Despite education’s capacity to significantly alter students’ life trajectories, little is known about how education affects the political party an individual identifies with and votes for later in life. Using a difference-in-differences design to exploit variation in state compulsory schooling laws (CSLs) across cohorts in the U.S., I find that raising the minimum school leaving age by a year causes a 1-3 percentage point increase in support for the Republican party per cohort. Instrumental variable estimates show that each additional grade of late high school increases the probability that a CSL-complier identifies as or votes Republican by around 10 percentage points. Analysis of the mechanisms suggests that high school’s conservative effects primarily operate by increasing income, which in turn increases support for conservative economic policies and ultimately the Republican party. These results suggest both that recent Democrat attempts to raise state leaving ages, and Republican opposition to CSLs, may be strategically misguided.
1 Introduction

Education is a central component of U.S. economic and social policy, and continues to be a salient political issue.\(^1\) Particularly for the Democratic party, raising the high school dropout age is now a key tool helping to ensure that children from disadvantaged backgrounds share in the economic returns to education (see Oreopoulos 2009). Dropout ages in the U.S. have generally increased over the 20th century, and many states are currently considering raising their minimum leaving age to 18.\(^2\) President Obama underscored the importance of this issue in his 2012 State of the Union address, stating that “When students are not allowed to drop out, they do better. So ... I am proposing that every state ... requires that all students stay in high school until they graduate or turn 18.”

While education likely imparts valuable labor market benefits, educating large numbers of future voters could also alter the political equilibrium. Given that education can transform students’ life trajectories, and plays a key formative role at early stages of development (see Cunha et al. 2006), keeping students in high school could cause such voters to adopt very different policy preferences and support different political parties. Since voter partisan identities show considerable persistence from an early age (see Bartels 2010), education has the potential to significantly alter long-run electoral outcomes. This in turn affects the policies implemented by legislators, and ultimately the welfare of the electorate (e.g. Jones and Olken 2005; Lee, Moretti and Butler 2004; Pettersson-Lidbom 2008).

This article investigates the political implications of expanding education in the U.S. In particular, I identify the effects of high school education on individual voting behavior and

\(^1\)Across the federal, state and local levels, spending on education represents 5% of GDP in 2015. It is the third largest budget item behind pensions and health care.

\(^2\)In 2013, Kentucky amended its school attendance law passed in 1934, increasing the dropout age to 18 (or receiving a high school diploma). Although success has varied, similar bills have recently been introduced in Alaska, Delaware, Illinois, Maryland, Massachusetts, and Rhode Island since 2011.
policy preferences in later life. To date, the evidence is correlational and often conflicting. While some surveys document a positive association between greater education and voting Republican (e.g. Gelman et al. 2010), any correlation between education and voting behavior may simply reflect differences in the types of people that receive more education. Furthermore, by failing to disentangle either the direction of this relationship or its mechanisms, scholars have struggled to square the widely-documented correlations between income (which education increases) and support for conservative economic policies (e.g. Erikson and Tedin 2007; Gelman et al. 2010) and between education and socially liberal attitudes (e.g. Dee 2004; Gerber et al. 2010).

To identify the partisan effects of schooling, I employ a difference-in-differences strategy exploiting differences in compulsory schooling laws (CSLs) determining a state’s minimum school leaving age across states and cohorts (see also Acemoglu and Angrist 2000; Angrist and Krueger 1991). CSLs remain an important policy instrument, particularly since nearly 20% of students per national cohort still fail to graduate from high school (Murnane 2013). However, I also use variation in CSLs to instrument for an individual’s level of education. To overcome the problem that the number of years of schooling is not measured in prominent surveys of political behavior, and after demonstrating that instrumenting for an indicator for completing high school can substantially upwardly bias instrumental variable (IV) estimates, I use two-sample IV methods to estimate the political effects of schooling. Given CSLs do not affect post-high school education, the IV estimates identify the effect of an additional year of late high school for students that only remained in school due to a higher state dropout age.

I find that staying in high school causes voters to become more politically conservative in later life. I first show that CSLs have had a large political effect in their own right. Increas-

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3Classic accounts in political science pointing at a strong correlation include Campbell et al. (1960), Converse (1972), Verba, Schlozman and Brady (1995) and Wolfinger and Rosenstone (1980).
ing the minimum school leaving age by a year causes a 1-3 percentage point shift toward identifying with or voting for the Republican party per birth-year cohort. The IV estimates show that an additional completed grade of late high school increases the probability that a student will subsequently identify as a Republican partisan and vote for a Republican Presidential candidate by around 10 percentage points. Among compliers, compulsory education thus dramatically affects downstream political behavior, causing students to become considerably more conservative in their partisan choices. This finding raises a “catch 22” for the Democratic party: while increasing access to education is an important plank of their left-wing agenda, it comes at the cost of the beneficiaries of additional schooling supporting the Republican party in the future.

These results are robust to a variety of potential empirical concerns. First, I show that changes in CSLs—which have historically been implemented equally by Democrats and Republicans—are uncorrelated with the party controlling the state legislative assemblies. Second, the results are unaffected by the inclusion of state-specific cohort trends and economic and political controls that could violate the parallel trends assumption. Third, the considerable selective migration required to explain the large effects of schooling is neither consistent with migration patterns nor results from a second survey. Fourth, there is no evidence to suggest that the exclusion restriction is violated by trends in political support or changes in fertility that might arise from keeping a student in school (but not because of the education they received).

I also provide evidence suggesting that high school’s political effects work primarily through an income-based channel. Previous evidence from the U.S. indicates that an additional year of schooling increases wages by around 10 percent income (e.g. Acemoglu and Angrist 2000; Angrist and Krueger 1991; Ashenfelter and Krueger 1994; Ashenfelter and Rouse 1998; Oreopoulos 2006). Consistent with education’s economic returns inducing voters to support conservative policies (Meltzer and Richard 1981; Romer 1975), the political
effects of education are greatest at an individual’s mid-life earnings peak, while an additional
year of school also causes voters to favor Republican positions on economic issues. Furthermore, while this could simply reflect voters becoming Republican partisans and adopting the
party’s policy positions (Lenz 2012; Zaller 1992), education does not increase support for Re-
publican positions on non-economic issues such as abortion, gun control, health care spending
and military spending. Contrary to the predictions of civic education (Dee 2004; Hillygus
2005) and social network (Green, Palmquist and Schickler 2002; Nie, Junn and Stehlik-Barry
1996) socialization theories, I find no evidence that education increases socially liberal values
or political engagement. Together, these findings suggest that, by increasing an individual’s
subsequent income, education causes voters to support economic policies that, ultimately,
lead them to identify as and vote Republican.

By identifying how education affects which party an individual voter supports, this article
departs from the existing political economy literature focusing on the relationship between
education and the quality of democracy. One influential strand of this literature continues to
debate whether increases in mass education cause countries to democratize (e.g. Acemoglu
et al. 2005; Glaeser, Ponzetto and Shleifer 2007). At the individual level, there is also signif-
icant debate as to whether education, particularly at the university level, increases political
participation in the U.S. (e.g. Berinsky and Lenz 2011; Dee 2004; Milligan, Moretti and
Oreopoulos 2004; Sondheimer and Green 2010), as well as in developing contexts (Friedman
et al. 2011; Larreguy and Marshall 2014; Wantchekon, Novta and Klašnja 2013). In con-
trast, my findings suggest that education affects policy outcomes by increasing the likelihood
that more conservative politicians are elected. This implies both that Democrat attempts
to raise state leaving ages, and recent Republican opposition to CSLs, may be strategically
misguided in terms of maximizing political support.

The results also contribute to a well-established literature identifying education’s down-
stream non-labor market externalities (see Oreopoulos and Salvanes 2011). Previous research
has suggested that education increases health outcomes (Lleras-Muney 2005), child health outcomes (Currie and Moretti 2003), and reduces criminal activity (Lochner and Moretti 2004). My findings thus emphasize that education has important political as well as socioeconomic consequences.

Finally, this article also highlights an important methodological issue that arises in an IV context when an endogenous variable is coarsened. Building on Angrist and Imbens (1995), I show that dichotomizing a multi-valued or continuous endogenous variable can substantially upwardly bias estimates by introducing a subtle exclusion restriction violation. Intuitively, this bias arises when the instrument affects the intensity of the underlying treatment, which in turn affects the outcome of interest, but that change in treatment intensity is insufficiently large to pass the threshold required to register in the first stage. For this reason, I instrument for years of schooling, instead of an indicator for completing high school, in order to consistently estimate the local average causal response.

This article proceeds as follows. Section 2 examines potential links between schooling and support for particular political parties. Section 3 then explains the empirical design and describes the data. Section 4 presents the main results. Section 5 explores the mechanisms linking high school to greater support for the Republican party. Finally, section 6 concludes by briefly considering the implications of the results.

2 Theoretical motivation

Although much has been written about how education affects income, socioeconomic outcomes and civic participation, education’s effect on political behavior has received limited theoretical attention. I first consider the economic and sociological mechanisms through which schooling could affect which political party a voter will support.
2.1 Income decreases support for taxation

A considerable body of research suggests that greater education increases an individual’s wage income. Twin studies in the U.S. suggest that an additional year of schooling increases annual wages by 11-16 percent (Ashenfelter and Krueger 1994; Ashenfelter and Rouse 1998), while studies using compulsory schooling laws to instrument for schooling estimate this rate of return to be 8-18 percent (Acemoglu and Angrist 2000; Angrist and Krueger 1991; Oreopulos 2006).\(^4\) The human capital interpretation of this relationship claims that education imparts productive skills, which are rewarded in competitive labor markets (e.g. Becker 1964; Mincer 1974).

Linking schooling’s effect on income to political behavior, Romer (1975) and Meltzer and Richard (1981) (henceforth RMR) have argued that individuals with higher wages will prefer lower income tax rates and less income redistribution. To the extent that redistributive policies affect vote choice, preferring lower income taxation entails supporting conservative political parties advocating low tax rates. Particularly since Ronald Reagan became President, the Republican party has consistently supported lower income taxation. Furthermore, as ideological and policy differences between the Democrat and Republican parties have grown (McCarty, Poole and Rosenthal 2006), the difference between the Democrat and Republican parties on this issue has become easy for voters to discern. Survey correlations consistently show that higher-income voters are more likely to identify as and vote for the Republican party (e.g. Erikson and Tedin 2007; Gelman et al. 2010).

Together, the human capital and RMR models imply that increased schooling should make voters more favorable toward Republican economic policies, and particularly their arguments for lower income tax rates, by increasing their income. Furthermore, given that voters can relatively accurately predict their future income, redistributive preferences are

also likely to be correlated with expected earnings (Alesina and La Ferrara 2005; Benabou and Ok 2001).

Critics of human capital theory have instead argued that education is a costly and unproductive signal of a worker’s productivity (Spence 1973). Accordingly, an increase in education should not affect the income distribution or a voter’s support for the economic policies proposed by different political parties.\(^5\) While such a signaling model, in conjunction with the RMR logic, could be still be consistent with a correlation between education and vote choices, there should remain no empirical relationship once selection problems are be resolved.

\subsection*{2.2 Political socialization}

While education bestows economically valuable knowledge and skills, which may in turn affect support for different political parties, education could also affect political attitudes through socialization. A large literature shows that education is correlated with socially and politically liberal attitudes. For example, more educated respondents display greater support for freedom of expression (Dee 2004; Gerber et al. 2010), ethnic diversity (e.g. Campbell et al. 1960; Schoon et al. 2010), and immigration (e.g. Hainmueller and Hiscox 2010). At least in recent decades, which the data in this study covers, these values have become strongly associated with the Democratic party (Levendusky 2009). Some studies find that these correlations also carry over to economic policy preferences (Gerber et al. 2010), but many do not (e.g. Weakliem 2002).

Two main mechanisms have been proposed to explain these associations: direct effects of curricula; and changes in social network composition. First, civic education and social science classes generally encourage political engagement and trust in the political system,

\footnote{This result requires that the increase in education is sufficiently small that high-ability workers are willing to pay for further education to separate themselves.}
and seek to instill liberal values of tolerance (Dee 2004; Hillygus 2005). In the U.S., such classes start in late high school and can intensify if students choose to concentrate in such areas at college. Furthermore, formative experiences appear to affect political beliefs decades later in life (Ghitza and Gelman 2014; Jennings, Stoker and Bowers 2009; Mullainathan and Washington 2009). However, recent experimental evidence finds that while enhanced high school civics classes increase political knowledge, this knowledge quickly decays and never affects support for civil liberties (Green et al. 2011). Given other empirical evidence has emphasized the importance of college education (see Galston 2001 for a review), civic education’s importance may be confined to higher education.

Second, education could more indirectly affect a voter’s political beliefs by altering the composition of their social network. One possibility is that more educated individuals sort into more prestigious, ethnically diverse and politically-connected networks (Green, Palmquist and Schickler 2002; Nie, Junn and Stehlik-Barry 1996). Such networks are often characterized by liberal social values and high levels of political interest, knowledge and discussion, and might thus increase support for the Democratic party.

While both mechanisms could induce socially liberal attitudes, the observed correlations may be spurious. Family background, values, cognitive abilities and life experiences are likely to be correlated with both education and socially liberal attitudes (Jencks et al. 1972; Kam and Palmer 2008). Excepting Green et al.’s (2011) randomized control trial, this concern is particularly salient because these studies do not exploit plausibly random variation in education, and often control for variables like income that are themselves functions of education.

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6There is debate over whether it is facts themselves (e.g. Sniderman, Tetlock and Brody 1993) or democratic awareness (e.g. Galston 2001) that drives this potential mechanism. For the purpose of this study, these mechanisms cannot be differentiated.
3 Empirical design

Estimating the partisan effects of education is not straight-forward. As noted above, there are many potentially confounding explanations for any correlation between schooling and support for political parties. Omitted variables like family background and individual ability are particularly problematic because they cannot easily be observed.

A further issue is the heterogeneity of schooling’s political effects. First, the effect of schooling is likely to be heterogeneous across types of individual (see Card 1999). Second, schooling’s effects may be non-linear since the effect of an additional grade depends upon the grade in question. As suggested above, different levels of schooling may differentially stimulate income-based and socialization-based mechanisms.

To address these concerns, I exploit cross-state variation in CSLs—primarily as an instrument for schooling, but also as a policy variable in its own right—in a difference-in-differences framework. Since I show that CSLs do no induce students to attend college, I specifically estimate the effect of late high school (as distinct from college education) for individuals that would otherwise have dropped out of high school. This section first describes CSLs in the U.S., before discussing identification, data and estimation.

3.1 State compulsory schooling laws

CSLs are legislated by U.S. state governments and define the minimum legal age at which a student must enter and may drop out of schooling. The first CSL was adopted in Massachusetts in 1852 and 41 states had adopted CSLs by 1910, principally to meet demand for educated workers and promote assimilation (Goldin and Katz 2008; Kotin and Aikman 1980). The partisan origins of state CSLs are considered below. While CSL enforcement is relatively weak, states can and do punish habitual truancy (see Oreopoulos 2009).

I follow Oreopoulos (2009) and focus simply on the minimum age at which a student is
permitted to drop out of school. Although more complex measures of compulsory schooling have been adopted (e.g. Acemoglu and Angrist 2000; Angrist and Krueger 1991), the minimum drop out age captures the relevant legislation for the empirical analyses conducted here.\footnote{Child labor laws are often used alongside CSLs, and were particularly important before 1940 (Goldin and Katz 2008). They are omitted here because they offer limited variation in the data analyzed here, and thus have a weak first stage. Their inclusion as additional instruments does not affect the results.} CSL data are from Oreopoulos (2009), and based on the National Center for Education Statistic’s \textit{Education Digest}.

Figure 1 plots changes in drop out ages across the 48 contiguous states and Washington, DC since 1914. Unsurprisingly, there has been a general upward trend in minimum leaving ages. However, there remain numerous instances of reversal as in Maine, Mississippi, and Oregon. Given that high school drop out rates remain non-trivial—consistently registered at around 20\% for all students since the 1970s, with considerably higher rates among ethnic minorities (Murnane 2013)—many states are actively seeking to raise the legal drop out age. Since 1914, 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 on schooling outcomes (see also Lochner and Moretti 2004). Using $1(CSL = 18)$ as an additional instrument, or using indicators for each minimum leaving age, provide similar results.}

Pioneered by Angrist and Krueger (1991), CSLs have since been used widely as instruments to estimate the effects of schooling on various outcomes in the U.S. (see e.g. Acemoglu and Angrist 2000; Dee 2004; Lleras-Muney 2002, 2005; Lochner and Moretti 2004; Milligan, Moretti and Oreopoulos 2004; Oreopoulos 2006). However, no study has identified how education affects support for political parties.
Figure 1: Variation in U.S. state CSLs, 1920-2004

Source: Oreopoulos (2009).
3.2 Identification strategy

To identify the effects of schooling on which party voters identify with and vote for later in life, I exploit changes in CSLs as a source of plausibly exogenous variation. While the reduced form identifies the political effects of CSLs in their own right, the IV estimates identify the effect of an additional year of schooling for individuals that only remained in school because of their state’s CSL.

3.2.1 Reduced form

To identify the political effects of CSLs, I employ a difference-in-differences (DD) design. Specifically, I leverage cohorts in states that did not change their CSLs as controls to separate trends in political support from the impact of CSLs on cohorts in states where CSLs changed. This entails estimating equations of the form:

\[ Y_{ict} = \delta_1 1(CSL_{cs} = 16) + \delta_2 1(CSL_{cs} \geq 17) + \alpha_c + \theta_s + \eta_t + W_{it}\gamma + \varepsilon_{icst}, \]

where \( Y_{ict} \) is the binary observed outcome (Republican identifier/voter) at survey period \( t \) for an individual \( i \) from cohort \( c \) in state \( s \). \( \alpha_c \), \( \theta_s \), and \( \eta_t \) are respectively birth-year (cohort), state grew up in, and survey-year fixed effects. Pre-treatment individual characteristics \( W_{it} \)—male, race indicators and quartic age polynomial terms—are included to enhance estimation efficiency.\(^9\) State-specific cohort trends are included as a robustness check. Standard errors are clustered by state-cohort.

This estimation strategy thus compares changes in support for the Republican party across cohorts within states which changed their CSLs to changes across cohorts within states which did not, while \( \eta_t \) captures common shocks moving all voters towards one party in a given survey year. The key identifying assumption is that in the absence of changing

\(^9\)Results are essentially identical when such variables are excluded.
CSLs, individuals from treated states would experience parallel trends in Republican support to individuals from control states. There are two main types of challenge to this assumption: individual selection into CSL regimes; and the occurrence of CSL reforms reflecting unobserved trends which also affect Republican support.

A key selection concern is that certain types of CSL-compliers may move more when the reform occurs. However, among poor women of childbearing age—whose children are more likely to be CSL-compliers—cross-state migration is especially low (Molloy, Smith and Wozniak 2011), and is concentrated well before their children typically reach high school age (Gelbach 2004). Furthermore, the principal reasons to move across states—for college, marriage, family reasons, and natural disaster (Molloy, Smith and Wozniak 2011)—are unlikely to be linked to CSL changes. Moreover, it is likely that parents willing to move to improve their children’s education would also be able to persuade their children to remain in school without CSLs. As a robustness check, I use a bounding exercise below to show that the selective migration required to nullify the results is very large.

Since legislation is not randomly enacted, the greater concern is the adoption context. Perhaps the most plausible violation of the exogeneity of CSLs with respect to political outcomes is if CSL reforms are correlated with state-level political changes. For example, state legislatures dominated by Republicans may disproportionately raise the minimum leaving age. I investigate this concern by testing whether CSLs are correlated with partisan political forces in state $s$ in year $t$ by estimating the following DD equation:

$$
CSL_{st} = P_{st} \lambda + \theta_s + \omega_t + \xi_{st},
$$

where $P_{st}$ is a vector of state-level political variables, and $\theta_s$ and $\omega_t$ are respectively state and year fixed effects.

The results in Table 1 provide no evidence for the political endogeneity concern. Column
Table 1: Effect of state political control on state CSLs

<table>
<thead>
<tr>
<th></th>
<th>(1) OLS</th>
<th>(2) OLS</th>
<th>(3) OLS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Republican House majority</td>
<td>0.016</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.061)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Republican Senate majority</td>
<td>-0.024</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.096)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unified Republican houses</td>
<td>-0.009</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.078)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Republican seat share House</td>
<td>0.110</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.271)</td>
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<tr>
<td>Republican seat share Senate</td>
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</tr>
<tr>
<td></td>
<td>(0.266)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Republican Governor</td>
<td>0.095</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.058)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>3,890</td>
<td>3,890</td>
<td>3,725</td>
</tr>
</tbody>
</table>

Notes: Outcome is the minimum drop out age in a given state. All specifications include state and year fixed effects. Standard errors clustered by state in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$ *** denotes $p < 0.01$. 
(1) shows no statistical association between the minimum dropout age and indicators for whether the Republicans controlled the upper, lower or both state houses. Using the Republican seat shares instead, column (2) again finds that political support is uncorrelated with changes in CSLs. Finally, column (3) finds no significant correlation with Republican state Governors. The lack of such correlations suggest that CSL reforms were unlikely to have been implemented strategically, or have been correlated with waves in local political sentiment.\(^\text{10}\)

Although political trends are uncorrelated with changes in CSLs, reforms could still reflect socioeconomic changes. However, Lochner and Moretti (2004) show that state high school graduation trends are uncorrelated with CSL changes, while Lleras-Muney (2002) uses placebo tests to demonstrate that CSLs cannot explain prior enrollment rates. More specific concerns are addressed below by controlling for state-level political and economic conditions when the respondent attended school. Finally, I include state-specific cohort trends as a general check against CSLs reflecting long-run processes that may differ across states, such as changes in school quality (Stephens Jr. and Yang 2014), and also affect political behavior.

### 3.2.2 Instrumental variables

In order to estimate the effect of schooling on identifying as and voting Republican, I build upon the reduced form DD specification by using CSLs to instrument for schooling. The structural equation is:

\[
Y_{icst} = \beta S_i + \alpha_c + \theta_s + \eta_t + W_i \gamma + \epsilon_{icst},
\]

\(^{10}\)Unreported checks using the three leaving age indicators and lagging the independent variables to capture delayed implementation similarly show no statistically significant associations. The null results are also robust to including state-specific time trends.
where $S_i$ captures the number of years of schooling received by individual $i$. I instrument for schooling by estimating the following first stage:

$$S_i = \pi_1 1(CSL_{cs} = 16) + \pi_2 1(CSL_{cs} \geq 17) + \alpha_c + \theta_s + \eta_t + W_i \gamma + \nu_{ict}. \tag{4}$$

Using the predicted values from equation (4), I estimate equation (3) with 2SLS to yield the local average causal response for compliers (Abadie 2003; Angrist and Imbens 1995).\footnote{This estimand weights the reduced form effects at each level of schooling by the proportion of people induced by the respective instruments to achieve that level of schooling.}

In addition to the parallel trends assumption, identification further requires a strong first stage, that the instruments satisfy monotonicity, and an exclusion restriction requiring that CSLs only affect political outcomes through increased schooling. Confirming the first stage results from previous studies, the results below show that increasing the drop out age significantly increases the level of schooling obtained. Furthermore, it is very unlikely that a higher leaving age would cause an individual to choose less schooling. The Online Appendix supports such monotonicity by showing that the cumulative distribution of years of schooling for higher CSLs lies strictly to the right of the distribution for lower CSLs. Given that education is temporally proximate to the CSL binding, and thus most downstream behavior is a function of a respondent’s level of schooling, there is limited scope for CSLs to violate the exclusion restriction. Nevertheless, I consider potential violations of this assumption in detail below.

### 3.3 Data

I principally use data from the National Annenberg Election Survey (NAES) and the American Community Survey (ACS). Using just the NAES is sufficient to estimate the reduced form relationships enumerated above. However, because years of schooling is not measured
in the NAES, and I demonstrate below that using an indicator for completing high school can substantially upwardly bias IV estimates, I use schooling data from the ACS to estimate the first stage. The sample moments from the ACS are matched to the NAES, and I use two-sample methods to compute the IV estimates.

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 over the period of each campaign, and together these yield a maximum pooled sample of 176,796 respondents.\textsuperscript{12} In addition to its large sample size, a key advantage of the NAES over other widely used political surveys is the wide range of questions eliciting policy preferences.\textsuperscript{13} The diverse range of political outcomes is essential for assessing the mechanisms underpinning education's political effects. To demonstrate the robustness of the results, I also examine the American National Election Survey (ANES); this serves as an important out-of-sample robustness check because it uses different survey protocols and covers all Congressional elections since 1952.

The ACS, which has conducted interviews with random U.S. citizens at monthly intervals since 2000, supplements the NAES's political questions by measuring the number of years of schooling. To match the years when NAES respondents were surveyed, I use ACS data from the publicly-available microdata from the 2000, 2001, 2003, 2004, 2007 and 2008 samples (Ruggles et al. 2010).\textsuperscript{14}

I now briefly describe how the main variables are measured. Summary statistics are

\textsuperscript{12}Interviews were conducted for around a year over the following months: 12/1999-1/2001, 10/2003-11/2004, 12/2007-11/2008. No survey was conducted in 2012.

\textsuperscript{13}The General Social Survey, which does collect the number of years of schooling, only registers the location of the respondents Census division at age 16 (there are only 9 divisions). Furthermore, questions regarding the political mechanisms are asked irregularly or not at all.

\textsuperscript{14}Although the samples collected for the earlier years are smaller because the ACS has sampled one in every hundred citizens since 2005, I ensure that the proportion of respondents from each year exactly matches the NAES distribution.
provided in Table 10. The Online Appendix provides detailed variable definitions.

3.3.1 Political outcomes

I use three main measures of support for the Republican party in the NAES. The first, which does not depend on voter turnout, is partisan self-identification. In the sample, 31% of respondents identify as Republicans, while 35% identify as Democrats; the residual are independents or “don’t know”\(^\text{15}\). Throughout, I use an indicator to capture Republican partisanship. Secondly, I measure vote intention at the forthcoming Presidential election, coding an indicator for intending to vote for the Republican Presidential candidate (either George W. Bush or John McCain). Finally, I code an indicator for whether a respondent voted Republican at the last Presidential election (for Bob Dole or George W. Bush)\(^\text{16}\). In the sample, the Republican candidates received 47% of intended Presidential votes while 45% of respondents actually voted for the Republican Presidential candidate at the previous election\(^\text{17}\).

3.3.2 Minimum drop out age

Survey respondents are mapped to CSLs based on their year of birth and the state in which they resided when the minimum drop out age binds. Birth year cohort is inferred from a respondent’s age when surveyed by the NAES and given as year of birth in the ACS\(^\text{18}\).

\(^{15}\)Considering only strong partisans and removing “don’t know” responses provides similar results.

\(^{16}\)To ensure that the results are not driven by changes in turnout, the denominator includes voters that did not turn out. The results are mirrored if Democrat indicators are used instead.

\(^{17}\)This corresponds to 53% of voters that turned out voting for the Republican candidate. The Republican Presidential candidate in the 1996, 2000, 2004 and 2008 elections ultimately received 40.7%, 47.9%, 50.7% and 45.7% of the votes cast. Weighting by the NAES sample sizes, the actual vote share corresponding to the intended vote measure is 48.7%, while 47.9% of voters voted Republican in the previous election.

\(^{18}\)This approach is common and yields similar first stage results to studies using month of birth (Acemoglu and Angrist 2000).
### Table 2: Summary statistics: NAES and ACS samples

<table>
<thead>
<tr>
<th></th>
<th>Obs.</th>
<th>Mean</th>
<th>Std. dev.</th>
<th>Min.</th>
<th>Max.</th>
<th>Obs.</th>
<th>Mean</th>
<th>Std. dev.</th>
<th>Min.</th>
<th>Max.</th>
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<tbody>
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<td><strong>Dependent variables</strong></td>
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<td>Republican partisan</td>
<td>176,796</td>
<td>0.30</td>
<td>0.46</td>
<td>0</td>
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<td>Intend to vote Republican for President</td>
<td>144,742</td>
<td>0.46</td>
<td>0.50</td>
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</tr>
<tr>
<td>Voted Republican for President at last election</td>
<td>125,196</td>
<td>0.45</td>
<td>0.50</td>
<td>0</td>
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<td><strong>Education (endogenous variables)</strong></td>
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<tr>
<td>Schooling</td>
<td>440,892</td>
<td>11.64</td>
<td>1.33</td>
<td>0</td>
<td>12</td>
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<td></td>
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<tr>
<td>Beyond 12th grade</td>
<td>440,892</td>
<td>0.52</td>
<td>0.50</td>
<td>0</td>
<td>1</td>
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<td><strong>Excluded instruments</strong></td>
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<tr>
<td>CSL=16</td>
<td>177,653</td>
<td>0.72</td>
<td>0.45</td>
<td>0</td>
<td>1</td>
<td>440,892</td>
<td>0.73</td>
<td>0.44</td>
<td>0</td>
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<td>CSL≥17</td>
<td>177,653</td>
<td>0.26</td>
<td>0.44</td>
<td>0</td>
<td>1</td>
<td>440,892</td>
<td>0.25</td>
<td>0.43</td>
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<td>1</td>
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<td><strong>Pre-treatment control variables</strong></td>
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<tr>
<td>Age</td>
<td>177,653</td>
<td>49.28</td>
<td>16.67</td>
<td>18</td>
<td>97</td>
<td>440,892</td>
<td>49.34</td>
<td>16.49</td>
<td>18</td>
<td>95</td>
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<td>Male</td>
<td>177,653</td>
<td>0.44</td>
<td>0.50</td>
<td>0</td>
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<td>440,892</td>
<td>0.44</td>
<td>0.50</td>
<td>0</td>
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<td>White</td>
<td>177,653</td>
<td>0.87</td>
<td>0.34</td>
<td>0</td>
<td>1</td>
<td>440,892</td>
<td>0.87</td>
<td>0.34</td>
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<td>Black</td>
<td>177,653</td>
<td>0.08</td>
<td>0.28</td>
<td>0</td>
<td>1</td>
<td>440,892</td>
<td>0.08</td>
<td>0.27</td>
<td>0</td>
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<td>Asian</td>
<td>177,653</td>
<td>0.01</td>
<td>0.08</td>
<td>0</td>
<td>1</td>
<td>440,892</td>
<td>0.01</td>
<td>0.08</td>
<td>0</td>
<td>1</td>
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<td><strong>Mechanisms</strong></td>
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<td>Reduce tax</td>
<td>136,870</td>
<td>0.35</td>
<td>0.48</td>
<td>0</td>
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<td>Ban abortion</td>
<td>131,690</td>
<td>0.21</td>
<td>0.41</td>
<td>0</td>
<td>1</td>
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<tr>
<td>Low gun controls</td>
<td>81,633</td>
<td>0.38</td>
<td>0.49</td>
<td>0</td>
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<tr>
<td>Low health spending</td>
<td>69,550</td>
<td>0.31</td>
<td>0.46</td>
<td>0</td>
<td>1</td>
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<tr>
<td>high military spending</td>
<td>78,741</td>
<td>0.48</td>
<td>0.50</td>
<td>0</td>
<td>1</td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Republican non-economic issues</td>
<td>163,365</td>
<td>0.00</td>
<td>1.00</td>
<td>-1.15</td>
<td>2.42</td>
<td></td>
<td></td>
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<tr>
<td>Political interest scale</td>
<td>177,624</td>
<td>0.00</td>
<td>1.00</td>
<td>-2.23</td>
<td>2.33</td>
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<tr>
<td>Political knowledge scale</td>
<td>123,436</td>
<td>0.00</td>
<td>1.00</td>
<td>-1.19</td>
<td>1.36</td>
<td></td>
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<tr>
<td>Discuss politics</td>
<td>168,961</td>
<td>0.00</td>
<td>1.00</td>
<td>-1.18</td>
<td>1.56</td>
<td></td>
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</tr>
<tr>
<td>Trust federal government</td>
<td>54,631</td>
<td>0.25</td>
<td>0.43</td>
<td>0</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Protect environment more</td>
<td>126,522</td>
<td>0.14</td>
<td>0.35</td>
<td>0</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ban gay marriage</td>
<td>72,335</td>
<td>0.32</td>
<td>0.47</td>
<td>0</td>
<td>1</td>
<td></td>
<td></td>
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</tr>
</tbody>
</table>
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. Since state of residence as a teenager is not available, this is approximated by current state of residence in the NAES and state of birth in the ACS.\footnote{An almost identical first stage is obtained if ACS respondents are instead assigned CSLs by current state of residence.}

This approximation could induce bias if CSL-compliers supporting particular political parties systematically migrate to states with different drop out ages. However, this is not a major concern. First, relatively few respondents move across states: although the NAES does not ask about cross-state migration, 61% of ACS respondents live in their state of birth and 72% of ANES respondents live in the state they lived in at age 14. Second, estimates using the ANES data—which measures state of residence at age 14—show that CSL-compliers are not significantly more likely to migrate.\footnote{These results were estimated using equations (1) and (3) above. The (statistically insignificant) IV point estimate is small, indicating that CSL-compliers are only 3.3 percentage points more likely to still reside in the state.} Third, the robustness checks below demonstrate that the ANES dataset yields similar estimates.

### 3.3.3 Years of schooling

To translate the reduced form effect into IV estimates for an additional year of schooling, I wish to measure the number of grades of completed schooling. However, the NAES only assembles nine educational qualification categories. While previous studies have used similar categorical measures to measure schooling using an indicator for completing high school (e.g. Lochner and Moretti 2004; Milligan, Moretti and Oreopoulos 2004), this can significantly upwardly bias the magnitude of IV estimates. Intuitively, this is because coarsening years of schooling causes the first stage to only capture the effect of the instrument on completing high school, and thus the exclusion restriction is violated if any other level of schooling (induced by the instrument) affects identifying as or voting Republican.
To illustrate this bias formally, consider the simple case of a single binary instrument $Z_i \in \{0, 1\}$. Denote the potential outcomes of schooling as $S_{iz} \equiv S(Z_i = z|X_i)$, which conditions on the assignment of the instrument $Z_i = z$ and a set of covariates $X_i$ (e.g. those required for the DD strategy employed here). Similarly, provided $Z_i$ does not affect $Y_i$ except through $S_i$, the potential outcomes of $Y_{ik} \equiv Y(S_i = k|X_i)$ correspond to $i$’s treatment assignment $S_i = k$. Let $HS_i \equiv 1(S_i \geq 12|X_i)$ indicate that individual $i$ completes 12th grade. Assuming that monotonicity holds, Angrist and Imbens (1995) show that the IV estimator for the effect of completing high school can be written as the weighted sum of the local average treatment effects of moving from $k - 1$ to $k$ years of schooling, denoted by $\beta_k \equiv \mathbb{E}[Y_{ik} - Y_{ik-1}|S_i \geq k > S_i 0, X_i]$: $\beta_{IV}^{HS} \equiv \frac{\mathbb{E}[Y_i|Z_i = 1, X_i] - \mathbb{E}[Y_i|Z_i = 0, X_i]}{\mathbb{E}[HS_i|Z_i = 1, X_i] - \mathbb{E}[HS_i|Z_i = 0, X_i]} = \frac{\sum_{k=1}^{K} p_{ik}\beta_k}{p_{iHS}},$ (5) where $p_{ik} \equiv \Pr(S_i \geq k > S_i 0|X_i)$ denotes the probability that $i$ only achieves $k$ years of schooling because they received the instrument $Z_i = 1$, while $p_{iHS}$ is the first stage for completing high school.

The researcher seeking to identify the effect of completing high school is typically interested in estimating the effect of just completing high school: $\beta_{HS} \equiv \mathbb{E}[Y_{iHS} - Y_{iHS-1}|S_i \geq HS > S_i 0, X_i].$\(^{21}\) Therefore, the inconsistency of the IV estimate is: $\beta_{IV}^{HS} - \beta_{HS} = \frac{\sum_{k \neq HS} p_{ik}\beta_k}{p_{iHS}}.$ (6)

The coarsened IV estimator is only consistent if the instrument does not affect any level of schooling other than completing high school, the only effect of schooling is from completing

\(^{21}\)This counterfactual definition is often left implicit. Although other counterfactuals including lower levels of schooling could be imagined, they cannot be interpreted as the effect of completing high school unless they can be purged of $\beta_k$ for all $k \neq HS$.  

22
high school, or the weighted effects cancel out. If the effect of other levels of schooling is of the same direction as completing high school, i.e. $\text{sign}(\beta_{HS}) = \text{sign}(\beta_k)$, then the IV estimate is upwardly biased (in magnitude). The bias is increasing in both the effect at each other education intensity ($\beta_k$) and the relative magnitude of the effect of the instrument on levels of schooling other than completing high school ($p_{ik}/p_{iHS}$).

Using an indicator for completing high school would yield limited bias if the true causal response function is only positive for completing high school. However, if the benefits of education are not solely a function of graduating high school, but rather monotonically increasing in the number of years of schooling, the upward bias could be substantial. This could explain why Dee (2004) finds the effect of an additional year of secondary education on political participation to be orders of magnitude lower than Milligan, Moretti and Oreopoulos’s (2004) estimate of the effect of completing high school, or why Sondheimer and Green (2010) find that completing high school increases the probability of turning out by around 50 percentage points. In the Online Appendix, I provide the IV estimates when instrumenting for completing high school, which suggest that estimates for Republican partisanship and voting also suffer from considerable bias.

Fortunately, using a interval measure of schooling, like the number of completed grades, allows for consistent estimation. As Angrist and Imbens (1995) show, replacing the denominator of equation (7) with the first stage for the number of years of schooling returns,

$$\beta^{IV}_S \equiv \frac{\mathbb{E}[Y_i|Z_i = 1, X_i] - \mathbb{E}[Y_i|Z_i = 0, X_i]}{\mathbb{E}[S_i|Z_i = 1, X_i] - \mathbb{E}[S_i|Z_i = 0, X_i]} = \frac{\sum_{k=1}^{K} p_{ik}\beta_k}{\sum_{k=1}^{K} p_{ik}}$$

(7)

yields a consistent estimate of the local average causal response. This estimand is weighted by the relative contributions of the first stage at each additional year of schooling.\textsuperscript{22}

Since years of schooling is not measured in the NAES (or ANES), I combine the NAES

\textsuperscript{22}With multiple instruments, $\beta^{IV}_S$ is-as usual—a weighted combination of the separate IV estimates.
data with data from the ACS using two-sample IV methods. The ACS provides fine grained
data on each citizen’s education, and permits schooling to be measured by the number of
grades completed. To avoid complications with coding different types of post-high school
education, the variable is top-coded at completing 12th grade, and thus ranges from 0 to
12. This coding is inconsequential because, as shown below, CSLs do not affect college
attendance or attaining grades of schooling greater than 12.

3.4 Two-sample estimation

I use two-sample 2SLS (TS2SLS) to estimate equation (3). This entails computing the first
stage in equation (4) using the ACS dataset and the reduced form in equation (1) using
the NAES dataset, before efficiently combining the two as a consistent two-step estimator
(Inoue and Solon 2010).23 Exchanging sample moments across datasets in this manner was
was first proposed by Angrist and Krueger (1992), and has since been employed to answer a
variety of empirical questions (see Angrist and Pischke 2008). The cluster-robust covariance
matrix for TS2SLS is derived analytically in the Online Appendix.

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 voting age citizens (once ineligible voters from the NAES sample and
those aged below 18 and born outside the U.S. are removed from the ACS sample), I then
stratified by year of birth, male, race and survey year to randomly draw ACS observations to
replicate the distribution of NAES respondents over these pre-treatment covariates.24 Look-

23TS2SLS specifications were estimated in R; the program I wrote is available in the replication code. Note that an OLS benchmark for years of schooling cannot be estimated because it is not measured in the NAES.

24More specifically, 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), were born outside the U.S., or became naturalized citizens. I then stratified by cohort-gender to draw a random sample of 2,000,000 ACS respondents to ensure that the subsample has the same by-cohort-by-gender distribution as the NAES. I then re-
ing across Table 10, the summary statistics demonstrate that this approach almost exactly replicates the first two moments across the samples.

4 Effects of high school on Republican support

This section identifies the downstream political effects of high school education. The main finding is that both CSLs and particularly an additional year of high school substantially increase the probability that an individual will identify with, or vote for, the Republican party later in life.

4.1 Reduced form estimates

I first estimate the effect of increasing the high school dropout age on the propensity of an individual to identify as or vote Republican. Columns (1)-(3) in panel A of Table 3 report these reduced form DD estimates, where a positive coefficients signifies an increase in support for the Republican party. Respectively, columns (1), (2) and (3) report the effects on identifying as a Republican partisan, intending to vote for the Republican Presidential candidate, and voting for the Republican Presidential candidate at the previous Presidential election.

The results show that, compared to cohorts in states requiring students to remain in school until at most age 15, raising the dropout age to 16 significantly increased the probability of affected cohorts identifying as Republican and intending to vote for the Republican Presidential candidate by 3 percentage points. The effect on actually voting for the Republican Presidential candidate by 3 percentage points. The effect on actually voting for the Republican...
Table 3: The effect of CSLs and schooling on identifying as and voting Republican

<table>
<thead>
<tr>
<th>Panel A: Reduced form (OLS)</th>
<th>(1) Partisan</th>
<th>(2) Intend</th>
<th>(3) Vote</th>
<th>(4) Partisan</th>
<th>(5) Intend</th>
<th>(6) Vote</th>
</tr>
</thead>
<tbody>
<tr>
<td>CSL=16</td>
<td>0.036***</td>
<td>0.029***</td>
<td>0.013</td>
<td>0.013</td>
<td>0.035***</td>
<td>0.026*</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.011)</td>
<td>(0.013)</td>
<td>(0.013)</td>
<td>(0.013)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>CSL ≥ 17</td>
<td>0.051***</td>
<td>0.035***</td>
<td>0.023*</td>
<td>0.030**</td>
<td>0.050***</td>
<td>0.043***</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.012)</td>
<td>(0.013)</td>
<td>(0.012)</td>
<td>(0.014)</td>
<td>(0.016)</td>
</tr>
</tbody>
</table>

Linear state-specific cohort trends
Observations
Test: CSL=16 = CSL ≥ 17 (p value)

<table>
<thead>
<tr>
<th>Panel B: First stage (OLS)</th>
<th>(7) Schooling</th>
<th>(8) Beyond 12th grade</th>
<th>(9) Schooling</th>
<th>(10) Beyond 12th grade</th>
</tr>
</thead>
<tbody>
<tr>
<td>CSL=16</td>
<td>0.333***</td>
<td>-0.006</td>
<td>0.188***</td>
<td>-0.010</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.006)</td>
<td>(0.036)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>CSL ≥ 17</td>
<td>0.434***</td>
<td>-0.004</td>
<td>0.239***</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.048)</td>
<td>(0.007)</td>
<td>(0.038)</td>
<td>(0.008)</td>
</tr>
</tbody>
</table>

Linear state-specific cohort trends
Observations
First stage F statistic

<table>
<thead>
<tr>
<th>Panel C: IV (TS2SLS)</th>
<th>(11) Partisan</th>
<th>(12) Intend</th>
<th>(13) Vote</th>
<th>(14) Partisan</th>
<th>(15) Intend</th>
<th>(16) Vote</th>
</tr>
</thead>
<tbody>
<tr>
<td>Schooling</td>
<td>0.120***</td>
<td>0.076***</td>
<td>0.059*</td>
<td>0.134**</td>
<td>0.215***</td>
<td>0.191***</td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
<td>(0.028)</td>
<td>(0.031)</td>
<td>(0.056)</td>
<td>(0.069)</td>
<td>(0.071)</td>
</tr>
</tbody>
</table>

Linear state-specific cohort trends
Reduced form (NAES) observations
First stage (ACS) observations
First stage F statistic

Notes: Outcomes are: Republican partisan (“partisan”), intending to vote Republican for President (“intend”), voting Republican in previous presidential election (“vote”), completed years of schooling (“school”), and an indicator for attending any education beyond 12th grade (“Beyond 12th grade”). All specifications include male, white, black and Asian dummies, quartic (demeaned) age polynomials, and state grew up, cohort and survey fixed-effects. The omitted CSL category is CSL ≤ 15. First stage observations decline for vote intention because the ACS sample is reduced to match the variables in the NAES sample. Standard errors clustered by state-cohort in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.
can candidate is also relatively large but is not statistically significant. Furthermore, keeping students in school until at least 17 adds nearly an additional percentage point, including inducing a significant increase in reported Republican votes. The tests at the foot of panel A indicate that the additional impact of raising the dropout age from 16 to at least 17 is itself also generally statistically significant. For each outcome, the increases represent between 5% and 10% increases relative to their sample means. The slightly larger effect at 16 most likely reflects comparison to an omitted category containing respondents who comply with CSLs mandating students only remain in school until between 12 and 15.25

Given that CSLs did not affect every student in each cohort, the political effects of implementing policies to increase the dropout age are remarkably large. At least in politically competitive areas, the changes are sufficiently large that they may have persistently altered state-level electoral outcomes, potentially including district as well as electoral college outcomes, as the number of affected cohorts accumulates.

### 4.2 Instrumental variable estimates

Turning to the IV estimates, I estimate the effect of completing an additional grade of high school for individuals that only remained at school because of the dropout age affecting their state-cohort. In Table 3, the first stage estimates from the ACS sample are presented in panel B, while the IV estimates are presented in panel C.

The first stage estimates in column (7) show that CSLs have substantially increased levels of schooling. Relative to state-cohorts facing a dropout age below 15, raising the leaving age to 16 increases the average number of years of schooling by 0.33 years. Further raising the dropout age to at least 17 keeps students in school for an additional 0.10 years. These positive effects are similar in magnitude to previous estimates pertaining to cohorts generally born somewhat earlier in the twentieth century (Acemoglu and Angrist 2000; Dee

25Most students in this group face a leaving age of 14.
Furthermore, the large $F$ statistic of 50.6 demonstrates that the instruments explain a significant portion of variation in schooling, and surpass the standards required to avoid weak instrument biases.

To understand which levels of schooling the IV estimates pertain to, it is important to examine which levels of education are affected by the instruments. While column (7) demonstrated an increase in the number of years of education up to high school level, it is possible that CSLs also affect post-secondary education. However, column (8) shows that, on average, increasing the dropout age does not increase the probability that an individual completes schooling beyond 12th grade (including any amount of college). Consequently, the average complier was only induced by CSL reforms to complete additional years of late high school.

Reinforcing the reduced form results, the TS2SLS estimates in panel C show that late high school has a large pro-Republican effect on CSL-compliers. The estimates in columns (11)-(13) show that an additional grade of high school increases the probability that an individual identifies as a Republican or votes for the Republican Presidential candidate by around 10 percentage points. Particularly in the case of voter partisanship, where only 30% of the population identify as Republicans, this effect is substantial: an additional year of late high school increases the probability of identifying as a Republican by 12 percentage points. However, the 6-8 percentage point increases in Republican voting are also notable, and combined with the reduced form estimates suggest that high school has major downstream conservative effects on voters. These large effects likely reflect CSL-compliers coming from relatively disadvantaged backgrounds where education is uncommon, and thus the marginal effects of schooling on life opportunities may be greatest. After demonstrating the robustness of these findings, the next section investigates the mechanisms underlying this effect.
4.3 Robustness checks

These findings rest on two key identifying assumptions. The parallel trends assumption is required for both the reduced form and IV estimates, while an exclusion restriction is required for consistent IV estimation. I now consider potential violations beyond those addressed above, and demonstrate the robustness of the findings.

Although state CSL reforms are uncorrelated with individual respondent characteristics within a state, they could correlate with underlying trends in state-level characteristics. However, supporting the parallel trends assumption, the final three columns in each panel of Table 3 show that the reduced form, first stage and IV estimates are all robust to including linear state-specific cohort trends. In fact, the effects are often larger using this more robust specification. The results are thus robust to a stronger test of the central concern recently raised by Stephens Jr. and Yang (2014).

Nevertheless, state-specific cohort trends may be insufficient to capture more particular concerns. Accordingly, I also control for two potential confounding covariates that vary by state and across cohorts. First, the Presidential vote share by state at the most recent election at age 16 and 18 is included to control for the national political context when respondents were in high school. Second, statewide labor market opportunities are proxied for by state personal income per capita at ages 16 and 18. The results in Table 4 show that controlling for these state-level variables provides estimates of similar magnitude.

Given the temporal proximity of a binding dropout age to the decision to remain in school, the most plausible exclusion restriction violations reflect short-term or concurrent changes induced by CSL reforms. First, by simply keeping a student out of the labor force (rather than through the direct effects of an additional year of schooling), CSLs could affect early life choices like marriage and fertility. If, for example, schooling reduced early marriage

\[26\] At the cost of sample size, similar results are obtained when controlling for state House and state Senate Republican seat shares.
Table 4: TS2SLS estimates, controlling for state-level covariates

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: State controls at age 16</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Partisan Intend Vote</td>
<td>Partisan Intend Vote</td>
<td>Partisan Intend Vote</td>
<td>Partisan Intend Vote</td>
<td>Partisan Intend Vote</td>
<td>Partisan Intend Vote</td>
<td>Partisan Intend Vote</td>
</tr>
<tr>
<td>Schooling</td>
<td>0.131***</td>
<td>0.083***</td>
<td>0.052</td>
<td>0.176**</td>
<td>0.220**</td>
<td>0.190**</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.029)</td>
<td>(0.037)</td>
<td>(0.074)</td>
<td>(0.086)</td>
<td>(0.089)</td>
</tr>
<tr>
<td>Linear state-specific cohort trends</td>
<td></td>
<td>✔</td>
<td>✔</td>
<td>✔</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reduced form (NAES) observations</td>
<td>171,202</td>
<td>140,365</td>
<td>120,914</td>
<td>171,202</td>
<td>140,365</td>
<td>120,914</td>
</tr>
<tr>
<td>First stage (ACS) observations</td>
<td>432,110</td>
<td>425,106</td>
<td>432,110</td>
<td>432,110</td>
<td>425,106</td>
<td>432,110</td>
</tr>
<tr>
<td>First stage $F$ statistic</td>
<td>47.3</td>
<td>46.7</td>
<td>47.3</td>
<td>17.7</td>
<td>17.1</td>
<td>17.7</td>
</tr>
<tr>
<td><strong>Panel B: State controls at age 18</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Partisan Intend Vote</td>
<td>Partisan Intend Vote</td>
<td>Partisan Intend Vote</td>
<td>Partisan Intend Vote</td>
<td>Partisan Intend Vote</td>
<td>Partisan Intend Vote</td>
<td>Partisan Intend Vote</td>
</tr>
<tr>
<td>Schooling</td>
<td>0.131***</td>
<td>0.083***</td>
<td>0.052</td>
<td>0.152**</td>
<td>0.231**</td>
<td>0.197**</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.029)</td>
<td>(0.037)</td>
<td>(0.073)</td>
<td>(0.090)</td>
<td>(0.093)</td>
</tr>
<tr>
<td>Linear state-specific cohort trends</td>
<td></td>
<td>✔</td>
<td>✔</td>
<td>✔</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reduced form (NAES) observations</td>
<td>171,146</td>
<td>140,235</td>
<td>120,247</td>
<td>171,146</td>
<td>140,235</td>
<td>120,247</td>
</tr>
<tr>
<td>First stage (ACS) observations</td>
<td>423,225</td>
<td>416,377</td>
<td>423,225</td>
<td>423,225</td>
<td>416,377</td>
<td>423,225</td>
</tr>
<tr>
<td>First stage $F$ statistic</td>
<td>53.0</td>
<td>51.6</td>
<td>53.0</td>
<td>14.1</td>
<td>13.6</td>
<td>14.1</td>
</tr>
</tbody>
</table>

Notes: All specifications include male, white, black and Asian dummies, quartic (demeaned) age polynomials, state personal income per capita at age 16 or 18, Republican Presidential vote share at age 16 or 18, and state grew up, cohort and survey fixed-effects. The omitted CSL category is CSL $\leq$ 15. Differences in sample size reflect data availability. Standard errors clustered by state-cohort in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 
and fertility, then students may be more likely to avoid interaction with the welfare state and ultimately support the Republican party. However, reduced form DD estimates in the Online Appendix show that CSLs do not significantly affect the likelihood that a respondent has ever been married or the age of either a respondent’s oldest or youngest child living at home. Second, raising (or lowering) the minimum leaving age could create cross-cohort spillovers causing older (younger) cohorts to behave more like treated cohorts. However, this would reduce between-cohort differences within states, and thus downwardly bias the estimates obtained here. Third, to address general exclusion restriction violations, I use Conley, Hansen and Rossi’s (2012) sensitivity analysis to calculate the exclusion restriction violation required to nullify the results. Their (most conservative) union of confidence intervals approach shows that, depending on the outcome variable, between one fifth and two thirds of the reduced form effect of the drop out age must operate through avenues other than schooling for the TS2SLS estimates reported here to become statistically insignificant.27

Although migration is unlikely to be driving the results (see above), the possibility cannot be excluded. To evaluate the extent of biased migration required to explain the observed reduced form effect of CSLs on Republican partisanship, I conduct a simple bounding exercise. Consider the most extreme case where voters migrate from low-CSL (baseline category) states to high-CSL states (CSL of 17 or higher). In the sample, the average proportion of Republican partisans in these states are, respectively, 31.4% and 34.4%; the sample-weighted average population in high-CSL states is 3.3 times larger. To account for the reduced form

---

27In practice, I replace $Y_{icst}$ with $Y_{icst} - \delta_1 I(CSL_{cs} = 16) - \delta_2 I(CSL_{cs} \geq 17)$ in equation (3). I then estimate this using TS2SLS, and examine the sensitivity of the results for different $\delta_1$ and $\delta_2$ parameter specifications. I restrict attention to violations such that the relationship between $\delta_1$ and $\delta_2$ reflects the ratio between the reduced form coefficients estimated in columns (1)-(3) of Table 3. For example, for Republican partisanship I consider $\delta_1 = \delta$ and $\delta_2 = (0.051/0.036)\delta$. The TS2SLS estimates become statistically insignificant only when $\delta$ reaches 0.022 and 0.008 for the partisanship and vote intention respectively. (The past vote outcome is not quite statistically significant at the 95% level until state-specific cohort trends are included.)
estimate (0.051 for Republican partisanship), if 50% of migrants from the low-CSL state are Republican partisans (far higher than the sample average), the net migration rate from low to high-CSL states must be 19% per cohort. Alternatively, if migrants to high-CSL states are twice as likely to be Republicans, a per cohort migration rate of 12% is required. In reality, net migration between such states is significantly lower in the sample: in the ACS data, respondents are slightly more likely to migrate to a lower-CSL than the state they were born in. Furthermore, I now show that the results are robust to using an alternative dataset where state of residence at age 14 is measured.

Finally, to ensure that the results do not simply reflect idiosyncratic features of the NAES sample, I also examine the ANES data. Combining Census extracts for the first stage with an ANES reduced form, the TS2SLS estimates in Panel A of Table 5 again show that schooling significantly increases Republican partisanship and self-reported Republican voting in prior House and Senate elections. Although the ANES has many fewer observations, it represents an informative check for two 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 10.8 grades). Second, since the ANES allows CSLs to be mapped to individuals by location at age 14, obtaining similar results suggests that cross-state migration did not bias the NAES estimates. To further address the migration concern, Panel B shows that these results are robust to focusing only on respondents who live in the same state where they resided at age 14.

28 Using the ACS data, I coded as 1 respondents who live in a higher-CSL state than that of their birth, and and a lower-CSL state as -1. Summing across the sample (including non-movers), net migration was 4.5 percentage points toward low-CSL states.

29 The Census sample was chosen to match the sample distribution of the ANES, and is used because ACS surveys do not extend back far enough to cover all ANES survey waves. The Online Appendix provides details regarding these samples.
## Table 5: The effect of schooling on identifying as and voting Republican—ANES dataset

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
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<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Partisan</td>
<td>Vote</td>
<td>House</td>
<td>Senate</td>
</tr>
<tr>
<td>Schooling</td>
<td>0.103***</td>
<td>0.050</td>
<td>0.163***</td>
<td>0.071*</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.035)</td>
<td>(0.033)</td>
<td>(0.038)</td>
</tr>
</tbody>
</table>

**Panel A: Full sample**

- Reduced form observations: 35,873, 14,712, 19,081, 13,339
- First-stage observations: 636,713
- First-stage $F$ statistic: 196.6

<table>
<thead>
<tr>
<th></th>
<th>(5)</th>
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<th>(8)</th>
</tr>
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<tbody>
<tr>
<td></td>
<td>Partisan</td>
<td>Vote</td>
<td>House</td>
<td>Senate</td>
</tr>
<tr>
<td>Schooling</td>
<td>0.135***</td>
<td>0.091**</td>
<td>0.160***</td>
<td>0.095**</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.042)</td>
<td>(0.040)</td>
<td>(0.043)</td>
</tr>
</tbody>
</table>

**Panel B: Non-migrants**

- Reduced form observations: 25,823, 10,505, 13,700, 9,504
- First-stage observations: 389,442
- First-stage $F$ statistic: 141.9

**Notes:** Outcomes are Republican partisanship ("partisan"), and voting Republican for President ("vote"), House and Senate. All specifications are estimated using TS2SLS, and include pre-treatments controls (male, white, black and Asian dummies, and quartic demeaned age polynomials), state grew up in, survey decade, and cohort fixed effects. Differences in sample size reflect data availability. Standard errors clustered by state-cohort in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$. 

33
5 Mechanisms

Having shown that additional high school increases support for the Republican party, I now explore the channels through which this substantial effect operates. In particular, I utilize the same IV methods to test whether schooling affects the mechanisms underpinning the income and socialization-based theories discussed above, and provide correlations and placebo tests to support the claim that, by increasing income, schooling affects economic policy preferences, which in turn increase the likelihood that such voters ultimately support the Republican party.\footnote{Establishing the effects of potential mediators is impossible without making extremely strong assumptions (Imai, Keele and Yamamoto 2010). However, examining a range of potential mediators in conjunction with placebo tests can support some mechanisms and eliminate others. For brevity, all mechanism results are for the most robust specifications containing state-specific cohort trends, although similar results are obtained when such trends are excluded.}

5.1 Increased income and opposition to taxation

I provide four different types of evidence consistent with the income-based RMR channel. First, although potentially common to some alternative explanations, evidence has consistently shown that education increases wages. As noted above, studies using CSLs as instruments show that each additional year of schooling increases wages at peak working age in the U.S. by around 10 percent (see Acemoglu and Angrist 2000; Oreopoulos 2006, 2009).

Second, although it is reasonable to expect voters to support the party that they expect will benefit them in the future (Alesina and La Ferrara 2005), if education is driving opposition to taxation by significantly increasing an individual’s income, then this effect should be most pronounced when the return to education is greatest at an individual’s earnings peak. To examine this implication, I compare the effects of CSLs on the 40% of respondents aged between 45 and 65 at the date of the survey to all other respondents. Based on the ACS
sample, wages are highest between these ages.\textsuperscript{31} The results in Table 6 show that the effect of the dropout age on support for the Republican party is substantially higher among 45-65 year olds: the interactions indicate that, at an individual’s earnings peak, the effect of both CSL levels on identifying as and voting Republican are around 15 percentage points higher, while intention to vote Republican is also higher but not significantly so. This suggests that realizing the returns to education is a key component of education’s political effect, although the results still point to a non-negligible effect outside the peak earnings period. Moreover, the lack of differential effect on years of schooling in column (4) shows that this result does not simply reflect CSLs producing larger effects on education for a particular generation. Columns (5)-(8) show that these results are robust to simultaneously controlling for the interaction of the earnings peak indicator with all other covariates.

Third, I use survey attitudes to examine whether schooling increases opposition to taxation. The IV estimate in column (1) of Table 7 shows that educated respondents regard themselves as more conservative when asked to place themselves on a (standardized) 100-point liberal-to-conservative scale. An additional year of late high school equates to more than a quarter of a standard deviation increase in conservative preferences. Although economic policy preferences are an important element determining how voters place themselves on such scales (e.g. Conover and Feldman 1981), this evidence does not necessarily pinpoint economic policies as the source of the change. To better capture economic policy preferences, Table 7 shows that an additional grade of high school increases the (binary) belief that taxes should be reduced by 17 percentage points. Furthermore, this belief is strongly correlated with identifying with or voting for the Republican party.\textsuperscript{32} This is consistent with

\textsuperscript{31}I regressed (log) wage on dummies for age, controlling for state and cohort fixed effects. The coefficients for ages 45-65 systematically produced the largest coefficients (ignoring the smaller fraction of working respondents aged 80 or higher).

\textsuperscript{32}While this link from potential mediator to outcome cannot be well-identified, specifications akin to equation (1) showed that voters believing that taxes should be reduced are more than 10 percentage points more likely to be Republican partisans or vote for the Republican party.
### Table 6: Heterogeneous effects of CSLs on Republican support by earnings peak

<table>
<thead>
<tr>
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<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Partisan</td>
<td>Intend</td>
<td>Vote</td>
<td>Schooling</td>
<td>Partisan</td>
<td>Intend</td>
<td>Vote</td>
<td>Schooling</td>
</tr>
<tr>
<td>CSL=16</td>
<td>0.012</td>
<td>0.034***</td>
<td>0.018</td>
<td>0.216***</td>
<td>0.001</td>
<td>0.018</td>
<td>0.008</td>
<td>0.126***</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.013)</td>
<td>(0.015)</td>
<td>(0.037)</td>
<td>(0.012)</td>
<td>(0.014)</td>
<td>(0.017)</td>
<td>(0.039)</td>
</tr>
<tr>
<td>CSL=16 × Earnings peak</td>
<td>0.143***</td>
<td>0.059</td>
<td>0.169***</td>
<td>-0.118</td>
<td>0.183***</td>
<td>0.101</td>
<td>0.190***</td>
<td>-0.112</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
<td>(0.066)</td>
<td>(0.042)</td>
<td>(0.108)</td>
<td>(0.047)</td>
<td>(0.084)</td>
<td>(0.055)</td>
<td>(0.134)</td>
</tr>
<tr>
<td>CSL≥17</td>
<td>0.029**</td>
<td>0.048***</td>
<td>0.034**</td>
<td>0.238***</td>
<td>0.012</td>
<td>0.027*</td>
<td>0.015</td>
<td>0.152***</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.014)</td>
<td>(0.017)</td>
<td>(0.038)</td>
<td>(0.013)</td>
<td>(0.016)</td>
<td>(0.019)</td>
<td>(0.040)</td>
</tr>
<tr>
<td>CSL≥17 × Earnings peak</td>
<td>0.142***</td>
<td>0.063</td>
<td>0.168***</td>
<td>-0.141</td>
<td>0.189***</td>
<td>0.126</td>
<td>0.198***</td>
<td>-0.164</td>
</tr>
<tr>
<td></td>
<td>(0.035)</td>
<td>(0.066)</td>
<td>(0.043)</td>
<td>(0.108)</td>
<td>(0.049)</td>
<td>(0.086)</td>
<td>(0.057)</td>
<td>(0.138)</td>
</tr>
</tbody>
</table>

All earnings peak interactions | ✓ | ✓ | ✓ | ✓ |

Observations: 176,796 144,742 106,879 440,892 176,796 144,742 106,879 440,892

**Notes:** Earnings peak is an indicator for respondents aged between 45 and 65. For outcome definitions, see Table 3. All specifications include male, white, black and Asian dummies, quartic (demeaned) age polynomials, linear state-specific cohort trends, and state grew up, cohort and survey fixed-effects. The specifications in columns (5)-(8) interact all covariates with the earnings peak indicator. Differences in sample size reflect data availability. The omitted CSL category is CSL≤15. Standard errors clustered by state-cohort in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.
the argument that high school causes voters to become more fiscally conservative, and thus support the Republican party.

Fourth, I use placebo tests to suggest that economic policy preferences drive political choices. If voters adopt the policy positions of the political party or candidate they identify with (Lenz 2012; Zaller 1992), changes in economic policy preferences could instead reflect changes in partisanship. If voters simply reflect their party’s positions, they should also shift toward prominent Republican stances on other issues. Columns (3)-(6) of Table 7 provide no evidence for such changes: an additional grade of schooling does not significantly increase the likelihood that respondents oppose abortion, gun control, school spending, health spending, or cutting military spending. The effects of schooling on these issues are consistently smaller in magnitude than schooling’s effect on economic policy preferences, while the coefficients for military spending, gun controls and banning abortion point in the opposite direction to what the correlation with Republican support would suggest. Combining all the non-economic policy indicators as a scale, column (7) shows that schooling does not shift respondents toward Republican positions on non-economic policy issues. These results are in line with Gerber, Huber and Washington (2010), who find that experimentally inducing a shift in the party a voter identifies with does not affect their policy positions.

5.2 Political socialization

The socialization theories reviewed above argue that schooling cultivates political engagement and induces socially liberal values. Although such values are typically associated with supporting the Democratic Party, it remains possible that socialization mechanisms operate

Presidential candidate. Although the Republicans did not become the party of low taxes in the South until the 1970s, the survey responses examined here occur three decades later in a period where party positions are clear.

33 These issues are all strongly correlated with identifying as and voting Republican in the NAES data.
Table 7: The effect of schooling on potential income and socialization mechanisms

<table>
<thead>
<tr>
<th>(1) Conservative scale</th>
<th>(2) Reduce taxes†</th>
<th>(3) Ban abortion†</th>
<th>(4) Low gun controls†</th>
<th>(5) Low health spending†</th>
<th>(6) High military spending†</th>
<th>(7) Republican non-economic issues</th>
</tr>
</thead>
<tbody>
<tr>
<td>Schooling</td>
<td>0.260**</td>
<td>0.169***</td>
<td>-0.043</td>
<td>-0.093</td>
<td>0.086</td>
<td>-0.047</td>
</tr>
<tr>
<td></td>
<td>(0.112)</td>
<td>(0.065)</td>
<td>(0.057)</td>
<td>(0.087)</td>
<td>(0.093)</td>
<td>(0.095)</td>
</tr>
<tr>
<td>Linear state-specific cohort trends</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Reduced form (NAES) observations</td>
<td>172,400</td>
<td>136,830</td>
<td>131,662</td>
<td>81,022</td>
<td>69,513</td>
<td>78,703</td>
</tr>
<tr>
<td>First stage (ACS) observations</td>
<td>440,892</td>
<td>438,497</td>
<td>438,497</td>
<td>305,417</td>
<td>308,114</td>
<td>308,114</td>
</tr>
<tr>
<td>First stage $F$ statistic</td>
<td>22.7</td>
<td>22.7</td>
<td>22.7</td>
<td>11.2</td>
<td>12.3</td>
<td>12.3</td>
</tr>
</tbody>
</table>

| (8) Political knowledge scale | (9) Political interest scale | (10) Discuss politics | (11) Trust federal govt.† | (12) Protect enviro. more† | (13) Ban gay marriage† |
| Schooling                  | -0.102               | -0.060            | -0.132                | -0.003                   | -0.083                     | 0.171                         |
|                           | (0.125)              | (0.100)           | (0.106)               | (0.088)                  | (0.053)                    | (0.105)                       |
| Linear state-specific cohort trends | ✓           | ✓               | ✓                   | ✓                       | ✓                         | ✓                             |
| Reduced form observations  | 123,411            | 177,624           | 177,624               | 54,631                   | 126,522                    | 72,313                        |
| First stage (ACS) observations | 411,707       | 440,892           | 440,892               | 436,095                  | 310,509                    | 184,818                       |
| First stage $F$ statistic  | 23.1                | 22.7             | 22.7                 | 22.2                     | 12.7                      | 8.5                           |

Notes: All outcome variables are standardized, except binary outcomes denoted by †. See Online Appendix for detailed variable definitions. All specifications include male, white, black and Asian dummies, quartic (demeaned) age polynomials, state grew up, cohort and survey fixed-effects, and linear state-specific cohort trends. All specifications are estimated using TS2SLS. Differences in sample size reflect data availability. Standard errors clustered by state-cohort in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 
alongside income channels (or to instead increase support for the Republicans). However, I find no evidence for the basic mechanisms underpinning these theories.

The civic education and social network hypotheses both emphasize the importance of changes in political engagement in changing political attitudes. However, consistent with existing research from political science (Galston 2001), columns (8) and (9) in Table 7 find that an additional grade of high school does not significantly affect, and if anything reduces, multi-item scales of political knowledge or political interest. Furthermore, columns (10) and (11) find no association between schooling and greater discussion of politics with friends or family or trust in the federal government. This evidence suggests that CSL-compliers were not induced to engage and participate in politics more.34

Moreover, policy and issue preferences do not change in accordance with socialization explanations. As noted above, there is no evidence to suggest that greater schooling is associated with socially liberal positions on gun control or abortion. Albeit with smaller samples, columns (12) and (13) further show that if anything an additional year of high school is not significantly correlated with support for environmental protection and support for constitutionally banning gay marriage. In combination with not voting for the Democrats,

34Given previous research finds that political engagement is most strongly correlated with education among college graduates rather than high school graduates (Gerber et al. 2010), the lack of change in political engagement may only pertain to high school education’s political effects because schooling does not affect college attendance. Dee (2004) and Milligan, Moretti and Oreopoulos (2004) find more robust increases in political engagement at the high school level, but use less demanding specifications: the child labor law instruments in Dee (2004) are assigned according to 9 Census divisions, and thus exploit neither state-level variation nor (directly) the minimum drop out age; the large estimates in Milligan, Moretti and Oreopoulos (2004) may differ because their specifications do not include cohort fixed effects and because they instrument for completing high school rather than years of schooling (which could produce substantial upward bias; see above); finally, both articles use far older samples—neither includes data from the 2000s, which are the subject of analysis here. Furthermore, looking at similar outcomes in the NAES sample—such as watching the news on television, and the number of days a week the respondent reads a newspaper—I find no effects of CSLs or schooling.
there is thus no clear evidence suggesting that voters become more socially liberal on civil liberties issues.

6 Conclusion

Despite education's potential to alter students' life trajectories, this is the first study to identify how high school impacts which political party a voter supports later in life. While previous studies have linked education to civic and political participation, this article gives schooling a partisan tint by showing that it causes voters to become more conservative on economic issues, and vote on the basis of such policy preferences. The findings from the U.S. are stark: I show that an additional grade of late high school substantially increases the probability that a voter will identify with or vote for the Republican party later in life. The magnitude of this effect implies that state policies to increase the school leaving age may have significantly altered electoral outcomes, and thus indirectly impacted policy outcomes and the welfare of the electorate. In the context of significant efforts, particularly from Democrats, to increase state dropout ages, the results pose an important strategic problem for Democrats aiming to simultaneously help poorly-educated voters while winning elections.

Both income and socialization theories could explain this finding. However, an explanation combining human capital theory (including evidence that there is a significant economic return to schooling) with the RMR model of income-based policy preferences receives significant empirical support. Consistent with this explanation, I show that additional high school education causes voters to support low tax rates, without adopting Republican positions on non-economic issues. Furthermore, the largest effects of education are registered around voters' mid-life earnings peak. Conversely, there is no evidence to suggest that additional schooling increases political engagement or socially liberal values.

School dropout ages only significantly increase levels of high school education, without
affecting the probability that a student attends college or any other form of post-secondary education. Consequently, my results speak only to high school education. While this remains an important issue in the U.S., where nearly 20% of students still fail to graduate from high school (Murnane 2013), an important question for future research is whether the effects of college education are substantially different. For example, many scholars argue that college education is particularly effective at instilling socially liberal values. Furthermore, additional evidence from outside the U.S. would illuminate the generality of these findings.
References


Hainmueller, Jens and Michael J. Hiscox. 2010. “Attitudes toward highly skilled and low-


Appendix

Detailed variable definitions

The variables used are defined in detail below, including source variable names corresponding to National Annenberg Election Survey (NAES) and American Communities Survey (ACS) variables. All NAES variable codes correspond to their definition in the codebook for their respective waves, where wave 1 is the 2000 Presidential election, wave 2 is the 2004 Presidential election, and wave 3 is the 2008 Presidential election. Table 1 in the main paper provides summary statistics.

- *(Republican) Partisan.* Indicator coded 1 for the respondent identifying as a Republican. Residual category is independents, Democrat partisans and “don’t know”. Non-answers were deleted. This question was asked both before and after the Presidential election. NAES variables “cV01” in wave 1, “cMA01” in wave 2, and “MA01” in wave 3.

- *Intend (to vote Republican for President).* Indicator coded 1 for respondents intending to vote for the Republican Presidential candidate in the forthcoming election. The omitted category contains any other vote, excluding non-voters and those who do not provide an answer. This question is not asked in post-election surveys. NAES variable “cR23” in wave 1, “cRC03”, “cRC07” and “cRC10”-“cRC14” in wave 2, and “RCa03”, “RCa05”, “RCa07” and “RCa08” in wave 3.

- *Vote (Republican for President).* Indicator coded 1 for respondents reporting that they voted for the Republican Presidential candidate at the previous election (Bob Dole in 1996, George W. Bush in 2000 and 2004, or John McCain in 2008). The omitted category contains any other vote or not turning out. NAES variable “cR35” in wave
1, “cRD01” in wave 2, and “RDc01” in wave 3.

- **Male.** Indicator variable for respondents identifying as male. Non-responses were deleted. NAES variable “cW01” in wave 1, “cWA01” in wave 2, and “WA01” in wave 3; ACS variable name “sex”.

- **Race.** Three indicators for respondents identifying their race as White (including Hispanic, because this is how the ACS is coded), Black, or Asian (Chinese, Japanese and Other Asian of Pacific Islanders in the ACS). The omitted category is all other races. Coded from NAES variable “cW03” in wave 1, “cWA04” in wave 2, and “WC03” in wave 3; ACS variable name “race”.

- **Age.** Years of age at date at which survey was conducted. Quartic versions of age are standardized. NAES variable “cW02” in wave 1, “cWA02” in wave 2, and “WA02” in wave 3; ACS variable name “age”.

- **Cohort (year aged 14).** Calculated as the year of the survey, minus the respondent’s age at the date of the survey, plus 14. NAES variables “cW02” and “cdate” in wave 1, “cWA02” and “cDATE” in wave 2, and “WA01” and “DATE” in wave 3; ACS variables “age” and “year”.

- **State grew up.** Respondent’s current state of residence in NAES and state of birth in ACS. NAES variable “cST” in wave 1, “cST” in wave 2, and “WFc01” in wave 3; ACS variable name “bpl”.

- **Survey year.** Set of indicators for the year—2000, 2001, 2003, 2004, 2007 or 2008—in which the survey was conducted. Because month of survey is not available in the ACS, the precise survey completion date must be coarsened to year. NAES variable “cdate” in wave 1, “cDATE” in wave 2, and “DATE” in wave 3; ACS variable name “year”.

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• **CSLs.** See definition in the Appendix of the main paper. State-year CSL data from Oreopoulos (2009) were kindly provided by Philip Oreopoulos, and are based on the National Center for Education Statistics’ Education Digest.

• **Schooling.** Number of completed years of grade school (excluding Kindergarten) or above; top coded at 12 years of education (i.e. completing high school). Those that did not graduate high school with a diploma are coded as 11. ACS variable name “educd”.

• **Beyond 12th grade.** Indicator coded 1 for respondents registering any education beyond the high school level. Coded from ACS variable name “educ”.

• **Reduce taxes.** Indicator coded 1 for respondents who strongly think taxes should be reduced; “don’t know” was excluded. Coded from NAES variable “cBB01” in wave 1 (coded 1 for respondents who see the amount Americans pay in taxes as an “extremely serious” problem), “cCB13” in wave 2 (respond with “strongly” favoring tax reduction), and “CBb01” (respond with “cut taxes”) in wave 3.

• **Ban abortion.** Indicator coded 1 for respondents who stated that abortion should never be permitted. Coded from NAES variable “cBF03” in wave 1 (coded 1 for respondents who agreed that the federal government should ban all abortions), “cCE01” in wave 2 (coded 1 for respondents who “strongly favor” or “somewhat favor” banning all abortions), and “CEa01” in wave 3 (coded 1 for respondents who state that abortion is “not permitted under any circumstances”).

• **Low gun controls.** Indicator coded 1 for respondents who answered that the federal government should do “more” to restrict the kinds of guns that people can buy. Coded from NAES variable “cBG06” in wave 1 and “cCE31” in wave 2; this was not asked in wave 3.
• **Low health care spending.** Indicator coded 1 for respondents who said that the federal government should spend more money on providing health care to those that do not already have it. Coded from NAES variable “cBE02” in wave 1 and “cCC02” in wave 2; this was not asked in wave 3.

• **High military spending.** Indicator coded 1 for respondents who said that the federal government should spend more on military defense. Coded from NAES variable “cBJ07” in wave 1 and “cCD03” in wave 2; this was not asked in wave 3.

• **Republican non-economic issues.** Standardized summative rating combining the ban abortion, do not increase gun controls, do not increase health care spending, and increase military spending. Cronbach's alpha inter-item reliability coefficient of 0.39, pooled across samples.

• **Political knowledge scale.** Standardized summative rating scale combining correct/incorrect answers to up to six 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; ability to name own Senator. Cronbach’s alpha inter-item reliability coefficient of 0.60 in 2003/2004 and 0.60 in 2007/2008; underlying variable were unavailable for 2000/2001. NAES variables “MC01”-“MC04” in wave 3, and “cMC01”, “cMC03”, “cMC05”, “cMC07”, “cMC09” and “cUA02” in wave 2.

• **Political interest scale.** Standardized summative rating scale combining the following five measures of interest in politics: four-point scale measuring regularity with which the respondent reports following government and public affairs (“cK01” in wave 1, and “cKA03” in wave 2); four-point scale measuring regularity with which the respondent
reports follows the Presidential campaign ("cK02" in wave 1, “cKA01” in wave 2, and “KA01” in wave 3); number of days in the last week that the respondent watched a public or cable news program ("cE01” and “cE02” in wave 1, and “cEA01” and “cEA03” in wave 2); number of days in the last week (0 to 7) that the respondent read a daily newspaper ("cE13” in wave 1, and “cEA01” in wave 2); number of days in the last week (0 to 7) that the respondent discussed politics with family or friends ("cK05” in wave 1, “cKB01” in wave 2, and “KB01” in wave 3). Cronbach’s alpha inter-item reliability coefficient pooled across all surveys is 0.61.

- **Discuss politics.** Standardized number of days in the last week (0 to 7) that the respondent discussed politics with family or friends. NAES variable “cK05” in wave 1, “cKB01” in wave 2, and “KB01” in wave 3.

- **Trust federal government.** Indicator coded 1 for respondents that trust the federal government to what is right either “always” or “most of the time”; residual category includes “some of the time”, “never” and “don’t know”. NAES variable “cM01” in wave 1, “cMB01” and “cMB02” in wave 2, and “MB01” in wave 3.

- **Protect environment more.** Indicator coded 1 for respondents who believe that the federal government should do more to protect the environment; responses of the same, less or nothing were coded as 0. NAES variable “cBS01” in wave 1 and “cCF08” in wave 2; this was not asked in wave 3.

- **Ban gay marriage.** Indicator coded 1 for respondents that would favor a constitutional amendment banning gay marriage.

- **Age of eldest child.** Age of oldest child in the respondent’s household. ACS variable name “eldch”.

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• *Age of youngest child*. Age of youngest child in the respondent’s household. ACS variable name “yngch”.

• *Single*. Indicator coded 1 for respondents that have never been married. ACS variable name “marst”.

**Further details on IV estimation**

**Support for the monotonicity assumption**

![Cumulative distribution function (CDF) plot](image)

Figure 2: Monotonic first-stage (ACS data)

Although monotonicity is fundamentally untestable, one implication of monotonicity with multi-valued treatments is that the CDFs $F(S|\text{CSL}=z)$ should not cross (Angrist and Imbens 1995). Figure 2 plots the CDFs for each CSL instrument, and support the monotonicity assumption as the CDF of higher CSLs lie everywhere below the CDF of lower CSLs. Although important covariates such as state grew up in fixed-effects are not included, these
figures provide an alternative graphical depiction of the first-stage relationship with the largest changes in the relative CDFs occurring where the instruments apply. While this cannot prove monotonicity holds, because the counterfactual is never observed and the controls used in the main analysis are not used here, it is suggestive in that a possible violation receives no empirical support.

**Two-sample 2SLS estimation**

The goal is estimate the following system of IV equations:

\[
Y_i = T_i \beta_T + W_i \beta_T + u_i = X_i \beta + u_i \tag{8}
\]

\[
T_i = Z_i \Pi + \varepsilon_i, \tag{9}
\]

where \(X_i\) includes exogenous covariates \(W_i\) and the treatment variable(s) \(T_i\), while \(Z_i\) includes \(W_i\) and \(q\) excluded instruments. Identification requires that only \(p \leq q\) treatment variables can be instrumented for.

Two methods have been proposed for IV estimation with two samples. Angrist and Krueger (1992) propose a Wald-style estimator where the reduced form estimates are divided by their first stage counterparts, which can be generalized to the overidentified case where the number of instruments outnumber the number of endogenous variables. Inoue and Solon (2010) show that this estimator is less efficient than the 2SLS counterpart—first proposed by Franklin (1989)—that will be used in the empirical application here. The advantage of this estimator is that it corrects for finite-sample differences between the two samples.\(^3^5\)

Furthermore, its extension to multiple instruments and multiple endogenous variables is straight-forward—both of which are important in many empirical applications, including the analysis in this paper.

\(^3^5\)Inoue and Solon (2005) show that the TS2SLS estimator remains consistent even when differences in the sampling rates vary with some of the instrumental variables.
In matrix form (stacking over \(i\) in each sample), the two-sample 2SLS (TS2SLS) estimator is:

\[
\hat{\beta}_{TS2SLS} = (\hat{X}'_1\hat{X}_1)^{-1}\hat{X}'_1Y_1,
\]

where \(\hat{X}_1 = (\hat{T}_1, W_1)\) is the matrix of predicted values in sample 1. The OLS regression coefficients generating \(\hat{T}_1\) are based on \(p\) first stage regressions estimated in sample 2:

\[
\hat{X}_1 = Z_1\hat{\Pi} = Z_1(Z'_2Z_2)^{-1}Z'_2X_2.
\]

The following assumptions are required to ensure the consistency of the TS2SLS estimator (see Franklin 1989; Inoue and Solon 2010):

1. **Random sampling from the same population:** \(\{Y_{1i}, Z_{1i}\}_{i=1}^{n_1}\) and \(\{T_{2i}, Z_{2i}\}_{i=1}^{n_2}\) are independently and identically distributed draws of size \(n_1\) and \(n_2\) from the same population with finite second moments.

2. **Instrument exogeneity:** \(E[Z'_1\varepsilon_{1i}] = E[Z'_2\varepsilon_{2i}] = 0\).

3. **Exclusion restriction:** \(E[Z'_1u_{1i}] = 0\).

4. **Rank conditions:** (a) \(Z'_1Z_{1i}\) and \(Z'_2Z_{2i}\) have full rank, (b) \(X'_1Z_{2i}\) and \(X'_2Z_{2i}\) have full rank.

5. **Interchangeable sample moments:** (a) \(E[Z'_1X_{1i}] = E[Z'_2X_{2i}]\), (b) \(E[Z'_1Z_{1i}] = E[Z'_2Z_{2i}]\).

Assumption 1 says that the samples must draw from the same population. Assumption 2 requires that the instrument be exogenous in the first stage. Assumption 3 is implied by the exclusion restriction, but is written in terms of expectations. Assumption 4 is a standard
rank condition required for matrix invertibility. Assumption 5 requires that crucial samples moments can be interchanged, thereby permitting substitution between samples. As \( n_1 \) and \( n_2 \) converge to the population size, Assumption 5 necessarily holds.

Franklin (1989) proves the \( n_1 \)-consistency of the TS2SLS estimator.\(^{36}\) However, calculating the TS2SLS standard errors is not obvious. Calculating the standard errors from a regression of \( Y_1 \) on \( \hat{X}_1 \) neglects the uncertainty in the first stage, in addition to distributional differences between the first stage and reduced form samples.

The Murphy and Topel (1985) two-stage framework for understanding “generated regressors”—accounting for the uncertainty introduced where a variable is estimated as a proxy to enter a separate regression—incorporates such estimation uncertainty.\(^{37}\) Proposition 1 derives the homoskedastic and cluster-robust variance (matrices), of which the robust variance is the particular case of \( G_1 = n_1 \) and \( G_2 = n_2 \) clusters. \((i \text{ is dropped to facilitate exposition.})\)

**Proposition 1.** The asymptotic variance of the TS2SLS estimator, \( \mathbb{V}[\hat{\beta}^{TS2SLS}] \), is

\[
\begin{align*}
\left[ \sigma_u^2 + \frac{n_1}{n_2} \hat{\beta}^{TS2SLS} \Omega \hat{\beta}^{TS2SLS} \right] \mathbb{E}[\hat{X}_1' \hat{X}_1]^{-1}, & \quad \Omega = \mathbb{E}[\varepsilon' \varepsilon | \hat{X}_1] = \\
\sigma_1^2 & \quad \cdots \quad \sigma_{1,p} \\
\vdots & \quad \ddots \quad \vdots \\
\sigma_{p,1} & \quad \cdots \quad \sigma_p^2
\end{align*}
\]

(12)

when the reduced form squared error \( \sigma_u^2 = \mathbb{E}[u^2 | \hat{X}_1] \) and the error covariances \( \Omega \) of the \( p \) first stage regressions are homoskedastic; when the reduced form and first stage errors are grouped into \( G_1 \) and \( G_2 \) clusters respectively, the cluster-robust variance is

\[
\mathbb{E}[\hat{X}_1' \hat{X}_1]^{-1} \left[ \mathbb{V}[\hat{\beta}^{TS2SLS}] + \frac{n_1}{n_2} \mathbb{E}[\hat{X}_1'(\hat{\beta}^{TS2SLS} \Omega Z_1)] \mathbb{V}(\hat{\Pi}) \mathbb{E}[(\hat{\beta}^{TS2SLS} \Omega Z_1)' \hat{X}_1] \right] \mathbb{E}[\hat{X}_1' \hat{X}_1]^{-1}. \quad (13)
\]

---

\(^{36}\)Angrist and Krueger’s (1995) proof rests on showing that the TS2SLS estimator converges to the consistent Angrist and Krueger (1992) estimator, because of Assumption 5.

\(^{37}\)Inoue and Solon (2010) acknowledge this approach but derive homoskedastic and heteroskedastic variance matrices in an alternative way, but do not provide a cluster-robust variance estimate.
where $\hat{\beta}_{TS2SLS}^T$ is the vector of coefficients on $p$ endogenous variables, the uncorrected $TS2SLS$ variance is given by $\nabla[\hat{\beta}_{TS2SLS}] = \frac{G_1}{G_2} \sum_{g=1}^{G_1} E[X'_{1g}\hat{u}_{1g}\hat{u}'_{1g}X_{1g}]$ and the variances from $m$ first-stage regressions are $\nabla(\hat{\Pi}) = \frac{G_2}{G_2-1} \Phi \otimes E[Z'_{2}Z_{2}]^{-1}$, where

$$\Phi = \begin{bmatrix}
E[Z'_{2}Z_{2}]^{-1} \sum_{g=1}^{G_2} E[Z'_{2g}\hat{\varepsilon}_{2g1}\varepsilon'_{2g1}Z_{2g}] & \cdots & E[Z'_{2}Z_{2}]^{-1} \sum_{g=1}^{G_2} E[Z'_{2g}\hat{\varepsilon}_{2g1}\varepsilon'_{2g1}Z_{2g}]

\vdots & \ddots & \vdots

E[Z'_{2}Z_{2}]^{-1} \sum_{g=1}^{G_2} E[Z'_{2g}\hat{\varepsilon}_{2gp}\varepsilon'_{2gp}Z_{2g}] & \cdots & E[Z'_{2}Z_{2}]^{-1} \sum_{g=1}^{G_2} E[Z'_{2g}\hat{\varepsilon}_{2gp}\varepsilon'_{2gp}Z_{2g}]
\end{bmatrix}. \tag{14}$$

Proof: Start by separating $\hat{X}$ into its endogenous and exogenous components,

$$Y_{i1} = X_{i1}\beta_{-S} + T_{i1}\beta_{S} + u_i = X_{i1}\beta_{-S} + \hat{T}_{i1}\beta_{S} + [T_{i1} - \hat{T}_{i1}] + u_i, \tag{15}$$

where $\hat{T}_{i1} = Z_{i1}\hat{\Pi} = Z_{i1}(Z'_{2}Z_{2})^{-1}Z'_{2}T_{2}$ is the predicted value of the treatment using the first stage estimates, and $T_{i1}$ is the true and unobserved treatment in sample 1. An OLS regression would yield:

$$\sqrt{n_1}(\hat{\beta}_{-T} - \beta_{-S}) = \left(\frac{1}{n_1} \hat{X}'_{i1}\hat{X}_{i1}\right)^{-1} \frac{1}{\sqrt{n_1}} \hat{X}'_{i1}u_{1} + \left(\frac{1}{n_1} \hat{X}'_{i1}\hat{X}_{i1}\right)^{-1} \frac{1}{\sqrt{n_1}} \hat{X}'_{i1}[T_{i1} - \hat{T}_{i1}]\beta_{S}, \tag{16}$$

where subscripts $i$ and superscripts $TS2SLS$ are omitted to save space. Using the expansion result in Murphy and Topel (1985: 374) yields:

$$\sqrt{n_1}(\hat{\beta} - \beta) \equiv \sqrt{n_1}(\hat{\beta}_{-T} - \beta_{-S}) \equiv \left(\frac{1}{n_1} \hat{X}'_{i1}\hat{X}_{i1}\right)^{-1} \frac{1}{\sqrt{n_1}} \hat{X}'_{i1}u_{1}$$

$$+ \left(\frac{1}{n_1} \hat{X}'_{i1}\hat{X}_{i1}\right)^{-1} \left(\frac{n_1}{n_2}\right)^{1/2} \frac{1}{n_1} \hat{X}'_{i1}(\hat{\beta}'_T \otimes Z_1)\sqrt{n_2}(\hat{\Pi} - \Pi)\hat{\pi},$$

where $(\hat{\beta}'_T \otimes Z_1)$ is the matrix of defined in equation (12) of Murphy and Topel (1985).

Let $\hat{\Pi}$ be a consistent estimator of the first stage for the endogenous variables, such that
\[ \sqrt{n_2(\hat{\Pi} - \Pi)} \overset{a}{\sim} N(0, \mathbb{V}(\Pi)). \] Using our consistent first stage estimate, the asymptotic variance is therefore given by:

\[ \mathbb{V}(\hat{\beta} - \beta) = \mathbb{E}[\hat{X}_1'\hat{X}_1]^{-1}\left[\mathbb{V}[\hat{\beta}] + \frac{n_1}{n_2}\mathbb{E}[\hat{X}_1'(\hat{\beta}'_T \otimes Z_1)^{-1}\mathbb{V}[\Pi](\hat{\beta}'_T \otimes Z_1)'\hat{X}_1]\right]\mathbb{E}[\hat{X}_1'\hat{X}_1]^{-1}, \tag{18} \]

where \( \mathbb{V}[\hat{\beta}] \) is the variance of the naive TS2SLS estimator. (Note that \( \mathbb{E}[\hat{X}_1'u_1] = 0 \), in conjunction with a consistent first stage, implies the consistency of the estimator.)

This establishes the general asymptotic variance formula in Proposition 1. We now apply the homoskedastic and cluster-robust error structures:

1) Homoskedastic errors. Under homoskedasticity, the naive variance from the TS2SLS regression is simply \( \sigma_u^2(\hat{X}_1'\hat{X}_1)^{-1} \). To correct for the first stage estimation, we have:

\[ \hat{X}_1'(\hat{\beta}'_T \otimes Z_1)\mathbb{V}(\hat{\Pi})(\hat{\beta}'_T \otimes Z_1)'\hat{X}_1 = \hat{X}_1'(\hat{\beta}'_T \otimes Z_1)(\Omega \otimes (Z_1'Z_1)^{-1})(\hat{\beta}'_T \otimes Z_1)'\hat{X}_1 \] \tag{19}

\[ = \hat{X}_1'(\hat{\beta}'_T \otimes Z_1)\mathbb{V}(\hat{\Pi})(\hat{\beta}'_T \otimes Z_1)'\hat{X}_1 \] \tag{20}

\[ = \hat{\beta}'_T \Omega \hat{\beta}_T (\hat{X}_1'\hat{X}_1), \] \tag{21}

where the first line uses the definitions of homoskedasticity given in the proposition, the second line applies the mixed product property of Kronecker products, and the third line exploits \( Z_1(Z_1'Z_1)^{-1}Z_1'\hat{X}_1 = \hat{X}_1 \) (because all exogenous variables are contained in both \( \hat{X}_1 \) and \( Z_1 \)) and the fact that \( \hat{\beta}'_T \Omega \hat{\beta}_T \) is a scalar. Substituting into the general variance matrix yields the homoskedastic variance formula in Proposition 1.

2) Clustered errors. In the clustered case, we simply let \( \mathbb{V}(\hat{\Pi}) = \frac{G_2}{G_2^{-1}} \Phi \otimes \mathbb{E}[Z_2'Z_2]^{-1}. \]

Standard errors are given by the square roots of the diagonal elements of \( \mathbb{V}[\hat{\beta}^{TS2SLS}]/n_1 \).

Using the analogy principle, expectations can be replaced by sample moments.

In the case of a single endogenous regressor, \( \mathbb{V}(\hat{\Pi}) \) is simply the standard cluster-robust
variance matrix for the first stage:

$$\mathbb{E}[Z'_2Z_2]^{-1} \left[ \frac{G_2}{G_2 - 1} \sum_{g=1}^{G_2} \mathbb{E}[Z'_{2g}\hat{\varepsilon}_{2g}\hat{\varepsilon}'_{2g}Z_{2g}] \right] \mathbb{E}[Z'_2Z_2]^{-1}. \tag{22}$$

When there are multiple endogenous variables, the first stage estimates may be correlated across models. This requires the more complex formulation in Proposition (1).

**Upwardly biased estimates of completing high school**

Table 8 provides the estimate when CSLs are used to instrument for completing high school. As noted in the main text, these estimates will be biased if the instruments induce additional schooling beyond completing high school and such years of schooling affect the political outcomes for such compliers.

Consistent with such an exclusion restriction violations inducing considerable bias, the estimates in Table 8 for both the NAES and ANES samples are extremely large. Relative the effect sizes for each additional year of schooling, the effect magnitudes increases substantially.\(^\text{38}\) Of the specifications, four produce a coefficient that exceeds the theoretical maximum of one. In the other three, the coefficient is again far larger than the comparable weighted per-year estimates obtained in the main paper. These estimates are thus consistent with a large bias due to dichotomizing an endogenous variable with multiple intensities. Furthermore, since the first stage is relatively strong in most specifications, the inflated coefficients cannot simply be attributed to weak instrument bias (see Staiger and Stock 1997). Nevertheless, while the bias is large in this application, it is important emphasize that the extent of bias is application-specific (see Marshall 2015).

\(^\text{38}\)Equations including linear state-specific cohort trends are excluded because the first stage becomes very weak.
Table 8: IV estimates of the effect of completing high school on Republican partisanship and voting

<table>
<thead>
<tr>
<th>Panel A: NAES IV (2SLS)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Complete high school</td>
<td>1.372***</td>
<td>0.937***</td>
<td>1.307</td>
</tr>
<tr>
<td></td>
<td>(0.360)</td>
<td>(0.338)</td>
<td>(0.893)</td>
</tr>
<tr>
<td>Observations</td>
<td>176,796</td>
<td>144,742</td>
<td>106,879</td>
</tr>
<tr>
<td>First stage $F$ statistic</td>
<td>12.8</td>
<td>10.6</td>
<td>2.4</td>
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<table>
<thead>
<tr>
<th>Panel B: ANES IV (2SLS)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Complete high school</td>
<td>0.884***</td>
<td>0.587**</td>
<td>1.518***</td>
<td>0.847**</td>
</tr>
<tr>
<td></td>
<td>(0.235)</td>
<td>(0.244)</td>
<td>(0.463)</td>
<td>(0.402)</td>
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<tr>
<td>Observations</td>
<td>35,669</td>
<td>14,638</td>
<td>18,980</td>
<td>13,262</td>
</tr>
<tr>
<td>First stage $F$ statistic</td>
<td>11.5</td>
<td>10.8</td>
<td>6.9</td>
<td>5.2</td>
</tr>
</tbody>
</table>

Notes: Outcomes are: Republican partisan (“partisan”), intending to vote Republican for President (“intend”), voting Republican in previous presidential election (“vote”), voting Republican in the most recent House election (“House”), and voting Republican in the most recent Senate election (“Senate”). All specifications include gender, white, black and Asian dummies, quartic (demeaned) age polynomials, and state grew up, cohort and survey fixed-effects. The omitted CSL category is CSL ≤ 15. First stage observations decline for vote intention because the ACS sample is reduced to match the variables in the NAES sample. Standard errors clustered by state-cohort in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 

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Table 9: Exclusion restriction tests

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>Age of eldest child</td>
<td>Age of youngest child</td>
<td>Single</td>
<td>Age of eldest child</td>
<td>Age of youngest child</td>
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<td>0.100</td>
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<td>0.007*</td>
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<td></td>
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<td>(0.158)</td>
<td>(0.004)</td>
<td>(0.182)</td>
<td>(0.179)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>CSL≥17</td>
<td>-0.043</td>
<td>0.112</td>
<td>0.000</td>
<td>-0.070</td>
<td>0.238</td>
<td>0.001</td>
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<tr>
<td></td>
<td>(0.161)</td>
<td>(0.164)</td>
<td>(0.004)</td>
<td>(0.192)</td>
<td>(0.186)</td>
<td>(0.005)</td>
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<tr>
<td>Linear state-specific cohort trends</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
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</table>

Notes: All specifications include gender, white, black and Asian dummies, quartic (demeaned) age polynomials, state grew up, and cohort and survey fixed-effects. Omitted CSL category is CSL≤15. Standard errors clustered by state-cohort in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Additional robustness checks

Exclusion restriction tests

Table 9 reports the exclusion restriction tests cited in the main paper. The results suggest that CSLs do not meaningful effect fertility and martial choices. The results show that CSLs do change the age of a respondent’s children or their probability of being married at the time of the survey. The age of children variables, in particular, suggest that adolescent life choices were not substantially affected.

ANES data

The American National Election Survey (ANES) collects many political variables which are pooled across presidential and mid-term elections 1952-2000 and 2008.\textsuperscript{39} Surveys typically \textsuperscript{39}2002-2006 surveys were omitted because they did not ask where respondents grew up, a central component of the identification strategy.
interview several thousand randomly-sampled voting-age citizens; pooling across elections produced a maximum sample of 35,873 respondents.\footnote{Observations suffering missingness were deleted.}

The ANES has a far smaller sample size than the NAES. Given the large number of fixed-effects and trends included in the main specifications, the greater sample size of the NAES is preferred. This section will show that the results using the ANES data are similar. The ANES has several advantages over the NAES, and thus showing similar results is an important robustness check: the ANES covers a wider range of election extending much further back in time; by extending further back in time, the ANES also encompasses greater variation in schooling; and by asking respondents which state they lived in at age 14, cross-state migration concerns are minimized.

Like the NAES, the number of years of schooling is not measured in the ANES. Accordingly, this paper complements the ANES with extracts from the 1950-2000 decennial Censuses. By using decennial Census data instead of ACS surveys from the year of NAES survey, the assumption that respondents are drawn from the same population is a little less likely to hold. Nevertheless, I again ensure the Census sample of 636,713 individuals is approximately a random sample from the same population the ANES is drawn from. To achieve this, I followed exactly the same stratified procedure as for the ACS data, as detailed in the Appendix of the main paper. The two datasets are again combined using two-sample methods. Figure 3 shows average years of schooling by cohort, while Figure 4 shows that the monotonicity check is satisfied.

The variables used in this analysis are defined in detail below, including source variable names corresponding to ANES and Census variables. All ANES variables codes correspond to the Time Series Cumulative Data File. Table 10 provides summary statistics.

- \textit{Republican partisan}. Indicator coded 1 for the respondent identifying as a Republican, including those weak partisans who when pressed leaned toward the Republicans.
Figure 3: US years of schooling by cohort (Census data)
Table 10: Summary statistics: ANES and Census samples

<table>
<thead>
<tr>
<th></th>
<th>ANES</th>
<th></th>
<th></th>
<th></th>
<th>Census</th>
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<td></td>
<td>Obs.</td>
<td>Mean</td>
<td>Std. dev.</td>
<td>Min.</td>
<td>Max.</td>
<td>Obs.</td>
<td>Mean</td>
<td>Std. dev.</td>
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<td><strong>Dependent variables</strong></td>
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<tr>
<td>Republican partisan</td>
<td>35,873</td>
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<td>0.48</td>
<td>0</td>
<td>1</td>
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<tr>
<td>Vote Republican for Pres</td>
<td>14,712</td>
<td>0.48</td>
<td>0.5</td>
<td>0</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Vote Republican for House</td>
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<td>0.5</td>
<td>0</td>
<td>1</td>
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<tr>
<td>Vote Republican for Sena</td>
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<td>0.45</td>
<td>0.5</td>
<td>0</td>
<td>1</td>
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</tr>
<tr>
<td>Schooling</td>
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<td></td>
<td></td>
<td>636,713</td>
<td>10.75</td>
<td>2.62</td>
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<td><strong>Excluded instruments</strong></td>
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<tr>
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<td>0.75</td>
<td>0.43</td>
<td>0</td>
<td>1</td>
<td>636,713</td>
<td>0.74</td>
<td>0.44</td>
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<tr>
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<td>0.37</td>
<td>0</td>
<td>1</td>
<td>636,713</td>
<td>0.18</td>
<td>0.38</td>
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<td><strong>Pre-treatment covariates</strong></td>
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<td>Age</td>
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<td>43.72</td>
<td>16.04</td>
<td>17</td>
<td>97</td>
<td>636,713</td>
<td>43.07</td>
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<td>0.5</td>
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<td>636,713</td>
<td>0.42</td>
<td>0.49</td>
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<td>White</td>
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<td>0</td>
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<td>636,713</td>
<td>0.86</td>
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<tr>
<td>Black</td>
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<td>0</td>
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<td>636,713</td>
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<td>0.06</td>
<td>0</td>
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<td>636,713</td>
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<td>0.06</td>
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<tr>
<td>Year aged 14</td>
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<td>1,950.60</td>
<td>19.4</td>
<td>1,914</td>
<td>1,996</td>
<td>636,713</td>
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<tr>
<td>Census year (decade)</td>
<td>35,997</td>
<td>1,980.05</td>
<td>13.34</td>
<td>1,950</td>
<td>2,000</td>
<td>636,713</td>
<td>1,980.27</td>
<td>13.4</td>
</tr>
</tbody>
</table>
Residual category is independents and Democrat partisans; non-answers were deleted.
ANES variable VCF0303.

- **Vote Republican.** Vote Republican for President, Vote Republican for House and
  Vote Republican for Senate are indicators for voting Republican in any of these elec-
  tions, including at mid-terms. The omitted category contains Democrat and other party/candidate voters; non-voters and those who do not provide an answer are deleted. ANES variables VCF0704, VCF0707 and VCF0708.

- **Gender.** Indicator variable for respondents identifying as male. Non-responses were deleted. ANES variable VCF0104; Census variable name “sex”.

- **Race.** Three indicators for respondents identifying their race as White or Hispanic,
  Black, or Asian (Chinese, Japanese and Other Asian of Pacific Islanders in the Census).
The omitted category is all other races. Hispanics are grouped with whites under the
Census. Coded from ANES variable VCF0106a; Census variable “race”.

- **Age.** At time of survey. Quartic version of age is not standardized to ensure comparability across ANES and Census datasets. ANES variable VCF0101; Census variable name “age”.

- **Year aged 14.** Calculated as the year of the survey, minus the respondent’s age at the date of the survey, plus 14. Since no person in the Census could be 14 later than 1996, the small number of ANES observations for respondents 14 after 1996 were dropped. Since no person in the ANES was 14 before 1914, all such Census observations were dropped (see above). 1914 is the omitted category when a full set of indicators is used. Coded from ANES variables VCF0101 and VCF0104; Census variables “age” and “year”.

- **State grew up.** The ANES asks respondents what state they grew up in, taking the state where more time between 6 and 18 was spent if respondents moved across states. The Census asks which state respondents were born in, which requires a stronger no-migration assumption when matching individuals to the state CSLs. Foreign-born respondents were excluded. ANES variable VCF0132 and VCF0133; Census variable “bpl”.

- **Census decade.** Indicator for each decade of the Census conducted, 1950-2000. For the ANES, I count the nearest years as part of that Census decade; e.g. the ANES surveys 1986-1994 are coded for the 1990 Census indicator; the 2008 election is included with the 2000 Census. Because TS2SLS estimation requires using exactly the same variables across datasets, election-specific indicators (which would be possible if we were only using the ANES) cannot be used. Coded from ANES variable VCF0004; Census variable name “year”.

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• *CSLs.* Same as ACS.

• *Schooling.* Same as ACS. Census variable name “educd”.