Culture and the Historical Fertility Transition *

Brian Beach
Vanderbilt University
and NBER

W. Walker Hanlon
Northwestern University
NBER and CEPR

September 16, 2021

Abstract

The historical transition to a low fertility regime was central for long-run growth, but what caused it? Existing economic explanations largely focus on the economic incentives to limit fertility. This paper presents new evidence highlighting the importance of cultural forces as a complementary 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.

*Beach: Department of Economics, Vanderbilt University, VU Station B, Box 351819, 2310 Vanderbilt Pl, Nashville, TN 37235. brian.beach@vanderbilt.edu Hanlon: Northwestern University, 2211 Campus Drive, Evanston, IL 60208-2600. whanlon@northwestern.edu. We are particularly grateful to Nico Voigtlander for his contribution to the early stages of this project. We also thank Francesco Cinnirella, Bill Collins, Martin Dribe, Casper Worm Hansen, Stephan Heblich, Jane Humphries, Kevin O'Rourke, Peter Sandholt Jensen, Krishna Pendakur, Ethan Schmick, Marianne Wanamaker, Ariell Zimran, and seminar participants at Chapman, Columbia, Copenhagen, the 2019 annual meeting of the Economic History Association, George Mason, Harvard Business School, Lund, Michigan's H2D2 Workshop, LSE, the 2020 NBER Summer Institute (DAE), NYU, Oxford, U. of Pittsburgh, and Vanderbilt for helpful comments. This project was funded by a seed grant from the California Center for Population Research and NSF Career Grant No. 1552692. This paper subsumes NBER Working Paper W25752 "Censorship, family planning, and the historical fertility transition."

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.

Understanding the causes of this turning point has been the focus for a large literature in economics and demography. This interest is motivated both by the importance of the fertility transition within economic history, as well as the central role that this transition plays in models of long-run economic growth (Galor & Weil, 1999; Galor, 2011). In the 1970s, work by the European Fertility Project led to Ansley Coale's "Ready, Willing, and Able" model of fertility transition.[2] This framework argues that fertility reductions occur once three conditions are satisfied: fertility control must be "within the calculus of conscious choice" ("readiness"), costs and benefits must work to provide an incentive for couples to reduce fertility ("willingness"), and families must have the knowledge and means to reduce fertility ("ability"). Since that time, most work by economists and economic historians has focused on 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.[3] Others have focused on the role of female education and labor force opportunities.[4]

Recently, a growing economics literature has highlighted the influence of culture

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

[2] 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).

[3] 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).

[4] 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.

and social norms on economic decisions, including fertility choices (Munshi & Myaux, 2006; Fernandez & Fogli, 2009; Bassi & Rasul, 2017). A recent cross-country analysis by Spolaore & Wacziarg (Forthcoming), for example, finds evidence that fertility reductions in Europe diffused based on a country's cultural proximity to France.[5] This work has shifted attention toward the influence of culture during the historical fertility transition.

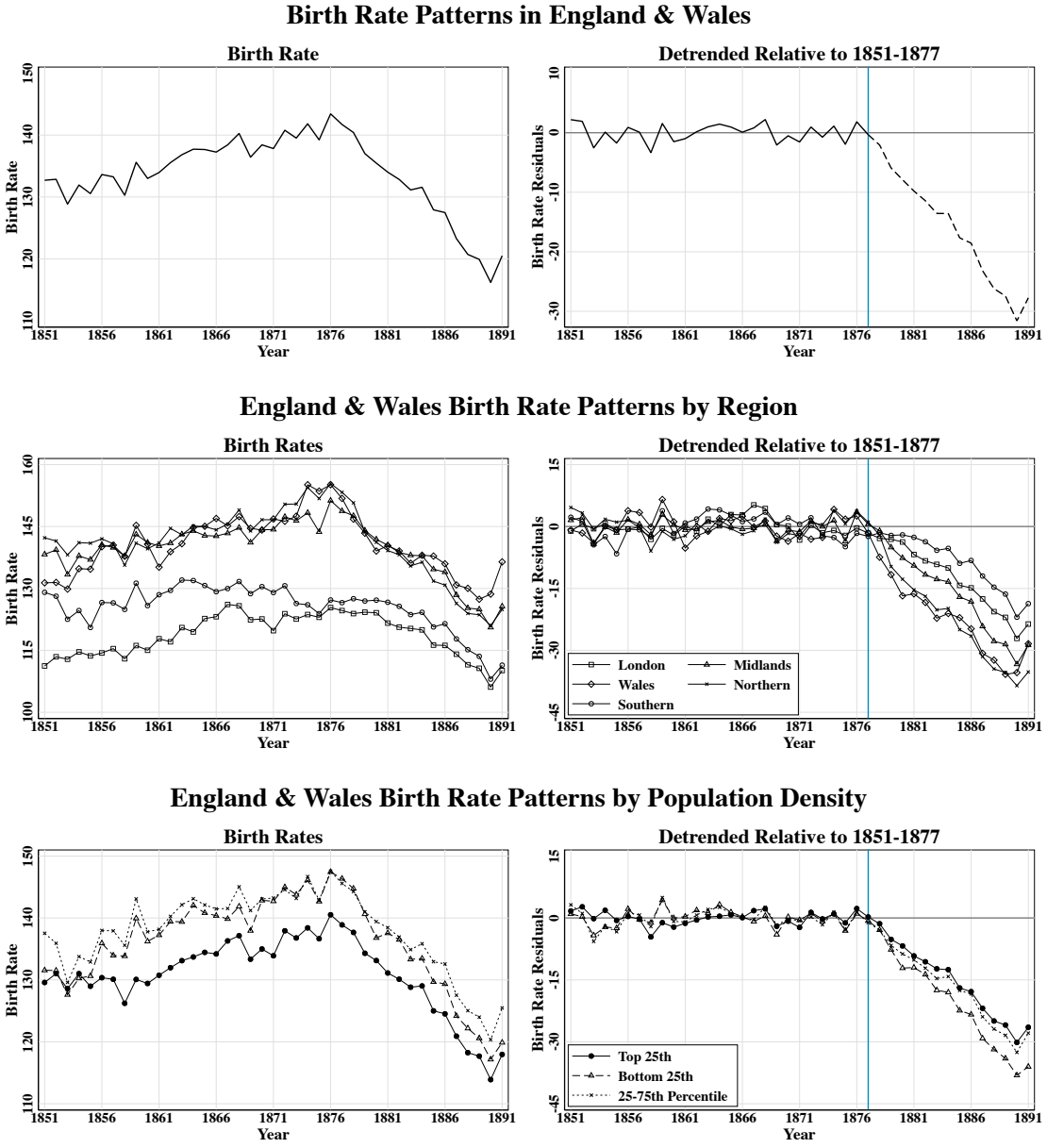
Despite the enormous interest in the historical fertility transition, key features of this transition remained unexplained. One of these features, shown in Figure 1, is the starting point for our analysis. This figure describes the birth rate in England and Wales from 1851 to 1891.[6] The key feature in this graph is the sharp reduction in fertility that began in 1877. 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 widespread nature of this change are features that are difficult to explain using either standard models based on economic factors, such as a rising return to education, or the type of slow cultural diffusion emphasized in recent work such as Spolaore & Wacziarg (Forthcoming).

This paper offers an explanation for the sharp fertility reduction described in Figure 1 while offering new insight into the way that culture can influence fertility decisions. Our analysis comes in two parts. First, we isolate the role of culture by showing that culturally-British populations around the world experienced a sharp synchronized fertility decline starting in 1877, relative to other culturally-different populations living in the same locations and experiencing the same economic and policy environments. In Canada, for example, we compare the fertility of Anglophone Canadians to Francophone Canadians in the same location before and after 1877. In the United States, we compare second-generation British immigrants to other European immigrants. In South Africa, we compare the British-origin population to the Dutch-origin Afrikaners. Comparing fertility shifts occurring among these

[5] A related paper, Blanc & Wacziarg (2020), examines 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. Also in France, Blanc (2021) provides evidence that religiosity played an important role in France's unusually early fertility transition.

[6] For a longer time-series, see Appendix 5.1.

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. In the bottom two figures, districts within England & Wales are divided into three groups depending on where they fall within the overall population density distribution. The "Top 25th" quartile corresponds to districts with an 1851 population density density of 0.59 persons per acre or higher. The bottom 25th quartile refers to districts with a density of 0.23 persons per acre or lower. These charts 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 5.1.

culturally distinct groups sharing the same economic and policy environments allows us to isolate the influence of culture in a way that differs from previous work.[7]

This part of the paper contributes to existing work emphasizing the importance of cultural influences on economic decisions. Our findings show that cultural ties acted as a mechanism that rapidly transmitted changes in attitudes about fertility to culturally-linked populations around the world. These results differ from most existing work on culture, which tends to emphasize the persistent effects of culture (e.g., Nunn & Wantchekon (2011), Alesina *et al.* (2013), Fernandez & Fogli (2009), Alsan & Wanamaker (2018), Bazzi *et al.* (2020), Giuliano & Nunn (2021)) or the slow diffusion of new attitudes (Spolaore & Wacziarg, Forthcoming).

The second part of the paper focuses on understanding what may have caused the change in fertility norms that affected Britain and culturally British populations abroad in 1877. We provide evidence that one event, the famous Bradlaugh-Besant trial that took place in Britain in 1877, played a key role in starting a conversation about fertility restriction that contributed to this rapid fertility decline. 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 gave Bradlaugh and Besant a national stage to advocate for fertility control. The trial was widely reported on, sparking 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 importance of the trial has long been recognized by historians, such as Norman Himes (1970), who wrote that (p. 240), "The social effects of the publicity attending this prosecution were nothing less than revolutionary."[8] However, currently no quantitative analysis exists that would allow us to assess exactly how important this trial was to the fertility reduction described in Figure 1.

[7]To our knowledge, the closest any paper has come to identifying a synchronized fertility decline among British-origin populations around the world is Caldwell (1999), which provides evidence that Britain and Australia experienced fertility declines at the same time. We do not examine patterns in Australia because we do not have a good comparison population there, as we do in Canada, the U.S. and South Africa.

[8]Other historians who have remarked on the importance of the trial include Elderton (1914), Glass (1967), McLaren (1978), Teitelbaum (1984) and Szreter (1996).

To study the influence of this trial, we exploit variation in exposure to newspapers, the most important way that people learned about the trial (Banks & Banks, 1954). One effect of the trial was to legitimize reporting on a topic that had been taboo in Victorian society. 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." The result was widespread reporting on the trial, both in Britain and in English-language newspapers around the world.

To isolate the influence of exposure to news about the trial on fertility, we use new data from an 1895 newspaper directory that describes all of the newspapers operating in Britain and their year of opening. This allows us to adopt an identification strategy in which we compare fertility in locations that gained a newspaper just before the trial to those that gained a new paper just after reporting on the trial had died down. We verify that locations that gained a newspaper just before the trial were more exposed to news about the trial. Our identifying assumption is then that the exact timing of a newspaper opening, within a narrow window before or after 1877, was not related to other factors linked to the change in fertility after 1877. We provide several pieces of evidence suggesting that this assumption is reasonable. The results of this analysis show that exposure to news about the trial had a strong effect on fertility after 1877, substantial enough to indicate that the trial could explain the sharp change in fertility shown in Figure 1. Thus, our results suggest that the trial is a plausible catalyst for the sharp onset of the fertility transition in Britain, as well as among culturally-British populations abroad.

The findings from the second part of the paper contribute to the long-running debate on the causes of the British demographic transition. While a number of historians and demographers (Elderton, 1914; Glass, 1967; McLaren, 1978; Teitelbaum, 1984; Szreter, 1996) have noted the timing of the trial and the attention it received, this is the first study to provide direct quantitative evidence that it had a major impact on fertility.

Our results also contribute to research describing the impact of media on fertility behavior. Recent work (Jensen & Oster, 2009; La Ferrara *et al.* , 2012; Kearney & Levine, 2015) has shown that exposure to certain types of media content can

have meaningful effects on fertility behavior in modern settings.[9] Our findings are distinguished from previous results in two ways. First, a subtle difference between our results and previous studies is that they often focus on the impact of content created by media companies, such as Brazilian soap operas. In our case media matterred because it made people more aware of real-world events. Second, we find effects that are quite large relative to those in most existing studies. This should not be surprising given the magnitude and rapidity of the fertility reduction we are trying to explain. Thus, our findings indicate that, in certain environments, media exposure can have a substantial impact on attitudes and behavior.

We speculate that this large magnitude can be explained by a combination of strong economic incentives for fertility reduction, due to factor such as the rising importance of education, combined with rigid Victorian social norms that, until the trial, constrained household's willingness to reduce fertility. Given this setting, a focal event–the Bradlaugh-Besant trial–that shifted beliefs about the morality of fertility control, had the potential to induce a sharp response.[10]

Next, we present our analysis of fertility patterns in Canada, followed by similar evidence from the U.S. in Section 2.2 and South Africa in Section 2.3. These results allow us to isolate the role of shifting cultural norms from other economic or policy factors that may have influenced fertility. Then, in Section 3, we discuss the Bradlaugh-Besant trial and analyze its influence on fertility in Britain. This provides a plausible explanation for the rapid shift in attitudes about fertility among culturally British populations documented in the first part of the paper. Section 4 concludes.

[9] Jaeger *et al.* (2020) questions the validity of the analysis in Kearney & Levine (2015).

[10] The idea that a particular focal event might have a substantial impact on social norms is consistent with existing models of social norms which 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.[11] Recent empirical evidence, such as (Bursztyn *et al.* , 2020b), provides support for these predictions. 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. 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.

2 Evidence from British populations abroad

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 can exploit the fact that some Anglophone Canadians had strong cultural and linguistic links to Britain while Francophone Canadians did not. Following our analysis of Canada, we look for evidence of similar patterns among second generation British immigrants in the United States, which we compare to other second generation European immigrants, as well as in South Africa, where we compare the British-origin population to the Dutch-origin Afrikaners.

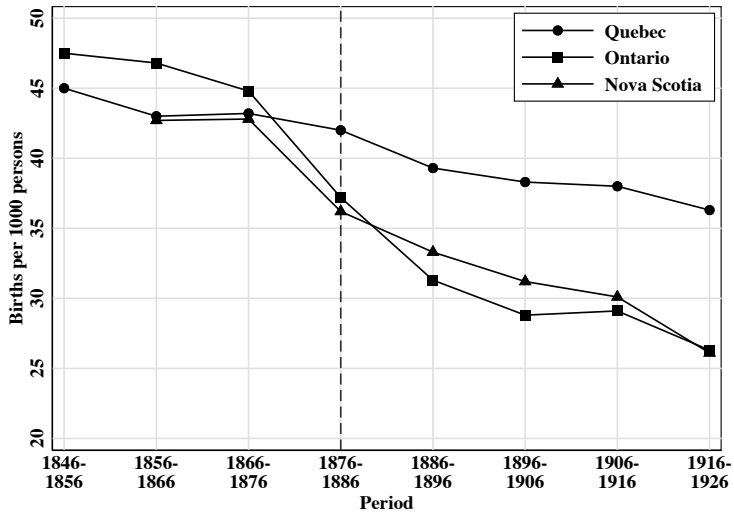
2.1 Evidence from Canada

Our Canadian 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 examine fertility patterns at the county level within a difference-in-difference approach. The time variation in this 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, we use microdata from the 1881 census to examine fertility patterns at the individual-level. The microdata allow us to analyze relative fertility changes between women residing in the same county, and with similar characteristics in terms of ages and occupations, before and after 1877, but with different cultural ties to Britain.

As a starting point, 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.[12] 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. Next we will show 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.

[12]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 10. 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. The difference may also be due to the differential influence of the Catholic church. Recent findings by Blanc (2021) suggest that a decline in the influence of the Catholic Church played an important role in France's early fertility transition, whereas studies on Quebec, such as McQuillan (2004), point to the influence of the Catholic Church in slowing the fertility decline among French Canadians.

2.1.1 Canadian data

Canada did not have a registry of births at this time, and so we infer fertility patterns from census data.[13] The census asked individuals to report their age at the time of enumeration (April 2, 1871; April 4, 1881; and April 6, 1891). While we can map these ages to birth years, our birth years will start in April rather than January. This mapping offers an imperfect proxy for births because those that die before the enumeration date will not appear in our sample. This affects the precision of our estimates, but within the difference-in-differences estimation strategy we employ, our results will not be biased unless mortality rates are differentially changing in locations or among households with stronger British ties right around 1877.

The first part of our analysis examines fertility patterns at the county-level. We digitize county-by-age tabulations from the 1871, 1881, and 1891 censuses. Those tabulations are recorded for each age up to some threshold (typically 5), at which point the tabulations appear as five year bins (e.g., 5-9, 10-14). To account for this aggregation, we first divide the population in a given bin by the number of age categories in that bin, yielding a county-specific estimate for the average (annualized) number of births over that period.[14] We then divide that estimate of births by the number of women (in thousands) between the ages of 15 and 50 that were enumerated in that county in the previous census.[15] While the use of averages is informed in part by data availability and a desire to maintain comparability across censuses, averaging also helps overcome other data issues (e.g., age heaping, or the phenomenon where less numerate individuals are more likely to report even or "round" ages). The time period covered by our analysis is April 1864-April 1886, or roughly a decade on either side of the 1877 change in fertility observed in England and Wales.[16]

[13]The census asked individuals whether they gave birth in the year prior to enumeration, but 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.

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

[15]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, do not change our results.

[16]We hesitate to use data prior to 1864 because of 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

Our county-level measure of cultural ties to Britain is based on the share of the population in the 1861 Census that is of British origin.[17] This measure 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.[18] All of these measures deliver similar results.

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

The second part of our Canadian analysis uses individual-level microdata from the 1881 census, as made available by ipums.org. The microdata include information on ethnic origin. This allows us to adopt an empirical specification that examines relative changes in fertility among British and French households residing in the same location, while controlling for characteristics such as age of the fertile-aged-woman of interest and the household occupation.[19] The main limitation of the microdata analysis is that it will only allow us to study a few years after 1877, because comprehensive microdata from later or earlier Canadian censuses are not available.

---

systematically different in locations with many English-speaking British-origin residents, and may have affected fertility patterns.

[17] The census asked respondents to record the country that represents their ethnic origins. Our British-origin population includes Irish 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.

[18] Technically only Quebec and Ontario were part of Canada at the start of our study period, with Nova Scotia and New Brunswick joining Canada upon confederation in 1867. Other provinces were sparsely populated during this period. In Ontario and Quebec, where both measures of British connection are available, we observe strong correlations between the two measures.

[19] For couples, we use the spouse's occupation. For single women living with their parents we use the father's occupation. For single women living alone we use their own occupation. Note that while the census distinguishes between households, it does not provide familial relationships. We thus have to make an assumption about whether two married individuals are in fact married to each other. We also have to make an assumption about whether young children in the household are observed with their mother or not. Details on these procedures are available in Appendix 5.3.1.

2.1.2 Canada county-level analysis

Our difference-in-differences specification compares fertility patterns in counties with stronger vs weaker ties to Britain before vs. after 1877, the year in which fertility starts to fall in England and Wales (see Figure 1). We estimate the following equation,

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

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 each age group. The denominator, FEM_{ct}, is the number of women between 15-49 (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 1877.

The identifying assumption in equation 1 is that there are no unobserved factors that caused a change in fertility (before vs after 1877) that differentially affected locations with a larger British-origin population share. 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 fertile-aged-female population in 1871.[20] Spatial autocorrelation is a potential concern, but spatially adjusted standard errors are typically smaller (see Appendix Table 10), and so we report more conservative county-clustered standard errors.

Table 1 presents our county-level regression results. The first two columns compare fertility in one "treatment" period, 1878-1880 to two pre-treatment periods, 1864-1870 and 1873-1878.[21] In Column 1, which includes just county and year fixed effects, we

[20] Weighting has little impact as county populations tend to be similar (see Table 9).

[21] 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, what we call the 1878-80 period actually includes children born after April 1878 and as late as April of 1881. Note that length of each period varies and not every year is included. This is because we are constrained by the age groupings available in difference censuses (e.g., the 1871 census only reports the number of children in the 0-6 age group) and also because we want to begin our first treatment period with children conceived after 1877. Since we are studying births per year and using a difference-in-differences framework, variation in years should

find strong evidence of a relative reduction in fertility in counties with stronger British ties. Column 2 adds in our controls. These have little effect on our estimated results. 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 statistically insignificant estimate in the 1864-70 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 1881-1885 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:	1864-1880	1864-1880	1864-1885
	(1)	(2)	(3)
British-origin shr. × 1864-70 Period			-3.343
			(5.644)
British-origin shr. × 1872-77 Period			.
			.
British-origin shr. × 1878-80 Period	-16.291***	-17.892***	-17.892***
	(1.593)	(2.277)	(2.298)
British-origin shr. × 1881-85 Period			-13.017**
			(6.351)
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

*** 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 10. Observations weighted by county fertile female population in 1871. 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.

not affect our results. We have verified this by examining results with various alternative lengths for years when that is possible. Another important consideration is that there is age heaping on age 5 that is differential across groups, so when we construct periods that include age 5, we always include ages 4 and 6. Not doing so results in inflated coefficients.

The results indicate that a county with a completely British-origin population experienced a decrease in fertility of 12-13% after 1877 relative to a county with an entirely French-origin population.[22] For a one standard deviation change in the British origin share (0.235) this implies a decrease in fertility of around 2.8-3.1%. These are fairly large effects that can explain the substantial changes in relative fertility levels found by Henripin and shown in Figure 2 (note that in Figure 2 the denominator is inhabitants rather than fertile-aged females).

Appendix Table 9 presents a variety of robustness checks. The results are unaffected when we alter the periods to only consider comparable age bins. We present unweighted results, 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 related to the share of native-born Canadians of British origin, immigrants from Britain, and other non-British immigrants. We find meaningful fertility declines in locations with a greater share of British-origin native born and those with more first-generation British immigrants, though the results for British immigrants, while larger than for the Canadian-born of British origin, are not precisely estimated. In contrast, we do not any evidence of larger fertility declines associated with having more immigrants with other backgrounds. This result provides a valuable placebo check on our findings.

Finally, we examine two alternative measures of British connection. One treats all of the population that was not of French-origin as British, while the second is 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 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, after 1877, 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

[22] In 1871-76 there were around 135 children born per 1,000 fertile-aged females with relatively similar levels in both Ontario and Quebec.

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

2.1.3 Individual-level analysis

This section pushes identification further by using microdata to compare changes in fertility patterns among Anglophone women relative to Francophone women within the same Canadian county, before and after 1877. This analysis is possible because the Canadian census asked individuals to report the country representing their ethnic background, which allows us to separate native-born Anglophone Canadians from native-born Francophone Canadians.[23] These microdata come from the 1881 census and so we infer past fertility behavior using the ages of children in the household at the time of enumeration. This approach has the advantage of allowing us to compare individuals within the same county, but we are not able to look at effects beyond 1881. Thus, we view the microdata analysis as complementary to the county-level analysis presented above, though more cleanly identified.

We organize our data as an individual-level two-period panel where the unit of observation is a fertile-aged woman (15-49). Our-post period includes children aged 0, 1, and/or 2 at the time of enumeration, i.e. births occurring between April 5, 1878 and April 4, 1881. Our pre-period fertility is derived from the number of children reporting an age between 3 and 8 (i.e., births occurring between April 5, 1872 and April 4, 1878). These periods maintain comparability with our county-level results, although as a robustness check we vary the pre-period length and find similar results.[24] Note that we rescale the outcome—dividing by the number of years in the period and multiplying by 1000—so that our estimates reflect average births per thousand women per year. This rescaling maintains comparability with our county-level analysis. Summary

[23] As in our county-level analysis, we classify Irish households as British-origin, as explained in Footnote 17. However, robustness exercises show that our results are not dependent on this decision.

[24] Age heaping is an important issue to consider when choosing a pre-period window. That is, the number of 5 year olds appearing in the census is expected to be some combination of true 5 year olds as well as some 6 year-olds and 4 year-olds that have their age incorrectly stated as 5. While we always find statistically significant negative effects, the coefficients are somewhat sensitive to whether age 6 is included or not.

statistics for these data are in Appendix Table 7.

Our preferred specification is:

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

where Y_{hdt} is the number of children per year for woman h in county d in period t (multiplied by 1000 to deliver coefficients in terms of births per thousand women per year). The key explanatory variable is an interaction between an indicator for whether the woman is culturally British ($BRIT_h$) and an indicator for the post-1877 period ($POST_t$). C_{hdt} controls for the mother's age, and in our preferred specification we include household head occupation fixed effects, which we allow to vary by period.[25] ϕ_{dt} represents county-by-period fixed effects. Given this setup, β_2 reflects the change in fertility among British-origin woman, after 1877, relative to other women living in the same county in the same period.

Estimation results appear in Table 2. Column 1 presents results from a regression using only data from Quebec and Ontario and including county and year fixed effects. These results are the most similar to those presented in Table 1. We see strong evidence that British women reduced their probability of having additional children in the household, but it is interesting to note that the coefficient in Column 1 is smaller than the estimates in Table 1. The primary cause of this difference is that, because the unit of observation in the microdata analysis is a woman, and we infer fertility based on children living in the same location, this analysis does not account for children living with relatives or in an institutional setting such as orphanages.[26] This highlights one of the benefits of looking at results from both the aggregated and microdata perspective.

In Column 2 we expand our sample to include all of the provinces covered by the Canadian census. In Column 3, we include county-by-year fixed effects. This substantially reduces the estimated effect, though it remains strongly statistically significant. One reason that we may find a smaller effect when county-by-year fixed

[25]For married women we use the spouse's occupation. For single women that still live at home we use the occupation of the father. For all other women we use their own occupation.

[26]We know this is a primary cause because when we aggregate the microdata to the county-by-period level, we obtain results that are close to those in Table 1 when we include all children, but close to those in Table 2 Column 1 when we include only the children used in our microdata sample.

effects are included is that any spillovers from British-origin households that also reduce the fertility of their French-origin neighbors will be absorbed by these effects. Columns 4 and 5 add in mothers' age-by-period fixed effects and head of household occupation-by-period fixed effects, neither of which substantially affects our results.

Table 2: Canada individual-level difference-in-differences results (OLS regressions)

DV: Children per year per woman /1000					
	Quebec and Ontario	All Counties	All Counties	All Counties	All Counties
	(1)	(2)	(3)	(4)	(5)
British × Post 1877	-10.090***	-8.051***	-5.202***	-5.219***	-5.212***
	(1.788)	(1.652)	(1.404)	(1.251)	(1.203)
British ind.	Yes	Yes	Yes	Yes	Yes
Period FEs	Yes	Yes			
County FEs	Yes	Yes			
County-by-Per. FEs			Yes	Yes	Yes
Age-by-Period FEs				Yes	Yes
Occ-by-Period FEs					Yes
Observations	1,186,367	1,525,296	1,525,296	1,525,296	1,525,296

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by county. Sample is restricted to women between the ages of 15-49 at the start of the period (1872 or 1878). Age fixed effects are in 5-year bins. 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.

The results indicate that fertility among British-origin women fell by just over 5 births per 1000 women, after 1877, relative to non-British women residing in the same county. This estimate is meaningful, representing a 4% decline from the sample mean.

Appendix Table 11 examines the robustness of our preferred specification (Column 5). We present results considering alternative pre-period windows, restricting attention to women that we infer to be married by the start of the period, allowing the effect to vary separately for British and Irish households, dropping Irish households, and using "Non-Catholic" as our measure of cultural ties. All of these results continue to indicate that fertility rates among culturally British women fell after 1877.

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.

These results, together with the discussion of the Bradlaugh-Besant trial in Section 3, contribute to the surprisingly small existing literature on the Canadian fertility transition.[27] Henripin (1968) and, more recently, McInnis (2000), have noted the differences in fertility behavior among Anglophone and Francophone Canadians. Bouchard & Lalou (1993) and Gauvreau & Gossage (2001) document that the origin of this difference can be traced back to the 1870s, dispelling the myth that fertility had always been substantially higher among the Francophone population. However, neither study identifies a cause of the sharp differential change in fertility between Anglophone and Francophone populations that emerged at that time. Some have argued that Catholicism can explain the persistence of high fertility rates among Francophone Canadians (McQuillan, 2004). However, the real mystery is why fertility among Anglophone Canadians, which was as just as high in the middle of the nineteenth century, started to fall in the late 1870s.

Our results offer an explanation for the emergence of differential fertility patterns among the Anglophone and Francophone populations that began in the 1870s. By showing that the fertility reduction among Anglophone Canadians was part of a pattern observed among British-origin populations around the world, we are able to isolate the role of changing fertility norms, transmitted along cultural and linguistic lines, in the Canadian fertility transition. Later, we will also offer an explanation for the event that set this change in motion.

[27] McInnis (2000) writes that "It is regrettable that historical demographers have paid so little attention to nineteenth-century Canada, since this country makes an intriguing population laboratory."

2.2 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. As in the previous section, we use microdata to examine fertility changes: the 1880 census, as made available from ipums.org. However, one important difference between the two settings is that the United States did not ask about ethnic origin. The U.S. census did ask individuals to report their birthplace as well as the birthplace of their parents. We thus set out to compare second generation British immigrants to second generation immigrants from other European countries.[28]

To be clear, this analysis does not aim to explain aggregate U.S. fertility patterns (see Haines (2000) for a review of that literature). The group of second generation British-origin immigrants that we study is simply too small to affect aggregate fertility in the U.S. The goal of this analysis is to provide another way of isolating the role of culture in influencing to fertility decisions. However, our results do add a new dimension to the large literature on the fertility transition in the U.S.

As in our analysis of the Canadian microdata, our analysis treats fertile-aged women as the unit of observation and looks at the number of children per year in the years from 1879-1880 compared to years just before 1877. We compare fertility patterns for women with two British-born parents (the treatment group) to those with two European-born parents (the control group).[29] Because our U.S. sample is focused on second-generation immigrants, the age distribution and marital status of the women we study fluctuate substantially as a result of previous immigration waves. To deal with this, our U.S. analysis always includes age-by-period fixed effects and we analyze single and married women separately.

The results of our analysis of U.S. data, presented in Appendix 5.4, show that married second-generation immigrants of British descent experienced fertility declines after 1877, relative to before, when compared to other second-generation European-origin immigrants. We find no fertility change among single women, which is not

[28] We focus on second-generation immigrants to decrease the impact of immigrant selection on our results, and for consistency with previous work such as Fernandez & Fogli (2009).

[29] We exclude Irish-origin households from the main analysis. This differs from Canada, where the majority of Irish immigrants were protestants from Northern Ireland, with stronger cultural ties to Britain. Further discussion of the data used in the U.S. analysis, and summary statistics, are in Appendix Table 12.

surprising given that single women rarely had children during this period. In terms of magnitude, British-origin women who were married by 1880 reduced fertility by between 2.5 and 3.1 children per thousand women per year. These effects are comparable, but smaller than those found in the most similar Canadian results (an estimated reduction of 5.2 children per thousand women shown in Column 5 of Table 2) but this difference should be interpreted with caution since the Canada microdata analysis extends to April 1881, while the treatment period in the U.S. data ends in 1880. Overall, the U.S. results confirm, in a second setting, that there was a fertility decline among British-origin women after 1877.

2.3 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 Africa 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.[30] Data tracking fertility for these groups are drawn from the Cape Colony censuses of 1875 and 1891.[31] 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

[30]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.

[31]See Appendix 5.5 for further discussion of the South Africa data.

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 17, display the same basic pattern observed in Canada: locations with a greater connection to Britain experience a reduction in fertility after 1877, relative to 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.

These findings, together with our discussion of the Bradlaugh-Besant trial below, add to the small existing literature on the fertility transition in South Africa. An important recent study in this area, Cilliers & Mariotti (2019), dates the start of South Africa's fertility transition to the cohort of women who began having children in the 1870s, but does not provide an explanation for why fertility began changing at this time.[32] What our study adds to this discussion is a plausible explanation for what caused fertility to begin changing at this time, and the role of cultural ties in bringing it about.

2.4 Summary

Thus far, we have shown: (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 the U.S., are extremely difficult

[32] Cilliers & Mariotti (2021) explores how fertility changed during this period.

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 a natural question: what event, or set of events, might have acted as the catalyst for the cultural changes that we observe? The remainder of this paper is focused on offering a potential answer to this question.

3 A plausible catalyst: The Bradlaugh-Besant trial

3.1 Context: Britain's fertility transition in perspective

The timing of the fertility decline in England and Wales shown in Figure 1, as well as a similar decline observed in Scotland (Appendix Figure 8) was not an outlier when compared to other European countries. A comparison between France, England & Wales, and Germany (in Appendix 5.2) shows that England & Wales was an intermediate case, while France transitioned much earlier and Germany transitioned later, after 1900. Among other European countries, most show declines in fertility beginning in the second or third quarter of the nineteenth century (Appendix 5.2).

Where England & Wales, as well as Scotland, stand out is in the sharpness of the onset of their decline, a pattern due in part to the fact that the decline appears simultaneously in almost all parts of the country. That many Western European countries experienced fertility declines in the second half of the nineteenth century suggests that there were common underlying forces, such as a growing need to education children, pushing families across Europe to restrict fertility. The sharpness of the decline observed in England & Wales, as well as Scotland, suggest an additional role for a focal event that rapidly shifted fertility norms.

3.2 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 trial was the 1875-76 publication of *The Fruits of Philosophy*, a book written by the American doctor Charles Knowlton in 1832. The book had been available in England since 1834 and was never challenged, perhaps because it always sold in small numbers.[33] 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 activist, 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 of 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,[34]

> *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...*

The jury found the pair guilty of publishing a book "calculated to deprave the public morals." Bradlaugh and Besant were each fined £200 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

[33]Some evidence on this point, based on Google Ngrams mentions of the book, is available in Appendix Figure 13.

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

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."[35] 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."[36] 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 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.

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.[37] The pattern we ob-

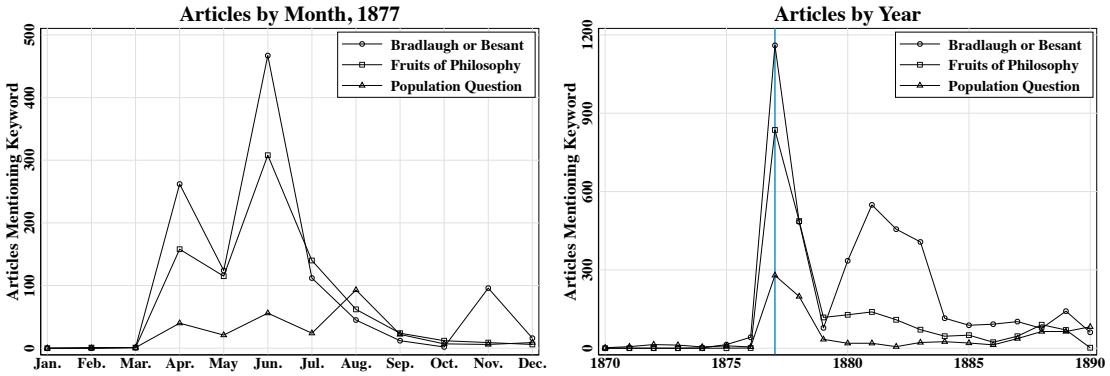
[35]Quoted from Banks & Banks (1954).

[36]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.

[37]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

serve 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 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.

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.

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 5.7.3). 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

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.

during this period were dedicated to arguing that couples had a right, and even a responsibility, to choose.[38]

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 numerous pamphlets published during and after the trial, though the techniques were rudimentary and in some cases information was incorrect.[39] It seems unlikely that the trial improved available contraceptive technology, but it may have facilitated the dissemination of information about available techniques.[40] Because 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.[41] Bradlaugh and Besant also gave several public lectures around the country.[42] Finally, there was the Malthusian League, an organization dedicated to spreading family planning ideas, which was

[38] 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.

[39] 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.

[40] 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).

[41] 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.

[42] 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."

established following the trial.[43] The trial also generated an enormous increase in sales of books on family planning in England and Wales.[44]

Perhaps surprisingly, there is no evidence of a forceful negative reaction by religious leaders to the trial. As D'Arcy (1977) writes, "neither the [Malthusian] League's *The Malthusian* nor Bradlaugh's *National Reformer*, both ever on the alert to notice attacks on neo-Malthusian propaganda, recorded any significant attacks on them by Church bodies or Victorian clergymen."[45]

3.3 Reports of the trial abroad

News about the trial spread rapidly outside of England, particularly in British colonies. The trial was covered in English-language Canadian newspapers.[46] 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 (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).[47] Other channels, such as letters and the flow of immigrants, were also likely important in transmitting news of the trial across the Atlantic.[48]

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

[44] 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.

[45] See D'Arcy (1977) for further discussion.

[46] 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.

[47] 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."

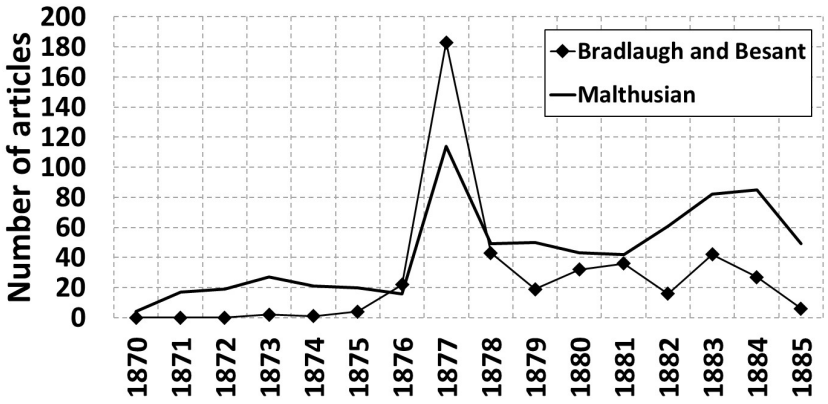
[48] Letter flows between the U.K. and Canada during this period were substantial; in 1884 the Postmaster General's report shows over 1.8 million letters reached Canada from the U.K, along with over 2.1 million circulars, pamphlets, books and newspapers. In addition, tens of thousands of migrants that arrived on Canadian shores each year, many from the British Isles (McInnis, 1994).

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.

The fact that French-language Canadian newspapers were reporting on Bradlaugh's activities in the 1880s illustrates that language barriers did not limit the ability of these papers to report on events in the U.K. Thus, language differences alone cannot account for the lack of reporting on, and thus exposure to, the Bradlaugh-Besant trial. Instead, cultural differences among either editors or readers must have played a role.

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 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 5.6.2), 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.

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



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

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 provide further indications of the impact of the trial. These data, presented in Appendix 5.6.1, 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. Notably, the N-grams data show that mentions of Bradlaugh and Besant increased substantially more in sources using British English than those using American English. This provides additional evidence that, even within language groups, culturally-British populations showed greater interest in the trial.

3.4 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 rich information on newspapers that allows us to identify local variation in exposure to the trial.

3.4.1 Newspaper data

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 measure of exposure to the trial is driven by local newspapers. 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.[49] 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). Since our analysis is based on exposure through the provincial press, London is excluded from all of our results, since that was the center of the trial and the home of the national press.

We use two main types of newspaper data. The first, which comes from a directory of newspapers published in 1895, provides a complete listing of newspapers active in England & Wales including, crucially, their opening date.[50] We match the location of these newspapers to registration districts, the geographic unit we will use in our analysis.[51] As described below, we will use the timing of these openings as a source of variation in exposure to news about the trial.

[49] 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).

[50] See Appendix 5.7.1 for further discussion of these data.

[51] 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.

The second set of newspaper data covers actual articles about the trial. The source for these data is britishnewspaperarchive.co.uk, a partnership between the British Library and the UK genealogy service findmypast.com to digitize newspapers from the British Library's extensive collection. While only a selection of newspapers survived to make it into this database, the available set is quite rich, containing over 40 million newspaper pages; no similarly comprehensive database exists in the other locations we study.[52] 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. 1,149 articles match these criteria, and as already shown in Figure 3, the timing of these articles matches pivotal moments of the trial, indicating that our search is a good proxy for trial coverage.[53]

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 5.7.3, 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.

3.4.2 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.[54] We focus on

[52] 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.

[53] We manually reviewed each article to ensure that we correctly identify articles related to the trial and classified each article into several categories (Appendix Table 21). 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 from our search were related to the trial in some way.

[54] 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

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.[55]

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 affiliation, literacy at the time of marriage, etc. Further details on the data and construction are in Appendix 5.7.4.

3.4.3 Analysis strategy and results

Our analysis aims to compare a set of similar locations, but where some locations received more news exposure to the trial than others. We use the opening of new newspapers as a source of variation and compare changes in fertility among locations where a new newspaper opened in a short window before the trial (up to the end of 1877) to locations that did not receive a new newspaper before the trial but did receive a new newspaper in a window of similar length just after the trial (starting in 1878).[56] The key identifying assumption is that, within a narrow window, the determinants of receiving a newspaper just before vs just after the trial are not related to other factors linked to the change in fertility after 1877 relative to before. To be clear, the window around 1877 is used only to identify treatment and control locations. Our analysis will examine changes in fertility over a much longer time horizon.

We can provide support for our key identifying assumption by showing that our treated and control locations become increasingly balanced on observables as we narrow the window around the trial used to identify our set of treatment and control locations. Table 3 presents these balance test results.[57] The sample in the first col-

all births by the early 1860s and that registration was essentially complete by the mid-1870s.

[55]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. We chose five years to maintain comparability with the Canadian analysis, but results are not sensitive to this choice.

[56]We include 1877 as part of our treatment period since there was substantial coverage of the trial through November and into early 1878 as Bradlaugh and Besant fought to have the conviction overturned. If a newspaper opened in a location in both the window before the trial and the window after the trial, that location is considered a treated location, since the earlier paper would have been able to report on the trial. This assumption is not crucial: dropping locations where newspapers opened both just before and just after the trial does not meaningfully affect our results.

[57]The results in Table 3 are weighted as in our main results. An unweighted version points to fewer

umn compares districts where a newspaper opened in the five years before 1877 (which includes 1877) to districts where a newspaper opened in the 5 years after 1877. The remaining columns examine balance when the window is shortened to four, three, and two years before and after 1877. The results show a clear pattern of improved balance as we shorten the windows used to build our treatment and comparison groups. This makes sense: whether a district received a paper just a few years earlier or a few years later is likely to be driven by many chance factors, while over long time horizons we expect that the number of openings will be more linked to underlying economic and demographic factors. Our identification strategy relies on isolating the largely random variation in the exact timing of openings. As we shorten the windows to two or three years, the number of significant coefficients approaches what we would expect to observe given random variation, suggesting that our identification strategy is likely to be working well. Moreover, as we show in Appendix Table 25, using narrower windows is associated with a stronger estimated effect of exposure to the trial through new newspapers, suggesting that any remaining differences are likely working against our findings.

Our empirical approach is illustrated in Figure 5, which is based on fertility patterns in two types of locations: those where a newspaper opened within three years before the trial, and those where a newspaper opened within three years after the trial. We focus on three-year windows here and in our main analysis because this generates a sample that is close to balanced on observables while still providing sufficient power, though we also consider alternative window sizes (see Appendix 5.7.5). As in Figure 1, 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 "more exposed" locations (those receiving a newspaper in the three years before the trial) and "less exposed" districts (those where a new newspaper opened in the three years after the trial).

statistically significant differences and smaller coefficients that continue to show increased balance as the window is narrowed.

Table 3: Assessing Sample Balance for Various Newspaper Opening Windows

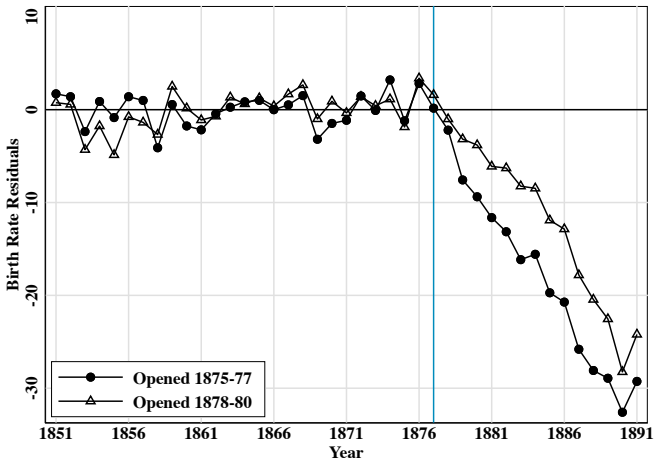
Dependent Variable (Standardized))	**5 Year Window**	**4 Year Window**	**3 Year Window**	**2 Year Window**
Marriage rate, 1873-1877	0.282**	0.308*	0.061	-0.217
	(0.137)	(0.157)	(0.147)	(0.247)
Shr. marriages at Registrar, 1873-1877	0.001	0.032	0.015	0.220
	(0.094)	(0.104)	(0.124)	(0.134)
Shr. marriages Catholic church, 1873-1877	-0.081	0.003	0.012	-0.208
	(0.131)	(0.138)	(0.142)	(0.167)
Minor shr. of marrying parties, 1873-1877	0.321***	0.315**	0.227*	0.105
	(0.111)	(0.127)	(0.137)	(0.141)
Illiterate shr. of marrying parties, 1873-1877	0.159	0.206	0.075	-0.160*
	(0.118)	(0.129)	(0.119)	(0.089)
First marriage shr. 1873-1877	-0.075	-0.076	-0.018	0.047
	(0.090)	(0.107)	(0.129)	(0.159)
Population density, 1871	0.112	0.209	0.049	-0.475
	(0.219)	(0.249)	(0.404)	(0.324)
Average mortality rate, 1871-1880	0.058	0.066	-0.220	-0.696*
	(0.217)	(0.267)	(0.320)	(0.383)
Average under 5 mortality rate, 1871-1880	0.150	0.155	-0.024	-0.389
	(0.152)	(0.181)	(0.225)	(0.254)
Illegitimate birth share, 1873-1877	-0.013	-0.024	-0.124	-0.099
	(0.100)	(0.107)	(0.083)	(0.102)
Manufacturing employment share, 1861	0.278*	0.223	0.027	-0.240
	(0.159)	(0.181)	(0.196)	(0.252)
Agriculture employment share, 1861	-0.135	-0.138	-0.146	0.065
	(0.107)	(0.116)	(0.128)	(0.143)
Share of fertile-aged women under 30, 1871	0.076	0.095	0.071	0.053
	(0.069)	(0.078)	(0.101)	(0.109)
Num. Established Newspapers	0.833**	0.581***	0.555***	0.483
	(0.331)	(0.201)	(0.191)	(0.296)
Birth Rate, 1873-1877	0.237	0.285	0.346	0.303
	(0.159)	(0.183)	(0.229)	(0.289)
Districts	148	129	98	73

This table presents coefficients and 95% confidence intervals (based on robust standard errors) for regressions looking at the association between treatment (whether a newspaper opened in a location in the years before 1877) and each dependent variable. The sample includes all districts where a newspaper opened in the specified years before 1877 (treated locations) or the specified years after 1877 (control locations). To maintain comparability with our main results, regressions include region fixed effects and are weighted by 1871 population.

Figure 5 indicates that, at least after accounting for regional trends, birth rates in districts where newspapers opened just before and just after the trial were trending similarly prior to the trial. After the trial, birth rates decline in both sets of dis-

tricts, but the decline is relatively stronger in districts where new newspapers had just opened. This graph presents a useful framework for thinking about our results. While both sets of districts are likely affected by the trial, we expect the ideas to be more widely discussed in districts with additional newspapers, and thus we expect a stronger decline. Later, we will use data on articles to show that there was, in fact, relatively more newspaper coverage of the trial in our treated locations.

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). 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. Districts are grouped based on whether they had a newspaper open in the three years before the trial ("Opened 1875-77") or the three years just after the trial ("Opened 1878-80").

Our main 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. We restrict our sample to districts where a newspaper opened within three-year windows before or after the trial (so 1875-1877 vs 1878-1880). We present results from alternative windows in the appendix. Using this sample, we estimate the following:

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

where BR_{dt} is the average birth rate in district d during period t, defined as births per

1000 fertile-aged women. $EXPOSURE_d$ is the number of newspapers that opened in a location before the trial (1875-77).[58] 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 γ_d represents our district fixed effects while ϕ_{rt} represents a set of region-by-year fixed effects. Observations are weighted based on 1871 district populations.[59] Finally, we adjust the standard errors by clustering at the district level.[60]

To strengthen identification, our analysis includes controls for a rich set of factors that existing work suggests may have influenced fertility during this period. The vector X_{dt} contains three sets of controls which we allow to fully interact with our period indicators. Our set of "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. We also include an "existing newspaper" control reflecting the number of newspapers in a location that opened before the window used to identify new papers. Finally, in some specifications we include lagged fertility as a control, to ensure that our results are not being driven by mean reversion in fertility rates.

Table 4 presents our primary results, which compare locations where a newspaper opened in the three years before 1877 to those where one opened in the three years after. Column 1 offers a baseline estimate adjusting only for differential regional trends.[61] Birth rates in districts that received a newspaper in the three years before

[58] Results are almost identical if we instead use an indicator variable for whether any newspaper opened, reflecting the fact that less than 15% of our treated districts had more than one newspaper open in the three years before the trial.

[59] Unweighted results show statistically significant effects of a similar magnitude, see column 1 of Appendix Table 26.

[60] We have also tried correcting our standard errors to account for serial and spatial autocorrelation at the 25km, 50km, and 100km level (see Appendix Table 26). 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.

[61] We find stronger results if region effects are not included in the specification, but there is evidence

the trial fell by 4.5 births per 1000 fertile-aged women following the trial, as compared to districted that received a new paper after the trial.

Columns 2, 3, and 4 assess the sensitivity of the results to including additional controls. We continue to find strong evidence of a decline in fertility in districts with relatively more exposure to the trial. The point estimate does vary depending on which set of controls are included, but the results in Columns 2-4 are statistically indistinguishable. Our preferred specification, in Column 4, which includes all of our controls, implies that exposure to a new newspaper was associated with a fertility reduction of 2.7 births per thousand. Relative to Column 1, the magnitude of the estimated coefficient in Column 4 has dropped somewhat, though the R-squared has increased from 0.211 in Column 1 to 0.704. 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.

Appendix Table 27 examines the role of local child and female labor force participation rates, as well as a measure of feminist sentiment in each district before the trial. The findings are robust to controlling for these additional factors. An annual event-study version of the analysis is also available, in Appendix Figure 18.

Finally, in Column 5 we extend our analysis back to 1868 and forward to 1887. The much smaller and statistically insignificant coefficient estimated for the 1868-72 period indicates that locations where a newspaper opened in 1875-77 did not have significantly different pre-existing fertility trends compared to those where a newspaper opened in 1878-1880. The negative coefficient estimated for the 1883-7 period is smaller than that estimated for 1878-82 (and statistically insignificant), which suggests that the differential fertility response associated with newspaper exposure was fading over time, though the estimated magnitude remains substantial. This pattern makes sense if we think that diffusion of new attitudes toward fertility would slowly erode the differential effect of direct exposure to the trial.

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.

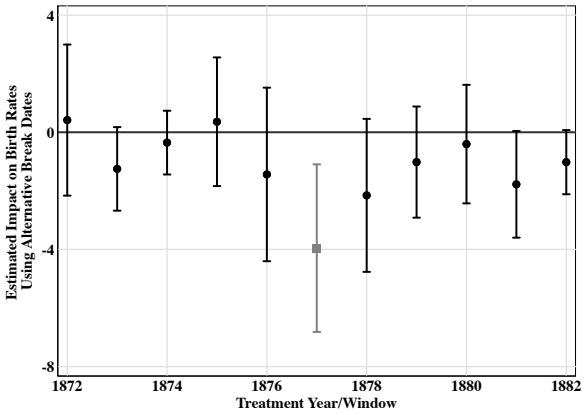
Table 4: Difference-in-Differences results for England and Wales

DV is Births per 1000 fertile-aged women					
	(1)	(2)	(3)	(4)	(5)
Newspaper Openings (75-77) × 1868-1872 Period					-0.135
					(1.095)
Newspaper Openings (75-77) × 1873-1877 Period					.
					.
Newspaper Openings (75-77) × 1878-1882 Period	-4.544**	-2.962***	-2.598***	-2.673***	-2.974**
	(1.859)	(0.853)	(0.923)	(0.875)	(1.292)
Newspaper Openings (75-77) × 1883-1887 Period					-2.552
					(1.775)
District fixed effects	Yes	Yes	Yes	Yes	Yes
Region-by-period fixed effects	Yes	Yes	Yes	Yes	Yes
Marriage controls		Yes	Yes	Yes	Yes
Other district controls		Yes	Yes	Yes	Yes
Est. newspaper control			Yes	Yes	Yes
Lagged fertility control				Yes	Yes
No. districts	98	98	98	98	98
Observations	196	196	196	196	392
R-squared	0.211	0.671	0.679	0.704	0.525
Coef. implied lower bound following Oster (2019):				-2.146	

*** 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 1871 district population. Birth rates are defined as number of births per 1000 fertile aged women (ages 15-49). These birth rates are averaged over five year windows (e.g., 1873-1877, the pre-trial period, and 1878-1882, the post-trial period). Sample is restricted to the set of districts where a newspaper opened between 1875 and 1877 (treatment) and districts that did not have a newspaper open between 1875-1877 but did have a newspaper open between 1878 and 1880 (control). Our marriage, district, and newspaper controls are fully interacted with 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. Established newspapers is the number of newspapers that opened up in the district before 1875. In Column 5, 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).

Next, we ask: when did exposure to additional newspapers matter for fertility behavior? To answer this question, we apply exactly the same method used above, but with different focal years in place of 1877. So, for example, if 1874 is our focal year, then we study the differential fertility response in the five years just before vs. just after 1874, in locations where a newspaper opened in the three years just before 1874 to locations where a newspaper opened in the three years after. Results from applying this method with focal years ranging from 1872-1882 are presented in Figure 6. These results show that exposure to new newspapers only affected fertility in 1877/78. This pinpoints when differential media exposure mattered: only right during 1877. In other years, having additional newspapers had no strong association with changing fertility patterns.

Figure 6: Falsification Exercise to Assess when Newspaper Openings Affect Fertility



Each coefficient and confidence interval comes from a regression that follows columns 1-4 of Table 4, but with the birth rates and newspaper openings centered around a different break date. Because of data availability, we cannot fully mimic the structure of the district and marriage controls. These regressions thus include: district fixed effects, region-by-period fixed effects, the number of established papers (interacted with the post period), and lagged birth rates.

The results in Figure 6 bring us one step closer to linking the impact of media exposure on changing fertility behavior to the Bradlaugh-Besant trial. To make this link more concrete, we bring in the district-level data on newspaper articles written about the trial mentioned earlier. There are two important challenges in using articles about the trial to study the impact of exposure to the trial on fertility. First, the available set of articles represent only a sample of articles about the trial: the sample

available from newspapers that survived to be digitized and included in the British Library's collection. This means that there is potentially important measurement error in our observations of articles about the trial. Second, the distribution of articles about the trial will be linked to the underlying distribution of newspapers, which is likely to be correlated with local economic features that may also be influencing fertility patterns. Both of these issues can be addressed by using an instrumental variables strategy in which variation in newspaper exposure driven by the opening of new newspapers just before 1877, compared to just after, is used to instrument to exposure to articles about the trial.

Table 5 presents results using the articles data. Column 1 provides naive results looking at the relationship between the number of articles written about the trial by a local newspaper (in the same district) and the change in fertility in the five years after 1877 compared to the five years before.[62] This suggests that exposure to an additional article about the trial in our sample is associated with a reduction in fertility of 0.063 births per thousand. In Columns 2 and 3, we then provide IV results using the opening of a new newspaper just before the trial, compared to locations where one opened just after, to instrument for the number of new articles about the trial in a location. The first stage results, at the bottom of the table, show that the opening of a new newspaper was associated with 9-11 additional articles about the trial in our articles sample. This effect is strongly statistically significant. These results provide important support for the idea that the opening of newspapers increased exposure to the Bradlaugh-Besant trial.

The coefficients in Columns 2-3 indicate that exposure to an additional article in our sample was associated with a fertility reduction of about 0.27 births per thousand. That this coefficient is larger than the one in Column 1 is sensible given that we know that there is measurement error in our sample of articles. Multiplying these coefficients by the first stage estimates suggests that the opening of a new newspaper, operating through articles on the Bradlaugh-Besant trial, was associated with a

[62]For comparability, this regression is run on the same sample that we will use for the IV analysis in Columns 2-3, i.e., the set of districts where at least one newspaper opened sometime in 1875-1880. This regression contains our full set of control variables except the control for the pre-existing newspaper network. We do not include that control here because the number of articles is closely related to the pre-existing newspaper network, and since the article data is measured with error, the two are difficult to distinguish in the reduced-form specification. However, this variable can be included in the IV specification, where our instrument helps deal with measurement error in the articles data, as is done in Column 3.

reduction in fertility of 2.6-3 births per thousand. Thus, the magnitude implied by these IV regressions is essentially identical to the magnitudes obtained in Table 4.

These IV results help us fill in the final link between fertility, newspaper exposure, and the trial. However, one must recognize that the IV strategy is still reliant on an exclusion restriction. In this case, the exclusion restriction is that there was no other channel through which exposure to additional newspapers affected fertility after 1877, compared to just before, other than through exposure to news about the trial (or events arising from the Bradlaugh-Besant trial, such as the subsequent trial of Edward Truelove). While this exclusion restriction is ultimately untestable, the work of numerous historians and demographers, who have studied the British fertility transition intensively, provides reassurance that the exclusion restriction is reasonable. As D.V. Glass put it, "Certainly the trend in fertility in England and Wales showed no signs of falling in the 'fifties and 'sixties. The most that can be said is that the ground was being prepared during this period. Some special influence was, however, necessary...That influence was the Bradlaugh-Besant trial of 1877."

Table 5: Impact of Trial Coverage on Fertility in England and Wales

DV is Births per 1000 fertile-aged women			
	Reduced Form	**IV**	**IV**
	(1)	(2)	(3)
Newspaper Coverage of Trial × 1878-1882 Period	-0.063*	-0.269**	-0.274*
	(0.035)	(0.120)	(0.138)
First Stage			
Newspaper Openings (75-77) × 1878-1882 Period		11.129***	9.756***
		(2.832)	(2.671)
Cragg-Donald F-Statistic		90.491	63.555
District fixed effects	Yes	Yes	Yes
Region-by-period fixed effects	Yes	Yes	Yes
Marriage controls	Yes	Yes	Yes
Other district controls	Yes	Yes	Yes
Lagged fertility control	Yes	Yes	Yes
Est. newspaper control			Yes
No. districts	98	98	98
Observations	196	196	196

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the district level, in parentheses. All regressions weighted by 1871 district population. All controls and sample restrictions follow from Table 4.

3.4.4 Mechanisms: Delayed marriage or adjustment within marriage?

Women might have reduced fertility using a variety of mechanisms, including delayed marriage, reduced fertility within marriage through either spacing or stopping, or forgoing fertility altogether. In our context, where most women married and the vast majority of births occurred within marriage, the two most likely mechanisms are delayed marriage or adjustment within marriage. In this section, we examine these mechanisms, focusing specifically on the impact of delayed marriage on fertility. The availability of a comprehensive annual registry of marriages in England and Wales offers the opportunity to analyze the impact of exposure to the trial on marriage rates, as well as the impact on fertility operating through changing marriage rates, something that is not possible in other settings.

Our analysis begins by looking at the impact of the trial on marriage rates. Using the same approach as in our analysis of fertility declines, we find clear evidence of a reduction in marriage rates in locations more exposed to news about the trial (described in detail in Appendix 5.7.7). Marriages in locations where an additional newspaper opened just before the trial declined by just over 2 percent (0.8-0.9 marriages per thousand women aged 15-49).

To estimate the impact that these changes in marriage patterns had on fertility, we estimate the relationship between marriage and fertility rates in the pre-1877 period and then calculate the reduction in the fertility rate associated with exposure to the trial due to changes in the marriage rate. Our estimates show that an increase in the marriage rate was associated with an increase in births contemporaneously and over the following three years. These are not causally identified estimates, and there is reason to believe that many marriages were the result of a pregnancy rather than the other way around. Thus, applying this estimated relationship to the change in marriages caused by the trial will yield an upper-bound estimate of the impact of the trial on fertility through delayed marriage. These upper-bound estimates indicate that at most 30-40% of the effect of the trial on fertility could be due to delayed marriage. Thus, while we do find clear evidence that marriage rates dropped in locations more exposed to the trial, most of the fertility decline must have come through reductions of fertility within marriage. This pattern is consistent with Bradlaugh and Besant's clear goal of influencing poorer families to reduce fertility within marriage.

4 Conclusion

This paper provides evidence on the impact of cultural factors during the historical fertility transition. 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. 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 were 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. We show that cultural and linguistic ties can act as conduits transmitting changing social norms around the world, resulting in quick and meaningful changes in behavior. While these results highlight the role of culture, we are not arguing that economic factors were unimportant for the fertility changes observed during this period. Our results are most consistent with a story in which economic forces generated strong incentives for reductions in family size but cultural norms 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.

References

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

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

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

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

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

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

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

Bazzi, Samuel, Fiszbein, Martin, & Gebredsilasse, Mesay. 2020. Frontier Culture: The Roots and Persistence of "Rugged Individualism" in the United States. *Econometrica*, **88**(6), 2329–2368.

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

Becker, Sascha O, Cinnirella, Francesco, & Woessmann, Ludger. 2010. The trade-off between fertility and education: evidence from before the demographic transition. *Journal of Economic Growth*, **15**(3), 177–204.

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

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

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

Blanc, G, & Wacziarg, R. 2020. Change and Persistence in the Age of Modernization: Saint-Germain-D'Anxure 1730-1895. *Explorations in Economic History*, **78**, 101352.

Blanc, Guillaume. 2021 (Jan.). *Modernization Before Industrialization: Cultural Roots of the Demographic Transition in France*. Mimeo.

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

Bouchard, Gérard, & Lalou, Richard. 1993. La surfécondité des couples québécois depuis le XVIIe siècle, essai de mesure d'interpréation. *Recherches Sociographiques*, **34**(1), 9–44.

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

University Press.

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

Bursztyn, L, Egorov, G, & Fiorin, S. 2020a. From Extreme to Mainstream: The Erosion of Social Norms. *American Economic Review*, **111**(11), 3522–3548.

Bursztyn, L, Gonzalez, AL, & Yanagizawa-Drott, D. 2020b. Misperceived Social Norms: Women Working Outside the Home in Saudia Arabia. *American Economic Review*, **110**(10), 2997–3029.

Caldwell, JC. 1999. The Delayed Western Fertility Decline: An Examination of English-Speaking Countries. *Population and Development Review*, **25**(3), 479–513.

Chandrasekhar, S. 1981. *"A Dirty Filty Book"*. Berkeley: University of California Press.

Cilliers, Jeanne, & Mariotti, Martine. 2019. The shaping of a settler fertility transition: eighteenth- and nineteenth-century South African demographic history reconsidered. *European Review of Economic History*, **23**(4), 421–445.

Cilliers, Jeanne, & Mariotti, Martine. 2021. Stop! Go! What can we learn about family planning from birth timing in settler South Africa, 1835-1950? *Demography*, **58**(3), 901–925.

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

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

Crafts, Nicholas, & Mills, Terence C. 2020 (Aug.). *The Race Between Population and Technology: Real Wages in the First Industrial Revolution*. CEPR Discussion Paper No. 15174.

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

Daudin, Guillaume, Franck, Raphaël, & Rapoport, Hillel. 2019. Can Internal Migration Foster the Convergence in Regional Fertility Rates? Evidence from Nineteenth Century France. *The Economic Journal*, **129**(620), 1618–1692.

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

Diebolt, C, Menard, A-R, & Perrin, F. 2017. Behind the Fertility-Education Nexus: What Triggered the French Development Process? *European Review of Economic History*, **21**(4), 357–392.

Dribe, M. 2008. Demand and supply factors in the fertility transition: a county-level analysis of age-specific marital fertility in Sweden, 1880-1930. *European Review of Economic History*, **13**, 65–94.

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

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

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

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

Galor, Oded. 2011. *Unified Growth Theory*. Princeton University Press.

Galor, Oded, & Weil, David N. 1996. The Gender Gap, Fertility, and Growth. *The American*

Economic Review, **86**(3), 374–387.

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

Gauvreau, Danielle, & Gossage, Peter. 2001. Canadian Fertility Transitions: Quebec and Ontario at the Turn of the Twentieth Century. *Journal of Family History*.

Giuliano, Paola, & Nunn, Nathan. 2021. Understanding Cultural Persistence and Change. *Review of Economic Studies*, **88**(4), 1541–1581.

Glass, DV. 1951. A Note on the Under-Registration of Births in Britain in the Nineteenth Century. *Population Studies*, **5**(1), 70–88.

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

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

Haines, Michael R. 2000. *A Population History of North America*. Cambridge, UK: Cambridge University Press. Chap. The White Population of the United States, 1790-1920, pages 305–370.

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

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

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

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

Jaeger, DA, Joyce, TJ, & Kaestner, R. 2020. A Cautionary Tale of Evaluating Identifying Assumptions: Did Reality TV Really Cause a Decline in Teenage Childbearing? *Journal of Business Economics and Statistics*, **38**(2), 317–326.

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

Jensen, Robert, & Oster, Emily. 2009. The Power of TV: Cable Television and Women's Status in India. *The Quarterly Journal of Economics*, **124**(3), 1057–1094.

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

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

Kelly, Morgan. 2019. *The Standard Errors of Persistence*. University College Dublin Working Paper No. 201913.

Klemp, Marc, & Weisdorf, Jacob. 2019. Fecundity, Fertility and The Formation of Human Capital. *The Economic Journal*, **129**(618), 925–960.

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

La Ferrara, Eliana, Chong, Alberto, & Duryea, Suzanne. 2012. Soap operas and fertility: Evidence from Brazil. *American Economic Journal: Applied Economics*, **4**(4), 1–31.

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

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

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

McInnis, Martin. 2000. *A Population History of North America*. Cambridge, UK: Cambridge University Press. Chap. 9. The Population of Canada in the Nineteenth Century, pages 371–432.

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

McQuillan, Kevin. 2004. When Does Religion Influence Fertility? *Population and Development Review*, **30**(1), 25–56.

Mitchell, B. R. 2003. *International Historical Statistics, Europe 1750-2000*. Hampshire, England: Palgrave Macmillan.

Munshi, K, & Myaux, J. 2006. Social norms and the fertility transition. *Journal of Development Economics*, **80**, 1–38.

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

Nunn, N, & Wantchekon, L. 2011. The Slave Trade and the Origins of Mistrust in Africa. *American Economic Review*, **101**, 3221–3252.

Oster, Emily. 2019. Unobservable selection and coefficient stability: Theory and evidence. *Journal of Business & Economic Statistics*, **37**(2), 187–204.

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

Spolaore, Enrico, & Wacziarg, Romain. Forthcoming. Fertility and Modernity. *Economic Journal*.

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

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

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

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

Williams, Kevin. 2010. *Read All About It! A History of the British Newspaper*. London: Routledge.

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

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

Woods, Robert. 2000. *The Demography of Victorian England and Wales*. Cambridge, UK: Cambridge University Press.

Wrigley, E.A., & Schofield, R.S. 1981. *The Population History of England 1541-1871*. Cambridge: Cambridge University Press.

Young, HP. 2015. The Evolution of Social Norms. *Annual Review of Economics*, **7**, 359–87.

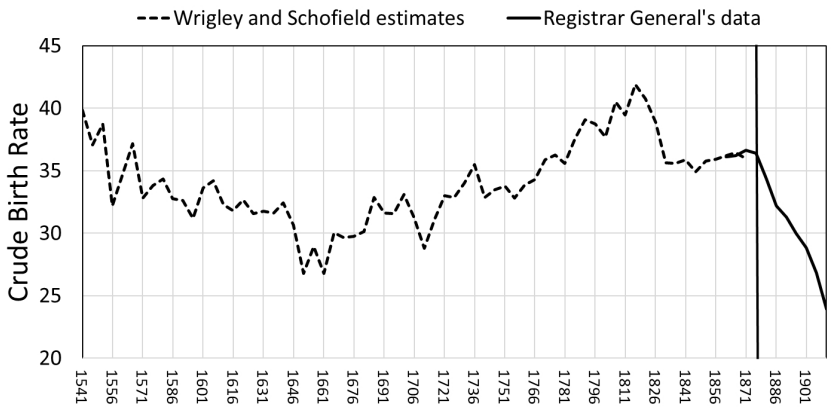
5 Appendix

5.1 Longer fertility time series for England & Wales

Figure 7 presents the crude birth rate in England and Wales from 1541-1915. The dotted line in this figure presents estimates for England from Wrigley & Schofield (1981, Appendix Table A3.1). The solid line is calculated using Census population data and birth data from the Registrar General's annual reports.[63] 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 1877 (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 1877, 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 preceded 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.

[63] 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 7: Fertility in England and Wales, 1541-1915

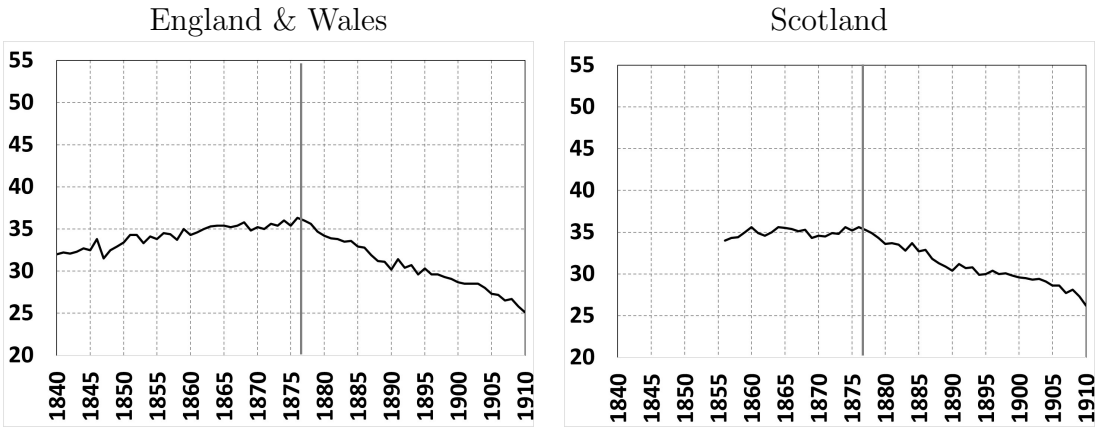


5.2 Comparing Britain with other European countries

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 8 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 in 1877 characterized by a sharp decline leading to a reduction equal to about five births per thousand by 1890 and roughly ten births per thousand by 1900.

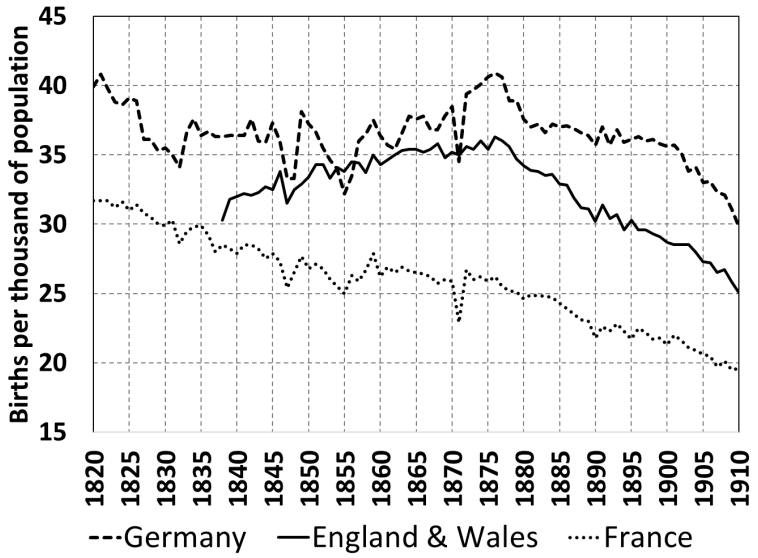
Figure 8: Fertility in England, Wales and Scotland



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

Figure 9 compares the pattern observed in England & Wales to France and Germany, the most comparable large and more industrialized European countries during this period. We can see that even by 1820 France had much lower fertility than the others, and French fertility continued to consistently decline throughout the nineteenth century. England & Wales represents an intermediate case. Before 1877, fertility was just a bit below Germany, but after 1877 fertility declined sharply. Germany is at the other extreme. While the country experienced a brief baby boom in the 1870s, following victory over France in 1870 and unification in 1871 (France also shows a decline in births during the war), the birth rate in the 1880s and 1890s was almost exactly the same as the level experienced from 1830-1870, at just over 35 births per person. Only after 1900 do we observe the onset of a sustained (and rapid) fertility decline in Germany. Thus, England & Wales represents an intermediate case when compared to either France or Germany. As discussed below, the timing of the onset of the decline in Britain looks closer to the European average, though the sharpness of the decline is unusual.

Figure 9: Comparing fertility in England & Wales, France, and Germany

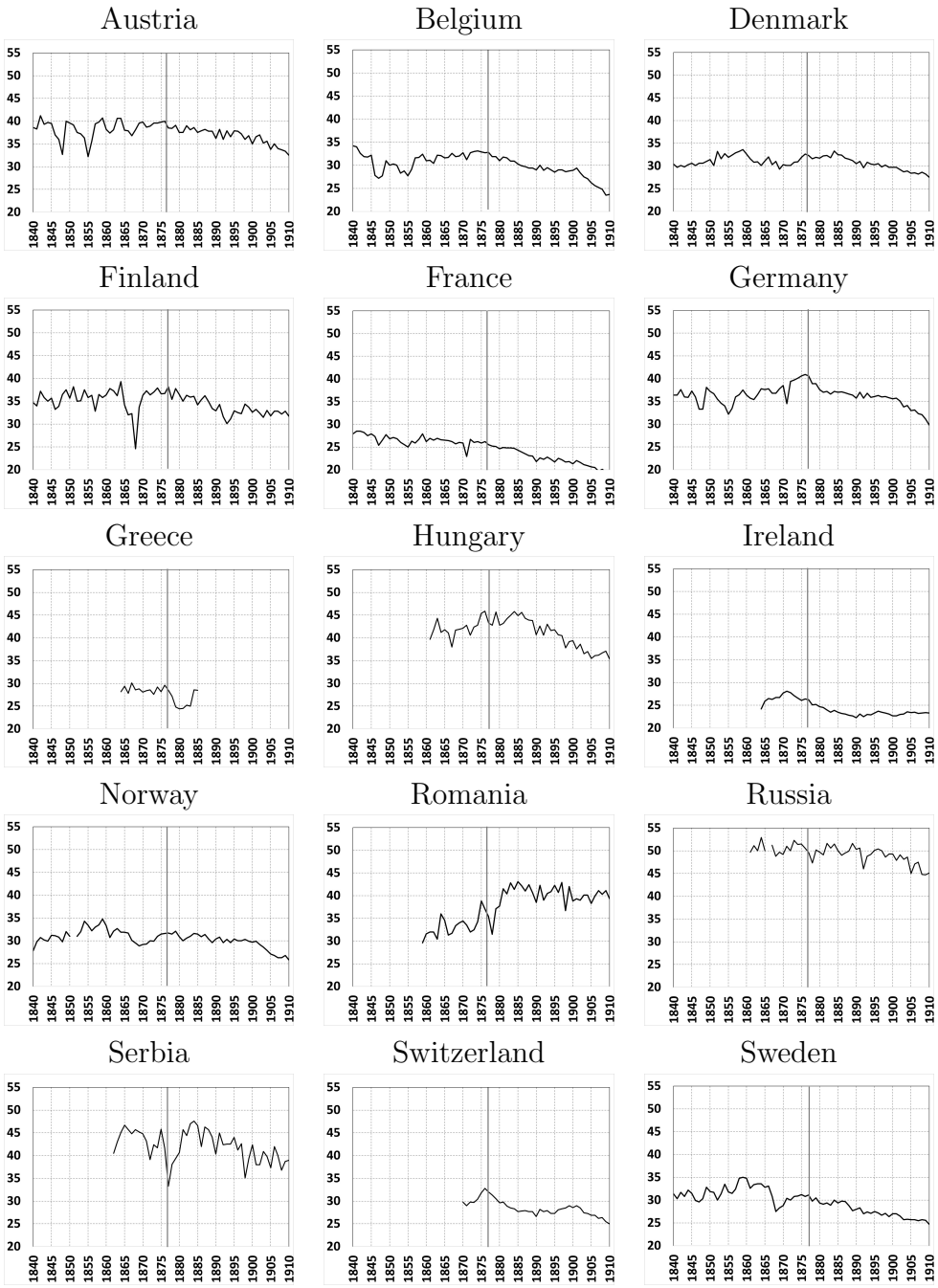


Births per thousand of total population. Data from Mitchell (2003).

Figure 10 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, though many countries do exhibit fertility declines starting sometime in the second half of the nineteenth century. However, this pattern was not universal, and a number of countries, such as Germany, Russia, and Romania, do not show striking declines until after 1900.

A few patterns in these graphs are worth remarking on. Beyond France, the other country that shows very low fertility across all observations is Ireland, where fertility remained low in the wake of the Great Famine of the 1840s. Another interesting pattern is the sharp reduction in fertility in Scandinavia (except Denmark) in the late 1860s, following the famine of 1867-69. This is followed by very slow fertility declines. 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 10: Comparing fertility patterns in major European countries



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

5.3 Appendix to the analysis of Canada

5.3.1 Inferring familial relationships in the 1881 Microdata

In order to implement our microdata analysis, we have to overcome the fact that the Canadian census did not record familial relationships. While we observe all members of a household, as well as key features of each member of the household (e.g., surname, age, marital status), we have to make assumptions about whether two married individuals are in fact married to each other or whether a young child is being observed with his or her mother. The latter of these relationships is crucial for our analysis.

Our process for identifying children and spouses proceeds in several steps. We begin by throwing out any household with incomplete information (i.e., if any member of the household is missing their age, sex, ethnic origin, surname, or marital status), because these pieces of information will be crucial for identifying familial relationships. We also discard households with suspicious information: e.g., an individual that appears as being married but is under the age of 13. Finally, we discard any household with more than 16 members (the 99.5 percentile).

Our goal is to construct a data set that identifies, for every fertile-aged woman whether she had any children and if so when those children were born. While some households are too complicated, we ultimately identify fertility histories for about 96% of the households containing a fertile-aged woman that will be relevant for our analysis. Below is a brief description of how those fertility histories are constructed.

A relatively straightforward household is one where every member of the household shares the same surname, there is one married man, one married woman, and all other members of the household report never being married. In this situation, we can assume that the man and woman are married to each other. We then assume that any unmarried women in the household have also never had any children. Finally, we assume that the the remaining "never married" individuals are potential children, which we attach to the married woman, so long as the inferred mother's age when the potential child is born is between the ages of 13 and 50.

Slightly more complicated households might include individuals with more than one surname. This can arise in multi-generational households, when a household contains boarders, or in some French households we may observe husbands and wives with different surnames because it was less common for French women to change their

surname after marriage. To deal with these situations, we remove obvious boarders, and then if there is only one married man and one married woman, we assume the couple is married to each other, regardless of whether the surnames match.

5.3.2 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 139 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% were of French origin.

Table 7 presents summary statistics for the data used in the individual-level analysis in Canada, which are taken from the 1881 Census. We can see that the women in the microdata sample had about 129 births per thousand women per year in this sample. It is not surprising that this figure is smaller than the rate in the aggregate analysis, since this analysis will miss some children who ended up in institutional settings such as orphanages. The share of British mothers, at 0.62, is also higher than the share in the aggregate analysis. This is largely due to shifting the year of observation from 1861 to 1881, a reflection of the fact that Canada was receiving substantial numbers of British immigrants during this period, but very few new Francophones.

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)	139.407	43.374	64.626	438.152	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
Church of Eng/Scot. share, 1861	0.156	0.136	0	0.461	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
Over 20 illiterate share, 1871	0.269	0.175	0.03	0.589	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 (1881)

Variable	Mean	Std. Dev.	Min.	Max.	N
Births per 1000 women per year	128.87	193.598	0	3333.333	1,525,296
British Mother	0.622	0.485	0	1	1,525,296
Mother's Age (start of period)	28.233	9.766	15	49	1,525,296

It is also useful to examine baseline differences between the British and French-origin Canadians in our analysis. To look at this, we turn to the 1881 census microdata, where we can identify the background of individuals as well as several informative individual characteristics. In particular, we look for differences in occupation across groups, as well as numeracy, which we measure based on age heaping using the Whipple index. Table 8 describes these differences for all native-born Canadians. We focus on native-born Canadians because that group is our primary interest, and so that our results are not influenced by migrant selection.

The first four columns in Table 8 look at occupational differences between the two groups. These show that British-origin Canadians were more likely to be professionals, clerks, or craftsmen, and less likely to be in agriculture, than other native-born Canadians (the vast majority of whom were of French-origin), but the differences are small. The last column looks at the Whipple index, which uses age heaping as an indicator of numeracy. A value of 100 in the index indicates no age heaping, while higher values indicate more age heaping, a sign of lower numeracy. For both British-origin and other Canadians we observe evidence of age heaping, but differences between the two groups were small. Overall, these results suggest that, at the aggregate level, differences between our treated (British-origin) and control populations were not large.

Table 8: Differences between British and non-British origin native-born Canadians

	Occupation shares				Whipple
	Professionals	Clerks	Agriculture	Craftsmen	index
British	0.056	0.131	0.488	0.187	126.7
Non-British	0.052	0.100	0.506	0.162	125.2

5.3.3 Additional Canada county-level analysis results

Table 9 presents robustness results for our analysis of the Canadian aggregate data. 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 1878-80 period to just the 1864-70, which are similar because both infer fertility based on the number of younger children. Similarly, Column 2 compares just the 1881-1885 period to 1872-1877, where in both fertility is inferred based on somewhat older children. In both cases we see patterns consistent with those shown in our main results.

Column 3 presents results without weights. Column 4 shows that we still see significant relative effects if we confine our analysis only to the province of Quebec. In Column 5 we control for local schooling rates. In Column 6, we separate out native-born Canadians of British origin, immigrants from Britain, and other immigrants. The effect for first-generation British immigrants is somewhat larger but less precise than the effect for native-born Canadians with British ties. 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. The mean and standard deviations of the Catholic share variable are both just a bit smaller than the British origin share variable: the mean of the Catholic share is 0.39 compared to 0.46 for the British share and the standard deviations are, respectively, 0.36 and 0.39. Given this, the magnitude of the effects of British connections are fairly similar regardless of whether they are measured using British descent or religion.

Table 9: Robustness of Canadian county-level analysis

	DV is children born per 1000 fertile-aged women								
	1878-1880 against 1864-1870	1881-1885 against 1872-1877	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 × 1878-80 Period	-14.549**		-14.843***	-13.761**	-14.482*				
	(6.170)		(3.862)	(6.363)	(8.388)				
British-origin × 1881-85 Period		-13.017**							
		(6.292)							
First gen. British imm. shr. × 1878-80 Period						-20.656			
						(17.054)			
Native-born British shr. × 1878-80 Period						-17.826***			
						(6.267)			
Other first gen. imm. shr. × 1878-80 Period						43.167			
						(36.480)			
Non-French Share × 1878-80 Period							-14.575***		
							(3.693)		
Non-Catholic Share × 1878-80 Period								-15.967***	-13.504***
								(4.189)	(3.641)
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 county fertile female population in 1871 unless otherwise noted. All regressions 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 10 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 10 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).[64] 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 10 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.

[64]The average distance (centroid to centroid) from each county its nearest neighbor is about 30 km.

Table 10: 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. × 1878-80	-14.843*** (3.862)	-14.843*** (3.165)	-14.843*** (3.722)	-14.843*** (3.667)	-14.843*** (3.299)
Panel b: Bartlett Linear Decay					
	Baseline	25 km	50 km	100 km	200 km
British-origin shr. × 1878-80	-14.843*** (3.862)	-14.843*** (3.321)	-14.843*** (3.358)	-14.843*** (3.574)	-14.843*** (3.522)

*** 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.

5.3.4 Canada microdata robustness

Table 11 presents some robustness checks on the Canada individual-level analysis. The first three columns examine our results obtained from alternative pre-periods. All of these deliver qualitatively similar results, but the estimated magnitude does vary across the specification. One reason for this is that when we cut the pre-period at 1874 or 1875, the results are being influenced by the impact of age heaping at age 5, with children most likely being underreported at nearby ages (four and six). Since age heaping is differential across Anglophone and Francophone Canadians, this has the potential to substantially impact our results. This is one reason why we prefer to use a longer per-period starting in 1872.

In the fourth column, we include only women that we believe were married by the time of the 1881 census. This yields stronger results in terms of the number of children per thousand women per year, since it eliminates single women, a group that was very unlikely to have children during our study period.

In Column 4, we separately estimate the response among British-origin (but not

Irish) and Irish-origin women.[65] Both groups experienced fertility declines after 1877 relative to Francophone Canadians in the same location. It is interesting to see that the Irish-origin group shows stronger effect than the British-origin group. In interpreting this, it is useful to remember that most Irish immigrants in Canada were Protestants from Northern Ireland, a group that may have had strong cultural ties to Britain.

Columns 5 and 6 present results excluding any Irish women from the sample. Column 5 shows that this does not affect our main results. In Column 6, we show that we also find similar results using an alternative measure of British connection based on the non-Catholic share of the district population. We study this measure after dropping the Irish since we do not want to include Catholic Irish in the control group.

[65]Since, respondents were asked to report the country that best represents their ethnic origin, it is possible that some women reported "England" even though their lineage may have originated from Ireland.

Table 11: Robustness of Canada household-level difference-in-differences analysis

	DV: Children per year per woman /1000						
	Alt. Pre Periods						
	1874-1877	1874-1876	1875-1877	Inferred Married	Separating Irish	No Irish	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
British × Post 1877	-3.475***	-4.589***	-2.724***	-8.374***		-3.342***	
	(1.170)	(1.358)	(0.998)	(1.656)		(1.155)	
British (but not Irish) × Post 1877					-4.095***		
					(1.208)		
Irish × Post 1877					-6.830***		
					(1.253)		
Non-Catholic × Post 1877							-3.061*
							(1.774)
British ind.	Yes	Yes	Yes	Yes	Yes	Yes	
Irish ind.					Yes		
Non-Catholic ind.							Yes
County-by-Per. FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age-by-Period FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occ-by-Period FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,585,433	1,585,433	1,611,892	642,462	1,525,296	1,159,437	1,159,437

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by county. Age fixed effects are in 5-year bins. 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.

5.4 Appendix to the U.S. analysis

This appendix describes our analysis of fertility changes among British immigrants in the U.S. We begin by discussing the data used in the analysis, followed by the results. The analysis broadly follows the approach used to analyze the Canadian microdata, but there are some important differences due to differences in the information available in the two data sets, as well as complications introduced by the fact that our analysis of the U.S. data is more reliant on recent immigrants.

As in our analysis of the Canadian microdata, our U.S. analysis treats fertile-aged women as the unit of observation and examines relative changes in fertility before and after 1877. The U.S. census asked respondents to report their age at the time of enumeration (June 1, 1880). Our post-period fertility is based on 0 and 1 year olds, which spans births from June 2, 1878 to June 1, 1880. For the pre-period we omit 2 year olds (births between June 2, 1877 and May 31, 1878) given the substantial overlap between the break year (1877) and the post period (1878 and beyond). Thus, our pre-period uses 3, 4, 5, and 6 year olds. This covers births occurring between June 1, 1873 and May 31, 1877.

We identify cultural connections to Britain using information on the birthplace of each woman's parents, which are reported in the census. Specifically, we compare fertility patterns for women with two British-born parents (the treatment group) to those with two non-British European-born parents (the control group). Summary statistics for the data used on this analysis are presented in Table 12. Note that the baseline fertility rate (119 births per 1000 women per year) is somewhat smaller than what we observe in Canada (128 births per 1000 women per year).

Table 12: Summary statistics for U.S. household-level analysis

Variable	Mean	Std. Dev.	Min.	Max.	N
Births per 1000 women per year	119.637	207.923	0	2000	916,535
British Mother	0.236	0.425	0	1	916,535
Mother's Age (start of period)	22.789	7.214	15	49	916,535

The main challenge faced when analyzing the U.S. data is the large number of single women in the data, and in particular the sharp increase in single women observed across the 1870s. This pattern is described in Table 13. Recall that marriage

status is identified only in 1880, so changes in the shares of women in different groups is driven by women aging into or out of our sample (women aged 15-49 at the start of each period). There is a very large increase in the number of single women in the period starting in 1878. This is driven by a large demographic "bump" of young women aging into the fertile-aged group, a bump that is somewhat larger for other Europeans than for the British.[66] If we pool single and married women in the analysis, then this shift, together with the fact that single women are much less likely to have a child than those who were married by 1880, will drive our results. Motivated by these features of the U.S. data, we analyze single women and couples separately in the U.S. context. This avoids our results being driven by the shifts in the relative shares of single vs. married women in our treatment and control groups.

Table 13: Married vs. single women by origin and period

		Second-generation immigrant women aged 15-49 in the U.S.					
		British-origin			Other European-origin		
		Married	Single	Widow/other	Married	Single	Widow/other
Pre-period	Count:	65,518	21,383	5,061	184,989	63,995	8,777
(1873-1877)	Share:	0.71	0.23	0.05	0.72	0.25	0.03
Post-period	Count:	71,613	48,327	4742	218,129	215,138	8,863
(1878-1880)	Share:	0.57	0.39	0.04	0.49	0.49	0.02

A related issue is that, because we are focusing on the children of immigrants rather than a more stable population, the age distribution can vary substantially by immigrant group as a result of previous immigration waves. This is not simply a theoretical issue, as the age distribution of second-generation British-origin women is older than that of the other European-origin women, reflecting differential immigration patterns among the parents of the women we study in the decades before 1870. To account for this difference, all of our results include age-by-period fixed effects.

A final difference between the U.S. analysis and our analysis of Canada is that we exclude women with parents born in Ireland from both our treatment and our control groups. We treat the Irish differently in the U.S. because the Irish immigrant population differed in important ways from the Irish population in Canada. Most

[66]These features contrasts with the Canadian data, where we do not observe reports of single women with children and the number of single women is more stable across the study period.

importantly, Irish immigrants in the U.S. were more likely to be Catholics from the south, rather than Protestants from the north, as in Canada. Given this feature, it is natural to exclude Irish-origin women from our U.S. analysis.

Table 14 presents our U.S. results. Columns 1-3 focus on fertility among women who were married in the 1880 census. Column 4-6 look at changes among women who were unmarried in 1880. The results in the first three columns indicate that, among married women, there was a substantial decline in fertility, equal to between 2.5 and 3 children per woman per year. These results are a bit smaller than the most comparable results obtained from Canada, the estimated reduction of 5.2 children per women per year from Table 2, Column 5. However, these results are in general not statistically distinguishable. The fact that the estimated coefficients in the U.S. are somewhat smaller than in Canada may indicate that cultural ties were weaker, or that this group was less exposed to news about the Bradlaugh-Besant trial. However, we should be careful in comparing these results, because our U.S. analysis can only consider births up to 1880, while our Canadian analysis includes births out to early 1881. If it took time for fertility norms to shift, then this might also explain why we observe somewhat smaller results in the U.S. In Columns 4-6 we find only very weak evidence that births may have decreased among women who were unwed. This is not surprising given that women in this group have very few children.

Table 15 examines the robustness of our U.S. results to using alternative pre-period windows. We can see that the magnitude and statistical significance of the estimated coefficients bounces around depending on the pre-period window used, though the estimates are always negative and of an economically significant magnitude. One cause of this variation appears to be differential rates of age heaping around age 5. Another factor may be differential mortality rates across the groups for the age groups used to construct the pre-period. Given these patterns, we view the U.S. results as mainly providing qualitative support for our story, rather than precise quantitative estimates of magnitudes.

Table 14: US individual-level difference-in-differences analysis

	DV: Children born per woman per year (divided by 1000)					
	Married women (in 1880)			**Single women (in 1880)**		
	(1)	(2)	(3)	(4)	(5)	(6)
British ×	-2.813**	-2.938**	-2.458**	0.002	-0.115	-0.111
Post 1877	(1.289)	(1.221)	(1.200)	(0.149)	(0.152)	(0.152)
British ind.	Yes	Yes	Yes	Yes	Yes	Yes
Age-by-period FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes			Yes		
County-by-period FEs		Yes	Yes		Yes	Yes
Occ-by-period FEs			Yes			Yes
Observations	567,692	567,692	567,692	348,843	348,843	348,843

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by state. The post-1877 period includes children born in 1879-1880. The pre-1877 period includes children born from 1874-1877. We exclude 1878 because some of the children born in that year would have been conceived before the Bradlaugh-Besant trial and some would have been conceived after. Occupation fixed effects are based on 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. Treatment group is made up of women with both parents born in Britain. The control group is made of of women with both parents born in Europe (excluding Britain).

Table 15: Assessing Robustness to Alternative Pre-Period Windows

DV: Children born per woman per year (divided by 1000)				
Pre-Period Spans:	June 1873-June 1877	June 1873-June 1876-	June 1872-June 1877-	June 1872-June 1876-
	(1)	(2)	(3)	(4)
British ×	-2.458**	-3.606**	-1.410	-2.260
Born June 1878-June 1880	(1.200)	(1.537)	(1.065)	(1.370)
British ind.	Yes	Yes	Yes	Yes
Age-by-period FEs	Yes	Yes	Yes	Yes
County-by-period FEs	Yes	Yes	Yes	Yes
Occ-by-period FEs	Yes	Yes	Yes	Yes
Observations	567,692	567,692	547,768	547,768

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by state. The post-1877 period includes children born in 1879-1880. The pre-1877 period includes children born from 1874-1877. We exclude 1878 because some of the children born in that year would have been conceived before the Bradlaugh-Besant trial and some would have been conceived after. Occupation fixed effects are based on 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. Treatment group is made up of women with both parents born in Britain. The control group is made of of women with both parents born in Europe (excluding Britain).

5.5 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 γ_d reflects our district fixed

Table 16: 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 ϕ_t captures our period fixed effects. Summary statistics for the variables used in our analysis are presented in Table 16.

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 17. 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 17: Regression results for South Africa analysis

	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 18, 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 17. 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 18: 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 17, which includes controls for population density and literacy.

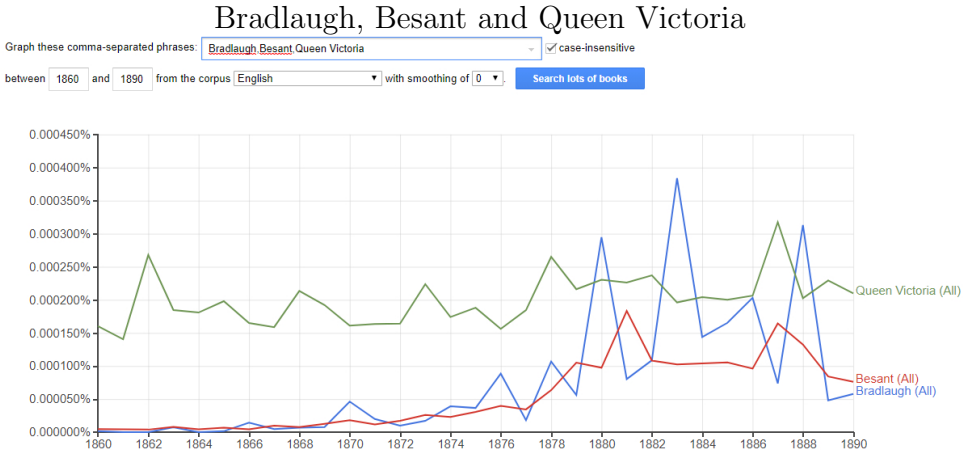
5.6 Additional evidence on the importance of the trial

5.6.1 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 11 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.

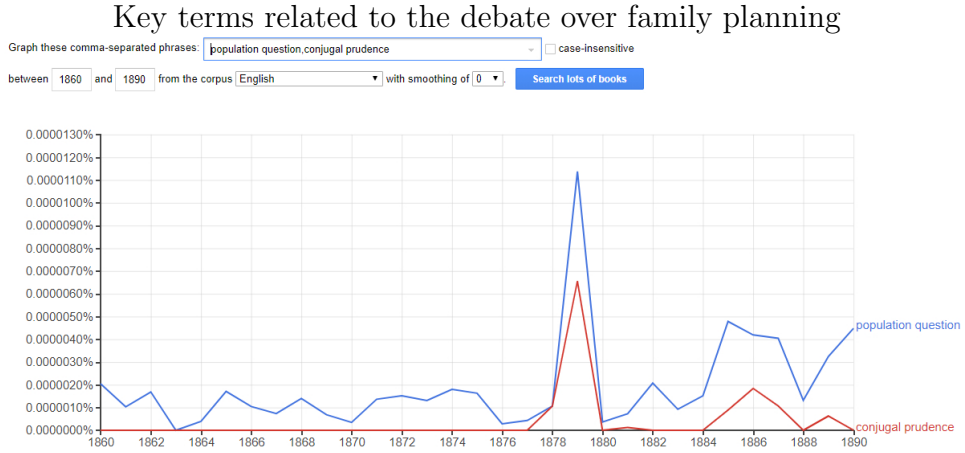
Figure 12 provides results from searches of two of the phrases most commonly used in the discussion of family planning during this period, "population question" and "conjugal prudence." For both terms we see a sharp increase right around the timing of the trial.

Figure 11: Google N-grams results: Key names



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

Figure 12: Google N-grams results: Family planning phrases



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

Google Ngrams can also be used to look at the popularity of Knowlton's book and how this was influenced by the trial. Figure 13 provides two plots of Ngrams. Both look for "Fruits of Philosophy", the title of the book published by Bradlaugh and Besant, with the top plot focused on sources written in British English and the bottom focused on American English. We have included a longer time-span in these plots in order to cover the period, during the 1830s, when the book was actually published. There are a couple of interesting patterns to note here. First, it is clear that the book was relatively unknown prior to 1877, though there are a few mentions of it in American English sources just after it was published (1832). Another notable feature is that the scale is much larger when looking at British English sources than American, which suggests that the impact of the trial was likely more pronounced among British readers than Americans.

Figure 13: Google N-grams results: Fruits of Philosophy

"Fruits of Philosophy" in British English sources

"Fruits of Philosophy" in American English sources

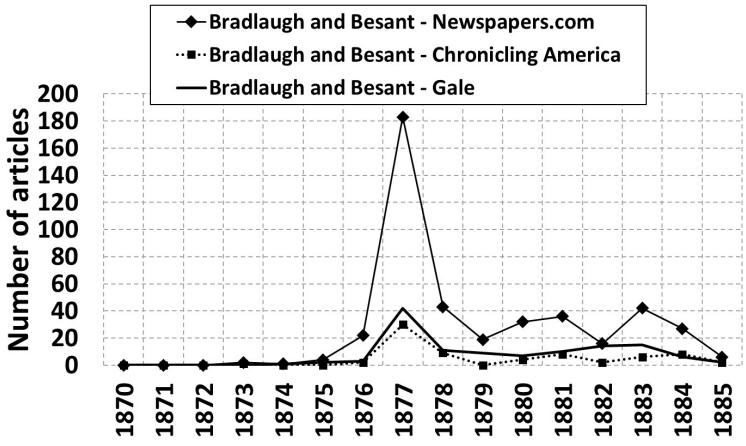
Results obtained from Google N-gram Viewer (https://books.google.com/ngrams) accessed on July 15, 2021.

5.6.2 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 14 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 14: Articles Mentioning Bradlaugh and Besant by Newspaper Database



Data obtained on August 20, 2020.

5.7 Appendix to the England and Wales analysis

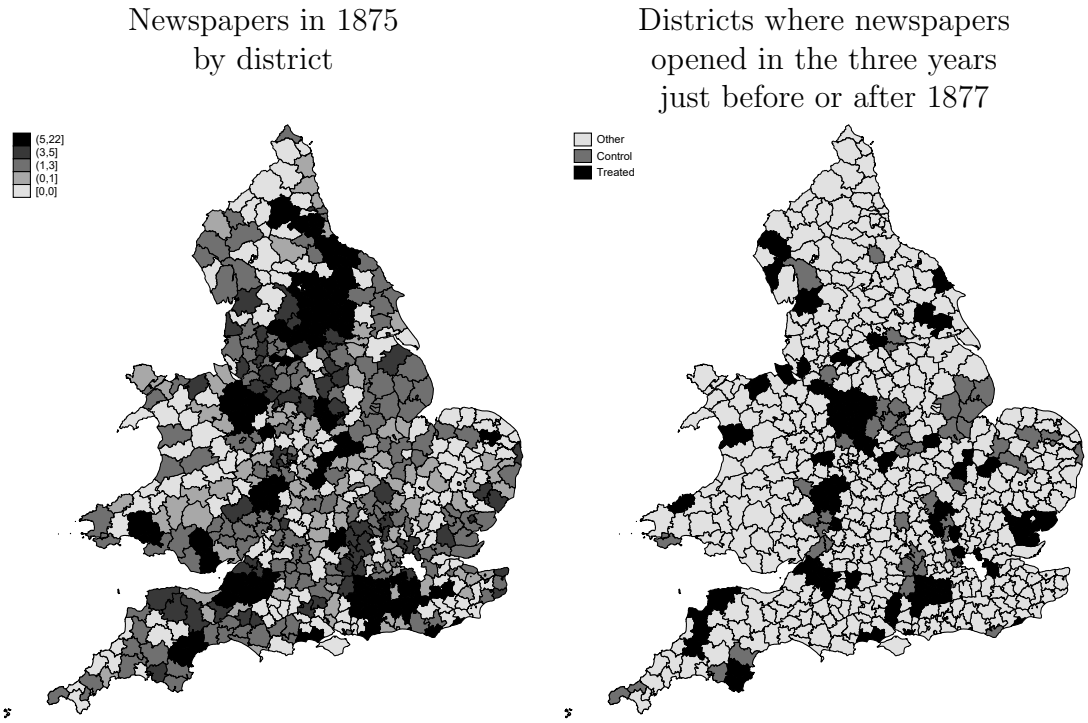
5.7.1 England and Wales newspaper directory data

The newspaper directory data used in the analysis of the impact of the Bradlaugh-Besant trial in England and Wales was digitized from the 1895 edition of the *Newspaper Press Directory*, published by C. Mitchell & Co. of London. The press directory provided a listing of newspapers in the U.K. for use by advertisers. For each newspaper, the directory includes the name of the paper, location, frequency and days of publication, cost, political view, date of establishment, a short description of the paper, and the paper's proprietor. We digitized a subset of this information–the paper name, location, year of establishment, and political view–for all of the provincial papers in England & Wales. These entries cover 1,479 papers in total, 1,378 in England and 101 in Wales. Of these, 136 were daily papers, 1,161 were weekly, 122 published two or three times per week, and the remainder published at less than weekly frequency or without frequency information. In terms of political affiliation, 409 papers were Liberal, 328 were Conservative, 298 were Independent, 244 were Neutral, and the remainder either specified a different affiliation, a compound affiliation (such as Liberal-Independent), or no affiliation at all.

A relatively small number of papers in the directory are not relevant for our analysis. Examples include sporting papers, entertainment papers, society papers, military papers, and commercial papers. We drop 39 of these papers from our analysis, which leaves us with 1,440 papers.

Figure 15 maps the newspapers used in our analysis. The left panel shows the density of newspapers in each district (excluding London) that had opened before 1875. The right panel maps those districts where a newspaper opened from 1875-1877, the treated districts in our analysis, and those where instead a new newspaper opened in 1878-1880, the control districts in our analysis. A useful feature to note in this graph is that our treatment and control districts are scattered throughout the country, without any strong geographic clustering.

Figure 15: Maps of the newspaper directory data



Data from the *Newspaper Press Directory* of 1895.

Table 19 examines the factors that predict whether a paper had surviving articles that appeared in the British Newspaper Archive in 1877.[67] Specifically, we run a search looking for the term "monday" in 1877 and look at whether a paper had any hits, and the number of article hits that appeared. The results in Column 1 show that papers published daily or several days a week were more likely than other papers (almost all of which were weekly) to have surviving articles in the BNA. Columns 2 and 3 show that papers affiliated with one of the two major political parties, the Liberal or Conservatives, were also more likely to show up in the BNA, most likely because they were more important papers. In Column 4, we compare Liberal and Conservative papers directly and find no statistically significant differences. Columns 5 and 6 look at the number of articles found in the BNA conditional on a paper appearing in the BNA at all. In Column 5, can see that both conservative and liberal papers tended to be larger than unaffiliated papers, and dailies tended to have

[67]More details on the British Newspaper Archive are provided in the following section.

many more articles than weeklies. However, we do not find statistically significant differences when liberal and conservative papers are compared to directly in Column 6.

Table 19: Predictors of appearing in the BNA

Dep var:	**Any articles in the BNA**				**No. of articles**	
Sample:	All papers	All papers	All papers	Liberal vs. Conser.	All papers	Liberal vs. Conser.
	(1)	(2)	(3)	(4)	(5)	(6)
Politics:						
Conservative		0.0859*	0.0724	-0.0224	188.1*	-180.4
		(0.0454)	(0.0455)	(0.0424)	(97.96)	(147.6)
Liberal		0.108**	0.0921**		159.4*	
		(0.0432)	(0.0430)		(93.68)	
Independent		-0.0155	-0.00943		102.1	
		(0.0472)	(0.0475)		(97.60)	
Frequency:						
Every day	0.185***		0.163***		2,016***	
	(0.0596)		(0.0594)		(238.7)	
Several days	0.205***		0.197***		152.0*	
	(0.0525)		(0.0519)		(88.84)	
Constant	0.295***	0.279***	0.250***	0.387***	571.4***	1,182***
	(0.0172)	(0.0326)	(0.0332)	(0.0283)	(66.25)	(104.3)
Observations	887	887	887	530	297	200
R-squared	0.028	0.012	0.036	0.001	0.510	0.007

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis. Sample includes all papers established by 1877.

More important for our study is whether certain types of papers were more likely to report on the trial. To examine this, in Table 20 we look at whether different types of papers had a greater share of articles on the trial relative to the total number of articles for the paper appearing in the BNA. These results indicate that papers of all types of political affiliation were roughly equally likely to report on the Bradlaugh-Besant trial. The non-partisan nature of the reporting on the trial may help explain why it had such a broad-based impact. The only factor that does seem to predict coverage of the trial was whether a paper was published at a daily frequency.

Table 20: Share of trial articles relative to all BNA articles

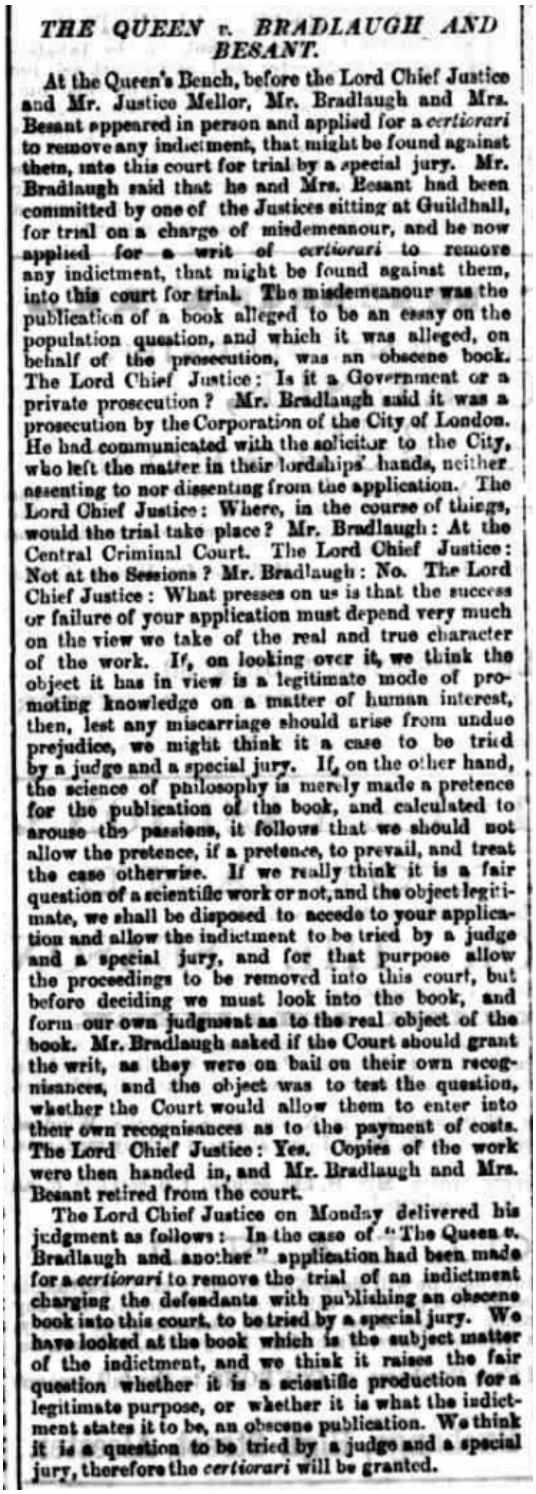
Dep var:	**Ratio of articles mentioning Bradlaugh or Besant to total BNA "monday" articles**			
Sample:	All papers	All papers	Liberal vs. Conser.	Liberal vs. Conser.
	(1)	(2)	(3)	(4)
Conservative	-0.000827	-0.000933	-0.000244	-0.000110
	(0.000830)	(0.000830)	(0.000562)	(0.000559)
Liberal	-0.000583	-0.000820		
	(0.000774)	(0.000789)		
Independent	-0.000650	-0.000769		
	(0.00101)	(0.00101)		
Every day		0.00133*		0.00125**
		(0.000687)		(0.000603)
Several days		-0.000525		0.000410
		(0.000637)		(0.000780)
Constant	0.00322***	0.00328***	0.00263***	0.00229***
	(0.000697)	(0.000705)	(0.000337)	(0.000377)
Observations	297	297	200	200
R-squared	0.004	0.019	0.001	0.015

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis. Sample includes all papers established by 1877.

5.7.2 England and Wales newspaper article data

The newspaper articles 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 is extremely rich relative to other historical newspaper article databes, containing millions of articles from papers throughout the country, including many of the most important provencial papers. Still, only a fraction of active papers are respresented in the database. Each article was digitized using a high-quality scanner. The text was then identified using optical character recognition. Figure 16 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.

Figure 16: Example article



THE QUEEN v. *BRADLAUGH AND BESANT.*

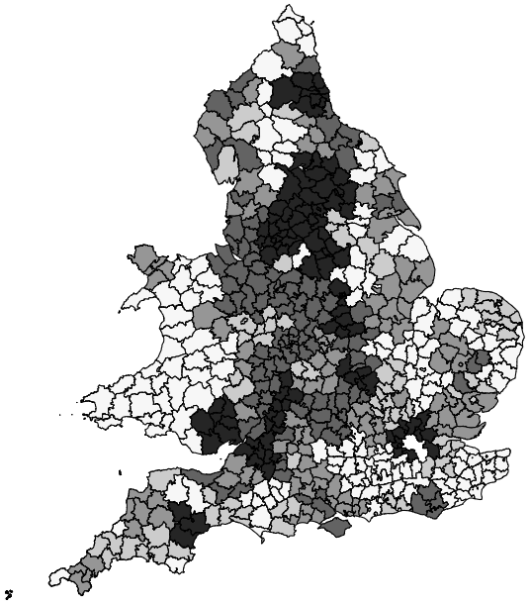
At the Queen's Bench, before the Lord Chief Justice and Mr. Justice Mellor, Mr. Bradlaugh and Mrs. Besant appeared in person and applied for a *certiorari* to remove any indictment, that might be found against them, into this court for trial by a special jury. Mr. Bradlaugh said that he and Mrs. Besant 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.

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 17 plots the spatial variation of our newspaper exposure variable.

Figure 17: Spatial distribution of newspaper articles about the trial



5.7.3 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 21 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 21. 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 Northampton, 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

Table 21: 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%

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).[68] 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.

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,

[68]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.

we begin by taking the raw text and removing any punctuation or common words.[69] 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 22 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.[70] 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 open up conversations about the topic of family planning.

[69]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."

[70]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.

Table 22: 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.

5.7.4 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.[71] Summary statistics for the key analysis and control variables at the district level are presented in Table 23.

[71]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 23: Summary statistics for England and Wales

Variable	Mean	Std. Dev.	Min.	Max.	N
Birth rate panel data (5yr averages)					
Birth Rates, 1868-1887	135.88	20.441	78.499	234.951	1720
Control Variables					
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
Share of Fertile Women Under 30, 1871	0.526	0.019	0.475	0.577	430
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

Table 24: Summary statistics for England and Wales (DD Sample)

Variable	Mean	Std. Dev.	Min.	Max.	N
Birth rate panel data (5yr averages)					
Birth Rates, 1868-1887	134.826	21.960	78.499	209.899	392
Control Variables					
Population density, 1871	3.666	14.425	0.085	107.392	98
Average mortality rate, 1871-1880	0.02	0.003	0.014	0.034	98
Average under 5 mortality rate, 1871-1880	0.055	0.015	0.034	0.119	98
Illegitimate birth share, 1873-1877	0.054	0.015	0.022	0.111	98
Manufacturing employment share, 1861	0.233	0.113	0.114	0.585	98
Agriculture employment share, 1861	0.19	0.11	0.008	0.463	98
Share of Fertile Women Under 30, 1871	0.531	0.016	0.488	0.571	98
Marriage Rate, 1873-1877	31.064	6.11	19.249	53.632	98
Shr. marriages at Registrar, 1873-1877	0.107	0.102	0	0.498	98
Shr. marriages Catholic church, 1873-1877	0.027	0.04	0	0.243	98
Minor shr. of marrying parties, 1873-1877	0.148	0.048	0.031	0.235	98
Illiterate shr. of marrying parties, 1873-1877	0.197	0.074	0.053	0.365	98
First marriage shr. 1873-1877	0.822	0.02	0.764	0.873	98

5.7.5 Results for alternative newspaper opening windows

Table 25 provides estimates when our sample is built using windows ranging from two to five years, with various controls added going down the panel in the same manner as our main results table. The main feature to note here is that our results become stronger as we focus on increasingly narrow windows, where, as we have shown, the treatment and control locations are more balanced. This pattern tells us that our results are unlikely to be due to underlying differences between treated and control locations.

Table 25: Main Dif-in-Dif results for Various Newspaper Opening Windows

DV is Births per 1000 fertile-aged women				
	5 Year	**4 Year**	**3 Year**	**2 Year**
	(1)	(2)	(3)	(4)
Baseline Estimates				
Newspaper Openings ×	-2.983*	-3.290*	-4.544**	-4.792**
1878-1882 Period	(1.676)	(1.757)	(1.859)	(2.080)
Main District Controls				
Newspaper Openings ×	-2.124***	-2.232***	-2.962***	-3.499***
1878-1882 Period	(0.705)	(0.745)	(0.853)	(1.021)
Established Newspaper Control				
Newspaper Openings ×	-1.790**	-1.889**	-2.598***	-3.242***
1878-1882 Period	(0.727)	(0.771)	(0.923)	(1.223)
Lagged Fertility Control				
Newspaper Openings ×	-1.718**	-1.850**	-2.673***	-3.167***
1878-1882 Period	(0.699)	(0.741)	(0.875)	(1.194)

*** 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 1871 district population. Birth rates are defined as number of births per 1000 fertile aged women (ages 15-49). 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). Sample is restricted to the set of districts where a newspaper opened between 1875 and 1877 (treatment) and districts that did not have a newspaper open between 1875-1877 but did have a newspaper open between 1878 and 1880 (control). The controls in each panel are added cumulatively, so the second panel adds in our main controls for local demography, economic structure, and marriage patterns, the third panel adds to this our control for pre-existing newspapers (before 1871) and the fourth panel adds in a control for lagged fertility to ensure that our results are not being driven by mean reversion.

5.7.6 Additional robustness for England & Wales results

This appendix subsection presents some additional results for our analysis of fertility in England and Wales. One potential worry in our main analysis is that the standard errors may be influenced by spatial correlation. To examine this, Table 26 presents results for our preferred specification while allowing for spatially correlated standard errors over various lengths. Note that, to accommodate the implementation of spatial standard errors, these results are unweighted, so the baseline estimate differs somewhat from the weighted results presented in the main text. Overall, the results in Table 26 show that allowing spatial correlation results in smaller standard errors, suggesting that there was some negative spatial correlation in the data. Given this, in the main text we have decided to report the more conservative robust standard errors.

Table 26: Examining spatial autocorrelation in England and Wales Directory Results

DV is Children Born per 1000 Fertile-Aged Women					
	(1)	(2)	(3)	(4)	(5)
Panel a: Uniform Weights					
	Unweighted	25 km	50 km	100 km	200 km
Newspaper Openings (75-77) ×	-2.069**	-2.069***	-2.069***	-2.069***	-2.069***
1878-1882 Period	(1.036)	(0.697)	(0.579)	(0.729)	(0.548)
Panel b: Bartlett Linear Decay					
	Unweighted	25 km	50 km	100 km	200 km
Newspaper Openings (75-77) ×	-2.069**	-2.069***	-2.069***	-2.069***	-2.069***
1878-1882 Period	(1.036)	(0.691)	(0.645)	(0.653)	(0.661)

*** p<0.01, ** p<0.05, * p<0.1. Column 1 corresponds to our preferred specification (Column 4 of Table 4) 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).

Table 27 provides results from some additional robustness checks using the data from England & Wales. These regressions follow the approach shown in Table 4 and include all of the controls used in that table (as in Columns 4 and 5). For comparison purposes, Column 1 reproduces our baseline results as in Column 4 of Table 4.

Column 2 then adds in a control for the level of child labor force participation.

This variable is built by combining information on the distribution of occupations across districts in 1871, using census data and the share at the national level of workers under 20 in each occupation in order to construct an estimate of local child labor force participation rates. This is not an ideal measure, but it takes advantage of the fact that there were strong differences across occupations in the rates of child employment. Column 3 uses a similar approach to construct a control for female labor force participation.

In Column 4, we include a control that is meant to pick up "feminist" sentiment at the district level. This is based on articles between 1860 and 1869 mentioning "Bodichon." Barbara Leigh Smith Bodichon was a prominent leader in the campaign for women's rights during this period, and one with a sufficiently unique name for our purposes.

In Column 5, we include all three of these additional controls. Looking across all of these columns, we can see that our results are not substantially affected by any of the individual controls and are only slightly smaller when all three are included.

Table 27: Additional robustness results for England and Wales

	DV is Births per 1000 fertile-aged women				
	Baseline	With child labor force participation	With female labor force participation	With "Bodichon" control	Including all three
	(1)	(2)	(3)	(4)	(5)
Newspaper Opened (75-77) × 1878-1882 Period	-2.673*** (0.875)	-2.557*** (0.839)	-2.470*** (0.845)	-2.713*** (0.881)	-1.984** (0.785)
District FEs	Yes	Yes	Yes	Yes	Yes
Region-by-period FEs	Yes	Yes	Yes	Yes	Yes
Marriage controls	Yes	Yes	Yes	Yes	Yes
Other district controls	Yes	Yes	Yes	Yes	Yes
Est. Newspaper control			Yes	Yes	Yes
Lagged Fertility Control				Yes	Yes
No. districts	98	98	98	98	98
Observations	196	196	196	196	392
R-squared	0.704	0.724	0.710	0.706	0.756

*** 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 1871 district population. Birth rates are defined as number of births per 1000 fertile aged women (ages 15-49). These birth rates are averaged over five year windows (e.g., 1873-1877, the pre-trial period, and 1878-1882, the post-trial period). Sample is restricted to the set of districts where a newspaper opened between 1875 and 1877 (treatment) and districts that did not have a newspaper open between 1875-1877 but did have a newspaper open between 1878 and 1880 (control). 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. Established newspapers is the number of newspapers that opened up in the district before 1875.

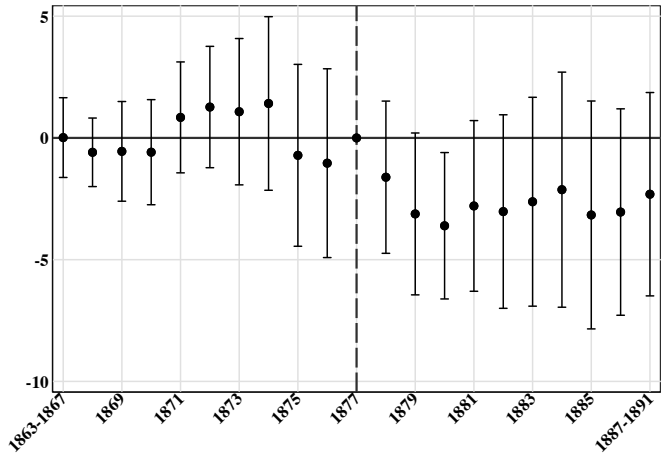
In our main analysis of England & Wales, we study fertility in five-year bins. That approach has the advantage that it smooths over some of the noise in annual birth rates, which vary substantially from year to year. However, an alternative approach is to study annual data and estimate effects for each year before and after the trial. The specification is,

$$BR_{dt} = \beta_0 + \sum_{t \neq 1877} \beta_{1t} EXPOSURE_d * YEAR_t + X_{dt}\lambda + \gamma_d + \phi_{rt} + \epsilon_{dt} \qquad (5)$$

where $YEAR_t$ is an indicator variable for year t and X_{dt} contains all of our control variables (except lagged fertility) interacted with a full set of year indicators. That includes all of our "Marriage controls," "District controls," as well as a control for the number of newspapers in the location prior to 1875.

The estimated β_{1t} coefficients and 95% confidence intervals obtained from this specification are presented in Figure 18. This results show that there were no obvious pre-trends in locations that were more exposed to the trial relative to less exposed locations, but that more exposed locations show consistently lower fertility in the post-1877 period.

Figure 18: Event study results for the England & Wales analysis



See text for additional details.

5.7.7 Marriage analysis

In England and Wales, it is possible to study changes in marriage patterns associated with the Bradlaugh-Besant trial using the comprehensive marriage registry data collected by the Registrar General's Office. We look for evidence of changing marriage patterns using exactly the same approach that we used when looking at fertility in Table 4. These results are in Table 28. The first four columns focus on the change in overall marriages per 1000 women aged 15-49, starting with our simplest specification and then adding in various controls.[72] These results show that locations that were more exposed to the newspapers in 1877 saw decreases in the number of marriages. In percentage terms, marriages in locations that received an additional newspaper were on average a bit more than 2% lower 1878-82 compared to 1873-77. The next three columns look at three aspects of heterogeneity that are observable in the data. Column 5 shows that we still find substantial effects if we focus only on first marriages. Column 6 shows a reduction in the rate of marriages involving women that were minors at the time of marriage. Column 7 points towards a decline in marriages involving women that were illiterate at the time of marriage, but the effect is not statistically significant.

Given the results in Table 28, it is natural to ask how much of the decline in fertility might be associated with this reduction in marriages. To provide an assessment of that effect, we begin by estimating the relationship between the fertility rate and the marriage rate, contemporaneously and with several lags, using annual data from the period before 1877. The resulting estimates, shown in Figure 19, indicate that the fertility rate shows a positive association with the contemporaneous marriage rate as well as the marriage rate in the past four years, with the strongest association with marriages in the previous year.

Using the estimated impact of exposure to the trial on marriages in Table 28 and the association between marriages and fertility shown in Figure 19 (including contemporaneous effects and impacts up to three years later) it is possible to construct counterfactual fertility eliminating the impact of the reduction in marriages. Figure 20 provides a sense of the impact of removing the marriage effect. This figure shows the

[72]Note that the age restrictions apply to the denominator, but not the numerator, since we do not have marriages broken down by age group. However, we expect the vast majority of marriages to occur among women in the 15-49 age group.

marriage rate in control districts, those where a newspaper opened just after 1877 but not just before, the true fertility rate in treated locations, where a newspaper opened just before 1877, and the counterfactual fertility rate in treated locations after 1877 where we have eliminated the impact of the trial, operating through marriages, on fertility. We can see that the impact of marriages is meaningful, but this channel does not explain the majority of the reduction in fertility rates.

If anything, the impact of marriages described in Figure 20 is likely to be an overstatement for two reasons. First, there is reason to believe that a non-trivial fraction of marriages were the result of an upcoming birth. We can see evidence of this in Figure 19, which shows that roughly one-third of the overall relationship between marriages and births across the four-year period showing significant effects is due to birth rates rising in the same year in which the marriage rate rose. This association seems too large to be due only to conceptions that occur rapidly following marriages taking place in the first quarter of the year, which suggests that many marriages took place after, rather than before, conception. A second reason our counterfactual provides an upper bound on the marriage effect is that, because it relies on the relationship between marriages and fertility estimated using data prior to 1876, it does not account for any reduction in the fertility rate within marriages associated with the trial.

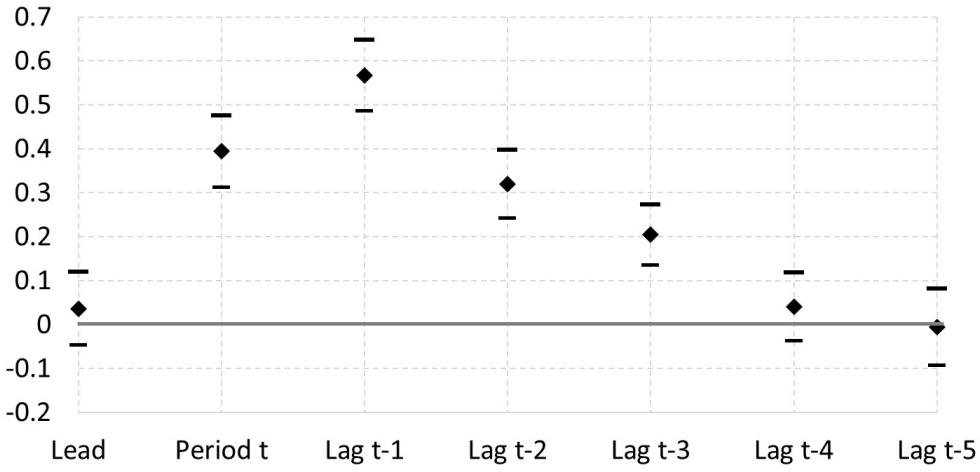
Another way to look at the effect of marriages on fertility is to compare regressions run using actual fertility rates, using the approach from our main analysis, to those run using counterfactual fertility rates where we have undone the marriage effect. These results are available in Table 29. The first two columns present our baseline regressions (as in Column 1 of Table 4) for actual and counterfactual births, while the third and fourth columns include our full set of district and marriage controls (as in Column 2 of Table 4). In both cases, we can see that eliminating the marriage effect reduces the fertility response in locations more exposed to news about the trial. However, the remaining response is still quite large and, when controls are included, strongly statistically significant. Comparing the magnitude of the estimates in Columns 1 and 2, or Columns 3 and 4, indicates that, as an upper bound, changes in marriage rates could have accounted for as much as 30-40% of the observed reduction in the fertility rate in treated locations. Thus, we can conclude that changes in marriage behavior were important, but that this channel also accounts for only part of the reduction in fertility associated with the Bradlaugh-Besant trial.

Table 28: Marriage results for England and Wales

	Marriages per 1000 fertile-aged women				First Mar. Rate	Minor Wife	Illiterate Wife
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Newspaper Opened (75-77) ×1878-1882 Period	-1.539** (0.704)	-0.952*** (0.358)	-0.899** (0.377)	-0.828** (0.347)	-0.711** (0.294)	-0.400** (0.166)	-0.160 (0.105)
District FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-period FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marriage controls		Yes	Yes	Yes	Yes	Yes	Yes
Other district cont.		Yes	Yes	Yes	Yes	Yes	Yes
Est. Newspaper cont.			Yes	Yes	Yes	Yes	Yes
Lagged Fert. cont.				Yes	Yes	Yes	Yes
Mean of DV	32.247	32.247	32.247	32.247	26.365	7.719	7.713
No. districts	98	98	98	98	98	98	98
Observations	196	196	196	196	196	196	196
R-squared	0.188	0.637	0.638	0.685	0.660	0.620	0.789

*** 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 1871 district population. Marriage rates are defined as number of marriages per 1000 fertile aged women (ages 15-49). These marriage rates are averaged over a five year windows (e.g., 1873-1877, the pre-trial period, and 1878-1882, the post-trial period). Sample is restricted to the set of districts where a newspaper opened between 1875 and 1877 (treatment) and districts that did not have a newspaper open between 1875-1877 but did have a newspaper open between 1878 and 1880 (control). 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 established newspapers control is the number of newspapers that existed in the district before 1875, interacted with a period indicator variable.

Figure 19: Relationship between fertility and marriage rates



This figure presents estimated coefficients and 95% confidence intervals, based on robust standard errors, for regressions in which fertility per thousand fertile-aged females is the dependent variable and leads and lags of the number of marriages per thousand fertile-aged females are the key explanatory variables. These regressions are run on annual district-level panel data from 1851-1876 with year and district fixed effects.

Figure 20: Counterfactual fertility rates eliminating the effect of fewer marriages

This figure presents the actual fertility rate (normalized so that 1876=1) in 'treated' districts, where a newspaper opened up in the three years before the trial, the actual fertility rate in 'control' districts, where a newspaper opened in the three years after the trial, and a counterfactual fertility rate in treated districts in which the impact of the change in the marriage rate operating through fertility has been eliminated. This counterfactual was constructed using the estimated reduction in marriages associated with treatment based on the results in Column 4 of Table 28 together with the estimated contemporaneous and lagged (up to t-3) relationship between marriages and fertility shown in Figure 19 to add back in an upper-bound estimate of the number of births averted in treated districts as a result of the reduction in marriages associated with exposure to the Bradlaugh-Besant trial.

Table 29: Comparing results with actual fertility and counterfactual fertility rates without marriage effects

	Actual fertility (1)	Counterfactual fertility (2)	Actual fertility (3)	Counterfactual fertility (4)
Newspaper Openings (75-77) ×	-3.329*	-2.312	-2.672***	-1.654**
1878-1882 Period	(1.998)	(1.997)	(0.745)	(0.745)
District fixed effects	Yes	Yes	Yes	Yes
Region-by-period fixed effects	Yes	Yes	Yes	Yes
Marriage controls			Yes	Yes
Other district controls			Yes	Yes
No. districts	98	98	98	98
Observations	196	196	196	196

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the district level, in parentheses. All regressions weighted by 1871 district population. Birth rates are defined as number of births per 1000 fertile aged women (ages 15-49). Counterfactual fertility rates eliminate the impact of the reduction in marriages on fertility in treated locations after 1877. Sample is restricted to the set of districts where a newspaper opened between 1875 and 1877 (treatment) and districts that did not have a newspaper open between 1875-1877 but did have a newspaper open between 1878 and 1880 (control). 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.