Frontiers of the family: fertility, marriage, and human capital in Quebec 1620–1970

By

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Abstract

The last two centuries have seen dramatic shifts in labor and demography: a precipitous drop in fertility rates, a dramatic increase in human capital, and the economic empowerment of women. Economic theorists often hypothesize that these transformations played a vital role in the transition to modern economic growth. This dissertation uses historical vital records from the Canadian province of Quebec and applied microeconomic methods to test the assumptions that underpin these theories. First, it shows that parents did not target a family size before the demographic transition. Second, it shows that family size only had a modest impact on the accumulation of human capital. Third, it shows that assortative marriage, unlike family size, mattered long before many might expect. Quebec, with its unusual demographic regime and exceptionally high quality demographic data, provides solid empirical evidence to support these claims.

The IMPQ (*l'Infrastructure intégrée des microdonnées historiques de la population québécoise*) is a large new database of family reconstitutions from baptism, burial, and marriage records (IMPQ 2020). It integrates two previous databases, the BALSAC database and the RPQA (Project Balsac 2020, PRDH 2020). While the dataset is still being extended, as of writing it contains 1.8 million unique births, 0.8 million unique deaths, and 4.2 million unique marriages from 1481–1992 (though births and deaths are limited to a particular region after

1849, there are very few records from before 1620, and the births and marriages are only available through 1971). Moreover, in those records a total of 2.8 million other individuals are mentioned besides the main participants, which provides additional observations over time for many people besides their own vital events.

The first chapter of my dissertation, co-authored with Gregory Clark and Neil Cummins and published in *Demography*, considers the demographic transition. Was this decline in fertility due to changing economic incentives or was it a more fundamental change in the ability or willingness of parents to control fertility? To bring contemporary methods to this old debate, we use the natural experiment of twins to test if pretransition parents targeted an optimal family size. We find strong evidence of no fertility response to an additional child in England, France, and Quebec before the demographic transition. This research led directly to the second chapter of my dissertation.

A dramatic increase in human capital was crucial for the transition to modern economic growth. It is often theorized that a key mechanism behind this increase was a trade-off between the quantity of children and the quality of their human capital. However, there are few papers that have estimated the size of the trade-off in a society just on the cusp of modernity. I use twins as a source of exogenous variation in quantity to estimate the trade-off in Quebec 1620–1850. I find that one additional child born decreased the literacy rate (proxied by signatures) of their older siblings by 0.6 percentage points. While statistically significant and robust, this estimate is too small to permit a large increase in human capital. The trade-off present before Quebec industrialized appears insufficient to initiate modern economic growth by itself. While the findings of the first two chapters suggest that parents had little influence on child outcomes through fertility decisions, the third chapter shows that they had a significant influence through their choice of spouse.

The final chapter in my dissertation asks the question: when did marriage become assortative on economic ability? While past research has considered if rising female labor force participation increased assortment, we know surprisingly little about marriage matching from before the late 20th century. Using a large new dataset of vital records from Quebec, I find that marriage was as assortative in the 1830s as it was in the 1960s. Not only was this assortment strong, it was on the human capital of individual men and women, not merely between families or social classes. Finally, I show that the human capital of both men and women mattered equally for child outcomes, confirming that assortment mattered for social mobility long before the late 20th century.

Contents

Chapter 1

Twins Support Absence of Parity-Dependent Fertility Control in Pretransition Populations (with Gregory Clark and Neil Cummins)

1.1 Introduction

This paper[1] proposes a new test, based on parents' response to the accident of a twin birth, of whether pretransitional populations were practicing parity-specific fertility control. We apply this quasi-experimental test to four micro-demographic datasets; the well-known family reconstitution studies of Henry and Campop, a large genealogical database from Quebec, and a novel genealogical database from England. In sum we analyze the effects of 16,580 twins on 709,262 births. This test confirms that for English marriages pre-1880, French marriages pre-1789, and Quebec marriages pre-1830 there was an absence of any sign of significant parity-specific fertility control.

[1]Published as Clark et al. (2020)

Why is such a test needed? After all, historical demographers, by the 1980s, had concluded that parity-specific fertility control was absent from most pretransition populations (Henry 1953, 1961; Coale 1971; Coale and Trussell 1974; Knodel 1974; Coale and Trussell 1978; Knodel 1978; Knodel and van de Walle 1979; Knodel 1983; Coale and Watkins 1986).[2]

There are two reasons why this test is needed. First, in contradiction to that earlier literature, there has emerged a literature that claims to establish that even in the pretransition era, there is strong empirical evidence of parity-dependent birth control. These papers claim that with populations which passed the traditional tests of natural fertility, for example England pre-1850, there is substantial parity-dependent control (Cinnirella et al. 2017, 2019). Anderton and Bean 1985, David and Sanderson 1988, Van Bavel and Kok 2004, Kolk 2011, similarly claim to find parity-dependent control or its like in pretransition populations in the United States, France, Sweden and the Netherlands. These claims are possible because the methods used to establish an absence of parity-specific control in the "natural fertility" literature depend on untested assumptions that the very population the new literature claims to consist of controllers actually was operating with natural fertility.

There is a further empirical literature which argues that pretransition populations controlled birth spacing in response to annual variations in living standards, or numbers of dependent children (Amialchuk and Dimitrova 2012; Van Bavel 2004; Van Bavel and Kok 2004; Cinnirella et al. 2017, 2019; Dribe and Scalone 2010; Kolk 2011). Such control in response to material conditions is not in itself evidence of parity-specific control. But if there was control in response to annual fluctuations in living conditions, then families at higher net parities, who faced resource constraints equivalent to those of bad harvests, would have had both the means and the inclination to increase spacing.

Second there is a significant literature in economics that assumes all pre-industrial populations exercised parity-dependent fertility control. In theorizing about the demographic transition economic models almost universally assume that pretransition fertility was con-

[2]See also Wrigley et al. (1997) which supported the natural fertility interpretation in pre-industrial England.

trolled fertility. Pretransition families had higher family target sizes, as a result of such factors as lower child survival rates, costs of child rearing, child earnings, and the education premium in earnings. But parents still had target family sizes and exercised parity-dependent control (Cervellati and Sunde 2007; Doepke 2004; Ehrlich and Kim 2005; Clark 2005; Galor 2012; Lagerlof 2003; Strulik and Weisdorf 2014; Weisdorf 2004). This view has not been challenged even by economic historians with a strong background in historical demography. Thus, Tim Guinnane, in a widely cited guide on the demographic transition for an economics audience (2011), reviews the factors that might lead higher desired numbers of births pre-transition but is silent on the question of whether there was parity-specific control before the transition.

This economics literature will only be convinced of an absence of parity-dependent control by demonstrations to the contrary based on quasi-random interventions, which was not the method of the 1960s and 1970s historical demography studies of natural fertility. In the first section below we review the traditional historical demography tests of natural fertility and the reasons for returning to this subject despite the earlier consensus.

In this paper we use the accident of twin births to confirm 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 births 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.[3] These pre-industrial families also often ended up, depending on relative twin and singleton death rates, with additional

[3]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).

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

The twins test will not detect, however, deliberate pre-industrial fertility control in response to adverse economic conditions. Nevertheless, it would be a 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 1.3, the four datasets to be analysed are described in Section 1.4, results are reported in Section 1.5, and Section 1.6 concludes.

1.2 What we know about pretransition Fertility Control

Henry (1961) defined natural fertility as "fertility which exists or has existed in the absence of deliberate birth control." In the natural fertility regime, fertility depends only on physiological and social factors effecting the level of fecundity. Henry identified 13 populations that he considered as being natural fertility regimes, though realized fertility varied considerably across these groups. Parity-dependent birth control in other populations was identified by observing a decline in fertility relative to natural fertility populations at older ages for women.

This raises an immediate logical issue about how we know whether even in the reference group there is an absence of any parity-specific control. In the natural fertility literature of the 1960s to 80s, there were never specific tests of whether fertility truly was 'uncontrolled' in such populations. Fertility levels at any age however varied substantially across the 13 reference populations. These level variations were not seen as evidence of parity-specific fertility control. Control was evidenced only by deviations from the age-pattern of natural fertility. The reference populations were assumed, without any formal tests, to practice no fertility control. The decline in fertility with maternal age was asserted to be to be completely a product of declining fecundity.

Coale (1971) introduced the parameters M and m as a way of formally characterizing fertility regimes and for testing for the presence of parity-dependent birth control. M represents the average ratio of observed fertility relative to natural fertility at a given age, where natural fertility was initially represented by the Hutterite population, an early twentieth century Anabaptist religious group who married early and prohibited contraception. $m$was the deviation of the observed age pattern of fertility from that of a natural fertility population, again represented by the Hutterites. m, alone, was the measure of parity-specific control. Thus:

> If $m = 0$ the resultant schedule is simply a constant multiple at every age of 'natural' fertility (represented by the Hutterite schedule); if $m = 1$ the schedule deviates from natural fertility to an extent that is the average degree of deviation of 43 schedules in the early 1960's; if m is very large the schedule has very rapidly diminishing ratios of fertility relative to the Hutterite schedule as age increases. Only the second of the parameters (m) affects the age structure of fertility; the other (M) only helps determine the level of fertility (Coale 1971, p.207).

Coale and Trussell (1974) present model schedules of fertility designed to be "schedules encompassing the full range of human experience" (p.185). These model schedules are based upon M and m parameters applied to natural fertility schedules. When does m indicate fertility control? Threshold levels were proposed as "$m = 0.2$ (very moderate control of fertility) and $m = 0.4$ (quite moderate control of fertility)" Coale and Trussell (1974, p.195). In other words, no matter what the level of fertility, natural fertility populations are characterized by a relatively invariant, convex, age pattern of fertility. Coale and Trussell (1978) showed that in the 10 well documented (of the original 13) supposed natural fertility populations identified by Henry, the estimated value of m was between -0.152 and 0.236 (Coale and Trussell 1978, Table 2, p. 205).

Knodel and van de Walle concluded that:

> Application of this technique [M&m] to the results of the many family reconstitution studies, as well as to official statistics when available, indicates that family limitation in Western Europe was either absent or quite minimal (perhaps limited only to special segments of society, such as social elites) prior to the onset of the long-term decline in marital fertility. When the index of family limitation can be computed prior to the secular decline in fertility, it is usually close to zero and unchanging. . . the evidence does not suggest that family limitation was practiced at some moderate but constant level prior to the secular fall in marital fertility rates. Instead, its incidence seems to have been quite minimal and in many cases completely absent. (Knodel and van de Walle 1979, p. 227.)
>
> " Couples do not have target family sizes. They accept, in some cases reluctantly, as many children 'as God sends.' " (Knodel and van de Walle 1979, p. 235).

Similarly the *Cambridge Group in the History of Population and Social Structure* 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). Livi-Bacci did, however, detect evidence of parity-dependent control for some upper class groups in Europe before

1850 using such measures as m, and the mother's age at last childbirth: aristocrats in France, Florence and Milan, the bourgeoisie in Geneva, and families in Genoa (Livi-Bacci 1986).

However, the Coale-Trussell test has been subject to criticism, in that it may only detect particular forms of parity-dependent birth control. Spacing might be systematically used in natural fertility populations to limit family size throughout the course of marriage (slowing), yet be undetectable from the Coale-Trussell m parameter. This was a possibility noted even by Knodel (1979, p.504). So in later work scholars looked for an effect of "net parity" on subsequent fertility, which allowed for a mix of both spacing and stopping behavior. One such method was Cohort Parity Analysis (CPA). (David et al. 1988, David and Sanderson 1988).

Further, both the $M\&m$ approach and CPA 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). In particular:

> Coale and Trussell's index m takes values very close to zero (e.g.< 0.2) in simulated populations in which as many as 40 per cent of the population practise effective, parity-dependent control. In particular, values of $m < 0.2$ cannot justifiably be cited as evidence of the absence of significant fertility control. (Okun 1994, p. 221).

Note that Okun's simulations themselves have to employ a baseline fecundity which is estimated assuming, again, that there are some populations observed with no parity-specific control. Thus, the methods for establishing the presence or absence of parity-dependent birth control employed by the *European Fertility Project* have significant weaknesses. Note further that Coale and Trussell do not give any confidence intervals for their estimates of m. Given that these estimates of m are based on samples from modest sized populations compared to another set of population samples, the possibilities are for substantial error components in the estimates of m. This further reduces the $M\&m$ methods ability to detect with high confidence the absence of parity-dependent fertility control. Thus, this earlier literature is both based on untested assumptions about the reference population and has poor ability to detect the presence of substantial minorities of controllers.

Since the end of the *European Fertility Project* the tendency in the published literature has been to challenge the conclusion that the pre-industrial regime in Europe was largely one of natural fertility. In particular, another method has emerged for estimating parity-dependent fertility control. This is where hazards models for another birth are estimated controlling for economic circumstances, numbers of dependent children, net parity, and mother's age. These models thus concentrate on spacing and the response of spacing to such factors as net parity. The published estimates from such models generally suggest significant pre-industrial fertility control in response to economic circumstances (Amialchuk and Dimitrova 2012; Bengtsson and Dribe 2006; Cinnirella et al. 2017, 2019; Dribe and Scalone 2010; Bengtsson and Dribe 2006; Cinnirella et al. 2017; Dribe and Scalone 2010); to numbers of dependent children (Bavel 2004); or to net parity itself (Anderton and Bean 1985; David and Mroz 1989a; Kolk 2011; Cinnirella et al. 2017, 2019; Van Bavel and Kok 2010). Van Bavel and Kok (2010), for example, conclude that:

> The main substantive conclusion is that the married couples in our Dutch study population were controlling their fertility by means of birth spacing before the onset of the fertility transition (pp. 136–7).

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 pretransitional populations.

However, Clark and Cummins (2019) show that the Cinnirella et al. (2017) results are an artifact of the estimation methods with impossible implications.

Thus, the debate on whether parity-dependent birth control existed in pretransitional populations is unresolved.

Another factor suggesting the possibility of parity-specific control in pretransition populations are substantial social class differences in gross fertility. Wealthier families in England marrying before 1780 had substantially more births within marriage than poorer ones, with shorter birth intervals and later stopping (Clark and Hamilton 2006; Clark and Cummins 2015b). This differential within marriage was also linked to social status in England (Boberg-Fazlic et al. 2011; de la Croix et al. 2019). Similar patterns are found in pretransition France (Cummins 2013, 2019). It is unclear what created this differential, but this again creates the possibility that there was deliberate parity-specific fertility control.

1.3 Using Twins to Detect Parity-Dependent Birth 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).[4] While twin births are more common among older women, they are largely a random event. There is only 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 at least one birth) 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

[4] Pison and Couvert (2004) report such a rate for France 1700–89 (Figure 1), based on the Henry data.

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 1.1 shows the expected effect of twins on total births by parity at the time of the twin birth with parity-independent fertility.

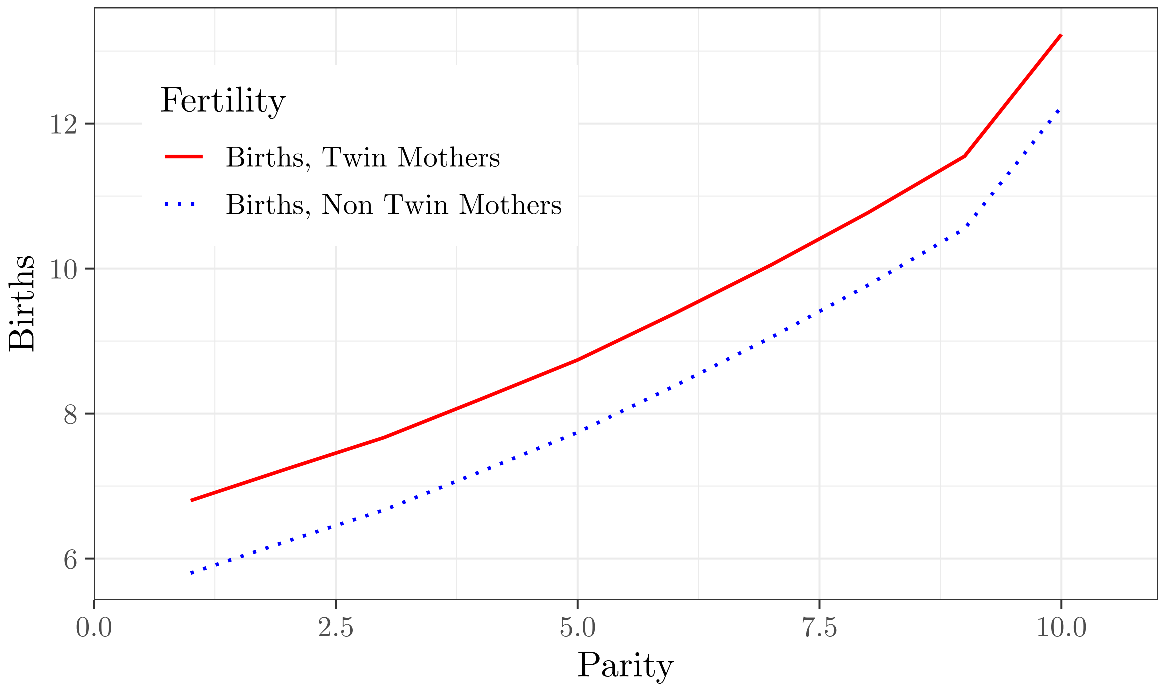


Figure 1.1: Expected effect of twins on total births

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 1.2 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.[5]

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

[5]This number will be negative if twin survival rates are less than singleton survival rates.

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:

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

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 $\phi\left(2\theta_t-\theta_s\right)$.

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 targeting 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 therefore have to also control for mother age at marriage. Thus, to test for the effect of twin births on total births we postulate

$$NB_{pk} = \alpha_b DTWIN + \sum \beta_j DPARITY_j + \sum \delta_l DMAGE_l + \varepsilon \tag{1.1}$$

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.[6]

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

$$NS_{pk} = \alpha_c DTWIN + \sum \gamma_j DPARITY_j + \sum \eta_l DMAGE_l + \varepsilon \tag{1.2}$$

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.[7]

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 (2004) 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 2004, p. 781, Figures 10–11). With a population that is a mix of natural fertility couples and

[6] 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.

[7] 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

$$NB_{pk} = \alpha_b DTWIN + \sum \beta_{jl} D(PARITY_j, DMAGE_l) + \varepsilon \tag{1.3}$$

$$NS_{pk} = \alpha_c DTWIN + \sum \gamma_{jl} D(PARITY_j, DMAGE_l) + \varepsilon \tag{1.4}$$

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

$$NB_{pk} = \alpha_b DTWIN + \lambda_b DTWIN \bullet PARITY + \sum \beta_j DPARITY_j + \sum \delta_l DMAGE_l + \varepsilon \tag{1.5}$$

$$NS_{pk} = \alpha_c DTWIN + \lambda_c DTWIN \bullet PARITY + \sum \gamma_j DPARITY_j + \sum \eta_l DMAGE_l + \varepsilon \tag{1.6}$$

where $PARITY$ is the parity of the twin birth. With no fertility control λ_b and 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.[8] 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

$$Interval_{pk} = \omega DTWIN + \sum \beta_j DPARITY_j + \sum \delta_l DMAGE_l + \varepsilon \tag{1.7}$$

as a potential detector of behavioral responses to twinning.

[8] 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.

1.4 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 (*l'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.

(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.[9] 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 is 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).

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

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, for 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.[10] 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. Fs 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

[10]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, Henri an Méric (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.

twin. However, as noted above, in the CAMPOP data twins are only detected through the baptismal records. The baptismal records sometimes explicitly note children baptised on the same day are twins, but in other cases are silent. We do not know how the creators of the CAMPOP data concluded that the children they identified as twins were indeed twins. We show shortly that we 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.[11] 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 father was born in Quebec, married before 1830, and all children have a known parish of birth in Quebec.[12]

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

[11] IMPQ 2020, Project Balsac 2020, PRDH 2020.

[12] n.b.: The data extract used for the first chapter was a previous version of the IMPQ. At the time of writing, we did not have access to quality flags for the day of birth variable. To compensate, we used other stricter criteria for inclusion in the sample than I use in Chapter 2.

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, 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 1.3 to 4.0 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.[13] 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.[14] 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. We can test what fraction of actual twining events we are correctly identifying in our data, ϕ, since the fraction of the putative twins having the same gender will be

$$R_{SS} = 0.5 + \frac{0.4}{R_t}\phi \tag{1.8}$$

where R_{SS} is the same sex ratio and R_t is the observed twinning rate per 1,000 births.

[13]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.

[14]"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).

If we are missing many true twining events and instead are misattributing singleton births, then the gender ratio will be closer to 0.5 than predicted by this formula when $\phi = 1$. In general, as Table 1.2 shows, the French and the Quebec data has sex ratios consistent with complete of detection of true twinning events. For the Families of England sample for twins born in marriages before 1880, the fraction which were same sex was 0.66, less than an expected rate of 0.72. So for the FOE data, there is some evidence that not all twins are being detected and some non-twins included. For the CAMPOP data, however, the same sex ratio for their identified twins is only 0.59 compared with an expected rate of 0.70, implying that many twins were not detected. Table 1.2 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 1.1 reports the summary statistics for the studies used: how many births, how many potential twin births, and the years covered. Table 1.2 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 1.2 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.

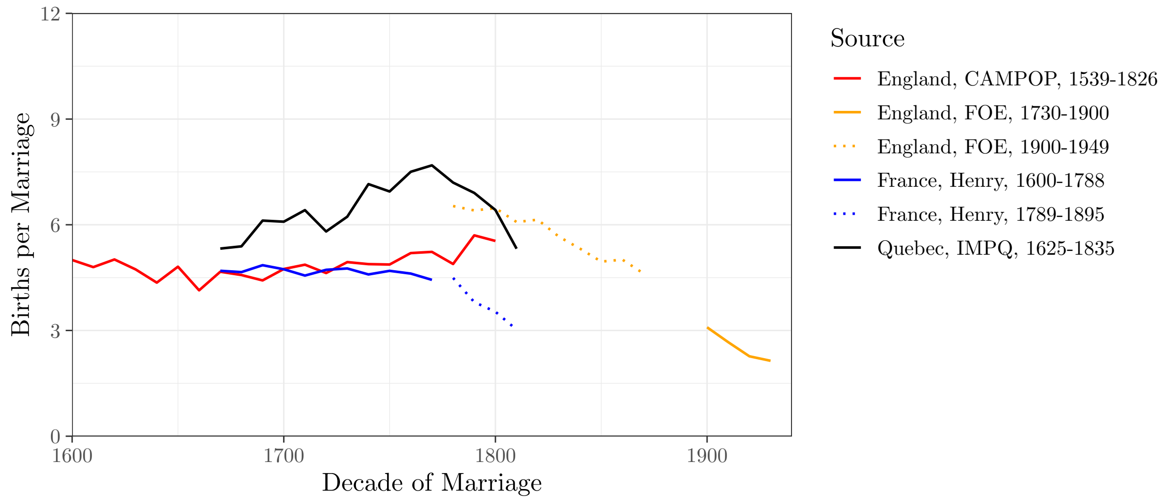


Figure 1.2: Average births, by sample and decade

Table 1.1: 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	374,082	8,780	0.023	52,725	1625	1835	IMPQ

Note: Years refer to observed births.

Table 1.2: 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	0.71	0.52	5.64	0.68	0.65

Twinning is mostly uncorrelated with the observable social characteristics of families. Table 1.3 shows the coefficient for each of a variety of parent characteristics regressed singly on an indicator for whether a birth is a twin: age of mother (in years), parity, the 1st-2nd birth interval as an 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.[15] 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%.

Table 1.3: Twinning correlates

Variable	France, Henry	England, CAM-POP	England, FOE	Quebec, IMPQ
Mother's Age	-0.00005	0.0005***	0.0009***	0.0004***
	(0.003)	(0.0001)	(0.0001)	(0.00003)
Parity	0.03***	0.002***	0.004***	0.0006***
	(0.002)	(0.0002)	(0.0002)	(0.00005)
1st->2nd Birth Interval	0.002	-0.002**	-0.002	0.00002
	(0.004)	(0.001)	(0.002)	(0.00004)
Literate Father	0.003	0.0001		0.0003
	(0.019)	(0.002)		(0.0006)
Literate Mother	-0.0005	0.002		0.0008
	(0.026)	(0.002)		(0.0007)
ln(Wealth), Father			0.0001	
			(0.0003)	
Occupational Rank, Father			-0.000002	-0.005
			(0.00004)	(0.003)
Educated Father			-0.004	
			(0.003)	

Note: *p<0.10; **p<0.05; ***p<0.01. Standard errors in parentheses.

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 (2004) show that even controlling for mother age there is a positive parity effect (Figure 4, p. 770). Pison and Couvert (2004) 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 1.3 our proxy for fecundity, the 1–2 birth interval, is never significantly associated with higher twinning rates. Since Pison and Couvert (2004) report "The most fecund couples

[15]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.

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 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 2004, 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 (2004) 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.

1.5 Results

Table 1.4 summarizes the estimates of α_b the effect of a twin birth on total births for the six population samples we have, for Equation (1.1) 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 1.2, 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 1.4, 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% or 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.

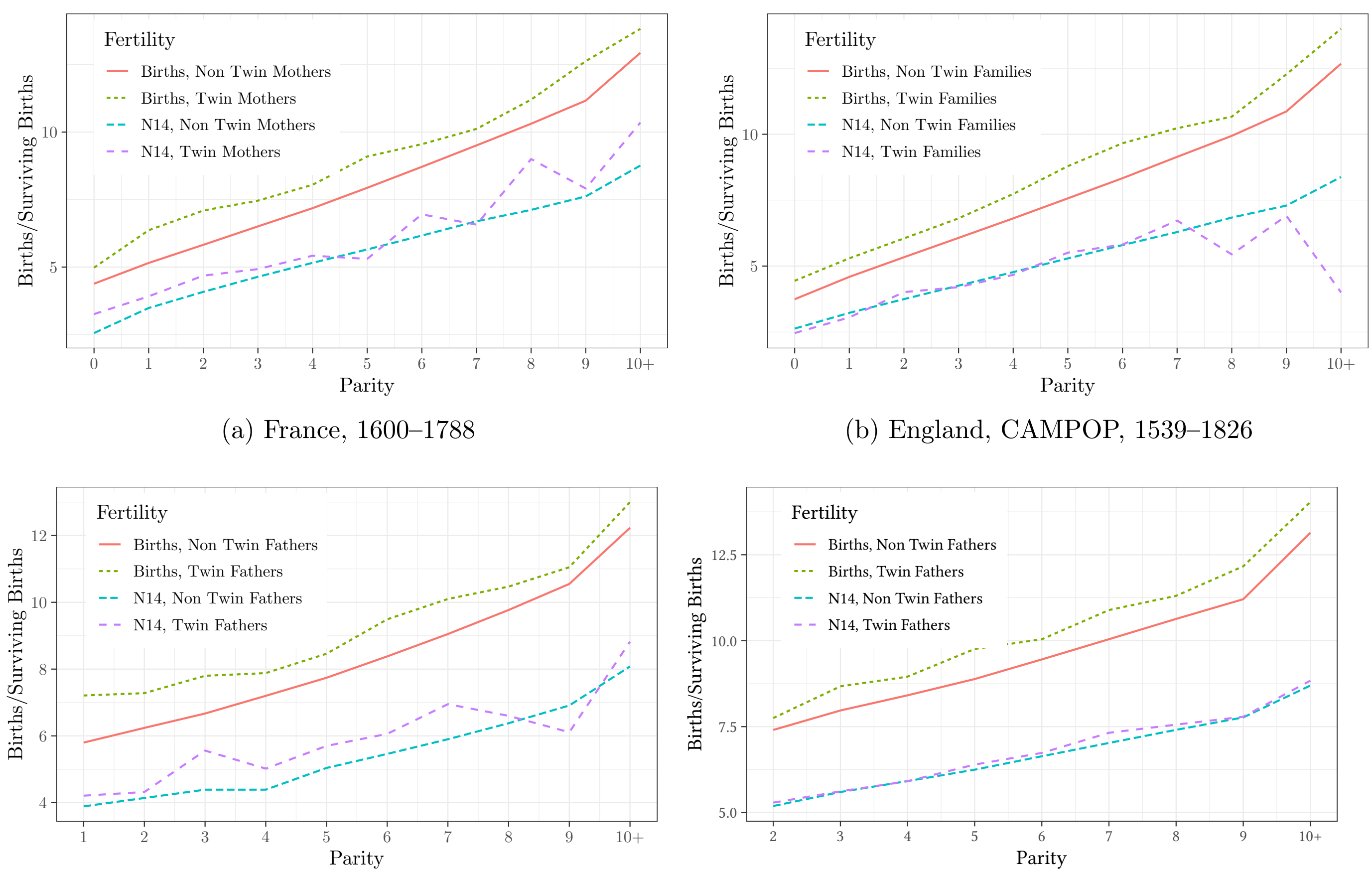
Note also that while with the samples used here we may not be able to reject any pre-industrial targeting behavior at the 95% confidence limit in France and England, in the future much larger bodies of data are likely to become available. Thus, while in England our sample includes 55,533 births for marriages 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) and Graham et al. (2018)). Similarly in Quebec research teams are at work extending the information on births and deaths from 1850 forward to 1916. 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 a year.

If we instead estimate α_b non-parametrically from Equation (1.3), where interactive effects between parity and mother's age are allowed, we get a very similar set of results as in 1.4.

Table 1.4: 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	55,590
Quebec	1	0.61^R	1.03	0.05	324,202
Post-Fertility Decline					
France, post 1800	1	0.87	0.89	0.12	18,454
England 1900-49	1^R	0.6	0.72	0.10	27,399

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



(a) France, 1600–1788

(b) England, CAMPOP, 1539–1826

(c) England, 1730–1879

(d) Quebec, 1621–1835

Figure 1.3: Observed births by parity at twin birth

Figure 1.3 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 1.5 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 (1.5) and (1.6) 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 1.5: The Effect of twinning as function of parity

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	-0.09	0.05	55,590	-0.04	0.05	52,706
Quebec	-0.01	0.10	324,202	-0.02	0.09	324,202

Note: *p<0.10; **p<0.05; ***p<0.01. γs from OLS estimation of Eqns 1.7 and 1.8.

Because of the higher infant mortality rates for twins the effects of twinning on completed family sizes with natural fertility are smaller than on births. Table 1.7 summarizes the estimates of α_c in Equation (1.2), 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.[16] Thus, the evidence for net fertility in England (FOE) implies with 95% confidence that no families were operating with target family sizes.

Table 1.6 shows both for births and for surviving children, the 5% confidence intervals for the proportion of the population which potentially had target family sizes, for the three populations for which we believe the twins are well identified.[17] Note two things. First in no case can we be confident at the 5% level that there were any controllers in these pretransition populations. There is no substantial evidence for control. Second, we can be confident at the 5% level that no more than 25% of the French pre-1789 might have been controllers, that none of the English pre-1880 might have been controllers, and that no more than 5% of the Quebecois pre-1850 might have been controllers.

Table 1.6: 5% confidence intervals, proportion potentially controlling in sample-populations

	Births	Surviving Children
France pre 1789	0.00–0.35	0.00–0.25
England pre 1880	0.00–0.36	0.00–0.00
Quebec pre 1850	00.0–0.05	0.00–0.52

Notes: Confidence intervals for a one-sided test on the potential fraction of controllers.

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

[16]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.

[17]Since the question of interest is what proportion of the population at maximum could be families with target family sizes we use here a one-sided test.

Table 1.7: 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	0.70	0.13	52,706
Quebec	0.33	0.05^R	0.24	0.05	324,202
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	0.36	0.10	27,288

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

We also estimated for the pretransition populations ω in Equation (1.7) to test whether there was any lengthening or shortening of the birth interval immediately following a twin birth compared to singleton 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 5 days, with a standard error of 9 days.

1.6 Conclusion

There is good evidence that at least in some Western European and Western European derived populations — England, France, and 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 basis of 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 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, 2015b). 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.[18] 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.

[18] See, for example, Mokyr 2010.

Appendix

A1 Families of England Database

Gregory Clark and Neil Cummins (henceforth C&C) created the Families of England database 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. C&C expect to add considerably more data on all the social outcome variables.

To enable high linkage rates with the sources, have 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 C&C 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, they 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%.[19] 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, C&C identify a father or mother for 88.%[20] 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, their father dies, they are living with grandparents in the census, or the family emigrates.[21] 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

[19] Long and Ferrie (2018), for example, link only 20% of adult sons to their fathers in England between 1851 and 1881.

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

[21] C&C could identify the father by getting the birth certificate but this is prohibitively costly.

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. C&C also try and follow the lineages of those 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, C&C 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 C&C 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, C&C can for a 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. They plan on doing this for a select sample of people marrying around 1990 so that they 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, New Zealand, and the 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 C&C 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 C&C have for this database. The probate information is searchable at 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, C&C 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),and UK Articles of Clerkship (1756–1874). From all these measures C&C 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 C&C 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 and, from 1995 on, that of mothers also. Marriage certificates record the occupations of the husband, of the wife, and of the fathers. So for a select sample, C&C can estimate occupations for people born up to around 1980.

Dwelling Value: From the electoral censuses of 1999–2012, C&C have the address where adults were living in 1999–2012 from which they 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 C&C can assign status measures to parents based on their child name choices.

After completing the genealogical links, and the status information, they 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.

Chapter 2

Before the fall: Child quantity and quality in pre-demographic transition Quebec

2.1 Introduction

Since Becker (1960) economists have posited a trade-off between the average human capital ("quality") and the quantity of children. Growth theorists often emphasize the importance of such trade-off for the transition to modern economic growth (Galor 2005). In these theories, the fall in fertility during the demographic transition and the growth of technology during the industrial revolution are related parts of a positive feedback loop that led to rapid economic growth. However, few estimates of the trade-off exist for pre-demographic transition populations (Clark and Cummins 2018, Klemp and Weisdorf 2018, Tan 2018, Galor and Klemp 2019). If this trade-off was key to modern growth, then was it present in populations on the eve of industrialization?

This paper estimates the quantity-quality trade-off using the IMPQ vital records from

1620–1849.[1] Over 8,000 pairs of twins, an unusually large sample for a historical study, provide a source of exogenous variation in family sizes. Using twins, it finds a trade-off, albeit a small one. One additional birth on average decreases the probability that a sibling signs their marriage record, a proxy for literacy, by 0.6 percentage points.

This estimate is small — seemingly too small for the trade-off to permit rapid increases in aggregate human capital — yet is statistically significant and robust. As the vital records are linked over multiple generations, I show that the effect of quantity on quality does strongly persist between generations. However, as I illustrate with a simple simulation exercise, it is so small that even a large decrease in fertility would result in only moderate gains in human capital.

The structure of the paper is as follows. First, I describe the data. Second, I describe the empirical methods and the baseline results. Third, I demonstrate the results are robust to various threats to identification. Finally, I conclude with a discussion of the broader implications of the finding for the literature on long run growth.

2.2 Data

2.2.1 Sources

While the IMPQ contains marriage records through the 20th century, in this chapter I rely on birth and death records which are only available for the entire province through 1849. The data is near to a complete record of the Catholic population with high quality linking of families together (Bourque 2011, Dillon et al. 2018).[2]

Using the birth records, I am able to identify twins by the exact day of birth.[3] This is an advantage of using vital records instead of census records; many censuses do not record

[1] IMPQ (2020), Project Balsac (2020), and PRDH (2020).

[2] While some Protestants are included, I drop them from the sample as the records are less complete.

[3] I define twins as children born within a day of each other. There are a tiny number of children reported born an implausible amount of time apparant, e.g. one month, but this is likely measurement error.

months of birth, let alone days (Tan 2018). I exclude any family I detect with any triplets or higher order births, as they both are extreme outliers and complicate the empirical strategy. Despite these strict restrictions, I am still able to identify 8,121 pairs of twins (2.7% of children born). Summary statistics, averaged separately for twins and for singletons, are presented in Table 2.1 below.

The main measure of human capital used in this paper is the presence of a signature on a marriage record. A 1678 ordinance required both the bride and the groom to sign their marriage records if able and the priest to record if they complied (Magnuson 1992). However, before 1800 the rate at which individuals were reported unable to sign varies substantially from year to year. Therefore, I define ability to sign as a variable that is 1 if the individual definitely signed and 0 otherwise. See Appendix 1 for further discussion.

Table 2.1: Summary statistics

Variable	Singleton, mean	Twin, mean	Singleton, N	Twin, N
Year of birth	1796	1794	598,384	16,243
Parity	6.04	6.73	598,384	16,243
Signed	0.10	0.08	239,991	2,882
Surv. to 1	0.80	0.53	598,384	16,243
Surv. to 14	0.68	0.44	576,500	15,765
N born	10.99	12.12	598,384	16,243
N surv. 1	8.70	8.97	596,553	16,199
N surv. 14	7.31	7.47	492,046	13,685
Mother's age at birth	30.64	32.22	598,384	16,243
Mother surv. 40	0.9	0.9	598,384	16,243
Mother signed	0.07	0.07	598,384	16,243
Father signed	0.07	0.07	598,215	16,243
Share of sibs signed	0.13	0.11	598,384	16,243

Note: Signed is an indicator which is one if the individual signed their first marriage certificate, zero if they did not or there is no record of a signature, and missing if they did not marry. Surivival to age one is inferred from either a missing death record and a birth more than one year before 1849 or from a death at an age greater than one. Surivival to age fourteen is defined similarly. N represents the count of siblings (and potentially half-siblings) that share a mother.

Was a signature really a measure of human capital — that is, a productive attribute? The qualitative evidence suggests that it was. Signatures are a proxy for the ability to write, a form of human capital that was particularly associated with business activity (Greer 1997). Literacy also allowed young men to become a lay tutor, a frequent stepping stone towards a career as an administrator or notary (Magnuson 1992). Another career choice that required literacy was the Church.[4] Moreover, for the marriages with a known occupation, it does appear that there is a fairly steep signature gradient across different occupations (Table 2.2).

Table 2.2: Occupations by average signature rate, marriage records

HISCO	Occupation	Translation	% of total	Share signed
41025	*Marchand*	Merchant	0.02	0.71
79100	*Tailleur*	Tailor	0.01	0.57
58340	*Soldat*	Soldier	0.02	0.41
77620	*Boulanger*	Baker	0.01	0.38
98135	*Navigateur*	Sailor	0.01	0.34
80110	*Cordonnier*	Shoemaker	0.02	0.34
95410	*Menuisier*	Carpenter	0.05	0.29
76145	*Tanneur*	Tanner	0.01	0.26
83110	*Forgeron*	Blacksmith	0.02	0.25
95135	*Macon*	Mason	0.01	0.21
61110	*Cultivateur*	Farmer	0.50	0.12
98620	*Charretier*	Carter	0.01	0.11
99910	*Journalier*	Worker	0.15	0.09
43220	*Voyageur*	Fur trader	0.01	0.07
62105	*Laboureur*	Laborer	0.04	0.04

Note: All marriages before 1850 with a definite location in Quebec are included. Only men are included in the sample as female occupational titles are rare. The most common title is taken for each HISCO category. % of total is the percent of all males with known occupations that are coded into that HISCO category. *Journalier* is an ambiguous category, as in Quebec it refers to a worker payed by the day regardless of the task or industry.

2.2.2 Comparison to France

Before its demographic transition, Quebec had very high levels of fertility compared to France. In Table 2.3 below, I compare the main dataset to French data from Louis Henry's survey of rural parishes (Henry 1968). The data is partitioned into women born before and after 1748, as the French demographic transition began during the Revolution (Cummins 2013).

Quebec's high fertility, as suggested by the table, was due to a marriage regime where

[4] Though not one I observe in marriage records, so average population-wide literacy is likely underestimated by the signature proxy.

women married younger than their peers in France. Quebec also had relatively low human capital, though the gender gap was also notably quite small. The decrease in literacy from the first to second period, while perhaps surprising, is documented in the historical literature (Greer 1985). The initial colonists were often drawn from urban areas, but their children frequently became *habitants* — Quebec's colonial equivalent of peasants — who neither had easy access to schooling nor a large economic incentive to learn to read and write.

Table 2.3: Comparison of married women who survive to 40, Quebec and France

Period	Country	Age, 1st mrrg	Age, hsbnd	N births	N surv. to 1	Signed	Hsbnd signed
Born 1636–1748	Quebec	22.6	27.5	9.17	7.08	0.10	0.12
	France	24.9	28.9	6.50	5.54	0.09	0.24
Born 1748–1803	Quebec	22.6	26.9	9.31	7.38	0.05	0.05
	France	25.5	29.0	5.05	4.42	0.21	0.44

Note: Sample consists of all women who married, had at least one child, never remarried, and survived to age 40. I only consider women who never remarried as in the Henry data the number of births is per couple, not per woman. I also drop the very few observations where either spouse has a negative age at marriage, as this is presumably due to errors in the records or digitization.

2.3 Methods and results

2.3.1 Identification from twins

There are two types of twins, monozygotic (identical) and dizygotic (fraternal). Monozygotic twins occur at a remarkably consistent rate across societies (around 0.7–0.9 percent of children).[5] The rate of dizygotic twins is more varied and is influenced by several maternal characteristics. The rate increases with a mother's age and previous number of births, and is higher for mothers who previously delivered twins. Some studies find other maternal characteristics associated with higher rates, an endogeneity concern which I address below (Farbmacher et al. 2018). Controlling for maternal age and parity, twins should therefore be

[5] In fact, this rate is observed in all mammals except some species of armadillos (Pison and Couvert 2004).

effectively random. This conditional randomness is key to my identification strategy.

2.3.2 Binned scatter plots

Twins are random conditional on both parity and mother's age at birth. As there are two variables on which to condition, plotting the underlying relationships in two dimensions is not straightforward. Below, I construct a series of four binned scatter plots that show the relationship between these two controls, the number of siblings born, and the average literacy of siblings. In each scatter plot, I hold one control constant using a fixed effects regression on the entire sample and then average the data, separately for twins and singletons, into twenty equal-sized bins over the other control variable.

In the scatter plots holding mother's age constant (Figure 2.1) and holding parity constant (Figure 2.2), there is a higher total number of births at every parity and mother's age for twins. The difference between twins and singletons seems to be the same regardless of parity or mother's age. For the share of siblings who signed, twins have a lower average, though the overall relationship is less clear. All together, these binned scatter plots suggest that twins will have the same effect on family size regardless of what mother's age or parity they occur at. Moreover, if twins are used as an instrument, the first stage is likely to be strong. The reduced form relationship of twinning on sibling literacy appears to be fairly noisy, although it is negative as the theories of the trade-off would predict.

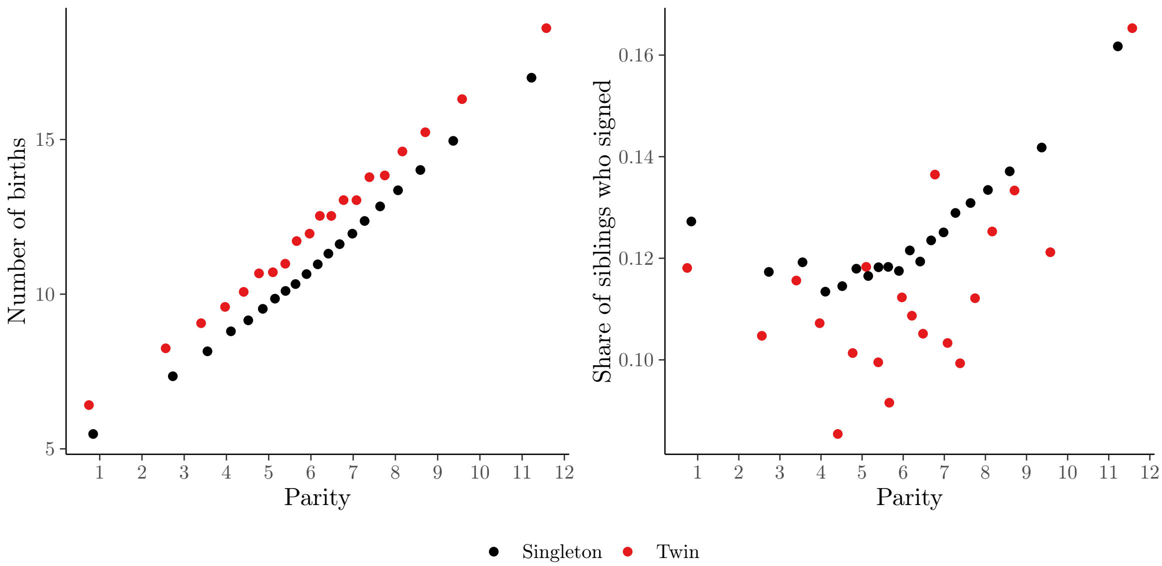


Figure 2.1: Binned scatter plots holding mother's age constant *Note:* All variables are first adjusted by regression on indicator variables for mother's age at birth. Adjusted variables are then computed as the residuals plus the estimated fixed effect for the age closest to the mean. Then, the data are averaged over twenty equal-sized parity bins, seperately for twins and singletons.

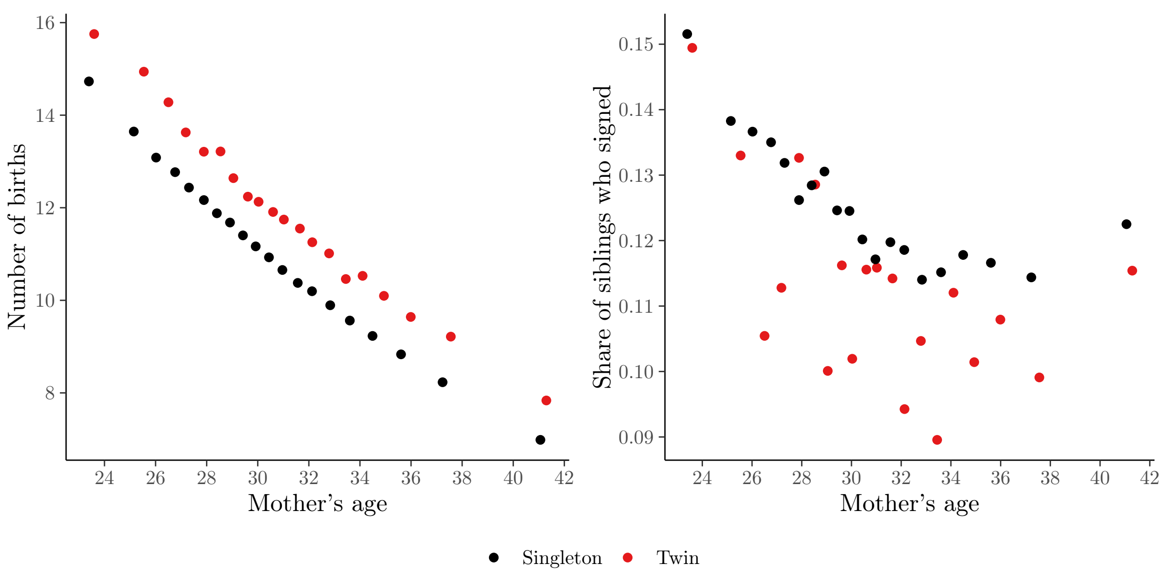


Figure 2.2: Binned scatter plots holding parity constant *Note:* All variables are first adjusted by regression on indicator variables for parity at the birth. Adjusted variables are then computed as the residuals plus the estimated fixed effect for the parity closest to the mean. Then, the data are averaged over twenty equal-sized mother's age bins, seperately for twins and singletons.

2.3.3 Empirical specification

Modern implementations of the twin instrument face different data constraints than those imposed by the historical Quebec data. In populations with parity-dependent control, twin births only increase a family size if they are the last birth planned. This introduces a complex empirical challenge. A twin birth is more likely to occur at a higher parity, but since families with a higher family size target will have a higher average parity, there is the potential for reverse causation. Moreover, the average effect of a twin birth on family size will depend both on parity and on the target family size of the parents. To attempt to address these issues, researchers will often use analysis by parity (c.f. Tan (2018)). This approach uses an instrument for a twin born at a parity n, considers only families with at least n children, and looks only at the effect on children born at parities less than n (Black et al. 2005a, Black et al. 2010, Angrist et al. 2010). As an alternative, the parity-pooled approach uses multiple instruments: a series of indicator variables for a twin birth at different parities (Angrist et al. 2010). This allows for a non-linear effect of twinning on family size depending on the parity of the birth at the cost of increasing potential weak-IV bias.[6]

These approaches are not well-suited to the Quebec data. Fortunately, they are also not necessary. With modern populations, the small average family sizes mean that splitting the sample by parity does not substantially reduce the effective sample size. In the Quebec data, average family sizes are much larger (Table 2.1), dramatically reducing the sample size available for each regression (or increasing the number and decreasing the strength of the instruments in the pooled approach). However, as shown in Clark et al. 2020 and in the binned scatter plots above, parents did not practice parity dependent fertility control and twins did not have a heterogenous effect on family size at different parities. Therefore, I do not use analysis by parity or the parity-pooled approach. Instead, I use a single indicator variable for a twin birth, regardless of parity, as my instrument and look at the effects on all

[6] As the indicator variable for twin at parity n is coded 0 for each birth at parity $m \neq n$, the instruments are weaker as well as more numerous.

other siblings. As twins are only random conditional on parity and mother's age, I control for both using fixed effects. In other words, I include twins and parity as covariates, but not the interaction between the two. This more parsimonious specification requires fewer observed twins at every parity.

The reduced form of the baseline regression is as follows. For each pair of child i and birth j sharing mother m, $i \neq j$, I regress:

$$signature_{i,m} = \alpha\ twin_{j,m} + \gamma_{mother's\ age_{j,m}} + \delta_{j,m} + \beta X_{i,j,m} + \epsilon_{i,j,m} \tag{2.1}$$

where $signature_{i,m}$ is if the child definitely signed their marriage record, $twin_{j,m}$ is an indicator for whether birth j is a twin birth, $\gamma_{\text{mother's age}_{j,m}}$ are fixed effects for the mother's age at birth j, $\delta_{j,m}$ are fixed effects for the parity of birth j, $X_{i,j,m}$ is a vector of controls not required for identification, and $\epsilon_{i,j,m}$ is the error term. In most of the regressions, for $X_{i,j,m}$ I only include fixed effects for the year of birth of i to control for trends in literacy over time.

In other words, I compare two mothers, one who gave birth to twins at a given parity and age, and one that gave birth to a singleton at the same parity and age. I then look at each other child of the mothers. Do the children of the twin family have a lower probability of signing than those of the singleton family?

The IV regressions are set up the same way, except various measures of family size are used as endogenous independent variables and the twin indicator is used as an instrument.

2.3.4 Main results

As shown in Table 2.4, one additional birth reduces the probability a child signs their marriage certificate by 0.6 percentage points. This estimate is statistically significant, but the magnitude of the effect is quite small. As shown in Figure 2.3, the trade-off explains little of the observed differences in the average literacy of children of fathers of different occupations.

The number of children surviving to one and fourteen, instrumented by the twin indicator, reduce the probability of signing by 2.2 percentage points and 2.7 percentage points respectively (Table 2.5 and Table 2.6). Comparing first stage Kleibergen-Paap F-stats, the instrument is notably weaker than in the number of births regression (Kleibergen and Paap 2006). This is because, as shown in Table 2.1, twins are less likely to survive childhood.

Table 2.4: Effect of number of siblings born on signature rates

	OLS	IV	1st stage	Reduced form
Number of births	0.001***	−0.006***		
	(0.000)	(0.001)		
Twin birth			1.127***	−0.006***
			(0.013)	(0.002)
N	2,298,174	2,298,174	2,298,174	2,298,174
FE: Mother's age	X	X	X	X
FE: Parity	X	X	X	X
FE: Year of sib's birth	X	X	X	X
KP F−stat			7,493	

Note: *p<0.10; **p<0.05; ***p<0.01. The dependent variable in each regression is an indicator which is one if the sibling signed their first marriage certificate, zero if they did not or there is no record of a signature, and missing if they did not marry. To observe completed family sizes, the sample is restricted to mothers born before 1810.

Table 2.5: Effect of number of siblings surviving to 1 on signature rates

	OLS	IV	1st stage	Reduced form
Number surv. to 1	−0.006***	−0.022***		
	(0.000)	(0.006)		
Twin birth			0.291***	−0.006***
			(0.015)	(0.002)
N	2,295,644	2,295,644	2,295,644	2,295,644
FE: Mother's age	X	X	X	X
FE: Parity	X	X	X	X
FE: Year of sib's birth	X	X	X	X
KP F−stat			383	

Note: *p<0.10; **p<0.05; ***p<0.01. The dependent variable in each regression is an indicator which is one if the sibling signed their first marriage certificate, zero if they did not or there is no record of a signature, and missing if they did not marry. Surivival to age one is inferred from either a missing death record and a birth more than one year before 1849 or from a death at an age greater than one. To observe completed family sizes, the sample is restricted to mothers born before 1809.

Table 2.6: **Effect of number of siblings surviving to 14 on signature rates**

	OLS	IV	1st stage	Reduced form
Number surv. to 14	−0.008***	−0.027***		
	(0.000)	(0.009)		
Twin birth			0.181***	−0.005***
			(0.015)	(0.002)
N	2,119,381	2,119,381	2,119,381	2,119,381
FE: Mother's age	X	X	X	X
FE: Parity	X	X	X	X
FE: Year of sib's birth	X	X	X	X
KP F−stat			139	

Note: *p<0.10; **p<0.05; ***p<0.01. The dependent variable in each regression is an indicator which is one if the sibling signed their first marriage certificate, zero if they did not or there is no record of a signature, and missing if they did not marry. Surivival to age fourteen is inferred from either a missing death record and a birth more than fourteen years before 1849 or from a death at an age greater than fourteen. To observe completed family sizes, the sample is restricted to mothers born before 1786.

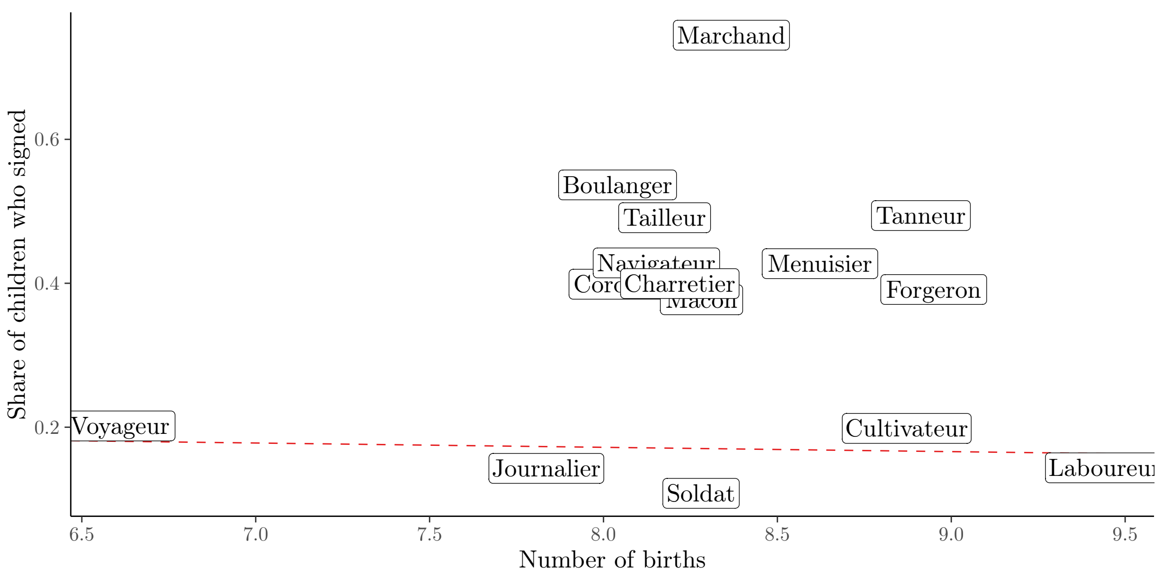


Figure 2.3: Quantity and quality for most common occupations *Note:* This figure plots the average child signature rate and number of births per mother by the occupation of her first husband. The occupations are from Table 2.2 and the sample is restricted to women born before 1849 with at least one married children. To illustrate how the estimated quantity-quality trade-off is small in magnitude, the red dashed line is a line with a slope of −0.006 drawn through the average quantity and quality for this sample.

2.3.5 Intergenerational-effects of the trade-off

Table 2.7 shows the effects on the second generation. Assuming the exclusion restriction holds — that is twins effect the quantity and quality of a second generation only through increasing quantity in the first— then one additional birth increases the average family size of siblings by 0.095 births and decreases the average signature rate of their children by 0.5 percentage points. Accounting for the small increase in fertility, this implies that 92% of the direct effect on signature rates persists to the next generation.

Does this change in human capital have a possible spillover effect beyond the direct descendants? After all, growth theories often assume parents decide between child quantity and quality depending on the rate of technological progress, which in turn depends on aggregate human capital (Galor and Weil 2000). As show in Column 4, for this sample there is no significant interaction between the average human capital in the borough and decade of the twin birth and the size of the trade-off. Note that while this regression provides a causal estimate of the direct effects of a twin birth, the interaction term does not have a causal interpretation. If there was a significant and negative interaction term, the trade-off would in fact be larger in boroughs and decades with relatively higher aggregate human capital. This would not be proof that this steeper trade-off was caused by the higher aggregate human capital. Regardless, I find no evidence of such an interaction. It is also plausible that local aggregate human capital might be of little importance compared to national or even global aggregate human capital. However, without controlling for unrelated time trends with a panel fixed effects regression, it is even less likely that a correlation represents a causal link between aggregate human capital and the magnitude of the trade-off. Overall, while I can't conclusively rule it out, I find no evidence that the trade-off is higher as aggregate human capital increases. Therefore, any increase in human capital from a decrease in fertility would plausibly have no spillovers in the form of a steeper trade-off for subsequent generations.

Together, these estimates allow a simulation of the dynamic effects of reductions in fertility rates. While much simpler than a full structural model of the trade-off, these simulations give a rough illustration on just how small the effects are. Assume that the estimates from Table 2.7 are correct and that the percent of the effect inherited by the next generation is always constant. Permanently decreasing fertility from n_0 to a constant n_1 would have a net effect of $-0.005(n_1 - n_0)\frac{(1-0.92^g)}{(1-0.92)}$ on signature rates after g generations.[7]

The results of this simple simulation (Figure 2.4 below) show that a permanent decrease in fertility to French levels for women born 1636–1748 from Table 2.3 would allow Quebec to close the signature rate gap in 5 generations. As 5 generations is roughly 150 years, this is not a fast convergence. Moreover, the French average of 16.5% is a relatively low bar; five generations from those born in 1748 would be roughly in the 1920's, which as shown in Appendix A1 was a period of near universal literacy.[8]

For the 1748–1803 period, convergence is even slower.

Table 2.7: Estimates of the dynamics of the trade-off

	Sib signed	Sib's N births	Share of sib's children signed	Sibs signed
N births	−0.005***	0.080**	−0.005**	
	(0.002)	(0.037)	(0.002)	
Twin birth				−0.005**
				(0.002)
Lit. rate				0.026***
				(0.006)
Twin birth × Lit. rate				0.025
				(0.028)
N	855,861	855,861	855,861	1,225,871
FE: Boro. of birth				X
FE: Mother's age	X	X	X	X
FE: Parity	X	X	X	X
FE: Year of sib's birth	X	X	X	X

Note: *p<0.10; **p<0.05; ***p<0.01. The first three columns consider intergenerational effects. The sample is children of mothers born before 1760 with at least one child of their own. The first dependent variable is the same signature variable as before. The second is the number of births the sibling had. The third is the average signature rate of the sibling's own children. The third column has the same sample and dependent variable as the baseline regression in Table 2.4. Lit. rate is the average signature rate of first marriages in the borough and decade of the potential twin birth.

[7]Note that while is true that the estimates above imply that decreasing fertility from n_0 to a n_1 would also decrease fertility in the next generation by $0.08(n_1 - n_0)$, it is a decrease in reference to n_0 not n_1. Therefore, I instead simply assume an exogenous permanent decrease in fertility to n_1.

[8]And, ironically, when the demographic transition began to reach a substantial share of Quebec's families (Vézina et al. 2014).

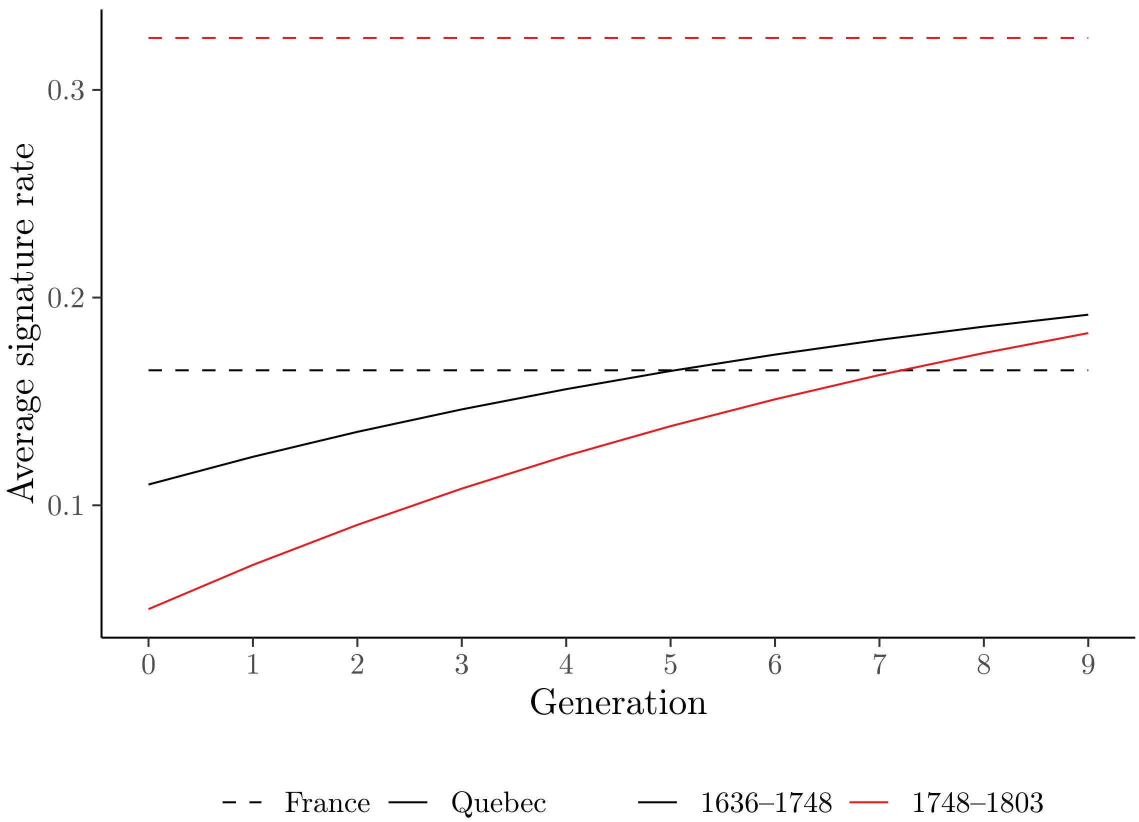


Figure 2.4: Simulation of reducing Quebec fertility to French rates *Note:* The simulation uses the values from Table 2.3 and Table 2.7 to roughly illustrate how the estimated quantity-quality trade-off is small in magnitude. Even with a decrease in births to French levels and an intergenerational persistence of 92% of the effect from one generation to the next, the accumulation of aggregate human capital is very slow.

2.4 Robustness

2.4.1 Alternative specifications

Below, I demonstrate that finding of a small yet statistically significant trade-off is robust to various tests of the empirical framework. The first test omits opposite-sex twins (Table 2.8, Column 2). As monozygotic twins are always same-sex, this restriction increases the share of twins that are monozygotic (Farbmacher et al. 2018). Therefore, any endogeneity bias from the probability of dizygotic twinning is reduced. The estimate is very similar. Moreover, including various parental and sibling characteristics as controls to the baseline specification also does not substantially change the results (Table 2.8, Column 3). These tests suggesting that there is little concern of endogeneity bias from non-random twinning after controlling for parity and mother's age.

Twins are not quite the same as one additional singleton birth. First, twin births are more likely to cause complications in childbirth, potentially leading to maternal death. Dropping mothers who died before age 40 (Table 2.8, Column 4) does not change the result. Second, twins result in atypical spacing of children over time (Black et al. 2005a, Black et al. 2010). If birth order has a different effect on outcomes than child order, then twins have a different effect on the following children than two singleton births. Restricting the sample to just older siblings addresses this concern. Moreover, it also rules out an effect on later siblings from parents altering spacing in response to twins. This restriction does not substantially change the result (Table 2.9, Column 2). Families in Quebec are also quite large, suggesting that siblings sufficiently older or younger than the twin birth might not be affected. Restricting the sample to only siblings within two births of the potential twin birth also does not change the result (Table 2.9, Column 3). Finally, although each mother contributes multiple observations to the regression, clustering the standard errors by mother only modestly increases the standard errors (Table 2.9, Column 4).

Twin infants are typically of below average health (see Table 2.1). While I primarily look at the outcomes of non-twin siblings, it is possible that parents either under-invested in sickly twins or compensated them with additional resources (Rosenzweig and Zhang 2009). To address this concern, I estimate the trade-off using two other instrumental variables.

Table 2.8: Alternative specifications, part 1

	Baseline	Same−sex twins	Extra controls	Mothers surv.
Number of births	−0.006***	−0.005***	−0.005***	−0.005***
	(0.001)	(0.002)	(0.001)	(0.001)
N	2,298,174	2,284,403	2,297,613	2,125,558
FE: Mother's age	X	X	X	X
FE: Parity	X	X	X	X
FE: Year of sib's birth	X	X	X	X
1st stage KP F−stat	7,493	4,244	7,976	8,680

Note: *p<0.10; **p<0.05; ***p<0.01. The first column is the baseline result from Table 2.4. The second column drops all opposite-sex twins, reducing any bias from non-random dizygotic twinning. The third column adds several control variables (the signature variable for both parents, the gender of the child, and the parity of the child). The fourth column restricts the sample to mothers who were not recorded dying before age 40.

Table 2.9: Alternative specifications, part 2

	Baseline	Older sibs	Sib parity within 2	Clustered SE
Number of births	−0.006***	−0.005***	−0.006**	−0.006**
	(0.001)	(0.002)	(0.002)	(0.002)
N	2,298,174	1,316,500	834,267	2,298,174
FE: Mother's age	X	X	X	X
FE: Parity	X	X	X	X
FE: Year of sib's birth	X	X	X	X
1st stage KP F−stat	7,493	5,705	2,269	1,145

Note: *p<0.10; **p<0.05; ***p<0.01. The first column is the baseline result from Table 2.4. The second column restricts the sample to only children born before the potential twin birth. The third column restricts the sample to children born within two births of the potential twin birth. The final column clusters the standard errors by mother since each mother has multiple associated observations.

2.4.2 Alternative instruments

The twin instrument might suffer from bias due to the relatively low health endowments of twin children. If parents reallocate resources towards children with higher birth endowments (reinforcement) or away from them (compensation), the IV estimates will be biased. I explore this potential source of bias by using two additional instruments for family size. Both have more concerning potential challenges to identification that the twin instrument. However, these potential challenges are different, and the results from these instruments corroborate those from the twin instrument.

For a second (novel) instrument, I argue that the province-wide infant mortality rate during the year a younger child was born is exogenous to individual family characteristics. I again compare two mothers with the same age and parity, one who gave birth during a year with relatively high infant mortality rate. I then look at other children from both families born in the same year in order to control for both their disease exposure and aggregate trends in literacy rates. For this regression, the spacing of subsequent births will be changed if the instrument has a valid first stage; infant mortality decreases birth spacing. There is also potentially higher risk of maternal mortality in high infant mortality years. Therefore, I restrict the sample to just mothers who do not die before age 40 and older siblings.

As shown in Table 2.10, both twins and children born in high mortality years have on average lower literacy rates than their siblings. As one shock increases and one shock decreases family size but both result in children born with lower endowments, reinforcement or compensation would bias the estimates in different directions. The IV estimates from both measures are in fact quite similar, despite the infant mortality rate being a weaker instrument and proving a less precise estimate (Table 2.11). Assuming both IV's are otherwise valid, this suggests that bias from compensation or reinforcement is not a major concern.

Table 2.10: Effect of twinning and infant mortality rates on signatures

	Self, IMR	Self, twin	Siblings, IMR	Siblings, twin
IMR	−0.779***		0.021*	
	(0.019)		(0.011)	
Twin birth		−0.019***		−0.006***
		(0.005)		(0.002)
N	230,415	259,187	1,215,119	2,295,644
FE: Mother's age	X	X	X	X
FE: Parity	X	X	X	X
FE: Year of sib's birth			X	X

Note: *p<0.10; **p<0.05; ***p<0.01. The dependent variable in columns 1 and 2 is the signature variable of the children born. The sample is the same as in Table 2.5. The dependent variable in columns 3 and 4 is the signature variable for other children who share a mother. The sample is restricted to both children born before the birth and mothers who were not recorded dying before age 40. IMR is the aggregate infant mortality rate for the entire province of Quebec during the year of the birth.

Table 2.11: Alternative instrument: infant mortality rates

	IV, twin	IV	1st stage	Reduced form
Number surv. to 1	−0.022***	−0.028*		
	(0.006)	(0.015)		
IMR			−0.762***	0.021*
			(0.091)	(0.011)
N	2,295,644	1,215,119	1,215,119	1,215,119
FE: Mother's age	X	X	X	X
FE: Parity	X	X	X	X
FE: Year of sib's birth	X	X	X	X
KP F−stat			70	

Note: *p<0.10; **p<0.05; ***p<0.01. Column 1 is from Table 2.5. In columns 2–4, the sample is restricted to both children born before the birth and mothers who were not recorded dying before age 40. IMR is the aggregate infant mortality rate for the entire province of Quebec during the year of the birth.

For the third instrument, I use the protogenesic interval, the time between the mother's first marriage and first birth (Klemp and Weisdorf 2018, Galor and Klemp 2019). This instrument is a measure of fecundity (i.e. potential fertility) as it captures biological variation in ability to conceive. While this instrument can be used in samples too small to effectively use twin births, it also has some less desirable properties. First, it makes a very strong assumption: that there was no premarital conception. If a couple conceived before marriage, the protogenesic interval will be too small. If premarital conception is more likely for some parents than others, this introduces endogeneity bias. Second, even absent premarital conception, the instrument's validity is uncertain. Various factors influence conception odds, such as maternal health, nutrition, and age. While age can be controlled for, there is still a serious possibility that socioeconomic status is somehow correlated with conception chances. This is why previous studies using the protogenesic interval rely on other control variables for identification.[9]

Despite the concerns related to its exclusion restriction, the protogenesic interval instrument gives a very similar estimate to the twin instrument (Table 2.12). It also would not

[9] For example, Galor and Klemp (2019) use lineage head fixed effects, arguing they control for non-random biological factors. Note that their regressions control for age irrespective of gender, which potentially leaves the instrument invalid. Female age at first marriage almost certainly has a different non-random effect on PI than male age at first marriage. The PI instrument, while potentially much stronger in a small sample, has much more serious endogeneity concerns than the twin instrument.

have bias from reinforcement or compensation, again suggesting that bias from compensation or reinforcement is not a major concern.

Table 2.12: Alternative instrument: protogenesic interval

	IV, twin	IV	1st stage	Reduced form
Number of births	−0.006***	−0.004***		
	(0.001)	(0.001)		
PI			−0.686***	0.003***
			(0.013)	(0.001)
N	2,298,174	221,309	221,309	221,309
FE: Mother's age	X			
FE: Parity	X			
FE: Year of sib's birth	X	X	X	X
KP F−stat			2,710	

Note: *p<0.10; **p<0.05; ***p<0.01. Column 1 is from Table 2.4. Columns 2–4 look at children as the unit of analysis, not child-birth pairs. PI is the protogenesic interval of the mother, the time between her first marriage and her first birth. PI is not arbitrarily trimmed as in Galor and Klemp (2019), leaving values with implausibly short gestation periods. I argue this is better than arbitrarily censoring implausible values, as that only omits premarital conceptions with short PIs and leaves premarital conceptions with longer PIs in the sample, introducing potential sample selection bias.

Together, the estimates from alternative instruments suggest that bias from compensation or reinforcement is not a major concern in this population. It certainly does not imply, however, that in other populations twin instruments would not suffer from compensation or reinforcement. The relatively egalitarian treatment of children with lower or higher birth endowments could be particular to Quebec.

2.5 Discussion

In pre-demographic transition Quebec, the natural experiment of twins provides evidence that there was a small trade-off between family size and the average human capital of children. The estimated trade-off — a decrease in the odds a sibling signed their marriage record of 0.6 percentage points per additional child born — is statistically significant and robust to various tests of potential threats to identification. Using multigenerational linkages, I show that although the effect of an increase in family sizes seems to be strongly inherited, the magnitude of the trade-off is too small to be of major economic significance.

Why was the trade-off so low? One explanation for the lack of substantial trade-off in contemporary populations is the availability of public education (Angrist et al. 2010). At one extreme, if education is free, there is no trade-off. At the other extreme, if education is prohibitively expensive, an additional child makes no difference. Before the 1840's, Quebec's formal education system was almost entirely provided by the Catholic Church. Various religious orders and parish schools provided a range of educational services.[10] While tuition was often free, food and board was not, and there was always the opportunity cost in terms of forgone child labor (Magnuson 1992). Perhaps the small trade-off was a product of Quebec's limited public education during the period. Then again, evidence from rural France during a similar period suggests that fertility fell before education rose, which in turn occurred before industrialization (Blanc and Wacziarg 2020).

What, then, do we learn about theories of long-run economic growth? Quebec had already began industrialization by the 1830's, with modern water-powered factories emerging after the Lachine Canal was enlarged in the 1840's and a major railway with the Grand Trunk in the 1860's. (Courville et al. 2006, Bradbury 2003).[11] Montreal emerged as a major industrial center in the second half of the 19th century. Moreover, Quebec's demographic transition occurred substantially later, only reaching substantial numbers of French-speaking Québécois by the 1920's (Vézina et al. 2014).

It appears that the existence of a substantial quantity-quality trade-off is not a necessary condition for industrialization. This does not, however, necessarily contradict the theories that place it at the heart of modern economic growth. These theories allow that regions importing preexisting technologies from an already industrialized area might have different dynamics (Galor and Weil 2000). Perhaps the important trade-off was that faced by families in London or Boston, not Montreal or Quebec City. If so, the so-called "Western

[10] The female Congrégation de Notre-Dame was particularly active. In general, nuns were more focused on education, though there were a few schools run by male orders such as the Recollects (Greer 1997).

[11] The marriage records aren't particularly suited to detecting industrialization because they list only occupations, not industries. The first mechanic shows up in the marriage records in 1818, machine maker in 1832, railway worker in 1855.

Offshoots" (the United States, Canada, Australia and New Zealand), considered to be on the growth frontier alongside Western Europe, should be revised to exclude Quebec (Galor 2005). Alternatively, Quebec could be added alongside France (with its inconveniently early demographic transition) as an example that however elegantly a theory unifies economic growth with demography, the empirical evidence eludes a straightforward grand narrative.

Appendix

A1 Signatures and literacy

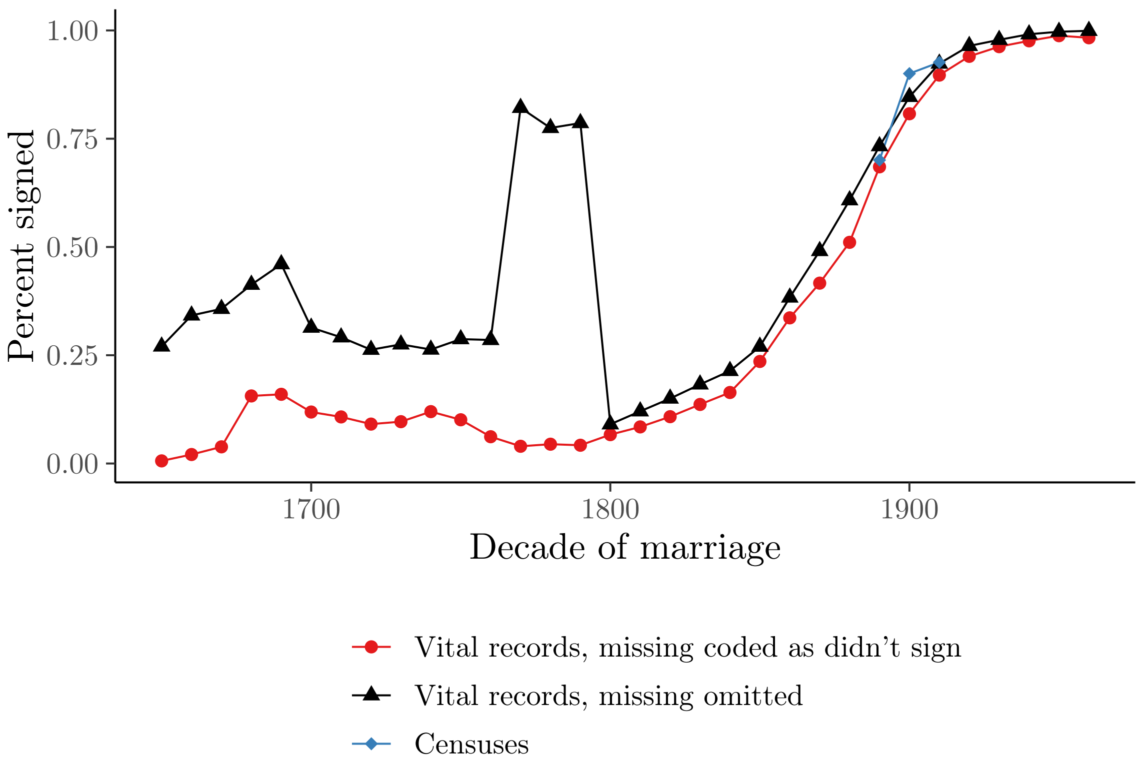


Figure 2.5: Different ways to code the signature variable *Note:* The vital record literacy rate is the average of an indicator variable that is one if a signature was recorded, zero if the absence of a signature was recorded, and either zero or omitted otherwise. The census record literacy rate is the fraction of individuals who were reported as able to write, reweighted to match the age distribution in the vital records.

Figure 2.5 above compares two different ways of coding the signature variable. Four extracts of Canadian censuses 1891–1911 provide external points of comparison.[12] After 1800, coding missing signatures as missing values provides an estimate of literacy closer to that in the censuses. Before 1800, coding the missing signatures as 0 provides a more stable measure over time.

[12]The 5% 1891 sample (Inwood and Jack 2011), the 5% 1901 sample and 1901 oversample (Canadian Families Project 2002), and the 5% 1911 sample (Gaffield et al. 2009). Data provided by the Minnesota Population Center (2019).

Chapter 3

The her in inheritance: marriage and mobility in Quebec 1800–1970

3.1 Introduction

Women had limited educational and employment opportunities until the late 20th century. Before then, it is easy to assume that as only men held formal employment, who they married would not matter for income inequality. However, this assumption overlooks an important channel through which marriage — and women — mattered: intergenerational mobility.

To determine if marital assortment had economic consequences before the late 20th century, this paper sets out to answer three related questions. First, how did the degree of marital assortment evolve over the long run? Second, did the abilities of women determine her match, or was assortment just between families? Third, how much did the human capital of mothers matter compared to that of fathers for the outcome of their children?

Using the IMPQ data from Quebec 1800–1970,[1] this paper shows that the degree of marital assortment was surprisingly high and stable from the 1830s through the 1960s. Moreover,

[1]IMPQ 2020, Project Balsac 2020, PRDH 2020.

this assortment was on the human capital of both men and women. Using remarriages, I show that child outcomes depended equally on the ability of mothers and fathers. Despite deeply conservative gender roles, in Quebec 1800–1970 women mattered as much as men for marriage and mobility. Thus, assortative marriage was economically significant long before the rise of female employment and education in the late 20th century.

The historical data from Quebec have several features that are particularly suited to answering these questions. First, the data are close to a complete population registry. Families in the sample are not selected by cohabitation (like in census records) or by living descendants (like in many genealogical records). Second, complete family structures are reconstructed in the dataset. Unusually, Québécoise women retained their family name after marriage and therefore can be linked to their parents. I am able to use within-family variation combined with the size of the dataset (several million observations) to identify the mechanisms linking assortment and mobility.

Before I consider mobility, I first develop a method to measure the degree of assortment. The data have two complementary proxies for a woman's human capital: her ability to sign her name and the occupational status of her husband or father. The ability to write is a direct measure of human capital that is consistently available for both men and women. However, it corresponds to a very low level of education. After 1900 almost all people were literate. Occupations are a more detailed measure but are only available for men. Therefore, I use signatures when I need a direct measure and the occupation of a woman's husband or father when I need a detailed measure. To infer the degree of assortment from occupations, I construct a simple model of assortment. In this model, a man is two degrees separated from his father-in-law (husbands are similar to their wives and children are similar to their fathers), whereas he is only one degree separate from his father. The correlation between spouses is thus equal to the correlation between men and their father-in-laws divided by the correlation between men and their fathers. This correlation was surprisingly strong and stable from the 1830s through the 1960s.

Does this very high and stable level of assortment, though, have any economic significance? I show it does by using within-family variation to disentangle the interactions between matching and mobility. First, I use family-fixed effects to determine if spouses matched on individual human capital. I find that literate women typically married higher status husbands than their illiterate sisters. The same is true for brothers. Notably, the returns to literacy are symmetric across gender. When it came to marriage, the human capital of women mattered as much as that of men.

Second, I use remarriages to show that the human capital of women mattered for child outcomes. A mother's literacy was correlated with her children as strongly as that of her husband. However, this would still be possible if the mother did not directly matter as long as marriages were assorted on literacy. To address this empirical challenge, I use remarriages to control for the father, both through fixed effects and by directly comparing half siblings. I find evidence that the human capital of mothers mattered as much as that of fathers.

The consequences of these findings are twofold. First, they strongly imply that assortment contributed to the overall distributions of human capital and socioeconomic status in the province. Moreover, there was remarkably symmetrical effects across the gender of both the parents and children. Despite severe legal and economic disadvantages, women appear to have had an equal role in this at least. Second, the results have implications for standard estimates of intergenerational mobility. I show that assortative marriage directly influences estimates of rates of mobility even when only men are considered. A change in an estimate of mobility could be entirely driven by a change in assortment or in the influence of mothers.

This paper thus contributes directly to two literatures. First, the paper provides longer-run context to more contemporary studies of assortative marriage. The late 20th century saw a rise in female employment rates and the closing of the gender gap in educational attainment (Goldin 2006). This has led to widespread concern that an increase in both assortative marriage and two-income households would lead to greater inequality. The empirical evidence however suggests that were was little resulting change in assortment or inequality (Eika et al. 2019, Hryshko et al. 2017). This paper provides a longer-run context that suggests an explanation: marriage had long been highly assortative.

Second, this paper adds to our understanding of intergenerational mobility over the long run. Until recently, studies of intergenerational mobility have traditionally overlooked women (Black and Devereux 2011). More recent work considers the mobility of daughters as well as sons (Chadwick and Solon 2002). In historical settings, this is typically done by comparing daughters to their fathers (Olivetti and Paserman 2015, Craig et al. 2020,Olivetti et al. 2020). This paper goes even further by considering the role of their mothers as well. It finds that mothers mattered and demonstrates that even if they are not observed they can effect the standard estimates of mobility.

The structure of the paper is as follows. First, I provide a brief summary of the historical context. Second, I describe the dataset, how families were linked, and the measures I use of human capital. Third, I construct a measure of the degree of assortment over time. Fourth, I present evidence that this assortment consisted of matches between individuals of similar human capital. Fifth, I estimate the effect of parental human capital on child outcomes. Sixth, I discuss the broader implications of these findings. Finally, I conclude: assortment mattered for inequality long before the late 20th century because women had always played an important role in marriage and mobility.

3.2 Historical context

Before its Quiet Revolution of the 1960s, Quebec was much less secular than its North American neighbors.[2] Catholicism asserted significant control over public education and social norms, and deeply conservative beliefs about gender roles were enshrined by law and public policy. For example, while Québécoise women could vote in federal elections after

[2]The term quiet revolution is also used for the increase in female labor force participation and educational achievement of women in many countries starting in the 1970s (Goldin 2006).

1918, they could not vote in local elections until 1940 (Tremblay and Roth 2010).[3]

3.2.1 The legal rights of women

Women in most historical societies faced systematic legal disadvantages; Quebec was no different. While Quebec was ceded to the British in 1763, laws pertaining to civil matters remained governed by the Custom of Paris, a codified system of customary French law. Under the Custom of Paris, and unlike in English-speaking legal traditions, married couples formed a legal entity called the *communauté de biens* (community of property) in which both partners theoretically had equal stakes (Greer 1997). As a consequence, both the husband and wife were required to sign legal documents,[4] though the husband alone was expected to manage the joint property. Worse still, married women were legally considered incapable, being unable to sign contracts or initiate a lawsuit (Baillargeon 2014). The Civil Code of Lower Canada, introduced in 1866, only clarified the legal disadvantages of women. Only after reforms starting in 1964 were women no longer legally incapable.

In theory, the law did not discriminate when it came to inheritance. The community of property was dissolved by giving the surviving spouse their half and dividing the rest equally amongst the children regardless of gender.[5] Perhaps as a consequence of being unable to write children out of a will, parents had little legal recourse to block a match they disapproved of after the children reached a certain age (Greer 1997). However, it gradually became the norm to "gift" property to favored heirs, typically an older son (Greer 1985).

[3] Unusually, from the first elections in 1792 until 1849, suffrage was only restricted to individuals meeting age and property requirements; a very small number of women who independently owned property could and did vote based on these criteria. This was considered by male 19th century reformers a concerning oversight that needed to be addressed.

[4] As mentioned below, this greatly aids the linking of vital records.

[5] One of the few legal advantages women had was that a widow could renounce the debts of the community of property as it was assumed she was not responsible for their accumulation.

3.2.2 The demographic regime

Was an unequal partnership in marriage the typical experience for women in Quebec? Quebec had a variant of the European marriage pattern, with younger marriages and less frequent celibacy than France (Greer 1997). Quick remarriage upon the death of a spouse was common. While married, a woman typically gave birth to a child roughly every two years until her forties. The majority of women married and, for most, marriage marked the beginning of many years of pregnancy and childcare.

While this variant fertility pattern was common in European settler colonies, Quebec kept it longer than most. The demographic transition occurred later, only reaching substantial numbers of French-speaking Québécois by the 1920s (Vézina et al. 2014). Moreover, from first settlement through at least 1835, there appears to have been no attempt of parents to target a specific family size (Clark et al. 2020).

3.2.3 Women and the workforce

While the economy of Quebec evolved dramatically from 1800–1970, women consistently had limited opportunities in the formal labor market. In the first half of the 19th century, women usually contributed labor to household production as well as to domestic tasks (Baillargeon 2014). In urban areas, formal employment was available to unmarried women as domestic servants. Women also found employment as educators, first as nuns and and later as secular teachers. As the economy began to industrialize in the 1840s, unmarried women were also employed by factories (typically clothing or tobacco), albeit with substantially lower wages than men. As it did elsewhere, industrialization also led to the decline of household production and the rise of the male breadwinner household (de Vries 2008). By the late 19th and early 20th century, female dominated occupations emerged such as telephone operators, typists, and secular nurses. However, married women were still expected to be housewives until the 1970s.

3.2.4 Quebec, North America, and external validity

Overall, how much was Quebec an outlier? In general, it was worse-off than the rest of North America but followed the same trends. For example, it had lower wages until the late 20th century but the gap was stable over time (Albouy 2008, Geloso and Lindert 2020). Its deeply conservative society delayed the extension of rights to women, but not indefinitely. Its demographic regime was characterized by large family sizes and a delayed demographic transition, but it was still a variant of the European marriage pattern. The role of women in the labor force evolved roughly the same as the rest of North America (Goldin 2006).

I argue, therefore, that my findings from Quebec are likely generalizable to the rest of North America. While Quebec was behind its neighbors in levels of human capital and in rights for women, it was not fundamentally different when it came to marriage and mobility. If women and assortment mattered for mobility even in Quebec, they probably mattered in neighboring regions.

3.3 Data

While the IMPQ contains data as far back as the founding of the colony, in this paper I use data from a period with frequently reported occupations for men, 1800–1969.

3.3.1 Linked family vital records

Two unusual institutional features of Quebec have resulted in vital records that are particularly easy to link. First, due to the system of community property, both husbands and wives signed their names on all legal documents. Second, women kept their family names when they married. This means both that women can be linked to their fathers and that most vital records have four names on which to link (the first names and last names of both the husband and wife or mother and father).

In both of the databases, links were formed using two similar computer-assisted matching systems (Vézina et al. 2013, Dillon et al. 2018). These two procedures differ slightly,[6] but they both use the four names to link records and resort to manual linkage in difficult cases. Manual linkages are not necessarily better than automatic linkages; in some applications they produce both more true matches and more false positives (Abramitzky et al. 2019). However, the fact that the Quebec vital records have four names to match on should increase the accuracy of matching regardless of the method used. Moreover, the parish records of Quebec have survived remarkably intact as local priests were required to send duplicates of all records to their superiors (Dillon et al. 2018). Therefore, records of almost the entire population survive; this will reduce the rate of false positive matches in an analogous way to the linking of full count to full count censuses (Abramitzky et al. 2019).

3.3.2 Measures of human capital

The direct measure of human capital I use in this paper is the presence of a signature on a marriage record. Catholic churches had long required required both the bride and the groom to sign their marriage records if they were able and the priest to record if they were not. I code a signature indicator variable as one if the individual signed their marriage record, and zero if they were unable to sign (Gagnon et al. 2011). I omit cases that are either missing or ambiguous. As shown in Figure 3.1 below, this definition produces a trend that is close to external estimates of literacy. Was a signature really a measure of human capital, that is a productive attribute? The qualitative evidence suggests it was. Signatures are a proxy for the ability to write, a form of human capital that had always been particularly associated with business activity in Quebec (Greer 1997).[7]

The vital records also often state the occupations of men. I assign each individual the occupation listed at their first marriage (if any). The occupations are assigned HISCO codes, a classification system designed for comparative studies of historical social mobility

[6] e.g. the BALSAC database standardizes names using the FONEM phonetic algorithm (Bouchard et al. 1981), whereas the RPQA uses a custom-made name dictionary

[7] Reading too was likely associated with economic activity in Quebec. In contrast to their Protestant neighbors in New England, for Quebec's Catholics reading the Bible was not a religious necessity.

(Van Leeuwen et al. 2004). I then assign various occupational status scores to these HISCO codes aggregated to the three digit level. The primary score I construct is the average yearly earnings reported by men with that occupation in Quebec in a 5% sample of the 1901 Canadian Census (Canadian Families Project 2002 and Minnesota Population Center 2019).[8] There are numerous other ways to rank occupations, as discussed in Appendix A2. However, these imputed earnings are easy to interpret,[9] are at least a proxy for the standard variable of interest in mobility studies (lifetime earnings), and produce similar estimates to the other ranking systems. Therefore, the main results in this paper use the 1901 imputed earnings as the primary measure of occupational status.

3.3.3 Women's presence in the vital records

Do the vital records accurately record the occupational status and human capital of women? Four extracts of Canadian censuses 1881–1911 and data compiled by Long (1958) for 1920–1960 provide external points of comparison (Dillon et al. 2008, Inwood and Jack 2011, Canadian Families Project 2002, Gaffield et al. 2009, Minnesota Population Center 2019, Long 1958, Killingsworth and Heckman 1986).[10]

Figure 3.1 compares the fraction of individuals signed their first marriage record to the fraction who self-reported the ability to write in the censuses. Unlike the censuses, individuals only appear in the marriage records during a specific time in their lives. To account for this, I reweight the census data to match the age distribution of the vital records. As shown in the figure, my estimated literacy rate closely tracks the rate in the census. Two patterns are particularly notable. First, Quebec went from a very low human capital society to a high human capital society from 1800 to 1920. Second, there was actually a gender gap in favor of women from 1850 to 1920.

[8] This requires crosswalking occupations from IPUMS's occupational codes to the original HISCO scheme (Zijdeman 2014).

[9] i.e. how much the individual would earn, on average, with their occupation in 1901.

[10] The census extracts are the 100% 1881 sample, the 5% 1891 sample, the 5% 1901 sample and 1901 oversample, and the 5% 1911 sample.

In contrast, the vital records do a poor job of recording female occupations. Figure 3.2 shows the employment rate of women by marital status. Here, I reweight the vital records data to match the age distribution in the census. Compared to the other sources, the vital records underestimate the employment rate of married women and almost entirely omit unmarried women with occupations. One pattern, however, is clear. While unmarried women often worked outside the home, married women did not begin to work in substantial numbers until the 1940s.

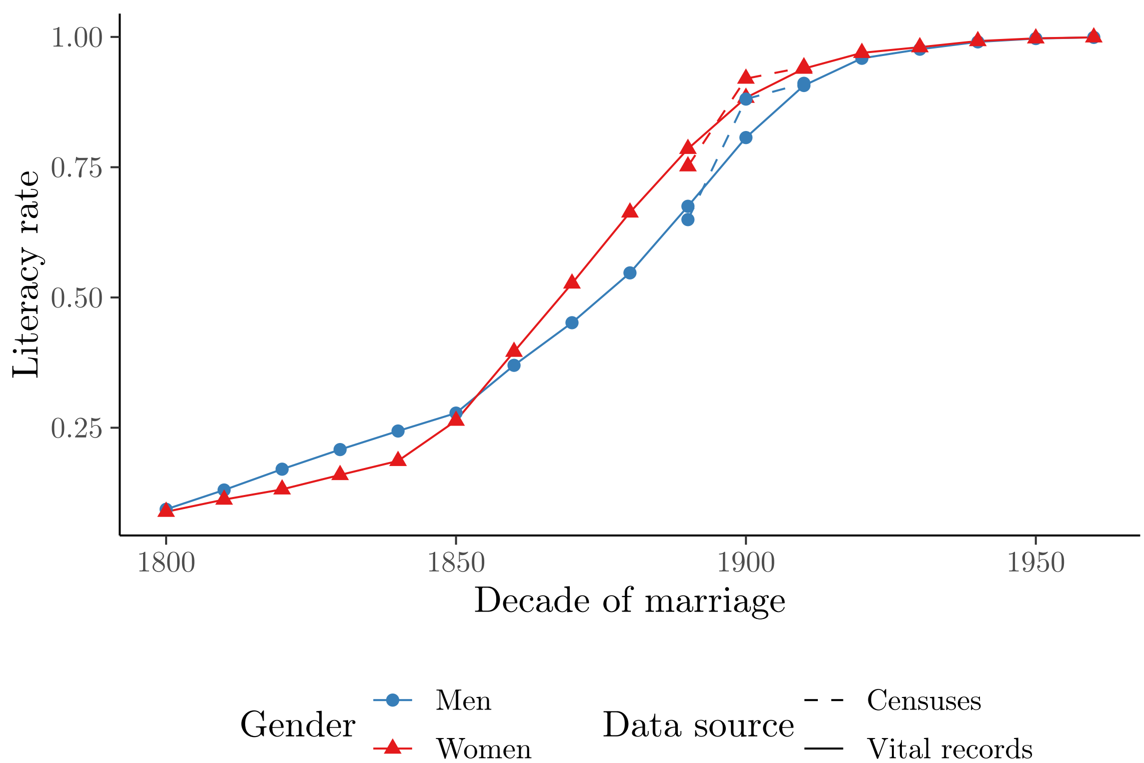


Figure 3.1: The vital records accurately report the ability to write
Note: The vital record literacy rate is the average of an indicator variable that is one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. The census record literacy rate is the fraction of individuals who reported that they were able to write, reweighted to match the age distribution in the vital records. The two sources broadly agree.

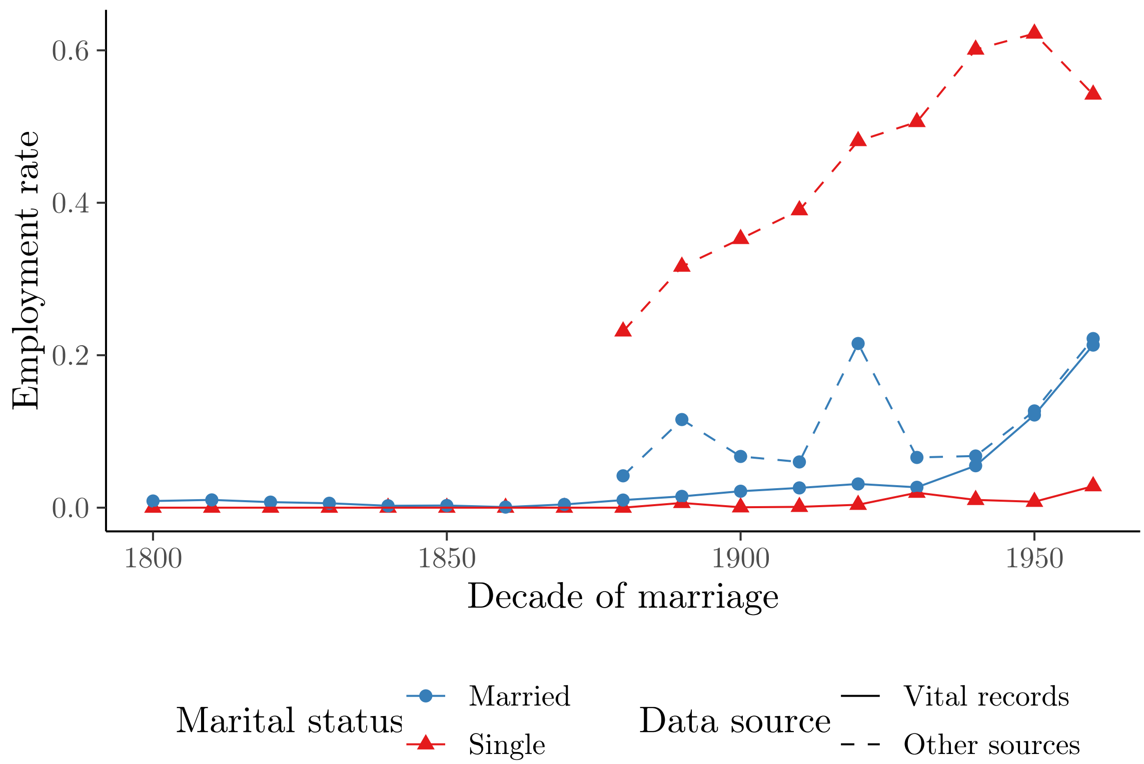


Figure 3.2: The vital records fail to record female employment
Note: Each woman in the vital records is counted as employed if she ever had a job reported and as not employed otherwise. She is then assigned a year equal to the median of all the years at which she is observed. Last, I compute the average employment rate for each decade, reweighted by age to match the age distribution in the census data. Before 1920, the other sources are census extracts and the rates the fraction of women aged at least 16 with an occupation. After 1920, the other sources are rates compiled by Long (1958), with the married rate calculated as an average of the rates for currently married women and widowed or divorced women weighted by the relative frequencies of the two categories in the censuses.

3.4 Measuring the degree of marital assortment

Below, I develop a model to illustrate how marriage and intergenerational mobility contribute to inequality over the long run. This framework, while simple, suggests a new method to measure the degree of marital assortment. Using this method, I show that assortment was surprisingly high and stable over the period 1830–1969.

3.4.1 A simple model of marriage, mobility, and inequality

We are interested in a specific measure of potential socioeconomic status, call it x. In the intergenerational mobility literature, the typical measure of status is lifetime earnings. The distinction I make here is that I consider *potential* socioeconomic status. For example, while women often were not employed, they still possessed human capital that could, in different circumstances, be used to earn a wage.

Following Solon (1992) and Clark and Cummins (2015a), assume only an imperfect measure or proxy y is observed for x. The typical example is proxying for lifetime earnings with the earnings in a specific year. As this model considers potential status, for some individuals no y is observed. For the others, y is observed with classical measurement error:

$$y_i = x_i + u_i \tag{3.1}$$

for individual i, where u_i is an error term uncorrelated with x_i.

Then assume that the status of child c, x_c, is inherited depending on the status of the child's father x_f and mother x_m:

$$x_c = \beta_f x_f + \beta_m x_m + e_c \tag{3.2}$$

where e_c is a random term uncorrelated with the x's. For now, assume that the effect on children is the same regardless of gender. While this seems a strong assumption, it makes the model much more tractable and I will later provide evidence that it appears reasonable in my context. Moreover, note that u_c and e_c can be allowed to vary by the gender of the child to model the likely scenario where there is a gender gap in realized status.

Following Chadwick and Solon (2002), assume that the assortment on potential status can be summarized by:

$$corr(x_f, x_m) = \gamma \tag{3.3}$$

Now note that if variances are equal, I can re-write the assortment correlation equation as a linear relationship:

$$x_i = \gamma x_s + v_i \tag{3.4}$$

where s is i's spouse and v_i is an uncorrelated error term. If I substitute this into the intergenerational mobility equation, I get:

$$x_c = (\beta_f + \gamma\beta_m)x_f + \gamma\beta_m v_f + e_c \tag{3.5}$$

This can be estimated with a regression:

$$y_c = \alpha_0 + \alpha_1 y_f + \epsilon_c \tag{3.6}$$

where $\alpha_1 = (\beta_f + \gamma\beta_m)$. However, as y_f is correlated with u_f the estimate is attenuated down. Specifically, as:

$$y_c = (\beta_f + \gamma\beta_m)y_f - (\beta_f + \gamma\beta_m)u_f + \beta_m v_f + e_c - u_c \tag{3.7}$$

there is bias of the form:

$$plim\ \hat{\alpha}_1 = (\beta_f + \gamma\beta_m)\frac{\sigma^2_{x_f}}{\sigma^2_{x_f} + \sigma^2_{u_f}} \tag{3.8}$$

If β_f and γ are greater than zero, then mothers contribute to the observed correlation of x between fathers and sons. Appendix A1 extends this model, demonstrating the effects of β_f and γ on inequality.

3.4.2 Measuring assortment if women are not observed

Typically, assortment is measured by the correlation between the education levels of spouses. However, as shown in Figure 3.1, the average signature rate changed dramatically during this interval. As the average education level increased, the average education level of individuals who could at least sign their name would have also increased. Therefore, the relationship between the underlying degree of assortment on human capital and the observed degree of assortment on signature rates is likely changing over time.

A more stable measure of ability is an individual's occupational status. However, in most of the sample married women have no observed occupational status (and in the mid 20th century when it became more common, they are concentrated in lower status occupations such as housekeeping). As an alternative, a measure can be constructed by comparing the correlation of the occupational status of sons-in-law and fathers-in-law to that of sons and fathers. The former are two degrees separate: an intergenerational link from father-in-law to daughter and a marriage link from daughter to son-in-law. The latter has only one degree of separation: an intergenerational link from father to son. In the simple model of intergenerational mobility I develop above, the ratio of correlations is equivalent to the degree of assortment. Relaxing the model's assumptions, the ratio will still hopefully control for trends in intergenerational mobility and leave only the trends in assortment.

Letting y_{fl} be the observed status of the father-in-law of i:

$$y_i = \gamma(\beta_f + \gamma\beta_m)y_{fl} - \gamma(\beta_f + \gamma\beta_m)u_{fl} + \gamma\beta_m v_{fl} + \gamma e_i + v_i - u_i \tag{3.9}$$

and:

$$y_i = (\beta_f + \gamma\beta_m)y_f - (\beta_f + \gamma\beta_m)u_f + \beta_m v_f + e_i - u_i \tag{3.10}$$

Regressing y_i on y_{fl} and on y_f, the ratio of the coefficients has the probability limit of:

$$\gamma \frac{\sigma^2_{x_{fl}}(\sigma^2_{x_f} + \sigma^2_{u_f})}{\sigma^2_{x_f}(\sigma^2_{x_{fl}} + \sigma^2_{u_{fl}})} \tag{3.11}$$

which should be equal to γ if the distribution of x_f is the same as that of x_{fl}.[11]

[11] Appendix A2 estimates γ using an alternative IV method that does not require this assumption, and

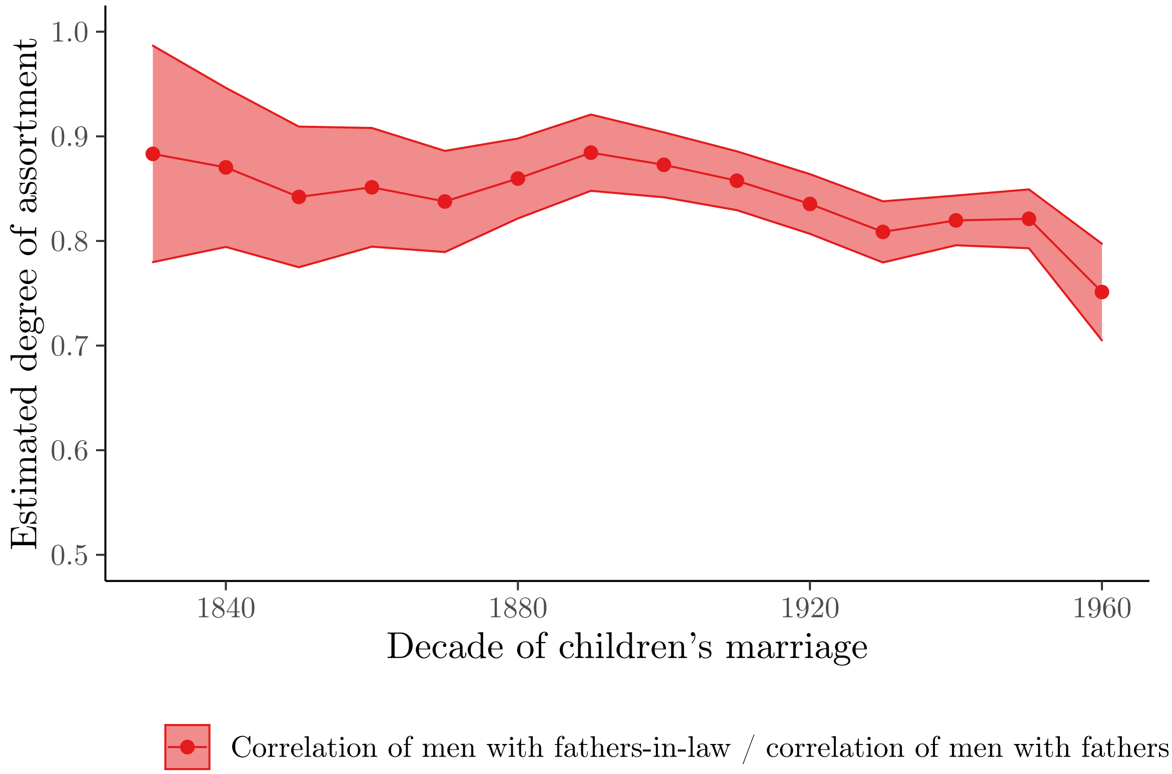


Figure 3.3: Ratio measure of martial assortment using imputed earnings
Note: 95% confidence interval shaded. Standard errors are bootstrapped with 50,000 repetitions. In the simple model described in the paper, this ratio is equal to the correlation in potential socioeconomic status between spouses. The father-in-law and son-in-law correlation measures both assortment and intergenerational mobility with attenuation bias. The father and son correlation measures only intergenerational mobility, again with attenuation bias. The ratio then, under a few assumptions, measures assortment directly. The correlations are using imputed earnings which are the average earnings for the individual's occupation in Quebec based on a 5% extract of the 1901 Canadian Census. The ratio is computed as the ratio of two rank-rank regression coefficients. (Chetty et al. 2014). As ties are not broken randomly, the rank variables do not have exactly equal variances and therefore the coefficients only approximately equal to Pearson correlation coefficients.

3.5 Spouses matched on their human capital

Were marriages matches on the individual characteristics of the spouses? One could imagine a society in which this was not the case. For example, perhaps, a society where marriages were negotiated to form an alliance between a husband and his father-in-law with

the results are very similar.

the characteristics of the wife an afterthought at best.[12] In this hypothetical society, there could still be an observed correlation in the human capital of spouses if a woman's human capital is partially determined by her father.

To test if individual characteristics mattered, consider the following fixed effects regression:

$$y_s = \alpha y_i + \phi_F + \beta \mathbf{X_s} + \epsilon_{i,F} \tag{3.12}$$

where y_i is a characteristic of individual i of family F, i_s is a characteristic of spouse s of individual i, ϕ_F are the crucial fixed effects that control for family, $\mathbf{X_s}$ is a vector of controls, and $\epsilon_{i,F}$ is a family-clustered error term. To address any time trends, $\mathbf{X_s}$ includes fixed effects for both decade and the order of siblings.[13]

In other words, the regression asks if, compared to their siblings, an individual with higher ability matches with a spouse of higher ability? If so, α will be positive.

As shown in Table 3.1 Panel A below, a woman who signed her marriage record married a man with higher status than her sisters who did not. Being able to write was associated with an increase in the probability a woman's husband was literate by 30 percentage points, an increase in her husband's imputed earnings by 4%, and an increase in her father-in-law's imputed earnings by 2%. This is evidence that marriage matches took into account individual characteristics. Note that while the family fixed-effect does reduce $\hat{\alpha}$, this does not reveal the degree to which matches are coordinated by families. If matching is only on individual characteristics, the family fixed-effect will still reduce $\hat{\alpha}$ as long as the human capital of sisters is correlated.

What about men and their brothers? As shown in Table 3.1 Panel B below, men who were able to write also were at an advantage when finding a wife. Being able to write associated

[12] Marriage as a way of cementing a commercial alliance was not unknown to the early settlers in Quebec. Indeed, marriage *à la façon du pays* ("after the custom of the country") between an indigenous woman and a French fur trader was commonly practiced to cement commercial relationships (Baillargeon 2014).

[13] As I only have date of birth through 1849, I order siblings by the date of their first marriage.

with an increase in the probability that a man's wife was literate by 28 percentage points and an increase in his father-in-law's imputed earnings by 3%. These estimates are remarkably similar to those for women. The returns to human capital for marriage matching appear to be the same regardless of gender. Appendix A3 discusses the robustness of these estimates.

What is the economic significance of marriage matches being formed on the individual characteristics of women? If individual characteristics did not matter, the woman's family could after all still matter for the outcomes of her children. First, it shows that there was a return to education for women in terms of the economic status of the household she formed at marriage. This was the case even if she did not employ her human capital in a formal occupation. Second, assuming mothers mattered directly for their children's outcomes, it implies a stronger role for assortative marriage in intergenerational mobility. Finally, it at least hints that women may have had some agency over the marriage matching process.

Table 3.1: Marriage matches were determined by individual characteristics

	Dependent variable: Spouse's characteristic					
	Signed		Log imp. earnings		Father's log imp. earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of wife's human capital						
Wife signed	0.49***	0.30***	0.17***	0.04***	0.07***	0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Wife's family FE		X		X		X
Identifying observations		203,284		124,731		108,199
Observations	1,937,871	1,937,871	1,148,769	1,148,769	971,173	971,173
Adjusted R^2	0.60	0.64	0.06	0.38	0.03	0.32
Panel B: Effect of husband's human capital						
Husband signed	0.41***	0.28***			0.11***	0.03***
	(0.00)	(0.00)			(0.00)	(0.00)
Husband's family FE		X				X
Identifying observations		230,364				123,465
Observations	1,928,239	1,928,239			986,398	986,398
Adjusted R^2	0.62	0.64			0.04	0.33

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. The sample excludes individuals with one or more unknown parents. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. Decade fixed effects are included in every specification as control variables. Note that after adding family fixed effects, the effects are close to symmetrical across gender. Identifying observations are the number of observations from families where at least one child signed and one child did not sign.

3.6 The ability of mothers mattered for child outcomes

Did the abilities of mothers directly influence the outcomes of their children? Here, I am not interested in the exact mechanism.[14] All that is required for women to play a role in intergenerational mobility is for their abilities to matter as well as those of their husbands.

[14]Such as, for example, the distinction between human capital and innate ability (Black et al. 2005b).

3.6.1 Controlling for the father with fixed effects

A mother's literacy is correlated with that of her children even after controlling for that of the father (Table 3.2). However, this pattern could still be observed if the mother did not directly matter for the outcomes of the children. With assortment, if the husband's ability is observed with measurement error, the mother's ability would be correlated with the residual even if its true effect is zero. Therefore, the simple regressions in Table 3.2 do not identify the effect of the ability of mothers. Notably, there appears to be no difference between the associations with children of different genders.

To identify a causal effect, ideally we would control for the father but randomize the mother. A less ideal yet possible approach is to consider the case where a father has children from more than one marriage. However, this results in two complications. The first is the chance that the children are scarred by whatever event resulted in a second marriage (such as a death or divorce). Assuming the penalty is a constant, it can be controlled for by including fixed effects for the marriage number the children are from. Second, as marriage is assortative on the ability of mothers, the abilities of each wife of the father will be correlated. Therefore, similar to the family fixed effects above, the father fixed effect will absorb part of the effect of the mother's ability.

I regress:

$$y_c = \alpha y_m + \phi_f + \beta \mathbf{X_c} + \epsilon \tag{3.13}$$

where y_c is an outcome of a child, y_m is a characteristic of the mother, ϕ_f are the crucial fixed effects that control for the father, and $\beta \mathbf{X_c}$ are controls. To address any time trends, X_c includes fixed effects for decade, the marriage number of the father, and for the order of siblings.

As shown in Table 3.3 Panel A, even controlling for the father, a mother who could sign her name had children who were 3% more likely to be able to sign their names and had a

3% higher occupational status score.

For comparison, Table 3.3 Panel B estimates the effects of the ability of a father controlling for the mother. Notably, the results are very similar to those controlling for the father. Once the correlation between the ability of spouses is accounted for through fixed effects, the effect of parental human capital appears to be symmetrical across gender. Appendix A4 discusses the robustness of these estimates.

Table 3.2: The association of parental human capital with child outcomes

	Dependent variable:			
	Signed	Signed	Log imp. earnings	
	Daughter	Son	Daughter's-husband	Son
	(1)	(2)	(3)	(4)
Mother signed	0.12***	0.12***	0.06***	0.07***
	(0.001)	(0.001)	(0.002)	(0.002)
Father signed	0.07***	0.09***	0.15***	0.16***
	(0.001)	(0.001)	(0.002)	(0.002)
Observations	1,551,089	1,435,443	938,558	875,264
Adjusted R^2	0.51	0.50	0.06	0.06

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. The sample excludes individuals with one or more unknown parents. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. Fixed effects for decade, marriage number, and sibling order are included in every specification as control variables.

Table 3.3: The effect of parental human capital on child outcomes

	Dependent variable:			
	Signed Daughter	Signed Son	Log imp. earnings Daughter's husband	Log imp. earnings Son
	(1)	(2)	(3)	(4)
Panel A: Controlling for father				
Mother signed	0.02***	0.03***	0.01	0.03***
	(0.01)	(0.01)	(0.01)	(0.01)
Father FEs	X	X	X	X
Identifying observations	18,407	16,058	8,532	7,537
Observations	1,571,362	1,454,557	950,687	886,907
Adjusted R^2	0.68	0.67	0.37	0.41
Panel B: Controlling for mother				
Father signed	0.02***	0.03***	0.03*	0.04**
	(0.01)	(0.01)	(0.02)	(0.02)
Mother FEs	X	X	X	X
Identifying observations	6,488	5,516	2,906	2,385
Observations	1,563,894	1,447,566	946,275	882,625
Adjusted R^2	0.69	0.68	0.37	0.41

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. The sample excludes individuals with one or more unknown parents. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. Fixed effects for decade, marriage number, and sibling order are included in every specification as control variables. Identifying observations are the number of observations with a parent with one spouse who signed and one who did not.

3.6.2 Directly comparing half siblings

One downside of the father fixed effects approach is that it relies on observing a measure of the ability of the mother. As shown in Table 3.3, the effective sample size is quite low: very few parents had two spouses, one of which was literate and one of which was not. Hence, not all results are significant at the 5% level.

Fortunately, I can conduct another test using parents with more than one marriage that only relies on the characteristics of the children. Consider a pair of children who could be either half siblings or full siblings. If they share both a mother and a father and the abilities of mothers matter directly, their outcomes should be more correlated than if they share only a father. Again, there is a concern that the event that resulted in a second marriage could have harmed the children of the first marriage. Again, assuming the penalty is a constant, fixed effects can control for it.

I estimate the regression:

$$y_{i,m_i,f} = \alpha Y_{j,m_j,f} \times I(m_i = m_j) + \beta \mathbf{X_{i,j}} + \epsilon_f \tag{3.14}$$

Where $y_{i,m_i,f}$ is a characteristic of child i with father f and mother m_i, i is less than j, $I(m_i = m_j)$ is an indicator that is one if the children share a mother, $\mathbf{X_{i,j}}$ are control variables, and ϵ_f is an error term. The controls include fixed effects for decade, the order of the siblings, and the marriage number of the father.

The results are shown in Table 3.4 below. Full siblings are more strongly associated than half siblings. Moreover, the results are very similar regardless of I let the mothers or fathers vary. Appendix A4 discusses the robustness of these estimates.

Table 3.4: The effect of parental human capital on half vrs. full siblings

	Dependent variable: Younger sibling's characteristic			
	Signed Daughter	Signed Son	Log imp. earnings Daughter's husband	Log imp. earnings Son
	(1)	(2)	(3)	(4)
Panel A: Controlling for father				
Older sibling's characteristic	0.36***	0.36***	0.22***	0.26***
	(0.00)	(0.00)	(0.01)	(0.01)
Signed × same mother	0.06***	0.05***	0.05***	0.05***
	(0.00)	(0.00)	(0.01)	(0.01)
Observations	2,050,264	1,853,707	839,388	756,645
Adjusted R^2	0.64	0.63	0.11	0.14
Panel B: Controlling for mother				
Older sibling's characteristic	0.36***	0.33***	0.24***	0.22***
	(0.01)	(0.01)	(0.02)	(0.02)
Signed × same father	0.07***	0.08***	0.03*	0.09***
	(0.01)	(0.01)	(0.02)	(0.02)
Observations	1,965,701	1,777,710	806,656	727,123
Adjusted R^2	0.64	0.63	0.11	0.14

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. The sample excludes individuals with one or more unknown parents. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. Fixed effects for decade, marriage number, and sibling order as well as the non-interacted same mother indicator variable are included in every specification as control variables.

3.7 Discussion

3.7.1 Assortment matters for intergenerational elasticities

If women directly matter for the outcomes of their children and marriages are assortative, the correlation between characteristics of fathers and sons will be partially determined by the mother. In the simple model in Section 3.4.1, the association between fathers and sons is:

$$y_c = (\beta_m + \gamma\beta_f)y_f + \epsilon_c \tag{3.15}$$

Note that $\beta_m + \gamma\beta_f$ is often the correlation of interest, as it shows how strongly associated sons are with their fathers. However, it should not be interpreted as the direct effect of the father. If the parents matched on individual characteristics, the mother increases the association through the $\gamma\beta_f$ term. Changes in the observed rates of intergenerational mobility, even if women are omitted, could be driven by changes in marriage matching (γ) or in how strongly mothers influence their children (β_f).

Table 3.5 shows the intergenerational elasticity of imputed earnings estimated separately for more and less assorted parents. The less assorted parents are those where only one parents was literate and the more assorted parents are those where both parents were either literate or illiterate. The elasticities for the less assorted parents are 0.30 for sons and 0.28 for daughters (using their husbands' imputed earnings as a proxy). For the more assorted parents, the elasticities are 0.42 for sons and 0.41 for daughters. As predicted, the more strongly assorted parents have higher estimate rates of intergenerational mobility. Appendix A5 considers the possibility of endogeneity bias in these estimates.

Table 3.5: Father-son intergenerational elasticities, more and less assorted marriages

	Dependent variable:			
	Son's log earnings score		Daughter's husband's log earnings score	
	(1)	(2)	(3)	(4)
Father's log earnings score	0.30***	0.42***	0.28***	0.41***
	(0.01)	(0.00)	(0.01)	(0.00)
Parents differ on signature	X		X	
Parents same on signature		X		X
Decade FE	X	X	X	X
Observations	27,278	125,094	30,022	129,928
Adjusted R^2	0.06	0.11	0.05	0.12

Note: *p<0.10; **p<0.05; ***p<0.01. Standard errors in parentheses. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. Decade fixed effects are included in every specification as control variables.

3.7.2 Assortment matters for multigenerational mobility

Although they are less common due to data constraints, several studies also consider correlations across more than two generations (Clark 2014, Olivetti et al. 2018, Solon 2018, Long and Ferrie 2018). I am able to estimate multigenerational mobility with the Quebec data, as shown in Table 3.6 below.

Note that when estimated separately, the intergenerational elasticities between grandfathers and grandchildren seem to be the same regardless of if the grandfathers are maternal or paternal. When, however, the partial elasticities are estimated controlling for the log imputed earnings of the other grandfather and the father, there is a larger coefficient for the maternal grandfathers.

Table 3.6: Grandfather-grandson intergenerational elasticities

	Dependent variable: Child's status measure		
	(1)	(2)	(3)
Panel A: Log imp. earnings (male)			
Maternal grandfather's log imp. earnings	0.30***		0.10***
	(0.00)		(0.00)
Paternal grandfather's log imp. earnings		0.30***	0.13***
		(0.00)	(0.00)
Father's log imp. earnings			0.36***
			(0.00)
Observations	214,856	214,856	214,856
Adjusted R^2	0.04	0.04	0.13
Panel B: Husband's log imp. earnings (female)			
Maternal grandfather's log imp. earnings	0.27***		0.10***
	(0.00)		(0.00)
Paternal grandfather's log imp. earnings		0.28***	0.14***
		(0.00)	(0.00)
Father's log imp. earnings			0.31***
			(0.00)
Observations	219,646	219,646	219,646
Adjusted R^2	0.03	0.04	0.11

Note: *p<0.10; **p<0.05; ***p<0.01. Standard errors in parentheses. Imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. Decade fixed effects are included in every specification as control variables.

Should we interpret this as maternal grandfathers being more important to the outcomes of grandchildren? The answer is no. To illustrate why, refer back to the model in Section 3.4.1. If it is directly related to the mother's true status, a grandfather's observed status will have a coefficient biased upwards as the mother is omitted. Likewise, if it is directly related to the father's true status, it will have a coefficient biased upwards if the father is omitted. Controlling for the father's observed status will reduce the omitted variable bias from the true status of the father much more than it would reduce the omitted variable bias from the mother. In this framework, we'd expect the maternal grandfather to be more strongly correlated with the mother. We would therefore expect it to have a larger coefficient after controlling for the father, which is in fact what we observe in Table 3.6.

This exercise demonstrates how caution must be taken in interpreting intergenerational correlations without accounting for the role of women. It would, at first glance, seem plausible to have found evidence that maternal grandfathers mattered more for the outcomes of children than paternal grandfathers. However, it might merely be an artifact of measurement error.

3.8 Conclusion

In this paper, I construct a simple model of marriage and mobility. It shows that even with no female employment, assortment will increase inequality if the ability of a woman determines whom she marries and the success of her children. To test if this was true in Quebec 1800–1970, I consider millions of families reconstructed from marriage records in the new IMPQ database. Unusually, married women are linked to their fathers; I use this to estimate assortment, finding it surprisingly high and stable over time. Next, I find pairs of sisters where only one was able to write. Even though she likely never earned a wage after she married, I show that the the more educated sister still typically earned a premium when it came to the status of her husband. Moreover, her ability mattered as much as her husband's for the outcomes of their children. As quick remarriage after losing a spouse was the norm, when comparing child outcomes I can hold one parent constant and allow the second to vary. Sharing a mother mattered almost the same as sharing a father. Altogether, I conclude that assortment had always mattered. It mattered because, despite severe legal and economic disadvantages, women played a major role in mobility and marriage. Overlooking women would leave any story of assortative marriage, intergenerational mobility, or inequality incomplete.

Appendix

A1 Modeling inequality

The two main findings of this paper — that assortment was on individual characteristics and that the ability of mothers mattered for child outcomes — individually answer somewhat narrow questions. Together, they imply that a high degree of assortment would have contributed to inequality over the long-run. Here, I illustrate the logic behind the claim with the simple model I use to estimate the degree of assortment in Section 3.4.1.

To summarize inequality in a given generation, consider the variance of potential status:

$$\sigma^2_{x_c} = \beta_f^2 \sigma^2_{x_f} + \beta_m^2 \sigma^2_{x_m} + 2\beta_f \beta_m (\gamma \sigma_{x_f} \sigma_{x_m}) + \sigma^2_{e_c} \tag{3.16}$$

Now define a steady-state equilibrium as when there is no change in inequality from generation to generation:

$$\sigma^2_{x_c} = \sigma^2_{x_f} = \sigma^2_{x_m} \tag{3.17}$$

Then:

$$\sigma^2_{x_c} = \frac{\sigma^2_{e_c}}{1 - \beta_f^2 - \beta_m^2 - 2\gamma \beta_f \beta_m} \tag{3.18}$$

As the error term u_c is assumed to be independent of x_c, the observed inequality is given by:

$$\sigma^2_{y_c} = \sigma^2_{x_c} + \sigma^2_{u_c} \tag{3.19}$$

Unsurprisingly, the more children take after their parents (i.e. the higher the β_f and β_m), the higher the level of steady state inequality. Further, if both β_f and β_m are greater than zero, the degree of assortment γ will increase steady state inequality as well (Figure 3.4).

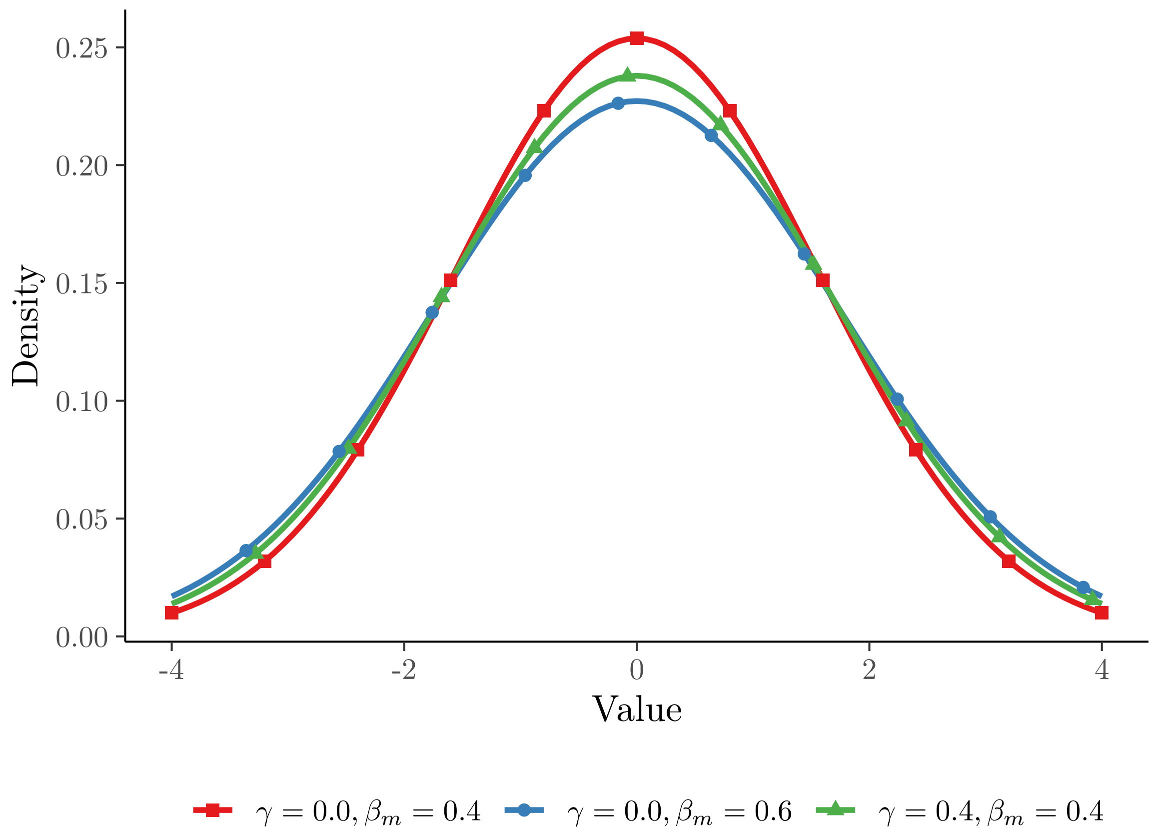


Figure 3.4: Steady state inequality
Note: Simulated data based on model (see text). γ is the degree of assortment, β_f and β_m are the strength of intergenerational inheritance of potential status from fathers and mothers respectively. I assign e_c (the random component of intergenerational mobility) and u_c (the classical measurement error term) a variance of one in all simulations. As shown by the simulations, increasing γ or β_m increases inequality.

A2 Robustness of estimates of assortment

Figure 3.5 below computes the ratio measure of assortment used in Figure 3.3 using a number of different ways to measure human capital. With the exception of literacy, all the measures give roughly the same picture of the overall trend. It is not surprising that literacy becomes less informative about socioeconomic status in the late 19th century, as an increasingly small snare of the population is illiterate.

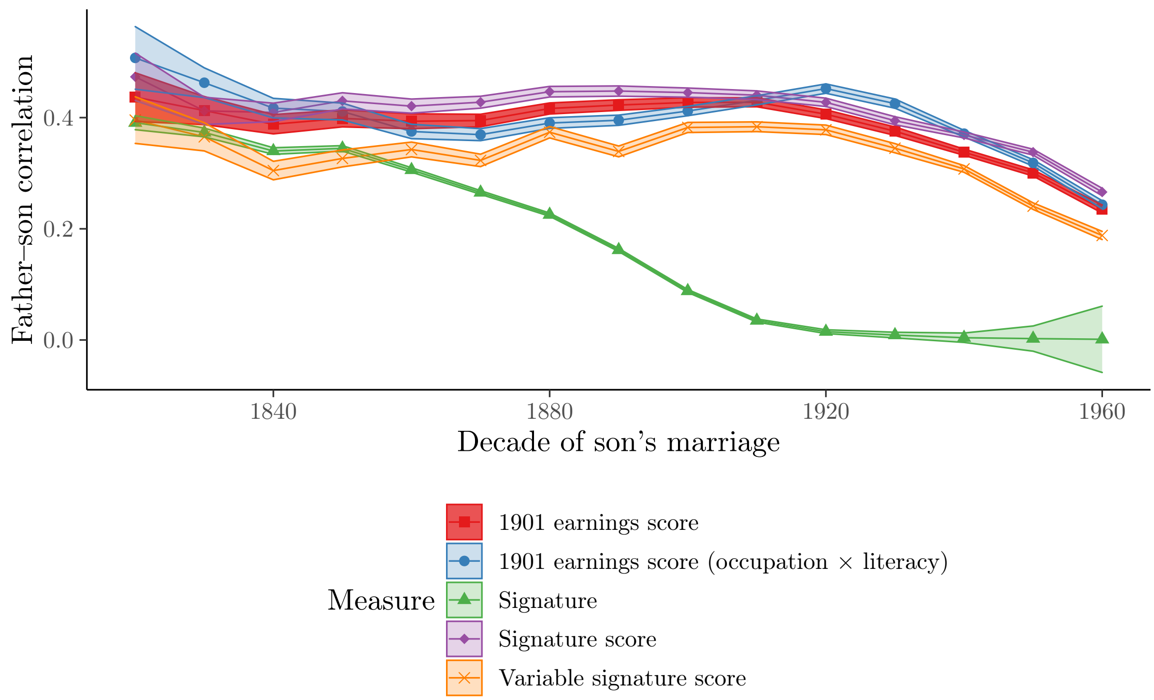


Figure 3.5: Alternative measures of economic status
Note: 95% confidence intervals shaded. The estimates are ratios of rank-rank regression coefficients, which are equal to the correlation coefficients for the ranked variables assuming no ties in rank (and a reasonable approximation if not) (Chetty et al. 2014). 1901 imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. 1901 imputed earings with signature are the average earnings for a man with that occupation and literacy status. OCCSCORE is the IPUMS imputed earnings which is based on 1950 US Census earnings (Minnesota Population Center 2019). Literate is an indicator variable that is one an individual signed their first marriage certificate and zero if they did not. Occupational literacy scores are the percent of men with that occupation in the 1890s in the vital records who could sign their name. Variable occupational literacy scores are computed for each decade using the method in Song et al. (2020): for each occupational category and decade, the score is the sum of the percentile rank of each educational group (signed and not signed) weighted by the share of the occupation in that category. This is essentially a rescaled version of the average signature rate in that group that accounts for the varying rate of signatures over time. Note that while the levels differ, all of the measures display similar trends over time except for literacy alone; this is likely because the literacy rate is rising substantially over time and therefore it becomes less informative about socioeconomic status.

Figure 3.6 below computes the ratio measure used in Figure 3.3 but first estimates both correlations using an instrumental variable regression. The instrument used is the imputed earnings for a second occupation recorded closest to the individual's first marriage. This use of IV regression is a standard approach when there is classical measurement error, and it requires no assumption about the relative magnitudes of attenuation bias on the numerator and denominator. The resulting measure is very similar.

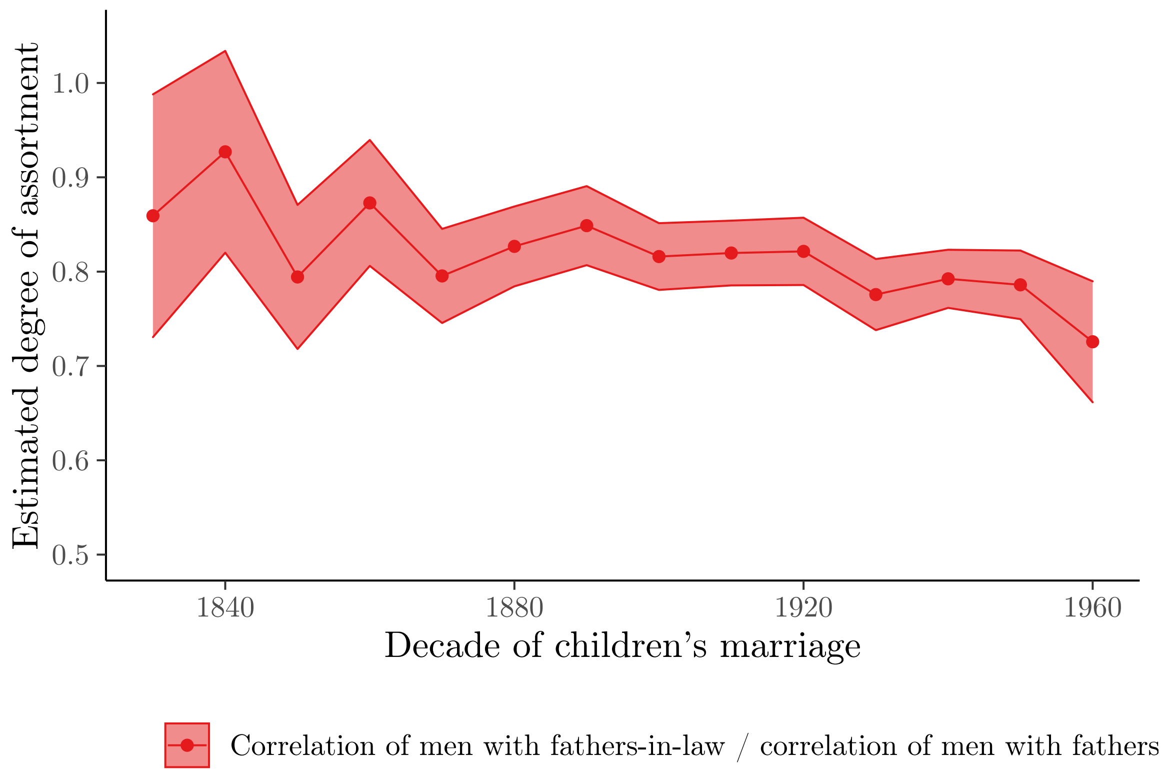


Figure 3.6: Estimated degree of marital assortment, IV
Note: 95% confidence intervals shaded. Imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. The estimates are ratios of rank-rank regression coefficients, which are equal to the correlation coefficients for the ranked variables assuming no ties in rank (and a reasonable approximation if not) (Chetty et al. 2014). To reduce attenuation bias, the dependent variable in the regressions is instrumented using a second measure of imputed earnings (using a second occupation recorded closest to the individual's first marriage).

Figure 3.7 estimates only the correlation between fathers and fathers-in-law using the same IV strategy mentioned above. This measure is more typically used in the literature. However, it is only equivalent to the correlation between wives and husbands if the husband is matching to his father-in-law instead of to his spouse. As I demonstrate in Table 3.1, husbands and wives are at least partially assorted on ability. If matching was between a husband and some weighted average of their wife and father-in-law, the true degree of assortment would be between this measure and the ratio measure.

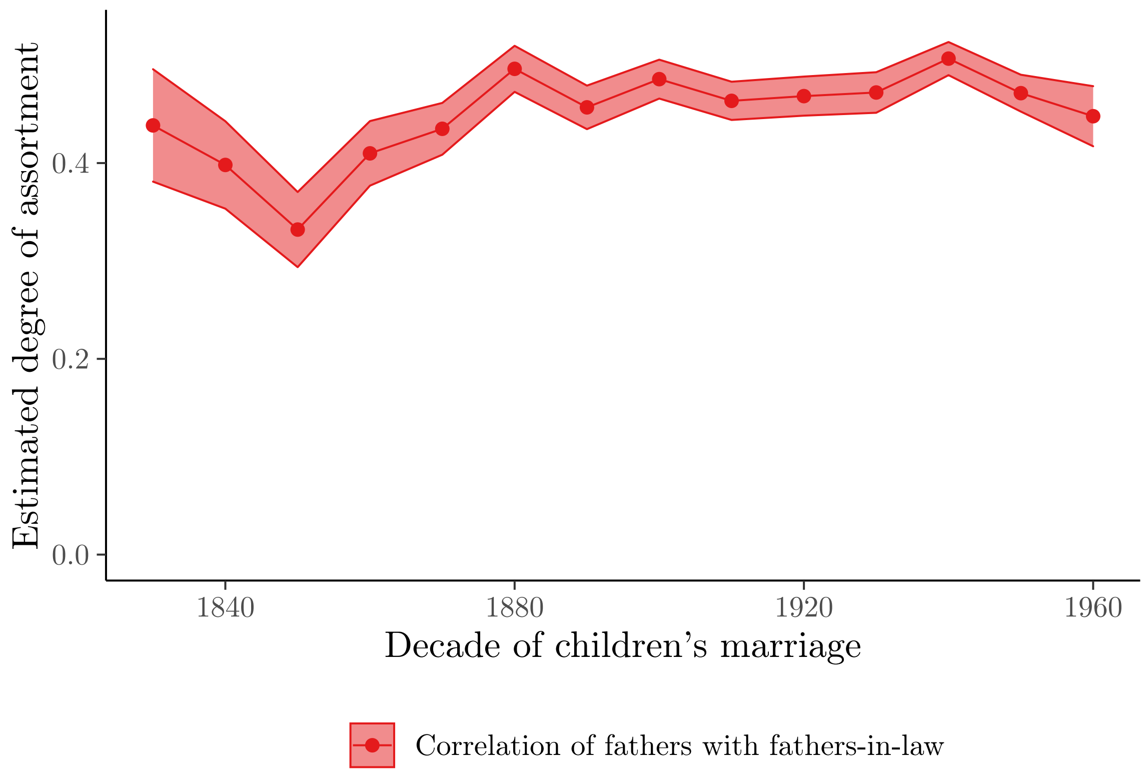


Figure 3.7: Father-father-in-law correlation, IV
Note: 95% confidence intervals shaded. Imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. The estimates are rank-rank regression coefficients, which are equal to the correlation coefficients for the ranked variables assuming no ties in rank (and a reasonable approximation if not) (Chetty et al. 2014). To reduce attenuation bias, the dependent variable in the regressions is instrumented using a second measure of imputed earnings (using a second occupation recorded closest to the individual's first marriage).

However, I can also directly test if the matching is between husbands and fathers-and-laws. For fathers-in-law who die before 1849, are the sons-in-law who married before the death different than those married after? As shown in Table 3.7, there appears to be no difference. In other words, if husbands are matching to their fathers-in-law, they don't seem to mind if their father-in-law is deceased before their marriage.

Table 3.7: Marriage matching appears not to have been between husbands and fathers-in-law

	Dependent variable:	
	Signed Son-in-law	Log imp. earnings Son-in-law
	(1)	(2)
Married after father-in-law's death	−0.01	0.00
	(0.01)	(0.00)
Family FEs	X	X
Observations	83,988	147,641
Adjusted R^2	0.37	0.70

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. Fixed effects for decade, marriage number, and sibling order are included in every specification as control variables. Deaths are only observed before 1849 and only sons-in-law of men who died before 1849 are included.

A3 Robustness of assortment on individual human capital

The estimates in Table 3.1 Columns 2 and 4 are identified using family fixed effects. This means that only families where one child signed and one child did not sign are driving the estimation results. The estimated coefficients are an average treatment effect of individuals in these treated families being able to sign. However, it is possible that these families have unusual characteristics. A more interesting average treatment effect is, perhaps, that for the entire population.

One method of estimating this population-wide effect is to estimate the effect separately for each treated family and use a weighted average of the effects (Miller et al. 2019). The weights are inverse propensity scores, estimated from a logistic regression of an indicator for being treated regressed on observed family characteristics using the entire sample and normalized to sum to one. For this to be a true average treatment effect, the method does come at the cost of several fairly strict assumptions.[15] However, regardless of the assumptions, it is still a useful exercise to see if the estimates are robust to reweighting.

Here, I estimate the propensity scores using indicator variables for the parent's signatures, the mother's decade of first marriage, the mother's borough of first marriage, the denomination of the parish where the mother first got married, and the number of married children in the family. Missing values are included as an additional category for each indicator variable. As shown in Table 3.8 below, there is still a positive and significant marriage premium for literacy.

[15]The assumptions: 1. There is no selection into treatment within groups. 2. Conditional on observables, there is no selection into treatment between groups based on heterogenous effects. 3. The logistic regression is the correct functional form. 4. There is a non-zero probability of treatment for every value of observable.

Table 3.8: Marriage selection, reweighting for selection into identification

	Dependent variable: Husband's characteristic					
	Signed		Log imputed earnings		Father's log imputed earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of wife's human capital						
Wife signed	0.31***	0.20***	0.02***	0.03***	0.04***	0.05***
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Re-weighted		X		X		X
Wife's family FE	X	X	X	X	X	X
Decade FE	X	X	X	X		X
Observations	1,937,871	1,937,871	971,173	971,173	1,148,769	1,148,769
Panel B: Effect of husband's human capital						
Wife signed	0.28***	0.35***			0.03***	0.11***
	(0.00)	(0.01)			(0.00)	(0.00)
Re-weighted		X				X
Husband's family FE	X	X			X	X
Decade FE	X	X			X	X
Observations	1,928,239	1,928,239			982,166	982,166

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. The sample excludes individuals with one or more unknown parents. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. Fixed effects for decade, marriage number, and sibling order are included in every specification as control variables. Re-weighted estimates are constructed by estimating the effect separately for each family and then taking the weighted average of the effects. The weights are inverse propensity score weights constructed by running a logistic regression of an indicator for if a family had at least one child who signed and one who did not on indicator variables for the parent's signatures, the mother's decade of first marriage, the mother's borough of first marriage, the decade, and the number of married children in each family. Missing values are included as an additional category for each indicator variable in the logistic regression.

A4 Robustness of estimates of effects of parental human capital

The identifying parallel trends assumption for the analysis in Table 3.3 is that the human capital of children of parents who remarry did not change over time faster than those of parents who do not remarry. Table 3.9 replicates the analysis except it drops children of parents who remarry if they were more than one sibling away from a half-sibling in the order of siblings.[16] This is analogous to restricting the sample to children born on either side of

[16] As elsewhere, I order siblings by date of first marriage as I do not have birth dates for the entire sample.

the remarriage which should limit the importance of differential time trends. The results are very similar.

Table 3.9: Effect of parents, fixed effects with window

	Dependent variable:			
	Signed Daughter	Signed Son	Log imp. earnings Daughter's husband	Log imp. earnings Son
	(1)	(2)	(3)	(4)
Panel A: Controlling for father				
Mother signed	0.02***	0.03***	0.01	0.03**
	(0.01)	(0.01)	(0.01)	(0.01)
Father FEs	X	X	X	X
Identifying observations	18,303	16,130	8,440	7,438
Observations	1,559,975	1,445,605	944,017	881,588
Adjusted R^2	0.68	0.67	0.37	0.41
Panel B: Controlling for mother				
Father signed	0.02***	0.03***	0.03*	0.04**
	(0.01)	(0.01)	(0.02)	(0.02)
Mother FEs	X	X	X	X
Identifying observations	6,486	5,500	2,799	2,289
Observations	1,522,830	1,411,662	921,530	860,897
Adjusted R^2	0.69	0.68	0.37	0.41

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. The sample excludes individuals with one or more unknown parents. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. Fixed effects for decade, marriage number, and sibling order are included in every specification as control variables. Children of parents who remarry are only included if they were one away from a half-sibling in the order of siblings.

Table 3.10 replicates Table 3.4 except it drops children of parents who remarry if they were more than two siblings away from that a half-sibling in the order of siblings. The logic is the same as above, except I widen the band as the regression compares siblings to full siblings. Again, the results are very similar.

Table 3.10: Effects of parents, half siblings with window

	Dependent variable: Younger sibling's characteristic			
	Signed Daughter	Signed Son	Log imp. earnings Daughter's husband	Log imp. earnings Son
	(1)	(2)	(3)	(4)
Panel A: Controlling for father				
Older sibling's characteristic	0.38***	0.36***	0.23***	0.26***
	(0.00)	(0.00)	(0.01)	(0.01)
Signed × same mother	0.05***	0.05***	0.04***	0.05***
	(0.00)	(0.00)	(0.01)	(0.01)
Observations	1,953,016	1,853,707	800,284	756,645
Adjusted R^2	0.63	0.63	0.11	0.14
Panel B: Controlling for mother				
Older sibling's characteristic	0.36***	0.33***	0.24***	0.22***
	(0.01)	(0.01)	(0.02)	(0.02)
Signed × same father	0.07***	0.08***	0.02	0.09***
	(0.01)	(0.01)	(0.02)	(0.02)
Observations	1,946,719	1,777,710	799,342	727,123
Adjusted R^2	0.64	0.63	0.11	0.14

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. The sample excludes individuals with one or more unknown parents. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Imputed earnings are the average earnings for men with the individual's occupation in Quebec in the 1901 Canadian Census sample. Fixed effects for decade, marriage number, and sibling order as well as the non-interacted same mother indicator variable are included in every specification as control variables. Children of parents who remarry are only included if they were in a window of two or less away from the nearest half-sibling in the order of siblings.

A5 Robustness of effect of assortment on intergenerational elasticity

One concern with Table **??** is that families where only one parent was literate were selected on some omitted factor that decreases intergenerational mobility. One way to overcome this endogeneity is to find a variable that changes the degree of assortment of the parents' marriage and only matters for the outcome of the children through the degree of assortment. A plausible variable that meets these criteria is the fraction of the mother's older siblings who are female. Unfortunately, I do not observe ages in most of the sample, I instead use the percentage of the mother's siblings who got married before her who are female.

The gender of children should be as good as random, at least at birth, especially as there is no evidence of parity-dependent fertility control (Clark et al. 2020). Why should this matter for assortment? One could imagine a scenario where a set of sisters has multiple potential suitors of similar characteristics in their neighborhood or social network. As more of the sisters marry, the remaining sisters will have to be less picky (Dillon 2010,Caron et al. 2017). It is possible that older sisters have a different effect on younger sisters compared to older brothers.[17] However, if it merely changes the human capital of the younger sister, who then matches accordingly, it shouldn't introduce bias.

As shown in Table 3.11, the fraction female decreases the association between the signature rates of spouses and decreases the intergenerational elasticity between fathers and sons. This is exactly what we'd expect if the mother directly mattered for the outcomes of children.

[17]In fact, this is fairly likely. In preliminary research I have conducted for another project, I find that before 1849, the fraction of older siblings that are male increases the rate of infant mortality for younger sisters.

Table 3.11: Father-son intergenerational elasticities, more and less assorted marriages

	Dependent variable:	
	Mother signed	Log earnings score
	(1)	(2)
Fraction female (married before mother)	0.01^{***}	0.13^{***}
	(0.002)	(0.03)
Father signed	0.37^{***}	
	(0.002)	
Father signed X fraction	-0.01^{***}	
	(0.003)	
Father's log earning score		0.41^{***}
		(0.004)
Father's log earning score X fraction		-0.02^{***}
		(0.005)
Sibling marriage order FEs	X	X
Decade FEs	X	X
Observations	379,251	390,145
Adjusted R^2	0.55	0.13

Note: *p<0.10; **p<0.05; ***p<0.01. Family-clustered standard errors in parentheses. Signature variables are indicators that are one if a signature was recorded, zero if the absence of a signature was recorded, and omitted otherwise. Earning scores are the average earnings for the individual's occupation in Quebec based on the 1901 Canadian Census sample. The fraction is the fraction of the mother's siblings who married before her that were female. Decade fixed effects are included in every specification as control variables.

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