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

November 3, 2018

Abstract

The historical demographic transition is one of the most important events in history. This study provides new evidence highlighting the key role that censorship and the release of family planning information played in this event. We begin by providing evidence linking the sharp decline in fertility in Britain starting in 1877 to the public release of family planning information that resulted from the famous Bradlaugh-Besant trial. Then, to better isolate the effect of information, we show that similar declines took place in Canada among populations with strong cultural and linguistic ties back to Britain.

*We are grateful to Nico Voigtlander for his contribution to the early stages of this project. We also thank Martin Dribe, Stephan Heblich, Peter Sandholt Jensen, Krishna Pendakur, and seminar participants at Columbia and Lund for helpful comments. This project was funded by a seed grant from the California Center for Population Research and by NSF Career Grant No. . 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 turning points on the road to modern economic growth. In Britain, where the Industrial Revolution began, 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 characterize 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 a large body of work examines the underlying causes of this change. In the 1970s, work by the European Fertility Project sought to document and understand the demographic transition in Europe. One result of this effort was Ansley Coale's "Ready, Willing and Able" model, which suggested that three conditions must be satisfied for a fertility transition to take place.[2] First, as Coale argued, people must be ready, meaning that fertility control must be "within the calculus of conscious choice" (van de Kaa, 2004). Second, they must be willing, in the sense that the costs and benefits of raising children provide incentives to reduce fertility. Third, people must be able to reduce fertility, for example by having an understanding of and access to contraceptive methods for reducing fertility.

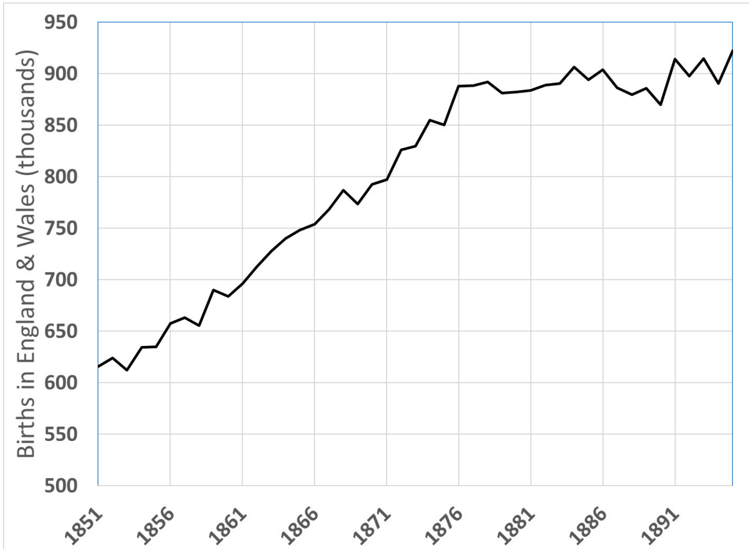
Since the 1980s, most attention, particularly among economists, has focused on the "willing" condition, i.e., on factors affecting the costs and benefits of having additional children. Motivated by theoretical work such as Becker & Lewis (1973), Galor & Weil (1999) and Galor & Weil (2000), many studies have examined how fertility responds to factors such as an increased desire to invest in human capital, changing opportunity costs of female time, the influence of industrialization, etc.[3]

[1] Allen (2001).

[2] See Coale (1973) and van de Kaa (2004).

[3] 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. Another active area 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). Some have also examined the impact of mortality (Kalemli-Ozcan *et al.*,

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



Less attention has been paid to the "ready" and "able" conditions, in part because of the difficulty in generating quantitative evidence (Guinnane, 2011).[4]

This study resurrects Coale's "ready" and "able" conditions, by presenting well-identified evidence that the release of information on family planning and contraception played an important role in the historical fertility transition. The starting point for our analysis is Figure 1, which plots the number of births in England & Wales from 1851 to 1895.[5] The sharp change in the number of births after 1876 shown in this graph is striking. As we show later, this change appears simultaneously across different regions of England & Wales and in both primarily rural and heavily urban locations. Both the sharpness of this change and its appearance across locations with a wide range of socioeconomic conditions are difficult to reconcile with factors, such as rising returns to human capital, emphasized in existing work.

2000; Ager *et al.*, 2018) and industrialization (Wanamaker, 2012; Franck & Galor, 2015) on the historical demographic transition. Additionally, there are a number of studies evaluating a range of potential determinants of the historical fertility transition in different settings. These include two studies on Sweden, Dribe (2008) and Bengtsson & Dribe (2014), as well as work on Bavaria (Brown & Guinnane, 2002).

[4]Cultural conditions received substantial attention in the work of the European Fertility Project (Knodel & van de Walle, 1986).

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

We argue that changing societal norms about family planning and access to contraceptive information explains this rapid slowdown in births. In particular, we provide evidence that this change can be traced to the famous Bradlaugh-Besant trial of 1877.[6] 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 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 and provided information about contraceptive techniques. 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.

Our argument relies on two separate but mutually-reinforcing pieces of analysis. The first focuses on England & Wales, where the trial took place, while the second part of the analysis looks at Canada, where it is easier to isolate the effect of information on fertility behavior.

We begin by studying England & Wales, where the Bradlaugh-Besant trial took place. This is the natural starting point, but it is also a difficult setting in which to study the impact of new information because reports of the trial were rapidly disseminated throughout the country by newspapers, pamphlets and books, public lectures and other means. 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.

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. We argue that one factor that satisfies these conditions, particularly after including 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

[6]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, support for this connection is based entirely on the timing of the event.

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. 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 infection. 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, providing women and their husbands with a strong incentive to avoid additional pregnancies.

Using rich panel data covering over 600 local districts of England & Wales, we show that fertility declined more rapidly after 1877 in locations with higher pre-existing maternal mortality rates. This is consistent with a lower desired fertility level in areas with higher maternal mortality which was only attainable after family planning information became available. These results hold after controlling for a rich set of variables reflecting factors commonly thought to have played a role in the historical fertility transition. We conduct a variety of placebo tests in the pre-trial period to help rule out competing hypotheses. In addition, analyzing individual-level microdata from the 1881 Census provides evidence that the key dimension of adjustment was stopping fertility earlier. These results are consistent with a story in which the information released by the trial changed social norms, allowing families to reduce fertility, with larger adjustments taking place in locations with greater latent demand for fertility control.

The second, and perhaps more convincing, part of our analysis focuses on the impact of these events on fertility in Canada. Canada is an interesting setting to consider because it was the closest large colony to Britain, with strong cultural ties. However, there were also substantial Francophone parts of the country with very little cultural or linguistic connection to Britain. Our analysis exploits this difference to provide more direct evidence on the impact of information and cultural connections in influencing fertility. We begin by providing evidence that the Bradlaugh-Besant trial and its aftermath was covered by Canadian English-language newspapers but largely ignored by the French-language press. Next, we construct panel data that allow us to study changes in fertility in the years just before and after the trial, in locations with stronger vs. weaker cultural and linguistic ties to Britain. Our results show a substantial slowdown in fertility in Canadian counties with strong cultural and linguistic ties to Britain, relative to those with weaker ties. This pattern is robust to including a fairly rich set of control variables, and even holds when focusing only on variation within the province of Quebec.

Together, the two arms of our analysis provide convincing evidence that the information released by the Bradlaugh-Besant trial had an important effect on the fertility transition in both Britain and British Canada. These results help explain both the surprising rapidity of the reduction in fertility as well as the fact that it is observable in locations with widely varying economic conditions.

In addition to work on the historical fertility transition cited above, our results contribute two other strands of research. First, our results relate to existing work on the importance of contraception, such as 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.[7] 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.[8] Another example is Bassi & Rasul (2017), which documents the fertility impact of a visit by Pope John Paul II to Brazil in 1991.

An important difference between our study and these modern studies is that we document the importance of family planning and contraceptive information in a historical setting without access to modern birth control techniques. Thus, our results show that family planning information can have an impact even when available contraceptive techniques are rudimentary. One argument against the importance of information in this historical fertility transition is that evidence suggests that the main contraceptive techniques, abstinence and withdrawal, does not appear to have changed substantially during the period of the transition.[9] However, a careful review of the literature surrounding the Bradlaugh-Besant trial shows that the main focus of these works was making the moral argument that couples have a right, and a responsibility, to choose their family size, rather than disseminating more technical contraceptive information. This point, which may seem obvious today, was

[7]See also (Bailey *et al.*, 2018).
[8]These results have been disputed by Jaeger *et al.* (2018).
[9]See Guinnane (2011) and Szreter (1996) Ch. 8.

controversial at the time. As Annie Besant wrote in 1877, "Many people, perfectly good-hearted, but somewhat 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." This suggests that the dissemination of the idea that family size should be part of "the calculus of conscious choice" may be as important as providing more specific information on contraceptive techniques.

This paper is closely related to a recent working paper by Spolaore & Wacziarg (2016), which argues that the decline in fertility in Europe was associated with the diffusion of social and behavioral norms from France, where the changes first appeared. They provide evidence that this diffusion occurred more rapidly in locations with stronger connections to France (as measured by genetic distance). We view these studies as complementary. Both papers emphasize the importance of information diffusion and cultural norms and we view our results as supportive of their main hypothesis. The main difference is that, where they focus on long-run diffusion patterns using fairly aggregated data, we study a relatively sharp change in information resulting from a particular event using detailed micro-data. On advantage of our approach is that it is somewhat easier to establish clean causal connections.

The existing literature on the historical demographic transition is extensive, so there are some important topics within this literature that we will not directly address in this study. For example, there are a substantial set of studies examining the extent to which fertility reduction appears first among elite segments of society (see Drive *et al.* (2017) for a review of this literature). While understanding these patterns is important, the changes revealed by Figure 1 are far too large to be driven solely or even mainly by the upper classes, so we set this issue aside. We also abstract from patterns observed in other countries, though it is worth noting that, with the exception of Scotland, other parts of Europe do not show a sharp break in fertility patterns around 1877 similar to the one we observe in England & Wales.

The remainder of the paper is organized as follows. We begin by introducing the empirical setting and discussion the Bradlaugh-Besant trial. We then present our analysis of fertility in England and Wales, in Section 3, followed by our analysis of fertility in Canada in Section 4. Section 5 concludes.

2 Setting: The 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.[10] 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 minister at age 20, but her increasingly secular views eventually led to a separation.[11] By 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, perhaps because it 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 and the publisher of the pamphlet, Charles Watts. The prosecutions 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 published a new version of Knowltons' book, with some updated medical knowledge and informed the magistrates and city police of the time and place of sale in order to prompt arrest and trial.

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. Against them, the Solicitor-General of England argued in his closing that[12],

[10] See Robertson (1920).
[11] See Besant (1893).
[12] Quoted from Manvell (1976), p. 147.

> *Their notion is that the population should be limited, that it would be a desirable thing that conception should be prevented. I say that this is contrary both to the law of God and the law of man, and if they choose to circulate a document of this sort, which is intended to produce that result...I say that it is immoral, and under the circumstances of the case, an obscene book, and one which ought to be condemned by any jury before whom this question might come...this is a dirty, filthy book, and the test of it is that no human being would allow that book to lie on his table; no decently educated English husband would allow even his wife to have it...*

In his summation, the Lord Chief Justice wrote, "whatever outrages the decency, whatever tends to corrupt the morals of society, and especially the morals and purity of women–whatever tends to have that result, when published, is an offense against the law."[13] The jury duly found the pair guilty. However, after numerous appeals through 1878, the verdict was finally reversed on a technicality.

The trial was widely followed by newspapers throughout the country. Banks & 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 families."[14]

Several events kept attention on the issue of family planning through 1878. First, there was 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. Also, in January 1878 Annie Besant received a notice that her husband had petitioned to forcibly remove her

[13] Manvell (1976), p. 151-2.
[14] Quoted from Banks & Banks (1954).

daughter from her care because of her secularism and advocacy of family planning. This was argued in the courts and also widely covered in the news, both in Britain and Canada. Besant eventually lost the case and was forced to give up her child.

Sales of books on family planning in England & Wales also took off. Following 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*, which had attracted little attention before 1877.[15] 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 trial could be obtained. Word of mouth was likely the most important. By 1875, 80% of Britains 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. For example, Teitelbaum (1984) suggests that many lower-class women may have been exposed to the trial by working as servants in middle-class households. In addition, Bradlaugh, Besant, and their supporters held numerous public meetings throughout the country following the trial. On 25 June, 1877, for example, the *Times* reported that,[16]

> *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...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.*

With respect to information delivery, Britain was particularly well-connected during this period. 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.[17] In that year the Post Office carried 915 millions letters, 99 million news-

[15] Owen's *Moral Physiology* is the book that Truelove was prosecuted for publishing.

[16] Quoted from Banks & Banks (1954).

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

papers, and 103 million books. There were, in addition, more than 5,000 telegraph offices and just under 12 million telegraph messages sent. Not only was Britain well-connected internally, it also maintained strong links to the colonies. Colonial papers regularly reported on important events in London, often directly quoting leading London papers such as *The Times*. Later, we will review evidence on the coverage of the trial in Canada.

Reflecting on these events, Himes (1970) writes (p. 243) that, "The social effect of these two trials [of Bradlaugh-Besant and of Truelove] 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 worth recognizing 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. As Besant writes "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.

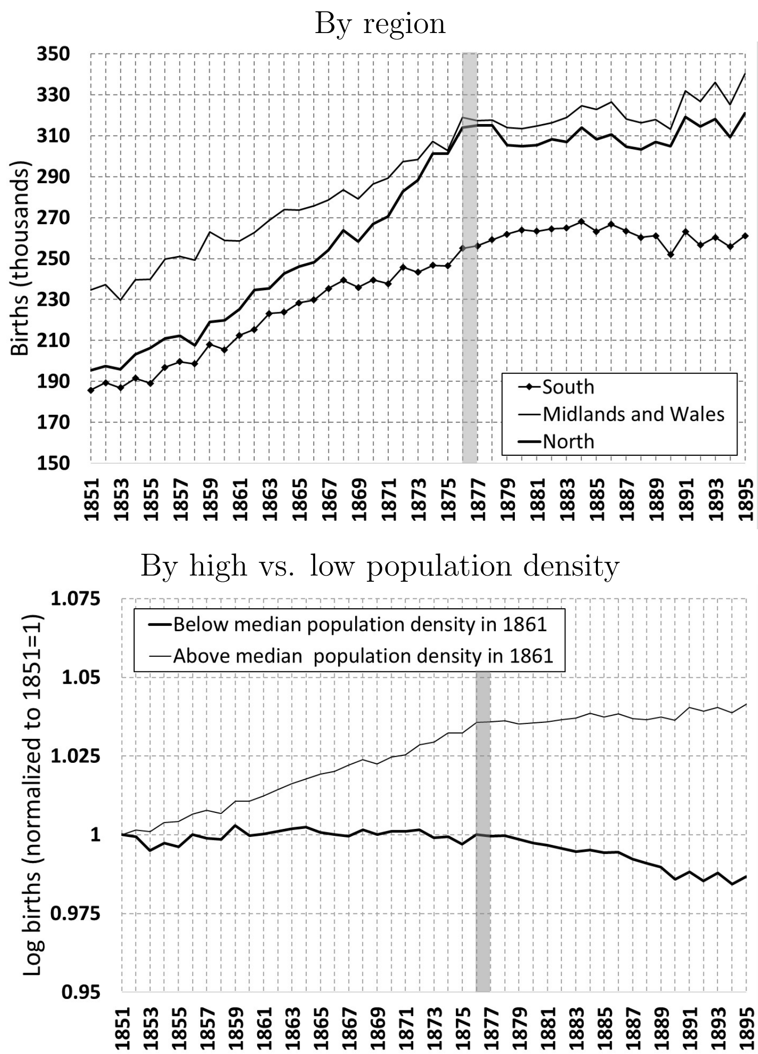
As Glass (1967) writes (p. 43), "Until the end of the War of 1914-18 the birth-control movement in England concentrated almost exclusively on spreading the *idea* of and reasons for family limitation." While more technical information was also disseminated, these often had a secondary place.[18] While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

3 Analysis of fertility in England & Wales

We have already seen, in Figure 1, 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 that this was not the result of a change in data collection practices. Rather, it appears to be a real and substantial change in fertility behavior. Moreover, the change appears to be quite broad based, as shown 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. 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 is a reduction in births which is observable in both types of districts.

[18] Knowlton's book advocates a syringe douche, while Besant recommended the sponge and *coitus interruptus*. Besant also provided erroneous information about the safest times in the cycle for intercourse and argued that nursing had no effect on conception.

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



These patterns indicate that the factor behind the change in fertility must have been national in scope. Moreover, 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 order to isolate the impact of information on fertility in Britain we need to identify a factor that shifts the demand for children but is plausibly orthogonal to the factors thought to be behind the fertility transition. 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 left 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's 9 deaths per hundred-thousand births.[19]

A brief case-history, provided in Appendix 6.1.1 illustrates some of the key features of maternal mortality during this period. One feature was that it was typically sudden and unexpected, striking women who were otherwise in good health. Maternal mortality affected all classes of women, and there is even evidence that rates were higher among the well-off, possibly because of the role that doctors played in spreading the disease (Loudon, 1986). In most cases death occurred after birth and the infant survived, which explains the weak correlation between maternal and infant mortality. The main cause of maternal mortality was puerperal infection, followed by eclampsia/convulsions and hemorrhage leading to blood loss (see Appendix 6.1.1).

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 neighbor in a nearby street who had died in childbirth."[20] 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.

3.1 Data for England & Wales

The data used in our analysis of England & Wales come from administrative sources and are available at the district level. While there are over 600 districts in England

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

[20]A back-of-the-envelope calculation suggests that this statement is reasonable. If a women knew 100 fertile-aged women over a 20 year period, say from aged 5 to 25, then given the fertility rate in England & Wales in 1871 and the maternal mortality rate of 470 per 100,000 births, we would expect 1.43 of those women to have died of maternal causes.

& Wales, many districts changed their boundaries at some point during our study period. Adjusting for these changes leave us with 430 consistent districts, spanning 1851 to 1891.[21]

Aggregate district-level data on births come from 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, reported annually, were collected on a continuous basis by trained registrars who were present in each district. For births, the data we use cover 1851-1895.[22] We combine these data with data from the Census to calculate birth rates relative to the fertile-aged female population. When calculating these rates, we use either three-year or 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, but it also means our analysis is conducted at the decade level.

Using a variety of sources we have constructed a fairly rich set of control variables reflecting key factors thought to influence fertility behavior. These include controls for female and child labor-force participation, infant and overall mortality rates, population density, local industrial structure, religious affiliation, literacy at the time of marriage, etc. Further details on the data and construction are in Appendix 6.1.2.

We also take advantage of individual-level census data from the 1881 census in order to look at the margins through which fertility adjustment occurs. These data allow us to look within families, to study whether couples were putting off 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.

[21] We combine any pair of districts in which there was a boundary change that shifted 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.

[22] There are data separating legitimate and illegitimate births starting in 1871. These show that the vast majority of births were within marriage and it is among those births that the change in fertility behavior observed in Figure 1 occurred.

3.2 Empirical Approach

We adopt a differences-in-differences framework to examine the extent to which the Bradlaugh-Besant trial affected district-level birth rates. Our main 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 rate in district d during 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).[23]

The variable MMR_d captures the pre-existing maternal mortality environment in district d. Specifically, we take the average number of maternal deaths per 1,000 births over the entire 1856-1875 time period. This is roughly the period over which a women of 25 in the mid-1870s would have been conscious of maternal deaths occurring among those she knew. $TRIAL_t$ 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.[24]

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.

[23] 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.

[24] 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.

The key assumption in this 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, which we interact with the post-trial indicator in order to allow them to have a time-varying impact on fertility.[25] 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.

As a preliminary step it is important to verify that maternal mortality is not associated with a differential trend in fertility in the pre-trial period. This is done in Appendix 6.1.4. In addition, that Appendix also shows that maternal mortality was not associated with differential trends in other factors, such as marriage rates, illiteracy, population density, or mortality, that are thought to influence fertility choices.

3.3 Results for England & Wales

Table 1 presents our main results for England & Wales. 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.[26] 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. Note that what we are identifying is the difference in the change in birth rates after 1877 in high vs. low maternal mortality districts. Low maternal mortality districts were also treated by the trial, but we expect their response to be weaker than in districts where the risk of maternal death was more salient. Thus, the magnitude of our estimates will not reflect the full effect of treatment. Despite this we still observe a substantial effect: 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 with a continuous measure of MMR. There we see that a log point increase in underlying MMR is

[25]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).

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

associated with a decline in birth rates between 1871 and 1881 on the order of 5%.

Column 2 consider our outcome as 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 sharper fertility declines following the trial.

Table 1: Main results for England & Wales

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).

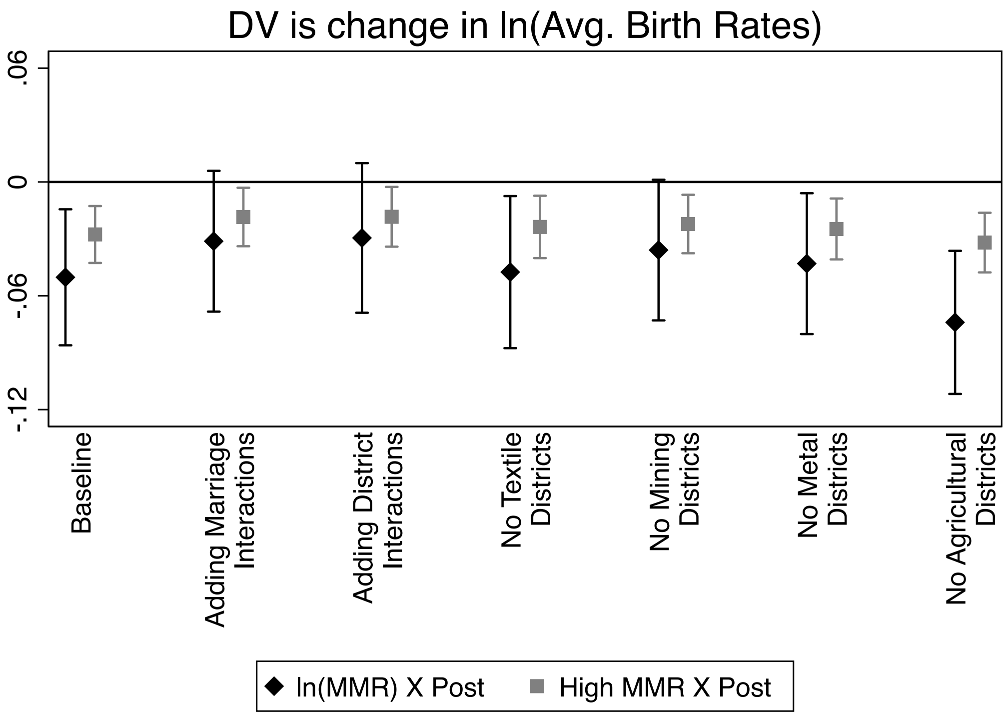
Figure 3 presents results from a series of robustness checks. For comparison purposes, we reproduce our baseline results (column 1 of Table 1) using both our continuous and discrete measures of treatment in the first entry of Figure 3.[27] The second set of results, labeled "Adding Marriage Interactions" include controls for a range of factors that are observable in the marriage data we 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

[27]The table version of these results is appears as Appendix Table 8

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. Even with these rich sets of control variables, the results change relatively little and remain statistically significant at standard levels. This indicates that maternal mortality is proxying for demand for family planning instead of other features 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 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.

Figure 3: 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 1. 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 interacted with 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.

Next, we study the mechanisms behind this change in fertility. 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. Our final sample includes 2.4 million households with 6.3 million births occurring between 1871 and 1881.[28] 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 examining heterogeneity in the response to the trial, 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. 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 dimensions along which couples could have adjusted fertility in order to generate the aggregate results documented thus far: they may choose to put off having children longer, they may choose to increase the spacing between births, they may choose to end fertility earlier, or they may choose to completely forgo having children. Our analysis (see Appendix 6.1.6) suggests that the most important margin of adjustment is earlier stopping. In particular, we estimate that amongst families that have three or more (surviving) children in 1877, those in locations with a greater pre-existing exposure to maternal mortality are less likely to have an additional child between 1877 and 1881.

These estimates are shown in Table 2. In these regressions we partition the sample based on the number of children born before 1877 and 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 2 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, or 6+ children). Each regression includes fixed effects for when the most recent pre-trial child was

[28] See Appendix 6.1.2 for details on the construction of this sample.

born, as well as fixed effects for the mother and father's birth year of birth. The results in Table 2 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 was more salient. In additional analysis, presented in Appendix 6.1.6, we also find evidence of small increases in the age of first birth in locations with higher levels of maternal mortality. However, the magnitude of this effect is relatively small.

Table 2: Effect on subsequent childbearing conditional on family size

	DV=1 if household had an additional child after 1877					
Families with X children born before trial:	1 child	2 children	3 children	4 children	5 children	6 or more children
	(1)	(2)	(3)	(4)	(5)	(6)
High Initial MMR × Born after 1877	-0.00329	-0.00508	-0.00554	-0.0109***	-0.0114***	-0.0182***
	(0.00425)	(0.00426)	(0.00378)	(0.00388)	(0.00413)	(0.00433)
Observations	319,975	334,803	310,902	257,156	182,895	186,751
R-squared	0.185	0.220	0.254	0.274	0.283	0.275
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. These OLS family-level 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.

Overall, the results from England & Wales are consistent with a story in which the release of family planning information in 1877 resulted in a fertility reduction, and in particular a reduction that was larger in places with greater latent demand for fertility control due to an elevated salience of maternal mortality risk. However, because of the inter-connected nature of the country, isolating the effect of information in this context is difficult. Thus, based only on the evidence presented thus far, one may be hesitant to draw strong conclusions. To provide more direct evidence we therefore turn to a different setting, Canada, where isolating the impact of information is easier.

4 Analysis of fertility in Canada

In this section we study the impact of the Bradlaugh-Besant trial on fertility in Canada. Here, we exploit the fact that some Anglophone areas of the country had strong cultural and linguistic links to Britain, while Francophone areas did not. This variation allows us to isolate the impact of information, transmitted through cultural or linguistic links, on fertility.

Our analysis focuses on four provinces of Canada, Quebec, Ontario, Nova Scotia and New Brunswick.[29] The population of Quebec was mainly French speaking and of French extraction with little cultural or linguistic tie to London, though some parts of the province had substantial English-speaking populations of British ancestry. The populations of the remaining three provinces were mainly English-speaking and of British (including Irish) ancestry.

As a first step we study the coverage of the trial in the Canadian press. Canadian newspapers regularly covered events in England. A review of available digitized newspapers reveals a number of articles covering the Bradlaugh-Besant trial in English-language papers. Many of these articles appear in 1878, when the conviction was overturned on appeal. For example, Montreal's English-language paper, The Gazette, ran articles related to the trial on Feb. 13, March 2, May 30, June 4, June 6, June 22 and July 15 of 1878. Similar reports appear in other English-language papers. There is also some evidence that other material related to the trial was being circulated. For example, on June 20, 1878, the Ottawa Citizen reported: "Toronto, 19th – A man named Robert Robins, alias Whittaker, was arrested today for sending indecent literature through the post...the indecent publication for circulating which he is arrested is Bradlaugh's Prints of [*The Fruits of*] *Philosophy*, the book recently prohibited in England."

We also searched for articles about Bradlaugh and Besant in the French-language press, in the Biblioteque et Archives National du Quebec. This searched turned up no articles about Bradlaugh, Besant or the trial in the late 1870s.[30] Thus, it does not

[29] Technically only Quebec and Ontario were part of Canada at the start of our study period. The Maritime Provinces of Nova Scotia and New Brunswick only joined Canada upon confederation in 1867. We do not consider other provinces of because their populations were relatively small.

[30] Reports about Charles Bradlaugh do appear after 1880, when he was elected to Parliament but argued that he should be allowed to offer an affirmation rather than taking the Oath normally required to take his seat. The controversy led to the appointment of two different Select Committees

appear that the Francophone press covered the trial extensively.

4.1 Data used in the Canadian analysis

To study the impact that these events had on fertility, we use data from the Canadian Census of Population. We conduct our main analysis at the county level. We collapse some counties in order to obtain areas that are fairly geographically consistent over time. This leaves us with 133 counties. Sixty-one of these are in Quebec, forty are in Ontario, fourteen are in New Brunswick and eighteen are in Nova Scotia. The time period covered by our analysis is 1865-1886, or roughly a decade on either side of the Bradlaugh-Besant trial. We do not use data prior to 1865 to avoid disruptions associated with the U.S. Civil War, which substantially affected the Canadian economy and in which it is estimated that forty-thousand Canadians, or about 2.5% of the male population, fought (Winks, 1998). Since these effects were likely to have been systematically different in locations with many English-speaking British-origin residents, and may have affected fertility patterns, we do not want to include years from 1861-65 in our analysis.

Because Canada did not have a registry of births at this time, we focus on the number of children available at different ages at the time of the Census, relative to the population of fertile-aged women (which typically is taken from the previous census).[31] Since the population of children is typically reported in age groups up to age five, and then from five to ten (or in some cases six to eleven), we divide our data into roughly five-year age groups. So, for example, children aged 6-10 in the 1881 census give a proxy for births occurring from 1871-76, while children aged 0-5 provide a proxy for births occurring from 1877-81. Both of these are then divided by the number of fertile-aged women in the county in 1871, and the same procedure is used

to rule on the matter and eventually led to a brief imprisonment.

[31]The Census did ask residents about the number of births that they had in the past year. However, several factors are likely to make these data problematic. First, they cover just one year, and therefore are vulnerable to random year-to-year fluctuations in births. Second, there are concerns about how consistently parents were able to recall whether children were born within a year of the census. This issue can be seen when looking at the data on children aged 0-1 and 1-2, which show a tendency for over-reporting in the 0-1 category and under-reporting in the 1-2 age category. Another reason for concern about these data is that in Ontario this series shows a suspicions very large decrease of around 3,000 births in 1871 compared to 1861, which seems unrealistic. Because of these concerns, we follow previous research by Henripin (1968) and focus instead on the number of living children enumerated in the Census.

for children listed in the 1871 and 1891 censuses. Using five-year age groups is useful because it allows us to focus more closely on the changes occurring after 1877. Of course, the number of children alive in a period will be an imperfect proxy for births in that county in that period, particularly because of infant and child deaths. This affects precision but will not bias our results unless mortality rates are differentially changing in locations with stronger British ties right around 1877.

The census provides us with several ways of measuring the strength of each county's ties to Britain. Im the main analysis we focus on the share of the population in the 1871 Census that is either Canadian born and not of French origin or born in Britain (which at this time included Ireland). Alternatively we can consider share of the population that was not Canadian-born of French origin, but these variables are only available for Ontario and Quebec. We also consider two other variables which are available for all four provinces: the share of the population that attended the Church of England or Church of Scotland, or conversely, share that was not Catholic. These are certainly not perfect measures. Much of the British-origin population were not adherents of the Church of England or Church of Scotland, and some of them, particularly the Irish, were Catholics. However, in Ontario and Quebec, where both sets of variables are available, we observe strong correlations between the indicators based on religion and those based on ancestry or location of birth. As shown below, all four measures ultimately deliver very similar results.

The census also provides a number of useful control variables, including information on population density, the share of employment in agriculture or in manufacturing, the male/female ratio (important in a society with a lot of immigration, which skewed male), as well as information about literacy and school attendance.

4.2 Analysis approach and results

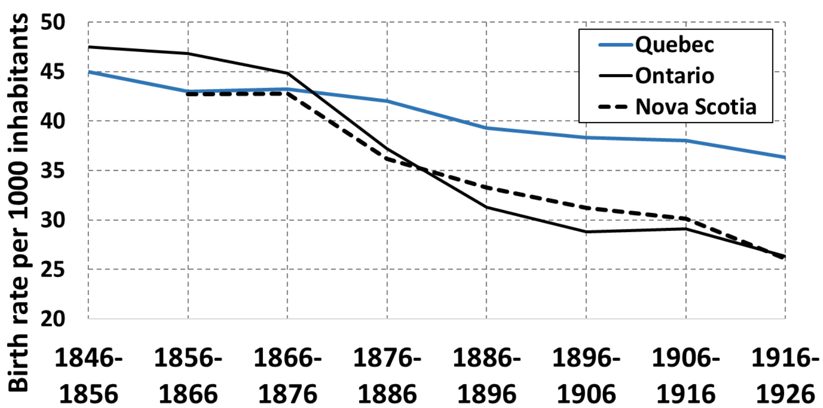
This analysis follows a standard difference-in-difference approach. The regression is,

$$ln\left(\frac{CHILD_{ct}}{FEM_{ct}}\right) = \alpha_0 + \alpha_1 BRIT_c * TRIAL_t + X_{ct}\lambda + \phi_c + \eta_t + \epsilon_{ct} \tag{2}$$

where $CHILD_{ct}$ is the number of children born in county c during period t, which is inferred based on the children observed in the census within each age group. For example, for the 1877-81 period, $CHILD_{ct}$ is the number of children in the 1881 census aged 0-5, while for the 1872-76 period we use the number of children in the 1881 Census aged 5-9. The denominator, FEM_{ct}, is the number of fertile-aged females (aged 15-50) in the previous census.[32] The main explanatory variable is an interaction between a county's pre-existing connection to Britain, based on data from the 1871 census, and a time indicator for the period after the trial. Standard errors are clustered by county and regressions are weighted by county population in 1861.[33]

Figure 4 provides a preview of our main results with raw data. These series are taken from Henripin (1968) and are calculated with census data using an approach similar to our main analysis. Up to 1876, fertility in the mainly British-origin provinces of Nova Scotia and Ontario was similar to or even higher than in Quebec. After 1876 there is a sharp decrease in fertility in the British-origin provinces and their rates drop substantially below the rates in Quebec. This provides the first piece of evidence suggesting that something substantially affected fertility in Canada after 1876, and that the effects were concentrated in locations with stronger ties to Britain.

Figure 4: Fertility patterns in some Canadian provinces



Data from Henripin (1968) Table B.6.

Our baseline regression are presented in Table 3. The first column compares fertility in the 1871-76 and 1877-81 periods while Column 2 adds additional controls.

[32]For example, when we use the 1881 census to measure the number of children born between 1871 and 1881, the fertile-aged female population denominator comes from the 1871 census. Alternatives, such as using the population of fertile-aged women in the county in the nearest census, rather than the previous census does not substantially impact results.

[33]County populations tend to be similar and so weighting has little impact on the results.

In both cases we see strong evidence that after 1877 fertility in counties with a greater share of British-origin population fell relative to fertility in the earlier period and relative to counties with a smaller British-origin population share. Columns 3 and 4 extend the study period backwards and forwards. The results in Column 4 show that there was no evidence of a similar relative reduction in fertility in more British counties between the 1865-70 and 1871-76 period, which provides support for our identification strategy. We also see that the reduction in fertility persisted into the 1881-86 period.

Table 3: Baseline regression results for Canada

DV: Children born per year / 1000 fertile-aged females				
Periods included:	1871-1881	1871-1881	1865-1886	1865-1886
	(1)	(2)	(3)	(4)
British-origin shr. × 1871-76				1.210
				(4.586)
British-origin shr. × 1877-81	-17.93***	-19.89***	-19.29***	-18.68***
	(1.554)	(1.570)	(2.526)	(4.562)
British-origin shr. × 1882-86			-22.17***	-21.57***
			(6.698)	(7.169)
Controls		Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	Yes	Yes
Observations	202	202	404	404
R-squared	0.754	0.811	0.520	0.520
No. of counties	101	101	101	101

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by county. Observations weighted by county population in 1861. Columns 2-4 include the following controls interacted with period indicator variables for each period after the first: population density in 1861, population growth in 1861-71, agricultural employment share in 1871, and the male/female ratio in 1871.

Table 4 considers alternative ways of measuring each county's connection to Britain. Column 1 reproduces the results from the last column of Table 3, which uses the share of the population of British origin to measure the county's British connection. Column 2 uses the population that is not Canadian of French origin. Column 3 uses the population of non-Catholics to measure the British connection, reflecting the fact that French Canadians were predominantly Catholic. Column 4 uses the share of the population that attended the Church of England or the Church of Scotland.[34] Note that Columns 3-4 have more observations because these measures are available for Nova Scotia and New Brunswick, while the other measures are only available in Quebec and Ontario. All four measures show very similar results, with no evidence of differential trends between 1865-70 and 1871-76 and clear evidence of a relative reduction in fertility in counties with a stronger British connection after 1876.

Table 4: Results for Canada with alternative explanatory vars.

DV: Children born per year / 1000 fertile-aged females				
Explanatory variable:	British origin pop. shr. (1)	Non-French origin pop. shr. (2)	Non-Catholic share (3)	Church of Eng/Scot share (4)
British connection × 1871-76	1.210	1.423	-0.0843	4.251
	(4.586)	(4.337)	(4.215)	(11.70)
British connection × 1877-81	-18.68***	-17.13***	-18.95***	-37.94***
	(4.562)	(4.351)	(4.420)	(12.92)
British connection × 1882-86	-21.57***	-20.30***	-20.97***	-44.25**
	(7.169)	(6.671)	(6.709)	(21.18)
Controls	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	Yes	Yes
Observations	404	404	532	532
R-squared	0.520	0.520	0.496	0.481
No. of counties	101	101	133	133

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors clustered by county. Observations weighted by county population in 1861. All regressions use data for 1865-1886 and include the following controls interacted with period indicator variables for each period after the first: population density in 1861, population growth in 1861-71, agricultural employment share in 1871, and the male/female ratio in 1871.

Table 5 considers the robustness of these results. Column 1 shows that the same patterns hold if we focus only on Quebec, exploiting the fact that even within that province there is substantial variation in the share of population of British origin

[34]Of course, only a part of those of British origin were part of one of these churches, but they do provide a good indicator of local cultural ties back to Britain.

across counties. In Columns 2-3 we add controls for the share of children in school and the share of literate adults, respectively. These are fairly strongly correlated with the share of the population of British origin. However, including these controls does not alter our main findings. It is interesting to note that there is some evidence, though not statistically significant, suggesting that fertility fell more in more educated areas after 1877. This likely reflects the impact of literacy in facilitating the spread of information. In additional results (not reported) we also find evidence a strong interaction between literacy and British connections.

In Columns 4-5 we study the impact of different immigrant groups. In Column 4 we look at the share of the population born in England, Wales and Scotland and the share born in Ireland separately. The relative reduction in fertility after 1876 appears to be even stronger with predicted using the share of English, Welsh, Scottish or Irish migrants, suggesting that the trial may have had a greater effect in places with fresher connections back to Britain. It is interesting to note that the effect of Irish migrants appears stronger than that of the English, Welsh and Scottish immigrants, though this difference is not statistically distinguishable.Two features must be kept in mind when evaluating patterns among the Irish. First, many Irish came from Ulster, and a majority of Irish immigrants were Protestant (Houston & Smyth, 1999). Also, Ireland had much lower fertility rates than in England & Wales during the decades after the Great Famine, which may have meant that the Irish-born were more open to changes in social norms surrounding fertility behavior.

Column 5 adds in the share of all non-British immigrants to the country. This provides an important check on whether the results are being driven by connections to Britain, or just the share of immigrants in a location. The fact that the share of other immigrants has no independent effect provides additional support for our identification strategy. Finally, Column 6 presents results without weighting by initial county population. These are very similar to the results obtained when weighting.

Table 5: Robustness results for Canadian analysis

DV: Children born per year / 1000 fertile-aged females						
	Only within Quebec	Controlling for shr. of children in school	Controlling for shr. of illiterate adults	Separating Eng/Scot and Irish immigrants	Separating all other immigrants	Without weights
	(1)	(2)	(3)	(4)	(5)	(6)
British-origin × 1871-76	-11.95***	-16.08***	-15.70***		-20.21***	-18.97***
	(2.934)	(4.283)	(5.422)		(1.525)	(1.911)
Eng/Scot imm. shr. × 1871-76				-42.75***		
				(10.74)		
Irish imm. shr. × 1871-76				-61.52***		
				(14.01)		
Other imm. shr. × 1871-76					5.641	
					(9.803)	
Shr. children in school × 1871-76		-10.30				
		(9.424)				
Shr. illiterate adults × 1871-76			9.923			
			(11.63)			
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	122	202	202	202	202	202
R-squared	0.864	0.814	0.813	0.751	0.811	0.745
No. of counties	61	101	101	101	101	101

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors clustered by county. Observations weighted by county population in 1861. All regressions use data for 1871-1881 and include the following controls interacted with period indicator variables for the post-trial period: population density in 1861, population growth in 1861-71, agricultural employment share in 1871, and the male/female ratio in 1871.

To summarize, the results show that after 1877 there was a substantial reduction in fertility in counties with stronger cultural ties to Britain. That this effect is closely associated with ties to Britain, whether measured by ancestry or religion, and the close temporal correspondence between this change and the reduction in birth rates within Britain, provides fairly clear evidence in favor of the idea that fertility patterns were being strongly influenced by information transmitted through cultural or linguistic links. These results are particularly striking given the enormous differences economic conditions that existed between Canada and Britain at this time.

5 Conclusion

This paper provides evidence that the release of family planning and contraceptive information resulting from the famous Bradlaugh-Besant trial of 1877 played an important role in Britain's historical fertility transition, and that this information was transmitted to Canada where it also impacted fertility among those with cultural or linguistic ties to Britain. Largely due to a lack of direct and convincing evidence, the importance of family planning information in the historical demographic transition has been set-aside in recent economic literature in favor of explanations that rely on changes in the costs and benefits of having children (Guinnane, 2011).[35] Our results resuscitate the importance of family planning information, as argued by an older generation of demographers led by Ansley Coale.

One important message to take from this paper is that family planning should be thought of more broadly 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 inform one of the most important debates in economic history. They also 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 the costs and benefits of having children.

[35] With the exception of a recent working paper by Spolaore & Wacziarg (2016).

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.

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

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.

Coale, Ansley. 1973. The demographic transition reconsidered. *Pages 53–57 of: International Population Conference, Leige 1973*, vol. 1. International Union for the Scientific Study of Population.

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.

Drive, M, Breschi, M, Gagnon, A, Gauvreau, D, Hanson, HA, Maloney, TN, Mazzoni, S, Molitoris, J, Pozzi, L, Smith, KR, & Vezina, H. 2017. Socio-economic Status and Fertilty Decline: Insights from Historical Transitions in Europe and North America. *Population Studies*, **71**(1), 3–21.

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.

Henripin, Jacques. 1968. *Tendances et Facteurs de la Fecondite au Canada*. Ottawa: Bureau Federal de la Statistique.

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

Houston, CJ, & Smyth, WJ. 1999. *Irish Emigration and Canadien Settlement*. Toronto: University of Toronto Press.

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).

Knodel, J, & van de Walle, E. 1986. *The Decline of Fertility in Europe*. Princeton, NJ: Princeton University Press. Chap. Ch. 10: Lessons from the Past: Policy Implications of Historical Fertility Studies, pages 390–419.

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.

Manvell, Roger. 1976. *The Trial of Annie Besant and Charles Bradlaugh*. New York: Horizon 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.

Spolaore, E, & Wacziarg, R. 2016 (Feb.). *Fertility and Modernity*. California Center for Population Research Working Paper No. 2016-016.

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

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

van de Kaa, D. 2004. "Ready, Willing and Able": Ansley J. Coale, 1917-2002. *The Journal of Interdisciplinary History*, **34**(3), 509–511.

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.

Winks, Robin. 1998. *The Civil War Years: Canada and the United States*. McGill-Queen's University Press.

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

6 Appendix

6.1 Appendix to the England & Wales analysis

6.1.1 Additional background on maternal mortality

This section presents additional background information on maternal mortality in England & Wales during the study period. We begin with an illustrative case-history from the Registrar General's Annual Report of 1876 (published 1878):[36]

> *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

[36]Quoted from Loudon (1992).

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). That Mrs K was attended by a doctor was not atypical, though the use of midwives was more prevalent during this period than today, and a not insignificant number delivered their baby themselves. Nevertheless, most doctors and midwives were not well 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, which helped spread 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.

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 was unknown at the time, there was very little that could be done to reduce risk, even amongst the wealthy (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 of the three, though interventions to fight hemorrhaging also 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.

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.

6.1.2 Further description of the E&W data

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.[37]

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.

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.[38]

Summary statistics for the key analysis and control variables at the district level are presented in Table 6.

[37]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.

[38]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 6: 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

To examine the mechanisms of adjustment, we also draw on micro-data from the 1881 Census of Population. To construct the sample used for this analysis, we begin with the full-count Census dataset digitized by findmypast.org and standardized by the Integrated Census Microdata Project (I-CeM). The sample we analyze is constructed 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.[39]

[39]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

This leaves us with a final sample of 2,482,788 households, 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.

6.1.3 Patterns in the raw E&W data

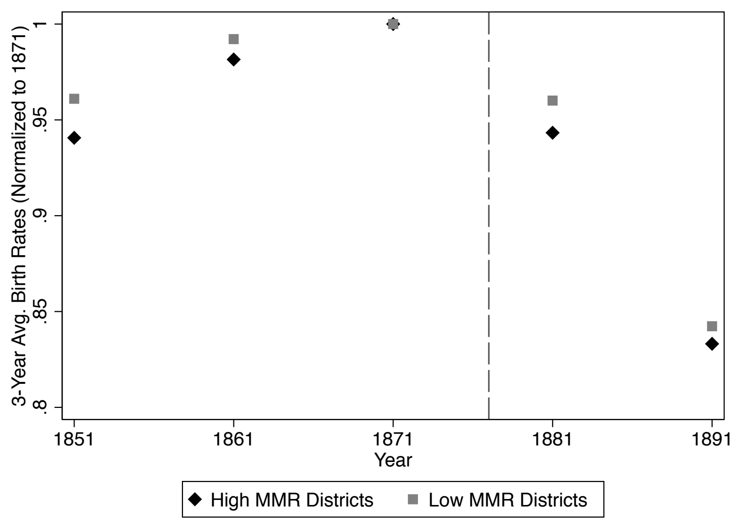
Figure 5 plots birth rates for each decade, with locations divided into those with above or below median initial maternal mortality rates. To ease interpretation we normalize each district's birth rate by dividing by its 1871 birth rate and then plot the average (normalized) birth rates for each group of districts. Although both types of districts will be treated by the information released by the trial, Low MMR districts can be thought of as the control in the sense that they should be less responsive than high MMR districts.

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.

---

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.

Figure 5: 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.

6.1.4 Placebo regressions in the pre-trial period

This section presents results from a series of placebo checks that assess 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 7. 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.

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 7: 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	0.005	0.004
	(0.010)	(0.001)	(0.018)	(0.013)	(0.009)	(0.103)	(0.016)	(0.004)	(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	0.546	0.281
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	0.019*	0.008
	(0.022)	(0.002)	(0.044)	(0.032)	(0.019)	(0.152)	(0.038)	(0.010)	(0.007)
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	0.548	0.281
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).

6.1.5 Additional robustness results for E&W analysis

This section presents the results that assess the robustness of our main result. We begin with Table 8, which presents the results that were displayed visually in the text as Figure 3. The first specification replicates our main result. The second specification examines how that main result changes when we allow contemporaneous changes in the marriage market to interact with our same trial decade indicator. The third specification presents a similar test, but uses other district-level characteristics. The final four specifications remove from our sample districts that specialized in various industries. Across each of these specifications we continue to find robust evidence that, between 1871 and 1881, birth rates fell quite substantially in districts with higher initial maternal mortality rates.

Table 9 provides some additional robustness results looking at changes in fertility in England & Wales. In Columns 1-2 we divide the available maternal mortality statistics up into the two basic types reported in the mortality statistics. Column 1 looks at deaths not attributed to puerperal infection while Column 2 looks at deaths attributed to puerperal infection. Note that there are questions about the accuracy of this division in the data, which is why we focus on overall maternal mortality in our main analysis. In any case, these results show that we obtain statistically significant results at at least the 10% level with either measure. In Column 3 we consider a different discrete cutoff for identifying high maternal mortality districts. Our results do not appear to be particularly sensitive to this change. In Column 4 we look at maternal mortality across a shorter period just before the trial. Again this adjustment does not appear to substantially impact our findings.

Table 8: Table equivalent of Figure 3

	DV is Δln(avg. birth rate)						
	Full Sample	Full Sample	Full Sample	No Textiles	No Mining	No Metals	No Farming
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Discrete treatment							
High Initial MMR × Born after 1877	-0.028***	-0.018**	-0.018**	-0.024***	-0.022***	-0.025***	-0.032***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Marriage Interactions	N	Y	Y	N	N	N	N
Distric Interactions	N	N	Y	N	N	N	N
Observations	1,720	1,720	1,720	1,548	1,548	1,548	1,548
R-squared	0.557	0.575	0.585	0.561	0.575	0.558	0.559
No. districts	430	430	430	387	387	387	387
Continuous treatment							
Ln(Initial MMR) × Born after 1877	-0.050***	-0.031*	-0.030	-0.048**	-0.036*	-0.043**	-0.074***
	(0.018)	(0.019)	(0.020)	(0.020)	(0.019)	(0.019)	(0.019)
Marriage Interactions	N	Y	Y	N	N	N	N
District Interactions	N	N	Y	N	N	N	N
Observations	1,720	1,720	1,720	1,548	1,548	1,548	1,548
R-squared	0.556	0.574	0.584	0.561	0.574	0.557	0.559
No. districts	430	430	430	387	387	387	387

*** 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 "Marriage Interactions" specifications add 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. The "District Interactions" specification interacts the following district-level characteristics with 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 exclude the top 10% of districts based on their respective textile production, mining, metal, or agriculture intensities.

Table 9: Robustness results for E&W analysis

	DV is Decadal Change in ln(Avg. Birth Rates)			
	MMR based on Non-Puerperal deaths	MMR based on Puerperal deaths	High MMR is 75-100%	Pre-existing MMR from 1871-75
	(1)	(2)	(3)	(4)
High Initial MMR × Trial Decade	-0.025***	-0.014*	-0.022**	-0.027***
	(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).

6.1.6 Microdata analysis of E&W mechanisms

This section presents additional results from analyzing microdata from the 1881 Census, which can help assess the mechanisms at plsy. 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's and 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 10. While the coefficients are positive they are small and not precisely estimated. We see that first-time father's in high MMR districts were about 1 month older on average following the trial (significant at the 10 percent level). For first time mothers we see an imprecise increase in age on the order of 2 weeks. The scale of these coefficients suggests that delaying first births was likely not the primary driver of the results that we saw earlier.

Table 10: 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	0.0320 (0.0322)	0.0818* (0.0439)		
Ln(Initial MMR) × Born after 1877			0.103 (0.0875)	0.200* (0.111)
Observations	1,074,378	1,031,318	1,074,378	1,031,318
R-squared	0.017	0.018	0.017	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, couples engaging in family planning might choose to engage in family planning once they have reached their optimal fertility level. If the spacing between the optimal fertility level and the next birth is systematically longer, say because those births are unexpected, then the introduction of family planning information might work to decrease the time between births.

Table 11 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. Families families that already have two more more children, we see that the average time between births decreases by about two weeks following the Bradlaugh-Besant trial. This pattern suggests that the selection effect is likely overwhelming the direct effect of family planning information. sant trial. This pattern suggests that the selection effect is likely overwhelming the direct effect of family planning information.

Table 11: Was birth spacing affected?

	DV is years since last birth				
	2nd Child	3rd Child	4th Child	5th Child	6th Child
	(1)	(2)	(3)	(4)	(5)
Discrete treatment					
High Initial MMR ×	0.0182	-0.0264**	-0.0363**	-0.0309**	-0.0154
Born after 1877	(0.0127)	(0.0114)	(0.0155)	(0.0146)	(0.0126)
Observations	1,078,959	967,545	767,900	530,535	314,724
R-squared	0.007	0.006	0.008	0.013	0.019
No. districts	430	430	430	430	430
Continuous treatment					
Ln(Initial MMR) ×	0.0389	-0.0539*	-0.112***	-0.106**	-0.0322
Born after 1877	(0.0345)	(0.0303)	(0.0387)	(0.0421)	(0.0314)
Observations	1,078,959	967,545	767,900	530,535	314,724
R-squared	0.007	0.006	0.008	0.013	0.019
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.

6.2 Appendix to the analysis of Canada

6.2.1 Further details on the Canada data

Table 12 presents summary statistics for the data used in the analysis of Canada. Table 13 presents the correlations between the share of British-origin population in a county and other county features. This table shows that all four of our main measures of connections to Britain are strongly correlated, though naturally for the share of the population of French ancestry or attending the Catholic church this correlation is negative. We can also see that counties with a greater British-origin population had greater school attendance rates and a greater share of the adult population that was literate. The British-origin population tended to live in counties that were somewhat less agricultural and had somewhat greater population density.

Table 12: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Children per 1,000 women per year (1865-86)	142.5	39.4	70	455	532
British origin share 1861	0.478	0.394	0.003	0.973	101
French origin share 1861	0.486	0.422	0	0.997	101
Catholic share 1861	0.524	0.362	0.012	1	133
Church of Eng/Scot share 1861	0.162	0.129	0	0.461	133
Ag. employment share 1861	0.603	0.203	0.006	0.878	133
Male/female ratio 1861	1.057	0.147	0.878	1.917	133
Share of children in school 1871	0.691	0.169	0.261	0.986	133
Share over 20 can't read 1871	0.242	0.172	0.022	0.589	133
Eng/Wal/Scot imm. share 1861	0.054	0.076	0	0.276	133
Irish immigrant share 1861	0.057	0.072	0	0.299	133
Other immigrants share 1861	0.036	0.046	0	0.272	101
Density in 1861 (persons per acre)	0.615	2.818	0.00017	27.379	133

Table 13: Correlation of British origin share with other variables

Variable	Correlation
French origin share	-0.9958
Catholic share	-0.9685
Church of Eng/Scot share	0.9192
Ag. employment share	-0.2343
Male/female ratio	0.2437
School attendance rate	0.7588
Share adults that cannot read	-0.8956
Population density	0.114
Eng/Wales/Scot. imm. share of pop.	0.8255
Irish imm. share of pop.	0.7925
Other imm. share of pop.	0.5613