Empirical Investigations in Unified Growth Theory

Marc Klemp

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University of Copenhagen
Department of Economics

Contents

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Papers Included in the Thesis

I "The Lasting Damage to Mortality of Early-Life Adversity: Evidence from the English Famine of the Late 1720s," with Jacob Weisdorf.

II "Malthus in the Bedroom: Birth Spacing as a Preventive Check Mechanism in Pre-Modern England," with Francesco Cinnirella and Jacob Weisdorf.

III "Fecundity, Fertility and Family Reconstitution Data: The Child Quantity-Quality Trade-Off Revisited," with Jacob Weisdorf.

IV "Intergenerational Reproductive Trade-Off Faced by Inhabitants of Early Quebec," with Oded Galor.

Published Papers

Part of Thesis

KLEMP, M. AND J. WEISDORF (2012) "The Lasting Damage to Mortality of Early-Life Adversity: Evidence from the English Famine of the Late 1720s," *European Review of Economic History*, 16, 233–246.

Not Part of Thesis

KLEMP, M. (2012): "Prices, Wages, and Fertility in Pre-Industrial England," *Cliometrica*, 6, 63–77.

KLEMP, M., C. MINNS, P. WALLIS, AND J. WEISDORF (forthcoming) "Picking Winners? The Effect of Birth Order and Migration on Parental Human Capital Investments in Pre-Modern England," *European Review of Economic History*.

Abstract

The main topic of this thesis is the interaction between economic development and demographic circumstances in pre-industrial economies. The main objective is to evaluate some of the central assumptions behind Unified Growth Theory. Two papers investigate Malthusian mechanisms, a third paper investigates the child quantity-quality trade-off hypothesis, and a fourth paper investigates the existence of an intergenerational reproductive trade-off. The thesis finds empirical support for the investigated assumptions.

Based on the use of a vast historical database of English church book records, the first paper documents that individuals born during the English famine in 1727–1728 were affected by increased mortality throughout life. This effect was strongest among the poorest families. In the second paper, the data are combined with estimates of income, and it is documented that higher income had a negative causal effect on the intervals between births in England before the demographic transition. The effect appears to arise from deliberate actions. The third paper is based on the same data and shows that children of couples with long intervals from their marriage to their first birth (and thus low fecundity and small families) were more likely to become literate and employed in a skilled profession. The paper thereby documents a negative causal effect of family size on the education of children. The fourth paper is based on a similar and more comprehensive database from pre-industrial Quebec and establishes that individuals of intermediate fecundity (and thus an intermediate number of children) produced more descendants after two or more generations than individuals of high or low fecundity. In the light of the heritability of fecundity, the finding suggests that the forces of natural selection generated an evolutionary advantage for individuals characterized by an intermediate level of fecundity, raising their representation in the population.

Sammendrag (Abstract in Danish)

Denne afhandling omhandler samspillet mellem økonomisk udvikling og demografiske forhold i førindustrielle økonomier. Hovedformålet er at evaluere nogle af antagelserne bag *forenet vækstteori* (engelsk: Unified Growth Theory). To artikler undersøger Malthusianske mekanismer, en tredje artikel undersøger hypotesen om et *trade-off* mellem kvaliteten og kvantiteten af børn, og en fjerde artikel undersøger eksistensen af et intergenerationelt reproduktivt *trade-off*. Afhandlingen finder empirisk belæg for de undersøgte antagelser.

Ved brug af en stor historisk database baseret på engelske kirkebøger, dokumenteres det i den første artikel, at individer født under den engelske hungersnød i 1727–1728 havde en øget mortalitet igennem hele livet. Denne effekt var stærkest blandt de fattigste familier. I den anden artikel kombineres datasættet med estimater for indkomst, og det dokumenteres, at højere indkomst havde en negativ kausal effekt på intervallet imellem fødsler i England før den demografiske transition. Effekten synes at være et resultat af bevidste handlinger. Den tredje artikel er baseret på de samme data og viser, at børn af forældre med lav frugtbarhed (og derfor små familier) havde en øget chance for at opnå skrivefærdighed og erhverv som krævede særlige kvalifikationer. Denne artikel dokumenterer dermed en negativ kausal effekt af forøget familiestørrelse på børnenes uddannelsesniveau. Den fjerde artikel er baseret på en tilsvarende og mere omfattende database fra det førindustrielle Quebec, og den etablerer, at individer med mellemliggende niveau af frugtbarhed (og dermed et mellemliggende antal børn) producerede flere efterkommere efter to eller flere generationer end individer med høj eller lav frugtbarhed. I lyset af arveligheden af frugtbarhed indikerer resultatet, at kræfterne i naturlig selektion skabte en evolutionær fordel for individer med et mellemliggende niveau af frugtbarhed, som dermed forhøjede deres andel af befolkningen.

Introduction

This thesis is concerned with the interaction between economics and population dynamics in pre-industrial economies. The main objective is to evaluate some of the central assumptions behind *Unified Growth Theory*. To enable the uninitiated reader to appreciate that context, I will summarize the main narrative of the baseline theory below.

The four papers have been selected from a larger set of relevant work to focus attention to the problem of drawing causal inference using historical microdata. The papers deal with that problem in different ways by using and developing identification strategies that are generally applicable to historical microdata. It is my hope that other researchers can find inspiration in some of these research designs.

The thesis furthermore shows the viability of examining issues in long-run economic growth using historical microdata. Historical data can help us understand the dynamics of pre-industrial and pre-demographic transition economies over several centuries, and micro-level data enables us to examine the validity of the economic growth theories on the individual level. These sources of evidence still remain largely unexplored in the contemporary economic growth literature.

The findings of the present thesis generally support the investigated central assumptions of Unified Growth Theory.

Theoretical Background

The unprecedented growth in income per capita that the world economy has experienced during the recent two centuries is a recent phenomenon in the history of the human species.[1] While living standards have arguably remained largely unaltered for the majority of human history, world income per capita has doubled multiple times since England first underwent the Industrial Revolution in the late 18$^{\text{th}}$ century. Differences in the extent to which the economies of the world have transitioned from an epoch of stagnation to an era of sustained growth have resulted in large disparities in living standards across countries. Unified Growth Theory seeks to understand the fundamental forces that have led to the transition of the world economy in a comprehensive and coherent framework.

[1] This section draws on Galor (2011).

The interplay between economics and demography is at the center of Unified Growth Theory. According to the theory, the transition from stagnation to growth is causally linked to a substantial decline in fertility. These events are identified historically as the Industrial Revolution, originating in England in the late 18$^{\text{th}}$ and early 19$^{\text{th}}$ centuries, and the Demographic Transition, first experienced by Western European nations towards the end of the 19$^{\text{th}}$ century.

According to the theory, the economy is initially in a *Malthusian* regime, meaning that there is a positive effect of living standards (as measured by income per capita) on the population size and a negative effect of population size on living standards. This means that both quantities stagnate in the absence of technological growth. Furthermore, a technological development will only temporarily raise living standards and will ultimately be translated into a higher population size.

The theory further assumes a positive effect of population size on the rate of technological growth, which is in turn assumed to increase the returns to education. An initial level of technological growth slowly expands the population while living standards inch forward at a miniscule rate, resulting in a rising technological growth rate. Meanwhile, parents prefer, according to the theory, more children to fewer children while they also prefer more productive children to less productive children. Because of this, the rising technological growth rate has two opposing effects. It enables parents to produce more (surviving) children while raising their incentive to reallocate their resources to investment in the education of their offspring. As the economy first escapes the Malthusian regime, and the demand for human capital is still limited, the first effect is dominating, and fertility increases. The rising level of education and accelerating technological growth generate a virtuous circle between these two variables. Eventually, the parents' incentive to invest in the quality of their offspring offsets the positive effect of income on fertility and a demographic transition ensues. The dilution of income per capita that is a byproduct of population growth declines, and the economy enters an era of sustained economic growth. Finally, the economy converges toward a steady state with zero population growth and a constant positive growth rate of income per capita.

Overview of Papers

This thesis is comprised of four self-contained research papers that empirically investigate some of the assumptions of Unified Growth Theory. The analyses employ extensive historical databases with several hundred thousand observations on the individual level. The data originate from church books in England and North America (Quebec) and contain precise information on location, the dates of births, marriages, and deaths, as well as occasional information on literacy, occupation, and more.

Papers **I** and **II** deal with the Malthusian assumption of a positive effect of living standards on the size of the population in pre-demographic transition societies. As recognized by Malthus (1798), living standards can potentially affect population growth through both mortality and fertility, and the two papers deals with both channels respectively.

In paper **I** we estimate the effect of being exposed to a famine *in utero* and as an infant. We show that individuals exposed to the famine in England in 1727–1728, arguably an exogenous event, were affected by increased mortality throughout life. This means that famines and the associated spread of disease can have long-lasting effects on mortality of surviving individuals exposed early in life, consistent with the epidemiological *fetal origins* hypothesis. We show that the effect is strongest among the poorest families, and in the English Midlands. These findings illustrate the existence of a delayed reaction of mortality to changes in living standards, consistent with the Malthusian hypothesis.

In paper **II** we estimate the effect of income on various events related to reproduction with a focus on the duration between births in pre-demographic transition England. The main finding of the paper is that higher wages had a negative causal effect on the duration between births. This effect was present among both rich and poor individuals, indicating that the effect was, at least partly, a product of deliberate actions. These findings contrasts with studies that claim that England was historically a *natural fertility* society that did not practice birth control beyond adjustment of the marriage age. The analysis indicates that the population of pre-industrial England responded to advancements in living standards in a way that was, *ceteris paribus*, conducive to population growth, consistent with the Malthusian hypothesis.

Papers **III** and **IV** deal with the existence and implications of a historical child quantity-quality trade-off.

In paper **III** we estimate the causal effect of family size on the human capital of the offspring. To avoid omitted-variable bias, we instrument family size with a proxy of parental fecundity, the duration between the marriage and the first live birth. We show that one extra child reduced the proportion of literate and skilled children by respectively 6.7 and 7.5 percentage points. This finding supports the notion that households operated on their budget constraint and is consistent with the hypothesis of a historical child quantity-quality trade-off.

In paper **IV** we present the first comprehensive evidence for the presence of an inter-generational trade-off in reproductive success within the human species. According to our analysis, parental fecundity, and hence the reproductive success of the initial generation, had a hump-shaped effect on reproductive success in the long run. Thus, in light of the established heritability of fecundity (also verified in the paper), the finding suggests that the forces of natural selection generated an evolutionary advantage for individuals characterized by an intermediate level of fecundity, raising the representation of individuals with genetic pre-disposition towards a quality strategy in the population.

Discussion and Conclusion

The present analyses open up for many avenues of further research. For example, in relation to paper **I**, one could investigate if the exposure to famine affected the formation of human capital. Furthermore, it could be interesting to examine if and how the effect presented in the paper generalize and investigate if early-life living standards in non-famine periods also affect children's later-life outcomes. It could also be interesting to examine if the effect on birth intervals found in paper **II** translate into an effect on lifetime fertility. Further research related to paper **III** could investigate the possible non-linearity of the effect of family size on human capital. Furthermore, it could be enlightening to investigate the quantitative implications of the results presented in paper **IV** for economic growth.

The analyses performed in this thesis do not reject the validity of the probed assumptions, and the four papers taken together find evidence in favor of Unified Growth Theory. Thus, the prospects for Unified Growth Theory as an encompassing and stringent theory of long-run economic growth that is consistent with observed regularities appear promising.

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The lasting damage to mortality of early-life adversity: evidence from the English famine of the late 1720s

MARC KLEMP* and JACOB WEISDORF**
**University of Copenhagen, marc.klemp@econ.ku.dk*
***Department of Business and Economics, University of Southern Denmark, Campusuej 55, DK-5230 Odense M*

This paper explores the long-term impact on mortality of exposure to hardship in early-life. Using survival analysis, we demonstrate that birth during the great English famine of the late 1720s entailed an increased death risk *throughout* life among those who survived the famine years. Using demographic data from the Cambridge Group's *Population History of England*, we find the death risk at age 10 among the most exposed group—children born to English Midlands families of a lower socioeconomic rank—is up to 66 percent higher than that of the control group (children of similar background born in the 5 years following the famine). This corresponds to a loss of life expectancy of more than 12 years. However, evidence does not suggest that children born in the 5 years *prior to* the famine suffered increased death risk.

1. Introduction

The existence of Malthusian "positive checks" in pre-industrial England has been the subject of considerable scholarly interest in recent years.[1] Although the magnitude of the *short-term* effects of hardship on mortality has heavily been debated, no attention has been paid to the *long-term* effects: the influence of hardship on mortality later in life. The relevance of long-term effects has been highlighted by scholars both of medicine and of demography, who hold that exposure to adverse conditions, such as famine and plague, in early life has an impact on the subsequent mortality risk of the population by two opposing effects: a "selection" effect whereby hardship kills off the weak, leaving only the strong (and thus potentially longer lived) individuals to survive; and a "scarring" effect where survivors suffer lasting damage to their vital organs and immune systems and, hence, incur increased death risk throughout life.[2]

In this paper, we use survival analysis to test the so-called "fetal origins hypothesis" which holds that under-nutrition in early life leads to a disproportionate growth *in utero* and in infancy, which in turn enhances the susceptibility to illness and hence increases the death risk later in life (Barker 1995). We have focused on the English famine of the late 1720s, the most severe of the eighteenth century. The data are taken from the Cambridge Group's *Population History of England from Family Reconstitution*, which are documented in Wrigley *et al.* (1997). An important advantage of these data is that individuals can be followed

[1] See Nicolini (2007), Kelly and Ó Gráda (2010), Rathke and Sarferaz (2010) and Møller and Sharp (2008).
[2] See Barker (1998), Bozzoli *et al.* (2009), Doblhammer (2004) and Hatton (2011).

throughout life, allowing us to compute and compare the death risks and life expectancies of those cohorts born during the crisis years with cohorts born in adjacent years.

Using the Kaplan–Meier estimator of survival curves and the Cox proportional hazard model to conduct the analysis, we have looked at cohorts born during each of the probable famine years of 1727–1730, as well as those born during the 5 years both immediately preceding and immediately following the famine. The estimates offer ample evidence that a "scarring" effect outweighed a potential "selection" effect in 1727–1728, the 2 years identified as crisis years, lending strong support to the "fetal origins hypothesis." More specifically, we find that children of families from the English Midlands born during 1727–1728 suffer an increased death risk and a lower life expectancy *throughout* life compared with those of the control group (Midland-family children born in the 5 years following the famine). The death risk at age 10 among the most exposed group—children born to Midlands families of a lower socioeconomic rank—is up to 66 percent higher, and the life-expectancy up to 12 years lower, than those in the control group.[3] The effect of early-life under-nutrition is also long-lasting: even at age 30, children born to Midlands families of a lower socioeconomic rank face a greatly amplified death risk of up to 71 percent, and a life-span as much as 10 years shorter than their control-group counterparts. There is no evidence in the data, however, that individuals born in the 5 years preceding the famine suffer a significantly increased death risk later in life.

The remainder of the paper explains how the results were derived. First, we explain in more detail the "fetal origins hypothesis", and we describe the English famine of the late 1720s as it is reported in the existing literature. Next, we offer a more exact account of the data, the methodology used and the methodological issues that we encountered. We then report, specify and discuss the results, before presenting our conclusions.

2. Background

As has been documented in detail by Barker (1995, 1998) and Doblhammer (2004), individuals who are subject to under-nourishment in the very early stages of life are more likely to be diagnosed with a wide range of illnesses later in life, such as coronary heart disease, stroke, diabetes, chronic bronchitis etc.[4] The underlying view is that the damage inflicted during early childhood lies dormant until adulthood, or even old age, and is not clinically measureable before that point in time.

The mechanism by which disease experienced early in life affects the waiting time to the onset of illness is still unclear, but scholars seem to agree that exposure to undernourishment during periods when cell growth is particularly rapid—especially *in utero* and during infancy—can lead to long-lasting impairments of the vital organs. Barker (1995, 1998)

[3] As will become apparent below, the term "death risk" here refers to the logarithm of the proportional hazard of a cohort relative to the control cohort. A death risk of 0.1 for a given cohort means that the individuals in that cohort have a roughly 10 percent higher risk of dying at any given age when compared with the individuals in the control cohort.

[4] Not all studies, however, are able to detect such effects. Kannisto *et al* (1997), who have studied cohorts born in Finland during the severe famine of 1866–1868, and Stanner *et al.* (1997), who looked at cohorts born during the siege of Leningrad 1941–1944, find little support for the "fetal origins hypothesis." Stanner *et al.* suggest that one reason for the absence of such effects is that malnutrition is necessary for prolonged periods. This conclusion is consistent with our finding that effects are greater among the poorer groups of society, as these are more likely to face hardship during non-famine years.

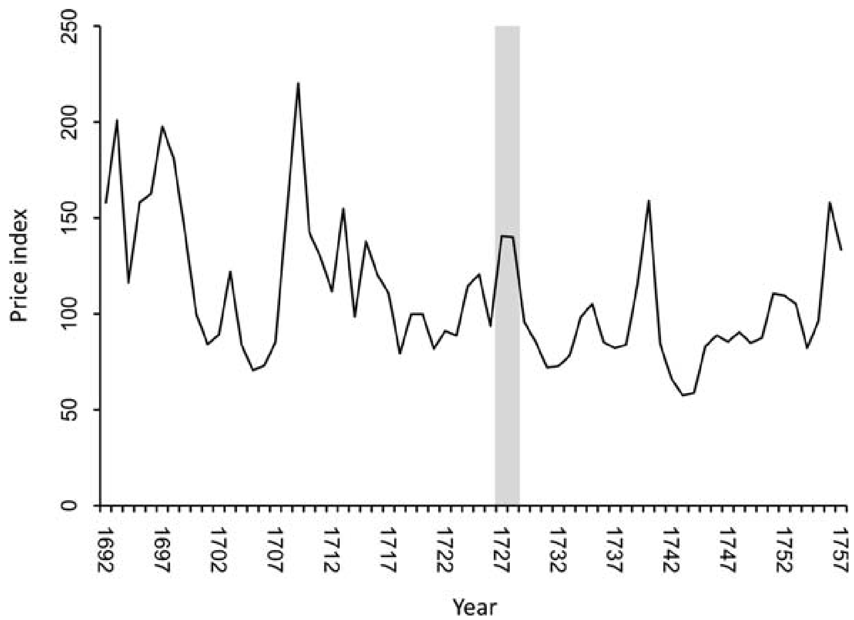

Figure 1. *Real wheat prices in England, 1692–1757 (indexed: 1700 = 100).*

points out that the foetus is dependent on the nutrients from the mother and adapts to an inadequate nutrient supply by prioritization of brain growth at the expense of vital organs such as the heart and lungs. Barker mentions that, although occurring in response to a transient phenomenon, these adaptations become permanent or "programmed", resulting in irreparable and abnormal development of vital organs and immune systems, which in turn cause increased risk of autoimmune diseases and other illnesses later in life.

Periodic food shortage, and hence the risk of under-nutrition, was an unavoidable fact of life for ordinary people in pre-industrial times. Historical England was no exception. As Wrigley and Schofield (1989, 263) put it: "Until well into the nineteenth century no other aspect of economic life was consistently of such great concern to private individuals and to public authorities alike as the scale of the last harvest and the prospects for the next year." In the run up to the Industrial Revolution, England witnessed several incidents of poor harvests, and thus the potential for periodic starvation. Reviewing these incidents, Appleby (1980, 882) concludes that "Of all the bad harvest years of the late seventeenth and early eighteenth centuries, 1727–1728 is the only likely candidate for a subsistence crisis in England."

During 1727–1729 grain prices were demonstrably in excess of their trend. Real wheat prices were 40.9 percent above the 25-year moving average in 1727 and 39.25 percent above the 25-year moving average in 1728. These two years are highlighted in figure 1. It is clear from figure 1 that grain prices in 1727–1728 were nowhere near their levels in the 1690s or in 1709–1710.[5] But, as Appleby (1980, 886) points out, the years 1727–1728 were also the only years between 1692 and 1757 when England's grain imports exceeded its grain exports.

There is reason to believe, however, that increased food prices led to more than simply hunger. Indeed, before the twentieth century, most famine-related mortality was due to

[5] Real wages are nominal prices of wheat deflated by nominal agricultural day-wages from Allen (2001).

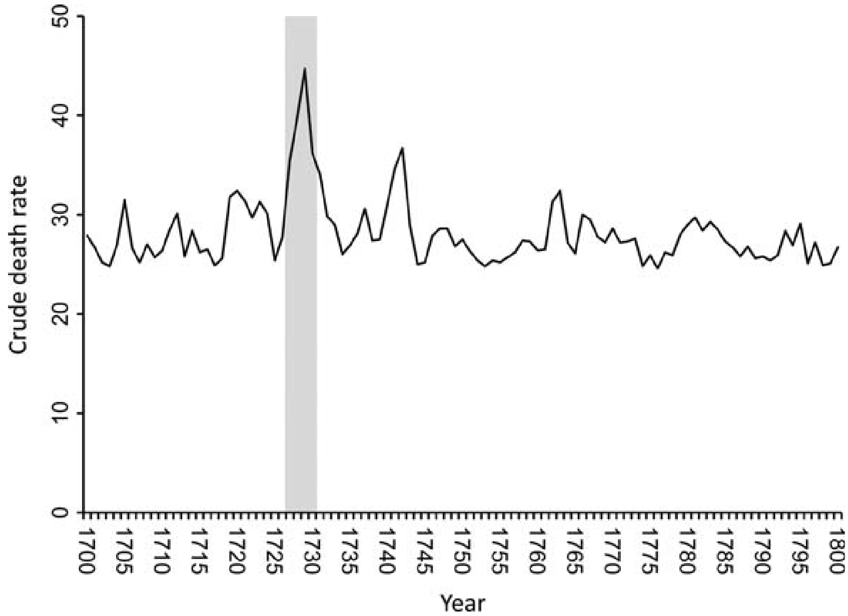

Figure 2. *Crude death rates, England, 1700–1800.*

epidemic disease (Mokyr and Ó Gráda 2002; Ó Gráda 2007). Campbell (2009, 25) asserts that "[t]he heightened grain prices [. . .] lend 1728 the appearance of a classic famine year, except that the death toll was heavier and net loss of population greater than is consistent with the scale of the price inflation and absolute level of real wages." This led him to conclude that the "demographic crisis" of 1727–1730 "looks like a double disaster characterised by dearth and disease operating in tandem" (ibid. 25).

Rather than looking at price levels as an indicator of crisis, Wrigley and Schofield (1989, 332) have sought to classify crisis years by peaks in death rates. Those years where crude death rates were at least 10 percent above the 25-year moving average were categorized as the years of crisis. Crisis years are then further subdivided into three categories: a deviation of more than 10 percent from the trend is denoted by one star; more than 20 percent by two stars; and more than 30 percent by three stars. The median crude death rate in the period examined by Wrigley and Schofield (1989) was 25.9 deaths per thousand. Death rates in the crop years 1727–1728, 1728–1729, and 1729–1730 were 41.8, 43.2, and 42.2, respectively. In all three crop years, there was a deviation of more than 35 percent from the 25-year moving average, placing the crisis of 1727–1728 to 1729–1730 in the most brutal category: a three-star crisis.[6] Figure 2 shows the spike in the crude death rates in the years 1727–1730. Although, as we shall see below, mortality was subject to local variation, the crisis was deemed "national" by Walter and Schofield (1991, 59) in the sense that more than 28 percent of the 404 parishes analysed showed excess rates of mortality.

3. Data and methodology

Below we proceed to analyse the effects on life expectancy for those who were born during the famine of the late 1720s. Life expectancy is the mean longevity of a given population,

[6] A crop year here runs from 1 July to 30 June.

and longevity is the time interval between an individual's birth and death dates. The birth and death dates of individuals, as well as their sex, location, and social background, are taken from the Cambridge Group's *Family Reconstitution* project, documented in Wrigley *et al*. (1997). These data are collected from the parish registers of a total of twenty-six parishes spread across England, selected to be representative of the country as a whole; they comprise the following locations: Aldenham, Alcester, Ash, Austrey, Banbury, Birstall, Bottesford, Bridford, Colyton, Dawlish, Earsdon, Gainsborough, Gedling, Great Oakley, Hartland, Ipplepen, Lowestoft, March, Methley, Morchard Bishop, Odiham, Shepshed, Southill, Reigate, Terling, and Willingham.[7]

In the Cambridge records, it is very often the case that an individual's birth and death dates are missing. As substitutes, demographers normally rely on baptism and burial dates. According to Wrigley and Schofield (1989, 96), the time interval between birth and baptism dates was rarely more than 1 month (typically less than 2 weeks), and the gap becomes smaller the further back in time one moves. Using baptism dates as a proxy for birth dates in the present case, therefore, does not seem to be a significant issue. As with baptisms versus births, burial dates are often reported in the parish registers instead of the date of death. For obvious reasons, a burial normally took place within a few days of death, so any inaccuracy in this respect is unlikely to have a significant impact on the results derived below.[8]

A more serious issue is that the Cambridge data are often "censored" in the sense that death/burial information is missing due to migration out of the parishes examined (Souden 1984). Because the probability of migration increases with longevity, the mean longevity based on observations of birth and death dates in the data provides a downward-biased estimate of the mean longevity of the population. We tackle this issue with survival analysis, using the Kaplan–Meier estimator of survival curves and the Cox proportional hazard model. An important advantage of these methods is that they take into account the kind of censoring that occurs if (as in medicine) a patient withdraws from a study, that is, he is lost from the sample before the final outcome is observed. This is precisely what happens in the current data.

More specifically, we use any information available that a censored individual is still alive at a given point in time, as indicated by the individual's marriage date or the births or deaths of siblings or parents. For example, out of those whose death dates are censored, nearly 20 percent have their date of marriage available. That date then acts as their censoring date. For the remaining individuals whose death dates are censored, we adapt the following procedure: if their youngest sibling is born within 10 years, the birth date of their youngest sibling acts as the censoring date. If the youngest sibling is born *after* 10 years, then 10 years after the individual's birth date acts as the censoring date, on the assumption that the individual did not move away from his/her family (and thus potentially out of the parish observed) before the age of 10. A similar approach is used regarding the death of the individual's mother, father or the youngest sibling that did not survive to the age of 10 (by direct observation). These assumptions make it possible to estimate the survival curves of the various cohorts used in the analysis. Likewise, the hazard ratio between the

[7] The data from the parishes of Aldenham and Earsdon do not include any observations that we have found useful for this analysis.

[8] The proportion of burials in Hawkshed, Lancashire, during the late eighteenth century at different intervals after death were as follows: same day, 1 percent; first day following, 21 percent; second day, 50 percent; third day, 25 percent; fourth day, 2 percent; fifth to seventh day, 1 percent (Schofield 1970).

crisis cohorts and the control group can be calculated using the Cox proportional hazard model.

There is some disagreement within the existing literature as to when exactly the famine occurred. Appleby (1980) believes that the crisis years were 1727–1728; Wrigley and Schofield (1989) identify 1727–1729 as the crisis years; and Campbell (2009) contends that the crisis period covered the years 1727–1730. We therefore begin the analysis by looking at the cohorts born during each of the probable famine years, 1727–1730, and comparing them with those born during the 5 years immediately preceding and the 5 years immediately following the famine.

The data used in the analysis contain a total of 12,640 individuals born in the period 1722–1735. Among them, 53 percent were censored in terms of missing death dates. The individuals were divided into three main groups: those born in the 5-year period *before* the crisis, 1722–1726; those born in one of the four potential crisis years, 1727–1730; and those born in the 5-year period following the crisis, 1731–1735. The latter cohort functions as a control group. The reason for this is that the control cohort is exposed to similar macroeconomic conditions throughout the life as the crisis cohorts, except for the fact that the post-crisis cohort is not exposed to the famine. Taking the "fetal origins hypothesis" as a guide, the *a priori* assumption is that individuals among the crisis cohorts who survive the crisis will have increased death risk, and thus a lower life expectancy, compared with the control group. However, the "fetal origins hypothesis" would also imply that those born *before* the crisis do not incur lasting effects to their mortality, because exposure to under-nourishment does not take place during the periods when the cell growth is particularly rapid, i.e. *in utero* or during infancy.

As concerns geography, Appleby (1980) and Wrigley and Schofield (1989) both mention that the famine struck mostly in the English Midlands, and that the Southwestern and the Northern parts of England were largely unaffected. Accordingly, we subdivide the samples depending on whether the individual is born in a Midland or a non-Midland parish. Parishes situated in the Midlands include exactly half of the twenty-six locations, comprising Alcester, Austrey, Banbury, Bottesford, Gainsborough, Gedling, Great Oakley, Lowestoft, March, Shepshed, Southill, Terling, and Willingham.[9]

Finally, since it is also clear that the control group individuals did not necessarily have the same socioeconomic background as those born during the crisis, we use a two-step procedure to subdivide individuals into two groups, depending on the father's occupation. First, as described in van Leeuwen *et al.* (2007), the *History of Work Information System* (HISCO) provides standardized codes for hundreds of occupational titles existing in England between the sixteenth and the twentieth centuries. Using these codes in conjunction with the HISCLASS system, documented in Van Leeuwen and Maas (2011), we are able to map all occupational titles in the data into one of the two social classes: manual and non-manual labourers. Secondly, by analysing the wealth at death among male testators, Clark and Hamilton (2006) have demonstrated (unsurprisingly) that the wealth of manual labourers was significantly lower than that of their non-manual counterparts. By combining the HISCLASS and the wealth information, we thus obtain a crude proxy for the wealth among those families in

[9] We have experimented with a subdivision of parishes according to the elevation of the location. Subdividing parishes in this way yields largely the same results as subdividing into Midlands and non-Midlands locations. The reason for this is that more than 80 percent of all individuals born in the Midlands are also born into a parish of low elevation.

the data where the husband's occupation is available. Occupational data are available in 5,675 cases, or roughly half of the sample.

4. Results

We begin by presenting the survival curves for the different cohorts. Survival curves capture the probability that an individual will survive beyond a specified age. According to the "fetal origins hypothesis", cohorts subject to under-nutrition *in utero* or infancy will suffer increased death risk throughout the life. In that case, we would expect to see that the survival curves of the famine cohorts lie below the survival curves of the cohorts born in the 5 years immediately following the famine.

Plotted in figure 3A–E are the estimated survival curves for the cohorts born in 1727, 1728, 1729, and 1730, respectively, as well as the pre-crisis cohort, displayed relative to the survival curve of the control group (i.e. those born during the period 1731–1735). The graphs give a clear indication that the cohorts born during 1727 and 1728 suffer an increased death risk throughout life, compared with their control group. On the other hand, the survival curves of those born during the years 1729 and 1730 do not appear to suffer from an increased death risk compared with the control group. Nor do the pre-crisis cohorts seem to differ from the post-crisis cohorts in terms of death risk.

These suppositions are backed up by the log-rank tests for equal survival distributions. The tests confirm that the survival curves of cohorts born in 1727 and 1728 are (excepting one borderline case) significantly different from that of the control group, when we look at each year separately (p-value equal to 0.101 for 1727 and to 0.0348 for 1728). Moreover, the combined crisis cohort of 1727–1728 is significantly different from the control group ($p = 0.014$). However, the survival curves of the 1729 and 1730 cohorts are not statistically different from those of the control group (p-values equal to 0.991 for 1729 and to 0.145 for 1730).

In order to anticipate the possibility of confounding variables, we use the Cox proportional hazard model stratified by sex, birth order, Midlands location, and the father's occupation (a manual/non-manual dummy variable). Information about the father's occupation was available in roughly 45 percent of the cases, so we also include a dummy for unknown occupation in the stratas.

The results of the Cox proportional hazard model are reported in tables 1 and 2. All estimates reported are the logarithms of the hazard ratios. The numbers indicate to what extent the death risk of the pre-crisis and the crisis cohorts, i.e. those born during the period 1722–1726 and during each of the years 1727 to 1730, differ to that of the control group. Positive numbers indicate an increased death risk vis-à-vis the control group; conversely, negative numbers indicate a decreased risk. One, two, and three stars indicate a statistical significance at the 10, 5, and 1 percent level. The numbers in parentheses are the standard deviation.

Column 1 of table 1 shows that all crisis cohorts, with the exception of those born during 1729, have an increased death risk compared with the control group. Only in the case of the 1728 cohort is the death risk *significantly* higher. Furthermore, the subdivision of parishes into Midlands and non-Midlands (Columns 2 and 3) reveals—consistent with the *a priori* assumption inspired by the existing literature—that only the Midland parishes were affected significantly. The subdivision also shows that only the Midlands cohorts of 1727 and 1728 suffered a significantly higher death risk (roughly 30 percent) compared with the control

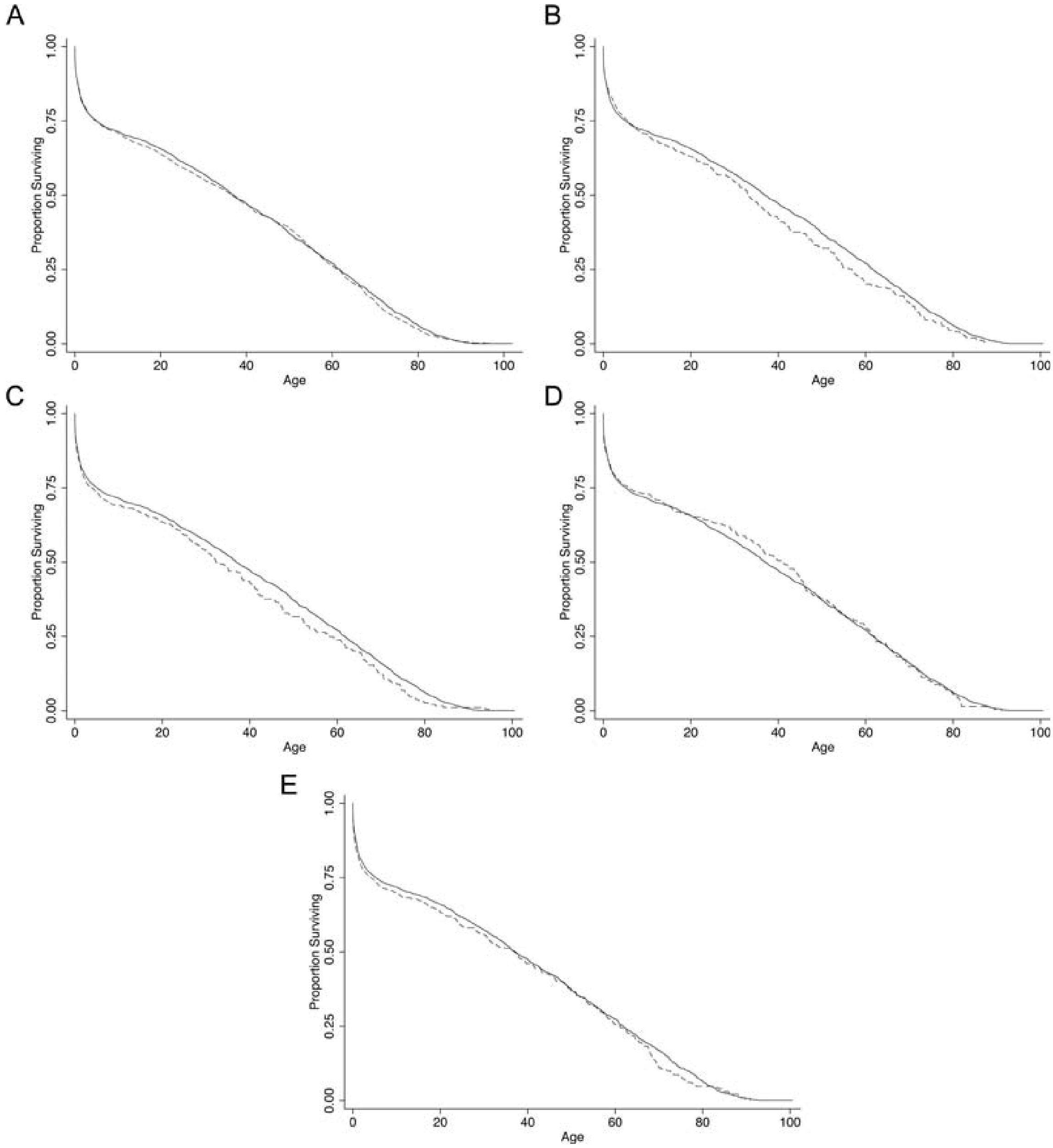

Figure 3. *(A) Survival curves: pre-crisis cohort versus the control group (Solid). (B) Survival curves: 1727-cohort versus the control group (solid line). (C) Survival curves: 1728-cohort versus the control group (solid line). (D) Survival curves: 1729-cohort versus the control group (solid line). (E) Survival curves: 1730-cohort versus the control group (solid line).*

group. The remaining Midlands cohorts were also exposed to increased death risks, but the differences compared with the control group are not statistically significant.

The subsample of individuals for whom father's occupation is available (5,675 cases out of the 12,640 of the full sample) offers a picture that is largely identical to that of the full sample. Column 1 of table 2 shows that the death risk of the cohorts born in 1727 and 1728 is

Table 1. *Death risks: all and by region*

Birth cohorts	(1) All	(2) Non-midlands	(3) Midlands
1722–1726	0.033 (0.037)	−0.018 (0.055)	0.075 (0.050)
1727	0.102 (0.063)	−0.069 (0.078)	0.290*** (0.098)
1728	0.151** (0.064)	−0.026 (0.089)	0.308*** (0.088)
1729	−0.025 (0.062)	−0.125 (0.089)	0.063 (0.089)
1730	0.0666 (−0.056)	−0.018 (0.094)	0.112 (0.083)
N	12,640	6,275	6,365

Robust standard errors clustered by family are used to calculate *p*-values. One, two, and three stars indicate statistical significance at the 10, 5, and 1 percent level.

Table 2. *Death risks: all and by socioeconomic group*

Birth cohorts	(1) All	(2) Non-manual	(3) Manual
1722–1726	0.065 (0.052)	−0.006 (0.099)	0.089 (0.061)
1727	0.254** (0.113)	−0.335 (0.215)	0.458*** (0.110)
1728	0.360*** (0.094)	0.127 (0.180)	0.463*** (0.110)
1729	−0.074 (0.095)	−0.083 (0.165)	−0.084 (0.116)
1730	0.043 (0.089)	0.052 (0.148)	0.039 (0.107)
N	5,675	1,565	4,110

Robust standard errors clustered by family are used to calculate *p*-values. One, two and three stars indicate statistical significance at the 10, 5, and 1 percent level.

significantly higher than that in the control group, and that except for the 1729 cohort, the remaining cohorts suffer slightly elevated, but not significantly higher, death risks relative to the control group. The most striking result emerges when we subdivide individuals according to their father's occupation (manual versus non-manual labour). Column 2 shows that the death risk of the individuals belonging to the non-manual worker families is not significantly higher than that in the control group. Column 3, on the other hand, demonstrates that individuals born in 1727 and 1728 to families of a lower socioeconomic rank (i.e. manual worker families) are hit extremely hard by the famine, with a significantly increased death risk of roughly 45 percent compared with their control group.

Before proceeding any further, it is sensible to test the assumption of proportional hazards underlying the Cox proportional hazard model. We have compared the plots of the scaled Schoenfeld residuals against age by each of the five cohort groups (1722–1726, 1727, 1728, 1729, and 1730) for each of the six subsamples used in tables 1 and 2. None of the plots raised doubts about the validity of the proportional hazard assumption. The null hypothesis of a zero slope cannot be rejected (even at the 10 percent level) in all cases but one. The rejected case is for the 1727 cohort in the non-Midlands parishes ($p = 0.038$) for the full samples used in table 1. The *p*-values of the global tests for non-zero slopes corresponding to the samples used in Columns 1, 2, and 3 of table 1 were 0.95, 0.17, and 0.49, respectively, while the *p*-values corresponding to the samples used in Columns 1, 2, and 3 of table 2 were 0.80, 0.64, and 0.78, respectively.

The results of tables 1 and 2 lead us to conclude that the most severe famine years were 1727–1728, as these are the only years in which the death risks were significantly increased

Table 3. *Death risk at various ages of the 1727–1728 cohort: all and by region*

Age	(1) All	(2) Non-Midlands	(3) Midlands
>0	0.125*** (0.049)	−0.050 (0.064)	0.299*** (0.070)
	1,599	877	722
>10	0.158*** (0.072)	0.092 (0.093)	0.280** (0.111)
	515	349	166
>20	0.150* (0.079)	0.099 (0.102)	0.255** (0.119)
	455	315	140
>30	0.165* (0.085)	0.135 (0.190)	0.230* (0.133)
	260	178	82

Robust standard errors clustered by family are used to calculate *p*-values. Bottom line numbers are number of observations in 1727–1728 cohorts. One, two and three stars indicate statistical significance at the 10, 5, and 1 percent level.

compared with the post-crisis control group. Hence, in the following analysis, we proceed to test the "fetal origins hypothesis" for cohorts born during 1727 and 1728 relative to the control group. For parsimonious reasons, and in order to generate as many observations from the crisis cohorts as possible, we will consider the cohorts born during the years 1727–1728 as one group, and then compare them with the cohorts born during the post-crisis period. For consistency, we proceed to keep the cohorts born during the years 1731–1735 as the control group.[10]

Table 3 reports the death-risk estimates of the 1727–1728 cohort at ages 0, 10, 20, and 30, respectively. The numbers underneath the parentheses are the number of individuals in the 1727–1728 cohort included in the regression. Column 1 reports the estimates using all observations, while Columns 2 and 3 subdivide observations into those born in the non-Midland and Midland parishes, respectively. It was evident from tables 1 and 2 that the 1727–1728 cohort was subject to increased death risk at age 0 (table 3, first row). Yet, this could merely reflect the fact that death set in more or less immediately after the famine struck, and that there were no long-term effects on mortality of those who survived the famine years. Column 1 of table 3 shows, however, that individuals born during the famine years of 1727–1728 *also* suffer a statistically significantly increased death risk of more than 15 percent at ages 10, 20, and 30. Meanwhile, consistent with the findings reported in table 1, Columns 2 and 3 of table 3 demonstrate that only individuals born in the Midlands parishes are subject to an increased death risk. Column 3 shows that the death risks are up to 28 percent higher when performing the analysis using only individuals born in the Midlands.

When we look at the death risk of the subsample of individuals for whom we know the father's occupation, presented in table 4, the data clearly show that *only* individuals born to families of a lower socioeconomic rank suffered a death risk that was significantly increased by comparison to their control group. Judging by the magnitude of the estimates, individuals born to the more affluent (non-manual worker) families were also exposed to a higher death risk relative to their control group. However, at the age of 10, the increased death risk of children of manual workers is nearly ten times greater than that of their non-manual counterparts, showing clearly that the rich were far less exposed to the famine conditions, and thus to its lasting impact, than the poor.

[10] It does not make any qualitative difference to the conclusions obtained below if the cohorts born during the years 1730–1734 are used as a control group instead.

Table 4. *Death risk at various ages of the 1727–1728 cohort: all and by socioeconomic group*

Age	(1) All	(2) Non-manual	(3) Manual
>0	0.310*** (0.077)	−0.063 (0.155)	0.461*** (0.085)
	584	168	416
>10	0.381*** (0.109)	0.067 (0.177)	0.529*** (0.133)
	159	60	99
>20	0.381*** (0.125)	0.159 (0.208)	0.488*** (0.192)
	132	51	40
>30	0.458*** (0.162)	0.370 (0.303)	0.518*** (0.192)
	69	29	40

Robust standard errors clustered by family are used to calculate *p*-values. Bottom line numbers are number of observations in 1727–1728 cohorts. One, two and three stars indicate statistical significance at the 10, 5, and 1 percent level.

Table 5. *Death risk at various ages of the 1727–1728 cohort in non-Midlands: all and by socioeconomic group*

Age	(1) All	(2) Non-manual	(3) Manual
>0	0.002 (0.137)	−0.224 (0.240)	0.173 (0.145)
	186	91	95
>10	0.083 (0.172)	−0.211 (0.251)	0.279 (0.234)
	74	40	34
>20	0.082 (0.208)	−0.198 (0.324)	0.250 (0.271)
	64	34	30
>30	0.076 (0.272)	−0.031 (0.483)	0.154 (0.317)
	34	18	16

Robust standard errors clustered by family are used to calculate *p*-values. Bottom line numbers are number of observations in 1727–1728 cohorts. One, two and three stars indicate statistical significance at the 10, 5, and 1 percent level.

Given what we now know about the individuals whose fathers were manual workers, an interesting question is whether the Midlands were hit harder by the famine because its parishes contained more manual-worker families, or whether this had something to do with geography (or perhaps both). Together, tables 5 and 6 can shed some light on the issue, though we should keep in mind that the number of individuals becomes rather low, especially as we approach the later stages of life. Table 5 shows the death risk of individuals born in a non-Midlands parish. Here, it is clear that, while individuals from manual-worker families suffered an increased death risk, none of the individuals of the two socioeconomic groups was hit significantly by the famine (although this could be due to the small number of observations). In contrast, table 6 shows that individuals born in a Midlands parish were significantly worse off than the control group, particularly later in life. Basing our judgments merely on the *magnitude* of the estimates, the conclusion is similar in the sense that the death risks of manual workers in the Midlands parishes are twice as great as that of their non-Midlands counterparts. Likewise, it is clear that the Midlands individuals from non-manual families are subject to a substantially increased death risk compared with their non-Midlands equivalents (who appear to have reduced death risks compared with the control group). In summary, the key message to be taken from tables 3–6 is that those who

Table 6. *Death risk of the 1727–1728 cohort in Midlands: all and by socioeconomic group*

Age	(1) All	(2) Non-manual	(3) Manual
>0	0.417*** (0.090)	0.025 (0.204)	0.527*** (0.096)
	398	77	321
>10	0.590*** (0.135)	0.378 (0.246)	0.660*** (0.154)
	85	20	65
>20	0.606*** (0.153)	0.561*** (0.246)	0.630*** (0.184)
	68	17	51
>30	0.730*** (0.208)	0.844** (0.356)	0.711*** (0.247)
	35	11	24

Robust standard errors clustered by family are used to calculate p-values. Bottom line numbers are number of observations in 1727–1728 cohorts. One, two and three stars indicate statistical significance at the 10, 5, and 1 percent level.

Table 7. *Differences in life expectancy at age 10: 1727–1728 cohort versus control group*

N = 12,640	(1) All	(2) Non-Midlands	(3) Midlands
All	−2.7	−2.3	−4.3
N = 5,675	All	Non-Manual	Manual
All	−6.4	−2.4	−8.5
Non-Midlands	−3.8	−4.3	−2.8
Midlands	−9.2	1.3	−12.5

Differences are based on the estimates of the mean longevity obtained as the integral of the survival functions estimated with the Kaplan–Meier estimator for the 1727–1728 cohorts and the control-group cohorts.

where hit the hardest—individuals of poor families in the Midlands area—suffered around a 60 percent increase in their death risk throughout life.

Our findings are consistent with the idea that poor relief per capita was relatively small in many parts of the Midlands (Boyer 1990), which led Schofield (1991, 37) to conclude that "[...] in some parts of the Midlands the harvest failure of 1728–29 may have seen prices rise to a point that made existing levels of poor relief inadequate for a population already weakened by disease. Moreover, the consequences may have been especially serious in areas where poor communications continued to hinder the ready transportation of bulky foodstuffs."

Another interesting question is how great was the reduction in life expectancy among the various groups at different stages of life. These results are reported in table 7. The estimates—i.e. the number of years of life expectancy lost among the individuals of the crisis cohort—are based on differences in the restricted means between the 1727–1728 cohort and the control group. Whilst the overall loss of life expectancy among all individuals in the sample is 2.7 years (Column 1), it is clear that this number hides much information about geographic and socioeconomic differences in the population. The largest effect is found among individuals born to poor (manual-worker) families in the Midlands, showing that the average loss of life at age 10 is more than 12 years compared with the control group. Given that the life expectancy at age 10 among control-group individuals is 40 years (meaning that, on average, they live to reach age 50), the life expectancy at birth of an affected individual is 25 percent shorter, which represents a substantial loss of life.

5. Conclusion

Using the demographic data from the Cambridge Group's *Population History of England*, this study documents that children born to English Midlands families of a lower socioeconomic rank during the famine of the late 1720s suffer markedly higher death risk and considerably lower life-expectancy compared with the control group, not only at birth but also later in life. This suggests that a "scarring" effect was dominating an eventual "selection" effect, lending a strong support to the "fetal origins hypothesis" proposed by Barker (1995).

Our findings lead us to conclude that scholars who study the role of mortality changes in the early modern period and through the Industrial Revolution should consider not merely the short-term impact of hardship but also the long-term effect on mortality of early-life adversity. And, furthermore, that changes in mortality during the early stages of life may be correlated with the changes in mortality later in life.

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Malthus in the Bedroom: Birth Spacing as a Preventive Check Mechanism in Pre-Modern England*

With Francesco Cinnirella and Jacob Weisdorf

Abstract We question the received wisdom that birth limitation was absent among historical populations before the fertility transition of the late nineteenth-century. Using duration and panel models on family-level data, we find a causal, negative short-run effect of living standards on birth spacing in the three centuries preceding England's fertility transition. While the effect could be driven by biology in the case of the poor, a significant effect among the rich suggests that spacing worked as a control mechanism in pre-modern England. Our findings support the Malthusian *preventive check* hypothesis and rationalize England's historical leadership as a low population-pressure, high-wage economy.

Keywords Birth Intervals, Fertility Limitation, Natural Fertility, Preventive Check, Spacing

JEL Classification Codes J11, J13, N33

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1 Introduction

The timing of the industrial revolution and the onset of the demographic transition (i.e. fertility decline) strongly influenced the growth pattern of modern economies (Galor, 2011, 2005). Unified growth theory (Galor and Weil, 2000; Galor and Moav, 2002; Hansen and Prescott, 2002; Jones, 2001) provides a framework that explains the long-run transition from Malthusian stagnation to modern economic growth. England was the first country to make the transition from a Malthusian economy to one of sustained economic growth, and its world leadership during the eighteenth century is often attributed to its fertility restrictions (Voigtländer and Voth, 2009; Voigtländer and Voth, 2011). The argument is that Malthusian *preventive checks* (i.e. birth limitations in periods of economic hardship) kept the population pressure low, allowing higher incomes per capita (Wrigley and Schofield, 1989).

Scholars have long believed that late age at first marriage of women was the main preventive check mechanism operating in England prior to its late nineteenth-century demographic transition (Voigtländer and Voth, 2011). Indeed, early work by Wilson concludes that marital birth limitation was absent in pre-industrial England, classifying it as a *natural fertility* society (Wilson, 1984). Later, more statistically advanced studies have found little or no effect of living standards — measured in terms of real wages and food prices — on aggregate birth rates.[1] This fits well with the conclusion reached by the European Fertility Project that marital birth limitation was invented during the European fertility decline of the late nineteenth century and implemented by the diffusion of knowledge about contraceptives, such as *coitus interruptus*, sexual abstention, and extended breastfeeding (Coale, 1986).

However, recent studies, especially in the field of historical demography, have shown that marital birth limitation was practiced in the Low Countries, Germany, and Sweden from the late eighteenth century onwards (Bengtsson and Dribe, 2006; Dribe and Scalone, 2010; Van Bavel and Kok, 2004; Van Bavel, 2004). But despite England's key role in the long-term economic development of the west, with the exception of Wrigley's study of the parish of Colyton (Wrigley, 1966) and Wilson's subsequent analysis of 13 English parishes (Wilson, 1984), no attempts have been made to analyze fertility restriction at the household level using data from English parish records.

It is equally surprising that the numerous attempts to document a short-term response of marriage rates and birth rates to living standards in the aggregate offer very little evidence that Malthusian preventive checks were operating in England before 1800 (Bailey and Chambers, 1993; Crafts and Mills, 2009; Lee, 1981; Lee and Anderson, 2001; Weir, 1984). This lack of evidence may be grounded in two key issues. First, the use of

[1] Bailey and Chambers (1993); Crafts and Mills (2009); Kelly and O Grada (2012); Lee and Anderson (2001).

aggregate data tends to average out the fertility response of different socio-economic groups, making it difficult to study the impact of living standards for the marital and reproductive behavior of those most prone to economic distress, i.e. the poor. Second, the crude birth rates, as well as the crude marriage rates, are incomplete proxies for marriage and birth decisions within the family as crude vital rates fail to fully reflect the demographic composition of the population. In fact, the vital rates do not capture entirely the household's birth spacing behavior, the study of which requires access to the demographic statistics at the family level.

In this paper we investigate marital birth limitation in pre-transition England, casting serious doubts about the notion that England was a natural fertility society. We show that the length of the birth interval functioned as a preventive check mechanism among English couples whose response to falling living standards was a prolongment of the time-span between the births of their offspring. More generally, this is the first study to provide a comprehensive picture, at the micro-level, of the relationships between English living standards and the patterns of family planning (including marriage, starting, spacing, and stopping) before its demographic transition.

We use family reconstitution data from Anglican parish registers to investigate the effect of living standards on the timing of family births in the three centuries leading to England's fertility decline in the late nineteenth century. Equipped with a variety of econometric tools (i.e. duration, panel, and instrumental variable models) we attempt to advance the research frontier along several dimensions. First, we exploit a smaller but substantially richer sample of the data previously used to study effects at the aggregate level.[2] Second, the nature of our data (family reconstitutions) allows us to control for a wide range of family characteristics, including the location, education, and fecundity of the spouses. Third, information about the occupations of the husbands enables us to isolate the families most vulnerable to economic hardship: the poor. Finally, vital dates in the data permit the use of duration analysis, meaning that we can study the influence of living standards on the *timing* of events.

Malthus conjectured that periods of economic difficulty were met by delayed marriages (Malthus, 1798), a hypothesis which we are the first to test directly. However, delaying marriage was not the only precautionary action a couple could take to reduce births. Historical families were relatively large (averaging 6-7 children) and the decision regarding the timing of a birth could be made repeatedly throughout the marriage. This makes the spacing of family births a potential preventive check mechanism. For completeness, we also investigate the influence of living standards on the timing of the first- and last-born (known in demography as "starting" and "stopping").

[2]Wilson (1984) uses a sub-sample of our data, but applies a somewhat less advanced statistical strategy.

Virtually all our econometric specifications demonstrate a negative effect of living standards (real wages and wheat prices) on the spacing of family births in the three centuries leading up to England's fertility transition, supporting the notion that economic hardship led to longer birth intervals. Importantly, the effect is prevalent among the poor (laborers, servants, and husbandmen) as well as among their more affluent counterparts (farmers, traders, merchant, and gentry), with the poorest groups displaying the largest effects (as expected).

We argue that the increased spacing between childbirths resulted from actions taken by the couples and, thus, was *not only* due to a biological effect (i.e. infertility caused by famine or malnutrition). This is substantiated by two main findings: (*i*) a negative relationship between living standards and birth intervals exists across the entire socio-economic spectrum and not only among the poor; and (*ii*) the negative effect remains large and significant even when we exclude the years of severe economic depression (causing failed harvests and food shortages).

Consistent with the findings of Clark and Hamilton (2006), and Boberg-Fazlic et al. (2011), demonstrating that the rich had more offspring than the poor, our results imply that this was achieved through relatively shorter birth intervals among the rich. In addition, our investigation into the behavior of different socio-economic groups reveals that, as expected, farmers responded differently to other occupational groups, showing *reduced* birth intervals in response to higher wheat prices. This finding further rationalizes the notion of behavioral effects (in addition to biological explanations).

Our analysis confirm the Malthusian hypothesis that lower living standards led to delayed marriages and later first conceptions (i.e. postponed "starting"). Moreover, the fact that living standards have *no* effect on the waiting time from a couple's marriage date to their first conception verifies the presumption that, in the past, English marriage marked the onset of unprotected sex (Wrigley et al., 1997). We also conclude that the timing of the last delivery ("stopping") is unaffected by living standards. However, the finding that the rich stopped earlier than the poor is further evidence of the existence of birth limitation in the centuries prior to the demographic transition of the late nineteenth century.

In addition to the use of duration models we also analyze the data in a panel setting, which enables us to account for heterogeneity at the family level. Moreover, the possible existence of unobserved time-varying variables, correlated with both real wages and birth intervals, raises concerns that our estimates could be biased. To address this issue, we adopt an instrumental variable approach identifying exogenous variation in real wages using monthly air temperatures. Weather conditions, as captured by the air temperature, have an impact upon crop yields and thus wheat prices and real wages. The identifying assumption is that the monthly air temperatures have only an effect on the birth intervals through the real wages and wheat prices. The instrumental variable estimates confirm the negative effect of living standards on the spacing of births.

The remainder of the paper is structured as follows: In Section 2 we describe the key features of the data and the potential problems related to their use. In Section 3 we analyze fertility patterns by estimating duration models. In Section 4 we adopt a panel structure and address the issue of causality using an instrumental variable approach. In Section 5 we present some robustness checks to confirm that the adjustment of birth intervals is not only a biological mechanism. Section 6 concludes.

2 Data

The analysis below is conducted using three main pieces of data: real wages, wheat prices and demographic statistics. Beginning with the latter, the demographic data used to compute the timing of our events come from Anglican parish registers (English church books). Collected over the past 40 years by the *Cambridge Group for History of Population and Social Structure* the full data comprise a total of 404 parish records. Documented by Wrigley and Schofield (1981) this sample provides yearly birth-, death-, and marriage-rates covering the period 1541–1871. Counting the number of events per 1,000 persons, these rates have been previously used to test the Malthusian preventive check hypothesis.

Meanwhile, inspired by Louis Henry's family reconstruction of French parish data, the Cambridge Group selected 26 of their 404 English parishes and used the ecclesiastical events to reconstitute over 80,000 families, comprising nearly 280,000 individuals. The 26 parishes (forming what we call the *Reconstitution data*) were chosen for their remarkable quality and because they appeared to be representative of the entire country. The sampled parishes range from market towns to remote rural villages, including proto-industrial, urban and agricultural communities. The data is documented in detail by Wrigley et al. (1997).

In a descriptive analysis of the parish of Colyton (one of the 26 reconstituted parishes) Wrigley found evidence pointing towards deliberate birth limitation occurring around 1700. This was attained, he argued, through late marriages, extended birth intervals, and low stopping ages (Wrigley, 1966). However, after adding a further 12 parishes to the sample (totalling 13 of the 26 parishes) Wilson revised Wrigley's conclusion, stating that "while the existence of family limitation in pre-industrial England cannot be ruled out, it is highly unlikely that it was of any significance in determining the overall pattern of marital fertility" (Wilson, 1984, p. 240).[3] Below we extend the work of Wrigley (1966) and Wilson (1984) by including all 26 parishes in our sample. Moreover, by means of more

[3]Reviews and criticisms of the Wrigley and Schofield (1983) study are also included in a special issue of the *Journal of Interdisciplinary History* published in 1985.

advanced econometric techniques we are able to deal more substantially with geographical and family heterogeneity present in the data.

Family reconstitution data offer more information (and hence covariates) compared to the (aggregate) birth and marriage rates used in the recent studies of birth patterns.[4] Indeed, every family in the Reconstitution data is built around a marriage, providing information about the birth (baptism) dates and death (burial) dates of the spouses, as well as the gender, birth, and death dates of their offspring.

Typically, the church recorded baptism dates rather than birth dates. We generate a birth date variable using the actual birth dates where available. To obtain the date of conception, which we will use in the analysis, we subtract 280 days from the birth date variable.[5] Moreover, in order to assess the quality of the birth dates, Figure 1 and Figure 2 illustrate the distribution of births by month and day of the month, respectively. The distribution by month does not show any significant heaping. However, Figure 2 indicates some heaping, especially in the months of January and December. The spike on the 25th of December can be explained by the preference of families to baptize their children on Christmas Day. The spike on the 1st of January is possibly related to missing (unreadable) dates, imputed by the transcribers as the first date of the year. It should be noted that since England switched from the Julian to the Gregorian calendar during our period of study, we have converted all dates into the Gregorian calendar.[6] The spike on the 11th of January is thus due to the same reason for the spike on the 1st January. Hence, in the analysis below we use controls for the following dates: 25th December, 1st January, and 11th January.

The data also provide ample information about the socio-economic background of the family, as well as the education and fecundability of the couple. For example, the clergy frequently reported the occupation of the spouses (albeit far more frequently for men than for women). The occupations were recorded at the time of marriage and burial, as well as at the baptisms or burials of the offspring. Using will records from historical England, Clark and Cummins (2010) have constructed seven socio-economic groups, ordered according to the wealth information found in the wills. The occupational titles thus permit a classification of our families according to their wealth or income potential. From the poorest to the richest these are: laborers, husbandmen, craftsmen, traders, farmers, merchants, and the gentry.[7] We use the earliest known occupation of the husbands to classify our sampled families (and a binary variable if the occupation is missing). Educational information comes from the spouses' signature on their wedding certificates (as opposed

[4] Bailey and Chambers (1993); Crafts and Mills (2009); Kelly and O Grada (2012); Lee and Anderson (2001).

[5] The traditional definition of a full-term pregnancy is 40 weeks. Our results are not sensitive to a different definition.

[6] Britain adopted the Gregorian calendar in 1752, by which time it was necessary to correct by 11 days.

[7] We are grateful to Greg Clark for providing us with the mapping procedure.

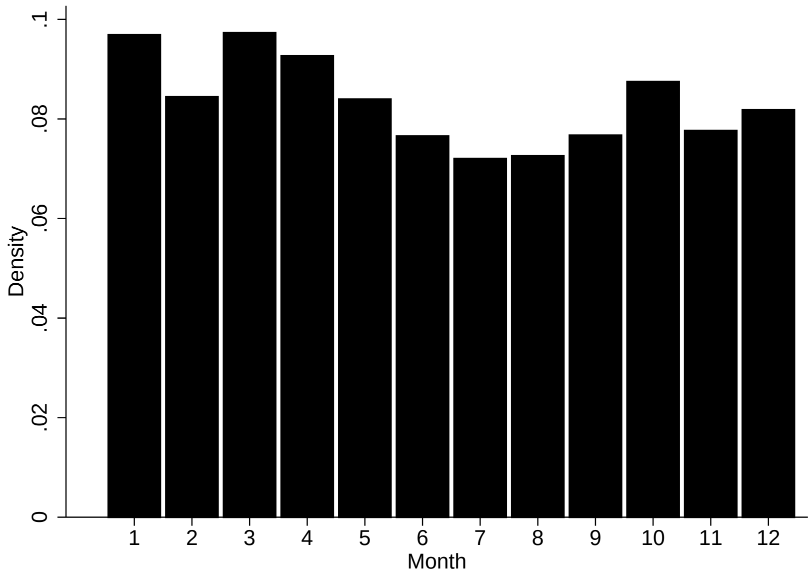

Figure 1: Distribution of births by months.

to leaving a cursory mark) which reveals their literacy status. This is a widely used indicator of human capital for the time before public schooling became prevalent (Clark, 2008). Finally, as is standard in historical demography (e.g. Wrigley et al. (1997)), the fecundability of our couples is inferred from the time-interval between their marriage and their first birth.[8]

While family reconstitution data provide an invaluable source of information, they are also subject to a set of restrictions.[9] A natural limitation is that any ecclesiastical event occurring outside of the parish of origin is not recorded in the parish register and, therefore, does not appear in the reconstitution. It is reasonable to assume that migrating and non-migrating families did not differ systematically with respect to their fertility response to changes in living standards. However, we performed several robustness checks to ensure that our estimates are not biased because of selective migration. Indeed, we can show that when constraining the sample to families that are *completed* (that is, when we can observe them through to the end of the wife's reproductive period) we obtain qualitatively the same results.[10]

A related issue is that some couples may only have temporarily migrated. Thus, if these couples had children before and after the migration period, an unusually large birth interval may occur since we cannot detect any children born and baptized elsewhere.

[8]Fecundability is the probability that conception will occur in a given population of couples during a specific time period.

[9]For a more in-depth explanation of the possible sources of error in the English family reconstitution, and in the analysis performed by Wrigley et al. (1997), see Ruggles (1999).

[10]A family with completed fertility is defined as a marriage in which both the wife and the husband survived (at least) until the wife reached the age of 50 years. It therefore consists of a couple that exhausted their reproductive lifetime in the parish of origin.

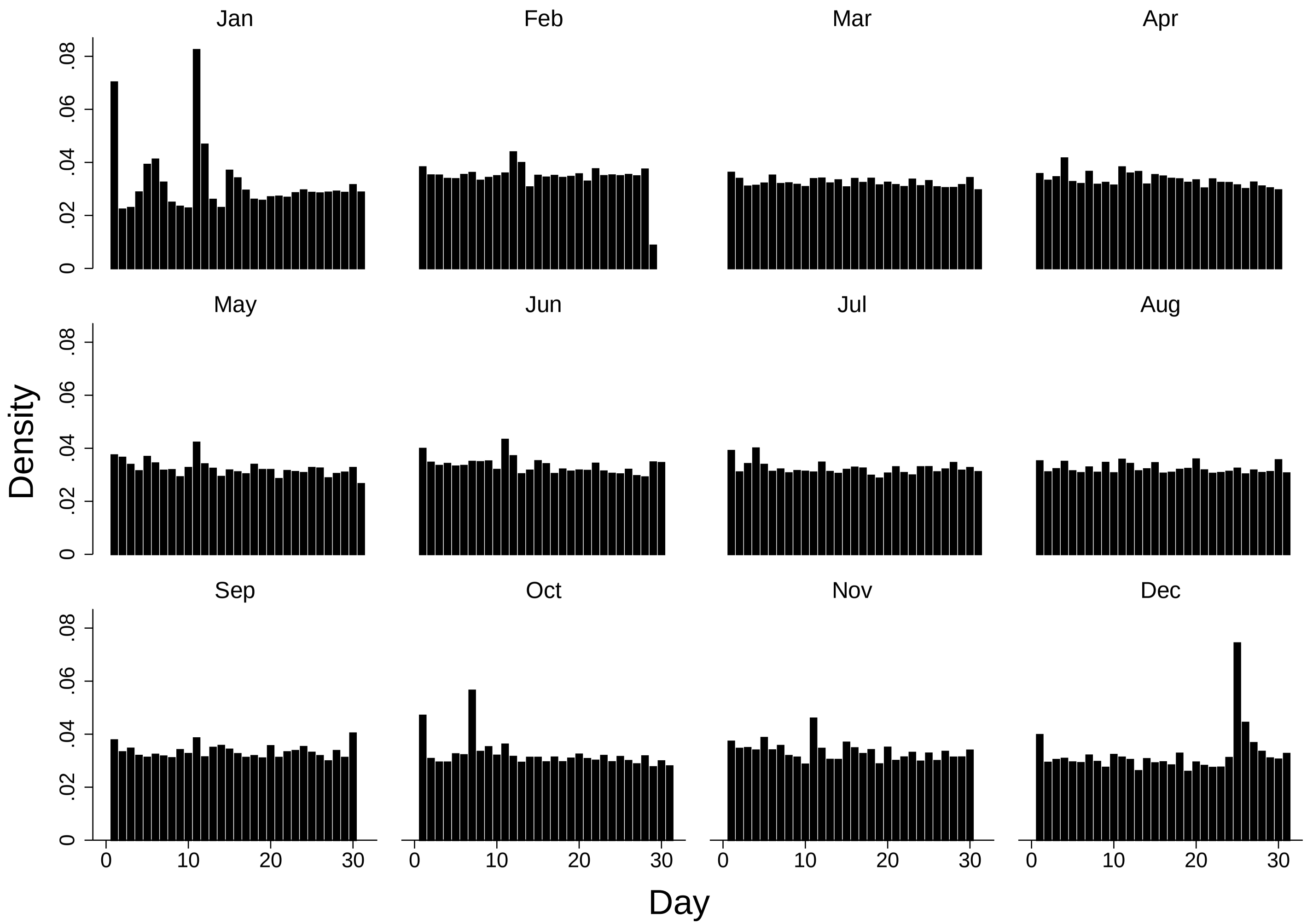

Figure 2: Distribution of births within the twelve months of the year.

Similarly, a miscarriage early into the pregnancy was not recorded in the parish registers, but can nevertheless create an extended birth interval.[11]

The issue of migration will bias our results to the extent that migration patterns and spacing behavior are correlated. Miscarriages, on the other hand, touch upon the problems involving separating the actions taken to limit fertility from biological reactions (such as temporary infertility) caused by malnutrition or poor health conditions. We will address these issues by performing various robustness checks. Note, finally, that in the duration models we will take into account the problem of right-censoring due to the death of a spouse or the wife reaching the age of 50, after which we assume sterility has set in and conception is no longer possible.

2.1 Outcome Variables

As a first step, we will investigate the effect of real wages on the hazards of five different demographic events: (*i*) marriage, (*ii*) starting, (*iii*) first birth, (*iv*) spacing, and (*v*) stopping. In the "marriage", "starting" and "stopping" analysis, every wife (i.e. every couple) is included once, and the events examined involve the points in time at which she married, conceived her first child, and conceived her last child, respectively. We assume that the wife becomes at "risk" of encountering these events from the age of 15. In the case of the "first birth" variable, the event analyzed is the conception of the first child, conditional on the wife being married. This analysis, therefore, includes only couples that conceived their first child while married (thus excluding prenuptially conceived births). Finally, in the analysis of the "spacing" variable, the event analyzed is the conception of a child, conditional on having given birth to a child of lower order. Each of the five outcome variables are regressed on real wages (the sources and methodology are described below), as well as a set of family-background covariates including the couple's socio-economic rank, literacy status, and fecundity.

The summary statistics are reported in Table 1. The average age at marriage of wives is 23.7 years and the average age at starting is 25. Thus, the time interval between marriage and the first birth is slightly over one year. The average length of a birth interval is 929 days (roughly 2.5 years) with a standard deviation of 475 days. Twin births (less than 2 percent of all births) are considered as single events, whereas the relatively few cases (n=986) in which the birth intervals are less than 40 gestational weeks (stemming either from preterm births, transcription or data errors, or delayed baptisms) are removed from the sample.[12]

[11] However, the data suggest that stillborn children are present in the parish register as we have about 2700 observations for which the date of birth coincides with date of death. This is consistent with the parents' desire to baptize the stillborn children to save them from purgatory.

[12] As their inclusion has no impact on our qualitative conclusions.

Table 1: Summary statistics

Variable	*Mean*	*SD*	*Min*	*Max*	*N*
Spacing (days)	929.238	475.058	260	4,368	191,892
Mother's age at marriage (years)	23.669	4.275	15.001	46.667	62,515
Mother's age at starting (years)	24.972	4.510	15.110	47.606	71,556
Time to first birth (years)	1.194	1.131	-0.077	11.975	116,220
Prenuptially conceived	0.215	0.411	0	1	191,892
Mother's age at stopping (years)	38.411	5.858	16.794	49.993	71,556
Labourers	0.153	0.360	0	1	191,892
Husbandmen	0.085	0.279	0	1	191,892
Craftsmen	0.101	0.301	0	1	191,892
Traders	0.047	0.212	0	1	191,892
Farmers	0.030	0.171	0	1	191,892
Merchant	0.057	0.232	0	1	191,892
Gentry	0.015	0.122	0	1	191,892
Occupation unknown	0.511	0.500	0	1	191,892
Mother's age when born (years)	30.014	5.875	15.110	48.997	71,556
Mother literate	0.334	0.472	0	1	36,126
Mother's literacy unknown	0.812	0.391	0	1	191,892
Birth order	3.082	2.137	1	19	191,892
Household size	6.175	2.703	2	21	191,892
Child mortality (0-1 year)	0.138	0.345	0	1	191,892
Child mortality (1-3 years)	0.057	0.231	0	1	191,892
Child mortality unknown	0.593	0.491	0	1	191,892

Source: Cambridge reconstitution data.

Table 2: Summary statistics of aggregate variables

Variable	*Mean*	*SD*	*Min*	*Max*
Real wage	0.199	0.064	0.078	0.418
Wheat price	2.892	2.625	0.222	14.837
Crude death rate	26.633	4.479	19.200	53.900
Mean temperature	9.214	0.659	6.840	10.82

Source: Real wages and wheat prices are from Clark (2007). Crude death rates (per 1000 people) are from Wrigley (1997). Mean temperatures (in degrees Celsius) from Manley (1953).

The most common occupations in the data are laborers, husbandmen, and craftsmen. For roughly fifty percent of the sample we have no information about the parental occupation. Information about the literacy status of women is available only after 1750. About 33 per cent of the brides were able to sign their names.

2.2 Living Standards

Our key explanatory variable is living standards, measured by the level of the real wage. Following the recent literature, the real wages used come from Clark (2007). The real wage series is constructed by dividing the nominal wage rate of unskilled rural laborers by the cost-of-living index.[13] It should be noted that the wage series combine wage observations from throughout England, as documented by Clark (2007).

We also use two alternative measures of living standards. First, since wheat was a main staple in historical England, we use yearly data on wheat prices, again provided by Clark (2007), to proxy the living standards. In addition we use a national series of the crude death rates, provided by Wrigley et al. (1997), to account for famine and disease. The descriptive statistics of these series are presented in Table 2.

Figures 3 and 4 illustrate the relationship between average birth intervals and real wages. Figure 3 reveals the evolution of the two time-series for the entire period of 1540–1850, whereas Figure 4 shows the average birth intervals when we subdivide the standardized real wages in percentiles. In fact, the latter figure shows a cross-sectional gradient in birth intervals: higher levels of the real wage are associated with shorter spacing. We obtain a similar picture when looking at average birth intervals by occupational group (Figure 5): more affluent social groups (traders, merchants and gentry) are associated with shorter birth intervals.

[13]Gregory Clark kindly provided the annual data. A related real wages series constructed by Allen, which has less variation in the nominal wages than Clark's, provides results that are quantitatively similar to those obtained by using the Clark series. Allen's data is available at http://www.nuffield.ox.ac.uk/users/allen/data/labweb.xls.

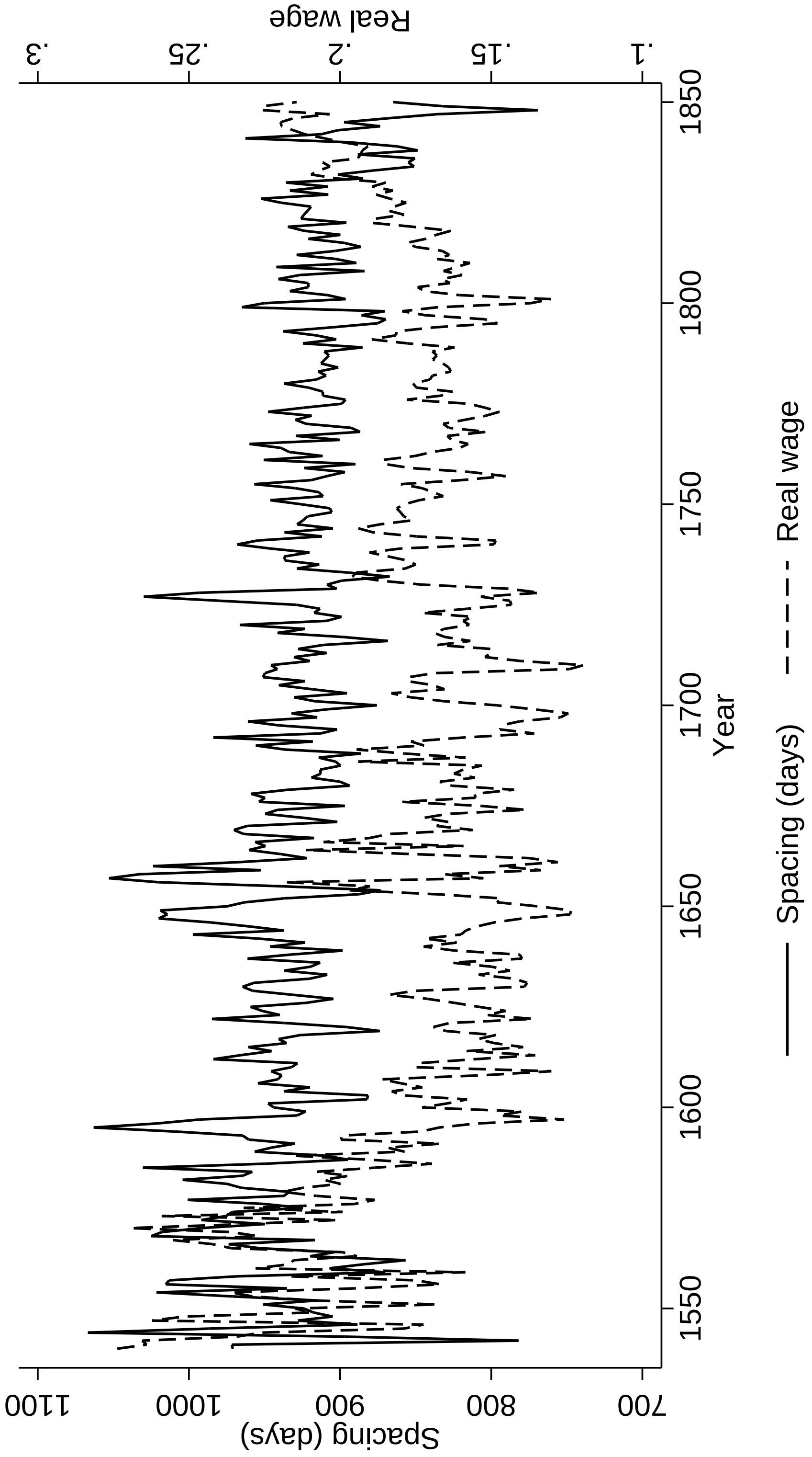

Figure 3: Real wages and average spacing, 1540–1850.

Table 3: Average birth intervals (days) within family

Period	*First interval*	*Second last interval*	*Last interval*
1540–1699	830.4	936.0	1,066.3
1700–1749	803.3	926.4	1,076.6
1750–1799	798.2	922.9	1,053.0
1800–1850	805.9	916.4	1,005.3

Source: Cambridge reconstitution data.

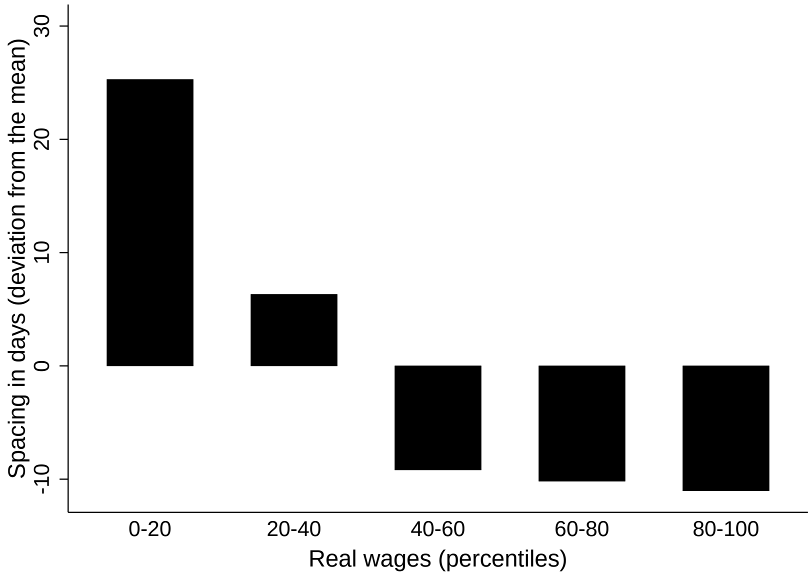

Figure 4: Average spacing by real wage percentiles.

Figure 5: Average spacing by occupational group.

3 Duration Analysis

In this section, we explore the effect of living standards on the five variables defined above: "marriage", "starting", "first birth", "spacing," and "stopping". We use the Cox Proportional Hazard (CPH) model (Cox, 1972) and estimate the effects of time-varying covariates on the hazard function. The CPH model with time-varying covariates is specified as follows:

$$h(t) = h_0(t) \exp\left(\beta_1 x_1 + \cdots + \beta_k x_k + g(t)(\gamma_1 z_1)\right). \quad (1)$$

The term $h_0(t)$ is the baseline hazard function; $(x_1, \ldots, x_k)$ are socio-economic and demographic covariates; and z the (time-varying) real wage. Estimates are stratified by parish and quarter centuries, with each stratum having its own baseline hazard $h_0(t)$. Durations are measured at the individual level, whereas the real wages are measured annually at the national level. Therefore, we cluster the standard errors by the year of the respective demographic outcome, namely the marriage year and the conception year of the first ("starting"), successive ("spacing"), or last offspring ("stopping").

The last birth intervals in the sample (spanning the time from the penultimate to the final delivery) are significantly longer (on average) than the previous intervals (see Table 3). Although this could be attributed to fertility decreasing with age (Baird et al., 2005), demographers have argued that longer spacing to the last birth captures a failed attempt to end the wife's childbearing period (Van Bavel, 2004; Okun, 1995; Knodel, 1987; Anderton, 1989). For this reason we include two versions of the "spacing" model. In the first version, we include all birth intervals, while in the second version we exclude the last. Note also that in the stopping analysis, we are only able to consider completed marriages.[14] This way we will know that the last birth recorded was indeed the final delivery of the couple, and not the last birth record before the couple moved to an (unobserved) parish where they continued to have children.

3.1 The Effect of Real Wages on Fertility Outcomes

Table 4 reports the results of the duration models for the full period, 1540–1850, with the living standards measured by real wages. To ease the interpretations, the real wages are standardized with a mean of zero and a standard deviation of one. The coefficients are reported as semi-elasticities, with a *positive* coefficient indicating a *higher* "risk" that the event occurs (broadly speaking, a higher probability of marriage or conception), and *vice versa*.

Table 4 shows that the real wage has a significant, positive impact on the risk of "marriage" and "starting" (Columns 1 and 2). A one-standard deviation increase in the

[14] See footnote 10.

real wage increases the probabilities of marriage, as well as first conception, by roughly 52 percent. The former effect — falling real wages delay the marriage — is first-hand evidence that Malthusian preventive checks operated at the family level in historical England. The latter effect — falling real wages delay the first conception — could potentially be attributed to a biological effect (i.e. lower real wages resulting in undernourishment and hence infertility). Yet, when fitting the model for "first birth" we find no significant effect of changes in the real wage, suggesting that, *ceteris paribus*, couple's fecundability (as measured by the time from the marriage to the first birth) is *not* influenced by real wages (Column 3).[15] Since the magnitude of the two effects on "marriage" and "starting" are almost identical, and because the real wage has no significant effect on fecundability, it appears that the timing of the first conception ("starting") lies within the decision variables of the couple and is not biology-driven.

During a time without access to modern contraceptives, and with marital births continuing throughout most (if not all) of a woman's reproductive period, Malthus emphasized that couples would largely seek to act prudently *prior* to marriage. Yet we know from the fertility decline of the nineteenth century that parental prudency *within* marriage was also perfectly feasible by means of withdrawal, abstention, or extended breastfeeding (Coale and Watkins, 1986). Contrary to the conclusion reached by the European Fertility Project, these methods may indeed have been practised even *before* the nineteenth century, and, hence, may well have contributed to England's low population-pressure, high-wage regime. In fact, the coefficients of Columns (4) and (5) lend strong support to the idea of within-marriage preventive checks, with the real wage exercising a significant, negative impact on the spacing of consecutive births. Column (4) reports the effect of real wages on *any* birth interval (including the last birth interval) while Column (5) shows the effect on the birth spacing excluding the last interval (see above). The latter (most relevant) effect implies that a one-standard deviation reduction in the real wage increases the risk of a birth by 18 percent. To eliminate the bias of a failed attempt to stop childbearing, in the following analysis the "spacing" variable excludes the last birth interval.

To ensure that the effect on spacing is not a spurious finding, we can perform a placebo test shifting the real wage series forward by 3, 5, and 10 years, respectively. It follows that the effect of the real wages on the birth intervals is small and highly insignificant in all the cases (Table 5).

Turning to the question of "stopping" (Table 4, Column 6), there is no significant effect of the real wage on the risk of a last conception. However, the "stopping" interval

[15]Couples with longer than two years and 40 weeks from their marriage to their first birth, corresponding to a period of two years between marriage and the first conception, were excluded from this analysis.

Table 4: Duration models

Dependent variable: Spacing in days	*Marriage (1)*	*Starting (2)*	*Time to first birth (3)*	*Spacing (4)*	*Spacing (w/o) (5)*	*Stopping (6)*
Real wage	0.419*	0.423*	-0.045	0.057*	0.166***	0.050
	(0.237)	(0.250)	(0.103)	(0.032)	(0.022)	(0.244)
Wealth group:						
Husbandmen	-0.031	-0.035	0.066**	0.060***	0.073***	0.222***
	(0.032)	(0.032)	(0.028)	(0.013)	(0.016)	(0.079)
Craftsmen	-0.076***	-0.076***	0.043	0.071***	0.086***	0.111
	(0.027)	(0.028)	(0.028)	(0.014)	(0.016)	(0.069)
Traders	-0.039	-0.048	0.019	0.151***	0.185***	0.180*
	(0.042)	(0.035)	(0.037)	(0.019)	(0.022)	(0.103)
Farmers	-0.042	-0.071*	-0.053	0.142***	0.222***	0.233**
	(0.045)	(0.039)	(0.047)	(0.020)	(0.023)	(0.101)
Merchant	-0.013	-0.037	0.024	0.164***	0.205***	0.222**
	(0.039)	(0.038)	(0.038)	(0.021)	(0.022)	(0.094)
Gentry	0.130	0.082	0.003	0.169***	0.303***	0.832***
	(0.086)	(0.070)	(0.066)	(0.034)	(0.037)	(0.225)
Unknown	-0.105***	-0.124***	-0.016	-0.101***	0.069***	0.298***
	(0.024)	(0.025)	(0.023)	(0.015)	(0.013)	(0.068)
Mother literate	-0.004	-0.011	-0.013	0.026*	0.068***	0.212***
	(0.023)	(0.022)	(0.028)	(0.015)	(0.015)	(0.073)
Mother's literacy unknown	-0.121***	-0.300***	-0.003	-0.017	-0.008	0.109
	(0.037)	(0.026)	(0.042)	(0.030)	(0.021)	(0.083)
Time to first birth (years)				-0.098***	-0.052***	0.011
				(0.003)	(0.004)	(0.014)
Prenuptially conceived				-0.020**	-0.017*	0.020
				(0.008)	(0.009)	(0.042)
Child mortality at age (years):						
0–1				0.460***	0.737***	-0.048
				(0.013)	(0.015)	(0.062)
1–3				0.200***	0.162***	-0.152*
				(0.016)	(0.017)	(0.090)
Unknown				-0.011	0.029***	-0.084*
				(0.010)	(0.009)	(0.043)
Crude death rate		-0.023	-0.006	-0.007***	-0.007***	-0.014
		(0.022)	(0.009)	(0.002)	(0.002)	(0.020)
Birth order				-0.094***	-0.011***	
				(0.002)	(0.002)	
Mother's age at marriage	No	No	Yes	Yes	Yes	Yes
Observations	214,939	262,618	58,619	351,815	225,312	93,781
Subjects	20,040	22,621	28,100	142,009	85,147	3,795

Note: Cox proportional hazard model with time-varying real wages. Real wages are standardized. In Column 5 we do not consider the last closed birth interval. Coefficients (semi-elasticities) reported. Estimates are stratified by parish and quarter century. Standard errors are clustered by the year of the demographic outcome. Laborers are the reference wealth group. *** p<0.01, ** p<0.05, * p<0.10. Source: Own estimates.

Table 5: Placebo test on duration models

Dependent variable: Spacing in days	*Shift 3 years (1)*	*Shift 5 years (2)*	*Shift 10 years (3)*
Real wage	-0.008	-0.006	-0.011
	(0.025)	(0.023)	(0.023)
Controls	Yes	Yes	Yes
Observations	225,312	225,312	225,312
Subjects	85,147	85,147	85,147

Note: Cox proportional hazard model with time-varying real wages. Real wages are standardized. Coefficients (semi-elasticities) reported. Estimates are stratified by parish and quarter century. Standard errors are clustered by the year of the demographic outcome. *** p<0.01, ** p<0.05, * p<0.10. Source: Own estimates.

(from when the wife turns 15 to her final conception) can comprise some 35 years, so a lacking effect is, perhaps, unsurprising.[16]

3.2 Occupational Groups

Our covariates can help shed light on the bearing of socio-economic rank for fertility patterns in the past. The reference group in the specifications of Table 4 are those whose occupation is "laborer". We find that the lower socio-economic ranks (laborers and husbandmen) had on average longer birth intervals but also that they stopped later than their more affluent counterparts, such as farmers, merchants, and gentry (Table 4, Columns 4 to 6). This result — that the hazard of a further birth increases with family affluence — has already been noted in Figure 5, which demonstrates average spacing by occupational group.

In order to establish whether the effect of the real wage on spacing differs across the various socio-economic groups, we sub-divide the sampled families into poor (laborers and husbandmen) and rich (craftsmen, traders, farmers, merchants, and gentry). Table 6 reports the results when estimating the model for each group. As expected, the point estimates suggest that the risk of a further birth is higher among the poor (Column 1) than among the rich (Column 2) when the living standard (real wage) increases. Nevertheless, the fact that both groups respond significantly, and similarly, to changes in living standards provides additional evidence that the effect cannot be only driven by a biological mechanism. Dribe and Scalone (2010) reached a similar conclusion in their investigation of German data from 1766–1863.

The fact that the rich had more offspring than the poor, as recently demonstrated by Clark and Hamilton (2006) and Boberg-Fazlic et al. (2011), can be partly ascribed to their shorter birth intervals (Table 4, Columns 4 and 5). Early "stopping" among the rich, (i.e.

[16] We have experimented with different starting points of the risk of "stopping" (i.e. from when the wife turned 25, 30 and 35 etc) but these specifications also did not generate any significant effect.

Table 6: Spacing by economic status

Dependent variable: Spacing in days	*Poor* (1)	*Rich* (2)
Real wage	0.231***	0.146***
	(0.036)	(0.036)
Mother literate	0.060***	0.094***
	(0.023)	(0.023)
Mother's literacy unknown	-0.054	0.045
	(0.034)	(0.029)
Time to first birth (years)	-0.045***	-0.065***
	(0.008)	(0.008)
Prenuptially conceived	-0.008	-0.018
	(0.018)	(0.019)
Child mortality at age (years):		
0–1	0.764***	0.651***
	(0.028)	(0.029)
1–3	0.156***	0.144***
	(0.031)	(0.033)
Unknown	0.044**	0.027
	(0.017)	(0.017)
Birth order	-0.011***	-0.016***
	(0.004)	(0.004)
Crude death rate	-0.005	-0.004
	(0.004)	(0.003)
Mother's age at marriage	Yes	Yes
Observations	62,128	54,945
Subjects	23,346	21,762

Note: Cox proportional hazard model with time-varying real wages. Real wages are standardized. Poor are laborers and husbandmen; rich are craftsmen, traders, farmers, merchants, and gentry. Coefficients (semi-elasticities) reported. Estimates are stratified by parish and quarter century. Standard errors are clustered by the year of the demographic outcome. *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Source: Own estimates.

presumably before the end of their reproductive period - as inferred from the fact that the poor are able to continue), seems to suggest that families of higher socio-economic rank had a target number of offspring (Table 4, Column 6).[17]

3.3 Other Covariates

Among the remaining covariates it is interesting to note that female literacy is related to shorter birth intervals and early stopping (Table 4), even after controlling for affluence. A couple's fecundity — measured by the time-interval from the marriage to the first conception — also significantly reduces the spacing of the couple's later birth intervals, i.e. low-fecundity couples face a lower hazard of subsequent births. Couples with prenuptially conceived children also demonstrate a lower propensity for subsequent births.[18]

Also in line with our expectations, child mortality during infancy (ages 0–1) or in early childhood (ages 1–3) substantially raises the hazard of a next birth, indicating an attempt to immediately replace a deceased child. We have also included the annual crude death rate (at the national level) to account for situations such as famines or war, which might have impacted upon the fertility of the households. We find that periods of high mortality significantly reduce the hazard of a next birth and hence extend the spacing of births. This is consistent with the idea that famines and diseases had a negative impact on women's fertility. However, it supports the assertion that the effect of real wages on spacing reflects a choice rather than a biological effect, the latter being captured by the crude death rate.

Finally, we can see that birth order has a significant, negative effect on the hazard of a next birth, meaning that birth intervals increase with the birth order of the child.[19] This is wholly consistent with the fact that female fecundity declines with age (Baird et al., 2005).

3.4 Wheat Prices

The conclusions made above regarding the effect of living standards on birth spacing remain valid when measuring living standards by wheat prices rather than real wages. Using the same econometric approach as above, we find that rising wheat prices significantly reduce the hazard of a next birth, hence increasing the birth spacing intervals (Table 7). The fact that the rich have shorter birth spacing intervals than the poor is repeated in

[17] See Van Bavel (2004).

[18] The variable "Prenuptially conceived" is a binary variable which takes on a value of one if the difference between the marriage date and the date of the first born is less than 40 weeks, the average length of the gestation period.

[19] We experimented to see if there is any effect of child gender on the birth intervals, but this was never the case.

Table 7: Wheat prices and spacing

Dependent variable: Spacing in days	*Main effect (1)*	*Interaction terms (2)*
Wheat price	-0.059***	-0.056***
	(0.009)	(0.010)
Wealth group:		
Husbandmen	0.073***	0.081***
	(0.016)	(0.025)
Craftsmen	0.086***	0.111***
	(0.016)	(0.023)
Traders	0.184***	0.177***
	(0.022)	(0.032)
Farmers	0.223***	0.159***
	(0.023)	(0.033)
Merchant	0.206***	0.243***
	(0.022)	(0.038)
Gentry	0.304***	0.336***
	(0.037)	(0.054)
Unknown	0.069***	0.070***
	(0.013)	(0.020)
Interaction terms:		
Husbandmen × wheat price		-0.007
		(0.018)
Craftsmen × wheat price		-0.021
		(0.017)
Traders × wheat price		0.009
		(0.023)
Farmers × wheat price		0.058***
		(0.018)
Merchant × wheat price		-0.038
		(0.027)
Gentry × wheat price		-0.042
		(0.050)
Unknown × wheat price		0.000
		(0.011)
Control variables	Yes	Yes
Observations	225,312	225,312
Subjects	85,147	85,147

Note: Cox proportional hazard model with time-varying wheat prices. Wheat prices are standardized. Coefficients (semi-elasticities) reported. Estimates are stratified by parish and quarter century. Standard errors are clustered by the year of the demographic outcome. Laborers are the reference wealth group. *** p<0.01, ** p<0.05, * p<0.10. Source: Own estimates.

the present specification, i.e. the higher the socio-economic rank, the higher the hazard of a next birth.

Note that the interaction terms between the wheat price and the occupational categories reveal an interesting result: the "farmers" category responds to higher wheat prices by *expanding* their birth intervals. This suggests that farmers (unlike the other groups) benefitted from higher wheat prices, and that they adjusted their spacing strategy accordingly — a clear sign of deliberate birth regulation within marriage. The remaining covariates (not displayed in the sake of space) confirm the findings in Table 4 above when using real wages.

3.5 Sub-Periods

Does the effect of living standards on birth intervals change over time? Using our preferred measure for living standard, the real wages, Table 8 shows the results when we divide the full period into 50-year sub-periods. With the exception of the last period 1800–1850, the effect of the real wages on spacing is always significant. The largest effects occur between 1600 and 1800. Among the few studies finding evidence of preventive checks using aggregate data, Kelly and O Grada (2012) also conclude that the real wage coefficients are the largest between 1600 and 1800. The reason for this is likely to be found in Figure 3 (above): the periods between 1600 and 1800 are characterized by relatively low real wages when compared to the periods before and after. These conclusions show clearly how the English resorted to the use of preventive checks mainly during times of economic hardship.

Looking at the different socio-economic groups, it is interesting to note that up until 1650, only the middle and upper classes (traders, farmers, merchants, and gentry) differed significantly from the very poor (the laborers) in terms of spacing. But, as time passed, the lower socio-economic groups (craftsmen and husbandmen) also began to differ significantly, indicating that these groups became gradually more affluent relative to the very poor in the run up to 1850.

4 Panel Analysis

We can also estimate the effect of living standards on spacing using a panel structure, which allows us to deal more directly with family heterogeneity. This comes at a cost, in that we are unable to include covariates that remain constant over time (such as the occupational and educational information of the family).

We estimate a model with family-fixed effects defined as follows:

$$\text{spacing}_{ijt} = q_t + a_i + \beta_1 \text{realwage}_{j,t-\tau} + X_{ijt}g + \varepsilon_{ijt}. \tag{2}$$

The variable spacing is the birth interval (in days) for family i of a childbirth j in year t; q denotes a time-varying intercept; a includes unobserved family fixed effects; realwage is the real wage in year $t-\tau$ for childbirth j (common to all families); and finally X is a vector of other covariates, including the wife's age at each of her births, child birth order, and child mortality.[20]

Due to the time interval between conception and birth, we do not expect the real wage in year t to impact on the birth in year t. The descriptive statistics show that the average birth interval is roughly 2.5 years (Table 1). So the effect of living standards is likely to

[20] Similar to the duration analysis, we exclude the last birth interval from the analysis. The inclusion of the last birth interval does not qualitatively change our results.

Table 8: Spacing by sub-periods

Dependent variable *Spacing in days*	*1540–1599* *(1)*	*1600–1649* *(2)*	*1650–1699* *(3)*	*1700–1749* *(4)*	*1750–1799* *(5)*	*1800–1850* *(6)*
Real wage	0.089*	0.134**	0.164***	0.126***	0.205***	0.102
	(0.048)	(0.058)	(0.035)	(0.046)	(0.061)	(0.093)
Wealth group:						
Husbandmen	0.076	0.058	0.122***	0.071*	0.053*	0.098***
	(0.084)	(0.047)	(0.046)	(0.041)	(0.029)	(0.033)
Craftsmen	0.059	0.062	0.160***	0.110***	0.099***	0.051*
	(0.085)	(0.049)	(0.046)	(0.037)	(0.029)	(0.030)
Traders	0.106	0.146**	0.201***	0.217***	0.200***	0.220***
	(0.125)	(0.060)	(0.063)	(0.046)	(0.040)	(0.049)
Farmers	0.297***	0.161***	0.182**	0.075	0.236***	0.284***
	(0.092)	(0.062)	(0.092)	(0.069)	(0.038)	(0.043)
Merchant	0.200**	0.344***	0.424***	0.280***	0.144***	0.118**
	(0.097)	(0.062)	(0.071)	(0.054)	(0.034)	(0.058)
Gentry	0.509***	0.392***	0.351***	0.141	0.189***	0.360**
	(0.140)	(0.087)	(0.084)	(0.121)	(0.056)	(0.141)
Unknown	0.088	0.084*	0.098**	0.040	0.041*	0.112***
	(0.080)	(0.044)	(0.041)	(0.036)	(0.022)	(0.024)
Mother literate	0.075	-0.897*	0.889**	0.160	0.088***	0.044**
	(0.459)	(0.495)	(0.419)	(0.197)	(0.020)	(0.021)
Mother's literacy unknown	-0.048	-0.297	0.020	0.028	-0.003	0.006
	(0.270)	(0.275)	(0.292)	(0.164)	(0.026)	(0.031)
Time to first birth (years)	-0.028**	-0.048***	-0.059***	-0.059***	-0.052***	-0.057***
	(0.012)	(0.009)	(0.011)	(0.011)	(0.007)	(0.008)
Prenuptially conceived	0.077**	-0.024	0.012	0.021	-0.054***	-0.041*
	(0.033)	(0.020)	(0.026)	(0.025)	(0.017)	(0.021)
Child mortality at age (years):						
0–1	0.736***	0.887***	0.786***	0.686***	0.676***	0.698***
	(0.047)	(0.048)	(0.036)	(0.035)	(0.029)	(0.040)
1–3	0.232***	0.152***	0.193***	0.088**	0.186***	0.174***
	(0.077)	(0.037)	(0.041)	(0.038)	(0.028)	(0.049)
Unknown	0.016	-0.013	0.046**	0.022	0.028*	0.065***
	(0.031)	(0.024)	(0.020)	(0.021)	(0.015)	(0.025)
Birth order	-0.014*	-0.009	-0.012*	-0.014***	-0.012***	-0.004
	(0.008)	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)
Crude death rate	-0.007*	-0.004	0.000	-0.013***	-0.001	0.010
	(0.004)	(0.003)	(0.003)	(0.004)	(0.005)	(0.010)
Observations	17,746	30,979	27,928	41,204	64,647	42,781
Subjects	6,503	11,484	10,374	15,518	24,858	16,400

Note: Cox proportional hazard model with time-varying real wages. Real wages are standardized. Coefficients (semi-elasticities) reported. Estimates are stratified by parish and quarter century. Standard errors are clustered by the year of the demographic outcome. Laborers are the reference wealth group. *** p<0.01, ** p<0.05, * p<0.10. Source: Own estimates.

occur in the two years preceding the year of the birth. Thus, if sibling n is born in year t, we will estimate the effect of the average real wages of time $t+1$ and $t+2$ on the spacing between siblings n and $n+1$. For reasons of tractability, standard errors are clustered by the year of the firstborn, thus grouping all families that had their first delivery in the same year.[21]

4.1 Panel Results

Table 9 reports the estimates of equation 2 for the entire period (Column 1) and by sub-periods (Columns 2–7). Overall, the panel analysis provides the same results as the duration model: higher living standards reduce the birth spacing intervals. Note that the coefficients now express the change (in days) in the length of the birth interval. It thus follows that an increase of one standard deviation in the real wage decreases the average birth interval by 64 days (Column 1). Again, we find that child mortality drastically reduces the subsequent birth interval; that higher birth order increases birth spacing; and, finally, that the crude birth rate has a positive effect on spacing, suggesting once more that famine and disease had a negative impact on a couple's fertility.

Looking at the sub-periods (Columns 2–7), the pattern of the duration analysis is largely repeated: the effects are only significant in the middling period (here between 1650 and 1800) and insignificant (but still with the expected sign) before and after.

4.2 The causal effect of real wages on spacing

The existence of an omitted time-varying variable correlated with both real wages and birth spacing may bias our estimates and, therefore, question the causality of the effect. To overcome this potential bias we adopt an instrumental variable approach. That is, we identify exogenous variations in real wages using variation in monthly air temperature in the relevant years. The line of reasoning is that the air temperature (especially during certain seasons) affects the harvest outcome, which in turn influences food prices and, through the consumer price index, the real wage. The exclusion restriction is that the temperature affects the birth intervals only *indirectly*, i.e. through prices and wages.

For every year after 1659 we have monthly temperature readings for England, provided by the Hadley Centre Central England Temperature dataset (Manley, 1953, 1974) and Parker et al. (1992). The dataset offers the longest available series of monthly temperatures based on instrumental observations, and is widely used in climatology. We use the average monthly temperature by season (spring, summer, autumn, and winter) for the relevant year to identify variation in real wages.[22] Since our real wages are averages

[21] We are unable to cluster the standard errors by birth year as the panels (i.e. the families) are not nested within the clusters.

[22] Using monthly temperatures instead of averages by season does not change our results.

Table 9: Panel estimates by sub-periods

Dependent variable: Spacing in days	*1540–1850* *(1)*	*1540–1599* *(2)*	*1600–1649* *(3)*	*1650–1699* *(4)*	*1700–1749* *(5)*	*1750–1799* *(6)*	*1800–1850* *(7)*
Real wage (std)	-64.126***	-27.168	-35.573	-78.246***	-72.725***	-55.746**	-8.267
	(8.019)	(22.341)	(22.835)	(17.850)	(18.251)	(22.013)	(27.148)
Mother's age at birth (years):							
25–29	43.472***	30.365	79.369***	25.309	70.914***	47.210***	25.162**
	(5.636)	(35.604)	(18.956)	(22.591)	(16.021)	(9.150)	(9.589)
30–34	47.636***	75.827*	110.799***	19.472	93.832***	43.452***	16.561
	(7.511)	(44.847)	(25.213)	(30.062)	(19.775)	(14.490)	(15.919)
35–39	8.324	27.643	105.947***	-50.586	20.947	9.479	-6.024
	(9.515)	(74.619)	(32.811)	(33.202)	(27.208)	(18.797)	(20.829)
40–44	-57.091***	-93.572	6.881	-154.033**	16.423	-42.897	-100.874***
	(16.482)	(194.640)	(50.149)	(69.445)	(44.414)	(27.271)	(32.967)
45–	-187.490***			-197.532	-95.466	-148.778**	-261.388**
	(49.170)			(229.311)	(98.412)	(71.255)	(107.355)
Child mortality at age (years):							
0–1	-213.512***	-242.346***	-257.507***	-210.156***	-197.103***	-189.908***	-200.605***
	(4.157)	(11.930)	(9.119)	(9.419)	(8.557)	(7.867)	(10.509)
1–3	-35.326***	-47.744***	-32.298***	-32.177***	-24.582**	-43.637***	-32.837**
	(4.446)	(17.465)	(10.850)	(11.619)	(9.370)	(7.152)	(13.864)
Unknown	-9.506***	-14.577*	-6.959	1.263	-1.936	-6.619	-26.846***
	(2.782)	(8.550)	(7.123)	(6.534)	(5.952)	(5.283)	(8.490)
Birth order	230.793***	273.763***	238.086***	238.977***	214.826***	229.958***	251.988***
	(3.606)	(12.276)	(10.931)	(10.032)	(8.236)	(7.086)	(10.065)
Crude death rate	2.781***	2.464	1.567	3.040*	4.068***	4.321*	-6.574*
	(0.663)	(1.738)	(1.336)	(1.550)	(1.201)	(2.434)	(3.687)
Time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	137,032	12,290	20,047	21,859	25,364	33,368	24,069
Number of groups	41,866	4,406	7,045	7,703	8,591	10,410	7,392

Note: Family fixed effects. Robust standard errors in parenthesis. Standard errors are clustered by the year of the first childbirth. *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Source: Own estimates.

of the two years preceding childbirth, we use average seasonal temperatures of the same two years.

The instrumental variable estimates are shown in Table 10 (column 2), with corresponding standard panel estimates for comparison (column 1). The first aspect to note is the strong partial correlation of the seasonal average temperatures with the real wages (Column 2, upper panel). The first stage F-statistic is reassuringly high (bottom of Table 10). We find that an increase of the real wage by one-standard deviation causes a reduction of the birth spacing interval by about two months. The instrumental variable estimate is remarkably similar to the standard fixed-effect estimate, suggesting an absence of omitted variable bias.[23]

Average temperatures are also a plausible source of variation for wheat prices. Hence, we can adopt the same instrumental variable approach when using wheat prices as an indicator of the standard of living. The results are presented in Table 11. In this case the first stage estimates (upper panel) also show a strong correlation between average seasonal temperatures and wheat prices. The instrumental variable estimate (Column 2) is larger when compared to the standard panel estimate (Column 1). In this case, an increase of the wheat price by one-standard deviation causes a delay of the next childbirth by roughly 30 days. The quantitative conclusions from the analysis above thus remain intact.

5 Robustness Checks

In the previous section we have shown that the negative effect of living standards on birth spacing has a causal interpretation. Throughout the paper we have also provided evidence suggesting that the effect is the result of behavior rather than biology (i.e. undernourishment causing amenorrhea and hence infertility).[24] We can stress this point further by excluding from the sample those years in which the living standards were exceptionally low, i.e. years in which the biological mechanism may have manifested itself, such as during the great famine of 1727–28.[25]

To this end we re-estimate equation 2 excluding the years in which (*i*) the real wages are below the 10^{th} percentile; (*ii*) the wheat prices are above the 90^{th} percentile; and (*iii*) the crude death rates are above the 90^{th} percentile. Moreover, to ensure that we exclude the peaks of extremely low living standards, we focus on the period 1600–1800, characterized by the absence of long-term trends (see Figure 4).[26] As can be seen in Table

[23] Reverse causality should also not be an issue in our models.

[24] Amenorrhea is the temporary absence of menstruation among otherwise fertile women of average reproductive age (15 to 50) and has been demonstrated to result from physical stress, malnutrition, eating disorders and extreme weight losses.

[25] See Klemp and Weisdorf (2012).

[26] This is also the period during which we find the strongest preventive checks. Using the full time-period, however, does not change the direction of our results.

Table 10: The causal effect of real wages on spacing — Instrumental variable estimates

	Panel (1)	*Panel IV* (2)
Dependent variable: *Real wages (standardized)*		*First stage*
Average temperature:		
Spring		0.043*** (0.008)
Summer		0.043*** (0.012)
Autumn		-0.013 (0.010)
Winter		0.059*** (0.006)

	Panel (1)	*Panel IV* (2)
Dependent variable: *Spacing in days*		*Second stage*
Real wage (std)	-59.802*** (9.183)	-63.609*** (23.326)
Mother's age at birth (years):		
25–29	41.031*** (6.004)	41.053*** (6.007)
30–34	41.524*** (8.191)	41.568*** (8.211)
35–39	0.401 (10.715)	0.461 (10.755)
40–44	-62.042*** (17.832)	-61.941*** (17.895)
45–	-186.628*** (49.659)	-186.453*** (49.629)
Child mortality at age (years):		
0–1	-196.578*** (4.530)	-196.564*** (4.519)
1–3	-33.438*** (5.220)	-33.431*** (5.209)
Unknown	-7.393** (3.250)	-7.389** (3.244)
Birth order	225.244*** (3.859)	225.231*** (3.857)
Crude death rate	2.928*** (0.863)	2.877*** (0.897)
Time trend	Yes	Yes
Observations	102,026	102,026
Number of groups	30,626	30,626
1^{st} stage F		54

Note: Family fixed effects. Robust standard errors in parenthesis. Standard errors are clustered by the year of the first childbirth. Real wages are instrumented with average seasonal air temperatures. *** p<0.01, ** p<0.05, * p<0.10. Source: Own estimates.

Table 11: The causal effect of wheat prices on spacing — Instrumental variable estimates

	Panel *(1)*	*Panel IV* *(2)*
Dependent variable: *Wheat prices (standardized)*		*First stage*
Average temperature:		
Spring		-0.076*** (0.021)
Summer		-0.155*** (0.026)
Autumn		0.077*** (0.021)
Winter		-0.091*** (0.013)
Dependent variable: *Spacing in days*		*Second stage*
Wheat price (std)	19.490*** (3.739)	30.500** (11.960)
Mother's age at birth (years):		
25–29	40.102*** (6.024)	39.777*** (5.941)
30–34	39.719*** (8.263)	39.086*** (8.133)
35–39	-2.299 (10.873)	-3.285 (10.685)
40–44	-65.303*** (17.960)	-66.245*** (17.668)
45–	-189.997*** (49.160)	-190.345*** (48.594)
Child mortality at age (years):		
0–1	-196.655*** (4.518)	-196.568*** (4.504)
1–2	-33.515*** (5.210)	-33.501*** (5.196)
Unknown	-7.385** (3.248)	-7.341** (3.241)
Birth order	225.288*** (3.854)	225.199*** (3.846)
Crude death rate	3.391*** (0.862)	3.200*** (0.862)
Time trend	Yes	Yes
Observations	102,026	102,026
Number of groups	22,831	22,831
1^{st} stage F		50

Note: Family fixed effects. Robust standard errors in parenthesis. Standard errors are clustered by the year of the first childbirth. Wheat prices are instrumented with average seasonal air temperatures. *** p<0.01, ** p<0.05, * p<0.10. Source: Own estimates.

Table 12: Spacing behavior

Dependent variable: Spacing in days	*Excluding years of low wages (1)*	*Excluding years of high wheat prices (2)*	*Excluding years of high mortality rates (3)*
Real wage (std)	-57.086***	-74.491***	-69.235***
	(12.198)	(9.678)	(9.422)
Mother's age at birth	Yes	Yes	Yes
Child mortality	Yes	Yes	Yes
Birth order	Yes	Yes	Yes
Time trend	Yes	Yes	Yes
Crude death rate	Yes	Yes	Yes
Observations	88,456	88,251	98,017
Number of groups	30,101	30,182	31,076

Note: Family fixed effects. Robust standard errors in parenthesis. Standard errors are clustered by the year of the first childbirth. *** p<0.01, ** p<0.05, * p<0.10. Source: Own estimates.

12, the effects on birth spacing remain significant and negative, even after the exclusion of years of very low living standards.

Alternatively, we can compare the effect of real wages on spacing in "good" and "bad" years, for "poor" and "very rich" families, respectively. The "good" years are those in which real wages are above the long-run median (vice versa for "bad" years). The "poor" families are laborers and husbandmen, while the "very rich" families include only merchants and gentry.[27] The results are reported in Table 13, looking again at the period 1600 to 1800. Both very rich and poor families adjusted their spacing behavior during bad years (columns 1 and 3). In those years, a decrease in the real wage by one-standard deviation increases the birth spacing interval by 86 days for the very rich and 102 days for the poor. We cannot entirely rule out that this was a biological mechanism in the case of the poor. However, because the very rich were unlikely to suffer from starvation, even during bad years, the delay strongly indicates a behavioral mechanism for this group. When turning to the good years, the coefficient for the very rich group becomes insignificant (Column 2), while even during prosperous years, the poor still respond to falling real wages by significantly increasing their birth spacing (Column 4).

6 Conclusion

Britain was the first nation to escape the Malthusian trap and enter into the current regime of modern economic growth. The relatively late age at marriage, as well as the high share of unmarried people, has long been attributed as the main reason for Britain's low

[27] Including also craftsmen, farmers, and traders among the rich (see Table 6) provides virtually the same results.

Table 13: Spacing behavior of very rich and poor in good and bad years

Dependent variable:	*Very rich*		*Poor*	
Spacing in days	*Bad years* (1)	*Good years* (2)	*Bad years* (3)	*Good years* (4)
Real wage (std)	-85.723*	39.136	-101.932***	-102.387*
	(47.239)	(80.399)	(33.596)	(57.105)
Mother's age at birth	Yes	Yes	Yes	Yes
Child mortality	Yes	Yes	Yes	Yes
Birth order	Yes	Yes	Yes	Yes
Time trend	Yes	Yes	Yes	Yes
Crude death rate	Yes	Yes	Yes	Yes
Observations	5,202	3,433	12,102	8,293
Number of groups	1,983	1,597	5,106	3,996

Note: Family fixed effects. Robust standard errors in parenthesis. Standard errors are clustered by the year of the first childbirth. Real wages are instrumented with average seasonal air temperatures. *** p<0.01, ** p<0.05, * p<0.10. Very rich are merchants and gentry; poor are laborers and husbandmen. Good (bad) years are those in which the real wage is above (below) the long-run median. Source: Own estimates.

population-pressure, high-wage economy, and its early transition to sustained economic growth (Voigtländer and Voth, 2011).

It has also long been thought that within-marriage birth limitation behavior was absent in pre-industrial England, and that it only emerged at the end of the nineteenth century, when the fertility transition swept across Western Europe. Previous research investigating the short-term response of aggregate demographic variables (i.e. crude marriage and birth rates) to changing living standard has been largely unsuccessful in demonstrating that this kind of Malthusian preventive check operated in pre-industrial England. Moving the issue of preventive checks "to the bedroom', we provide ample evidence that such checks existed in the three centuries leading up to England's fertility transition.

Specifically, we find that falling real wages not only increased the age at first marriage among women (as is generally assumed to have been the case) but also that this extended the time-interval between family births. The preventive checks are especially strong between 1600 and 1800, a period characterized by relatively low and stagnant real wages, but they seem to vanish when wages rise. In terms of magnitude, we find that an increase in the real wage by one-standard deviation decreased the birth spacing interval by roughly two months during the seventeenth and eighteenth centuries.

Our results are robust to different estimation methods, including duration and panel models. Instrumenting changes in living standards by variation in monthly air temperatures, we also find that the effect has a causal interpretation. Although we cannot entirely rule out the possibility that a biological mechanism was at play, with undernourishment leading to infertility and hence extended birth spacing among the poor, the fact that falling real wages exercised a negative effect on the spacing of births among the rich makes it likely that delayed births signifies economically rational behavior. Alternative specifications and several robustness checks support this assertion.

The presence of preventive checks in pre-industrial England, both in the form of late age at marriage and of extended birth intervals, helps explain England's leading position as a low population-pressure, high-wage economy, and hence its primacy in the transition from a Malthusian to a post-Malthusian regime.

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Fecundity, Fertility and Family Reconstitution Data: The Child Quantity-Quality Trade-Off Revisited*

With Jacob Weisdorf

Abstract Growth theorists have recently argued that western nations grew rich by parents substituting child quantity (number of births) for child quality (education). Using family reconstitution data from historical England, we explore the causal link between family size and human capital of offspring measured by their literacy status and professional skills. We use a proxy of marital fecundity to instrument family size, finding that children of couples of low fecundity (and hence small families) were more likely to become literate and employed in a skilled profession than those born to couples of high fecundity (and hence large families). Robust to a variety of specifications, our findings are unusually supportive of the notion of a child quantity-quality trade-off, suggesting this could well have played a key role for the wealth of nations.

Keywords Child Quantity-Quality Trade-off, Demographic Transition, Industrial Revolution, Instrumental Variable Analysis, Human Capital Formation

JEL Classification Codes J13, N3, O10.

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1 Introduction

Recent years have seen a rising interest in how western nations grew rich. Growth theorists speculate that technological progress raised the incentive to invest in the education of offspring, and that this investment was financed by a reduced number of births, both of which led to economic growth (Galor, 2011). Despite considerable theoretical attention, there have been very few attempts to test the child quantity-quality trade-off historically.[1] Becker et al. (2010) have used Prussian data and landownership inequality to instrument education, finding a negative effect of education on family size in 1816. Bleakley and Lange (2009) have argued that the eradication of the hookworm disease in South America brought about a fall in the price of education, showing that this was associated with declining fertility in 1910. Fernihough (2011) has used Irish data and instrumented family size by multiple births, finding a negative effect of family size on school enrolment in 1911. However, to date, the trade-off has not been investigated for the world's first economy to undergo the transition from economic stagnation to sustained growth: historical England.

In this paper we use 18$^{\text{th}}$–19$^{\text{th}}$-century family reconstitution data from English parish records to study the effect of family size on the human capital of offspring, advancing the research frontier along three dimensions. First, our data cover an exceptionally long period of time, over 130 years, during which England underwent the Industrial Revolution (Clark, 2007; Galor, 2011). Although public education was not yet widespread during this period, we are able to use literacy information (derived from signatures on marriage certificates), as well as professional skills (derived from occupational titles), to measure individual human capital achievements. Second, and unusually for historical records, the family reconstitution data provide statistics at the micro level, enabling us to explore the effect of the number of siblings on the siblings' human capital while controlling for a large variety of family characteristics, including parental human capital, longevity and social class. Third, we introduce a new identification strategy in the context of testing the child quantity-quality trade-off hypothesis, using marital fecundity to instrument family size. In societies where marriage marks the onset of unprotected sex, such as in many present-day Muslim countries as well as historical western societies, marital fecundity is estimated by demographers using the waiting time from a couple's marriage date to their first birth. The great variability in fecundity as well as fertility among our sampled couples makes the family reconstitution data particularly appropriate for testing the child quantity-quality trade-off in historical England.

Our data show that children of parents of low fecundity (and hence few siblings) were significantly more likely to become literate and find employment among the skilled professions than those of parents of high fecundity (and hence many siblings). Specifically,

[1] Recent attempts to measure the trade-off effects using contemporary data include Angrist et al. (2010); Black et al. (2005); Caceres (2006); Li et al. (2008); and Rosenzweig and Zhang (2009). Support for the trade-off is normally found among developing countries but far less so among developed countries.

we find that an extra sibling reduced the probability of acquiring a skilled profession by 7.5 percentage points and the probability of being literate by 6.7 percentage points. Since double-digit sibship sizes were rather common in historical England, the chances of achieving literacy and employment in a skilled profession were cut dramatically among those born to larger families. Our identifying assumption is that the waiting time from a couple's marriage date to their first birth is not deliberately influenced by the couple. Nor must the waiting time be correlated with any of the socio-economic characteristics of the couple that are correlated with the human capital of their offspring. To validate our empirical strategy we show that family characteristics, such as parental human capital, longevity and social class, have no significant influence on the waiting times of the sampled couples. We also assess the validity of the exclusion restriction by comparing the waiting time of the sampled couples to those of Muslim couples in rural Palestine, among whom marriage marks the onset of unprotected sex, finding that the rates of fecundability are virtually identical in the two samples. Our results are robust to excluding extreme outliers; including potentially unobserved births; treating issues of censoring due to migration; and using an alternative specification of marital fecundity. Our findings are unusually supportive of the child quantity-quality trade-of hypothesis and thus of *unified growth theory* in which the trade-off is key in understanding the emergence of the wealth of nations (Galor and Weil, 2000; Galor and Moav, 2002).

2 Data and Data Limitation

The family reconstitution data used for the analysis below come from Anglican parish registers (English church books). The data were transcribed by the *Cambridge Group for the History of Population and Social Structure* and is documented by Wrigley et al. (1997). The family reconstitutions are based on ecclesiastical events recorded in a total of 26 English parishes (Figure 1). The full data set covers more than three centuries of English demographic history, from the first emergence of parish registration, in 1541, until population census became common in 1871. The subsample most relevant for our purpose, however, comes mainly from the 18^{th} and early 19^{th} centuries. The sampled parishes were selected by the Cambridge Group due to the high quality of the data and with the intention of making them representative of the entire country. The parishes range from market towns to remote rural villages, including proto-industrial, retail-handicraft, and agricultural communities, and have been organised by Schofield (2005) into four groups: "agriculture", "industry", "retail and handicraft" and "other" (a mix), enabling us to control for their occupational structure in the analysis below.

Each family in the reconstitution data is built around a marriage, including information about the birth, marriage, and death dates of the spouses, as well as the gender and birth and death dates of their offspring. For certain periods (mostly after 1700) the

Figure 1: Locations of the parishes (source: Schofield (2005))

records also contain the literacy status of the spouses (literate/illiterate) as well as the father's occupational title. We explore the information hidden in the occupational titles with regards to the working skills of individuals and their income potential. First, looking at pre-modern wills from London and South-East Anglia, Clark and Cummins (2010) have classified the recorded occupations according to the information regarding wealth that is given in the wills. From poorest to richest these are: labourers, husbandmen, craftsmen, traders, farmers, merchants, and gentry. By grouping labourers and husbandmen together we are able to separate in our data the poorest from the more affluent segments of English society. Second, using the so-called HISCO/HISCLASS schemes, documented by Leeuwen et al. (2007) and Leeuwen and Maas (2011), we sub-divide the sampled individuals into two groups – skilled and unskilled workers – depending on the educational training needed to conduct the work described by the occupational title.[2] To this end, we employ a standard two-step procedure. First, we assign the occupational title a five-digit code using

[2]The HISCO system is a historical extension of the ISCO (International Standard Classification of Occupations) for which the ILO (International Labour Organization) is responsible. The HISCLASS system is a historical extension of the DOT (Dictionary of Occupations) system, which gives scores for the skill-content for a wide range of occupations, originally created in the 20$^{\text{th}}$ century by the US Employment Service to match job seekers to jobs.

the HISCO system. Next, we enter the code into the HISCLASS system, which classifies the professional skills of an individual using a two-dimensional scheme quantifying the academic and vocational training needed to conduct the work. For example, according to the HISCO scheme, an English factory worker would be classified as code number 99930, which according to the HISCLASS scheme designates an "unskilled" profession.[3]

Using the earliest recorded occupations of the sampled individuals (and a binary variable in the case of missing occupations) we map over one hundred distinct occupational titles in the data into skilled and unskilled professions by means of the procedure described above. Some 89% of the occupations are derived from marital records or the earliest ecclesiastical event thereafter (typically the baptism of firstborns). Around 7% of the occupations stem from the time of the burial. The remaining occupations (about 4%) are from an intermediate point in time, i.e. the time of the baptism (or burial) of offspring of parity two or above. The titles "Paupers" and "Gentry" were excluded from the sample.[4]

Table 1 provides an example of a reconstituted family. It includes the statistics transcribed from the church book as well as those inferred either by us or by the Cambridge Group.[5] The records almost always report the baptism and burial dates rather than the birth and death dates. Where available, we always use the latter (i.e. in 87% of the cases). Meanwhile, the time intervals between the ecclesiastical and the vital events were rather short. For obvious reasons people were buried as soon as possible after death, usually within three days (Schofield, 1970). Furthermore, almost all children were baptized within one month of birth (Midi Berry and Schofield, 1971). To allow for the period of time between birth and baptism, we subtract three weeks from all baptism dates.[6] We refer to the combined birth/baptism and death/burial dates as birth and death dates. Interestingly, although the Prayer Books of the English Church prescribed that baptisms take place on Sundays, not all families would comply with this rule. The baptism fees paid varied according to family income, and often it was the rich who paid the church for a non-Sunday baptism service, a fact that becomes apparent in the analysis later on.

[3]The occupational titles of our sample were coded using http://historyofwork.iisg.nl/.

[4]Our findings are robust in their inclusion on the assumption that paupers are unskilled and gentry skilled.

[5]The record shows that in Odiham on 15 Oct. 1761 Edward Neville (baptized 14 May 1733, buried 3 Nov. 1816 at age 83) married Hannah Sury (baptized 21 July 1740, buried 10 Nov. 1816 at age 76). At the time of the marriage, husband Edward was registered in the church book as a labourer, which according to the HISCLASS is an unskilled occupation. He was recorded as being illiterate, as was his wife. Wife Hannah gave birth to a total of nine children (seven boys and two girls), two of which (Thomas and Francis) died before reaching the age of five, leaving a total of seven "surviving" children. Six of the seven survivors married in their parish of birth. James (a labourer) was unskilled, while Edward (a baker), John and Thos (both sawyers) were skilled workers. The record also shows that Edward was literate but that his siblings were all illiterate, except for lastborn Hannah who at some stage during her life moved away to a parish outside the sample (indicated by her missing death date) rendering her marriage and literacy status unknown.

[6]Our results are robust to different specifications.

Table 1: Example family

PARISH LEVEL INFORMATION

Parish	Occupational Type
Odiham	Mixed

FAMILY LEVEL INFORMATION

TTFB	TTSB
0.92	3.45

INDIVIDUAL LEVEL INFORMATION

Family Member	Name	Birth Date	Death Date	Age at Death	Occupation	Skilled Profession	Literate	Marriage Age
Mother	Hanna Sury	21 July 1740	10 Nov. 1816	76.3	-	-	No	21.2
Father	Edward Neville	14 May 1773	3 Nov. 1816	83.5	Labourer	No	No	28.4
Daughter	Ann	8 Oct. 1762	-	-	-	-	No	
Son	John	17 Apr. 1765	13 Oct. 1850	85.5	Sawyer	Yes	No	
Son	Edward	3 Mar. 1767	8 May 1852	85.2	Baker	Yes	Yes	
Son	James	3 May 1769	14 Apr.1849	79.9	Labourer	No	No	
Son	Thomas	6 Mar. 1771	20 Mar. 1771	0.0	-	-	-	
Son	Daved	28 Mar. 1773	13 May 1858	85.1	-	-	No	
Son	Thos	23 Apr. 1775	21 Dec. 1855	80.7	Sawyer	Yes	No	
Son	Francis	8 June 1777	9 May 1780	2.9	-	-	-	
Daughter	Hannah	5 Dec. 1779	-	-	-	-	-	

A "-" indicates missing information. TTFB is the time from the marriage to the first birth, measured in years. TTSB is the time from the marriage to the second birth, measured in years.

Table 2: Summary statistics

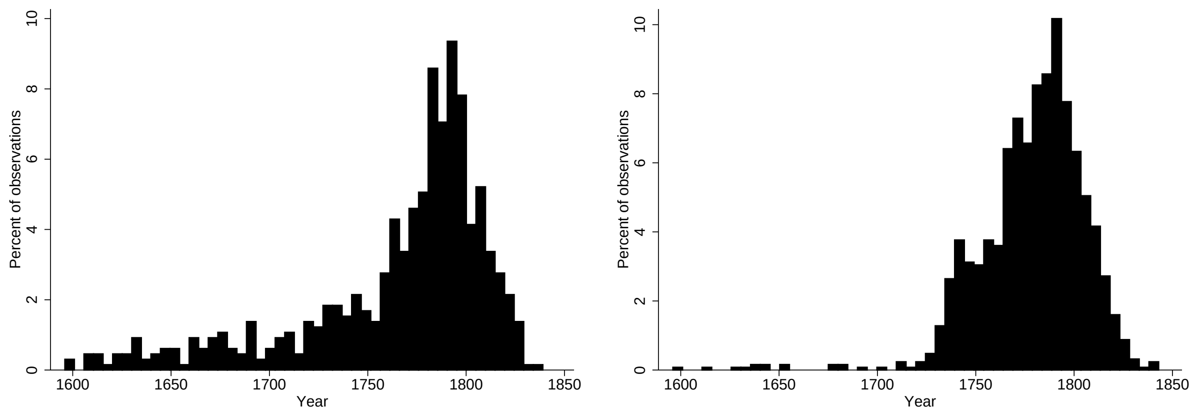
	Mean	SD	Count	P10	P90
Sibship Size	6.96	2.94	1508	3	10
Surviving Siblings (> 5 Years)	4.83	2.51	1508	2	8
Literate	0.56	0.50	1,248	0	1
Skilled	0.68	0.47	652	0	1
TTFB	1.59	1.18	1508	0.81	2.99
TTSB	3.84	1.66	1481	2.37	5.89
Male	0.53	0.50	1508	0	1
Non-Sunday Baptism	0.53	0.50	1476	0	1
Skilled Father	0.69	0.46	918	0	1
Poor Father	0.56	0.50	960	0	1
Skilled Mother	0.63	0.49	35	0	1
Literate Father	0.60	0.49	969	0	1
Literate Mother	0.32	0.47	942	0	1
Longevity of Mother (Years)	71.28	10.36	1508	57.2	84.4
Longevity of Father (Years)	72.38	9.75	1508	59.0	84.1
Age at Marriage of Mother (Years)	25.07	4.67	1508	19.8	31.0
Retail-Handicrafts Location	0.16	0.36	1508	0	1
Industrial Location	0.24	0.43	1508	0	1
Agricultural Location	0.25	0.43	1508	0	1
Mixed Activity Location	0.35	0.48	1508	0	1
Centuries since 1500	2.72	0.37	1508	2.34	3.07
Number of Observations	1508				

Note that the sample is restricted to couples from completed marriages (see text), which explains the high longevity of the spouses. TTFB is the time from the marriage to the first birth, measured in years. TTSB is the time from the marriage to the second birth, measured in years.

Following the procedure used in demography, we exclude the couples unable to accomplish their desired family size because of divorce or premature death (i.e. death before the wife completes her reproductive period). In other words, we restrict the sampled couples to those with *completed* marriages, meaning that the wife survived in marriage until the age of 50 (Wrigley et al., 1997, p. 359). Since we compute the wife's age using her birth and death dates, and because a missing birth or death date imply migration in or out of the sampled parishes (Souden, 1984), a completed marriage automatically exclude the possibility that the wife had children from an unobserved marriage (i.e. outside the sampled parishes). For similar reasons, we exclude from our sample husbands of missing birth and death dates.

Our data limitations leave us with two main samples. One includes the offspring about which we know their occupation and thus their skill status (652 individuals from 453 families), and the other includes the offspring whose literacy status is available (1,248 individuals from 571 families). The literacy and skill status are jointly known in one-third of all cases (392 individuals from 280 families).[7] The combined sample includes

[7]Note that skilled workers were not always literate. A simple linear regression, clustered at the family level, of working skills on literacy using the subset of overlaps ($N = 392$) yields a slope coefficient of 0.382 ($p < 0.000$).



(a) Number of observations with skill status

(b) Number of observations with literacy status

Figure 2: Histograms of numbers of observations per sample

1,508 individuals from 721 families, and the summary statistics of these individuals are presented in Table 2. Figure 2 show the distributions of observations across time in the two main samples. Of the sampled individuals, 90% were born between 1690 and 1814, comprising the years of England's Industrial Revolution.

3 Empirical Strategy

Our aim is to quantify a child quantity-quality trade-off effect based on the sampled couple. To this end, we need knowledge about the family size and the human capital of the offspring, and how the former influence the latter. Because of potential issues of endogeniety (discussed below), we will adopt an instrumental variable approach, using a proxy of marital fecundity to instrument the number of family offspring. To see why this is a sensible strategy, we begin by describing the key differences between historical and contemporary family planning in England.

Historical families were rather large by today's standard. The fertility rate in the UK is currently two children per woman, while in the 18^{th} century it was close to five (Wrigley et al., 1997). Although child mortality was rather high in the 18^{th} century, three to four children per women nevertheless made it to adulthood (ibid.). The family planning of the 18^{th} century was also rather different from that of contemporary England. First and foremost, births outside marriage were a highly immoral act in the eyes of the English Church and society as a whole, making the postponement of marriage a key form of contraceptive in the past (Cinnirella et al., 2012; Wrigley et al., 1997). Another major difference is that women, once they were married, continued to have children until the menopause set in, which usually happened at around age 40 (ibid.). Figure 3 captures the main implications of these features, showing how the average number of family births

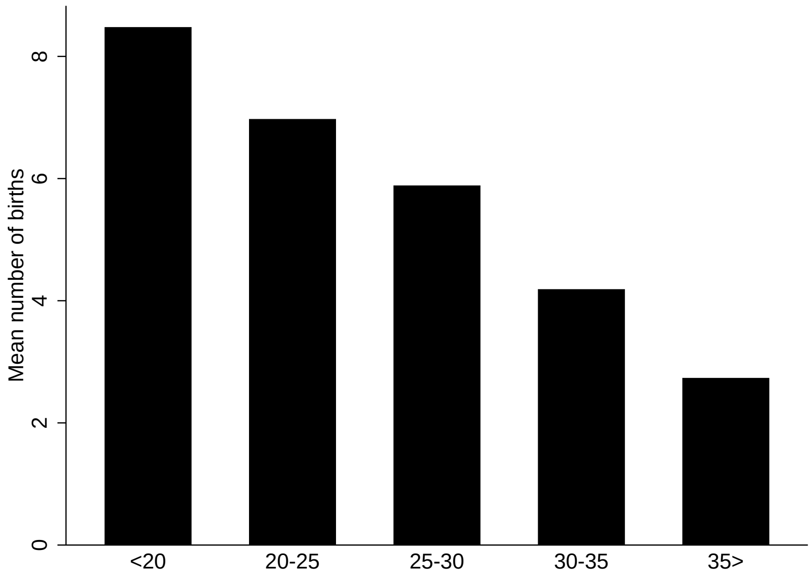


Figure 3: Family size by the wife's age at marriage

decreased with the wife's age at marriage. Birth control was also practised *within* marriage. Using an extended sample of our data, Cinnirella et al. (2012) have found that historical couples responded to lower living standards (measured by real wages and wheat prices) by increasing their birth-spacing intervals, achieved through sexual abstinence, coitus interruptus, and extended breastfeeding (McLaren, 1978; Santow, 1995). The fact that couples were able to control the size of their family this way, raises a number of issues regarding endogeneity, which we discuss in detail in the following sections.

3.1 The Quantity-Quality Trade-Off and Issues of Endogeneity

A test of the child quantity-quality trade-off hypothesis is not straightforward to conduct. Certain factors influencing family size may also affect the human capital formation of the offspring. Income is one example. For instance, evidence suggests that the rich gave birth to more children than the poor (Boberg-Fazlic et al., 2011; Clark and Hamilton, 2006), but also that the rich invested more heavily in the education of their offspring than their less affluent counterparts (Leunig et al., 2011). Factors such as parental human capital and morbidity are also likely to affect both the quantity and quality of offspring. By excluding variables like parental income, education and morbidity, an estimated OLS effect of family size on human capital will tend to be biased upwards. Likewise, some determining factors may be difficult to fully observe or quantify. In such cases, the estimated OLS effect would potentially suffer from omitted variable bias. By inferring information about family income from occupational titles we can capture some of the variation in income among the sampled couples. Similarly, we can capture some of the variation in parental human capital and morbidity by controlling for the education, literacy, and longevity of parents.

But to fully treat the issues of endogeneity we have to adopt an instrumental variable approach.

3.2 Fecundity as an Instrument for Fertility

To this end, we use a novel identification strategy in the context of the child-quantity-quality trade-off literature, exploring the waiting times from a couple's marriage to their first birth to instrument the couple's fertility. To begin with, note that a couple's *fecundity* refers to their reproductive potential while their *fertility* captures the fulfilment of this potential, i.e. their actual number of births (Gini, 1924). Also, a couple's *effective fecundability* measures the probability of conception within one month (or one menstrual cycle) among a non-contraceptive, non-sterile, and sexually active couple, leading to a live birth.

Demographers often use the waiting time from the marriage to the first birth to estimate the mean effective fecundability of a population in societies where marriage marks the onset of unprotected sex.[8] Here, instead, we exploit the information at the micro level as explained in the following. If there was full homogeneity in the fecundability of a given population, then the waiting time from the marriage to the first birth among couples would follow a geometric distribution: some parents would fall pregnant in their first cycle, others only after several cycles. However, since fecundability in reality varies among individuals, the actual distribution will have a fatter tail than that predicted by the geometric distribution, with a higher representation of low-fecundity individuals among those with long waiting time. Therefore, among a non-contraceptive, non-sterile, and sexually active couple, our instrumental variable (the waiting time) thus captures not only the random variation in the waiting time from marriage to first birth, but also the variation in the couples' fecundity.

Can an individual choose a partner so as to determine his or her own fecundity? The answer is: not entirely. It is clear that information about the fecundity of an individual is partly inferable from that of other family members. Based on this information, a couple *in spe* can to some extent approximate their marital fecundity in advance of the marriage. But the mix of the genetic material of two non-related individuals introduces a random component regarding their potential joint fecundity. Their actual joint fecundity will not be known until after the decision to start a family is made and the firstborn is delivered. Moreover, the couples of low fecundity will have longer birth-spacing intervals, and reach sterility earlier, than couples of high fecundity, meaning that the random component of the joint fecundity persists.

In historical times when birth continued until sterility set in, it is clear that highly fecund couples, realizing they may end up with more children than expected, could at-

[8]E.g. Bongaarts (1975); Gini (1924); Olsen and Andersen (1999); and Woods (1994).

tempt to adjust for this by extending their birth-spacing intervals. However, since the spacing of births was relatively short in the first place (i.e. slightly more than two years, according to Cinnirella et al. (2012)), there are limits to how much low-fecundity couples could cut their spacing of subsequent births in order to reach a target. Giving birth to fewer children than expected, a couple of low fecundity could thus afford to allocate more resources to their offspring by comparison to more fecund couples, capturing in this way the main principles behind the use of marital fecundity as an instrument for fertility.

Below we exploit the idea that the waiting time between marriage and the first birth (henceforth the TTFB) may be correlated with family size.[9] We already know, from a duration analysis made on over a quarter million births using an extended sample of data (Cinnirella et al., 2012), that the TTFB is positively correlated with the birth-spacing intervals of subsequent births. Cinnirella et al. (2012) have also found that the TTFB was not influenced either by changes in real wages or food prices. Instead, the sampled couples were found to control the timing of the first birth by adjusting the timing of their marriage to changes in real wages or food prices.

We can demonstrate, analytically as well as empirically, that the length of the TTFB negatively affects the number of family births among our sampled couples, i.e. among couples of completed marriages. We begin by demonstrating the relationship formally, and then turn to an empirical demonstration. Let f denote the fertile period of a married couple, i.e. the time-period spanned by the marriage date and the date when sterility sets in. The remaining fertile period after a couple's first birth is $f - t$, where t represents the TTFB. In this period the total number of births is determined by the average frequency of births, which is inversely related to the average birth-spacing interval, denoted $s(t)$. If x denotes the total number of births, then $x = (f - t)/s(t) + 1$. We can approximate the average birth-spacing interval as a linear function of t, so that $s(t) = c + \lambda t$ where c and λ are constants, hence obtaining the expression $x = (f - t)/(c + \lambda t) + 1$. Linearizing this expression around the average TTFB, denoted by $\bar{t}$, means that $x \approx \gamma_0 - \gamma_1 t$, where $\gamma_0 \equiv (f - \bar{t})/(c + \lambda \bar{t}) + \bar{t}(c + \lambda f)/(c + \lambda \bar{t})^2$ and $\gamma_1 \equiv (c + \lambda f)/(c + \lambda \bar{t})^2$.

A lower bound of the point in time when sterility set in is given by the couple's final delivery. Using this to proxy for the actual time of sterility, we obtain on the basis of our sample an estimate of the mean fertile period (i.e. the average period from marriage to sterility), which is $\bar{f} = 16.16$ years. Similarly, we can estimate the mean TTFB which is $\bar{t} = 1.59$ years (cf. Table 2). A simple regression of the length of birth-spacing intervals on the TTFB, with standard errors clustered at the family level, yields $\bar{c} = 2.48$ ($p < 0.000$) and $\bar{\lambda} = 0.08$ ($p = 0.010$). These numbers imply that $\bar{\gamma}_0 = 6.47$ and $\bar{\gamma}_1 = 0.56$. Hence,

[9]The abbreviation "TTFB" is short for the time to the first birth.

an increase in the TTFB by one year on average decreases the number of births in a completed marriage by roughly half a child.[10]

3.3 The Exclusion Restriction

The exclusion restriction applies only if the couple do not control the length of the TTFB. However, historical couples may in fact have had incentives to deliberately postpone their first births. For instance, a poor couple could have made an effort to delay their first pregnancy in an attempt to reduce the number of births in order to be able to afford to educate their offspring. Other social groups, such as literate or skilled couples, could have pursued the same strategy for similar reasons. Meanwhile, we will demonstrate that there is no evidence in the data of such behaviour. To this end, we first show that there are no socio-economic variables available in the data that are significantly correlated with the TTFB. Next, we compare the distribution of the TTFBs in our sample with that from a sample of contemporary Muslim couples, who we know do not delay their waiting time, showing that the two distributions are practically identical.

3.3.1 Socio-Economic Determinants of the TTFB

An assessment of the validity of the exclusion restriction comes from regressing the TTFB on the socio-economic characteristics of the couple, to see if the TTFB is influenced by such traits. While the conditional exclusion restriction cannot be formally tested this way, we can nevertheless assess the possibility of excluding certain determinants by investigating the degree to which our instrument is correlated with our key explanatory variables. In this case we do not require any knowledge about the human capital acquisition of the offspring, meaning that we can perform the analysis on a larger sample than the one used in the main analysis below.

Table 3 shows the results of a set of OLS regressions, conducted at the family level, using the following regression model:

$$\text{TTFB}_i = \boldsymbol{X}_i\boldsymbol{\alpha} + \varepsilon_i. \tag{1}$$

The variable i is indexing the families; $\boldsymbol{X}$ is a vector of family-level control variables; and ν_i is an error term. The regressions include all of the relevant family-level and geographical control variables (as well as subsets) used in the main analysis further below. The results (Table 3) do not suggest any deliberate delaying behaviour: none of the social characteristics have any significant impact on the TTFB, including vital socio-economic

[10] Unsurprisingly, a simple regression of family size on the TTFB among the couples in our sample, with standard errors clustered at the family level, similar estimates of ($\hat{\gamma}_0 = 7.87$ ($p < 0.000$) and ($\hat{\gamma}_1 = 0.57$ ($p < 0.000$).

Table 3: Assesment of the instrument

Dependent variable: TTFB	(1)	(2)	(3)	(4)	(5)	(6)
Skilled Father	.012					.028
	(.153)					(.155)
Poor Father	-.006					-.013
	(.126)					(.129)
Skilled Mother	-1.183					-1.263
	(.793)					(.775)
Literate Father		-.045				-.026
		(.143)				(.147)
Literate Mother		.044				.077
		(.146)				(.151)
Longevity of Mother (Years)			.006*			.005
			(.003)			(.003)
Longevity of Father (Years)			-.000			.000
			(.004)			(.004)
Age at Marriage of Mother (Years)				.010		.010
				(.006)		(.006)
Retail-Handicrafts Location					-.080	-.093
					(.099)	(.106)
Industrial Location					-.031	-.012
					(.098)	(.101)
Agricultural Location					.103	.115
					(.099)	(.108)
Centuries since 1500	.113*	.155*	.093	.102	.092	.145
	(.067)	(.086)	(.064)	(.064)	(.064)	(.089)
Constant	2.347***	1.333***	1.127***	1.272***	1.549***	1.567*
	(.789)	(.271)	(.346)	(.219)	(.172)	(.869)
R^2	.005	.003	.003	.003	.003	.011
Adjusted R^2	.000	-.000	.001	.002	.001	.001
Number of Observations (Families)	1639	1639	1639	1639	1639	1639

Dummies for missing information are excluded from the Table. Standard errors clustered at the family level are reported in parentheses. TTFB is the time from the marriage to the first birth, measured in years. * p <0.10, ** p <0.05, *** p <0.01.

traits such as parental human capital and longevity. Note also that the control variables together explain only 1.1 percent of the TTFB (the adjusted R2 never exceed 0.2%).

3.3.2 Comparison of TTFB Distributions

As a further assessment of the exclusion restriction, we now compare the distribution of the TTFBs among our sampled couples to those of a group of newly wed Muslim couples in rural Palestine, documented by Issa et al. (2010). There are two main reasons why the Palestinian data is appropriate for the comparison. Firstly, pre-marital sex is culturally forbidden according to Muslim tradition. Indeed, Issa et al. found no evidence of pre-marital pregnancies, or even co-habitation, among the observed couples (ibid., p. 4). Secondly, it is cultural tradition that the Palestinian couples strive to become pregnant immediately after the marriage. According to Issa et al. (2010), the sampled couples all reported that their wedding night marked the onset of unprotected sex, after which intercourse occurred frequently up until the time of pregnancy (ibid., p. 2).[11]

If there were a tendency among our sampled couples to delay the first birth after marriage, then we would expect to see a lower proportion of births following the marriages of our English couples compared to the Palestinians. However, this is not the case. In fact, after one year the couples of our sample were slightly *more* likely to have become pregnant compared to their Palestinian counterparts.[12] The chances of conception in the most relevant control group among the Palestinians (women with less than 10 years of schooling) were 12% after one month; 64% after six months; and 76% after 12 months (ibid., Table 1). In our sample the numbers are 17% after one month; 57% after six months; and 77% after 12 months. When presented this way, small monthly differences will cumulate. Hence, we can also calculate the average monthly probability of conception, which for the Palestinian sample was 12% in month 0-1; 11% in months 2-6; and 5% in months 6-12. The numbers of our sample are 17% in month 0-1; 10% in months 2-6; and 6% in months 6-12. Note that the falling probability of conception supports the notion that the TTFB actually measures fecundity. Overall, the comparison with the Palestinian data supports the findings of Stone (1977) and others, concluding that marriage in pre-modern England marks the onset of unprotected sex.

4 Analysis and Results

Having described above how the TTFB can potentially function as an instrument for fertility, we now turn to the main analysis of the paper, attempting to quantify a child quantity-quality trade-off effect based on the sampled couples. For comprehensiveness, we first investigate the partial correlations in the data between the quantity and quality of children by conducting a standard OLS analysis. Then we turn to the instrumental variable (IV) analysis.

The OLS model is given by the following equation:

$$\text{Outcome}_j = \beta_1 \text{SurvivingSiblings}_j + \boldsymbol{Z}_i \boldsymbol{\beta}_2 + \mu_j, \tag{2}$$

where j is indexing the individuals; $\boldsymbol{Z}$ is a vector of family- and individual-level control variables; and ε_j is an error term. The two outcome variables – literacy and skill status of the individual offspring – are regressed on the number of family siblings and a set of covariates.

[11]According to Issa et al. (2010, p. 2), 16% reported having had sexual intercourse between one and six times per week, while 73% had intercourse more than seven times weekly. The remaining 11% refused to answer.

[12]Note that the pregnancies in our sample all lead to a live birth; this was not necessarily the case among the Palestinians.

The second step of our analysis is a 2SLS model. We first regress the number of siblings on the TTFB and the control variables, i.e. we estimate the model:

$$\text{SurvivingSiblings}_j = \gamma_1 \text{TTFB}_j + \boldsymbol{Z}_j \boldsymbol{\gamma}_2 + \nu_j, \tag{3}$$

where j is indexing the individuals and ε_2 is an error term. Next we regress the two outcome variables (individual literacy and skills) on the predicted number of siblings, as well as the control variables, using the empirical specification given by Equation (2).

4.1 OLS Results

The OLS results are reported in Table 4. The robust standard errors are clustered at the family level. The cases where the TTFBs are less than 40 weeks, stemming either from premature births or firstborns conceived pre-nuptially, are removed from the sample (excluding 22% of all couples).[13] The covariate "Centuries Since 1500" is the number of centuries from year 1500 to the birth year of the individual. We measure family size by the number of children born who survive to age five, reflecting the fact that, naturally, children suffering from child mortality do not present a large financial burden on the family budget.[14]

The sign of the conditional correlation between family size and human capital of offspring is negative, as predicted by the child quantity-quality trade-off hypothesis. This is regardless of whether the outcome variable is literacy or skills (Table 4, Columns 1 and 4). However, the partial correlations are insignificant and very close to zero in both cases.[15]

Turning to the covariates, the coefficients all appear to be in line with the a priori. Males are significantly more likely than females to be literate, but significantly less likely to be skilled. At first glance, the latter finding may appear surprising. However, unskilled work was physically very demanding and working women were, therefore, usually engaged in skilled work (notably spinning and weaving). It also follows that children who were not baptized on a Sunday, as was the convention, are significantly more likely to become literate and skilled, capturing the notion that the higher socioeconomic ranks were able to afford a non-Sunday baptism. Having a skilled father significantly increases the likelihood that the offspring is literate and skilled. Having a literate father, or mother, also makes it significantly more likely that the offspring is literate, while there is no significant effect on skills. Having a poor father makes it significantly less likely that the offspring is skilled and literate. Long-lived parents generally have no significant effect on the human

[13]The results are robust to their inclusion.

[14]Our findings are robust to using number of births instead.

[15]Appendix A includes the results of a series of OLS regressions using a variety of subsets of the control variables.

Table 4: Baseline results

	Dependent variable: Literate			Dependent variable: Skilled		
		IV			IV	
	OLS	1st stage	2nd stage	OLS	1st stage	2nd stage
	(1)	(2)	(3)	(4)	(5)	(6)
Surviving Siblings (> 5 Years)	-.011		-.067***	-.009		-.075***
	(.009)		(.024)	(.008)		(.028)
TTFB		-.436***			-.483***	
		(.054)			(.063)	
Male	.113***	.016	.113***	-.284***	.199	-.273***
	(.027)	(.105)	(.027)	(.071)	(.316)	(.075)
Non-Sunday Baptism	.068**	-.136	.061*	.106***	-.232	.096***
	(.030)	(.133)	(.032)	(.036)	(.177)	(.037)
Skilled Father	.084	-.233	.068	.212***	-.142	.192***
	(.062)	(.300)	(.063)	(.066)	(.290)	(.067)
Poor Father	-.204***	.264	-.191***	-.236***	.587**	-.202***
	(.055)	(.264)	(.058)	(.054)	(.297)	(.058)
Skilled Mother	.379	3.218**	.576**	.727***	4.091**	1.030***
	(.230)	(1.620)	(.260)	(.209)	(1.800)	(.323)
Literate Father	.192***	.687**	.232***	.076	.849***	.128**
	(.048)	(.274)	(.054)	(.058)	(.307)	(.063)
Literate Mother	.216***	-.177	.202***	.027	-.190	.009
	(.047)	(.277)	(.050)	(.061)	(.332)	(.061)
Longevity of Mother (Years)	.003*	.022**	.004**	.003	.024***	.004**
	(.002)	(.009)	(.002)	(.002)	(.009)	(.002)
Longevity of Father (Years)	-.000	-.004	-.000	-.004**	.011	-.004*
	(.002)	(.008)	(.002)	(.002)	(.009)	(.002)
Age at Marriage of Mother (Years)	.000	-.195***	-.010*	-.001	-.236***	-.016**
	(.004)	(.018)	(.006)	(.004)	(.020)	(.008)
Retail-Handicrafts Location	.132**	-.518*	.103	.022	-.358	.006
	(.062)	(.289)	(.066)	(.051)	(.248)	(.053)
Industrial Location	.103**	.201	.115**	.240***	.345	.269***
	(.051)	(.263)	(.054)	(.057)	(.343)	(.063)
Agricultural Location	.112**	.224	.117**	-.098*	.289	-.087
	(.051)	(.263)	(.055)	(.058)	(.295)	(.062)
Centuries since 1500	.128*	.687**	.158**	-.075*	.547**	-.038
	(.067)	(.332)	(.071)	(.044)	(.219)	(.049)
Constant	-.517	9.980***	.027	1.026***	8.623***	1.521***
	(.366)	(1.917)	(.455)	(.307)	(1.776)	(.391)
R^2	.215	.465	.170	.317	.496	.248
F (Kleibergen-Paap)			66.3			59.1
Endogeneity Test p-value			.019			.010
Number of Observations	1,248	1,248	1,248	652	652	652
Number of Families	571	571	571	453	453	453

Dummies for missing information and birth order dummies are excluded from the Table. Standard errors clustered at the family level are reported in parentheses. TTFB is the time from the marriage to the first birth, measured in years. * p <0.10, ** p <0.05, *** p <0.01.

capital of their children,[16] although it should be kept in mind that the sampled parents are long-lived by construction (cf. the restriction regarding completed marriages above). Furthermore, the children of parents located in parishes dominated by industrial activities are significantly more likely to be skilled than those located in parishes of mixed professions (the background variable). The children of parents located in parishes dominated by retail and handicraft are significantly more likely to be literate. Finally, the time trend suggests that children become significantly less skilled over time (at the 10% level). This is consistent with the deskilling hypothesis, holding that the shift from workshop to factor production during the Industrial Revolution was a skill-saving development (Goldin and Katz, 1998; Nuvolari, 2002).

4.2 IV Results

The results of the first-stage regression of the 2SLS analysis, where we regress the family size on the TTFB and covariates, are presented in Table 4, Columns (2) and (5).[17] It follows that a one-year increase in the TTFB reduces the number of surviving offspring by close to half a child, depending on the sample. Slightly less than half of the variation in the family size is explained by the TTFB and the covariates. The covariates have practically the same partial effects regardless of the sample used (literate or skilled individuals). The fact that low-income fathers have relatively many children is consistent with evidence showing that the poor were eventually outcompeting the rich in terms of births after 1800 (Boberg-Fazlic et al., 2011). In addition, it appears that literate fathers and long-living mothers give birth to relatively many offspring, while older brides have (as expected) relatively few. In both samples there is a gradual increase in family size over time (roughly half a child per century), consistent with the growing size of England's population at the time Wrigley and Schofield (1989).

In the second stage we regress the literacy and skill status of the offspring on the predicted number of surviving children, as well as the covariates. The findings (Table 4, Columns 3 and 6) reveal a sizeable and significant quantity-quality trade-off effect: an extra sibling on average reduces the chances of obtaining literacy by 6.7 percentage points and of obtaining a skilled profession by 7.5 percentage points.[18] Hence, being born to a large family drastically cuts the changes of achieving literacy and skills, even when controlling for the child's parents being educated, long living and economically affluent. Note that the endogeneity test of family size rejects in both regressions ($p = 0.019$ and $p = 0.010$). Also, the Wald F-test statistics ($F = 66.3$ and $F = 59.1$, respectively),

[16]Except for the fact that long-lived fathers have a very small, significantly negative effect on skills.

[17]Using the ivreg2 module, version 03.1.04, for Stata, provided by Baum et al. (2007a).

[18]Appendix B shows the results of a series of estimates, based on 2SLS regressions, using different subsets of the control variables.

based on the Kleibergen-Paap *rk* statistic Kleibergen and Paap (2006), do not generate suspicion regarding a weak instrument Baum et al. (2007b).

4.3 Robustness

To gauge the robustness of our results we now perform four main robustness checks, dealing with (*i*) some relatively long TTFBs in the data; (*ii*) some potentially missing births due to the possibility of temporary migration; (*iii*) potential hereditary variations in fecundity; and (*iv*) an alternative measure of marital fecundity, i.e. the waiting time from the first to the second births.

4.3.1 Winsorizing the TTFBs

Some of the sampled couples have extraordinary long waiting times from their marriage to the arrival of their first child (up to ten years). Although the waiting time to a conception can generally be rather extensive we wish to ensure that extraordinary long waiting times are not the source of our findings. Hence, we have repeated the analyses above using a Winzorised version of the instrument, where any TTFB exceeding three years (i.e. falls outside of the 90$^{\text{th}}$ percentile) is set to three years. The results of the Winsorized regressions (Table 5, Column 1 and Table 6, Column 1) demonstrate an even larger effect than in the baseline run, verifying that the main findings (Table 4) are not driven by the TTFBs falling outside of the 90$^{\text{th}}$ percentile of the distribution. The same conclusion is reached even if we remove the couples of TTFB greater than 3 years from the analysis instead. It is also possible that the long TTFBs could be the result of unobserved firstborns. However, if we impute an extra child wherever the TTFB exceeds three years and use the Winsorized instrument then we still obtain a significant effect (Table 5, Column 2 and Table 6, Column 2).

4.3.2 Potentially Unobserved Births

By confining the sample to couples who have completed their marriage (i.e. the wife survives in marriage until age 50) we automatically exclude the possibility of permanent migration and for that reason are able to steer clear of births occurring in parishes outside of our sample (see the discussion above). Nevertheless, it was not unusual for a married couple to migrate to an unobserved parish temporarily (Souden, 1984). Being away for more than a couple of years, it is not unlikely that the couple would conceive (and thus baptise) a child in their interim location. Such incidences would appear in the data as an extended birth-spacing interval, and the resulting child would remain unobserved. To address this issue we impute an extra sibling for all the birth-spacing intervals exceeding three years, thus increasing the average family size by 1.3 children. The revised trade-off

Table 5: Robustness of the literacy results

Dependent Variable: Literate	Winsorized TTFB (1)	Winsorized TTFB (2)	Imputed Siblings (3)	Controlling for own TTFB (4)	IV: TTSB (5)
Surviving Siblings (> 5 Years)	-.100***			-.067***	-.058***
	(.036)			(.025)	(.022)
Surviving and Imputed Siblings		-.195**			
		(.086)			
Surviving and Imputed Siblings (Spacings > 3 Years)			-.069***		
			(.026)		
TTFB < 40 Weeks				.016	
				(.039)	
40 Weeks ≤ TTFB < 1 Year				-.001	
				(.045)	
1 Year ≤ TTFB < 2 Years				-.035	
				(.041)	
2 Years ≤ TTFB < 3 Years				.084	
				(.063)	
TTFB ≥ 3 Years				.064	
				(.064)	
R^2	.097	-.269	.162	.172	.177
F (Kleibergen-Paap)	31.5	8.4	38.4	63.7	59.4
Endogeneity Test p-value	.006	.004	.024	.019	.016
Number of Observations	1,248	1,248	1,248	1,248	1224
Number of Families	571	571	571	571	547

Control variables are excluded from the Table. Standard errors clustered at the family level are reported in parentheses. TTFB is the time from the marriage to the first birth, measured in years. * p <0.10, ** p <0.05, *** p <0.01.

effects are reported in Table 5, Column 3 and Table 6, Column 3. In both regressions, the estimated effects are nearly unchanged and remain significant.

4.3.3 Controlling for the Hereditary Variations in Fecundity

If fecundity is hereditary then this could influence our findings (e.g. if fecundity and quality are correlated). In order to rule out this potential factor of endogeneity, we can control for the fecundity of offspring, accounting for any variations in the hereditary components of fecundity. The TTFB of offspring is known in 71% of the cases. We have addressed the issue by including dummy variables capturing if the children's own TTFB is less than 40 weeks; between 40 weeks and 1 year; between 1 and 2 years; between 2 and 3 years; or 3 years and above. The background variable is unknown TTFB. Table 5, Column 4, and Table 6, Column 4, show that the baseline results (Table 4) are robust to handling the potential hereditary effects appearing through fecundity. The Table also shows that the children's own TTFBs are not significantly correlated with their human capital outcome.

Table 6: Robustness of the skill status results

Dependent Variable: Skilled	Winsorized TTFB (1)	Winsorized TTFB (2)	Imputed Siblings (3)	Controlling for own TTFB (4)	IV: TTSB (5)
Surviving Siblings (> 5 Years)	-.110***			-.074***	-.081***
	(.038)			(.028)	(.030)
Surviving and Imputed Siblings		-.186**			
		(.075)			
Surviving and Imputed Siblings (Spacings > 3 Years)			-.083**		
			(.032)		
TTFB < 40 Weeks				.004	
				(.046)	
40 Weeks ≤ TTFB < 1 Year				-.058	
				(.051)	
1 Year ≤ TTFB < 2 Years				.007	
				(.046)	
2 Years ≤ TTFB < 3 Years				-.022	
				(.073)	
TTFB ≥ 3 Years				-.021	
				(.069)	
R^2	.157	-.156	.212	.253	.235
F (Kleibergen-Paap)	31.6	11.2	29.7	60.8	47.1
Endogeneity Test p-value	.002	.002	.009	.010	.007
Number of Observations	652	652	652	652	640
Number of Families	453	453	453	453	441

Control variables are excluded from the Table. Standard errors clustered at the family level are reported in parentheses. TTFB is the time from the marriage to the first birth, measured in years. * p <0.10, ** p <0.05, *** p <0.01.

4.3.4 An Alternative Instrument: the TTSB

There is reason to believe that a couple of low fecundability, and hence a long TTFB, will also have a long waiting time between the wedding and the second birth (henceforth: the TTSB). We can therefore test the robustness of our findings by running the analysis with the TTSBs instead of the TTFBs, bearing in mind that it entails a component of endogeneity because the spacing between the first and second birth are potentially controlled by the couple by means of regulating the breast-feeding period (something which of course is not possible during the time leading up to the first birth). Meanwhile, the use of the TTSB yields results that are virtually identical to those of the baseline run, with estimates of 5.8 percentage points (Table 5, Column 5) and 8.1 percentage points (Table 6, Column 5), for literacy and skills respectively, comparable to the 6.7 and 7.5 percentages points of using the TTFB (Table 4, Column 3 and Column 6), suggesting that our findings are not driven by TTFB anomalies.

Table 7: Heckit analysis

	Dependent variable: Literate			Dependent variable: Skilled		
	1st stage (1)	2nd stage (2)	3rd stage (3)	1st stage (4)	2nd stage (5)	3rd stage (6)
Surviving Siblings (> 5 Years)			-.066***			-.074***
			(.025)			(.028)
Missing Death Date	-.608***	.068	.021	-.144**	-.042	.046
	(.077)	(.199)	(.045)	(.057)	(.215)	(.047)
Missing Marriage Date	-3.985***	-.619	.141	-1.751***	.571	-.006
	(.148)	(1.738)	(.391)	(.069)	(1.623)	(.413)
TTFB	.018	-.437***		.007	-.483***	
	(.035)	(.053)		(.024)	(.063)	
Inverse Mills Ratio		.166	-.018		-.335	.003
		(.599)	(.135)		(1.225)	(.312)
R^2		.466	.172		.497	.253
F (Kleibergen-Paap)			66.7			58.6
Endogeneity Test p-value			.021			.010
Number of Observations	8647	1,248	1,248	8647	652	652
Number of Families	1639	571	571	1639	453	453

Control variables are excluded from the Table. Standard errors clustered at the family level are reported in parentheses. TTFB is the time from the marriage to the first birth, measured in years. * p <0.10, ** p <0.05, *** p <0.01.

4.4 Heckit Analysis

Our sampled individuals were selected based on their presence at the time when their literacy or skill status was reported. This fact may potentially introduce a bias, e.g. if family size affects the probability of death or of migration to an unobserved parish before the literacy or skill status is recorded. It is relevant to ask, therefore, if the trade-off we observe applies to the entire population of completed marriages from which our sample is drawn, or just those for whom we know their literacy or skill status.

To answer this, we perform a three-step Heckit analysis (Wooldridge, 2010, Procedure 19.2). In the first stage we extend the sample to also include observations where literacy and skill status are unknown, thus expanding the sample to 8,647 individuals representing a total of 1,639 families. Next, we estimate the probability of observing human capital with a probit model, using dummies for missing marriage or death dates as instruments in addition to the TTFB (and covariates). We have 6,037 observations with missing marriage dates; 4,405 observations with missing death dates; and 2,976 cases where both dates are missing. Based on the predicted probabilities, we calculate the inverse Mills ratio, proceeding to estimate Equation (2) by 2SLS including the inverse Mills ratio as a control variable. We conduct the procedure for both outcome variables (i.e. literacy and skill status). If the inverse Mills ratio is statistically significant in the first or second stage, then it means our estimations possibly suffer from a sample selection bias.

Table 7 shows that the dummies for missing marriage and death dates are both highly significant, emphasising their accuracy in predicting a missing literacy or skill status. The inverse Mills ratio turns out to be highly insignificant in both stages of both regressions, verifying the absence of a sample selection bias.

5 Conclusion

We have used marital fecundity, measured by the waiting time from the marriage to the first birth, as an instrument for marital fertility, showing that additional siblings significantly reduced the chances of the offspring becoming literate (by 6.7 percentage points) and skilled (by 7.5 percentage points) in the 18^{th} -19^{th}-century England. Our findings lend strong support, not only to the child quantity-quality trade-off hypothesis, but also to unified growth theory (Galor, 2011) and to theoretical work by (Galor and Moav, 2002) who conjecture that the trade-off was decisive for economic development throughout the entire history of humanity. Our identification strategy, instrumenting fertility through fecundity, can be employed for a wide range of data, in developing countries and historical economies alike, and is a particularly useful tool for estimating the child quantity-quality trade-off effects in the growing number of family reconstructions of historical populations that are currently becoming available.

A Various OLS Specifications

A.1 Literacy

Dependent Variable: Literate	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Surviving Siblings (> 5 Years)	-.010	-.007	-.014*	-.012	-.008	-.008	-.011
	(.008)	(.008)	(.008)	(.008)	(.010)	(.009)	(.009)
Male	.083***	.094***	.097***	.077***	.077***	.084***	.113***
	(.029)	(.028)	(.027)	(.029)	(.030)	(.029)	(.027)
Non-Sunday Baptism	.131***						.068**
	(.032)						(.030)
Skilled Father		.143**					.084
		(.065)					(.062)
Poor Father		-.344***					-.204***
		(.055)					(.055)
Skilled Mother		.306					.379
		(.220)					(.230)
Literate Father			.272***				.192***
			(.047)				(.048)
Literate Mother			.287***				.216***
			(.047)				(.047)
Longevity of Mother (Years)				.002			.003*
				(.002)			(.002)
Longevity of Father (Years)				-.002			-.000
				(.002)			(.002)
Age at Marriage of Mother (Years)					.004		.000
					(.005)		(.004)
Retail-Handicrafts Location						.283***	.132**
						(.058)	(.062)
Industrial Location						.039	.103**
						(.049)	(.051)
Agricultural Location						.048	.112**
						(.051)	(.051)
Centuries since 1500	.041	.087	.047	-.004	-.006	-.010	.128*
	(.069)	(.060)	(.072)	(.069)	(.070)	(.071)	(.067)
Constant	.377	.220	.183	.546*	.470*	.528**	-.517
	(.249)	(.269)	(.255)	(.309)	(.278)	(.248)	(.366)
R^2	.033	.135	.154	.020	.018	.040	.215
Number of Observations	1,248	1,248	1,248	1,248	1,248	1,248	1,248
Number of Families	571	571	571	571	571	571	571

Dummies for missing information and birth order dummies are excluded from the Table. Standard errors clustered at the family level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2 Skills

Dependent Variable: Skilled	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Surviving Siblings (> 5 Years)	-.011	-.007	-.018*	-.013	-.012	-.015	-.009
	(.011)	(.009)	(.010)	(.010)	(.012)	(.011)	(.008)
Male	-.247***	-.247***	-.262***	-.244***	-.243***	-.276***	-.284***
	(.061)	(.064)	(.067)	(.058)	(.059)	(.064)	(.071)
Non-Sunday Baptism	.170***						.106***
	(.039)						(.036)
Skilled Father		.310***					.212***
		(.069)					(.066)
Poor Father		-.209***					-.236***
		(.055)					(.054)
Skilled Mother		.741***					.727***
		(.205)					(.209)
Literate Father			.276***				.076
			(.060)				(.058)
Literate Mother			.055				.027
			(.069)				(.061)
Longevity of Mother (Years)				.003			.003
				(.002)			(.002)
Longevity of Father (Years)				-.004*			-.004**
				(.002)			(.002)
Age at Marriage of Mother (Years)					.002		-.001
					(.005)		(.004)
Retail-Handicrafts Location						.104**	.022
						(.052)	(.051)
Industrial Location						.328***	.240***
						(.054)	(.057)
Agricultural Location						-.041	-.098*
						(.063)	(.058)
Centuries since 1500	-.146***	-.062*	-.198***	-.181***	-.181***	-.136***	-.075*
	(.039)	(.037)	(.047)	(.039)	(.039)	(.040)	(.044)
Constant	1.382***	.812***	1.461***	1.659***	1.503***	1.427***	1.026***
	(.203)	(.217)	(.239)	(.268)	(.249)	(.206)	(.307)
R^2	.092	.242	.120	.069	.060	.118	.317
Number of Observations	652	652	652	652	652	652	652
Number of Families	453	453	453	453	453	453	453

Dummies for missing information and birth order dummies are excluded from the Table. Standard errors clustered at the family level are reported in parentheses. * p <0.10, ** p <0.05, *** p <0.01.

B Various 2SLS Specifications

B.1 Literacy

Dependent variable: Literate	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Surviving Siblings (> 5 Years)	-.072**	-.071**	-.071**	-.072**	-.071**	-.072**	-.067***
	(.031)	(.030)	(.028)	(.033)	(.031)	(.032)	(.024)
Male	.085***	.093***	.099***	.080***	.078**	.087***	.113***
	(.030)	(.029)	(.028)	(.030)	(.031)	(.031)	(.027)
Non-Sunday Baptism	.121***						.061*
	(.035)						(.032)
Skilled Father		.139**					.068
		(.068)					(.063)
Poor Father		-.312***					-.191***
		(.063)					(.058)
Skilled Mother		.564*					.576**
		(.325)					(.260)
Literate Father			.305***				.232***
			(.052)				(.054)
Literate Mother			.262***				.202***
			(.053)				(.050)
Longevity of Mother (Years)				.003			.004**
				(.002)			(.002)
Longevity of Father (Years)				-.002			-.000
				(.002)			(.002)
Age at Marriage of Mother (Years)					-.009		-.010*
					(.008)		(.006)
Retail-Handicrafts Location						.232***	.103
						(.065)	(.066)
Industrial Location						.062	.115**
						(.052)	(.054)
Agricultural Location						.063	.117**
						(.055)	(.055)
Centuries since 1500	.096	.133*	.089	.054	.046	.058	.158**
	(.080)	(.069)	(.080)	(.081)	(.076)	(.085)	(.071)
Constant	.845**	.634*	.624*	.957**	1.218**	.977***	.027
	(.376)	(.366)	(.362)	(.420)	(.489)	(.363)	(.455)
R^2	-.041	.061	.093	-.051	-.047	-.040	.170
F (Kleibergen-Paap)	67.8	60.6	63.0	65.2	72.3	64.1	66.3
Endogeneity Test p-value	.033	.022	.035	.049	.035	.029	.019
Number of Observations	1,248	1,248	1,248	1,248	1,248	1,248	1,248
Number of Families	571	571	571	571	571	571	571

Dummies for missing information and birth order dummies are excluded from the Table. Standard errors clustered at the family level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.2 Skills

Dependent variable: Skilled	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Surviving Siblings (> 5 Years)	-.073*	-.064*	-.052	-.068	-.059*	-.083**	-.075***
	(.040)	(.034)	(.040)	(.043)	(.035)	(.038)	(.028)
Male	-.239***	-.247***	-.254***	-.237***	-.230***	-.257***	-.273***
	(.064)	(.065)	(.069)	(.061)	(.063)	(.071)	(.075)
Non-Sunday Baptism	.146***						.096***
	(.043)						(.037)
Skilled Father		.299***					.192***
		(.075)					(.067)
Poor Father		-.179***					-.202***
		(.063)					(.058)
Skilled Mother		.995***					1.030***
		(.366)					(.323)
Literate Father			.295***				.128**
			(.065)				(.063)
Literate Mother			.040				.009
			(.072)				(.061)
Longevity of Mother (Years)				.003			.004**
				(.002)			(.002)
Longevity of Father (Years)				-.003			-.004*
				(.002)			(.002)
Age at Marriage of Mother (Years)					-.009		-.016**
					(.009)		(.008)
Retail-Handicrafts Location						.052	.006
						(.061)	(.053)
Industrial Location						.350***	.269***
						(.063)	(.063)
Agricultural Location						-.028	-.087
						(.070)	(.062)
Centuries since 1500	-.098**	-.027	-.173***	-.137***	-.142***	-.098**	-.038
	(.049)	(.043)	(.054)	(.052)	(.048)	(.046)	(.049)
Constant	1.844***	1.206***	1.712***	1.947***	2.032***	1.958***	1.521***
	(.378)	(.347)	(.388)	(.374)	(.478)	(.380)	(.391)
R^2	.004	.172	.093	.001	.021	.014	.248
F (Kleibergen-Paap)	40.0	36.1	38.4	36.4	58.9	45.9	59.1
Endogeneity Test p-value	.106	.081	.380	.191	.159	.060	.010
Number of Observations	652	652	652	652	652	652	652
Number of Families	453	453	453	453	453	453	453

Dummies for missing information and birth order dummies are excluded from the Table. Standard errors clustered at the family level are reported in parentheses. * p <0.10, ** p <0.05, *** p <0.01.

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Intergenerational Reproductive Trade-Off Faced by Inhabitants of Early Quebec*

With Oded Galor

Abstract This research presents the first comprehensive evidence for the presence of an intergenerational trade-off in reproductive success within the human species. Exploiting an extensive genealogy record for nearly half a million individuals in Quebec between the 16th and the 19th centuries, the study traces the number of descendants of early inhabitants of this Canadian province in the subsequent four generations. Using the time interval between the date of marriage and the first live birth as a proxy of marital fecundity, and thus as a source of exogenous variation in family size, the research establishes that there exists a hump-shaped effect of fecundity on the size of the lineage in the long run. Thus, in light of the established heritability of fecundity over this time period, the finding suggests that the forces of natural selection generated an evolutionary advantage for individuals characterized by an intermediate level of fecundity, raising the representation of individuals with genetic predisposition towards a quality strategy in the population.

Keywords Demography, Evolution, Natural Selection, Quantity-Quality Trade-Off, Reproductive Success

JEL Classification Codes J10, O10

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1 Introduction

Inspired by Lack (1947), who argued that intermediate clutch sizes in birds maximizes the number of surviving offspring, life-history theory posits that organisms adapt to maximal reproductive success through an optimal division of resources devoted to the quantity and the quality of their offspring (Roff, 1992; Stearns, 1992). The assumption of a quantity-quality trade-off results from the fact that parents have finite resources and that an increased number of offspring must necessarily decrease the investment per offspring.

Economic growth theorists have recently theorized that an offspring quantity-quality trade-off has been closely interlinked to technological development throughout history (Galor and Moav, 2002; Galor, 2011). The theory predicts a rise in the representation of heritable traits predisposing individuals to high levels of investment in their children's human capital, such as a bias towards a small family size, which ultimately leads to a transition from stagnation to sustained economic growth combined with a demographic transition.

Offspring quantity-quality trade-offs have been found among plants, e.g. between seed number and size (Salisbury et al., 1942; Harper et al., 1970) and between and within animals (Charnov and Ernest, 2006; Perrins and Moss, 1975; Roff, 2002; Smith and Fretwell, 1974; Walker et al., 2008). For example, Perrins and Moss (1975) have manipulated clutch sizes of the Great Tit and found a negative effect of clutch size on offspring survival. Among humans, a negative association between fertility and offspring survival has also been documented (Gillespie et al., 2008; Meij et al., 2009; Strassmann and Gillespie, 2002).

While most biological models operationalize the quantity-quality trade-off as a matter of number versus survival of offspring (Hill and Kaplan, 1999), "quality" can be interpreted more broadly. In humans, higher levels of income and wealth, for example, have historically increased the propensity to marry and to start having children early (Boberg-Fazlic et al., 2011; Cinnirella et al., 2012). Recently, a trade-off between fertility and the education of offspring in historical human populations has been documented (Becker et al., 2010; Bleakley and Lange, 2009; Klemp and Weisdorf, 2012). However, few studies have investigated the effect of parental fertility on subsequent offspring reproductive success (Borgerhoff Mulder, 1998), and the existing evidence of an intergenerational reproductive trade-off is based on a small set of observations.

To our knowledge, the only other study that presents evidence for a level of fertility that optimizes the number of grandchildren is Borgerhoff Mulder (2000). In that study, the author has used data on 82 men and 64 women from the Kipsigi tribe of Kenya and found that intermediate levels of fertility optimized the number of grandchildren for women (but not for men) in three out of four wealth groups, although the differences were not statistically significant.

Kaplan et al. (1995) have used data from a sample of men living in Albuquerque, New Mexico, between 1990 and 1993. They collected information on the men's number of full and half siblings, the fertility of siblings, and the men's own fertility. The authors found a linear association between the number of children and the number of grandchildren. Ignoring possible selection problems with the author's sampling method that can arise from only including surviving fathers, this study has indicated the absence of an intergenerational reproductive trade-off in developed post-demographic transition economies. To identify an intergenerational reproductive trade-off, one must therefore first turn to pre-demographic transition populations.

The common use of fertility *per se* as an explanatory variable in the referenced literature is problematic. First, the early death of a child tends to decrease the time to the next birth through premature cessation of lactational amenorrhea and possibly through deliberate attempts to replace the lost child (Cinnirella et al., 2012). Second, as widely recognized in the literature, fertility is affected by many factors that directly affect the quality of offspring, such as income. The inability to properly control for the heterogeneity between individuals with respect to income will therefore obscure the hypothesized reproductive trade-off. In addition, fertility depends on the age at marriage, which is at least partly culturally determined. By focusing on a proxy of fecundity, this study aims to capture a purely biological determinant of reproduction.[1]

The present finding of an intergenerational reproductive trade-off operating through fecundity combined with the fact that fecundity is a heritable trait indicates that human fecundity can be explained by life-history theory.[2]

2 Framework and methodology

We propose a novel way to study the trade-off that is not afflicted by these biases. Inspired by demographers and physicians, we use the time between marriage and the first conception (the Marriage-First Conception Interval, or MFCI) in a pre-demographic transition population as a proxy for marital fecundity. On the assumption that marriage marks the intention to start a family, a longer MFCI is associated with lower levels of fecundity. Furthermore, a longer MFCI (even if experienced by otherwise fecund individuals) will inevitably reduce the time available for subsequent births until the onset of age-related

[1]Please note that we use the term "fecundity" for "potential to conceive" and the term "fertility" for the actual number of offspring produced. Note further that the relevance of our results does not require that our proxy of fecundity is not affected by cultural factors. In that case our results can be interpreted as indicating a type of cultural selection.

[2]The contention that fecundity is a heritable trait is also documented in other studies Christensen et al. (2003); Kosova et al. (2009); Pettay et al. (2005); Ramlau-Hansen et al. (2008). A previous study on a subset of our data has produced similar results, although it did not obatin statistical significance Desjardins et al. (1991). The analysis below shows that fecundity is statistically significantly heritable using the full dataset.

sterility. Therefore, conditional on the time between marriage and the last birth, the MFCI has a negative direct effect on fertility.

The onset of unprotected intercourse started historically with marriage due to the religious prohibition of pre-marital sex and illegitimate births. The MFCI is not controlled by the couples, unlike the time intervals between subsequent births, (Cinnirella et al., 2012). Furthermore, the MFCI is furthermore not affected by subsequent child mortality, and the it is widely used as a measure of fecundity (Bongaarts, 1975; Klemp and Weisdorf, 2012; Milot et al., 2011; Olsen and Andersen, 1999; Woods, 1994; Wrigley et al., 1997) and has been linked to semen quality (Ramlau-Hansen et al., 2008).

Smaller family sizes resulting frow longer parental MFCI's has been associated with improved chances of obtaining literacy and skilled professions in pre-industrial England, consistent with the existence of a historical child quantity-quality trade-off (Klemp and Weisdorf, 2012). Our main focus is to investigate if the trade-off translates into an intergenerational reproductive trade-off.

We show first that the MFCI affects the number of children negatively, controlling for the time between the marriage and the last birth. We then show that intermediate levels MFCI's maximizes the number of grandchildren, great-grandchildren and great-great-grandchildren. The nonparametric relationship (Figure 1 below) indicates that individuals with intermediate levels of fecundity produce on average more than one third as many great-great-grandchildren compared to the most fecund individuals, although they produce almost 7 percent fewer children.

This finding arises through a combination of a positive effect of fertility on offspring mortality and a negative effect of fertility on the subsequent fertility of surviving offspring. These effects are diminishing, implying that increasing the MFCI above a certain level is not offset by higher reproduction of the subsequent generations.

3 Data

We use the genealogies reconstructed from Quebec's parish registers covering 471,412 individuals living in the Canadian province from before its settlement in 1608 to 1800. The data was collected and kindly provided by *le programme de recherce en démographie historique* (PRDH) at the University of Montreal. The dataset is ideally suited for our purposes, since it covers all parishes in Quebec and the attrition is therefore neglible. This enables us to track the reproductive success of individuals over several generations.

We counted the observable number of children, grandchildren, great-grandchildren, and great-great-grandchildren of each individual in the data. Individuals born in early Quebec does not suffer from end-of-reconstitution censoring bias, since we can successfully count all of their children and grandchildren, and in many cases all of their great-grandchildren. The observed number of great-great-grandchildren is possibly affected by

Table 1: Summary statistics

	Mean	SD	P10	P50	P90
Children	9.45	4.17	3	10	14
Grandchildren	44.10	29.46	8	41	83
Great-grandchildren	180.93	149.63	12	156	383
Great-great-grandchildren	359.50	385.68	6	248	869.5
MFCI (years)	0.49	0.48	0.052	0.31	1.27
Years from marriage to last birth	19.01	8.38	6.43	20.3	28.0
Marriage age (years)	22.47	5.87	15.6	22.0	29.6
Longevity (years)	60.40	18.50	32.3	65.0	81.9
Male	0.45	0.50	0	0	1
N	1800				

end-of-reconstitution bias even for these individuals. Generally, the later an individuals is born, the greater is the effects of end-of-reconstitution bias.[3]

This feature combined with the fact that we want to focus on the least economically and structurally developed period lead us to focus on individuals born before the end of the 17$^{\text{th}}$ century, while maintaining a large number of observations. The reason for focusing on the least developed period is to avoid the obfuscation of the hypothesized trade-off introduced by institutionalization of health and social care (Galor, 2011). Quebec was properly established by the end of the 17$^{\text{th}}$ century (Charbonneau et al., 2000). We therefore select individuals born in Quebec (i.e. we exclude immigrants) before 1675 who did not emigrate. To exclude the possibility that our results arise from confounding factors that changed over time, we decided to restrict the sample to a 25-year period. Thus, we did not include 86 individuals born in Quebec before 1650.[4]

The next set of restrictions and changes was chosen to maximize the precision of the MFCI in capturing fecundity. We excluded individuals for whom their first marriage date was estimated by the PRDH.[5] For the individuals with unknown birth date, we estimated their birth date by subtracting 14 days from their baptism date. We excluded individuals with less than 38 weeks from their first marriage to their first birth (since they were likely to have conceived their firstborn prenuptially). The MFCI was calculated by subtracting 38 weeks from the time from their first marriage to their first birth, and individuals with MFCI's longer than 2 years (i.e. approx. the top 13 percent) were excluded because of the their scarcity, combined with the variance in MFCI's and their great leverage.[6] These restrictions resulted in a sample of 1,800 individuals.

[3]The end-of-reconstitution bias will only affect our conclusions in case it is correlated with our explanatory variable, the MFCI, which is unlikely.

[4]Our results are robust to their inclusion.

[5]The reason for this is that the marriage date was often estimated on the bases of the time of birth of the first child. Our results are robust to the inclusion of these observations.

[6]Winsorizing these MFCI's at 2 years strengthened our conclusions.

The summary statistics of the variables included in the analysis can be seen from Table 1. One thing to note from Table 1 is the moderate progression in numbers between the mean number of great-grandchildren and the mean number of great-great-grandchildren. This is a reflection of the end-of-reconstitution bias mentioned above.

Because individuals can remarry, the MFCI of a wife need not be equal to the MFCI of her husband. Therefore, both men and women were included in the analysis. To prevent artificially small standard errors, we clustered the standard errors on the level of each individual's firstborn child (i.e. on the level of the child that was used in the calculation of an individual's MFCI).

4 Analysis

4.1 Nonparametric analysis

We initially estimated nonparametric models by fitting multivariate locally weighted polynomial regressions predicting the number of children, grandchildren, great-grandchildren and great-great-grandchildren by the MFCI of the individuals in our sample, controlling for the time between the marriage and the last birth. To this end, we used the *mlowess* procedure for Stata with a bandwidth of 0.8 (Cox, 2006).

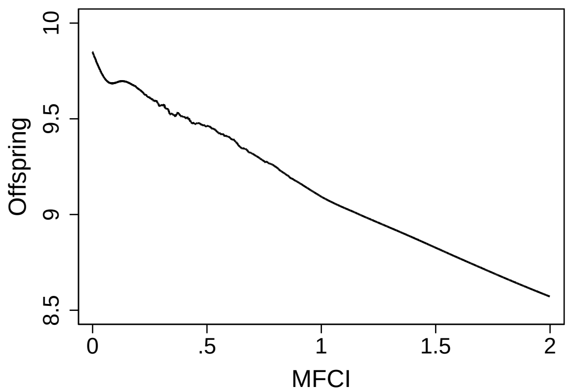
Figure 1 presents the result of this exercise. Panel 1(a) shows the negative effect of MFCI on fertility. The effect is almost exactly linear. In panel 1(b), the outcome variable is the number of grandchildren. It is clearly the case, that the individuals with intermediate MFCI's, and therefore intermediate levels of fecundity, maximized their number of descendants. At around 0.75 years, the positive effect of reduced fecundity on the subsequent fertility of offspring is offset by the adverse effect of the number of offspring in the first generation. Individuals with intermediate levels of fecundity produced approximately two and a half more children on average, corresponding to approximately 6 percent, compared to the most fecund individuals. Panel 1(b) and 1(c) show the same pattern, while the advantage of intermediate fecundity in terms of reproductive success becomes even more apparent in terms of the number of offspring.

4.2 Parametric analysis

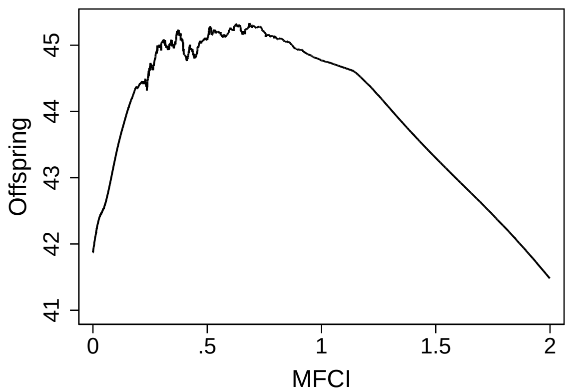
We estimate the relationship between the number of descendants in generation g and the MFCI, using a series of regression models on the form

$$C_g = \beta_0 + \beta_1 \text{MFCI} + \beta_2 \text{MFCI}^2 + Z\beta_3 + \varepsilon,$$

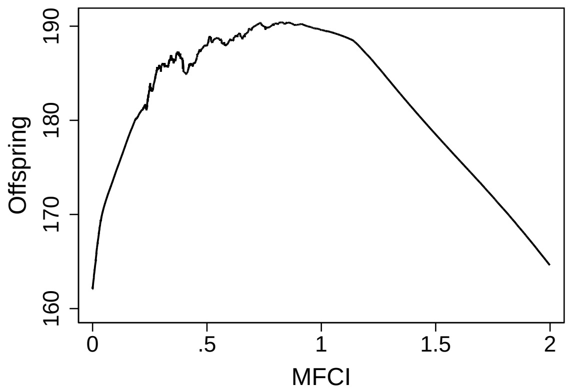
where C_g is the number of children in generation g, Z is a vector of control variables and ε is an error term clustered at the couple level of the individual's firstborns. The MFCI



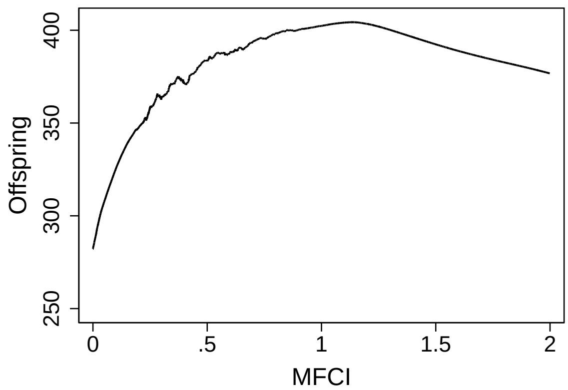
(a) Number of children



(b) Number of grandchildren



(c) Number of great-grandchildren



(d) Number of great-great-grandchildren

Figure 1: Smoothed number of offspring after one or more generations versus MFCI, controlling for the marriage age and the age at the last birth. The smoothed values are computed using the multivariate locally weighted polynomial regression procedure *mlowess* for Stata Cox (2006) with a bandwidth of 0.8. The R^2 of the smooths are 0.78, 0.41, 0.28 and 0.15, respectively.

Table 2: Regression results

	(1) C_1	(2) C_2	(3) C_3	(4) C_4
MFCI (years)	-.650***	6.982*	60.348***	208.962***
	(.113)	(4.201)	(23.333)	(64.934)
MFCI (squared years)		-4.610*	-36.174***	-101.359***
		(2.487)	(13.617)	(38.254)
Years from marriage to last birth	.441***	2.214***	9.190***	16.129***
	(.008)	(.068)	(.368)	(1.000)
Constant	1.389***	.736	-6.440	-2.258
	(.127)	(1.345)	(7.231)	(20.697)
R^2	.77	.40	.27	.14
N	1800	1800	1800	1800
Test of $\beta_1 = \beta_2 = 0$ p-val.	0	.17	.03	0
Optimal MFCI		.75	.83	1.03
(1) Offspring (MFCI=0)		42.83	168.29	304.42
(2) Offspring (MFCI=Optimal)		45.48	193.46	412.12
Increase		2.64	25.16	107.69
Increase (percent)		6.17	14.95	35.36
Test of (1)<(2) p-val.		.07	0.00	0.00

* p <0.10, ** p <0.05, *** p <0.01

is measured in years. The quadratic terms was excluded in regressions using C_1 as the outcome, since it was universally statistically insignificant.

The theory predicts that $\beta_1 < 0$ when $g = 1$ (i.e. that fecundity positively affects fertility), and that $\beta_1 > 0$ and $\beta_2 < 0$ when $g \geq 2$ (i.e. that fecundity is negatively affecting the number of offspring after 2 generations, except at low levels).

We first estimated the model including only the time from the wedding to the last birth as a covariate. Table 2 reports the results of these regressions. The estimates reproduce the basic insights from Figure 1. Column 1 contains the estimates for the regression using the number of children (C_1) as the outcome variable. The partial effect of a one year longer MFCI is 0.650 fewer children, on average. The partial effect of one year longer between the marriage and the last birth is 0.441 children, on average. The fact that the effect of increased MFCI is bigger than the effect of more time to reproduce is consistent with the notion that MFCI represents fecundity, since decreased fecundity will not only delay the first birth but also prolong the subsequent birth intervals (Cinnirella et al., 2012; Klemp and Weisdorf, 2012).

Interestingly, the optimal MFCI is longer than the average (as well as the median) MFCI in the population (Table 1). This indicates that the forces of natural selection had a negative effect on average fecundity in the population, consistent with the notion that technological improvements increased the returns to human capital, shifting the optimal

Table 3: Regression results with controls

	(1) C_1	(2) C_2	(3) C_3	(4) C_4
MFCI (years)	-.567***	6.850*	54.632**	118.605**
	(.116)	(4.118)	(22.726)	(58.770)
MFCI (squared years)		-4.796**	-36.178***	-83.217**
		(2.406)	(13.047)	(34.105)
Years from marriage to last birth	.446***	2.019***	7.782***	10.471***
	(.010)	(.089)	(.474)	(1.114)
Marriage age (years)	.019*	-.593***	-4.248***	-20.821***
	(.011)	(.134)	(.726)	(1.824)
Longevity (years)	-.003	.071**	.553***	1.468***
	(.003)	(.034)	(.190)	(.523)
Male	.123	7.006***	36.183***	33.469**
	(.110)	(1.217)	(6.723)	(16.173)
Constant	-.068	6.812	69.313*	750.585***
	(.479)	(6.583)	(39.733)	(141.915)
Birth year FE	Yes	Yes	Yes	Yes
Birth place FE	Yes	Yes	Yes	Yes
R^2	.78	.42	.30	.29
N	1800	1800	1800	1800
Test of $\beta_1 = \beta_2 = 0$ p-val.	0	.1	.02	.03
Optimal MFCI		.71	.75	.71
(1) Offspring (MFCI=0)		42.99	171.12	340.42
(2) Offspring (MFCI=Optimal)		45.43	191.74	382.68
Increase		2.44	20.62	42.25
Increase (percent)		5.68	12.05	12.41
Test of (1)<(2) p-val.		.08	.02	.05

* p <0.10, ** p <0.05, *** p <0.01

evolutionary strategy towards increased investment in offspring quality at the cost of offspring quantity.[7]

We calculated the average marginal effect of MFCI on the number of descendants at MFCI=0 and at the optimal level of MFCI as implied by the coefficient estimates.[8] Based on these, one can see that the optimal level of MFCI at C_3 and C_4 is statistically higher than the number of offspring by the most fecund individuals (MFCI=0) with $p < 0.00$ in both cases. At C_2, the difference is statistically positive at the 10 percent level, with $p = 0.07$.

We then estimated the model including a range of control variables in addition to the time from the wedding to the last birth, namely the age at marriage, sex, longevity, and

[7]Indeed, it is also consistent with the observed decrease in sperm quality in many countries in the 20^{th} century (Merzenich et al., 2010).

[8]Thus, the optimum is MFCI=$-\hat{\beta}_1/(2\hat{\beta}_2)$.

indicators for each of the 25 different birth years and 15 different birthplaces.[9] These indicators allow for birth year and place fixed effects.[10]

Table 3 reports the results from these regressions. It is clear from Table 3, that the results for C_2 and C_3, i.e. the results based on using the number of grandchildren and the number of great-grandchildren respectively, do not change substantially. The increase in the number of offspring associated with optimal fecundity at C_4, however, a little more than halves. This change probably reflects the correction of the possible end-of-reconstitution bias made through the inclusion of the birth year fixed effects.

The age at marriage can be regarded as a proxy for income, since more affluent men were able to marry younger brides and to start a family earlier. Furthermore, fecundity is affected by the age at marriage (especially after the age of around 30–35), and the seperate effects of fecundity and age at marriage are disentagled by the inclusion of the age at marriage in the model.[11] Men had more children than women (and in particular more surviving children), and by controlling for the sex we can potentially increase precision of the estimates. Longevity can be regarded as a proxy of morbidity. Conditional on the time available for reproduction (the number of years from the wedding to the last birth), longevity does not affect the number of children (Column 1). But parents that lived longer had more surviving offspring, consistent with the interpretation of longevity as a proxy for morbidity. Thus, parents that lived longer had more children over several generations (Columns 2–4).

Numbers and variance of descendants grow exponentially in the number of generations. Thus, one could worry that the significance of the results arise from a few influential observations whose leverage is magnified as we count descendants over several generations. One way to investigate this potential problem is to transform the outcome variables by calculating their geometric means. In other words, we perform the transformation:

$$\tilde{C}_g = C_g^{1/g}.$$

The values of $\tilde{C}_g$, i.e. the geometric average number of children, are comparable across different values of g. We performed the regressions with controls using $\tilde{C}_g$ as the outcome variables for the second, third and fourth generation. The results are presented in Table

[9] Two observations did not include a known birthplace, and an indicator for unknown birthplace was also included.

[10] It would be interesting to control for the fecundity of the children to break any correlation between fecundity and quality arising from the investigated mechanism. This is left for future research. However, there are strong reasons to believe that the results are robust in that dimensions. First, Klemp and Weisdorf (2012)'s finding of a quantity-quality trade-off is unaffected by the inclusion of the children's fecundity in their model. Second, as will be seen below, the proportion of variation in MFCI that is due to additive genetic variance is very low compared to the environmental (including random) variation. Therefore, the correlation in the lineages between fecundity and quality is likely to be minute and therefore negligible.

[11] See also the analysis of heritability of fecundity below.

4 (Column 1–3). The results are for all practical purposes identical, although precision increases.

We also performed quantile regressions (not shown) on the 25^{th}, 50^{th} and the 75^{th} quantiles of the outcome. Quantile regression is more robust to outliers than the ordinary least squares estimator. These regressions showed the same qualitative conclusions. Furthermore, we performed the outlier-robust regression procedure in Stata, *rreg*, (not shown), resulting in the same qualitative findings. We therefore conclude that outliers do not drive our results.

Finally, we investigated if the reproductive trade-off was operating through mortality alone or also through effects on offspring fertility. We counted the number of children surviving to age 15 in the first generation (denoted by $C_1^{>15}$), and asked if those with fewer siblings due to lower parental fecundity produced more children. In Column 4 and 5 of Table 4 we present the results of regressions of the number of offspring produced per child in generation 1 surviving to age 15. In Column 4 we used a linear specification that shows that one year longer parental MFCI results in 0.430 more children per surviving offspring. In Column 5 we used a quadratic specification that shows that this effect is diminishing, i.e. it is stronger for short parental MFCI's than for long parental MFCI's. However, the second-order term is not significant. These results show that the reproductive trade-off is operating at least partly through the fertility of surviving offspring, and in other words that the quantity-quality trade-off is not operating through the offspring's chances of survival to reproduction alone.

5 Heritability of fecundity

To asses the heritability of MFCI in the population of Quebec, we performed a parent-offspring regression. For each individual, we calculated the average parental MFCI (note that the mother's and the father's MFCI does not need to be equal because of the possibility of remarriage), referred to as the "mid-parent MFCI".

For consistency with the analysis above, we excluded individuals for which the mother, the father or the individual itself had an MFCI of less than zero years or more than two years. We then regressed the mid-parent MFCI's on the individual's MFCI's while clustering the standard errors at the level of the mother. This analysis enabled us to use information on individuals born throughout the period covered by the dataset.

Table 5 reports the estimates of this exercise, and shows that average parental MFCI is significantly correlated with the MFCI of offspring. The heritability of MFCI, h^2, is between 0.027 and 0.021 depending on the set of environmental variables that we hold fixed (Column 1–3) (Falconer and Mackay, 1996). Our estimates are in line with other studies, and show that fecundity peaks in the early 20's (around the age of 23) and that it declines thereafter, consistent with Wood (1989).

Table 4: Regression results with controls: alternative outcomes

	(1) $\tilde{C}_2$	(2) $\tilde{C}_3$	(3) $\tilde{C}_4$	(4) $C_2/C_1^{>15}$	(5) $C_2/C_1^{>15}$
MFCI (years)	.644*	.803***	.579**	.447**	1.101*
	(.348)	(.297)	(.239)	(.200)	(.650)
MFCI (squared years)	-.446**	-.514***	-.367***		-.410
	(.198)	(.165)	(.133)		(.372)
Years from marriage to last birth	.184***	.119***	.064***	-.011	-.011
	(.008)	(.007)	(.006)	(.016)	(.016)
Marriage age (years)	-.055***	-.063***	-.091***	-.085***	-.084***
	(.013)	(.011)	(.008)	(.025)	(.025)
Longevity (years)	.008**	.007***	.004*	.000	.000
	(.003)	(.003)	(.002)	(.006)	(.006)
Male	.622***	.623***	.449***	.951***	.947***
	(.113)	(.101)	(.078)	(.244)	(.244)
Constant	2.730***	3.278***	4.481***	8.152***	8.020***
	(.549)	(.471)	(.418)	(1.078)	(1.083)
Birth year FE	Yes	Yes	Yes	Yes	Yes
Birth place FE	Yes	Yes	Yes	Yes	Yes
R^2	.46	.37	.31	.04	.04
N	1800	1800	1800	1731	1731
Test of $\beta_1 = \beta_2 = 0$ p-val.	.05	.01	.02		
Optimal MFCI	.72	.78	.78		
(1) Offspring (MFCI=0)	6.02	4.84	3.56		
(2) Offspring (MFCI=Optimal)	6.25	5.16	3.79		
Increase	.23	.31	.22		
Increase (percent)	3.85	6.48	6.4		
Test of (1)<(2) p-val.	.06	.01	.01		

* p <0.10, ** p <0.05, *** p <0.01

Table 5: Heritability of MFCI

	(1) MFCI	(2) MFCI	(3) MFCI
Mid-parent MFCI (years)	.027***	.026***	.021***
	(.005)	(.006)	(.006)
Years from marriage to last birth		-.000	-.001***
		(.000)	(.000)
Marriage age (years)		-.047***	-.046***
		(.003)	(.003)
Marriage age (squared years)		.001***	.001***
		(.000)	(.000)
Longevity (years)		.000**	.000***
		(.000)	(.000)
Male		.032***	.033***
		(.004)	(.004)
Constant	.362***	.977***	.669***
	(.005)	(.041)	(.040)
Birth year FE	No	No	Yes
Birth place FE	No	No	Yes
R^2	.00	.01	.02
N	67912	47120	47120

* p <0.10, ** p <0.05, *** p <0.01

Heritability is defined as the ratio of additive genetic variance to phenotypic variance. Thus, our small but highly significant estimate of heritability means that the phenotypic variance is very large relative to the additive genetic variance. The reason for this is likely to be the high variance in MFCI due to random chance alone. Indeed, if there were no heterogeneity in fecundity, the MFCI would follow a geometric distribution. Assuming a monthly probability of conception (*fecundability*) of 0.15, the variance would be $(1 - 0.15)/(0.15^2) = 37.78$ months or 3.15 years. Allowing for heterogeneity in fecundity can be expected to increase the variance further.

6 Conclusion

Our analysis demonstrates that early inhabitants of Quebec faced an intergenerational reproductive trade-off. Individuals with intermediate levels of fecundity produced an intermediate number of children. However, increased survival and reproduction of the offspring more than compensated for the reduced fertility in the initial generation, resulting in a higher number of descendants in the second, third and fourth generations.

The optimal MFCI was approximately 0.71–0.75, about 50 percent higher than the average MFCI. Combined with the established heritability of fecundity, these results imply an evolutionary advantage of sub-fecundity and a change in the composition of traits in the population. Our results are consistent with theories of human evolution and economic growth that emphasize a historically increasing representation of traits associated with elevated investment in the human capital of offspring

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