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The effects of compulsory schooling reforms on women's marriage outcomes—evidence from Britain

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Abstract

This paper estimates the policy effect of a compulsory schooling reform in Britain in 1972 on women's marriage outcomes. Using a regression discontinuity design and data from the General Household Survey 1982–2001, I find that although the reform reduced women's probability of marriage as a teenager, it has no effects on their probability of never being married. For ever married women, I find that the effects of the reform on their probability of being divorced or separated are not statistically significant. Moreover, for currently married women, I find that the reform reduces the age gap between husband and wife by about 0.3 to 0.4 years. To explore the mechanisms, I find that the reform increases women's probability of marrying a similarly aged husband by about 4.8 to 5.8 percentage points, implying that the reform strengthens assortative mating in terms of age. Overall, the findings imply that compulsory schooling reforms aimed at improving citizens' educational attainment can also have substantial impacts on their marriage outcomes.

Keywords Education · Marriage · Compulsory schooling · RD design

JEL Classification H52 · I26 · J12

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1 Introduction

Public policies aimed at improving citizens' educational attainment may also have large effects on their labor market and non-market outcomes. For instance, longer compulsory education has been found to increase individuals' wages (e.g., Grenet 2013), encourage civic participation (e.g., Dee 2004; Milligan et al. 2004), reduce teenage fertility (e.g., Black et al. 2008; DeCicca and Krashinsky 2020; Silles 2011), and affect values and attitudes (e.g., Du et al. 2021; Meyer 2017; Yang 2021a, b).[1] But to date little is known about how compulsory schooling reforms affect individuals' marriage outcomes.

Only a few studies have estimated the effects of compulsory schooling reforms on individuals' marriage outcomes. First, Kirdar et al. (2018) find that the extension of compulsory schooling from five to eight years in Turkey in 1997 reduces women's probability of marriage by 16 by 50%. They also find that although most women complete Grade 8 by age 14, the effects of the reform on women's marriage persist until age 16. In addition, Hener and Wilson (2018) investigate the effect of the same 1972 UK educational reform on marital matching. They find that treated women (born after the cohort whose birth month just missed the reform) decrease the marital age gap to avoid marrying less qualified men, whereas treated men are able to marry similarly educated women without substantially changing the age gap.

To fill the gap in the literature, this study estimates the effect of a compulsory schooling reform in Britain on women's marital outcomes. This reform took place in 1972, which raised the school leaving age from 15 to 16. This study estimates the causal effect of the reform on women's marital outcomes, using a regression discontinuity (RD) design. Using data from the General Household Survey 1982–2001, I find that the reform increases women's years of schooling by about 0.42 to 0.48 years.

Moreover, I find that the reform significantly reduces women's probability of marriage as a teenager. To examine whether the reduction is temporary or permanent, I further examine the effects of the reform on women's probability of never being married. I find that the reform has no effects on their probability of never being married, implying that the reform has only postponed their marriage. For ever married women, I find that the effects on their probability of being divorced or separated are statistically insignificant. For currently married women, I find that the reform reduces the age gap between husband and wife by about 0.3 to 0.4 years. Furthermore, I explore whether the reform reduces the age gap between spouses by increasing women's probability of marrying a similarly aged husband. Indeed, I find that the reform increases women's probability of marrying a similarly aged husband by about 4.8 to 5.8 percentage points, suggesting that the reform contributes to assortative mating in terms of age. The findings imply that public policies aimed at improving individuals' educational attainment can have important implications on their marriage outcomes as well.

This study expands on the findings reported in Hener and Wilson (2018) in three directions. First, this study shows the robustness of the main findings by using data from the General Household Survey, whereas Hener and Wilson (2018) analyzed

[1] Longer compulsory education may have little effects on health and mortality (e.g., Clark and Royer 2013) and attitudinal trust (e.g., Yang 2019).

the Labor Force Survey. Second, this study estimates the policy effect on women's marital outcomes, whereas Hener and Wilson (2018) focus on the trade-off between age and academic qualification in the marital matching. Lastly, this study additionally examined the effects of the reform on (i) the probability of marriage as a teenager, (ii) the probability of never being married, (iii) the probability of being divorced or separated, and (iv) the probability of marrying a similarly aged husband.

This paper adds new evidence to the effects of compulsory education reforms on marriage and fertility outcomes (e.g., Black et al. 2008; Kirdar et al. 2018). This study is generally related to but distinct from the studies that use instruments for women's education and estimate the local average treatment effect (e.g., Anderberg and Zhu 2014; Lefgren and McIntyre 2006). Using birth quarter as an instrument for women's educational attainment, Lefgren and McIntyre (2006) find that additional schooling does not influence women's probability of marriage, but it does increase their husbands' earnings. Using an instrument based on month of birth, Anderberg and Zhu (2014) find that although obtaining some academic qualification has no effects on women's probability of marriage, it does increase their husbands' probability of holding some academic qualification or being economically active.

This paper progresses as follows. Section 2 discusses potential mechanisms and introduces the compulsory schooling reform in Britain; Sect. 3 discusses the empirical strategy; Sect. 4 describes the data; Sect. 5 reports the estimation results; Sect. 6 checks the robustness of the results; and the last section concludes the paper.

2 Background

2.1 Compulsory schooling reforms and marital outcomes

Compulsory schooling reforms can affect individuals' marriage outcomes both by increasing their own educational attainment and by generating general equilibrium effects in the marriage market.

Compulsory schooling reforms can increase individuals' own educational attainment, which can affect their marriage outcomes. First, more education increases individuals' human capital and earnings, which in turn have substantial impacts on their marriage outcomes.[2] On the one hand, more schooling may make women more appealing, so they may have a better position in the marriage market (Juhn and Murphy 1997). On the other hand, more education can make women more economically independent, so women with more schooling may have a higher probability of staying out of marriage (e.g., Stevenson and Wolfers 2007; Weiss and Willis 1997). Second, more education improves individuals' social skills, which play an important role in the marital relationship (e.g., Oreopoulos and Salvanes 2011). Third, education can affect individuals' marriage outcomes through its impact on their social networks. Individuals with more schooling usually have fewer age-diverse social networks (Mansour and McKinnish 2014). Also, many women meet their husbands while they attend high

[2] It is well documented that education can increase the wage rate and earnings (e.g., Angrist and Krueger 1991; Card 1999, 2001; Grenet 2013).

school or college (Mare 1991; Goldin 1992). As a result, more schooling may increase women's probability of marrying a similarly aged husband. Last but not least, more education postpones individuals' transition to work status, which may in turn postpone their marriage, since people usually need economic independence to proceed to marriage (Oppenheimer 1988).

Compulsory schooling reforms can affect individuals' marriage outcomes also by generating general equilibrium effects, regardless of individuals' own educational attainment. To illustrate, compulsory schooling reforms increase both women's and men's schooling, so the reforms can change both the distributions of women's and men's education in the marriage market. Hence, the reforms can affect women's marriage outcomes by changing the schooling of potential husbands and competing women, regardless of women's own educational attainment. Thus, the overall effects of the reforms would capture both the effects of an increase in women's own educational attainment and potential general equilibrium effects.

2.2 The compulsory schooling reform in Britain

In England and Wales, the school term starts in September, and children must enroll in a primary school after they turn age five. They must stay in school until a certain age which is regulated by the compulsory schooling law. The British compulsory schooling law was changed twice in the twentieth century: in 1947, the minimum school leaving age was increased from 14 to 15, and in 1972, it was raised from 15 to 16. I focus on the second reform in 1972, since the data contain very limited observations of women whose schooling was not affected by the first reform. The second reform was approved by the House of Parliament on March 22, 1972, and was implemented on September 1, 1972 (Statutory Instruments 1972 No. 444).[3]

Individuals who were born after August 31, 1957, had not yet reached the previous legal school leaving age of 15 when the new law was implemented, so they could only leave school in 1973 or later. However, those who were born before September 1, 1957, were already 15 years old when the new law was implemented. If they wanted to leave school at age 15, they could have already done so legally before the new law became effective.

To check whether this new compulsory schooling law induced variation in educational attainment across birth cohorts, I plot the proportion of people who had already left school by the age of 15 against month-year birth cohort in Fig. 1. For comparison, I also plot the proportion of people who had left school by the age of 16 or 17. If the law was really binding, the proportion of people who had left school by the age of 15 would be much smaller among those born in September 1957, compared with those born in August 1957. Because all people could leave school legally at the age of 16, the proportion of people who had left school by the age of 16 or 17 should not be affected. The figure indeed shows that the proportion of people who left school by age 15 drops dramatically at the cutoff: it is more than 30% for those born in August

[3] Enforcement of school attendance: If students are missing from school at compulsory schooling ages, the parents may be prosecuted and may face a fine of up to 2500 pounds, a community order, or a jail sentence up to 3 months.

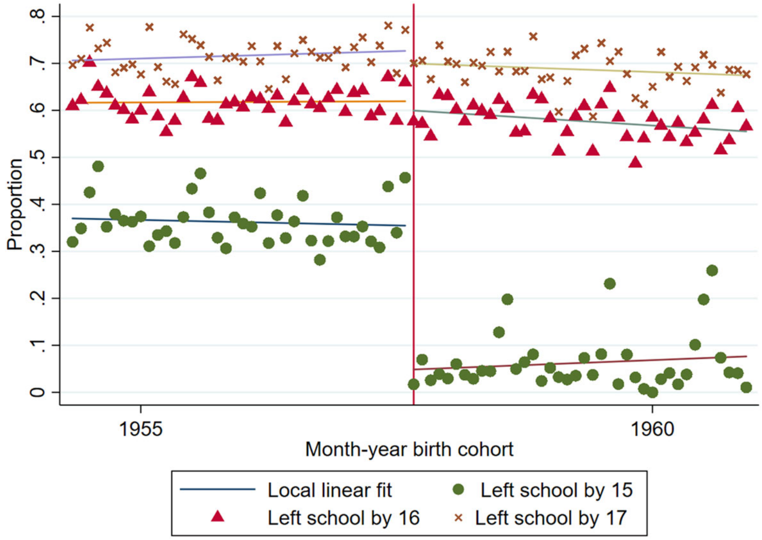


Fig. 1 Age of leaving full time education by month-year birth cohort. Notes: The horizontal axis represents the month-year birth cohort. From top to bottom, the fitted lines represent the proportions of women who had left school by age 17, 16, and 15, respectively. Each dot represents the mean of a cohort. The vertical line denotes the cutoff—September 1957

1957, whereas it is less than 10% for those born in September 1957. By contrast, little change is found in the proportion of people who left school by the age of 16 or 17. These results imply this reform did affect people's school leaving decision.

To the right of the cutoff, there seems to be a small fraction of "non-compliant" cohorts, for whom the proportion of people who left school by age 15 is as high as 20% or even 30%. In fact, these cohorts were mainly born in June, July, and August. They were permitted by law to leave school several months before they reached 16, since they had started attending school earlier and had already finished secondary school by then. This pattern also appears in other studies, such as Clark and Royer (2013). To deal with any possible seasonality which potentially affects both educational attainment and fertility outcomes, I control for "month of birth" dummies in all of the estimations.

3 Strategy

3.1 RD design

We cannot use the reform as an instrument for women's education to estimate the effects of one additional year of schooling on women's marriage outcomes, since the exclusion restriction is likely violated. To illustrate, the reform can increase women's own educational attainment, which can affect their marriage outcomes directly. Meanwhile, the reform substantially changes both women's and men's distributions of education in the marriage market, so it can affect women's marriage outcomes also by changing the schooling of potential husbands and competing women, regardless of women's own education.[4] That is to say, the reform can affect women's marriage outcomes both by increasing women's own educational attainment and by generating general equilibrium effects in the marriage market. Hence, in this study, I estimate the effects of the reform on women's marriage outcomes.[5] The effects of the reform would capture both the effects of an increase in women's own educational attainment and potential general equilibrium effects.

The RD design is appealing here because the compulsory schooling reform in 1972 generated variation in educational attainment across birth cohorts, and there is a clear cutoff—September 1957. Assuming women born just before and just after the cutoff are comparable in terms of ability and preference, we can use the outcome of women born just before the cutoff as the counterfactual for those born just after the cutoff.

3.2 Estimation equations

I use the equation below to estimate the impact of the compulsory schooling reform on women's educational attainment:

$$E_{ic} = \alpha_0 + \alpha_1 R_{ic} + s(Z_{ic}, R_{ic}) + \alpha_2 X_{ic} + \nu_{ic}, \tag{1}$$

where E_{ic} is years of schooling of woman i in cohort c; R_{ic} is the "Reform" dummy, indicating whether a woman is subject to the new compulsory schooling law or not; Z_{ic} is the assignment variable (normalized month-year birth cohort relative to the cutoff); $s(Z_{ic}, R_{ic})$ is a function of the assignment variable Z_{ic}, which allows for different trends on the two sides of the cutoff (specifically, $s(Z_{ic}, R_{ic}) = a_1 Z_{ic} + a_2 R_{ic} \times Z_{ic}$); X_{ic} denotes the other observable characteristics of woman i, such as month of birth and ethnicity; and ν_{ic} is the error term, which captures all of the other unobserved factors which may affect educational attainment, including personal intelligence, family background, etc.

I use the following equation to estimate the impact of the reform on women's marital outcomes:

$$Y_{ic} = \gamma_0 + \gamma_1 R_{ic} + g(Z_{ic}, R_{ic}) + \gamma_2 X_{ic} + \mu_{ic}, \tag{2}$$

where Y_{ic} denotes the marital outcomes of woman i in cohort c; $g(Z_{ic}, R_{ic})$ is a function similar to $s(Z_{ic}, R_{ic})$, which allows for different trends on the two sides of the cutoff; and μ_{ic} is the error term, which captures all of the other unobservable factors which may affect the marital outcomes, including personal intelligence, family background, etc. Because the marital outcomes (e.g., the probability of never being married and the probability of being separated or divorced) vary considerably with age, I also control for age and age squared in the regressions.

[4] See Lefgren and McIntyre (2006) for a detailed discussion about this issue.

[5] Many previous studies also estimate the effects of compulsory schooling reforms directly rather than use the reforms as instruments for schooling (e.g., Black et al. 2008; Kirdar et al. 2018). Also, Godefroy and Lewis (2018) estimate the effects of educational reforms in Mali in 1992 on men's fertility decisions.

The reduced form estimate γ_1 is the parameter of interest, which captures the overall average effect of the reform, including both the effect of an increase in women's own educational attainment and potential general equilibrium effect. Note that using this strategy, I am estimating the overall effect of the reform, rather than the treatment effect of a woman acquiring an extra year of schooling.

In the baseline regressions I use the local polynomial approach (Cattaneo et al. 2019, 2020b). I run the regressions using the Stata package "rdrobust" (Calonico et al. 2017). I use different methods to select the bandwidths (mean squared error optimal versus coverage error-rate optimal) and equal/unequal bandwidths on the two sides of the cutoff. I leave the bandwidth unspecified in the command, i.e., the bandwidth is computed automatically by the companion command "rdbwselect." Besides, although it is not necessary to include additional control variables (X_{ic}) in the RD design, including additional control variables could potentially increase the precision of the estimates. In the regressions, age, age squared, the "white" dummy, and month of birth dummies are included as covariates.

In the baseline regressions, I use the triangular kernel. To check whether my estimates are sensitive to the choice of kernels, I re-run all regressions, using the Epanechnikov kernel. The results are reported in Online Appendix B. Moreover, I complement my results by parametric regressions.[6] Specifically, I use bandwidths from 36 to 72 months and report the results for each 12-month increment. The results are reported in Online Appendix C.

The RD estimates may be sensitive to the polynomial order, so care must also be paid to the choice of polynomial order. Gelman and Imbens (2019) argue that high-order polynomials may perform poorly in some contexts and suggest that high-order polynomials should not be used. Recently, Pei et al. (2021) propose to use the estimated asymptotic mean squared error (AMSE) to guide polynomial order selection. Following their suggestion, I calculate the estimated AMSE for polynomial orders 1, 2, and 3 using the Stata package "rdmse" developed by the authors. For all dependent variables, I find that the estimated AMSE is the smallest when I choose a polynomial order of 1.[7] Thus, in this study I use a polynomial order of 1 in all regressions.

3.3 Identification assumption

The key RD assumption is that the conditional expectations of the potential outcomes are all continuous at the cutoff point in the absence of the reform. This assumption implies two conditions: (1) all of the predetermined variables, observable or unobservable, are continuous at the cutoff point; (2) individuals cannot fully manipulate the assignment variable—month-year birth cohort.

I take two steps to check the validity of the assumption. First, I check the continuity of the predetermined variables. Specifically, I test the continuity of two variables: the

6 For applications of both local polynomial and parametric (global) approaches, see, e.g., Akyol and Kirdar (2020) and Aydemir et al. (2021).

7 For instance, for years of schooling, the estimated AMSE are 0.0060, 0.0132, and 0.0238 for polynomial orders 1, 2, and 3, respectively.

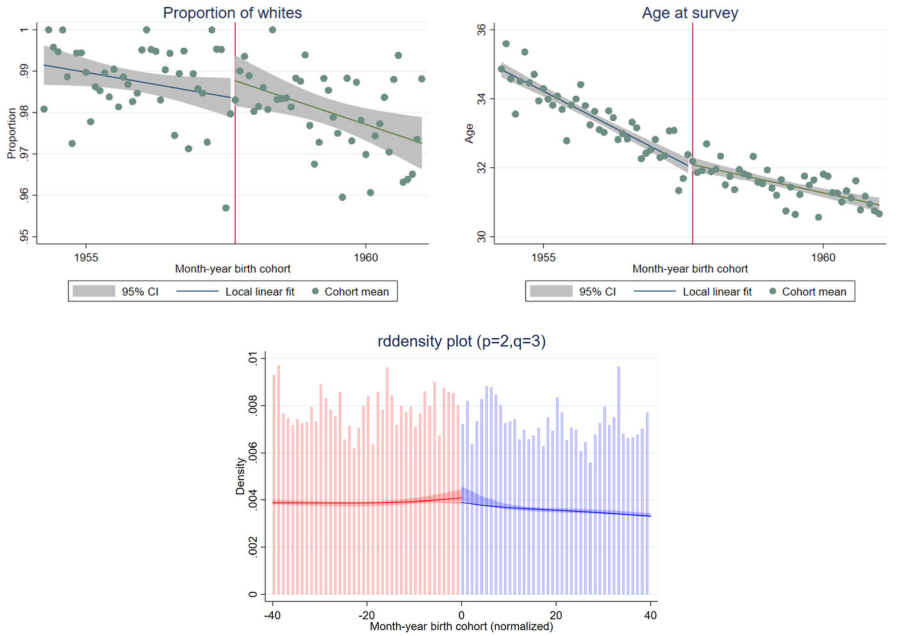


Fig. 2 Balancing check. Notes: (1) In the first two panels, the fitted values of the local linear regressions and the 95% confidence intervals are plotted within the chosen bandwidths. Each dot represents the mean of a cohort, and the vertical line denotes the cutoff—September 1957. (2) In the last panel, the line denotes the point estimate of the density, the shaded area denotes the 95% confidence intervals, and the histogram is plotted in the background

proportion of whites and age at the time of the survey. I do not find any discontinuity at the cutoff point for the two variables, as shown in Fig. 2.

Second, I check whether individuals can fully manipulate the assignment variable. I use the density test developed by Cattaneo et al. (2020), which is based on the comparison of the density of the observations close to the cutoff. I plot the manipulation test and show it in Panel 3 of Fig. 2. I find the test statistic is small (0.288) and the associated *p*-value is large (0.773), suggesting that the null hypothesis of no manipulation cannot be rejected. To summarize, I find that the cohorts just below and just above the cutoff are comparable, indicating that the identification assumption is valid. Thus, I can attribute any discontinuities I find to the effect of the reform.

Moreover, my identification is based on comparisons between the marital outcomes of the cohorts just above and just below the cutoff. Hence, I need to assume that the two cohorts are competing in a common market for husbands. This assumption is reasonable because the age difference between the two cohorts is only one month.

4 Data

The General Household Survey (GHS) was a repeated annual national survey of people living in private households in Great Britain. It was conducted by the Office for

National Statistics and ran from 1972 to 2011.[8] The main aim of the survey was to collect data on a range of core topics, covering household, family, and individual information. The households were selected randomly each year. Because I adopt month-year birth cohort as the assignment variable in the RD design, I use only the surveys containing information on month and year of birth, namely the surveys from 1982 to 2001.[9]

4.1 Measure of educational attainment

In the survey, individuals were asked about the age at which they left full time education. Because the maximum age by which children must start to attend school in Britain is five, the completed years of schooling equals to the age at which an individual left full time education minus five.

4.2 Measures of marital outcomes

The data contain information on the current marital status, which includes five categories: married, single (never got married), divorced, separated, and widowed.[10] Unfortunately, there is no information on marriage history, so I do not know whether the current marriage is the first marriage or not. For those who were ever married, there is information on their age at first marriage; for those who were currently married, there is information on both their age at first marriage and their spouses' age. Women's marital status at teenage years is generated using the information on "age at first marriage."

4.3 Summary statistics

In the RD design, only the observations near the cutoff are useful. Thus, I choose the sample based on the subjects' year of birth. First, I choose the subjects who were born between 1945 and 1970. This provides us with 159,856 observations, among which there are 81,309 females.[11] Then I delete 12,855 observations with missing information on major variables from the female sample.[12] Excluding 13,464 observations who were

[8] The GHS has been carried out continuously every year, except for breaks in 1997–1998 when the survey was reviewed, and 1999–2000 when the survey was redeveloped. The GHS data have been used in many studies, such as Oreopoulos (2007) and Clark and Royer (2013).

[9] The latest survey data sets are not available to researchers outside the UK.

[10] The marriage indicator does not include common law marriage (informal marriage or cohabiting couples). Note that in the UK, common law marriage does not confer on cohabiting parties any of the rights or obligations enjoyed by married spouses or civil partners. Thus, it is reasonable to distinguish common law marriage from formal marriage. Previous studies such as Anderberg and Zhu (2014) also exclude informal marriage or cohabitation from their analysis.

[11] In the sample, the proportion of females is about 0.51 and that of males is about 0.49.

[12] The missing information is mainly on "age left full-time education" and older people are more likely to have missing information. From the survey, we do not know why this information is missing, which is a deficiency of the data. But the missing information is not a big concern here, since my analysis focuses on women who are relatively younger (25–46).

younger than 25 at the time of survey gives us a sample of 54,990 observations.[13] Lastly, dropping 1025 outliers who claimed they left full-time education before age 10 or after age 30, I have a sample of 53,965 observations, which is the full sample.

The upper panel of Table 1 shows the summary statistics for the full sample. Their ages range between 25 and 66, with a mean of 35. The proportion of whites is about 94%, and the proportion of immigrants is about 8.7%. The sample is representative since the statistics are very close to the census estimates.[14] The average years of schooling is about 12.2. For marital status, about 16.5% of the women have never been married before, 72.2% are currently married, about 7.2% are divorced, 3.3% are separated, and about 0.8% are widowed. For ever married women, the mean age at first marriage is about 22. For currently married women, the mean age of their husbands is 38, and the husbands are about 2.63 years (31.56 months) older than the wives on average.

Besides, I report the summary statistics for the "discontinuity sample," which consists of women born close to the cutoff (women born between September 1954 and August 1960). Immigrants are excluded since I cannot calculate their completed years of schooling.[15] The lower panel of Table 1 shows the summary statistics for the "discontinuity sample." The characteristics of the women in the "discontinuity sample" are similar to those in the full sample, except that they are about 2.65 years younger on average, and the proportion of whites is about 4.4 percentage points higher.

5 Results

In this section, I first estimate the impact of the reform in 1972 on women's years of schooling. Then I investigate the effects of the reform on women's marital outcomes.

5.1 The effects on women's years of schooling

First, to examine the impact of the reform on women's years of schooling, I plot women's years of schooling against their birth cohorts in Fig. 3. I impose linear trends and the 95% confidence intervals on the two sides of the cutoff separately to fit the relationship between years of schooling and birth cohort. Each dot represents the mean of a month-year birth cohort. I find that there is a significant jump in years of schooling at the cutoff, and the confidence intervals do not overlap with each other at the cutoff.

[13] In my sample, the 50th percentile of age at first marriage is about 22, and the 75th percentile is about 25. Because I explore the effects on women's probabilities of marriage and divorce, it is better not to include women who are still at a relatively young age (for example at 20 years old). As a result, I choose a sample of women aged 25 and above. Moreover, I have tried to restrict my sample to women aged 20 and above and found that all the results are similar.

[14] In the 2001 census, the proportion of immigrants is about 8.3% and the proportion of whites is about 92.12%. The sample statistics are reasonable because the surveys were conducted in the 1980s and 1990s, when the proportion of whites was a little bit higher.

[15] The immigrants are excluded for two reasons. First, I do not know when they arrived in the UK, so I cannot decide whether they were affected by the law or not. Second, the immigrants came from different countries, so they could have started school at different ages in their home countries.

Table 1 Summary statistics

Variables	(1) N	(2) mean	(3) sd	(4) min	(5) max
		Full sample			
Age	53,965	35.11	6.748	25	56
White	53,965	0.940	0.238	0	1
Age left school	53,965	17.20	2.859	10	30
Never married	53,965	0.165	0.371	0	1
Married	53,965	0.722	0.448	0	1
Divorced	53,965	0.0722	0.259	0	1
Separated	53,965	0.0333	0.179	0	1
Widowed	53,965	0.00782	0.0881	0	1
Age at first marriage	44,504	21.76	3.690	15	49
Age of spouse	41,916	38.05	7.864	18	85
Age gap	41,916	2.632	4.594	−24	39
Year of birth	53,965	1955	6.570	1945	1970
Month of birth	53,710	6.348	3.414	1	12
Survey year	53,965	1991	5.056	1982	2001
Immigrant	53,965	0.0869	0.282	0	1
		Discontinuity sample			
Age	13,343	32.46	4.907	25	46
White	13,343	0.984	0.125	0	1
Years of schooling	13,343	12.26	2.706	6	25
Never married	13,343	0.170	0.375	0	1
Married	13,343	0.721	0.449	0	1
Divorced	13,343	0.071	0.257	0	1
Separated	13,343	0.035	0.184	0	1
Widowed	13,343	0.00345	0.0586	0	1
Age at first marriage	11,057	21.55	3.632	15	43
Age of spouse	10,309	35.15	6.449	18	72
Age gap	10,309	2.615	4.499	−18	38
Year of birth	13,343	1957	1.753	1954	1960
Month of birth	13,343	6.344	3.416	1	12
Survey year	13,343	1990	4.862	1982	2001

The upper panel shows the summary statistics for the full sample, i.e., women who were born between 1945 and 1970. The lower panel shows the summary statistics for the "discontinuity" sample, i.e., women who were born between September 1954 and August 1960. Immigrants are excluded from the discontinuity sample because we cannot calculate their completed years of schooling

To estimate the effect of the reform on women's years of schooling, I regress women's years of schooling on the "Reform" dummy and the other control variables, allowing for different linear trends on the two sides of the cutoff. I report the estimation results in Table 2. I use equal bandwidths on the two sides of the cutoff in regressions

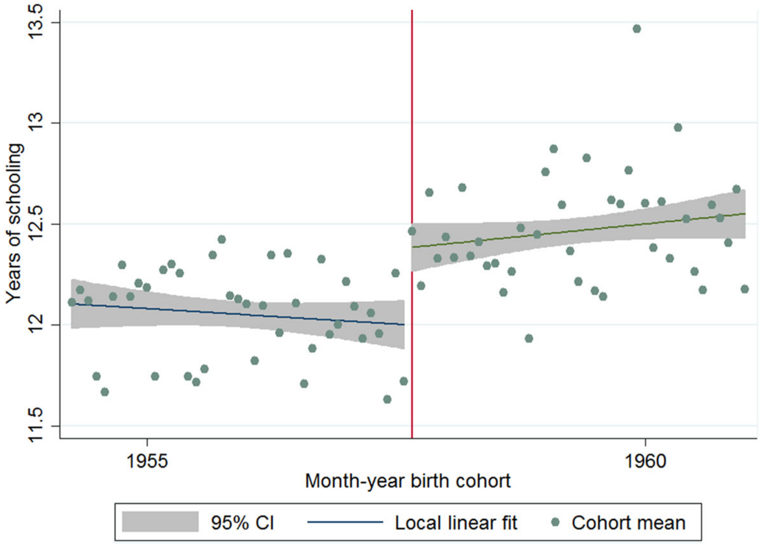


Fig. 3 Impact of the reform on women's educational attainment. Notes: The fitted values of the local linear regressions and the 95% confidence intervals are plotted within the chosen bandwidths. Each dot represents the mean of a cohort. The vertical line denotes the cutoff—September 1957

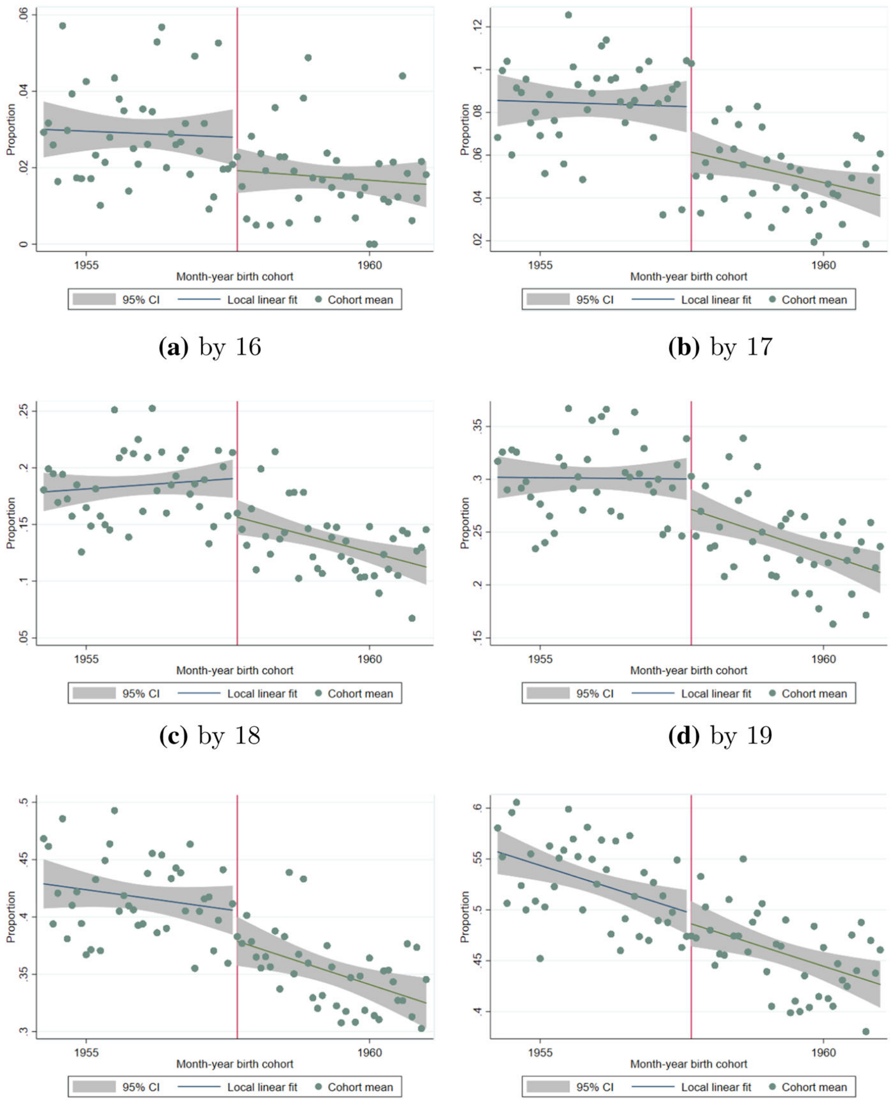
Table 2 Effects of the reform on years of schooling

Variables	(1)	(2)	(3)	(4)
	Years of schooling			
Reform	0.429***	0.482***	0.415***	0.474***
	(0.096)	(0.088)	(0.096)	(0.091)
Bandwidth: left	23.42	17.57	27.25	20.45
Bandwidth: right	23.42	17.57	23.58	17.69
Obs.: left	4469	3376	5200	3948
Obs.: right	4359	3278	4359	3278
Kernel	Triangular	Triangular	Triangular	Triangular
Bandwidth type	mserd	cerrd	msetwo	certwo

The estimates are obtained using the Stata package "rdrobust," and the bandwidths are computed automatically by the companion command "rdbwselect." The "Reform" dummy equals one if a woman is subject to the new compulsory schooling law. The "white" dummy and month of birth dummies are included as covariates. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

(1) and (2) and unequal bandwidths in regressions (3) and (4). The bandwidth type indicates the method used to calculate the bandwidth.

The estimated coefficient on the "Reform" dummy is significant at the 1% significance level in all columns, and the magnitude ranges from 0.42 to 0.48. Besides, the estimates are similar when I use the Epanechnikov kernel (Appendix Table B1). Moreover, the estimates from the parametric regressions indicate that the reform increases women's years of schooling by about 0.30 to 0.34 years (Appendix Table C1). Over-



(a) by 16 **(b)** by 17

(c) by 18 **(d)** by 19

(e) by 20 **(f)** by 21

Fig. 4 The impact of the reform on marriage rate by a given age. Notes: The fitted values of the local linear regressions and the 95% confidence intervals are plotted within the chosen bandwidths. Each dot represents the mean of a particular cohort. The vertical line denotes the cutoff—September 1957

all, the estimates are comparable to those of the previous studies in the UK setting. For instance, Clark and Royer (2013) find that the 1972 reform in Britain increases women's years of schooling by about 0.314 years, Grenet (2013) finds that the 1972 reform increases women's years of schooling by about 0.31 years, and Silles (2011) finds the 1972 reform increases women's years of schooling by about 0.26 years.

5.2 The effects on women's marital outcomes

5.2.1 The probability of marriage as a teenager

First, I plot the probability of getting married by a given age against birth cohort in Fig. 4. The discontinuity at the cutoff is larger in panels (b), (c), and (d), implying the reform has larger effects on the probabilities of getting married by ages 17, 18, and 19.

I estimate the impact of the reform on the probability of getting married by ages 16 to 23 and display the results in Panels A to H of Table 3, respectively. In Panel A, the estimates are all negative and significant, and the magnitude ranges from 0.6 to 0.9 percentage points, indicating that the reform reduces women's probability of getting married by age 16. The estimates in Panel B suggest that the effects on the probability of getting married by age 17 are insignificant. The estimates in Panel C indicate that the reform reduces women's probability of getting married by age 18. The estimates are all significant, and the magnitude ranges from 1.4 to 2.6 percentage points. The estimates in Panels D and E are negative but insignificant. Besides, the estimates in Panels F to H are all insignificant. The results are similar when I use the Epanechnikov kernel (Appendix Table B2).

Moreover, the estimates from the parametric regressions suggest that the reform significantly reduces women's probability of getting married by age 16 to 19, but the effects are insignificant after age 20 (Appendix Table C2). Overall, the results in Table 3 and Appendix Table C2 imply that the reform reduces women's probability of marriage as a teenager, but the effects disappear after age 20.

This reform forces individuals to go to school until age 16, but the effects of the reform persist to ages when the compulsory schooling law is no longer binding. One possible explanation is that the rise in school leaving age increases women's (and men's) work and income prospects, which in turn affect their marriage decisions. Consistent with this argument, Grenet (2013) finds that one additional year of schooling increases individuals' hourly wage rate by 6–7% in Britain, utilizing the same compulsory schooling reform in 1972. Similarly, in the Turkey setting, Kirdar et al. (2018) find that although the Turkey reform forces children to go to school until age 14, the reform significantly reduces women's probability of marriage by age 16.

5.2.2 The probability of never being married

Having found that the reform reduces women's probability of marriage as a teenager, I further check whether the reform affects women's probability of never being married. First, I examine the visual evidence. In Fig. 5, I impose linear trends and the 95% confidence intervals on the two sides of the cutoff separately to fit the relationship between the probability of never being married and birth cohort. Each dot represents the mean of a month-year birth cohort. Figure 5 shows that there is no discontinuity at the cutoff, and the 95% confidence intervals overlap with each other. I do not find any visual evidence that the reform affects the probability of never being married.

I also estimate the effects of the reform on the probability of never being married by running local liner regressions (Eq. (2)). I report the baseline estimates in Panel

Table 3 Effects of the reform on the probability of getting married by a given age

Kernel Bandwidth type	(1) Triangular mserd	(2) Triangular cerrd	(3) Triangular msetwo	(4) Triangular certwo
	Panel A: by age 16			
Reform	−0.006*	−0.009***	−0.006**	−0.009***
	(0.003)	(0.002)	(0.003)	(0.003)
Bandwidth: left	21.65	16.25	17.00	12.76
Bandwidth: right	21.65	16.25	28.16	21.13
Obs.: left	4034	3101	3290	2307
Obs.: right	3951	3067	5045	3951
	Panel B: by age 17			
Reform	−0.005	−0.006	−0.005	−0.008
	(0.011)	(0.010)	(0.011)	(0.011)
Bandwidth: left	21.82	16.38	22.64	16.99
Bandwidth: right	21.82	16.38	24.40	18.31
Obs.: left	4034	3101	4194	3101
Obs.: right	3951	3067	4447	3398
	Panel C: by age 18			
Reform	−0.021**	−0.026***	−0.017**	−0.014**
	(0.009)	(0.006)	(0.008)	(0.007)
Bandwidth: left	20.48	15.37	18.91	14.19
Bandwidth: right	20.48	15.37	29.05	21.80
Obs.: left	3843	2872	3492	2672
Obs.: right	3768	2894	5217	3951
	Panel D: by age 19			
Reform	−0.018	−0.015	−0.015	−0.008
	(0.017)	(0.022)	(0.016)	(0.021)
Bandwidth: left	36.36	21.22	33.66	19.64
Bandwidth: right	36.36	21.22	59.16	34.52
Obs.: left	6710	4034	6191	3645
Obs.: right	6474	3951	10010	6153
Bandwidth type	mserd	cerrd	msetwo	certwo
	Panel E: by age 20			
Reform	−0.017	−0.010	−0.018	−0.005
	(0.017)	(0.023)	(0.017)	(0.022)
Bandwidth: left	40.07	23.38	37.94	22.14
Bandwidth: right	40.07	23.38	49.29	28.76
Obs.: left	7520	4338	6885	4194
Obs.: right	7153	4277	8510	5045

Table 3 continued

Kernel Bandwidth type	(1) Triangular mserd	(2) Triangular cerrd	(3) Triangular msetwo	(4) Triangular certwo
	Panel F: by age 21			
Reform	0.001	0.011	0.002	0.009
	(0.017)	(0.022)	(0.017)	(0.022)
Bandwidth: left	45.77	26.71	49.95	29.14
Bandwidth: right	45.77	26.71	39.18	22.86
Obs.: left	8458	4875	9193	5438
Obs.: right	7920	4748	6988	4107
	Panel G: by age 22			
Reform	0.006	0.020	0.004	0.020
	(0.017)	(0.022)	(0.017)	(0.022)
Bandwidth: left	42.21	24.63	41.72	24.34
Bandwidth: right	42.21	24.63	48.87	28.51
Obs.: left	7917	4510	7725	4510
Obs.: right	7466	4447	8358	5045
	Panel H: by age 23			
Reform	−0.005	0.008	−0.006	0.008
	(0.014)	(0.019)	(0.015)	(0.019)
Bandwidth: left	57.51	33.56	51.37	29.97
Bandwidth: right	57.51	33.56	57.74	33.69
Obs.: left	10711	6191	9593	5438
Obs.: right	9718	5991	9718	5991

The estimates are obtained using the Stata package "rdrobust," and the bandwidths are computed automatically by the companion command "rdbwselect." The "Reform" dummy equals one if a woman is subject to the new compulsory schooling law. The "white" dummy and month of birth dummies are included as covariates. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

A of Table 4. I find that none of the estimates are statistically significant at the 10% significance level. Besides, the magnitudes range from 0.6 to 1.2 percentage points. The magnitude of the coefficient is small, compared to the mean of the dependent variable (see Table 1), so we can conclude that the reform has no effects on women's probability of never being married. Moreover, as a robustness check, I estimate the effects using a sub-sample of women aged 30 and above and report the results in Panel B.[16] The estimates are similar to those in Panel A. Overall, the estimates in Panels A and B of Table 4 suggest that the reform has no effects on the probability of never being married.

[16] Note that in my sample, the 95th percentile of age at first marriage is 29 years old. Thus, I choose a sub-sample of women who are aged 30 and above. I also tried to estimate the effects using a sub-sample of women aged 40 and above. But there are very few observations aged above 40 in my sample, which cannot generate meaningful estimates.

Table 4 Effects of the reform on the other marital outcomes

Kernel Bandwidth type	(1) Triangular mserd	(2) Triangular cerrd	(3) Triangular msetwo	(4) Triangular certwo
Panel A: The probability of never being married (women aged 25 and above)				
Reform	−0.007	−0.012	−0.006	−0.011
	(0.013)	(0.017)	(0.012)	(0.016)
Bandwidth: left	42.53	24.78	41.95	24.45
Bandwidth: right	42.53	24.78	48.39	28.20
Obs.: left	8149	4644	7950	4644
Obs.: right	7617	4530	8531	5140
Panel B: The probability of never being married (women aged 30 and above)				
Reform	−0.015	−0.016	−0.014	−0.016
	(0.017)	(0.022)	(0.016)	(0.020)
Bandwidth: left	36.62	21.64	48.26	28.52
Bandwidth: right	36.62	21.64	33.47	19.78
Obs.: left	4955	2817	6936	3749
Obs.: right	4124	2545	3829	2319
Panel C: The probability of being divorced or separated				
Reform	0.013	0.009	0.012	0.012
	(0.011)	(0.015)	(0.011)	(0.014)
Bandwidth: left	37.34	21.76	52.76	30.75
Bandwidth: right	37.34	21.76	32.03	18.67
Obs.: left	7087	4144	10083	5814
Obs.: right	6768	4026	5873	3462
Panel D: The age gap between spouses				
Reform	−0.168	−0.225	−0.290**	−0.359***
	(0.154)	(0.145)	(0.122)	(0.132)
Bandwidth: left	23.25	17.45	39.50	29.64
Bandwidth: right	23.25	17.45	19.88	14.92
Obs.: left	3511	2645	5922	4388
Obs.: right	3310	2502	2771	2127
Panel E: The probability of marrying a similarly aged husband				
Reform	0.048***	0.055***	0.051***	0.058***
	(0.015)	(0.010)	(0.015)	(0.011)
Bandwidth: left	19.58	14.69	18.98	14.24
Bandwidth: right	19.58	14.69	20.49	15.38
Obs.: left	2940	2127	2812	2127
Obs.: right	2771	2127	2927	2249

The estimates are obtained using the Stata package "rdrobust," and the bandwidths are computed automatically by the companion command "rdbwselect." The "white" dummy, age, age squared, and month of birth dummies are included as covariates. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

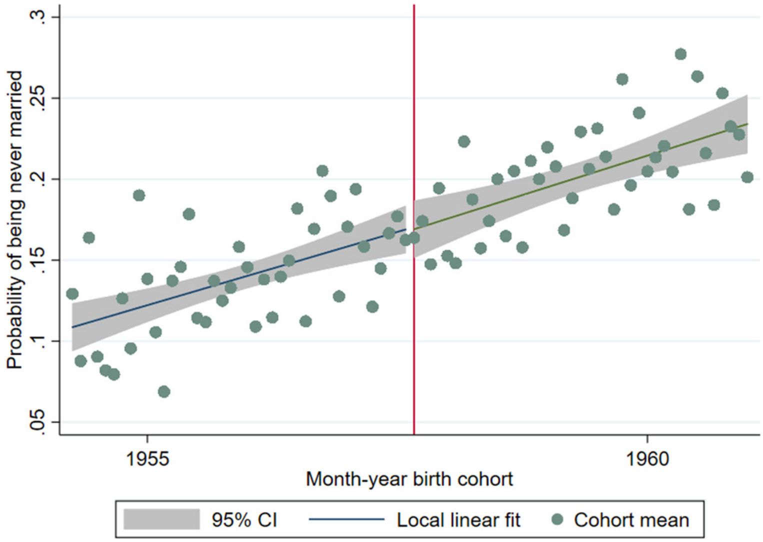


Fig. 5 Effects of the reform on the probability of never being married. Notes: The fitted values of the local linear regressions and the 95% confidence intervals are plotted within the chosen bandwidths. Each dot represents the mean of a cohort. The vertical line denotes the cutoff—September 1957

Besides, the results are similar when I use the Epanechnikov kernel (Appendix Table B3, Panels A and B). Moreover, the estimates from the parametric regressions also suggest that the reform has no effects on women's probability of never being married (Appendix Table C3).

5.2.3 Marital instability

For ever married women, I examine whether the reform reduces their probability of being divorced or separated. To do so, I need to assume that the reform has no effects on women's probability of marriage. This assumption is plausible since I have just found that the reform has no effects on women's probability of never being married. Besides, previous studies also find that women's education has no effects on their probability of marriage (Anderberg and Zhu 2014; Lefgren and McIntyre 2006).

In Fig. 6, I impose linear trends and the 95% confidence intervals on the two sides of the cutoff separately to fit the relationship between the probability of being divorced or separated and birth cohorts. Each dot represents the mean of a month-year birth cohort. The figure shows that there is a moderate jump at the cutoff, but the confidence intervals overlap a little bit at the cutoff.

I estimate the effects of the reform on the probability of being divorced or separated by running local liner regressions. I report the estimates in Panel C of Table 4. Because the probability of being divorced or separated is likely correlated with age, I also control for age and age squared in the regressions. I find that none of the estimates are statistically significant at the 10% significance level, and the magnitudes range from 0.9 to 1.3 percentage points. The magnitudes are not small, compared to the mean of

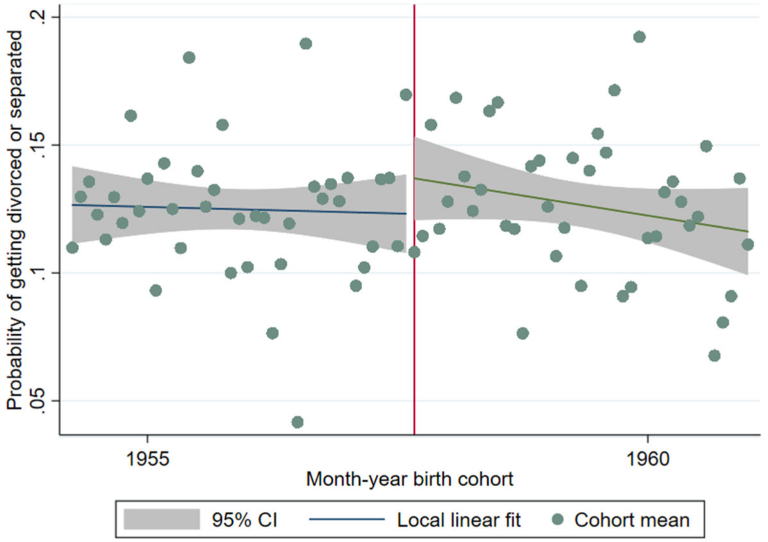


Fig. 6 Effects of the reform on the probability of being divorced or separated. Notes: The fitted values of the local linear regressions and the 95% confidence intervals are plotted within the chosen bandwidths. Each dot represents the mean of a cohort. The vertical line denotes the cutoff—September 1957

the dependent variable (see Table 1). Hence, we cannot conclude that the reform has no effects on the probability of being divorced or separated.

Besides, the results are similar when I use the Epanechnikov kernel (Appendix Table B3, Panel C). Moreover, the estimates from the parametric regressions are generally small in magnitude and statistically insignificant, except column (1) (Appendix Table C4, Panel A).

5.2.4 The age gap between spouses

The age gap between spouses matters because it may affect marital satisfaction. For example, differently aged couples have been found to suffer a more rapid decline in marital satisfaction, compared to similarly aged couples (Lee and McKinnish 2018). As a result, differently aged couples may be more likely to divorce than similarly aged couples (Cherlin 1977; Lillard et al. 1995).

For currently married women, I explore the effects of the reform on the age gap between spouses. I first examine the visual evidence shown in Fig. 7. I impose linear trends and the 95% confidence intervals on the two sides of the cutoff separately to fit the relationship between age gap and birth cohorts. Each dot represents the mean of a month-year birth cohort. The figure shows that there is a significant drop at the cutoff, and the confidence intervals do not overlap with each other.

I estimate the effects of the reform on the age gap between spouses by running local linear regressions and report the results in Panel D of Table 4. The estimates are all negative in columns (1) to (4). When I use equal bandwidths on the two sides of the cutoff, the estimates are insignificant (columns (1) and (2)). When I use unequal

Fig. 7 Effects of the reform on the age gap between spouses. Notes: The fitted values of the local linear regressions and the 95% confidence intervals are plotted within the chosen bandwidths. Each dot represents the mean of a cohort. The vertical line denotes the cutoff—September 1957

bandwidths on the two sides of the cutoff, the estimates are all significant and the magnitudes range from 0.29 to 0.36 (columns (3) and (4)).

Besides, the results are similar when I use the Epanechnikov kernel (Appendix Table B3, Panel D). Moreover, the estimates from the parametric regressions suggest that the reform significantly reduces the age gap between spouses by about 0.27 to 0.34 years (Appendix Table C4, Panel B). Overall, the estimates suggest that the reform significantly reduces the age gap between spouses.

The probability of marrying a similarly aged husband

I check whether the reform reduces the age gap between spouses by increasing women's probability of marrying a similarly aged husband.[17] Here, I define a similarly aged husband as follows: the husband has exactly the same age as the wife or the husband is only one year older than the wife.[18]

I first plot the probability of marrying a similarly aged husband against birth cohort in Fig. 8. I impose linear trends and the 95% confidence intervals on the two sides of the cutoff separately to fit the relationship between the probability of marrying a similarly aged husband and birth cohorts. I find there is a jump in the probability of marrying a similarly aged husband at the cutoff.

[17] I would like to check whether the reform increases women's probability of marrying a classmate. But in the data there is no information on how the spouses came to know each other.

[18] I have tried an alternative way to define a similarly aged husband: the husband has exactly the same age as the wife or the husband is one year older or younger than the wife. The results are similar.

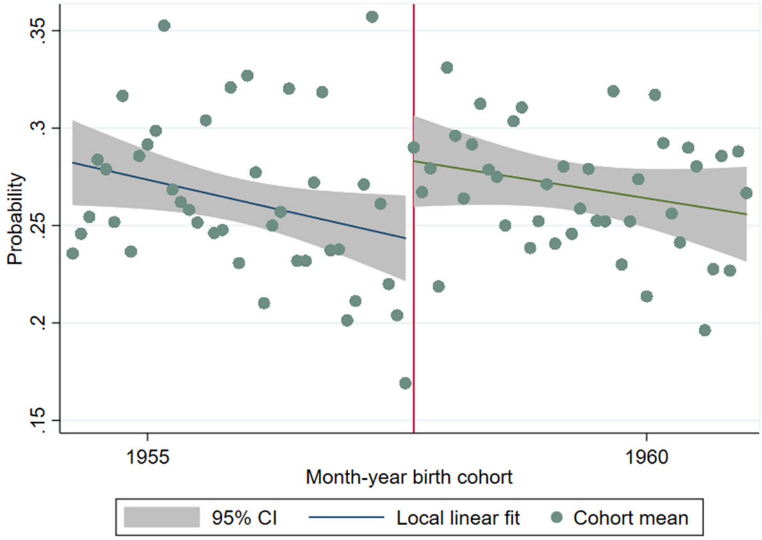


Fig. 8 Effects on the probability of marrying a similarly aged husband. Notes: The fitted values of the local linear regressions and the 95% confidence intervals are plotted within the chosen bandwidths. Each dot represents the mean of a cohort. The vertical line denotes the cutoff—September 1957

Then I estimate the effects of the reform on the probability of marrying a similarly aged husband and report the results in Panel E of Table 4. The estimates are all positive and significant, and the magnitudes range from 4.8 to 5.8 percentage points. Besides, the results are similar when I use the Epanechnikov kernel (Appendix Table B3, Panel E). Moreover, the estimates from the parametric regressions suggest that the reform significantly increases women's probability of marring a similarly aged husband by about 2.5 to 5.0 percentage points (Appendix Table C4, Panel C).

Overall, the estimates suggest that the reform may reduce the age gap between spouses by increasing women's probability of marrying a similarly aged husband. The findings imply that the reform strengthens assortative mating in terms of age.

6 Robustness

6.1 Estimates at alternative cutoffs (Placebo tests)

My identification strategy relies on comparison of cohorts born just before and just after the cutoff point. One may concern that the "reform" dummy captures some unspecified time trend or the effect of a structural change instead of a true treatment effect. To address this concern, I conduct placebo tests. I construct hypothetical compulsory schooling reforms which "took place" 7, 5, and 3 years before and after the actual reform, respectively. By definition, the placebo reforms should have no impact on either women's educational attainment or their marriage outcomes. The placebo tests are equivalent to estimating the treatment effect at alternative cutoff values, which is a falsification test in the RD design (Cattaneo et al. 2020b).

The results are displayed in Table 5. We can see that for each dependent variable, the estimated coefficients have different signs across the columns. Also, although the estimates in column (4) of Panel D and column (5) of Panels D, E, and F are significant, they have the opposite signs to the baseline estimates. Although there are a few exceptions, the estimated coefficients on the placebo reforms are generally small in magnitude and statistically insignificant. The results of the placebo test indicate that the baseline estimates are unlikely to be driven by unspecified time trend or structural changes.

6.2 Using parametric regressions

In the baseline estimations, I use local polynomial approach. Now I examine whether the results are robust when I use parametric approach. Specifically, I use a polynomial order of 1 and incremental bandwidths: 36, 48, 60, and 72. The results are reported in Online Appendix C. The results are generally similar to the baseline estimates.

6.3 Using subjects born in England and Wales only

In the baseline estimations, I include subjects born in England, Wales, Scotland, and Northern Ireland. Someone may concern that the educational system in Scotland (Northern Ireland) is different from that in England and Wales. To examine whether the baseline results are sensitive to excluding subjects born in Scotland and Northern Ireland, I re-run the regressions, using subjects born in England and Wales only. The results are reported in Online Appendix D. The results are similar to the baseline estimates, except for some changes in the statistical significance. In general, the results reported in Appendix D indicate that the baseline estimates are not sensitive to excluding subjects born in Scotland and Northern Ireland.

7 Conclusion

This paper examines the impact of the compulsory schooling reform in Britain in 1972 on women's marital outcomes, using data from the General Household Survey 1982–2001. First, I find that the reform significantly increases women's years of schooling. Besides, I find that the reform reduces women's probability of marriage by ages 16 to 19, but the effects disappear after age 20, implying that the reform has only temporary effects on women's probability of marriage. Indeed, I find that the reform has no effects on women's probability of never being married. Moreover, for ever-married women, I find that the effects on their probability of being divorced or separated are not statistically significant.

In addition, for currently married women, I find that the reform reduces the age gap between spouses. Specifically, I find the reform reduces the age gap between husband and wife by about 0.3 to 0.4 years. To explore the mechanisms, I find that the reform increases women's probability of marrying a similarly aged husband by about 4.8 to 5.8 percentage points. The results suggest that the reform contributes to assortative mating in terms of age. Previous studies have found that the age gap between spouses may affect marital satisfaction, and differently aged couples may suffer a more rapid decline in marital satisfaction, compared to similarly aged couples (e.g., Lee and McKinnish 2018). Also, other studies have found that differently aged couples may be more likely to divorce than similarly aged couples (e.g., Cherlin 1977; Lillard et al. 1995). In my setting, due to the reform, women just above the cutoff are more likely to marry a similarly aged husband, but they are not less likely to get divorced than the cohorts just below the cutoff. Hence, my findings are in contrast to previous studies which find that differently aged couples may be more likely to divorce than similarly aged couples (e.g., Cherlin 1977; Lillard et al. 1995).

Teenage marriage is found to be associated with a higher risk of divorce than that formed at later ages (e.g., Lampard 2013; Lehrer 2008; Lehrer and Chen 2013; Lehrer and Son 2017). For instance, in England and Wales, women who married under age 20 between 1974 and 1994 had much higher risk of ending in divorce by the tenth anniversary than those who married at later ages (Lampard 2013). Moreover, those who marry in their teens are usually less educated (e.g., Lehrer 2008; Lehrer and Chen 2013; Lehrer and Son 2017; Kiernan 1986; Rotz 2016). Hence, if longer compulsory education can reduce women's probability of marriage as a teenager, these policies might also reduce their probability of divorce. However, this study finds that although the compulsory schooling reform has increased women's educational attainment and reduced their probability of marriage as a teenager, the reform has no significant impacts on their probability of being divorced or separated. Overall, the findings imply that public policies aimed at improving citizens' educational attainment can also have important implications on their marriage outcomes.

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Declarations

Conflict of interest The author declares that he has no conflict of interest.

Appendix

See Table 5.

Table 5 The estimates at alternative cutoffs (placebo tests)

Years shifted	(1) −7	(2) −5	(3) −3	(4) +3	(5) +5	(6) +7
Panel A: Effects on years of schooling						
"Reform"	−0.058	0.066	0.050	−0.176	−0.128	0.134
	(0.136)	(0.120)	(0.112)	(0.117)	(0.153)	(0.170)
Panel B: Effects on the probability of getting married by 16						
"Reform"	0.008	0.003	−0.004	−0.002	0.002	0.006
	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)
Panel C: Effects on the probability of getting married by 17						
"Reform"	0.014	−0.001	−0.008	0.013*	0.007	0.003
	(0.012)	(0.011)	(0.011)	(0.007)	(0.008)	(0.010)
Panel D: Effects on the probability of getting married by 18						
"Reform"	0.011	−0.008	−0.030*	0.018	0.046**	0.021
	(0.018)	(0.015)	(0.016)	(0.012)	(0.019)	(0.014)
Panel E: Effects on the probability of getting married by 19						
"Reform"	0.031	−0.006	−0.041**	0.020	0.062***	0.034
	(0.020)	(0.021)	(0.019)	(0.016)	(0.023)	(0.022)
Panel F: Effects on the probability of getting married by 20						
"Reform"	0.007	−0.007	−0.024	0.013	0.045*	0.014
	(0.026)	(0.021)	(0.017)	(0.018)	(0.025)	(0.023)
Panel G: Effects on the probability of getting married by 21						
"Reform"	0.019	0.004	−0.016	0.016	0.050*	0.034
	(0.023)	(0.020)	(0.018)	(0.019)	(0.027)	(0.023)
Panel H: Effects on the probability of never being married						
"Reform"	−0.002	−0.006	−0.004	−0.018	−0.019	0.002
	(0.014)	(0.011)	(0.012)	(0.018)	(0.022)	(0.027)
Panel I: The probability of getting divorced or separated						
"Reform"	0.018	0.015	0.001	−0.012	0.036*	0.003
	(0.013)	(0.013)	(0.010)	(0.015)	(0.019)	(0.019)
Panel J: Effects on the age gap between spouses						
"Reform"	0.170	0.123	−0.023	0.238	0.141	0.271
	(0.236)	(0.208)	(0.170)	(0.191)	(0.279)	(0.286)
Panel K: The probability of marrying a similarly aged husband						
"Reform"	−0.034	−0.001	0.027	−0.011	0.027	0.005
	(0.025)	(0.019)	(0.018)	(0.020)	(0.026)	(0.027)

The triangular kernel and MSE-optimal bandwidth selector are used in all regressions. To save space, the bandwidths and number of observations are not reported. The "white" dummy and month of birth dummies are included as covariates in all regressions (age and age squared are also included in Panels H–K). $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

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