Abstract

Research has struggled to causally disentangle the mechanisms linking education to downstream political behavior. Political economy theories imply schooling makes citizens more conservative by increasing current or expected income, or by imparting general skills reducing demand for social insurance. Socialization theories instead suggest schooling affects political preferences by increasing political engagement and engendering socially liberal attitudes. Utilizing variation in compulsory schooling laws (CSLs) in the US and Britain, and multiple identification strategies, I show that raising CSLs by a year induces a 2-5 percentage point swing toward the Republican or Conservative party per cohort. Instrumental variable estimates show an additional year of late high school increases the probability a CSL-complier supports right-wing parties by more than ten percentage points. By showing that high school’s conservative effects operate through income channels, this article reconciles university’s predominantly left-wing impetus with high school’s economic benefits by emphasizing the potentially non-linear effects of education.
1 Introduction

Research seeking to identify education’s causal effects has not generally examined the mechanisms linking education to downstream political behavior. By failing to understand these mechanisms, scholars have struggled to reconcile the widely-documented correlations between income (which education increases) and support for right-wing parties (e.g. Erikson and Tedin 2007; Gelman et al. 2010; Thomassen 2005; Van der Waal, Achterberg and Houtman 2007) and between attending university and support for left-wing parties and socially liberal attitudes (e.g. Dee 2004; Gerber et al. 2010; Heath et al. 1985; Schoon et al. 2010). By showing that high school makes voters more conservative predominantly through income channels, this article reconciles university’s predominantly left-wing impetus with high school’s economic benefits by emphasizing the non-linear effects of education. In contrast to university education, these results show that income-induced effects dominate potentially counterveiling changes in social attitudes for high school education.

Given its centrality in young people’s lives, there are various avenues through which education could affect political preferences in later life. This paper focuses on two prominent approaches. Political economists claim that richer individuals prefer lower tax rates and so vote for conservative parties (Romer 1975; Meltzer and Richard 1981). If human capital theory is correct and schooling is rewarded financially in the labor market, high school education makes voters more conservative once the income benefits are in sight. Others suggest the general skills schooling provides also reduces demand for social insurance (Iversen and Soskice 2001). Alternatively, political sociologists have argued that schooling changes an individual’s political values (Inglehart 1981; Inglehart and Abramson 1994; Marshall 1950), and imparts skills that affect political engagement (Nie, Junn and Stehlik-Barry 1996; Rosenstone and Hansen 1993) and the political composition of their social network (Nie, Junn and Stehlik-Barry 1996). These hypotheses differ in their partisanship predictions, when their effects should occur, and their theoretical mechanisms.

Researchers have long been interested in the impact of education on individual-level politi-
cal behavior, but have only recently addressed the “education as a cause” versus “education as a proxy” debate (Kam and Palmer 2008; Sondheimer and Green 2010). Recent contributions have used innovative research designs to confirm that high school and university education cause increased civic participation (Dee 2004; Henderson and Chatfield 2011; Milligan, Moretti and Oreopoulos 2004; Sondheimer and Green 2010). While education’s participation bias is now well-supported, the more complex causal association between schooling and political preferences has not yet been established. Comparing the US and Great Britain, this article demonstrates that high school has such a partisan bias, before adjudicating between plausible theoretical mechanisms.

To estimate the partisan effects of an additional year of high school, this paper uses compulsory schools laws (CSLs)—a widely-used quasi-experiment among labor economists (e.g. Acemoglu and Angrist 2000; Angrist and Krueger 1991; Oreopoulos 2006)—to generate variation in the numbers of years of schooling a student acquires in the US and Britain. Because exposure to CSLs does not increase university attendance, the results specifically identify the impact of late high school education. With many US states considering raising their school leaving age (Oreopoulos 2009) and the UK increasing the education leaving age to 17 in 2013 and 18 in 2015, CSLs are themselves both policy-relevant and politically salient.

Employing a difference-in-difference strategy in the US and cohort group comparison and regression discontinuity strategies in Britain, I show that CSLs have had a large causal effect in their own right. Increasing the minimum school leaving age by a year induces a 2-5 percentage point swing toward conservative parties per academic/birth-year cohort. Instrumental variable estimates show that an additional year of high school—specifically the final years of high school—makes citizens that comply with CSLs more than ten percentage points more likely to identify as or vote

1E.g. Almond and Verba (1963); Brody (1978); Nie, Junn and Stehlik-Barry (1996); Rosenstone and Hansen (1993); Verba, Schlozman and Brady (1995).

2Some studies using particular instruments (Berinsky and Lenz 2011) or asking respondents when in university (Tenn 2007) fail to confirm this association.
for the Republican or Conservative parties.

The analysis of the mechanisms underpinning schooling’s effect show that the effect of additional years of high school is most pronounced at the mid-life earnings peak before tapering off into retirement, while high school reduces support for taxation and welfare. With the proposed mechanisms of sociological explanations receiving no support and high school not pushing students toward less skill-specific vocations, these results strongly support the Romer-Meltzer-Richard political economy explanation and thus show that the economic effects of late high school education outweigh changes in social attitudes. The large effects for CSL-compliers reflect late high school acting as a critical career juncture, and the greater benefits of schooling for the population of compliers whom I show come from more disadvantaged backgrounds.

This paper is organized as follows. Section 2 considers hypotheses linking schooling and partisanship. Section 3 describes the data. Section 4 explains the research design. Section 5 provides the results and characterizes compliers. Section 6 tests the mechanisms linking high school to more conservative preferences. Section 7 concludes.

2 Theory and hypotheses

Although much has been written about how education affects civic participation, income and social attitudes, education’s effect on political preferences has received limited theoretical attention beyond serving as a standard control variable. By explicitly considering the economic and sociological mechanisms through which high school could affect partisan preferences, emphasizing different causal predictions and observable implications that empirical tests can differentiate, this section presents a clear framework for understanding and assessing education’s political effects.
2.1 Political economy arguments

Much research suggests that an individual’s education increases their wage income (e.g. Angrist and Krueger 1991; Oreopoulos 2006). The human capital interpretation suggests that education imparts productive skills, which are rewarded in competitive labor markets. Romer (1975) and Meltzer and Richard (1981) (RMR) argue that richer individuals will support less redistribution, which typically entails supporting right-wing political parties. While the national-level implications of the RMR model receive mixed support (Karabarbounis 2011), survey correlations consistently show higher-income individuals are more conservative (e.g. Erikson and Tedin 2007; Gelman et al. 2010) while class voting remains prevalent (e.g. Thomassen 2005; Van der Waal, Achterberg and Houtman 2007).

Together, the human capital and RMR models imply that increased schooling should make voters more favorable to the economic policies of conservative parties. If this link depends upon education’s expected income benefits having been realized, the effects of schooling should be most pronounced at an individual’s earnings peak in their mid/late-40s (e.g. Heckman 1976), when the education premium is greatest. However, if current government policy affects future policy, voters expecting high future incomes could pre-emptively become more conservative. Alesina and La Ferrara (2005) find support for redistribution declines with one- and five-year expected future income. In retirement, education ceases to provide monetary rewards, so schooling’s effect should subside.

Schooling could equally affect political preferences by reducing demand for social insurance. Iversen and Soskice (2001) argue schooling provides students with general rather than industry-specific skills. Since general skills are rewarded in all industries, employees with such transferable skills expect shorter spells of unemployment and smaller wage reductions if forced to shift industries. Like the RMR model, this theory therefore predicts that better educated workers will support conservative parties offering less social insurance, but distinctively implies that this can only occur
where education pushes individuals into industries rewarding general skills.

2.2 Political socialization arguments

Socialization theories offer a variety of plausible mechanisms pointing in different directions. First, Inglehart’s (1981) scarcity hypothesis proposes that citizens become more socially liberal (in Mill’s sense) and democratic, expressing post-materialist values, as education increases financial and cognitive resources. US post-materialists identify as Democrats (Layman and Carmines 1997), while Labour and especially the Liberals in Britain have been associated with pro-environmental, anti-nuclear and political inclusion policies (Heath et al. 1985). Inglehart’s socialization hypothesis suggests schooling’s effects may only apply once income has increased, but differs from political economy arguments in also predicting increased post-materialist values and political engagement. However, such correlations are most pronounced among those with university degrees rather than high school diplomas (Gerber et al. 2010; Heath et al. 1985).

Second, schooling—especially by late high school where citizenship and politics represent a greater focus in curricula—may increase political information and interest, or at least provide skills reducing acquisition costs (Nie, Junn and Stehlik-Barry 1996; Rosenstone and Hansen 1993). Although greater knowledge and interest may not be intrinsically politically biased, they could create more balanced voting shortcuts (Lupia 1994) or encourage reading higher-quality newspapers (Heath et al. 1985). If the voting cues used by low-educated voters bias them toward left-wing parties, perhaps because union and social networks exert relatively larger pulls, further schooling could expose voters to right-wing arguments and induce a rightward shift.

Third, education opens doors to opportunities beyond the labor market. Beyond increasing an individual’s political engagement, better-educated voters have greater access to politically-engaged networks (Nie, Junn and Stehlik-Barry 1996), whose greater affluence, prestige or ideological diversity could expose them to more conservative perspectives. Such peer-group logic applies to voter turnout (Abrams, Iversen and Soskice 2010).
Finally, the experience of education itself could more directly develop left-wing preferences. Continuing education could instill greater respect for government and the state (Marshall 1950), leaving voters favorable toward leftist “big government” policies. At the university level, Horowitz (2007) argues that liberal faculty use their curricula to indoctrinate students.

However, examining 18-24 year-olds, Tenn (2007) finds that while being a university student increases voter turnout and vote registration, years of education has no effect on turnout. Showing that the effects of education quickly decay, Tenn (2007) thus casts doubt on the plausibility of immediate partisan socialization effects.

2.3 Education as a proxy

Critics of human capital and socialization theories have argued that education merely serves as a costly signal of unobservable productive characteristics without adding much productive value itself (Spence 1973), or reflects early life characteristics, values, cognitive abilities and experiences (Kam and Palmer 2008) or social class (Nie, Junn and Stehlik-Barry 1996). While human capital, some socialization and signaling theories imply that education is correlated with conservative voting, if selection problems can be resolved signaling explanations imply there should be no empirical relationship.

2.4 Summary

Table 1 summarizes the central predictions of the main theories. While several hypothesize that high school makes voters more conservative, they can be differentiated by their timing and mechanisms. Given the difficulty of identifying causal relationships, this is the first study of which I am aware that is able to convincingly identify the direction of this relationship and illuminate which mechanisms apply.

[Table 1 about here]
<table>
<thead>
<tr>
<th>Theory</th>
<th>Causal direction</th>
<th>Effect timing</th>
<th>Mechanisms</th>
</tr>
</thead>
<tbody>
<tr>
<td>Human capital/RMR</td>
<td>More conservative</td>
<td>Maximized around earnings peaks, declines into retirement</td>
<td>Higher income, lower preference for redistribution and social welfare</td>
</tr>
<tr>
<td>Social insurance</td>
<td>More conservative</td>
<td>Rises in early life, declines in late career</td>
<td>Relatively more general skill vocation, lower preference for social welfare</td>
</tr>
<tr>
<td>Post-materialism</td>
<td>Less conservative</td>
<td>Probably increasing with age</td>
<td>Higher income, higher job prestige, post-materialist preferences, attend university</td>
</tr>
<tr>
<td>Political engagement</td>
<td>Probably more conservative</td>
<td>Immediate, age-invariant</td>
<td>Better politically informed, interested and active</td>
</tr>
<tr>
<td>Social networks</td>
<td>Uncertain</td>
<td>Probably increasing with age</td>
<td>Higher income, discuss politics more</td>
</tr>
<tr>
<td>School ideology</td>
<td>Less conservative</td>
<td>Immediate, age-invariant</td>
<td>Greater trust in government</td>
</tr>
<tr>
<td>Education as proxy</td>
<td>No effect</td>
<td>No difference</td>
<td>Correlation with income, correlation with being conservative, but no causal effect</td>
</tr>
</tbody>
</table>
Previous research has also brushed over two important conceptual and estimation issues: different mechanisms may dominate at different levels of education; and particular levels of education affect different types of individuals differently. Previous analyses have typically correlated educational qualification indicators or the number of years of schooling with partisanship, implicitly assuming constant effects across individuals and failing to differentiate high school from university education. However, the marginal effect of schooling is likely to vary substantially across individuals and could be non-linear if income incentives only overpower socially liberal attitudes at certain levels of education (Morton, Tyran and Wengström (2011)). Understanding heterogeneous responses is important for targeting public policy.

This paper addresses both problems. By exploiting variation in high school attendance, I am able to differentiate the effects of additional years of late high school from university on political preferences, before probing the mechanisms by examining the observable implications of different theories. I also show that schooling has its largest effect among low-SES citizens, for whom additional high school represents a critical juncture in a way that it does not for those with more opportunities in life.

3 Data

I use data from the National Annenberg Election Survey (NAES) and extracts from the American Community Survey (ACS) in the US, and the British Election Survey (BES) in Great Britain. This section describes the main variables; see Appendix for more detailed variable definitions, sources and summary statistics.

3.1 United States

The NAES collates rolling surveys conducted throughout the 2000, 2004 and 2008 Presidential election campaigns. More than 50,000 randomly-sampled adults were interviewed by telephone
during each campaign, and together these yield a maximum pooled sample of 161,181 respondents.³

The ACS—which has conducted random interviews with US citizens at monthly intervals since 2000⁴—supplements the NAES’s political questions by measuring the number of years of schooling, which the NAES does not measure. After ensuring the ACS sample of 440,963 individuals is a random sample from a population almost-identical to that from which the NAES draws, the NAES and ACS datasets can be combined using two-sample methods that allow for separate estimation in each sample (see below).

As a robustness check, I also combine the political questions from the American National Election Survey (ANES) with the schooling measure from Census extracts. Although the ANES has a substantially smaller sample, it serves as an important out-of-sample robustness check using different survey protocols and covering elections since 1952.

### 3.1.1 Political outcomes

This paper uses two measures of political preference in the NAES. The first is (strong and weak) partisan self-identification: 33% of respondents identify as Republicans, while 35% identify as Democrats; the residual are independents, while non-responses were excluded. Throughout I use an indicator—*Republican partisan*—to capture partisanship. Secondly, and conditioning on those intending to vote, I employ vote intention at the forthcoming Presidential election, coding an indicator for *Vote Republican for President*.⁵ In the sample, the Republican candidates received 53% of Presidential votes.⁶ The results are mirrored if Democrat indicators are used instead.

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³Online survey responses are not used here.
⁴Although the samples collected for the early test years are smaller, I ensure the proportion of respondents from each year exactly matches the NAES distribution.
⁵The results are robust to instead using self-reported vote at the previous Presidential election.
⁶The increase relative to Republican partisanship likely reflects higher Republican turnout (Lijphart 1997) and Democrats identifiers being more likely to switch (Miller 1991).
3.1.2 Years of schooling

The central independent variable is the number of years of completed schooling. This improves upon a binary measure of high school completion because it captures increases in schooling that do not push the respondent past the threshold of completing high school, whereas dichotomizing a continuous variable can seriously accentuate instrumental variable estimates (Angrist and Imbens 1995).\footnote{Mis-specifying a continuous treatment intensity as binary induces multiplicative bias; Milligan, Moretti and Oreopoulos’s 2004 very large estimates suffer from such bias.} Using the ACS, Schooling is coded as the number of grades completed; to focus on high school, the variable is top-coded as completing 12th grade, and thus ranges from 0 to 12.\footnote{This top-coding is inconsequential because the instrument only affects high school education.} High school drop-out rates—which vary across states—remain non-trivial (Oreopoulos 2009). In the sample, 13\% of students did not graduate from high school.

3.2 Great Britain

In Britain, the BES randomly samples several thousand citizens from the British (excluding Northern Irish) electoral register after general elections for in-person interviews.\footnote{The 1997 exception randomly sampled households. Pre-election and non-interview surveys were excluded.} Although surveys have been run since 1964, only data from seven elections—1974(Feb.)-1997—contained comparable variables.\footnote{1964, 1969 and 2001-2010 do not contain relevant education questions.} Pooling across elections, the maximum sample is 20,647.

3.2.1 Political outcomes

Like the US, I construct measures of partisan self-identification and self-reported voting. In the sample, 35\% of respondents identify as Conservative, while 36\% identify as Labour, and 12\% identify as Liberal; the residual are for small parties, non-partisans, or uncertain. The analysis focuses
principally on an indicator—Conservative partisan—for identifying as Conservative because the modal theoretical prediction suggests schooling makes voters more conservative. This paper also considers how schooling affects Labour partisan and Liberal partisan indicators. Similarly, I code three vote indicator variables—Conservative vote, Labour vote and Liberal vote—for self-reported vote choice; the Conservatives, Labour and the Liberals received 38%, 37% and 19% respectively.

3.2.2 Years of schooling

The BES contains richer data on a student’s education than the NAES, asking at what age a student left continuous full-time education since 1983 and, up to 1983, what age a student left school.\(^\text{11}\) To ensure comparability across surveys and since the analysis pertains to inducing additional years of schooling for 14 and 15-year-olds, I again restrict attention to high school education. Accordingly, Leave schooling combines the age left education and school variables, and top-codes at leaving aged 18 or above.

4 Research design

This section first explains the causal identification problem, before introducing CSLs as a source of variation in school attendance. The section then details how CSLs can identify both the causal effect of CSLs and, by using CSLs as an instrument, the causal effect of an additional year of late high school for CSL-compliers. The section concludes by supporting the assumptions required to estimate these effects across a variety of identification strategies.

\(^{11}\)In the 2000s, the lowest leaving age that respondents could provide was 15. Given the instrument used focuses on keeping student in school until 15, this prohibited using the 2001-2010 surveys.
4.1 What we would like to estimate

To identify the effect of schooling on partisanship and voting, the ideal linear probability model would estimate:

\[
\Pr(Y_{it} = 1) = \beta S_i + X_{it} \gamma + \epsilon_{it}, \tag{1}
\]

where \(Y_{it}\) is the binary observed outcome (conservative identifier/voter) for individual \(i\) at survey period \(t\); \(S_i \in \{1, ..., S\}\) is the discrete treatment intensity of the number of completed years of school; \(X_{it}\) is a vector of pre-treatment covariates; and \(\epsilon_{it}\) is the error term. If \(\beta\) is constant across individuals and interval intensities, OLS identifies the average treatment effect on the treated—provided there is no interference across individuals and, conditional on \(X_{it}, S_i\) is randomly assigned.

As noted above, estimating equation (1) in the hope of identifying a population causal effect of an additional year of schooling is problematic. First, which individuals receive longer schooling is very unlikely to be even conditionally random (Kam and Palmer 2008). Second, the effect of schooling is likely to be heterogeneous across types of individual, while schooling’s effects may be non-linear since the effect of an additional year of schooling depends upon the year in question. The methods used here can estimate schooling’s causal effects and both pinpoint which types and what stage of schooling drives the results.

4.2 Variation in compulsory schooling

A CSL defines the minimum legal age or level of education at which a student may drop out of school. Using CSLs to generate variation in incentives to attend school was pioneered by Angrist and Krueger (1991), and CSLs have since been used widely by labor economists as instruments to
estimate the effects of schooling on wages and various other outcomes in the US and Britain.\footnote{In the US, see e.g. Acemoglu and Angrist (2000), Dee (2004), Goldin and Katz (2008), Lochner and Moretti (2004), Lleras-Muney (2002), Milligan, Moretti and Oreopoulos (2004); for Britain, see Clark and Royer (2013), Devereux and Hart (2010), Harmon and Walker (1995), Milligan, Moretti and Oreopoulos (2004), Oreopoulos (2006).}

### 4.2.1 CSLs across US states

In the US, CSLs are legislated by state governments and are defined by age.\footnote{Child labor laws are often used alongside CSLs, and were important before 1940 (Goldin and Katz 2008). They are omitted here because they offer limited temporal variation, provide few compliers, and predominantly affect rural states with lower compliance.} Although CSL enforcement is relatively weak, states can and do punish habitual truancy (Oreopoulos 2009). The CSL data used here are from Oreopoulos (2009), and based on the National Center for Education Statistic’s *Education Digest*.

The first CSL was adopted in Massachusetts in 1852 and 41 states employed them by 1910, principally to meet demand for educated workers and promote assimilation (Goldin and Katz 2008). Figure 1 plots changes in CSLs across the 48 contiguous US states and Washington, DC since 1914.\footnote{Following Oreopoulos (2009), Alaska and Hawaii are omitted because of different demographic and economic characteristics.} Although there has been a general upward trend in CSLs, there remain numerous instances of reversal as in Maine, Mississippi, and Oregon.

[Figure 1 about here]

The majority (74.6%) of state-year CSLs specify a leaving age of 16; 8.8% are below 16; 7.8% use 17; and 8.7% use 18. I create two indicators—1($CSL = 16$) and 1($CSL \geq 17$)—to capture the effect of these leaving ages relative to the baseline of below 16.\footnote{The leaving ages of 17 and 18 are combined because forcing students only months from completing high school to remain in school until 18 has little impact (Lochner and Moretti 2004).} Given month of birth is not
Figure 1: Variation in US state CSLs, 1914-2005
included in the NAES, cohort (birth year) is inferred from age when surveyed by the NAES and given as year of birth for the ACS. Survey respondents are then mapped to the CSL operating in their state at the age at which the law binds their decision to leave school, based on current state of residence in the NAES and state of birth in the ACS.

4.2.2 CSLs in Britain

In Britain, the central government determines the minimum school leaving age for each academic cohort. Following the Education Act 1918, there were two landmark changes to the leaving age in the twentieth century. Greater historical detail is provided in the Online Appendix (see also Gillard 2011; Woodin, McCulloch and Cowan 2012).

First, Winston Churchill’s wartime coalition government passed the Education Act 1944, which increased the leaving age from 14 to 15 in England and Wales. The Education (Scotland) Act 1945 enacted the same reform in Scotland. The new leaving age, which had repeatedly failed to pass in the 1920s and 1930s due to financial constraints, came into force 1st April 1947. By fully funding secondary education, the Act also made education more accessible, although classroom content was unchanged (Clark and Royer 2013). Figure 2 shows the 1947 reform substantially reduced the proportion of pupils in each cohort leaving school at 14, and slightly reduced the proportion leaving at 15.

[Figure 2 about here]

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16 This approach is common and yields similar first-stage results to studies using month of birth (Acemoglu and Angrist 2000).

17 61% of ACS respondents live in their state of birth, although many could have migrated to their current state of residence before being affected by CSLs. Nevertheless, using the ANES dataset—which measures state of residence at age 14—demonstrates that NAES results are robust; see checks below.

18 Since only 5% of the sample were 13 before the 1918 Act took effect, and because the policy’s effect were limited in practice, the 1918 reform is not considered.
Figure 2: Proportion leaving school aged 14 and 15 by cohort in Britain
Second, under Conservative Prime Minister Harold Macmillan, Parliament passed the Education Act 1962 raising the school leaving age to 16, although it was Conservative Edward Heath who finalized the update to the current system under Statutory Instrument 444 (1972). Like the 1947 reform, Labour had consistently pushed for the increase,\textsuperscript{19} while education was widely seen as an economically and socially beneficial investment at the time (Woodin, McCulloch and Cowan 2012). The new rule was implemented in England and Wales on 1st September 1972. Statutory Instrument 59 (1972) also raised the leaving age in Scotland, although teacher shortages meant not all local authorities fully implemented the reform until the Education Act 1976. Figure 2 shows these reforms again substantially raised school retention rates.

I code CSLs for England, Wales and Scotland using 1\( (CSL = 15) \) and 1\( (CSL = 16) \) as indicators for the minimum schooling leaving age; the residual category is below 15. The BES measures age in years at the date of its survey, and this is mapped to CSLs for year aged 14 and 15.\textsuperscript{20} Although Scottish students faced a weaker law between 1972 and 1976, they are coded identically to England/Wales as a similarly large drop in the proportion leaving occurred.\textsuperscript{21}

4.3 Identification strategies

I use variation in CSLs as a quasi-experiment to generate variation in individual schooling. If CSLs are effectively randomly assigned then this approach identifies the causal effect of CSLs, while instrumental variable (IV) techniques identify the causal effect of schooling for individuals that comply with the laws provided that CSLs only affects political preferences through schooling.

To identify the effect of CSLs themselves on partisan preferences, CSL reforms must be \textit{conditionally ignorable}. This means that after conditioning on appropriate variables, changes in CSLs

\textsuperscript{19} Under Labour Prime Minister Gordon Brown, Parliament passed the Education and Skills Act 2008, raising the education leaving to 18 by 2015.

\textsuperscript{20} Clark and Royer (2013) use monthly data and show very similar first stage estimates.

\textsuperscript{21} Results are robust to excluding Scottish students aged 15, 1972-76.
are effectively random. To achieve this in the US, I use a difference-in-difference strategy leveraging cohorts in states that did not change their CSLs as controls to separate trends in partisan preferences from the impact of CSLs on cohorts in states where CSLs changed. Identification requires that in the absence of changing CSLs, individuals from treated states would experience parallel trends in partisan preferences to individuals from control states.

Since CSLs affect all students in Britain, a different strategy is required. I argue the timing of Britain’s CSL reforms is exogenous, and use a regression discontinuity (RD) design to exploit the discontinuities around the reforms as a robustness check requiring only weak assumptions (DiNardo and Lee 2011). The key RD identifying assumption is that partisan preferences are continuous in all covariates other than school leaving age at the discontinuity.

In order to estimate the effect of schooling on partisan preferences, I build upon the reduced form approaches to use CSLs as an instrument for schooling in both the US and Britain. Beyond the conditional ignorability of CSLs and non-interference assumptions, identifying the causal effect for individuals that only stayed in high school because of the instrument (compliers) requires that higher CSLs increase the likelihood that an individual remains in high school (the first-stage), without ever decreasing the propensity to stay in school (monotonicity), and only affect political outcomes through increased schooling (the exclusion restriction) (Imbens and Angrist 1994; Angrist, Imbens and Rubin 1996). Angrist and Imbens (1995) show this IV strategy generalizes to cases where \( S_i \) is a multi-valued treatment intensity.\(^{22}\)

### 4.4 Supporting the identification assumptions

Previous studies find a strong first-stage for CSLs on years of schooling in the US and Britain. As statistical tests confirm below, and Figure 2 clearly shows in Britain, CSLs increase schooling. Although monotonicity is fundamentally untestable, it is hard to argue a higher leaving age would

\(^{22}\)Specifically, 2SLS estimates the local average causal response by weighting the reduced form CSL effect at each level of schooling by the proportion of people affected by the CSL at that value.
make an individual choose less schooling. Cumulative distribution plots by CSL category in the Online Appendix support this claim.

There are two main challenges to the ignorability of CSL reforms: individual selection into CSL regimes; and CSL reforms reflecting unobserved processes which also affect political preferences. Given individuals cannot manipulate their date of birth and parents could not have predicted CSL reforms over a decade in advance, selection into cohorts in Britain is implausible.\(^{23}\) In the US, CSLs affect all cohorts in a given state, and therefore changes in CSLs are uncorrelated with individual characteristics unless certain types of CSL-compliers move more when the reform occurs. However, among poor women of childbearing age—whose children are most likely to be CSL-compliers—cross-state migration is especially low (Molloy, Smith and Wozniak 2010), and is not systematically associated with destination welfare benefits (Hanson and Hartman 1994) which could correlate with a state’s education policy. Furthermore, the principal reasons to move across states—for college, marriage, family reasons, and natural disaster (Molloy, Smith and Wozniak 2010)—are unlikely to be linked to CSL changes.

The greater concern is adoption context, since legislation is not random. However, extensive analysis in the Online Appendix finds no statistical association between CSLs and indicators of Republicans control of upper, lower or both state houses, or Republican state-level seat share. Rather than immediately follow changes in government or political sentiment, Britain’s long-advocated CSL reforms were popular with all political parties and based on independent reports and inter-war and post-war pressure for social reforms (Gillard 2011; Woodin, McCulloch and Cowan 2012). Moreover, since Britain’s nationwide reforms also required considerable preparation and financing, the first cohorts to be affected were effectively random.

\(^{23}\)The implausibility of heaping around the reform discontinuities is confirmed by annual birth rate data (Office for National Statistics 2013). Cohort trends in gender, age, survey year, race, father and mother voting conservative when young and father’s social grade are continuous at both discontinuities.
Although political trends are uncorrelated with CSL reforms, reforms could still reflect socioeconomic changes. However, Lochner and Moretti (2004) show state high school graduation trends are uncorrelated with CSL changes in the US, while Lleras-Muney (2002) uses placebo tests to demonstrate CSLs cannot explain prior enrollment. Additional concerns are addressed below by controlling for labor market conditions and educational inputs, while including state-specific cohort trends in the US and examining British cohorts around the reform discontinuities provide powerful checks against CSLs reflecting general underlying processes. In particular, the RD approach eliminates any shift that does not differentially affect adjacent cohorts.

Given the close proximity of CSLs to schooling choices, there is limited scope for CSLs to violate the exclusion restriction by affecting an individual’s political preferences through other channels, especially since reforms are not correlated with waves of political sentiment. State-specific cohort trends and placebo tests bolster these claims, and rule out possible spillovers from CSL reforms, while controlling for state Republican vote share and state income block the most plausible exclusion restriction violations.

5 Effects of schooling on political preferences

This section identifies large political effects of additional years of late high school. Inducing students to attend high school using CSLs considerably increases the probability that an individual will identify with, or vote for, the Republicans or Conservatives later in life.

5.1 Partisanship and voting in the US

To estimate the reduced form effects of CSLs on Republican partisanship and voting, I estimate the following difference-in-difference equation:

\[
\Pr(Y_{igct} = 1) = \delta_1 1(CSL_{gc} = 16) + \delta_2 1(CSL_{gc} \geq 17) + \alpha_g + \theta_c + \eta_t + W_i \gamma + \epsilon_{igct},
\]  

(2)
using OLS in the NAES sample, where $\alpha_g$, $\theta_c$, and $\eta_t$ are respectively state grew up in, birth-year cohort and survey-year fixed-effects. Individual-specific pre-treatment characteristics $W_{it}$—gender, race indicators and fourth-order demeaned age polynomials—are included primarily to enhance estimation efficiency. As noted above, this estimation strategy compares changes in Republican support among respondents from states which changed their CSLs for a given cohort to respondents from states which did not, while $\eta_t$ captures common shocks moving all voters towards one party in a given survey year.

IV techniques build upon the difference-in-difference approach above to identify the causal effect of schooling by instrumenting for $S_i$ with the indicators $1(CSL_{gc} = 16)$ and $1(CSL_{gc} \geq 17)$ in the following equation:

$$\Pr(Y_{igt} = 1) = \beta S_i + \alpha_g + \theta_c + \eta_t + W_{it} \gamma + \epsilon_{igt}. \quad (3)$$

I use the two-sample-2SLS (TS2SLS) estimator, which computes the first-stage using the ACS dataset and reduced form using the NAES dataset, efficiently combining the two as a consistent two-step estimator (Inoue and Solon 2010). Because years of schooling is not measured in the NAES, an OLS benchmark cannot be estimated.

Beyond the standard IV assumptions discussed above, TS2SLS requires that both datasets independently sample from the same population (Inoue and Solon 2010). Since both samples draw randomly from the voting age citizen population (once ineligible voters from the NAES sample and those aged below 18 and born outside the US are removed from the ACS sample), I then stratified by year of birth, gender, race and survey year to randomly draw ACS observations to replicate the distribution of NAES respondents over these pre-treatment covariates.24

24The Online Appendix provides further details about TS2SLS, while Tables 5 and 6 in the Appendix demonstrate that stratified sampling successfully matches the distributions of the two samples.
In both the reduced form and IV specifications, standard errors are clustered by state-cohort. The cluster-robust covariance matrix for TS2SLS is derived in the Online Appendix.\(^{25}\)

### 5.1.1 Results

Specifications (1)-(4) in Table 2 report the reduced form estimates of the effect of increasing the school leaving age on the propensity for an individual from a particular cohort to identify as or vote Republican. Compared to states requiring students to remain in school until at most age 15, requiring a cohort to remain in school until 16 significantly increased their probability of identifying as Republican and intending to vote for the Republican Presidential candidate by 3 percentage points, while keeping students in school until at least 17 adds an additional 1-2 percentage points. These results clearly show that CSLs could persistently alter the outcomes of elections within a state.

[Table 2 about here]

The IV estimate of the effect of additional years of high school for particular individuals are more theoretically interesting. The first-stage coefficients for the excluded instruments in specification (5) show that, relative to state-years with CSLs below 15, keeping students in school until 16 increases the average number of years of schooling by 0.35 years, while keeping students until at least 17 adds a further 0.09 years. The larger effect at 16 reflects comparison to an omitted category containing respondents who comply with CSLs mandating students only remain in school until between 12 and 15.\(^{26}\) These positive effects are comparable to previous estimates (Acemoglu and Angrist 2000), and entail a large \(F\) statistic of 47.6.

Reinforcing the reduced form results, the TS2SLS estimates show an additional year of high

\(^{25}\)OLS specifications were estimated in Stata 13; TS2SLS specifications were estimated in \(\mathbb{R}\).

\(^{26}\)Most students in this group face a leaving age of 14. Using a dummy for all leaving ages shows approximately-linear increases in the effect of CSLs (up to 17) on schooling and produces similar TS2SLS results.
Table 2: Schooling’s effect on Republican partisanship and voting in the US

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
<th>(9)</th>
<th>(10)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Partisan OLS</td>
<td>Partisan OLS</td>
<td>Vote OLS</td>
<td>Vote OLS</td>
<td>Schooling OLS</td>
<td>Schooling OLS</td>
<td>Partisan TS2SLS</td>
<td>Partisan TS2SLS</td>
<td>Vote TS2SLS</td>
<td>Vote TS2SLS</td>
</tr>
<tr>
<td>CSL=16</td>
<td>0.036***</td>
<td>0.015</td>
<td>0.028**</td>
<td>0.035***</td>
<td>0.353***</td>
<td>0.219***</td>
<td>0.129***</td>
<td>0.122**</td>
<td>0.074***</td>
<td>0.183***</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.012)</td>
<td>(0.011)</td>
<td>(0.013)</td>
<td>(0.048)</td>
<td>(0.037)</td>
<td>(0.028)</td>
<td>(0.053)</td>
<td>(0.028)</td>
<td>(0.057)</td>
</tr>
<tr>
<td>CSL≥17</td>
<td>0.055***</td>
<td>0.035***</td>
<td>0.033***</td>
<td>0.049***</td>
<td>0.438***</td>
<td>0.256***</td>
<td>0.129***</td>
<td>0.122**</td>
<td>0.074***</td>
<td>0.183***</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.013)</td>
<td>(0.009)</td>
<td>(0.014)</td>
<td>(0.049)</td>
<td>(0.039)</td>
<td>(0.028)</td>
<td>(0.053)</td>
<td>(0.028)</td>
<td>(0.057)</td>
</tr>
<tr>
<td>Schooling</td>
<td>0.129***</td>
<td>0.122**</td>
<td>0.074***</td>
<td>0.183***</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
<td>(0.053)</td>
<td>(0.028)</td>
<td>(0.057)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Linear state-specific cohort trends</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Reduced form (NAES) observations</td>
<td>166,189</td>
<td>166,189</td>
<td>134,315</td>
<td>134,315</td>
<td>166,189</td>
<td>166,189</td>
<td>134,315</td>
<td>134,315</td>
<td></td>
<td></td>
</tr>
<tr>
<td>First-stage (ACS) observations</td>
<td>440,963</td>
<td>440,963</td>
<td>440,963</td>
<td>440,963</td>
<td>440,963</td>
<td>440,963</td>
<td>440,963</td>
<td>440,963</td>
<td></td>
<td></td>
</tr>
<tr>
<td>First-stage $F$ statistic</td>
<td>47.6</td>
<td>21.6</td>
<td>47.6</td>
<td>21.6</td>
<td>47.6</td>
<td>21.6</td>
<td>47.6</td>
<td>21.6</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: dependent variables (listed at the top of each column) are Republican partisan, voting Republican in previous presidential election, and completed years of schooling; omitted CSL category is CSL≤15; all specifications include gender, white, black and Asian dummies, quartic demeaned age polynomials, and state grew up, cohort and survey fixed-effects; standard errors clustered by state-cohort in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 
school has a large pro-Republican effect. The results predict a 7-18 percentage point increase in the probability that an individual supports the Republicans for each additional year near the end of high school. However, the effect of additional schooling is highly localized. CSLs keeps students in high school does not affect college attendance (see Figure 7 below). Because schooling’s effect pertains only to late high school, these results remain consistent with college education producing Democrat-leaning liberals. These results indicate that averaging across education levels is often misleading because education’s political effects are at least non-linear if not non-monotonic.

5.1.2 Robustness

Although state CSL reforms are uncorrelated with individual respondent characteristics within a state, they could correlate with underlying changes in state characteristics. However, supporting the difference-in-difference parallel trends assumption, Table 2 shows that the reduced form, first-stage and IV estimates are all robust to including linear state-specific cohort trends which capture varying political trajectories across states. The results are also robust to including state personal income per capita as a proxy for labor market opportunities.

Very similar results are found using the ANES data. Combining Census extracts for the first-stage with an ANES reduced form, TS2SLS estimates show schooling significantly increases Republican partisanship and self-reported Republican voting in Presidential, House and Senate elections. This check is important for several reasons. First, the ANES covers a much wider span of elections, and thus includes more voters from earlier cohorts when individuals left school earlier (average schooling is 10.1 grades). Second, since the ANES asks which state the respondent lived in at age 14, obtaining similar results indicates that cross-state migration did not bias the NAES estimates. Finally, since ANES respondents are asked how they voted just after the Presidential election, the similarity of the results indicates that NAES respondents go through with their intention to vote for Republican Presidential candidates.
5.2 Partisanship and voting in Great Britain

5.2.1 All cohorts

CSLs are a nationwide policy in Britain, so Britain’s analogue of equation (3) cannot include cohort effects. Instead, I estimate:

\[ \Pr(Y_{ict} = 1) = \beta S_i + \eta_t + W_{it} \gamma + \epsilon_{ict}, \]  

(4)

where \( \eta_t \) is an election/survey fixed-effect, and \( W_{it} \) includes a gender indicator and quartic demeaned age polynomials. Equation (4) is first estimated using OLS as a benchmark. As in the US, CSL reform indicators \( 1(\text{CSL}_{ge} = 15) \) and \( 1(\text{CSL}_{ge} = 16) \) replace \( S_i \) to produce reduced form estimates, before serving as excluded instruments for \( S_i \) in 2SLS specifications (estimated using only the BES data). All specifications report cohort-clustered standard errors.

[Table 3 about here]

Table 3 reports the results. The OLS estimates—which are likely biased—in columns (1) and (6) average across all eligible voters, indicating that each additional year of schooling is associated with a five percentage point greater likelihood of identifying as or voting Conservative. By dummying out age left school, Figure 3 shows that averaging across lengths of schooling masks important heterogeneity: while leaving at any age below age 14 is indistinguishable from no schooling at all, the effect of each additional year beyond age 14, 15 and 16 is around ten percentage points, preliminarily suggesting schooling’s effects on political preferences may be concentrated at the end of high school.\(^7\)

The reduced form estimates in specifications (2) and (7) indicate that raising the leaving age to either 15 or 16 increases the probability of supporting the Conservatives by up to five percentage points. Such large shifts for affected cohorts imply the reforms substantially altered national pol-

\(^7\)Results are very similar for voting Conservative.
Table 3: Schooling’s effect on Conservative partisanship and voting in Britain

<table>
<thead>
<tr>
<th></th>
<th>Outcome = Conservative partisan</th>
<th></th>
<th>Outcome = Conservative vote</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) OLS</td>
<td>(2) OLS</td>
<td>(3) OLS</td>
<td>(4) OLS</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(5) 2SLS</td>
<td></td>
</tr>
<tr>
<td>Leave schooling</td>
<td>0.055*** (0.004)</td>
<td>0.142*** (0.044)</td>
<td>0.053*** (0.004)</td>
<td>0.179*** (0.045)</td>
</tr>
<tr>
<td>CSL=15</td>
<td>0.051*** (0.015)</td>
<td></td>
<td>0.070*** (0.014)</td>
<td></td>
</tr>
<tr>
<td>CSL=16</td>
<td>0.089*** (0.025)</td>
<td></td>
<td>0.093*** (0.021)</td>
<td></td>
</tr>
<tr>
<td>CSL=15 (1942 placebo)</td>
<td>0.017 (0.014)</td>
<td></td>
<td>0.019 (0.015)</td>
<td></td>
</tr>
<tr>
<td>CSL=16 (1967 placebo)</td>
<td>-0.028** (0.013)</td>
<td></td>
<td>-0.009 (0.014)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>First-stage (Outcome = Leave schooling)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>CSL=15</td>
<td>0.382*** (0.052)</td>
<td></td>
<td>0.380*** (0.049)</td>
<td></td>
</tr>
<tr>
<td>CSL=16</td>
<td>0.591*** (0.086)</td>
<td></td>
<td>0.567*** (0.085)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>20,387 20,647 9,322 11,761 20,387</td>
<td></td>
<td>17,819 18,049 8,497 10,826 17,819</td>
<td></td>
</tr>
<tr>
<td>First-stage F statistic</td>
<td>28.4</td>
<td></td>
<td>30.2</td>
<td></td>
</tr>
</tbody>
</table>

Notes: omitted CSL category is CSL ≤ 14; all specifications include demeaned age quartics, a gender dummy, and survey fixed-effects; standard errors clustered by cohort in parentheses.
Figure 3: OLS estimates of the effect of schooling on Conservative partisanship in Britain, dummying out year left school (with 95% confidence interval)

Coefficient estimate (omitted category: leave school<8)
itics, particularly when elections were close throughout the 1970s. However, by averaging across many individuals that were not affected by the reforms, these estimates underestimate the impact on individuals who only remained in school because of the reforms. To calculate the effects for such compliers, I use 2SLS.

The final column of each panel shows significant first-stage coefficients and large $F$ statistics. The large coefficient for the 1947 reform is akin to Devereux and Hart (2010) and Oreopoulos (2006) using similar datasets, and Clark and Royer (2013) using month of birth data. The Online Appendix shows the 1947 reform increased the proportion staying until 15 and 16 at the 5% level, while the 1972 only significantly increased the probability of staying until 16. Neither reform increased the probability of attending university.

The uppermost rows in specifications (5) and (10) report the 2SLS estimates, showing—like in the US—an additional year of schooling raises the probability of identifying as or voting Conservative by around 15 percentage points. This large localized effect, which is consistent with but around 50% larger than the OLS patterns in Figure 3, is only identified for the compliers induced to remain in school until 15 and 16. As suggested above, larger effects among compliers accord with theoretical expectations.

Unlike the US, it is not clear which party loses support to the Conservatives. The Online Appendix shows Labour are the principal loser of partisans, but their losses are smaller and not quite statistically significant for voting. The Liberals lost voters, especially following the second reform. This may seem surprising at first glance since the Liberals are typically supported by the well-educated. However, CSL-compliers are not induced to attend university. Thus, increased Conservative support has come at the expense of both Labour and the Liberals.

5.2.2 Cohorts around the reforms

Since the full sample estimates compared cohort grouping conditional means without controlling for cohort effects, such groupings could be correlated with changes in society that affected cohort
groupings differentially. To mitigate such cohort bias concerns, I exploit the discontinuities around each reform by comparing students from cohorts either side of the 1947 and 1972 reforms.

Figures 4 and 5 depict the reduced form relationships by comparing Conservative support by cohort. Even after controlling for the effects of age,\textsuperscript{28} there remain steady downward trends in support for the Conservatives across cohorts. This suggests cohort bias is a concern, but since schooling increases for later cohorts while Conservative support declines any 2SLS bias is likely to be downward. Nevertheless, consistent with the 2SLS results, there is a distinct upward shift in the proportion of each cohort supporting the Conservatives associated with the 1947 reform, and a smaller increase in Conservative partisanship around the 1972 reform which becomes more pronounced for later cohorts. Picking up differences at the 1972 is also more difficult because the reform affected schooling less than the 1947 reform.

[Figures 4 and 5 about here]

An RD approach formally assesses these relationships. The reduced form discontinuity around each reform is estimated using a standard “sharp” RD:

\[
\Pr(Y_{ict} = 1) = \beta 1(CSL_c = z) + f(\text{Year aged } z - 1) + \eta_t + W_{it} \gamma + \epsilon_{ict}, \quad z = 15, 16. \quad (5)
\]

This equation is estimated parametrically where \( f(\cdot) \) adds quadratic trends in the running variable (cohort) that differ either side of each discontinuity.\textsuperscript{29} The effect of staying in school beyond the required age is similarly estimated using a “fuzzy” RD, allowing the running variable to discontinuously change the probability of remaining in school following the reform.\textsuperscript{30} Standard errors are

\textsuperscript{28}Other than trending more clearly downward (since younger people are less conservative), taking raw support proportions produced very similar results.

\textsuperscript{29}Non-parametric estimates (see Online Appendix) and higher-order polynomial trends provide very similar results.

\textsuperscript{30}The fuzzy RD instruments for \( S_i \) with \( 1(CSL_c = z) \) and \( \sum_{p=1}^{2} \tau_1 p(\text{Year aged } z - 1)^p \times 1(CSL_c = z) \).

30
Figure 4: Conservative support by cohort around Britain’s 1947 reform

Notes: proportion Conservative supporters by cohort computed using residuals from regressions of Conservative partisan and vote on quartic age polynomials.
Figure 5: Conservative support by cohort around Britain’s 1972 reform

Notes: see Figure 5.
Table 4: Sharp and fuzzy regression discontinuity estimates of Conservative partisanship and voting in Britain

<table>
<thead>
<tr>
<th>Outcome = Conservative partisan</th>
<th>Outcome = Conservative vote</th>
</tr>
</thead>
<tbody>
<tr>
<td>1947 reform</td>
<td>1972</td>
</tr>
<tr>
<td>(1)  OLS</td>
<td>(2)  OLS</td>
</tr>
<tr>
<td>CSL=15</td>
<td>0.026**</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
</tr>
<tr>
<td>CSL=16</td>
<td>-0.138</td>
</tr>
<tr>
<td></td>
<td>(0.115)</td>
</tr>
<tr>
<td>Leave schooling ≥ 15</td>
<td>0.108***</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
</tr>
<tr>
<td>RD type</td>
<td>Sharp</td>
</tr>
<tr>
<td>Observations</td>
<td>20,647</td>
</tr>
<tr>
<td></td>
<td>Fuzzy</td>
</tr>
<tr>
<td></td>
<td>20,387</td>
</tr>
<tr>
<td></td>
<td>Sharp</td>
</tr>
<tr>
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<td>20,647</td>
</tr>
<tr>
<td></td>
<td>1972</td>
</tr>
<tr>
<td></td>
<td>(3)  OLS</td>
</tr>
<tr>
<td></td>
<td>0.044***</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
</tr>
<tr>
<td></td>
<td>-0.077</td>
</tr>
<tr>
<td></td>
<td>(0.091)</td>
</tr>
<tr>
<td></td>
<td>0.165***</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
</tr>
<tr>
<td></td>
<td>1972</td>
</tr>
<tr>
<td></td>
<td>(5)  OLS</td>
</tr>
<tr>
<td></td>
<td>18,049</td>
</tr>
<tr>
<td></td>
<td>17,819</td>
</tr>
<tr>
<td></td>
<td>18,049</td>
</tr>
</tbody>
</table>

Notes: standard errors clustered by cohort in parentheses; all specifications include demeaned age quartics, a gender dummy, survey fixed-effects, and quadratic polynomials in year aged 14 or 15 either side of the relevant reform.

The results are shown in Table 4. The sharp RD estimates for the 1947 reform are slightly below the reduced form estimates in specifications (2) and (7) of Table 3, but remain statistically significant.\footnote{The reduction in coefficient magnitude could reflect minor adjustment for cohort effects in the full sample, although the graphical discontinuities also suggest negative bias in the RD estimates due to the “boundary problem” may be a concern even when using LLR (Hahn, Todd and Van der Klaauw 2001).} Suggesting the discontinuity estimates are consistent with the analysis comparing all cohorts, the fuzzy RD estimates are always significantly positive and similar in magnitude to the...
full sample 2SLS estimates. Because voting Conservative is trending downward across cohorts, the positive effect at the discontinuity adds credibility to a positive effect for additional years of high school because this could only be explained by a sharp and fleeting reversal in trend at the discontinuity.

Unsurprisingly, given Figure 5, there is no clear reduced form relationship at the 1972 discontinuity, as shown in specifications (3) and (6). Since subsequent cohorts are several points more conservative, this may reflect a failure to satisfy continuity around the discontinuity; see below for a coming of age explanation. Fuzzy estimates are omitted.

5.2.3 Robustness

A key concern is violation of the exclusion restriction. While the RD analysis confirmed a difference exists between cohorts at the 1947 discontinuity, changes in CSLs could reflect prior trends in opinion among those unaffected by the reform, or CSL reforms could also influence older voters via spillover effects. These concerns are assessed using placebo tests: restricting the sample to those born before each reform and using a placebo reform five years prior to the actual reform, specifications (3), (4), (8) and (9) in Table 3 show the placebo reforms had no effect on political outcomes.  

Nevertheless, if the timing of CSL reforms is correlated with contemporaneous societal changes, such changes could be incorrectly attributed to CSL reforms. One concern is that the quality of schooling inputs increased at the discontinuity (Goldin and Katz 2008), raising labor market returns and making voters more conservative. However, despite planning for the educational expansions, both reforms—especially in 1947—required considerable resource-stretching (e.g. temporary classrooms, accelerated teacher training, less competitive teacher selection; Oreopoulos 2006). This is corroborated by controlling for the teacher/pupil ratio at age 14 (available for non-war years) and average school size. Another possible societal confounder is labor market con-

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32Unreported specifications show the placebos did not affect schooling.
ditions, but controlling for labor’s production share, the unemployment rate, a (logged) index of average earnings and average weekly hours, separately at both ages 14 and 18 for each respondent leaves estimates unaffected. Finally, the results are robust to controlling for mother’s and father’s voting preferences when growing up, father’s social class when growing up, and race.

A final potential concern is “coming of age” effects, where the election cohorts are first eligible to vote at affects subsequent behavior (Mullainathan and Washington 2009). This is not a problem around the 1947 discontinuity as those affected turned 21 around 1954, leaving a buffer of three untreated cohorts since the election in 1951 for comparison, while the Conservatives performed similarly at the 1951 and 1955 elections. However, those affected by the 1972 reform turned 18 around 1975, a year after the 1974 election. Their first election was in 1979, which Margaret Thatcher won on a contentious platform likely to harm young adults entering manual vocations. Since compliers are likely to have been harmed (see characteristics below), this could explain initial the reluctance to support the Conservatives. Conversely, the pre-reform cohorts first voted at the two 1974 elections, which ultimately produced an extremely weak Labour government and ushered in an 18-year period of Conservative government. Therefore, the full sample results may be weak around the discontinuity because first election effects cannot be distinguished from CSLs effects.

5.3 Complier characteristics

The preceding analysis showed that IV estimates for CSL-compliers are consistently large. This paper has suggested this arises because compliers are likely to be disadvantaged and have more to gain from schooling. Although individual compliers cannot be identified without heroic assumptions, we can examine average complier characteristics using Abadie’s (2003) “kappa” method of weighting observations by their estimated complier probability.

The analysis confirms compliers are disproportionately from low-SES backgrounds. Around 16% of the British sample are compliers at the 1947 reform, while 5% were compliers at the 1972
reform. Compared to the sample average, British CSL-compliers have lower-class fathers and parents who did not support the Conservatives. Given many states have kept relatively constant CSLs, only 5% of the US sample are compliers, making complier characteristics harder to differentiate. Still, US compliers are generally younger and had parents who were more likely to be laborers or in semi-skilled vocations. Moreover, Lleras-Muney (2002) shows 1914-1939 state CSLs affected the lower percentiles of the education distribution. The Online Appendix explains this method and provides results.

6 Mechanisms

Having identified a large positive effect of high school on political preferences, I now explore the potential mechanisms predicting this effect outlined in Table 1. Clear evidence for a mechanism would verify the link (i) from schooling to a given mediator, and (ii) from the mediator to political preferences. Using the IV methods above, (i) can be tested and identified for the same set of CSL-compliers. However, establishing (ii) is very difficult without making strong assumptions (Imai et al. 2011), so I focus on eliminating potential mediators by showing (i) is not supported.

A key prediction differentiating the political economy from most socialization theories is when schooling’s effect should be greatest. While most socialization explanations imply an instantaneous jump or monotonic change in political preferences, the human capital/RMR and social insurance channels suggest schooling’s effect peaks when the nominal schooling premium is largest in mid-life. Exploiting the synthetic panel spanning several decades in Britain, Figure 6 plots the marginal effect of schooling on voting Conservative interacted with quadratic age terms using 95% confidence intervals. The mid-40s peak in schooling’s effect supports the political econ-
Figure 6: Life-cycle effect of schooling on Conservative voting in Britain (with 95% confidence interval)
omy explanations. By changing gradually over time, it also implies that political preferences reflect short-term income expectations or only cohere once sufficient information about the expected return becomes available. Especially problematic for socialization stories proposing an immediate change in behavior, the effect of schooling is insignificant immediately after the completion of schooling. Schooling’s declining effect in later life is also inconsistent with theories suggesting schooling facilitates resource accumulation without accounting for late-life atrophy, such as network-oriented theories or post-materialism.

[Figure 6 about here]

Nevertheless, the life-cycle results could still be consistent with Inglehart (1981) if socialization is slow to occur and scarcity returns later in life. Accordingly, it is important to examine how schooling affects partisan behavior. Asking respondents whether environmental concerns are more pressing than economic concerns in the US and using Inglehart’s standard post-materialism question in Britain, Figures 7 and 8 show no effect of schooling on holding post-materialist values. Since CSLs do not induce respondents to attend university, the null effect is perhaps unsurprising.

[Figures 7 and 8 about here]

Various political engagement theories propose that schooling should increase trust in government, political interest and political information (both measured with multi-item scales), watching the news, and newspaper readership. It is possible such mechanisms could turn voters more conservative. However, the results in Figures 7 and 8 show very limited support for these claims in the US or Britain. Social network explanations also receive no support, as there is no increase in political discussion with friends and family.

In contrast, the mechanisms underpinning the political economy explanations receive robust support. Although common to various theories, evidence has consistently found that schooling— instrumented with CSLs—substantially increases wages (Acemoglu and Angrist 2000; Harmon and Walker 1995; Oreopoulos 2006). Particular to political economy explanations, an additional reported in the Online Appendix.
Figure 7: Schooling’s effect on potential mediators in the US (with 95% confidence interval)

Notes: all coefficients from TS2SLS specifications including state-specific cohort trends used in Table 2; all outcomes standardized, except binary outcomes denoted by *; see Appendix for mediator definitions.
Figure 8: Schooling’s effect on potential mediators in Britain (with 95% confidence interval)

Notes: all coefficients from 2SLS specifications used in Table 3; all outcomes standardized, except binary outcomes denoted by *; see Appendix for mediator definitions. Insignificant effect on discuss politics with friends and family omitted due to large confidence interval.
year of US high school increases the (binary) belief that taxes should be reduced by 14 percentage points. In Britain, schooling also reduces support for higher tax and spending by 0.1 standard deviations, and increases the belief that welfare benefits have extended too far by 0.2 standard deviations.

While anti-redistributive preferences clearly fit with the RMR model, they are also consistent with social insurance motives. However, additional high school does not reduce vocation-based skill specificity among US compliers as social insurance models predict. Looking across industries, the Online Appendix shows schooling pushes CSL-compliers—at the expense of unskilled service and manual occupations—into managerial and professional industries, but almost into equally skilled craft and machine-operating labor. This suggests late high school may be a prerequisite for entering better-paid and more prestigious general and skill-specific occupations, which could both contribute to conservative political preferences through the RMR income channel. Since the most valuable general skills are acquired at university, this is probably where differential occupational choice and insurance preferences are principally manifested. Nevertheless, this analysis cannot show that schooling does not increase job security within a given occupational category.

This analysis cannot decisively support the human capital/RMR political economy channel over all other explanations. However, unlike all other theories the data is consistent with all its central implications. Plausible socialization explanations for the conservative effect of schooling receive no clear support, while there is little support for the key tenet of social insurance theories.

7 Conclusion

Despite its importance in shaping the lives of citizens, political scientists have neglected the causal effect of education on political preferences. Consistent across a variety of research designs requiring different identifying assumptions, this article demonstrates that high school has had large
partisan-political effects on citizens who were forced to stay in school for an additional year near
the end of high school. In both the US and Great Britain, additional schooling—and CSLs in
their on right—increase the propensity to identify with or vote for conservative political parties.
While numerous studies have linked education to civic and political participation, this paper gives
schooling a partisan tint by showing that it causes voters to become more conservative.

Although both political economy and socialization theories could explain this result, a human
capital combined with Romer-Meltzer-Richard explanation receives by far the greatest empirical
support. Conversely, there is no evidence to suggest that additional schooling affects an indi-
vidual’s occupational skill mix or increases post-materialist values, trust in government, political
information, political interest or newspaper readership.

As with any IV analysis, estimated causal effects pertain only to CSL-compliers in the later
years of high school in the US and Britain. Given CSLs continue to be an important policy lever,
this set of compliers is unusually relevant among quasi-experiments—and will remain so as Britain
increases its schooling leaving age to 18 in 2015 and US state actively consider raises their school
leaving ages. An important unanswered question is whether these causal effects extend to post-high
school vocational education.

To the extent that previous research has shown university education reduces conservative at-
titudes, these results suggest education’s effect on political preferences are non-linear because
additional high school makes students more conservative later in life. Late high school clearly
represents a critical juncture in an individual’s political life, and thus warrants further investigation
of the channels through which different political preferences are reinforced. More generally, these
non-linear effects of education necessitate researchers treating education as a simple linear effect
think more carefully about the role of different levels of education.

The large effects of high school have important political implications. Particularly for left
parties, they pose a strategic dilemma: increasing education opportunities for the disadvantaged
has long been a key plank in the policies of the left, but the results for secondary education suggest
that this has come at the cost of losing voters. One response from the left—typified by New Labour in the UK—may have been to move toward the political center. This could at least partially explain the large rightward shift in the median voter and left parties documented across advanced democracies (Pontusson and Rueda 2010), where schooling (and income) has risen considerably for post-WW2 cohorts.
8 Appendix

Variable definitions and summary statistics are provided below. See Online Appendix for details about variable construction and TS2SLS methods.

8.1 US data

ACS data for 2000, 2001, 2003, 2004, 2007 and 2008 was provided by Ruggles et al. (2010). In order to match the ACS respondent distribution to the NAES over cohort-year, gender, race and survey year, I first removed respondents aged 14 before 1920 (since no NAES respondent was 14 before 1920) and respondents who grew up in Alaska or Hawaii (where CSLs were unavailable) or were born outside the US. I then stratified by cohort-gender to draw a random sample of 2,000,000 ACS respondents to ensure the subsample has the same by-cohort-by-gender distribution as the NAES. I then reduced the sample to ensure the proportion of respondents from each year reflects the NAES dataset distribution, before finally stratifying to ensure the sample reflects the NAES distribution over the white, black and Asian race dummies. This produced a pooled sample of 418,974 individuals after observations suffering missingness were deleted.

The variables defined below are from the NAES or ACS unless otherwise stated. Tables 5 and 6 provide summary statistics.

[Tables 5 and 6 about here]

- Republican partisan. Republican identifier indicator; includes strong and weak identifiers, but not Republican leaning; residual are Democrats and independents.

- Vote Republican for President. Indicator for respondents intending to vote Republican in the forthcoming Presidential election; residual voted otherwise.

- Male. Male indicator.
Table 5: Summary statistics—NAES

<table>
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<th>Obs.</th>
<th>Mean</th>
<th>Std. dev.</th>
<th>Min.</th>
<th>Max.</th>
</tr>
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<td>Vote Republican for President</td>
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<td>1</td>
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<tr>
<td><strong>Excluded instruments</strong></td>
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<td>97</td>
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<td>2,000</td>
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<td>Discuss politics with friends and family</td>
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Table 6: Summary statistics—ACS

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<td></td>
<td></td>
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<td>Age</td>
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<td>Black</td>
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<td>0.27</td>
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<tr>
<td>Asian</td>
<td>440,963</td>
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<tr>
<td>Year aged 14</td>
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<td>1,968.61</td>
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<td>2,004</td>
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<td>Survey year</td>
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<td>2,003.98</td>
<td>3.04</td>
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<td>2,008</td>
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<td><strong>Mediators</strong></td>
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<tr>
<td>Skill specificity</td>
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<td>0.05</td>
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<td>University</td>
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</tr>
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</table>
• **Race.** Indicators for White (including Hispanic), Black, and Asian self-identification; residual are all other races.

• **Age.** Age (years) at survey date.

• **Year aged 14.** Survey year, minus age at survey, plus 14.

• **State grew up.** Respondent’s current state of residence. See text for discussion.

• **Survey year.** Set of indicators for the year—2000, 2001, 2003, 2004, 2007 or 2008—in which the survey was conducted. Both the NAES and ACS use rolling surveys.

• **CSLs.** CSL=16 and CSL≥17 indicators for a respondent’s CSL, defined by state grew up. Data from Oreopoulos (2009), based on the National Center for Education Statistics’ *Education Digest*. To ensure more effective matching of CSLs to individuals, instead of assigning CSLs at age 14, I assign individuals to the CSLs that constraints their behavior; for example, if the leaving age was 16 at age 14 but rose to 17 at age 15, then this method applies the leaving age of 15 instead of 14.

• **Schooling.** Years of completed grade school; pre-Kindergarten counted at 0 and those that did not graduate high school with a diploma are coded as 11; top coded at 12.

• **Reduce taxes.** Indicators for respondents who think taxes should be reduced.

• **Skill specificity.** Relative skill specificity, averaged across 1 and 2-digit ISCO occupational classifications (“s1” in Iversen and Soskice 2001).

• **University.** Indicator for attending at least one year of college.

• **Trust federal government.** Indicator for respondents that trust the federal government to always or mostly do what is right.
• **Post-materialist.** Indicator for respondents who believe environmental concerns should take precedence over economic concerns.

• **Political knowledge.** Summative rating scale combining correct/incorrect answers to general political knowledge questions, including: identifying Republicans as more conservative than Democrats; the Congressional majority required to overturn a Presidential veto; which body determines whether a law is constitutional; identity of the majority party in the House; who the Vice-President is. Cronbach’s alpha inter-item reliability coefficient of 0.60 in 2003/2004 and 0.60 in 2007/2008; knowledge was not asked about in 2000.

• **Political interest.** How regularly does respondent follow government and public affairs; ranges from “hardly at all” (1) to “most of the time” (4).

• **Watch TV news.** Number of days in the last week (0 to 7) that the respondent watched a public or cable news program.

• **Read newspaper.** Number of days in the last week (0 to 7) that the respondent read a daily newspaper.

• **Discuss politics.** Number of days in the last week (0 to 7) that the respondent discussed politics with family or friends.

### 8.2 Great Britain data

Where surveys permit (in February and October 1974, 1979 and 1987), those who grew up outside Britain were removed. The variables used for the analysis are:

[Table 7 about here]

• **Conservative/Labour/Liberal partisan.** Indicator for identifying as Conservative/Labour/Liberal (excluding follow-up question identifiers); residual are non-identifiers or don’t know.
Table 7: Summary statistics—Great Britain

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<th>Std. dev.</th>
<th>Min.</th>
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<td></td>
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<td>0.48</td>
<td>0</td>
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<tr>
<td>Labour partisan</td>
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<td>0.48</td>
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<td>1</td>
</tr>
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<td>Liberal partisan</td>
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<td>Conservative vote</td>
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</tr>
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<td>Schooling</td>
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<td>15.54</td>
<td>1.75</td>
<td>0</td>
<td>19</td>
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<td><strong>Excluded instruments</strong></td>
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<td></td>
</tr>
<tr>
<td>CSL=15</td>
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<td>0.49</td>
<td>0</td>
<td>1</td>
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<tr>
<td>CSL=16</td>
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<td>0.18</td>
<td>0.39</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td><strong>Pre-treatment covariates</strong></td>
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<td></td>
</tr>
<tr>
<td>Male</td>
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<td>98</td>
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<td></td>
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<td></td>
<td></td>
</tr>
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<td>Extend welfare benefits</td>
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<td>1.01</td>
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<td>0.46</td>
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</table>
• Conservative/Labour/Liberal vote. Indicator for self-reported Conservative/Labour/Liberal vote in recent election.

• Schooling. Age left full-time schooling (1974(Feb)-1983) or full-time education (1987-1997); top-coded at ≥19.

• Male. Male indicator.

• Age. Age (years) at survey.

• Year aged 14/15. Survey year minus age at survey; then add 14 for year aged 14, but also 14 for year aged 15 because the 1972/76 legislation affected only those after September 1972/76 (compared with April 1947 for year aged 14).

• CSLs. Indicators for CSL=15 and CSL=16; residual ≤15. CSLs are mapped to year aged 14/15.

• Support tax and spend. 11-point scale (“Government should cut taxes a lot and spend much less on health and social services”-“Government should increase taxes a lot and spend much more on health and social services”).

• Extend welfare benefits. 5-point scale (Welfare benefits have gone “much too far”-Welfare benefits have gone “not nearly far enough”).

• University. Indicator for completing a university or polytechnic degree or diploma.

• Post-materialist. Indicator for ranking “Giving the people more say in important political decisions” and “Protecting freedom of speech” as the two most desirable goals for the nation (order-invariant); residual is all other orderings (non-responses counted as missing).

• Political knowledge. Standardized scale for proportion of correct answers to a political information quiz from 1979, 1992 and 1997 surveys (standardized to also include the 2001-2010
elections). 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). Summative rating scale combined for each survey separately and normalized for comparability.

• *Political interest*. Standardized scale combining five responses: interest in politics (1-5); interest in particular election (1-4); attention to politics (0-10); discuss politics with friends and family (0-10); and read daily newspaper indicator. Summative rating scale combined for each survey separately and normalized for comparability.

• *Discuss politics with friends and family*. 11-point scale (“Very unlikely to discuss”—“Very likely to discuss”).

• *Newspaper reading frequency*. Indicator for reading a daily newspaper.
References


