

THE ANTI-DEMOCRAT DIPLOMA: HOW HIGH SCHOOL EDUCATION DECREASES SUPPORT FOR THE DEMOCRATIC PARTY*

JOHN MARSHALL[†]

MARCH 2018

Attending high school can alter students' life trajectories by affecting labor market prospects and through exposure to ideas and networks. However, schooling's influence competes with early socialization forces, and may be confounded by selection biases. Consequently, little is known about whether or how high school education shapes downstream political preferences and voting behavior. Using a generalized difference-in-differences design leveraging variation in U.S. state dropout laws across cohorts, I find that raising the school dropout age decreases Democratic partisan identification and voting later in life. Instrumental variables estimates suggest that an additional completed grade of high school decreases Democrat support by around fifteen percentage points among students induced to remain in school by higher dropout ages. High school's effects principally operate by increasing income and support for conservative economic policies, especially at an individual's mid-life earnings peak. In contrast, schooling does not affect conservative attitudes on non-economic issues or political engagement.

*I thank Jim Alt, Charlotte Cavaille, Dan Carpenter, Jon Fiva, Anthony Fowler, Andy Hall, Alex Hertel-Fernandez, Shigeo Hirano, Torben Iversen, Larry Katz, Horacio Larreguy, Rakeen Mabud, Jonathan Phillips, Jim Snyder, Harvard University workshop participants, and Bill Jacoby and three anonymous reviewers for insightful comments. I wish to thank John Bullock, Philip Oreopoulos, and Jim Snyder for kindly sharing data.

[†]Department of Political Science, Columbia University. jm4401@columbia.edu.

1 Introduction

Education is a fundamental component of U.S. economic and social policy, and remains a salient political issue. As recently as 2010, 16% of students per cohort still failed to graduate from high school by age 20-24 (Murnane 2013). 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. ... I am proposing that every state ... requires that all students stay in high school until they graduate or turn 18.” Dropout ages have generally increased over the 20th century, and many states have recently considered raising or have successfully raised their minimum leaving age to 17 or 18.¹

While high school education undoubtedly shapes labor market prospects and social interactions, its capacity to affect voters’ downstream political preferences and voting behavior is not theoretically obvious. First, schooling could exert limited influence on partisan identification and voting behavior in the face of early habitual and socialization forces beyond the classroom. A prominent literature argues that partisan identities and vote choices are highly durable over an individual’s lifetime (e.g. Bartels 2010; Campbell et al. 1960). Such behaviors are believed to develop primarily at home from a young age (Jennings, Stoker and Bowers 2009) or as a young adult (Meredith 2009; Stoker and Bass 2011).

Second, even if high school education is able to influence political preferences, it is not clear whether this predisposes voters toward the Democratic or Republican party. On one hand, education increases downstream income (e.g. Acemoglu and Angrist 2000), which may cause voters to support lower taxation (Meltzer and Richard 1981). At least once the returns to schooling are realized later in life, remaining in high school could thus *decrease* Democratic support. Conversely,

¹In 2013, Kentucky increased the dropout age to 18 (or receiving a high school diploma). Rhode Island raised its leaving age to 18, effective of 2011. In 2012, Maryland raised its dropout age to 17 for 16-year-old students for the 2015-2016 school year, and to 18 for the 2017-2018 school year. Alaska, Delaware, Illinois, Massachusetts, Montana, South Carolina, and Wyoming have introduced similar bills since 2011.

schooling may increase exposure to liberal values, either in the classroom (e.g. [Dee 2004](#); [Hillygus 2005](#); [Niemi and Junn 1998](#)) or through liberal social networks (e.g. [Huckfeldt and Sprague 1995](#); [Nie, Junn and Stehlik-Barry 1996](#)). The socialization induced by schooling could then instead *increase* Democratic support. Both sets of arguments suggest that schooling has the potential to transform students' life trajectories, causing them to change their policy stances and ultimately support different political parties later in life.

Existing evidence investigating high school education's partisan effects is correlational and often conflicting. Classic studies document a positive association between greater education and voting Republican (e.g. [Campbell et al. 1960](#); [Verba, Schlozman and Brady 1995](#)). However, such correlations may simply reflect differences in the types of people that become educated ([Kam and Palmer 2008](#); [Sondheimer and Green 2010](#)). Furthermore, by failing to disentangle either the direction of this relationship or its mechanisms, scholars have struggled to understand the basis of the widely-documented correlations between income (which education increases) and support for conservative economic policies (e.g. [Erikson and Tedin 2015](#); [Gelman 2010](#)), and between education and socially liberal attitudes (e.g. [Dee 2004](#)).

This article identifies how raising the school dropout age, and an additional completed grade of high school education that this imparts, affects voter policy preferences and voting behavior in later life. To estimate compulsory schooling's partisan effects, I employ a generalized difference-in-differences design leveraging cross-cohort differences in the dropout age across states (see [Acemoglu and Angrist 2000](#); [Angrist and Krueger 1991](#)). In addition to estimating the partisan effects of raising the dropout age, I also use dropout laws to instrument for the number of grades of schooling that an individual completes. Raising the dropout age increases completed grades of schooling, without affecting post-high school education. Consequently, the instrumental variables (IV) estimates identify the effect of completing an additional grade of high school for students, disproportionately from disadvantaged backgrounds, that only remained in school due to a higher state dropout age.

Examining nationwide surveys conducted around the 2000, 2004, and 2008 presidential campaigns, I find that high school education causes voters to become significantly less liberal in later life. First, raising the dropout age has a large partisan effect in its own right: increasing the minimum school leaving age by a year induces a 1-3 percentage point shift, per birth-year cohort, against identifying as a Democrat partisan or voting for the Democratic party. Increased Republican support is commensurate. Second, the IV estimates further indicate that an additional completed grade of high school reduces the probability that compliers—students that are disproportionately black, Hispanic, and male—will subsequently identify as a Democrat partisan and vote for a Democratic presidential candidate by around 15 percentage points.

Furthermore, my evidence suggests that high school's partisan effects on downstream political behavior operate primarily via an income-based channel. Previous studies indicate that an additional year of schooling increases wages by around 10% (Acemoglu and Angrist 2000; Angrist and Krueger 1991; Ashenfelter and Rouse 1998; Oreopoulos 2009). Consistent with education's economic returns inducing voters to support less redistribution, schooling's political effects are greatest at an individual's mid-life earnings peak, while additional schooling induces voters to favor Republican economic policy positions. Although this could simply reflect voters becoming Republican partisans and adopting the party's policy positions (Lenz 2012), education does not increase support for Republican positions on non-economic issues such as abortion, gun control, health care spending, and military spending. Furthermore, contrary to the predictions of civic education (Dee 2004; Hillygus 2005) and social network (Mendelberg, McCabe and Thal 2017; Nie, Junn and Stehlik-Barry 1996) socialization theories, there is little immediate effect of education and no evidence to suggest that schooling affects political engagement or socially liberal values. Together, these results suggest that, by increasing an individual's subsequent income, high school education causes voters to support economic policies that ultimately reduce their propensity to identify as and vote Democrat.

These findings challenge the predominant perspective on the life-cycle development of politi-

cal behavior. Previous studies have emphasized both the “primary principle,” that what is learned first—typically from parents (e.g. Jennings, Stoker and Bowers 2009)—is learned best (see Erikson and Tedin 2015), and that early adulthood is central to political socialization (Mendelberg, McCabe and Thal 2017; Stoker and Bass 2011). In contrast, although there is evidence that schooling increases political participation (Sondheimer and Green 2010), adolescent civics classes and peer interactions have produced limited effect on civic and partisan attitudes (Erikson and Tedin 2015; Green et al. 2011). However, my findings show that high school education also has important consequences for partisan identification and vote choice later in life, which can overcome other influences before and after high school, and reflect downstream income opportunities rather than socialized values. Given that high school’s anti-Democrat effects coincide with voters’ earnings peak, and thus vary across a voter’s lifetime, the results imply that partisan preferences are more responsive to changes in circumstances over a lifetime than previous studies highlighting the stickiness of political behavior suggest. My findings also suggest a “catch 22” for the Democratic party, whose support for increasing access to education comes at the cost of facilitating future Republican support.

By emphasizing high school’s *partisan* effects, this article departs from the large literature debating the relationship between education and political participation (e.g. Kam and Palmer 2008; Sondheimer and Green 2010). Rather, this study complements recent evidence that attending an affluent college with a norm of financial gain induces students to support conservative economic policies (Mendelberg, McCabe and Thal 2017). I show that a similar income-oriented mechanism applies to the realization of income gains accruing to high school education, and ultimately shapes voters to oppose taxation and vote Republican. Since high school’s effects are most pronounced at a voter’s earnings peak, my findings suggest that Mendelberg, McCabe and Thal’s (2017) study of students at college—before financial gains are realized—could also understate the long-term consequences of conservative college campuses.

2 Theoretical perspectives

Education plays a central role in the lives of American voters. To the extent that it can overcome other early life forces, I build upon contrasting economic and sociological mechanisms to consider how schooling might affect which political party a voter chooses to support.

2.1 Education decreases support for taxation?

Perhaps the most obvious effect of education is to alter an individual's earnings trajectory. Twin studies in the U.S. suggest that an additional year of schooling increases annual wages by 11-16% later in life (e.g. [Ashenfelter and Rouse 1998](#)), while studies using compulsory schooling laws to instrument for schooling estimate this rate of return to be 8-18% ([Acemoglu and Angrist 2000](#); [Angrist and Krueger 1991](#); [Oreopoulos 2006, 2009](#)). The human capital interpretation of this influential body of research claims that education imparts productive skills, which are rewarded by labor markets.

Linking schooling's effect on income to political behavior, [Meltzer and Richard \(1981\)](#) argue that individuals with higher wages will prefer lower income tax rates and less redistribution. To the extent that redistributive policy preferences influence vote choice, preferring lower income taxation entails supporting candidates advocating low tax rates. Particularly since Ronald Reagan's presidency, the Republican party has consistently supported lower income taxation than the Democratic party. As ideological and policy differences between the two parties have grown ([McCarty, Poole and Rosenthal 2006](#)), differences on this issue have become easier for voters to discern. According to American National Election Surveys conducted since 2000, 71% of voters identify the Democratic party as more liberal than the Republican party. Moreover, since the 1980s, survey correlations have consistently shown that higher-income voters are less likely to identify with and vote for the Democrats ([Erikson and Tedin 2015](#); [Gelman 2010](#)).

Together, the human capital and Meltzer-Richard models predict that—by increasing their

income—schooling makes voters more favorable toward Republican economic policies, and particularly their proposals for lower income tax rates. Differences in vote choices are thus expected to increase with realized returns to education. Given that voters can anticipate future income, redistributive preferences may also reflect expected earnings (Alesina and La Ferrara 2005).

Critics of human capital theory have instead argued that education does not itself impart skills that actually improve a worker’s productivity (Spence 1973). Rather, education enables productive workers to distinguish themselves from less productive workers, for whom the costs of signaling productivity by acquiring more education are greater, when employers cannot directly observe a worker’s productivity. Consequently, an increase in education should not affect wages, the national income distribution, or a voter’s support for the economic policies proposed by different political parties.² Such a signaling model, in conjunction with the Meltzer-Richard logic, could still be consistent with a *correlation* between education and vote choices. However, there should be no empirical relationship when using research designs that identify the effects of education by abstracting from the underlying differences in productivity that might induce more productive individuals to seek greater education.

2.2 Education increases exposure to liberal values?

From a political socialization perspective, education’s effects on political behavior could be very different. Many studies show that education is correlated with socially and politically liberal attitudes, such as greater support for freedom of expression (Dee 2004), ethnic diversity (e.g. Campbell et al. 1960), and immigration (e.g. Hainmueller and Hiscox 2010). In the period examined in this article, these values have become strongly associated with the Democratic party (McCarty, Poole and Rosenthal 2006). Thus, while education may bestow economically valuable knowledge and skills, it could also instill liberal values that cause voters to adopt liberal political preferences.

Two main mechanisms have been proposed to explain these associations: direct effects of cur-

²Assuming that additional education does not break the separating equilibrium.

ricula and changes in social network composition. First, civic education and social science classes generally encourage political engagement and trust in the political system (Niemi and Junn 1998), and seek to instill liberal values of tolerance (Dee 2004; Hillygus 2005). Such classes typically start in late high school. Furthermore, formative experiences appear to affect political beliefs decades later in life (Jennings, Stoker and Bowers 2009; Meredith 2009). However, recent experimental evidence finds that while enhanced high school civics classes increase political knowledge, this knowledge quickly decays and does not affect support for civil liberties (Green et al. 2011). Given that other empirical evidence has emphasized the importance of college education (see Galston 2001), civic education's importance could be confined to higher education.

Second, education could more indirectly affect a voter's political beliefs by altering their social network's composition, which may in turn influence their political attitudes (e.g. Huckfeldt and Sprague 1995; Lazarsfeld, Berelson and Gaudet 1948). More educated individuals sort into more prestigious, ethnically-diverse, and politically-connected networks (Nie, Junn and Stehlik-Barry 1996), or at least become exposed to such groups. 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 Democrats. At lower levels of education, individuals may enter social networks like labor unions with a clear left-wing political agenda.

While both mechanisms could induce socially liberal attitudes, observed correlations could still be spurious. Family background, values, cognitive abilities, and life experiences are correlated with both education and socially liberal attitudes (Kam and Palmer 2008). Similarly, correlated attitudes among peers could reflect homophily, rather than social influence. Excepting randomized experiments (e.g. Green et al. 2011), which have provided little evidence of durable value change, these concerns are particularly salient because extant studies have not exploited plausibly random variation in education, and often control for variables like income that are themselves consequences of education.

3 Empirical design

The difficulty of identifying the partisan effects of schooling is widely recognized. As noted above, there are many confounding explanations for any correlation between educational attainment and policy preferences and voting behavior (Kam and Palmer 2008; Sondheimer and Green 2010). Beyond the omitted variable concern that has besieged the extant literature, another difficulty is interpreting potential heterogeneity in schooling's political effects. In particular, schooling's effects may depend upon the grade in question, e.g. because middle school, high school, and college cover different content. As suggested above, different levels of education may differentially stimulate income-based and socialization-based mechanisms. Accordingly, a failure to separate levels of schooling would return a misleading null finding if, for example, high school's conservative effects were counterbalanced by liberal effects of attending college.

To address these concerns, I exploit cross-cohort variation across states in dropout ages—as both an important policy in their own right and as an instrument for schooling—using a difference-in-differences design. Since I show that dropout ages do not affect post-secondary education, this study offers a rare opportunity to identify the causal effects of a specific level of schooling on partisanship and voting behavior among individuals induced to remain in school by dropout laws.

3.1 State school dropout age laws

State compulsory schooling laws define the minimum age at which a student must enter and may drop out of school. The first such law was adopted in Massachusetts in 1852 and 41 states had adopted similar laws by 1910, principally to meet demand for educated workers and promote assimilation (Goldin and Katz 2008). The laws have since focused on the dropout age, which gradually increased throughout the twentieth century without affecting high school graduation re-

quirements.³ While dropout law enforcement is relatively weak, states can and do punish habitual truancy (Oreopoulos 2009). Labor economists often use compulsory schooling laws as instruments to estimate the economic effects of schooling in the U.S. (e.g. Acemoglu and Angrist 2000; Angrist and Krueger 1991; Oreopoulos 2006, 2009).

[Figure 1 about here]

Figure 1 plots the minimum age at which a student can legally drop out of school across the 48 contiguous states and Washington, DC between 1920 and 1997.⁴ Unsurprisingly, there has been a general upward trend in dropout ages, particularly in the midwest and the south. However, there are also instances of reversal, as in Maine, Mississippi, and Oregon. Given that high school dropout rates remain non-trivial—consistently registered at around 20% for all students since the 1970s, with higher rates among ethnic minorities and boys (Murnane 2013)—many states have recently sought to raise their dropout age. Between 1920 and 1997, the majority (72%) of state-years specified a dropout age of 16; 6% were below 16; 11% used 17; and 11% used 18. Since the leaving age reached 16 in all states in 1993, today’s policy debate has centered on raising the leaving age to 17 or 18.

I use two indicators— $1(\text{dropout age} = 16)$ and $1(\text{dropout age} \geq 17)$ —to examine the effect of these leaving ages on a given cohort, relative to the baseline of a dropout age below 16.⁵ The former indicator primarily captures changes in dropout ages in the early and mid twentieth century, while the latter captures more recent reforms that affected today’s younger generations.

³The dropout age captures the relevant legislation for the period studied here (see Appendix section A.3.1).

⁴Alaska and Hawaii are omitted due to lack of data.

⁵I combine dropout ages of 17 and 18 because requiring students close to completing high school to remain in school until 18 has no additional effect on schooling (see Appendix Table A10).

3.2 Identification strategy

To identify the effects of raising the dropout age and completing an additional year of schooling on which party voters identify with and vote for later in life, I leverage state-level reforms in the dropout age using a generalized difference-in-differences design.

3.2.1 Reduced form: identifying the effect of increasing the state dropout age

To identify the political effects of state dropout laws, I compare differences in support for the Democratic party across affected and unaffected cohorts within states that changed their dropout age with differences across the same cohorts within states that did not. This generalized difference-in-differences design separates out common trends across cohorts and time-invariant differences across states in political support from the effect of dropout laws on cohorts in states where a reform occurred. The design entails estimating regressions of the form:

$$Y_{icst} = \delta_1 1(\text{dropout age}_{cs} = 16) + \delta_2 1(\text{dropout age}_{cs} \geq 17) + \alpha_c + \theta_s + \phi_s \text{birth year}_c + \eta_t + \varepsilon_{icst}, \quad (1)$$

where Y_{icst} is an outcome (e.g. Democrat identifier/voter) from survey year t for an individual i from cohort c in state s . The difference-in-differences strategy includes fixed effects α_c for birth-year (cohort) and fixed effects θ_s for the state in which a respondent state grew up. Cohort fixed effects control for all differences across cohorts, including generation effects reflecting political events that may have occurred at more impressionable ages—such as early adulthood—for some cohorts (see [Stoker and Bass 2011](#)). State fixed effects capture all time-invariant state characteristics. In addition, state-specific linear cohort trends, $\phi_s \text{birth year}_c$, allow for underlying trends in Y_{icst} across cohorts to vary by state. capture common shocks across electoral campaign/survey periods. Standard errors are clustered by state.

The key identifying assumption is that, absent changes in dropout laws, voters from states where a reform occurred would experience parallel trends in Democrat support to voters from states

where no reform occurred. There are two main challenges to this “parallel trends” assumption.

The first is that individuals may select into particular dropout laws, by moving in response to dropout age reforms. However, among poor women of childbearing age—whose children are more likely to remain in school only because they are required to—cross-state migration is especially low, and is concentrated before their children reach high school age (Molloy, Smith and Wozniak 2011). Furthermore, the principal reasons to move across states—for college, marriage, and family reasons, and following natural disasters (Molloy, Smith and Wozniak 2011)—are unlikely to reflect dropout age reforms. Parents willing to move to improve their child’s education are also likely to persuade their child to remain in school past the dropout age anyway. Robustness checks in Appendix section A.5.2 suggest that the reforms did not affect cross-state migration and that the selective migration required to nullify the results is unlikely.

The second concern is that state dropout age reforms correlate with unobserved trends also affecting Democrat support. However, state-specific cohort trends provide a general check against the possibility that reforms coincide with linear processes that differ across states, such as changes in school quality, that could also affect political behavior. Stephens and Yang (2014) demonstrate the importance of including such trends. Moreover, Appendix Table A3 shows that changes in dropout ages are not predicted by changes in state-level political control. Appendix section A.5.1 further demonstrates the robustness of the results to incorporating lags and leads, including time-varying control variables, leveraging only within-region variation in dropout ages across state-cohorts, and using a regression discontinuity design to compare the first cohorts affected by leaving age reforms to the last cohorts that were not affected.

3.2.2 Instrumental variables: identifying the effect of completing an additional grade of schooling

To estimate the effect of completing an additional grade of schooling—as opposed to increasing the state’s dropout age—on identifying as and voting Democrat, I use the dropout age to instrument

for schooling in the following structural equation building on the difference-in-differences design:

$$Y_{icst} = \beta S_{icst} + \alpha_c + \theta_s + \phi_s \text{birth year}_c + \eta_t + \varepsilon_{icst}, \quad (2)$$

where S_{icst} is a measure of i 's schooling. I instrument for schooling using the following first stage:

$$S_{icst} = \pi_1 1(\text{dropout age}_{cs} = 16) + \pi_2 1(\text{dropout age}_{cs} \geq 17) + \alpha_c + \theta_s + \phi_s \text{birth year}_c + \eta_t + \nu_{icst}. \quad (3)$$

IV estimates of equation (2) identify the local average causal response for dropout law compliers (Angrist and Imbens 1995), i.e. on voters that only completed an additional grade of schooling due to a change in their state's dropout age. This causal quantity weights the local average treatment effect for each additional grade by the extent to which dropout laws contribute to the first stage for each additional grade of schooling.

In addition to the parallel trends assumption needed to estimate the effect of raising the dropout age, identification further requires a first stage, that the instruments satisfy monotonicity, and an exclusion restriction maintaining that the dropout age only affects political outcomes through increased schooling. First, the results in Table 2 below confirm that increasing the dropout age significantly increases completed grades of schooling. Second, a higher leaving age is unlikely to cause an individual to choose less schooling; Appendix Figure A1 supports this monotonicity assumption. Third, because additional schooling is temporally proximate to the point at which the dropout age binds, there is limited scope for changes in the dropout age to affect downstream behavior through channels other than additional education. Appendix section A.5.3 finds little evidence to support possible plausible exclusion restriction violations.

3.3 Data

I primarily use data from the National Annenberg Election Survey (NAES). 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 each campaign, and together yield a maximum pooled sample of 164,606 respondents aged 25 or above.⁶ The sample is restricted to those aged 25 or above to ensure that most respondents have completed full-time education.⁷ Beyond its large sample size, a key advantage of the NAES over other political surveys is its wide-ranging questions, which help assess the mechanisms underpinning education's political effects. I demonstrate robustness using the American National Election Survey (ANES); this is an important out-of-sample robustness check because the ANES uses different survey protocols and covers all Congressional elections since 1952.

The American Communities Survey (ACS), which has mailed surveys and conducted telephone interviews with randomly-sampled U.S. citizens at monthly intervals since 2000, is used to measure completed grades of schooling and estimate the first stage (see below). I use ACS microdata from the 2000, 2001, 2003, 2004, 2007, and 2008 samples (Ruggles et al. 2010) to match the years when NAES respondents were surveyed.⁸

I next briefly describe the main variables. Appendix section A.1 provides detailed variable definitions and summary statistics.

3.3.1 Dependent variable: Democrat support

I examine three measures of support for the Democratic party in the NAES. The first is an indicator for Democrat partisan self-identification. In the sample, 33% of respondents identify as Democrats, while 31% identify as Republicans; the residual are independents, other partisans, or “don't know.”

⁶Interviews were conducted over the following time spans: 12/1999-1/2001, 10/2003-11/2004, and 12/2007-11/2008. Online survey responses are not used here.

⁷The results are robust to restricting to 18 or above or 30 or above (Appendix Table A15).

⁸The few NAES observations from 1999 are dropped.

Secondly, vote intention at the forthcoming presidential election is an indicator for intending to vote for the Democrat presidential candidate (Al Gore, John Kerry, or Barack Obama). Finally, I code an indicator for whether a respondent reported turning out to vote Democrat at the last presidential election (for Bill Clinton, Al Gore, or John Kerry). Democrat candidates received 41% of intended presidential votes, while 37% of respondents reported voting for the Democrat presidential candidate at the previous election.⁹ I show similar results using Republican indicators instead.

[Figure 2 about here]

Figure 2 disaggregates Democrat support by education category, showing that support is concentrated among the least and most educated. High school dropouts are around ten percentage points more likely to identify as and vote for the Democratic party (once their lower turnout rates are accounted for) than those completing high school or college.

3.3.2 Assigning state dropout ages

Survey respondents are mapped to the dropout age in their state of residence at the age at which the law binds their cohort's decision to leave school. Birth year cohort is inferred from a respondent's age when surveyed by the NAES, and year of birth in the ACS.¹⁰ 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.

The absence of state of residence when attending high school could induce bias if compliers supporting particular political parties systematically migrate to states with different dropout ages. However, based on the ANES—which measures current state of residence *and* state of residence at age 14—there is little evidence to justify this concern. First, relatively few respondents—and

⁹Weighting by each NAES sample size, the ultimate Democrat presidential vote share corresponding to the intended vote measure is 50%, and 27% for actual votes at the previous election.

¹⁰This approach yields similar first stage results to studies using month of birth (Acemoglu and Angrist 2000).

especially those likely to drop out—move across states between their teenage and adult years. Specifically, 76% of ANES respondents that did not attend college reside in the state they lived in at age 14, and 88% reside in a state that had the same school leaving age. Second, Appendix Table A7 demonstrates that the dropout age in the state where the respondent lived at age 14 does not significantly affect whether they continue living in the state. Third, Table A8 shows that the ANES dataset yields similar point estimates. Furthermore, the bounding exercise in Appendix section A.5.2 demonstrates that, even under generous assumptions, substantial selective migration is required to account for the results.

3.3.3 Completed grades of schooling

The IV estimates require a measure of the number of grades of completed schooling. The NAES only measures *completing* high school, but does not distinguish between finer levels of partial high school completion that different dropout ages could affect. While previous studies have used an indicator for completing high school, Marshall (2016) demonstrates that this is likely to significantly upwardly bias IV estimates. Intuitively, this is because coarsening years of schooling into a dichotomous variable for completing high school causes the first stage to only capture the effect of the instrument on completing high school, neglecting any increases in schooling induced by raising the dropout age that did not entail completing high school. Consequently, the exclusion restriction is violated if any other level of schooling induced by changes in the dropout age affects identifying as or voting Democrat.

However, an interval measure of schooling, like the number of completed grades of schooling, allows for consistent estimation of the local average causal response, provided—as in this case—that multiple grades are affected by the instrument (Marshall 2016). Since the number of grades is not measured in the NAES (or ANES), I combine the NAES and ACS samples using two-sample IV methods. The ACS’s fine-grained education variable measures the number of completed grades of schooling. 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 grades. Table 2 below shows that dropout ages do not affect schooling beyond 12th grade.

3.4 Two-sample IV estimation

For the IV estimates, I use two-sample 2SLS (TS2SLS) to estimate equation (2). Because the IV estimator is approximately the reduced form estimate divided by the first stage estimate (e.g. Angrist and Krueger 1992), TS2SLS separately estimates these components using different samples before combining them as a consistent two-step estimator of the effect of an additional completed grade of schooling (Inoue and Solon 2010). This entails estimating the reduced form—equation (1)—in the NAES dataset and the first stage—equation (3)—in the ACS dataset. Intuitively, the first stage estimates from the ACS proxy for what the NAES first stage would have been had schooling been measured in the NAES. I demonstrate the consistency of the TS2SLS estimator and derive its cluster-robust covariance matrix in Appendix section A.4.2. The reduced form estimates use only the NAES data.

Beyond the standard IV assumptions discussed above, TS2SLS requires that the reduced form and first stage datasets independently sample from the same population (Inoue and Solon 2010). This ensures that key sample moments—variable means, variances, and cross-products—are identical in expectation, and are thus “exchangeable.” Exchangeability implies that the same first stage would have been estimated in the NAES dataset had completed grades of schooling been measured.

This assumption is plausible in this context. Both the ACS and NAES randomly sample voting age citizens, once ineligible voters from the NAES sample and those aged below 25 and born outside the U.S. are removed from the ACS sample. To address potential differences in response rates, I ensure that the ACS sample moments match the distribution of NAES respondents by randomly sampling ACS observations stratified by state-cohort, survey year, gender, and race in proportion with the NAES distribution of these predetermined covariates. Details of this procedure,

which yielded a sample of 380,685 observations, are provided in Appendix section A.4.3.¹¹ The summary statistics in Table A1 demonstrate that this approach almost exactly replicates the first two moments for each variable.

4 Downstream partisan effects of dropout ages and schooling

I now present the main findings that higher dropout ages and an additional grade of high school substantially decrease the probability that an individual identifies with, or votes for, the Democrats later in life.

4.1 Reduced form estimates

I first estimate the effect of raising the school dropout age. Columns (1)-(3) in panel A of Table 1 report the reduced form generalized difference-in-differences estimates, where a negative coefficient indicates a decrease in support for the Democratic party. Columns (4)-(6) report analogous estimates for Republican party support.

[Table 1 about here]

The results reveal that raising the dropout age decreases a voter's propensity to identify as or vote Democrat. The first coefficient in each column indicates that, compared to cohorts in states requiring students to remain in school until at most age 15, raising the dropout age to 16 decreases the proportion of Democrat partisans by 1.8 percentage points, those intending to vote Democrat by 3.1 percentage points, and those actually voting Democrat by 1.4 percentage points. Although only the decline in vote intention is statistically significant at the 95% level, these estimates imply around a 5% reduction in Democrat support relative to their sample means.

¹¹Appendix Figures A3 and A4 show that the particular random sample drawn for estimating the first stage is not an outlier.

To examine the more topical effect of further increasing the dropout age to 17 or above, the first coefficient in each column can be subtracted from the second. These comparisons at the foot of Table 1 show that, relative to cohorts in states requiring students to remain in school until age 16, further raising the dropout age to 17 or higher decreased the proportion of Democrat partisans by 1.5 percentage points, those intending to vote Democrat by 1.3 percentage points, and those that actually reported voting Democrat by 1.9 percentage points. Each difference is statistically significant at the 95% level. These results imply reductions in Democrat support of around 4% per cohort. The slightly larger effect at 16 likely reflects the comparison with the omitted category containing respondents primarily facing a dropout age of 14. The regression discontinuity estimates in Appendix Table A6 report slightly larger effects among those first affected by the reforms.

Columns (4)-(6) show commensurate increases in Republican support. This suggests that increasing the dropout age caused as many would-be Democrat supporters to become Republican supporters. Unfortunately, it is not possible to distinguish whether individuals ceasing to be Democrats become Republicans, or whether Democrats become independents and independents become Republicans.

4.2 Instrumental variables estimates

The IV results in Table 2 estimate the effect of completing an additional grade of schooling. In contrast with the reduced form estimates, which aggregate across state-cohorts, these estimates apply only to individuals induced to remain in school because of an increase in the dropout age affecting their state-cohort.

[Table 2 about here]

The first stage estimates from the ACS sample in column (1) show that raising the dropout age significantly increases schooling. Relative to state-cohorts facing a dropout age below 16, raising the leaving age to 16 increases the average number of completed grades of schooling by 0.18.

Further raising the dropout age to at least 17 keeps students in school for an additional 0.04 grades ($p = 0.11$). These estimates are similar in magnitude to previous estimates pertaining to cohorts born earlier in the twentieth century (Acemoglu and Angrist 2000; Oreopoulos 2006; Stephens and Yang 2014). The F statistic at the foot of column (1) indicates that the instruments are relatively strong.

While column (1) demonstrated that raising the dropout age increases completed grades of schooling, it could also affect post-secondary education. However, column (2) shows that, on average, increasing the dropout age does not increase the probability that an individual completes schooling beyond 12th grade.¹² This implies that the estimates in this article only apply to completing additional grades of pre-college schooling, and thus abstract from any potentially counter-vailing effects of attending college.

Reinforcing the reduced form findings, the TS2SLS estimates suggest that schooling has a large anti-Democrat effect among those induced to remain in high school by a higher dropout age. Columns (3)-(5) indicate that an additional grade of high school decreases the probability of identifying as a Democrat by 14 percentage points and intended and reported voting for the Democratic presidential candidate by 15-20 percentage points. The decrease in Democratic vote intention is statistically significant at the 95% level, while partisanship ($p = 0.14$) and reported voting ($p = 0.07$) fall just outside this standard. Combined with the reduced form results, these findings indicate that high school education has important downstream effects on voters among compliers with low education levels.

5 Possible mechanisms

Having shown that dropout laws inducing students to remain in high school decrease support for the Democratic party, I next use the same generalized difference-in-differences and IV methods to ex-

¹²Acemoglu and Angrist (2000) also find that college attendance was unaffected.

plore whether the dropout age and additional completed grades of schooling affect the mechanisms underpinning the income and socialization-based theories considered above. The results most strongly support the Meltzer-Richard model, suggesting that—by increasing income—schooling induces conservative economic policy preferences, which in turn decrease the likelihood that voters support the Democrats.

5.1 Evidence supporting an income channel

Beyond the well-established finding that an additional year of schooling induced by higher dropout ages increases wages at peak working age by around 10% (Acemoglu and Angrist 2000; Angrist and Krueger 1991; Oreopoulos 2006, 2009), I provide three further pieces of evidence consistent with the income-based Meltzer-Richard channel.

[Table 3 about here]

First, if education is driving lower support for the Democrats by increasing an individual's income, then this effect should be most pronounced when the greatest return to education is realized—at an individual's earnings peak. I test this implication by comparing the effects of the dropout age on the 44% of respondents aged 45-65 at the date of the survey to all other respondents.¹³ The ACS sample shows that wages are highest between these ages. The results in panel A of Table 3 demonstrate that, at an individual's earnings peak, the negative effects of raising both dropout age levels on identifying as and voting Democrat are significantly greater. This suggests that realizing the returns to education may be a key component of schooling's political effect, although the estimates still indicate smaller effects outside the peak earnings period. Panel B shows similar results when simultaneously controlling for the interaction of the earnings peak indicator with all other covariates. These findings imply that, even though voter policy preferences are fairly

¹³Income is post-treatment, so cannot be used directly.

sticky from early adulthood (Erikson and Tedin 2015; Stoker and Bass 2011), they can nevertheless respond to changes over a voter's life-cycle that correspond with their economic incentives.

Second, I use survey attitudes to examine whether additional grades of schooling increase opposition to taxation—the key driver of the Meltzer-Richard model. The IV estimate in column (1) of Table 4 shows that educated respondents regard themselves as significantly more conservative when asked to place themselves on a (standardized) five-point liberal-to-conservative scale.¹⁴ An additional grade of high school equates to a 0.29 standard deviation increase in conservative preferences. To better capture *economic* policy preferences, column (2) shows that an additional grade of high school increases the belief that taxes should be reduced by 18 percentage points. This belief strongly correlates with identifying with or voting for the Republican party,¹⁵ and is thus consistent with the argument that high school causes voters to become more fiscally conservative, and ultimately reject the Democrats.

[Table 4 about here]

Third, if voters adopt the policy positions of the political party or candidate that they identify with (e.g. Lenz 2012), changes in economic policy preferences could instead reflect changes in partisanship. If voters reflect their party's positions, they should also shift toward prominent Republican stances on other issues. However, columns (3)-(6) of Table 4 provide no evidence of such changes: an additional completed grade of schooling does not significantly increase the likelihood that respondents oppose abortion, gun control, health spending, or cutting military spending.¹⁶ The effects of schooling on these issues are 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.

¹⁴Appendix Table A13 reports similar reduced form estimates.

¹⁵Although this link from potential mediator to outcome cannot be well-identified, specifications akin to equation (1) suggestively show that voters believing that taxes should be reduced are more than 10 percentage points more likely to be Republican partisans or voters for Republican Presidential candidates.

¹⁶These positions are highly correlated with identifying as and voting Republican in the NAES.

Combined as a scale, column (7) shows that schooling does not shift respondents toward Republican positions on these non-economic policy issues. These results align with Gerber, Huber and Washington (2010), who find that experimentally inducing a shift in the party that a voter identifies with does not affect their policy positions. The results also challenge explanations where voters adopt the views of generally Republican social networks that education may induce them to enter. Together, these tests suggest that economic policy preferences drive the political choices of those receiving more high school education.

5.2 Little evidence of political socialization

In contrast, the socialization theories reviewed above argue that schooling cultivates political engagement and induces voters to adopt socially liberal values. Although such changes are typically associated with *supporting the Democrats*, it remains possible that socialization mechanisms operate alongside income channels or instead drive Republican support. However, in addition to the earnings peak results suggesting that education's effects do not principally reflect immediate classroom socialization, I find no evidence that schooling activates the basic mechanisms underpinning these theories on average.

The civic education and social network channels emphasize the importance of changes in political engagement and political attitudes. However, consistent with existing research (Galston 2001), columns (8) and (9) of Table 4 show that an additional completed grade of schooling does not significantly affect—and, if anything, reduces—multi-item scales of political knowledge or political interest. Furthermore, columns (10)-(12) report no effect on discussion of politics with friends or family, self-reported turnout in the last presidential election, or trust in the federal government. These findings suggest that the average voter induced to remain in high school by higher dropout ages are neither induced to engage and participate in politics more,¹⁷ nor less likely to

¹⁷Sondheimer and Green's (2010) positive findings on turnout may reflect pre-high school extra-curricula education or differences in educational quality particular to their experiments.

trust government institutions that required that they remain in high school longer.

Moreover, policy and issue preferences do not change as socialization explanations predict. As noted above, there is no evidence to suggest that greater schooling is associated with socially liberal positions on gun control or abortion. Column (13) further shows that completing an additional grade of high school does not significantly increase support for environmental protection. In combination with reduced support for the Democrats, there is thus little evidence to suggest that voters become more socially liberal.

6 Discussion of implications

The preceding analysis demonstrates that the additional high school education induced by raising the school leaving age significantly decreases voters' downstream probability of voting Democrat. To investigate the broader implications of this finding, I first consider which types of voters change their preferences and behavior. Appendix Table A11 examines differences in the first stage estimates by subgroup, showing that male and especially black or Hispanic students—who register the lowest propensities to complete high school—have been affected most by the reforms.

The large effects observed among these compliers are important to recognize, from both an academic and policy perspective, not least because disadvantaged minorities represent relatively large and—as their population share grows—increasingly important electoral groups in their own right. Although support for the Democrats remains high, particularly among African-Americans, it does not appear to be impregnable in the face of income-based incentives—at least in the twenty-first century context of weak labor movements (when the NAES surveys were conducted). This may be especially true among Hispanic voters and white working class men, whose support for the Democrats has become more marginal. Moreover, levers to affect high school remain important because nearly 20% of students still fail to graduate from high school (Murnane 2013).

Nevertheless, do the local effects identified here generalize to other subpopulations? Beyond

being the most affected, compliers from disadvantaged backgrounds could also be the most likely to benefit from the reforms. Conversely, since education raises the income of all types of workers, students from more advantaged backgrounds may respond similarly (e.g. Mendelberg, McCabe and Thal 2017). Providing some support for the latter perspective, the IV estimates in Appendix Table A12 suggest that black or Hispanic and male compliers are no more likely to cease supporting the Democrats, once induced to remain in school. This suggests that the effects may generalize to other types of students that could also be encouraged to remain in school by states that have yet to raise their leaving age to 17 or 18. It is harder to generalize to students that would always complete high school. However, the income-based mechanisms driving the effects could plausibly apply to all types of voters, while my estimates suggest that the effects of one or two years of schooling broadly align with the descriptive comparisons documented in Figure 2 for the entire sample.

Another approach to assessing the political importance of raising the dropout age is its downstream electoral consequences. Once the number of affected cohorts accumulates to reach a critical mass, the voting behavior induced by the reforms could change state-level electoral outcomes. Appendix Table A16 provides initial evidence of such changes, showing that raising the dropout age to 16 reduced the Democratic seat share in state Houses and Senates 45 years after the policy was enacted—the point when Democratic support is weakest among the first cohorts to be affected by the reform—by around four percentage points. These estimates are tentative because it is difficult to operationalize lag times between cause and effect, which also reduce the set of reforms that can be analyzed. Still, the estimates highlight the potential implications of schooling reforms for electoral outcomes, and suggest that future research could extend this analysis to examine potential increases in fiscally conservative policy outcomes. Non-economic policies could also change if Republicans elected for their fiscally conservative positions also pursue other policy changes.

In the context of recent efforts—particularly from Democrats—to increase dropout ages, my findings also suggest a “catch 22” for a long-sighted Democratic party. On one hand, increasing access to education is often a plank of the party’s state-level policy agendas, while a broader

goal of the party is to increase opportunities for disadvantaged minorities. In the short-term, the party may be rewarded electorally for acting on such objectives (e.g. [Levitt and Snyder 1997](#)). On the other hand, increasing access to education comes at the long-run cost of the beneficiaries voting Republican in the future. Similarly, the results suggest that Republican opposition to raising dropout ages—e.g. in South Carolina in early 2016—may neglect its potential long-run strategic benefits. In this respect, this phenomenon resembles other prescient temporal political trade-offs in U.S. politics, such as reducing taxes in the short-term while retaining a longer-term reputation for fiscal prudence (e.g. for Republicans working with President Trump) or the flexibility to raise spending (e.g. for Democrats following President George W. Bush’s income tax cuts), or selecting extremist primary candidates that appeal to a party’s base but not the general electorate. Although this article’s focus is not the political economy of reform, these considerations suggest interesting avenues for future research.

7 Conclusion

Despite education’s key role in determining students’ life trajectories, this is the first study to identify whether and how high school education affects downstream political preferences and voting behavior. My findings give schooling a clear partisan tint by showing that it causes voters to become more conservative on redistributive issues, and ultimately vote Republican. The results contrast with previous studies highlighting the stickiness of partisan preferences: I find that raising the minimum dropout age by a year decreases Democrat partisan identification and voting by 1-3 percentage points per cohort, while completing an additional grade of schooling decreases the probability that a voter identifies with or votes for the Democratic party later in life by around 15 percentage points. These magnitudes suggest that dropout laws may have meaningfully altered electoral and policy outcomes, particularly at the state level.

Both income and socialization theories could explain high school education’s conservative ef-

fects. However, an explanation combining human capital theory with the Meltzer-Richard model of income-based policy preferences receives greater empirical support. 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 systematic evidence suggesting that additional schooling affects political engagement or socially liberal values.

Because state dropout ages do not significantly increase post-secondary education, this article's estimates speak to high school education. Another question is then whether the anti-Democrat effects of high school—which may reflect a broader class of “catch 22” policies generating intertemporal economic and political trade-offs for progressive parties—extend to college education. On one hand, college education could more effectively instill socially liberal values, whether through curricula or social networks, than high school. On the other, [Mendelberg, McCabe and Thal \(2017\)](#)—who focus on student attitudes, rather than downstream voting behavior—suggest that colleges with a culture of financial gain could reinforce the income incentives suggested above by cultivating aspirations for financial gain. Moreover, any socialization effects might dissipate after leaving college. Systematic evidence separating these mechanisms represents an important avenue for future research.

References

- Acemoglu, Daron and Joshua D. Angrist. 2000. "How Large Are Human Capital Externalities? Evidence from Compulsory Schooling Laws." *NBER Macroeconomics Annual* 15:9–59.
- Alesina, Alberto and Eliana La Ferrara. 2005. "Preferences for Redistribution in the Land of Opportunities." *Journal of Public Economics* 89(5):897–931.
- Angrist, Joshua D. and Alan B. Krueger. 1991. "Does Compulsory School Attendance Affect Schooling and Earnings?" *Quarterly Journal of Economics* 106(4):979–1014.
- Angrist, Joshua D. and Alan B. Krueger. 1992. "The Effect of Age at School Entry on Educational Attainment: An Application of Instrumental Variables with Moments from Two Samples." *Journal of the American Statistical Association* 87(418):328–336.
- Angrist, Joshua D. and Guido W. Imbens. 1995. "Two-Stage Least Squares Estimation of Average Causal Effects in Models With Variable Treatment Intensity." *Journal of the American Statistical Association* 90(430):431–442.
- Ashenfelter, Orley and Cecilia Rouse. 1998. "Income, schooling, and ability: Evidence from a new sample of identical twins." *Quarterly Journal of Economics* 113(1):253–284.
- Bartels, Larry M. 2010. The Study of Electoral Behavior. In *The Oxford Handbook of American Elections and Political Behavior*, ed. Jan E. Leighley. Oxford University Press pp. 239–261.
- Campbell, Angus, Philip E. Converse, Warren E. Miller and Donald E. Stokes. 1960. *The American Voter*. Wiley.
- Dee, Thomas S. 2004. "Are there civic returns to education?" *Journal of Public Economics* 88(9):1697–1720.

- Erikson, Robert S. and Kent L. Tedin. 2015. *American Public Opinion (9th Edition)*. Pearson.
- Franklin, Charles H. 1989. "Estimation across data sets: two-stage auxiliary instrumental variables estimation (2SAIV)." *Political Analysis* 1(1):1–23.
- Galston, William A. 2001. "Political knowledge, political engagement, and civic education." *Annual Review of Political Science* 4(1):217–234.
- Gelman, Andrew. 2010. *Red State, Blue State, Rich State, Poor State: Why Americans Vote the Way They Do*. Princeton University Press.
- Gerber, Alan S., Gregory A. Huber and Ebonya Washington. 2010. "Party affiliation, partisanship, and political beliefs: A field experiment." *American Political Science Review* 104(4):720–744.
- Goldin, Claudia D. and Lawrence F. Katz. 2008. *The Race Between Education and Technology*. Harvard University Press.
- Green, Donald P., Peter M. Aronow, Daniel E. Bergan, Pamela Greene, Celia Paris and Beth I. Weinberger. 2011. "Does knowledge of constitutional principles increase support for civil liberties? Results from a randomized field experiment." *Journal of Politics* 73(2):463–476.
- Hainmueller, Jens and Michael J. Hiscox. 2010. "Attitudes toward highly skilled and low-skilled immigration: Evidence from a survey experiment." *American Political Science Review* 104(1):61–84.
- Hillygus, D. Sunshine. 2005. "The missing link: Exploring the relationship between higher education and political engagement." *Political Behavior* 27(1):25–47.
- Huckfeldt, R. Robert and John Sprague. 1995. *Citizens, politics and social communication: Information and influence in an election campaign*. Cambridge University Press.

- Inoue, Atsushi and Gary Solon. 2010. "Two-Sample Instrumental Variables Estimators." *Review of Economics and Statistics* 92(3):557–561.
- Jennings, M. Kent, Laura Stoker and Jake Bowers. 2009. "Politics across generations: Family transmission reexamined." *Journal of Politics* 71(3):782–799.
- Kam, Cindy D. and Carl L. Palmer. 2008. "Reconsidering the Effects of Education on Political Participation." *Journal of Politics* 70(3):612–631.
- Lazarsfeld, Paul F., Bernard Berelson and Hazel Gaudet. 1948. *The People's Choice. How the Voter Makes up His Mind in a Presidential Campaign*. Columbia University Press.
- Lenz, Gabriel S. 2012. *Follow the Leader? How Voters Respond to Politicians' Policies and Performance*. University of Chicago Press.
- Levitt, Steven D. and James M. Snyder, Jr. 1997. "The impact of federal spending on House election outcomes." *Journal of Political Economy* 105(1):30–53.
- Marshall, John. 2016. "Coarsening bias: How instrumenting for coarsened treatments upwardly biases instrumental variable estimates." *Political Analysis* 24(2):157–171.
- McCarty, Nolan, Keith T. Poole and Howard Rosenthal. 2006. *Polarized America: The Dance of Ideology and Unequal Riches*. MIT Press.
- Meltzer, Allan H. and Scott F. Richard. 1981. "A rational theory of the size of government." *Journal of Political Economy* 89(5):914–927.
- Mendelberg, Tali, Katherine T. McCabe and Adam Thal. 2017. "College Socialization and the Economic Views of Affluent Americans." *American Journal of Political Science* 61(3):606–623.
- Meredith, Marc. 2009. "Persistence in Political Participation." *Quarterly Journal of Political Science* 4(3):187–209.

- Molloy, Raven, Christopher L. Smith and Abigail Wozniak. 2011. "Internal Migration in the United States." *Journal of Economic Perspectives* 25(3):173–196.
- Murnane, Richard J. 2013. "U.S. High School Graduation Rates: Patterns and Explanations." *Journal of Economic Literature* 51(2):370–422.
- Murphy, Kevin M. and Robert H. Topel. 1985. "Estimation and Inference in Two-Step Econometric Models." *Journal of Business and Economic Statistics* 20(1):88–97.
- Nie, Norman H., Jane Junn and Kenneth Stehlik-Barry. 1996. *Education and Democratic Citizenship in America*. University of Chicago Press.
- Niemi, Richard G. and Jane Junn. 1998. *Civic Education: What Makes Students Learn*. Yale University Press.
- Oreopoulos, Philip. 2006. "Estimating Average and Local Average Treatment Effects of Education when Compulsory Schooling Laws Really Matter." *American Economic Review* 96(1):152–175.
- Oreopoulos, Philip. 2009. Would More Compulsory Schooling Help Disadvantaged Youth? Evidence from Recent Changes to School-Leaving Laws. In *The Problems of Disadvantaged Youth: An Economic Perspective*, ed. Jonathan Gruber. University of Chicago Press pp. 85–112.
- Ruggles, Steven, J. Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B. Schroeder and Matthew Sobek. 2010. "Integrated Public Use Microdata Series: Version 5.0."
- Sondheimer, Rachel M. and Donald P. Green. 2010. "Using Experiments to Estimate the Effects of Education on Voter Turnout." *American Journal of Political Science* 41(1):178–189.
- Spence, Michael. 1973. "Job market signaling." *Quarterly Journal of Economics* 87(3):355–374.
- Stephens, Jr, Melvin and Dou-Yan Yang. 2014. "Compulsory Education and the Benefits of Schooling." *American Economic Review* 104(6):1777–1792.

Stoker, Laura and Jackie Bass. 2011. Political Socialization: Ongoing Questions and New Directions. In *The Oxford Handbook of American Public Opinion and the Media*, ed. George C. Edwards, III, Lawrence R. Jacobs and Robert Y. Shapiro. Oxford University Press pp. 453–465.

Verba, Sidney, Kay Lehman Schlozman and Henry E. Brady. 1995. *Voice and Equality: Civic Voluntarism in American Politics*. Harvard University Press.

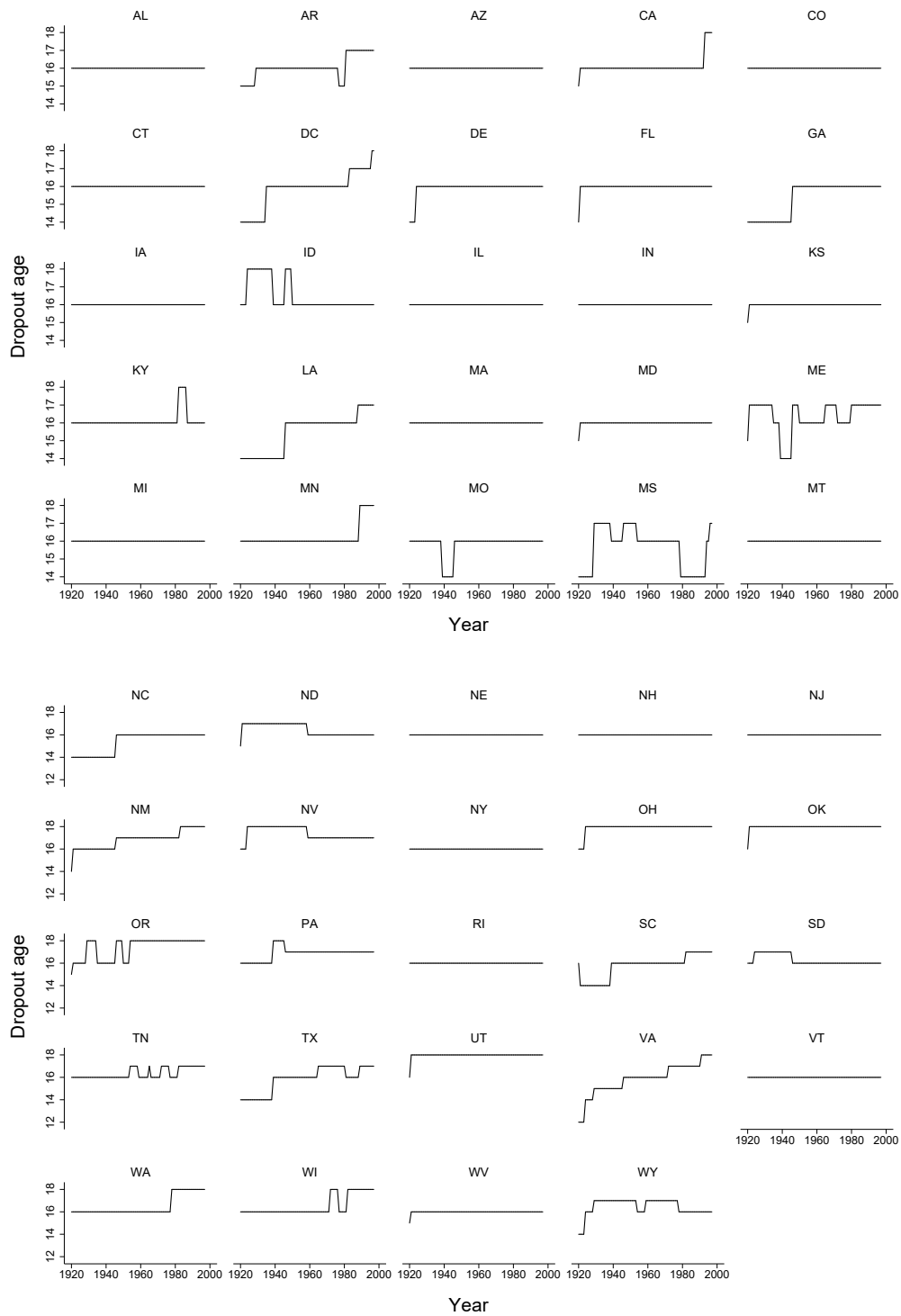


Figure 1: U.S. state minimum school leaving age laws, 1920-1997

Note: Dropout law data are from Oreopoulos (2009), and based on the National Center for Education Statistic's *Education Digest*.

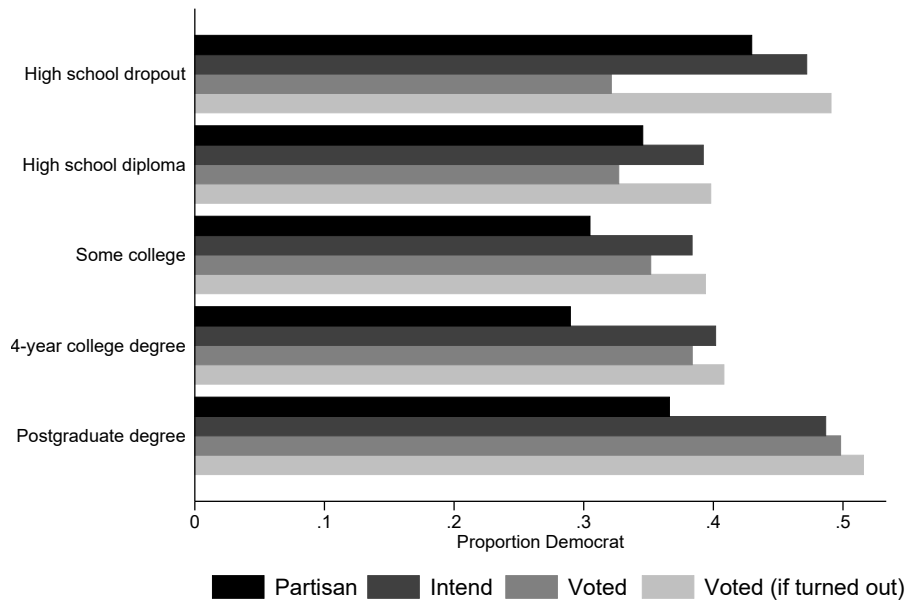


Figure 2: Democrat support by education level

Table 1: The effect of school dropout age on identifying as and voting Democrat and Republican

	Democrat support			Republican support		
	Partisan (1)	Intend (2)	Voted (3)	Partisan (4)	Intend (5)	Voted (6)
Dropout age=16	-0.017 (0.016)	-0.031* (0.015)	-0.014 (0.013)	0.011 (0.014)	0.039* (0.014)	0.028 (0.017)
Dropout age \geq 17	-0.032 (0.018)	-0.044* (0.017)	-0.033* (0.014)	0.030 (0.019)	0.054* (0.016)	0.040* (0.018)
Observations	164,606	134,504	117,733	164,606	134,504	117,733
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.33	0.41	0.37	0.31	0.46	0.47
Difference: Dropout age \geq 17 - Dropout age=16	-0.015* (0.006)	-0.013* (0.005)	-0.019* (0.007)	0.018* (0.008)	0.014 (0.007)	0.012 (0.006)

Notes: Outcome variables are indicators for: identifying as a Democrat/Republican partisan (“partisan”), intending to vote Democrat/Republican for president (“intend”), and voting Democrat/Republican in the previous presidential election (“voted”). All specifications include state grew up, cohort, and survey year fixed effects and state-specific cohort trends, and are estimated using OLS. Differences in the number of observations reflect differences in the NAES modules in which each outcome was included. The omitted dropout age category is dropout age \leq 15. Standard errors clustered by state are in parentheses. * $p < 0.05$.

Table 2: The effect of an additional completed grade of high school on identifying as and voting Democrat

	Completed	Beyond	Democrat support		
	grades	12th grade	Partisan	Intend	Voted
	OLS	OLS	TS2SLS	TS2SLS	TS2SLS
	(1)	(2)	(3)	(4)	(5)
Dropout age=16	0.175*	-0.018			
	(0.044)	(0.014)			
Dropout age \geq 17	0.214*	-0.010			
	(0.048)	(0.015)			
Completed grades			-0.143	-0.195*	-0.148
			(0.096)	(0.091)	(0.080)
First stage (ACS) observations	380,685	380,685	380,685	374,306	380,685
Reduced form (NAES) observations			164,606	134,461	117,733
Outcome range	[0,12]	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	11.64	0.52	0.33	0.41	0.37
First stage F statistic	10.1	2.6	10.1	9.9	10.1
Difference: Dropout age \geq 17	0.038	0.008			
- Dropout age=16	(0.024)	(0.004)			

Notes: All specifications include state grew up, cohort, and survey year fixed effects and state-specific cohort trends. The specifications in columns (1) and (2) are estimated using OLS, and the specifications in columns (3)-(5) are estimated using TS2SLS. Differences in the number of reduced form observations reflect differences in the NAES modules in which each outcome was included. First stage and reduced form observations decline in column (4) because the ACS sample is reduced to match the NAES survey years where vote intention was elicited; first stage estimates are reported in Appendix section A.6.5. The omitted dropout age category is dropout age \leq 15. Standard errors clustered by state are in parentheses. * $p < 0.05$.

Table 3: Heterogeneous effects of dropout ages on Republican support by earnings peak

	Democrat support		
	Partisan (1)	Intend (2)	Voted (3)
Panel A: Baseline estimates			
Dropout age=16	-0.016 (0.016)	-0.031* (0.014)	-0.011 (0.011)
Dropout age=16 × Earnings peak	-0.114* (0.016)	-0.066* (0.016)	-0.193* (0.013)
Dropout age \geq 17	-0.032 (0.017)	-0.042* (0.016)	-0.029* (0.012)
Dropout age \geq 17 × Earnings peak	-0.110* (0.015)	-0.068* (0.016)	-0.194* (0.014)
Panel B: Including earnings peak interactive controls			
Dropout age=16	-0.008 (0.018)	-0.015 (0.017)	-0.007 (0.014)
Dropout age=16 × Earnings peak	-0.125* (0.022)	-0.110* (0.025)	-0.231* (0.017)
Dropout age \geq 17	-0.016 (0.019)	-0.016 (0.018)	-0.006 (0.017)
Dropout age \geq 17 × Earnings peak	-0.148* (0.023)	-0.144* (0.028)	-0.263* (0.021)
Observations	164,606	134,504	117,733
Earnings peak mean	0.44	0.44	0.46

Notes: All specifications include state grew up, cohort, and survey year fixed effects and state-specific cohort trends, and are estimated using OLS. Differences in the number of observations reflect differences in the NAES modules in which each outcome was included. Earnings peak is an indicator for respondents aged between 45 and 65; the lower-order term is omitted. The specifications in panel B interact all covariates with the earnings peak indicator. The omitted dropout age category is dropout age \leq 15. Standard errors clustered by state are in parentheses. * $p < 0.05$.

Table 4: The effect of an additional completed grade of high school on potential income and socialization mechanisms

	Conservative scale (1)	Reduce taxes (2)	Ban abortion (3)	Low gun controls (4)	Low health spending (5)	High military spending (6)	Republican non-economic issues (7)
Completed grades	0.293* (0.132)	0.175* (0.087)	-0.079 (0.078)	-0.040 (0.049)	0.131 (0.107)	-0.078 (0.126)	-0.168 (0.147)
Reduced form (NAES) observations	160,593	127,479	123,350	74,368	63,134	71,693	152,068
First stage (ACS) observations	380,685	378,454	378,454	265,041	265,041	265,041	378,454
Outcome range	[-2.17,1.75]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	[-1.17,2.45]
Outcome mean	-0.00	0.36	0.20	0.38	0.31	0.49	0.00
First stage F statistic	10.2	9.8	9.8	11.9	11.9	11.9	9.8
	Political knowledge scale (8)	Political interest scale (9)	Discuss politics (10)	Turnout (pres. election) (11)	Trust federal govt. (12)	Protect enviro. more (13)	
Completed grades	-0.123 (0.156)	-0.158 (0.146)	-0.256 (0.133)	0.036 (0.036)	-0.009 (0.088)	-0.078 (0.043)	
Reduced form observations	116,233	165,370	157,454	140,316	50,440	116,050	
First stage (ACS) observations	356,058	380,685	380,685	378,454	376,537	267,272	
Outcome range	[-1.21,1.33]	[-2.27,2.30]	[-1.19,1.53]	{0,1}	{0,1}	{0,1}	
Outcome mean	-0.00	0.00	-0.00	0.86	0.25	0.13	
First stage F statistic	9.6	10.2	10.2	9.8	10.5	12.8	

Notes: All non-binary outcome variables are standardized; see Appendix section A.1 for detailed variable definitions. All specifications include state grew up, cohort, and survey year fixed effects and state-specific cohort trends, and are estimated using TS2SLS. Differences in the number of reduced form observations reflect differences in the NAES modules in which each outcome was included. Differences in the number of first stage observations reflect adjustments to the ACS sample to account for survey years, cohorts, and states where the NAES survey outcome was not elicited; first stage estimates are reported in Table A14. Standard errors clustered by state in parentheses. * $p < 0.05$.

A Online Appendix

Contents

A.1	Detailed variable definitions	A2
A.2	ANES data for robustness checks	A7
A.3	Additional information about state schooling laws	A9
A.3.1	Other aspects of schooling laws	A9
A.3.2	Political control and dropout age reforms	A10
A.4	Technical details for IV estimation	A10
A.4.1	Support for the monotonicity assumption	A10
A.4.2	Two-sample 2SLS estimation	A11
A.4.3	Procedure for matching NAES and ACS sample moments	A15
A.5	Robustness to potential violations of the identifying assumptions	A16
A.5.1	Parallel trends	A19
A.5.2	Sensitivity to cross-state migration	A22
A.5.3	Exclusion restriction	A27
A.6	Additional tests referenced in the main article	A28
A.6.1	Separating leaving ages of 17 and 18	A28
A.6.2	Heterogeneous effects of first stage	A28
A.6.3	Heterogeneous instrumental variable estimates	A31
A.6.4	Reduced form mechanism estimates	A31
A.6.5	Additional first stage estimates	A33
A.6.6	Alternative restrictions on minimum age in the sample	A33
A.7	Implications of raising the dropout age for downstream electoral outcomes	A33

A.1 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. NAES interviews were conducted over the following time spans: 12/1999-1/2001, 10/2003-11/2004, and 12/2007-11/2008. Table A1 provides summary statistics.

- *(Democrat) Partisan*. Indicator coded 1 for the respondent identifying as a Democrat. Residual category is independents, Republican 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 Democrat for president)*. Indicator coded 1 for respondents intending to vote for the Democrat 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.
- *Voted (Democrat for president)*. Indicator coded 1 for respondents reporting that they voted for the Democrat presidential candidate at the previous election (Bill Clinton in 1996, Al Gore in 2000, John Kerry in 2004, or Barack Obama 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 “sex”.
- *White*. Indicator coded 1 for identifying their race as white (including Hispanic, because this is how the ACS is coded). 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 “race”.
- *Black*. Indicator coded 1 for respondents identifying their race as black. Coded from NAES variable “cW03” in wave 1, “cWA04” in wave 2, and “WC03” in wave 3; ACS variable “race”.
- *Asian*. Indicator coded 1 for respondents identifying their race as South or East Asian (including Chinese, Japanese, and Other Asian or Pacific Islanders in the ACS)). Coded from NAES variable “cW03” in wave 1, “cWA04” in wave 2, and “WC03” in wave 3; ACS variables “race” and “hispan”.

- *Black or Hispanic*. Indicator coded 1 for respondents identifying their race as black or their ethnicity as Hispanic. Coded from NAES variable “cW03” in wave 1, “cWA04” in wave 2, and “WC03” in wave 3; ACS variables “race” and “hispan”.
- *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 “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”.
- *Dropout ages*. See definition in the main article. State-year dropout age 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 “educ”.
- *Beyond 12th grade*. Indicator coded 1 for respondents registering any education beyond the high school level. Coded from ACS variable name “educ”.
- *State income per capita at age 14/16/18*. State income per capita at respondent age of 14/16/18.
- *Republican presidential vote share at age 14/16/18*. Republican presidential vote share in the most recent prior presidential election at age 14/16/18.
- *Pupil-teacher ratio at age 14/16/18*. State-level pupil-teacher ratio at age 14/16/18, interpolated for missing cases where data is available in later and earlier years.
- *Black proportion of state population at age 14/16/18*. Black proportion of the state population age 14/16/18, interpolated from Census data.

- *Conservative scale.* Standardized (and reversed) variable derived from the question asking respondents to rate themselves on a scale from 1 (very conservative) to 5 (very liberal); “don’t know” was excluded. Coded from NAES variable “cV04” in wave 1, “cMA06” in wave 2, and “MA04” in wave 3.
- *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.
- *Turnout (pres. election).* Indicator coded 1 for respondents that reported turning out in the most recent presidential election.
- *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.
- *Family size.* Number of own-family members in the respondent’s household. ACS variable name “famsize”.
- *Age of eldest child.* Age of oldest child in the respondent’s household. ACS variable name “eldch”.
- *Age of youngest child.* Age of youngest child in the respondent’s household. ACS variable name “yngch”.
- *Never married.* Indicator coded 1 for respondents that have never been married. ACS variable name “marst”.

Table A1: Summary statistics

	<i>National Annenberg Electoral Study</i>				<i>American Communities Survey</i>			
	Obs.	Mean	Std. dev.	Min. Max.	Obs.	Mean	Std. dev.	Min. Max.
<i>Dependent variables</i>								
Democrat partisan	164,606	0.33	0.47	0 1				
Intend to vote Democrat for president	134,504	0.41	0.49	0 1				
Voted Democrat for president at last election	117,733	0.37	0.48	0 1				
<i>Education (endogenous variables)</i>								
Completed grades					380,685	11.64	1.34	0 12
Beyond 12th grade					380,685	0.52	0.50	0 1
<i>Excluded instruments</i>								
Dropout age=16	165,397	0.74	0.44	0 1	380,685	0.73	0.44	0 1
Dropout age≥17	165,397	0.24	0.43	0 1	380,685	0.25	0.43	0 1
<i>Predetermined control variables</i>								
Age	165,397	51.36	15.33	25 97	380,685	50.86	15.15	25 95
Male	165,397	0.44	0.50	0 1	380,685	0.44	0.50	0 1
White	165,397	0.87	0.33	0 1	380,685	0.87	0.33	0 1
Black	165,397	0.08	0.27	0 1	380,685	0.08	0.27	0 1
Asian	165,397	0.01	0.08	0 1	380,685	0.01	0.08	0 1
Cohort (year aged 14)	165,397	1966.58	15.15	1920 1997	380,685	1967.09	15.13	1920 1997
Survey year	165,397	2003.94	3.08	2000 2008	380,685	2003.94	3.08	2000 2008
<i>Potential mechanisms</i>								
Reduce tax	127,519	0.36	0.48	0 1				
Ban abortion	123,378	0.20	0.40	0 1				
Low gun controls	74,409	0.38	0.49	0 1				
Low health spending	63,171	0.31	0.46	0 1				
High military spending	71,731	0.49	0.50	0 1				
Republican non-economic issues	152,119	-0.00	1.00	-1.17 2.45				
Political interest scale	165,370	0.04	1.00	-2.27 2.30				
Political knowledge scale	116,258	0.03	1.00	-1.21 1.33				
Discuss politics	157,454	0.02	1.00	-1.19 1.53				
Turnout (pres. election)	140,356	0.86	0.34	0 1				
Trust federal government	50,440	0.25	0.43	0 1				
Protect environment more	116,050	0.13	0.34	0 1				

A.2 ANES data for robustness checks

The American National Election Survey (ANES) collects many political variables which are pooled across presidential and mid-term elections 1952-2000 and 2008.¹ Surveys typically interview several thousand randomly-sampled voting-age citizens; pooling across elections produced a maximum sample of 31,991 respondents.² The ANES thus 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. Nevertheless, the ANES has several advantages over the NAES: the ANES covers a wider range of elections 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. Consequently, showing similar results in the ANES is an important robustness check, particularly with respect to the assignment of dropout ages.

Like the NAES, the number of years of schooling is not measured in the ANES. Accordingly, this article complements the ANES with extracts from the 1950-2000 decennial Censuses (Ruggles et al. 2010). 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 that the Census sample of 33,478 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 main article and in section A.4.3 below, with the exception that I match on survey decade (rather than election survey, due to the unavailability of yearly Census data).

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 A2 provides summary statistics.

- *Democrat partisan*. Indicator coded 1 for the respondent identifying as a Democrat, including those weak partisans who when pressed leaned toward the Democrats. Residual category is independents and Republican partisans; non-answers were deleted. ANES variable VCF0303.
- *Vote Democrat for president/House/Senate*. Vote Democrat for president, Vote Democrat for House and Vote Democrat for Senate are indicators for voting Democrat in any of these elections, including at mid-terms. The omitted category contains Republican and other party/candidate voters; non-voters and those who do not provide an answer are deleted. ANES variables VCF0704, VCF0707, and VCF0708.
- *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”.
- *Male*. Indicator variable for respondents identifying as male. Non-responses were deleted. ANES variable VCF0104; Census variable name “sex”.

¹2002-2006 surveys were omitted because they did not ask where respondents grew up, a central component of the identification strategy.

²Observations suffering missingness were deleted.

Table A2: Summary statistics: ANES and Census samples

	<i>American National Election Study</i>					<i>Census</i>				
	Obs.	Mean	Std. dev.	Min.	Max.	Obs.	Mean	Std. dev.	Min.	Max.
<i>Dependent variables</i>										
Democrat partisan	31,867	0.53	0.50	0	1					
Vote Democrat for president	13,529	0.48	0.50	0	1					
Vote Democrat for House	17,881	0.56	0.50	0	1					
Vote Democrat for Senate	12,428	0.55	0.50	0	1					
<i>Endogenous variable</i>										
Schooling						701,578	10.70	2.63	0	12
<i>Excluded instruments</i>										
Dropout age=16	31,981	0.75	0.43	0	1	701,578	0.75	0.43	0	1
Dropout age \geq 17	31,981	0.16	0.36	0	1	701,578	0.15	0.36	0	1
<i>Predetermined covariates</i>										
Age	31,981	46.44	14.91	25	97	701,578	47.90	15.24	25	93
Male	31,981	0.44	0.50	0	1	701,578	0.44	0.50	0	1
White	31,981	0.86	0.35	0	1	701,578	0.86	0.35	0	1
Black	31,981	0.12	0.32	0	1	701,578	0.11	0.32	0	1
Asian	31,981	0.00	0.06	0	1	701,578	0.00	0.05	0	1
Year aged 14	31,981	1,947.79	18.25	1914	1989	701,578	1,947.10	17.56	1914	1989
Census year (decade)	31,981	1,979.98	13.48	1950	2000	701,578	1,981.00	13.52	1950	2000

- *Race*. Three indicators for respondents identifying their race as White or Hispanic, Black, or Asian (Chinese, Japanese, and Other Asian or 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”.
- *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”.
- *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”.
- *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 dropout ages. Foreign-born respondents were excluded. ANES variable VCF0132 and VCF0133; Census variable “bpl”.
- *Dropout age*. Same as ACS and NAES.
- *Schooling*. Same as ACS. Census variable name “educd”.

A.3 Additional information about state schooling laws

A.3.1 Other aspects of schooling laws

Compulsory schooling laws are legislated by state governments and define the minimum age at which a student must enter and may drop out of schooling. As noted in the main text, the schooling leaving age—or dropout age—is the most relevant aspect of these laws today. However, in the late nineteenth and early twentieth century, other aspects of these laws were also important. In particular, child labor laws defined when children could start working, and played an important role in determining educational choice before 1940 (Goldin and Katz 2008). Similarly, the school entry age was also more relevant in a context of lower levels of education. Neither feature plays a prominent role today. This in part reflects social shifts away from child labor, but also a homogenization of laws across states. Consequently, these aspects of school attendance laws offer limited variation and are mostly irrelevant for the period I study. This is reflected in their weak first stage

effect on completed grades of schooling, although their inclusion as additional instruments does not affect the results.

A.3.2 Political control and dropout age reforms

To investigate the concern that school leaving age reforms may reflect strategic behavior by particular parties or reflect waves of political sentiment, I test whether a state’s minimum schooling leave age is correlated with partisan political forces in state s in year t by estimating the following equation:

$$dropout\ age_{st} = P_{st}\lambda + \vartheta_s + \omega_t + \phi_s year_t + \xi_{st}, \quad (A1)$$

where P_{st} is a vector of state-level political variables, θ_s and ω_t are respectively state and year fixed effects, and $\phi_s year_t$ are state-specific trends. Standard errors are clustered by state. This specification is analogous to the baseline specification used in the main paper.

The results in Table A3 provide little evidence to support the political endogeneity concern. Column (1) shows no statistical association between the minimum dropout age and indicators for whether the Democrats controlled the upper, lower, or both state houses. Using the Democrat seat share instead, column (2) again finds that political support is uncorrelated with changes in dropout ages. Finally, column (3) finds no correlation with Democrat state Governors, which runs contrary to the idea that Democrats are more likely to enact progressive education reforms. The lack of systematic correlations suggest that minimum schooling leaving age reforms were unlikely to have been implemented strategically, or have been correlated with waves in local political sentiment.³

A.4 Technical details for IV estimation

A.4.1 Support for the monotonicity assumption

Although monotonicity is fundamentally untestable, one implication of monotonicity with multi-valued treatments is that the CDFs $F(S_i | dropout\ age_{ics} = z)$ should not cross (Angrist and Imbens 1995). Figure A1 plots the CDFs for each dropout age instrument, and support the monotonicity assumption as the CDF of higher dropout ages lie everywhere below the CDF of lower dropout ages. To take account of the differences that arise across state-cohorts, Figure A2 adjusts the figure by subtracting the predicted level of education for an individual in a given state-cohort (based on a specification including the variables used for the difference-in-differences analysis, namely state grew up, cohort, and survey year fixed effects, as well as state-specific cohort trends. These figures also provide an alternative graphical depiction of the first-stage relationship with the largest changes in the relative CDFs occurring where the instruments apply. While these tests cannot prove that 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 little empirical support. Moreover, it is intuitively unlikely that raising the school leaving age would create defiers

³Unreported checks using the three leaving age indicators and lagging the independent variables to capture delayed implementation similarly show no statistically significant associations.

Table A3: Correlation between state political control and state minimum schooling leaving age

	Dropout age in the state		
	(1)	(2)	(3)
Democrat House majority	0.043 (0.059)		
Democrat Senate majority	0.067 (0.057)		
Unified Democrat houses	0.012 (0.067)		
Democrat House seat share		0.360 (0.254)	
Democrat Senate seat share		0.092 (0.249)	
Democrat Governor			-0.020 (0.031)
Observations	3,336	3,304	3,136
Outcome range	[12,18]	[12,18]	[12,18]
Outcome mean	16.26	16.26	16.26

Notes: All specifications include state and year fixed effects and state-specific trends, and are estimated using OLS. Standard errors clustered by state in parentheses. * denotes $p < 0.05$.

that disobey the law and choose to receive less education than they would have otherwise have received.

A.4.2 Two-sample 2SLS estimation

The goal is estimate the following system of IV equations:

$$Y_i = T_i\beta_T + W_i\beta_{-T} + u_i = X_i\beta + u_i \quad (\text{A2})$$

$$T_i = Z_i\Pi + \varepsilon_i, \quad (\text{A3})$$

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

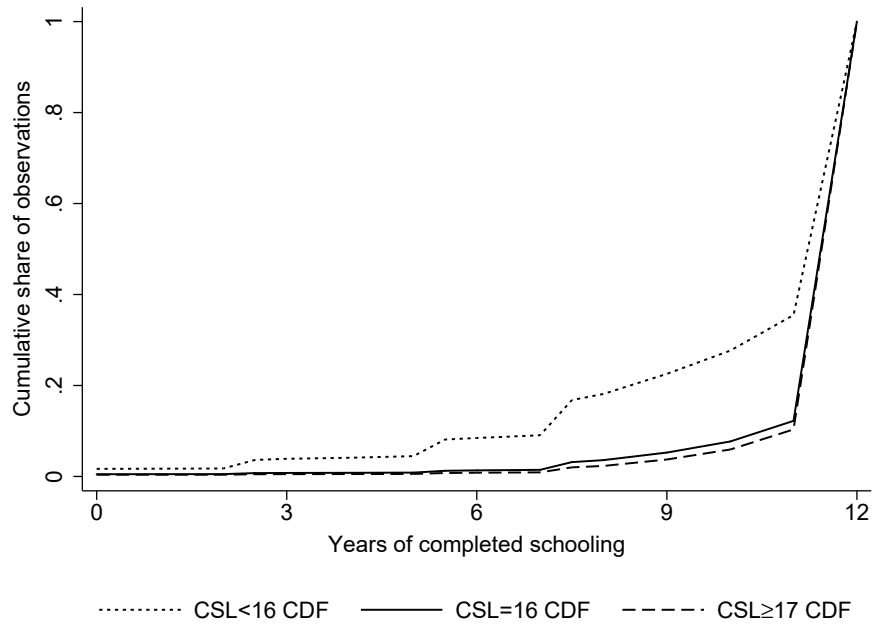


Figure A1: Monotonic first-stage (ACS data)

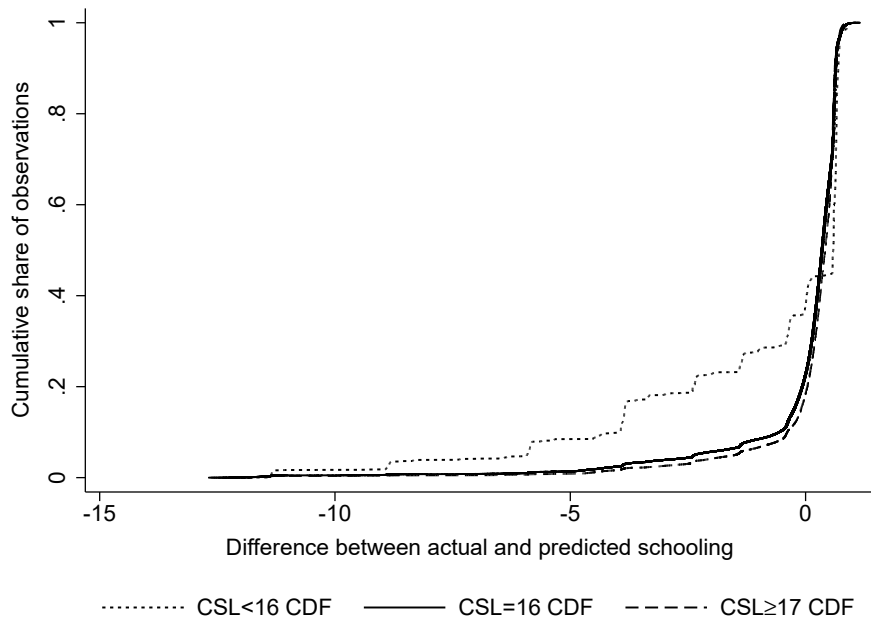


Figure A2: Monotonic first-stage for difference between actual and years predicted years of schooling (ACS data)

be used in the empirical application here. The advantage of this estimator is that it corrects for finite-sample differences between the two when sampling rates differ with any control variable (Inoue and Solon 2010). 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 article.

In matrix form (stacking over i in each sample), the two-sample 2SLS (TS2SLS) estimator is:

$$\hat{\beta}^{TS2SLS} = (\hat{X}'_1 \hat{X}_1)^{-1} \hat{X}'_1 Y_1, \quad (\text{A4})$$

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:

$$\hat{X}_1 = Z_1 \hat{\Gamma} = Z_1 (Z'_2 Z_2)^{-1} Z'_2 X_2. \quad (\text{A5})$$

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:** $\mathbb{E}[Z'_{1i} \varepsilon_{1i}] = \mathbb{E}[Z'_{2i} \varepsilon_{2i}] = 0$.
3. **Exclusion restriction:** $\mathbb{E}[Z'_{1i} u_{1i}] = 0$.
4. **Rank conditions:** (a) $Z'_{1i} Z_{1i}$ and $Z'_{2i} Z_{2i}$ have full rank, (b) $X'_{1i} Z_{2i}$ and $X'_{2i} Z_{2i}$ have full rank.
5. **Interchangeable sample moments:** (a) $\mathbb{E}[Z'_{1i} X_{1i}] = \mathbb{E}[Z'_{2i} X_{2i}]$, (b) $\mathbb{E}[Z'_{1i} Z_{1i}] = \mathbb{E}[Z'_{2i} Z_{2i}]$.

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. 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.⁴ 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 is dropped to facilitate exposition.)

⁴Inoue 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.

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

$$\left[\sigma_u^2 + \frac{n_1}{n_2} \hat{\beta}_S^{TS2SLS'} \Omega \hat{\beta}_S^{TS2SLS} \right] \mathbb{E}[\hat{X}'_1 \hat{X}_1]^{-1}, \quad \Omega = \mathbb{E}[\varepsilon' \varepsilon | \hat{X}_1] = \begin{bmatrix} \sigma_1^2 & \cdots & \sigma_{1,p} \\ \vdots & \ddots & \vdots \\ \sigma_{p,1} & \cdots & \sigma_p^2 \end{bmatrix} \quad (\text{A6})$$

when the reduced form squared error $\sigma_u^2 = \mathbb{E}[u^2 | \hat{X}_1]$ and the error covariances Ω 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

$$\mathbb{E}[\hat{X}'_1 \hat{X}_1]^{-1} \left[\overline{\mathbb{V}}[\hat{\beta}^{TS2SLS}] + \frac{n_1}{n_2} \mathbb{E}[\hat{X}'_1 (\hat{\beta}_S^{TS2SLS'} \otimes Z_1)] \mathbb{V}(\hat{\Pi}) \mathbb{E}[(\hat{\beta}_S^{TS2SLS'} \otimes Z_1)' \hat{X}_1] \right] \mathbb{E}[\hat{X}'_1 \hat{X}_1]^{-1}, \quad (\text{A7})$$

where $\hat{\beta}_S^{TS2SLS}$ is the vector of coefficients on p endogenous variables, the uncorrected TS2SLS variance is given by $\overline{\mathbb{V}}[\hat{\beta}^{TS2SLS}] = \frac{G_1}{G_1-1} \sum_{g=1}^{G_1} \mathbb{E}[\hat{X}'_{1g} \hat{u}_{1g} \hat{u}'_{1g} \hat{X}_{1g}]$ and the variances from m first-stage regressions are $\mathbb{V}(\hat{\Pi}) = \frac{G_2}{G_2-1} \Phi \otimes \mathbb{E}[Z'_2 Z_2]^{-1}$, where

$$\Phi = \begin{bmatrix} \mathbb{E}[Z'_2 Z_2]^{-1} \sum_{g=1}^{G_2} \mathbb{E}[Z'_{2g} \hat{\varepsilon}_{2g1} \hat{\varepsilon}'_{2g1} Z_{2g}] & \cdots & \mathbb{E}[Z'_2 Z_2]^{-1} \sum_{g=1}^{G_2} \mathbb{E}[Z'_{2g} \hat{\varepsilon}_{2g1} \hat{\varepsilon}'_{2gp} Z_{2g}] \\ \vdots & \ddots & \vdots \\ \mathbb{E}[Z'_2 Z_2]^{-1} \sum_{g=1}^{G_2} \mathbb{E}[Z'_{2g} \hat{\varepsilon}_{2gp} \hat{\varepsilon}'_{2g1} Z_{2g}] & \cdots & \mathbb{E}[Z'_2 Z_2]^{-1} \sum_{g=1}^{G_2} \mathbb{E}[Z'_{2g} \hat{\varepsilon}_{2gp} \hat{\varepsilon}'_{2gp} Z_{2g}] \end{bmatrix}. \quad (\text{A8})$$

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

$$Y_{i1} = X_{i1} \beta_{-S} + T_{i1} \beta_S + u_i = X_{i1} \beta_{-S} + \hat{T}_{i1} \beta_S + [T_{i1} - \hat{T}_{i1}] + u_i, \quad (\text{A9})$$

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:

$$\sqrt{n_1} \begin{pmatrix} \hat{\beta}_{-T} - \beta_{-S} \\ \hat{\beta}_S - \beta_S \end{pmatrix} = \begin{pmatrix} \frac{1}{n_1} \hat{X}'_1 \hat{X}_1 \end{pmatrix}^{-1} \frac{1}{\sqrt{n_1}} \hat{X}'_1 u_1 + \begin{pmatrix} \frac{1}{n_1} \hat{X}'_1 \hat{X}_1 \end{pmatrix}^{-1} \frac{1}{\sqrt{n_1}} \hat{X}'_1 [T_{i1} - \hat{T}_{i1}] \beta_S, \quad (\text{A10})$$

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

$$\begin{aligned} \sqrt{n_1} (\hat{\beta} - \beta) &\equiv \sqrt{n_1} \begin{pmatrix} \hat{\beta}_{-T} - \beta_{-S} \\ \hat{\beta}_S - \beta_S \end{pmatrix} \stackrel{a}{=} \begin{pmatrix} \frac{1}{n_1} \hat{X}'_1 \hat{X}_1 \end{pmatrix}^{-1} \frac{1}{\sqrt{n_1}} \hat{X}'_1 u_1 \\ &\quad + \begin{pmatrix} \frac{1}{n_1} \hat{X}'_1 \hat{X}_1 \end{pmatrix}^{-1} \left(\frac{n_1}{n_2} \right)^{1/2} \frac{1}{n_1} \hat{X}'_1 (\hat{\beta}'_T \otimes Z_1) \sqrt{n_2} (\hat{\Pi} - \Pi) \end{aligned} \quad (\text{A11})$$

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:

$$\mathbb{V}(\hat{\beta} - \beta) = \mathbb{E}[\hat{X}'_1 \hat{X}_1]^{-1} \left[\mathbb{V}[\bar{\beta}] + \frac{n_1}{n_2} \mathbb{E}[\hat{X}'_1 (\hat{\beta}'_T \otimes Z_1)]^{-1} \mathbb{V}[\Pi] \mathbb{E}[(\hat{\beta}'_T \otimes Z_1)' \hat{X}_1]^{-1} \right] \mathbb{E}[\hat{X}'_1 \hat{X}_1]^{-1} \quad (\text{A12})$$

where $\mathbb{V}[\bar{\beta}]$ 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:

$$\hat{X}'_1 (\hat{\beta}'_T \otimes Z_1) \hat{\mathbb{V}}(\hat{\Pi}) (\hat{\beta}'_T \otimes Z_1)' \hat{X}_1 = \hat{X}'_1 (\hat{\beta}'_T \otimes Z_1) (\Omega \otimes (Z'_1 Z_1)^{-1}) (\hat{\beta}'_T \otimes Z_1)' \hat{X}_1 \quad (\text{A13})$$

$$= \hat{X}'_1 (\hat{\beta}'_T \Omega \hat{\beta}_T \otimes Z_1 (Z'_1 Z_1)^{-1} Z'_1) \hat{X}_1 \quad (\text{A14})$$

$$= \hat{\beta}'_T \Omega \hat{\beta}_T (\hat{X}'_1 \hat{X}_1), \quad (\text{A15})$$

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:

$$\mathbb{E}[Z'_2 Z_2]^{-1} \left[\frac{G_2}{G_2 - 1} \sum_{g=1}^{G_2} \mathbb{E}[Z'_{2g} \hat{\varepsilon}_{2g} \hat{\varepsilon}'_{2g} Z_{2g}] \right] \mathbb{E}[Z'_2 Z_2]^{-1}. \quad (\text{A16})$$

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

This procedure is implemented using an R program that I wrote, which is provided in the replication code.

A.4.3 Procedure for matching NAES and ACS sample moments

As the proof above illustrates, TS2SLS depends on random sampling from the same population (assumption 1 above) and exchangeability of sample moments (assumption 5 above). As noted in the main text, I use stratified random sampling to ensure that the sampling moments of the ACS data used to estimate the first stage (for the IV estimates) match the analogous sampling moments

in the NAES dataset used to estimate the reduced form. This corrects for observable differences across these samples, which draw from the same population (once ineligible voters are removed from the NAES and those aged below 25 and born outside the U.S. are removed from the ACS).

To match the sample moments, I first removed respondents aged 14 before 1920 (since no NAES respondent was 14 before 1920), respondents aged below 25, and respondents who grew up in Alaska or Hawaii, were born outside the U.S., or became naturalized citizens. I then stratified by state-cohort to draw a random sample of 2,000,000 ACS respondents to ensure that the subsample has the same state-cohort distribution as the NAES.⁵ Specifically, this entailed randomly selecting ACS observations from each (non-empty) state-cohort cell in the NAES) up to the exact proportion in the NAES. Next, I followed the same stratified procedure to randomly drop respondents from survey-year cells, then gender, and then self-identified race cells (white, black, and Asian) that were over-represented relative to the NAES.⁶ This produced a maximum pooled sample of 380,685 respondents. As Table A1 in the main paper shows, this procedure ensures that the ACS sample very closely matches the observable sample moments for the predetermined covariates in the NAES data.

Given that the first stage and IV estimates reported in the main paper rely on a single random draw (to avoid exorbitant computation times for IV estimates),⁷ a potential concern is that this particular draw is an outlier producing particularly large or small first stage estimates. To show that this is not the case, I redrew 1,000 samples of the ACS dataset according to the protocols described in the previous paragraph. The distribution of first stage estimates in Figure A3 indicates that the estimated effects on completed grades of schooling are a little above average, but far from the tail of the distribution. A typical randomly-drawn first stage thus generates slightly small first stage estimates than those reported in the main paper, which will slightly increase the IV estimates. The reduced form estimates, which drive the IV estimates, are unaffected. Figure A4 similarly demonstrates that the lack of an effect on post-secondary education reported in the main paper is not an anomaly of the random sampling.

A.5 Robustness to potential violations of the identifying assumptions

The estimates in the main paper rest on several key assumptions required for causal identification. The parallel trends and no differential migration assumptions are required for both the reduced form and IV estimates. The IV estimates additionally require an exclusion restriction requiring that the dropout age only affect political behavior through additional schooling. The following robustness checks suggest that the results are not driven by violations of these assumptions.

⁵The ACS sample only included a small number of respondents born in 1923, so all such respondents are retained.

⁶When randomly drawing from survey-year cells, observations from cohorts born in 1922 and 1923 were not deleted, in order to retain a small number of respondents from those cohorts.

⁷This trade-off may come at the cost of slightly underestimating the uncertainty underpinning first stage estimation. Nevertheless, an alternative approach using the full ACS sample to compute the first stage, but instead weighting observations to match the distribution of the NAES characteristics, yielded similar results.

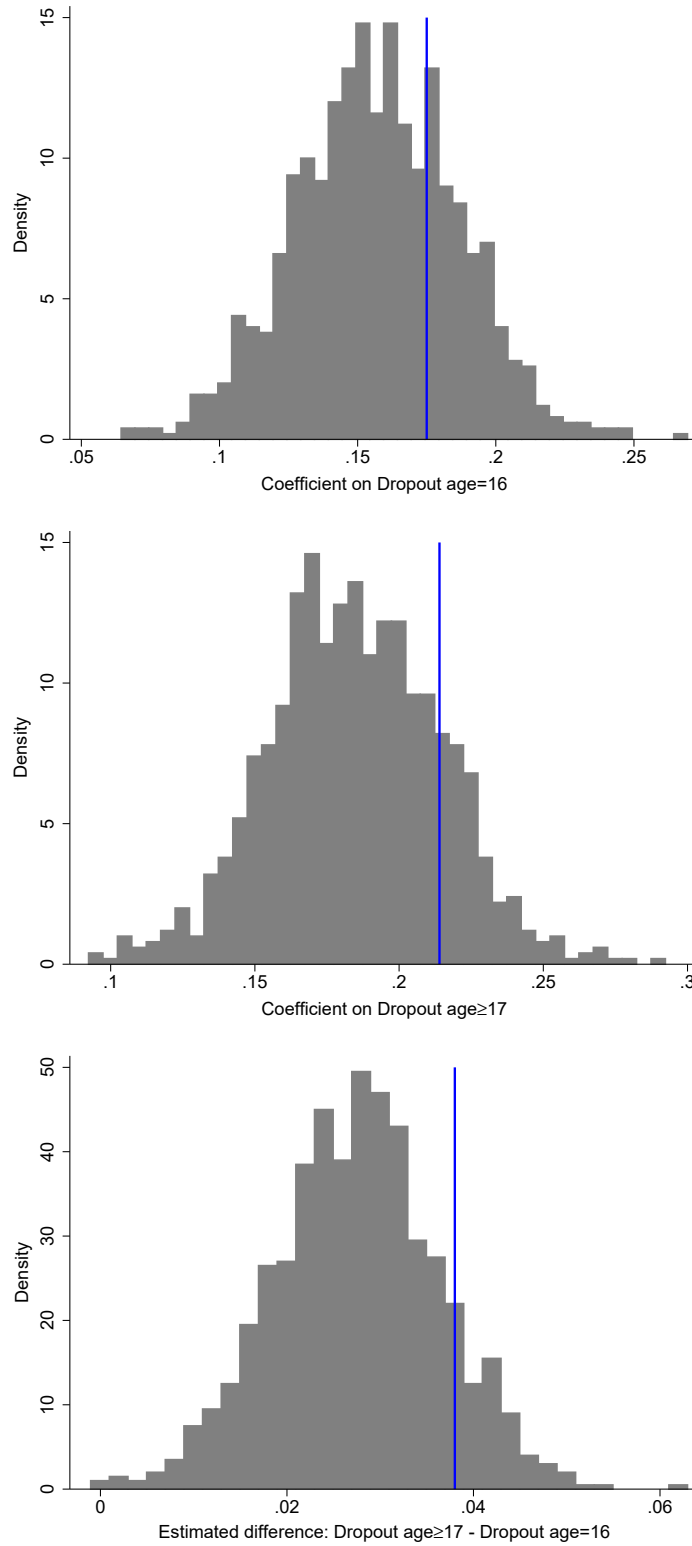


Figure A3: Distribution of effects of dropout ages on completed grades of schooling
Note: The vertical blue lines indicate the point estimates from the random draw used in the main paper.

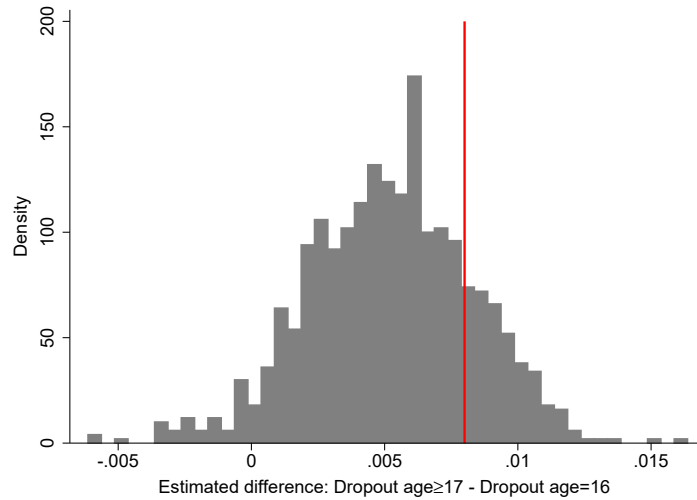
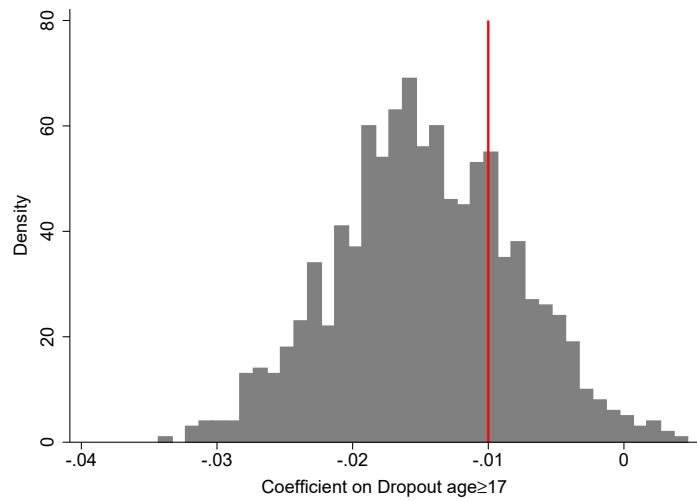
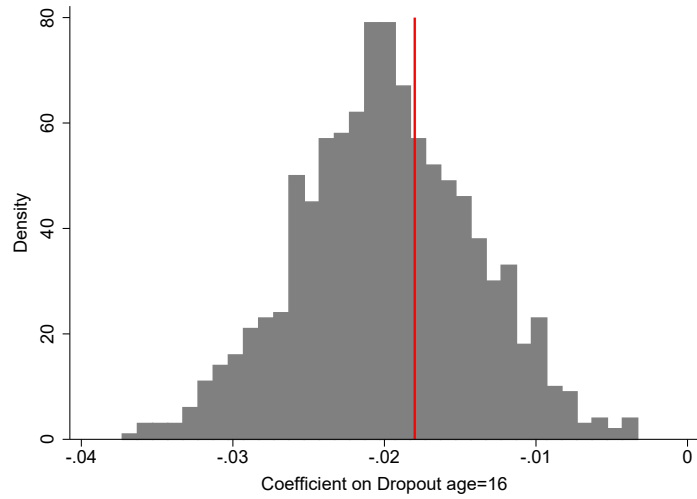


Figure A4: Distribution of effects of dropout ages on education beyond 12th grade
Note: The vertical red lines indicate the point estimates from the random draw used in the main paper.

A.5.1 Parallel trends

State-specific cohort trends provide a powerful general check against parallel trend concerns (Stephens and Yang 2014). Another common test includes lags and leads of dropout age reforms: if the estimates indeed capture the effects of the reform, rather than differential pre-trends across cohorts in states that did and did not enact reforms, leads should not affect Democrat support. Figure A5 compares the five cohorts before and after a reform. Consistent with the parallel trends assumption, there is limited evidence of pre-trends. Furthermore, there is a relatively swift and durable decrease in Democrat support among cohorts affected by a higher dropout age. Reiterating the findings in Table 1, the additional effect of increasing the leaving age to 17 or above is precise and durable across cohorts, while the decrease among cohorts facing an increased dropout age of 16 is somewhat more volatile.

Nevertheless, these general checks—linear state-specific cohort trends, and lags and leads—could still fail to capture more specific concerns. In particular, the types of states that changed their dropout laws—predominantly in the midwest and south—were also starting to follow different demographic, economic, and political trajectories. This could induce bias if, for example, partisan realignment in the south differentially influenced affected cohorts. Table A4 addresses more specific potential confounds. First, I control for state-level factors that vary across cohorts when respondents attended high school. Indeed, column (2) shows that the results are robust to including the Republican presidential vote share, state personal income per capita, the pupil-teacher ratio, and the black proportion of the state population at age 14. Columns (3) and (4) show similar results when controlling for these variables at ages 16 and 18. Second, column (5) includes region-cohort fixed effects to show that—despite some loss of precision—the results are robust even when exploiting only changes in the dropout age occurring across cohorts within similar parts of the country.⁸ Third, the context in which the survey was conducted could have differentially affected voters that also faced higher dropout ages. Column (6) addresses this concern by including cohort-survey year and state-survey year fixed effects, thereby showing that the results still hold when only leveraging the difference-in-differences variation from within each year of a presidential election campaign.

Finally, to further allay lingering concerns about non-parallel trends, I use a regression discontinuity (RD) design to estimate the effect of dropout age reforms among cohorts just young enough to be affected (Oreopoulos 2006). This approach abstracts from any differential trends across cohorts by focusing only on those cohorts close to the eligibility threshold. Rather than parallel trends, the RD design relies on the weaker assumption that potential outcomes are continuous through the reform threshold. In other words, affected cohorts are comparable to unaffected cohorts around the reform. Using the ten cohorts either side of each reform, I estimate the following local-linear RD specification:

$$Y_{icst} = \tau T_{cs} + \kappa_0 X_{cs} + \kappa_1 (X_{cs} \times T_{cs}) + \theta_s + \eta_t + v_{icst}, \quad (\text{A17})$$

where $X_{cs} \equiv \text{birth year of first cohort affected by reform}_{cs} - \text{birth year}_{cs}$ is the forcing variable defining whether an individual i from cohort c in state s was subject to a reform, i.e. $T_{cs} \equiv I(X_{cs} \geq$

⁸The census regions are: midwest, northeast, south, and west.

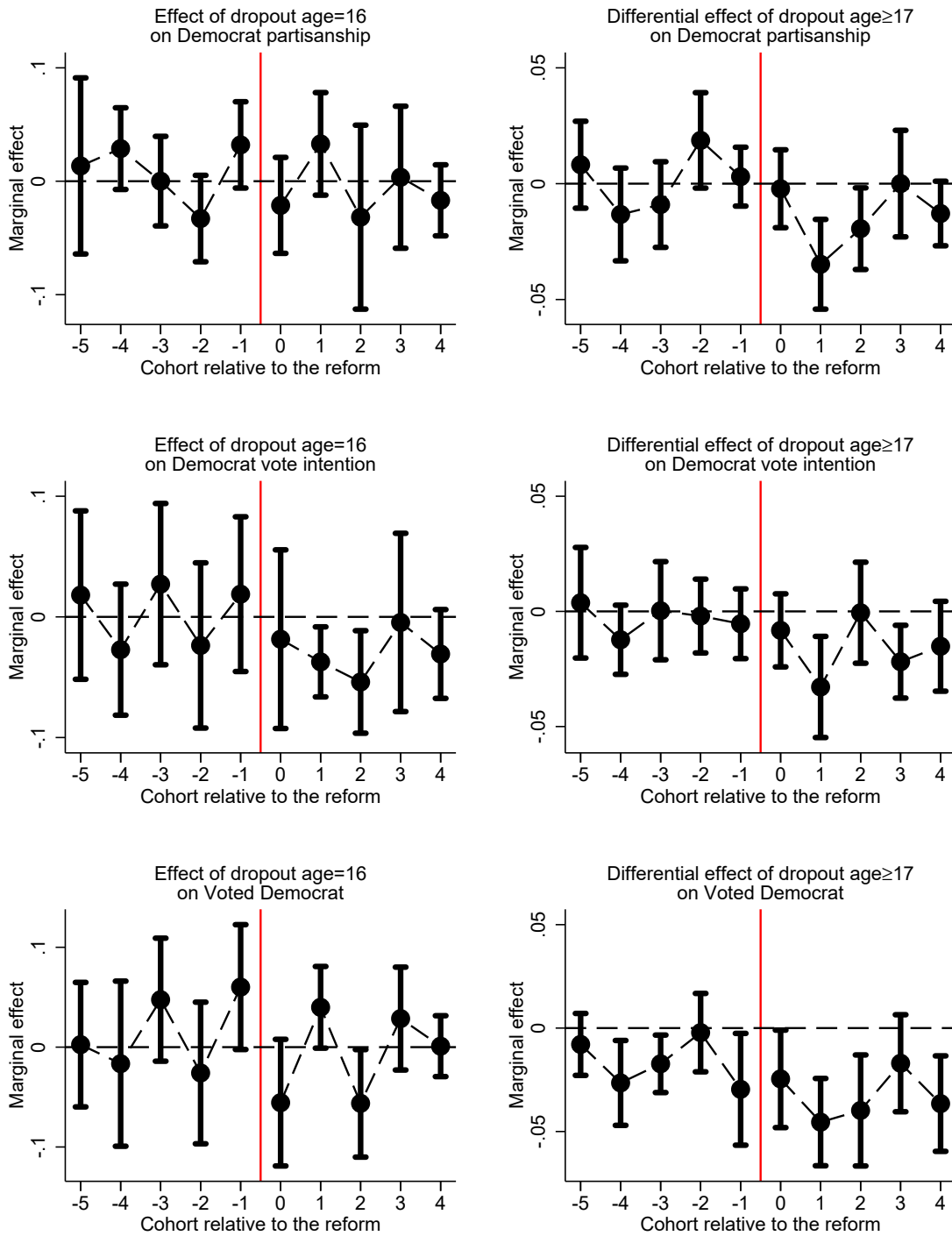


Figure A5: Effect of raising the dropout age on cohorts either side of the reform date

Notes: All estimates are from specifications including five pre- and post-reform variables, where the baseline is six or more years before the reform. Bars represent 95% confidence intervals.

Table A4: Further parallel trends checks

	Baseline estimates from				Cohort-region fixed effects	Cohort- and state-year fixed effects
	Table 1	State-level controls at...				
	(1)	...age 14	...age 16	...age 18	(5)	(6)
Panel A: Reduced form effect on Democrat partisanship						
Dropout age=16	-0.017 (0.016)	-0.026 (0.018)	-0.025 (0.018)	-0.020 (0.016)	-0.011 (0.018)	-0.016 (0.016)
Dropout age \geq 17	-0.032 (0.018)	-0.040 (0.020)	-0.040* (0.020)	-0.037* (0.018)	-0.027 (0.019)	-0.031 (0.018)
Observations	164,606	162,316	161,171	162,734	164,606	164,606
Difference: Dropout age \geq 17 - Dropout age=16	-0.015* (0.006)	-0.014* (0.006)	-0.016* (0.006)	-0.016* (0.005)	-0.016* (0.005)	-0.014* (0.005)
Panel B: Reduced form effect on Democrat vote intention						
Dropout age=16	-0.031* (0.015)	-0.032 (0.017)	-0.031 (0.018)	-0.029 (0.016)	-0.024 (0.018)	-0.032* (0.015)
Dropout age \geq 17	-0.044* (0.017)	-0.040* (0.019)	-0.040* (0.020)	-0.040* (0.019)	-0.036 (0.020)	-0.044* (0.017)
Observations	134,504	132,631	131,735	132,970	134,504	134,504
Difference: Dropout age \geq 17 - Dropout age=16	-0.013* (0.005)	-0.008 (0.006)	-0.008 (0.005)	-0.012* (0.005)	-0.012* (0.006)	-0.012* (0.005)
Panel C: Reduced form effect on Voted Democrat						
Dropout age=16	-0.014 (0.013)	-0.012 (0.014)	-0.011 (0.015)	-0.013 (0.014)	-0.010 (0.015)	-0.012 (0.013)
Dropout age \geq 17	-0.033* (0.014)	-0.030 (0.015)	-0.029 (0.017)	-0.033* (0.015)	-0.029 (0.016)	-0.031* (0.014)
Observations	117,733	116,326	115,508	116,500	117,733	117,733
Difference: Dropout age \geq 17 - Dropout age=16	-0.019* (0.007)	-0.018* (0.007)	-0.018* (0.007)	-0.020* (0.007)	-0.019* (0.007)	-0.019* (0.007)

Notes: All specifications include state grew up, cohort, and survey year fixed effects and state-specific cohort trends, and are estimated using OLS. Differences in the number of observations reflect differences in the NAES modules in which each outcome was included. State-level controls include: pupil-teacher ratio, previous Republican presidential vote share, state income per capita, and black proportion of the state population. Differences in the number of observations in columns (2)-(4) reflects missing state-level controls. The omitted dropout age category is dropout age \leq 15. Standard errors clustered by state in parentheses. * $p < 0.05$.

0). The interaction term $X_{cs} \times T_{cs}$ allows cohort trends to vary either side of the discontinuity. Observations are weighted using a triangular kernel to assign greatest weight to individuals from cohorts closer to the discontinuity.

Table A5 tests the RD identifying assumption by reporting estimates from regressions analogous to equation (A17), but where the predetermined covariates used as controls in the main paper are used as outcomes. These variables are well-balanced across the reform.

The estimates in Table A6 show that the first cohorts to experience an increased leaving age are often significantly less likely to support the Democrats later in life; Figure A6 graphs the results.⁹ These results concentrating on cohorts immediately around the reforms reinforce the difference-in-differences estimates, and further indicate that such estimates are not driven by differential trends. The slightly larger, albeit less precise, RD estimates suggest that the cohorts first affected by the reform may have responded more than later cohorts.

A.5.2 Sensitivity to cross-state migration

Since the NAES surveys do not collect state of residence when at high school, leaving ages are assigned on the basis of current state of residence (at the time of the survey). The analysis thus further relies on the assumption that compulsory schooling laws do not induce differential cross-state migration patterns. This could generate systematic biases if the leaving age in the true state of residence affects the probability of subsequently relocating to a state that had a different leaving age at the time when the respondent attended high school.

Although the main article argues that cross-state migration is unlikely to be driving the results, I nevertheless conduct three robustness checks to further show that this is unlikely to be a concern: a direct test of the extent to which the dropout age affects migration to states with different dropout ages; a bounding exercise that shows that the type of migration required to upwardly bias the estimates is implausible; and analysis using the ANES, where state of residence at age 14 can be used to assign the dropout age. These tests complement the regression discontinuity estimates in Table A6, which are hard to reconcile with migration unless most of the individuals driving the results moved *immediately* as the reform came into effect.

I first assess the extent to which dropout age laws affect cross-state migration. To test this, I used the ANES data because it includes location of residence at both the time of the survey and at age 14, and I define an indicator for respondents residing in the same state at both points in time. The results in Table A7 show that dropout age reforms do not significantly affect cross-state migration. This is consistent with the likelihood that cross-state migration is low among individuals that only stayed in school because of the schooling reforms (who, as the main paper shows, do not go on to attend college). State-specific cohort trends are excluded, given the comparatively weak power of the analysis and the weak first stage.

Although there is little evidence of differential migration in the ANES, it remains possible that this finding does not extend to NAES respondents. I thus conduct a bounding exercise to evaluate

⁹To extract the state and survey fixed effects, the figure plots the residualized outcomes (i.e. the residuals from a regression of these outcomes on state and survey fixed effects using the sample used to estimate the RD) on the y-axis.

Table A5: Regression discontinuity balance tests

Covariate as dependent variable	Dropout age=16	Standard error	Observations	Dropout age \geq 17	Standard error	Observations
Male	0.009	(0.041)	8,282	0.002	(0.010)	21,254
White	-0.014	(0.028)	8,282	0.005	(0.012)	21,254
Black	-0.004	(0.018)	8,282	-0.001	(0.008)	21,254
Asian	0.002	(0.001)	8,282	-0.002	(0.002)	21,254
Pupil-teacher ratio at age 14	-2.695	(1.573)	8,282	0.118	(0.238)	21,254
Pupil-teacher ratio at age 16	-2.494	(2.288)	8,282	0.301	(0.354)	21,254
Pupil-teacher ratio at age 18	-2.509	(1.665)	8,282	-0.219	(0.237)	21,254
Republican presidential vote share at age 14	0.097	(0.077)	8,082	-0.001	(0.020)	21,034
Republican presidential vote share at age 16	0.089	(0.080)	7,839	0.029	(0.031)	20,801
Republican presidential vote share at age 18	0.132	(0.064)	8,082	0.015	(0.016)	21,034
State income per capita at age 14	1,354.797	(1924.973)	8,203	-364.614*	(131.248)	21,128
State income per capita at age 16	2,167.510	(1861.716)	8,215	-93.389	(127.776)	21,143
State income per capita at age 18	2,526.460	(1546.638)	8,226	-32.342	(133.434)	21,158
Black proportion of state population at age 14	-0.007	(0.004)	8,282	-0.000	(0.000)	21,254
Black proportion of state population at age 16	-0.007	(0.004)	8,282	-0.000	(0.000)	21,254
Black proportion of state population at age 18	-0.006	(0.003)	8,282	-0.000	(0.000)	21,254

Notes: See above for detailed variable definitions. All estimates are from separate specifications including linear cohort trends either side of the reform, state grew up, and survey fixed effects, and are estimated using OLS. Only respondents within 10 cohorts of the reform are included, and all observations are weighted by the inverse of the number of cohorts from the discontinuity. Standard errors clustered by state in parentheses. Standard errors clustered by state in parentheses. * $p < 0.05$.

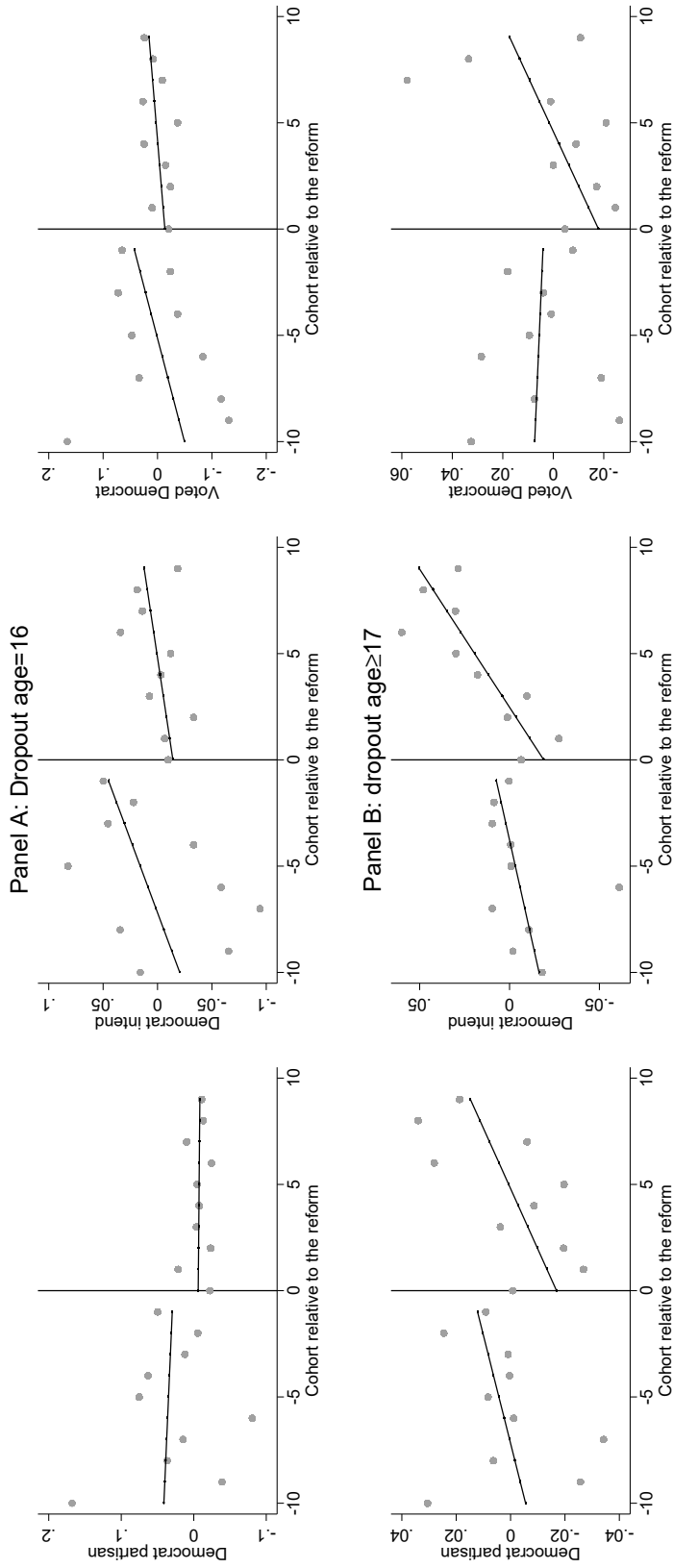


Figure A6: Effect of school drop out age reforms on identifying as and voting Democrat around the reforms

Notes: All variable on the y-axis are residuals from a regression of the outcome on state and survey fixed effects in the sample used to estimate the RD. All figures plot linear trends either side of the reform cutoff. Only respondents within 10 cohorts of the reform are included.

Table A6: Regression discontinuity estimates of the effect of school dropout age on identifying as and voting Democrat

	Partisan		Democrat support		Voted	
	(1)	(2)	(3)	(4)	(5)	(6)
Dropout age=16	-0.046 (0.049)		-0.074 (0.044)		-0.074* (0.030)	
Dropout age \geq 17		-0.033* (0.008)		-0.029* (0.011)		-0.019 (0.012)
Observations	8,235	21,142	6,489	16,624	6,083	16,203
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.33	0.31	0.35	0.38	0.34	0.35

Notes: The control group in odd columns faces a leaving age of 15 or lower; the control group in even columns faces a leaving age of 16. All specifications include linear cohort trends either side of the reform, state grew up, and survey fixed effects, and are estimated using OLS. Only respondents within 10 cohorts of the reform are included, and all observations are weighted by the inverse of the number of cohorts from the discontinuity. Standard errors clustered by state in parentheses. * $p < 0.05$.

Table A7: The effect of school dropout age on downstream cross-state migration in the ANES

	Currently reside in same state as when aged 14 (1)
Dropout age=16	0.012 (0.024)
Dropout age \geq 17	0.038 (0.028)
Observations	31,863
Outcome range	{0,1}
Outcome mean	0.71
Difference: Dropout age \geq 17 - Dropout age=16	0.026 (0.019)

Notes: Specification includes state grew up, cohort, and survey fixed effects, and is estimated using OLS. The omitted dropout age category is dropout age \leq 15. Standard errors clustered by state in parentheses. * $p < 0.05$.

the extent of biased migration required to explain the reduced form NAES estimates. Consider the most extreme case where voters migrate from a state with a low dropout age (baseline category of 15 or lower) to a state with a high dropout age (17 or higher). In the NAES sample, the average proportion of Democrat partisans in these states are, respectively, 37.1% and 32.3%; the sample-weighted average population in high-dropout age states is 3.3 times larger. To account for the smallest reduced form estimate (a 0.033 percentage point decrease in Democrat partisanship), if 18.6% of migrants from the low-dropout age state are Democrats partisans (which is 50% lower than the sample average), the *net migration* rate from low to high-dropout age states would have to average 10% per cohort. For Democrat vote intention and reported vote choice, selective migration (50% below the sample average for each measure of voting) would respectively require net per cohort migration rates of 11% and 9% to fully explain the reduced form estimates. Beyond the fact that migration is unlikely to be so politically biased, net migration between such states is substantially lower than the rates required to account for the main results. In the ACS data, respondents are in fact slightly more likely to migrate to a state with a lower dropout age than the state in which they were born.¹⁰ Moreover, less than 15% of ACS respondents moved to a state that had a different dropout age at the age when they attended school.

Finally, I examine the extent to which the ANES dataset, which uses a substantially smaller and less recent sample, yields similar results. Despite these flaws, the ANES has several advantages over the NAES, and thus showing similar results is an important robustness check: 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 9.8 grades); and by asking respondents which state they lived in at age 14, cross-state migration concerns are minimized because dropout ages can be mapped to individuals by location at age 14. The second feature is particularly relevant for addressing the migration concern because it ensures that the state dropout age is correctly assigned at the start of high school. The ANES data and two-sample procedures are described in section A.2.

The TS2SLS estimates in panel A of Table A8 show that schooling has generally decreased Democrat partisanship and self-reported Democratic voting in prior House elections. The point estimates are similar in magnitude to those found in the NAES, although the reduced form sample size is substantially smaller. State-specific cohort trends are excluded, given the comparatively weak power of the analysis and the weak first stage. To further address the migration concern, the results in panel B show that the ANES estimates are robust to focusing only on respondents who live in the state where they resided at age 14. Although conditioning the sample on such a post-treatment variable could induce bias, Table A7 suggests that the instruments did not significantly affect cross-state migration. More generally, these findings also indicate that the political effects of increasing the dropout age do not simply reflect idiosyncratic features of the NAES sample.

¹⁰Using the ACS data, I coded as 1 respondents who live in a state with a high dropout age than their state of birth, and a lower-dropout age state as -1. Summing across the sample (including non-movers), net migration was 4.5 percentage points toward lower-dropout age states.

Table A8: The effect of schooling on identifying as and voting Democrat—ANES dataset

	Partisan (1)	Democrat support		
		President (2)	House (3)	Senate (4)
Panel A: Full sample				
Schooling	-0.115* (0.051)	-0.048 (0.051)	-0.173* (0.055)	-0.070 (0.053)
Reduced form observations	31,867	13,529	17,881	12,428
First-stage observations	701,578	701,578	701,578	701,578
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.53	0.26	0.56	0.55
First-stage F statistic	16.2	16.2	16.2	16.2
Panel B: Non-migrants				
Schooling	-0.128* (0.058)	-0.047 (0.071)	-0.158* (0.066)	-0.075 (0.063)
Reduced form observations	22,581	9,532	12,696	8,755
First-stage observations	423,896	423,896	423,534	422,798
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.54	0.26	0.57	0.55
First-stage F statistic	11.9	11.9	11.9	11.9

Notes: Outcomes are Democrat partisanship (“Partisan”), and voting Democrat for president (“President”), House (“House”), and Senate (“Senate”). All specifications include state grew up in, survey decade, and cohort fixed effects, and are estimated using TS2SLS. Differences in sample size reflect data availability; first stage samples change to reflect missing data from the reduced form dataset. Standard errors clustered by state in parentheses. * $p < 0.05$.

A.5.3 Exclusion restriction

Although the preceding checks also suggest that the results are robust to plausible exclusion restriction violations, the IV estimates could still be biased if the dropout age directly decreased Democrat support through other channels. Given the temporal proximity of a binding dropout age to the decision to remain in school, exclusion restriction violations most likely reflect short-term or concurrent changes induced by school leaving age reforms.

Most plausibly, by keeping a student out of the labor force or reducing their interaction with uneducated peers, a greater minimum leaving age could affect life choices like marriage and fertility. If, for example, schooling reduced early marriage and fertility, then students may be less likely to experience the welfare state and ultimately support the Republican party. Table A9 uses

Table A9: Exclusion restriction tests (ACS data)

	Family size	Age of eldest child	Age of youngest child	Never married
	(1)	(2)	(3)	(4)
Dropout age=16	-0.002 (0.025)	-0.296 (0.263)	-0.174 (0.284)	0.009 (0.009)
Dropout age \geq 17	-0.012 (0.048)	-0.371 (0.322)	-0.185 (0.299)	0.008 (0.011)
Observations	380,685	153,381	153,381	380,685
Outcome range	[1,16]	[0,81]	[0,81]	{0,1}
Outcome mean	2.59	16.32	13.54	0.13
Difference: Dropout age \geq 17 - Dropout age=16	-0.010 (0.034)	-0.074 (0.106)	-0.011 (0.059)	-0.001 (0.004)

Notes: The outcomes in columns (2) and (3) condition on having a child; see Appendix section A.1 for detailed variable definitions. All specifications include state grew up, cohort, and survey year fixed effects and state-specific cohort trends, and are estimated using OLS. The omitted dropout age category is dropout age \leq 15. Standard errors clustered by state in parentheses. Standard errors clustered by state in parentheses. * $p < 0.05$.

ACS data to assess such concerns, and finds that the dropout age does not, on average, decrease the likelihood that a respondent has ever been married, or lower the family size or age of either a respondent's oldest or youngest child living at home. These results suggest that the main findings are unlikely to reflect adolescent life choices induced by the reforms.

A.6 Additional tests referenced in the main article

A.6.1 Separating leaving ages of 17 and 18

Table A10 reports the first stage and reduced form estimates when leaving ages of 17 and 18 are separate categories rather than pooled. As noted in the main text, the point estimates for leaving ages of 17 and 18 are very similar in magnitude and—as the tests at the foot of the table show—cannot be differentiated statistically. This is not surprising since some students complete high school before turning 18, and many only turn 18 just before completing high school.

A.6.2 Heterogeneous effects of first stage

Table A11 examines how the first stage varies with two pre-treatment characteristics: an indicator for being black or identifying ethnically as Hispanic; and an indicator for being male. Both types of student are notably more likely to fail to complete high school (Murnane 2013). The results

Table A10: The effect of school dropout age on schooling and identifying as and voting Democrat and Republican, separating out a drop out age of 18

	Completed grades		Democrat support			Republican support		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Dropout age=16	0.176* (0.044)	-0.018 (0.016)	-0.031* (0.015)	-0.015 (0.012)	0.012 (0.014)	0.040* (0.014)	0.029 (0.017)	
Dropout age=17	0.215* (0.051)	-0.034 (0.018)	-0.042* (0.017)	-0.039* (0.013)	0.034 (0.019)	0.056* (0.017)	0.046* (0.017)	
Dropout age=18	0.211* (0.051)	-0.030 (0.020)	-0.047* (0.018)	-0.024 (0.016)	0.022 (0.018)	0.050* (0.017)	0.029 (0.019)	
Observations	380,685	164,606	134,504	117,733	164,606	134,504	117,733	
Outcome range	[0,12]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	
Outcome mean	11.64	0.33	0.41	0.37	0.31	0.46	0.47	
Test: Dropout age=16 =	0.145	0.002	0.077	0.001	0.006	0.058	0.003	
Dropout age=17 (<i>p</i> value)								
Test: Dropout age=16 =	0.318	0.301	0.084	0.328	0.272	0.317	0.977	
Dropout age≥18 (<i>p</i> value)								
Test: Dropout age=17 =	0.936	0.784	0.593	0.180	0.256	0.657	0.070	
Dropout age≥18 (<i>p</i> value)								

Notes: All specifications include state grew up, cohort, and survey year fixed effects and state-specific cohort trends, and are estimated using OLS. Differences in the number of observations reflect differences in the NAES modules in which each outcome was included. The omitted dropout age category is dropout age ≤15. Standard errors clustered by state are in parentheses. * $p < 0.05$.

indicate that black and Hispanic and male students drive most of the response to raising the school leaving age. This is consistent with their baseline propensities to complete high school.

Table A11: The effect of school dropout age on completed grades of schooling, by minority status and gender

	Completed grades	
	(1)	(2)
Dropout age=16	-0.010 (0.091)	0.096* (0.038)
Dropout age \geq 17	0.000 (0.099)	0.127* (0.048)
Black or Hispanic	-1.412* (0.462)	
Dropout age=16 \times Black or Hispanic	1.008* (0.403)	
Dropout age \geq 17 \times Black or Hispanic	1.150* (0.432)	
Male		-0.285* (0.078)
Dropout age=16 \times Male		0.219* (0.076)
Dropout age \geq 17 \times Male		0.237* (0.076)
Observations	380,685	380,685
Outcome range	[0,12]	[0,12]
Outcome mean	11.64	11.64
Test: Dropout age=16 = Dropout age \geq 17 (<i>p</i> value)	0.647	0.229
Test: Dropout age=16 \times Interaction variable = Dropout age \geq 17 \times Interaction variable (<i>p</i> value)	0.010	0.239

Notes: All specifications include state grew up, cohort, and survey year fixed effects and state-specific cohort trends, and are estimated using OLS. Differences in the number of observations reflect differences in the NAES modules in which each outcome was included. The omitted dropout age category is dropout age \leq 15. Standard errors clustered by state are in parentheses. * $p < 0.05$.

More specifically, column (1) shows that raising the school leave age to 16, relative to 15 or less, increases the number of completed grades of high school by 1.01 grades for black and Hispanic students. Relative to a leaving age of 16, further raising the leaving age to 17 or above increases the number of completed grades of high school by 0.15 for black and Hispanic students. Both differences are statistically significant. Similarly, column (2) shows that raising the leaving age to 16 increases the number of completed grades of schooling for women by 0.10, while raising

Table A12: The effect of an additional completed grade of high school on identifying as and voting Democrat, by minority status and gender

	Democrat support					
	Partisan (1)	Intend (2)	Voted (3)	Partisan (4)	Intend (5)	Voted (6)
Completed grades	-0.154 (0.131)	-0.213 (0.136)	-0.192 (0.112)	-0.131 (0.100)	-0.185 (0.094)	-0.136 (0.083)
Completed grades × Black or Hispanic	0.050 (0.091)	0.069 (0.095)	0.064 (0.078)			
Completed grades × Male				0.009 (0.018)	-0.005 (0.023)	-0.015 (0.023)
First stage (ACS) observations	380,685	374,306	380,685	380,685	374,306	380,685
Reduced form (NAES) observations	164,606	134,461	117,733	164,606	134,461	117,733
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.33	0.41	0.37	0.33	0.41	0.37

Notes: All specifications include state grew up, cohort, and survey year fixed effects and state-specific cohort trends, and are estimated using TS2SLS. Lower-order interaction terms are omitted. Differences in the number of observations reflect differences in the NAES modules in which each outcome was included. Standard errors clustered by state are in parentheses. * $p < 0.05$.

the leaving age adds a further 0.03 grades. The effects among male students are significantly larger: raising the leaving age to 16 increases the number of completed grades of schooling for men by 0.32, while raising the leaving age adds a further 0.04 grades.

A.6.3 Heterogeneous instrumental variable estimates

Table A12 examines how the IV estimates vary with two pre-treatment characteristics: an indicator for being black or identifying ethnically as Hispanic; and an indicator for being male. In these regressions, I use both leaving age cutoffs and their interactions with either conditioning variable as instruments for the two endogenous variables—completed grades of schooling and its interaction with the conditioning variable. Although such students were more likely to be affected by raising the school leaving age (see Table A11 above), the interaction coefficients do not suggest that black or Hispanic or male students are more likely to change their voting behavior after receiving an additional grade of schooling. In addition some loss of precision arising from the inclusion of these interactions, some caution is required in interpreting these estimates because the first stage is significantly stronger for black or Hispanic and male students.

A.6.4 Reduced form mechanism estimates

Table A13 shows the reduced form estimates of the dropout age on the possible mediators examined in Table 4. The results reinforce the IV estimates.

Table A13: The effect of dropout ages on potential income and socialization mechanisms

	Conservative scale (1)	Reduce taxes (2)	Ban abortion (3)	Low gun controls (4)	Low health spending (5)	High military spending (6)	Republican non-economic issues (7)
Dropout age=16	0.053* (0.026)	0.033* (0.015)	-0.013 (0.017)	0.029 (0.020)	0.024 (0.023)	-0.005 (0.026)	-0.031 (0.027)
Dropout age \geq 17	0.061* (0.025)	0.038* (0.016)	-0.018 (0.017)	0.017 (0.023)	0.028 (0.027)	-0.018 (0.031)	-0.041 (0.032)
Observations	160,593	127,479	123,350	74,368	63,134	71,693	152,068
Outcome range	[-2.17,1.75]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	[-1.17,2.45]
Outcome mean	0.02	0.36	0.20	0.38	0.31	0.49	0.00
Difference: Dropout age \geq 17 - Dropout age=16	0.008 (0.012)	0.005 (0.004)	-0.005 (0.004)	-0.012 (0.007)	0.004 (0.01)	-0.013 (0.008)	-0.010 (0.011)
	Political knowledge scale (8)	Political interest scale (9)	Discuss politics (10)	Turnout (pres. election) (11)	Trust federal govt. (12)	Protect enviro. more (13)	
Dropout age=16	-0.034 (0.033)	-0.030 (0.027)	-0.046* (0.023)	0.009 (0.006)	0.004 (0.018)	-0.018 (0.012)	
Dropout age \geq 17	-0.037 (0.036)	-0.034 (0.030)	-0.055* (0.026)	0.008 (0.007)	-0.003 (0.018)	-0.020 (0.014)	
Observations	116,233	165,370	157,454	140,316	50,440	116,050	
Outcome range	[-1.21,1.33]	[-2.27,2.30]	[-1.19,1.53]	{0,1}	{0,1}	{0,1}	
Outcome mean	0.03	0.04	0.02	0.86	0.25	0.13	
Difference: Dropout age \geq 17 - Dropout age=16	-0.003 (0.012)	-0.004 (0.009)	-0.008 (0.008)	-0.001 (0.003)	-0.007 (0.006)	-0.002 (0.003)	

Notes: All non-binary outcome variables are standardized; see Appendix section A.1 for detailed variable definitions. All specifications include state grew up, cohort, and survey fixed effects and state-specific cohort trends, and are estimated using OLS. Differences in sample size reflect data availability. Standard errors clustered by state in parentheses. * $p < 0.05$.

A.6.5 Additional first stage estimates

The first stage estimates (and standard errors in parentheses) corresponding to column (4) of Table 2 are 0.180 (0.045) and 0.218 (0.049) respectively for the Dropout age=16 and Dropout age \geq 17 coefficients. As noted in the main text, the first stage F statistic is 9.9 and the sample contains 374,306 observations. The p value for the difference between the coefficients is 0.125.

Table A14 reports the first stage estimates corresponding with the instrumental variable estimates in Table 4.

A.6.6 Alternative restrictions on minimum age in the sample

To demonstrate that the results do not reflect restricting the sample to those aged 25 or above, panels A and B of Table A15 respectively show similar results restricting the sample to those aged 18 or above or those aged 30 or above.

A.7 Implications of raising the dropout age for downstream electoral outcomes

The preceding analysis has identified substantial political effects of implementing policies to increase the dropout age. This suggests that, at least in politically competitive areas, the changes in voting behavior could have altered state-level electoral outcomes, at least once the number of affected cohorts accumulate to reach a critical mass.

Quantifying such effects on electoral outcomes is challenging for at least two reasons. First, it is difficult to estimate the proportion of the electorate affected by dropout age reforms at any given election. Second, as shown above, the effects of schooling become most pronounced in later life when the returns to education are greatest. Both challenges imply that the effect of raising the dropout age on electoral outcomes may only become noticeable many decades after the reform. To tentatively estimate the electoral effects of raising the minimum school leaving age, I use the following generalized difference-in-differences specification:

$$Y_{st} = \delta_1 1(\text{dropout age}_{st-45} = 16) + \delta_2 1(\text{dropout age}_{st-45} \geq 17) + \theta_s + \eta_t + \varphi_s \text{year}_t + \varepsilon_{st} \quad (\text{A18})$$

where Y_{st} is a measure of state-level Republican political representation in state s in election year t between 1959 and 2010. The dropout age is lagged by 45 years to allow the number of affected cohorts to accumulate over time and reach the peak effect of high school's influence on political behavior. State-specific trends, $\varphi_s \text{year}_t$, are included to control for differential trends across states which implemented a leaving age reform 45 years earlier and those that did not.

The results in Table A16 suggest that prior dropout age reforms have influenced state electoral outcomes. Columns (1) and (3) show that increasing the dropout age to 16 decreased the Democrat seat share in the state House and Senate 45 years later by 3.8 and 5.1 points respectively. These estimates broadly parallel the individual-level estimates in Table 1. Raising the leaving age to 17 or above similarly decreased Democratic representation in state chambers, but has not reduced the seat share beyond raising the dropout age to 16. This may reflect the fact that such reforms

Table A14: First stage estimates accompanying Table 4

	Completed grades						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dropout age=16	0.175* (0.044)	0.176* (0.044)	0.176* (0.044)	0.209* (0.047)	0.209* (0.047)	0.209* (0.047)	0.176* (0.044)
Dropout age \geq 17	0.214* (0.048)	0.213* (0.048)	0.213* (0.048)	0.246* (0.052)	0.246* (0.052)	0.246* (0.052)	0.213* (0.048)
Observations	380,685	378,454	378,454	265,041	265,041	265,041	378,454
Outcome range	[0,12]	[0,12]	[0,12]	[0,12]	[0,12]	[0,12]	[0,12]
Outcome mean	11.64	11.64	11.64	11.63	11.63	11.63	11.64
First stage F statistic	10.1	9.7	9.7	11.7	11.7	11.7	9.7
	Completed grades						
	(8)	(9)	(10)	(11)	(12)	(13)	
Dropout age=16	0.187* (0.046)	0.175* (0.044)	0.175* (0.044)	0.176* (0.044)	0.179* (0.045)	0.209* (0.046)	
Dropout age \geq 17	0.223* (0.052)	0.214* (0.048)	0.214* (0.048)	0.213* (0.048)	0.219* (0.048)	0.247* (0.050)	
Observations	356,058	380,685	380,685	378,454	376,537	267,272	
Outcome range	[0,12]	[0,12]	[0,12]	[0,12]	[0,12]	[0,12]	
Outcome mean	11.64	11.64	11.64	11.64	11.64	11.63	
First stage F statistic	9.5	10.1	10.1	9.7	10.3	12.5	

Notes: All specifications include state grew up, cohort, and survey year fixed effects and state-specific cohort trends, and are estimated using OLS. Differences in the number of first stage observations reflect adjustments to the ACS sample to account for survey years, cohorts, and states where the NAES survey outcome was not elicited. The omitted dropout age category is dropout age ≤ 15 . Standard errors clustered by state are in parentheses. * $p < 0.05$.

Table A15: The effect of school dropout age on schooling and identifying as and voting Democrat and Republican, alternative age restrictions

	Completed grades		Democrat support		Republican support		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Aged 18 or above							
Dropout age=16	0.199*	-0.020	-0.029*	-0.012	0.015	0.038*	0.028
	(0.046)	(0.014)	(0.013)	(0.013)	(0.014)	(0.012)	(0.017)
Dropout age≥17	0.241*	-0.032	-0.040*	-0.031*	0.032	0.052*	0.045*
	(0.053)	(0.016)	(0.015)	(0.016)	(0.018)	(0.014)	(0.018)
Observations	398,629	176,796	144,742	125,196	176,796	144,742	125,196
Outcome range	[0,12]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	11.64	0.33	0.42	0.36	0.30	0.46	0.45
Difference: Dropout age≥17	0.041*	-0.012*	-0.011	-0.019*	0.017*	0.014	0.017*
- Dropout age=16	(0.020)	(0.006)	(0.006)	(0.008)	(0.007)	(0.007)	(0.007)
Panel B: Aged 30 or above							
Dropout age=16	0.150*	-0.017	-0.034*	-0.016	0.010	0.039*	0.029
	(0.042)	(0.017)	(0.015)	(0.014)	(0.015)	(0.015)	(0.018)
Dropout age≥17	0.190*	-0.030	-0.049*	-0.037*	0.031	0.055*	0.042*
	(0.046)	(0.020)	(0.018)	(0.015)	(0.019)	(0.018)	(0.019)
Observations	354,821	153,066	124,826	110,533	153,066	124,826	110,533
Outcome range	[0,12]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	11.62	0.33	0.41	0.38	0.31	0.47	0.48
Difference: Dropout age≥17	0.040	-0.013*	-0.015*	-0.022*	0.021*	0.015	0.013
- Dropout age=16	(0.025)	(0.006)	(0.006)	(0.007)	(0.008)	(0.008)	(0.007)

Notes: All specifications include state grew up, cohort, and survey year fixed effects and state-specific cohort trends, and are estimated using OLS. Differences in the number of observations reflect differences in the NAES modules in which each outcome was included. The omitted dropout age category is dropout age ≤15. Standard errors clustered by state are in parentheses. * $p < 0.05$.

Table A16: Effect of dropout age reform on downstream Democrat state-level seat shares

	Democrat state House seat share (1)	Democrat state Senate seat share (2)
Dropout age (45 year lag)=16	-0.037* (0.013)	-0.049* (0.016)
Dropout age (45 year lag) \geq 17	-0.028 (0.019)	-0.032 (0.024)
Observations	1,120	1,037
Outcome range	[0.1,1]	[0.1,1]
Outcome mean	0.55	0.55
Difference: Dropout age \geq 17 - Dropout age=16	0.009 (0.017)	0.017 (0.021)

Notes: All specifications include state and election fixed effects and state-specific time trends, and are estimated using OLS. Differences in the number of observations reflect missing data. The omitted dropout age category in columns (1) and (2) is dropout age (45 year lag) \leq 15. Standard errors clustered by state in parentheses. * $p < 0.05$.

became more frequent later in the twentieth century (as indicated by the larger standard errors), and thus the effects have yet to fully materialize. However, these estimates remain tentative, given the challenges noted above and the large time gap between treatment and outcome. Nevertheless, the results suggest that the dropout age may have produced important electoral consequences demanding further investigation.