Censorship, Family Planning, and the British Demographic Transition *

Brian Beach
William & Mary
and NBER

W. Walker Hanlon
NYU Stern School of Business
and NBER

PRELIMINARY AND INCOMPLETE

October 19, 2018

Abstract

Starting in 1877, Britain experienced a substantial and persistent decline in fertility. We establish a causal link between the timing of this decline and the public release of family planning information that resulted from the famous Bradlaugh-Besant trial. Our empirical approach exploits plausibly exogenous variation in latent demand for family planning information based on the salience of maternal mortality. Results indicate that, following the trial, birth rates fell more rapidly in places where maternal mortality risk was more salient. Families with more children born before the trial were the most responsive to this information. These results highlight the complementarity between fundamental drivers of the demographic transition and norms surrounding the idea of family planning.

*We are grateful to Nico Voigtlander for his contribution to the early stages of this project. This project was funded by a seed grant from the California Center for Population Research. Author contact information: Hanlon – whanlon@stern.nyu.edu; Beach – bbbeach@wm.edu.

1 Introduction

The demographic transition stands alongside the Industrial Revolution as one of two key turning points on the road to modern economic growth. In Britain, where the Industrial Revolution began, the early gains in output from industrialization were largely offset by rapid population growth, limiting the rise in per capita income. Only with the onset of the demographic transition in the second half of the nineteenth-century did Britain begin to experiencing the sustained increases in real wage that characterizes modern economic growth.[1] This paper aims to improve our understanding of the factors that led to these pivotal events.

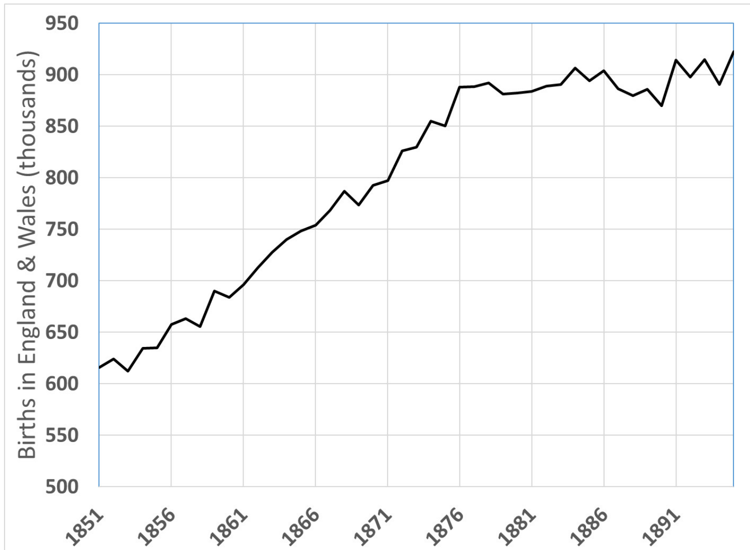
Given the importance of the historical demographic transition, it is not surprising that there is a large body of work that seeks to understand the underlying causes of this change. The vast majority of existing work has focused on forces that shift household's demand for children. Prominent theories emphasize factors that increase demand for fewer but more educated children (the quality-quantity hypothesis) or factors, such as female labor market opportunities, that may influence the opportunity cost of children. Empirical work supports the idea that these are indeed important factors. While these fundamental drivers offer an explanation for why British demand for children fell, they do not match one key empirical fact: that the start of Britain's demographic transition can largely be traced to one year, 1877. The sharpness of this transition is illustrated in Figure 1, which plots the number of births in England & Wales from 1851 to 1895.[2] This graph shows a clear inflection point in 1877. From 1851 to 1876 the number of births consistently increased, while after 1876 the number of births is essentially flat. It is hard to reconcile this rapid shift with slow-moving factors such as the rising return to human capital.

This paper argues that changing societal norms about family planning and access to contraceptive information can help explain the rapid slowdown in births shown in Figure 1. In particular, we provide evidence suggesting that the sharp change in births can be traced to the famous Bradlaugh-Besant trial, which took place in 1877. This trial was initiated by Charles Bradlaugh and Annie Besant, two secularist and free-thought activists, who published a book by Charles Knowlton with the intent

[1] Allen (2001).

[2] We plot births rather than birth rates because calculating birth rates requires population denominators that are only observed once every decade, in Census years.

Figure 1: Births in England & Wales, 1851-1895



of being arrested and triggering a test of existing censorship laws. Knowlton's book argued in favor of the moral right to engage in family planning as well as information about contraception. The trial was widely covered in the press and this, together with Bradlaugh and Besant's victory, opened up a national conversation on family planning and led to a surge in sales of books and pamphlets on family planning and contraception.

We are not the first to suggest that the Bradlaugh-Besant trial may have played an important role in the sharp slowdown in fertility in Britain after 1877.[3] However, while many authors have noted that the timing of the trial matches the slowdown in British fertility, drawing a causal connection between the trial and the fertility decline has proven difficult.

This paper provides new evidence that there was a causal link between the release of family planning information generated by the Bradlaugh-Besant trial and

[3] A number of previous researchers, including Elderton (1914), Glass (1967), Himes (1970), McLaren (1978), Teitelbaum (1984) and Szreter (1996) have speculated that the public release of information related to family planning and contraception may explain the relationship observed in Figure 1. However, existing support for this connection is largely based on the fact that these events occurred at the same time. While the timing of the trial nicely matches the timing of the change in overall British fertility, there is currently no more convincing evidence of a link between the trial and the British demographic transition.

Britain's demographic transition. The key challenge in establishing a link between the trial and the change in British birth rates is that the information released by the trial is not directly observable. Moreover, there is evidence that information about the trial rapidly diffused around the country, thanks to Britain's integrated media environment. To overcome these challenges, we propose an approach that relies on identifying a plausibly exogenous shifter for the latent demand for family planning. If the key impediment to reduced family size was family planning information, then we should expect to see a greater response to the release of information of this type in places where there is a larger latent demand for reduced family size. Thus, we can identify the impact of information by studying the change in fertility across locations that vary in their latent demand for family planning.

Ideally, we want to identify a source of latent demand for family planning information that is unrelated to other factors likely to affect fertility trends in the post-1877 period. Coming up with such a factor is difficult. However, we argue that one factor that satisfies these conditions, after the inclusion of appropriate controls, is maternal mortality. Maternal mortality was widely feared in 19th century Britain. For a healthy adult woman, it represented the most important cause of death. It was also a rapid and unpredictable killer. Unlike almost any other cause of death, maternal mortality was largely uncorrelated with factors such as wealth and education. To this end, maternal mortality was very weakly correlated with infant mortality, overall mortality, as well as mortality among fertile-aged women from other causes.

What made maternal mortality such a random process during the Victorian era was that its causes were not understood. Pregnancy occurred among women of all social classes and in all locations. Pregnancy would occasionally lead to death, most often as a result of infections picked up during childbirth. Because the Victorians didn't understand the source of these infections, there was little that could be done to protect women, and, once infected, the available medical technology was essentially useless. There is evidence that access to medical care actually increased the risk of maternal mortality, because doctors and midwives were key vectors in the spread of infection (Loudon, 1986). As a result, maternal mortality rates were no lower among the rich than they were among the poor (Loudon, 1986). Maternal mortality varied across locations, but much of this variation appears to have been largely due to chance, such as the presence of a local doctor or midwife that was an asymptomatic carrier of the rather than correlated with other underlying factors. This variation, however, means that fear or salience of maternal mortality was likely to have been greater in places where more maternal deaths had taken place in the recent past. Thus, maternal mortality provided women and their husbands with a strong incentive to avoid additional pregnancies.

To study how information interacted with the latent demand for family planning, we draw on a uniquely rich panel of data covering all of England & Wales at the level of over 600 local districts. These data allow us to track births over time, but also to control for a wide variety of other factors that are thought to influence maternal mortality, such as changing infant mortality rates, female and child labor force participation, marriage patterns, literacy, religious affiliation, population density, or local industrial structure. We begin our analysis by using aggregate data to study how overall fertility responded to the release of information generated by the Bradlaugh-Besant trial. Next, we use individual micro-data to study the extent to which these changes were driven by various margins of adjustment: couples delaying their first child, increasing the time between births, or ending fertility sooner.

Our main results show that in locations with a higher pre-existing maternal mortality rates, there was a more rapid decline in fertility in the years just after 1877. This is consistent with a lower desired fertility level in areas with higher maternal mortality which was attained only after family planning information became available. These results hold after controlling for a variety of other factors. We also conduct a variety of placebo tests in the pre-trial period to help rule out other competing hypotheses.

These results provide more direct evidence than has previously been available that the family planning information released as a result of the actions of Bradlaugh and Besant had a direct and important influence on the British fertility rate. These findings contribute to existing work documenting the importance of contraception and family planning in a more modern context. Our study is closely related to Bailey (2010), which shows that contraception availability, in the form of the "Pill," had an important effect on fertility in the post-WWII U.S. Bailey (2012) shows that, during the same period, government family planning programs also had important fertility effects.[4] There are also similarities to more recent literature looking at the impact of information on fertility decisions. One recent example in this area is Kearney & Levine (2015), which provides evidence that reality TV can change fertility levels.[5]

[4]See also (Bailey *et al.*, 2018).

[5]These results have been disputed by Jaeger *et al.* (2018).

Another example is Bassi & Rasul (2017), which documents the fertility impact of a visit by Pope John Paul II to Brazil in 1991.

Our study differs from these previous studies in two main ways. First, we show that family planning and contraceptive information also played an important role during the historical demographic transition. Had Bradlaugh and Besant not taken action, it is likely that the onset would have been further delayed. Second, we provide evidence that family planning can have an important effect on fertility even in the absence of modern contraceptive technologies. In particular, our setting suggests that in some circumstances, disseminating the idea that couples have a moral right to choose their family size can be important may be just as important as spreading more technical contraceptive knowledge. Our study also highlights the fact that the dissemination of family planning knowledge need not rely on government action. Instead, private actors such as Bradlaugh and Besant had a substantial impact, even in the face of government opposition. These results have potentially important implications for family planning in modern developing countries. The role of censorship remains an important issue today, as some countries, particularly in the developing world, continue to suppress the distribution of information or technology related to family planning and contraception.

Our results contribute to the larger literature on the historical demographic transition which is reviewed by Guinnane (2011). Much of the existing empirical work in this area focuses on the shift from child quantity to child quality (the quantity-quality or QQ trade-off), motivated by theoretical work by Becker & Lewis (1973), Galor & Weil (1999) and Galor & Weil (2000). Empirical studies of the quality-quantity trade-off include Bleakley & Lange (2009), Aaronson *et al.* (2014), and Hansen *et al.* (2018) on the U.S., Fernihough (2017) on Ireland, Diebolt *et al.* (2016) on France, Becker *et al.* (2010) and Becker *et al.* (2012) on Prussia, and Klemp & Weisdorf (n.d.) on England. Consistent with existing theories, these studies find broad support that parents trade off the number and quality of children.

Another active area of work focuses on the role of female education and labor force opportunities, which may increase the cost of raising children. Work on this topic includes Schultz (1985), Crafts (1989), Galor & Weil (1996), Jensen (2012), Becker *et al.* (2013), Diebolt & Perrin (2013) and Murphy (2015). There has been somewhat less work examining the impact of mortality (Kalemli-Ozcan *et al.*, 2000;

Ager *et al.*, 2018) and industrialization (Wanamaker, 2012; Franck & Galor, 2015) on the historical demographic transition. In addition to these studies, a number of others focusing on evaluating a range of potential determinants of the historical fertility transition in different settings.[6]

Our results complement existing work emphasizing forces that can shift the demand for children. At the end of the paper, we provide evidence supporting existing findings that suggest that factors such as female employment can have an important influence on a couple's desired number of children. However, our results emphasize the fact that lack of information due to government censorship, or societal norms against fertility limitation, can limit a couple's ability to achieve what would otherwise be their desired level of fertility.

A final contribution of our paper is to point out that to understand the impact of family planning information, one needs to consider something broader than simply technical contraceptive knowledge. A careful reading of the literature surrounding the Bradlaugh-Besant trial shows that in most cases more effort went into making the argument that couples had a right, and a responsibility, to limit the size of their family than went into technical descriptions of contraceptive information. Today, we may take this idea for granted, but at the time it was considered both revolutionary and controversial. Once one broadens the definition beyond the technical aspects of conception to include the very idea of family planning and the social norms that surrounded it, it is much easier to understand how the dissemination of new ideas about family planning may have had the rapid and widespread effect on behavior needed to produce the pattern shown in Figure 1.

2 The Empirical Setting

2.1 The Demographic Transition in England & Wales

We have already seen, in Figure 1, that there was a visible change in the number of births occurring in England and Wales in the years just after 1877 compared to the decades just before. A review of historical reports of the registrar general suggests

[6]These include two studies on Sweden, Dribe (2008) and Bengtsson & Dribe (2014), as well as work on Bavaria (Brown & Guinnane, 2002).

that this was not the result of a change in data collection practices. Rather, it appears to be a real and substantial change. Moreover, the change appears to be quite broad based, as shown by the evidence in Figure 2. The top panel of this figure breaks down births into broad regions of the country. Though the three regions show different trends in the period before 1877, with faster growth in births (and population) in the more industrialized Northern and Midland areas, in all cases the increase in births slows down after 1877. The bottom panel of Figure 2 divides districts into urban and rural based on whether they are above or below the median district population density in 1861. We can see that births (following population) were growing more rapidly in urban areas in the period before 1877, while births in rural areas were fairly flat. After 1877, there appears to be a reduction in the number of births which is observable in both types of districts.

These patterns indicate that whatever factor was behind the change in fertility that occurred around 1877, it must have been national in scope. Moreover, the fact that we see similar changes in a diverse set of areas, with different underlying economic and social structures, tells us that whatever occurred must have affected people working in very different types of industries and living in many different types of communities. In the next section we outline what we believe is the most plausible explanation for the rapid broad-based change that occurred after 1877.

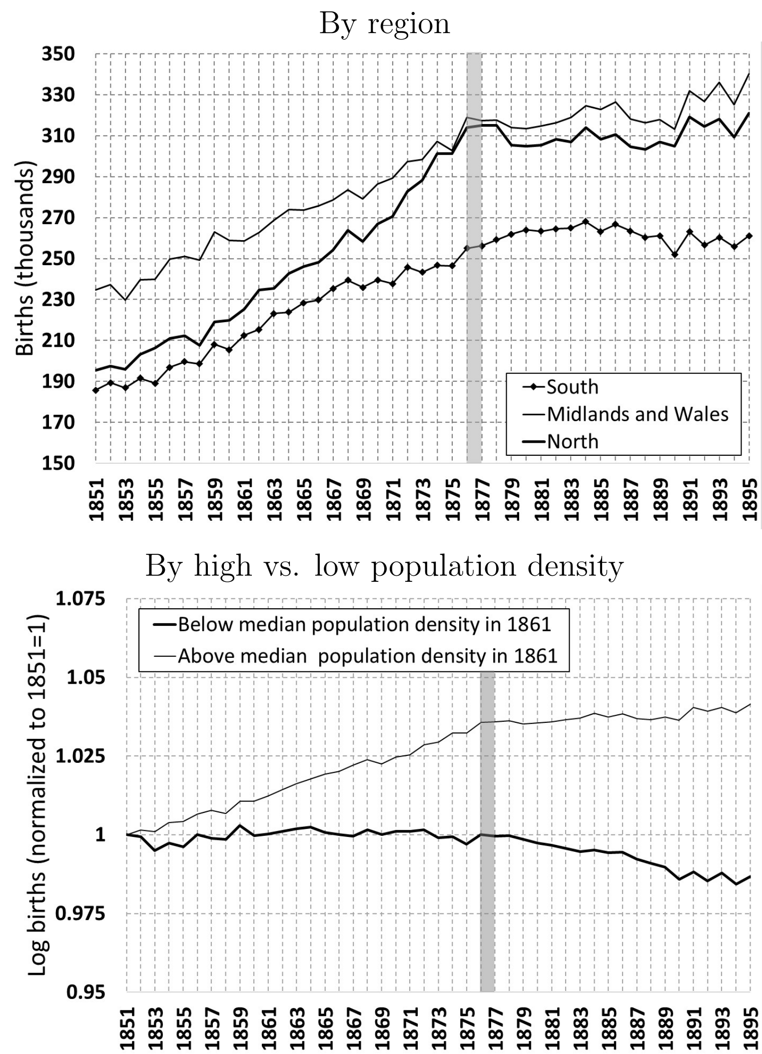
2.2 Bradlaugh-Besant Trial

Charles Bradlaugh was born poor in Hoxton, East London, in 1833. After a hard childhood and a stint in the Army, he began a political career, and by the mid-1870s he was a well-known secularist reformer.[7] Unlike Bradlaugh, Annie Besant came from a middle-class background, which, in a class-conscious society, lent some "respectability" to her work with Bradlaugh. Born in 1847, she married a priest at age 20 but her increasingly secular views led to a separation.[8] By the mid-1877 she was an active speaker on secularism.

The impetus for the Bradlaugh-Besant trial was the 1875-76 publication of *The Fruits of Philosophy*, a book written by the American Charles Knowlton in 1832. The book itself had been available in England since 1834 and was never challenged, though

[7] See Robertson (1920).
[8] See Besant (1893).

Figure 2: Births broken down by region and urban/rural



it had always sold in small numbers. The 1856-76 edition, however, was challenged after a Bristol bookseller named Henry Cook allegedly added "obscene" pictures to the pamphlet (Ledbetter, 1976, p. 29). This prompted the prosecution of Henry Cook as well as the publisher of the pamphlet, Charles Watts. The prosecutions of Watts and Cook might have gone unnoticed, except that Watts was a friend of Charles Bradlaugh. Bradlaugh realized the case against Watts could be used as a means of gaining publicity for his views on family planning as well as a test case on the government's right to censor work of this kind. When Watts accepted a guilty plea and agreed not to sell the book again, Bradlaugh and Besant decided to take matters into their own hands. They proceeded to publish a new version of Knowltons' book, with some updated medical knowledge. They intentionally informed the magistrates and city police of the time and place of sale in order to prompt arrest and trial as a test case on whether such material could be legally published.

The first hearing of the trial was at Guildhall in April, 1877. Ledbetter (1976) reports that over 20,000 people gathered outside. For trial, the case was moved to the Queen's Bench. In the trial, which began in June and lasted for five days, Bradlaugh and Besant made a case for population control as a solution to poverty and argued against restrictions on access to contraceptive information. Despite a skillful defense, they were found guilty at the trial. In his summation, the Lord Chief Justice wrote, "the law is this–that whatever outrages the public decency and actually tends to corrupt the public morals is an offense...It is enough to say that the work is a corrupt publication, that it tends to corrupt the morals of the population, and it is therefore an offense against morality." However, this verdict was reversed on appeal.

The trial was widely followed by newspapers throughout the country. J. A. Banks (1954) reviewed a sample of newspapers from around the country and found that most ran articles about the trial. Coverage was found in national papers such as the conservative *Times* and more liberal *Daily Telegraph* as well as local papers throughout the country such as the *Exeter and Plymouth Gazette*, the *Leeds Mercury*, the *Blackburn Times*, the *Birmingham Daily Gazette* and the *Sussex Daily News.* Many of these papers were critical of family planning, but that didn't stop them from writing about the trial. As the *Exeter and Plymouth Gazette* reported (23 June, 1877), "Many journalists-with the Times at their head-have seen fit to reproduce long extracts from it in their reports of the trial...The moral ordure served up in the case of Mr. Bradlaugh and Mrs. Besant has been spread out upon the breakfast table of thousands of English family."[9]

Attention continued after the trial. Soon after, a second case took place: the prosecution of Edward Truelove on the charge of publishing similar manuscripts. Less lucky than Bradlaugh and Besant, Truelove, aged seventy, was sentenced to four months in prison with hard labor. Bradlaugh and Besant continued to speak out about family planning in public meetings throughout the country. On 25 June, 1877, for example, the *Times* reported that,[10]

> *Last night the new Hall of Science, Old Street, was densely crowded, it having been announced that Mr. Bradlaugh and Mrs. Besant were to deliver addresses. Of the 600 persons who filled the hall, one-third were women, many very young. Prices of admission ranged from 2d. to 2S. 6d. In the streets were some 400 people who were unable to obtain admission. Copies of the Fruits of Philosophy were sold by the hundred, young women and lads purchasing largely. When Mr. Bradlaugh and Mrs. Besant and Mr. Truelove of Holborn entered the hall, they were received with great cheering.*

Sales of books on family planning took off. Soon after the trial, Besant published her *Law of Population*, which sold 175,000 copies by 1891. Other similar works, such as Dr. H.A. Allbutt's *Wife's Handbook* appeared soon after, and demand increased for books, such as George Drysdale's *Elements of Social Science* and Robert Dale Owen's *Moral Physiology* that had been available before 1877. Overall, Himes (1970) estimates that (p. 251), "Probably not less than a million tracts furnishing elaborate contraceptive information were sold in England between 1876 and 1891." This is substantial given that the population of England was 25 million in 1881.

Even for those who could not read the newspapers, books, and pamphlets themselves, there were numerous channels through which information about the Bradlaugh-Besant trial could be obtained. Word of mouth was likely the most important. By 1875, 80% of people were literate at the time of marriage, including 77% of women, so most people would have known at least someone who could read and write. In addition, Bradlaugh, Besant, and their supporters held numerous public meetings throughout the country following the trial where those who could not read could still hear them speak about family planning issues.

[9]Quoted from J. A. Banks (1954).
[10]Quoted from J. A. Banks (1954).

Britain was incredibly well-connected by the period we study. As one illustration, in 1872 the Postmaster General's report notes that there were 12,000 Post Offices in the country and around 8,000 additional letterboxes.[11] In that year the Post Office carried 915 millions letters as well as 99 million newspapers and 103 million books. There were, in addition, more than 5,000 telegraph offices and just under 12 million telegraph messages sent.

Looking back on these events, Himes (1970) writes (p. 243) that, "The social effect of these two trials upon the public mind was electric. The Bradlaugh-Besant trial went far to make legal the *general*, free distribution of contraceptive knowledge...There can be no doubt that the publicity gave wide advertising to the idea that contraception was possible. Millions of people learned of more effective methods." Elderton (1914) writes that the trial, "legitimized the teaching of practical methods for the limitation of the family."

It is important to recognize that the information contained in Knowlton's book and the other works produced subsequent to the trial was much broader than simply a physical description of contraceptive techniques, though that information was included. These books were aimed primarily at young couples and much of their content was directed at providing an argument in favor of the moral right of couples to choose to limit their offspring. While the idea that couples should have such a right may sound obvious today, this was a controversial point at the time. As an example, the first chapter of Knowlton's book aimed at, "Showing how desirable it is, both in a political and a social point of view, for mankind to be able to limit at will the number of their offspring." Besant's book, written just after the trial, was given the title *The Law of Population. Its Consequences and its Bearing Upon Human Conduct and Morals.* Moral questions of the correctness of family planning were central to the book, which dedicated three chapters to making an argument for the righteousness of family limitation, writing that "It is not right, it is not moral, that mothers of families should thus ruin their health, causing suffering to themselves and misery to those around them...the over-procreation of children, is as immoral as intemperance in drink." These were revolutionary ideas which may have had as much impact on changes in fertility as the more technical contraceptive knowledge. In her book Besant describes how (p. 36), "Many people, perfectly good-hearted, but some-

[11] The U.K. population in 1871 was just over 31 million.

what narrow-minded, object strongly to the idea of conjugal prudence, and regard scientific checks to population as 'a violation of nature's laws, and a frustration of nature's ends.'

As Glass (1967) writes (p. 43), "Until the end of the War of 1914-18 the birth-control movement in England concentrated almost exclusively on spreading the *idea* of and reasons for family limitation." While more technical information was also disseminated, these often had a secondary place. Knowlton's book advocates a syringe douche, while Besant recommended the sponge and *coitus interruptus*. While often rudimentary and sometimes incorrect, this knowledge provided new options for avoiding pregnancy.[12]

2.3 Maternal mortality

The reasons for families to want to avoid or limit pregnancy in the 19th century were numerous. One important factor was the risk of maternal mortality, which not only cost a mother her life, but also left the husband to raise any surviving children alone, and those children without a mother. In England and Wales the maternal mortality ratio was fairly stable at around 470 deaths per hundred-thousand live births from 1851 to 1890, an extremely high level compared to today: in the U.K. in 2015 the ratio is just 9 deaths per hundred-thousand births.[13]

A brief example, provided as a case-history in the Registrar General's Annual Report of 1876 (published 1878), serves to illustrate the nature of maternal mortality during this period:[14]

[12]Besant, for example, provided erroneous information about the safest times in the cycle for intercourse and argued that nursing had no effect on conception.

[13]The modern ratio is from (WHO, 2015). The historical ratio is calculated by the author using data collected by the Registrar General's office.

[14]Quoted from Loudon (1992).

Mrs K, who was born in 1849, was a woman of exceptional talent. In 1873, she won three scholarships to Newnham Hall, Cambridge...On leaving Cambridge she married a German doctor and was appointed in November 1877 to the principalship of a new teacher-training college...She was due to take up this post in Easter 1878 some three months after her first baby was due to be born. She was 29 years old...Mrs K went into labour on Saturday, 12 January 1878. Dr. X was called and arrived at 10pm...It was a normal delivery. The husband, standing outside the door, heard the baby cry and his wife say in a firm and cheerful voice: 'O nurse what is it? A girl? Oh I am so glad.' An hour and twenty minutes later Dr K heard his wife say to the nurse, 'Wasn't I brave? You will tell my husband, nurse, how brave I was.' More time elapsed and the doctor rushed out in a very excited state...'I am afraid your wife is seriously ill; she has suddenly taken a very bad turn.' It was now 4:30 a.m. The husband was told to run quickly and fetch the doctor's assistant (Mr. F). They both arrived back at the house at 5am. The doctor entered an adjoining room with the assistant, stayed a few seconds, returned to the bedroom, and then rushed out calling 'she is dead, she is dead.'

This brief story illustrates both the sudden and unexpected nature of maternal mortality in the period we study, as well as the fact that it could impact even those receiving the best class of medical care. Like most women, Mrs. K had her baby at home. This was typically safer than a maternity hospital where infectious spread easily in the days before antiseptic practices were introduced (these came into broad use late in the 19th century). Mrs K was typical in that she was attended by a doctor, though many women were instead attended by midwives during this period, and a not insignificant number delivered their baby themselves without attendance. Both doctors and midwives were typically extremely poorly trained in obstetric care during this period (Loudon, 1986). There is some evidence that doctors may have had higher maternal mortality rates because of their eagerness to intervene in births, spreading disease (Loudon, 1986). Another common feature of maternal mortality illustrated by Mrs K's experience was that the baby survived. In most cases, death occurred after a successful birth had taken place, leaving the father with a newborn infant to care for alone.

The three main causes of maternal mortality during this period were, in order of importance, puerperal infection, pre-eclampsia/eclampsia, and hemorrhage (bleeding). Infection, which remained the largest cause of death until the introduction of sulfa drugs in the 1930s, typically occurred in the hours or days just after parturition. Infections were caused by bacteria present on the woman's skin, bedding or medical instruments, or carried on the hands or cloths of doctors and midwives. In some cases doctors or nurses could be asymptomatic carriers, leading to many deaths. Because the cause of infection were unknown during the period we study, there was very little that could be done to reduce risks, even by the wealthiest families (Loudon, 1986). Pre-eclampisia/eclampsia is a hypertensive disorder that remains poorly understood, even today. Hemorrhage leading to excessive bleeding (flooding), which is what killed Mrs. K, was perhaps the most preventable cause of death in the 19th century, though interventions aimed at reducing hemorrhage deaths most likely increased the spread of infection. The result of the way maternal mortality occurred, together with the general lack of understanding of these processes, meant that while maternal mortality rates varied substantially across locations, they do not appear to do so in a systematic way. Later, we will examine how this variation compares to other observable district characteristics.

Maternal mortality was sufficiently common during the 19th century that Loudon (1992) reports that (p. 164), "Until the mid-1930s a majority of women in their childbearing years had personal knowledge of a member of her family, a friend, or a neighbour in a nearby street who had died in childbirth." However, while this quote reflects an average experience, the substantial variation across space and time in maternal mortality meant that some woman may have known several friends or family who died in childbirth, while others knew none. The basis for our approach is that these varied local experiences with maternal mortality would have shifted expectations about the chances of maternal death, with implications for desired fertility.

An interesting feature of maternal mortality that is useful to keep in mind is that risk is higher for first births and then declined for second births, before slowly rising again after 3 or 4 births (a "swoosh" pattern). However, it is unlikely that typical families would have been aware of this pattern, since even among medical professionals understanding of maternal mortality patterns was limited.

3 Data

The main data used in this analysis come from administrative sources and are available at the district level. While there are over 600 districts in England & Wales, many of those districts changed their boundaries at some point during our study period. Thus, for consistency, we combine any pair of districts in which there was a boundary change that resulted in the shift of more than 200 residents from one district to another. We also exclude the districts that comprise London because London differed from the rest of the country in a number of important ways. These adjustments leave us with 430 consistent districts, spanning 1851 to 1891.

Aggregate district-level data on births, deaths and marriages come from the reports produced by the Registrar General's Office. Almost all of these data, with the exception of the decadal mortality data, were digitized from original source documents for the purposes of this study. These data were collected on a continuous basis by trained registrars who were present in each district. The data are reported annually. For births, the data we use cover 1851-1895, with additional data on the number of illegitimate births starting in 1871.

We also take advantage of individual-level census data from the census of 1881. Using individual-level census data allows us to look within families, to study whether couple's were putting of childbirth, allowing more time between children, or ending fertility earlier. However, individual level-data also has some disadvantages relative to the aggregate statistics. Most importantly, the results rely on children surviving until the census, while aggregate data capture all births. Thus, we view these two sources of data as complementary.

The district-level marriage data that we use cover 1851-1884. This series includes quite a bit of useful detail, including the number of marriages broken down by whether the marriage was Established (Anglican), Catholic, or another denomination, or whether marriage took place in the Registrar's Office (i.e., non-religious). There is also information on whether the number of marriages where both parties were previously unmarried, the number in which either the man or women (or both) were minors, and the number in which either the man or woman (or both) were illiterate. These provide a wealth of useful controls for conditions in each district.

We use two types of mortality data in this study, both available at the annual district level. For maternal mortality, the key explanatory variable, we use data collected from the Registrar General's annual reports. That annual series does not include a breakdown by age, so for the remaining mortality control variables – the total mortality rate, under five mortality rate, and the mortality rate among women of fertile age (15-55) – we use decadal data compiled by Woods (1997), obtained from the UK Data Archive.

Population data for each decade from 1851 to 1901 were digitized from the Census of Population. These data break population down by age group and gender, which is useful when calculating fertility, mortality, and marriage rates. When calculating these rates, we use either three-year of five-year windows following each census. So, for example, the birth rate in each district in 1851 is calculated as the average annual number of births in either 1851-53 or 1851-55, divided by the number of fertile-aged women in the district in 1851. This approach avoids the need to use interpolated population denominators. However, the need for population denominators means that my analysis is conducted using decade-level rather than annual-level data.

The Census also reports the area of each district. We use this to calculate population density, a potentially important control variable. Data from the Census of Population is also used to construct controls for the industrial structure of each district, a factor that could potentially influence birthrates. Specifically, we use the district-level occupation data reported in the census to calculate the share of local employment in various sectors, such as agriculture, textiles, mining, metal goods, other manufacturing, government employment, professional occupations, etc. These occupation data come from 1861.[15]

The district occupation data from the 1861 Census of Population also identify gender. This allows us to construct controls for female labor force participation in each district. It is worth noting that female labor force participation was generally high in Britain during this period, but varied substantially across locations. However, national data shows that female labor force participation was also falling across the study period, as Britain transitioned towards the single-breadwinner economy that dominated during the first half of the 20th century.

[15]Detailed occupations are not reported at the district level after 1861. It is worth noting that the occupation data reported in the Census of Population often corresponds more closely to industry than to what we think of as occupation data today. It is worth noting that this occupation data covers only those over age twenty.

While employment is not broken down by age at the district level, it is possible to construct a control for the number of young workers in a district by exploiting the fact that the use of child labor during this period depended on the local industrial structure. Some industries, such as textiles, were heavily dependent on child labor, while others, such as engineering and metal industries, used relatively few child workers. Thus, we infer child labor by calculating the ratio of workers under 20 to those 20 and over, by industry, using national-level data, multiplying this ratio by employment of workers 20 and over in each industry and district, and then summing by district. The result is an inferred share of child workers in total employment as well as a child (ages 10-19) labor force participation rate.[16]

Summary statistics for the key analysis and control variables at the district level are presented in Table 1.

[16]This procedure gives results that look very reasonable. The industries with the greatest child labor ratios are the textile sectors, messengers/porters, and miscellaneous services while the lowest ratios are government employment, clergy, and utilities. The districts with the highest child labor shares are the main Lancashire textile districts, starting with Blackburn and Ashton-under-Lyme.

Table 1: Summary statistics for the district-level data

Variable	Mean	Std. Dev.	Min.	Max.	N
Birth rate panel data (decadal)					
$\Delta\ln(BR_{dt})$, all decades	-0.032	0.094	-0.417	0.281	1720
$\Delta\ln(BR_{dt})$, 1871-1881	-0.052	0.066	-0.282	0.247	430
$\ln(BR_{dt})$	-1.926	0.155	-2.6	-1.419	2150
Maternal mortality rates from 1856-1875					
MM ratio (/1000 births)	4.799	1.086	2.193	8.692	430
Log MM ratio (/1000 births)	1.543	0.226	0.785	2.162	430
Cross-sectional variables from 1871					
Total mortality rate	0.019	0.003	0.014	0.034	430
Under 5 mortality rate	0.049	0.014	0.025	0.119	430
Fertile-age female mort. rt.	0.009	0.002	0.006	0.018	430
Shr. marriages in Estab. church	0.762	0.158	0.173	1	430
Shr. marriages Catholic church	0.019	0.035	0	0.248	430
Shr. marriages at Registrar	0.095	0.107	0	0.51	430
Shr. of first marriages	0.831	0.025	0.759	0.923	430
Shr. of minors marrying, all	0.138	0.043	0.033	0.283	430
Shr. of minors marrying, Fem.	0.204	0.062	0.032	0.406	430
Shr. of illiterate marrying, all	0.211	0.078	0.042	0.564	430
Shr. of illiterate marrying, Fem.	0.215	0.103	0.039	0.617	430
Female labor force part. rate	0.399	0.094	0.173	0.747	430
Child labor force part. rate	0.483	0.096	0.237	0.957	430
Emp. shr. in agriculture	0.243	0.12	0.004	0.62	430
Emp. shr. in metal goods	0.031	0.033	0.006	0.276	430
Emp. shr. in mining	0.023	0.045	0	0.283	430
Emp. shr. in textiles	0.047	0.08	0.002	0.447	430

4 Empirical Approach

We adopt a differences-in-differences framework to examine the extent to which the timing of the Bradlaugh-Besant trail affected district-level birth rates. Our main regression specification is,

$$\Delta ln(BR_{dt}) = \beta_0 + \beta_1 MMR_d * TRIAL_t + X_{dt}\lambda + \phi_d + \eta_t + \epsilon_{dt} \tag{1}$$

where BR_{dt} is a measure of the birth rates in a district d in decade t and Δ is a difference operator. In most of our analysis, BR_{dt} is calculated using the total number of births in district d spanning the year of enumeration, the year after enumeration, and two years after enumeration, and then dividing by the female population aged 15-45 (as measured in the enumeration year). Averages have the advantage of reducing measurement error by cutting through year-to-year variation. The reason we use a forward looking average is that birth data are not available prior to 1851 census, and so we would have to discard the 1851-1861 decadal change if we were to instead use averages centered on the year of census enumeration.

The variable MMR_d captures the pre-existing maternal mortality environment in district d. Specifically, we take the average number of maternal deaths per 1000 births over the entire 1856-1875 time period. $TRIAL_t$ is an indicator for the decade during which the Bradlaugh-Besant trial took place, i.e. the change between 1871 and 1881. The variable ϕ_d represents a set of district fixed effects, which can be interpreted as district-specific time trends since our outcome variable is in changes. Our regressions also include decade fixed effects, denoted η_t. Finally, we adjust the standard errors by clustering at the district level.[17]

The period fixed effects absorb average changes between each period, including our primary period of interest (1871 to 1881). However, as noted above, we interpret underlying maternal mortality rates as a proxy for latent demand for family planning information. Thus, identification of the response to the trial comes from pre-existing variation in maternal mortality rates. Assuming that the underlying maternal mortality environment is a suitable proxy for demand for family planning, we should expect any responses to the trial to be more pronounced in these areas.

The key assumption in our identification strategy is that the maternal mortality variable is not related to other factors that may cause a change in fertility patterns between the pre-trial and post-trial periods. Given this concern, an important part of our identification strategy is our ability to include a wide range of controls (X_{dt}) reflecting the key factors thought to influence the demand for children. Among the controls we consider are population density, the child labor force participation rate, the female labor force participation rate, the level of infant mortality, overall mortality, or mortality among fertile-aged women, literacy at marriage, and the share of marriages involving minors. We interact each of these controls with the post-trial

[17]We have also tried correcting our standard errors to account for serial and spatial correlation at the 25 km, 50 km, and 100 km level. These standard errors are generally 10-15% smaller than district-clustered standard errors, and so we stick with district clustering since it is more conservative.

indicator in order to allow them to have a time-varying impact on fertility.[18] We also calculate results while dropping particular types of locations, such as those with economies based on textile production or mining, to ensure that local industrial composition is not driving our results. Naturally, we also provide additional evidence showing that locations with higher maternal mortality do not appear to have substantially different trends in birth rates or the observable factors thought to influence fertility in the pre-treatment period.

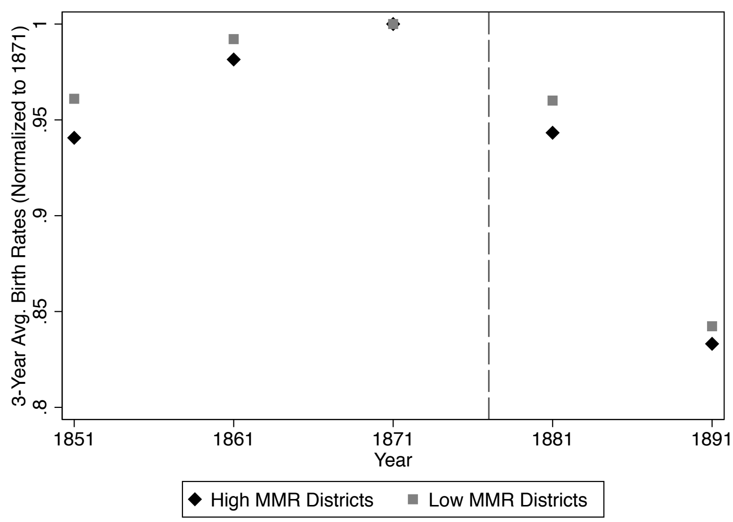
Before presenting econometric results, it is useful to start by looking at the patterns observed in the raw data. Figure 3 plot birth rates for each decade, with locations divided into those with high or low initial maternal mortality rates. To ease interpretation we normalize each district's birth rate by dividing by its 1871 birth rate and then plot the average (normalized) birth rates for each group of districts. Low MMR districts can be thought of as the control in the sense that they should be less responsive to the information than high MMR districts. Note that both types of districts will be treated by the information released by the trial, so we should expect to see a response in both. However, we should expect this treatment to have more effect in locations with greater latent demand for family planning, which we proxy with higher maternal mortality.

In the years leading up to the trial, both sets of districts seem to be experiencing a slight upward trend in birth rates with that trend being somewhat more pronounced for high MMR districts. In 1881, the first observation following the trial, we see a relative decline in birth rates everywhere and, consistent with high MMR being more responsive to this information, birth rates in high MMR districts decline faster than in low MMR districts. Birth rates continue to decline between 1881 and 1891. We also see some convergence between the two sets of districts between 1881 and 1891, we don't see complete convergence: high MMR districts still appear to have experienced a more dramatic decline in birth rates even in 1891.

Next, we turn to our main analysis, which utilizes aggregate district-level data on births from 1851-1891. Following that, we turn to the micro-data from the 1881 census, which allows us to control for individual-level factors and to study the various mechanisms through which fertility adjusted.

[18] This is consistent with the methods to strengthen identification when using difference-in-difference estimation suggested by Jaeger *et al.* (2018) and Kahn-Lang & Lang (2018).

Figure 3: Avg. Birth Rates in high and low MMR districts by decade



Notes: The 3-year average birth rate is calculated by taking the average births over the 3-year period spanning the enumeration year and the two subsequent years and dividing by the number of women of fertile women (those between the ages of 14 and 45) at the time of enumeration. These birth rates are normalized by dividing each district-year observation by the 1871 birth rate. This figure then plots the average birth rate separately for districts with above or below median initial maternal mortality rates. The median initial maternal mortality rate is 4.59 deaths per 1000 births.

5 Main Results

Table 2 presents our baseline difference in difference results. In the top panel we use our preferred specification, which compares high vs. low maternal mortality districts. In the bottom panel we present results using an alternative approach where maternal mortality is a continuous variable.[19] Column 1 of the top panel corresponds directly to equation 1, where we discretely categorize districts based on whether they have above or below median underlying maternal mortality environments (as in Figure 3). Note that what we are identifying here is just the difference in the change in birth rates after 1877 in high vs. low maternal mortality districts. The low maternal mortality districts we also treated by the trial, but we expect their response to be weaker than in districts where the risk of maternal death had more salience. Thus, the magnitude of our estimates will not reflect the full effect of treatment. Despite this we still observe a substantial effect: in the specification in Column 1, we find that high MMR districts saw their birth rates fall by roughly 3 percent following the trial. The bottom panel presents results where we use a continuous measure of MMR. There we see that a log point increase in underlying MMR is associated with a decline in birth rates between 1871 and 1881 on the order of about 5%.

In Column 2 we consider as our outcome the change in the birth rates as measured in levels rather than logs. Columns 3 and 4 replicate columns 1 and 2 except that the outcome variable is computed with 5-year rather than 3-year averages. Regardless of specification, we find robust evidence that districts with high underlying maternal mortality environments experienced a sharper decline in their birth rates following the Bradlaugh-Besant trial.

A central concern in our identification strategy is that the maternal mortality effects described in Table 2 may be driven by other factors that affect fertility levels. Both theory and existing empirical evidence suggest a number of potential factors that may influence mortality, such as the demand for child labor, female employment opportunities, changing mortality, or the education level of mothers. In Table 3 we look at how our results are affected when we include variables reflecting these channels. Importantly, the controls included in this table reflect not just alternative factors that

[19] We prefer the discrete high/low maternal mortality variable to the continuous version because maternal mortality is quite noisy.

Table 2: Baseline estimates

DV is Decadal Change in Avg. Birth Rates				
	3-year averages		5-year averages	
	Δ ln(BR)	Δ BR	Δ ln(BR)	Δ BR
	(1)	(2)	(3)	(4)
Discrete treatment				
High Initial MMR ×	-0.028***	-0.005***	-0.044***	-0.007***
Trial Decade	(0.008)	(0.001)	(0.008)	(0.001)
R-squared	0.557	0.507	0.553	0.483
Continuous treatment				
Ln(Initial MMR) ×	-0.050***	-0.009***	-0.081***	-0.014***
Trial Decade	(0.018)	(0.003)	(0.019)	(0.003)
R-squared	0.556	0.505	0.550	0.479
Observations	1,720	1,720	1,720	1,720
No. districts	430	430	430	430

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors, clustered at the district level, in parentheses. All regressions include district and period fixed effects. The treatment period is 1881-1871 change. Birth rates are forward looking averages (i.e., centered on the year after enumeration in the case of 3-year averages or centered on three years after enumeration in the 5-year averages).

may drive fertility changes, but also factors that may influence the underlying demand for family planning. Thus, beyond simply proving controls that strengthen our main analysis, the estimates associated with these control variables also provide evidence on the influence of other factors that may shift the demand for family planning information.

In Table 3, we interact the post-trial variable with previous district trends in child labor force participation, female labor force participation, mortality at ages 0-5, and literacy at marriage. These variables are meant to reflect the fundamental drivers of the fertility transition suggested by existing work. We focus on pre-existing trends in these variables, rather than levels, since existing theories rely on *changes* in these variables to drive changes in fertility rates.

There are two main takeaways from this table. First, including these variables does not substantially affect the influence of maternal mortality. This reflects the relatively weak association between maternal mortality and these other factors. Second, these results provide support for some of fundamental drivers of the fertility transition suggested by previous work. For example, we find evidence that in districts were child labor force participation was trending down, which we would expect to lower the desired level of fertility, this effect is magnified in the decade of the trial. Similarly, in locations experiencing relatively larger increases (or smaller decreases) in female labor force participation, we would expect increased demand for fertility restriction. These districts thus show larger decreases in fertility during the decade of the trail. Thus, in both cases these fundamental drivers act as expected. Of course, we have to be careful in drawing strong conclusions from these patterns, since both child and female employment are strongly linked to other factors such as income levels and industrial structure.

There is also some evidence that fertility fell more slowly during the trial decade in locations in which the child mortality was falling more rapidly. This runs counter to theories in which families have a fixed level of adult children that they are trying to target. However, this pattern does make sense if we think that parents are more reluctant to have children when they are more likely to have to watch them die. In additional results, we also find evidence that locations with higher baseline levels of child mortality experienced more rapid declines in fertility during the trial decade. Finally, at the bottom of the table we find no clear evidence that locations were experiencing a greater reduction in illiteracy had more demand for fertility restriction.

Next, we conduct a series of placebo checks assessing whether districts with higher maternal mortality exhibit differential trends in the pre-trial period. In this analysis, we shorten our panel to only consider changes between 1851-1861 and 1861-1871. We then classify 1861-1871 as our treatment decade and compare trends in that period to the 1851-1861 decade. We consider the following outcomes: changes in birth rates, marriage rates, the share of marriages where both parties were minors, the share of marriages where both parties were illiterate, district population, population density, the under 5 mortality rate, the district's child labor force participation rate, and the district's female labor force participation rate. These results appear in Table 4. The top panel presents results where we use our preferred discrete maternal mortality variable, while in the bottom panel we replace this with the continuous log maternal mortality variable. Of the nine outcomes considered in the top panel, we only see one statistically significant change at the 10% level (female labor force participation), which is roughly what we would expect by chance. The fact that we do not see any relationship to trends in the birth rate in the pre-trial period is particularly comforting.

Table 3: Assessing other drivers of the demographic transition

	DV is Decadal Change in ln(Avg. Birth Rates)					
	(1)	(2)	(3)	(4)	(5)	(6)
High Initial MMR × Trial Decade	-0.028***	-0.026***	-0.027***	-0.023***	-0.028***	-0.020***
	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)
61-71 chg. CLFP × Trial Decade		0.190**				0.575***
		(0.084)				(0.198)
61-71 chg. FLFP × Trial Decade			0.091			-0.629***
			(0.097)			(0.218)
61-71 chg. ln(under 5 m.r.) ×Trial Decade				-0.206***		-0.196***
				(0.039)		(0.040)
61-71 chg. illit. share × Trial Decade					-0.005	0.049
					(0.065)	(0.064)
Observations	1,720	1,720	1,720	1,720	1,720	1,720
R-squared	0.557	0.557	0.559	0.565	0.564	0.572
No. districts	430	430	430	430	430	430

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors, clustered at the district level, in parentheses. All regressions include district and period fixed effects. The treatment period is 1881-1871 change. Birth rates are forward looking averages (i.e., centered on the year after enumeration in the case of 3-year averages or centered on three years after enumeration in the 5-year averages). CLFP and FLFP refer to child labor force participation rates and female labor force participation rates.

As to the mechanisms these results help address, the first column (changes in ln(birth rates)) is designed to address the idea that the decline in birth rates may have started before 1871. The next three results, which focus on marriage rates and the composition of those marriages, are designed to alleviate concerns about prior changes in the marriage market manifesting as a subsequent decline in birth rates. The results where we consider changes in population, population density, and the under 5 mortality rate assess whether other public health changes that might explain a drop in fertility. Here our logic is that if the health of district is improving, then families with a desired family size need to have fewer children to achieve that optimal size. Finally, we consider changes in the child labor force participation rate and female labor force participation rate. Similar to the previous set of results, these results consider a shock to the cost/benefit of having a child.

Table 4: Placebo tests to rule out changes in district characteristics between 1861 and 1871

	3-year averages								
	Δ ln(BR)	Δ Marriage Rate	Δ Minor Mar. Share	Δ Illit. Mar. Share	Δ ln(Pop.)	Δ Pop. Density	Δ ln(Under 5 Mort. Rate)	Δ Child Lab. Force Partic.	Δ Fem. Lab. Force Partic.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Discrete treatment									
High Initial MMR × 1871 Decade	-0.001	0.000	0.029	0.010	-0.005	-0.010	0.011	-0.003	0.006*
	(0.010)	(0.001)	(0.018)	(0.013)	(0.009)	(0.103)	(0.016)	(0.005)	(0.004)
Observations	860	860	860	860	860	860	860	860	860
R-squared	0.049	0.121	0.044	0.044	0.038	0.002	0.227	0.009	0.030
No. districts	430	430	430	430	430	430	430	430	
Continuous treatment									
Ln(Initial MMR) × 1871 Decade	0.021	0.001	0.060	0.014	-0.027	0.136	0.035	0.007	0.019**
	(0.022)	(0.002)	(0.044)	(0.032)	(0.019)	(0.152)	(0.038)	(0.013)	(0.008)
Observations	860	860	860	860	860	860	860	860	860
R-squared	0.051	0.120	0.043	0.043	0.042	0.003	0.227	0.009	0.036
No. districts	430	430	430	430	430	430	430	430	430

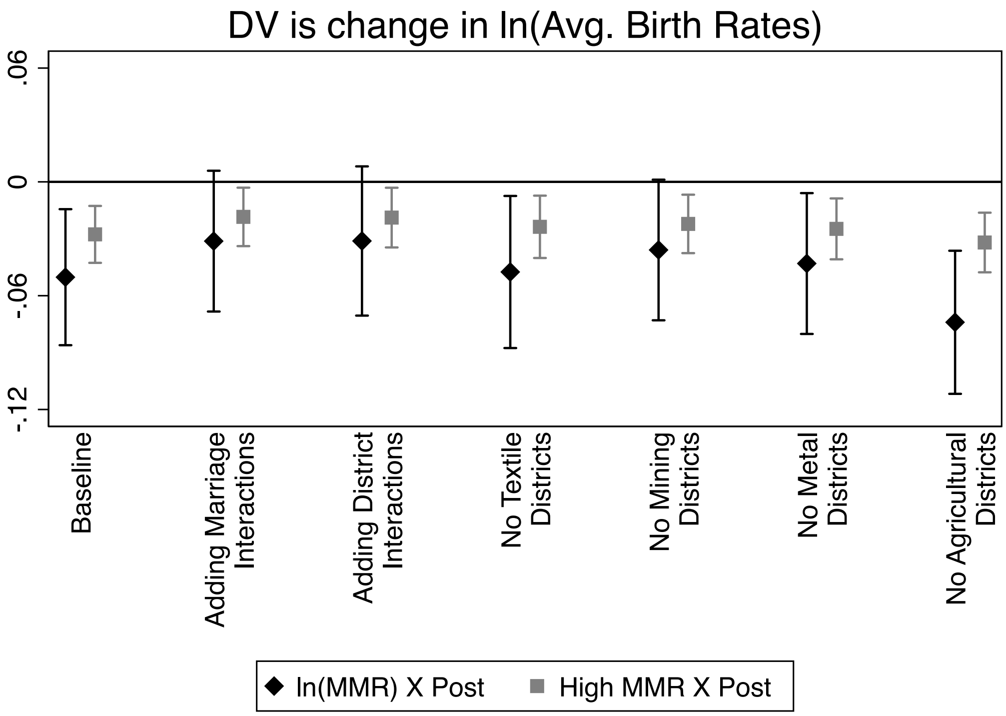
*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors, clustered at the district level, in parentheses. All regressions include district and period fixed effects. Sample includes changes between 1851-61 and changes between 1861-71. The treatment period is the 1861-1871 change. All averages are forward looking (i.e., centered on the year after enumeration).

In Figure 4 we present results from a series of additional robustness checks. For comparison purposes, we reproduce our baseline results (column 1 of Table 2) using both our continuous and discrete measures of treatment in the first entry of Figure 4. The second set of results, labeled "Adding Marriage Interactions" include controls for a range of factors that are observable in the marriage data that we have collected: the marriage rate from 1871-73, the share of marriages that took place at the registrars office (i.e., non-religious), the share of marriages that took place in a Catholic church, the share where the bride or groom were minors, and the share where they were illiterate. Each of these are interacted with the post-trial indicator so that they can have time-varying effects. The "Additional District Interactions" results include controls for population density, the share of births that were illegitimate, female and child labor force participation, the share of workers employed in "professional" occupations in 1861, the overall district mortality rate, the district mortality rate among fertile-aged women due to causes other than maternal mortality, and the district mortality rate among those aged 0-5. Despite the inclusion of these rich sets of control variables, the results obtained from our preferred specification change relatively little and remain statistically significant at standard levels. This indicates that maternal mortality is in fact proxying for demand for family planning and not other district characteristics that might have differentially affected birth rates between 1871 and 1881.

Our final set of robustness checks returns to our baseline specification and considers a series of sample restrictions where we throw out districts that specialize in certain occupations. In these regressions we throw out the top 10% of districts based on the share of their population that is engaged in each of the following categories: textile production (specification 4), mining (specification 5), metal (specification 6), or agriculture (specification 7). While the point estimates and standard errors do vary from restriction to restriction the results are not different in a qualitative sense – across each of these restrictions we continue to find evidence that birth rates in high MMR districts fell dramatically following the Bradlaugh-Besant trial.

Together, the results in this section tell us that fertility rates in locations with higher maternal mortality declined more rapidly in the years after 1877 than in locations where initial maternal mortality rates were lower. This pattern does not appear to be driven by any relationship between maternal mortality and other factors commonly thought to influence fertility decisions. Next, we turn to individual

Figure 4: Assessing the robustness of our baseline results



Notes: Each coefficient corresponds to a different regression. "ln(MMR) X Post" represents the interaction between initial MMR (the 1856-1875 average) and the trial decade (changes between 1871 and 1881). "High MMR X Post" represents the interaction between having above median initial MMR and the trial decade. All regressions include period fixed effects and district fixed effects. Standard errors are clustered at the district level. The baseline specification corresponds to column 1 of Table 2. The "Adding Marriage Interactions" specification adds to the baseline specification by including the interaction between our trial indicator and each of the following district-level marriage pattern variables: the district-level marriage rate from 1871-73, share of marriages spanning 1871-75 that took place at the Registrar's Office (which we interpret as non-religious), share of marriages that took place in a Catholic church, share of 1871-1875 marriages that were first time marriages, the share where the bride and groom were minors, and share of marriages where the bride and groom were illiterate. In the "Adding District Interactions" specification we include the previous marriage interactions as well as each of the following district-level characteristics and our "Trial Decade" indicator: population density, average share of births that were illegitimate (1871-1875), female labor force participation rate, child labor force participation rate, share of workers that were in the "professional" class in 1861,and three measures of district health (overall mortality rate, mortality rate for fertile women, and the under 5 mortality rate). The last four specifications return to our baseline specification and exclude the top 10% of districts based on their respective textile production, mining, metal, or agriculture intensities.

level micro-data which can shed further light on these patterns.

6 Evidence from microdata

Thus far our analysis has focused on aggregate birth and population statistics since they allow us to take a comprehensive look at the impact on birth rates. However, our ability to examine household-level responses as a way of better understanding the mechanisms at play is limited by the fact that these data are not tabulated by parental characteristics. To remedy this, we draw on individual microdata from the 1881 census, which was digitized by findmypast.org and standardized by the Integrated Census Microdata Project (I-CeM). The census manuscripts maintain the household structure, which allows us to construct a panel of births where we can also attach any parental characteristic that is captured in the 1881 census. A second advantage of the micro-data is that it allows us to look at results at the annual level. This means we can study impacts right around 1877, which was not possible in the aggregate data analysis due to the lack of annual data on population (needed as the denominator in the fertility rate).

We construct this panel in the following way. We begin by extracting all households residing with at least one child born between 1871 and 1881. This yields 2,562,164 households. We discard roughly 3 percent of this initial sample because of data discrepancies that decrease our confidence the fact that we are observing actual biological relationships.[20] This leaves us with a final sample of 2,482,788 households,

[20]We then throw out any household with any of the following data discrepancies: more than one individual is coded as the household head (974 instances), more than one individual is coded as the head's spouse (4,231 instances), the household head is under the age of 16 (377 instances), there are more than 10 biological children (2,748 instances). We then impost some assumptions to increase the likelihood that the mother and father are the biological parents for each of the children in the household. Specifically, we throw out any household where: the mother or father was under the age of 14 when their first child was born (18,484 and 3,887 instances, respectively), the mother was over the age of 50 when their youngest child was born (16,215 instances), or the father was over the age of 60 when their youngest child was born (9,650 instances). We then throw out any household where the spouse identifier is internally inconsistent – that is, the individual whose relationship is coded as spouse does not match the person identifier (4,460 instances). We also discard any household where the head and spouse are of the same sex (2,057 instances) or where the household head is female and the spouse is male, which is rare (112 instances) and inconsistent with the enumeration instructions. Finally, we discard 2,625 households because age is missing for one or more of the children, which limits our ability to actually infer birth order, and we discard 2502 households because the span between the births of their oldest and youngest child is greater than 25 years.

spanning all of England & Wales. The total number of children residing in these households is 8,588,101 with 6,268,593 of those children born between 1871 and 1881.

This final dataset allows us to attach parental characteristics, such as parental age and the child's birth order, to each birth. While this is useful for allowing us to examine heterogeneity in the response to the trial, it is worth noting that this approach has the drawback that our sample is selected because it requires that the child, their parents, and siblings survive until enumeration. The most concerning form of selection will be our inability to identify children that die before enumeration. However, all of our analysis will examine relative differences based on pre-existing maternal mortality rates, and so as long as infant mortality rates are not systematically differing for high-pre-existing maternal mortality rate districts near 1877 then this type of selection should only be concerning to the extent that it affects the precision of our estimates.

There are a number of dimension along which couples may have adjusted fertility in order to generate the aggregate results documented in the previous section. First, may choose to put of having children longer. Second, they may choose to increase the spacing between births. Third, they may choose to end fertility earlier. Fourth, they may choose to completely forgo having children. In the following tables we examine these channels one at a time.

We begin by looking at whether couples are delaying the birth of their first child. To assess this, we again adopt a differences-in-differences strategy taking mother of father's age at the time of birth as our outcome variable of interest. We restrict our sample to the set of "first births" (the birth of the oldest child observed in the household in 1881). We include district fixed effects and birth year fixed effects. Our independent variable of interest is an indicator variable for being born between 1878 and 1881, which we interact with pre-existing maternal mortality rates, our proxy for demand for family planning. These results appear in Table 5. There we see that a log point increase in pre-existing maternal mortality rates is associated with a 2.2 month increase in mother's age at first birth and a 3.5 month increase in father's age at first birth. Columns 1-2 use our preferred discrete indicator for whether the birth occurred in a district with above median pre-existing maternal mortality rates while Columns 3-4 use the continuous maternal mortality variable. In all cases, we see evidence that both mothers and fathers were older at the time when the first child appears in districts where maternal mortality was higher, in the years after 1877 compared to before.

Table 5: Did households delay their first birth?

DV is age when first child was born	Mother's Age (1)	Father's Age (2)	Mother's Age (3)	Father's Age (4)
High Initial MMR × Born after 1877	0.0710* (0.0362)	0.120*** (0.0451)		
Ln(Initial MMR) × Born after 1877			0.189** (0.0948)	0.303*** (0.109)
Observations	1,172,594	1,124,301	1,172,594	1,124,301
R-squared	0.018	0.018	0.018	0.018
No. districts	430	430	430	430

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the district level, in parentheses. All regressions include district and birth year fixed effects. The sample includes all first births occurring between 1871 and 1881.

Next, we study birth spacing. Note that two factors may affect birth spacing. First, couples may choose to use family planning information to increase birth spacing. Second, because parents appear to have delayed the timing of their first birth, the characteristics of the average person having a child in any given year may have changed. This selection effect may work against the direct effect of increased family planning information if those who decide to delay fertility (or end fertility earlier) were also the type of people who preferred greater spacing between births.

Table 6 examines whether the time elapsed between births changes systematically following the trial. Each column restricts the sample based on birth order. Using our same difference-in-differences empirical design, we find evidence that the spacing between births actually fell in locations with higher initial maternal mortality in the years after 1877. We see no effect for families that have had only one child, but a negative effect for families families that already have two more more children. This pattern suggests that the selection effect is likely to be overwhelming the direct effect of family planning information. It is important to note that these spacing results are conditional on families with X number of children choosing to have another child. Instead, it is possible that some families choose to end fertility earlier once they have received family planning information.

In Table 7 look at whether family planning information is working to remove births in excess of desired fertility in locations with higher maternal mortality risk. To do

Table 6: Was birth spacing affected?

	DV is years since last birth				
	2nd Child (1)	3rd Child (2)	4th Child (3)	5th Child (4)	6th Child (5)
Discrete treatment					
High Initial MMR × Born after 1877	0.0180	-0.0360***	-0.0412***	-0.0373***	-0.00505
	(0.0116)	(0.0117)	(0.0157)	(0.0132)	(0.0110)
Observations	1,177,438	1,041,988	812,701	552,027	322,444
R-squared	0.007	0.006	0.009	0.015	0.020
No. districts	430	430	430	430	430
Continuous treatment					
Ln(Initial MMR) × Born after 1877	0.0252	-0.0729**	-0.135***	-0.118***	-0.0135
	(0.0302)	(0.0333)	(0.0403)	(0.0371)	(0.0289)
Observations	1,177,438	1,041,988	812,701	552,027	322,444
R-squared	0.007	0.006	0.009	0.015	0.020
No. districts	430	430	430	430	430

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the district level, in parentheses. All regressions include district and birth year fixed effects. The sample includes births occurring between 1871 and 1881. Each column restricts the sample to a different sample based on birth order.

so, we partition the sample based on the number of children born before 1877. We then ask whether households residing in districts with higher pre-existing maternal mortality rates were less likely to have a subsequent child born between 1878 and 1881. Each column of Table 7 restricts the sample based on the number of children born before 1877 (we specifically consider households with 1 child, 2 children, 3 children, 4 children, 5 children, 6 children or 7+ children). Each regression includes fixed effects for when the most recent pre-trial child was born, as well as fixed effects for the mother and father's birth year of birth. The results in Table 7 show a clear gradient: relative to families residing in low maternal mortality districts, families residing in high maternal mortality districts are always less likely to have a subsequent child born between 1878 and 1881. This effect is larger for families with more children born before 1877, consistent with the idea that those with more children were more responsive to the availability of family planning information, and more so in locations where the risk of maternal death with more salient.

Table 7: Did this affect subsequent childbearing?

Sample restricted to families with X children born before trial	1 child (1)	2 children (2)	3 children (3)	4 children (4)	5 children (5)	6 children (6)	7 or more children (7)
	DV=1 if household had an additional child after 1877						
Discrete treatment							
High Initial MMR ×	-0.000995	-0.00352	-0.00703*	-0.00935**	-0.0134***	-0.0149***	-0.0173***
Born after 1877	(0.00372)	(0.00385)	(0.00381)	(0.00420)	(0.00464)	(0.00484)	(0.00560)
Observations	315,265	321,385	295,040	239,959	162,708	90,166	60,380
R-squared	0.193	0.241	0.278	0.298	0.306	0.307	0.295
No. districts	430	430	430	430	430	430	430
Continuous treatment							
Ln(Initial MMR) ×	-0.0114	-0.0203*	-0.0243**	-0.0343***	-0.0397***	-0.0411***	-0.0545***
Born after 1877	(0.0105)	(0.0108)	(0.0106)	(0.0108)	(0.0124)	(0.0135)	(0.0136)
Observations	315,265	321,385	295,040	239,959	162,708	90,166	60,380
R-squared	0.193	0.241	0.278	0.298	0.306	0.307	0.295
No. districts	430	430	430	430	430	430	430

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the district level, in parentheses. All regressions include fixed effects for year in which most recent birth (before the trial) occurred, as well as fixed effects for mother's year of birth and father's year of birth. The sample includes births occurring between 1871 and 1881. Each column restricts the sample to a different sample based on birth order.

7 Conclusion

This paper provides evidence that the availability of family planning information played an important role in Britain's historical fertility transition. Numerous existing studies examine the impact of what we call fundamental drivers of the fertility transition, factors such as the rising opportunity cost of female time, or the increasing returns to having fewer, more educated children. While these fundamental factors played a central role in the historical fertility transition, our results suggest that their impact could also be impeded by a lack of information about family planning. The release of this information, which occurred in Britain as a result of the widely-publicized trial of Annie Besant and Charles Bradlaugh, explains the rapid decline in births after 1877.

Two features of our study allow us to go beyond what has been possible in previous work. First, we assemble a uniquely detailed set of data. This includes both detailed data on births as well as data allowing us to directly assess, and control for, factors that are widely expected to influence fertility rates. Second, we offer a novel identification strategy which uses patterns of maternal mortality to identify latent demand for reductions in family size that are plausibly orthogonal to other factors that may have been altering fertility decisions during the period we study.

One important message to take from this paper is that family planning should be thought of more broader than simple technical information on contraception. Some authors have suggested that information could not have played a key role in the historical British fertility transition because contraceptive methods changed relatively little during the period in which fertility declined dramatically. However, a careful review of the debate surrounding these issues in the period in which fertility abruptly declined shows that most of the focus was on making the argument that couples had a right to choose their family size. Discussions of contraceptive information was secondary, rudimentary, and often inaccurate.

These results have two useful implications for modern developing countries. First, they suggest that family planning information can have a substantial impact on fertility rates, even when access to modern contraceptive methods is limited. Second, they highlight how government policies, such as the censorship of family planning information, can delay a fertility transition even in the face of substantial shifts in more fundamental drivers.

References

Aaronson, D, Lange, F, & Mazumder, B. 2014. Fertility Transitions Along the Extensive and Intensive Margins. *American Economic Review*, **104**(11), 3701–3724.

Ager, P, Hansen, CW, & Jensen, PS. 2018. Fertility and Early Life Mortality: Evidence from Smallpox Vaccination in Sweden. *Journal of the European Economic Association*, **16**(2), 487–521.

Allen, RA. 2001. The Great Divergence in European Wages and Prices from the Middle Ages to the First World War. *Explorations in Economic History*, **38**, 411–447.

Bailey, M. 2010. "Momma's Got the Pill": How Anthony Comstock and Griswold v. Connecticut Shaped US Childbearing. *American Economic Review*, **100**(1), 98–129.

Bailey, Martha J. 2012. Reexamining the Impact of Family Planning Programs on US Fertility: Evidence from the War on Poverty and the Early Years of Title X. *American Economic Journal: Applied Economics*, **4**(2), 62–97.

Bailey, MJ, Malkova, O, & McLaren, ZM. 2018 (April). *Does Access to Family Planning Increase Children's Opportunities? Evidence from the War on Poverty and the Early Years of Title X.* NBER Working Paper No. 23971.

Bassi, V, & Rasul, I. 2017. Persuasion: A Case Study of Papal Influences on Fertility-Related Beliefs and Behavior. *American Economic Journal: Applied Economics*, **9**(4), 250–302.

Becker, GS, & Lewis, HG. 1973. On the Interaction between the Quantity and Quality of Children. *Journal of Political Economy*, **81**(2).

Becker, Sascha O., Cinnirella, Francesco, & Woessmann, Ludger. 2010. The trade-off between fertility and education: evidence from before the demographic transition. *Journal of Economic Growth*, **15**(3), 177–204. Date revised - 2010-10-01; Last updated - 2011-11-08; SubjectsTermNotLitGenreText - Schooling; Population economics; Fertility; Education; Historical demography; Trade-off; Growth models; Population dynamics; Germany.

Becker, Sascha O., Cinnirella, Francesco, & Woessmann, Ludger. 2013. Does women's education affect fertility? Evidence from pre-demographic transition Prussia. *European Review of Economic History*, **17**(1), 24–44.

Becker, SO, Cinnirella, F, & Woessmann, L. 2012. The Effect of Investment in Children's Education on Fertility in 1816 Prussia. *Cliometrica*, **6**, 29–44.

Bengtsson, T, & Dribe, M. 2014. The Historical Fertility Transition at the Micro Level: Southern Sweden 1815-1939. *Demographic Research*, **30**, 493–533.

Besant, Annie. 1893. *Annie Besant: An Autobiography.* The Theosophical Society.

Bleakley, Hoyt, & Lange, Fabian. 2009. Chronic Disease Burden and the Interaction of Education, Fertility, and Growth. *Review of Economics and Statistics*, **91**(1), 52–65.

Brown, JC, & Guinnane, TW. 2002. Fertility Transition in a Rural, Catholic Population: Bavaria, 1880-1910. *Population Studies*, **56**(1), 35–49.

Crafts, N. F. R. 1989. Duration of Marriage, Fertility and Women's Employment Opportunities in England and Wales in 1911. *Population Studies*, **43**(2), pp. 325–335.

Diebolt, C, & Perrin, F. 2013. From Stagnation to Sustained Growth: The Role of Female Empowerment. *American Economic Review, Papers and Proceedings*, **103**(3), 545–549.

Diebolt, C, Menard, A-R, & Perrin, F. 2016 (February). *Behind the Fertility-Education Nexus: What Triggered the French Development Process?* Working Paper.

Dribe, M. 2008. Demand and supply factors in the fertility transition: a county-level analysis of age-specific marital fertility in Sweden, 18801930. *European Review of Economic History*, **13**, 65–94.

Elderton, EM. 1914. *Report on the English Birthrate*. Cambridge University Press.

Fernihough, A. 2017. Human Capital and the Quantity-Quality Trade-off During the Demographic Transition. *Journal of Economic Growth*, **22**, 35–65.

Franck, R, & Galor, O. 2015 (August). *Industrialization and the Fertility Decline*. Working paper.

Galor, Oded, & Weil, David N. 1996. The Gender Gap, Fertility, and Growth. *The American Economic Review*, **86**(3), 374–387.

Galor, Oded, & Weil, David N. 1999. From Malthusian Stagnation to Modern Growth. *American Economic Review*, **89**(2), 150 – 154.

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

Glass, DV. 1967. *Population Policies and Movements in Europe*. Second edition edn. Frank Cass and Company Limited.

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

Hansen, CW, Jensen, PS, & Lonstrup, L. 2018. The Fertility Decline in the United States: Schooling and Income. *Macroeconomic Dynamics*, **22**, 1584–1612.

Himes, NE. 1970. *Medical History of Contraception*. Schocken Books.

J. A. Banks, Olive Banks. 1954. The Bradlaugh-Besant Trial and the English Newspapers. *Population Studies*, **8**(1), 22–34.

Jaeger, DA, Joyce, TJ, & Kaestner, R. 2018 (July). *A Cautionary Tale of Evaluating Identifying Assumptions: Did Reality TV Really Cause a Decline in Teenage Childbearing?* NBER Working Paper No. 24856.

Jensen, Robert. 2012. Do Labor Market Opportunities Affect Young Women's Work and Family Decisions? Experimental Evidence from India*. *Quarterly Journal of Economics*, **127**(2), 753 – 792.

Kahn-Lang, A, & Lang, K. 2018 (July). *The Promises and Pitfalls of Differences-in-Differences: Reflections on "16 and Pregnant" and Other Applications*. NBER Working Paper No. 24857.

Kalemli-Ozcan, S, Ryder, HE, & DN, Weil. 2000. Mortality Decline, Human Capital Investment, and Economic Growth. *Journal of Development Economics*, **62**, 1–23.

Kearney, MS, & Levine, PB. 2015. Media Influences on Social Outcomes: The Impact of MTVs 16 and Pregnant on Teen Childbearing. *American Economic Review*, **105**(12), 3597–3632.

Klemp, Marc, & Weisdorf, Jacob. Fecundity, Fertility and The Formation of Human Capital. *The Economic Journal*, **0**(0).

Ledbetter, R. 1976. *A History of the Malthusian League, 1877-1927.* Ohio State University Press.

Loudon, Irvine. 1986. Obstetric Care, Social Class, and Maternal Mortality. *British Medical Journal*, **293**, 606–608.

Loudon, Irvine. 1992. *Death in Childbirth: An International Study of Maternal Care and Maternal Mortality, 1800-1950.* Oxford: Clarendon Press.

McLaren, A. 1978. *Birth Control in Nineteenth-Century England.* Holmes & Meier.

Murphy, TE. 2015. Old Habits Die Hard (Sometimes). Can Department Heterogeneity Tell Us Something About the French Fertility Decline? *Journal of Economic Growth*, **20**, 177–222.

Robertson, J.M. 1920. *Charles Bradlaugh.* Watts & Co.

Schultz, T. Paul. 1985. Changing World Prices, Women's Wages, and the Fertility Transition: Sweden, 1860-1910. *Journal of Political Economy*, **93**(6), pp. 1126–1154.

Szreter, S. 1996. *Fertility, Class and Gender in Britain, 1860-1940.* Cambridge University Press.

Teitelbaum, MS. 1984. *The British Fertility Decline.* Princeton University Press.

Wanamaker, MH. 2012. Industrialization and Fertility in the Nineteenth Century: Evidence from South Carolina. *Journal of Economic History*, **72**(1), 168–196.

WHO. 2015. *Trends in Maternal Mortality: 1990-2015: Estimates by WHO, UNICEF, UNFPA, World Bank Group and the United Nations Population Division.* Tech. rept. Geneva.

Woods, R. 1997 (March). *Causes of Death in England and Wales, 1851-60 to 1891-1900 : The Decennial Supplements.* [computer file].

Table 8: Robustness

	DV is Decadal Change in ln(Avg. Birth Rates)			
	MMR based on Non-Pueperal deaths	MMR based on Pueperal deaths	High MMR is 75-100%	Pre-existing MMR from 1871-75
	(1)	(2)	(3)	(4)
High Initial MMR ×	-0.025***	-0.014*	-0.022**	-0.027***
Trial Decade	(0.008)	(0.008)	(0.010)	(0.008)
Observations	1,720	1,720	1,720	1,720
R-squared	0.556	0.554	0.555	0.557
No. districts	430	430	430	430

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the district level, in parentheses. All regressions include district and period fixed effects. The treatment period is 1881-1871 change. Birth rates are forward looking averages (i.e., centered on the year after enumeration in the case of 3-year averages or centered on three years after enumeration in the 5-year averages).

8 Appendix