DISCUSSION PAPER SERIES

DP13539

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ECONOMIC HISTORY

ISSN 0265-8003

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Discussion Paper DP13539
Published 18 February 2019
Submitted 17 February 2019

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JEL Classification: J13, J12, N33, N34, N31, D01

Keywords: Fertility, family planning, natural fertility, economic history, Economic Growth

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Gregory Clark, Neil Cummins and Matthew Curtis*

February 17, 2019

Abstract

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1 Introduction

Louis Henry, who pioneered the study of pre-industrial European demography with his studies of French parish registers, famously developed the notion that pre-industrial populations experienced natural fertility (Henry (1961)). In the regime of natural fertility there was no attempt by parents at parity-dependent birth control.

Evaluation of fertility control in pre-industrial Europe initially concentrated on the idea that parity-specific control was revealed by a decline in fertility relative to the "natural fertility" standard at later ages for women, and an earlier age for the last-born child. Employing this approach Knodel (1978), for example, found no evidence of parity-specific control in Germany pre 1850. The Cambridge Group concluded that for England before 1838 "... small groups may have been practicing family limitation, but the reconstitution evidence suggests that such behaviour was restricted to a small minority of the population, if present at all" (Wrigley et al. (1997), 461). But

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this approach assumes an absence of conscious fertility control through means of longer spacing of births throughout marriage and/or delay of marriage.

So in later work scholars looked for an effect of "net parity" on subsequent fertility, which allows for a mix of both spacing and stopping behavior. One such method is Cohort Parity Analysis (CPA). (David et al. (1988), David and Sanderson (1988), David and Sanderson (1990)).

However, both of these techniques have been criticized regarding their ability to detect the presence of a minority of controllers within the population. Both involve significant assumptions about the nature of control, or the characteristics of controllers versus non controllers. Thus Okun (1994), who tested the effects of these assumptions on the ability of these methods to detect control using simulations, summarized that "...neither M & m nor CPA can be used reliably to test alternative theories of the fertility transition when, as is often the case, the tests revolve around identification of a minority of controllers." (p. 222).

This led to a third method whereby hazards models for another birth were estimated controlling for economic circumstances, numbers of dependent children, net parity and mother's age. These models, in contrast to the earlier tests, have seemed to suggest significant elements of pre-industrial fertility control in response to economic circumstances (Amialchuk and Dimitrova (2012), Bengtsson and Dribe (2006), Cinnirella et al. (2017), Dribe and Scalone (2010)), to numbers of dependent children (Van Bavel (2004)), or to net parity itself (Anderton and Bean (1985), David and Mroz (1989a,b), Kolk (2011), Cinnirella et al. (2017))). Cinnirella et al. (2017), in what was a lead article in Demography, note "Our findings on the existence of parity-dependent as well as parity-independent birth spacing in England are consistent with the growing evidence that marital birth control was present in pre-transitional populations." Thus the state of the debate on parity-dependent birth control in pre-industrial European (or European derived) populations at present seems to be that such control, including in particular parity-dependent control, was evident. This conclusion has been congenial to economists' attempts to build growth models all populations control fertility, and that hence the large size of pre-industrial families in terms of births represents an optimizing choice given the circumstances of that era, not an absence of control (Galor and Weil (2000); Galor and Moav (2002)).

In this paper we use the accident of twin births to argue that in the pre-industrial Western European populations examined (including Quebec whose population derived mainly from France) there was no conscious attempt to control fertility. Families which experienced a twin birth ended up on average with one additional childbirth compared to those with only singleton births. In contrast in modern populations with fertility control, twins result in an increase in births within families that is significantly less than 1. But since families in the pre-industrial world with children had average numbers of birth of 6 or more, if they had target numbers of births they could adjust more easily to the accident of a twin birth than in a modern world where average births per family is only 2-3.[1] These pre-industrial families also often ended up, depending on relative twin and singleton death rates, with additional children surviving to age 14 and above than comparable families with a single birth at the same parity. There is no sign of any change in later fertility behavior in response to the accident of twining.

Our approach, using the random occurance of twin births has the advantage of being agnostic about the exact means that couples were being employing to limit births – stopping, or spacing, or some combination of both. If twinning induces earlier stopping we will detect the effect. If twinning

[1] See, for example, Braakmann and Wildman (2014). For a sample of 17,862 British women not receiving fertility treatments who had a birth in 2000-1, six years later the average total of births to those who had a multiple birth was only .66 greater than those who had a singleton birth (Table 6, standard error 0.103).

induces greater spacing between births we will also detect the effect. We can test simply whether the accidental occurrence of an additional birth, through twinning, creates any behavioral response in families towards limiting fertility either through earlier stopping or increased spacing. Since twinning also increases the number of dependent children in the family we will also find the effects of numbers of dependent children claimed by van Bavel (2004b) for Belgium if they exist in other populations. The twins test will not detect, however, deliberate pre-industrial fertility control in response to adverse economic conditions. However, it would be a bit surprising if families had the ability and inclination to reduce fertility, but only used that ability in response to external economic shocks and not to the equally significant shock of having many surviving children to provide for.

The methodology for using twins as a detector of parity-independent fertility is described in Section 2, the four datasets to be analysed are described in Section 3, results are reported in Section 4 and Section 5 concludes.

2 Using The Occurrence of Twins to Detect Fertility Control

Twins have been estimated to be about 1.8-2.7% of all births in pre-industrial European populations (0.9-1.9% of deliveries).[2] While twin births are more common among older women, they are largely a random event, with just a modest tendency to repetition within the same family, and, as we show, little or no connection with economic and social status. Thus with average numbers of births per married woman with some children of 6, about 5-11% of families with children would experience a twin birth in the pre-industrial era.

Consider a population where there is no control of fertility within marriage. In this case whenever and however the marriage terminates, the expected number of births will be increased by 1 by a twin birth, assuming the twin birth has no effect on the length of the subsequent birth interval. Also the increase in the final number of births will be the same whatever is the parity at the time of the twin birth.

Figure 2.1 shows the expected effect of twins on total births by parity at the time of the twin birth, with parity-independent fertility. If we define net fertility as the number of children born to the family reaching age 14 then with uncontrolled fertility the effect on net fertility will be smaller than 1 because of infant and child mortality. Here it also matters than twins showed a higher child death rate than singleton children (see table 3.2 below). With uncontrolled fertility the increase in the number of surviving children would be $2\theta_t - \theta_s$, where θ_t is the twin survival rate to age 14, and θ_s is the single child survival rate.[3]

In contrast, in populations where families control fertility, and have a target number of children entering marriage, twin births will induce a more muted increase in completed family size. Suppose, as seems reasonable, the target is defined in terms of children reaching age 14. Call this number for family i, N_i. Assume also the marriage lasts to the end of planned births. Then with only singleton births, the number of births needed to achieve this target will average $\frac{N_i}{\theta_s}$. With a twin birth the number of births required to achieve the target becomes

$$2 + \left(\frac{N_i - 2\theta_t}{\theta_s}\right) = \frac{N_i}{\theta_s} + 2\left(1 - \frac{\theta_t}{\theta_s}\right)$$

[2]Pison and Couvert (2004b)report such a rate for France 1700-89 (Figure 1), based on the Henry data.
[3]This number will be negative if twin survival rates are less than singleton survival rates.

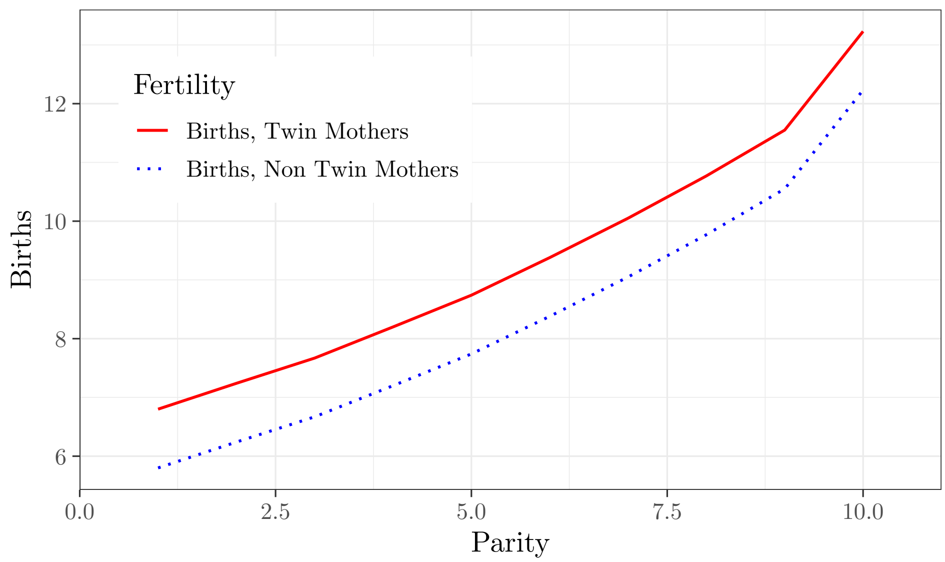


Figure 2.1: Expected

As long as $\theta_t > 0.5\theta_s$, the number of additional births required to achieve the target will be less than 1. If $\theta_t = \theta_s$then there will be no additional births.

However, if the twin birth is the last planned birth then it will add 1 additional birth (assuming $\theta_t > 0.5\theta_s$). Since all marriages end with a last birth, the fraction of twin births that occur as the last planned birth will just be the inverse of average family size. In England, for example, for men whose first marriages took place 1730-1879, average family size for those with at least one child was 5.9. Thus if each family had a planned target size, a twin birth would occur as the last planned birth 17% of the time. Suppose in general the fraction of births which are the last planned birth is ϕ. Then for a marriage reaching completion of planned births a twin birth will on average induce an increase in births of

$$2\left(1-\frac{\theta_t}{\theta_s}\right)(1-\phi)+\phi$$

The number of additional children reaching age 14 induced by the twin birth will, however, with controlled fertility be just $\phi\,(2\theta_t - \theta_s)$.

To summarise, we expect the effect of a twin birth on the change in final family size to be the following, where ϕ is the fraction of births which are the last birth:

All Births, No Parity-Dependent Control	1
All Births, Complete Parity-Dependent Control	$2(1-\frac{\theta_t}{\theta_S})(1-\phi)+\phi$
Surviving Births, No Parity-Dependent Control	$2\theta_t - \theta_s$
Surviving Births, Complete Parity-Dependent Control	$\phi(2\theta_t - \theta_s)$

Also, with controlled family sizes, the effect of twin births in increasing total births and family size will be stronger the greater the parity at which a twin birth occurs, since this will increase the probability that the twin birth is the last birth planned in this family. Indeed if the twin birth is not the last birth recorded for the family then with control it should have no effect on average net family size.

To give a sense of the magnitudes of the posited effects on births and net family size with and without families having target fertility, consider the case of England for marriages 1730-1879 where $\phi = 0.17$, $\theta_s = 0.65$, and $\theta_t = 0.55$. For uncontrolled fertility births increase by 1 on average with a twin birth, and net fertility increases by 0.44. For families with target net fertility and completion of that target, births increase by 0.43 on average with a twin birth, and net fertility increases by on average just 0.07 children.

To estimate the effect of twin births on family size, however, we have to control for the parity at which the birth occurs. The more births there are, the greater the chance of a twin birth. So just in terms of raw size, families with twin births will be larger. Twin births are also more common in older mothers. So at a given parity mothers giving birth to twins are on average slightly older than mothers of singletons. In the Families of England sample of marriages 1730-1879 this age difference is just that mothers of twin births at a given parity are 0.93 years older than for mothers of singletons. This will lead to mothers of twins having lower expected future fertility. It will bias the estimates downwards. But to have a truly comparable set of singleton births we thus have to also control for mother age at marriage. Thus to test for the effect of twin births on total births we postulate

$$NB_{pk} = \alpha_b DTWIN + \sum \beta_j DPARITY_j + \sum \delta_l DMAGE_l + \varepsilon \tag{1}$$

where NB_{pk} is the total number of births in a family where a birth is observed at parity p and mother age k, DTWIN is an indicator for that birth being a twin, the $DPARITY_j$ are indicators which are 1 at parity p, 0 otherwise, and the $MAGE_l$ are indicators which are 1 at the mother's age k, 0 otherwise. For populations where there is no control of fertility α_b will average 1. Where there is complete fertility control, α_b will vary depending on the relative child mortality rates of twins and singletons, and on average births per family.[4]

Similarly if we look at completed family size NS, we would estimate

$$NS_{pk} = \alpha_c DTWIN + \sum \gamma_j DPARITY_j + \sum \eta_l DMAGE_l + \varepsilon \tag{2}$$

For pre-industrial populations where there is no control of fertility α_c will now vary, but will often exceed 0 by significant amounts. Where there is complete fertility control but pre-industrial average family sizes, α_c will average 0.0-0.10 only.[5]

[4]There are cases with multiple twins in one family. These are included in the estimate, since if there is a twin at a later parity it will increase NB by the same amount whether or not the current birth is a twin, and so will not affect the estimate of α_b.

[5]Note that these estimates, while easy to implement, are parametric estimates, which assume no interaction effects between mothers age and parity. An alternative completely non-parametric estimate of the effects of twins uses just average family size for a mother of age k at parity p, where the birth is a singleton, versus average family size where the birth is a twin. This non-parametric estimate, however, does not use all the data since some cells in the mother age/parity matrix contain no twin births. We check our estimates in all cases using this non-parametric alternative. If we define a set of indicator variables for each combination of parity and mother age, $D(PARITY_j, MAGE_l)$ which has value 1 at parity p and mother age k, then the estimating equations will be

$$NB_{pk} = \alpha_b DTWIN + \sum \beta_{jl} D(PARITY_j, DMAGE_l) + \varepsilon \tag{3}$$

$$NS_{pk} = \alpha_c DTWIN + \sum \gamma_{jl} D(PARITY_j, DMAGE_l) + \varepsilon \tag{4}$$

With natural fertility a twin birth will add one child to the family regardless of which parity the twinning occurs at. If all families in a population are controllers with target family sizes, the effect of a twin birth on total births and on completed family size will be greater on average the higher is the parity at which a birth occurs. Thus Pison and Couvert (2004a) find that in France for fertility surveys 1975-1999 the chance a mother aged 25-9 who gave birth to a singleton at parity 0 gave birth to a second child was 0.77. Thus a twin at parity 0 induced an increase in family size of on average 0.23 children. But the chance of a mother with singleton births at parity 0 and parity 1 would have a third child was 0.52. Thus a twin at parity 1 induced a greater increase in family size of 0.48 (Pison and Couvert (2004a), p. 781, figures 10-11). With a population that is a mix of natural fertility couples and controllers, however, the relationship between parity and the magnitude of the effect would be complicated by the greater share of high parity births from natural fertility couples. But we can test whether the pattern of additional births with parity is consistent with an entire population of non-controllers by estimating the equations

$$NB_{pk} = \alpha_b DTWIN + \lambda_b DTWIN \bullet PARITY + \sum \beta_j DPARITY_j + \sum \delta_l DMAGE_l + \varepsilon \quad (5)$$

$$NS_{pk} = \alpha_c DTWIN + \lambda_c DTWIN \bullet PARITY + \sum \gamma_j DPARITY_j + \sum \eta_l DMAGE_l + \varepsilon \quad (6)$$

where $PARITY$ is the parity of the twin birth. With no fertility control λ_b,λ_c should be 0.

One other way we can check for a behavior response to the accident of twinning is by looking at the length of the birth interval following a twin birth compared to a singleton at a given parity and mother age. If twin births are going to add exactly one birth to the total number of births in a family then this interval should be the same following a twin birth as following a singleton.[6]If there is a behavioral response then the interval will potentially lengthen as families seek to reduce future births, as would be predicted based on the finding of van Bavel (2004b), for example, that the numbers of children under age 10 increases subsequent birth intervals. Thus we estimate the parameter ω in the equation

$$Interval_{pk} = \omega DTWIN + \sum \beta_j DPARITY_j + \sum \delta_l DMAGE_l + \varepsilon \quad (7)$$

as a potential detector of behavioral responses to twinning.

3 Data

The data for analysis are multiple, independently constructed family history databases. The Families of England (FOE) database for England, the Henry data for France, the CAMPOP data for England, and the Quebec IMPQ (*Infrastructure Intégrée Des Microdonnées Historiques De La Population Québécoise*) database. The latter three databases use the techniques of family reconstitution:

> Life consists only of birth, marriage and death. If the dates...of each member of a family are known, the reconstitution of that family is complete

[6]Since infant mortality affects this interval twinning could change the interval even without any behavioral response. But the chance of at least one surviving child after a twin birth will not be too dissimilar to the chance of a surviving child after a singleton birth. Twins have higher infant death rates, but there are two of them.

(Wrigley et al. (1997), p.13). There are, however, in these databases many people with a baptism record but no burial record, or with a burial record but no baptismal record.

The Families of England database is a set of complete family genealogies for English families with births in the interval 1730-2007, comprising now 296,489 individuals. Since this is a new database, we detail how it was constructed in Appendix A. We constructed two samples of twins from this database. The first sample was drawn from men whose first marriage occurred 1730-1879, and whose fertility record appears complete. The dates here were chosen as those for which marriages appear to have largely uncontrolled fertility, as measured by births per father. The range in date for the twin births in this period is thus 1730-1915.[7] Of the near 60,000 births attributable to these fathers, 471 deliveries were identified as a pair of twins, a twinning rate of 1.6% for all births. The second comparison sample was of twin births 1900-1949 to men whose first marriage occurred 1880 or later. This is a period where couples were clearly exerting some fertility control. Here there were near 31,000 births and 406 pairs of twins (2.6% of births). The higher proportion of twin births in the early twentieth century is quite consistent with the history of twinning. National figures for twinning rates for England starting 1938 show a twinning rate of 2.5% 1938-49 (Pison and D'Addato (2006), figure –).

Detecting twin births in historical data sources is not a trivial exercise. In particular, in England where attendance in the established Church of England was not particularly strict in the eighteenth century and later, children were sometimes not baptised until years after their birth. Thus the baptismal records contain cases where non-twins are baptised on the same day. The Families of England data has the advantage that births are registered to within a quarter of a year for 1837 and later. Also 1841 and later children appear on census records, where if they are twins they will be listed with the same age. Thus for births after 1830 we have multiple other sources indicating whether they are truly twins or not. For the second period, births 1900-1949 we know also the mother's name from the birth record 1911-1939, which for rare names almost conclusively identifies twins in these years. For births 1900-1911 we see both children if they survive in the 1911 census. Thus the accuracy of twin attributions is high 1900-49. For births 1745-1830, however, we must rely on baptism records. Where we have complete fertility records, however, we can see cases where a multiple baptism is preceded by a gap of more than three years in baptisms, and we have not included such potential non-twin births.

The French data are the complete Louis Henry led demographic survey of 41 rural French villages, 1670-1895.[8] To allow mother's dates of death to be observed we look at twin births just in the interval 1670-1829. The period covered by the Henry data covers a period 1670-1789 which traditionally was regarded as being one of natural fertility, and a period 1789-1829 when families were believed to be exercising some fertility control. The Henry data contains a field indicating whether a child was a twin. Because of the Catholic practice of baptising children as soon as possible after birth, the detection of twins is reliable in the Henry database.

The CAMPOP data was assembled in the same way as the Henry data for 26 English rural parishes. It also has a field indicating whether a child was a twin. However, as noted above, in the CAMPOP data twins are only detected through the baptismal records. The baptismal records

[7] We counted births for all marriages for these men, which explains why a man first marrying before 1880 could have twins in 1915.

[8] The summary papers of the Enquête Henry are: Henry (1972); Henry and Houdaille (1973); Houdaille (1976) and Henry (1978). A summary of all studies using the Henry data (before 1997) is listed in Renard (1997), and detailed discussion of the database can be found in Séguy and Méric (1997); Séguy (1999); Séguy and Colençon (1999); Séguy and la Sager (1999); Séguy et al. (2001). See also Cummins (2013) for a recent analysis of the Henry data for fertility and wealth patterns during the fertility decline.

sometimes explicitly note children baptised on the same day are twins, but in other cases are silent. We do not know how the creaters of the CAMPOP data concluded that the children they identified as twins were indeed twins. We shall see below that we can test for the reliability of the twin designation using the same-gender ratio for the twins. On this test the share of same gender children is too low in the CAMPOP data, implying significant numbers of misidentified twins.

The *Infrastructure intégrée des microdonnées historiques de la population du Québec* (IMPQ) is a set of family reconstitutions of the Catholic population of Quebec using baptisms, burials and marriages 1621-1849.[9] Not all births are linked to death records. We only consider those born in the interval 1621-1835, interpreting the lack of a death record as survival until age 14. Again Catholic doctrine strongly encouraged prompt baptisms, going as far as threatening delinquent parents with excommunication. Thus there should not be any significant occurrence, as in the English baptismal data, of non-twins being baptized on the same day. To ensure that completed family are observed, we restrict the sample to families where the parents were married in Quebec and the birth parishes of all children are recorded.

The unit of observations could potentially be one of three things: births per mother, births per father, or births per marital union. For our purposes the ideal measure is all births per mother or all births per father, since if a marriage is terminated early by the death of one party, the other has the option to remarry to attain the desired family size, if there is controlled fertility. We would also ideally use only mothers who reached age 40, or fathers who reached age 45, so that we observe close to complete reproductive intervals. However, in the Henry data, the CAMPOP data, and the Quebec data, birth and death dates can be missing. So in the main estimation tables we include all families except those where the parents are known not to have attained ages 40 for women and 45 for men (Henry and CAMPOP), or 45 for men (FOE and Quebec).

For the Henry and CAMPOP data sets we take the unit of observation as marital unions, simply because of the way the data was constructed. For the FOE data, which was constructed around fathers with rare surnames, the unit is total births per father. Cases where first wives died before age 40 are included since men had the option of remarrying. For the Quebec data we can measure either total births per father, or per mother, or per marital union. Here we have chosen to use births per father because of the fact that twin births in Quebec were associated with a significant increase in observed maternal mortality in childbirth (from 1.1 to 3.5 per 100 births). If we instead use fathers then there is no issue of twinning induced parent mortality. If a mother died in childbirth then the father would often remarry. Some fathers will have died before their wives reach the end of their reproductive careers. But this effect will be found equally among twinning and non-twinning families.

As noted, we can test the accuracy of our twin attributions by looking at the gender composition of twins. Two children who are not twins will have a roughly 50% chance of being same gender.[10] Monozygotic twins, of course, have the same gender. They show a constant rate across a wide variety of societies at around 0.7-0.9% of children.[11] Twining rates for dizygotic twins vary substantially across time and across populations. But for each twining rate there will be an implied same gender ratio among twins. Thus we can test how accurate our twin attributions are by looking at the same gender percentage. And we can indeed estimate what fraction of supposed twins are not indeed

[9] **?**. See Bourque (2011) and Dillon et al. (2018).

[10] We assume in this calculation the same number of males as females at birth. In practice the ratio is about 1.05 boys per girl. However this means that two non-twin children will share gender 50.03% of the time compared to 50%. So the bias created by the simpler 50:50 assumption is very small.

[11] "The proportion of identical twin births is always between 3.5 and 4.5 per 1,000, regardless of the mother's age, birth order, or geographical origin." (Pison and Couvert (2004), p. 769).

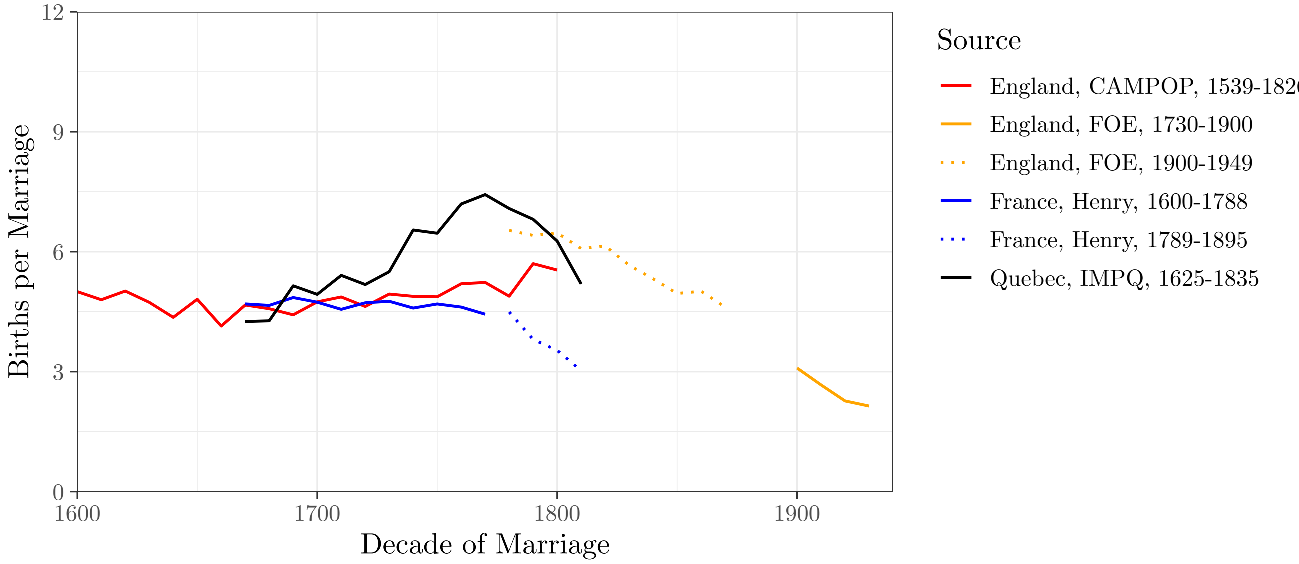


Figure 3.1: Average Births, by Sample and Decade

twins by comparing their same gender ratio to this benchmark. Thus for the Families of England sample for twins born in marriages before 1880, the fraction which were same sex was 0.66. Based on the observed twinning rate of 1.6% per thousand we should observe a same gender rate of 72% or more, implying that we have reasonable accuracy in terms of the children identified actually being twins. However, if we are missing some twins the twinning rate could be higher than 1.6%, and the implied same-gender ratio correspondingly lower. The Henry and Quebec data similarly shows same gender shares in the region of 0.66-0.68. For the CAMPOP data, however, the same sex ratio for their identified twins is only 0.55, implying that the many of the supposed twins are actually singletons. Table 3.2 shows actual and expected same-gender shares for the various twin samples. This test suggests that the Henry and IMPQ data is of the best quality, followed by FOE, but with CAMPOP possibly including many non-twins in their twin attributions.

The effects of attributing twins when the children in a family are actually singletons will be to bias the estimated coefficients on DTWIN towards 0. Thus mistakes here in the data will bias us towards finding evidence of fertility control.

Table 3.1 reports the summary statistics for the studies used: how many births, how many potential twin births, and the years covered. Table 3.2 reports the diagnostic parameters for the twin samples: the survival rate to age 14 of singleton births and twins, the average number of births per family, the same-gender ratio for the putative twin births, and the expected same-gender ratio. Figure 3.1 reports the mean number of births - for women surviving to at least 40 - for each of the samples. Two of the six samples, France for marriages 1790-1829 and England 1900-49 show signs of fertility control in having substantially lower numbers of births per marriage or per father.

Twinning was mostly uncorrelated with the observable social characteristics of families. Table 3.3 shows the regression coefficient for each of a variety of parent characteristics singly on an indicator for whether a birth is a twin: age of mother (in years), parity, the 1st-2nd birth interval as an

Table 3.1: Summary Statistics for Studies

Country	N Births	N Potential Twins	Potential Twin Rate	N Parents	Year Min	Year Max	Source
France	130,746	3,756	0.029	35,849	1600	1788	Henry
France	49,742	1,520	0.031	16,018	1789	1895	Henry
England	59,687	950	0.016	10,153	1730	1949	FOE
England	31,001	814	0.026	11,287	1900	1949	FOE
England	76,959	1,355	0.018	7,731	1539	1826	CAMPOP
Quebec	361,127	8,185	0.011	51,657	1625	1835	IMPQ

Years refer to observed births.

Table 3.2: Twin Parameters

Sample	Survival Rate, Non-Twins	Survival Rate, Twins	Average Births per Marriage	Same-Sex Ratio, Twins	Same-Sex Ratio, Expected
France, pre 1789	0.71	0.47	5.31	0.64	0.62
France, post 1789	0.70	0.41	4.66	0.68	0.61
England, CAMPOP	0.70	0.46	4.66	0.59	0.70
England, 1780-1879	0.65	0.55	5.96	0.64	0.72
England 1900-49	0.91	0.72	3.37	0.55	0.63
Quebec	0.70	0.49	5.36	0.68	0.81

indicator of fecundity (which correlates with total births), literacy of mother, literacy of father, education of father, occupational status of father, and wealth of father. Mother age correlates significantly with twinning rates in all except the Henry data.[12] For the FOE database, for example, for marriages 1730-1879 the implied twin birth rate at mother age 20 is 0.98%, but at 40, 2.07%. Parity is always significantly correlated with twinning rates. But parity will be highly correlated with mother age, so part of the parity effect will be an age effect. However, Pison and Couvert (2004a) show that even controlling for mother age there is a positive parity effect (figure 4, p. 770). Pison and Couvert (2004a) also report "These differences...have been interpreted as resulting from a physiological phenomenon (Henry, 1975), though the mechanism is unknown." (p. 770). The correlation with age of the mother and parity is not a problem, since we control for both of these in the estimations.

A more important issue is whether twinning correlates with fecundity. As can be seen in table 3.3 our proxy for fecundity, the 1-2 birth interval, is never significantly associated with higher twinning rates (in Quebec a longer interval implies higher twinning rates). Since Pison and Couvert (2004a) report "The most fecund couples have a greater propensity to bear twins" (p. 785) this seems surprising. However, the basis of Pison and Couvert's assertion is an association between the first

[12]In the Henry data only a small proportion of mothers have birth dates listed, so the standard error of the estimated mother age coefficient is very large. Thus we cannot rule out a substantial mother age effect also in this population.

Table 3.3: Twinning Correlates

Variable	France, Henry	England, CAM-POP	England, FOE	Quebec, IMPQ
Mother's Age	-0.0006	0.0005***	0.0009***	0.001***
	(0.012)	(0.0001)	(0.0001)	(0.00002)
Parity	0.03***	0.002***	0.004***	0.002***
	(0.002)	(0.0002)	(0.0002)	(0.00004)
1st->2nd Birth Interval	0.002	-0.002**	-0.002	0.001***
	(0.004)	(0.001)	(0.002)	(0.0003)
Literate Father	0.003	0.0001		0.0005
	(0.019)	(0.002)		(0.0006)
Literate Mother	-0.0005	0.002		-0.003***
	(0.026)	(0.002)		(0.0006)
ln(Wealth), Father			0.0001	
			(0.0003)	
Occupational Rank, Father			-0.000002	-0.004
			(0.00004)	(0.004)
Educated Father			-0.004	
			(0.003)	
Note:	Standard Errors in Parentheses *p<0.1; **p<0.05; ***p<0.01			

birth interval and the chance of a twin birth as the first birth. For first birth intervals in the range 10 months to 36+ months there is no association between the length of the interval and the chance of a twin birth, either in early twentieth century France or in the Henry data. The positive association between a short interval and twinning only appears for first birth intervals of 8-9 months (but not for even shorter first birth intervals). However, in the modern USA the average pregnancy length for twin births is 35 weeks. Thus in the 8-9 months category will be an unusual proportion of twin births (Pison and Couvert (2004a), figure 15, p. 787, appendix table 2, p. 792). Thus the Pison and Couvert data is perfectly compatible with our finding of no positive association between fecundity and twinning rates.

Pison and Couvert (2004a) also report that dizygotic twins "have a tendency to be repeated among the same women" (p. 770). A test for whether some couples have a higher tendency to produce twin births comes from looking at the incidence of multiple twin births for a given father. In the FOE data with 473 twin deliveries for marriages before 1880 there are 18 cases of two such deliveries in a family, and 1 of three sets of twins. If we randomly allocate twin deliveries across the observed deliveries per father at the observed twining frequency we find 11 cases of two such twin deliveries in a family (with a standard error of 3). This implies only a slight tendency in some couples towards twin births. But any distorting effects this would have on the estimation will be small. We see below with the FOE data for 1730-1879 that the 473 families produce 473 extra births. The familial association of twinning accounts for 9 of these extra births. Thus the familial association will bias upward the estimated effect of twinning on births by 0.019. The simulation also implies that from the perspective of couples twins represent overwhelmingly random and unpredictable events.

4 Results

Table 4.1 summarizes the estimates of α_b the effect of a twin birth on total births for the six population samples we have, for equation (1) which controls for mother age and parity. In three of the four pre-industrial population samples - FOE, CAMPOP, Henry, and Quebec - the estimate of α_b is very close to the value 1 predicted by natural fertility. In the case of Quebec the standard error of this estimate is only 0.05, so this is a very precise estimate. In these cases the estimate of α_b is also significantly above the value that would be predicted from having control and a target family size. In the case of the CAMPOP data, however, the estimate of α_b is closer to that produced by control than by natural fertility, though it is not statistically different from 1 at the 5% level. However, as we saw in table 3.2, the CAMPOP twins show a greater deviation from the predicted same-gender ratio for twins than any other sample. That deviation implies singleton children being misclassified as twins, and will bias the estimate of α_b towards 0.

For the populations exercising at least some fertility control, France 1800-1829 and England 1900-1949, α_b falls statistically significantly below 1 for England 1900-49 at the 1% level. It is not different statistically from the value predicted for families with a target family size. For France post 1800 the expected α_b with fertility control, because of the low survival rate of twins, is close to 1 at 0.87. The estimated α_b at 0.89 is close to that predicted by fertility control, but the standard error is large enough that the estimated coefficient is compatible with either control or its absence. So the estimates for births are consistent with the earlier populations having no control of fertility, and the later ones at least some controllers.

What about the possibility that while the majority of pre-industrial families were not exercising any parity-dependent control, a significant minority were exercising such behavior? Given the standard errors in table 4.1 while we can be 95% confident that no more than 3% of the Quebec population had a target family size, for both England and France pre 1789, given smaller sample sizes, at the 95% confidence interval we can only conclude that 35% of less of families were exercising control. We can state, however, that in France pre 1789 with 75% probability less than 10% of families had a target family size they controlled. For England pre 1880 with 68% probability less than 10% of families exercising control (and for Quebec we can conclude that with 98% probability). Thus the paper contributes evidence that most likely almost no families in France pre 1789, England pre 1880, and Quebec pre 1835 exercised any targeting behavior with respect to fertility.

Note also that while with the samples used here we may not be able to reject any pre-industrial targeting behavior at the 95% confidence limit in France and England, in the future much larger bodies of data are likely to become available. Thus while in England our sample includes 55,533 births for marriages before 1880, there were 28,700,565 English births 1838-1880. For those with rarer surnames, 10% or more, twins can be identified with high reliability from the birth register. Thus it will be possible using curated family trees from Ancestry and the other genealogical services to get sample sizes that will conclusively establish whether there was any parity-dependent control of fertility before 1880 (recent studies have exploited such curated data to investigate the inheritance of longevity. See for example, Kaplanis et al. (2018), Graham et al. (2018)). Similarly in Quebec research teams are at work extending the information on births and deaths from 1850 forward to 1917. Thus the sample size for pre-industrial Quebec, where already we get a good estimate of the likely share of controllers, is likely to be 2-3 times as large within 10 years.

If we instead estimate α_b non-parametrically from equation (3), where interactive effects between parity and mother's age are allowed, we get a very similar set of results as in 4.1. Figure 4.1

Table 4.1: Twin Effect, births

Sample	Expected α, No Control	Expected α, Control	α	Standard Error	N
Pre-Fertility Decline					
France, pre 1789	1	0.73^R	1.02	0.07	65,722
England, CAMPOP	1^R	0.75	0.83	0.09	76,239
England, pre 1880	1	0.43^R	0.99	0.12	51,624
Quebec	1	0.65^R	1.05	0.05	329,141
Post-Fertility Decline					
France, post 1800	1	0.87	0.89	0.12	18,454
England 1900-49	1^R	0.6	0.76	0.10	25,741

^{R}: Hypothesised Coefficient Rejected at $p = .05$

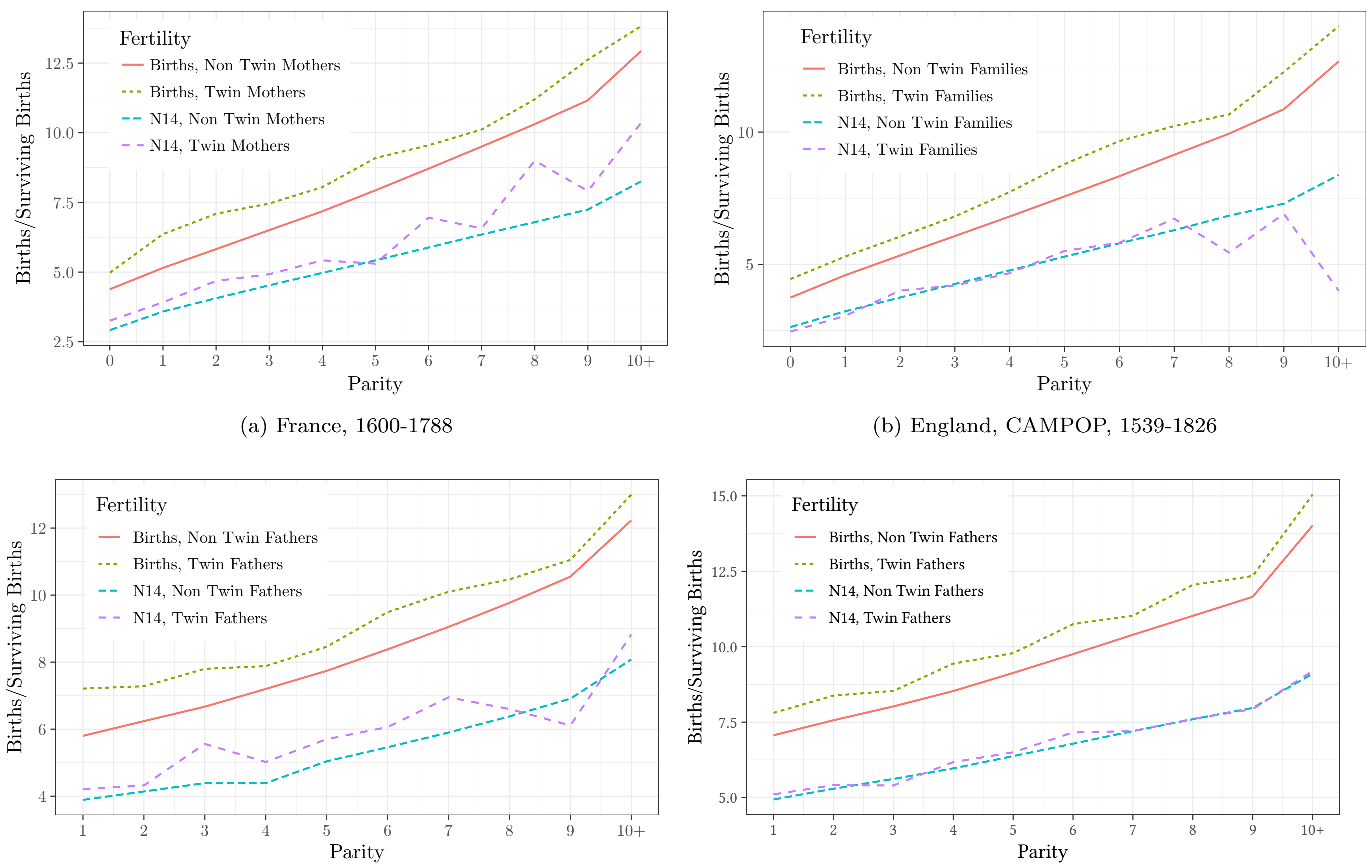
illustrates the pattern of total births and completed family size (measured here as the number of children attaining age 14) at each parity for twin births versus singleton deliveries for France, 1670-1789 (Henry), England 1538-1826 (CAMPOP), England 1730-1879 (FOE), and Quebec 1621-1835 (IMPQ). In each case there is clear sign that the increase in total births created by twinning has the same magnitude independent of the parity at which the birth occurs. Again this is consistent with the whole population exercising natural fertility. Table 4.2 shows the formally estimated coefficients for λ_b and λ_c, the effect of parity at the twin birth on total births and completed family size, from equations (5) and (6) for the supposed natural fertility populations. These coefficients are all not distinguishable from 0 for the populations with hypothesized natural fertility. Thus there is sign that the increase in family size from twinning was as strong for the first birth being a twin as for births at high parities. This is entirely consistent with a whole population of non-controllers.

Table 4.2: The Correlation of Parity and Twinning, births

Sample	γ_b	Standard Error	N	γ_c	Standard Error	N
France, pre 1789	-0.03	0.03	65,722	0.05	0.04	24,609
England, Campop	0.06	0.04	76,239	0.03	0.04	76,239
England, pre 1880	-0.11	0.05	51,624	-0.06	0.05	48,977
Quebec	-0.00	0.08	329,141	-0.03	0.07	329,141

Parity coefficient is from OLS estimation of equations 7 and 8

Because of the higher infant mortality rates for twins the effects of twinning on completed family sizes with natural fertility are more muted than on births, and potentially harder to distinguish from



(a) France, 1600-1788

(b) England, CAMPOP, 1539-1826

(c) England, 1730-1879

(d) Quebec, 1621-1835

Figure 4.1: Observed Births by Parity at Twin Birth

what would happen with a population of fertility controllers. Table 4.3 summarizes the estimates of α_c in equation (2), which shows the effects of twins on numbers of children reaching age 14 controlling for parity and mother's age. For all the "natural fertility" populations with no parity-specific control there should be an increase in numbers of surviving children of 0.22-0.44 as a result of twinning, while with family size targets and control the increase would be only 0.04-0.07. The empirical estimates confirm a rise in net fertility significantly above what would be expected with target family sizes in England (FOE), France, and Quebec. The only sample where there is no rise in net fertility is CAMPOP. In the other three cases the rise is as large, or even larger, than would be predicted from twin versus singleton survival rates.[13] Thus the evidence for net fertility in England (FOE) implies with 95% confidence that no families were operating with target family sizes.

For the two populations which were controlling fertility, the rise in net fertility from twinning is again greater than would be predicted from control. This can be explained by these populations containing a mix of families, some with fertility controls, some without.

Table 4.3: Twin Effect, Surviving Children

Sample	Expected α, No Control	Expected α, Control	α	Standard Error	N
Pre-Fertility Decline					
France, pre 1789	0.24	0.04^R	0.37	0.11	24,609
England, CAMPOP	0.22^R	0.05	0.00	0.08	76,239
England, pre 1880	0.45	0.08^R	0.72	0.14	48,977
Quebec	0.28	0.04^R	0.25	0.04	329,141
Post-Fertility Decline					
France, post 1800	0.12	0.03^R	0.37	0.15	8,132
England 1900-49	0.52	0.15^R	0.38	0.10	25,642

^{R}: Hypothesised Coefficient Rejected at $p = .05$

We also estimated for the pre-transition populations ω in equation (7) to test whether there was any lengthening or shortening of the birth interval immediately following a twin birth, compared to singelton births at the same parity and mother age. Parity-dependent birth control could take the form of longer spacing after twin births. In all cases the post twins birth interval does not differ significantly from the interval after a singleton birth. This is consistent with the evidence above of no behavioral response to twinning. For example, for Quebec where there is the most abundant data, the point estimate of the effect of a twin birth is that is shortens the following birth interval by 4 days, with a standard error of 7 days.

[13] For FOE the rise in net fertility at 0.70 is significantly greater than the 0.44 that would be predicted with natural fertility, though only at the 5% level of statistical significance.

5 Conclusion

There is good evidence that at least in some Western European and Western European derived populations - England, France and French Quebec - there was a period where families exercised no parity-dependent fertility control within marriage, for at least the great majority of the population. The accident of twinning produced no behavioral response. A family which had a twin birth ended up with one extra child born, at whatever parity the twinning occurred. Depending on singleton and twin survival rates it also generally saw some fractional increase in completed family size. Families were not attempting parity-dependent fertility control within marriage, even in England as late as the period of the Industrial Revolution, 1780-1879. Interestingly this was an era where there were already significant investments in education and training even for poorer English families. For children born 1840-60 in the FOE database, 31% were at school or in training ages 14-16 (and only 54% at work). Yet there was no sign that parents were limiting births within marriage to control such expenses.

Clark and Cummins (2019) argue that the finding of Cinnirella et al. (2017) of substantial parity dependent birth control in England 1538-1850 was just an unfortunate artifact of the estimation method used. The absence of any behavioral response to twinning in England before 1879 reinforces the conclusion parity-dependent spacing was also absent. The increase in parity induced by a twin birth would on the Cinnirella et al. (2017) estimates induce much longer spacings of subsequent births. That longer spacing would lead to little or no increase in total births from twinning. Yet even within the CAMPOP data they employed, twining leads to a significant increase in total births.

The findings with the Henry data for France pre 1789 also cast doubt on the earlier claim of David and Mroz (1989a) and David and Mroz (1989b) to have found similar evidence for control through birth spacing in France 1749-1789.

With enough data the non-response to twinning implies that twins can be used as an instrument for family size, as an exogenous source of variation in family size. However the variance in size induced by twinning is a small component of the overall variance, so there would have to be enormous amounts of data to estimate with any precision the coefficients linking child outcomes to family size, especially if the relevant measure is completed family size as opposed to births. More promisingly, the response to twins, by showing that families were not choosing family size, suggests that in these populations we can consider all the variation in family size as exogenous. In particular, in England, for marriages in the years 1780-1879 fertility was uncorrelated with family social status. Average completed family size was the same on average for the poorest as for the richest families. Thus in this period in England we can get very simple estimates of the effect of family size on child outcomes, since the variation in family size is exogenous to the social status of the family, and not a choice made by parents. In another paper, Clark and Cummins (2018), two of the authors estimate these effects of child quantity on child quality. While the quantity effect generally produces a statistically significant negative effect on child quality, the effect is very small in terms of magnitude (Clark and Cummins (2018, 2015)). In the Quebec sample, the simple estimates might be biased as fertility appears to be negatively correlated with social status. However, this sample is large enough to use twins as an instrument. Again, we find that child quantity has a statistically significant yet very small negative effect on child quality.

The lack of any fertility response to twinning by families in England throughout the years 1780-1879 is also interesting in light of recent theories of the Industrial Revolution. By 1780 the rate of technological advance had clearly increased significantly from that of the previous six centuries in England. Technological advance has been attributed to a democratization in England of the ideas of the Enlightenment, an intellectual movement which emphasized rationality, experiment, and embrace of novel theories of both science and society.[14] While the originators were an elite group of philosophers and scientists, the claim is that in England by the late eighteenth century these ideas had filtered down through lectures, demonstrations, and popular writings, to the mechanics and artisans whose many small scale innovations underpinned the Industrial Revolution. The Industrial Revolution was mostly a product of a new way of thinking. Thus "Economic change in all periods depends, more than most economists think, on what people believe." (Mokyr (2010), p. 1). If the foundation of the Industrial Revolution was indeed new, more instrumental ways of thinking about the world, it is puzzling that this instrumentality did not also induce parity-dependent fertility control within marriage long before 1880.

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[14]See, for example, Mokyr (2010).

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A Families of England Database

The Families of England database aims to construct a complete genealogy of a representative set of English families from 1730 to 2018, a period of 9 generations, using public data sources. The database has been constructed primarily to examine the process of social mobility over multiple generations. But it contains substantial amounts of information also on geographic mobility, fertility, and mortality.

The database currently contains 296,494 individuals. The database is still a work under construction. The intergenerational linkages for these individuals are substantially complete for those born before 1930, but for those born later there is more work to be done on establishing these links. Currently there are 184,902 children linked with a father. There are 114,716 children of 27,515 fathers where the complete fertility history of the father is known. However there is substantial ongoing work on establishing occupations, educational status, dwelling values, and wealth at death for each individual. We expect to add considerably more data on all the social outcome variables.

To enable high linkage rate with the sources we have we adopted the strategy of following families with rare surnames, and follow descent in those families along the male line. The vagaries of English spelling, and the varied ethnic background of the population in different parts of England, ensures that a substantial minority of the English population, even in 1800, held surnames that were shared with modest numbers of other individuals. To ensure that there is no bias in this procedure we will also link many of the daughters to their husbands, and wives to their fathers, to check that mobility and other characteristics along the female line have the same character as with the male line. Using such rare surnames we can achieve very high linkage rates between parents and children.

For men born 1850-1949, and living to reproductive age, the linkage rate to a father for those born with one of the target surnames is greater than 90 percent. Typical linkage rates for historical intergenerational databases, using all surnames, at least in the US, are only around 20%.[15]. These linkages are also of high reliability in the years 1800-1930, since there are multiple sources in many cases identifying parents - censuses, birth records, marriage records, passenger lists - and there are few alternative candidates who can get confused with the target individual. Thus for a sample of 7,626 recorded rare surname births 1860-1879, we identify a father or mother for 88%[16]. The reasons for failing to find at least one parent in the other 12% of cases are various. In some cases the name likely was misspelled in the birth record, and the person does not belong in the surname lineages used to form the sample. Of those not linked 60% show no further appearance in any record after their birth under the birth name. Likely in most of these cases the name is just misspelled on the birth register. In others the child dies before appearing in a census, or their father dies, or they are living with grandparents in the census, or the family emigrates[17]. Thus one third of those born not linked to a parent died before age 10. Again, in contrast, historical intergenerational databases in the US using the general population are claimed to mismatch one third of individuals to their parents (Bailey et al., 2017). A reflection of the likely high success rate in making linkages is the observed intergenerational correlation of occupational status. This is 0.7, which is much higher than that observed in other census based historical linked samples.

Though the numbers of recorded births for men and women is similar, and the match rate to fathers for the births is also similar by gender, the final dataset of family size by father is missing at least 12-14% of girls. This is because children in families can also be identified from the existence of a death record, or from their presence in a census or other record, where the birth was not recorded under the correct family surname. But adult women will only appear in a death or census record if they remain unmarried. Thus more sons are identified from such records, absent the birth record. The absence of these women should be neutral, however, between twin and non-twin families.

To ensure a representative sample of people in each generation we have followed the strategy of including in the database all individuals bearing one of the target surnames whenever there is a birth, death or marriage record under that surname. We also try and follow the lineages of those

[15] **?**, for example, link only 20% of adult sons to their fathers in England between 1851 and 1881.

[16] In some cases, where the child is illegitimate, only the mother is listed on birth records.

[17] We could identify the father by getting the birth certificate, but this is prohibitively costly.

who emigrate from England, typically to Canada, Australia, the USA, and New Zealand.

The genealogical linkages have been established in two ways. For a substantial subset of the data, 67,305 individuals we constructed the genealogical links ourselves. The other 229,189 individuals are from genealogies constructed by members of the Guild of One-Name Studies, a society devoted to studying the history and genealogy of rare surnames. The use of these Guild genealogies raises issues of selectivity, since it is more likely that a rare surname will be included in a Guild study if there is a current bearer of higher social status. But we do extensive checks on the representativeness of these Guild contributed surnames, and find that at least for the 19th century they have average social status.

In both our reconstructions and those of the Guild genealogies the familial linkages - assigning fathers, mothers, and spouses - are established using a wide range of evidence. For England there are census records 1841, 1851, 1861, 1871, 1881, 1891, 1901, 1911. There is the Population Register of 1939. There is the register of births, deaths and marriages 1837-2005. The birth register 1912-2005 gives the surname of the mother. There are selective parish registers of births and marriages 1730-1930. There are probate records nationally, 1858-2018, and for the Canterbury and York Ecclesiastical courts 1750-1858. There are passenger lists for those leaving the UK 1890-1960, and for those entering the UK 1878-1960. There are Electoral Registers 1900-2012.

In recalcitrant cases in England we can, at cost, order the actual birth certificate which list the father and mother, or marriage certificate which lists marriage partners, their occupations and those of the fathers. We plan on doing this for a select sample of people marrying around 1990, so that we can get their occupational status, where they would typically be born circa 1960, as well as the occupational status of their fathers born circa 1930.

It is possible in many cases to check proposed familial linkages against genealogies uploaded by ancestry.com members. These genealogies are not always reliable. But the better ones cite source documents which can be inspected to see if the link is sound.

Ancestry.com records the age at death of many migrants from the England to Canada, Australia, NZ and USA. For Australia the voting rolls 1903-1983 give occupations. For the US the censuses 1850-1940 record occupations. Canada and New Zealand also have some occupational information from voting rolls. However, wealth at death is generally not available for migrants outside England and Ireland.

The social status indicators we have are age at death, wealth at death, schooling, occupation, location, and first names of children.

Wealth at Death: For England and Wales the Principle Probate Registry records whether someone was probated, and the value of their estate for all deaths in England 1858-2018. This information is the most comprehensive and unusual outcome result that we have for this database. The probate information is searchable at https://probatesearch.service.gov.uk/#wills. However, the estate values 1996-2018 are now obtainable only at cost of 10 pounds per person.

Schooling and Training: The censuses of 1851-1911, and population register of 1939, record whether anyone aged 10-19 is still attending a school, which gives us a measure of education for the earlier years. From the previous NSF project we have a database of all students who attended Oxford or Cambridge, 1750-2015. But this constitutes only 1-2% of each cohort. Complete records are available for attendees at the Royal Military Academy Woolwich (1790-1839) and Royal Military College Sandhurst (1800-1946). Complete records are available for Masters and Mates Certificates, 1850-1927, UK Medical Registers, 1859-2015, UK, Civil Engineer Lists, 1818-1930, UK, Electrical Engineer Lists, 1871-1930, UK, Mechanical Engineer Records, 1847-1930, UK, Articles of Clerkship, 1756-1874. From all these measures we can construct indices of educational attainment for people in the database born before 1900.

Occupation Status: The censuses of 1851-1911, and the Population Register of 1939 record occupations, so we can estimate adult occupations for the cohorts born 1920 and before. Passenger lists give occupations for international travelers up to 1960. Birth certificates record the occupation of father's, and from 1995 on that of mothers also. Marriage certificates record the occupations of husband and wife, and of fathers. So for a select sample we can estimate occupations for people born up to around 1980.

Dwelling Value: From the electoral census of 1999-2012 we have the address where adults were living in 1999-2012, from which we can infer using the Land Registry the property value in 2017. This gives an indirect measure of family income.

Children's First Names: Children's first names are a good proxy for family social status in modern generations. Using records of Oxbridge attendance and property values we can assign status measures to parents based on their child name choices.

After completing the genealogical links, and the status information, we will have potentially the following information for each person in the database

Date of birth, longevity, wealth at death, educational attainment, occupation, birth location, fertility, child mortality, death location, birth order, number of siblings, age at marriage.