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The Causal Effects of Education on Age at Marriage and Marital Fertility

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The negative association of education and fertility, over time and between countries, is a central stylized fact of social science. Yet we have scant evidence on whether this is, or is not, causal. Using the universe of vital registration index data from England, 1912 to 2007, I first show that it is possible, using unique names, to construct a demographically and socioeconomically representative sample of 1.5 million women. Historical record linkage of women is typically not attempted but is possible here because of the unique characteristics of English civil registration. I then exploit the natural experiment of sharp discontinuities in who was affected by compulsory schooling law changes in 1947 and in 1972, which exogenously and effectively raised the minimum school leaving age. A Regression Discontinuity design, executed on the individual data, identifies the causal effect of education on age at marriage and fertility. Education may have raised age at marriage in 1972. However one extra year of education at 15 or 16 has a zero causal effect on marital fertility.

1. Introduction

A central stylized fact across the social sciences is the systematic negative association of education and fertility.[1] More educated mothers have smaller families.[2] The secular decline in fertility is also correlated with the rise of mass, compulsory school education in Europe.[3] The education–fertility correlation, both between and within countries, and over time, thus generates a popular conception that female education *causes* lower fertility.[4]

In demographic science, this education–fertility association is frequently treated as *causal*; both conceptually[5] and in specific practice: such as the population projections of

[1] "Almost universally, women with higher levels of education have fewer children, presumably because they want fewer and find better access to birth control" (Lutz & Samir 2011, p.588).
[2] See appendix figure A8 for a reproduction of global evidence on the education–fertility correlation, both between and within countries, 1950–2010. Appendix figure A10 reproduces the within country educational–fertility differences for the US and the UK.
[3] Appendix figure A10 illustrates the case for England. Caldwell (1980) outlines multiple mechanisms for the causal role of mass schooling in driving the global fertility transition.
[4] As summarized in appendix table A2, which reports recent pronouncements from well known news agencies.
[5] "There are many reasons to assume that these pervasive differentials are directly caused by education, which enhances access to information, changes the motivations for behavior, and empowers people to better pursue their own preferences" (Lutz & Samir 2011, p.588).

Samir et al. (2010), Vollset et al. (2021).[6] Despite its central, and critical importance to social science, there are remarkably few tests of whether the education–fertility relationship is *causal*.

This analysis exploits a well known natural experiment in exposure to education. The 1944 Education Act, also known as the Butler act, was a substantial reform to the provision of education in England (see Barber 1994). The act raised the minimum school leaving from 14 to 15, and was made law on April 1, 1947. Further, the act granted the Government the power to raise the school leaving age to 16, "as soon as the Minister is satisfied that it has become practicable" (His Majesty's Stationery Office 1944, p.30). This further increase was made law on the September 1st 1972. Evidence from surveys (as summarized in table A4) and contemporary attendance data suggest compliance with the new minimum school leaving age laws was high.[7]

The Act, and its effective enforcement on a specific date, means that I can compare the outcomes of those who are born very close together in a natural experiment style framework. Those who are exposed to the reform, and are forced by law to remain in school until they are 15 (1947), or 16 (1972), receive an exogenous education "treatment". I can then examine the effects on outcomes such as marriage age and fertility in a Regression Discontinuity (RD) econometric Design. Provided I specify the time trend correctly, I can then estimate the causal effect of the education treatment.

Why would education have any effect on fertility? The economic theory of fertility, according to (Becker 1960, 1991), applies the economic theory of consumer behavior to fertility decisions. If the number of children demanded is n, all other goods are represented by vector Z, and p_n, and π_Z represent prices, the budget constraint for a family with income I, can be represented as $p_n n + \pi_z Z = I$. The optimal bundle of n and Z will thus be related to the ratio p_n/π_z, the relative cost of children. Thus any factor that raises the shadow price of children, such as an increase in the value of a mother's time, will reduce the number of children demanded. Increased female education, by raising the opportunity costs of mother's time, reduces fertility by increasing p_n, the costs of a child.[8]

In the real world, mothers have to forecast the shadow price of children via the perceived value of their future labour income, and the non-monetary opportunity cost of their time.[9] So the fertility depressing effects of education could be powerful even if there is no actual "effect" on income per se. Rather it is the perception that matters. In this way education can reduce a woman's fertility even if it does not raise her wages. Of course, mothers' will dynamically update their "aspirations" but there will always be an assessment of the potential opportunity cost, in the rational Beckerian sense.[10]

[6] The estimates are highly cited alternative global population projections to the ones made by UNPD (Population Division of the Department of Economic and Social Affairs of the UN Secretariat), which do not account for education. These education adjusted population forecasts are the "most widely used in climate modeling" (Woods 2000, p.1287).

[7] See Clark & Cummins (2020a) for a description of this attendance data, which is sourced from the Annual Reports of the Board of Education.

[8] Note that this model does not include a parameter for child quality. It is the most fundamental version of Becker's economic theory of fertility with only one child cost. For a detailed empirical discussion of the quality–quantity model see Clark & Cummins (2018).

[9] The opportunity cost mechanism will also lead to increased fertility differentials between rich and poor, increasing inequality, and lowering growth (Croix & Doepke 2003).

[10] If education raises the ability to appreciate art, opera, or other such cultural goods that require some exposure and context to "appreciate", then the psychic cost of other goods is reduced (π_Z), which also raises the

While the theoretical focus has been on ultimate fertility, the potential role of education in delaying marriage is also important. Historically, marriage signified the beginning of a woman's fertility, as the vast majority of births were within marriage. Later marriage can mechanically lead to lower fertility due to the lower fecundity of older women. Education, by changing the opportunity costs of female time, and aspirations, will also potentially delay marriage.[11]

Do these theorized causal effects of schooling on age at marriage, and fertility, exist?

1.1 *The contribution*

This paper credibly estimates the causal effect of education on female age at marriage and fertility. To do this, I first form a sample of 1.5 million women linked from birth to marriage, and then to the birth records that correspond to that marriage.

Typically, the linkage of women across historical records is frustrated by the cultural tendency of women to change their surname upon marriage thus linking women across their life-course is typically not attempted in the literature.[12] Recently however, Price et al. (2021) have demonstrated new linking methods using family trees to complement and train the existing linking algorithms for historical censuses in the United States. Using these methods, Buckles et al. (2023) link millions of women across census records, 1840–1910, and estimate substantially higher intergenerational status correlations and levels of assortative mating, than the previous literature. Aizer et al. (2024) link 16,228 women who applied to the US "Mother's Pensions" welfare program between 1911 and 1930, to family trees from familySearch.org and census records, across their life-course. They find that these transfers did delay marriage but had no effect upon fertility.

For 20th century England in contrast, the inherent characteristics of the official vital records of births and marriages allow the successful linking of women across their life. Using the universe of individual vital registration records, 1912–2007, I first show that a large, representative sample of women can be formed by linking registration data using unique female names. A large minority, about 10%, of all English women have forename–surname combinations that only occur once 1837–2007. This is a function of both the relatively large number of English surnames and the cultural tendency for female children to be assigned relatively unique names.[13] For the economic historian, this allows the nominal linking across records in the absence of national ID numbers. This linking is also facilitated by the high level of population literacy, and spelling consistency, in official records in 20th century England.

relative cost of having children. (Even if the "appreciation" is simply conspicuous consumption, or some kind of signaling.) Basu (2002) reviews the empirical literature on the role of aspirations in reducing fertility, via the influence of education on exposure to mass media. Caldwell (1980) argues that education in the UK, by demonstrating middle-class values to working class students "destroyed a family system of morality that had made high fertility no disadvantage" (p.235).

[11] Higher relative female wages after the Black Death has been proposed as a mechanism for the European Marriage Pattern of late female marriage (Voigtländer & Voth 2013). Age at marriage has also been hypothesized to have effects upon marital stability (Becker et al. 1977).

[12] See for example (Abramitzky et al. 2012; Ferrie & Long 2013; Song et al. 2020).

[13] For example, I count 1,252,361 unique surnames, 29,112 unique male names, and 30,767 unique female names in the birth registers 1837–2007.

The method only allows me to measure the fertility of first marriages. However, due to the relatively low divorce and illegitimacy rates over the sample period, first marriage fertility accounts for the vast majority of overall fertility, on average, roughly 80%.[14]

I show that the linked samples are demographically representative. They match closely the levels and time-trends for the age at first marriage, fertility, and the age-specific fertility rates, of the general population, as reported by the Office for National Statistics (ONS). As the links are based *exclusively* on names, and not age, these comparisons serve as a robust test of the linking, and its representativeness. This is because the age patterns of marriage and fertility change dramatically over the sample period. Any significant linking bias will be immediately obvious from such time-series comparisons.

Further the socioeconomic status of those linked, as revealed by their surnames, also matches closely that of the general population.

I then use the implementation of compulsory schooling laws in 1947 and 1972 in a RD design to estimate the causal effect of education on age at first marriage and first marriage fertility. I find some evidence that the 1972 reform did raise marriage age and that this was causal. However, I precisely estimate a zero effect of education on fertility. Despite its theoretical importance, and its centrality in explanations for contemporary low fertility, education, in 20th century England, was not causal in reducing fertility rates. Both education and fertility were likely jointly determined by deeper, cultural factors.

1.2 Existing empirical evidence on the causal effect of education on fertility

Despite the centrality of the education–fertility dynamic to demography, there have been few published studies demonstrating a causal relationship. Here I critically assess the empirical base for the theorized education–fertility effect from the limited number of studies that exist. I report coefficient estimates with standard errors in parentheses, unless otherwise indicated.

For the developing World, there are no studies credibly identifying the causal effect of education on life-time fertility. However, existing studies typically report a negative causal effect of education on early-life fertility. For example, Osili & Long (2008) exploiting variation in the introduction of universal primary education in Nigeria estimate that increasing female education by one year reduces fertility before age 25 by 0.26 (t=2.28) births (p.57), with a sample average of 2.35. Keats (2018) estimate a similar effect for Uganda (p.148). Duflo et al. (2015) report a negative causal effect of an education subsidy on teen pregnancy rates for students in Kenya's Western Province. However this effect is statistically indistinguishable from zero in the seven year follow-up survey.[15]

[14] In the absence of precise reported data on the breakdown of fertility between different marriage orders, I make this claim based on the observed divorce rate over the sample period, roughly 2.5% in the 1950s and 1960s rising about 1970 to 12.5% to 2005, then declining after (Office for National Statistics 2015). The cohort illegitimacy rate is about 5% 1920 to 1950 and then rises to 25% by 1965 (Office for National Statistics 2008). Thus 75–95% of overall fertility is within marriage, and that roughly 87.5% to 97.5% of marriages over the sample period are first marriages. Taking the simple mid-point of these ranges implies that around 78% of all fertility is within the first marriage, over the sample period, on average. Here I use the crude equation $F^{m1}_{prop} \approx \bar{d} * (1 - \bar{ill})$ where the proportion of fertility attributable to first marriages (F^{m1}_{prop}) will approximately equal $\bar{d}$, the average period divorce rate 1950–2015, multiplied by one minus $\bar{ill}$, the average female cohort illegitimacy rate 1920 to 1963. See appendix figure A11 for a visualization of these two series over the sample period.

[15] "The point estimate of the impact of the stand-alone education subsidy on the probability of having ever started childbearing in the long-run follow-up is 3.2 percentage points...The p-value is just 0.12 however." (Duflo et al. 2015, p.2776).

For the developed World, there are several studies that credibly estimate the education–fertility causal effect. Currie & Moretti (2003) instrument for education using the local availability of colleges (US), and find a negative causal effect on fertility, a point estimate of -.09 (.01) of a child, relative to a sample mean of 2.4 (p.1517). Monstad et al. (2008) analyze the differential extension of mandatory schooling across Norway in a IV framework, 1960–72, and find strong evidence for a negative effect on teenage fertility. However, they find no evidence for a causal effect of education on life-time fertility (an IV estimated coefficient of -.009 (.087) with a sample mean of 2.04 (p.844).

For early life fertility, there are more studies; Black et al. (2008) exploit schooling law changes in the US and Norway and estimate strong casual effects of education on teenage fertility. They distinguish two education effects on fertility. Firstly, an "incarceration effect", where the legal compulsion to attend school reduces the time available to get pregnant. Secondly, a "human capital" effect, in line with Becker's economic theory of fertility, outlined above. Black et al. (2008) interpret their evidence as being suggestive of a "human capital effect rather than an "incarceration" effect (p 828).[16] McCrary & Royer (2011) study school entry policies in California and Texas (US) that produce discontinuities in when a person can start school. They find no significant effect of education on the probability of being a mother (before age 32) in their samples (p.172).

Of high relevance to the study executed here are two existing studies based around the 1947 and 1972 reforms in England and Wales, those of Fort et al. (2016) and Geruso & Royer (2018).

Fort et al. (2016) use the *English Longitudinal Study of Aging* (ELSA) to form a sample of women, over 45, and their fertility. For those affected by the compulsory schooling reform in 1947, they estimate an IV coefficient of -.282 (.072) (p.1836), with a sample average of 2.0 children (this variable is censored at 4). In the same paper, they estimate almost precisely the same causal effect, but this time positive, for women living in Continental Europe, exposed to other schooling reforms (here they use data from the *Survey on Health, Aging, and Retirement in Europe*). The results for the UK rely on the precise estimate of the time trend before and after the 1947 reform, from 3,009 observations, split into 20 birth cohorts. Inspecting their figure 2, which reports the "Adjusted no. of biological children" over the sample time period, one can see that the time trend pre-reform (before year 0) is estimated with considerable noise. This noise could be driving a spurious "effect".[17]

This study is significantly higher powered than that of Fort et al. (2016) (whose $N = 3,009$ for the 1947 reform, whereas the $N = 187,075$ for the 1947 reform here, and $N = 271,324$ for 1972).[18]

Geruso & Royer (2018) use a RD design based around the 1972 schooling reform to estimate the causal effects of education upon multiple measures of fertility, abortion use, and partner characteristics. The data they use to estimate fertility are complete counts of births, by year and month of birth of the mother, and of the child. They find evidence that the schooling reform lowered birth rates at age 16, and raised the educational status of future partners. However, at all other ages, and for complete fertility, they find no effects. For complete fertility, ages 16–45, they estimate a causal reform effect of.0036 (.0058) (Geruso & Royer 2018, table 5, column 1, p. 57). This estimate is based upon cohort Total Fertility

[16] This is because they find effects on the probability of a teen birth at ages older than the age that the reform enforced teenagers to be in school (p.1045–6).

[17] I include a reproduction of Fort et al. (2016) figure 2, as appendix figure A12.

[18] These are the available observations for the RD analysis from tables A12 and A13.

Rates, calculated by summing the observed age specific fertility rates by cohort, for the entire population effected by the reform and is precisely the zero effect I estimate here for the 1972 reform.

This paper also contributes to related debates on the selection effects of education, as summarized by Caplan (2019), and to a large literature in labor economics exploiting compulsory schooling reforms to estimate the labor market returns to education.[19] This literature is a mix of studies that find large, positive causal effects of schooling on outcomes such as earnings and mortality, and others that find no effect on these same outcomes. A related paper, Clark & Cummins (2020a) discusses this, and argues that publication bias distorts this literature.

Section 2 discusses the data used and details the linking process, section 3 presents the methodology behind the causal identification claimed by this paper, and section 4 presents the results. Section 5 discusses the implications of these results and section 6 concludes.

2. Data

2.1 Registers of births, marriages, and deaths, 1837–2007

Since the 1st of July 1837, a National Civil Registration system has been active in England and Wales. The internet age has led to the mass digitization of these records by various groups interested in family history, and its free dissemination, and they have posted this information online. I compiled a database of 125,005,217 births 47,082,406 marriages, and 85,932,666 deaths, from 1837 to 2007, for England and Wales by downloading the individual index entries from two such websites: freebmd.com (1837–1980) and familysearch.org (1980–2007).[20] The number of records collected match precisely the expected number from official sources (see appendix section A.4).[21]

2.2 Linking

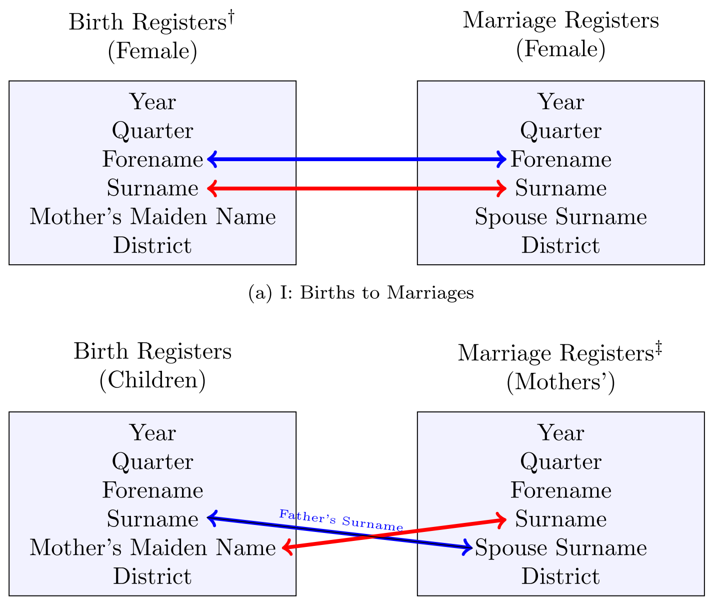
The UK has no nationally administered individual identification number so linking people between official records is more difficult than it is in the United States or the Nordic countries. As the marriage index used in this paper has no birth information, names are the only possible variable to link marriages to births. I link records using *unique* names. I define *unique* names as names of a given [first forename]–[surname] combination that only occur once in the 125 million individual birth register entries, 1837 to 2007. In order to minimize mistaken links across vital records, I use all available data to classify whether a female name is unique.

2.2 *Link I: Female Births → Marriages, age at first marriage.* Within the 125,005,217 birth records, I can identify 58,353,842 females, of whom 6,006,543 (10.3%) are the *only* female of

[19] For example, Angrist & Krueger (1991) on earnings (USA), Lleras-Muney (2005) on Mortality (USA), and Harmon & Walker (1995) and Oreopoulos (2006) on earnings (UK). See appendix table A3 for a summary of this literature.

[20] The collected marriage counts were divided by two because there are two index entries (one for the bride, one for the groom) for every one marriage.

[21] Birth registration quality, from 1831–1901, is assessed in Woods (2000)(figure 2.2, page 42). In 1831, about 9% of births are missing, in 1851 about 2% of births are missing, and by 1901, there is no suggestion of any birth under-registration. For the birth cohorts examined in this paper (1926–40, and 1950–64), there is no reason to suspect any systematic or significant birth under-registration.



(a) I: Births to Marriages

(b) II: Births to Parental Marriages

Figure 1. Linking schema.
Notes: †unique female births based upon first forename and surname (only one occurrence of name 1838–2007). ‡unique marriages based upon surname and spouse surname (only one occurrence of union 1838–2007).

a given first forename–surname combination born 1837–2007, and are therefore defined as *unique*.[22] As we are linking these women to marriages 1912–2007, we take those 4,601,644 unique women who are born 1852–1991. This ensures that women can marry as late as 60 in 1912, and as early as 16 in 2007. These unique women are then linked to a subset of the marriage registers using an exact concordance of first forename and surname, as illustrated by figure 1a.

The subset of the 47,082,406 marriage records utilized in this link are those 3,476,792 records where the combination of groom and bride surname only occurs once 1838–2007. This will be a necessary step in order to track the couple's fertility in the birth records in Link II, described below. Linking between the unique female births and the unique marriage records results in 1,483,426 women matched from a birth record to a marriage record. The matching rates are thus 32% of unique female births (1852–1991) linked to a marriage and 43% of unique marriages (1912–2007) linked to a female birth. Table 1 reports the sample sizes for each stage of the linking process. As with census matching, death, migration, and name changes will factor in here. But a direct comparison of linking rates with the census matching literature is difficult for three reasons. Firstly, marriage is a choice, and not universal. Therefore we should not expect to match all women. Secondly, the link executed here between unique women *and* unique marriages will be mechanically reduced by those unique women who marry men with more common names like Smith or Clark, and thus will not appear in

[22] Gender is assigned via forename, through the R package Gender.

Table 1. *Linking counts.*

Link	Data	Slice	N
	Births	All	125,005,217
		Female	58,353,842
		Unique Female	6,006,543
		Unique Female, 1852–1988	4,601,644
	Marriages	All	47,082,406
		After 1912	32,267,176
		Unique 1st, 1912–2007	3,476,792
I	B - M	Linked	1,483,426
II	M - B	Unique Marriages, Linked to Fertility	1,253,061

Notes: B = Births, M = Marriages. Unique female Births are those with one occurrence of a forename–surname combination, 1838–2007. Unique 1st marriages are those with one occurrence of a surname–spouse surname combination, 1912–2007.

the unique marriages set. Thirdly, the marriage data end in 2007 and many women born later in the 20th century will not have married by 2007. With these caveats we can assess the 32% overall matching rate here as in the mid range of the matching estimates reported by Bailey et al. (2020, Table 4, p.1019). However, for the Regression discontinuity estimate we will only take those women born around the effective birth cohort of the education reform. For the years 1926–1940, the match rate is significantly higher at 57% (223,579 of 391,177 unique births matched to a unique marriage), and for 1950–1964, 50% are matched (315,228 from 632,703). These rates are at the higher end of those typically found in the census matching literature (Bailey et al. 2020, Table 4, p.1019).

Appendix figure A15 reports the linkage rate between female unique births, and a unique marriage, by year, 1912–2007. As this link is purely executed on unique names, and not on age or any date information, variation in the linkage rate, by year of birth serves as a sanity check on the linkage. The vital records collected here go to 2007, and the severe decline in those linked after 1970 supports the veracity of the unique linkage process. If the linkage were completely random, the proportion linked would be flat throughout 1912–2007.

In appendix section A.11 I assess the accuracy of the links by comparing women who appear in the register data an also appear in a independently constructed genealogical database, the *Families of England* (FOE) Database of Gregory Clark and Neil Cummins. I find that over 98% of the links here are confirmed by the genealogical data. A false match rate of 2% compares very favorably with that typically found in the census linking literature, which are in the range of 20–40%. The false match rate here is in the range of hand-linked false match rates in the census literature (0–4%) (Bailey et al. 2020, Table 1 p.1012).

2.2.1 *Calculating age at first marriage.* The linked birth–marriage data generate an estimated age at first marriage, calculated as the difference between estimated "precise" date of birth and estimated "precise" date of first marriage. This age at first marriage, Age^M is simply

$$Age^M = M^D - B^D \tag{1}$$

The resulting ages were inspected, and ages under the legal minimum were dropped from analysis (under 12 before 1930, under 16 after 1930). Age at first marriage 60 and over were also dropped.

Figure 2a compares these estimates from the linked sample with existing estimates of the Singulate age at marriage from the Office for National Statistics (Office for National Statistics 2014a). Women in the linked data are slightly older brides than the average reported by the ONS, from 1900–1925. From 1925 to about 1975 they are very slightly younger, than after 1975 they are very slightly older. However, these differences are extremely small in absolute terms.[23] Note that the estimates will also refer to slightly different populations; those of the ONS refer to all first marriages in England, whereas the linked data refer to first marriages for those women born in England. These estimates will vary to the extent that immigrant brides differ from English born brides. A comparison of the median age at first marriage results in an even closer overlap, as reported in figure 2b.

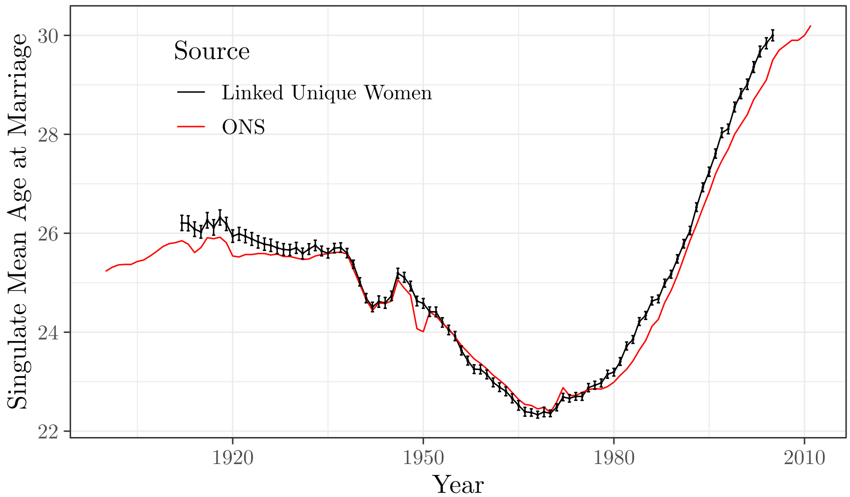
The comparison of the linked data with the ONS statistics serves as a strong test of the accuracy of the linking approach. If the unique matching between birth and marriage records was random, the estimated age at first marriage from this approach would not result in a time series that matched the official statistics. Recall, the only elements used in the linking are first-forename and surname; there is no age information in the marriage index to link to the birth index. Therefore the precise correspondence of ages at first marriage between these sources is a strong validation that the vast majority of the links are true links, and also that the sample is representative of the general population.

2.2.1 *Link II: Births → Parental Marriages, Marital Fertility.* To construct a sample of marital fertility I take all births and link them to the marriage of their parents. This is possible for "legitimate" (within wedlock) births because after 1912, the birth records record the surname of the child (which is typically the surname of the father), and the maiden name of the mother. Also after 1912, each marriage record records the surname of the spouse. Therefore each birth can be linked to the parental marriage record, after 1912. Here, I only match those marriage records that are unique combinations of mother's maiden name (surname in the female marriage record, as illustrated in figure 1b), and surname (which will be the father's surname, or the spouse's surname in the female marriage record). "Unique" combinations of parental surnames are those combinations that occur only once over the entire sample period, 1912–2007.

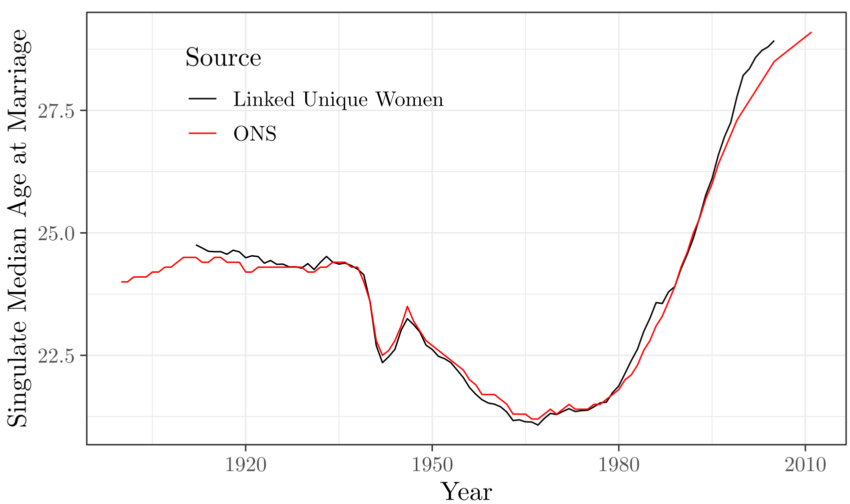
Starting from the set of 1,483,360 linked marriages from Link I above, I drop all those combinations of female surname and spouse surname that occur in more than one marriage record. This results in 1,252,992 unique marriage records 1912–2007. I then search the 70,231,570 birth records 1912–2007 and attribute to the unique marriages the count of births that match on birth surname (linked to the spouse surname from female marriage record), and mother's maiden name (linked to female surname from the marriage record).[24] This process is illustrated in figure 1b and the sample sizes are also reported in table 1.

[23] An OLS regression of age at first marriage of the annual estimates of the linked data (as dependent variable) on the ONS series (as independent variable) series yields a coefficient of 1.101 (standard error.012) with an R-squared of.989.

[24] Some of these "birth to marriage-of-parent" links resulted in implausible ages of mother at birth (such as under 12, or over 55). I drop these from the sample. I also dropped births that occur over 10 years before a marriage, mothers whose age-at-first-birth was over 50, and mothers who are attributed over 20 births. Some of these mistaken links occur, I speculate, as a result of births to couples who marry abroad but have births in England and Wales. The link mistakenly attributes these births to unique English marriages of the same parental surname combination.



(a) Mean Age



(b) Median Age

Figure 2. Singulate age at first marriage, linked sample v ONS.
Source: Linked Birth–Marriage data and (Office for National Statistics 2014a). 95% confidence intervals are plotted for the linked data (mean age at first marriage). As the links are based exclusively on names and not on any date information, the close correspondence of the levels, and the time trend, of the linked series and that from the ONS, strongly support the veracity of the linking process. If the links were completely random, the average for the linked women would be a flat line.

Again I can test the validity of this linking by comparing the results with official statistics from the ONS. First I compare the linked sample's childlessness rate with that from the ONS (Office for National Statistics 2013) in appendix figure A22a. The two series are not exactly comparable. The linked data represent estimates of childlessness for those linked to a first marriage, and relate to that marriage only, whereas the ONS estimates are for all women and all marital statuses. Further, the linked data childlessness rate is likely inflated by couples who marry in England and then emigrate and therefore do not have their complete fertility observed. As expected, the linked series does report higher childlessness rates than the ONS, as reported in appendix figure A22a.

To account for migration I adjust the observed linked childlessness rate by a factor of .895. The logic is that if the average migration rate in the second half of the 20th century is about .5% per annum (Sturge 2020), this will leave about 89.5% of those marrying in a given year still in England 21 years later. The value of 21 years is the approximate span from the average age at first marriage (24.05 from table A5), to age 45. This is a crude adjustment so that the two estimates of childlessness can be broadly compared.

As appendix figure A22a illustrates the adjusted childlessness series for the linked data is marginally closer in level and trend to that reported by the ONS. But the linked women have significantly higher childlessness rates. However, as there is not abrupt discontinuity in this, it will not complicate the RD analysis.

I next compare the age specific fertility rates of the linked sample with official data from the ONS. The ONS report cohort age specific fertility rates (Office for National Statistics 2020)[25] . However, the fertility rates from the linked sample are *marital* fertility rates. So I adjust the ONS cohort age specific marital fertile rates by ONS cohort measures of the proportion of women married by age (Office for National Statistics 2014b).[26] Next, I adjust the ONS data for the proportion of births that are outside marriage, by age (Office for National Statistics 2008).[27]

Age specific marital fertility rates (*ASFMR*) are then calculated from the ONS data as

$$ASMFR = (1 - ill)\frac{ASFR}{MR} \tag{2}$$

where *ASFR* are age specific fertility rates, adjusted by the proportion of births that are outside marriage (*ill*), and the marriage rate per 1,000 women (note that the *ASFR* is also measured per 1,000 women). The components of this calculation are reported in appendix figure A20.

Figure 3 plots the age specific marital fertility rates from the ONS, calculated as per equation 2, together with age specific fertility rates calculated for the linked fertility sample. The level and trend of all fertility series are closely aligned. However, the higher childlessness rate of the linked sample reflects in lower *ASFRs* for the 20–24, and 25–29, age groups, relative to that reported by the ONS.

Due to the concordance between the linked sample and official population level data, in both levels and trends, for first marriage age, childlessness, and age specific fertility rates, the linked samples are credibly representative of the demography characteristics of the general population.

[25] Table 2: “Cumulative fertility: Proportion of women who have had at least one live birth1, age, and year of birth of woman, 1920–2004”.

[26] Table 2: “Proportions of men and women who had ever married by certain ages, for birth cohorts, 1900–1995”.

[27] Table 10.4: “Components of average family size: occurrence within/outside marriage, birth order, mother’s year of birth, and age of mother at birth, 1920–1989”.

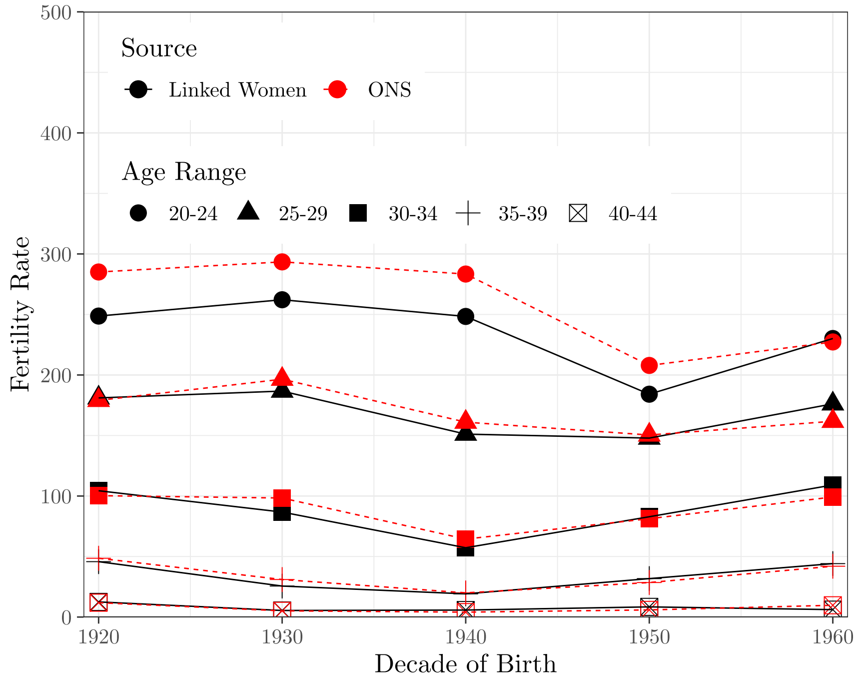


Figure 3. Age specific marital fertility rates, linked women compared with ONS statistics.
Notes: Fertility rates are age specific marital fertility rates per 1,000 women. Please see text and appendix figure A20 for details on how the ONS Age Specific Marital Fertility estimates were derived. An alternative comparison where age is located on the X-axis is reported in appendix figure A21. As the links are based exclusively on names and not on any date information, the close correspondence of the levels, and the time trend, of the linked series and that from the ONS, strongly support the veracity of the linking process. If the links were completely random, the average for the linked women would be a flat line.

2.3 *Socioeconomic representativeness*

How representative are the linked samples of the *socioeconomic* characteristics of the general population? Here I use surnames to infer a surname averaged status for the linked samples. English surnames are numerous; the birth registers used here report 1,255,490 distinct surnames. Outside the most common names, there is a long tail of less numerous names where surname averaging status provides meaningful information on the likely economic status of surname holders. Clark & Cummins (2015a) use surname averaging to measure social mobility rates in England from 1858 to 2012.

Using a complete digitization of all wealth passing at death in England, 1858–1992 (as detailed in Cummins 2021), I assign a measure of average wealth to every surname. I average all the individual level observations of (log) wealth across surnames, then normalize that measure to have mean zero and a standard deviation equal to one. The distribution of this surname wealth in the birth registers, 1930–60 (roughly the interval of birth of women analyzed here) is plotted in figure 4a for all women. Also plotted is the surname wealth distribution of all *unique* women. As mentioned in section 2, these are the 6,006,543 (10.4% of 58,353,842 female births) that form the universe of rare women that I attempt to link to age at marriage and fertility in this paper (they are the only individual of a given forename–surname combination born 1838–2007). The final linked samples, I and II, are almost identical to that of all *unique* women suggesting that the linkage process is not introducing biased selection.[28]

Next I inspect the status of the geographic location of the linked women with that of the general population. Every birth record is attached to a registration district. I geolocated these registration districts, 1838–2007, and matched them to one of the 340 modern equivalents. This allows the examination of the 2019 "Index of Multiple Deprivation" across the various samples (note that higher values on this index denote poorer average social conditions). The Index of Multiple Deprivation varies from a high of 45 in Blackpool, Lancashire to a low of 5.5 in Hampshire.[29] Again, the distributions for rare women, and for those women linked, are almost identical.[30]

Thus the economic information from surnames and locations, together with the close correspondence of marriage and fertility characteristics, suggest that the linked samples are both socioeconomically and demographically representative.

3. Regression discontinuity design

The paper's empirical strategy is to implement a Regression Discontinuity (RD) design.[31] The key characteristic of an RD design is that the probability of receiving a "treatment" changes discontinuously with respect to some variable (Hahn et al. 2001, p.201). The discontinuity I analyze is the imposition of compulsory education extensions, where the minimum legal school leaving age is raised, and it's discontinuous effect upon on students who are very close in age. When sorted by date of birth, the age a student ends education "jumps" at the effective date of the new law. This results in a discontinuity of total years of education, by date of birth. As the law only applied to students of a certain age at a certain date, students, nor their parents, could choose the "treatment". In this way, a RD design is similar to a randomized experiment; I can compare groups just before and just after the effective birth date of the education treatment.[32]

Before the 1st of April 1947, students who were aged 14 could leave school. After the 1st of April 1947, 14 year olds had to stay in school until they were at least 15. This means that

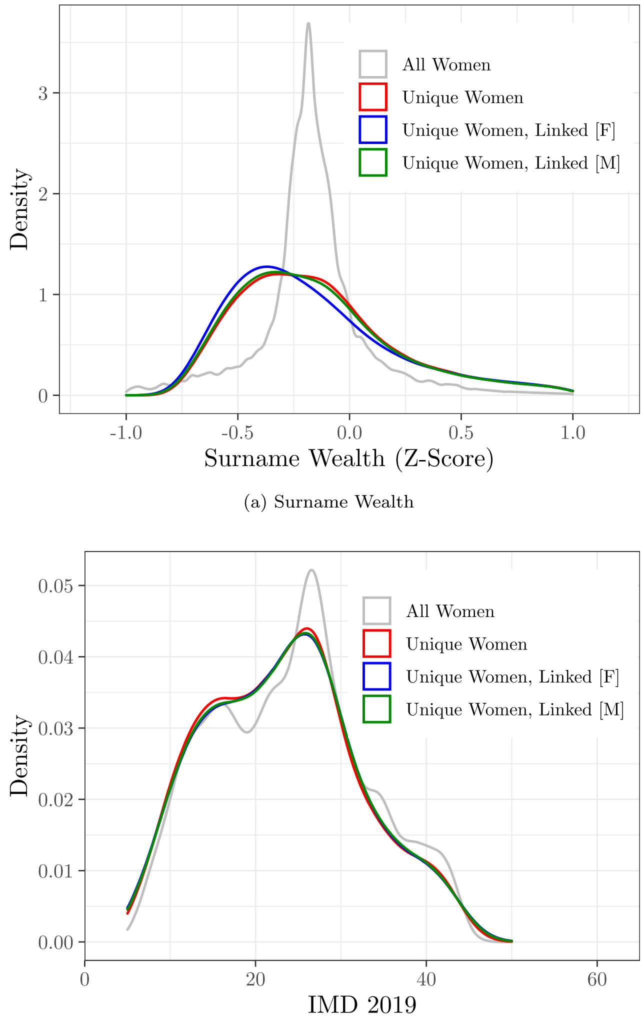
[28] The distribution of this measure across common surnames has low variance as these names do not convey useful socioeconomic information on their holders. For rarer names however, this measure captures substantial variance, as is evident from figure 4a. Rare surname grouped wealth-shares match those of the individual wealth data, see Cummins (2019).

[29] See figure A19 in the appendix for more detail on this matching.

[30] In appendix section A.10, I formally assess the selection effects of each linking stage using regression models. For surname wealth, there are small positive selection effects (.07–.09 of one standard deviation) for the sample of female unique names, relative to the complete population. This small effect is reversed when using the IMD score as the status outcome (an elasticity of -.4). Link I also is associated with a small negative selection coefficient (-.02–.03 of one standard deviation) for surname wealth but close to zero for IMD score. The selection effects for Link II are all very close to zero. The formal selection effect regressions support the assessment that the linked samples are generally representative of the population.

[31] I follow Lee & Lemieux (2010) and Cattaneo et al. (2020, 2021) who provide useful guides to the use of RD design models.

[32] Appendix figure A13 illustrates this concept by plotting a simulated version of equation 3 for some specified parameters.



(a) Surname Wealth

(b) Index of Multiple Deprivation (IMD) of District of Birth

Figure 4. Economic representativeness of the linked samples.
Notes: [M] denotes linked to first marriage, [F] denotes link to first marriage fertility. Panel (a) plots the distribution of surname averaged log wealth (from the complete Probate Registry data compiled by Cummins 2021) in the birth registers 1930–60. The distribution for all women is different to that of women with rare surnames because of the effects of averaging over large numbers of people. As individual wealth is non-normally distributed with a small proportion of wealth holders having extremely large estates at death, the surname averaged wealth for names such as *Smith* and *Clark* will have less information on the holders than rarer surnames. Thus the tails of common surname averaged wealth are smaller than rare surname averaged wealth, and the average is higher because of the fat upper tail of individual wealth holding. Panel (b): source for Index of Multiple Deprivation (2019): www.gov.uk. Section A.10 examines the selection effects of taking unique names, and of each of the linking stages in more detail.

someone born on the 31st of March 1933 could leave school at 14, while a person born a day later was required to stay a full extra year in full-time education. Similarly, the 1972 law change meant that someone born on the 31st August 1957 could leave education at 15 but someone born on the 1st September 1957 had to stay in school until they were 16. Administrative data suggest that observance of the new law was high (Clark & Cummins 2020a).

I denote the treatment of the education law as D. Where an individual's quarter of birth is before the date of birth that the law bound (such as 1st April 1933, or the 1st of September 1957), $D = 0$. Where the quarter of birth is on or after the effective date of the law, I set $D = 1$. I denote quarter of birth, our "running" variable, as X, and the outcome as Y, and estimate the treatment effect τ as

$$Y = \alpha + D\tau + X\beta + \varepsilon \tag{3}$$

where ε is a random error. In the specification of equation 3 I assume that there is no other reason for the discontinuity at date X and that the time trend is specified correctly (Lee & Lemieux 2010, p.287). Under these assumptions, I can interpret the treatment effect τ as causal.

As some students would have stayed in school, regardless of the minimum age, the effect of the law can only add significantly less than one year to average years of education, when comparing those treated versus non-treated. Further, observance of the new law could be less than perfect (but attendance data suggest 99–100% compliance; Clark & Cummins 2020a). For these reasons the RD design used here is not perfectly "sharp" (where the probability of receiving treatment D goes from $0 \rightarrow 1$ at date X) but "fuzzy" where the probability of receiving treatment goes from $0 \rightarrow < 1$. However the treatment effect of an extra year of education on outcomes can be recovered by dividing the treatment effect τ by estimates of the effect of the law change on realized years of eduction (τ_e).

Therefore it essential for this analysis to have estimates of the effect of the law changes on actual years of education. Table A4 summarizes estimates from the economics literature, using multiple datasets, of this "first-stage" effect for women. These are our best estimates of the treatment effect, τ_e, of the law changes on actual years of education (here denoted by subscript e) from equation 3 and allows us to scale the treatment effect τ of one extra year of education on outcome i by calculating $\tau = \frac{\tau_i}{\tau_e}$. However, as discussed in section 2, there is a delay between birth and birth registration. Thus I further scale the treatment effect by the observed proportion of births registered after the date of the discontinuity that are actually born after the discontinuity date, here denoted as r_w (reported in appendix table A7). Therefore the scaled treatment effect is calculated as

$$\tau = r_w * \frac{\tau_i}{\tau_e} \tag{4}$$

The average estimate from the literature of the treatment effect of the schooling reforms on total years of education is .535 for 1947 and .328 for 1972. To account for the delay in registering a birth I further adjust these treatment effects by the observed proportion of births are actually born after the treatment date. This adjustment depends on the length of the bandwidth of the RD regression (varying here from 12–28 quarters).[33]

I thus assign t_e as $\approx .514 - .525$ for the 1947 reform and $\approx .318 - .322$ for the 1972 reform.

3.1 Implementation

I estimate the treatment effect τ of outcome Y by running a pooled, linear regression of the form

$$Y = \alpha_l + \tau D + \beta_l(X - c) + (\beta_r - \beta_l)D(X - c) + \varepsilon \tag{5}$$

where X is birth quarter. This specification that allows the regression function to be separately estimates on both sides of the cutoff c by interacting birth quarter X with the treatment dummy D, and has the advantage of directly yielding the coefficient and standard error of the treatment effect τ.

For each education reform and outcome analyzed I first examine the visual evidence for a discontinuity at the cutoff date $X-c$. I then present estimates of equation 5 for each education reform. This regression form is flexible to allow the slope and coefficient to be different on both sides of the treatment date, which is in the spirit of the RD design as analogous to a randomized experiment (Lee & Lemieux 2010, p.319).[34]

The running variable, birth quarter, X, or more accurately registration-of-birth quarter, is a discrete variable taking one of four values for each birth year observed.[35] Standard RD estimates using such discrete running variables have been shown to lead to inconsistent treatment effects (Dong 2015, p.422). As noted by Lee & Card (2008), a discrete running variable makes it impossible to compare those born "just before" and those born "just after" the RD treatment cutoff.[36] The "standard practice" is to estimate parametric models using low polynomial transformations of the running variable with standard errors clustered on the discrete running variable (Dong 2015; Kolesár & Rothe 2018). [37] However, it has recently been shown that this clustering leads to incorrect confidence intervals for the estimated treatment effects (Kolesár & Rothe 2018).

Therefore, I also employ in section A.13 the standard error corrections proposed by (Kolesár & Rothe 2018), which take into account this possible bias, to construct "honest" confidence intervals for the estimated treatment effects using the `RDHonest` package in

[33] In practice this is small adjustment of.96–.98. See section 2, and appendix section A.5 for the rationale and more detail on this adjustment.

[34] In other words, I do not allow data from either side to be informative of the constant or slope of the other, which would be the case if I simply estimated equation 3.

[35] I make the assumption that actual birth dates period are "rounded down" to the nearest birth registration quarter and that within birth registration quarters, actual birth dates follow a uniform distribution.

[36] Lee and Card state "with an irreducible gap between the "control" observations just below the threshold and the "treatment" observations just above, the causal effect of the program is not even identified in the absence of a parametric assumption about this function" (Lee & Card 2008, p.656).

[37] Alternatively, if the number of discrete running variables is sufficiently high, continuous running variable RD methods can be used (Cattaneo et al. 2021, p.63).

R (Kolesár 2020).[38] Note that I can only construct these confidence intervals for local polynomial regressions, without controls.[39]

The empirical models control for linear and quadratic polynomials of the running variable only. The justification is based upon the striking linearity of our outcome variables with respect to our running variable, and is also in line with recent scholarship. Gelman & Imbens (2019) for example argue that higher order polynomials lead to noisy estimates, sensitivity to the degree of the polynomial employed, and incorrect confidence intervals.

Birth quarter dummies are included in all specifications to control for seasonal fixed effects (which are evident from the various RD plots in section 4). To have a balanced RD design, and to estimate a time trend, this means that the minimum bandwidth has to be over one year before and one year after the cutoff date, c.

The RD model is estimated for both linear and quadratic transformations of the running variable X, separately estimated for bandwidths of 3, 5, and 7 years, before and after, the effective date of the schooling extension. The optimal bandwidth is typically the bandwidth that minimizes the mean squared error, or its positive squared root, the residual standard error, which is reported for every RD regression.

4. Results

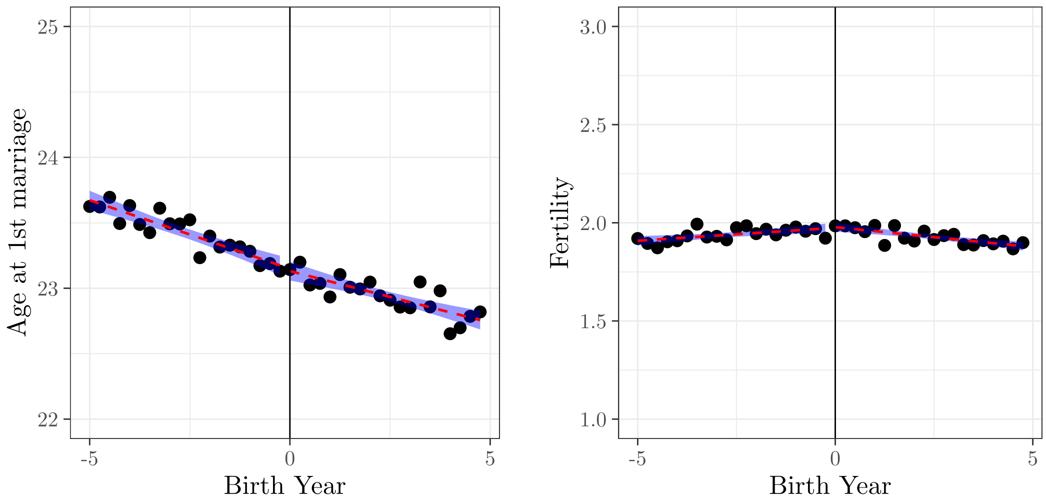
Appendix tables A5 and A6 report the summary statistics for the linked women, for the link to marriage, and the link to fertility, respectively. For age at first marriage, there are 1,483,360 observations. However, the available number of observations for each RD regression is restricted by the choice of bandwidth, and are reported separately in each regression table. Average age at first marriage is 24.83 (median is 23.18), for women born 1853–1989, and marrying 1912 to 2006. For first marriage fertility, there are 1,252,992 observations, with similar average (24.66) and median (23.14) age at first marriage as the age at first marriage sample (recall that the samples are slightly different). Average first marriage fertility is 2.17 children, with a range of 0–19. Age at first birth is on average 26.05, age at last birth is on average 30.14, and the average birth interval is 3.62 years.

Figure A23 reported in the appendix reports the linkage probability across the education discontinuity threshold date. There are no breaks in this probability, which suggests the linked data are suitable for the RD setup.

Figure 5 presents the four RD plots of the educational discontinuity by quarter of birth for age at marriage and first marriage fertility for both the 1947 and 1972 reform. Those born at, and after, the cutoff quarter zero, are exposed to the reform, where the minimum school leaving age was raised by one year. For every plot apart from figure 5 (c) there is no visual suggestion of any substantial change across the discontinuity. The time trend is apparently constant before and after the discontinuity. This is consistent with a zero causal effect of education on age at marriage and fertility. However, for the 1972 reform there is suggestive visual evidence of a small positive effect on age at first marriage. However, figure 5 reveals clear seasonal patterns in these outcomes. What are the magnitude of the RD estimates controlling for this seasonality?

[38] The discrete running variable problem is therefore dealt with; "...because the CIs explicitly take into account the possible bias of the estimators, the asymptotic approximation doesn't rely on the bandwidth to shrink to zero at a particular rate." (Kolesár 2020, p.3).

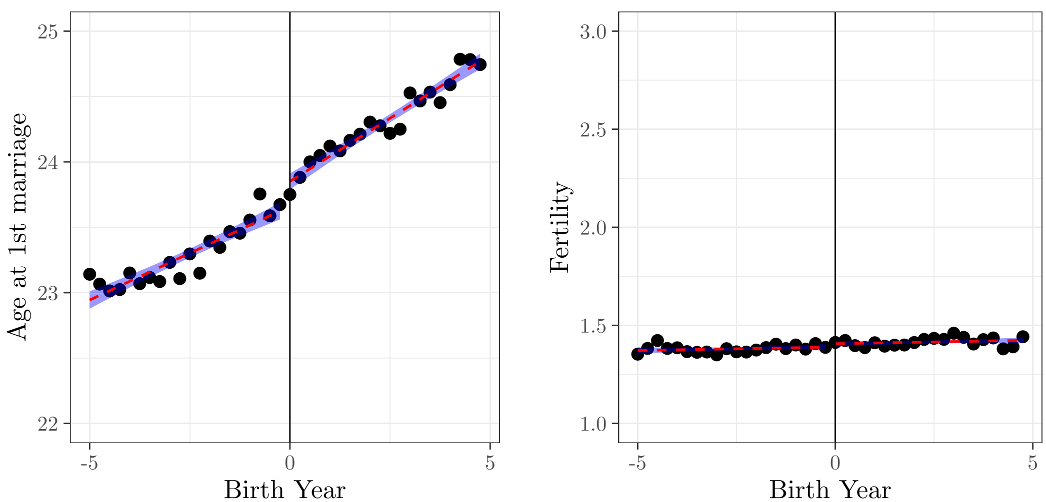
[39] This means that these confidence intervals are not precisely comparable to the coefficient estimates from the pooled regression of equation 5 with controls.



(b) Age at 1st Marriage

(c) 1st Marriage Fertility

(c) The 1947 Reforms



(e) Age at 1st Marriage

(f) 1st Marriage Fertility

(f) The 1972 Reform

Figure 5. The causal effects of education reforms.
Notes: *Quarter* = 0 is the effective birth quarter date of the educational extension reform. Everyone born on or after this date is treated by the reform. Apart from panel C, there is no evidence of any discontinuity in age at first marriage or fertility across the RD horizon at zero.

The regressions analyzed here are consistent with this initial visual inspection. For every reform, there are six variations of the RD regression, varying the polynomial order (either linear or quadratic), for each of the bandwidths 12, 20, and 28 birth quarters on either side of the effective birth quarter of the school reform. Within the four RDs, the residual standard error is approximately the same for each model.

Table A10 reports the RD regressions for age at first marriage for the 1947 reform. In all six models, the constant is about 23 years. The scaled treatment effect, τ, is estimated in the range -.065 to .059 of one year. Although here the scaled standard errors mean I cannot precisely estimate this effect, and therefore cannot rule out a small causal education effect that could be either positive or negative.

Table A11 reports the RD regressions for Age at 1st Marriage for the 1972 reform. Here age at marriage is higher at around 23.6–23.85. Consistent with the visual evidence there is evidence that the 1972 educational reform where the minimum school leaving age was raised from 15 to 16 was causal in raising women's age at first marriage. Columns 3 and 5 estimate a scaled treatment effect of .522 and .835 of one year. This is a large effect. However, this result rests entirely upon the empirical specification of the time trend. Where I allow for a quadratic trend, the estimated treatment effect is substantially reduced (.046 and .167 for 20 and 28 quarters respectively). Further, the effect is now statistically indistinguishable from zero.

Table A12 reports the RD regressions for first marriage fertility for the 1947 reform. The constant here is almost two children. The scaled treatment effect of the educational reform, τ, is estimated in the range -.046 to .132 of one child. Where the treatment effect is more precisely estimated, in regressions of bandwidth 20, or 28, quarters, the treatment effect is effectively zero. The treatment of the time trend makes no difference to the estimated effect of the reform treatment.

Table A13 reports the RD regressions for first marriage fertility for the 1972 reform. Fertility has fallen since the birth cohort around 1947, and the constant here is now 1.4 births. The scaled treatment effect of the educational reform, τ, is estimated in the range -.030 to .061 of one child. Note that in column 5, the linear RD model across 28 birth quarters before and after the discontinuity threshold, the estimated treatment effect is positive, and statistically significant at the .01 % level. As with the 1947 reform, the scaled standard errors here allow us to rule out any substantial negative causal effect of the education reform on fertility.

Figure 6 summarizes the RD regressions by plotting the scaled treatment effect of the educational reform, τ, and a scaled 95% confidence interval, for each reform-outcome studied here. For concision, I plot only the effects for one model, that of the linear time trend with a bandwidth equal to 20 birth quarters (column 3 of each regression table). For age at first marriage, the 1947 reform treatment effect is too imprecisely estimated to rule out a modest effect, which could be either positive or negative. There is evidence that that 1972 reform did causally raise age at first marriage. (However, as discussed, this rests entirely on the treatment of the time-trend, and disappears in a quadratic formulation.) For first marriage fertility, the RD analysis rules out any substantial causal role for education in reducing fertility. As the effect is so precisely estimated for both reforms, I can rule out any non-trivial negative effect of education on fertility.

5. Interpretation

The evidence from 20th century England is clear. Education is not causal in fertility. This finding of a zero causal effect is novel, and relatively surprising, given theory, observational correlations, assumptions, and perceptions, both general and scientific.

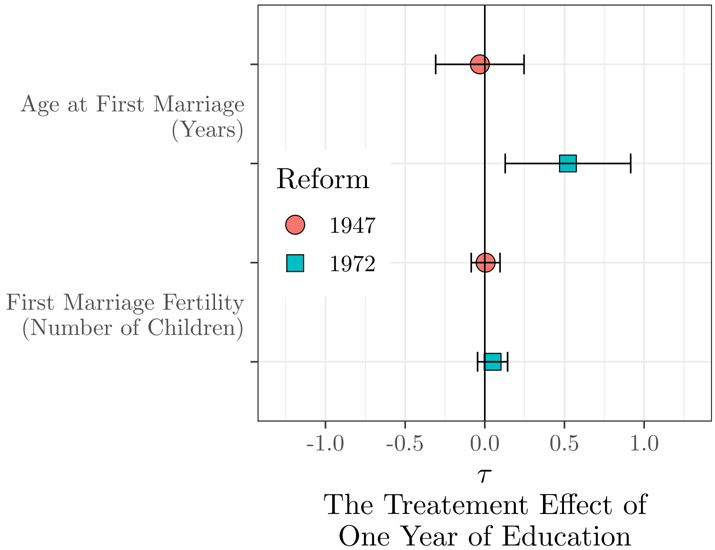


Figure 6. The causal effects of education on female age at first marriage and first marriage fertility.
Notes: The figure plots the scaled treatment effect of the educational reform, τ, and the scaled confidence intervals, from tables A10, A11, A12, and A13. For simplicity I plot only those estimates from column 3 of each table where the time-trend is linear and the bandwidth is 5 years (or 20 birth quarters) before and after the RD cutoff. This is consistent with the RD plots from figure 5.

While there is some evidence that the 1972 reform raised age at first marriage, this dissolves once we allow for a more flexible time trend. Thus we cannot say that either the 1947 or 1972 reform had any consistently estimated effect on age at first marriage. Here the exogenous exposure to education at 15, and at 16, did not unambiguously delay the onset of women's marital fertility.

Why is education not causal in fertility? Economic theory presumes that education will raise human capital and labour market returns. Existing evidence on the labour market returns for these same education reforms is mixed. A zero education–fertility effect could simply be a result of a zero education–wage effect.[40]

[40] As discussed in a related paper (Clark & Cummins 2020a, p.1): "Harmon & Walker (1995) measure the effects of the 1947 and 1972 schooling extensions in the UK using data on the earnings of 34,336 employed males aged 18–64 surveyed 1978–1986 in the Family Expenditure Survey. The find an estimated 15% earnings gain from an additional year of compulsory schooling. Oreopoulos (2006) similarly finds that the extension of the school leaving age from 14 to 15 in Britain in 1947, and Northern Ireland in 1957, also created a 15% increase in wages from a year of schooling. However, Devereux & Hart (2010), using the same data as Oreopoulos (2006), report a 0% gain in earnings for women, and a 5.5% gain for men (they also report the Oreopoulos result was partly created by programming errors in the original paper). With richer data on earnings from another much larger UK earnings survey they find for the 1947 schooling extension a year of schooling led again to a 0% gain for women, and now just a 3–4% gain for men. Using that same earnings survey (Delaney & Devereux 2019) find that the 1972 extension had no statistically significant effect on earnings averaged across ages 20–60 for men or women, though a point estimate of a 6% gain from a year of additional schooling for men. Dolton & Sandi 2017, however, report that for the 1947 extension, rate of return estimates are surprisingly sensitive to the polynomials

However, the potential for education to reduce fertility can also work through perceptions and aspirations. Here the zero causal effect estimate is also unexpected.

Two economic mechanisms could offset the theorized "negative" effect of education on female fertility. One is the effect of free schooling on the anticipated cost of children. Women could dynamically update their best estimate of the cost of their future children based on their teenage experience. The 1944 Butler act established free secondary schooling for all pupils. Free publicly funded education can increase fertility through a positive income effect (de la Croix & Doepke 2009). However this would not invalidate the RD design of this paper as this effect would apply equally to the future fertility calculations of those pupils born both just before and just after the effective threshold of the reform.[41]

The second mechanism that could be at play is the effect of the reforms upon husbands. As women tend to marry men close to them in age, those women impacted by the reform will marry men who are also more likely to have been impacted by the reform. For those men, their labour market returns could have been improved by their exposure to more education. This positive income effect could offset the negative "opportunity cost" effect. Two considerations indicate this is unlikely. One is the fact that women, on average, marry older men. The age gap of spouses at marriage is consistently about 2 years in 20th century England and Wales. Thus only a small minority of future spouses of the women impacted by the reform are also be impacted by the reform. Second, many recent papers show a very modest, or zero, causal effect of these same education reforms on labour market outcomes (as summarized above, and in Clark & Cummins 2020a).

This paper has nothing to say on the quality of education received by those impacted by the 1947 and 1972 reforms. Those seeking to resurrect a causal role for education on fertility might pursue this direction in future research. However, the timing of the extra schooling examined here, at ages 15 and 16, would appear to be prime for directing aspirational change, and rational assessment, to young women at the start of their reproductive life. Regardless of what was actually taught in the classroom, the substitution of work, unemployment or leisure, for a year at least minimally targeting improved human capital, would be expected to have some effect on future fertility, based upon theory and the observational correlations. Thus the precisely estimated zero effects documented here are surprising.

One reconciliation of the consistently observed and large correlation of education and fertility, with a zero casual effect, is presented in figure 7. The paper has demonstrated that education–fertility relationship A is non-causal. An omitted variable, which one can loosely term "Culture" likely jointly determines both education and fertility (connections B and C).

The appearance of large fertility differences by parents level of education is thus an observational, non-causal, correlation. Supporting this is evidence from economic history and historical demography, which suggests that the more educated had *higher* fertility than

controlling for year and age effects. Returns 'appear sensitive to the choice of the polynomial used to describe the underlying unobservable trends in education and earnings in the sample: our estimates range from 5–6% and statistically significant when using polynomials of order three or four to 0–3% and non-statistically significant when using polynomials of order one and two.'(p. 100)." Clark & Cummins (2020a) argue that the labour market effect of the equation reforms was likely zero.

[41] That is unless there is some extra psychological effect of internalizing the reform by being directly impacted by it.

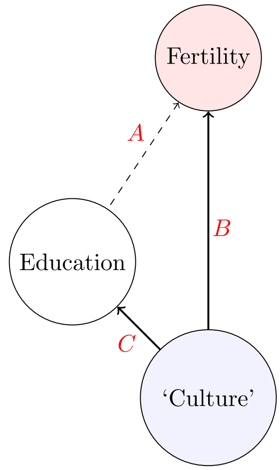


Figure 7. Education, fertility, and "culture".
Notes: Solid lines indicate potential causal connections, dashed lines are non-causal connections driven by the other factor.

the less educated, pre-1800.[42] The modern negative education–fertility correlation is a recent phenomenon.

This suggests that while demographic projections built upon a causal education–fertility effects may be predicatively accurate they are not scientifically robust. They will be right until they are wrong.

Some may argue that individual education may itself cause cultural change in the aggregate (reversing connection C in figure 7) and this aggregate cultural change is responsible for B, the cultural driver of lower fertility. In this "general equilibrium" channel the causal role for education in inducing reduced fertility is preserved, even in the absence of individual effects operating at the margin. While this analysis cannot rule this effect out, the idea cannot be tested with micro-data. Absent an actionable, testable hypothesis, this author applies Occam's razor to suggest that the zero causal effect of education on fertility is most likely.

6. Conclusion

Using a newly constructed database of 1.5 million women from England and Wales, I have presented here a credible analysis of the causal effects of education on age-at-first marriage and first marriage fertility. While there is some limited evidence for a potentially large effect on age at marriage, the fertility effects are precisely zero. This result undermines the claim that modern low fertility rates are a causal result of increased education. Why then do the more educated limit their fertility? Perhaps education, along with low fertility, is one component of a set of behavioral preferences, a cultural "package" that has come to dominate high income, and low fertility countries today. Social science must continue to explore the causal forces behind this historically unique pattern, and to unearth the underlying causes behind the mysterious decline in human fertility over the past two centuries.

[42] On this see the evidence summarized in (Clark & Cummins 2015b; Cummins 2013, 2009; Skirbekk 2008). Here this may be driven by the positive Malthusian wealth effect on fertility in pre-Industrial societies.

Data availability

The data and code underlying this article is available at https://www.openicpsr.org/openicpsr/project/214101/version/V1/view.

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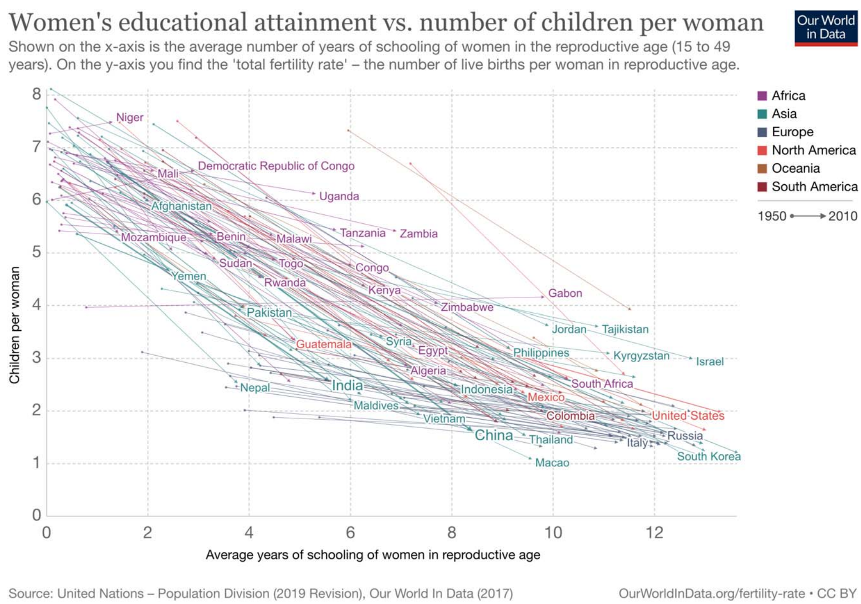
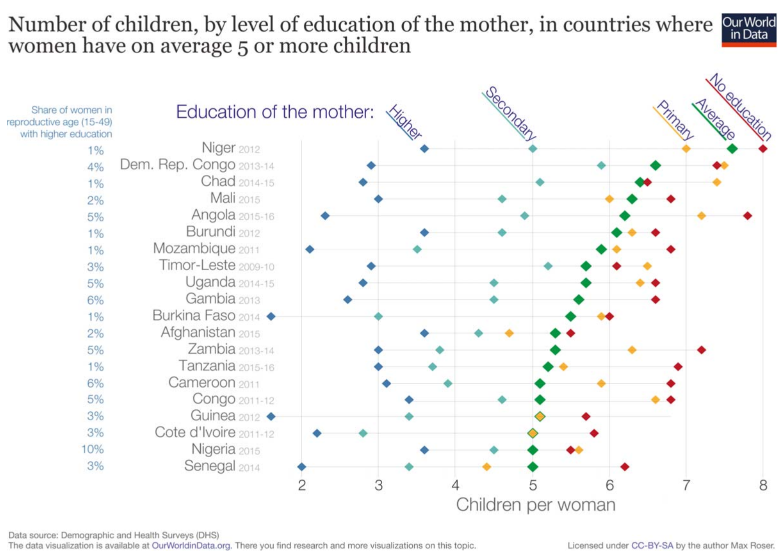
A Appendix

A.1 General background material

Figure A8a reports the relationship between average years of schooling, and fertility, between countries, 1950–2010. Figure A8b reports the number of children by the educational attainment level of the mother, and figure A9 reports the education–fertility differences across time for the US and the UK. Both between and within countries more educated women have smaller families. Figure A10 reports the time trend of aggregate fertility (as indicated by the Crude Birth Rate), and average years of schooling, for England 1870–2000. Table A2 reports recent headlines from major new organizations revealing a popular conception that education and fertility are causally connected.



(a) Between



(b) Between, High Fertility Countries Only

Figure A8. Education and fertility at the global country level, 1950–2010.
Source: https://ourworldindata.org/grapher/womens-educational-attainment-vs-fertility Our World in Data Panel A chart based upon United Nations, Department of Economic and Social Affairs, Population Division (2019). World Population Prospects: The 2019 Revision, https://population.un.org/wpp2019/Download/Standard/Interpolated/, and Barro and Lee, census data, http://www.barrolee.com/data/wholepop.htm. Panel B chart based upon data from the Demographic and Health Survey (DHS).

Table A2. *Education and fertility, popular conceptions.*

Title	Outlet	Date
Thanks to education, global fertility could fall faster than expected	The Economist	2 Feb 2019
Better education of women helped push total fertility rate down	Times of India	1 July 2020
"Educate girls to stop population soaring"	The Independent	4 Dec 2008
Educating young women is the climate fix no one is talking about	Wired	27 Jan 2021

Notes: This article states: "Girls who have been able to go to school grow up to be women who are economically and politically empowered, and who are not forced into early marriage to bear children."

Figure A12 reproduces Fort et al. (2016) figure 2. Due to the small sample size per time period, it is evident that the time trend around the discontinuity year zero is estimated with considerable noise ruling out any positive interpretation of this evidence.

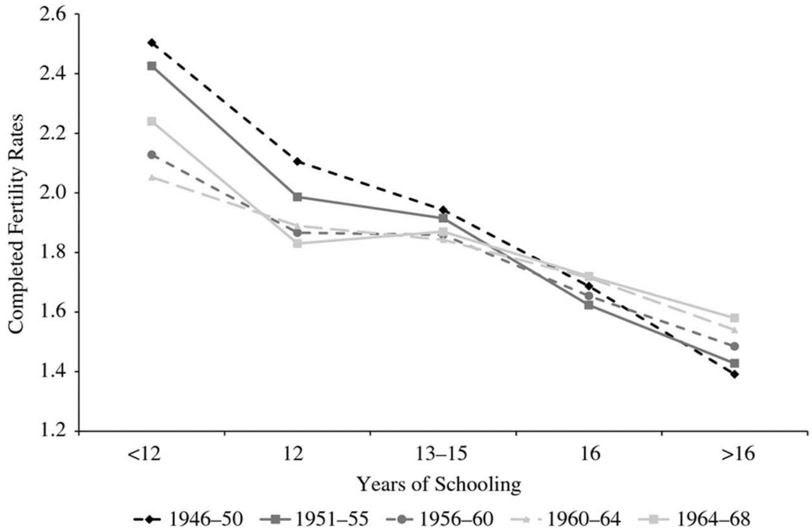
Table A3 summarizes the economics literature on the causal connection between education and labor market outcomes. (This table is also reported in Clark & Cummins 2020a). In general, this literature finds mixed effects. Some studies report large, positive causal impacts while others find no causal effect.

A.2 Assigning precise dates to registered births and marriages

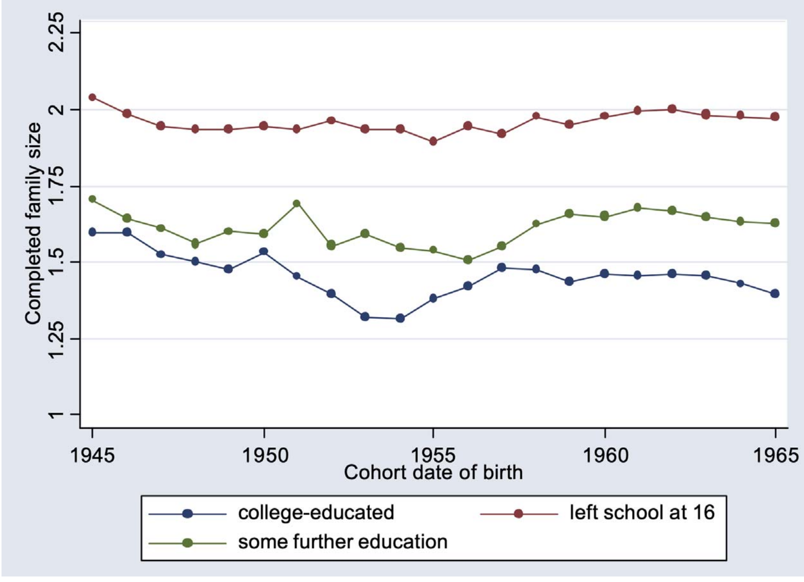
The data used here is generated by linking the birth and marriage registers (for age at marriage), and the marriage and birth registers (for marital fertility). However, both the birth and marriage records do not report the precise date of the event but rather report the year and quarter that the event was registered. The Regression Discontinuity design of this analysis rests upon the accurate assignment of births to an untreated and treated categories based upon a specific date of birth. Thus it is essential to interrogate fully the typical interval between the occurrence of an birth and its registration.

From 1837 to 1874, parents were not legally obligated to register the birth of a child. However, the Registration of Births and Death Act (1874) made registration compulsory, under penalty of a fine, within 42 days of a birth. Today, in 2021, the law still gives parents 42 days to register the details of a new born (Gov.UK). But what was the average interval in practice? A useful feature of the death registers is that the precise date of birth is reported for every death, after 1970. By linking these death records to their corresponding births, I can compare the precise date of birth with the year and quarter of birth registration. A description of this link, and figures reporting the average time to registration, and the proportion of births, registered in the same quarter, the following quarter (and so on), is reported in appendix section A.5.[43]

[43] For this link I use those male records that are unique combinations of birth year, surname, first forename, and the first letter of the second forename, in both the birth and death records. I only attempt to link if an individual is the only person of a certain name, born in a given year, as reported by both birth and death registers. I then link birth records to death records based upon an exact concordance of these characteristics. To account for births that may be register in the year after they occur I search for matches in the birth registers in the years after the death register recorded precise birth date.



(a) US



(b) UK

Figure A9. Education and fertility, within countries.
Source: Panel A: Figure 5 "Completed Fertility Rates by Education for Cohorts Born Between 1946 and 1968" from Hazan & Zoabi (2015). Panel B: Figure 10 from Ratcliffe & Smith (2006).

Table A3. *The effects of compulsory education extensions.*

Authors	Year	Journal	Outcome	Effects	Citations
Angrist & Krueger 1991	1991	QJE	Earnings, USA	+	2,859
Lleras-Muney, 2005	2005	REStud	Mortality, USA	$+^{A}$	1,499
Harmon & Walker 1995	1995	AER	Earnings, UK	+	759
Oreopoulos 2006	2006	AER	Wages, UK	+	736
Oreopoulos 2007	2007	JPubE	Earnings, Health, US, Canada, UK	+	562
Arendt 2005	2005	EER	Health, Denmark	?	407
Machin et al. 2011	2011	EJ	Crime, UK	+	427
Clark & Royer 2013	2013	AER	Mortality, UK	**0**	332
Silles, 2009	2009	EcEdRev	Health, UK	+	278
Pischke & Wachter 2008	2008	REStat	Earnings, Germany	**0**	272
Albouy & Lequien 2009	2009	JHE	Mortality, France	**0**	249
Kemptner et al. 2011	2011	JHE	Health, Germany	+	196
Lindeboom et al. 2009	2009	JHE	Child Health, UK	**0**	196
Devereux & Hart 2010	2010	EJ	Earnings, UK	+/0	183
Stephens & Yang 2014	2014	AER	Wages, employment, divorce, USA	0/__	167
Mazumder 2008	2008	EP	Mortality, USA	**0**	150
Lager & Torssander 2012	2012	PNAS	Mortality, Sweden	**0**	104
Grenet 2013	2013	ScanJEcon	Earnings, France, UK	+/0	101
Dickson et al. 2016	2016	EJ	Children's Education	+	60
Black et al. 2015	2015	JHE	Mortality, USA	0	13

Notes: QJE = Quarterly Journal of Economics, REStud =Review of Economic Studies, AER = American Economic Review, JPubE = Journal of Public Economics, EER = Economics of Education Review, EcEdRev = Economics of Education Review, REStat = Review of Economics and Statistics, JHE = Journal of Health Economics, EJ = Economic Journal, EP = Economic Perspectives, PNAS = Proceedings of the National Academy of Sciences, ScanJEcon = Scandinavian Journal of Economics. *A* Positive effect is beneficial. Citations from Google Scholar as of 11 Feb 2020. This table also appears in Clark & Cummins (2020a).

The birth-death linked sample ($N = 3,959,570$)[44] reveals that the average time to registration for births is on average 30–40 days, varies by quarter but is more or less unchanged throughout the sample period, 1912–1970. Further, from 1912 to about 1945, 63% of births are registered within the same quarter. After 1945, 75% of births are registered in the quarter they occur. Throughout, from 1912–1970, over 95% of all births are typically registered within the quarter they occur, or the following quarter. A small minority of births (less than 5% in 1970) take longer than two quarters to register.

Based on this evidence, I assign to every observation a randomized "precise" date of birth based upon the year and quarter of birth registration. I assume that individuals are born, and are married in, the quarter that their birth or marriage is registered. So if someone is registered as being born, or a couple registers a marriage, in quarter 1, 1950, I assume they were born, or were married, sometime between the 1st January 1950 and the 31 March 1950.

For the RD analysis, it is important to draw as accurate a line as possible between the treated and the non treated. As births cannot be registered before they occur the bias that my assignment of precise birth dates will be to mix some "untreated" individuals into

[44] This male birth-death linked sample is significantly larger than the female birth–marriage sample used in the RD analysis because here I have an approximate year of birth to help identify matches.

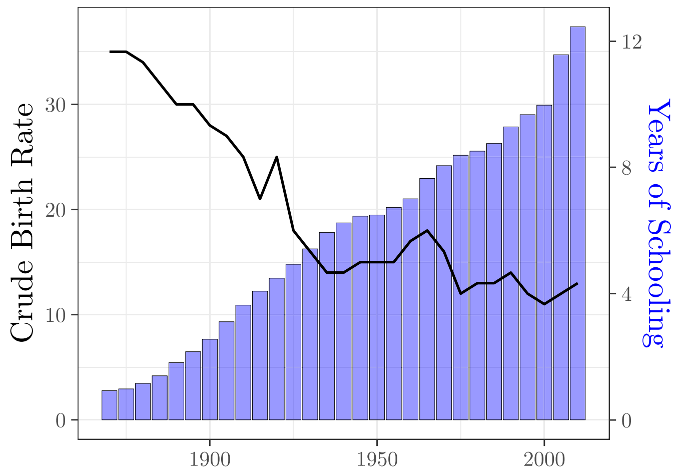


Figure A10. Schooling and fertility transition in England and Wales, 1870–2010. *Sources*: Crude Birth Rate (England and Wales): (Dyson & Murphy 1985; Office for National Statistics 2021), Total Years of Schooling per Person (UK): (Lee & Lee 2016).

the "treated" side of the discontinuity. This is because some births registered as being in say quarter 2, 1933, will actually be from quarter 1 1933. However, the advantage of this assignment is that it ensures that no births registered in quarter 2 1933 are assigned to quarter 1 1933. Thus the bias the birth date assignment can be accounted for. It will reduce the expected average treatment effect for this treated. I can directly account for the bias this introduces by scaling the RD treatment coefficient by the observed proportions of registered births that occur in that specific quarter (as reported in appendix table A7).

I assign everyone an estimated "precise" birth date, B^D as

$$B^D = B^{yr} + (B^Q - 1)/4 + r \tag{A1}$$

where B^{yr} is birth year, B^Q is birth quarter (1, 2, 3 or 4), and r is a random number following a uniform distribution in the range {0, 0.25}. Similarly an estimated "precise" first marriage date, M^D, is calculated as

$$M^D = M^{yr} + (M^Q - 1)/4 + r \tag{A2}$$

For the RD running variable I use simply birth quarter, and not the randomized precise birth date.

A.3 Estimates of the effect of the education extensions on female years of schooling in the UK

Table A4 summarizes estimates from the economics literature, using multiple survey datasets, of the effect of the schooling law changes on average years of schooling for women.

As also reported in Clark & Cummins (2020b), figure A16 reports the effect of the raising of the minimum school leaving age from 14 to 15 in 1947. This data is sourced from the

Table A4. *Estimates of the effect of the education extensions on female years of schooling in the UK.*

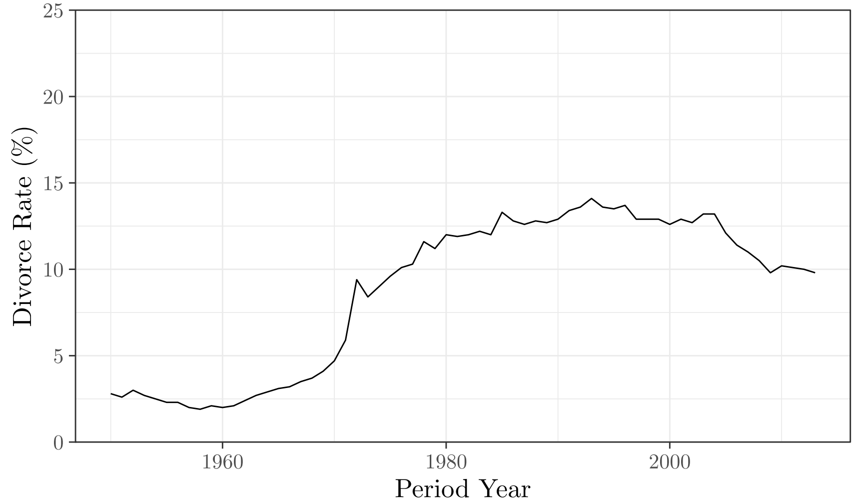
1944 Parliamentary Act Implementation [Effective Birth Cohort] Law Change	Effects on Years Schooling (se)†	*N*	Source
1st April 1947 [1st April 1933] *14 -> 15*			
	.550 (.042)	38,771	Devereux & Hart (2010, p.1354)[a]
	.511 (.027)	43,589	Devereux & Hart (2010, p.1354)[b]
	.697 (.130)	3,009	Fort *et al.* (2016, p.1836)[c]
	.535		*Weighted Average*
1st September 1972 [1st September 1957] *15 -> 16*			
	.356 (.079)	755	Buscha & Dickson (2012, p.392)[d]
	.328 (.020)	75,304	Grenet (2013, Appendix p.23)[e]
	.328		*Weighted Average*

Note: The treatment cohort represents the first cohort fully affected by the schooling extension. τ_e is the treatment effect of the education reform on years of schooling from the literature, weighted by sample size *N*. A similar table is reported in Buscha & Dickson (2016, table 1, p.6). Datasets used: [a]: General Household Survey 1979–98, [b]: New Earnings Survey 1975–2001, [c]: English Longitudinal Study of Aging, [d]: UK Household Longitudinal Study 2011, [e]: Quarterly Labour Force Survey 1993 to 2004. † The estimated effect is less than one because a significant proportion would have stayed in school regardless of the legal minimum age at leaving.

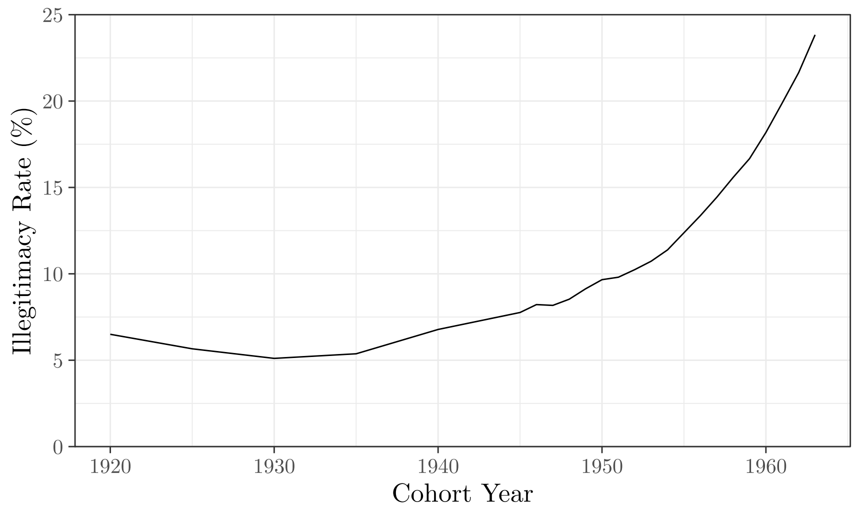
Annual Reports of the Ministry of Education (see Clark & Cummins 2020b). Attendance is about 40% of those age 14, 1900–38, and for 1946 before the reform was enacted on 1 April, 1947. In January 1948 enrollment jumps to 85% of those aged 14, and by January 1949 it was 100%, consistent with full implementation of the new leaving age. For the 1972 reform, Bolton (2012, Table 5, page 18) reports that in 1970 57.3% of 15 year olds are in school. By 1976, it is 100% also consistent with full implementation of the law. Both the survey and attendance records are consistent with each other.

A.4 Civil registration data: number collected compared with number recorded in official sources

Figure A17 reports by year for each vital series (births, marriages and deaths), a comparison of the numbers collected versus that recorded by the official records (from Office for National Statistics 2021). In all cases the harvested counts closely match that expected from official statistics for the vast majority of years between 1837 and 2007. The exceptions are the sharp drops in numbers harvested in the 1970s for births and marriages; this its because the underlying website (freebmd.com) was incomplete for those years when the data was collected.



(a) Period Divorce Rates



(b) Cohort Illegitimacy rates

Figure A11. Divorce and illegitimacy rates.
Source: Period divorce rates are from Office for National Statistics (2015), cohort illegitimacy rates are from Office for National Statistics (2008).

A.5 Civil registration: date of registration compared with date of event

The Registration of Births and Death Act (1874) obligated parents to register the birth of a child, from 1875 onwards, within 42 days. Before 1875, parents were not legally obligated to register the birth. Today, in 2021, the law still gives parent 42 days (Gov.UK).

This analysis relies upon dates of registration to estimate dates of events. However, the Civil Registers report the quarter of registration of an event, not the precise date of the event

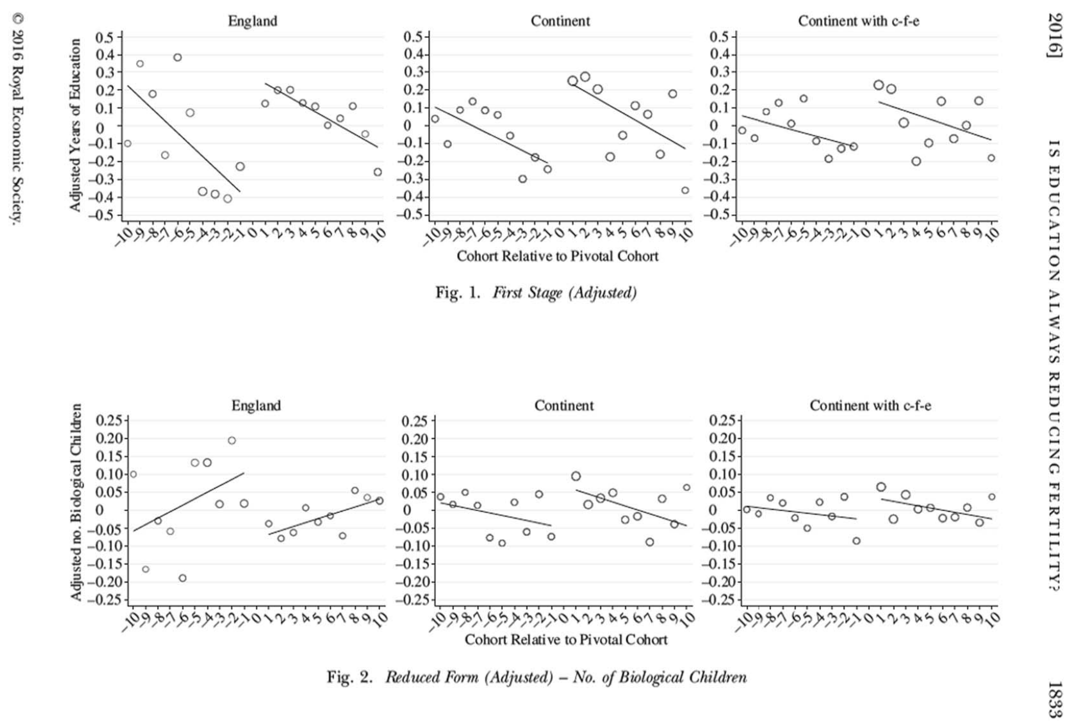


Figure A12. Reproduction of Fort et al. (2016) Figure 2.

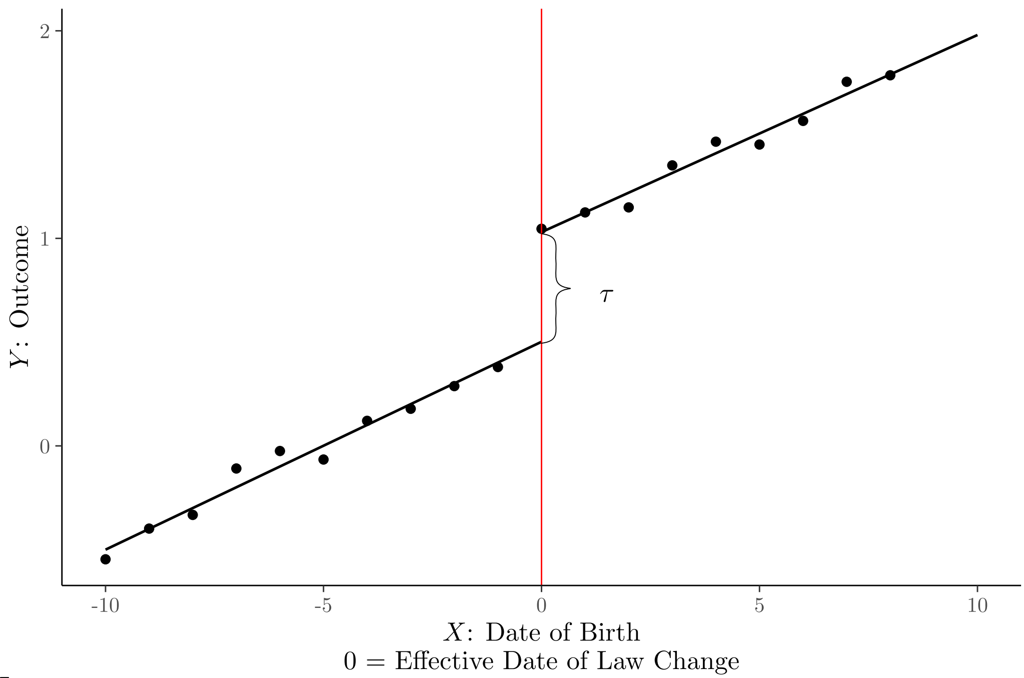
itself. What is the typical time lag between the occurrence of an event and it's submission for registration?

A useful feature of the death registers is that after 1970, the index entries record for every decedent the precise date of birth. By linking these post 1970 death records to their corresponding birth records it is possible to compare the quarter of registration with the precise date of birth. Here I only attempt to link male records. This is because women traditionally tend to change their surname upon marriage in England, with the result being that women's *birth* surname will likely differ from their *death* surname in the majority of cases.

In this link I use only those records that are unique combinations of birth year, surname, first forename and the first letter of the second forename, in both the birth and death records. In other words, I only attempt to link if an individual is the only person of a certain name, born in a given year, as reported by both birth and death registers. I then link birth records to death records based upon an exact concordance of these characteristics.

I link these records to each other using birth year, surname, forename, and first letter of second forename. By construction there can be no duplicate links. However because there will be a lag between birth, and the registration of that birth, I check the birth register records for the reported year of birth, from the death records, and the birth register records for the *following* year. In other words I search for a death that reports a year of birth y in the birth register for year y and for year $y + 1$. Further, as I only estimate year of birth before 1970, I also search for deaths in the birth records in year y and also $y - 1$, for these years only.[45] Where these procedures result in more than one death being matched to a birth, Ie drop both

[45] A specific example would be an individual who I observe dying before 1970 with an inferred year of birth of 1900. Here I search for the birth in 1900, 1901 (because of the lag between birth and birth registration), and



τ

Figure A13. The RD concept.
Notes: Data is generated from a simulation version of equation 3; where $Y = .5 + D_\tau + .1X + \varepsilon$, where D is a dummy set at 0 before $X = 0$, and set at 1 where $X >= 0$, τ is a treatment effect set at.5 and is a random error of mean 0 and a standard deviation of.05.

observations from the final sample. This set of linking steps correspond exactly to the steps followed in a related paper (Clark & Cummins 2020a).

This produces a linked sample with $N = 3,959,570$ observations linking a precise birth date to the quarter of birth registration. Figure A18a plots the average time to registration for births by quarter of birth 1912 to 1970. This is on average 30–40 days, varies by quarter but is more or less unchanged throughout the sample period (by birth year). The seasonal patterns, where quarter 4 births consistently have lower times to registration, and quarter 1 births have higher, are consistent with delayed registration in quarter 4 and a rebound in quarter one due to Christmas, and the holiday period, delaying a large proportion of births from being registered.

Figure A18b reports the proportion of both those born and registered in the same month, and those registered in later quarters. From 1912 to about 1945, 63% of births are registered within the same quarter. After 1945, 75% of births are registered in the quarter they occur. Throughout over 95% of all births are typically registered within the quarter they occur, or the following quarter. A small minority of births (less than 5% in 1970) take longer than two quarters to register.

Table A7 reports for the education "treated" cohorts of 1947 and 1972, what proportion of those registered in the treatment window were actually born in the treatment window. For

also 1899 because for many individuals the real year of birth will be 1899. For those dying after 1970 with an reported year of birth of 1900, I only search in the birth registers of 1900 and 1901.

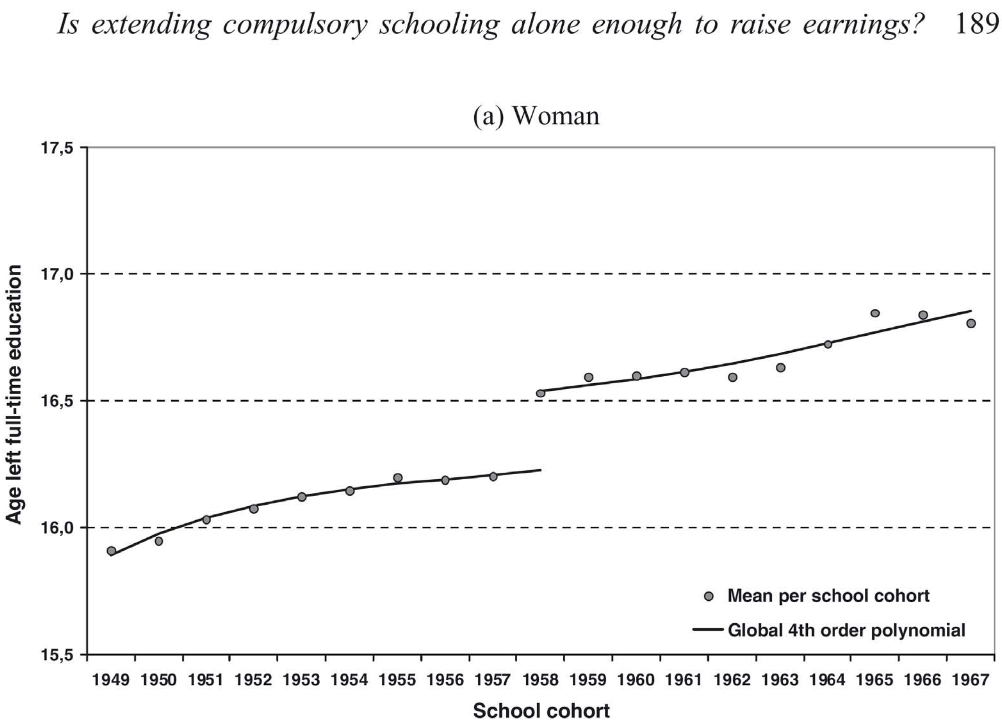


Figure A14. Reproduction of Grenet (2013, fig. 4(a) p.188).
Notes: The figure shows the discontinuity in the average age of leaving full-time education for women impacted by the 1972 reform, by year of birth (referred to as "school cohort" above), from the Quarterly Labour Force Survey 1993 to 2004.

all RD bandwidths at least 96% of births registered in the treatment window occur in the treatment window. These proportions are used to scale the estimated treatment effect by $\frac{1}{r}$ where r_w is the mean proportion of births registered in the treatment window.

A.6 Geolocating all observations

Figure A19 illustrates the registration districts reported in the vital registry data used here, 1837–2007, their modern equivalents, and the Index of Multiple Deprivation in 2019.

A.7 Age specific fertility rates

Figures A20 and A21 illustrate the underlying data for constructing age specific marital fertility rates for the general female population in England and Wales, 1920–60. This is done for a "like for like" comparison with the linked women in figure 3. Figure A22a compares the linked sample's childlessness rate with that from the ONS (Office for National Statistics 2013).

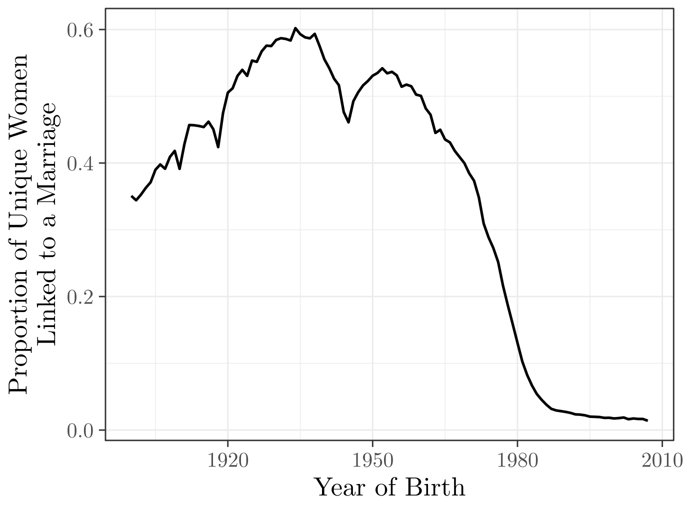


Figure A15. Linkage rate, unique females -> unique marriage, by year of birth.
Notes: Unique female births based upon first forename and surname (only one occurrence of name 1838–2007). Unique marriages based upon surname and spouse surname (only one occurrence of union 1838–2007).

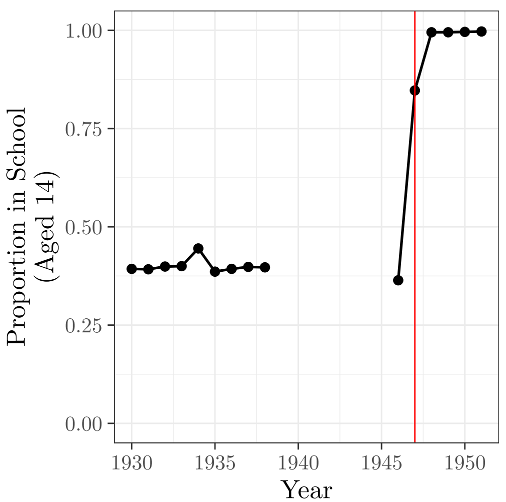
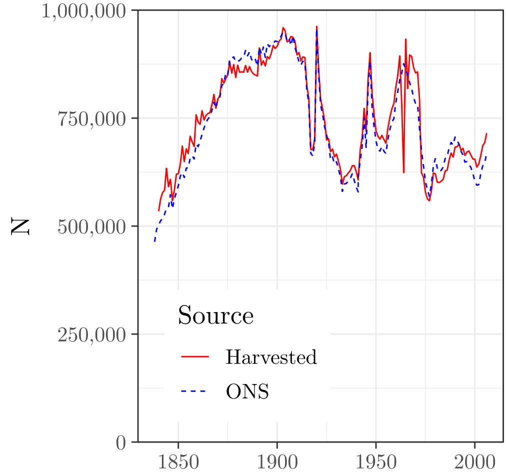


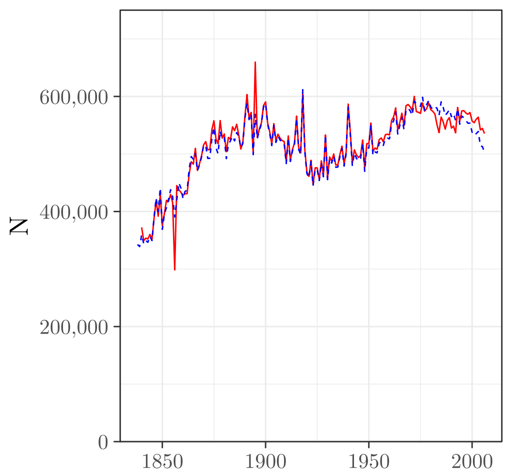
Figure A16. The effects on school attendance of the 20th century schooling reforms.
Source: Annual Reports of the Ministry of Education. This figure is also reported, and discussed in Clark & Cummins (2020b).

A.8 Summary statistics

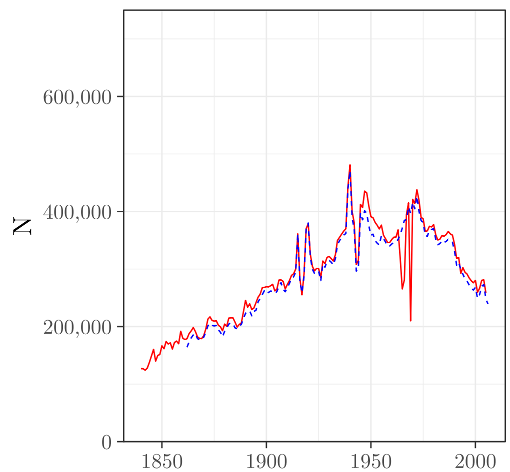
Tables A5 and A6 report the summary statistics for the linked women, for the link to marriage, and the link to fertility, respectively.



(a) Births



(b) Deaths

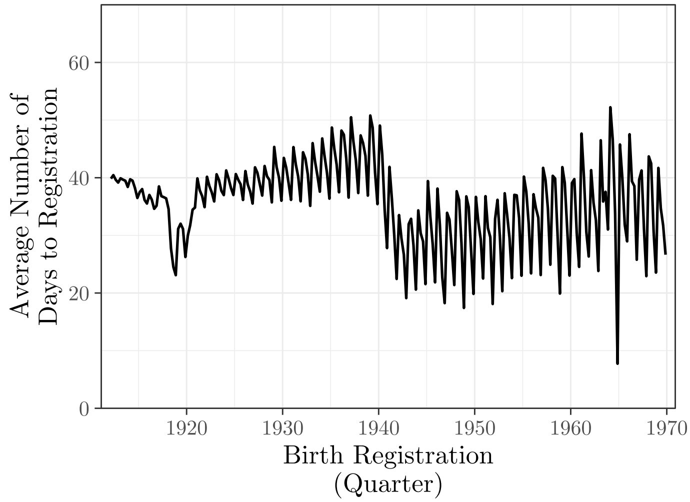


(c) Marriages

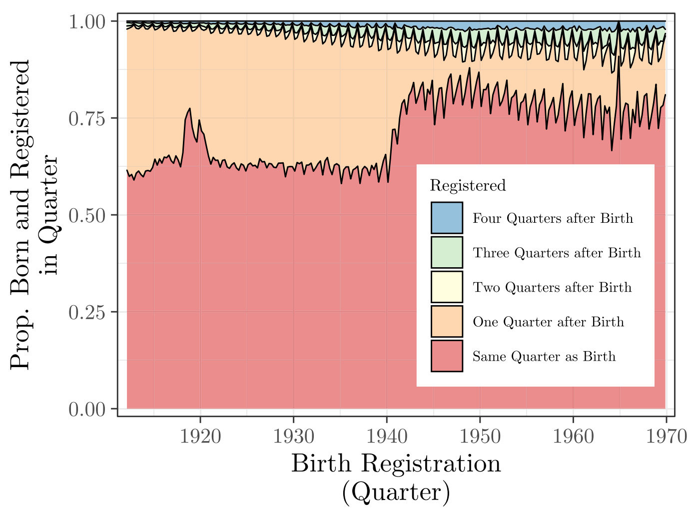
Figure A17. Data collection verification, 'harvested' versus official count comparison. *Notes*: The source for the Offical Counts is Office for National Statistics (2021).

A.9 The distribution of the assignment variable

The assignment variable of students into treated versus non-treated by the educational reform is quarter of birth. Figure A23 reports the distribution of our assignment variable. Any major discontinuities here would invalidate the RD design.



(a) Average Time to Registration, 1912-70

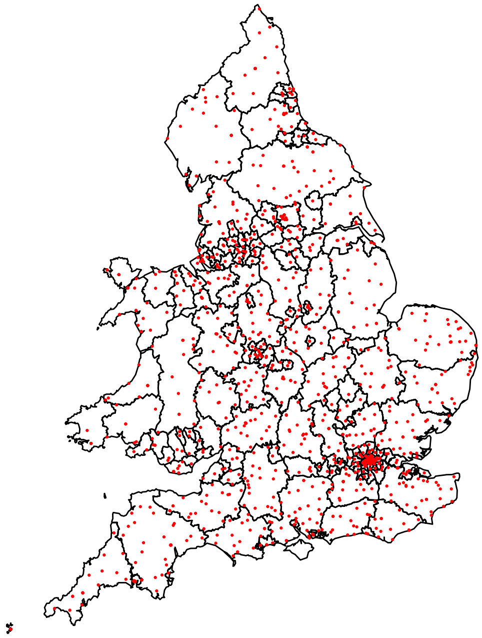


(b) Average Time to Registration, 1912-70

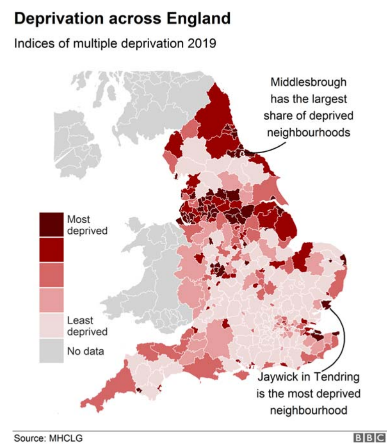
Figure A18. Comparing precise birth dates with quarter of birth registration.
Source: $N = 3,959,570$ linked birth–death records. The birth records report the quarter of birth only but after 1970 the death records report precise birth dates for all decedents. Linking the two record sources through unique names allows us to calculate the time to registration for births by quarter over the sample period (by year of birth).

A.10 Selection bias from linking

Here I assess the potential for selection bias from the linking methods employed in the paper. The empirical exercises here should be read in conjunction with the distributional figures 4a and 4b.



(a) Geo-Located Registration Districts (1838-2007[Blue dots]), Matched to Modern Equivalents (Borders)



(b) The Geographic Distribution of the IMD in 2019

Figure A19. Index of multiple deprivation (2019).
Source: www.gov.uk and https://www.bbc.co.uk/news/uk-england-49812519.

Table A5. *Summary statistics, linked women, birth -> first marriage.*

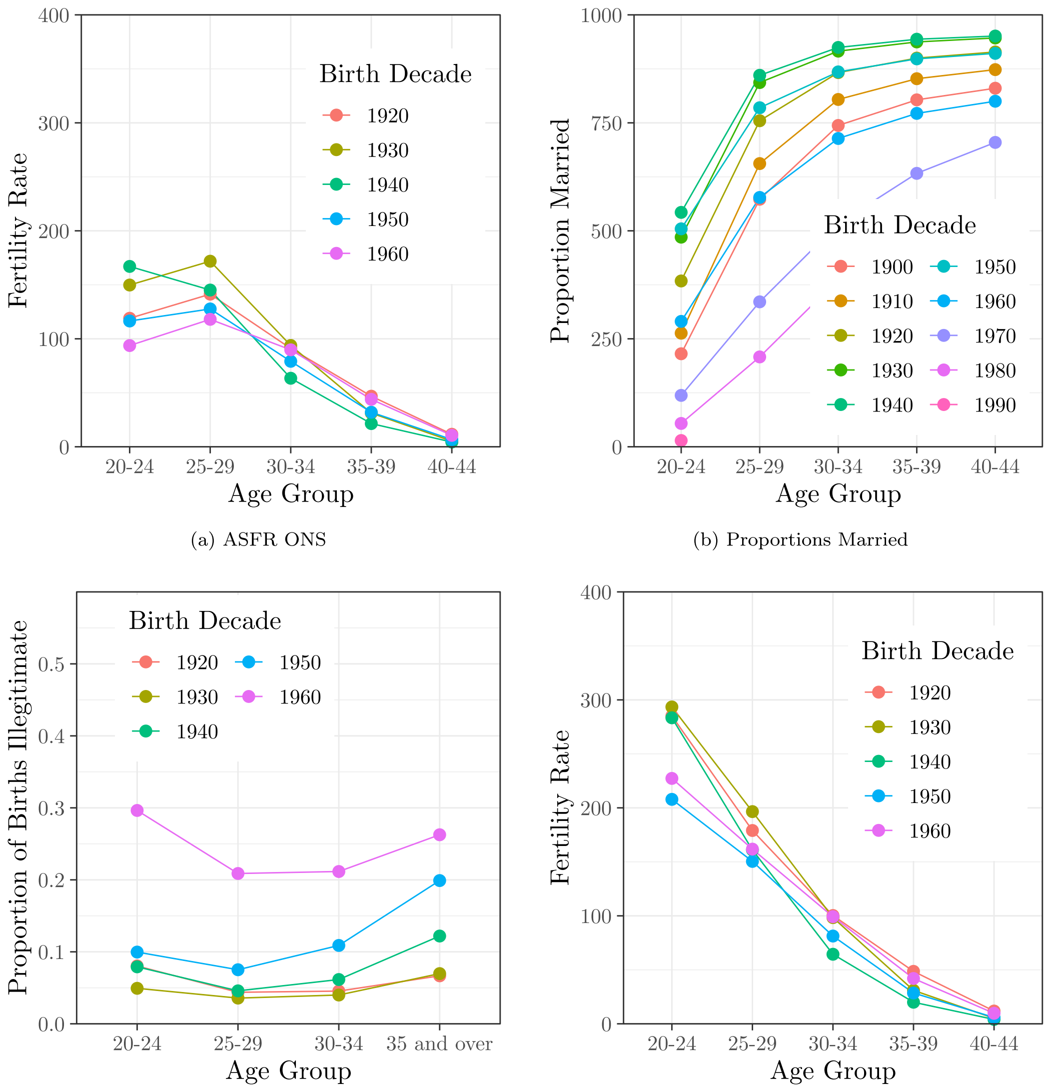
Statistic	N	Mean	St. Dev.	Min	Median	Max
Birth Date	1,483,360	1,938.32	25.31	1,852.94	1,941.80	1,989.87
Marriage Date	1,483,360	1,963.14	24.80	1,912.00	1,964.36	2,006.00
Age at First Marriage	1,483,360	24.83	6.36	12.01	23.18	60.00

Notes: Dates of birth and marriage are estimated from quarter of registration.

Table A6. *Summary statistics, linked women, birth -> first marriage -> fertility.*

Statistic	N	Mean	St. Dev.	Min	Median	Max
Birth Date	1,252,992	1,938.34	25.02	1,863.09	1,941.75	1,989.87
Marriage Date	1,252,992	1,963.00	24.66	1,912.00	1,964.15	2,006.00
Age at First Marriage	1,252,992	24.66	6.03	12.01	23.14	60.00
Childless	1,252,992	.30	.46	0	0	1
Fertility	1,252,992	1.52	1.48	0	1	19
Fertility, at least one child	880,629	2.17	1.32	1	2	19

Notes: Age and Interval are in years.



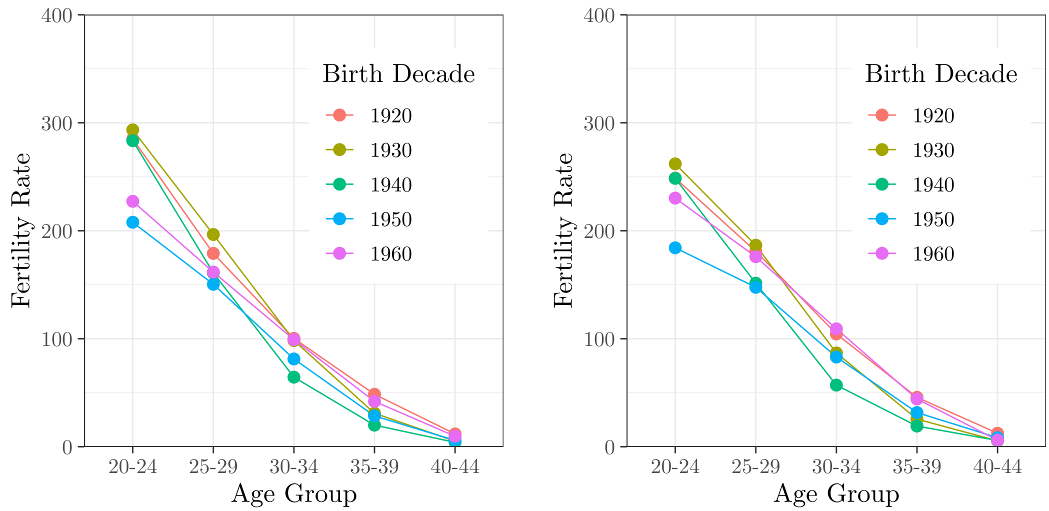
(a) ASFR ONS

(b) Proportions Married

(c) Proportion of Births Outside Marriage

(d) ASMFR ONS†

Figure A20. The components of the age specific marital fertility rate comparison. *Notes*: ASMFR is Age Specific Marital Fertility Rate, ASFR is Age Specific Fertility Rate. Age specific marital fertility rates ($ASFMR$) are then calculated as $ASMFR = (1 - ill)\frac{ASFR}{MR}$ where $ASFR$ are adjusted by the proportion of births that are outside marriage (ill), and the marriage rate per 1,000 women (note that the $ASFR$ is also measured per 1,000 women). The source for the ONS ASFR data is (Office for National Statistics 2020), for the outside marriage birth proportion (Office for National Statistics 2008), and for proportions married (Office for National Statistics 2014b). (All calculated on a birth cohort basis.) ASMFR ONS† denotes the derived ONS Age specific marital fertility rates for comparison with the linked data.



(a) ASMFR ONS† (b) ASMFR Linked Women

Figure A21. Age specific marital fertility rates, ONS derived compared with linked data.

Table A7. *Mean proportion of registered births born in treatment window, 1947 and 1972 reform.*

Year of Reform	Birth Registration Quarter	Bandwith (Quarters)		
		12	20	28
1947	1933,Q2	.96	.97	.98
1972	1957,Q3	.97	.98	.98

Note: $N = 266,556$ linked birth–death records.

First I ask, what is the section bias of only linking women with unique names? To examine this I compare the status differences between women with a unique name and all other women by running a simple regression of the form

$$y_i = \alpha + \beta D^U + t + \varepsilon \tag{A3}$$

where D^U is a dummy variable indicating a unique female name, y is a status outcome for each individual woman i (either surname wealth or the Index of Multiple Deprivation score of the district of birth, and t is a time-trend, in years, included as a quadratic (but not reported). In a similar vein, I assess the selection impacts of the linking process by running a regression of the form

$$y_i = \alpha + \beta_1 D^U + \beta_2 D^{LI} + \beta_3 D^{LII} + t + \varepsilon \tag{A4}$$

where the notation is as before, and D^{LI} and D^{LII} are categorical variables indicating that the female birth is linked to a unique marriage (Link I, as in the paper), and also linked to

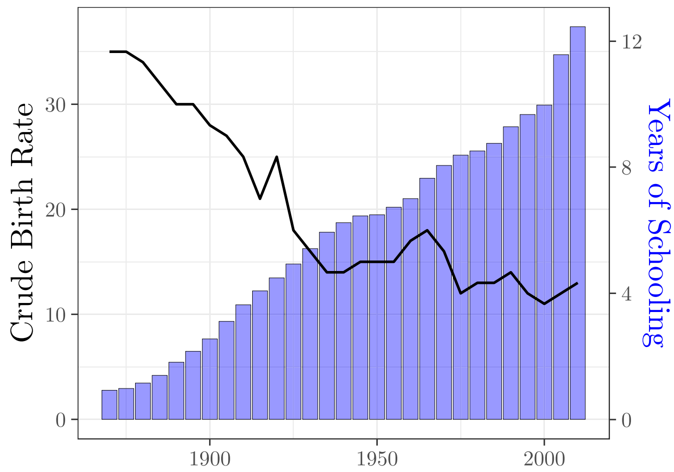


Figure A22. Comparing childlessness.
Notes: I adjust the fertility measures from the linked data by adjusting the observed childlessness rate by a factor of.895. The logic is that if the average migration rate in the second half if the 20th century is about.5% per annum (Sturge 2020), this will leave about 89.5% of those marrying in a given year still in England 21 years later. The value of.895 is applied to the average childlessness rate from the linked data in A22a above. This is a crude adjustment so that the two estimates of childlessness can be broadly compared. Source for ONS data: (Office for National Statistics 2020). As the links are based exclusively on names and not on any date information, the correspondence of the levels, and the time trend, of the linked series and that from the ONS, strongly support the veracity of the linking process. If the links were completely random, the average for the linked women would be a flat line.

Table A8. *The selection-status correlations of the linking stages, for females born 1926–40.*

	Surname Wealth			IMD of District of Birth		
	(1)	(2)	(3)	(4)	(5)	(6)
Unique Name	0.070***	0.088***	0.074***	−0.038***	−0.037***	−0.039***
	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)
Link I		−0.031***			−0.002	
		(0.004)			(0.001)	
Link II			−0.009*			0.001
			(0.004)			(0.001)
Observations	4,347,620	4,347,620	4,347,620	4,056,149	4,056,149	4,056,149
Adjusted R^2	0.0003	0.0003	0.0003	0.001	0.001	0.001

Note: *p<0.05; **p<0.01; ***p<0.001 IMD: Index of Multiple Deprivation (2019)

complete fertility (link II). Table A8 and A9 reports the results of these regressions for the birth cohorts used in the Regression Discontinuity design for the 1947 and 1972 reforms respectively. Table A8 includes those females born 1926 to 1940, (7 years before and after 1933), and table A9 for those females born 1950–1964, (7 years before and after 1957).

Is extending compulsory schooling alone enough to raise earnings? 189

(a) Woman

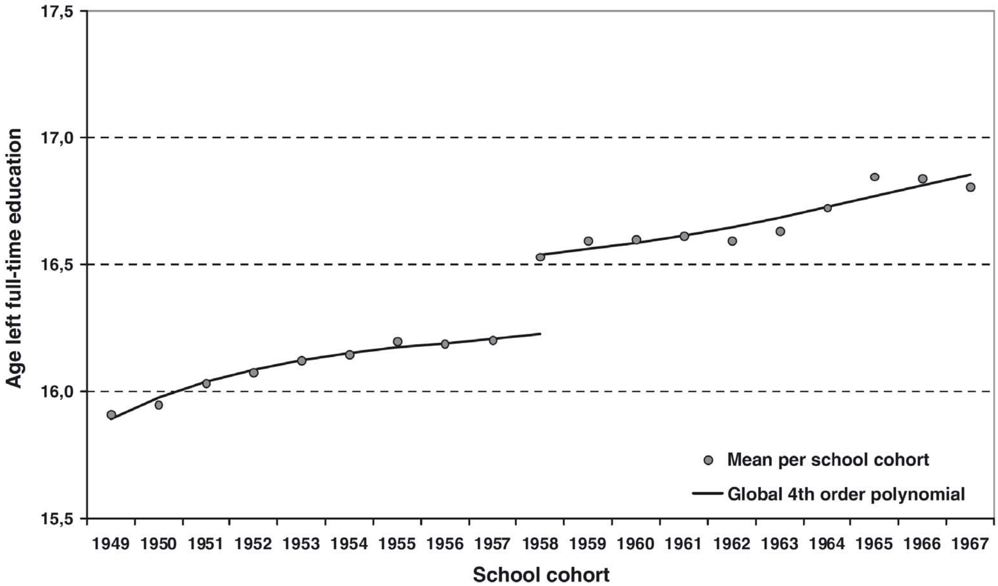


Figure A23. Probability of being linked, link I, over RD window.

Table A9. *The selection-status correlations of the linking stages, for females born 1950–64.*

	Surname Wealth			IMD of District of Birth		
	(1)	(2)	(3)	(4)	(5)	(6)
Unique Name	0.069^{***}	0.082^{***}	0.071^{***}	-0.036^{***}	-0.034^{***}	-0.034^{***}
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Link I		-0.026^{***}			-0.003^{*}	
		(0.003)			(0.001)	
Link II			−0.006			-0.003^{**}
			(0.003)			(0.001)
Observations	4,858,285	4,858,285	4,858,285	4,602,056	4,602,056	4,602,056
Adjusted R^2	0.0003	0.0003	0.0003	0.001	0.001	0.001

Note: $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$ IMD: Index of Multiple Deprivation (2019)

The tables assess selection for each of the linking three stages. First, selecting a unique name implies a mild positive selection of +.07–.09 of one standard deviation of surname wealth, in both sample periods. However, when status is measured by location (using the Index of Multiple Deprivation (IMD) score from 2019), the selection is mildly negative, with the coefficients in both periods around -.04 in all formulations (this is semi-elasticity as IMD is entered in log form). Thus we can calculate modest positive and negative selection from using rare names. The selection effects of Link I is mildly negative in both sample periods, and

Table A10. *The causal effect of the 1947 education extension on age at first marriage.*

	Age at First Marriage					
	(1)	(2)	(3)	(4)	(5)	(6)
τ_i	0.003	−0.001	−0.016	0.030	−0.034	0.022
	(0.073)	(0.112)	(0.055)	(0.084)	(0.047)	(0.071)
α	23.087***	23.091***	23.147***	23.099***	23.156***	23.098***
	(0.058)	(0.092)	(0.045)	(0.068)	(0.039)	(0.056)
τ	0.006	-0.003	-0.031	0.059	-0.065	0.043
	(0.143)	(0.217)	(0.107)	(0.162)	(0.089)	(0.136)
Bandwidth in Quarters	12	12	20	20	28	28
Polynomial Order	1	2	1	2	1	2
Birth Quarter Dummies?	✓	✓	✓	✓	✓	✓
Observations	94,367	94,367	158,743	158,743	219,580	219,580
Adjusted R^2	0.001	0.001	0.003	0.003	0.005	0.005
Residual Std. Error	5.330	5.330	5.380	5.380	5.404	5.404

Note: *p<0.05; **p<0.01; ***p<0.001 τ_i is the coefficient of the treatment dummy. The treatment effect of one extra year of education, τ is calculated by scaling τ_i by $1/\tau_e$, the treatment effect of the reform on average years of education, which is $\approx .535$ here.

are estimated close to zero for Link II, for both periods also. Thus I take the linked samples as broadly representative of the female population.

A.11 Assessing the accuracy of the links using a genealogical database

How accurate is the linking methodology employer in the paper? Here I compare the linked birth–marriage information quality with that recorded in an independently constructed genealogical database, the *Families of England* (FOE) Database, built by Gregory Clark and Neil Cummins. The FOE database collects information from genealogical databases such as personal family trees contributed by members of the Guild of One Name Studies, and triangulates entries with vital, probate, census and other records. The data has been used to study fertility (Clark & Cummins 2018; Clark et al. 2020), the genetic structure of status inheritance (Clark 2023), and is being actively added to, as of 2024. See the appendix to (Clark 2023) at https://www.pnas.org/doi/suppl/10.1073/pnas.2300926120/suppl_file/pnas.2300926120.sapp1.pdf for further information on the FOE database. It is crucial to note here that there has been as yet no updating of the FOE data with the links from this study (but this may happen in future FOE databases).

The information in the FOE data does have weaknesses for assessing the quality of the links made and analyzed in this paper. First, the FOE data is incomplete, and it is particularly incomplete for women. Because of this problem, we cannot use the FOE data to assess missed links ("Type II errors" in the literature). Further, while the information builds upon family and oral history knowledge, many of the links are likely evidenced from the same source, the official vital records series. However, much of the information in the FOE data is independently constructed and verified and can be compared with the automated links of this paper to give a ball park estimate of the "Type I error" rate, used in the census linking literature (false match rates).

I focus the comparison upon those women analyzed around the years of birth associated with the education reform, taking seven years before and after the effective birth year. For the 1947 reform, I have linked 227,650 women who were born between 1926 and 1940 (these

Table A11. *The impact of the education extension, 1972, age at first marriage.*

	Age at First Marriage					
	(1)	(2)	(3)	(4)	(5)	(6)
τ_i	0.097	−0.022	0.168***	0.015	0.268***	0.054
	(0.065)	(0.098)	(0.048)	(0.073)	(0.040)	(0.060)
α	23.774***	23.791***	23.682***	23.846***	23.596***	23.831***
	(0.056)	(0.085)	(0.041)	(0.060)	(0.034)	(0.049)
τ	0.305	-0.069	0.522	0.046	0.835	0.167
	(0.203)	(0.308)	(0.15)	(0.226)	(0.125)	(0.188)
Bandwidth in Quarters	12	12	20	20	28	28
Polynomial Order	1	2	1	2	1	2
Birth Quarter Dummies?	✓	✓	✓	✓	✓	✓
Observations	136,979	136,979	230,433	230,433	316,524	316,524
Adjusted R^2	0.004	0.004	0.010	0.010	0.016	0.016
Residual Std. Error	5.710	5.709	5.677	5.677	5.636	5.635

Note: *p<0.05; **p<0.01; ***p<0.001 τ_i is the coefficient of the treatment dummy. The treatment effect of one extra year of education, τ is calculated by scaling τ_i by $1/\tau_e$, the treatment effect of the reform on average years of education, which is $\approx$ .328 here.

are the "Type I links" discussed in the paper). Of these, I find 818 in the FOE database. For 807 of these, the marriage information is in agreement. For 11 there is a disagreement. However, for 2 of these 11, there are obvious spelling mistakes. For example, the automated links have a Doreen Errey, born 1926, marrying a man surnamed Poke in 1946, whereas Doreen Errey in the FOE data, born 1926, married a Frank Stephen Pope in 1947. Also, Constance Wimbleton, born 1928, marries a man surnamed Mihell in 1953 in the automated link. In the FOE data, Constance Wimbleton, born 1928, marries a man called Albert V Mihell in 1949. These discrepancies can easily be attributed to clerical error, or a lag between a church wedding and a civil registration. Thus it is evident that 9 of the 818 matches between 1926 and 1940 found in the FOE data are false matches. This is about 1.1%, meaning that 98.9% of the automated are corroborated by the FOE data.

For the 1972 reform I take the 321,826 women born 1950–64 that are linked to a marriage (type I). I find 699 of them in the FOE data, of whom 685 agree precisely. Of the 14 that disagree, 3 are close correspondence of the character described in the discrepancies above, and 11 are true disagreements. This suggests that the false match rate is 1.6%.

As noted in the paper, these false match rates compare very favorably with that typically found in the census linking literature, which are usually in the range of 20–40%, and they are in the range of hand-linked false match rates (0–4%) (Bailey et al. 2020, Table 1 p.1012).

A.12 Detailed regression tables

Table A10 reports the RD regressions for age at first marriage for the 1947 reform, table A11 reports the RD regressions for Age at 1st Marriage for the 1972 reform, table A12 reports the RD regressions for first marriage fertility for the 1947 reform and finally, table A13 reports the RD regressions for first marriage fertility for the 1972 reform.

Table A12. *The impact of the education extension, 1947, N.*

	First Marriage Fertility					
	(1)	(2)	(3)	(4)	(5)	(6)
τ_i	0.020	0.068	0.003	0.038	−0.024	0.037
	(0.024)	(0.036)	(0.018)	(0.027)	(0.015)	(0.023)
α	1.959***	1.924***	1.974***	1.946***	2.001***	1.949***
	(0.019)	(0.030)	(0.015)	(0.022)	(0.012)	(0.018)
τ	0.04	0.132	0.005	0.073	-0.046	0.07
	(0.047)	(0.071)	(0.034)	(0.052)	(0.029)	(0.043)
Bandwidth in Quarters	12	12	20	20	28	28
Polynomial Order	1	2	1	2	1	2
Birth Quarter Dummies?	✓	✓	✓	✓	✓	✓
Observations	80,635	80,635	135,472	135,472	187,087	187,087
Adjusted R^2	0.00002	0.00003	0.0002	0.0002	0.001	0.001
Residual Std. Error	1.604	1.604	1.604	1.604	1.596	1.596

Note: *p<0.05; **p<0.01; ***p<0.001 τ_i is the coefficient of the treatment dummy. The treatment effect of one extra year of education, τ is calculated by scaling τ_i by $1/\tau_e$, the treatment effect of the reform on average years of education, which is $\approx .535$ here.

Table A13. *The impact of the education extension, 1972, N.*

	First Marriage Fertility					
	(1)	(2)	(3)	(4)	(5)	(6)
τ_i	−0.009	0.019	0.016	−0.010	0.042***	−0.007
	(0.015)	(0.023)	(0.012)	(0.017)	(0.010)	(0.015)
α	1.410***	1.399***	1.388***	1.405***	1.372***	1.407***
	(0.013)	(0.020)	(0.010)	(0.015)	(0.008)	(0.012)
τ	-0.028	0.061	0.049	-0.03	0.129	-0.022
	(0.049)	(0.074)	(0.036)	(0.054)	(0.03)	(0.046)
Bandwidth in Quarters	12	12	20	20	28	28
Polynomial Order	1	2	1	2	1	2
Birth Quarter Dummies?	✓	✓	✓	✓	✓	✓
Observations	117,814	117,814	197,628	197,628	271,276	271,276
Adjusted R^2	0.0002	0.0002	0.0002	0.0002	0.0001	0.0002
Residual Std. Error	1.265	1.265	1.264	1.264	1.265	1.265

Note: *p<0.05; **p<0.01; ***p<0.001 τ_i is the coefficient of the treatment dummy. The treatment effect of one extra year of education, τ is calculated by scaling τ_i by $1/\tau_e$, the treatment effect of the reform on average years of education, which is $\approx .328$ here.

A.13 Robustness: Alternative confidence intervals using RDHonest

Table A14 reports alternative scaled confidence intervals for the education treatment effect as suggested by Kolesár & Rothe (2018). Here I allow a bandwidth to be chosen optimally, based upon the Mean Squared Error of the estimator, for both linear and quadratic formulations of

Table A14. *RDHonest confidence intervals.*

				Scaled		
Outcome	Reform	Bandwidth	τ	se	Maximum Bias	Confidence Interval
Age at 1st Marriage	1947	4.3	-0.004	0.123	0.064	[-0.272, 0.265]
	1972	4.0	0.364	0.179	0.090	[-0.022, 0.757]
Fertility	1947	2.3	0.030	0.036	0.019	[-0.048, 0.108]
	1972	2.3	0.048	0.057	0.030	[-0.077, 0.174]

Notes: Optimal Kernel and Bandwidth (years) selected for Taylor smoothness class with smoothness constant $M = .01$, using `RDHonest` in R (Kolesár 2020).

the RD model.[46] As the `RDHonest` treatment effect is estimated without seasonal controls they are not precisely comparable to the standard RD models estimate before. Despite this, the estimated scaled treatment effects are almost identical, as are the standard errors, to the standard RD results. Here the maximum bias that the discrete running variable can impart upon the treatment effect is incorporated into the `RDHonest` confidence intervals. While these resulting confidence intervals are marginally larger than before, they also rule out any large negative causal effect of the educational efforts in reducing fertility.

In sum, the visual evidence from the RD plots, the estimates of the treatment effect from standard RD models, and the `RDHonest` confidence intervals all suggest that their is no evidence for a causal effect of education on age at first marriage and first marriage fertility. Further, for first marriage fertility, this analysis can rule out any non-trivial role for education in reducing fertility.

[46] I follow the R vignette by Kolesár (2020). The confidence intervals are “honest” in that they are “guaranteed to achieve correct coverage in finite samples, and achieve correct coverage asymptotically uniformly over the parameter space” (Kolesár 2020, p.3).