# Culture and the Historical Fertility Transition *

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**Abstract**

The historical transition to a low fertility regime was central for long-run growth, but what caused it? Existing explanations largely focus on the economic incentives to have children. This paper presents new evidence highlighting the importance of cultural forces as a distinct driver of the fertility transition. We leverage a sharp change in fertility in Britain in 1877 and document large synchronized declines in fertility among culturally-British households residing outside of Britain, in Canada, the U.S. and South Africa, relative to their non-British neighbors. We propose a plausible catalyst for the change: the famous Bradlaugh-Besant trial of 1877.

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# 1 Introduction

The fertility transition stands alongside the Industrial Revolution as a key turning point on the road to modern economic growth. While the Industrial Revolution began a period of sustained technological progress, early gains in output were largely offset by rapid population growth, limiting the rise in per-capita income.[1] Only with the onset of the fertility transition did population growth begin to slow, paving the way for the sustained increases in income that characterize modern economic growth.

Most work by economists and economic historians on the historical fertility transition has focused on explanations involving changes in the economic incentives for having children. Much of this work incorporates the influential Becker & Lewis (1973) framework of a quantity-quality trade-off and emphasizes rising demand for education as a determinant of family size.[2] Others have focused on the role of female education and labor force opportunities.[3]

This paper provides evidence that, in addition to the influence of economic incentives highlighted by most existing work, cultural forces were an important and complementary driver, influencing the timing and speed of the historical fertility transition. We focus on the fertility transition in Britain and among British-origin populations living elsewhere. Britain is interesting not only because it was the first industrial economy, but also because it exhibited a sharp change in fertility, as shown in the top panels of Figure 1.[4] This sharp break appears simultaneously across all regions of the country (as shown in the middle panels of Figure 1), and in both urban and rural areas (as shown in the bottom panels of Figure 1). The speed, size, and

[1] See, e.g., Crafts & Mills (2020), Allen (2001), and many others.

[2] Work in this vein includes Bleakley & Lange (2009), Aaronson *et al.* (2014), Hansen *et al.* (2018), Fernihough (2017), Diebolt *et al.* (2017), Becker *et al.* (2010), Becker *et al.* (2012), and Klemp & Weisdorf (2019).

[3] This work 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 and industrialization (Kalemli-Ozcan *et al.*, 2000; Ager *et al.*, 2018; Wanamaker, 2012; Franck & Galor, 2015). Other studies have looked into the role of new contraceptive technology, but most existing work has concluded that contraceptive techniques remained stable during the period we study (Guinnane, 2011). Some studies evaluate multiple determinants, such as Dribe (2008) and Bengtsson & Dribe (2014) on Sweden and Brown & Guinnane (2002) on Bavaria.

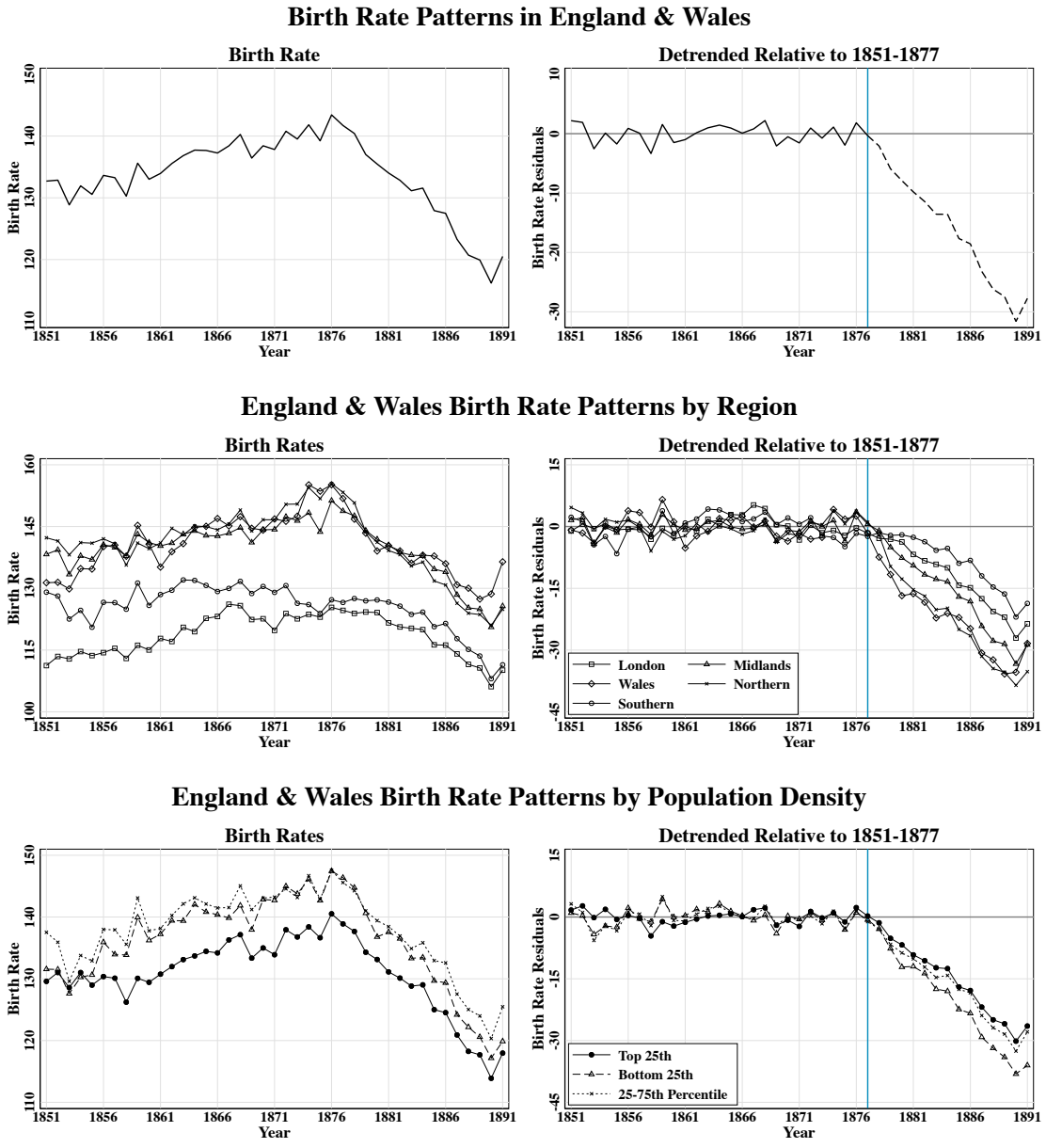
[4] For a longer time-series, see Appendix 8.1. This pattern differed from other major industrialized European countries such as France, Austria, Germany, and Sweden. In Appendix 8.2 we present patterns for a number of European countries. This shows no evidence that the decline observed in Britain is part of a broader European pattern.

widespread nature of this change raises questions about the forces at work.

Our challenge is isolating the role of culture from policy or economic changes that may have contributed to the patterns seen in Figure 1. To overcome this challenge, we look at whether similar patterns are observed in culturally-linked populations outside of Britain, in Canada, South Africa, and the United States. Tracing out synchronized fertility changes among culturally-linked populations indicates that cultural forces played a role in the fertility transition. These cultural connections could have influenced fertility either through shaping social norms or through facilitating the dissemination of contraceptive knowledge. Moreover, by documenting simultaneous fertility changes among British-origin populations living in vastly different economic and policy environments, we are able to rule out that the change we see was due to economic shifts or policy innovations. Finally, by using rich census micro-data to compare fertility patterns among British-origin populations and other European-origin populations without cultural ties to Britain but living in the same location and working in similar occupations, and thus subject to the same economic and policy environment, we show that the changes we document were largely confined within culturally British communities, despite the fact that these communities were spread around the world. Together, these pieces of evidence allow us to confirm that it was cultural ties, rather than economic or policy forces, that was the key mechanism behind the synchronized fertility reduction that we observe.

Our analysis starts in Canada where there was a large Anglophone population with cultural and linguistic ties to Britain and a large Francophone population without similar ties. A simple plot of regional fertility patterns shows that, starting around 1877, fertility declined sharply in locations with substantial British-origin populations. Not only does the timing match Figure 1, there is no similar reduction in Quebec, which was mainly Francophone. To improve identification, we adopt a difference-in-differences framework that compares fertility in counties with higher vs. lower British-origin population shares. Those results show that fertility fell just after 1877 in locations with stronger cultural ties to Britain. We push identification even further with micro-data from the 1881 census, which allow us to compare the fertility patterns of Anglophone and Francophone households *within the same Canadian county* before and after 1877. Our findings show that British-origin Canadians reduced their fertility after 1877 relative to their Francophone neighbors. Not only does the timing match the pattern observed in England and Wales, but the magnitude is similarly large.

Figure 1: Birth Rate in England and Wales, 1851-1891




Notes: Births rates are defined as births per 1000 fertile-aged women (women between the ages of 15 and 50). The births data are from annual reports of the Registrar General. The population denominator is linearly interpolated between census years. The right-hand panel plots residuals from a regression that fits a linear trend between 1851 and 1877. Note that these data begin in 1851, when higher-quality disaggregated birth registry and population census data become available. For a longer time-series of births in England and Wales, extending before the registration period, see Appendix 8.1.

We then look to other locations with a mix of British-origin and other populations. In South Africa, where we compare fertility among the British-origin population to patterns among the Dutch-origin Afrikaners, we find a similar pattern of relative fertility decline in locations with stronger cultural or linguistic ties to Britain. In the United States, we compare first and second-generation British-origin immigrants to other first and second-generation European immigrants living in the same area and working in the same broad occupation. Again, we observe a relative decline in fertility among the British-origin population with a timing that precisely matches the fertility decline in England and Wales. Moreover, the magnitude of the estimates obtained in the U.S. closely matches the results found in Canada.

These results provide clear evidence that culture played an important role in the historical fertility transition. The main alternative explanations for the historical fertility transition, such as changes in the economic incentives for investing in human capital or changing policies related to child or female labor or women's rights, cannot plausibly explain these patterns. In contrast, cultural ties offer a straightforward explanation for how we can observe simultaneous changes in fertility behavior among culturally linked populations living in otherwise very different environments, while at the same time not observing similar changes across cultural groups sharing the same environment (such as Anglophone and Francophone Canadians living in the same location and employed in the same occupation).

One way that culture may shift fertility behavior is through changing social norms. Models of social norms emphasize that, because norms are often sustained through coordinating beliefs, focal events that affect beliefs can lead to rapid shifts from one equilibrium to another.[5] Recent empirical evidence, such as (Bursztyn *et al.*, 2020b), provides support for these predictions.[6] As (Bursztyn *et al.*, 2020a) write, "Social norms, usually persistent, can unravel quickly when new public information arrives." Moreover, since different cultural groups sustain different social norms, a shift in norms can plausibly generate changes that are largely confined to one cultural group.

[5]See Young (2015) for a recent review of theories of social norms. Several recent studies have examined the impact of social norms on fertility patterns, including Munshi & Myaux (2006), Fernandez & Fogli (2009), Spolaore & Wacziarg (2019) and de Silva & Tenreyro (2020).

[6]Another recent paper showing that single events can have substantial impacts on behavior are Alsan & Wanamaker (2018), which 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., and Bassi & Rasul (2017), which documents the substantial fertility impact of a visit by Pope John Paul II to Brazil in 1991.

A second way that cultural ties may affect fertility is through disseminating contraceptive information. While there is evidence that available contraceptive technology was fairly stable during the period we study, there is every reason to believe that what contraceptive information was available may have been difficult for many people to access.[7] As a result, a change in the willingness of people within a group to discuss contraceptive information could plausibly lead to a substantial fertility change that was largely confined within culturally and linguistically linked communities.

Naturally, these results raise questions about what event, or set of events, might have initiated the onset of the changes that we document. This is the topic addressed in the second part of our paper. Identifying a single catalyst is empirically challenging, so our aim is simply to show that there exists a plausible event that could have acted as the catalyst (or one of the catalysts) for the culturally-transmitted changes that we have documented. A careful review of historical records from this period shows that one such event, dealing specifically with fertility behavior and the morality of limiting family size, occurred just before the sharp fertility transition that we observe. This was the famous Bradlaugh-Besant trial, which took place at the Queen's Bench in London in 1877. The 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 choose one's family size and provided rudimentary information about contraceptive techniques. The trial started a national conversation on the morality of choosing family size and led to a surge in sales of books and pamphlets on the topic.

The Bradlaugh-Besant trial was extensively covered by the English-language press, not just in Britain, but around the world. As Chandrasekhar (1981) writes (p. 42), "At no time in British social history had the arguments in favor of a small family been presented so fully and freely...this was a remarkable turning point, for until this moment no regular newspaper would touch the subject." In Canada, for example,

[7] Teitelbaum (1984) writes (p. 218), "We can also say on qualitative grounds that the spread of knowledge and availability of contraceptive methods...may have contributed importantly to the ability of couples to implement their desires for fewer children." Abstinence and withdrawal were the primary contraceptive techniques used throughout this period (Szreter, 1996, Ch. 8). Even though methods such as withdrawal have high failure rates relative to modern contraceptive methods, David & Sanderson (1986) show that such methods, even when used only part of the time, would have been sufficient to generate fertility declines of the magnitude observed in our study.

we document that the trial was covered in the English language press, but not by French language papers. In the U.S. we observe over 500 articles about the trial in 1877, despite the incomplete nature of digitized newspaper databases in that setting. Thus, it is plausible that this event could have acted as a catalyst for the changes that we document.

The importance of the Bradlaugh-Besant trial is widely appreciated by historians and demographers such as Himes (1970) who, in his seminal history of contraception, wrote that (p. 240), "The social effects of the publicity attending this prosecution were nothing less than revolutionary."[8] Similar statements can be found from a number of other leading historians who have studied British fertility patterns, including Elderton (1914), Glass (1967), McLaren (1978), Teitelbaum (1984) and Szreter (1996). However, our current understanding is based largely on the timing of the trial relative to the British fertility decline. To our knowledge, no quantitative examination of the impact of the Bradlaugh-Besant trial has been undertaken.

The second part of our analysis provides an evaluation of the impact of the Bradlaugh-Besant trial on fertility. To provide more direct evidence that the trial had a meaningful effect on fertility, we focus on England and Wales, where we are able to take advantage of a uniquely rich database of newspaper articles in order to identify locations that were more or less exposed to news about the trial. Difference-in-differences results indicate that fertility declined more rapidly after 1877 in locations with greater newspaper coverage of the trial. The results are robust to including a rich set of variables reflecting factors commonly thought to have played a role in the historical fertility transition as well as controls for pre-existing newspaper exposure. This evidence tells us that the Bradlaugh-Besant trial provides a plausible explanation for the large fertility reductions among culturally-British populations documented in our main analysis.

The idea that cultural forces may have played an important role in the historical fertility transition is not new. Historians and demographers, such as Ansley Coale, have argued that cultural practices related to acceptance of fertility restraints mattered.[9] However, separating cultural forces from economic and policy incentives has

[8]Himes devoted an entire chapter to the Bradlaugh-Besant trial and related events such as the formation of the Malthusian League, which resulted from the trial.

[9]See Coale (1973) and van de Kaa (2004). Cultural conditions received substantial attention in the work of the European Fertility Project (Knodel & van de Walle, 1986).

proven challenging, though progress has recently been made by Spolaore & Wacziarg (2019), Blanc & Wacziarg (2020), and Daudin *et al.* (2019).[10] Relative to existing work, our contribution is to provide an approach that isolates cultural factors from other leading alternative explanations for the historical fertility transition. This allows us to provide stronger evidence that culture mattered for the timing of the transition, as well as to pinpoint one event that is a plausible catalyst.

This paper also contributes to work on the impact of culture and norms more broadly. In addition to documenting that culture can play a key role in contributing to substantial changes in behavior, our findings offer a new twist relative to existing empirical evidence on culture and norms. Most work on this topic emphasizes the persistent effects of culture (e.g., Nunn & Wantchekon (2011), Alesina *et al.* (2013), Fernandez & Fogli (2009), Alsan & Wanamaker (2018), Bazzi *et al.* (Forthcoming)). Our results show that cultural and linguistic ties can also be an agent of change, transmitting new norms or ideas rapidly around the world.

The next section of this paper presents our analysis of fertility patterns in Canada, followed by similar evidence from the U.S. in Section 3 and South Africa in Section 4. We briefly discuss these results and their implications in Section 5. In Section 6 we introduce what we believe is the most plausible catalyst for the changes documented in our main analysis, the Bradlaugh-Besant trial of 1877. Section 7 concludes.

# 2 Main analysis: Canada

Comparing fertility patterns among culturally different populations operating within the same economic and policy environment makes it possible to isolate the impact of culture and cultural ties on fertility from alternative economic or policy explanations. Our richest evidence of this type comes from Canada, where we are able to exploit the fact that some Anglophone Canadians had strong cultural and linguistic links to Britain, while other Francophone Canadians did not. By studying whether Anglo-

[10] Spolaore & Wacziarg (2019) argue that the idea of fertility restriction arose in France and then diffused first to locations with stronger cultural ties to France. They test this idea using a cross-country approach. Blanc & Wacziarg (2020) examine over 150 years of fertility patterns for the French village of Saint-Germain-d'Anxure and find little evidence to support the idea that fertility declines followed changes in economic incentives. Daudin *et al.* (2019) provide evidence that the spread of cultural and/or economic information related to fertility via migration contributed to a convergence in fertility rates across French regions in the second half of the nineteenth century.

phone Canadians exhibit a decline in fertility corresponding to the sharp reduction observed in England and Wales, relative to Francophone Canadians sharing a common political, legal and economic environment, we will be able to isolate the impact of cultural influences on fertility behavior.

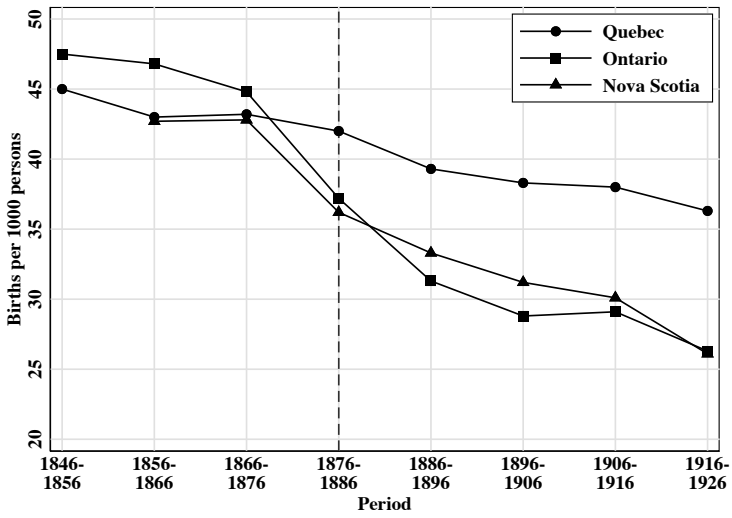
Our analysis proceeds in three steps. We begin by presenting time-series data on the changes in fertility patterns in different Canadian provinces. These are useful for getting an idea of the timing and magnitude of the fertility change. We then focus in on fertility patterns at the county level, using a difference-in-difference approach applied to aggregate county-level census data. The time variation in our difference-in-difference framework is based on the timing of the fertility change observed in England and Wales, while the spatial variation is based on measures of the strength of each county's cultural connection to Britain. This analysis improves identification while still allowing us to study fertility changes for a decade on either size of the 1877 fertility change. Finally, use micro-data from the 1881 census to take an even closer look at changing fertility patterns. Specifically, the micro-data allow us to analyze relative fertility changes between households residing in the same Canadian counties, and with similar parental characteristics in terms of ages and occupations, before and after 1877, depending on the strength of each household's cultural ties to Britain.

As a starting point for our analysis of Canadian fertility, Figure 2 plots raw fertility patterns in Quebec, Ontario, and Nova Scotia. These series are taken from Henripin (1968) and are calculated using the number of children in different age groups in each census to infer prior fertility patterns. These data show that, up to 1876, fertility in the mainly British-origin provinces of Nova Scotia and Ontario was similar to or even higher than in the mainly French-origin province of Quebec.[11] After that point, there is a sharp decrease in fertility in the mainly British-origin provinces and their rates dropped substantially below the rates in Quebec. The fact that the timing of this shift corresponds so closely to the timing of the change observed in England and Wales, the fact that it is largely confined to the provinces with the strongest cultural ties to Britain, and the very large magnitude of the change, are all striking. Below, we

[11] Notably, the French-Canadian population did not undergo the early fertility transition observed in France. We can see this clearly by comparing the fertility rates for Quebec in Figure 2 to the rates shown for France in Appendix Figure 8. The causes for this difference are beyond the scope of our study, but we would speculate that it is likely to be due to the fact that French Canada was separated from France before the French Revolution and may have therefor retained cultural habits that were more reflective of Royalist rather than Revolutionary France.

go further, by showing that similar relative changes can be observed when comparing populations within the same province, or even within the same county.

Figure 2: Fertility patterns in some Canadian provinces




Notes: Data from Henripin (1968) Table B.6. Henripin (1968) infers births in each period from the number of individuals enumerated in each census-specific age bin.

## 2.1 Canadian data

The first part of our Canadian analysis uses county-level data from the Canadian Census of Population. Canada did not have a registry of births at this time, and so we infer fertility patterns from census data on the number of children at different ages at the time of the Census, relative to the population of fertile-aged women (in thousands).[12] 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 the population in each bin by the number of age categories to obtain a county-specific

[12]The census did ask residents whether they gave birth in the year prior to enumeration. This question only corresponds to one year and thus does not allow us to examine whether fertility patterns differentially changed after 1877. The data are also noisy due to year-to-year fluctuations in births and recall bias. Nevertheless, when we analyze these data within a difference-in-differences framework we find negative and statistically significant declines in areas with stronger ties to Britain, reinforcing the conclusions generated from our preferred measure of period-specific fertility.

estimate for the average (annualized) number of births over that period.[13] We then divide that estimate of births by the number of women (in thousands) between the ages of 15-50 that were enumerated in that county in the previous census.[14] 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 because we employ a difference-in-differences estimation strategy it will not bias our results unless mortality rates are differentially changing in locations with stronger British ties right around 1877. The time period covered by our analysis is 1865-1886, or roughly a decade on either side of the 1877 change in fertility observed in England and Wales.[15]

To measure the strength of each county's cultural ties to Britain in the county-level analysis, we focus primarily on the share of the population in the 1871 Census that is either Canadian born and not of French origin or born in Britain.[16] This is only available for Ontario and Quebec so we begin our analysis with just those two provinces. That is a good starting point given the economic similarities between these provinces. We also consider two alternative measures. One is the share of the population that was not of French origin. The second, which is also available for the provinces of New Brunswick and Nova Scotia, is the share of the population that was not Catholic, a measure that reflects the fact that Francophone Canadians were overwhelmingly Catholic.[17] All of these measures deliver similar results.

[13] We collapse some counties in order to obtain areas that are geographically consistent over time, leaving us with 130 counties.

[14] 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 change our results.

[15] 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). These effects were likely systematically different in locations with many English-speaking British-origin residents, and may have affected fertility patterns.

[16] Our British-origin population includes Irish immigrants since the majority of the Irish immigrants to Canada were Protestants from Northern Ireland, which likely had strong cultural ties to Britain (Houston & Smyth, 1999). Our results are also robust to using adherence to the Roman Catholic church as our measure of a (lack of) cultural ties to Britain, which will pick up the effect of Irish Catholics alongside the predominantly Catholic French-origin population.

[17] 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. Other provinces were sparsely populated during this period. In Ontario and Quebec, where

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 school attendance.

The second part of our Canadian analysis uses individual-level micro-data from the 1881 census. While these data only allow us to study a few years after 1877, they offer an important advantage. Because the micro-data include information on ethnic origin, we are able to cleanly identify British households and compare them to French households living within the same county. This allows us to adopt an empirical specification that examines relative changes in fertility among British and French households residing in the same location. In addition, these data allow us to control for household-level characteristics such as parental age and father's occupation. Our final micro-data sample includes 176,318 households and 863,322 births.[18]

## 2.2 Canada county-level analysis

We begin by comparing fertility patterns in counties to the share of the population in each county with cultural ties to Britain before vs. after the fertility reduction observed in England and Wales in Figure 1, using a difference-in-differences approach. Our main specification is,

$$\frac{CHILD_{ct}}{FEM_{ct}} = \alpha_1 BRIT_c * POST_t + X_{ct}\lambda + \phi_c + \eta_t + \epsilon_{ct} \quad (1)$$

where $CHILD_{ct}$ is the average number of children born in county $c$ during each year of period $t$, which is inferred based on the children observed in the census within

both measures of British connection are available, we observe strong correlations between these two measures of British connections.

[18]The 1881 census did not record family relationships within the household, and so we overcome this issue in the following way. First, we discard 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 surnames of the inferred couple don't match and 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 1-99th percentile of differences among. Next we restrict the sample to the set of households where it is reasonable to assume that this married couple is 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 15.

each age group. The denominator, $FEM_{ct}$, is the number of women between 15-50 (in thousands), as noted earlier. 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 1876, roughly when the fertility decline begins in England and Wales.[19]

The identifying assumption in this regression framework is that there are no unobserved factors that caused a relative decline in fertility in locations with a larger British-origin population share in the years just after 1877, relative to the years just before. To strengthen identification, we include control variables reflecting initial conditions–population density (in 1861), population growth (1861-71), the agricultural employment share (in 1871), the male/female ratio in 1871, and the share of fertile aged women that are under 30 in 1871–that could have influenced fertility, interacted with period effects. Standard errors are clustered by county and regressions are weighted by county population in 1861.[20] Spatial autocorrelation is a potential concern, but spatially adjusted standard errors are typically smaller (see Appendix Table 9), and so we report more conservative county-clustered standard errors.

Baseline county-level regression results are presented in Table 1. The first two columns compare fertility in one "treatment" period, 1877-81 to two pre-treatment periods, 1865-71 and 1872-76.[21] In Column 1, which includes just county and year fixed effects, we find strong evidence of a relative reduction in fertility in counties with stronger British ties. Column 2 adds in our controls. This reduces the estimated coefficient somewhat, but also increases the within R-squared substantially; because the coefficient falls as controls are included, at the bottom of the table we report results from the Oster (2019) test, which shows that our results are unlikely to be driven by unobservables. In Column 3, we add an additional post period and estimate a separate coefficient for one of the pre-periods, to check for pre-trends. The small and

[19] We have to use 1876 as the cutoff here rather than 1877, which we use in the micro-data, because in the aggregate statistics the children are identified based on 5 year age bins leading up to the census year (e.g., 1881).

[20] County populations tend to be similar and so weighting has little impact on the results. To demonstrate this, we present unweighted estimates in our robustness checks.

[21] Note that the first pre-treatment period is longer because the data in the 1871 census are reported only for children aged 0-6 rather than 0-5. It is also useful to note that the years are approximations since they are based on children's age in the census (birth year is not reported) so, for example, some children included in the 1872-76 period were actually born in 1871 after the month of the census (April).

statistically insignificant estimate in the 1865-71 period suggests that our data are not characterized by strong pre-trends, and the negative coefficient on this estimate indicates that any pre-existing trend would run counter to our results. The negative and statistically significant coefficient estimated for the 1882-1885 period suggests that the differential decline in fertility was persistent, as suggested by Figure 2.

Table 1: Baseline regression results for Canada county-level analysis

| **DV: Children per 1000 fertile-aged women per year** | | | |
|---|---|---|---|
| Periods included: | 1865-1881 | 1865-1881 | 1865-1885 |
| | (1) | (2) | (3) |
| British-origin shr. × 1865-71 Period | | | -7.407 |
| | | | (4.915) |
| British-origin shr. × 1872-75 Period | | | . |
| | | | . |
| British-origin shr. × 1876-81 Period | -21.015*** | -16.107*** | -19.811*** |
| | (2.833) | (2.916) | (1.977) |
| British-origin shr. × 1882-85 Period | | | -16.136*** |
| | | | (6.002) |
| County FEs | Yes | Yes | Yes |
| Period FEs | Yes | Yes | Yes |
| Controls | | Yes | Yes |
| Observations | 294 | 294 | 392 |
| Within R-squared | 0.092 | 0.650 | 0.596 |
| No. of counties | 98 | 98 | 98 |
| Coef. implied lower bound following Oster (2019): | | -14.354 | |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors, clustered by county, reported in parenthesis. Spatially adjusted standard errors are smaller and thus less conservative, see Appendix Table 9. Observations weighted by county population in 1861. Controls: population density in 1861, population growth in 1861-71, agricultural employment share in 1871, the male/female ratio in 1871, and the share of fertile aged women that are under 30 in 1871. All controls are interacted with our period indicators. To implement the test suggested by Oster (2019), we follower her advice and assume a 30% greater increase in the R-squared relative to our baseline specification and unobservables as correlated with the treatment variable as the observables.

Our results indicate that a county with a completely British-origin population experienced a decrease in fertility of 13-16% after 1877 relative to a county with an entirely French-origin population.[22] For a one standard deviation change in the

[22]In 1871-76 there were around 135 children born per 1,000 fertile-aged females with relatively

British origin share (0.235) this implies a decrease in fertility of around 3.9-4.4%. These are fairly large effects that can explain the substantial changes in relative fertility levels found by Henripin and shown in Figure 1 (note that in Figure 1 the denominator is inhabitants rather than fertile-aged females).

Appendix Table 8 presents a variety of additional robustness checks. For example, we look at whether our results are affected by the fact that some periods in our analysis are based on the number of children 0-5 (or 6) alive in the census and others are based on children 6-10 (or 11). We find that our results are not driven by this feature. We also estimate results where we do not weight the regressions, which does not substantially change the results. Additionally, we show that even within Quebec, there is a sufficient mix of Anglophone and Francophone populations that we can still find statistically significant effects. Another robustness check adds in a control for the rate of schooling in each county.

One of the more interesting robustness checks presents separate effects for the share of native-born Canadians of British origin, immigrants from Britain, and other non-British immigrants. We find significant effects for both the British-origin native born and first-generation British immigrants, but not for other immigrants. Effects appear stronger for those with closer (first-generation) ties to Britain.

Finally, we examine two alternative measures of British connection. One is treating all of the population that was not of French-origin as British, and a second alternative focuses on the share of the population that was not a member of the Catholic church, the dominant religion among French Canadians. The non-Catholic measure is also available for counties in New Brunswick and Nova Scotia, and so we present results with and without these additional observations. All of these specifications deliver results that are very similar to our preferred measure of British connection.

In summary, we find that within the first five years after 1876 there was a substantial reduction in fertility in counties where a larger share of the population had cultural ties to Britain, whether measured by ancestry or religion. This, together with the close temporal correspondence between this change and the reduction in birth rates within Britain, provides evidence in favor of the idea that fertility patterns were being strongly influenced by information transmitted through cultural or linguistic links, results that are particularly striking given the enormous differences

similar levels in both Ontario and Quebec.

in economic conditions that existed between Canada and Britain at this time.

## 2.3 Household-level analysis

In this section we push the analysis further, by using census micro-data to compare the change in fertility patterns of Anglophone households relative to Francophone households within the same Canadian county before and after 1877. This analysis takes advantage of the fact that the Canadian census data allows us to directly identify Anglophone households, which we define as native Canadians of British origin or immigrants who were born in Britain.[23] Recall that these micro-data come from the 1881 census and we infer past household fertility behavior using the ages of children in the household at that time. This approach has the advantage of allowing us to compare households within the same county, but we are not able to look at effects beyond 1881. Thus, we view the household analysis as complementary to the county-level analysis presented above, though more cleanly identified.

For this analysis we organize our data as a household-level panel and examine fertility patterns between 1874 and 1881.[24] We restrict our sample to couples cohabiting with at least one child born before 1878 (in robustness results we also present evidence including couples with no children initially).[25] Summary statistics for these data are in Appendix Table 7. Our preferred Probit specification is:

$$Y_{hdt} = \begin{cases} 1 \text{ if } Y^*_{hdt} > 0 \\ 0 \text{ otherwise} \end{cases}$$

$$Y^*_{hdt} = \beta_0 + \beta_1 BRIT_h * POST_t + \beta_2 BRIT_h + C_{hdt}\Lambda + \phi_{dt} + \epsilon_{hdt} \quad (2)$$

where $Y_{hdt}$ is an indicator for whether household $h$ in district $d$ had a child in year $t$,

[23] As in our county-level analysis, we classify Irish households as British-origin, as explained in Footnote 16. However, robustness exercises show that our results are not dependent on this decision.

[24] Since fertility is inferred based on the number of children enumerated in the household it is important to examine cohorts that are unlikely to have started leaving the household. We choose 1874 as the starting cohort because it means we have the same number of years before and after treatment. A robustness check shows that starting the panel in 1871 yields nearly identical results.

[25] There are two reasons to focus our analysis on couples that already have children in the household. First, this helps us more accurately identify couples that are at risk of fertility. Second, the historical evidence suggests that it is among these couples that the fertility change occurred, through a reduction in higher-order births.

and $Y^*_{hdt}$ is a latent variable. The key explanatory variable is an interaction between an indicator for whether the household is culturally British ($BRIT_h$) and an indicator for years after 1877 ($POST_t$). $C_{hdt}$ controls for the mother's and father's ages, and in our preferred specification we include fixed effects for the father's occupation, which we fully interact with our year fixed effects. $\phi_{dt}$ represents district-by-year fixed effects. Given this setup, $\beta_1$ reflects the change in the probability that a British-origin household has an additional child in a particular year after 1877 relative to other households living in the same county in the same year.

Estimation results appear in Table 2. Column 1 presents results from a baseline regression in which we control only for year and parent age. Column 2 and 3 add in county and occupation fixed effects, respectively. In Column 4, we switch to county-by-year fixed effects. In this specification, we are comparing the fertility behavior of households within the same county in the same year. Note that this is a fairly demanding specification, and it will be biased toward zero if changes in the fertility behavior of one group has any spillover on the behavior of the other group within the same location, which may explain why the coefficient falls somewhat when we include county-by-year effects. In Column 5 we add occupation-year fixed effects as well. Across all specifications, our estimates suggest that British-origin households reduced their fertility by 1.1-1.8 % per year relative to Francophone households. This is a substantial change, equal to a 3.7-6.0% decrease in the baseline probability of having a child in a particular year (0.295 in our sample).

Appendix Table 10 examines the robustness of our preferred specification (in Column 5). We look at results restricting attention to only households that live in Quebec and Ontario, extending our panel back to 1871, including additional fertility controls (such as whether a household had a child in the previous year), using a stricter definition of British households, or dropping all Irish immigrants from the sample. All of those alternative approaches yield results similar to those in Table 2. The 1881 Canadian census asked households about their religion, and so we also consider results using non-Catholic status as our proxy for culturally British. The results from this specification are nearly identical. We have also considered including households with zero children in 1874. In that sample we still find statistically significant results, but with a smaller magnitude, which indicates that British-origin couples with no children initially were less likely to reduce their fertility after 1877 than those that already had one or more children. This provides some indication of the margin of adjustment

that may be behind our results, but for space reasons we leave a full examination of the mechanisms of adjustment for future work.

To summarize, the results in this section show that British-origin Canadians reduced their fertility sharply, even relative to other Canadians living in the same county, at exactly the same time as fertility declined in Britain. This pattern is hard to explain as a result of economic factors or, given that Canada made its own domestic policy during this period, by changes to laws governing things like child labor or women's rights. In contrast, cultural ties, operating through either changes in social norms or the dissemination of contraceptive knowledge, offer a plausible explanation for the patterns we observe, particularly when, as we discuss next, similar patterns are observed among culturally-linked populations in other parts of the world.

Table 2: Marginal effects from Canada household-level difference-in-differences

| **DV: Indicator for whether household is observed as having a birth in a given year** | | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| British HH × Post 1877 | -0.0178*** | -0.0177*** | -0.0177*** | -0.0111*** | -0.0110*** |
| | (0.0016) | (0.0016) | (0.0016) | (0.0019) | (0.0022) |
| British HH indicator | Yes | Yes | Yes | Yes | Yes |
| Parental age FEs | Yes | Yes | Yes | Yes | Yes |
| Year FEs | Yes | Yes | Yes | | |
| County FEs | | Yes | Yes | | |
| Occupation FEs | | | Yes | Yes | |
| County-by-year FEs | | | | Yes | Yes |
| Occupation-by-year FEs | | | | | Yes |
| Observations | 1,408,311 | 1,408,311 | 1,408,311 | 1,408,311 | 1,408,311 |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by county. A household is classified as British-origin if either the mother or father are British-origin. Occupation fixed effects correspond to the following broad categories: legislators, senior officials, and managers; professionals; technicians and associate professionals; clerks; service workers and shop workers; skilled agriculture and fishery workers; crafts and related trade workers; plant and machine operators; elementary occupations; and armed forces.

# 3 Evidence from the United States

This section provides a second view of the synchronized fertility changes that occurred among British-origin populations outside of Britain. The evidence comes from exam-

ining changes in fertility patterns at the household level.[26] As in our analysis of the Canadian micro-data, our interest is in whether the probability that a household had an additional child in a year, conditional on parents' age, county of residence, and father's occupation, changed differentially for British-origin households after 1877. The main challenge in comparing fertility patterns in the United States relates to the lack of observable information on cultural ties. This is most problematic for the native U.S. population, which tended to share a common language with Britain but included households that originated from many different European countries. The closest available proxy for cultural ties comes from birthplace information, which was recorded in the 1880 census. The 1880 census asked every individual where they were born and where their parents were born. Thus, our analysis focuses on (first and second-generation) European immigrant households where we differentiate between British-origin households (the treatment group) and other European-immigrant households that were not of British origin (the control group).

An individual (husband or wife) is said to be of British origin if they report being born in Britain or if they were born in the United States but report that both of their parents were born in Britain. An individual is said to be of non-British but otherwise European origin if they were born in any European country other than Britain or if they were born in the United States but both of their parents were born in Europe (but not Britain).[27] Our treatment group (British-origin households) is defined as any household that contains either a British-origin mother or a British-origin father, though in robustness exercises we also estimate results using a definition that requires both parents to be of British origin. The control group is the set of first and second generation European immigrant households where neither the mother nor the father are of British origin. As in the Canadian analysis, we focus our main results on fertility among couples who already had at least one child in the household in 1877 (though in robustness we include couples with zero children). Summary statistics for

[26] We use the complete count 1880 census, as made available by IPUMS.org. That census maintains the structure of the household and so, as in Canada, we infer fertility based on the ages of the children residing in the household at the time of enumeration.

[27] We exclude Irish-origin households from the main analysis because, while Ireland was unified with Britain at this time, many Irish immigrants to the U.S. were Catholics with limited cultural affinity to Britain. A number of them did not even speak English. Thus, they do not fit in with either the British-origin or non-British-origin immigrant groups. This differs from Canada, where the majority of Irish immigrants were protestants from Northern Ireland, with stronger cultural ties to Britain. However, our robustness checks show that our results are not dependent on this decision.

the U.S. data are in Appendix Table 11.

Table 3 presents our U.S. results which are based on the same estimating equation as the Canadian micro-data analysis (Eq. 2). In Column 1, we control only for parent age and year. In Column 2 we add in county fixed effects. Column 3 adds in occupation fixed effects for five broad categories (agriculture, trade and transport, etc.). In Columns 4 and 5 we add in state-by-year fixed effects and occupation-by-year fixed effects.[28] The estimates indicate that British-origin households reduced their fertility by about 2.1%, a figure that is remarkably stable across specifications.

Appendix Table 12 presents results from various robustness checks mirroring those in the Canadian section: with additional fertility controls, using a stricter definition of what is a British-origin household, including Irish households in the analysis, or including couples that did not have a child in the initial year. In all cases we find strong evidence of a relative fertility reduction among British-origin households. As in the Canadian results, the inclusion of couples without children initially reduces the estimated coefficient, which suggests that this group was not as affected by treatment as those households that already had one or more children.

Overall, the U.S. results confirm, in a different setting, the patterns identified in Canada. Specifically, British-origin first and second-generation immigrant households exhibit fertility reductions relative to other European-origin immigrant households living in the same area and with common parent characteristics. In terms of magnitude, the estimates in Table 3 are larger than those obtained in Canada. Note, however, that there are important data-driven differences between our definition of the British-origin population across these specifications. In Appendix Table 13 we provide estimates focusing on one group, first-generation immigrants from Britain, where we can observe a similar treated sample in the two countries. Those results show that estimates from the U.S. and Canada are remarkably similar and statistically indistinguishable when focusing on a comparable treated group. Thus, among the most comparable population that we can observe in both settings, we find effects of similar magnitudes in the U.S. and Canada.

[28] In principle we could include county-by-year fixed effects, but this is computationally challenging because there are over two thousand counties in our sample and each would have to be interacted with eight year indicators. Moreover, the inclusion of county fixed effects and state-by-year effects has little impact on our results, which suggests that variation across locations is not playing a major role in our results.

Table 3: Marginal effects from US household-level difference-in-differences analysis

| **DV: Indicator for whether household is observed as having a birth in a given year** | | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| British HH × Post 1877 | -0.0209*** | -0.0211*** | -0.0211*** | -0.0210*** | -0.0211*** |
| | (0.0031) | (0.0031) | (0.0031) | (0.0022) | (0.0022) |
| British HH indicator | Yes | Yes | Yes | Yes | Yes |
| Parental age FEs | Yes | Yes | Yes | Yes | Yes |
| Year FEs | Yes | Yes | Yes | | |
| County FEs | | Yes | Yes | Yes | Yes |
| Occupation FEs | | | Yes | Yes | |
| State-by-year FEs | | | | Yes | Yes |
| Occupation-by-year FEs | | | | | Yes |
| Observations | 3,647,803 | 3,647,803 | 3,647,803 | 3,647,803 | 3,647,803 |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by state. County-clustered standard errors are about 50% smaller (results available upon request). Occupation fixed effects correspond to the following 5 categories: agriculture; professional and personal services; trade and transport; manufacturing, mechanical, and mining; and not in the labor force. Those definitions are as described in the "OCC" variable on ipums.org. Sample is restricted to European-origin households, which are classified as having either a mother or father that is inferred to be of European origin. Origin inferred from reported information of where the individual was born or where their parents were born.

# 4 Evidence from South Africa

South Africa, specifically the Cape Colony, provides a third location for studying changes in fertility patterns among culturally-British households embedded among non-British households of European origin. While the Cape was 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. This facilitates a comparison of fertility patterns between the British and Afrikaner populations before and after the trial.

Our analysis of South African follows the same pattern as the aggregate-data 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.[29] Data tracking fertility for these groups are drawn from the Cape Colony censuses of 1875 and 1891.[30] 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 (somewhat like a U.S. county), the lowest geographic unit for which consistent data are available. We consider two measures of a division's British connection: the share of European-origin population in a division that was born in the British Isles or 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.

The results from South Africa, reported in Appendix Table 15, display the same basic pattern observed in Canada: locations with a greater connection to Britain experience a reduction in fertility after 1877, 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. The results are somewhat sensitive to whether the regressions are weighted because the British-origin population was disproportionately concentrated in more populated areas.

# 5 Summary

To summarize our main results, we have shown that (1) a sharp reduction in fertility took place in England and Wales starting in 1877, (2) a similar change, in terms of both the timing and magnitude, is observed among the culturally-British populations in Canada, but not among their Francophone neighbors, and (3) similar patterns are also observed when comparing culturally-British populations in the United States and South Africa to other non-British European-origin populations living in the same locations. These results, particularly our comparison of Anglophone and Francophone households living in the same counties in Canada and the comparison of British immigrants and non-British European immigrants living in the same locations in

[29]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.

[30]See Appendix 8.5 for further discussion of the South Africa data.

the U.S., are extremely difficult to explain as a consequence of changing economic conditions or government policies. This effectively allows us to rule out the two primary alternative explanations to cultural factors. Moreover, the fact that the changes we document occur specifically across culturally linked populations provides affirmative evidence that culture played a central role in the patterns we observe.

These results raise two important questions. First, what event, or set of events, might have acted as the catalyst for the cultural changes that we observe? Second, what were the mechanisms through which households adjusted fertility? Both of these are challenging questions to answer. In the remainder of this paper, we focus on the first of these. Given space limitations, we leave the second for future work.

# 6 A plausible catalyst: The Bradlaugh-Besant trial

A careful reading of the historical record suggests that the most plausible catalyst for the large change in cultural attitudes surrounding fertility restriction is the famous Bradlaugh-Besant that took place in London in 1877. The impetus for the Bradlaugh-Besant trial was the 1875-76 publication of *The Fruits of Philosophy*, a book written by the American doctor Charles Knowlton in 1832. The book itself had been available in England since 1834 and was never challenged, perhaps because it always sold in very small numbers. The 1875-76 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 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. Bradlaugh, together with Annie Besant, another active secularist reformer, realized the case against Watts could be used to gain publicity for their views on family planning while also testing the government's right to censor work of this kind. They decided to publish a new version of Knowlton's book, with some updated medical knowledge, and informed the magistrates and city police of the time and place of sale in order to prompt arrest and trial.

The first hearing of the trial, at Guildhall in April, 1877, is reported to have attracted a crowd of 20,000 people (Ledbetter, 1976). The main trial, at the Queen's Bench, began in June. Lasting five days, it provided Bradlaugh and Besant a public

forum for expounding their views. The key issue in the trial was the morality of choosing family size. Bradlaugh and Besant made a strong case for population control as a solution to poverty. Against them, the Solicitor-General argued[31],

> *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...*

Ultimately, the jury found the pair guilty of publishing a book "calculated to deprave the public morals." They were fined £200 each and sentenced to six months imprisonment, though the verdict was reversed on a technicality in 1878. However, while the jury found Bradlaugh and Besant guilty, in his remarks during the trial the presiding Lord Chief Justice made clear his disapproval of the prosecutor's decision to bring the case to trial in the first place. These remarks were broadly interpreted as allowing future publication of similar work (Himes, 1970, pp. 240 and 243).

The trial brought substantial attention to a subject which had long been taboo in Victorian society. Widespread newspaper coverage of the trial played a crucial role in disseminating Bradlaugh and Besant's message. 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."[32] Coverage was found in national papers such as the conservative *Times* and more liberal *Daily Telegraph* as well as local papers throughout the country (Banks & Banks, 1954).

Figure 3 offers a more comprehensive look at newspaper coverage in England and Wales. The left-hand panel plots the number of articles published in 1877 that mention each of the following keywords: "Bradlaugh" or "Besant"; "Fruits of Philosophy"; or "Population Question."[33] The timing of publications matches key moments of the trial. For instance, we see no mentions of any of these search terms prior to April of

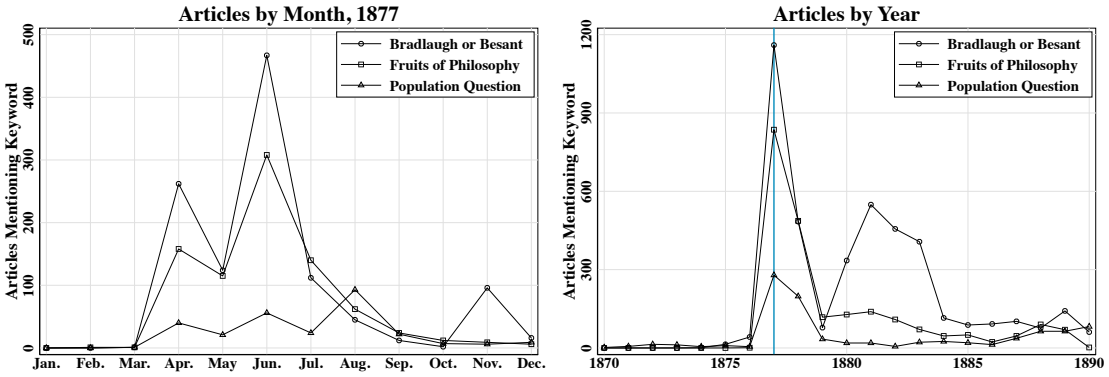
[31] Quoted from Manvell (1976), p. 147.

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

[33] The underlying data come from our own search of digitized articles from the British Library's holdings. We discuss this source in greater detail below. Note that these searches only include articles published in England and Wales.

1877, when Bradlaugh and Besant were arrested and the first hearing at Guildhall took place. We see a dip in publications in May and then the number of articles peaks in June, 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 3: British Newspaper Articles Mentioning Various Keywords




Data obtained from author's own search of digitized articles published in England and Wales as made available from https://britishnewspaperarchive.co.uk.

The right-hand panel plots, for these same keywords, the number of articles published on an annual basis from 1870 to 1890. 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 nearly 1200 articles mentioning either Bradlaugh or Besant, roughly 800 articles mentioning "Fruits of Philosophy", and just under 300 articles mentioning the "Population Question". Mentions of all three terms remain elevated for several years.[34] The pattern we observe for "Population Question" is particularly informative because this was a term that the Victorians used to describe what today we would call the debate over family planning. That the use of this term tracks mentions of Bradlaugh and Besant so

[34]In 1878 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. Mentions of Bradlaugh and Besant are a bit more volatile, reflecting other controversies that they would eventually get involved with. For instance, Bradlaugh's election to Parliament in 1880 resulted in a major controversy when he argued that he should not have to take the religious Oath of Allegiance.

closely illustrates the extent to which the trial drove the broader debate over family planning. Overall the patterns in this graph provide support for the claim that the issue of family planning was not widely discussed before the trial and that the trial helped start an open conversation of this topic.

What information were these newspaper articles and other pamphlets related to the trial disseminating? To answer this question, we conducted a thorough review of both the books and the newspapers related to the trial to understand the nature of the debate that it generated (see Appendix 8.7.2). Reading through these materials, it becomes clear that the central focus of the trial, and the broader debate that surrounded it, was over whether married couples–and the literature in this period was always aimed at married couples–had a moral right to choose their family size. While the idea that couples should have such a right may sound obvious today, this point was controversial 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 couples had a right, and even a responsibility, to choose.[35]

In addition to these moral questions, the trial and the material related to it also included a limited discussion of specific family planning techniques. Contraception itself was never discussed in the newspapers, but it was discussed extensively in the thousands of pamphlets published during and after the trial, though the techniques were rudimentary and in some cases information was incorrect.[36] It thus seems unlikely that the trial improved available contraceptive technology, but it may have facilitated the dissemination of information about available techniques.[37] Because

[35] 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.

[36] 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.

[37] This conclusion is consistent with existing work suggesting that the main methods remained largely unchanged during this period (see Guinnane (2011) and Szreter (1996) Ch. 8).

contraception was rarely discussed in a public way, assessing the extent of this knowledge dissemination directly has proven challenging.

Within England and Wales, the relatively high levels of literacy during this period (around 80% at marriage), and the large numbers of newspapers situated all around the country, meant that many people were probably exposed to the trial through this channel. In addition to reading newspapers, people were probably exposed to these ideas through conversations with others who read them.[38] Bradlaugh and Besant also gave several public lectures around the country.[39] Finally, there was the Malthusian League, an organization dedicated to spreading family planning ideas, which was established following the trial.[40] The trial also generated an enormous increase in sales of books on family planning in England and Wales.[41]

## 6.1 Reports of the trial abroad

News about the trial spread rapidly outside of England, particularly in British colonies. In Canada, for example, the trial was covered in English-language Canadian newspapers.[42] 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 also found 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

[38] Teitelbaum (1984) 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.

[39] 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 as far afield as Leigh, near Manchester, where "well-attended meetings were held at which neo-Malthusian doctrines were advocated and tracts distributed."

[40] D'Arcy (1977) estimates that the League printed over 850,000 pamphlets from 1879-1889.

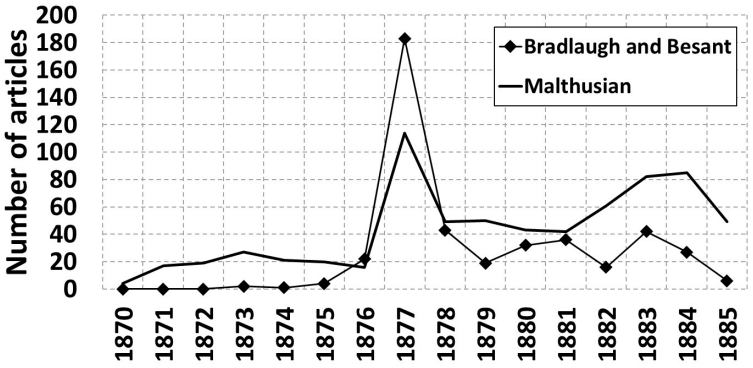
[41] This included Besant's book, *Law of Population*, which was published in 1877 and sold 175,000 copies by 1891. Other similar works, such as Dr. H.A. Allbutt's *Wife's Handbook* appeared soon after, and demand increased for books, such as George Drysdale's *Elements of Social Science* and Robert Dale Owen's *Moral Physiology*, which attracted little attention before 1877. Himes (1970) estimates that (p. 251), "Probably not less than a million tracts...were sold in England between 1876 and 1891." This is substantial since the population of the U.K in 1871 was just over 31 million.

[42] Unfortunately, to our knowledge, English-language Canadian newspapers have not been systematically digitized on a large scale. However, a review of the few digitized newspapers as well as a number on microfilm shows that the Bradlaugh-Besant trial was covered in English-language papers.

(June 19, 1877), the Naniamo Daily News (July 14, 1877) and the English-language Montreal Gazette (Feb. 13, March 2, May 30, June 4, June 6, June 20 and July 15, 1878).[43] Other channels, such as letters and the flow of immigrants, were also likely important in transmitting news of the trial across the Atlantic.[44]

In contrast to English-language papers, we find no evidence that the trial was reported in French-language papers in Canada. A search for articles about Bradlaugh and Besant in the rich French-language newspaper archive of the Biblioteque et Archives National du Quebec turned up no articles about Bradlaugh, Besant, or the trial in the late 1870s. This does not appear to be simply an issue with our ability to search the French-language papers: we do find numerous reports in the French-language press about Charles Bradlaugh after 1880, when he became embroiled in a different controversy after he was elected to Parliament but refused to take the oath required to take his seat.

Figure 4: U.S. Newspaper Articles Mentioning Bradlaugh and Besant




Data from Newspapers.com obtained on August 20, 2020.

In the United States, the trial was widely covered and "excited a great deal of interest on both sides of the Atlantic" (*The Hawaiian Gazette*, Aug. 29, 1877). Figure

[43]There is also some evidence that other material related to the trial was being circulated. For example, on June 20, 1878, the Ottawa Citizen reported: "Toronto, 19th – A man named Robert Robins, alias Whittaker, was arrested today for sending indecent literature through the post...the indecent publication for circulating which he is arrested is Bradlaugh's Prints of [*The Fruits of*] *Philosophy*, the book recently prohibited in England."

[44]Letter flows between the U.K. and Canada during this period 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. In addition, tens of thousands of migrants that arrived on Canadian shores each year, many from the British Isles (McInnis, 1994).

4 shows the number of U.S. articles appearing on Newspapers.com that mention either "Bradlaugh" and "Besant" or "Malthusian". Even in this incomplete database we find over 180 articles mentioning Bradlaugh and Besant in 1877 and over 100 mentioning "Malthusian" (if we search Bradlaugh and Besant separately we get 406 hits for Besant and 393 for Bradlaugh). Other U.S. newspaper databases show similar patterns (see Appendix 8.6.2), though their databases are more limited than Newspapers.com for this period. In addition to newspaper coverage, four new U.S. editions of *Fruits of Philosophy* were released in the second half of the 1870s, by publishers in Boston, Chicago and Kentucky (Brodie, 1994) while an American edition of Besant's book was published in New York in 1878 and second edition in 1886.

In South Africa, where digitized newspaper records are sparse, we also find evidence of arguments over family planning prompted by the trial playing out in the papers. To take one example, a letter to the editor of *The Natal Witness* (May 25 1880) lamented that *The Fruits of Philosophy* "would never have acquired its world-wide and much-to-be-deplored inodorous reputation, had not Bradlaugh propagated its doctrines..." As this quote suggests, the discussion of family planning was not universally welcomed. At the same time, the fact that a writer in Natal would recognize the "world-wide" effect of the trial, even if this particular writer was not supportive of the doctrines it disseminated, provides an indication of the extent to which the trial generated a broad discussion of family planning and contraception.

Data from Google N-grams provides further indications of the impact of the Bradlaugh-Besant trial. These data, presented in Appendix 8.6.1, show a dramatic increase in the appearance of terms related to the trial, such as "Bradlaugh," "Besant," "conjugal prudence," and "Fruits of Philosophy" in the years just after 1877.

## 6.2 Evidence on the impact of the Bradlaugh-Besant trial

While the historical evidence in the previous section suggests that the trial was important, we would like to have more direct evidence that it actually influenced fertility behavior. In this section, we look at how the change in fertility after 1877 differed in locations in England and Wales depending on how exposed each location was to news about the trial. We focus on England and Wales because, unlike the other locations we study, we are able to take advantage of a rich databases of newspaper articles that allows us to identify local variation in exposure to the trial. As noted earlier,

newspapers were only one way that people could be exposed to the trial, but we know that newspapers played a central role in disseminating information about the trial within England and Wales (Banks & Banks, 1954).

Our newspaper source is britishnewspaperarchive.co.uk, a partnership between the British Library and the UK genealogy service findmypast.com to digitize 40 million newspaper pages from the British Library's extensive collection. This database is quite rich; no similarly comprehensive database exists in the other locations we study.[45] 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 and Wales in 1877 where either of the two names appears in the text. There were 1,149 articles matching these criteria.

As already shown in Figure 3, the timing of the published articles matches pivotal moments of the trial, indicating that our search is a good proxy for trial coverage. We manually reviewed each article to ensure that we correctly identify articles related to the trial and manually classified each article into several categories (see Appendix Table 17). Roughly three quarters of the articles captured in our search were direct reporting on the trial. The remaining quarter of articles are either opinion pieces or tangential topics rather than explicit coverage of the ideas being discussed in the trial, but all of the articles identified in our search were related to the trial in some way. In our main analysis we measure exposure to the trial using all of the articles identified in our search, though in robustness checks we will explore alternative approaches.

We have also transcribed a sample of just under 500 articles on the trial published in England and Wales in 1877 (about 40% of the full set of articles). An analysis of the content of these articles, in Appendix 8.7.2, shows that, beyond factual reporting on trial, the discussion in the newspapers was centered on the morality of fertility control, offering support for our interpretation of news coverage as exposure to the ideas at the center of the debate.

Our measure of exposure to the trial is based on articles published in local papers.

[45] All of the U.S. databases, for example, are quite limited compared to what is available in Britain. To illustrate this, we searched for a generic word, "Monday" in 1877 in each database. This returned 472,283 hits in the British Newspaper Archive, 235,074 U.S. articles in Newspapers.com, 48,794 articles in the Chronicling America database, and just 25,987 in the Gale Nineteenth Century Newspaper Archive (as of August 24, 2020). Moreover, none of the U.S. databases appear to have consistent geographic coverage. In the Newspapers.com database, for example, a surprisingly large fraction of articles on the Bradlaugh-Besant trial come from Kansas.

This "provincial press" was highly influential during the second half of the nineteenth century, before the emergence of a national market dominated by London papers.[46] Provincial papers kept close track of events in London and beyond through their own reporters as well as telegraph services such as the Press Association (Williams, 2010, p. 117). We identify local articles using the place of publication reported in the newspaper data, which we match to registration districts, the smallest administrative unit with consistent birth and population data.[47] To reflect the fact that provincial papers usually served not just the town where they were published but also the surrounding area, we measure exposure to the trial in a district by tabulating the number of articles published in that district or other districts within a given geographic radius (usually 25km, though we explore alternatives). Appendix Figure 14 plots the spatial distribution of this measure.

### 6.2.1 Other Data for England and Wales

Our outcome variable is the district's birth rate: births per 1000 fertile aged women (between the ages of 15 and 50). The births data come from the annual Reports of the Registrar General, which we digitized for the purposes of this analysis.[48] We focus on the average birth rate over a five-year period (e.g., 1873-1877, and 1878-1882), which smooths over some of the random variation observed in annual births and increases comparability with our Canadian analysis.[49]

We have also constructed a rich set of district-level control variables reflecting key factors thought to influence fertility behavior. These include controls for infant and overall mortality rates, population density, local industrial structure, religious

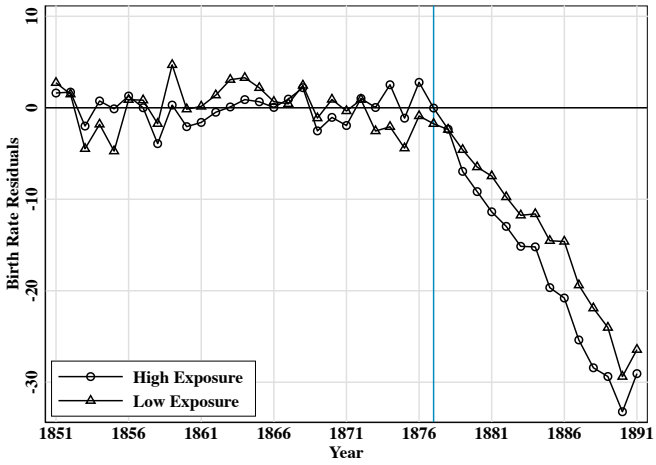
[46] In 1877, Gladstone said that "there was more political power in the provincial press than in the whole of the London press" (Williams, 2010, p. 8).

[47] While there are over 600 districts in England and Wales, many district boundaries changed at some point during our study period. We adjust for this by combining districts where boundary changes shifted more than 200 residents from one district to the other. Our main analysis will also exclude the districts that comprise London, since it was the location of the trial and also the center of the national press. As a result, newspaper articles may not be a good indicator of exposure. These adjustments leave us with 430 consistent districts spanning 1851 to 1891.

[48] Several papers, starting with Glass (1951) have examined the quality of these data. A review of these studies by Woods (2000) (Ch. 2) indicates that the registration data captured about 98% of all births by the early 1860s and that registration was essentially complete by the mid-1870s.

[49] District-level tabulations of population by age and sex are only available in census years, and so we linearly interpolate these tabulations to construct an annual estimate. Results are not sensitive to the length of our window, but we chose five years because that is the smallest window that we are able to consider in the Canadian analysis.

Figure 5: Birth Rates by Newspaper Exposure after Accounting for Regional Trends




Births rates are defined as births per 1000 fertile aged women (women between the ages of 15 and 50). The births data were transcribed from annual reports of the Registrar General. The population denominator is linearly interpolated between census years. Residuals obtained by regressing district-level birth rates on district fixed effects and region-specific time trends (fitted between 1851 and 1877). The vertical line at 1877 corresponds to the year of the Bradlaugh-Besant trial. High-exposure districts are those with more than the median number of articles published within 25km. The districts that comprise London are omitted from this figure.

affiliation, literacy at the time of marriage, etc. Further details on the data and construction are in Appendix 8.7.3.

### 6.2.2 Analysis of the Bradlaugh-Besant trial

Figure 5 offers a first look at the impact of the trial on fertility rates. There we normalize each district's birth rate by first regressing district-level birth rates (between 1851 and 1877) on a set of district fixed effects and region-specific linear time trends. We then take the residuals from this regression (for both the sample period, 1851-1877, and the post-trial period, 1878-1891) and then plot the average residual among "High Exposure" and "Low Exposure" districts for each year. "High Exposure" districts are those with more than the median number of articles (six) mentioning Bradlaugh or Besant in 1877 within a 25km radius. "Low Exposure" are districts with below median exposure. In this figure, and all subsequent analysis, we exclude London since that was the center of the trial and the home of the national press.

Figure 5 indicates that, at least after accounting for regional trends, birth rates in high and low exposure districts were trending together prior to the trial. After the trial, we see birth rates decline in both sets of districts but the decline is relatively stronger among districts with more trial coverage. This graph presents a useful framework for thinking about our results. While both sets of districts are likely treated by the shock generated by the trial, we expect the ideas to be more widely discussed in districts with greater coverage, and thus we expect a stronger decline. We will examine this relative divergence more formally within a difference-in-differences framework, though that framework is going to treat "Low Exposure" districts as our counterfactual. Note that, because people in our "Low Exposure" districts were also likely to have been exposed to the trial (just not as intensively as those in "High Exposure" locations), our results will be informative about whether the trial was having an impact, but not about the specific magnitude of the overall effect.

Before turning to those results, let us formally describe our estimating equation. Most of this analysis compares average district-level birth rates in two five-year periods (1873-1877, the pre-period, and 1878-1882, the treatment period), though some specifications examine additional periods to assess the dynamics of the effect. Our general specification is thus,

$$BR_{dt} = \beta_0 + \beta_1 EXPOSURE_d * TRIAL_t + X_{dt}\lambda + \gamma_d + \phi_{rt} + \epsilon_{dt} \tag{3}$$

where $BR_{dt}$ is the average birth rate in district $d$ during period $t$, defined as births per 1000 fertile aged women.

We use newspaper coverage as a proxy for treatment. For each district, $d$, we begin by calculating the number of articles published within a 25km band of the district that mention Bradlaugh or Besant.[50] We then classify districts as high-exposure ($EXPOSURE_d = 1$) and low-exposure ($EXPOSURE_d = 0$), where high-exposure districts are those with more than the median number of articles published within 25km.[51] This 25km band reflects the fact that our districts are small and the provincial papers we study usually served surrounding areas. In the robustness

[50] Our district centers are typically the main town of the district, though in some very rural districts we instead use the geographic center.

[51] In Appendix Figure 15 we present results using a more continuous measure of treatment. These results suggest that a simple division of high vs. low captures most of the impact of exposure to articles about the trial.

exercises we consider a variety of alternative approaches.

The variable $TRIAL_t$ is an indicator equal to one once the Bradlaugh-Besant trial took place, i.e. during 1878-1882 period. The variable $\gamma_d$ represents our district fixed effects while $\phi_{rt}$ represents a set of region-by-year fixed effects. Finally, we adjust the standard errors by clustering at the district level.[52]

To help strengthen identification, our analysis includes controls for a rich set of factors that existing work suggests may have influenced fertility during this period. Specifically, the vector $X_{dt}$ contains two broad sets of controls (district-level marriage controls and other district-level controls), which we allow to fully interact with our period indicators. Our "marriage controls" include: the district's marriage rate from 1872-1877, the share of those marriages that took place at the Registrar's office (i.e., non-religious), the share that took place in a Catholic church (which helps control for both religious affiliation as well as the Irish population in each location), the share of marriages that were first marriages, the share of brides and grooms that were minors, and the share of brides and grooms that were illiterate at the time of marriage. Our "district-level controls" include: the district's population density, total mortality rate, infant mortality rate, share of births between 1873 and 1877 that were illegitimate, share of workers engaged in agriculture-related occupations, share of workers engaged in manufacturing occupations, share of the fertile-aged population that is under 30.

In addition, one may worry that our results are biased because newspapers or newspaper articles about the trial were not randomly distributed across locations. As a step toward addressing this issue, we include three district-level controls to account for pre-existing exposure to other shocks that may have been transmitted through newspapers, as well as pre-existing liberal slant. The first of these controls is the number of articles published within a 25km radius that mentioned the word "Monday", a proxy for the overall distribution of newspapers. The second control is an indicator for whether the district had above median exposure to articles mentioning "Bodichon" between 1860 and 1869. The 1860s saw several campaigns for women's causes. Barbara Leigh Smith Bodichon was a prominent leader in this movement and one that also happens to have a sufficiently unique name to allow us to identify

[52] We have also tried correcting our standard errors to account for serial and spatial autocorrelation at the 25km, 50km, and 100km 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.

relevant articles.[53] Our final control is an indicator for whether the district had above median exposure to articles mentioning "Gladstone", a prominent Liberal politician from this period. While the inclusion of these controls may not capture all of the non-random factors that contribute to the geographic distribution of articles about the trial, together with our rich set of controls for factors that influence fertility they help limit the potential impact of this source of bias.

Table 4 presents our primary results for England and Wales. Column 1 offers a baseline estimate adjusting only for differential regional trends.[54] There we find that birth rates in high exposure districts fell by 3.4 births per 1000 fertile-aged women following the trial in more exposed locations.

In Column 2, 3, and 4 we assess the sensitivity of our results to (independently) including our three broad categories of controls. We continue to find strong evidence of a decline in fertility in districts with relatively more coverage of the trial. The point estimate does vary depending on which set of controls is being considered, but the results in columns 2-4 are often statistically indistinguishable.

In Column 5 we present results adding in all three categories of controls. We view the results in Column 5 as our preferred specification because it accounts for regional trends and a wide set of district-level controls. Relative to Column 1, the magnitude of the estimated coefficient has dropped somewhat, though the R-squared has increased from 0.261 in Column 1 to 0.605 in Column 3. At the bottom of the table we follow Oster (2019) and calculate the estimated coefficient assuming that the R-squared is increased by a factor of 1.3 by unobservables that influence the outcome as strongly as the observables. Here we still find evidence of a substantial differential change in fertility, suggesting that our results are unlikely to be driven by unobservable factors.

[53] Bodichon published "A Brief Summary of the Laws in England concerning Women: Together with a few observations thereon" in 1854 and "Reasons for the Enfranchisement of Women" in 1866. Other prominent women in this movement, such as Lydia Becker, Helen Taylor, and Emily Davies, had much more common surnames.

[54] We find stronger results if region effects are not included in the specification, but there is evidence that some regions were on differential fertility trends in the pre-period (see, Figure 1), so we include those controls in all of our main results.

Table 4: Difference-in-Differences results for England and Wales

| | **DV is Births per 1000 fertile-aged women** | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| High News Exposure × 1868-1872 Period | | | | | | -0.351 |
| | | | | | | (1.300) |
| High News Exposure × 1873-1877 Period | | | | | | . |
| | | | | | | . |
| High News Exposure × 1878-1882 Period | -3.392*** | -2.193*** | -1.973* | -3.108** | -2.864** | -2.555*** |
| | (1.057) | (0.833) | (1.017) | (1.473) | (1.110) | (0.949) |
| High News Exposure × 1883-1887 Period | | | | | | -2.864** |
| | | | | | | (1.174) |
| District fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Region-by-period fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Marriage controls | | Yes | | | Yes | Yes |
| Other district controls | | | Yes | | Yes | Yes |
| Newspaper controls | | | | Yes | Yes | Yes |
| No. districts | 430 | 430 | 430 | 430 | 430 | 430 |
| Observations | 860 | 860 | 860 | 860 | 860 | 1,720 |
| R-squared | 0.261 | 0.538 | 0.399 | 0.265 | 0.608 | 0.397 |
| Coef. implied lower bound following Oster (2019): | | | | | -1.353 | |

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the district level, in parentheses. Spatially-corrected standard errors are typically smaller, and thus less conservative. All regressions weighted by 1861 district population. Birth rates are defined as number of births per 1000 fertile aged women (ages 15-50). These birth rates are averaged over a five year windows (e.g., 1873-1877, the pre-trial period, and 1878-1882, the post-trial period). "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. Our marriage, district, and newspaper controls are fully interacted with our period indicators. The marriage controls include: the marriage rate from 1872-1877, the share of those marriages that took place at the Registrar's office (i.e., non-religious), the share that took place in a Catholic church (which helps control for both religious affiliation as well as the Irish population in each location), the share of marriages that were first marriages, the share of brides and grooms that were minors, and the share of brides and grooms that were illiterate at the time of marriage. The "Other district controls" include: population density, total mortality rate, infant mortality rate, share of births between 1873 and 1877 that were illegitimate, share of workers engaged in agriculture-related occupations, share of workers engaged in manufacturing occupations, and the share of the fertile-aged population that is under 30. The newspaper controls include: the number of articles published within a 25km radius that mentioned the word "Monday", a proxy for broader newspaper exposure, an indicator for whether the district had above median coverage of the prominent feminist "Bodichon", and an indicator for above median coverage of "Gladstone", the most prominent Liberal politician. In Column 6, we allow our controls to be time varying by fully interacting them with our period indicators. The implied lower bound is calculated assuming that the unobservables would increase the r-squared by 30% and that the selection on unobservables is equal to the selection on observables, as suggested by Oster (2019).

The fact that our coefficient estimates move as different sets of controls are added raises questions about the sensitivity of our results to the set of control variables included. To evaluate this, we estimate results using every possible combination of our sixteen control variables (i.e., including from one up to sixteen variables in various combinations), a total of 65,535 regressions. Across this large set, we estimate an average point estimate of -2.31 (s.d. of 0.75) with 95% of the estimates more negative than -1.08 and no positive point estimates. Given our rich set of controls, it is not surprising that our results vary as different combinations are included, but these results show that the basic message, that greater exposure to the Bradlaugh-Besant trial reduced fertility, is consistent throughout.

Finally, in Column 6 we look at patterns in the five years before 1877 and the five years after 1882. In the period just before treatment we see a small, positive, and statistically insignificant effect, which shows that high news exposure districts did not exhibit differential changes in birth rates on the eve of the trial. This suggests that the effects observed in Columns 1-5 were not driven by pre-existing trends and provides empirical support for the parallel trends assumption embedded in the analysis.[55] In the post period, we see a negative and statistically significant effect, which tells us that the effect observed in Columns 1-5 persisted. While the results in Column 6 indicate that our results are not driven by mean reversion in fertility rates, we provide additional evidence that that is not the case in Appendix Table 20.

In Table 5 we assess the robustness of our preferred estimate (Column 5 of Table 4). We begin by examining whether our main result is robust to changes in the underlying dataset. Columns 1 through 4 remove districts that were heavily dependent on textiles, mining, metals and engineering trades, or farming. These results show that our results are not being driven by populations working in any particular sector. Column 5 discards the 10% most rural districts based on population density while Column 6 discards the 10% most urban districts. Effects are stronger in urban than in rural districts, but we continue to see substantial effects in rural areas as well. Finally, Column 7 removes all districts in Wales to ensure that our results are not being driven by a simple England vs. Wales comparison. The main result is largely unaffected by these sample restrictions. In Column 8 includes controls for child labor force participation and female labor force participation (as observed in the 1871 cen-

[55] Also, note that the negative coefficient estimated for the 1868-72 period suggests that, if anything, any pre-existing trends would run counter to our results.

sus) interacted with our period indicators. These help us control for economic factors and policy innovations related to female and child labor. The main result is largely unaffected by the inclusion of these controls.

Next we consider alternative ways of measuring trial exposure. Column 9 drops all districts within 25km of London to be sure that our results are not being driven by the impact of the London papers in nearby districts (recall that London itself is excluded from our sample). In Column 10, we eliminate a small set of isolated districts by dropping any district more than 50km from a relevant newspaper article. Identification in this specification exploits both the comparison of districts within 25km of newspapers publishing an article to those outside of 25km but within 50km, as well as variation in the number of articles published in different locations. Column 11 explicitly controls for the number of articles published in the 25-50km band.

In Column 12, we define "high exposure" based on the number of articles published within the district. In Column 13 we define coverage based only on articles that directly reported on the trial, and in Column 14 we further restrict attention to articles with lengthy coverage of the trial. Results from these last two specifications are somewhat stronger, which is what we'd expect given that these are likely cleaner proxies for exposure to the trial.

It is natural to wonder whether the effect of exposure to the trial varied with other underlying district characteristics. It is possible to examine the interaction between our trial exposure variable and other district characteristics, though we often do not have enough power to generate sharp triple difference results. We do, however, find suggestive evidence that trial exposure had stronger effects in more urban areas and relatively weaker effects in areas where more couples married in the Catholic Church.

Table 5: Assessing the Robustness of the Impact of Trial Exposure on Fertility

| | No Textiles (1) | No Mining (2) | No Metals (3) | No Farming (4) | No Rural (5) | No Urban (6) | No Wales (7) |
|---|---|---|---|---|---|---|---|
| High News Exposure × 1878-1882 Period | -2.849*** (1.073) | -1.971* (1.071) | -2.155** (0.979) | -3.256*** (1.170) | -3.143*** (1.141) | -2.410** (1.120) | -2.121* (1.147) |
| Observations | 774 | 774 | 776 | 774 | 774 | 774 | 788 |
| R-squared | 0.621 | 0.512 | 0.499 | 0.617 | 0.621 | 0.602 | 0.589 |
| | Fem/Child Lab. Force Controls (8) | > 25km from London (9) | Within 50km of Article (10) | Control for 25-50km Exposure (11) | Own Article Only (12) | No Misc. Articles (13) | Only Lengthy Articles (14) |
| High News Exposure × 1878-1882 Period | -2.512** (1.033) | -2.688** (1.101) | -2.808** (1.121) | -2.777** (1.102) | -1.522* (0.822) | -3.119*** (1.059) | -3.945*** (0.972) |
| Observations | 860 | 840 | 824 | 860 | 860 | 860 | 860 |
| R-squared | 0.639 | 0.607 | 0.613 | 0.608 | 0.606 | 0.609 | 0.614 |

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the district level, in parentheses. All regressions include district fixed effects, region-by-period fixed effects, the marriage controls, other district controls, and newspaper controls as defined in Table 4, which we fully interact with our period fixed effects. All regressions are weighted by 1861 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). In columns 1-4 we drop the 10% of districts with the largest share of employment in textiles, mining, metals/engineering and agriculture, respectively. Since these industries were geographically concentrated this removes districts representing the vast majority of employment in each of these sectors. In columns 5 and 6 we drop the 10% most rural or most urban districts based on population density. Column 7 excludes wales. Column 8 includes as controls female labor force participation (in 1871) and child labor force participation in (1871), both interacted with our post indicator. Column 9 excludes any district within 25km of London. Column 10 restricts the sample to districts where an article was published within 50km. Column 11 controls for the number of articles published between 25 and 50km to better isolate the role of nearby article exposure. Column 12 defines treatment status based on the number of articles published within the district boundary. Column 13 defines treatment after discarding any of the miscellaneous articles while Column 14 defines treatment based on the set of lengthy articles that were published (both definitions discussed in the text).

To summarize, the results above provide evidence that the Bradlaugh-Besant trial had an important influence on fertility patterns in Britain starting in 1877. Further, available evidence shows that the trial was widely reported and discussed, including in all of the countries considered in our main analysis. Last, but just as important, the historical record does not offer any comparable event that could provide a plausible alternative.[56] This does not ensure that it was the only event that helped trigger the synchronized fertility reduction observed in culturally British populations around the world at this time, but the trial appears to be the most reasonable explanation for what could have set off the enormous fertility shift documented in our main analysis.

# 7 Conclusion

Largely due to a lack of direct and convincing evidence, the importance of cultural factors in the historical fertility transition has been set-aside in most recent economic literature in favor of explanations that rely on changes in the costs and benefits of having children.[57] By providing direct evidence on the impact of cultural factors during the historical fertility transition, our paper fills in a critical missing piece in our understanding of one of the most important events in economic history. Specifically, by examining the simultaneous fertility declines exhibited by culturally-British populations around the world, starting in 1877, we isolate the role of culture from other factors that affected fertility during this period. The fact that we observe British-origin populations reducing their fertility at the same time across a wide range of economic and policy environments allows us to rule out that the effects we document are driven by economic or policy changes. Moreover, because we rely on cultural ties to identify affected populations, our results speak directly to the role of culture.

Our results contribute to our understanding of the economic impact of culture. Specifically, we show that cultural and linguistic ties can act as conduits transmitting changing social norms or updated knowledge rapidly around the world, resulting in quick and meaningful changes in behavior.

[56] Chronological listings of key historical events, such as the *The People's Chronology*, do not identify plausible alternative events. In 1877, for example, *The People's Chronology*, includes only the following entry under the population category for 1877: "The tabooed subject of contraception comes into the open at the Bradlaugh-Besant trial."

[57] Though recent work by Spolaore & Wacziarg (2019) has shifted attention back toward culture.

It is important to recognize that, in highlighting the role of culture, we are not arguing that economic factors were unimportant for the fertility changes observed during this period. Rather, our results are consistent with a story in which economic forces generated strong incentives for reductions in family size but cultural norms or a lack of contraceptive knowledge constrained families away from these otherwise-optimal low fertility levels. In such a situation, the breakdown of cultural norms or the dissemination of contraceptive knowledge have the potential to generate rapid changes in behavior such as the shifts we document.

We have put forward a plausible catalyst for the cultural shifts that we observe: the famous Bradlaugh-Besant trial. This event was widely covered, including in all of the countries we analyze, but evidence from Canada suggests that this coverage was focused on the British-origin English-speaking population. Our evidence from Britain suggests that this trial did in fact have an impact on fertility patterns there, which lends credence to the idea that it may also have been the catalyst for the synchronized fertility changes observed across all of the populations that we study.

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, Britain's transition trailed the fertility reduction in France by roughly half a century, and preceded the onset of the transition in Germany by decades, suggesting that different cultural groups have the potential to maintain large differences in fertility patterns for long periods, even in the face of underlying economic forces, such as growing industrialization or the rising value of education, that incentivize fertility reduction.

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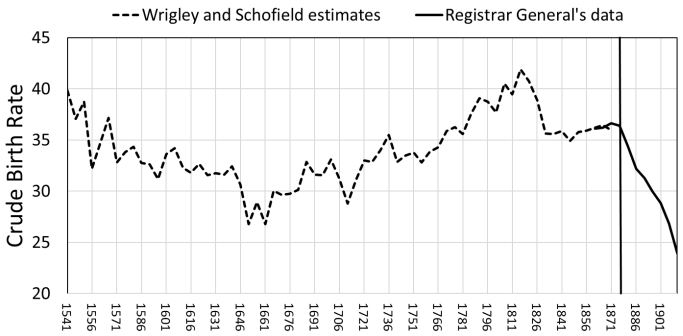
# 8 Appendix

## 8.1 Longer fertility time series for England & Wales

Figure 6 presents the crude birth rate in England and Wales from 1541-1915. The dotted line in this figure presents crude birth rate estimates for England from Wrigley & Schofield (1981) (Appendix Table A3.1). The solid line presents crude birth rates for England & Wales calculated using Census population data and birth data from the Registrar General's annual reports.[58] There are a couple of important patterns to take away from this graph. First, while birth rates had fluctuated substantially over the three centuries before 1876 (indicated by the vertical line on the chart), the sharp and consistent decline after that point looks completely unlike any previous decline. Second, prior to 1876, the birth rate had remained almost constant at between 35 and 37 births per thousand people for four decades. That four-decade period of stability was preceeded by several decades of relatively high fertility which generated the well-documented rise in population during the first few decades of the Industrial Revolution (lasting from roughly 1771-1831). The fact that fertility rates were stable for several decades before the sharp decline that began in the late 1870s, together with the unprecedented speed and consistency of the decline observed after that point, confirms that this was not simply a reversion to some historical mean. In fact, by 1886, less than ten years after the beginning of the decline, birth rates in England had reached a level last seen more than 150 years previously, during the food shortages of the early 18th century.

[58] For each quinquennial observation, the numerator is average births in the five years starting with the indicated year. For intercensal years we estimate population using linear interpolation.

Figure 6: Fertility in England and Wales, 1541-1915

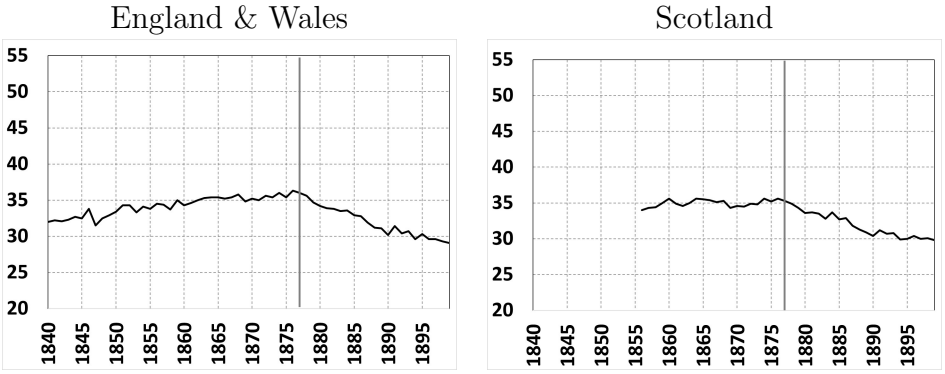



## 8.2 Comparing Britain with other European countries

This section compares the distinct pattern of fertility decline that we see in Britain to other major European countries. For comparability, all of the data used in this comparison comes from the same source, Mitchell (2003). Each series examines births per thousand of total population. This may not be the ideal statistic, because it does not account for variation in the distribution of population across age groups, but it has the advantage that it is consistently available for a number of countries.

As a baseline, Figure 7 presents the series for England and Wales (left panel) and Scotland (right panel) from this source. In both graphs, we see a clear break in fertility characterized by a sharp decline, equal to about five births per thousand, beginning in 1877.

Figure 7: Fertility in England, Wales and Scotland




Births per thousand of total population. Data from Mitchell (2003).

Figure 8 presents similar series for fifteen other European countries, the full set for which sufficient data are available from Mitchell (2003). The basic message from these graphs is that countries show a wide variety of patterns in terms of both the levels of fertility and the timing of any declines. No systematic European decline, matching the one observed in Britain, is observed. There are countries, such as Finland and Belgium, where fertility does appear to decline around the same time as the decline observed in England and Wales. In others, such as Denmark, Hungary, and Romania, fertility increases during this period. In Austria, Germany, Norway, and Russia, fertility is largely stable. In France and Ireland, there is a fertility decline after 1877, but that is merely the continuation of declines that began well before that point. Overall, there is no evidence that the decline observed in Britain is part of some more generalized European pattern. Below, we discuss these individual patterns in more detail.

Some countries, such as Austria, Germany, and Russia, exhibit a stable and relatively high levels of fertility across the graph. The main fluctuation observed is the baby boom in Germany following the country's victorious war with France in 1870. Despite this boom, Germany ended the period with a fertility rate that is almost exactly the same as at the beginning. Hungary also exhibited a high fertility rate during this period, which appears to be increasing across the 1870s and early 1880s and then declining after 1885.

France, in contrast, had a much lower, and declining, fertility rate. As in Germany,

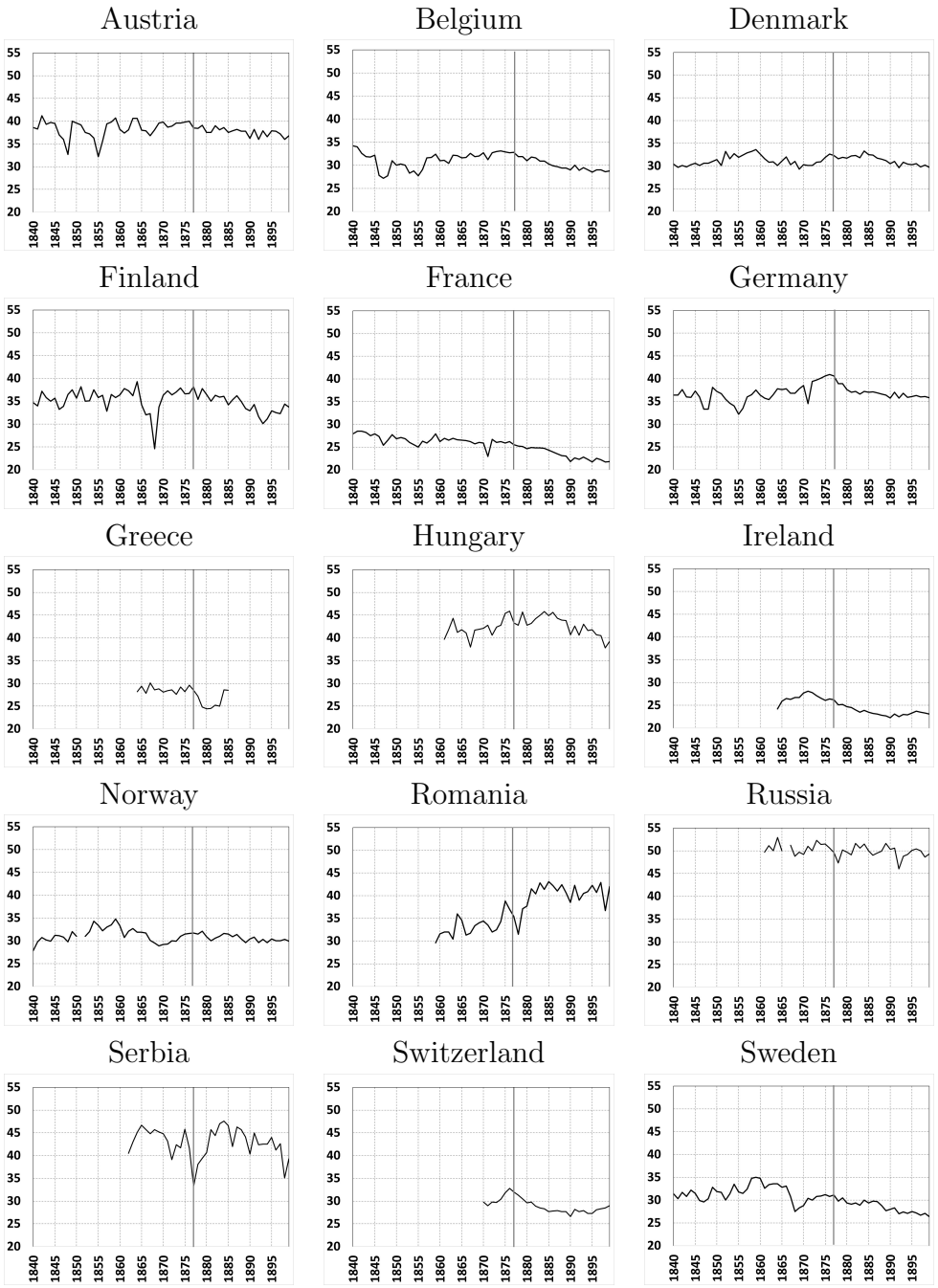
there is a drop in fertility during the war of 1870 and a small boom in the following years. The other country (though not independent at this time) that exhibits very low fertility is Ireland, where fertility was declining throughout the 1870s. This low rate was a consequence of the enormous demographic shock generated by the Irish famine in the late 1840s.

It is somewhat striking how much the patterns observed in Belgium differ from those found in France. Baseline fertility rates are much higher, though they appear to decline starting in the late 1870s. Of the countries in the sample, this is the one that appears the most similar to England & Wales in terms of the timing of the onset of a fertility decline, though the magnitude of the change is quite a bit smaller.

The other Scandinavian countries in this sample, Finland, Norway, and Sweden, start the period with moderate levels of fertility. Fertility in Norway and Denmark remained fairly stable across the study period, while Finland and Sweden show enormous reductions in fertility during the famine of 1867-69. Following the famine, fertility was fairly stable until 1880 and then begins a moderate decline. It is worth noting that the declines in the 1880s and 1890s in these two countries are likely to be the demographic echoes of the reduced fertility (and additional child mortality) in the cohorts born in the 1860s, which would have been in their prime fertile years two decades later.

In Southeastern Europe, Greece, Serbia, and to a lesser extent Romania exhibit sharp declines in fertility in the late 1870s followed by rapid rebounds. This pattern is probably associated with the Great Eastern Crisis, which included war between Serbia and the Ottoman Empire. However, in terms of levels, Greece exhibits much lower fertility than the others.

Figure 8: Comparing fertility patterns in major European countries




Births per thousand of total population. Data from Mitchell (2003).

## 8.3 Appendix to the analysis of Canada

### 8.3.1 Further details on the Canada data

Table 6 presents summary statistics for the data used in the county-level Canadian analysis. These data show that across the full sample period there were around 138 births per 1,000 fertile-aged females per year. Around 46% of the population of Canada in 1861 was of British origin according to the definition used in our main analysis, while 50.1% were of French origin.

Table 7 presents summary statistics for the data used in the household-level analysis in Canada, which are taken from the 1881 Census. We can see that the probability that a family in our sample had an additional child in a particular year is just under 0.3, while the total number of children observed is 4.9. This tells us that families were large and suggests that much of the adjustment we document may have been happening at parities above three.

Table 6: Summary statistics for Canada county-level analysis

| Variable | Mean | Std. Dev. | Min. | Max. | N |
|---|---|---|---|---|---|
| Children per 1,000 women per year (includes all periods, 1865-86) | 138.07 | 42.836 | 64.626 | 454.993 | 392 |
| British origin share, 1861 | 0.464 | 0.391 | 0.003 | 0.973 | 98 |
| French origin share, 1861 | 0.501 | 0.42 | 0 | 0.997 | 98 |
| Catholic share, 1861 | 0.61 | 0.361 | 0.042 | 1 | 98 |
| Ag. employment share, 1861 | 0.453 | 0.182 | 0.005 | 0.774 | 98 |
| Male/female ratio, 1861 | 1.071 | 0.161 | 0.878 | 1.917 | 98 |
| Share of children in school, 1871 | 0.696 | 0.168 | 0.325 | 1.02 | 98 |
| Eng/Wal/Scot. immigrant share, 1861 | 0.066 | 0.076 | 0 | 0.276 | 98 |
| Irish immigrant share, 1861 | 0.071 | 0.072 | 0 | 0.299 | 98 |
| Other immigrants share, 1861 | 0.035 | 0.047 | 0 | 0.272 | 98 |
| Density (persons/acre) 1861 | 0.731 | 3.19 | 0 | 27.379 | 98 |

Table 7: Summary statistics for Canada household-level analysis

| Variable | Mean | Std. Dev. | Min. | Max. | N |
|---|---|---|---|---|---|
| Birth occurred in a year (annual panel, 1874-81) | 0.295 | 0.456 | 0 | 1 | 1,408,311 |
| Number of children, 1877 | 4.047 | 2.294 | 1 | 11 | 176,318 |
| Number of children, 1881 | 4.896 | 2.21 | 2 | 11 | 176,318 |
| Mother's age, 1881 | 35.312 | 6.850 | 19 | 51 | 176,318 |
| Father's age, 1881 | 39.924 | 8.138 | 19 | 76 | 176,318 |
| British Household | 0.623 | 0.485 | 0 | 1 | 176,318 |

### 8.3.2 Additional Canada county-level analysis results

Table 8 presents our main robustness results. We begin by making comparisons where the number of births are inferred in a similar way. For example, in Column 1, we compare results from the 1876-81 period to just the 1865-71, which are similar because both infer fertility based on the number of young children (under 5 or under 6, divided by the number of years included) in the census. Similarly, Column 2 compares just the 1882-1885 period to 1872-1875, where in both fertility is inferred based on children from 6-10 or 7-11. In both cases we see patterns consistent with those shown in our main results.

In Column 3, we present results without weights. Column 4 shows that we still see significant relative effects if we confine our analysis only to the province of Quebec, though we have less power in this specification. In Column 5 we control for local schooling rates.[59] In Column 6, we separate out native-born Canadians of British origin, immigrants from Britain, and other immigrants. We find significant effects for both the British-origin native born and first-generation British immigrants, with stronger effects for those with closer (first-generation) ties to Britain. In Column 7 and 8 we consider two alternative measures of a county's connection to Britain, one based on the share of the population not of French-origin, and a second based on the share of non-Catholics. The non-Catholic measure is also available for counties in New Brunswick and Nova Scotia. Column 9 produces results when we include data from those provinces, but the main conclusion is largely unaffected.

[59]In additional results (not reported), we also find 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.

Table 8: Robustness of Canadian county-level analysis

| | DV is children born per 1000 fertile-aged women | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 1876-1881 against 1866-1871 | 1882-1885 against 1872-1875 | Without Weights | Only within Quebec | Controlling for shr. of children in school | Separating immigrants and native-born | Alternative measures of British Connection | | Including Nova Scotia and New Brunswick |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| British-origin | -12.404** | | -12.745*** | -10.719* | -12.336** | | | | |
| × 1876-81 Period | (4.987) | | (3.854) | (6.033) | (6.167) | | | | |
| British-origin | | -16.136*** | | | | | | | |
| × 1882-85 Period | | (5.946) | | | | | | | |
| First gen. British imm. shr. | | | | | | -28.789** | | | |
| × 1876-81 Period | | | | | | (14.121) | | | |
| Native-born British shr. | | | | | | -13.146** | | | |
| × 1876-81 Period | | | | | | (5.651) | | | |
| Other first gen. imm. shr. | | | | | | 26.723 | | | |
| × 1876-81 Period | | | | | | (27.057) | | | |
| Non-French Share | | | | | | | -14.654*** | | |
| × 1876-81 Period | | | | | | | (2.829) | | |
| Non-Catholic Share | | | | | | | | -16.501*** | -14.447*** |
| × 1876-81 Period | | | | | | | | (3.350) | (3.046) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Period FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 196 | 196 | 294 | 183 | 294 | 294 | 294 | 294 | 390 |
| Counties | 98 | 98 | 98 | 61 | 98 | 98 | 98 | 98 | 130 |
| R-squared | 0.655 | 0.369 | 0.703 | 0.448 | 0.654 | 0.654 | 0.647 | 0.647 | 0.592 |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by county. Observations weighted by 1861 population unless otherwise noted. 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, the male/female ratio in 1871, and the share of fertile-aged women that are under 30.

Table 9 studies the impact of spatial autocorrelation in our analysis of the county-level data for Canada. Recent work by Kelly (2019) has highlighted the potential for spatial autocorrelation to impact standard errors. To examine this issue, we study how our standard errors change as we control for spatial autocorrelation across various distances. We focus on unweighted regressions since it is challenging to implement spatial autocorrelation adjustments in weighted regressions. Note that in our setting, the standard errors would need to increase by 35% for our results to no longer be statistically significant at the 5-percent level.

Table 9 presents results after correcting for spatial autocorrelation across four bands (25, 50, 100, and 200 km) and with both uniform distance weights (Panel A) or weights that incorporate a linear decay function (Panel B).[60] To aid comparison, Column 1 presents baseline result, similar to Column 2 of Table 1, but calculated without weighting the regressions, though a comparison of Column 1 of Table 9 and Column 2 of Table 1 indicates that this has a negligible impact on our estimates. The remaining columns present estimates obtained allowing for spatial autocorrelation across varying distances using two different approaches to spatial decay. Correcting for spatial autocorrelation tends to decrease the size of our standard errors, suggesting that our data are characterized by weak negative spatial autocorrelation. Given this, in our main results we take the more conservative approach by reporting standard errors that are clustered at the county level.

[60] As a point of comparison, the average distance from each county centroid to the centroid of its nearest neighbor is about 30 km.

Table 9: Examining spatial autocorrelation in the Canada county-level analysis

| DV is Children Born per 1000 Fertile-Aged Women | | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **Panel a: Uniform Weights** | | | | | |
| | Baseline | 25 km | 50 km | 100 km | 200 km |
| British-origin shr. × 1876-81 | -12.745*** (4.767) | -12.745*** (3.128) | -12.745*** (3.807) | -12.745*** (4.103) | -12.745*** (4.238) |
| **Panel b: Bartlett Linear Decay** | | | | | |
| | Baseline | 25 km | 50 km | 100 km | 200 km |
| British-origin shr. × 1876-81 | -12.745*** (4.767) | -12.745*** (3.298) | -12.745*** (3.365) | -12.745*** (3.699) | -12.745*** (3.919) |

*** p<0.01, ** p<0.05, * p<0.1. Column 1 corresponds to our preferred specification, Column 2 of Table 1, but without regression weights (see explanation in text). The remaining columns begin with this specification and then implement spatially adjusted standard errors across various distances using either uniform weights (top panel) or Bartlett linear decay (bottom panel). Controls include population density in 1861, population growth in 1861-71, agricultural employment share in 1871, the male/female ratio in 1871, and the share of fertile aged women that are under 30 in 1871, all interacted with period fixed effects.

### 8.3.3 Canada micro-data Robustness

Table 10 presents robustness checks on the Canada household-level analysis. The first column restricts attention to households living in Quebec and Ontario (as in our main county-level results). The second column extends our pre-period back to 1871. The third column includes two additional household-level controls: the number of children present in the household at the beginning of each year, and whether there was a birth in the previous year. Both of these are factors that may influence fertility, but we do not include them in our preferred specification because they are also likely to be affected by treatment. None of these exercises affect our main conclusions.

In column 4 we use a stricter definition of British-origin, requiring that both the mother and the father must be British-origin. This does not have a large effect on our results, which reflect the fact that mixed marriages were not common in our data. In Column 5, we estimate results after dropping Irish households from our sample. We think it is more appropriate to classify Irish households as British since about two-thirds of them are Protestants from Northern Ireland with strong cultural ties to Britain, but this decision is not crucial for our results. In Column 6 we use the

census question on religion to consider an alternative measure of culturally British: whether the household is not-catholic. This proxy delivers nearly identical results.

Finally, in Column 7, we include couples that had no children by 1878. There are good reasons to exclude this group from our main analysis. However, the results show that we still find statistically significant effects when they are included. Moreover, the fact that the estimated coefficient falls indicates that this group was less affected by treatment than families that already had children. This indicates that the main impact of the change in fertility norms may have been in causing families that already had some children to end fertility earlier, though we leave a full examination of the mechanisms of adjustment for future work.

Table 10: Robustness of Canada household-level difference-in-differences analysis

| **DV: Indicator for whether household is observed as having a birth in a given year** | | | | | | | |
|---|---|---|---|---|---|---|---|
| | only Quebec & Ontario | start panel in 1871 | additional fertility controls | strict Brit. HH def | without Irish HHs | alt. culture def. | with childless HHs |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| British HH | -0.0124*** | -0.0109*** | -0.0120*** | -0.0113*** | -0.0102*** | | -0.0059** |
| × Post 1877 | (0.0025) | (0.0021) | (0.0025) | (0.0022) | (0.0025) | | (0.0025) |
| Non-Catholic HH | | | | | | -0.0116*** | |
| × Post 1877 | | | | | | (0.0020) | |
| British HH indicator | Yes | Yes | Yes | Yes | Yes | | Yes |
| Non-Cath. HH indicator | | | | | | Yes | |
| Parental age FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County-by-year FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Occupation-by-year FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1,100,816 | 2,229,144 | 1,404,313 | 1,303,455 | 1,006,351 | 1,408,311 | 1,834,956 |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by county. A household is classified as British-origin if either the mother or father are British-origin. Occupation fixed effects correspond to the following broad categories: legislators, senior officials, and managers; professionals; technicians and associate professionals; clerks; service workers and shop workers; skilled agriculture and fishery workers; crafts and related trade workers; plant and machine operators; elementary occupations; and armed forces. Other fertility controls include number of children in the household, which we fully interact with our post-trial period indicator, as well as an indicator for whether we observe the household as having a birth in the previous year, which we also fully interact with the post-trial period indicator. The strict British household column imposes the restriction that both the mother and the father are of British origin. Childless households are defined as those with zero children born before 1877.

## 8.4 Appendix to the U.S. analysis

Table 11 presents summary statistics for the data used in the U.S. analysis, which come from the 1880 Census. One notable feature here is that the probability of having a child in any particular year, 0.319, which is slightly higher than the probability observed in the Canada sample (0.295). This higher probability may be because the couples observed in the U.S. sample were on average slightly younger than the Canadian couples, with slightly smaller initial family sizes.

Table 11: Summary statistics for U.S. household-level analysis

| Variable | Mean | Std. Dev. | Min. | Max. | N |
|---|---|---|---|---|---|
| Birth occurred in a year (annual panel, 1874-80) | 0.319 | 0.466 | 0 | 1 | 3,647,803 |
| Number of children, 1877 | 3.437 | 1.866 | 1 | 9 | 521,407 |
| Number of children, 1880 | 4.233 | 1.804 | 2 | 9 | 521,407 |
| Mother's age, 1880 | 33.857 | 6.688 | 18 | 50 | 521,407 |
| Father's age, 1880 | 38.716 | 7.766 | 18 | 74 | 521,407 |
| British Household | 0.292 | 0.455 | 0 | 1 | 521,407 |

Table 12 presents additional robustness results for the U.S. analysis. These mirror the robustness check run on the Canadian data. The only exception here is that in the U.S., we exclude the Irish from the British-origin group because such a large fraction of the Irish immigrants to the U.S. were Catholics from outside of Ulster, a group likely to have weak ties to Britain. In Column 3, we examine the impact of instead treating the Irish as part of the British-origin group. These results suggest that our decision to exclude the Irish from the British-origin group in the U.S. analysis is not crucial to our results.

Table 12: Robustness of U.S. household-level difference-in-differences analysis

| | **DV: Indicator for whether household is observed as having a birth in a given year** | | | |
|---|---|---|---|---|
| | additional fertility controls | strict Brit. HH def | with Irish HHs | with childless HHs |
| | (1) | (2) | (3) | (4) |
| British HH × Post 1877 | -0.0252*** | -0.0177*** | -0.0148*** | -0.0098*** |
| | (0.0023) | (0.0024) | (0.0023) | (0.0011) |
| County FEs | Yes | Yes | Yes | Yes |
| British HH indicator | Yes | Yes | Yes | Yes |
| Parental age FEs | Yes | Yes | Yes | Yes |
| State-by-year FEs | Yes | Yes | Yes | Yes |
| Occupation-by-year FEs | Yes | Yes | Yes | Yes |
| Observations | 3,631,843 | 2,976,946 | 5,443,587 | 5,080,973 |

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by state. County-clustered standard errors are about 50% smaller (results available upon request). Occupation fixed effects correspond to the following 5 categories: agriculture; professional and personal services; trade and transport; manufacturing, mechanical, and mining; and not in the labor force. Those definitions are as described in the "Occ" variable on ipums.org. Sample is restricted to European-origin households, which are classified as having either a mother or father that is inferred to be of European origin. We infer origin based on the reported information of where the individual was born or where their parents were born. Other fertility controls include number of children in the household, which we fully interact with our post-trial period indicator, as well as an indicator for whether we observe the household as having a birth in the previous year, which we also fully interact with the post-trial period indicator. Irish households are the only European-origin households omitted from our regressions, except in column 6. The strict British household column imposes the restriction that both the mother and the father are of British origin. Childless households are defined as those with zero children born before 1877.

Table 13 provides a comparison between results obtained in the U.S. and in Canada when focusing on the most comparable group that can be identified in both datasets: first generation immigrants from Britain.[61] Both the results for Canada, in Column 1, and the results for the U.S., in Column 2, follow the approach used in our preferred specification for both of those countries, except for focusing only on first-generation British-origin immigrants as the treated group. Note that there are still important differences in the control groups. In Canada, we compare to the entire Francophone population while in the U.S. we compare to first-generation European-origin immigrants from countries other than Britain or Ireland. Despite difference in the control

[61] Note that we can't compare second-generation immigrants directly because the Canadian census data does not include information on parent's location of birth.

groups, we obtain estimates that are remarkably similar and statistically indistinguishable.

Table 13: Comparing magnitudes in U.S. and Canada analysis

| **DV: Indicator for whether household is observed as having a birth in a given year** | | |
|---|---|---|
| | Canada first-generation immigrants (1) | U.S. first-generation immigrants (2) |
| British HH × Post 1877 | -0.0178*** | -0.0185*** |
| | (0.002596) | (0.002596) |
| US State-by-year FE | | Yes |
| Canadian county-by-year FE | Yes | |
| British HH indicator | Yes | Yes |
| Parental age FEs | Yes | Yes |
| Occupation-by-year FEs | Yes | Yes |
| Observations | 929,490 | 3,235,052 |

*** p<0.01, ** p<0.05, * p<0.1. The estimates in Column 1 follow the approach used in Column 5 of Table 2 except that treatment group includes only first generation British immigrants. The estimates in Column 2 follow the approach used in Column 5 of Table 3 except that the treatment group includes only first generation British immigrants.

## 8.5 Appendix to the South Africa analysis

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 on a single difference taken between the Census of 1875 and the Census of 1891. We focus 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. Data limitations also mean that fertile aged females are defined as those aged 15-54, a group that is slightly different than the ages we used in the Canadian analysis. 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,

$$BR_{dt} = \beta_0 + \beta_1 BRIT_{d\,1875} * POST_t + X_{dt}\lambda + \gamma_d + \phi_t + \epsilon_{dt} \tag{4}$$

where $BR_{dt}$ is the ratio of children aged 0-14 to the fertile-aged (15-54) female population in district $d$ in period $t$, $BRIT_{d\,1875}$ is a measure of the location's British connection in 1875, which we interact with $POST_t$, an indicator for the post-1877 period. $X_{d\,1875}$ is a set of control variables reflecting conditions in each division in 1875, interacted with the post-period indicator, while $\gamma_d$ reflects our district fixed

Table 14: 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 | | | | |

effects and $\phi_t$ captures our period fixed effects. Summary statistics for the variables used in our analysis are presented in Table 14.

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 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%. 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 15. 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. 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 dropping the six locations with populations under 2,000 from the analysis leads to results that are almost identical to those obtained when weighting.

Table 15: Regression results for South Africa analysis

| | DV is Births per 1000 fertile aged women | | | | | | |
|---|---|---|---|---|---|---|---|
| | 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. share | -1.151** | -2.277*** | -1.859* | | -1.721** | -1.191 | -2.117** |
| × 1877-91 | (0.481) | (0.734) | (1.005) | | (0.714) | (0.836) | (0.938) |
| Not-Dutch share | | | | -0.319* | | | |
| × 1877-91 | | | | (0.168) | | | |
| Non-British imm. share | | | -1.183 | | | | |
| × 1877-91 | | | (0.974) | | | | |
| Period Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| District Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Density × Period | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Literacy × Period | | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 64 | 64 | 64 | 64 | 56 | 64 | 52 |
| R-squared | 0.223 | 0.329 | 0.350 | 0.184 | 0.222 | 0.086 | 0.189 |

*** 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.

We may worry about the impact of spatial autocorrelation on our standard errors. In Table 16, we examine the impact of allowing for spatial autocorrelation across various distances on results obtained when running unweighted regressions on locations with populations of at least 2,000. The first column presents baseline results equivalent to Column 7 in Table 15. Next, we present additional results allowing for spatial autocorrelation over various distances, using either uniform or Bartlett weights. We find that standard errors actually fall when we allow for spatial autocorrelation, which suggests that the robust standard errors shown in our main results table are relatively conservative.

Table 16: Robustness to spatial standard error correction

| **DV is Births per 1000 fertile aged women** | | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **Panel a: Uniform Weights** | | | | | |
| | Baseline | 25 km | 50 km | 100 km | 200 km |
| British immigrant shr. | -2.117** | -2.117** | -2.117** | -2.117** | -2.117** |
| × 1877-91 | (0.938) | (0.839) | (0.630) | (0.722) | (0.799) |
| **Panel b: Bartlett Linear Decay** | | | | | |
| | Baseline | 25 km | 50 km | 100 km | 200 km |
| British immigrant shr. | -2.117** | -2.117** | -2.117** | -2.117** | -2.117** |
| × 1877-91 | (0.938) | (0.862) | (0.848) | (0.788) | (0.795) |

*** p<0.01, ** p<0.05, * p<0.1. Column 1 corresponds to our preferred specification, column 7 of Table 15, which includes controls for population density and literacy.

## 8.6 Additional Evidence on the Importance of the Bradlaugh-Besant Trial

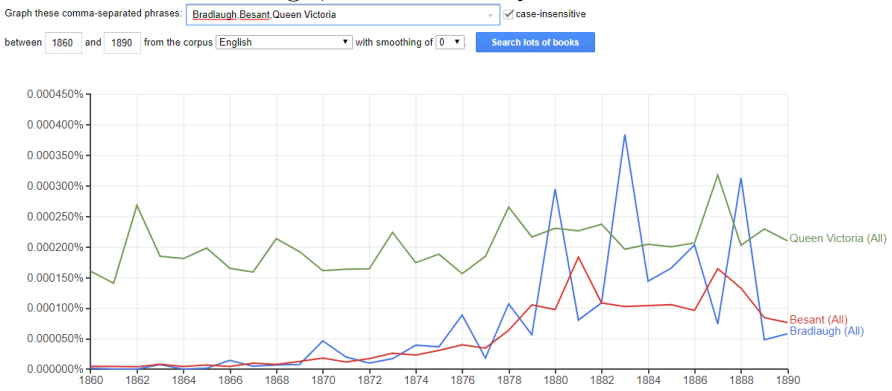
### 8.6.1 Google N-grams plots

Using Google N-grams to search for terms related to the trial provides an additional indicator of the importance of the events we study. For example, Figure 9 shows the appearance of "Bradlaugh" and "Besant" in the corpus of books searched by Google N-grams. As a point of comparison, we also plot the appearance of "Queen Victoria." Because Bradlaugh and Besant are reasonably unique names, this provides an indication of the prominence of these individuals in the years around the trial. We can see a clear increase in the appearance of these terms after 1877 with sustained high levels into the 1880.

Figure 10 provides results from searches of two of the phrases most commonly used in the discussion of family planning during this period, "population question" and "conjugal prudence." For both terms we see a sharp increase right around the timing of the trial. Finally, Figure 11 shows that we see a similar increase in the appearance of "Fruits of Philosophy" starting in 1878 and lasting through the 1880s.

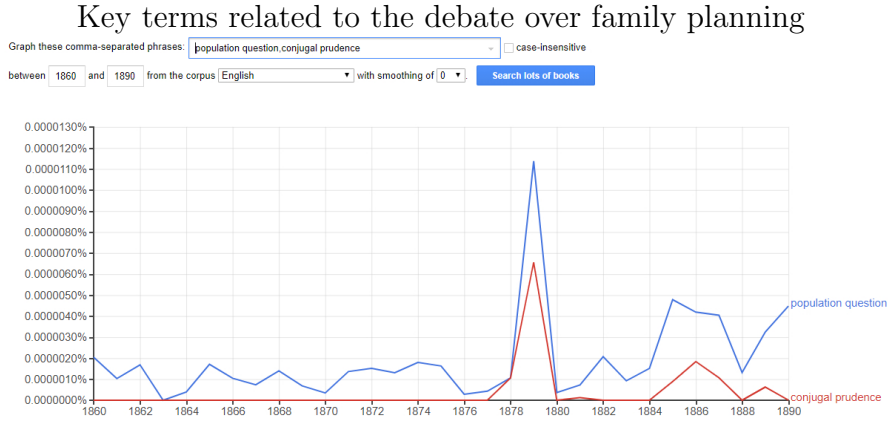
Figure 9: Google N-grams results: Key names

Bradlaugh, Besant and Queen Victoria




Results obtained from Google N-gram Viewer (https://books.google.com/ngrams) accessed on June 3, 2019.

Figure 10: Google N-grams results: Family planning phrases




Results obtained from Google N-gram Viewer (https://books.google.com/ngrams) accessed on June 3, 2019.

Figure 11: Google N-grams results: Fruits of Philosophy


Results obtained from Google N-gram Viewer (https://books.google.com/ngrams) accessed on June 3, 2019.

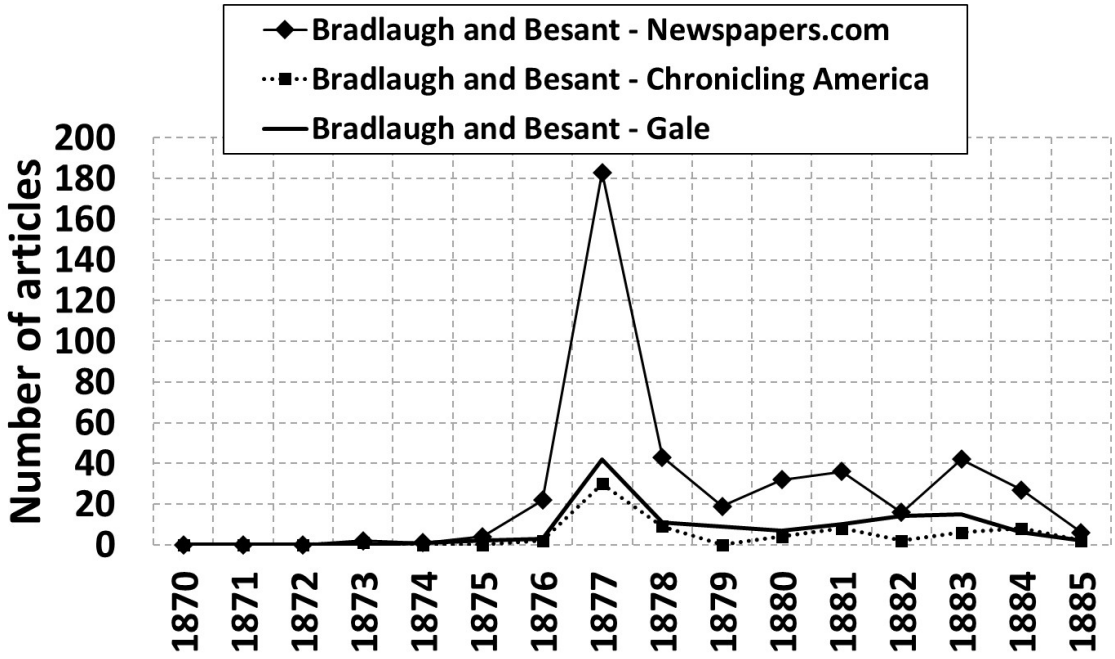
### 8.6.2 U.S. Newspaper data

This appendix presents some additional details on how the trial was covered in U.S. newspapers. We consider three U.S. newspaper databases: Newspapers.com; the Chronicling America database provided by the Library of Congress; and the Gale Nineteenth Century U.S. Newspaper Database. Of these, the Newspapers.com

database is substantially richer. As an example of this, searching for a generic word, "Monday," in 1877, results in 235,074 hits in the Newspapers.com database, 48,794 hits in the Chronicling America database, and 25,987 hits in the Gale database. All of these are substantially below the number of hits we get in the much richer British Newspaper Archive. This may be due either to a lack of coverage or because poorer scan quality results in fewer identifiable words. Note that these values come from August 24, 2020 and they will change over time, as new articles are constantly being added to these databases.

Figure 12 shows the number of hits in each of these three databases for articles mentioning Bradlaugh and Besant. All three sources of articles show a spike in articles in 1877, corresponding exactly to the timing of the trial.

Figure 12: Articles Mentioning Bradlaugh and Besant by Newspaper Database




Data obtained on August 20, 2020.

## 8.7 Appendix to the England and Wales analysis

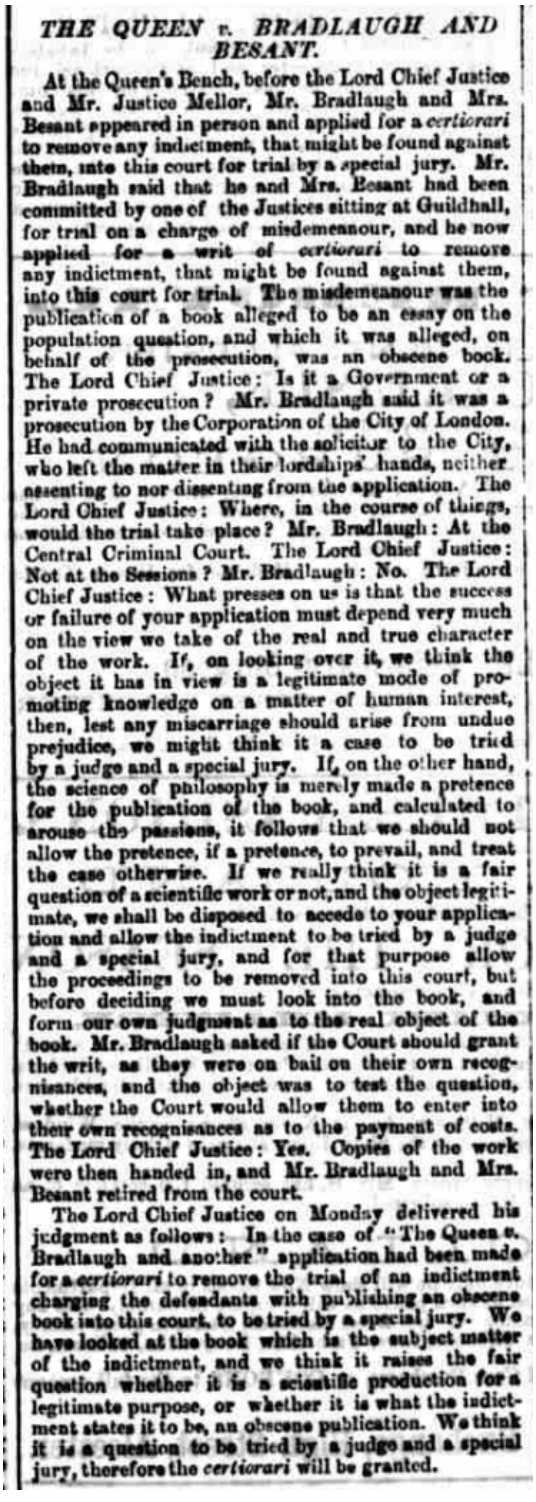
### 8.7.1 England and Wales newspaper data

The newspaper data used in our analysis of England and Wales comes from the British Newspaper Archive (www.britishnewspaperarchive.co.uk), a joint effort by the British Library and findmypast to digitize millions of newspapers from the British Library's extensive collection. This database contains many papers from throughout the country, though not every paper is included. Some very small papers may be missing from the British Library's collection, while a few national papers, such as the *Times* are not included, presumably because they are available through alternative digital libraries. Each article was digitized using a high-quality scanner. The text was then identified using optical character recognition. Figure 13 presents a typical example article from the Alcester Chronicle (May 5, 1877), which is the first article that appears in our search when newspapers are sorted alphabetically.

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. This leverages the fact that both names are fairly unique. As shown in Figure 3, simple plots of mentions by month and year match key moments of the trial, increasing our confidence that this query is in fact picking up exposure to the trial. Figure 14 plots the spatial variation of our newspaper exposure variable.

Because our analysis relies on the spatial variation of newspaper coverage of the trial, it is useful to consider the factors that may generate this variation. One source of spatial variation in coverage of the trial is underlying variation in newspapers in general. A second source of spatial variation in trial coverage is that some newspapers may have been more willing to publish articles about the trial than others. The analysis presented in the main text attempts to address both of these issues.

Figure 13: Example article



**THE QUEEN v. BRADLAUGH AND BESANT.**

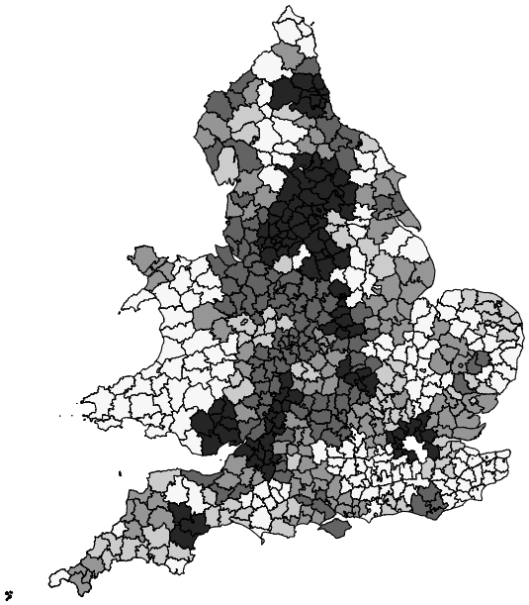
At the Queen's Bench, before the Lord Chief Justice and Mr. Justice Mellor, Mr. Bradlaugh and Mrs. Besant appeared in person and applied for a *certiorari* to remove any indictment, that might be found against them, into this court for trial by a special jury. Mr. Bradlaugh said that he and Mrs. Besant had been committed by one of the Justices sitting at Guildhall, for trial on a charge of misdemeanour, and he now applied for a writ of *certiorari* to remove any indictment, that might be found against them, into this court for trial. The misdemeanour was the publication of a book alleged to be an essay on the population question, and which it was alleged, on behalf of the prosecution, was an obscene book. The Lord Chief Justice: Is it a Government or a private prosecution? Mr. Bradlaugh said it was a prosecution by the Corporation of the City of London. He had communicated with the solicitor to the City, who left the matter in their lordships' hands, neither assenting to nor dissenting from the application. The Lord Chief Justice: Where, in the course of things, would the trial take place? Mr. Bradlaugh: At the Central Criminal Court. The Lord Chief Justice: Not at the Sessions? Mr. Bradlaugh: No. The Lord Chief Justice: What presses on us is that the success or failure of your application must depend very much on the view we take of the real and true character of the work. If, on looking over it, we think the object it has in view is a legitimate mode of promoting knowledge on a matter of human interest, then, lest any miscarriage should arise from undue prejudice, we might think it a case to be tried by a judge and a special jury. If, on the other hand, the science of philosophy is merely made a pretence for the publication of the book, and calculated to arouse the passions, it follows that we should not allow the pretence, if a pretence, to prevail, and treat the case otherwise. If we really think it is a fair question of a scientific work or not, and the object legitimate, we shall be disposed to accede to your application and allow the indictment to be tried by a judge and a special jury, and for that purpose allow the proceedings to be removed into this court, but before deciding we must look into the book, and form our own judgment as to the real object of the book. Mr. Bradlaugh asked if the Court should grant the writ, as they were on bail on their own recognisances, and the object was to test the question, whether the Court would allow them to enter into their own recognisances as to the payment of costs. The Lord Chief Justice: Yes. Copies of the work were then handed in, and Mr. Bradlaugh and Mrs. Besant retired from the court.

The Lord Chief Justice on Monday delivered his judgment as follows: In the case of "The Queen v. Bradlaugh and another" application had been made for a *certiorari* to remove the trial of an indictment charging the defendants with publishing an obscene book into this court, to be tried by a special jury. We have looked at the book which is the subject matter of the indictment, and we think it raises the fair question whether it is a scientific production for a legitimate purpose, or whether it is what the indictment states it to be, an obscene publication. We think it is a question to be tried by a judge and a special jury, therefore the *certiorari* will be granted.

*The Alcester Chronicle*, May 5, 1877.

Two other sources of variation in our data are related to measurement error. Our measure of the spatial variation in exposure to the trial will be affected by which newspapers are covered by the British Newspaper Archive. In addition, we may also miss some articles in newspapers that were included in the database, simply because optical character recognition failed to properly read the article text. Given the way the newspapers were printed, we find that the digitized text included in the British Newspaper Archive contains a large number of errors, which may cause us to miss some articles. However, most articles about the trial contain multiple mentions of Bradlaugh and Besant, giving the program several chances to digitize one of them correctly.

Figure 14: Spatial distribution of newspaper exposure



This figure maps our treatment variable of interest: number of articles published within 25 km of each district.

### 8.7.2 Article content

We have also examined the scope and content of the British newspaper articles about the trial that we have identified. As a first step, we manually reviewed each of the articles identified in our search and classified them based on content. Naturally these are somewhat rough classifications, but they can provide a useful idea of the types of articles found in our measure of trial exposure. Table 17 presents the breakdown of articles.

Roughly three quarters of the articles captured in our search were direct reporting on the trial, the first three categories shown in Table 17. We have broken these reports down into three types: regular articles, which range from a paragraph to almost a full vertical column of text; short snippets, which are typically just a couple of sentence updates about the trial; and longer articles, covering more than one full vertical column (unlike today, at this time papers published articles in columns that extended across the full height of the paper). While these length distinctions are arbitrary and not precise, they convey some idea of the extent of coverage of the trial. These reports focus mainly on the factual events of the trial, though some of them also include commentary or opinions.

About 6% of the articles mentioning Bradlaugh or Besant in 1877 discuss meetings, lectures or events associated with the Malthusian League that took place outside of the context of the trial. These include a number of public meetings that took place during the trial where either Bradlaugh or Besant spoke.

We identified 67 articles about the trial but with a focus on opinion and commentary, rather than more direct reporting of factual events. These articles span a wide range of views and many of them are particularly interesting.

A number of articles were published about a controversy related to the trial that involved the post office. For example, the Bradford Telegraph (May 18, 1877) reported that, "The Secretary of the General Post Office intimated that he claims the right to open, read and confiscate, without giving Mr. Bradlaugh any intimation, any work posted by him..." This decision, which was condemned by a number of papers, created quite a bit of controversy, including in the House of Commons.

We find 15 articles mainly related to sales of the *Fruits of Philosophy*. These come in two main varieties. One set focuses on the large number of manuscripts

that were sold as a result of the publicity generated by the trial. Another set of articles discusses court cases involving sellers other than Bradlaugh and Besant. In addition, a few articles discuss the sales of copies of other pamphlets that had been given fake *Fruits of Philosophy* covers, apparently because street hawkers had other pamphlets lying around that they wanted to get rid of and this was a way to get rid of them while turning a profit. Another topic of interest was Annie Besant's colorful biography, including the fact that she was previously married to a minister and had some famous relations. Several of these appear in papers from around Cheltenham, where her husband had worked.

Bradlaugh and Besant were also mentioned in connection with another controversial book, *Priest in Absolution*, that appeared around the time of the trial. This book was published by a group of Anglican clergy and provided instructions that included asking intimate questions of women during confession. Bradlaugh and Besant are typically mentioned as a point of comparison. For example, a letter by Sir Harry Verney published in the Bucks Herald, Uxbridge Advertiser, Windsor and Eton Journal, states that, "If Mr. Bradlaugh and Mrs. Besant are to be imprisoned for publishing obscene books on physical subjects, the authors of this book [the Priest in Absolution] ought to be doubly punished for making the Church the vehicle for suggestions leading to gross licentiousness."

A somewhat odd set of eight articles have to do with an appeal by one Dr. Kenealy, an MP from Stoke, for public subscriptions for an election fund. These articles uniformly mention how little funding (£20) Kenealy's appeal achieved in comparison to the £1,200 raised by Bradlaugh and Besant in a few weeks for their trial defense fund. At the end of the year, papers at this time commonly ran reviews of important events. A number of these mentioned the Bradlaugh-Besant trial.

Bradlaugh and Besant were also mentioned in relation to a debate over the Burial Bill, which dealt with whether religious "Dissenters" could be buried in parish graveyards. Articles against the Burial Bill typically mention Bradlaugh and Besant to raise the specter that the bill opens the door to atheists such as them orating in churchyards. A small number of articles mentioned Bradlaugh in connection with the prosecution of Edward Truelove, another secularist also arrested for publishing the *Fruits of Philosophy*. Bradlaugh was active in helping with his defense.

Four of the articles were published in Welsh. Two articles, both from Northamp-

Table 17: Types of articles

| Classification | No. articles | Share |
|---|---|---|
| Reporting on the trial – regular articles | 479 | 42.1% |
| Reporting on the trial – short snippets | 317 | 27.9% |
| Reporting on the trial – long articles | 73 | 6.4% |
| Meetings, lectures, Malthusian League | 76 | 6.7% |
| Opinion and commentary | 67 | 5.9% |
| Post Office controversy | 26 | 2.3% |
| Related to books/pamphlet sales | 15 | 1.3% |
| Besant biographical | 15 | 1.3% |
| Priests of Absolution controversy | 12 | 1.1% |
| Related to Dr. Kenealy's public plea | 8 | 0.7% |
| Related to the Burial Bill controversy | 7 | 0.6% |
| Reviews of important events in the year | 7 | 0.6% |
| Truelove prosecution | 5 | 0.4% |
| Articles in Welsh | 4 | 0.4% |
| Petition to the House of Commons | 3 | 0.3% |
| Bradlaugh's candidacy for parliament | 2 | 0.2% |
| Miscellaneous | 20 | 1.8% |

ton, mention Bradlaugh's effort to be elected as an MP for that area, which he eventually achieved in 1880. Two other articles are related to a petition in the House of Commons related to the trial in support of Bradlaugh and Besant. The remaining articles cover a wide range of miscellaneous topics, ranging from a poem about Bradlaugh and Besant to a discussion of a visit by an American Apostle of Free Love to London and even in a speech at a meeting of the North Myton Conservative Association (to "loud laughter and applause", Hull Packet and East Riding Times, Aug. 17, 1877). As another example, the pair were mentioned in connection with a debate over a proposal (rejected by the House of Commons by 229 to 87) to allow museums and galleries to open on Sundays (Huddersfield Daily Chronicle, June 11, 1877).

To gain a deeper understanding of the content of the articles, we transcribed the full content for a sample of 483 articles (about 40% of the sample).[62] The sampled articles were chosen semi-randomly by sorting the articles in alphabetical order based on place of publication and then digitizing the first 40% of articles that appear.

[62]Transcription was necessary because the fully automated approach employed by the British Newspaper Archive is sufficient for identifying the existence of strings of characters on a page, it often does a poor job of preserving the formatting of the article. This means that information from articles appearing before or after the relevant article are often assigned as part of the relevant article, and vice versa. Manual transcription is expensive, so our sample size was driven by budget concerns.

These transcribed data allow us to provide a more quantitative picture of the scope and content of the newspaper articles written about the trial. A good starting point is to look at the size of articles. The average article length in our sample is 497 words, with the shortest article being just 15 words (a short update of when sentencing was scheduled to occur) and the longest article being 5160 words. In total, the 483 articles covering the trial that we sampled contain just over 240,000 words.

Next, we attempt to parse out common themes appearing in the articles. To do so, we begin by taking the raw text and removing any punctuation or common words.[63] After these standard cleaning steps we then identified the frequency of every individual word stem, as well as the frequency of every two-word pair. Table 18 describes the 80 most common word stems appearing in the articles (after dropping common words) and the 80 most common consecutive two-word combinations. Looking over these data, one can discern some of the key themes that appear in the articles. Beyond the expected descriptive terms, such as "bradlaugh", "court", or "chief justice", one of the striking features is the number of terms dealing with issues of morality. Among the two-word combinations, we see that "obscene book" and "public morals" appear frequently, as does "calculated deprave" and "defendants corrupt." The frequency of terms like these, and the associated word roots, indicate the centrality of the debate over the morality of limiting family size in the trial. Other terms, like "population question" and "checks population" reflect the broader debate over population control.

Conspicuously absent from this list is any word related to the technical aspects of contraception. None of the articles we transcribed included terms such as 'withdrawal" or "douching", two of the main methods advocated in the pamphlet. This tells us that newspapers were not directly transmitting contraceptive information.[64] Instead, the bulk of the trial content released by newspapers related to the arguments surrounding the morality of using scientific checks to address the population question. Since this use of scientific checks was still taboo at the time, this is entirely consistent with our interpretation that the primary role of newspapers was to help

[63]For instance, the string "Mrs. Besant continued her defence, dealing with the checks on population. One of the first of these checks was infanticide but instead of that the poor wished for a scientific check, which would give them happiness and comfort in their homes." becomes "mrs besant continued defence dealing checks population one first checks infanticide instead poor wished scientific check would give happiness comfort homes."

[64]The articles did, however, generate publicity for "The Fruits of Philosophy", which may have further diffused knowledge of existing contraceptive technology by signaling where couples could obtain the relevant information.

open up conversations about the topic of family planning.

Table 18: Most frequent word stems and two-word pairs in the transcribed articles

| One-word stems | | | | | | Two-word grams | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Rank | Word | Freq. | Rank | Word | Freq. | Rank | Word | Freq. | Rank | Word | Freq. |
| 1 | mr | 2554 | 41 | intent | 314 | 1 | mr bradlaugh | 1520 | 41 | straight mr | 74 |
| 2 | bradlaugh | 1940 | 42 | time | 313 | 2 | mrs besant | 1000 | 42 | said would | 69 |
| 3 | book | 1694 | 43 | proceed | 309 | 3 | lord chief | 728 | 43 | case queen | 68 |
| 4 | would | 1445 | 44 | state | 306 | 4 | chief justice | 719 | 44 | entitled fruits | 65 |
| 5 | besant | 1282 | 45 | object | 305 | 5 | bradlaugh mrs | 461 | 45 | learned counsel | 64 |
| 6 | mrs | 1194 | 46 | say | 301 | 6 | queens bench | 262 | 46 | mr alderman | 63 |
| 7 | said | 1136 | 47 | medic | 294 | 7 | fruits philosophy | 234 | 47 | book calculated | 61 |
| 8 | work | 1068 | 48 | present | 292 | 8 | bradlaugh said | 227 | 48 | publication book | 60 |
| 9 | justic | 989 | 49 | circul | 287 | 9 | obscene book | 203 | 49 | queen bradlaugh | 57 |
| 10 | court | 975 | 50 | found | 286 | 10 | public morals | 199 | 50 | verdict jury | 57 |
| 11 | publish | 954 | 51 | person | 282 | 11 | charles bradlaugh | 145 | 51 | publishing book | 56 |
| 12 | case | 943 | 52 | made | 271 | 12 | annie besant | 145 | 52 | thomas dakin | 55 |
| 13 | juri | 917 | 53 | matter | 271 | 13 | mr charles | 137 | 53 | defendants corrupt | 55 |
| 14 | lord | 897 | 54 | year | 271 | 14 | verdict guilty | 134 | 54 | 24th march | 55 |
| 15 | defend | 884 | 55 | whether | 271 | 15 | bench division | 132 | 55 | jury found | 55 |
| 16 | public | 859 | 56 | philosophi | 269 | 16 | mr straight | 123 | 56 | mr bradlaughs | 54 |
| 17 | chief | 792 | 57 | point | 268 | 17 | quash indictment | 115 | 57 | court would | 53 |
| 18 | obscen | 782 | 58 | guilti | 267 | 18 | prosecution mr | 112 | 58 | douglas straight | 51 |
| 19 | prosecut | 766 | 59 | fruit | 267 | 19 | publishing obscene | 109 | 59 | essay population | 51 |
| 20 | question | 595 | 60 | address | 260 | 20 | solicitor general | 109 | 60 | mr douglas | 51 |
| 21 | indict | 586 | 61 | good | 253 | 21 | mrs annie | 108 | 61 | besant said | 50 |
| 22 | pamphlet | 577 | 62 | show | 250 | 22 | new trial | 108 | 62 | question whether | 50 |
| 23 | could | 570 | 63 | defenc | 248 | 23 | justice said | 107 | 63 | two years | 49 |
| 24 | law | 518 | 64 | peopl | 246 | 24 | mr justice | 105 | 64 | said case | 48 |
| 25 | one | 511 | 65 | general | 245 | 25 | special jury | 103 | 65 | years ago | 48 |
| 26 | verdict | 511 | 66 | two | 243 | 26 | justice mellor | 102 | 66 | sir thomas | 48 |
| 27 | trial | 483 | 67 | subject | 242 | 27 | alderman figgins | 97 | 67 | court justice | 47 |
| 28 | might | 477 | 68 | ask | 241 | 28 | trial mr | 96 | 68 | book published | 45 |
| 29 | popul | 444 | 69 | hand | 241 | 29 | population question | 88 | 69 | obscene libel | 45 |
| 30 | upon | 433 | 70 | dr | 238 | 30 | besant mr | 86 | 70 | moved quash | 44 |
| 31 | moral | 429 | 71 | appear | 235 | 31 | book obscene | 86 | 71 | book entitled | 44 |
| 32 | check | 415 | 72 | read | 235 | 32 | mr mead | 85 | 72 | jury would | 44 |
| 33 | queen | 384 | 73 | evid | 235 | 33 | deprave public | 84 | 73 | solicitorgeneral said | 43 |
| 34 | charg | 374 | 74 | word | 234 | 34 | bradlaugh besant | 81 | 74 | case adjourned | 43 |
| 35 | solicitorgener | 353 | 75 | refer | 232 | 35 | court queens | 81 | 75 | medical works | 43 |
| 36 | must | 343 | 76 | corrupt | 232 | 36 | unreadable text | 81 | 76 | besant publishing | 43 |
| 37 | put | 331 | 77 | man | 229 | 37 | checks population | 78 | 77 | appeared prosecution | 43 |
| 38 | bench | 325 | 78 | right | 228 | 38 | calculated deprave | 77 | 78 | court rose | 42 |
| 39 | judgment | 324 | 79 | use | 228 | 39 | central criminal | 75 | 79 | court error | 42 |
| 40 | call | 320 | 80 | charl | 225 | 40 | criminal court | 74 | 80 | solicitorgeneral mr | 42 |

The first two columns present the 80 most frequent word stems found in the 483 transcribed articles. The last two columns present the 80 most frequent two-word combinations, after common connector words have been removed.

### 8.7.3 Other data for England and Wales

In addition to the newspaper exposure variable, we also assemble a wealth of other district-level controls for use in our analysis of England and Wales. The first source is the reports of the Registrar general which cover births, deaths, and marriages. The birth series is discussed in the main text. 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 use, total mortality, under-5 mortality, and mortality among 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 average within each five-year period.

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.[65] Summary statistics for the key analysis and control variables at the district level are presented in Table 19.

[65]Detailed occupations are not reported at the district level after 1861. 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.

Table 19: Summary statistics for the England and Wales data

| Variable | Mean | Std. Dev. | Min. | Max. | N |
|---|---|---|---|---|---|
| Birth rate panel data (5yr averages) | | | | | |
| Birth Rates, 1868-1883 | 135.88 | 20.441 | 78.499 | 234.951 | 1720 |
| Birth Rates, 1878 | 135.935 | 19.041 | 81.846 | 201.066 | 430 |
| Control Variables | | | | | |
| Marriage Rate, 1873-1877 | 29.899 | 5.691 | 14 | 58.562 | 430 |
| Shr. marriages at Registrar, 1873-1877 | 0.098 | 0.108 | 0 | 0.535 | 430 |
| Shr. marriages Catholic church, 1873-1877 | 0.019 | 0.035 | 0 | 0.243 | 430 |
| Minor shr. of marrying parties, 1873-1877 | 0.14 | 0.044 | 0.031 | 0.314 | 430 |
| Illiterate shr. of marrying parties, 1873-1877 | 0.195 | 0.073 | 0.033 | 0.527 | 430 |
| First marriage shr. 1873-1877 | 0.830 | 0.025 | 0.757 | 0.934 | 430 |
| Population density, 1871 | 1.721 | 7.652 | 0.03 | 107.392 | 430 |
| Average mortality rate, 1871-1880 | 0.019 | 0.003 | 0.014 | 0.034 | 430 |
| Average under 5 mortality rate, 1871-1880 | 0.049 | 0.014 | 0.025 | 0.119 | 430 |
| Illegitimate birth share, 1873-1877 | 0.06 | 0.021 | 0.018 | 0.187 | 430 |
| Manufacturing employment share, 1861 | 0.201 | 0.099 | 0.078 | 0.585 | 430 |
| Agriculture employment share, 1861 | 0.243 | 0.12 | 0.004 | 0.62 | 430 |
| Number of "Monday" articles, 1877 | 4649.695 | 7254.574 | 0 | 44619 | 430 |

### 8.7.4 Additional Trial Results

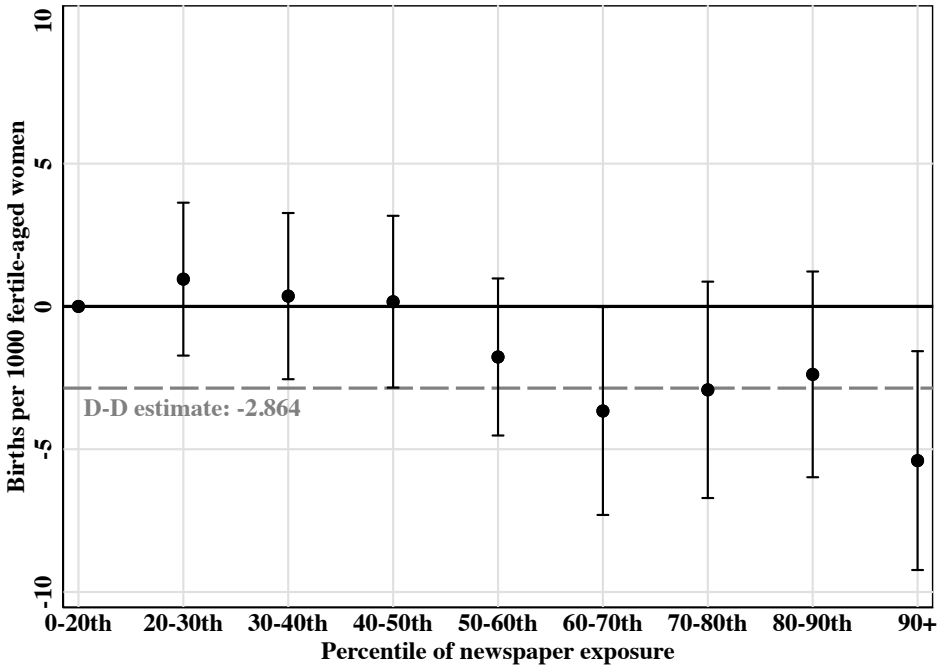
Our main regressions define treatment based on whether the district had exposure to more than the median number of articles. One reason to prefer a discrete measure is that it does not impose a linear functional form on the relationship between the number of articles a location is exposed to and the change in fertility. Such a linear relationship may not make sense if, say, the impact of going from zero to one to two articles is not similar to the impact of going from 25 to 26 to 27. However, it is interesting to consider what the relationship between the number of articles people in a location are exposed to and the change in fertility actually looks like.

In Figure 15, we take a less parametric approach to assessing this relationship. In particular, we bin districts based on their exposure to various number of articles within the overall distribution of exposure and then estimate separate effects for each exposure bin. While this reduces our ability to make precise statements about the effect of exposure to any particular level of articles, it does provide a way for getting a sense of how the number of articles a location is exposed to relates to the fertility change observed in that location.

In these regressions, our omitted category is the set of districts where 0 articles on the trial were published within 25 km. This corresponds to 22 percent of our districts. From there, we break the articles into deciles, although given the discrete nature of our treatment variable those deciles are not a perfect mapping. Districts with 1 article represent the 22 to 30th percentiles; districts with 2 or 3 articles represent the 30 to 41th percentiles; districts with 4-6 articles represent the 41-52th percentiles, districts with 7-9 articles represent the 52-61th percentiles; districts with 10-15 articles represent the 61-71th percentiles, districts with 16-26 articles represent the 71-80; districts with 27-41 articles represent the 80-90th percentiles, and districts 42 or more articles represent the 90-100th percentiles. Figure 15 presents the coefficients and confidence intervals estimated for each of these rough groupings of article exposure.

The results in Figure 15 indicate that locations that were exposed to more articles generally experienced larger declines in fertility, though the estimates for any particular bin are imprecisely estimated. The overall pattern also suggests that the simple high-low distinction used in our main analysis is likely to be a reasonable way of summarizing the effect of exposure.

Figure 15: Impact of News Exposure on Birth Rates, 1878-1883




Coefficients obtained from a version of our preferred specification (Column 5 of Table 4), which includes district fixed effects, period region-by-period fixed effects, marriage controls, other district controls, and newspaper controls. See notes in Table 4 for more detail. Because of the discrete nature of our treatment variable, the percentiles are approximations. See text for more.

In Table 4 we present additional results aimed at assessing the potential impact of mean reversion in fertility rates on our estimates. As a starting point, Column 1 reproduces the results from our preferred specification, Column 5 of Table 4. In Column 2, we then add a control for the change in the birth rate across the two pre-periods just before 1877 (so the difference between 1868-72 and 1873-77). The results in Column 2 show that controlling for the change in fertility in each district in the pre-period has no meaningful impact on our estimate of the effect of newspaper exposure. We do find evidence of mean reversion, indicated by the negative and statistically significant coefficient on our new control. However, the fact that including this control does not substantially affect our main coefficient estimate indicates that our newspaper exposure measure is not systematically related to the change in district level fertility in the pre-period. Thus, it is clear that mean reversion in fertility is not

driving our results.

Table 20: Assessing the role of mean reversion

| **DV is Births per 1000 fertile-aged women** | | |
|---|---|---|
| | (1) | (2) |
| High News Exposure × | -2.864** | -2.609** |
| 1878-1882 Period | (1.110) | (1.068) |
| Chg. Pre-period Birth Rate (1868-72 to 1873-77) × | | -0.386*** |
| 1878-1882 Period | | (0.065) |
| District fixed effects | Yes | Yes |
| Region-by-period fixed effects | Yes | Yes |
| Marriage controls | Yes | Yes |
| Other district controls | Yes | Yes |
| Newspaper controls | Yes | Yes |
| No. districts | 430 | 430 |
| Observations | 860 | 860 |
| R-squared | 0.608 | 0.672 |

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the district level, in parentheses. All regressions weighted by 1861 district population. Birth rates are defined as number of births per 1000 fertile aged women (ages 15-50). These birth rates are averaged over a five year windows (e.g., 1873-1877, the pre-trial period, and 1878-1882, the post-trial period). "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. See notes in Table 4 for definitions of marriage controls, other district controls, and newspaper controls.