Censorship, Family Planning, and the Historical Demographic Transition *

Brian Beach
William & Mary
and NBER

W. Walker Hanlon
NYU Stern
and NBER

December 18, 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. We then provide evidence that the trial had nearly simultaneous effects among British-origin populations abroad, particularly in Canada. These findings highlight the importance of information and changing social norms played in the historical demographic transition, as well as the role that cultural and linguistic ties played in transmitting this information around the world.

*Beach: Department of Economics, College of William & Mary, P.O. Box 8795, Williamsburg, VA 23187. bbbeach@wm.edu Hanlon: Stern School of Business, New York University, 44 West 4th St, 7th Fl, New York, NY 10012. whanlon@stern.nyu.edu. We are particularly grateful to Nico Voigtlander for his contribution to the early stages of this project. We also thank Francesco Cinnirella, Martin Dribe, Casper Worm Hansen, Stephan Heblich, Jane Humphries, Kevin O'Rourke, Peter Sandholt Jensen, Krishna Pendakur, and seminar participants at Columbia, Copenhagen, George Mason, Lund, NYU, Oxford, and U. of Pittsburgh for helpful comments. This project was funded by a seed grant from the California Center for Population Research and by NSF Career Grant No. 1552692.

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 experiencing the sustained increases in real wages that characterize modern economic growth.[1] This paper improves 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.

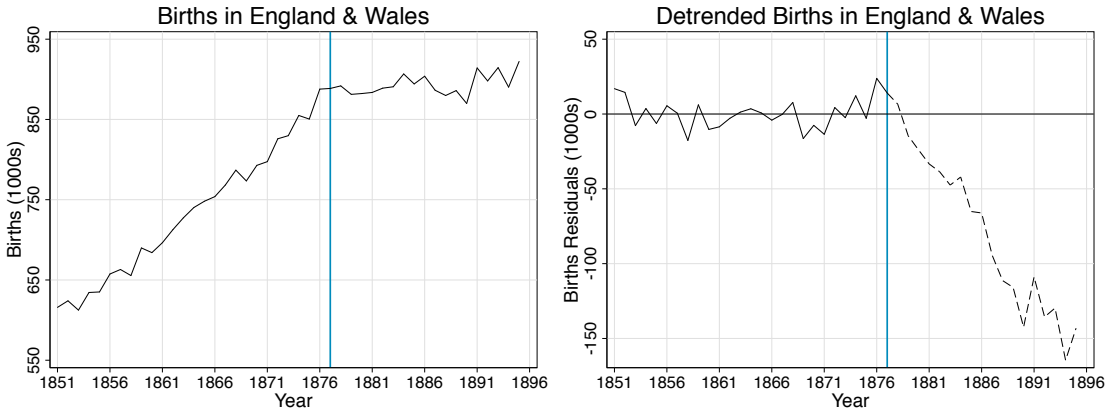
Since the 1980s, economists have focused primarily on the "willing" condition. Motivated by theoretical work such as Becker & Lewis (1973) and Galor & Weil (1999, 2000), many studies have examined how fertility responds to factors such as an increased desire to invest in human capital and changing opportunity costs of female time.[3] Less attention has been paid to the "ready" and "able" conditions, in

[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, 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.*, 2000; Ager *et al.*, 2018) and industrialization (Wanamaker, 2012; Franck & Galor, 2015) on the historical demographic

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



Births data were transcribed from annual reports of the Registrar General. The left-hand panel plots a simple time series of all births in England & Wales. The right-hand panel plots residuals from a regression that fits a linear trend between 1851 and 1877. The solid line corresponds to the observations that were in the regression while the dashed line corresponds to out of sample predictions. The vertical line at 1877 corresponds to the year of the Bradlaugh-Besant trial.

part because of the difficulty in generating quantitative evidence (Guinnane, 2011).[4]

While existing studies provide convincing evidence that the economic costs and benefits of having children influence family size, there are still some historical fertility patterns that remain difficult to explain based purely on economic forces. For example, as illustrated in Figure 1, the onset of Britain's demographic transition occurred rapidly, starting in 1877. The sharp change in the number of births in England & Wales is striking and, as we will show later, occurred simultaneously across regions of the country, and in both rural and urban areas. Both the sharpness of this change and its appearance across locations with a wide range of socioeconomic conditions are difficult to reconcile with purely economic drivers. An additional mystery appears when Britain is compared to France, which followed Britain in terms of industrialization and economic growth, but where fertility began to fall many decades earlier (Guinnane, 2011).

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).

This study offers an explanation that can help reconcile these patterns with standard models of the historical fertility transition. In particular, we argue that changing societal norms about family planning and access to contraceptive information explains this rapid slowdown in births. Put another way, we provide empirical support for "readiness" and "ability" being two of three necessary conditions for a demographic transition to occur, as first hypothesized by Coale (1973).

Our evidence indicates that the sharp regime change in British fertility can be traced to one event, the famous Bradlaugh-Besant trial of 1877, and that the impact of the information released by this trial reached far beyond British shores.[5] 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 arms of analysis. The first focuses on England & Wales, where the trial took place, while the second part of the analysis looks abroad, at the simultaneous fertility slowdowns among Anglophone populations in British colonies, particularly Canada.

England & Wales is the natural starting point for our analysis. To find variation in exposure to the information released by the Bradlaugh-Besant trial, we search through a rich archive of historical newspapers and identify articles about the trial. Our search identified over one thousand relevant articles published in the year that the trial took place. Each article observation includes details on the place of publication, allowing us to generate a local measure of exposure for each registration district in Britain based on the number of articles published within a given geographic radius. With this measure in hand, we adopt a differences-in-differences framework that classifies treated districts as those with above median exposure.

[5]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.

Our baseline results indicate that fertility declined more rapidly after 1877 in locations with greater exposure to newspaper articles on the Bradlaugh-Besant trial. To ease concerns about endogeneity, we include a rich set of variables reflecting factors commonly thought to have played a role in the historical fertility transition and find similar results. We also conduct a variety of placebo tests in the pre-trial period to help rule out competing hypotheses. In addition, we also analyze individual-level micro-data from the 1881 Census in order to identify the key dimension of fertility adjustment. This analysis provides evidence that ending fertility earlier was a key margin of adjustment, while we also find evidence consistent with increased birth spacing and an increase in mothers' age at the time of their first birth. 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 exposure to the trial.

In the second part of our analysis we consider the impact of these events among Anglophone populations outside of the U.K., particularly in British colonies. This provides additional evidence on the importance of information and changing social norms in the historical fertility transition, as well as the role played by cultural and linguistic ties in transmitting this influence around the world. Our main analysis in this section focuses on Canada, which provides an ideal setting to consider these effects. Canada 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. 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 (Francophone) areas with weaker ties, in the years just after 1877. 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. We also analyze microdata from the 1881 Canadian Census, where it is possible to separately identify British-origin and French-origin households. This analysis shows that, even within the same location, British-origin

families experienced a relative fertility decline just after 1877. Moreover, the margins of fertility adjustment that we document in Canada are very similar to those observed in England & Wales.

We then briefly consider the impact of the trial among other Anglophone populations. In South Africa, we compare fertility patterns among the British-origin population to those among the Dutch-origin Afrikaners. As in Canada, we find evidence of a relative decline in fertility in locations in South Africa where the population had stronger links back to Britain. We also document that fertility declined in Australia within a few years of the British decline.

Together, the two arms of 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 among more distant Anglophone populations. 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.

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.[6] To reconcile this existing evidence with our results, we conducted a careful review of the literature surrounding the Bradlaugh-Besant trial. This assessment revealed that the main debate during the trial, and the vast majority of the literature related to the trial, was not focused on specific contraceptive techniques. Rather, the central debate was over the very idea that couples should have a right, or even a responsibility, to choose their family size. As Woods (1987) writes, "The very question of 'how many children should we have?' was new to most Victorians." The idea that family size should be a conscious choice, which we take for granted today, was highly controversial in Victorian society. 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.

[6]See Guinnane (2011) and Szreter (1996) Ch. 8.

These findings enrich our understanding of one of the most important turning points in human history. In addition to contributing to the literature cited above, our results reinforce ideas recently put forth 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. We view our results as complementary to their empirical findings, which provide evidence that this diffusion occurred more rapidly in locations with stronger connections to France (as measured by genetic distance). 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 show that a specific event can lead to rapid and substantial changes.

Our findings also provide a new twist on existing work focusing on the role of cultural ties and social norms in influencing behaviors such as fertility decisions. One strand of existing work in this area relies on the epidemiological approach, which considers variation in outcomes among different immigrant groups exposed to the same local institutional and economic environment (Fernandez, 2011). Fernandez & Fogli (2009), for example, study how the fertility decisions of second-generation American women are influenced by culture, as proxied by the location their ancestors immigrated from. Many other studies document similar forms of cultural persistence (e.g., Alesina *et al.* (2013)). This paper offers a somewhat different take on the role of culture, by showing how cultural and linguistic ties can also act as agents of change, transmitting shifting cultural norms across locations.

Our results are also linked to work on the importance of contraception, such as Bailey (2010). An important difference between our study and these modern studies in this area is that we document the importance of family planning and contraceptive information in a historical setting without access to modern birth control techniques. Our results suggest that disseminating ideas related to the morality of family planning may be just as important as releasing more practical contraceptive information, and that such ideas may lead to substantial effects even when modern contraceptive technologies are unavailable.[7]

[7]A recent study by Kearney & Levine (2015) also provides evidence that information that alters social norms can have a substantial effect on fertility levels, though these results have been disputed by Jaeger *et al.* (2018).

It may be surprising that a single event can have such a profound impact on fertility behavior. However, other recent studies have documented similar impacts from individual events. For example, Alsan & Wanamaker (2018) shows that the disclosure of the Tuskegee Experiment in 1972 had important and long-lasting effects on mistrust of the medical system among African-American men. Another example is Bassi & Rasul (2017), which documents the fertility impact of a visit by Pope John Paul II to Brazil in 1991.

Finally, it is useful to note that the existing literature on the historical demographic transition is so extensive that there are some important topics 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, which also covered the trial, 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 provide evidence for other colonies, South Africa and Australia. Section 6 concludes.

2 Setting: The Bradlaugh-Besant Trial

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, a well-known secular activist and reformer.[8] Bradlaugh, together with Annie Besant, another active secularist reformer, realized the case against Watts could be used as a means of gaining publicity for their views on family planning as well as a test case on the government's right to censor work of this kind.[9] They decided to publish 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 trial attracted substantial public attention. A large crowd assembled for the first hearing of the trial was at Guildhall in April, 1877.[10] The main trial, at the Queen's Bench, 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.[11] Against them, the Solicitor-General argued in his closing that[12],

> *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...*

Ultimately, the jury found the pair guilty of publishing a book "calculated to deprave the public morals" while at the same time the jury "entirely exonerate the defendants from any corrupt motives in publishing it." They were fined £200 each and sentenced

[8]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. See Robertson (1920) for a more detailed biography.

[9]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 (Besant, 1893). By mid-1877 she was an active speaker on secularism.

[10]Ledbetter (1976) reports that over 20,000 people gathered outside.

[11]An account of the trial was published by Bradlaugh and Besant: fre (1877).

[12]Quoted from Manvell (1976), p. 147.

to six months imprisonment. However, after numerous appeals, the verdict was finally reversed on a technicality in 1878.

The trial brought substantial attention to a subject which had long been taboo in Victorian society. One reflection of this attention was the wide coverage in newspapers throughout the country as well as abroad. 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*. The relatively high levels of literacy among married couples during this period (around 80%) and the large numbers of newspapers situated all around the country, meant that many people were probably exposed to the trial through this channel.[13] 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] In the U.S., a front page article in the New York Times (April 23, 1877) complained that, "The mischief of this kind of trial lies in its being reported, if not in full, in a manner that is filthily suggestive, by all the papers. Young people who never of heard of the Malthusian philosophy will begin to inquire into it, and in the meantime *The Fruits of Philosophy* has had a circulation that it never would have had but for the Bradlaugh interference, while in the future all sorts of brutes will risk their liberty for the profits of a surreptitious sale of the obnoxious book." Interestingly, most of the coverage focused on the moral aspects; fearing prosecution themselves the papers appear to have completely avoided publishing any specific details related to contraception.

Newspapers, while important, were not the only way that people could have

[13]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. In that year the Post Office carried 915 million letters, 99 million newspapers, and 103 million books.

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

learned about the trial.[15] After the trial, Bradlaugh and Besant gave numerous public lectures around the country.[16] Word of mouth was no-doubt important. Teitelbaum (1984) also suggests that many working-class women may have been exposed to the ideas discussed in the trial through their work as servants in middle-class households. Private letters were also likely to have been important, particularly for spreading word of the trial overseas.

The trial generated an enormous increase in sales of books on family planning in England & Wales. 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.[17] Overall, Himes (1970) estimates that (p. 251), "Probably not less than a million tracts...were sold in England between 1876 and 1891." This is substantial given that the population of the U.K in 1871 was just over 31 million.

Two further events kept these issues in the newspapers. First, there was the prosecution in 1878 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 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. Besant eventually lost the case and was forced to give up her child.

[15]The trial also led to the founding of the Malthusian League, an organization dedicated to spreading family planning ideas. One of the chief activities of the League was producing pamphlets to publicize family planning knowledge. Rough estimates based on expenditure data from D'Arcy (1977) indicate that the League printed over 850,000 pamphlets in the years from 1879-1889.

[16]On 25 June, 1877, for example, the *Times* reported that, "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" (quoted from Banks & Banks (1954)). Elderton (1914) documents visits by Bradlaugh and Besant are far afield as Leigh, near Manchester, where "well-attended meetings were held at which neo-Malthusian doctrines were advocated and tracts distributed." Other examples documented by Elderton (1914) include that (p. 68) "Mr. Bradlaugh came to Manchester a number of times.," and that (p. 187) in Jarrow, near Newcastle, "There were occasional visits of Mrs. Besant and others to the district."

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

One indicator of the level of public interest in the trial are the amount of funds subscribed by the public to pay for Bradlaugh and Besant's expenses, as well as those related to the Truelove trial. Ledbetter (1976) reports that by 1878, £1,692 had been raised from public subscriptions. Letters of support also provide some indicator. Besant, in her autobiography, writes of "letters from thousands of poor married women–many from the wives of country clergymen and curates–thanking and blessing me..." (Besant, 1893, p. 82). The count may be an exaggeration, but her mention of wives of the clergy is revealing. By August 1893, the *Christian World* newspaper would write, "There was a time when any voluntary limitation [on family size] was regarded by pious people as interfering with Providence. We are beyond that now..."[18]

News about the trial spread rapidly outside of England, particularly in British colonies. In Section 4 we will discuss the coverage of the trial in Canada. In Australia, a similar trial, prompted by publication of Besant's *Law of Population*, reached the Supreme Court of New South Wales in January of 1888. The presiding judge, Justice Windemeyer, drawing heavily on the statements of the judge in the original Bradlaugh-Besant trial, ruled that Besant's book was not obscene and publication of the book not illegal. There is also some evidence of effects in the U.S. The trial was covered extensively in American newspapers from New York to Kansas to Hawaii. Four new U.S. editions of *Fruits of Philosophy* were released in the second half of the 1870, by publishers in Boston, Chicago and Kentucky (Brodie, 1994). An American edition of Besant's book was published in New York in 1878 and second edition in 1886.

We have also considered some retrospective indicators of the importance of the trial relative to other similar events during this period. One way to do this is to study chronologies of important events. The People's Chronology, for example, lists important events in each year and includes a category related to population. The only event in that category in 1877 is the Bradlaugh-Besant trial.[19] Moreover, there do not appear to be similarly important events in the surrounding years that were likely to have influenced fertility in a similar way. The *Canadian Almanac*, which also publishes a list of memorable events in each year, includes two entries related to the

[18]See (Besant, 1893, p. 82).

[19]The entry states that "The tabooed subject of contraception comes into the open at the Bradlaugh-Besant trial."

trial in 1877. Some historians are also convinced of the importance of the trial. Himes (1970), for example, writes that (p. 240), "The social effects of the publicity attending this prosecution were nothing less than revolutionary," while Elderton (1914) writes that the trial, "legitimized the teaching of practical methods for the limitation of the family."

Finally, it is important to recognize that, both in the trial and in the publications that surrounded it, there were two central issues being debated. The first issue, and the one that attracted the most attention, was the debate over whether couples–and the literature in this period was always aimed at couples and focused on choice within marriage–had a moral right to choose their family size. While the idea that couples should have such a right may sound obvious today, this was a controversial point at the time. Many, like the Solicitor General quoted above, believed that such a choice was "contrary both to the law of God and the law of man." In response, the bulk of the family planning books and pamphlets published during this period were dedicated to arguing that, in fact, couples did have a right, and even a responsibility, to choose.[20] The second important issue was whether specific contraceptive information could be provided at a cost that was affordable to the working class population. This was an important issue in the trial, but the literature spent far less attention on this topic, perhaps because reasonably effective methods, such as abstinence and withdrawal, were already well known. The specific contraceptive procedures recommended in literature during this period was also quite rudimentary, and in some cases dead wrong.[21]

[20] 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.

[21] Knowlton's book describes the unreliability of withdrawal, clarifying common misconceptions about partial withdrawal. He advocates a syringe douching with a chemical compound, which he believed was more effective and less intrusive than other methods. Besant recommended the sponge and withdrawal. Besant also provided erroneous information about the safest times in the cycle for intercourse and argued that nursing had no effect on conception. As to the efficacy of these rudimentary technologies, David & Sanderson (1986) develop a model of family size as a function of frequency, gestation, and barriers to fetus survival (e.g., miscarriage rates) and conception. Their baseline model predicts that a married couple taking no efforts to curb fertility could expect nine live births over the duration of their marriage. If the couple adopted a method of contraception with a 12.5% failure rate and they adopted that method just 10 percent of the time – numbers that match what we know about withdrawal in modern populations – their expected family size would

The importance of the debate over the morality of family planning is important for our story, because it suggests that information may matter even when specific contraceptive techniques do not change. 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." This can help reconcile our results with existing evidence suggesting that the methods of contraception changed little across our study period.

3 Analysis of fertility in England & Wales

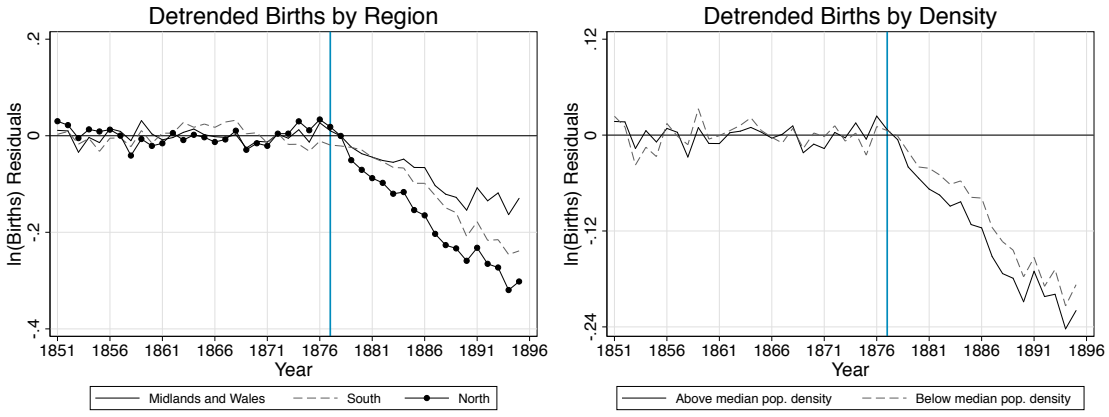
The starting point for our analysis is to simply examine patterns in the raw data. We have already seen, in Figure 1, a visible change in the number of births occurring in England and Wales in the years just after 1877 compared to the decades just before. A review of historical reports of the Registrar 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. As in Figure 1, we normalize the data by plotting residuals after fitting a linear trend between 1851 and 1877. The left panel of Figure 2 breaks down births into broad regions of the country. Despite specializing in different industries, all three regions experience a relative decline in fertility after 1877. The right panel divides districts into urban and rural based on whether they are above or below the median district population density in 1861. Again, we see that following 1877 there is a relative decline in fertility for both sets of districts, with the decline being most pronounced amongst urban districts.

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 many different industries and living in many different communities. A nationwide change in norms about family planning certainly fits these requirements, however, to better establish causality it is important to move beyond these simple time series comparisons.

---

fall to three.

Figure 2: Detrended Births by Region and Density



Births data were transcribed from annual reports of the Registrar General. The left-hand panel plots residuals from a regression that fits a linear trend between 1851 and 1877 for each region. The right-hand panel plots residuals from a regression that fits a linear trend between 1851 and 1877 for urban and rural areas. The vertical line at 1877 represents the year of the Bradlaugh-Besant trial.

3.1 Quantifying Exposure to the Trial

To assess the impact of the trial on fertility, we construct a measure of exposure based on a search of extant newspaper articles for mentions of the Bradlaugh and Besant trial. Of course, newspapers were only one way that people could be exposed to the trial, but historical sources suggest that they played a central role in disseminating information about the trial within England & Wales. Studying newspapers also has the advantage that we can track articles about the trial in a systematic way.

Our newspaper source is britishnewspaperarchive.co.uk, which partnered with the British Library and the UK genealogy service findmypast.com to digitize 40 million newspaper pages from the British Library's collection. Each newspaper article is indexed by place and date of publication. Given the uniqueness of the names "Bradlaugh" and "Besant," we identify relevant articles as those published in England & Wales in 1877 where either of the two names appears in the text. There were 1,149 articles matching these criteria.

In Appendix Figure 5 we show that the timing of the published articles matches pivotal moments of trial coverage and in Appendix Figure 6 we document similar aggregate patterns among other search terms (i.e. "Fruits of Philosophy" or "Population

Question"). Mentions of Bradlaugh and Besant appear to offer the most comprehensive measure of coverage, so we use articles identified based on those search terms as our primary measure. Notably, there were effectively no mentions of any of these terms from 1870 to 1876 (Figure 6). There was a clear peak in 1877 and sustained mentions in 1878, the year in which Bradlaugh and Besant successfully overturned their conviction on appeal.[22]

The place of publication is reported as the city in which the newspaper was headquartered. We match these locations to registration districts, the smallest administrative unit we have consistent birth and population data. While there are over 600 districts in England & 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.[23] For each registration district, we tabulate the number of articles published within a 25 km band, which we interpret as the district's exposure to the trial. The median district was exposed to 6 articles on the trial in 1877.

3.2 Data for England & Wales

Our primary outcome of interest is the district-level fertility rate. We construct this by transcribing data from annual reports from the Registrar General's Office. We transcribe each report on births from 1851 to 1895.[24] 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 three-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, 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

[22]Interestingly, from 1879 to 1890 we also see consistent mentions of "Fruits of Philosophy", though nowhere near the peak in 1877 and 1878. These mentions tend to fall into two categories: first, reports of arrests for selling counterfeit copies of the "Fruits of Philosophy" pamphlet, and second, articles on Bradlaugh or Besant who remained in the public-eye for much of the 1880s.

[23]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.

[24]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.

level.

Using a variety of sources, we have constructed a 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 7.1.1.

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.

3.3 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,

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

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).[25]

The variable $ARTICLES_d$ captures district d's exposure to the trial. Specifically, we consider a discrete measure where our treatment districts are those with above median exposure (i.e. more than 6 articles published within 25 km in 1877) and our

[25]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.

control districts are those with below median article exposure. The variable $TRIAL_t$ 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.[26]

The period fixed effects absorb average changes between each period, including our primary period of interest (1871 to 1881). However, as noted above, identification of the response to the trial comes from variation in the exposure to coverage of the trial. The key assumption in this identification strategy is that article exposure 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.[27] We also calculate results while dropping particular types of locations, such as those with economies based on agriculture, textile production, or mining, to ensure that local industrial composition is not driving our results.

3.4 Results for England & Wales

Table 1 presents our main results for England & Wales. Column 1 offers a baseline estimate before we add our district-level controls. Note that what we are identifying is the difference in the change in birth rates after 1877 in high vs. low exposure districts. Low exposure districts were also treated by the trial, but we expect their response to be weaker than in districts where the coverage was more widespread. 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 exposure districts saw their birth rates fall by roughly 3.5% following the trial relative to low-exposure districts.

[26] 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.

[27] 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).

To alleviate concerns that our results are being driven by changes in the marriage market, column 2 adds to our baseline specification the interaction between our trial decade indicator and each of the following district-level variables: the marriage rate from 1871-73, the share of marriages that took place at the Registrar's office (i.e., non-religious), the share of marriages that took place in a Catholic church (which also helps control for the Irish population in each location), the share where the bride or groom were minors, and the share where they were illiterate. The inclusion of these interactions slightly attenuates the results but the qualitative story is largely unaffected: places with more exposure to trial coverage saw large and statistically significant declines following the Bradlaugh-Besant trial.

Column 3 goes even further by including the interaction between our trial decade indicator and each of the following district-level characteristics: 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, and the district mortality rate among those aged 0-5. These interactions attempt to account for time-varying changes in other characteristics that are more closely related to existing theories of the demographic transition. Relative to column 2, the inclusion of this rich set of controls does not meaningfully affect the results.

We view column 3 as our preferred specification because it attempts to control for a wide variety of potential confounding mechanisms. Column 4, however, builds upon this specification to provide empirical support for the parallel trends assumption embedded in our identification strategy. That is, we want to make sure that changes in fertility in high news districts were not systematically different from low news districts prior to the trial, particularly after controlling for other confounding factors. We implement this by adding an indicator for the pre-trial decade change (i.e. 1861 to 1871 change), which we interact with our high news exposure treatment variable. This specification also includes the interactions with each of our controls. The inclusion of these interactions does not affect our point estimate for our treatment variable (high news exposure interacted with the trial decade), but more importantly the point estimate for the pre-trial decade interaction is effectively zero and statistically insignificant.

Table 1: Main results for England & Wales

DV is Decadal Change in ln(Avg. Birth Rates)				
	(1)	(2)	(3)	(4)
High News Exposure × Pre-Trial Decade				0.001
				(0.010)
High News Exposure × Trial Decade	-0.036***	-0.021***	-0.018**	-0.017**
	(0.008)	(0.008)	(0.008)	(0.008)
Period fixed effects	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes
Marriage X trial interactions		Yes	Yes	Yes
Other district X trial interactions			Yes	Yes
R-squared	0.594	0.618	0.628	0.669
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 weighted by 1851 district population. The trial decade is the 1881-1871 change, while the pre-trial decade is the 1861-1871 change. Birth rates are 3-year forward looking averages (i.e., centered on the year after enumeration). "High News Exposure" districts are those where the number of articles published on the Bradlaugh-Besant trial within a 25km band is above the median (i.e., 7 or more articles published in 1877). The "Marriage X trial 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 "Other district X trial 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). In column 4 follows this empirical specification, interacting each of our marriage and district characteristics as well as our "High News Exposure" variable with and indicator representing the 1861-1871 decadal change.

Appendix Table 8 provides further balance tests. That table also shows that high news exposure districts did not experience differential changes in other factors, such as marriage rates, literacy, population density, mortality, child labor force participation, or female labor force participation. All of these factors are often seen as important drivers of fertility choices. That we don't see differential changes in fertility or other factors that may, in turn, affect fertility increases our confidence in the parallel trends

assumption needed to interpret the main results of Table 1 as causal.

In Appendix Table 9 we show that the results from this specification are not being driven by districts that specialize in specific occupations. In that table, we consider what happens when 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 (column 1), mining (column 2), metal (column 3), or agriculture (column 4). We also show that results are not sensitive excluding heavily urban or rural districts (i.e. the top and bottom 10% of districts based on population density).

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 news exposure. As long as infant mortality rates are not systematically differing for high-exposure 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. For example, we find evidence that the age of first birth for both mothers and fathers increased in locations more exposed to the trial after 1877, by a few months (Appendix 7.1.3). However, the most important margin of adjustment

[28] See Appendix 7.1.1 for details on the construction of this sample.

is earlier or greater births spacing. 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 news exposure 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 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 exposure districts, families residing in high exposure 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 with more exposure to the argument that it was moral for families to cap their family size.

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 News Exposure District	-0.014***	-0.011***	-0.013***	-0.019***	-0.024***	-0.027***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
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	0.276
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 that these reductions were larger in places with greater exposure to the trial. Next, we look at whether similar effects can be observed among the British-origin

population living abroad.

4 Analysis of fertility in Canada

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

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 ancestry with little cultural or linguistic tie to London, though some parts of the province had substantial English-speaking populations of British ancestry.[30] The populations of the remaining three provinces were mainly English-speaking and of British (including Irish) ancestry.

There is evidence that the trial was covered in English-language Canadian newspapers, though other factors, such as private letters and the flow of recent immigrants, were likely to have been just as important in transmitting the effects of the trial across the Atlantic. Unfortunately, English-language Canadian newspapers have not been systematically digitized on a large scale. However, a review of some digitized newspapers as well as a number of microfilm shows that the Bradlaugh-Besant trial was covered in English-language papers. For example, The Globe, in Toronto, the largest English language newspaper, mentions the trial on April 20, June 19 and June 22 of 1877. We have also find reports in a number of other English-language papers, including the Ottawa Daily Citizen (July 23, 1877; Feb 12 and June 20, 1878), the New Brunswick Morning Advertiser (June 19, 1877), the Naniamo Daily News (July 14, 1877) and the Montreal Gazette (Feb. 13, March 2, May 30, June 4, June 6, June 20 and July 15, 1878). There is also some evidence that other material related to the trial was being circulated. For example, on June 20, 1878, the Ottawa Citi-

[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]Notably, the French-Canadian population did not undergo the early fertility transition observed in France.

zen 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." However, a search for articles about Bradlaugh and Besant in the French-language press, in the Biblioteque et Archives National du Quebec, turned up no articles about Bradlaugh, Besant or the trial in the late 1870s.[31] Thus, it does not appear that the Francophone press covered the trial extensively.

In addition to newspapers, the Anglophone population was likely to have learned about the trial through private correspondence with family and friends back in England. These letter flows were substantial; in 1884 the Postmaster General's report shows over 1.8 million letters reached Canada from the U.K, along with over 2.1 million circulars, pamphlets, books and newspapers. Information may have also flowed in through the tens of thousands of migrants that arrived on Canadian shores each year, many from the British Isles (McInnis, 1994).

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.

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

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).[32] 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 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. In 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

[32]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.

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.

As in our analysis of England & Wales, we also draw on individual-level data from the 1881 census in order to look at the margins through which fertility adjustment occurs. One particular advantage of the Canadian microdata is that it contains information on ethnic origin, which allow us to cleanly identify British households and compare them to French households. As we discuss below, this allows us to adopt an empirical specification that cleanly examines relative changes in fertility among British and French households, which in turn allows us to examine whether the Canadian results are being driven by cultural diffusion.

4.2 Analysis approach and results

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

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

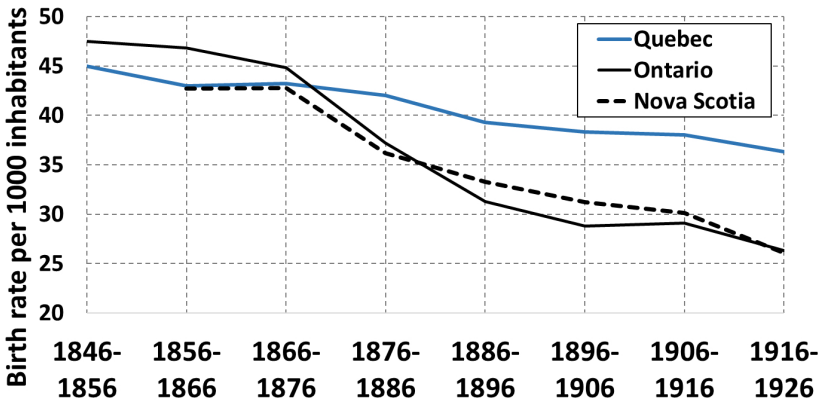
where $CHILD_{ct}$ is the number of children born in county c during period t, which is inferred based on the children observed in the census within each age group. For example, for the 1877-81 period, $CHILD_{ct}$ is the number of children in the 1881 census aged 0-5, while for the 1872-76 period we 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.[33] 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.[34]

[33]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.

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

Figure 3 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 3: Fertility patterns in some Canadian provinces



Data from Henripin (1968) Table B.6.

Our baseline regressions are presented in Table 3. The first column compares fertility in the 1871-76 and 1877-81 periods while Column 2 adds additional controls. 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, the 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.[35] 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.

[35]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.

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 (4.586)	1.423 (4.337)	-0.0843 (4.215)	4.251 (11.70)
British connection × 1877-81	-18.68*** (4.562)	-17.13*** (4.351)	-18.95*** (4.420)	-37.94*** (12.92)
British connection × 1882-86	-21.57*** (7.169)	-20.30*** (6.671)	-20.97*** (6.709)	-44.25** (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 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.

As in our analysis of England & Wales, we now draw on microdata to examine the mechanisms at play. The Canadian census was also enumerated in 1881 and our final sample of 288,628 households with 1.19 million births occurring between 1871 and 1881.[36] One advantage of the Canadian census is that, in addition to attaching parental age and birth order to each birth, this census allows us to identify whether the household is of British or French origin. This variable allows us to compare the responses of British-origin households to French-origin households that resided in the same districts after the trial, which in turn offers the cleanest test for our cultural transmission hypothesis.

The result of this exercise appears in Table 6. This table, and empirical approach, mirrors Table 2 in that we restrict our sample to families based on the number of children born before 1877 and then ask whether British households were less likely to have a subsequent child born between 1878 and 1881. For these Canadian results, however, we include district fixed effects because treatment is based on the household's cultural ties to Britain rather than their place of residence. Thus, our analysis is comparing Anglophone to Francophone families residing within the same county. Our results show a clear gradient: Canadian households of British origin are less likely have a subsequent child after the trial and the effect is larger for families with more children born before 1877. This suggests that earlier stopping at parities of 3 or above was a key mechanism of fertility adjustment in Canada.

[36]One drawback of the 1881 Canadian census is that it did not record family relationships within the household. We overcome this issue in the following way. First, we throw out any household with more than one married male or more than one married female. This removes households that contain boarders or extended family. We then ask whether it is reasonable to assume that the two married individuals are in fact married to each other. Here we discard any household where the inferred wife is either more than 7 years older than or 17 years younger than her inferred husband. These numbers correspond to the 99th percentile of what age distributions from the British microdata. Next we restrict the sample to the set of households where is reasonable to assume that this married couple is in the mother and father of all remaining members of the household. Specifically, we discard any household if the inferred mother's age at birth is greater than 50 or less than 14 or if the inferred father's age at birth is greater than 60 or less than 14.

Table 6: 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)
British Household	-0.004	0.006	-0.022***	-0.017**	-0.032***	-0.039***
	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.006)
Birth yr. FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	40,197	42,461	39,676	34,334	29,720	57,646
R-squared	0.142	0.160	0.200	0.231	0.265	0.306
No. districts	192	192	192	192	192	192

*** 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 county fixed effects as well as 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.

5 Response in other British Colonies

The previous section on Canadian fertility provides support for our hypothesis that the Bradlaugh-Besant trial shifted norms about family planning in Britain by tracing the fertility response of households residing outside of England & Wales but still have ties to Britain. In that section we consider evidence from two other colonies with substantial British-origin populations, South Africa and Australia.

In South Africa (or more specifically, the Cape Colony) we are able to conduct an analysis similar to what was done for Canada. This is possible because, while the Cape was a British during the second half of the 19th century, it also had a substantial European-origin population–the Afrikaners–that were not of British origin and did not speak English as a primary language. These residents, descended from Dutch settlers that immigrated to the Cape in the 17th and 18th centuries, formed the majority of the white Cape Colony population, but they were also mixed with substantial numbers of more recent immigrants, mainly from the British Isles.

Our analysis of changes in South African fertility patterns follows the same pattern as the analysis for Canada, though we are more limited in terms of statistical power.

Focusing on the white population only, we compare fertility patterns in locations with a greater share of British-origin population among the European-origin population.[37] Data tracking fertility for these groups are drawn from the Cape Colony Censuses of 1875 and 1891.[38] Since no comprehensive birth register is available, fertility rates are inferred using the number of children in different age groups observed in each census. Our analysis focuses on the division level, which is somewhat like a U.S. county. This is the lowest geographic unit for which consistent data are available. However, a number of changes took place in division boundaries between 1875 and 1891. After collapsing our data to account for these changes, we are left with data for 32 divisions with (close to) consistent boundaries across the two periods.

We consider two measures of a division's British connection. The first measure is the share of European-origin population in a division that was born in the British Isles. This variable ranges from 37% to essentially zero. As a second measure of the British-born population, we use the share of the white population in a division that was not a member of the Dutch Reform Church, the dominant religion among the Afrikaner population. This variable ranges from essentially one down to just 8%.

The results from South Africa, reported in Appendix Table 16, display the same basic pattern observed in Canada: locations with a greater connection to Britain experience a reduction in fertility in the years just after 1876, relative to the period just before. This pattern is robust to the inclusion of available control variables, using alternative measures of the British connection, or dropping the most populated locations, though the results are somewhat sensitive to whether the regressions are weighted because the British-origin population was disproportionately concentrated in more densely populated areas.

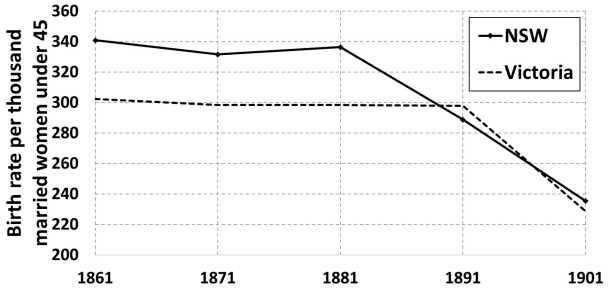
There is also evidence that Australia experienced a fertility decline in the years following the Bradlaugh-Besant trial.[39] The lack of a valid comparison population means that we cannot apply a difference-in-difference approach in that setting. However, a look at the simple time-series of fertility, shown in Figure 4, indicates that a

[37]The Cape Colony also contained large native African and mixed-race populations. Since these groups were less culturally similar than the different European-origin populations and faced a number of discriminatory practices that may have influenced their fertility patterns, we focus our analysis entirely on a comparison between the different European-origin populations.

[38]See Appendix 7.3 for further discussion of the South Africa data.

[39]The Bradlaugh-Besant trial was extensively covered in Australia. For example, in the Sydney Morning Herald articles discussing the trial appeared on Feb. 15, May 25, August 20, Sept. 27 and Oct. 15 of 1877.

Figure 4: Fertility patterns in New South Wales and Victoria



Data from Coughlin (1904).

sharp reduction in fertility took place in New South Wales between 1881 and 1891, followed by a similar change in Victoria in the decade after 1891.[40] Interestingly, most of the decline in New South Wales appears just after 1888, the year in which there was a similar trial in Australian courts, also over the publication of the *Fruits of Philosophy*.

Taken alone we would hesitate to draw strong conclusions from the evidence available for South Africa and Australia. However, in combination with the Canadian results as well as the patterns observed in Britain, we think it is reasonable to interpret the nearly simultaneous fertility declines observed among the British-origin populations in these locations as due to the same common cause.

6 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 and other colonies where it also impacted fertility among

[40]T.A. Coughlin (1904), the Government Statistician for New South Wales, wrote (p. 68), "For many years the Australian birth-rate was high, but within a certain short period between the years of 1880 and 1890, there was a complete change to a low rate. This change was manifest in all classes of the community, except amongst women of Irish birth, amongst people of every shade of opinion and of every social condition...The existing facts are compatible with only one explanation, viz., that in the years following 1880 the art of applying artificial checks to conception was successfully learnt and has continued in operation to this day."

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).[41] Our results resuscitate the importance of family planning information, as argued by an older generation of demographers led by Ansley Coale. At the same time, we see our results as strengthening the case for existing economic theories of fertility decline. By highlighting the important role played by the social norms surrounding fertility, our results help to reconcile the role of economic drivers of fertility decline with some of the patterns observed in the data, such as the simultaneous declines observed in locations with very different economic conditions, or the much earlier transition of less-industrialized France compared to Britain, that seem at odds with purely economic forces.

Given our results, it is natural to wonder whether, in the absence of the Bradlaugh-Besant trial, some other event would likely have happened soon after with the same results. Certainly that is possible. However, the fact that Britain's transition trailed the fertility reduction in France by roughly half a century, and preceded the onset of the transition in Germany by many years, suggests that differences in social norms have the potential to maintain cross-country differences in fertility patterns for long periods, even in the face of underlying economic forces tending towards fertility reduction.

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, with a broader conception of family planning it is easy to reconcile our results with evidence that changes in contraceptive methods were limited.

These results inform one of the most important debates in economic history and also come with 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

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

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.

References

1877. *The Queen vs. Charles Bradlaugh and Annie Besant*. London: Freethought Publishing Company.

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.

Alesina, Alberto, Giuliano, Paola, & Nunn, Nathan. 2013. On the Origins of Gender Roles: Women and the Plough. *The Quarterly Journal of Economics*, **128**(2), 469–530.

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.

Alsan, M, & Wanamaker, M. 2018. Tuskegee and the Health of Black Men. *Quarterly Journal of Economics*, **133**(1), 407–455.

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.

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.

Brodie, JF. 1994. *Contraception and Abortion in Nineteenth-Century America*. Ithaca: Cornell University Press.

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.

Coughlin, TA. 1904. *Report of the Royal Commission on the Decline of the Birth-rate and on the Mortality of Infants in New South Wales*. Vol. 1. William Applegate Gullick, Government Printer.

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.

D'Arcy, F. 1977. The Malthusian League and the Resistance to Birth Control Propaganda in Late Victorian Britain. *Population Studies*.

David, Paul, & Sanderson, Warren. 1986. Rudimentary contraceptive methods and the American transition to marital fertility control, 1855-1915. *Pages 307–390 of: Long-term factors in American economic growth*. University of Chicago Press.

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.

Fernandez, R, & Fogli, A. 2009. Culture: An Empirical Investigation of Beliefs, Work, and Fertility. *American Economic Journal: Macroeconomics*, **1**(1), 146–177.

Fernandez, Raquel. 2011. *Handbook of Social Economics*. North-Holland. Chap. Does Culture Matter?, pages 481–510.

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.

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

McInnis, M. 1994. *Migration and the International Labor Market*. New York: Routledge. Chap. Immigration and Emmigration: Canada in the Late Nineteenth Century, pages 139–155.

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.

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].

Woods, RI. 1987. Approaches to the Fertility Transition in Victorian England. *Population Studies*, **41**(2), 283–311.

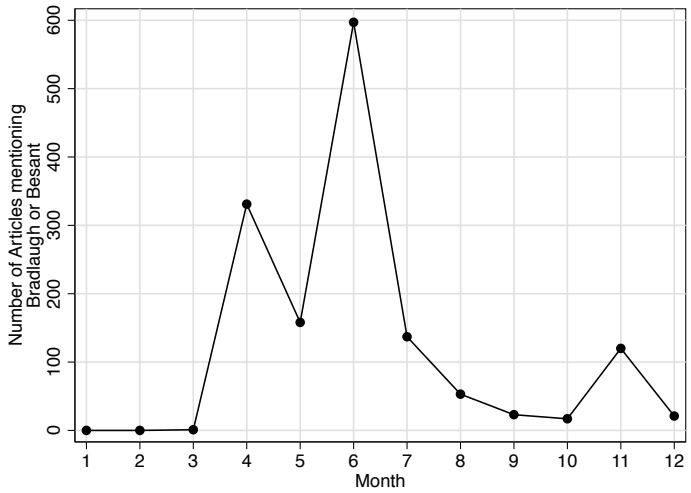
7 Appendix

7.1 Appendix to the England & Wales analysis

7.1.1 Further description of the E&W data

Our difference-in-differences empirical approach exploits variation in exposure to newspaper articles on the trial as a way of defining treatment and control groups. As mentioned in the main text, we classify articles as covering the trial if they were published in 1877 and they mention either "Bradlaugh" or "Besant" at least once throughout the article. Figure 5 shows that the timing of these articles matches key moments of the trial. For instance, we see no mentions of Bradlaugh or Besant in January, February, or March. In April, however, when they were arrested and the first hearing at Guildhall took place, we see roughly 350 articles published. There is a dip in May and then the number of articles peaks at nearly 600 articles in June. June is when the trial, conviction, and sentencing occurred. The relative increase in articles published in November matches a key hearing at Queen's Bench regarding Bradlaugh and Besant's attempt to appeal the conviction.

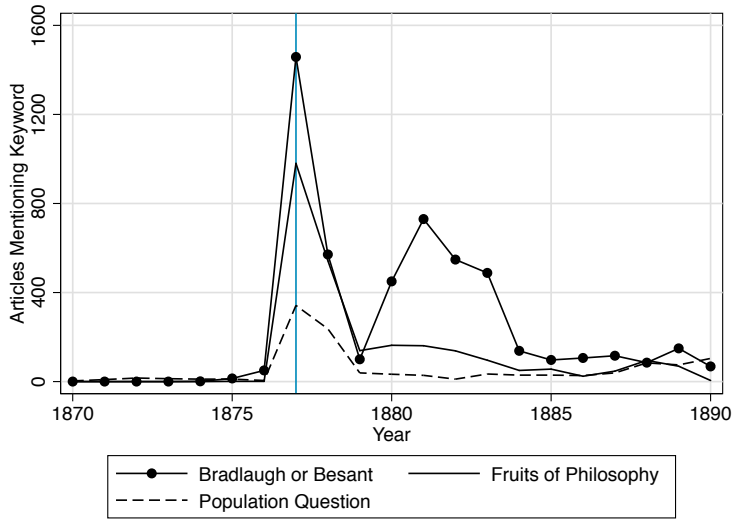
Figure 5: Articles Published on Trial in 1877



Data obtained from authors own search of digitized articles available from https://britishnewspaperarchive.co.uk.

Figure 6 provides additional support our classification. There we plot the number of articles published each year that mention each of the following keyword searches – "Bradlaugh OR Besant", "Fruits of Philosophy", and "Population Question". From 1870 to 1876 there are effectively no articles published mentioning any of these terms. In 1877, however, there is a dramatic rise in articles mentioning these terms. There were roughly 1500 articles mentioning either Bradlaugh or Besant, nearly 1000 articles mentioning "Fruits of Philosophy", and just under 400 articles mentioning the "Population Question". Mentions of all three terms remain elevated in 1878, the year in which Bradlaugh and Besant successfully overturned the ruling on appeal. From 1879 through 1890 we see consistent mentions of "Fruits of Philosophy" and the "Population Question", although the frequency is quite attenuated relative to 1877 and 1878 peaks. These mentions tend to relate to arrests of individuals for selling counterfeit pamphlets or other articles on either Bradlaugh or Besant. Indeed, after the trial both Bradlaugh and Besant were involved in several other newsworthy events. Bradlaugh, for instance, in 1880 was elected to Parliament but found himself in another controversy when he argued that he should be able to affirm his allegiance rather than take the religious Oath of Allegiance.

Figure 6: Prevalence of Keywords in Articles Published from 1870 to 1890



Data obtained from authors own search of digitized articles available from https://britishnewspaperarchive.co.uk.

In addition to the newspaper exposure variable, we also assemble a wealth of other district-level controls. The first source is the reports of the Registrar general which cover births, deaths, and marriages. The birth series was previously discussed in the main draft. The annual marriage series spans 1851-1884 and 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. The mortality data we are interested – total mortality, under 5 mortality, and mortality amongst fertile aged women (15-55) are not available on an annual basis. Instead, 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.[42]

[42]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.

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.[43]

Summary statistics for the key analysis and control variables at the district level are presented in Table 7.

[43]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 7: 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
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.[44]

[44]We then throw out any household with any of the following data discrepancies: more than one individual is coded as the household head (974 instances), more than one individual is coded as the head's spouse (4,231 instances), the household head is under the age of 16 (377 instances), there are more than 10 biological children (2,748 instances). We then impost some assumptions to increase the likelihood that the mother and father are the biological parents for each of the children in the household. Specifically, we throw out any household where: the mother or father was under the age of 14 when their first child was born (18,484 and 3,887 instances, respectively), the mother was over the age of 50 when their youngest child was born (16,215 instances), or the father was over the age

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.

7.1.2 Additional District-Level England & Wales Results

Table 8 presents results from a series of placebo checks that assess whether districts with higher trial exposure 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 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. Of the eight outcomes considered in the top panel, none of the changes are statistically significant at the 10% level.

As to the mechanisms these results help address, the first three columns, 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.

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.

Table 8: Placebo tests to rule out changes in district characteristics between 1861 and 1871

	3-year averages							
	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)
High News Exposure ×	0.001	0.001	0.007	0.007	0.116	-0.009	0.004	-0.000
1871 Decade	(0.001)	(0.015)	(0.011)	(0.010)	(0.117)	(0.016)	(0.005)	(0.003)
Period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marriage X trial interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other district X trial interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	860	860	860	860	860	860	860	860
R-squared	0.656	0.085	0.225	0.272	0.865	0.449	0.654	0.565
No. districts	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 weighted by 1851 district population. "High News Exposure" districts are those where the number of articles published on the Bradlaugh-Besant trial within a 25km band is above the median (i.e., 7 or more articles published in 1877). The "Marriage X trial interactions" include the interaction between our 1861-1871 decade 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 "Other district X trial interactions" include the following district-level characteristics interacted with our "1871 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).

Table 9 examines the robustness of our preferred specification (column 3 of Table 1. The first four columns throw out a number of districts based on occupational intensities (top 10% of occupations) to see whether the results are being driven by some occupation interaction. We find little evidence that this is the case. Column 5 discards the 25% most rural districts based on population density while Column 6 discards the 25% most urban districts. Again, results are largely unaffected by these sample restrictions.

Table 9: Additional England & Wales Robustness

	DV is Δln(avg. birth rate)					
	No Textiles	No Mining	No Metals	No Farming	No Rural	No Urban
	(1)	(2)	(3)	(4)	(5)	(6)
High News Exposure	-0.020***	-0.015**	-0.019**	-0.021***	-0.024***	-0.017**
× Trial Decade	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Marriage × trial int.	Yes	Yes	Yes	Yes	Yes	Yes
Other district × trial int.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,548	1,548	1,548	1,548	1,288	1,288
R-squared	0.643	0.647	0.634	0.630	0.639	0.649
No. districts	387	387	387	387	322	322

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the district level, in parentheses. All regressions weighted by 1851 district population. The trial decade is the 1881-1871 change, while the pre-trial decade is the 1861-1871 change. Birth rates are 3-year forward looking averages (i.e., centered on the year after enumeration). "High News Exposure" districts are those where the number of articles published on the Bradlaugh-Besant trial within a 25km band is above the median (i.e., 7 or more articles published in 1877). The "Marriage X trial interactions" include 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 "Other district X trial interactions" include 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).

7.1.3 Additional Microdata Results

This section presents additional results from analyzing microdata from the 1881 Census, which can help assess the mechanisms at play. 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 our "high news exposure district" indicator. These results, which appear in Table 10, indicate that there was an increase in age at first birth on the order of 2.5 months for mothers and 3.4 months for fathers. The scale of these coefficients suggests that delaying first birth was not the only driver of the earlier results.

Table 10: Did households delay their first birth?

DV is age when first child was born		
	Mother's Age	Father's Age
	(1)	(2)
High News District ×	0.198***	0.283***
Born after 1877	(0.0289)	(0.0383)
Observations	1074378	1031318
R-squared	0.017	0.018
No. districts	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 mixed evidence in this table and the coefficients are not economically meaningful (on the order of 1 month). This suggests that birth spacing was not a primary driver of the fertility decline.

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)
High News Exposure ×	0.0554***	-0.0478***	-0.0885***	-0.0833***	-0.0353***
Born after 1877	(0.0142)	(0.0122)	(0.0133)	(0.0129)	(0.0116)
Observations	1,078,959	967,545	767,900	530,535	314,724
R-squared	0.007	0.006	0.008	0.014	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.

7.2 Appendix to the analysis of Canada

7.2.1 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

7.3 Additional evidence on cultural transmission

7.3.1 Evidence from Wales

In Table 14 we return to the baseline differences-in-differences framework for England & Wales. In contrast to that analysis, which exploits variation in news exposure as a source of latent demand for family planning, these regressions focus entirely on the districts of Wales and exploit language barriers to define treatment exposure. We

consider both a discrete and a continuous measure of treatment. The discrete measure categorizes treatment districts as those where the share of the population that spoke only Welsh was less than 70%, the 75th percentile among the districts of Wales. The continuous measure is simply negative one times the share of the population that spoke only Welsh. Results from this specification indicate that areas with less Welsh speakers experienced larger declines in fertility in the decade following the Bradlaugh-Besant trial, although the estimates are less precisely estimated because the sample is significantly smaller.

Table 14: Results exploiting language barriers in Wales

DV is Δln(avg. birth rate)		
	(1)	(2)
High English Speaking Share × Trial Decade	-0.046**	
	(0.020)	
English Speaking Share × Trial Decade		-0.067
		(0.042)
Observations	144	144
R-squared	0.563	0.565
No. Districts	36	36

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the district level, in parentheses. All regressions include period and districted fixed effects. Regressions are weighted by 1851 district population. The trial decade is the 1881-1871 change. Birth rates are 3-year forward looking averages (i.e., centered on the year after enumeration).

7.3.2 Evidence from South Africa

South Africa, and specifically the Cape Colony, is a setting that is useful to study because it shares important features of the Canadian setting. In particular, the Cape was a British Colony during the second half of the 19th century, but one with a substantial European-origin population–the Afrikaners–that were not of British origin and did not speak English as a first language. These residents, descended from Dutch settlers that immigrated to the Cape in the 17th and 18th centuries, formed the majority of the Cape Colony population. However, the Cape Colony also included substantial numbers of more recent immigrants, mainly from the British Isles.

Our analysis of the Cape Colony compares fertility patterns in locations with a greater share of British-origin population among the European-origin population.

The Cape Colony also contained large native African and mixed-race populations. Since these groups were less culturally similar than the different European-origin populations and faced a number of discriminatory practices that may have influenced their fertility patterns, we focus our analysis entirely on a comparison between the different European-origin populations.

The data available for the Cape Colony are more limited than what we have access to in Canada. Our analysis relies primarily on a single difference taken between the Census of 1875 and the Census of 1891. Our analysis focuses on the division level, which is somewhat like a U.S. county. This is the lowest geographic unit for which consistent data are available. However, a number of changes took place in division boundaries between 1875 and 1891. After collapsing our data to account for these changes, we are left with data for 32 divisions with (close to) consistent boundaries across the two periods.

As in the Canadian analysis, it is necessary to use the population of children at particular ages to infer fertility levels. Unfortunately, however, at the division level the 1891 census only reports the total number of children aged 0-14, rather than in more detailed age categories. Thus, we calculate fertility rates as the ratio of children aged 0-14 in either 1875 or 1891, relative to the fertile-aged female population in those years. We then look at whether the difference in fertility rates across these two periods is related to the location's British connection in 1875. Our baseline regression specification is,

$$\ln(BR_{d\,1891}) - \ln(BR_{d\,1875}) = \beta_0 + \beta_1 BRIT_{d\,1875} + X_{d\,1875}\gamma + \epsilon_d$$

where BR_{dt} is the ratio of children aged 0-14 to the fertile-aged (15-55) female population in district d in period t, $BRIT_{d\,1875}$ is a measure of the location's British connection in 1875, and $X_{d\,1875}$ is a set of control variables reflecting conditions in each division in 1875. Summary statistics for the variables used in our analysis are presented in Table 15.

We consider two measures of a division's British connection. The first measure is the share of European-origin population in a division that was born in the British Isles. This variable ranges from 37% to essentially zero. As a second measure of the British-born population, we use the share of the white population in a division that

Table 15: Summary statistics for the South Africa analysis

Variable	Mean	Std. Dev.	Min.	Max.
$BR_{d,1875}$	1.879	0.24	1.208	2.314
$BR_{d,1891}$	1.773	0.188	1.194	2.04
British-born share	0.08	0.082	0.008	0.37
Not Dutch reform church shr.	0.395	0.286	0.082	1
Population density (per sq. mile)	5.687	11.842	0.116	52.892
Literacy rate (ages 15-55)	0.912	0.042	0.8	0.975
N		32		

was not a member of the Dutch Reform Church, the dominant religion among the Afrikaner population. This variable ranges from essentially one down to just 8%.

The set of available control variables is somewhat limited. We include controls for population density in 1875 in all of our regressions, as well a control for literacy rates among the population aged 15-55 in 1875. Literacy rates were relatively high, ranging from 80-97.5 percent. Regressions are weighted by each division's population in 1875, a decision that reflects the fact that our outcome variables are averages, which will be more precisely measured in locations with more observations. Weighting does make a difference, since the British-origin population tended to cluster in a relatively smaller number of divisions with greater populations.

Our results are presented in Table 16. The first column presents baseline results with our preferred measure of a location's connection to Britain: the share of British-born population in the district. Column 2 adds in a control for literacy in 1875. This is our preferred specification. In Column 3 we also consider the relationship between fertility and the share of the population that was not either born in the Cape Colony or in the British Isles. It is comforting to see that the share of other immigrants does not have the same strong relationship to fertility that we observe for those immigrants born in the British Isles. Note that some of the other immigrants may have been British citizens born in other locations, which may explain why we still observe a negative coefficient estimate for this variable.

Column 4 considers an alternative measure of connections based on the population that was not a member of the Dutch Reform Church. This alternative generates qualitatively similar results to our preferred specification, though the magnitude suggests that this is not as good a measure of a location's connection to Britain. It is

worth noting that if we include both this variable and our preferred measure based on the share of British-born population in the same regression, the effects appear to be driven entirely by the British-born population share.

The results in Column 5 are estimated while dropping locations with a population density above four persons per square mile. This eliminates the four major urban centers in the Cape Colony during this period: Cape Town, Stellenbosch, Paarl, and Port Elizabeth. This selection is not particularly sensitive to using a cutoff of 10 persons per square mile; Outside of these four locations, no other division had a density above five. Columns 6 and 7 present results where the regressions are unweighted. We can see in Column 6 that weighting is important. Without weighting we still observe a sizable negative coefficient, but it is no longer statistically significant. However, Column 7 shows that simply drop the six locations with populations under 2,000 from the analysis leads to results that are almost identical to those obtained when weighting.

Table 16: Regression results for South Africa analysis

	DV: ln($BR_{d\,1891}$) - ln($BR_{d\,1875}$)						
	Base	With literacy controls	With other imm.	Dutch Reform share	Drop if density > 10/sq. mi.	Unweighted	Unweighted pop. > 2000
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Brit. imm. shr.	-0.776**	-1.381***	-1.256*		-0.921**	-0.746	-1.252**
	(0.305)	(0.459)	(0.649)		(0.377)	(0.470)	(0.531)
Not Dutch Reform share				-0.178*			
				(0.0909)			
Non-British imm.			-0.355				
			(0.611)				
Pop. density	0.00538***	0.00686***	0.00677***	0.00349*	0.0270*	0.00359	0.00560**
	(0.00138)	(0.00156)	(0.00171)	(0.00186)	(0.0146)	(0.00263)	(0.00236)
Literacy		-1.398*	-1.383*	-0.511	-0.841	-0.484	-0.979
		(0.789)	(0.802)	(0.724)	(0.710)	(0.702)	(1.038)
Constant	-0.0335	1.274*	1.266*	0.447	0.692	0.425	0.904
	(0.0325)	(0.730)	(0.739)	(0.665)	(0.658)	(0.656)	(0.963)
Observations	32	32	32	32	28	32	26
R-squared	0.271	0.365	0.371	0.186	0.225	0.104	0.219

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Regressions in Columns 1-5 are weighted by district population in 1875.