Education and voting Conservative: Evidence from a major schooling reform in Great Britain

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May 2015

High school education is central to adolescent socialization and has important downstream consequences for adult life. However, scholars examining schooling's political effects have struggled to reconcile education's correlation with both more liberal social attitudes and greater income. To disentangle this relationship, I exploit a major school leaving age reform in Great Britain that caused almost half the population to remain at high school for at least an additional year. Using a fuzzy regression discontinuity design, I find that each additional year of late high school increases the probability of voting Conservative in later life by 12 percentage points. A similar relationship holds when pooling all cohorts, suggesting that high school education is a key determinant of voting behavior and that the reform could have significantly altered electoral outcomes. I provide evidence suggesting that, by increasing an individual's income, education increases support for right-wing economic policies, and ultimately the Conservative party.

1 Introduction

High school is a defining experience of an individual's adolescence, and has been linked to radically different life trajectories. High school education may permanently instill social and political attitudes and determine labor market prospects. Consequently, it has the potential to substantially alter a voter's political preferences and voting behavior in later life, and in turn impact electoral and policy outcomes.

Despite considerable interest in education's effect on political participation (see Sondheimer and Green 2010), strikingly little is known about education's effect on the party an individual chooses to vote for. Existing survey evidence, which has struggled to square the widely-documented correlations between income (which education increases) and support for conservative economic policies (e.g. Clarke et al. 2004; Gelman et al. 2010) and between education and socially liberal attitudes (e.g. Converse 1972; Nie, Junn and Stehlik-Barry 1996; Gerber et al. 2010), has failed to disentangle either the direction of the relationship or its mechanisms. In part, this reflects major empirical challenges stemming from the fact that better educated individuals differ substantially in other important respects, and also that education is itself a cause of many variables that researchers often choose to control for. Furthermore, because the direct link to vote choice has received limited attention, it is also possible that education affects attitudes without impacting vote choices (e.g. Adams, Green and Milazzo 2012).

In this article, I leverage a major educational reform to identify the effects of high school education on downstream voting behavior in Great Britain. In 1944, Winston Churchill's cross-party coalition government passed legislation raising the high school leaving age from 14 to 15. The reform, which came into effect in 1947, induced almost half the student population to remain in school for either one or two additional years (but did not affect tertiary education progression). The magnitude of Britain's 1947 compulsory education reform marks it apart from leaving age reforms in North America and Europe (see Brunello, Fort and Weber 2009; Oreopoulos 2006), and

experimental studies providing unrepresentative participants with incentives to remain in school (Sondheimer and Green 2010). This reform therefore represents a unique opportunity to estimate education's political effects for the lower half of the education distribution. Combining survey data across 10 elections between 1974 and 2010, I use a regression discontinuity (RD) design to compare voters from cohorts just young enough to be affected by the reform to voters from cohorts just too old to have been affected (see Devereux and Hart 2010; Oreopoulos 2006). I first identify the effect of the 1947 reform on the probability of voting for the Conservative party—Britain's most prominent economically conservative party. Given that some students would have remained in school regardless of the higher leaving age, and not quite all were compelled to remain in high school, I then use the 1947 reform to instrument for schooling in order to identify the effect of an additional year of late high school for students that only remained in school because of the reform.

I find that staying in high school substantially increases the likelihood that an individual votes for the Conservative party in later life. In particular, the instrumental variable (IV) estimates show that each additional year of high school increases the probability of voting Conservative by nearly 12 percentage points. For cohorts affected by the reform, this translates into a 4.4 percentage point increase in the Conservative vote share. Although the RD estimates are local to the cohorts aged 14 around 1947, a correlation of similar magnitude holds between completing the final years of high school and voting behavior in the full sample containing all cohorts. This supports the external validity of this finding, and suggests that late high school is a key point at which education affects political preferences. Furthermore, the fact that a similar correlation holds away from the discontinuity implies that Britain's 1947 reform changed the dynamics of national politics, and could have altered the outcomes of the close 1970 and 1992 Conservative election victories. Future educational reforms thus pose an important "catch 22" for the Labour and Liberal parties, who are ideologically committed to expanding educational opportunities for the least educated but also face an electoral cost of such policies.

Beyond demonstrating that education causes voters to support the Conservative party, I provide

evidence suggesting that education's effects operate according to a Meltzer and Richard (1981) distributive logic. By increasing an individual's income, education increases support for right-wing economic policies, which in turn leads an individual to vote Conservative. This mechanism is supported by a number of additional findings. First, education significantly increases income later in life (see Devereux and Hart 2010; Harmon and Walker 1995; Oreopoulos 2006), but only increases Conservative voting before retirement age (when education's effect on income is most salient). Second, and consistent with a permanent increase in income, an individual's greater support for the Conservatives is relatively durable: an additional year of schooling causes individuals to self-identify as partisans, and increases the likelihood that they decide how they will vote before the start of the electoral campaign. Third, education increases support for economic policies associated with the Conservative party, including opposition to higher taxes, redistribution and welfare spending. Fourth, to demonstrate that educated individuals vote Conservative because of their policy platform, rather than the reverse relationship where voters simply adopt the positions of the party they identify with, I show that education does not affect support for non-economic positions associated with the Conservative party. Finally, I find no evidence that additional high school education causes voters to become more socially liberal.

This article proceeds as follows. I first consider the theoretical mechanisms potentially linking schooling and vote choice. I then describe Britain's 1947 leaving age reform, the data and identification strategy. The next section presents the main results. The penultimate section examines the mechanisms linking high school to voting Conservative. I then conclude by considering the implications of the results.

2   Why might high school education affect political preferences?

Arguably the most obvious channel through which education might affect political preferences is via an individual's labor market position. An influential human capital literature argues that

education imparts valuable skills that make workers more productive employees for firms (Becker 1964). These skills are then generally rewarded in terms of higher wages (e.g. Angrist and Krueger 1991; Ashenfelter and Rouse 1998; Oreopoulos 2009). Linking education to political preferences, Romer (1975) and Meltzer and Richard (1981) (henceforth RMR) argue that workers receiving higher wages will prefer lower income tax rates and lower government spending, particularly on means-tested programs, because they are net losers when tax revenues are progressively redistributed. Similar arguments may also apply to expected income, such that voters support conservative policies in anticipation of their higher future income (Alesina and La Ferrara 2005). In the British context, the human capital and RMR models imply that, by increasing their income, greater education should cause voters to become more favorable toward the Conservative party, and particularly the party's relatively fiscally conservative platform.

However, a more sociological literature has instead suggested that education cultivates socially liberal attitudes. Lipset (1959) famously proposed that education encourages liberal attitudes by directly communicating support for toleration and democracy. Hyman and Wright (1979) go further, arguing that—by expanding their frames of reference—education causes students to think in a fundamentally more liberal fashion. Furthermore, the final years of high school may also be a particularly important moment in the crystallization of lifelong political views (Ghitza and Gelman 2014). In Britain, the Labour and Liberal Democrat parties are generally regarded as more socially progressive on issues of crime, immigration and giving voice to the disadvantaged. If education causes voters to become more socially liberal, then Labour and the Liberals may instead be expected to benefit electorally.

Existing evidence examining the relationship between education and political preferences paints a mixed picture. On one hand, there is a robust survey-level correlation between individual income—which education increases (e.g. Angrist and Krueger 1991; Oreopoulos 2006)—and opposition to taxation and redistribution across developed countries (Alesina and La Ferrara 2005; Iversen and Soskice 2001; Shayo 2009). Furthermore, an individual's income is positively corre-

lated with support for right-wing parties in the United States (e.g. Gelman et al. 2010), the United Kingdom (e.g. Clarke et al. 2004; Whitten and Palmer 1996), and Western Europe (e.g. Thomassen 2005).[1] On the other hand, the association between education and socially liberal attitudes and political engagement is also widely documented (e.g. Dee 2004; Nie, Junn and Stehlik-Barry 1996; Phelan et al. 1995). Rather than supporting right-wing parties, this impetus generally seems to push voters toward left-wing parties proposing more socially liberal policies (e.g. Heath et al. 1985; Inglehart 1981).

However, it is hard to attribute a causal interpretation to these intriguing if seemingly conflicting associations. One major problem is that more educated individuals also differ in other key respects, such as possessing greater labor market potential (Spence 1973), coming from more affluent social backgrounds (Jencks et al. 1972), or being exposed to different social and political values as a child (Jennings, Stoker and Bowers 2009). In light of such concerns, Kam and Palmer (2008) suggest that education may simply "proxy" for other variables.[2] Without isolating exogenous variation in education, identifying its effects may not be possible. Furthermore, interpreting existing estimates of education's effects is problematic when most studies also control for various "post-treatment" variables—such as income, partisanship, and social networks—that may themselves a function of education. Including such controls could induce severe post-treatment bias, and the direction of such bias is hard to establish (see King and Zeng 2007). This could explain why empirical analyses using different specifications yield very different conclusions.

Experimental and quasi-experimental studies are required to disentangle the complex layers of causality underpinning education's political effects. Recent work using such methods has made significant progress in identifying schooling's effects on political participation (see Sondheimer and Green 2010). However, the external validity of studies employing field or natural experiments

[1]However, the national-level implications of the RMR model have received mixed support (e.g. Karabarbounis 2011).

[2]Given that education is closely tied with idiosyncratic experiences, it is unlikely that matching designs can resolve such problems (see Henderson and Chatfield 2011; Kam and Palmer 2008).

is often limited by focusing on a small and unrepresentative participant pool.[3] Moreover, such methods have yet to be utilized to identify education's effect on vote choice.

3   Empirical strategy

To estimate the effect of high school education on political preferences, I leverage Great Britain's 1947 school leaving age reform as a source of exogenous variation. In particular, I use a RD design to compare essentially identical students born just too early and just early enough to be affected by the reform. Given the difficulty of identifying education's political effects for a substantial proportion of the population, Britain's 1947 reform—which affected nearly half the student population—represents a rare opportunity to disentangle the causal effects of education for a large and important segment of relatively uneducated voters.

3.1   Britain's 1947 school leaving age reform

Britain's education laws define the maximum age by which a student must start school and the minimum age at which they can leave school. In 1944, when barely 50% of students received any formal education beyond the age of 14, legislation was enacted to increase the school leaving age from 14 to 15 for all students. The landmark compulsory schooling reform was designed to increase the efficiency of the labor force and create a fairer society in recognition of the population's successful war effort (Woodin, McCulloch and Cowan 2013*b*), and was passed by Winston Churchill's cross-party coalition government. The Education Act 1944 raised the leaving age in England and Wales, while the Education (Scotland) Act 1945 subsequently enacted the same re-

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[3]For example, two of the experiments analyzed by Sondheimer and Green (2010) focused exclusively on children from families with very low income, while the U.S. compulsory schooling laws examined by Dee (2004) and Milligan, Moretti and Oreopoulos (2004) affected only a small fraction of the U.S. population.

form in Scotland.[4] The new leaving age, which had repeatedly failed to pass in the 1920s and 1930s due to financial constraints and the opposition of businesses seeking cheap labor (Gillard 2011; Woodin, McCulloch and Cowan 2013*b*), came into force on April 1st 1947 after several years of intensive preparation. A similar reform, albeit containing various exemptions to a universal increase in the leaving age (to mitigate costs and help parents that depended on their children's income), had been passed in the Education Act 1936, but had been delayed by the war (Woodin, McCulloch and Cowan 2013*b*). The additional year of schooling was primarily intended to ensure that students grasped the material they had previously been taught (Clark and Royer 2013). The Online Appendix describes the 1944 Act in greater detail, and locates it in the context of other (less major) educational reforms in Britain.[5]

The 1947 reform, which is arguably the largest post-war reform undertaken by any industrialized democracy, substantially increased educational attainment for a large proportion of Britain's students. As shown in Figure 1, the reform induced almost half of the student population to remain in school for at least an additional year. The majority only remained in school until age 15, but a non-trivial proportion continued until age 16 (the age at which most students complete high school). The proportion of students attending university, however, was unaffected. Therefore, in contrast to compulsory schooling reforms in Europe and North America that only affected a small and relatively unrepresentative set of students (see Brunello, Fort and Weber 2009; Oreopoulos 2006), Britain's 1947 reform will allow me to identify the effect of late high school education for almost the entire lower half of the education distribution.

Given that the most significant post-war changes in the education system had already been

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[4]No such reform occurred in Northern Ireland until 1957, which is not included in the analysis. Furthermore, the parties and political cleavages in Northern Ireland differ substantially from those in Great Britain.

[5]Britain's second major educational reform, which raised the school leaving age to 16, was implemented in 1972. Although this reform also kept students in school for longer, the first stage in the data used here is weak. Nevertheless, the noisy IV estimates are of a similar magnitude to the results presented below for the 1947 reform.
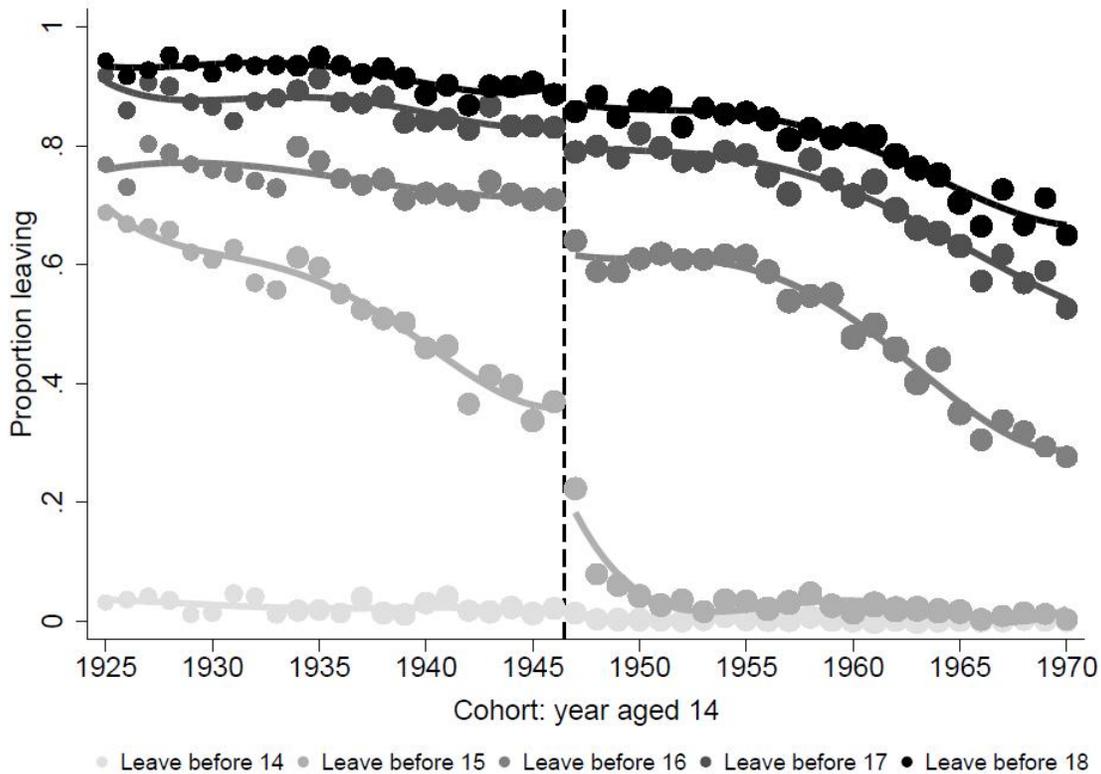
Figure 1: 1947 compulsory schooling reform and student leaving age by cohort

Notes: Data from the BES (described below). Curves represent fourth-order polynomial fits. Grey dots are birth-year cohort averages, and their size reflects their weight in the sample.

implemented by 1947, the large rise in enrollment reflected the higher leaving age rather than other changes in the education system. Fees for secondary schooling were removed in 1944, while the new Tripartite system—which formally established three types of secondary school emphasizing academic, scientific and practical skills—came into force in 1945. However, as Figure 1 indicates, these structural reforms did not affect enrollment (see also Oreopoulos 2006). Furthermore, prior to the 1947 reform, the government pre-emptively engaged in a major expansion effort to maintain school quality by increasing the number of teachers, buildings and classroom materials (Woodin, McCulloch and Cowan 2013*a*). Despite this, pupil-teacher ratios inevitably increased somewhat as the emergency measures to expand capacity could not fully match demand.

Although the end of the war allowed the British government to shift its public spending fo-

cus toward domestic issues, public spending dropped significantly. After spending well over £5 billion a year, public sector spending normalized and reached a low of £4 billion in 1947 as the government sought to generate a budget surplus to repay its wartime debts. Spending increased in the 1950s as the National Health Service expanded following its roll-out on July 5th 1948, and the Beveridge Report's social welfare provisions were implemented. Such universal programs did not differentially impact cohorts either side of the school leaving age reform.

3.2 Data

I use repeated cross-sectional surveys from the British Election Survey (BES) to examine the reform's political implications. The BES, which randomly samples voting age citizens with British postal addresses for in-person interviews,[6] has been conducted following every general election since 1964. The nine elections from 1974 to 2010, where the relevant variables are available, produced a maximum sample of 24,439 observations.

The empirical analysis utilizes three key variables. First, the principal outcome is an indicator for voting Conservative at the last election. In the sample, 35% of respondents reported voting Conservative, while 37% and 19% respectively reported voting Labour and Liberal. Suggesting that reported voting is relatively reliable, the survey-weighted Conservative vote share across the elections used in this study is 37%. To understand how changes in Conservative support affect other parties, I will also examine indicators for voting Labour and Liberal. Second, I define the minimum schooling leaving age for an individual in (birth year) cohort c by an indicator for whether the reform was binding when the student was aged 14, i.e. *Post 1947 reform$_c$* $= 1(Birth\ year + 14 \geq 1947)$.[7] Finally, I measure education as the number of years of schooling. This was computed by subtracting five—the age at which students start school—from the age at which a respondent

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[6]Additional pre-election and non-interview surveys were excluded.

[7]Month of birth is unavailable, so the instruments are assigned by birth year. However, the first stage is very similar to Clark and Royer (2013), who can assign the instruments using month of birth data. The clear graphical discontinuity in Figure 1 further supports this coding.

reported leaving formal schooling. Given that using a binary measure of an endogenous treatment variable such as completing high school can substantially upwardly bias IV estimate (Marshall 2015a), years of schooling represents a conservative coding approach that guarantees a consistent estimate of the average effect of an additional year of schooling. The Online Appendix provides detailed variable definitions and summary statistics.

3.3 Identification and estimation

To identify the effect of late high school education on vote choice, I exploit Britain's 1947 school leaving age reform as a natural experiment. Among cohorts aged around 14 in 1947, being subject to the higher leaving age effectively randomly assigned a strong incentive to remain in school for an additional year. Accordingly, I employ a RD design to identify the effect of the reform, where the running variable determining whether an individual is "treated" by the 1947 reform is an individual's birth year cohort. Since the reform could not force every student to remain in school, to estimate the effect of an additional year of late high school education I also leverage a "fuzzy" RD design where the 1947 reform is used as an instrument discontinuously increasing the *probability* of receiving an additional year of education (see Hahn, Todd and Van der Klaauw 2001).[8]

The key identifying assumption is that the decision to vote Conservative is continuous across cohorts at the reform discontinuity in all variables other than the school leaving age. In this particular case, there are good reasons to doubt the "sorting" concern that another key variable simultaneously changes at the discontinuity. First, selection into cohorts in Britain is implausible since parents could not have precisely predicted the 1947 reform more than a decade in advance. Formal tests in the Online Appendix confirm that there is no discontinuous change in the mass of respondents in the sample that were born either side of the reform. Second, broad shifts in political culture

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[8]Given the dramatic change in educational attainment it induced across neighboring cohorts, the reform has proved popular as an instrument for education among labor economists (see Clark and Royer 2013; Oreopoulos 2006). However, it has not been used in a political context.

are unlikely to have affected 15 year olds without also affecting 14 year olds. Flexible birth year trends are also included to address this concern. Furthermore, since cohorts born either side of the cutoff were first eligible to vote at the 1955 election, there is no differential "first election" effect whereby students facing a higher leaving age were first eligible to vote at a different election (e.g. Meredith 2009; Mullainathan and Washington 2009). Third, Figure 2 shows that pre-treatment demographic, socio-economic and labor market characteristics are essentially continuous through the discontinuity.[9] Fourth, the Online Appendix indicates that there is no significant change in Conservative support when treating any of the ten years prior to 1947 as placebo reforms. These placebo tests suggest that the 1947 reform is not simply capturing pre-trends or other proximate social or institutional changes.

To identify the effect of the 1947 reform itself on voting Conservative, I estimate the following reduced form regression using OLS:

$$\textit{Vote Conservative}_{ic} = \delta \textit{Post 1947 reform}_c + f(\textit{Birth year}_c) + \varepsilon_{ic}, \qquad (1)$$

where f is a flexible function of the running variable used to control for trends in Conservative support away from the discontinuity. In particular, I estimate local linear regressions (LLRs) where only observations within the Imbens and Kalyanaraman (2012) optimal bandwidth (of 14.7 cohorts) are included in the sample. The average age of a respondent in this sample, at the time of the survey, is 56. To ensure the comparability of treated and untreated cohorts, observations are weighted by their proximity to the discontinuity using a triangular kernel.[10] As robustness checks, I show below that the results do not depend upon the choice of bandwidth, kernel, or polynomial order of the local cohort trends.

The principal theoretical quantity of interest, however, is the effect of schooling on voting

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[9]Tests in the Online Appendix confirm that there is no significant change in the gender or racial composition of the sample or the proportion of respondents whose fathers were manual workers around the 1947 reform.

[10]Estimation uses the Stata command `rdrobust` (Calonico, Cattaneo and Titiunik 2014).
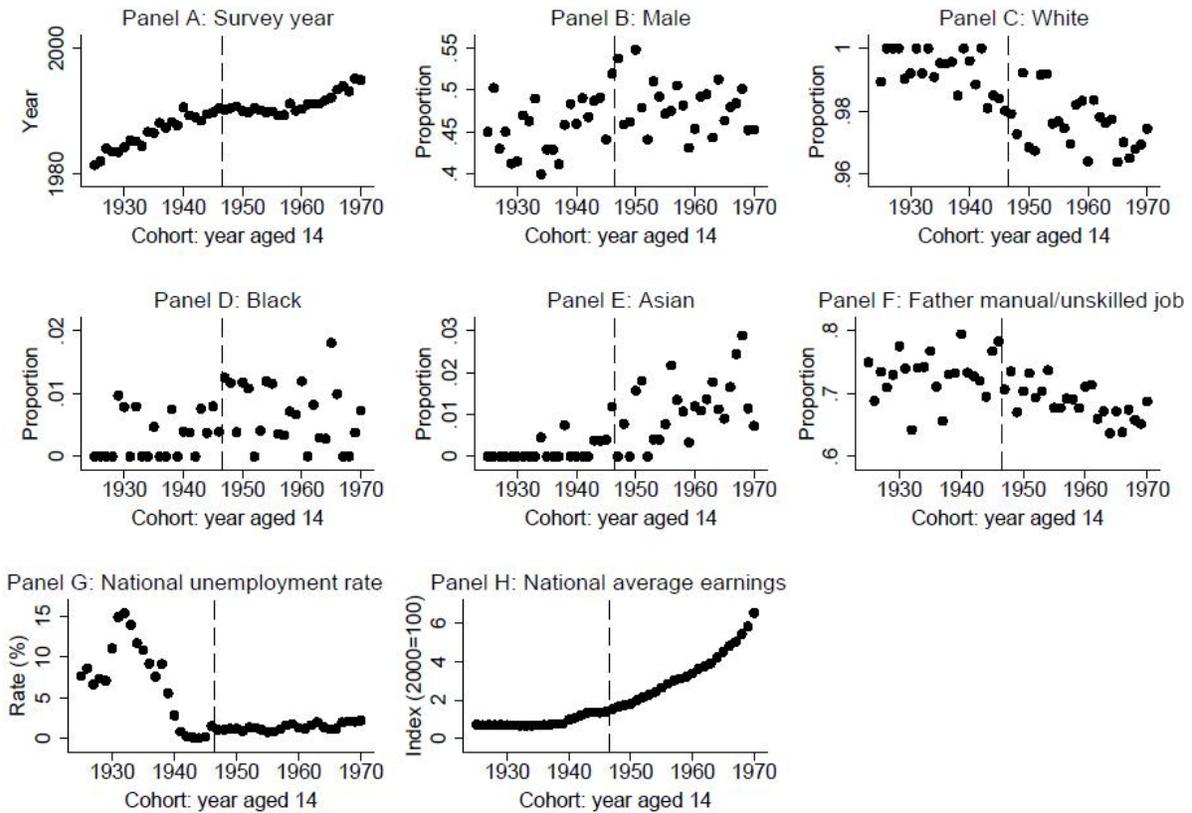
Figure 2: Trends in demographic, socio-economic and labor market demographic variables

Notes: The data in Panels A-F are from the BES. The data in Panels G and H is from the Bank of England "UK Economic Data 1700-2009" dataset.

Conservative. To estimate this, I instrument for years of schooling using the 1947 reform. Beyond the standard RD assumption discussed above, identification of schooling's effect on Conservative voting also requires that the instrument (a) never decreases an individual's level of education (monotonicity) and (b) only affects voting through years of schooling (exclusion restriction). As show in Figure 1, and consistent with monotonicity, the proportion of students leaving school at any age never increases. Given the proximity of the reform to the choice to remain in school, it is unlikely that raising the leaving age affected an individual's political preferences through channels other than additional schooling. Nevertheless, potential violations of the exclusion restriction are discussed below.

To identify the effect of an additional year of schooling among respondents that only remained in school because of the reform, I estimate the following structural equation using 2SLS:

$$Vote\ Conservative_{ic} = \beta Schooling_{ic} + f(Birth\ year_c) + \varepsilon_{ic}, \tag{2}$$

where exogenous variation in schooling comes from the first stage regression below:

$$Schooling_{ic} = \alpha Post\ 1947\ reform_c + f(Birth\ year_c) + \varepsilon_{ic}. \tag{3}$$

The results demonstrate that the strength of the first stage far exceeds the F statistic of 10 required to safely dismiss weak instrument bias (Staiger and Stock 1997).

4   High school education's effect on vote choice

This section presents the paper's main result: high school education causes a substantial increase in support for the Conservative party later in life. I first present the effect of the 1947 reform on years of schooling and downstream support for Conservatives, before turning to the IV estimates identifying the effect of an additional year of late high school.

4.1   Britain's 1947 reform increases schooling and Conservative voting

Confirming the dramatic increase in schooling registered in Figure 1, the first stage estimate in column (1) of Table 1 shows that the 1947 reform substantially increased education attainment. Specifically, the reform increased the schooling of an average student by 0.38 years. The F statistic of 25.4 indicates a strong first stage. Column (2) confirms that university attendance was not impacted by the reform.

Turning to Conservative voting, the reduced form plot in Figure 3 provides the first evidence that voters either side of the reform differ systematically in their vote choice. In particular, there is

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Table 1: Estimates of schooling's effect on voting Conservative

	Years of schooling	Attend university	Vote Con.	Vote Con.	Vote Con.	Vote Con.	Vote Labour	Vote Liberal
	LLR	LLR	LLR	LLR IV	OLS	OLS	LLR IV	LLR IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post 1947 reform	0.381***	0.009	0.044**					
	(0.076)	(0.013)	(0.020)					
Years of schooling				0.116**	0.021***		-0.071	-0.021
				(0.056)	(0.002)		(0.052)	(0.043)
8th year of schooling						-0.020		
						(0.036)		
9th year of schooling						Baseline		
10th year of schooling						0.126***		
						(0.013)		
11th year of schooling						0.213***		
						(0.014)		
12th year of schooling						0.289***		
						(0.017)		
13th year of schooling						0.306***		
						(0.018)		
14th year of schooling						0.281***		
						(0.020)		
Outcome range	0 to 40	0 or 1	0 or 1	0 or 1	0 or 1	0 or 1	0 or 1	0 or 1
Outcome mean	10.6	0.11	0.38	0.38	0.36	0.36	0.36	0.19
Outcome standard deviation	1.86	0.32	0.49	0.49	0.48	0.48	0.48	0.39
First stage F statistic	25.4	0.5	25.4	25.4			25.4	25.4
Observations	11,068	11,068	11,068	11,068	16,757	16,757	11,068	11,068

Notes: Specification (1) is the first stage estimates of the 1947 reform's effect on years of schooling. Specification (2) estimates the effect of the 1947 reform on attending university. Specification (3) is the reduced form estimate of the 1947 reform on Conservative voting. Specification (4) is the IV estimate for years of schooling. All specifications, excluding (5), are local linear regressions using a triangular kernel and the Imbens and Kalyanaraman (2012) optimal bandwidth of 14.7. Specifications (5) and (6) are OLS regressions (in the full BES sample) of voting Conservative on years of schooling (separately as a continuous variable and a set of indicators for each year of schooling), controlling for indicators for male, black, white and Asian respondents, standardized quartic polynomials in age and birth year cohort, and survey fixed effects. For the set of schooling indicators, the estimates for other years are omitted from this table. Specifications (7) and (8) respectively report the effects of schooling on the Labour and Liberal vote shares. Robust standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.
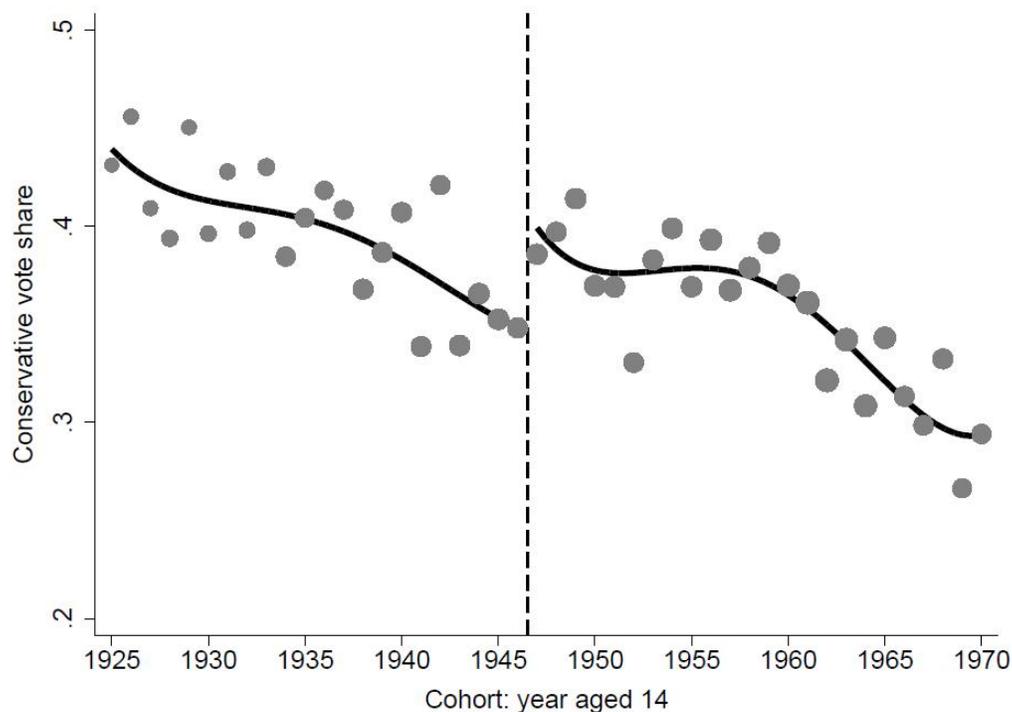
Figure 3: Proportion voting Conservative by birth year cohort

Notes: Black curves represent fourth-order polynomial fits either side of the 1947 discontinuity. Grey dots are birth-year cohort averages, and their size reflects their weight in the sample.

a notable jump in Conservative voting among cohorts affected by the 1947 reform. The fact that increasing the school leaving age reverses the relatively secular trend against the Conservatives—which is likely to be a function of both declining support over time and younger voters being more left-wing—adds weight to the plausibility of the relationship by suggesting that it does not simply reflect accelerating cohort trends. I show below that turnout is unaffected by the reform, and thus the results do not simply reflect rises in participation among Conservative supporters.

More formally, column (3) of Table 1 estimates the reduced form effect of the reform on voting Conservative later in life. The coefficient indicates that increasing the leaving age to 15 induced a large and statistically significant increase in support for the Conservative party. Students from cohorts affected by the 1947 reform are 4.4 percentage points more likely to vote Conservative. Relative to the 35% of the sample that vote Conservative, this implies that affected cohorts are

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around 13% more Conservative. This large difference implies that the reform substantially altered national politics, and could easily have altered the outcomes of the close Conservative election victories in 1970 and 1992. If the effects at the discontinuity generalize to more recent cohorts where completing high school education is the norm, the reform's legacy becomes increasingly important as the proportion of pre-reform voters in the population declines.

4.2   High school education's increases Conservative voting

By averaging across all individuals in a given cohort, and thus including students that would have remained in school regardless of the reform, the reduced form underestimates the 1947 reform's impact on individuals who only remained in school because the leaving age increased. To calculate the effect of late high school for such compliers, I turn to the IV/fuzzy RD estimates.

Instrumenting for years of schooling, column (4) presents the average effect of an additional year of schooling for compliers. Late high school substantially increases the probability of voting Conservative later in life—in fact, each additional year of high school increases this probability by almost 12 percentage points. Reinforcing the reduced form estimates—and consistent with surveys documenting a positive correlation between voting Conservative and greater education and higher social class (e.g. Clarke et al. 2004; Whitten and Palmer 1996)—this large and statistically significant coefficient provides clear causal evidence that high school education is a major determinant of long-run conservative political behavior among the least educated. Although there are few comparable studies able to identify schooling's political effects, this large finding is akin to estimates from the United States (Marshall 2015b). This finding most obviously fits with the income-based channels considered above, although further evidence supporting this mechanism is presented below.

By way of comparison, column (5) estimates the correlation between years of schooling and Conservative voting in the full sample. Including controls for gender and race, as well as flexible polynomials in age and cohort, the estimates suggest that each additional year of schooling is

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associated with a 2 percentage point greater likelihood of voting Conservative. However, this average masks an important non-linearity in the association between education and vote choice. Column (6) shows that even in the full BES sample, the coefficients for the 10th and 11th years of schooling—which generally correspond to leaving school at ages 15 and 16—are similar in magnitude to the IV estimates. Although there are insufficient instruments to estimate such a non-linear effect in the IV context, this suggests that late high school is a particularly consequential moment in a adolescent's life trajectory. The significant drop off in the correlation after high school also offers tentative support for the possibility that education's political effects are not linear. However, it is important to reiterate that, because the reform did not increase university attendance, the causal estimates exploiting the 1947 reform are local to late high school education and do not identify whether university similarly affects voting behavior.

In Britain's three-party system, it is not obvious which party loses potential supporters to the Conservatives. To address this question, columns (7) and (8) respectively estimate the effect of schooling on voting for the Labour and Liberal parties. Although neither coefficient is precisely estimated, the results suggest that Labour are the principal losers: an additional year of high school education decreases the probability of voting Labour by 7 percentage points, whereas the Liberals only suffer a 2 percentage point decline. Given that surveys typically document greater Liberal support among better-educated respondents (e.g. Sanders 2003), this smaller decline is relatively unsurprising. Nevertheless, the fact that greater education did not boost support for the Liberals suggests that the commonly-cited association between education and support for the Liberals may reflect other characteristics of educated voters, or may only arise from university education.

4.3   Robustness checks

I now demonstrate the robustness of the results to various potential concerns. First, the results are not artefacts of the particular RD specification used for the main estimates. Figure 4 shows that the point estimates are stable across bandwidths and the choice of (triangular or rectangular)
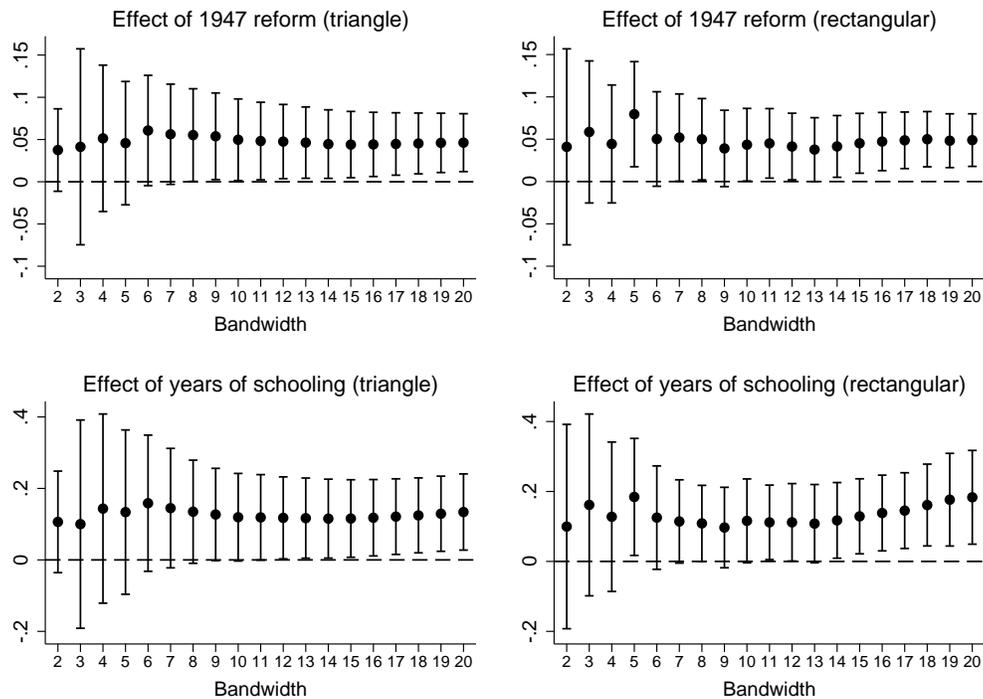
Figure 4: Robustness to choice of bandwidth and kernel

Notes: Triangle and rectangular denote the choice of kernel used for the specifications in each plot. Bars represent 95% confidence intervals (for robust standard errors).

kernel. Inevitably, the precision of the estimates declines at the smaller bandwidths with fewer observations, but the point estimates are remarkably stable across bandwidths. Nevertheless, I also adjust for potential biases that could arise from selecting an optimal bandwidth that trades off bias against the efficiency gained from including observations further from the discontinuity. Correcting for such bias using the approach proposed by Calonico, Cattaneo and Titiunik (2014), the estimates (in the Online Appendix) are almost identical, and thus reinforce the robustness of the finding with respect to bandwidth. Furthermore, to demonstrate that the results are not being driven by complex trends across cohorts, the Online Appendix shows similar estimates when using higher-order polynomial cohort trends and, as noted above, finds no significant change around placebo reforms at any of the ten prior years.

Second, the exclusion restriction (required for the IV estimates) is violated if the 1947 reform

affected political preferences through channels other than schooling. Although political or cultural changes are unlikely to have differentially affect cohorts one year apart, it is possible that an additional year in school could affect life choices—such as marriage or having children—by simply keeping students in school, but without operating through schooling itself. To address such concerns, I examine these possibilities using Labor Force Surveys from the same years as the BES data. The Online Appendix shows that the 1947 reform did not affect the age of a respondent's oldest (dependent) child, the number of children a respondent has, or whether the respondent has ever been married at the time of the survey. Furthermore, any reduction in schooling quality or spillover causing older cohorts to behave more like treated cohorts would reduce between-cohort differences around the reforms, and thus downwardly bias the estimates.

5   How does schooling affect vote choice?

To illuminate the mechanisms causing late high school education to substantially increase downstream Conservative voting, I leverage additional questions from the BES surveys, placebo tests and heterogeneous effects. Although demonstrating a causal mechanism is difficult, examining a range of potential mediators in conjunction with placebo tests can support some mechanisms and eliminate others (Gerber and Green 2012). These results principally suggest that education increases income, which in turn increases support for right-wing policies, and ultimately induces an individual to vote Conservative.

5.1   Greater income and persistent Conservative voting

The combination of human capital theory and the RMR model of income-based political preferences predict that education induces more conservative fiscal policy preferences by increasing an individual's income. There exists compelling evidence that the 1947 reform increased the income of affected cohorts. Exploiting similar RD designs using Britain's 1947 reform, previous

Table 2: Schooling, Conservative voting and income-based mechanisms

	Non-manual worker (below 60)	Vote Con. (below 60)	Vote Con. (60 or above)	Con. partisan	Decided before campaign
	(1)	(2)	(3)	(4)	(5)
Panel A: Reduced form estimates					
Post 1947 reform	0.075**	0.049**	0.006	0.039*	0.041**
	(0.030)	(0.024)	(0.029)	(0.022)	(0.019)
Panel B: IV estimates					
Years of schooling	0.144***	0.111**	0.027	0.092*	0.103*
	(0.055)	(0.056)	(0.138)	(0.052)	(0.051)
Outcome range	0 or 1	0 or 1	0 or 1	0 or 1	0 or 1
Outcome mean	0.46	0.38	0.35	0.37	0.74
Outcome standard deviation	0.50	0.48	0.48	0.48	0.44
Bandwidth	13.9	22.6	15.0	13.9	12.5
First stage F statistic	28.1	28.3	2.7	26.4	23.4
Observations	6,086	10,152	4,589	9,711	9,510

Notes: All specifications are local linear regressions using a triangular kernel and the Imbens and Kalyanaraman (2012) optimal bandwidth for each sample. Sample sizes differ due to bandwidth and the fact that not all variables are available in each survey round. Robust standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

studies have estimated that an additional year of schooling increases wage income by 5-15 percent (Devereux and Hart 2010; Harmon and Walker 1995; Oreopoulos 2006). This significant increase in annual income over the course of a working life has the potential to alter political behavior. Although the BES does not measure income, column (1) of Table 2 confirms that the reform significantly decreased the likelihood that an individual has a manual job (among working age respondents below 60).

If education is indeed driving support for the Conservative party by increasing an individual's income, education should predominantly affect respondents in the workforce. Once retired, education's ability to generate higher wages may no longer be relevant. To test whether schooling ceases

to affect vote choice once a respondent retires from the labor market, I compare the estimates for schooling between respondents aged above and below 60 years of age.[11] Using specifications analogous to equation (2), the results in Table 2 support this implication. The reduced form and IV estimates in column (2) show that respondents aged below 60 experience large increases in their probability of voting Conservative commensurate to the estimates in Table 1. However, consistent with schooling's conservative effects only operating among active workers earning an income, the reduced form estimate in column (3) indicates that elderly respondents affected by the 1947 reform are no more likely to vote Conservative. Even with a weaker first stage, the IV estimate in column (3) is less than a quarter of the effect of schooling among working-age respondents. These results suggest that education only affects vote choice to the extent that workers are continuing to accrue higher wages because of their greater education.

Furthermore, since education's economic returns are likely to hold throughout an individual's working life, the decision to support the Conservative party should be relatively durable. Table 2 also provides support for this claim. First, column (4) shows that education significantly increases the likelihood that an individual self-identifies as a Conservative partisan. Since partisanship likely entails a deeper and more persistent attachment than just voting for a party at the previous election (e.g. Campbell et al. 1960; Clarke and Stewart 1998), the results imply that education forges a lasting tie with the Conservative party. Second, the finding in column (5) that educated voters are significantly more likely to decide how they will vote before the electoral campaign starts further suggests that, consistent with a permanent increase in income, schooling durably increases Conservative support.

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[11]Since current employment may be endogenous to schooling, I use an age-based cutoff. Although workers increasingly retire in their 60s, the cutoff is chosen to conservatively capture retired respondents, and by including respondents still in the workforce should if anything under-estimate the difference. I find similar results when 65 and 70 are used as cutoffs.

5.2 Education increases support for Conservative economic policies

Given that education increases income, education should then also increase support for conservative economic policies such as lower taxation and lower redistributive spending (Meltzer and Richard 1981). To test this implication, I examine how education affects four economic policy attitudes: opposition to tax and spend policies, the belief that welfare spending has gone too far, opposition to income and wealth redistribution, and opposition to the belief that attempts to give women equal opportunities have not gone far enough. Columns (1)-(4) in Panel A of Table 3 suggest that increased education translates into more right-wing fiscal policy preferences. For each variable, the reduced form and IV estimates are large and positive, and only fail to achieve statistical significance in the case of redistribution. Furthermore, combining these variables as a simple (standardized) additive scale (Cronbach's alpha of 0.42), column (5) shows that an additional year of late high school increases support for conservative economic policies by one third of a standard deviation. Although the link from fiscal policy preferences to vote choice cannot be causally identified, there is a strong negative correlation between voting Conservative and opposing high taxation, redistribution, welfare spending, and equal opportunities.[12]

However, if voters adopt the policy positions of the political party or candidate they identify with (e.g. Lenz 2012; Zaller 1992), changes in economic policy preferences could reflect changes in partisanship rather than income-based incentives. To test this possibility, I examine whether respondents also adopt Conservative positions on three non-economic issues: emphasis on reducing crime over protecting citizen rights, support for Britain leaving the European community (EEC, EC or EU, depending on the survey year), and opposition to abolishing private education.[13] The

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[12]The significant correlations between voting Conservative and opposing tax and spend, not supporting welfare benefits, opposing redistribution, and non-support for gender equality are respectively 0.25, 0.41, 0.21 and 0.13.

[13]Unsurprisingly, emphasizing crime reduction ($\rho = 0.12$), not abolishing private education ($\rho = 0.26$), and leaving Europe ($\rho = 0.06$) are all significantly positively correlated with voting Conservative.

Table 3: Mechanisms through which schooling affects political preferences

Panel A: Economic policy preferences	Oppose tax and spend	Welfare benefits gone too far	Oppose redist.	Do not need further gender equality	Con. economic policy scale
	(1)	(2)	(3)	(4)	(5)
Reduced form estimates					
Post 1947 reform	0.229*	0.056**	0.081	0.062**	0.137**
	(0.120)	(0.025)	(0.060)	(0.024)	(0.040)
IV estimates					
Years of schooling	0.567*	0.100**	0.149	0.112**	0.329***
	(0.317)	(0.047)	(0.109)	(0.050)	(0.109)
Outcome range	0 to 10	0 or 1	0 to 4	0 or 1	-2.1 to 3.9
Outcome mean	3.35	0.27	1.66	0.60	-0.02
Outcome standard deviation	2.58	0.44	1.22	0.49	0.99
Bandwidth	14.6	17.7	16.7	16.1	17.2
First stage F statistic	18.5	31.4	30.7	30.2	29.8
Observations	7,370	6,928	8,231	8,221	11,806
Panel B: Non-economic policy preferences	Support crime reduction (over rights)	Support leaving Europe	Oppose abolishing private education	Abortion availability gone too far	Racial equality gone too far
	(1)	(2)	(3)	(4)	(5)
Reduced form estimates					
Post 1947 reform	0.007	-0.007	0.024	-0.019	0.021
	(0.168)	(0.023)	(0.023)	(0.026)	(0.014)
IV estimates					
Years of schooling	0.016	-0.017	0.035	-0.036	0.062
	(0.373)	(0.053)	(0.035)	(0.050)	(0.042)
Outcome range	0 to 10	0 or 1	0 or 1	0 or 1	0 or 1
Outcome mean	6.51	0.32	0.19	0.34	0.38
Outcome standard deviation	2.71	0.47	0.39	0.47	0.38
Bandwidth	18.9	13.7	14.1	14.8	20.3
First stage F statistic	21.2	22.2	25.3	22.5	27.6
Observations	5,716	8,561	5,610	6,556	14,946

Notes: For all outcomes, larger values are more pro-Conservative views. All specifications are local linear regressions using a triangular kernel and the variable's respective optimal bandwidth (given the number of observations per variable differs because not all questions are asked in each election survey). Robust standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table 4: Education and political engagement

	Political information index	Turnout
	(1)	(2)
Reduced form estimates		
Post 1947 reform	0.054	-0.005
	(0.054)	(0.011)
IV estimates		
Years of schooling	0.140	-0.015
	(0.133)	(0.036)
Outcome range	-5.2 to 1.7	0 or 1
Outcome mean	0.00	0.86
Outcome standard deviation	1.00	0.34
Bandwidth	14.1	18.5
First stage F statistic	11.8	26.8
Observations	6,097	16,063

Notes: All specifications are local linear regressions using a triangular kernel and the variable's respective optimal bandwidth (given the number of observations per variable differs because not all questions are asked in each election survey). Robust standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

results of these placebo tests—in columns (1)-(3) of panel B of Table 3—show that education does not significantly shift voters toward any of these Conservative positions. Furthermore, examining socially liberal values that are less closely tied to partisan allegiances, columns (4) and (5) show no difference in attitudes toward abortion and racial equality.[14] These results, in addition to the main finding that education increases support for the Conservative party, further indicate that high school education does not increase support for socially liberal values. The evidence thus suggests that education's political effects principally operate through economic policy preferences.

Another potential explanation for education increasing support for the Conservative party is

[14]Although it was only asked in 1997, there is also no evidence of a change in response to Inglehart's (1981) classic postmaterialist question.

that individuals are better informed or more politically engaged. Such engagement could counteracts pre-existing biases among poorer voters toward the Labour party. To examine this possibility, I examine a political information index which standardizes the proportion of factual political questions a respondent correctly answers across surveys, and an indicator for turnout. The results in columns (1) and (2) of Table 4 do not indicate that voters are either significantly more politically informed or more likely to turn out, and thus offer little support for this alternative interpretation.

6   Conclusion

In this article, I have shown that an additional year of late high school education substantially increases the probability that an individual votes for the Conservative party later in life. By leveraging a RD design to exploit exogenous variation in the cohorts affected by a major reform that increased the education of almost half the British population, I am able to overcome the identification challenges encountered by previous studies to estimate the causal effect of late high school for a significant group of students that only stayed in school because of the landmark reform. Furthermore, I provide evidence suggesting that education's large effects operate by increasing income, which increases support for right-wing economic policies and, ultimately, support for the Conservative party.

These findings suggest that by affecting an individual's labor market prospects and associated political preferences, high school education may be one of the most important determinants of voting behavior. The evidence thus supports influential studies arguing that early life events have important downstream consequences (see Jennings, Stoker and Bowers 2009; Kam and Palmer 2008). However, the mechanism is not the liberal socialization claim frequently attributed to schooling (Hyman and Wright 1979; Lipset 1959). Rather, high school education's effects appear to be predominantly economic in nature and most salient among active members of the workforce. This is consistent with the contested distributive logic of Meltzer and Richard (1981).

Furthermore, the effects of Britain's 1947 leaving age reform are sufficiently large to have potentially affected subsequent electoral outcomes. Affected cohorts are nearly 5 percentage points more likely to vote for the Conservative party, suggesting that the reform could have swung the 1970 and 1992 elections towards the Conservatives. To the extent that the reform has altered the composition of political office, it could also have meaningfully altered policy outcomes (e.g. Lee, Moretti and Butler 2004). As noted above, these findings pose a strategic dilemma for the Labour and Liberal parties, which have typically supported progressive education policies providing opportunities for the least educated.

Given that RD designs can only estimate causal effects at a given discontinuity, it is important to consider the generality of the findings. First, it should be reiterated that unlike many quasi-experimental methods, Britain's 1947 reform affected almost half the student population. Given that the results speak to an extensive set of voters affected by the reform, this study estimates an effect far closer to a population average treatment effect than many studies that examine specific and unrepresentative sets of compliers. Second, although it is only suggestive, the RD point estimates are very similar to correlations observed for the final years of high school in the full BES sample covering all available cohorts. This suggests that the findings may extend beyond the cohorts around the reform. Third, as the economic returns to education have changed, it is possible that university education has now become the key driver of education's political effects. Alternatively, education's income effects could be offset by socially liberal attitudes only instilled at university. Given that Britain's 1947 reform did not affect university attendance, future work is required to identify such effects. Finally, the results may have implications for understanding voting behavior outside Britain. Marshall (2015*b*) reports late high school effects of a similar magnitude in the United States, where more than 20% of students still fail to complete high school (Murnane 2013). More broadly, many developing countries are now implementing major schooling reforms designed to increase secondary attendance. To the extent that left-right economics divisions represent the main political cleavage in such contexts, this study predicts increasing support

for right-wing economic policies as today's students reap the economic benefits and choose to vote more conservatively.

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Online Appendix

Brief history of British education reforms in the twentieth century

There have been three landmark pieces of legislation in the area of CSLs in the twentieth century.[15] First, David Lloyd George's Liberal government moved on the recommendations of the Lewis Report 1916 to raise the school leaving age from 13 to 14 as part of the post-WW1 reforms under the Education Act 1918 (or Fisher Act). The Act was ambitious in that it also aimed to institutionalize schooling until 18 and expand higher education, as well as abolish fees at state-run schools (although secondary education beyond the age of 14 did not become free until 1944) and establish a national schooling infrastructure. However, the change in the school leaving age was not implemented by Lloyd George until the Education Act 1921, coming into effect in 1922. Although the 1918 Act had intended for further increases in the leaving age, these did not transpire for financial reasons despite repeated attempts in the 1920s and 1930s (Oreopoulos 2006). In practice, this Act had relatively little effect on school enrollment: most students remained in school until age 14. Staying in school beyond 14 typically required attending a grammar school (given secondary education was otherwise limited in supply and poorly subsidized), which entailed significant school fees and passing an entrance exam. Consequently, the large majority of working class families did not send their children to secondary school.

Second, as part of the Beveridge reforms, Churchill's wartime coalition government passed the Education Act 1944 (or Butler Act), which increased the school leaving age from 14 to 15 in England and Wales;[16] the Education (Scotland) Act 1945 cemented the same reform in Scotland. No such reform occurred in Northern Ireland until 1957, which is not included in the BES surveys.

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[15]This brief history borrows from Gillard (2011), Woodin, McCulloch and Cowan (2013*a*, 2013*b*) and the relevant legislative documents.

[16]The Education Act 1936 had determined that the age should be raised in 1939, but this did not occur because of the onset of WW2.

The leaving age did not come into force until 1st April 1947, giving the education system time to expand its operations to accommodate the changes in the system (as well as many other new provisions under the 116-page monolith).[17] As described in detail in Woodin, McCulloch and Cowan (2013*b*), this legislation followed intensive debate in the late 1920s and throughout the 1930s. This has culminated in the Education Act 1936, which raises the school leaving age with exemptions of various forms that would allow parents to obtain employment certificates to exempt their children from attending school. With the onset of the war in 1939, the raising of the school leaving age was suspended until the end of the war.

The Education Act 1994 also established the new Tripartite system (coming into force in 1945), which meant that in most parts of the country formal secondary education began at 11 (rather than 14) and whether students attended a grammar, secondary technical or secondary modern school was determined by the "eleven plus" examination taken at age 11.[18] As shown in the main paper, although fees had already been removed in 1944 and the new Tripartite system adopted in 1945, it was not until the schooling leaving age was raised that the dramatic increase in enrollment occurred (see also Oreopoulos 2006).

Furthermore, the 1944 Act provided for raising the leaving age to 16 once practical. Consequently the leaving age could be raised to 16 by an Order of Council.[19] Conservative Prime Minister Harold Macmillan presided over plans to raise the school leaving age to 16 in the Education Act 1962, which ultimately fixed spring and summer leaving dates, although it was Conservative Edward Heath who finalized the update to the current system under Statutory Instrument 444 (1972). The new rule, which had been overseen by Margaret Thatcher and heavily pushed by the Crowther Report 1959, was implemented for the academic year starting 1st September 1972 in

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[17]The lack of teachers was a serious concern, requiring an emergency training program in 1945 to address the lack of capacity.

[18]The Tripartite system was abolished in England in 1976 by the Labour government, and was replaced by the comprehensive system which did not bifurcate students at age 11.

[19]An Order of Council does not require approval like an Act. It may be lain before the House of Commons and is accepted unless a resolution is passed against it.

England and Wales. Statutory Instrument 59 (1972) raised the leaving age more flexibly in Scotland to allow local authorities, who were very concerned about teacher shortages (especially in Strathclyde/Glasgow), to allow part-time schooling and early leaving in the summer terms. Consequently, the 1972 reform was relatively weak for many Scottish students. The reform in Scotland was not fully implemented until the Education Act 1976. The Education (School-leaving Dates) Act 1976 introduced slightly more subtle leaving age rules—which are not utilized in this paper as they require monthly birth data (as in Clark and Royer 2013). In England, Wales and Scotland these reforms again raised education participation rates, although less dramatically than the 1944 Act (Milligan, Moretti and Oreopoulos 2004). Because the reform also introduced middle schools in some parts of the country, which meant that students entered secondary school one year later, the secondary population remained relatively steady.

In 2008, Labour Prime Minister Gordon Brown passed the Education and Skills Act 2008. This requires that by 2013 young people must remain in at least part-time education or training until age 17; by 2015, this rises to 18. Although regional implementation will vary, the Act applies across the entire United Kingdom.

Variable definitions and summary statistics

All variables are from the British Election Survey (BES). Summary statistics for the main RD sample and the full BES sample (for which vote choice was available) are provided in Table 5.

- *Vote Conservative/Labour/Liberal*. Indicator coded one for respondents who reported voting for the Conservative/Labour/Liberal Democrat party at the last general election. Note that Liberal party is used as a catch-all to include the Liberal Party, the Social Democrats in 1987 and the subsequent merged Liberal Democrats. Only respondents which refused to respond, did not answer or did not vote were excluded.

- *Years of schooling*. Years of schooling is calculated as the age that the respondent left full

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Table 5: Summary statistics: RD and full BES samples

	RD sample					Full BES sample				
	Obs.	Mean	Std. dev.	Min.	Max.	Obs.	Mean	Std. dev.	Min.	Max.
Dependent variable										
Vote Conservative	11,068	0.38	0.49	0	1	24,439	0.35	0.48	0	1
Vote Labour	11,068	0.36	0.48	0	1	24,439	0.36	0.48	0	1
Vote Liberal	11,068	0.19	0.39	0	1	24,439	0.19	0.39	0	1
Endogenous treatment variable										
Years of schooling	11,068	10.58	1.86	0	40	24,439	11.03	2.09	0	40
Excluded instrument										
Post 1947 reform	11,068	0.56	0.50	0	1	24,439	0.42	0.49	0	1
Pre-treatment covariates										
Survey year	11,068	1989.46	10.94	1974	2010	24,439	1990.85	11.13	1974	2010
Male	11,068	0.47	0.50	0	1	24,439	0.47	0.50	0	1
White	7,417	0.98	0.13	0	1	16,757	0.97	0.17	0	1
Black	7,417	0.01	0.07	0	1	16,757	0.01	0.09	0	1
Asian	7,417	0.01	0.08	0	1	16,757	0.01	0.12	0	1
Father manual/unskilled job	7,352	0.72	0.45	0	1	15,523	0.69	0.46	0	1
Age	11,068	55.54	13.10	27	91	24,439	49.56	17.59	18	98
Birth year	11,068	1933.92	8.31	1919	1947	24,439	1941.29	20.30	1877	1992
Mechanism variables										
Non-manual worker	11,068	0.40	0.49	0	1	24,439	0.38	0.49	0	1
Conservative partisan	10,424	0.37	0.48	0	1	22,844	0.34	0.47	0	1
Decided before campaign	11,068	0.74	0.44	0	1	24,439	0.68	0.47	0	1
Oppose tax and spend	7,370	3.35	2.39	0	10	17,188	3.48	2.34	0	10
Welfare benefits gone too far	5,786	0.29	0.46	0	1	11,799	0.27	0.44	0	1
Oppose redistribution	7,256	1.69	1.23	0	4	15,463	1.66	1.22	0	4
Do not need further gender equality	7,265	0.60	0.49	0	1	15,461	0.59	0.49	0	1
Conservative economic policy scale	9,913	-0.02	0.99	-2.10	3.86	22,185	0.00	1.00	-2.10	3.86
Support crime reduction (over rights)	4,550	6.53	2.83	0	10	10,443	6.51	2.71	0	10
Support leaving Europe	9,181	0.36	0.48	0	1	20,854	0.32	0.47	0	1
Oppose abolishing private education	5,610	0.19	0.39	0	1	12,444	0.19	0.39	0	1
Abortion availability gone too far	6,556	0.34	0.47	0	1	13,802	0.33	0.47	0	1
Racial equality gone too far	11,068	0.17	0.37	0	1	24,439	0.16	0.37	0	1
Political information index	6,097	0.12	0.93	-5.00	1.77	14,470	0.00	1.00	-5.00	1.77
Turnout	16,036	0.87	0.34	0	1	29,373	0.81	0.39	0	1

Notes: The RD sample is that used for the main specifications where Conservative vote is the dependent variable, except in the case of turnout where the sample is that from column 2 of Table 4 in the main paper.

time education minus five (the age at which students start formal schooling).

- *Birth year*. Birth-year is estimated by subtracting age at the date of the survey from the year in which the survey was conducted. Non-responses were deleted.

- *Post 1947 reform*. Indicator coded one for students aged 14 or below in 1947, and aged 15 or above in 1972.

- *Male*. Indicator coded one for respondents identifying as male. Non-responses were deleted.

- *Age*. Standardized age at the date of the survey.

- *Race*. Indicators coded one for respondents who respectively identify their ethnicity as white, black, or Asian (including South Asian ethnicities and Chinese).

- *Father manual/unskilled job*. Indicator coded one for respondents who answered that their father had a manual or unskilled job.

- *Survey year*. Year in which the survey was conducted.

- *Non-manual worker*. Indicator coded one for respondents that are a high manager or professional, lower manager, skilled non-manual worker or petty bourgeoisie, or lower non-manual worker. This indicator omits skilled manual workers and unskilled and semi-skilled manual workers.

- *Conservative partisan*. Indicator for identifying as a Conservative. The answer is to the following question: "Generally speaking, do you think of yourself as Conservative, Labour, Liberal, ..." (BES). Although a follow-up occurs if the respondent answers "none" or "don't know", this is treated as a zero in this analysis. Only respondents which refused to respond were excluded.

- *Decided before campaign.* Indicator coded one for respondents that reported that they had already decided who they would vote before the general election campaign.

- *Oppose tax and spend.* 11-point scale ranging from "Government should increase taxes a lot and spend much more on health and social services" (0) to "Government should cut taxes a lot and spend much less on health and social services" (10). Note that the scale in the raw data has been reversed. The 1974 and 1979 surveys did not ask this question, the 1983 survey used a 21-point scale that was re-scaled to 0-10, while the 1974 survey asked about just social services. Respondents that answered "don't know" were excluded.

- *Welfare benefits gone too far.* Indicator coded one for respondents that answer that welfare benefits available today have "gone too far" or "gone much too far". Not asked in 1997. Respondents that answered "don't know" were coded as zero.

- *Oppose redistribution.* 4-point scale ranging from strongly agree (0) to strongly disagree (4) that the government should redistribute income and wealth.

- *Do not need further gender equality.* Indicator coded one for respondents that answer that attempts to give equal opportunities to women in Britain have "gone too far" or "gone much too far", or are "about right". Respondents that answered "don't know" were coded as zero. Only available in the 1974-1997 surveys.

- *Conservative economic policy scale.* Standardized summative rating scale combining the following three variables: oppose tax and spend, welfare benefits gone too far, oppose redistribution, and gender equality. Cronbach's alpha of 0.42.

- *Support crime reduction (over rights).* 11-point scale ranging from government should protect citizen rights (0) to government should emphasize reducing crime (10). Question was only asked in 1983 (rescaled from a 21-point scale), 1987, 2005, and 2010. Respondents that answered "don't know" were excluded.

- *Support leaving Europe*. Indicator coded one for respondents that either support withdrawing from the EC, EEC or EU (1983, 1987, 1992, 1997 surveys) or disapprove or strongly disapprove of such membership (2001, 2005, 2010 surveys). Respondents that answered "don't know" were coded as zero.

- *Oppose abolishing private education*. Indicator coded one for respondents that do not believe that private "definitely should", "should" or "probably should" be abolished. Respondents that answered "don't know" were excluded. Only available in the 1983-1997 surveys.

- *Abortion availability gone too far*. Indicator coded one for respondents that do believe that abortion availability has "gone too far" or "gone much too far". Respondents that answered "don't know" were excluded. Only available in the 1974-1997 surveys.

- *Racial equality gone too far*. Indicator coded one for respondents that do believe that attempts to give equal opportunities to black and Asian people have "gone too far" or "gone much too far". Respondents that answered "don't know" were excluded. Only available in the 1974-1997 surveys.

- *Political knowledge index*. Standardized scale (standardized within the RD sample) for the percentage of correct answers to a political information quiz from 1979, 1992 and 1997 surveys (standardized to also include the 2001-2010 elections). These surveys respectively asked 24, 11 and 7 correct-incorrect questions asking about party positions, politician recognition and knowledge of political institutions (e.g. voting age, parliamentary rules). The Cronbach's alpha for each survey was very high. To ensure the scale is comparable across surveys, I created standardized mean scales for each survey and pooled across surveys.

- *Turnout*. Indicator coded one for respondents that self-reported turning out the previous election.
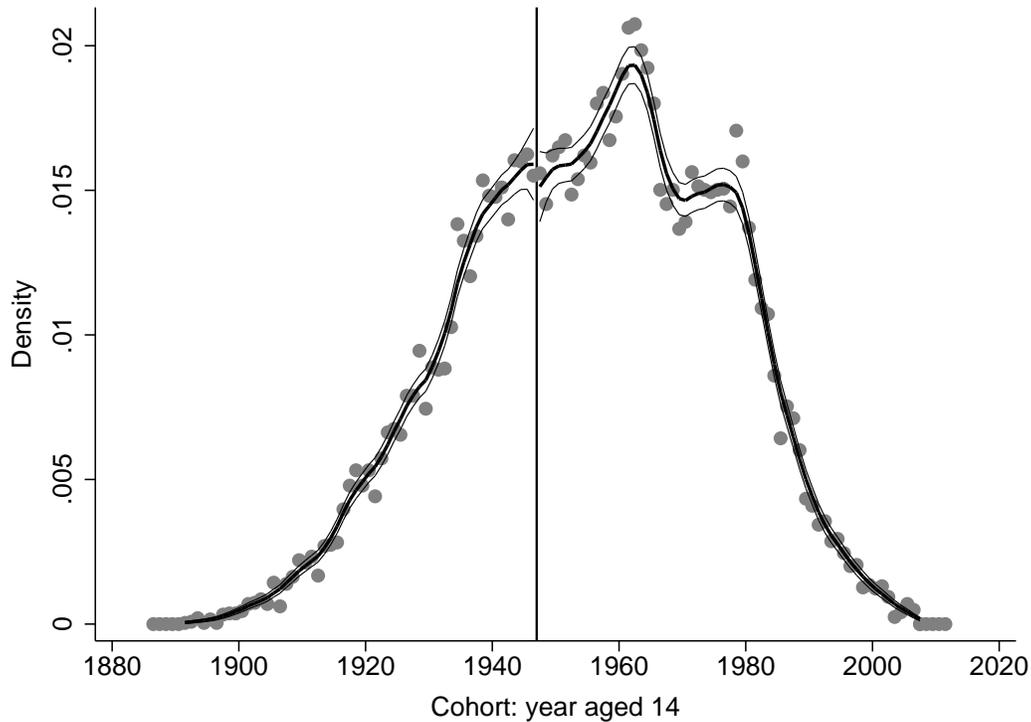
Figure 5: McCrary (2008) sorting test

RD identifying assumption checks

The key "sorting" concern for RD designs is that another key variable simultaneously changes at the discontinuity. However, selection into cohorts in Britain is implausible since parents could not have precisely predicted CSL reforms over a decade in advance. The lack of heaping around the 1947 reform is confirmed by annual birth rate data (Office for National Statistics 2013). Furthermore, a McCrary (2008) density, shown in Figure 5, confirms that the density of the data is indistinguishable across the reform discontinuities.[20] The test proposed by Frandsen (2014) for the case of a discrete running variable similarly finds no difference in density at the discontinuity ($p = 0.53$).

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[20]The null hypothesis of a difference in density across the discontinuity is far from rejected: the difference in density is 0.056 (0.071).

Table 6: Continuity in other variables around the 1947 reform

	Survey year	Male	White	Black	Asian	Father manual/ unskilled job
	LLR	LLR	LLR	LLR	LLR	LLR
	(1)	(2)	(3)	(4)	(5)	(6)
Post 1947 reform	-0.101	0.002	0.002	0.004	-0.002	-0.044*
	(0.456)	(0.021)	(0.007)	(0.004)	(0.004)	(0.023)
Observations	11,068	11,068	7,417	7,417	7,417	7,352

Notes: All specifications are local linear regressions using a triangular kernel and a bandwidth of 14.736. Robust standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table 6 confirms that pre-treatment variables do not differ significantly across the 1947 reform. In no case, is the coefficient on post 1947 reform close to being statistically significant. In each case I use the bandwidth used for the main results presented in the main paper.

Additional RD specification tests

Columns (1)-(4) in Table 7 shows that the results are robust to including quadratic or cubic polynomial trends in the running variable (birth year) either side of the discontinuity. Since I change the order of the polynomial, I also allow the Imbens and Kalyanaraman (2012) optimal bandwidth to adjust accordingly. Unsurprisingly, the estimates become less precise as higher-order polynomials are included. Columns (5) and (6) show the estimates when using the bias-corrected estimator proposed by Calonico, Cattaneo and Titiunik (2014).

Finally, I examine placebo tests treating each of the ten years prior to the 1947 reform as a placebo reform. Figure 6 presents the results of placebo reforms. Each bar is the estimate associated with treating the corresponding year as the 1947 reform. In no prior year, including those immediately before the reform, do we observe any evidence of an increase in Conservative

Table 7: Robustness to higher-order polynomials in the running variable and bias correction

	Vote Con. LLR (1)	Vote Con. LLR IV (2)	Vote Con. LLR (3)	Vote Con LLR IV (4)	Vote Con. LLR (5)	Vote Con. LLR IV (6)
Post 1947 reform	0.047* (0.025)		0.055* (0.033)		0.042** (0.019)	
Years of schooling		0.109* (0.062)		0.124 (0.080)	0.098* (0.054)	
Running variable polynomial order	quadratic	quadratic	cubic	cubic	linear	linear
Optimal bandwidth	20.9	20.9	23.2	23.2	16.3	16.3
Observations	14,946	14,946	16,505	16,505	12,491	12,491
First stage F statistic		19.8		12.1		26.9

Notes: All specifications are local linear regressions using a triangular kernel. In columns (1)-(4) the bandwidth is the Imbens and Kalyanaraman (2012) optimal bandwidth, while the bandwidth in columns (5) and (6) is the Calonico, Cattaneo and Titiunik (2014) bias-corrected optimal bandwidth. Robust standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.
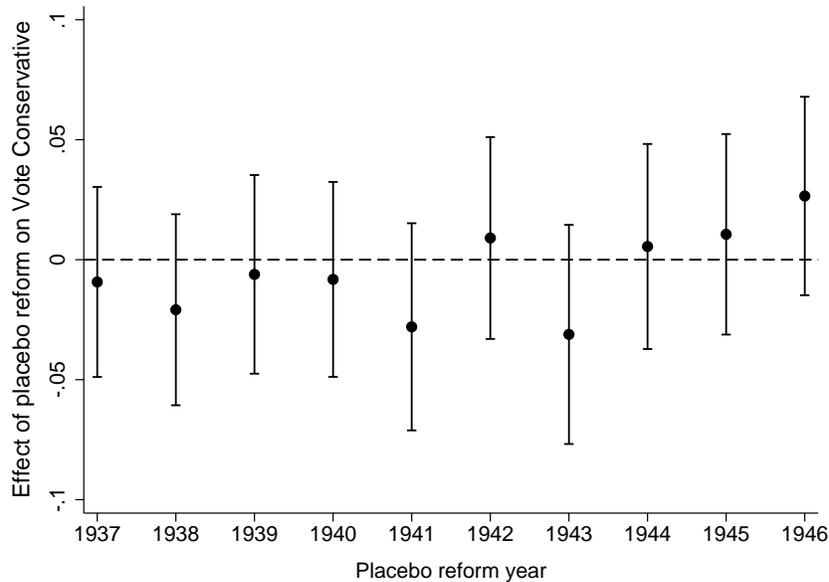

Figure 6: Effect of placebo reforms on Conservative voting

Notes: All specifications are local linear regressions using a triangular kernel and the Imbens and Kalyanaraman (2012) optimal bandwidth for each placebo reform. Bars represent robust 95% confidence intervals.

Table 8: Exclusion restriction checks

	Number of dependent children	Age of oldest dependent child	Married at one time
	(1)	(2)	(3)
Post 1947 reform	-0.020	-0.423	-0.009
	(0.016)	(0.462)	(0.010)
Observations	9,712	881	5,058
Bandwidth	7.9	9.1	11.0

Notes: All estimates are from the July-September Labor Force Survey corresponding to the election year of each BES survey, and matched to the pre-treatment characteristics of the BES sample. All specifications are local linear regressions using a triangular kernel and the Imbens and Kalyanaraman (2012) optimal bandwidth for each sample. Robust standard errors in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

voting. This provides strong support for the concern that the results are not being driven by pre-trend.

Exclusion restriction checks

Table 8 presents the results of the exclusion restriction tests cited in the main paper. The estimates are from British Labor Force Surveys (LFSs). I use the summer quarter of LFS because this best matches when British elections are typically held and thus BES surveys occur. I then matched the pre-treatment sample moments to the BES sample to that the procedure produced relatively comparable estimates to the BES estimates in the main paper. In no case is there evidence of a significant exclusion restriction violation.