 for the lower and upper bounds respectively (Measuringworth.com, 2024).

existing research on family planning via birth intervals to more general modes of fertility behaviour (e.g. marriage timing).

This study contributes to three literatures. First, unified growth models that assume endogenous fertility before the demographic transition can cite these findings in support of this assumption (e.g. Galor and Weil, 2000; Le Fur and Wasmer, 2025; Cervellati, Meyerheim and Sunde, 2023; Foreman-Peck, 2011). However, the presence of direct fertility control suggests that the fertility–income relationship may have been less constrained in pre-transition populations than in some versions of the theory, such as Galor and Moav (2002), which model a positive fertility–income gradient arising from the relaxation of a subsistence constraint.[3] Second, the family is an important economic institution, and this study may contribute to a reading of the historical family as rational and the fertility transition as an example of induced institutional change (North, 1990; Gay, Gobbi and Goñi, 2026; Anderson and Bidner, 2023; Lundberg and Pollak, 2007; Pollak, 1985). Third, the findings add to the literature on family planning policy in developing economies by showing that birth control was feasible even in the information– and contraceptive technology–poor setting of eighteenth-century London (de Silva and Tenreyro, 2020; Bongaarts, 2020; Cavalcanti, Kocharkov and Santos, 2021; Miller and Babiarz, 2016).

Section one describes the lottery and its historical context. Section two develops a parsimonious model to interpret the effect of the lottery on fertility. Section three describes the dataset, its representativeness, and imputations used in its construction. Sections four and five describe the identification strategy and present results. Section six concludes. This study provides novel empirical evidence of endogenous pre-transition fertility in the first population to experience an industrial revolution and sustained economic growth.

I. Historical Setting

Henry Raine (1679-1738) was a brewer who made a considerable fortune quenching the thirst of sailors in East London's dockland (Lincoln, 2018; Cockburn, King and Mc-

[3]This complements Davenport's (2019) research on infant mortality in the same historical setting, which finds a positive or flat wealth–infant mortality gradient.

Donnell, 1969). Simultaneously, he was an active and devout member of the Church of England. Raine apparently resolved the contradiction between his pious spirit and his profane livelihood through charitable acts, including a school established in 1719 that admitted boys and girls. Raine's was part of an evangelical wave of charity school foundations sweeping over London in the early eighteenth century in reaction to perceived irreligion among the poor (Rose, 1991; Jones, 1964). In addition to learning to read bible verses, however, girls who attended Raine's school had a chance to win the 'marriage portion'.

Although his precise motives are unclear, it seems that Raine introduced the marriage portion out of a similar concern for maintaining church membership, as his will required that both bride and groom were members of the Church of England (Rose, 1991). Per Raine's instructions, girls had to go through a number of steps before they could be eligible for the prize (Raine, 1748). First, to be admitted to the school, six local residents needed to vouch for their character and respectability. At the same time, the school's trustees had a mandate to admit children of poor families in the parish of St. George in the East who could not otherwise afford school fees. These combined constraints meant school children likely came from the households of local artisans or sailors who adopted middle-class respectability without the standard of living to match (Rose, 1991). For instance, Ann Cater's admission record in 1822 noted simply, 'Mother dead, father left with 7 small children'.[4] Next, girls were selected from the lower school to enter the upper 'asylum', where they were taught skills relevant to eventual employment in domestic service, which the school arranged (Cockburn, King and McDonnell, 1969). Finally, subject to a positive character reference from their employers, these women could step forward to claim the marriage portion after their twenty-second birthdays.

These conditions were not extraordinary for the time period despite their apparent strictness. For example, the Church of England, the state church, accounted for 49 per cent of all church attendances in 1851 and was the single-largest denomination, making a large pool of potential lottery participants (Snell and Ell, 2004). At a time of high

[4] TLA ACC/1811/8/11/1.

TABLE 1—SAMPLE REPRESENTATIVENESS

HISCLASS	Odds Ratio	S.E.
12 – Unskilled farm workers	1.05	(1.29)
11 – Unskilled workers	0.81	(0.19)
10 – Lower-skilled farm workers	1.05	(0.92)
9 – Lower-skilled workers	1.88	(0.63)
8 – Farmers and fishermen	-	-
7 – Medium-skilled workers	1.56	(0.37)
6 – Foremen	-	-
5 – Lower clerical and sales personnel (low skill)	0.29	(0.32)
4 – Lower clerical and sales personnel (medium skill)	0.36	(0.18)
3 – Lower managers	0.29	(0.32)
2 – Higher professionals	2.11	(2.99)
1 – Higher managers	-	-

Note: Estimated from a series of logistic regressions of occupational class on a dummy variable indicating lottery participation. The comparison group was a random sample of fathers' occupations from the St. George in the East parish registers for 1730-1840. The sampling scheme took the first occupation on every fifth page from 1730-1812 and every twentieth page after 1812, when register entries became lengthier. This scheme resulted in approximately three random occupations per year. These were classed using the HISCLASS schema. Missing values indicate the absence of that class in at least one of the comparison groups.
Source: The London Archive (2010*a*)

dependency ratios, the average family experienced life-cycle poverty when young children were present in the household, making also many potential candidates for charity (Horrell, Humphries and Weisdorf, 2022). Further, domestic service was a common experience for young girls. It was the largest occupational group in the nineteenth and possibly eighteenth centuries, employing as many as 40 per cent of all women in 1851 (Schwarz, 1999; You, 2024). There is no sign that these girls were employed in exceptionally 'elite' households. The school briefly recorded girls' wages in service between 1780 and 1790. Their median yearly earnings were £3, far below the £7.35 median wage earned by other domestic servants in London at the same time.[5] Further, character references were common in the labour market for domestic servants (Kaiser, 2025). Thus while it was unusual for girls to have their lives so thoroughly shaped by a charitable institution, the shape those lives took was not.

Table 1 compares the occupations of fathers of girls who participated in the lottery to a random sample of fathers' occupations taken from the baptismal registers of St. George

[5]TLA ACC/1811/8/14; London average from data in Kaiser (2025), kindly shared by the author.

in the East between the years 1730-1840, roughly corresponding to the birth cohorts that could have been eligible for the portion. The table reports odds ratios and standard errors estimated from a series of logistic regressions of occupational class (HISCLASS) on a dummy variable indicating lottery participation. In general, lottery participants were more likely to come from semi-skilled and artisan families and slightly less likely to come from unskilled families. However, they were also much less likely to come from professional families and those of retailers and wholesalers trading on their own account. This agrees with the intuition above that lottery participants came from modest backgrounds and were objects of charity more due to bad luck than destitution.

Twice yearly, up to six women could stand for the marriage portion. They drew sealed tickets from a tin canister, one of which was marked. The candidates simultaneously opened their tickets, revealing the winner to the assembled public. The winner was then allowed six months to find a suitable groom-to-be, whose character was also evaluated by the trustees. Eligible grooms needed to be resident in St. George in the East or two neighbouring parishes. The couple was then paid £100 on their wedding day. According to one trustee, most women had suitors at the time of the draw, but this was not always the case (Jones, 1875). If she was unable to find a groom after six months, the winning candidate received only £5 and became ineligible for future draws. Women who stood for the prize but drew blank tickets were allowed to re-enter subsequent draws 'so that every every one of them may happen, at one Time or other, to be elected, and entituled to such Sum of One Hundred Pounds for a Marriage Portion' (Raine, 1748). After the draw, £5 were expended on a wedding feast.

These terms were set out in Raine's will in 1736. However, because Raine had only endowed the fund with £4,000 in 3 per cent gilts, it was left to accumulate until it yielded the required £210 per year. This apparently occurred in 1758, when the first marriage lottery is recorded in surviving archival registers (Cockburn, King and McDonnell, 1969). This delay created a large pool of eligible women at the beginning of the lottery, making the early draws more competitive. Indeed, between 1758 and 1782, more than the maximum allowable six women often sought to participate in the lottery. Trustees carefully made note of the candidates, who became eligible for subsequent lotteries in the order they had signed up. After the initial glut cleared, lottery participation varied, apparently in line with the fortunes of the school, with a normal lottery consisting of around three participants (figure A1).

The last competitive lottery occurred in 1872, after which time the number of applicants declined, often leaving only one candidate for each lottery. This decline may have related to wider policy changes occurring at the time. The Endowed Schools Act of 1869 created a commission with wide powers to intervene in the administration of secondary schools, and trustees felt their rights threatened. To try to head off forced reform, the boys school voluntarily dropped elementary teaching and developed its secondary-level curriculum for fee-paying students, with the charity providing scholarships to examination candidates. One trustee of the charity expressed the urgency of reforming the marriage portion as well, and the girls' asylum appears to have admitted far fewer girls after 1869 (Jones, 1875). The asylum ultimately closed in 1883 (Cockburn, King and McDonnell, 1969). During its functional lifetime, then, the charity executed approximately 228 marriage lotteries according to the system described in Raine's will.

St. George in the East was a large docklands parish closely linked to the old Port of London. It had grown up in the seventeenth century, and although port expansion continued rapidly to the east, its population grew at a relatively modest pace over the eighteenth century (Marriott, 2011). There appears to have been nothing extraordinary about the parish's fertility rate. Figure 1 graphs the crude fertility rate (CFR) from 1850, when local civil registration data become available, to 1910. The CFR in St. George in the East is slightly higher than the average for England and Wales. However, because mariners would possibly have been away at sea on census night, this may be a partial artifact of underestimation in the denominator. Equally, there is no indication that the parish was an early participant in the fertility transition. While the national CFR begins to decline in the 1870s, there is no sign of decline in St. George until possibly 1905.

Finally, I note that innovation in contraceptive technology does not explain the fertility transition. While physical contraceptives did exist, they were not marketed or consumed

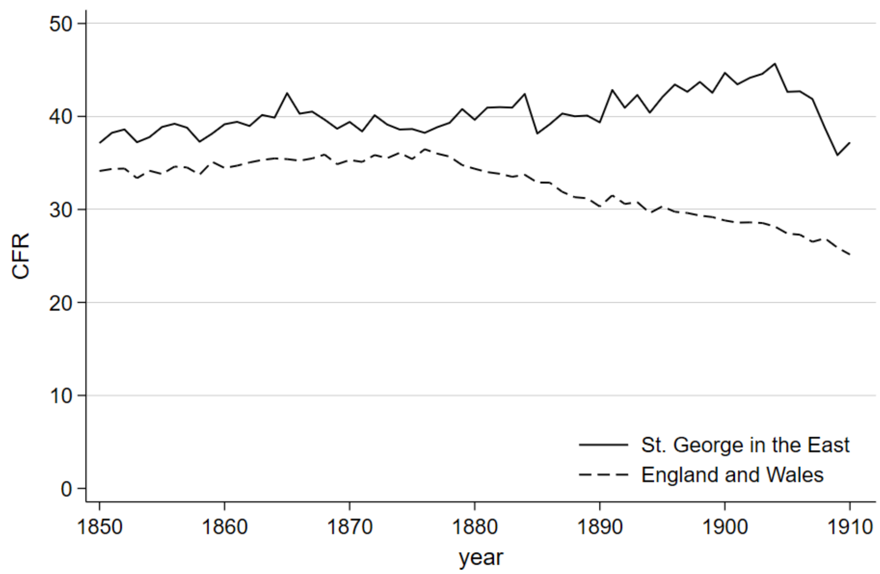

FIGURE 1. CRUDE FERTILITY RATE IN ST. GEORGE IN THE EAST, 1850-1910

Note: The crude fertility rate (CFR) is the number of births per 1,000 population. Births are reported yearly by the registrar general, while population is linearly interpolated between decadal censuses.

in significant numbers until the early twentieth century (Jones, 2020; Youssef, 1993). Changes to the frequency of sex and its distribution within marriage are sufficient to account for variation in fertility in this period (Szreter, 1996). For example, Stanford and Dunson (2007) show that a reduction in the frequency of intercourse from twice per week to once per week can increase the expected duration of the birth interval by 61 per cent if intercourse occurs randomly throughout the menstrual cycle.[6] Some couples practiced *coitus interruptus*, but women might also adopt strategies to reduce the frequency of intercourse without requiring male buy-in, such as 'staying up late at night working, sharing beds with children, complaining of pains, or ... enlisting the doctor's support', as Cook (2004) documents. Such methods were equally viable before and after the fertility transition.

Taken together, these features underscore that the institutional setting was distinctive,

[6]Own calculations from authors' model.

but the demographic context was not. What matters for the analysis is whether behaviour within this demographic regime was truly 'natural'. The next section develops a simple framework to evaluate Raine's lottery against this hypothesis.

II. Theory

In this pre-transition setting, one key unresolved issue is the relationship between marriage timing and fertility (Szreter and Garrett, 2000). Theoretical approaches to this question emphasise how marriage timing relates to the 'gains to trade' when spouses divide household tasks (Keeley, 1977). These are themselves affected by labour market structure, particularly married women's access to careers, the marginal utility from children and child quality, contraceptive technology, and institutional factors affecting, for example, divorce (Greenwood, Guner and Vandenbroucke, 2017; Doepke et al., 2023). However, in Hruschka and Burger's (2016) study of 200 high-fertility populations, the key stylised fact to emerge is that pre-transition fertility closely resembles a Poisson process, implying a relatively constant risk of childbirth over marriage. This may be due to the nature of contraception, discussed above, or because factors that motivate clustering births were largely absent. For example, in nineteenth-century England, returns to experience for women were negligible in both textile factories and agriculture (Burnette, 2006; Boot, 1995). On the other hand, a constant risk of childbirth would also be expected under the natural fertility hypothesis. The remainder of this section develops a parsimonious model to help distinguish natural from endogenous fertility empirically in the context of Raine's lottery.

Consider a woman choosing when to marry. She chooses $t \in \{0,1,2,\dots,T\}$, where t is the number of unmarried periods and T is her adult lifetime. This implies $T-t$ is the duration of marriage. For simplicity, assume no extramarital births so that her duration of marriage determines her time available for reproduction.

Take first the natural fertility case where women do not control fertility within marriage. Although in reality, fertility declines naturally with age, this is left out of the model for simplicity (Henry, 1961). Births therefore arrive stochastically within marriage at a constant rate, λ. The expected number of births in a marriage of duration $T-t$ is therefore

$$\mathrm{E}(N) = (T-t)\lambda. \tag{1}$$

Delaying marriage by one period will reduce expected family size by λ.

Under endogenous fertility, the woman may exert contraceptive effort e to reduce the probability of a birth, $\partial\lambda/\partial e < 0$. Although she may choose the level, I assume that she does not vary her contraceptive effort over time within marriage. This greatly simplifies the problem and reflects the stylized facts discussed above.[7] Assuming no discounting, lifetime expected utility will therefore be

$$\mathrm{E}(U) = ts - (T-t)c(e) + \mathrm{E}(v[N, \lambda(e)]) \tag{2}$$

where s is the per-period utility flow from being single and $c(e)$ is the utility cost of contraceptive effort in marriage, increasing in contraceptive effort ($\partial c_m/\partial e > 0$). Lifetime utility increases in completed family size ($\partial v/\partial N > 0$; $\partial v/\partial^2 N \leq 0$), and wider birth spacing (lower λ, i.e. $\partial v/\partial\lambda < 0$) to reflect a quantity-quality trade-off.[8] This feature of the model is intended to capture the association between longer birth spacing and better infant health via reduced breastfeeding-pregnancy overlap, for example (Dadi, 2015; Conde-Aguedelo et al., 2012).[9] Although t and N are discrete, I treat them as continuous and differentiable for tractability.

The woman chooses when to marry and her level of contraceptive effort, which determines her expected family size. To simplify the expectation operator in (2), I approxi-

[7] Dynamic fertility models in which contraceptive effort is allowed to vary across multiple periods often have no closed-form solution without making strong assumptions about the functional form of the utility function. See Arroyo and Zhang (1997).

[8] The separation of utility flows from lifetime utility derived from final family size is similar to Dioikitopoulos and Varvarigos (2023).

[9] Short birth intervals also impact infant health via maternal health. This parameter may therefore reflect preferences over maternal health. For modelling purposes, this parameter serves primarily to ensure that the model does not always collapse into a corner solution with no spacing, and either assumption serves this purpose.

mate the function with a second-order Taylor series evaluated at $E(N)$

$$\begin{aligned}
\mathrm{E}(U) &= ts - (T-t)c(e) \\
&\quad + \mathrm{E}\left[v[\mathrm{E}(N), \lambda(e)] + v_1[N - \mathrm{E}(N)] + \frac{1}{2} v_{11} [N - \mathrm{E}(N)]^2 \right] \\
\mathrm{E}(U) &= ts - (T-t)c(e) \\
&\quad + v[\mathrm{E}(N), \lambda(e)] + \frac{1}{2} v_{11} \mathrm{Var}(N),
\end{aligned}$$

where subscripts denote partial derivatives with respect to the relevant argument. It is then possible to rewrite the maximization problem in terms of (1)

$$\max_{t,e} U(t,e) = ts - (T-t)c(e) + v[(T-t)\lambda(e), \lambda(e)] + \frac{1}{2} v_{11} (T-t)\lambda(e). \tag{3}$$

The first-order conditions are:

$$\lambda(v_1 + \frac{1}{2} v_{11}) = s + c(e) \tag{4}$$

$$v_2 \lambda_1 = (T-t)(c_1 - \lambda_1 [v_1 + \frac{1}{2} v_{11}]). \tag{5}$$

The first, (4), says that the instantaneous utility of another period of marriage in terms of children must be equal to that of another period of singledom and the disutility of contraceptive effort. The second, (5), says that the advantage of contraceptive effort on spacing must offset the disadvantages of fewer children and the disutility of contraception in marriage. The second derivative adjusts the marginal utility of children for risk-aversion. I assume that risk-aversion is modest so that

$$v_1 + \frac{1}{2} v_{11} > 0.$$

Rearranging (5) and substituting in (1) provides an expression for the optimal expected number of children

$$\text{E}^*(N) = \lambda \left[\frac{v_2 \lambda_1}{c_1 - \lambda_1 (v_1 + \frac{1}{2} v_{11})} \right]. \tag{6}$$

Note that the term in brackets will be positive, given the assumptions. Thus the number of children born is increasing in the rate of births, as in the exogenous case. However, in this endogenous case, the relationship will be attenuated if the direct cost of contraceptive effort and its indirect cost via fewer children are large relative to the marginal benefit via spacing. Further, there is no direct relationship to the duration of marriage. Rather, from (4), the endogenous instantaneous birth rate, λ, will rise if single utility or the cost of reproductive effort rise. Insofar as these variables are also positively related to marriage age, there should nonetheless be a negative relationship between late marriage and endogenous fertility.

So far, I derived the optimum when the woman chooses both t and e. To analyze Raine's lottery, I now consider the case where t is fixed exogenously and effort adjusts endogenously. From (1), expected fertility in this scenario is

$$\text{E}(N \mid t) = (T - t)\lambda \left[e(t) \right]$$

$$\frac{\partial \text{E}(N \mid t)}{\partial t} = -\lambda + (T - t)\lambda_1 \frac{\partial e}{\partial t} \tag{7}$$

The first term is the mechanical effect of losing a period; the second term captures behavioral adjustment. Relative to the exogenous case, the impact of a shock to marriage timing will depend on the sign of $\partial e / \partial t$. If later marriage reduces contraceptive effort ($\partial e / \partial t < 0$), i.e., if marriage timing and contraceptive effort are substitute strategies, then the fertility penalty of delay is smaller than under natural fertility.

III. Dataset construction and validation

To study fertility outcomes among lottery participants, I hand-link information from manuscripts created during the administration of Raine's charity to data on life events contained in online genealogical databases (e.g. Ancestry.com and Findmypast.co.uk). Relative to automated linking, following a recent critical survey (Bailey et al., 2020), hand linking would be expected to produce links of the highest possible quality and minimize bias, and it is viable given the relatively small size of the dataset.

Two primary sources of information on the lottery are held in The London Archives: lot books and trustees' receipts (The London Archive, 1736). The lot books were created during the marriage portion ceremony, while the receipts record the marriage-portion transaction and provide a useful cross-reference for the lot books.[10] From these records, I note for each lottery the date of the draw, a list of candidates, the number of times each candidate participated, the winner, the name of her groom, the groom's occupation, and the date of their marriage. I then link each candidate to her school admission register, which notes her date of birth and often her father's name and occupation.

For lottery participants, I thus possess relatively rich pre-treatment information, but the quality of post-treatment information varies. The manuscripts provide no information about the marriages of women who either drew the prize but did not marry within six months or who dropped out of the lottery without winning. This introduces a correlation between treatment and data quality whose implications for causal identification are left to the next section. Nonetheless, I search for these women in the collection of all London parish marriage registers digitized by The London Archive to obtain their spouse's name and their date of marriage (The London Archive, 2010*a*,*b*). I restrict search to the three years immediately following the candidate's departure from the lottery and only accept links if the bride's name is unique within that window.

Next, I look for evidence of childbearing in the London baptismal records, which are also digitized by The London Archives. I restrict my search to the 30-year period after marriage. Here, I link on spouse-parent names and rely on rule-of-thumb tie breaking. Where two sets of parents share identical names, I favour those whose children were born shortly after the wedding date. Further, because mother and father must both have lived in or near St. George's parish to be eligible for the lottery, I favour matches living in

[10]The two-hundred-plus-year-old documents were occasionally illegible due to wear and tear.

East London. Finally, I use the father's occupation if this seemed to provide identifying information. That is, I interpret skilled occupations that likely required an apprenticeship as providing reliable information about identity because these are more likely time invariant. Where two potential matches have occupations that are closely related or in the same industry, I do not rely on this information to break ties. Where none of these rules of thumb provide grounds for disambiguating a match, I make no match and drop the couple from the sample.

Once I have identified the first child's baptismal record and birth date, another baptism usually follows within roughly two years. Where there is a sequence of baptisms of this kind, and none of the family's other details change, I am confident that I am identifying siblings. If there is a break in the sequence which starts up again roughly two years later in a nearby parish and the father's occupation has not changed, I also record these new children as siblings. When possible, I cross-reference against the decennial census, which began in 1841. In some cases, a child is only identified in the census and not in the baptismal records. I also include such children and subtract their reported age in years from the census date to reconstruct their approximate birth date. It was not always possible to unambiguously distinguish the children of one family from another, and such cases were dropped from the sample.

The resulting dataset is imperfect, but it represents an earnest effort at accuracy given the recognised challenges of linking across historical sources in London (Davenport, 2016).[11] The manuscript sources provide a unique means of verifying the accuracy of my reconstitution method. In 1851, the trustees wrote to all marriage portion recipients of the last decade and preserved some of their correspondence. If the trustees were able to locate the couple, they noted a residential address and the occupation of the groom. I have reproduced all identifying information from these manuscript sources in Table 2 compared to the information I obtained from the 1851 census and the baptismal record of the child born nearest to 1851. This is a blind validation exercise, as I did not draw on

[11] An annotated dataset, including direct URLs to the sources, is available on request to be checked by interested readers.

TABLE 2—VALIDATION OF RECONSTITUTION METHOD, 1841-1851

Manuscript		Census		Baptism		Type I	Type II
5 New Street, Horsley-down	Fellmonger	—	—	New Street, Horsleydown	Fellmonger		
15 Tottenham Place, Tottenham Court Road	Baker	15 Tottenham Place	Baker	Upper North Place, St Pancras	Baker		
Red Lion Passage	Pastry cook and confectioner	—	—	—	—		X
Unknown	—	—	—	Old Montague Street, Whitechapel	Bricklayer[1]	X	
2 Morpeth Street, Bethnal Green	Bell founder	Morpeth Street, Bethnal Green	Bell founder	Bethnal Green	Bell founder		
Unknown	Optical brass founder[2]	—	Brass finisher	—	Brass turner		
St. Katharine Docks	Fireman	—	—	—	—		X
10 Norfolk Street, Commercial Road	Gun Maker	New Norfolk Street, Stepney	Gun polisher	7 [illegible] Cornwall St	Gun maker		
Unknown	Shoemaker	—	—	—	—		
4 Little Abbey Street, Bermondsey	Silk weaver	4 Stephen Street, Bermondsey	Weaver	—	—		
Unknown	Shoemaker	27 St. James Terrace	Shoemaker journeyman	11 Tarling Street, Christ Church	Bootmaker		
3 Hope Place, Bermondsey	Warehouse man	3 Hope Place, Bermondsey	Porter	New Church Street, Bermondsey	Porter		
5 Curriers Hall Court, London Wall	Porter	9 Three Herring Ct, Cripplegate	Porter	Marshall St., Gripplegate	Porter		
Unknown	Shipmate	12 Prospect Place	Mariner	12 Prospect Place, St George in the East	Mariner		
Went abroad	Painter	—	—	—	—		
Unknown	Shoemaker	19 Lombard St, Chelsea	Shoemaker	16 Lombard, Chelsea	Cordwainer		
Unknown	Cooper	Denmark Street, St George in the East	Cooper	7 Denmark Street, St George in the East	Cooper		
19 Catherine St, St. George East	Oil & Colourman	—	—	27 Fenton Street, St George in the East	Colourman		

Note: 1: Groom was described as bricklayer at time of his marriage. 2: Mother of bride wrote to school to say her daughter had died, and husband was reported as a widower in 1851 census.
Source: See text.

this source in constructing the dataset.

In table 2, type I errors refer to cases where I have made a link that does not match the trustees' correspondence. However, because high-frequency, short-range mobility was common in London at this time (Davenport, 2016), I allow for some geographical mobility and do not flag as an error a change of address to another house in roughly the same neighborhood. In only one case, roughly 5 per cent of the sample, have I attributed lottery-winning to a family living in East London that the trustees did not themselves identify. In this case, however, the father had the same occupation, bricklayer, as the groom on his wedding night. It is possible this is no error and the trustees simply lost touch with this family. Type II errors refer to cases where the trustees have located the family, but I have been unable to do so. There are two such cases, representing 11 per cent of the sample. In other cases where I have been unable to make a link the trustees have also been unable to locate the family, indicating possible emigration from London, death and remarriage, or some other complication. I regard such cases as true negatives, not errors. For comparison, hand-linked US census samples have a type-I error rate of at least 4 per cent, while common automated linking methods have a type-I error rate ranging between 15 and 37 per cent and a type-II error rate between 63 and 79 per cent (Bailey et al., 2020).

However, comparing the dataset against other benchmarks indicates it is likely the reconstitution missed some births. The challenge of reconstituting families in London is well-known, stemming from a high prevalence of short-distance migration and the large number of urban parishes, each of which kept vital records of varying quality (Davenport, 2016). If a child died before their baptism, their birth would also tend to go unrecorded, and London's infant mortality rate was high (Wrigley et al., 1997). Table 3 presents summary statistics from the Raine's charity dataset and two comparable historical datasets from England. The first is Davenport's (2016) reconstitution for St. Martin in the Fields, another large urban parish west of the City of London. The second is Wrigley et al.'s (1997) reconstitution of 26 rural parishes and towns. The data quality is likely higher in the rural sample because the underlying population is less mobile, but it would miss

TABLE 3—SUMMARY STATISTICS AND COMPARISONS

	mean	sd	min	max
Start Age	23.7	2.15	20.1	29.6
Marriage Age	25.8	2.63	20.8	31.7
Total Bapt.	3.38	2.03	1	9
Final Birth Age	34.7	5.64	23.9	47.3
First Child	29.0	29.7	-4.64	171.2
Middle Child	29.8	16.1	6.51	128.3
Last Child	39.7	21.6	0	114.4
St. Martin in the Fields, 1752-1812				
Total Bapt.	4.43	2.43	2	15
Middle Birth Interval	25.52	12.29	7	127
Last Birth Interval	29.58	14.34	9	119
National sample, 1750-99				
Female Marriage age	24.0			
Completed family size	5.61			
Age at Final Birth	39.3			
First Birth Interval	15.0			
Middle Birth Interval	29.4			
Last Birth Interval	41.0			
After imputation				
Total Bapt.	3.61	1.90	1	9
Final Birth Age	34.4	5.22	23.9	47.3
First Birth Interval	22.7	17.1	1.97	85.6
Middle Birth Interval	29.8	12.1	10.6	70.0
Last Birth Interval	37.6	15.3	0	85.6

Note: Birth intervals in the national sample are only reported for the whole period 1580-1837.
Source: Wrigley and Schofield (1983); Wrigley et al. (1997). Davenport kindly shared data underlying her (2016) article.

urban-specific demographic traits if such exist.

The observed number of births in the Raine's lottery sample is much lower than either the rural or the urban sample. However, because Davenport's methodology depends on observing two subsequent births, singleton households are excluded (Davenport, 2016). The comparable restricted mean ($n > 1$) in Raine's charity is 4.23. This is closer to Davenport's estimate, but still far from the national rural sample. Fertility may have simply been lower in urban settings, possibly due to elevated disease prevalence (Szreter and Siena, 2021).

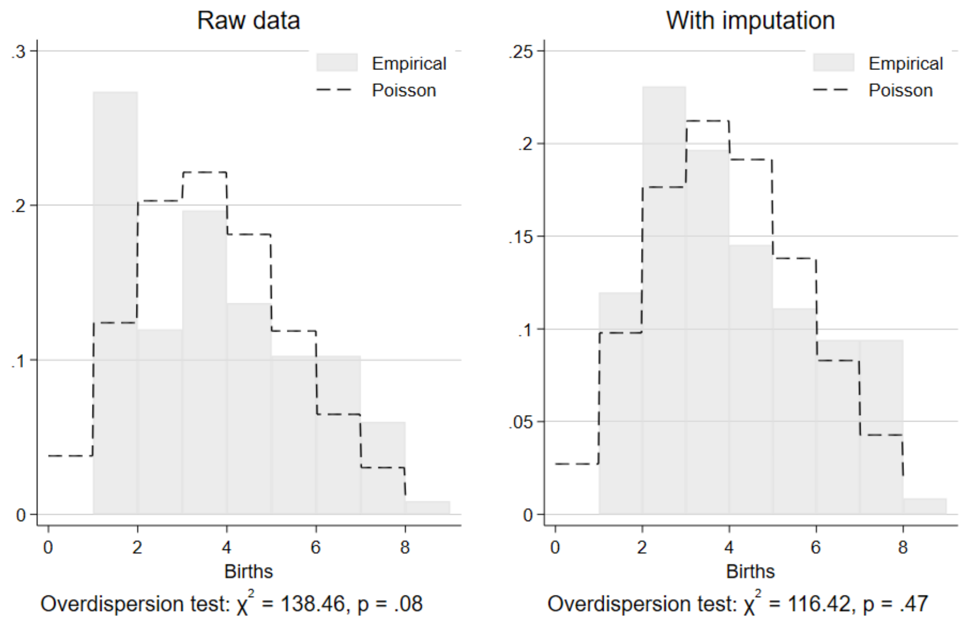

FIGURE 2. EMPIRICAL DISTRIBUTION OF NUMBER OF BIRTHS VERSUS POISSON DISTRIBUTION

The pattern of birth intervals in the Raine's dataset is more problematic. In historical fertility data, it is common to find relatively short first birth intervals, longer middle intervals due to breastfeeding, and longest final intervals due to declining fecundity (Wrigley et al., 1997), but this pattern is absent here. This difference is apparently driven by extreme outliers. Further, the relatively low mean age at final birth in the Raine's dataset suggests some birth histories may be prematurely truncated. Finally, as discussed above, the number of births should roughly follow a Poisson process, but an overdispersion test rejects this hypothesis (figure 2; $p = 0.08$). I provide evidence that these anomalies are more plausibly attributable to missing births than to atypical demographic behaviour, as they vanish after imputing births in long birth intervals.

To identify long birth intervals, I assume births follow a Poisson process and model inter-birth intervals using the exponential distribution, allowing a nine-month offset following multiparous births to account for gestation. I condition the exponential distribution on age at marriage and marriage duration, using means reported by Wrigley et al. (1997) for their rural sample (Table A2). If the cumulative probability of an observed interval exceeds 0.95 under this distribution (roughly seven years), I treat the interval as inconsistent with the assumed birth process and impute a missing birth at the midpoint. This threshold reflects a probabilistic criterion for identifying gaps in the birth record, rather than a formal hypothesis test.

Despite the inherent limitations of historical record linkage, imputation considerably improves the fit between the data and demographic priors. For example, the lower part of table 3 re-calculates the birth intervals using the imputed data, which now follow the expected increasing pattern by birth order. Further, an overdispersion test now fails to reject the hypothesis that the empirical distribution of family size is Poisson-distributed (figure 2; $p = 0.47$). However, this imputation strategy cannot address truncated birth histories so the age at final birth remains the same. I address this below by defining my dependent variable to minimize the potential impact of truncation. The main analysis is performed using the dataset with imputed births; the appendix reports results for the observed-only births. Results are qualitatively unchanged, but coefficients show signs of minor attenuation bias consistent with greater measurement error without imputation.[12]

IV. Identification

Identification first exploits random assignment in the lottery. Each draw was fair and public, so conditional on entering, the probability of winning was independent of potential outcomes. A potential concern is that participants could drop out after losing and so had partial control over their treatment. To address this, I restrict attention to the first round of the lottery. At this point, no prior draw had occurred, so the outcome is orthogonal to unobserved traits that might otherwise influence both continued participation and fertility. Winning in the first round provides an instrument for earlier marriage because it shifts the timing of marriage without directly affecting fertility, except through marriage timing. This satisfies the exclusion restriction under the assumption that the lottery

[12] As a zero coefficient leads to rejection of the natural fertility hypothesis, the imputed births make the main analysis the more conservative of the two.

outcome does not influence fertility through any other channel.

Second, I control for a participant's age at first entry into the lottery. This variable captures pre-treatment preferences over marriage timing before the draw outcome is known, in a manner roughly analogous to fixed effects in a panel setting. For example, as discussed above, many participants already had suitors when they signed up for the lottery (Jones, 1875). Including initial participation age therefore provides a powerful control for pre-treatment characteristics and preferences that could otherwise confound the relationship between marriage age and fertility.

This conditioning strategy mitigates a shortcoming of the first strategy. As discussed, the manuscript records do not contain information on the spouses of lottery participants who did not win the prize. Treatment thus reduces data-quality, which in turn lowers the probability of making a successful record link because less information is known about these individuals. Although the selection mechanism therefore operates mainly through the amount of information coded in a name (e.g. name uniqueness), which is plausibly orthogonal to marriage and fertility, this will nonetheless introduce bias if selection also depends on unobservables related to marriage age and fertility (Hughes et al., 2019). Insofar as initial lottery age captures many traits and preferences affecting marriage age, this bias should be minimized.

Nevertheless, selection remains a possible concern. To address this directly, I implement inverse probability weighting (IPW) based on estimated linkage probabilities (Hughes et al., 2019). I focus on marriage because this was the primary channel through which lottery outcome affected selection. I estimate these linkage probabilities from a logit regression predicting whether a lottery participant's subsequent marriage was found in the archive with dummy variables for each lottery outcome and initial lottery age (details in appendix). By reweighting observations according to their estimated probability of successful linkage, IPW reduces bias from differential linkage success and restores representativeness. This approach assumes data are missing at random (MAR) conditional on observed covariates and that the selection mechanism is well-defined (Little and Rubin, 2002). Because linkage success is primarily driven by name distinctiveness and record completeness—factors plausibly unrelated to fertility conditional on age and lottery outcome—these assumptions appear reasonable.

Taken together, these strategies are designed to isolate exogenous variation in marriage timing and correct for potential selection, allowing for credible identification of the effect of marriage age on fertility. In addition, following recent critiques that highlight how methods based on birth intervals can be prone to model misspecification, I adopt a transparent and parsimonious estimation strategy to avoid similar pitfalls (Clark and Cummins, 2019; Alter, 2019).

My preferred model is

$$y_{iT} = \beta_0 + \beta_1 x_i + \mathbf{A}\gamma + \varepsilon_i, \tag{8}$$

where y_{iT} is the number of children born to woman i before she reaches age T, x_i marriage age, and $\mathbf{A}\gamma$ is a vector of controls including initial lottery age. This approach keeps all post-treatment variables on the left-hand-side and avoids issues of serial dependency that may arise when estimating individual birth intervals. Further, because T is fixed, β_1 is naturally interpreted via the birth interval. Later marriage will lead to lower y_{iT} in the absence of a compensating change to birth spacing. A negative coefficient suggests birth spacing does not fully compensate for variation in marriage timing.

Finally, because the lottery incented earlier marriage by paying a £100 bounty, it is possible that this payment is the cause of fertility behaviour, not marriage timing. This would violate the exclusion restriction. To address this concern, I re-run the analysis in a subsample of only those who received the bounty after either one or two periods. While selection into treatment remains a possibility, this approach should minimize differences in unobservables affecting selection because the two groups differ on the smallest possible time margin.[13]

[13] The identifying assumptions in this case are similar to regression discontinuity.

TABLE 4—MAIN RESULTS

	(1) OLS: T=30	(2) IV: T=30	(3) OLS: T=35	(4) IV: T=35	(5) OLS: T=30	(6) IV: T=30	(7) OLS: T=35	(8) IV: T=35
Marriage Age	-0.29	-0.18	-0.12	0.019	-0.33	-0.19	-0.12	-0.042
	(0.05)	(0.14)	(0.07)	(0.22)	(0.04)	(0.15)	(0.05)	(0.21)
Start Age	0.019	-0.080	0.0053	-0.11	0.055	-0.055	0.0072	-0.059
	(0.06)	(0.13)	(0.09)	(0.20)	(0.05)	(0.12)	(0.08)	(0.18)
Constant	8.57	7.98	5.75	5.04	8.55	7.64	5.75	5.21
	(0.99)	(1.21)	(1.53)	(1.85)	(0.82)	(1.36)	(1.47)	(2.07)
R2	0.41	0.38	0.048	0.015	0.44	0.39	0.053	0.040
N	103	103	103	103	103	103	103	103
First-stage F		13.52		13.52		29.08		29.08
IPW					✓	✓	✓	✓

Note: Standard errors in parentheses.

V. Results

Table 4 presents the main estimates. Specification (1) is an OLS regression with the number of children born before age 30 (y_{i30}) as the outcome. The negative and statistically significant coefficient on marriage age ($\beta = -0.29$, SE = 0.05) suggests fertility declines with later marriage. Specification (2) is the IV estimate, which is attentuated ($\beta = -0.18$, SE = 0.14) and not statistically different from zero. Specification (3) increments T by five years and returns to OLS. The coefficient is further attenuated ($\beta = -0.12$, SE = 0.07), which is difficult to reconcile with the natural fertility hypothesis. One interpretation is that couples began to alter birth spacing later in the life cycle, but there may be other confounding issues in OLS. Specification (4) is the IV estimate, which is further attenuated and not different from zero ($\beta = 0.019$, SE = 0.22). Specifications (5)-(8) repeat the earlier exercise using IPW and do not qualitatively differ from the earlier estimates.

Some of the OLS estimates suggest fertility declined with later marriage. As discussed in the theory section, this pattern is consistent with both natural fertility and endogenous fertility. Under natural fertility, this is due to a reduction in the period over which a constant 'risk' of childbirth operates, whereas underlying preferences affecting both marriage timing and reproductive effort are responsible under endogenous fertility. The IV estimates, which should be unrelated to underlying preferences, are therefore more

TABLE 5—RESULTS FOR SUBSAMPLE IN RECEIPT OF MARRIAGE PORTION

	(1) OLS: T=30	(2) IV: T=30	(3) OLS: T=35	(4) IV: T=35	(5) OLS: T=30	(6) IV: T=30	(7) OLS: T=35	(8) IV: T=35
Marriage Age	-0.069	0.49	0.19	1.48	-0.068	0.49	0.19	1.48
	(0.20)	(0.45)	(0.32)	(0.78)	(0.19)	(0.45)	(0.30)	(0.77)
Start Age	-0.24	-0.87	-0.39	-1.82	-0.24	-0.87	-0.39	-1.83
	(0.23)	(0.51)	(0.37)	(0.88)	(0.21)	(0.49)	(0.33)	(0.84)
Constant	9.34	10.3	7.87	10.1	9.34	10.3	7.87	10.1
	(1.57)	(1.80)	(2.49)	(3.09)	(1.14)	(1.57)	(1.99)	(3.25)
R2	0.38	0.26	0.078	.	0.38	0.26	0.078	.
N	44	44	44	44	44	44	44	44
First-stage F		11.43		11.43		27.69		27.69
IPW					✓	✓	✓	✓

Note: Standard errors in parentheses.

revealing.

In every case, the IV coefficient was attenuated relative to its OLS pair, and in no IV specification was it possible to reject a null hypothesis of zero at conventional levels. In other words, the evidence offers little basis on which to reject endogenous fertility in favour of natural fertility. On the other hand, given that the average birth interval in the wider population at these ages and birth parities is around 22.65 months (table A2), delaying marriage by 12 months (1 year) should reduce fertility by around $\beta = -0.53$ under the natural fertility hypothesis. It is possible to reject this hypothesis in all specifications at conventional significance levels. The first-stage regressions exceed conventional thresholds for weak instruments ($F > 10$). Since weak instruments tend to bias estimates toward OLS, any remaining concern would, if anything, reinforce our findings.

Table 5 presents estimates from a subsample who received the marriage portion after one or two lotteries. The comparison is therefore between two groups who won, married, and were paid £100 within approximately six months of each other. Unobservable differences between these two groups driven by impatience and lottery exit should therefore be minimized, and their fertility behaviour should reflect the pure effect of marginal differences in marriage timing. As before, there is no basis for rejecting endogenous fertility in favour of natural fertility. On the other hand, natural fertility ($\beta = -0.53$) is rejected at conventional levels. Specifications (5)-(8) use IPW to correct for sample selection and do not qualitatively differ from unweighted results. In the appendix, jackknife standard errors for table 5 are presented to address concerns that individual observations might be driving the results given the small sample, and the findings appear robust.

VI. Conclusion

These results suggest contraceptive effort was decreasing in marriage age and contradict the predictions of the natural fertility hypothesis. This paper's claims are based primarily on a novel and possibly unique natural experiment that addresses persistent endogeneity issues in this literature. While this setting permits a convincingly unbiased estimate of the effect of marriage delay on fertility, concerns about external validity remain. I have argued that lottery participants were not otherwise exceptional or unusual, suggesting it may be possible to apply these findings beyond the sample. On the other hand, internal and external validity can be complements in the collective research process (Deaton and Cartwright, 2017). Studies of larger samples that argue for birth spacing or marriage timing as a mechanism of birth control in pre-transition populations may appear more convincing in light of these results (van Bavel, 2004*a*; Bengtsson and Dribe, 2006; Foreman-Peck, 2011; Cinnirella, Klemp and Weisdorf, 2017).

More broadly, the evidence of pre-transition birth control is consistent with endogenous fertility in the long run and supports an 'adaptation' interpretation of fertility decline. Couples were always capable of exercising some control over their fertility, given the right incentives. Economists since Becker have looked at the relative price of child quantity and quality for these changing incentives. However, given this paper's evidence that marriage timing and contraceptive effort were substitutes, future work may productively analyse changes in marriage markets as a complementary factor in demographic change.

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APPENDIX

A1. Additional details

INVERSE PROBABILITY WEIGHTS

Lottery outcome affects the probability of selection because the grooms of women who receive the marriage portion are known with certainty, while marriage records of those who leave early or turn down the portion must be found using only the bride's name. This leads to a higher probability of successfully linking recipients of the portion, as shown in table A1. Inverse probability weights (IPW) give observations that were unlikely to have been observed more weight in the regression to restore the representativeness of the sample. In practice, IPWs are often normalises to mitigate variance inflation due to large weights. I adopt this approach, weighting each observation by

$$\hat{w}_i = \frac{w_i}{\sum_{j=1}^{n} w_j} n$$

where $w_i = 1/p_i$, p_i is the estimated probability of selection, and n is sample size.

However, I do not observe the fertility of couples who have no children, either because such couples had no children or because I was unable to locate their baptismal records. If missing children were due only to lost baptism records, missingness might well be random conditional on having successfully linked the parents' marriage records. In practice, however, I am less likely to find the children of lottery candidates who won but did not immediately marry ('unmarried winners') even after they eventually married (table A1).

As de la Croix, Schneider and Weisdorf (2019) document, lifelong celibacy and marital childlessness was relatively common in England circa 1600-1840. I assume such women are overrepresented among unmarried winners, who participated in the lottery without the immediate intention to marry, and that lottery outcome did not affect whether these women eventually had children.[14]

[14] This assumption aligns with the standard no-defiers condition in instrumental variables. I treat these women as 'never-takers'.

TABLE A1—IPW SELECTION EQUATIONS

	(1) Logit: Marriage only	(2) Logit: Marriage and children
Married Winner	3.06	-0.074
	(0.54)	(0.34)
Unmarried Winner	-1.54	-2.44
	(0.52)	(0.71)
Start Age	0.0082	-0.018
	(0.03)	(0.02)
Constant	0.089	0.57
	(0.81)	(0.62)
Pseudo-R2	0.40	0.071
N	227	222

Note: Standard errors in parentheses. Dropped out of lottery before winning is ommitted category.

This implies that selection based on child missingness is exogenous to the instrument and the IPWs used in the main analysis remain appropriate. Nonetheless, I re-estimate the results using a broader definition of selection that includes both marriage and child missingness (appendix tables A6 and A7). The results are qualitatively unchanged.

Additional figures and tables

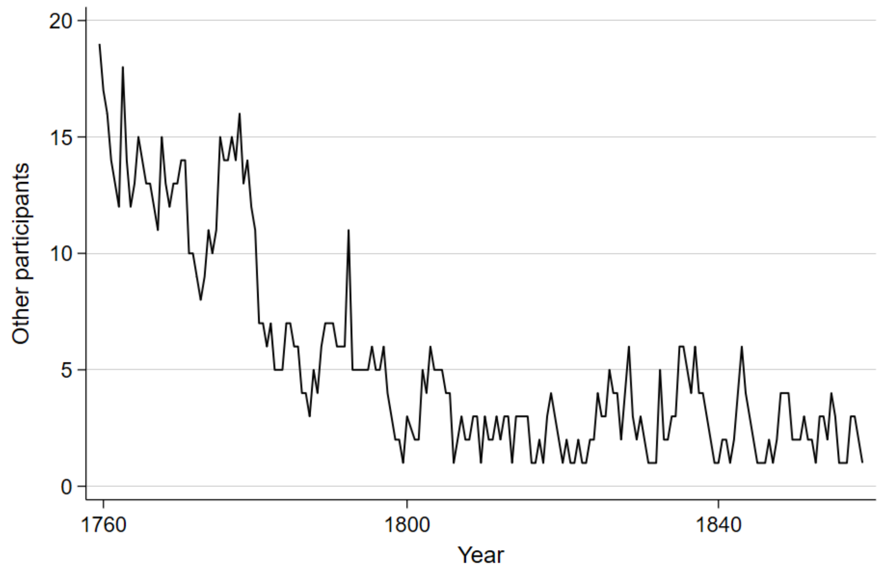

FIGURE A1. NUMBER OF PARTICIPANTS IN RAINE'S MARRIAGE PORTION CEREMONY, 1758-1872

The figure shows the number of participants by year.

TABLE A2—BIRTH INTERVALS BY WIFE'S AGE AT MARRIAGE AND DURATION OF MARRIAGE

	Duration of marriage (years)				
Wife's age at marriage	0-4	5-9	10-4	15-9	20-4
20-4	22.02	32.38	32.95	34.29	36.76
25-9	22.65	33.34	35.98	36.27	21.03
30+	22.83	33.84	38.54	33.50	–

Source: Wrigley et al. (1997)

Prior information on birth intervals from Wrigley et al. (1997)

A2. Robustness

TABLE A3—MAIN ANALYSIS WITHOUT IMPUTING MISSING BIRTHS

	(1) OLS: T=30	(2) IV: T=30	(3) OLS: T=35	(4) IV: T=35	(5) OLS: T=30	(6) IV: T=30	(7) OLS: T=35	(8) IV: T=35
Marriage Age	-0.25	-0.17	-0.090	-0.047	-0.29	-0.18	-0.085	-0.10
	(0.06)	(0.16)	(0.08)	(0.23)	(0.05)	(0.18)	(0.06)	(0.23)
Start Age	0.025	-0.044	0.032	-0.0054	0.062	-0.024	0.024	0.038
	(0.07)	(0.15)	(0.10)	(0.21)	(0.06)	(0.15)	(0.08)	(0.19)
Constant	7.31	6.90	4.16	3.93	7.29	6.59	4.14	4.26
	(1.15)	(1.39)	(1.69)	(2.02)	(1.05)	(1.57)	(1.62)	(2.24)
R2	0.27	0.26	0.016	0.013	0.30	0.27	0.017	0.016
N	103	103	103	103	103	103	103	103
First-stage F		13.52		13.52		28.88		28.88
IPW					✓	✓	✓	✓

Note: Standard errors in parentheses.

This regression excludes missing births imputed from long intervals and repeats the main analysis (table 4). Estimated coefficients tend to be closer to zero, most likely due to attenuation bias from missing births, and are qualitatively comparable to the main results.

TABLE A4—SUB-SAMPLE ANALYSIS WITHOUT IMPUTING MISSING BIRTHS

	(1) OLS: T=30	(2) IV: T=30	(3) OLS: T=35	(4) IV: T=35	(5) OLS: T=30	(6) IV: T=30	(7) OLS: T=35	(8) IV: T=35
Marriage Age	-0.060	0.47	0.20	1.32	-0.060	0.47	0.20	1.32
	(0.23)	(0.51)	(0.35)	(0.80)	(0.20)	(0.51)	(0.31)	(0.79)
Start Age	-0.21	-0.80	-0.36	-1.61	-0.21	-0.80	-0.36	-1.61
	(0.27)	(0.58)	(0.40)	(0.90)	(0.22)	(0.56)	(0.33)	(0.87)
Constant	8.37	9.31	6.86	8.83	8.37	9.31	6.86	8.83
	(1.83)	(2.04)	(2.70)	(3.17)	(1.33)	(1.75)	(2.16)	(3.26)
R2	0.26	0.17	0.047	.	0.26	0.17	0.047	.
N	44	44	44	44	44	44	44	44
First-stage F		11.43		11.43		11.28		11.28
IPW					✓	✓	✓	✓

Note: Standard errors in parentheses.

This regression excludes missing births imputed from long intervals and repeats the sub-sample analysis (table 5). The sample therefore consists of only those who won on their first attempt and those who won on their second attempt. Estimated coefficients tend to be closer to zero, most likely due to attenuation bias from missing births, and are qualitatively comparable to the results in the main paper.

TABLE A5—SUB-SAMPLE RESULTS WITH JACKKNIFE STANDARD ERRORS

	(1) OLS: T=30	(2) IV: T=30	(3) OLS: T=35	(4) IV: T=35	(5) OLS: T=30	(6) IV: T=30	(7) OLS: T=35	(8) IV: T=35
Marriage Age	-0.069	0.49	0.19	1.48	-0.069	0.49	0.19	1.48
	(0.25)	(0.51)	(0.39)	(0.89)	(0.25)	(0.50)	(0.39)	(0.89)
Start Age	-0.24	-0.87	-0.39	-1.82	-0.24	-0.87	-0.39	-1.83
	(0.26)	(0.54)	(0.40)	(0.95)	(0.26)	(0.54)	(0.40)	(0.94)
Constant	9.34	10.3	7.87	10.1	9.34	10.3	7.87	10.1
	(1.17)	(1.79)	(2.07)	(3.87)	(1.17)	(1.79)	(2.07)	(3.85)
R2	0.38	0.26	0.078	.	0.38	0.26	0.078	.
N	44	44	44	44	44	44	44	44
First-stage F		10.3		10.3		10.3		10.3
IPW					✓	✓	✓	✓

Note: Jackknife standard errors in parentheses.

This regression repeats the sub-sample analysis with jackknife standard errors due to small sample size. The sample therefore consists of only those who won on their first attempt and those who won on their second attempt. Natural fertility ($\beta = -0.40$) is rejected with at least 90-percent confidence in all IV regressions.

TABLE A6—MAIN ANALYSIS WITH ALTERNATIVE IPW WEIGHTS

	(1) OLS: T=30	(2) IV: T=30	(3) OLS: T=35	(4) IV: T=35	(5) OLS: T=30	(6) IV: T=30	(7) OLS: T=35	(8) IV: T=35
Marriage Age	-0.29	-0.18	-0.12	0.019	-0.36	-0.15	-0.15	-0.031
	(0.05)	(0.14)	(0.07)	(0.22)	(0.05)	(0.18)	(0.06)	(0.23)
Start Age	0.019	-0.080	0.0053	-0.11	0.099	-0.063	0.049	-0.040
	(0.06)	(0.13)	(0.09)	(0.20)	(0.07)	(0.13)	(0.08)	(0.17)
Constant	8.57	7.98	5.75	5.04	8.46	6.75	5.43	4.49
	(0.99)	(1.21)	(1.53)	(1.85)	(0.87)	(2.02)	(1.53)	(2.56)
R2	0.41	0.38	0.048	0.015	0.46	0.33	0.060	0.029
N	103	103	103	103	102	102	102	102
First-stage F		13.52		13.52		15.78		15.78
IPW					✓	✓	✓	✓

Note: Standard errors in parentheses.

This regression repeats the main analysis using alternative IPW weights.

TABLE A7—SUB-SAMPLE ANALYSIS WITH ALTERNATIVE IPW WEIGHTS

	(1) OLS: T=30	(2) IV: T=30	(3) OLS: T=35	(4) IV: T=35	(5) OLS: T=30	(6) IV: T=30	(7) OLS: T=35	(8) IV: T=35
Marriage Age	-0.069	0.49	0.19	1.48	-0.077	0.47	0.17	1.44
	(0.20)	(0.45)	(0.32)	(0.78)	(0.19)	(0.45)	(0.30)	(0.77)
Start Age	-0.24	-0.87	-0.39	-1.82	-0.23	-0.85	-0.37	-1.78
	(0.23)	(0.51)	(0.37)	(0.88)	(0.20)	(0.48)	(0.32)	(0.83)
Constant	9.34	10.3	7.87	10.1	9.35	10.3	7.83	10.0
	(1.57)	(1.80)	(2.49)	(3.09)	(1.13)	(1.54)	(1.96)	(3.18)
R2	0.38	0.26	0.078	.	0.39	0.27	0.079	.
N	44	44	44	44	44	44	44	44
First-stage F		11.43		11.43		11.06		11.06
IPW					✓	✓	✓	✓

Note: Standard errors in parentheses. Not qualitatively different.

This regression repeats the sub-sample analysis using alternative IPW weights.