Was there a family economics before 1870?: marital birth control in a long-running natural experiment, London, c. 1760-1870

By LOUIS HENDERSON*

Draft: April 10, 2025

EARLY DRAFT — SUBJECT TO CHANGE

In late nineteenth-century England, marital fertility rapidly declined. Demographers debate whether this was a behavioral 'innovation' or a rational 'adaptation'. This paper argues for the latter hypothesis with evidence from a London charity (1758–1870) exhibiting the existence of within-marriage fertility choice before the demographic transition. Unmarried women eligible for a £100 wedding lottery could re-enter subsequent draws if unsuccessful, delaying their marriage. Without fertility choice, later marriage would lead to fewer births. However, delayed marriage was compensated through shorter birth intervals, resulting in no difference in completed family size by lottery outcome—evidence that pre-transition married couples controlled births.

JEL: J13; N33;

Keywords: Fertility; demographic transition; economic growth

After centuries of relative stability, the mean number of children born to a woman in England (cohort fertility rate) declined from approximately 5 in the generation born in the 1830s to approximately 2 in the generation born in the 1900s (Guinnane, 2011).

[1]LSE, Department of Economic History, Sardinia House, London, l.henderson2@lse.ac.uk; University of Oxford, History Faculty, George Street. Thanks to participants at the University of Tübingen Winter School, ASSA 2025, and the LSE Historical Economic Demography Workshop for helpful comments and to Taylor Jaworski and Romola Davenport for reading and commenting on an early draft. Additionally, I thank Romola Davenport for sharing her data.

Although total fertility had fluctuated in line with changes in food availability and age of marriage in the past (Scott and Duncan, 1999), the nineteenth-century 'demographic transition' was marked by a decline in the fertility of married couples ostensibly practicing forms of birth control (Szreter, 1996). Opinion is divided on whether this represented a behavioural 'innovation' that was culturally proscribed and possibly inconceivable to earlier couples or an 'adaptation' to changing incentives (Carlsson, 1966). This question has been the subject of recent debate (Cinnirella, Klemp and Weisdorf, 2017; Clark and Cummins, 2019; Cinnirella, Klemp and Weisdorf, 2019), with Clark, Cummins and Curtis (2020) arguing that it is possible to regard 'all the variation in family size as exogenous' in pre-transition families. This paper presents empirical evidence of endogenous fertility in a relevant pre-transition population, arguing against this view.

It is possible that marriage age (or the onset of reproduction) is decided to achieve a desired family size (Szreter and Garrett, 2000); in other words, fertility and nuptiality may be part of one endogenous system. As discussed in more detail below, this is one reason that it is difficult to draw conclusions about the presence or absence of birth control from demographic data. However, a London charity and its archive provide a unique natural experiment with which to address these methodological challenges in this period (The London Archive, 1736). To women who had graduated from Raine's school and worked as domestic servants in good character, Raine's charity offered a 'marriage portion' of £100, which was distributed to the woman whose name was randomly drawn at a semi-annual public ceremony. Unsuccessful applicants were able to re-submit for the prize at subsequent ceremonies. This sum provided a major incentive to delay marriage until one's name was drawn, and because the draw was random, the effects of this delay can be interpreted as causal.

Reconstructing the life histories of married couples who received the portion suggests women responded rationally to these incentives. First, I demonstrate that later marriage caused by the lottery did not lead to significantly reduced family size. Rather, these couples compensated their late start with shorter birth intervals, the hazard of subsequent birth being around 13 per cent greater per six-month delay. This pre-transition group, therefore, exhibited evidence of family planning. Lottery participants had preferences over the timing of marriage, failing which they adopted shorter birth intervals to mitigate impacts on marital fertility.

Section one reviews the relevant literature, contrasting the behavioural assumptions of historical demographers to those underlying growth models with endogenous fertility. Section two reconciles these two views under a combined marital search and dynamic fertility framework. Section three describes the data, its representativeness, and the nature of the natural experiment. Section four estimates the effect of lottery outcome on family size and birth intervals. Section five concludes. In sum, this study provides important empirical microeconomic evidence of the existence of pre-transition marital fertility choice in the first population to experience an 'industrial revolution' and sustained economic growth.

I. Literature review

Demographic transition entails change from a world in which the income elasticity of population is strictly positive to one in which it is not (Galor and Weil, 2000; Galor, 2011). However, as Guinnane (2011) has emphasised, in European societies this was achieved through a change in the mechanism of fertility control: 'the fertility transition reflects a shift from controlling *marriage* to controlling fertility *within marriage*.' In other words, the fertility of pre-transition populations was largely determined by the duration and prevalence of sexual partnership, while post-transition families with comparable marital careers became smaller. This distinction is consequential.

The 'natural fertility' hypothesis, particularly as argued by Coale, holds that pre-transition fertility lay 'beyond the calculus of conscious choice' (Coale, 1973; Coale and Treadway, 1986; Clark, 2007; Alter, 2019). Incentives could therefore not act on fertility except indirectly, i.e.,

$$f\left[m^*(\mathbf{X})\right], \tag{1}$$

where f is the number of children born per woman and m^* is some mediator variable related to fertility whose value maximizes utility over a vector of relevant choice variables, $\mathbf{X}$.

Demographers working with the concept of 'natural fertility' regularly interpret the effect of marriage age and the extent of lifetime celibacy on total fertility in this way. For instance, in pre-transition England, Wrigley and Schofield (1989) argue single men and women entered annual service contracts to save for land, housing, and other productive capital necessary to establish independent households. Lower wages necessitated longer periods of saving and, because annual service contracts forbade it, later marriage. Celibacy has been attributed to the failure to achieve even a minimum level of savings for household formation, or possibly to female independence and skewed adult sex ratios related to migrant labour (Van Zanden, De Moor and Carmichael, 2019; Weir, 1984). As these examples demonstrate, because changes in the prevalence of marriage do not necessarily imply choices about fertility, demographers have tended to favour declining fertility *within* marriage as clearer evidence of deliberate birth control (Coale, 1967).

The evidential basis of this view for England is depicted in figure 1, copied from Woods (2000). The vertical axis is a measure of the fertility of married women indexed to a theoretical maximum at one.[1] The horizontal axis is a measure of the proportion of married women between the ages of 15 and 50, which decreases in both mean marriage age and the prevalence of lifetime celibacy. Isoquants are drawn at different levels of total fertility, also indexed relative to a theoretical maximum. Between the late sixteenth century and mid-nineteenth century, the index of marital fertility changed very little, and the primary engine of fertility change remained the prevalence of marriage. After the end of the nineteenth century, however, the pattern is practically reversed. The engine of fertility change was now marital fertility. It is this sharp break that demographers associate with the demographic transition and take to be indicative of a radical 'innovation' in sexual behaviour.

[1] Hutterites are held to exhibit the theoretical maximum fertility for human societies with monogamous marital institutions.

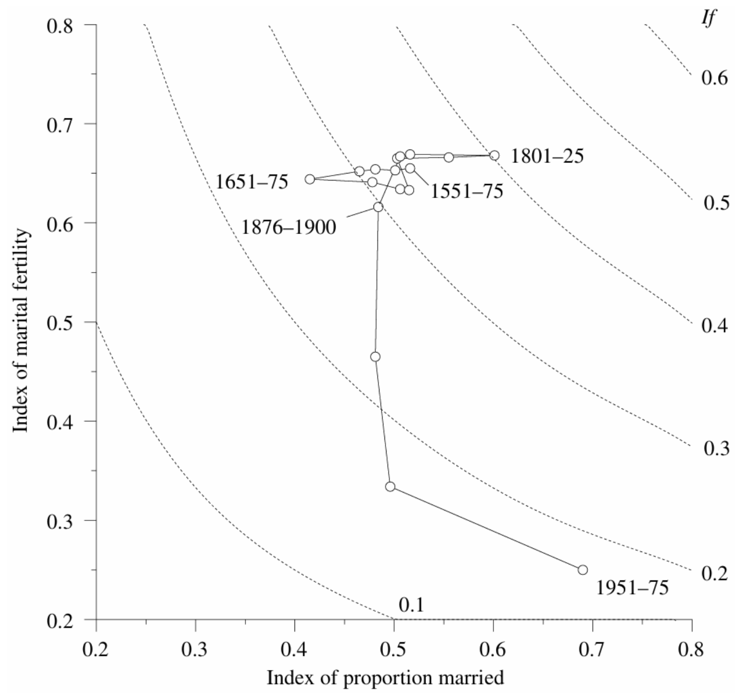

FIGURE 1. ENGLISH FERTILITY TRANSITION

Source: Woods (2000)

Although fertility decline is an important factor in unified growth models, radical 'innovation' fits awkwardly with their assumption of endogenous fertility because apparently novel behaviour must emerge from a single dynamic system. For example, in Galor (2011), households derive utility from the number of quality-adjusted surviving children and consumption. In the Malthusian era, a subsistence consumption constraint binds, leading to a corner solution in which there is a gap between the desired and actual number of surviving children, as households must attain the subsistence constraint at minimum. Any additional resources are expended on producing children, leading to the predicted positive income elasticity of population in the pre-transition period. When subsistence constraints no longer bind, the income elasticity of fertility is no longer positive but responds instead to the demand for human capital. This shift occurs without modifying the underlying utility function.

In (1), however, the number of children is the outcome of a choice about marriage. This choice may also lead to a positive income elasticity, but this correspondence may only be accidental, as in the example above emphasizing the institution of yearly service. Indeed, this example runs contrary to theoretical expectations, as search models applied to marriage markets generally predict that income (of singles) positively affects marriage age by raising the opportunity cost of search (Keeley, 1977). Horrell, Humphries and Weisdorf (2020) find evidence of this positive relationship in a time series of women's wages for pre-transition England.[2] If later marriage lowered fertility ($\frac{\partial f}{\partial m^*} < 0$), as is likely, the income elasticity of fertility would then be negative. Unified theories are not built on specific institutions and contingent behavioural responses.

It is also possible to rule out a shock to contraceptive technology at this time. Rudimentary and expensive physical contraceptives made of sponges and animal intestines, for example, had long existed. The invention of vulcanized rubber in 1844 and its application to condom technology improved access but not sufficiently to account for observed changes in fertility (Youssef, 1993; Szreter, 1996). In the earliest surveys, only 15 per cent of those married before 1910 reported using birth control, but this predominantly referred to *coitus interruptus* and was concentrated among the upper classes (Cook, 2004). Rather, the most plausible mechanism of fertility control in the late nineteenth century was decline in the frequency of sex in marriage (Szreter, 1996). This was clearly not a technological novelty but may have been a behavioural innovation.

On the other hand, applying a less restrictive behavioural model, fertility may have always been part of the decision set, i.e.,

$$U\left(f, m, \mathbf{X}\right), \tag{2}$$

where fertility and nuptiality are jointly selected to maximize utility. Moreover, it is possible that the optimally chosen marriage age is affected by planned fertility. In this case,

[2] They find a negative association with men's wages, which is consistent with economic theory because higher male wages are thought to increase the gains from trade in marriage and raise the marginal benefit of search. However, women's age at marriage and wages would seem to be the more relevant factors determining fertility.

there is no inconsistency for the theory of endogenous fertility, and declining marital fertility after about 1870 may have simply reflected an 'adaptation' to the relative costs of delayed marriage versus fertility control.

In support of the 'innovation' hypothesis, early research looked for the presence or absence of 'stopping', a decrease in fertility after a target number of living children (parity) had been reached. Louis Henry's seminal essay argued that this behaviour was the clearest evidence of birth control, as constant differences in birth intervals could result from confounding biological factors, such as innate fecundity, the duration of breast-feeding, or the effects of disease and nutrition, or from unconscious cultural taboos (Henry, 1961).

These arguments relied for evidence on aggregate data and deviations of age-fertility schedules from norms derived from high-fertility populations assumed to practice no birth control (Wilson, Oeppen and Pardoe, 1988). So-called 'parity progression ratios' indicated that the probability of having a subsequent child did not decline in the number of already-born children after holding maternal age constant (Wilson, Oeppen and Pardoe, 1988). Where fertility fell sharply from the norm at older ages or higher parities, demographers interpreted this as evidence of 'stopping'. However, endogeneity issues have plagued these methods from the beginning. For instance, Van Bavel pointed out that the lack of a fall in fertility at higher parities may be a compositional artifact, as more fecund couples are more likely to achieve higher parities (van Bavel, 2004*b*). On the other hand, individual-level data with which researchers might control for such issues are prone to truncation and censoring, as these datasets drop migrants and analysts struggle to identify the population that has become sterile. As Alter (2019) points out, this can lead to an underestimate of the population at risk of an event and bias estimated hazard ratios if truncation or censoring is also correlated with the covariate of interest.

An adjacent literature has demonstrated the importance of culture to fertility decline. For example, France's fertility transition occurred very early, corresponding roughly with the French Revolution (c. 1789), and a series of recent papers has pointed to cultural change as an important mechanism in this context (Blanc, 2024; Blanc and Wacziarg, 2020; de la Croix and Perrin, 2018; Perrin, 2022). Beach and Hanlon (2023) have demonstrated how the fertility of culturally English migrants living abroad declined in response to the widely-publicized Bradlaugh-Besant trial in England, which encouraged the publication of information on birth control. These studies tend to lend credibility to the 'innovation' argument insofar as they demonstrate fertility is driven by a series of exogenous cultural shocks.

On the other hand, a series of papers have demonstrated that wider birth spacing followed real-wage shocks and interpreted this as evidence of birth control (van Bavel, 2004*a*; Bengtsson and Dribe, 2006; Cilliers, Mariotti and Martins, 2024). Cinnirella, Klemp and Weisdorf (2017), with a paper in this genre, triggered a critical response calling into question the methodology and a defense from the original authors (Clark and Cummins, 2019; Cinnirella, Klemp and Weisdorf, 2019). Clark, Cummins and Curtis (2020) then published further evidence against the existence of birth control in three pre-transition populations. Because real wages may affect birth intervals through health and breastfeeding duration (Oris, Mazzoni and Ramiro-Fariñas, 2024), it is not clear that such studies identify deliberate birth control. Therefore Clark, Cummins and Curtis's claim to identify exogenous variation in fertility through twin births, overcoming stubborn endogeneity problems in this literature, deserves attention. In their study, families that experienced twinning had one additional child on average, suggesting these families did not compensate for the unexpected additional child and were therefore not practicing birth control.

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 neonatal mortality, leading to survivorship bias, particularly in developing contexts where maternal health is likely poor (Guo and Grummer-Strawn, 1993). Without measuring underlying fecundity, health, and zygosity, twin births cannot be interpreted as exogenous.[3] This paper therefore adopts a different approach to addressing endogeneity, randomization

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

through a lottery, to better understand pre-transition fertility.

II. Conceptual Framework

This section attempts to reconcile the views of historical demographers and economic historians on fertility behavior in a common framework derived from the microeconomic theory of marital search and dynamic fertility. A formal model is not developed.[4] Rather, the theoretical literature is surveyed to clarify ideas.

First consider a single woman choosing when to marry. In Keeley's (1977) seminal article, this choice is decomposed into entry to the marriage market and the subsequent duration of search. In the model, individuals receive a marital 'wage' once married, conventionally conceived as gains from the specialisation of women in household work and men in market work but possibly also reflecting other intrahousehold bargaining outcomes or preferences. In each period on the marriage market, our single woman receives marital wage offers, random draws from a known distribution, which she may either accept or decline. On the other hand, search on this market carries time costs (wages) and direct costs. The single woman will only enter the marriage market if her expected benefit of search exceeds these costs. Because Keeley models this process over an infinite time horizon without life-cycle variation in preferences, marriage-market entry is effectively a one-off choice about whether to marry at all.

If our single woman has decided to enter the marriage market, she must decide which offers she will accept. Keeley assumes she knows the distribution of marital wage offers and therefore the expected value of her search. Our single woman will continue to search if her realized offer falls below this value and accept otherwise. Generally, the duration of search will be decreasing in the direct costs of search and increasing in the single wage.

Reproduction is also understood as a random process, with the risk of pregnancy decreasing in contraceptive effort. Given the historical discussion above, it is helpful to think of contraceptive effort as a measure of the infrequency of sex. Contraceptive effort

[4]Many dynamic fertility models have no closed-form solution and must be solved numerically or adopt very restrictive assumptions about the utility function (Arroyo and Zhang, 1997)

is costly in terms of utility, and these costs are an increasing convex function of effort. The equilibrium level of contraceptive effort requires the usual condition that the expected marginal benefits of contraception (EMBC), avoiding the direct and opportunity costs of birth and raising children, equal the marginal costs of contraceptive effort (MCC) (Arroyo and Zhang, 1997). Where time horizons are not infinite, then such models generally predict declining contraceptive effort over time because there are fewer periods over which to reap the returns of contraception (Arroyo and Zhang, 1997). Contraceptive effort may also decline after several periods without a birth if the planning horizon is finite and couples have preferences over completed family size (Newman, 1988).

Fertility is related to marriage as a utility component of the marital wage. Childbirth outside of marriage in the past carried substantial social stigma, increased household exposure to downside labour-market risk, and limited women's prospects for future marriage (Gibson, 2022; Humphries, 1998). To avoid these negative outcomes, single women exerted greater reproductive effort, giving up some utility relative to married women. In other words, the EMBC was greater for unmarried women. Ceteris paribus, the larger this penalty, the larger the relative marital wage, and the earlier the marriage.

Figure 2 illustrates the basic concept, with the EMBC curve flat in contraceptive effort and at a lower level for married women (EMBC$'$) than for unmarried women. Equilibrium occurs where these curves intersect the MCC curve, leading unmarried women to exert greater contraceptive effort.

Now imagine that there is a secular increase in the expected marginal benefit of contraception, for example, as technological change drives a shift in the value of quality over quantity. If this change affects children born to both married and unmarried women, there may be no change in the prevalence of marriage because deciding to marry involves a comparison between married and unmarried states and not the level of contraceptive effort directly (Keeley, 1977). In other words, if both married and unmarried women move up the contraceptive effort curve, there may be little change in marriage age, yet couples would exert greater contraceptive effort both within and without marriage.

This conceptual framework is consistent with the empirical pattern in figure 1 without

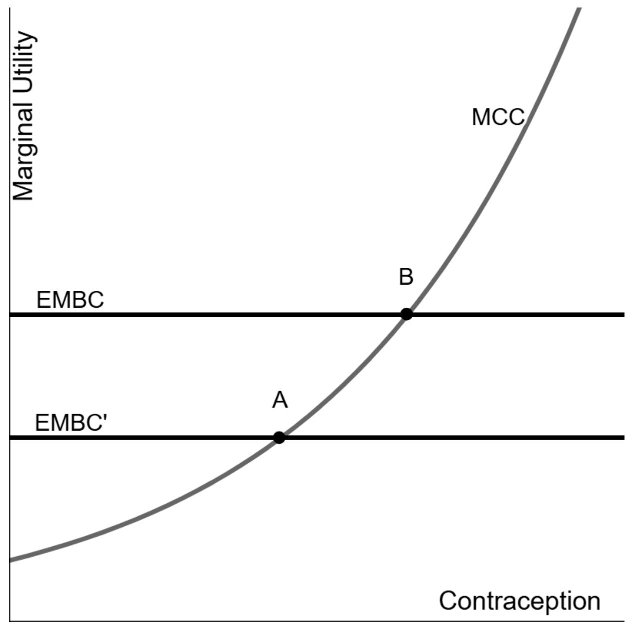

FIGURE 2. MODEL OF FERTILITY CHOICE

Note: EMBC is the expected marginal benefit of contraception for unmarried women and $EMBC'$ is for married women.

requiring radical behavioural change. However, if correct, we would also expect to see unmarried women exert greater contraceptive effort. In fact, the ratio of extramarital to marital births declined during the English demographic transition (Laslett, 1980), suggesting that the trend towards greater contraceptive effort was steeper among unmarried women, even more so due to the assumption of convexity. Perhaps social stigma associated with extramarital childbirth was also increasing at this time, raising the EMBC of unmarried women even faster than that of married women. In theory, this would incentivize earlier marriage, counteracting any effect of lower fertility.[5] However, the larger point is that while changes affecting only one of either married or unmarried women's EMBC may affect marriage age, changes affecting both need not.

The natural fertility hypothesis can be quite naturally incorporated in such a framework by assuming that only two levels of contraceptive effort are possible, some minimum

[5]Indeed, marriage age did briefly decline around this time (Wrigley and Schofield, 1989).

determined by, for instance, biology, breastfeeding, and maternal health, and a maximum of total abstinence. New behavioural possibilities, perhaps as a result of cultural change during the fertility transition, would introduce intermediate levels of contraceptive effort which may have been optimal in earlier populations but were not technically feasible.

This discussion suggests a viable test. Evidence in the pre-transition period that (1) contraceptive effort responded marginally to relevant incentives and (2) that the level of contraceptive effort was not at the biological minimum would contradict this hypothesis.

III. Data and identification

To study fertility choice before the demographic transition, I construct a dataset containing information on the marriage and birth histories of women who attended Henry Raine's charity school in London and married between 1762 and 1872. Uniquely, this school offered a £100 marriage portion to eligible graduates through a lottery.[6] Girls could re-enter subsequent rounds of the lottery if unsuccessful so the £100 acted as a substantial, exogenous incentive to change marriage timing. Under the natural fertility hypothesis, such variation should not affect fertility beyond any impact of biological age on fecundity. In contrast, if fertility responds to delayed marriage, perhaps because the remaining fertility horizon becomes shorter, this would cast doubt on the hypothesis. Moreover, because this variation is random it will be uncorrelated with background characteristics known to affect fertility, such as underlying health or propensity to breastfeed, which are difficult to observe but may contaminate efforts to identify fertility control. Lottery outcome should also be uncorrelated with any censoring or truncation in the sample.

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 McDonnell, 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

[6] Revaluing from 1760 to 2023 by the RPI gives £19,100 and by relative earnings £226,700. The same conversion from 1870 would be £11,280 and £71,040, respectively (Measuringworth.com, 2024).

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). 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 not exactly clear, 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'.[7] 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 fertility and growing dependency ratios, the average family experienced life-cycle poverty when young children were present in the household, making 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 nine-

[7]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*)

teenth and possibly eighteenth centuries, employing as much 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.[8] Further, character references were common in the labour market for domestic servants. Thus while it was unusual for girls to have their lives so thoroughly defined by a charitable institution, the actual content of marriage portion candidates' lives 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 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

[8]TLA ACC/1811/8/14; London average from personal correspondence with Moritz Kaiser on basis of forthcoming article.

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 such women could stand for the marriage portion. They drew paper tickets from a tin canister, one of which was marked. The winner was then allowed six months to find a suitable groom-to-be, whose character was also evaluated by the trustees. According to one trustee, most women had suitors at the time of the draw, but this was not always the case (Jones, 1875). Eligible grooms needed to be resident in St. George in the East or two neighbouring parishes. 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.

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.

Two primary sources of information on the lottery are held in The London Archives: lot books and trustees' receipts (The London Archive, 1736). The former relate to the administration of the lottery. Every page contains 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. Occasionally, if the winner did not present a suitable groom, it is noted that she instead received a £5 gift. The trustees' receipts were produced during the £100 transaction and duplicate much of the information on brides and their grooms. These are a useful check on the lot books, which occasionally neglect to record an occupation or may be illegible.

I hand-link the information contained in these sources to the collection of genealogical sources digitized by The London Archives and hosted on Ancestry.co.uk (The London Archive, 2010*a*,*b*). I draw particularly on records of baptism and marriage, although I also cross-reference against the census once this becomes available in 1851 for greater accuracy.[9]

My starting point is marriage. After 1837, parish marriage registers were required to note the bride's father's name. Because lottery weddings could only occur on one of two days in the year in the parish church of St. George's, I am able to confidently identify 91 per cent of these lottery marriages in genealogical sources.

[9]The first nominal census in England was in 1841, but the place identifiers used in this census were not sufficiently detailed and the quality is generally poor. The next census in 1851 improved on these shortcomings.

Using the bride's maiden name, her father's name if available, and her approximate age (≥ 22) I search for the bride's own baptismal certificate in the records of St. George's, where she must have been born to be eligible for entry to the school. Notably, the school would have checked these same registers when admitting students (Rose, 1991). Because Bailey et al. (2020) caution that bias introduced by false matches tends to be more serious than low match rates, I opt to discard ambiguous matches at this stage. For the pre-1837 period, the procedure is the same but, without father's names, more dependent on brides having relatively unique names. From baptismal certificates, I note brides' birth date, which provides the basis for all subsequent age calculations.[10]

For women who drop out of the lottery, I possess far more limited information, only a name, place of birth, and approximate age. Of these, only their name would be recorded on any eventual marriage document. This makes linking these women to marriage and subsequent childbirth very difficult and strongly dependent on girls having a unique name. In a large city like London, the problem is compounded. Therefore, I currently make no effort to identify later life outcomes of women who dropped out of the lottery without marrying. Subsequent estimates must therefore be interpreted as giving the local average treatment effect. A further discussion of the potential biases introduced by this choice is left to the next section.

The crucial next step is to look for evidence of fertility 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 East London. Finally, I use the father's occupation if this seemed to provide identifying information. That is, I interpret skilled trades that likely required an apprenticeship as providing reliable information about identity, whereas unskilled oc-

[10] Most parishes at this time only recorded the date of baptism, but many parishes in East London, including St. George's in the East, fortunately also recorded birth dates for the study period.

cupations do not because of the greater potential for occupational change. 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. Where possible, I also cross-reference the birthplaces of children living in the household in the 1851 census to their baptismal certificates. 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. I calculate the birth interval as the number of months between sibling birth dates and between marriage and the birth of the first child.

I also make an effort to capture infant mortality. English burial records contain less detail than other genealogical records, often only a name and date of burial, and are thus more challenging to link. I consider that an infant was likely to have died when an individual with the same name died in the same parish within a year of their birth date. A one-year observation window represents the period when a child was most at risk of death and reduces the probability of their family having moved. Where the burial notes the deceased's age, i.e., after reform of death registration in 1813, I use this to rule out erroneous links.

This linking procedure is not perfect, and I undoubtedly fail to identify all children and married couples with total accuracy. The dataset includes direct URLs to the sources so that the quality of links can be checked by interested readers. Nonetheless, because the charity provides restrictions on the parameters of my search and material against which to cross-reference information, I achieve fairly good results by the standards of

TABLE 2—VALIDATION OF RECONSTITUTION METHOD, 1841-1851

ID	Follow-up		Census		Baptism		Type I	Type II
193	5 New Street, Horsleydown	Fellmonger	—	—	New Street, Horsleydown	Fellmonger		
192	15 Tottenham Place, Tottenham Court Road	Baker	15 Tottenham Place	Baker	Upper North Place, St Pancras	Baker		
194	Red Lion Passage	Pastry cook and confectioner	—	—	—	—		X
191	Unknown	—	—	—	Old Montague Street, Whitechapel	Bricklayer[1]	X	
196	2 Morpeth Street, Bethnal Green	Bell founder	Morpeth Street, Bethnal Green	Bell founder	Bethnal Green	Bell founder		
195	Unknown	Optical brass founder[2]	—	Brass finisher	—	Brass turner		
197	St. Katharine Docks	Fireman	—	—	—	—		X
198	10 Norfolk Street, Commercial Road	Gun Maker	New Norfolk Street, Stepney	Gun polisher	7 [illegible] Cornwall St	Gun maker		
199	Unknown	Shoemaker	—	—	—	—		
202	4 Little Abbey Street, Bermondsey	Silk weaver	4 Stephen Street, Bermondsey	Weaver	—	—		
201	Unknown	Shoemaker	27 St. James Terrace	Shoemaker journeyman	11 Tarling Street, Christ Church	Bootmaker		
204	3 Hope Place, Bermondsey	Warehouse man	3 Hope Place, Bermondsey	Porter	New Church Street, Bermondsey	Porter		
208	5 Curriers Hall Court, London Wall	Porter	9 Three Herring Ct, Cripplegate	Porter	Marshall St., Gripplegate	Porter		
203	Unknown	Shipmate	12 Prospect Place	Mariner	12 Prospect Place, St George in the East	Mariner		
207	Went abroad	Painter	—	—	—	—		
211	Unknown	Shoemaker	19 Lombard St, Chelsea	Shoemaker	16 Lombard, Chelsea	Cordwainer		
214	Unknown	Cooper	Denmark Street, St George in the East	Cooper	7 Denmark Street, St George in the East	Cooper		
215	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.

the literature. Further, the charity's archive provides 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, which in table 2 I compare to information identified by linking to the 1851 census and/or the baptismal record of the child born nearest to 1851. As my reconstitution method did not draw on this source, this comparison represents a blind validation.

Type I errors here 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, 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 here 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).

The observed pattern of birth intervals is broadly consistent with existing research. Mean birth intervals of middle children in the sample, 28.3 months, are slightly longer than in Davenport's (2016) eighteenth-century London reconstitution but slightly shorter than in Wrigley et al.'s (1989) national sample, which excludes London.[11] A pattern of shorter first birth intervals, longer middle intervals due to breastfeeding, and longest last

[11] Davenport does not identify marriages so is unable to provide first-birth intervals.

TABLE 3—SUMMARY STATISTICS AND COMPARISONS

	mean	sd	min	max
Lotteries	3.42	3.35	1	17
Entry age	23.8	2.42	20.3	30.4
Total Bapt.	4.21	1.59	2	7
Age at final birth	35.5	4.26	26.6	45.2
First birth interval	25.8	28.9	2.60	138.7
Middle birth interval	28.2	12.7	10.6	84.8
Last birth interval	41.3	22.2	12.0	114.4
St. Martin in the Fields, 1752-1812				
Total Bapt.	4.43	2.43	1	15
Middle birth interval	27.20	13.33	7	127
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			

Note: Birth intervals in the national sample are only reported for the whole period 1580-1837.
Source: Wrigley and Schofield (1983, 1989). Davenport kindly shared data underlying her (2016) article.

intervals due to declining fecundity is observed in the data, although the mean interval between marriage and first childbirth in this case is longer than has been found elsewhere (Wrigley and Schofield, 1989). This is likely because childbirth before marriage would result in exclusion from further consideration for the portion, whereas elsewhere the decision to marry was often triggered by the initiation of sex. Indeed, only five brides gave birth within nine months of marriage and only Mary Downie was in her final trimester at marriage, a fact which did not go unnoticed. In contrast, elsewhere in England around a third of all first-born children arrived within nine months of marriage circa 1800 (Wrigley and Schofield, 1989). The linking methodology may also have disproportionately missed some first-born children, skewing the data on first birth intervals, although it is not clear why this would be the case.

Table 3 reports other relevant summary statistics, alongside comparable statistics from the wider population. Brides tended to stand for the lottery at 23.6 years old. They waited just under two years (3.42 draws) to marry on average, and they went on to have around 4 births before stopping at around 35 years old. In terms of marriage age, this is very similar to the 1750-1799 national estimates. Family size is somewhat smaller, which could be due to earlier final births, later marriage resulting from lottery participation, or simply lower levels of fertility in London (Levene, 2012). Indeed, in another London reconstitution from the same period, Davenport (2016) identifies a mean 4.43 baptisms per family, much more comparable to the families of lottery participants. On the other hand, Davenport concedes that a number of births may have gone unregistered or missed in her London reconstitution, and this is possible in the lottery sample too. Consequently, I make efforts to check the robustness of results to this kind of measurement error. More fundamentally, however, there is no reason to suspect these errors are correlated with the lottery outcome and confound estimates of its effect on behaviour.

I focus on estimating two effects. In line with the theoretical discussion, I focus first on contraceptive effort measured by the birth interval using a Cox proportional hazards model

$$\lambda(t|X_i, C_i) = \lambda_0(t)\exp(\beta_1 X_i + \mathbf{C}_i\beta) \tag{3}$$

where λ represents the conditional hazard function at time t, $\lambda_0(t)$ is the baseline hazard, X_i is the number of lottery draws by child i's parents, $\mathbf{C}_i$ is a vector of controls, and β are coefficients. The controls in $\mathbf{C}_i$ include mother's age at each birth event to capture the biological decline in fecundity with age. Her age and anticipated marriage delay when first seeking admission to the lottery are included to capture potential heterogeneity in preferences affecting how long she is willing to continue in the lottery (i.e., sample attrition). Anticipated marriage delay is constructed by summing the number of lotteries a woman could expect to spend on the waiting list before being admitted to the lottery (when there are more than six participants) with her expected wait once in the lottery.[12] Conceptu-

[12] Expected wait is equal to the number of other lottery participants, assuming a geometric distribution.

ally, these are both stochastic variables because some women dropped out, making space on the waiting list, in a way individual lottery participants could not anticipate. $\mathbf{C}_i$ also includes a dummy variable indicating whether the previous child died in infancy and a time trend.

In practice, a Schoenfeld residuals test indicates that specifying only one baseline hazard function is not consistent with the proportional hazards assumption. This is unsurprising, given the discussion of the relationship between birth order and birth spacing above. I therefore stratify on birth order, after which I fail to reject the hypothesis of proportional hazards in the Schoenfeld test. I cluster standard errors by family.

Changes to marriage age should also affect fertility. I therefore also regress my proxy for fertility on lottery outcome as in the following OLS equation

$$Y_j = \phi_0 + \phi_1 X_j + \mathbf{C_j}\phi + \varepsilon \tag{4}$$

where Y_j is the number of observed baptisms in family j, and the other variables are as above—except age at birth and the dummy for a previous death, which cannot be constructed at the family level.

IV. Results

Table 4 gives the effect of lottery participation on the birth interval in a proportional hazards framework. Column 1 implies that each round of lottery participation, which delays marriage by around six months, increases the hazard of each subsequent birth by 2 per cent.

Column 2 controls for the mother's age (in years) when first entering the lottery, a variable which may have been strategically manipulated by lottery participants and adds a control for maternal age at each individual birth event. Female fecundity (and male) declines with biological age so that later marriage resulting from the lottery should make conception less likely, all else equal. Indeed, being one year older reduces a mother's hazard of childbirth by 19 per cent. However, the random variation in age at marriage

TABLE 4—EFFECT OF LOTTERY ON BIRTH INTERVAL, COX PROPORTIONAL HAZARDS MODEL

	(1) B. Interval	(2) B. Interval	(3) B. Interval	(4) B. Interval	(5) B. Interval	(6) B. Interval
Lotteries	1.02	1.08	1.17	1.17	1.18	1.17
	(0.03)	(0.03)	(0.06)	(0.06)	(0.05)	(0.05)
Entry age		1.28	1.29	1.29	1.30	1.22
		(0.06)	(0.06)	(0.07)	(0.06)	(0.06)
Age at birth		0.79	0.80	0.80	0.80	0.84
		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Anticipated wait			0.93	0.93	0.96	0.96
			(0.03)	(0.03)	(0.04)	(0.04)
Post-death				1.06	1.05	1.17
				(0.50)	(0.52)	(0.57)
Year					1.01	1.01
					(0.00)	(0.00)
Sub-sample						Int. ≤ 70
N Births	218	202	202	202	202	190
N Families	66	61	61	61	61	56
Schoenfeld p-value	0.36	0.40	0.62	0.74	0.85	0.096

Note: Standard errors clustered by family in parentheses. Exponentiated coefficients. Stratified by birth parity (see text).

caused by the lottery now has a stronger effect on birth intervals, raising the hazard of subsequent birth by 8 per cent per six-month delay. The relationship between fecundity and age thus appears to have confounded the effect of the lottery, which caused couples to pursue closer birth spacing despite these biological obstacles. Notably, initial lottery entry at older ages also predicts a much greater risk of childbirth. This may indicate a similar mechanism to the lottery—couples 'catching up' after unplanned marriage delay—because finding a suitor for the lottery may itself have been a stochastic process. However, because later marriage through the lottery is more clearly exogenous, it provides more compelling evidence of this effect.

Because of the challenge of identifying unmarried women in London, I describe above how the sample is restricted to those lottery participants who eventually married and received the marriage portion. This could introduce bias if compliance is systematically related to birth intervals. However, I argue it is more likely that non-compliance acted

TABLE 5—EFFECT OF LOTTERY ON BIRTH INTERVAL BY BIRTH ORDER, COX PROPORTIONAL HAZARDS MODEL

	First Interval		Middle Interval		Last Interval	
	(1)	(2)	(3)	(4)	(5)	(6)
	B. Interval	B. Interval	B. Interval	B. Interval	B. Interval	B. Interval
Lotteries	2.12	2.05	1.22	1.25	1.21	1.04
	(0.40)	(0.40)	(0.09)	(0.09)	(0.12)	(0.14)
Sub-sample		Int. ≤ 70		Int. ≤ 70		Int. ≤ 70
N Births	61	56	96	95	45	39
N Families	61	56	40	40	45	39
Schoenfeld p-value	0.99	0.37	0.61	0.47	0.97	0.48

Note: Conditional on same controls as in column (6) above. Standard errors clustered by family in parentheses. Exponentiated coefficients.

to attenuate any observed effects because exiting the lottery reveals a relative preference for the non-pecuniary advantages of earlier marriage over money. Women who desire more children or dislike contraceptive effort more than the others, for example, should tend to drop out of the lottery earlier (or not participate at all) and realize shorter birth intervals, but I find the opposite. Those women whose name is seemingly never called but stay the course anyway have shorter intervals. A similar argument can be sustained for underlying fecundity, supposing pre-marital intercourse occurs.[13]

How long each lottery participant could reasonably expect to wait before winning is included in column (3) to capture some of these unobserved preferences. A woman who participated in the lottery despite there being eight other participants, for example, reveals a preference for the monetary prize over earlier marriage. Insofar as these preferences include preferences over fertility, such a woman should exert more contraceptive effort than her more impatient peers. Indeed, this coefficient predicts that the hazard of subsequent birth is about seven per cent lower for each anticipated six-month delay, as this logic predicts. Women whose expectations are not met, however, continue to alter their fertility behaviour accordingly. The coefficient on marital delay induced by the lottery, now conditional on expected delay, doubles in magnitude.

Column 4 includes a dummy variable indicating whether the child born previously died

[13] I thank Romola Davenport and Alice Reid for pointing this out.

in infancy. Historical demographers have demonstrated that birth intervals following the loss of an infant tend to be shorter because this leads to the cessation of breastfeeding (Wrigley and Schofield, 1989). Additionally, short birth intervals may cause higher infant mortality because siblings close in age will have similar, competing care needs. In table A1, I demonstrate that marriage delay through the lottery may lead to higher infant mortality, although the relationship is not robust. The coefficient on this dummy indicates, however, that infant mortality is not driving the main results, which remain unchanged.

Column 5 includes a time trend out of concern that unmeasured factors changing over time may be confounding the primary effect. For example, fertility was rising rapidly during this period at the national level (Wrigley and Schofield, 1989), and broad factors responsible for these changes may have affected lottery participants' decisions. At the same time, inflation eroded the real value of the marriage portion over time, changing the marginal benefit of lottery participation. However, the coefficient on lottery participation is unaffected after including a time trend, suggesting these concerns may be unfounded. In this preferred specification, the hazard of birth increases by 18 per cent per six-month delay. Finally, as a robustness check, column 5 drops all birth intervals greater than 70 months, as these may indicate uncounted births. The coefficients are largely unchanged. Measurement error, expectedly, does not appear to be driving the observed effect of the lottery.

Table 5 estimates the specifications in columns 4 and 5 separately for each of the first, middle, and final birth intervals. The effect is strongest for the first interval but is also present across the other intervals. Because it was impossible for mothers to be breastfeeding at the time of their first conception, this makes it unlikely that these mothers controlled the length of breastfeeding to limit fertility, as some have suggested elsewhere (Sharpe, 2002). Rather, couples likely resorted to periods of sexual abstinence to affect fertility (Szreter and Garrett, 2000; Cook, 2004).

Finally, table 6 presents the effect of the lottery on the number of observed baptisms in each family, a proxy for completed family size. In column 1, each additional lottery pre-

TABLE 6—EFFECT OF LOTTERY ON COMPLETED FAMILY SIZE, OLS

	(1) OLS: N Bapt.	(2) OLS: N Bapt.	(3) OLS: N Bapt.	(4) OLS: N Bapt.	(5) OLS: Corrected Bapt.
Lotteries	-0.060	-0.081	-0.037	-0.050	-0.094
	(0.08)	(0.08)	(0.14)	(0.14)	(0.14)
Entry age		0.12	0.12	0.12	0.097
		(0.10)	(0.10)	(0.10)	(0.10)
Anticipated wait			-0.043	0.040	0.077
			(0.12)	(0.14)	(0.13)
Year				0.013	0.012
				(0.01)	(0.01)
Constant	3.50	0.85	0.77	-22.7	-20.4
	(0.34)	(2.35)	(2.38)	(20.91)	(20.53)
R2	0.0097	0.044	0.047	0.068	0.060
N Families	66	61	61	61	61

Note: Standard errors in parentheses.

dicts 0.06 fewer births, and including controls in column 2 does not materially alter the coefficient. Column 3 controls for the expected lottery wait whose negative coefficient is again consistent with the idea revealed preference against fertility. Column 4 includes a time trend with no significant change.

Given that later marriage shortens the number of available fecund years, in the absence of shorter birth intervals, the lottery should have reduced fertility. However, it appears that birth intervals were sufficiently shortened to practically nullify this effect. Indeed, it is not possible to reject the possibility of no lottery effect on fertility in any of the models in the table. In column 5, to correct for the possibility of unmeasured childbirth, I add one child to each household with an excessively long birth interval, i.e., greater than 70 months. The results are virtually unchanged, suggesting again that the effect is not driven by measurement error. Rather, couples whose marriage was delayed by an unlucky lottery outcome appear to have responded with shorter birth intervals, resulting in little difference in fertility among lottery participants. Put another way, absent control a marriage delay equivalent to the average birth interval should result in one fewer children born. Taking the mean birth interval of middle children at 28.2 months, or 4.7

lotteries, the coefficients in columns 4 and 5 suggest fertility was reduced by only 24 per cent and 44 per cent, respectively, relative to this no-control scenario,.

V. Conclusion

These results contradict the predictions of the natural fertility hypothesis. First, because couples' fertility responded marginally to an exogenous change in marriage age, a range of different levels of contraceptive effort must have been available to them even before the fertility transition. Second, sampled couples cannot have been exercising a biological minimum of contraceptive effort within marriage because they responded to delayed marriage by apparently reducing their contraceptive effort still further.

A further point concerns the direction of the effect. Because the lottery would have led to lower fertility in the absence of a behavioural response, this study hints at the possibility that pre-transition families valued children positively and were not merely unwilling or unable to avoid them.

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, but there is no way to demonstrate this directly. On the other hand, internal and external validity have been interpreted as complements in research design (Deaton and Cartwright, 2017). Studies of larger or more representative samples that argue for birth spacing as a mechanism of birth control in pre-transition populations may appear more convincing in light of these results.

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. Late nineteenth-century fertility decline in England was a reproductive evolution, not a revolution.

REFERENCES

Alter, George C. 2019. “The evolution of models in historical demography.” *Journal of Interdisciplinary History*, 50(3): pp. 325–62.

Arroyo, Cristino R., and Junsen Zhang. 1997. “Dynamic microeconomic models of fertility choice: a survey.” *Journal of Population Economics*, 10: pp. 23–65.

Bailey, Martha J., Connor Cole, Morgan Henderson, and Catherine Massey. 2020. “How well do automated linking methods perform? lessons from US historical data.” *Journal of Economic Literature*, 58(4): pp. 997–1044.

Beach, Brian, and W. Walker Hanlon. 2023. “Culture and the historical fertility transition.” *Review of Economics Studies*, 90(4): pp. 1669–1700.

Bengtsson, Tommy, and Martin Dribe. 2006. “Deliberate control in a natural fertility population: Southern Sweden, 1766-1864.” *Demography*, 43(4): pp. 727–46.

Blanc, Guillaume. 2024. “The cultural origins of the demographic transition in France.” unpublished working paper.

Blanc, Guillaume, and Romain Wacziarg. 2020. “Change and persistence in the age of modernization: Saint-Germain-d’Anxure,1730-1895.” *Explorations in Economic History*, 78: 101352.

Carlsson, Gösta. 1966. “The decline of fertility: innovation or adjustment process.” *Population Studies*, 20(2): pp. 149–74.

Cilliers, Jeanne, Martine Mariotti, and Igor Martins. 2024. “Fertility responses to short-term economic stress: price volatility and wealth shocks in a pre-transitional settler colony.” *Explorations in Economic History*, 94: 101620.

Cinnirella, Francesco, Marc Klemp, and Jacob Weisdorf. 2017. “Malthus in the bedroom: birth spacing as birth control in pre-transition England.” *Demography*, 54(2).

Cinnirella, Francesco, Marc Klemp, and Jacob Weisdorf. 2019. "Further evidence of within-marriage fertility control in pre-transitional England." *Demography*, 56(4): pp. 1557–72.

Clark, Gregory. 2007. *A farewell to alms: a brief economic history of the world.* Princeton:Princeton University Press.

Clark, Gregory, and Neil Cummins. 2019. "Randomness in the bedroom: there is no evidence for fertility control in pre-industrial England." *Demography*, 56(4): pp. 1541–55.

Clark, Gregory, Neil Cummins, and Matthew Curtis. 2020. "Twins support the absence of parity-dependent fertility control in pretransition populations." *Demography*, 57(4): pp. 1571–95.

Coale, Ansley J. 1967. "The Voluntary Control of Human Fertility." *Proceedings of the American Philosophical Society*, 111(3): pp. 164–9.

Coale, Ansley J. 1973. "The demographic transition reconsidered." Vol. 1, pp. 53–72. Liège, Belgium.

Coale, Ansley J., and Roy Treadway. 1986. "A Summary of the Changing Distribution of Overall Fertility, Marital Fertility, and the Proportion Married in the Provinces of Europe." In *The decline of fertility in Europe*. , ed. Ansley J. Coale, pp. 31–181. Princeton:Princeton University Press.

Cockburn, J. S., H. P. F. King, and K. G. T. McDonnell, ed. 1969. *A history of the county of Middlesex.* London:Victoria County History.

Cook, Hera. 2004. *The long sexual revolution: English women, sex, and contraception 1800-1975.* Oxford:Oxford University Press.

Davenport, Romola. 2016. "Urban family reconstitution—a worked example." *Local Population Studies*, 96: pp. 28–49.

Deaton, Angus, and Nancy Cartwright. 2017. "Understanding and misunderstanding randomized controlled trials." *Social Science & Medicine*, 210: pp. 2–21.

de la Croix, David, and Faustine Perrin. 2018. "How far can economic incentives explain the French fertility and education." *European Economic Review*, 108: pp. 221–45.

Galor, Oded. 2011. *Unified Growth Theory.* Princeton, NJ:Princeton University Press.

Galor, Oded, and David N. Weil. 2000. "Population, Technology, and Growth: From Malthusian Stagnation to the Demographic Transition and Beyond." *American Economic Review*, 90(4): pp. 806–828.

Gibson, Kate. 2022. *Illegitimacy, family, and stigma in England, 1660-1834.* Oxford:Oxford University Press.

Guinnane, Timothy W. 2011. "The Historical Fertility Transition: A Guide for Economists." *Journal of Economic Literature*, 49(3): 589–614.

Guo, Guang, and Laurence M. Grummer-Strawn. 1993. "Child mortality among twins in less developed countries." *Population Studies*, 47(3): pp. 495–510.

Henry, Louis. 1961. "Some data on natural fertility." *Eugenics Quarterly*, 8(2): pp. 81–91.

Horrell, Sara, Jane Humphries, and Jacob Weisdorf. 2020. "Malthus's missing women and children: demography and wages in historical perspective, England 1280-1850." *European Economic Review*, 129: 103534.

Horrell, Sara, Jane Humphries, and Jacob Weisdorf. 2022. "Beyond the male breadwinner: life-cycle living standards of intact and disrupted English working families, 1260-1850." *Economic History Review*, 75(2): pp. 530–60.

Humphries, Jane. 1998. "Female-headed households in early industrial Britain: the vanguard of the proletariat?" *Labour History Review*, 63(1): pp. 31–65.

Jones, Harry. 1875. *East and west London: being notes of common life and pastoral work in Saint James's, Westminster and in Saint Georges'-in-the-East.* London:Smith, Elder & Co.

Keeley, Michael C. 1977. "The economics of family formation." *Economic Inqury*, 15(2): pp. 238–50.

Laslett, Peter. 1980. "Introduction: comparing illegitimacy over time and between countries." In *Bastardy and its comparative history*. , ed. Karla Oveenoster and Richard M. Smith, pp. 1–70. London:Edward Arnold.

Levene, Alysa. 2012. *The childhood of the poor: welfare in eighteenth-century London.* Basingstoke, Hamps.:Palgrave Macmillan.

Lincoln, Margarette. 2018. *Trading in War: London's Maritime World in the Age of Cook and Nelson.* New Haven:Yale UP.

Measuringworth.com. 2024. "Relative values - UK £."

Newman, John L. 1988. "A stochastic dynamic model of fertility." In *Research in population economics: a research annual*. Vol. 6, , ed. T. Paul Schultz, pp. 41–68. London:JAI Press.

Oris, Michel, Stanislao Mazzoni, and Diego Ramiro-Fariñas. 2024. "Did the 1917-21 economic depression accelerate the epidemiological transition? milk prices, summer peak of mortality, and food-and-water causes of death in Madrid, Spain." *Explorations in Economic History*, 94: 101613.

Perrin, Faustine. 2022. "On the origins of the demographic transition: rethinking the European marriage pattern." *Cliometrica*, 16(3): pp. 431–75.

Raine, Henry. 1748. *Copy of an indenture of release of several freehold and leasehold premises from the late Mr. Henry Raine.* London:J. Owen. Gale Primary Sources.

Rose, Craig. 1991. "Evangelical philanthropy and Anglican revival: the charity schools of Augustan London, 1698-1740." *The London Journal*, 16(1): pp. 35–65.

Schwarz, Leonard. 1999. "English servants and their employers during the eighteenth and nineteenth centuries." *Economic History Review*, 52(2): pp. 236–56.

Scott, Susan, and C. J. Duncan. 1999. "Nutrition, fertility and steady-state population dynamics in a pre-industrial community in Penrith, Northern England." *Journal of Biosocial Science*, 31(4): pp. 505–23.

Sharpe, Pamela. 2002. *Population and Society in an East Devon Parish: Reproducing Colyton 1540-1840.* Exeter:University of Exeter Press.

Snell, K. D. M., and Paul S. Ell. 2004. *Rival Jerusalems: The Geography of Victorian Religion.* Cambridge:Cambridge UP.

Szreter, Simon. 1996. *Fertility, class and gender in Britain, 1860-1940.* Cambridge:Cambridge University Press.

Szreter, Simon, and Eilidh Garrett. 2000. "Reproduction, compositional demography, and economic growth: family planning in England long before the fertility decline." *Population and Development Review*, 26(1): pp. 45–80.

The London Archive. 1736. "Raine's foundation school." ACC/1811.

The London Archive. 2010*a*. "London, England, Church of England Baptisms, Marriages and Burials, 1538-1812."

The London Archive. 2010*b*. "London, England, Church of England Births and Baptisms, 1813-1923."

Tong, Stephen, and Roger V. Short. 1998. "Dizygotic twinning as a measure of human fertility." *Human Reproduction*, 13(1): pp. 95–8.

van Bavel, Jan. 2004*a*. "Deliberate birth spacing before the fertility transition in Europe: evidence from nineteenth-century Belgium." *Population Studies*, 58(1): pp. 95–107.

van Bavel, Jan. 2004*b*. "Detecting stopping and spacing behaviour in historical demography: a critical review of methods." *Population*, 59(1): pp. 117–28.

Van Zanden, Jan Luiten, Tine De Moor, and Sarah Carmichael. 2019. *Capital Women: The European Marriage Pattern, Female Empowerment, and Economic Development in Western Europe, 1300-1800.* Oxford:Oxford University Press.

Weir, David R. 1984. "Rather Never than Late: Celibacy and Age at Marriage in English Cohort Fertility, 1541-1871." *Journal of Family History*, 9(4): p. 340–54.

Wilson, Chris, Jim Oeppen, and Mike Pardoe. 1988. "What is natural fertility? the modelling of a concept." *Population Index*, 54(1): pp. 4–20.

Woods, Robert. 2000. *The demography of Victorian England and Wales.* Cambridge:Cambridge University Press.

Wrigley, E. A., and Roger Schofield. 1989. *The Population History of England, 1541-1871: A Reconstruction.* Cambridge:Cambridge University Press.

Wrigley, E. A., and R. S. Schofield. 1983. "English population history from family reconstitution: summary results 1600-1799." *Population Studies*, 37: pp. 157–84.

Youssef, H. 1993. "The history of the condom." *Journal of the Royal Society of Medicine*, 86: pp. 226–8.

You, Xuesheng. 2024. "Female relatives and domestic service in nineteenth-century England and Wales: female kin servants revisited." *Economic History Review*, 77(2): pp. 444–71.

Appendix

A1. Infant Mortality

Table A1 investigates the relationship between lottery outcome and identified infant mortality using logistic regression, with standard errors clustered at the level of the family. In column 1, a six-month delay of marriage through the lottery increases the probability a child will die in infancy by 0.003 per cent at the mean. After controlling for age of birth and entry age in column 2, a six-month delay leads to a a 0.004 percent increase in this probability at the mean. Controlling for expected wait in column 3 attenuates the effect, which would now be considered statistically insignificant by conventional standards. A time trend in column 4 has little effect. Overall, this evidence suggests lottery outcome may increase infant mortality, but the effect is very small and not robust across specifications.

TABLE A1—EFFECT OF LOTTERY ON INFANT MORTALITY, LOGISTIC REGRESSION

	(1) Inf. Mortality	(2) Inf. Mortality	(3) Inf. Mortality	(4) Inf. Mortality
Lotteries	0.079	0.20	0.13	0.12
	(0.10)	(0.11)	(0.12)	(0.13)
Entry age		0.36	0.35	0.35
		(0.15)	(0.14)	(0.14)
Age at birth		-0.29	-0.29	-0.29
		(0.14)	(0.14)	(0.14)
Anticipated wait			0.072	0.089
			(0.16)	(0.19)
Year				0.0024
				(0.01)
Constant	-3.56	-3.93	-3.65	-8.17
	(0.50)	(2.56)	(2.58)	(19.43)
R2				
N Births	193	177	177	177
N Families	63	58	58	58

Note: Standard errors clustered by family in parentheses. Exponentiated coefficients.