From the Classroom to the Ballot Box: Turnout and Partisan Consequences of Education*

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October 2025

Abstract

We estimate the impact of education on voter turnout and partisanship using a regression discontinuity design based on school-entry cutoffs and exact date of birth. Drawing on nationwide administrative voter registration data, we find that individuals who were slotted to enter school one year earlier are more likely to vote and more likely to register as independents. These reduced-form effects may be driven by changes in educational attainment or by differences in the quality of individuals' educational experiences. We leverage age-related heterogeneity in effect sizes to isolate the role of educational attainment. Our results imply that an additional year of schooling increases turnout by about 3 percentage points.

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1. Introduction

Differences in educational attainment are not only responsible for daunting economic cleavages (Juhn et al. 2003; Goldin and Katz 2010). Educational divides also carry into contemporary politics, where polarization between more and less educated Americans has intensified in recent decades, and higher levels of schooling are associated with greater political participation (see, e.g., Bonica et al. 2013; Gethin et al. 2022). Motivated by these correlations, we ask: What is the causal effect of education on voter turnout and partisanship in the United States?

We provide new evidence on this question by leveraging a regression discontinuity (RD) design around school-entry cutoff dates. Prior research has shown that starting school at a younger age, by virtue of being born just before the cutoff, increases educational attainment (Angrist and Krueger 1991, 1992; Dobkin and Ferreira 2010; Cook and Kang 2016). We begin our analysis by verifying the first-stage effect of early school entry on education. Drawing on the universe of respondents to the long form of the 2000 Decennial Census, we find that individuals born just a few days before the cutoff date have, on average, 0.03 more years of schooling than those who were born just after.

In the second part of our analysis, we estimate the reduced-form effect of early school entry on voter turnout and partisanship. Our results indicate that early school entry has no appreciable impact on individuals' propensity to register to vote. Conditional on being registered, however, we estimate that being born just before the school-entry cutoff increases voter turnout by approximately 0.25 percentage points (p.p.). We also find a small reduction in partisanship. Children who were slotted to enter school earlier are about 0.18 p.p. less likely to register with *either* of the two major parties as adults.

In the third and final part of our analysis, we examine the mechanisms behind these reduced-form effects. We take seriously the idea that early school entry may affect both the quantity and the quality of individuals' education, e.g., through school readiness or relative-age effects (Bedard and Dhuey 2006; Elder and Lubotsky 2009; Dobkin and Ferreira 2010; Black et al. 2011). Our method to isolate the quantity channel builds on an observation about age-related heterogeneity in the effects of early school entry. We note that the discontinuity in educational attainment around the school-entry cutoff reflects both a temporary, one-year gap among college-going individuals and a persistent difference among adults who have completed their education. As a result, the reduced-form effect on years of schooling is much larger for individuals below the age of 25. Under the assumption that the effect of early school entry on political outcomes through the *quality* channel remains constant as adults age, we can use the observed heterogeneity to estimate a pure *quantity* effect. Our results imply that voter turnout increases by approximately 3 p.p. per additional year of education.

2. Related Literature

Our findings add to a large literature examining the impact of education on political participation, and a smaller one studying effects on partisanship. Early contributions to these literatures rely on observational comparisons of individuals with different levels of schooling, often by matching on observable characteristics (see, e.g., Tenn, 2007; Kam and Palmer, 2008; Berinsky and Lenz, 2011). These studies typically find that education and voter turnout are positively correlated. More credible research designs, however, have produced mixed results. Milligan et al. (2004), for instance, leverage changes in compulsory schooling laws across cohorts to provide evidence that educational attainment affects turnout in the United States but not the United Kingdom. Sondheimer and Green (2010) analyze data from the Project STAR and Perry Preschool experiments. Their findings suggest that educational interventions that increase high-school graduation also increase turnout. The point estimates, however, are too imprecise to be statistically distinguishable from zero.

More recently, Marshall (2016, 2019) exploits variation in school dropout laws across cohorts. His estimates for the U.S. (U.K.) imply that an additional year of high-school education decreases (increases) support for the Democratic (Conservative) Party by 15 (12) p.p. In simultaneous, unpublished work, Firoozi (2025) estimates the impact of education on partisanship using discontinuities in compulsory schooling laws and college admissions policies in California and Florida. He finds that an additional year of education reduces Republican Party affiliation by about 2 p.p.

Our work improves upon existing studies by combining the clean identification afforded by an exact-date-of-birth RD design with administrative data on partisanship and voter turnout in all fifty states. We thus circumvent common concerns about reporting biases in surveys on political behavior, while the nationwide samples help to bolster the external validity of our results.

We also contribute to a large literature in labor economics. Prior work studying early school entry and educational attainment has either relied on nationwide data in combination with the quarter-of-birth instrument pioneered by Angrist and Krueger (1991, 1992), or it has used state-level data in combination with an exact-date-of-birth RD (e.g., Dobkin and Ferreira 2010). To the best of our knowledge, we are the first to estimate the first-stage effect of early school entry with a clean research design in a large, nationally representative dataset. Given that the long form of the 2000 Census was administered to a random sample of roughly one in six U.S. households, we even have enough statistical power to examine age-related heterogeneity in the impact of early school-entry on years of schooling. This allows us to document that the discontinuity in educational attainment around the school-entry cutoff reflects both a temporary, one-year gap among college-going individuals and a persistent difference among adults who have completed their education. We build on this observation to develop a new method for disentangling how early school entry affects later-life outcomes through the quantity versus the quality of education.

3. Data and Descriptive Statistics

Our analysis relies on three main sources of data: state-by-year information on school entry cutoff dates from Bedard and Dhuey (2007, 2012), educational attainment from the Decennial Census, as well as administrative records for all registered voters in the United States.

Census Data Our restricted-use Census data cover the universe of respondents to the long form of the 2000 Decennial Census. The long form is distributed to roughly one in six households in the U.S. In contrast to the short form, it asks respondents the highest degree or level of schooling they completed. Critically for our purposes, the confidential micro-data also contain respondents' exact date of birth.

Both pieces of information are necessary to verify the first-stage effect of early school entry on educational attainment. Since we only have information on school-entry cutoffs from 1964–2005, we restrict the Census sample to individuals aged 18–40 in 2000.[1,2]

Voter Registration Records Our data on registered voters come from L2, Inc., a non-partisan for-profit data vendor that maintains high-quality databases of registered voters and political donors.[3] L2 collects information on voters from different administrative and commercial sources, including local election boards, Secretaries of State, and the Federal Election Commission (FEC). Importantly, the available information includes individuals' turnout history, party affiliation, and date of birth.

The partisanship of individuals in the L2 data coincides with the official party affiliation in the respective states' voter registration lists in all but sixteen states. The remaining states do not collect information on party preferences. For voters in these states, L2 uses predictive modeling to impute a likely party affiliation.[4] Per the company, their proprietary machine-learning algorithms use an array of public and private data sources, including participation in partisan primaries, demographics available through states' voter files, exit polling from presidential elections, commercial lifestyle indicators, census data, and self-reported preferences from private polling. L2 does not guarantee that any given voter will self-identify as

[1] Someone who turned five in 1964 and thus entered kindergarten in that year would be approximately 40 years old in 2000. We do not consider individuals younger than 18 because minors are not allowed to vote in federal elections.

[2] We work with the 2000 rather than the 2010 or 2020 Decennial Censuses because the latter did not administer a long form survey.

[3] Our description of the L2 data borrows heavily from Spenkuch et al. (2023) and Kaplan et al. (2025).

[4] Specifically, L2 models party affiliation in the following states: AL, GA, HI, IL, IN, MI, MN, MO, MT, ND, OH, SC, TX, VA, VT, and WA.

being associated with the assigned "likely party," but it claims an accuracy level of 85% or better. To address potential concerns about imputed partisanship, we show results using data from all states as well as from non-modeled states only.

Since the L2 data are current as of early 2021 and since we only have information on school-entry cutoffs for birth cohorts from 1959–2000, we restrict our sample of registered voters to individuals aged 20–61 in 2021.

School-Entry Cutoffs Bedard and Dhuey (2007, 2012) collect information on state-level school-entry cutoff dates for the period from 1964 to 2005. We use their data and assign each individual in our Census and voter samples the cutoff that is closest in time to their fifth birthday. Since we do not observe where someone lived when they entered school, we base this assignment on individuals' current state of residence.[5] Doing so introduces measurement error in school-entry cutoff dates, which biases our first-stage and reduced-form estimates towards zero.[6] Measurement error should not, however, affect the ratio of these estimates, which plays an important role in isolating the "quantity effect" of education (see Section 6).

Additional Sample Restrictions We impose three additional restrictions on both the Census and voter registration samples. First, voter registration offices often replace missing birth dates with January 1 (when month and day are missing) or the first day of the month (when only the day is missing). This practice creates a large spike in reported birthdays on January 1 and smaller spikes on the first of other months. The absence of corresponding dips on neighboring dates suggests that the excess mass is due to imputations rather than intentionally timed births or intentional misreportings. Since we cannot determine which voters with a recorded birthday on the first day of the month had their birth date imputed, we exclude all of them. To ensure comparability across samples, we apply the same restriction to the Census data.

Second, we drop all observations with school-entry cutoffs between October 15 and November 17. We impose this restriction because political campaigns often target first-time voters, with effects that can be highly persistent (Meredith 2009; Coppock and Green 2016). By excluding natural experiments that are susceptible to disproportionate targeting of individuals on one side of the cutoff, we guard against our results being driven by the (long-run) effects of political campaigning.

[5] To alleviate concerns that early school entry might induce differential out-migration, we have explored the robustness of our Census-based findings to assigning cutoffs based on individuals' state of birth. Reassuringly, we obtain qualitatively similar results.

[6] To get a better sense of the likely magnitude of the bias, we note that, as of the 2000 Census, approximately 56% of adults aged 18–40 still live in their state of birth, and 75% live in a state that shares the same entry cutoff as their state of birth. The corresponding numbers for adults aged 19–61 are 53% and 73%, as of the 2021 ACS. These statistics suggest our first-stage and reduced-form estimates may be understated by about one-quarter.

Third, we exclude individuals who were born on the day of, the day after, or the day before their assigned cutoff. Compliance with school-entry policies is likely especially low for children born on these three dates. Excluding them from our analysis helps to increase the strength of the instrument without sacrificing the exogeneity of our research design.[7,8]

Summary Statistics Table 1 displays descriptive statistics for the key variables in our analysis. Because the long form of the Census is randomly distributed to households, the demographic composition of individuals in the upper panel closely mirrors that of the broader population. The only notable difference is with respect to age, which is lower because of the sample restrictions detailed above. On average, individuals in the Census data have 13 years of education.

The lower panel of Table 1 summarizes our voter registration data. Approximately 42% of registrants are Democrats, while 29% are Republicans. The remaining 30% are affiliated with neither of the two major parties. For simplicity, we refer to the latter set of voters as "independents." Conditional on being registered to vote, the average turnout in general elections between 2008 and 2020 is 60.2%.

Cols. 2 and 3 restrict attention to individuals born within 30 days around the cutoff. Their characteristics closely resemble those of the full sample, which may help to alleviate concerns about the external validity of our RD design. We also observe that individuals on either side of the cutoff have similar observable characteristics, like gender and race. The small difference in age is mechanical.

Finally, the last column of Table 1 computes raw differences between individuals born on either side of the cutoff. Our main results manifest even in these simple comparisons of means: early school entry increases educational attainment, voter turnout, and the likelihood of registering with neither of the two major parties.

4. Econometric Strategy

A long literature, starting with Angrist and Krueger (1991), uses small differences in birth dates around school-entry cutoffs as an instrument to estimate the impact of education on wages. We directly build on this literature. To investigate the first-stage effect of early school entry on education, we estimate RD models of the following form:

$$(1) \qquad Y_{i,d,s,c} = \alpha + \beta T_{i,d,s,c} + f_0(d) + T_{i,d,s,c} \times f_1(d) + \tau_{s,c} + \epsilon_{i,d,s,c}$$

[7] We show below that children born a few days before and after the cutoff are still comparable on observables (see also Appendix Table A.1)

[8] Appendix Table A.5 demonstrates that our results are robust to including school-entry cutoffs near general election dates. Appendix Table A.6 shows that our results are robust to including the donut region of births within -1 to 1 days of the entry cutoffs. Finally, Appendix Table A.7 shows that our results are robust to including first-day-of-the-month births in states where dates of birth are only rarely imputed.

Table 1: Summary Statistics

	(1)	(2)	(3)	(4)
	Full Sample	Born 2 to 30 Days Before Cutoff	Born −30 to −2 Days After Cutoff	Col. (2) − Col. (3)
Panel A. Decennial Census Records				
Years of Schooling	13.150	13.160	13.130	0.0267***
	[2.646]	[2.607]	[2.631]	(0.0060)
Female	0.510	0.512	0.512	0.0003
	[0.500]	[0.500]	[0.500]	(0.0009)
Age in 2000	29.560	29.480	29.570	–0.0897**
	[6.231]	[6.208]	[6.253]	(0.0357)
White	0.689	0.687	0.688	–0.0005
	[0.463]	[0.464]	[0.463]	(0.0010)
Black	0.107	0.110	0.109	0.0009
	[0.309]	[0.313]	[0.312]	(0.0007)
Hispanic	0.154	0.153	0.154	–0.0004
	[0.361]	[0.360]	[0.361]	(0.0008)
Asian	0.044	0.044	0.044	–0.0003
	[0.206]	[0.205]	[0.205]	(0.0004)
Other Race	0.011	0.011	0.011	0.0000
	[0.102]	[0.102]	[0.102]	(0.0002)
Observations	8,057,000	661,000	671,000	1,331,000
Panel B. Voter Registration Records				
Democrat	0.417	0.416	0.418	–0.0012***
	[0.493]	[0.493]	[0.493]	(0.0003)
Republican	0.285	0.284	0.285	–0.0010***
	[0.451]	[0.451]	[0.452]	(0.0003)
Independent	0.299	0.299	0.297	0.0022***
	[0.458]	[0.458]	[0.457]	(0.0003)
Turnout, 2008-2020	0.602	0.602	0.600	0.0012***
	[0.363]	[0.363]	[0.363]	(0.0003)
Female	0.523	0.524	0.524	0.0002
	[0.499]	[0.499]	[0.499]	(0.0003)
Age In 2000	19.20	19.20	19.25	–0.0523***
	[12.00]	[12.05]	[12.05]	(0.0180)
Observations	90,120,739	7,485,811	7,523,282	15,009,093

Notes: Entries are summary statistics for the key variables in our analyses. Panel A is based on data from the 2000 Decennial Census. Entries in this panel are rounded per Census disclosure rules. Panel B is based on voter registration data from L2. Col. (1) displays statistics for the full sample, col. (2) restricts attention to individuals born just before the school-entry cutoff, col. (3) restricts attention to individuals born just after the cutoff, and col. (4) presents the difference between cols. (2) and (3). In cols. (1)–(3), we present means and, in brackets, standard deviations. Col. (4) shows differences in means and, in parentheses, standard errors.

where $Y_{i,d,s,c}$ denotes the outcome for individual i, whose birth date places them d days from the relevant cutoff for cohort c in state s. $T_{i,d,s,c}$ is an indicator variable that is equal to one if i is born *before* the cutoff, and zero otherwise. $f_0(d)$ and $f_1(d)$ control for trends in the running variable below and above the cutoff, and $\tau_{s,c}$ corresponds to a state-cohort fixed effect. Note, we "reverse code" the running variable so that positive (negative) values indicate that the person was born before (after) the relevant school-entry cutoff.

We rely on the same model to estimate the reduced-form effect of early school entry on partisanship. When examining turnout, we pool across all general elections from 2008–2020, interact $\tau_{s,c}$ with an indicator for each general election, and restrict attention to observations for individuals who were, in fact, old enough to vote in a given year.[9] In all specifications, standard errors are clustered at the state-cohort level, which is the level at which the cutoff is determined.

The identifying assumption in our research design is that potential outcomes are a continuous function of birth dates at the school-entry cutoff. Under this assumption, $\hat{\beta}$ corresponds to the causal effect of being eligible to enter kindergarten one year earlier. For simplicity, we refer to our RD estimates as "effects of early school entry" though we explicitly note that compliance with school entry laws is often far from perfect and that individuals' contemporaneous state of residence need not be their state of residence when they entered kindergarten. Both of these issues attenuate our first-stage and the reduced-form coefficients.

Under the additional assumption that early school entry affects political behavior only through its impact on years of schooling, we can scale our reduced-form estimates by the first-stage in order to recover the local average treatment effect of an additional year of education on turnout and partisanship. This exclusion restriction, however, is unlikely to be exactly satisfied, especially since prior research provides evidence that early school entry may negatively impact academic performance (see Bedard and Dhuey 2006; Dobkin and Ferreira 2010; Elder and Lubotsky 2009). In Section 6, we return to the issue of disentangling the quantity and quality channels. Until then, we content ourselves with presenting simple RD estimates based on the specification above.

Appendix Table A.1 presents standard balance checks in order to provide supporting evidence for the internal validity of our RD design. Although the only pre-determined covariates in our data are race and gender, it is reassuring that the coefficients in this table imply only trivial differences along either dimension, with 95%-confidence intervals that rule out discontinuities greater than 0.3 p.p.

Appendix Figure A.1 shows the number of individuals in our data for each value of the

[9] In other words, individuals who were 17 years old in 2008 do not enter the sample for the 2008 election, but are included for all general elections thereafter.

running variable. Given that parents have only limited control over the exact timing of births, and since school-entry laws can change from one year to the next, we would not expect systematic sorting of individuals around the cutoff. The upper panel of Appendix Figure A.1 shows that there is, indeed, no discontinuity in the number of observations on either side of the cutoff in the Census data. The lower panel of Appendix Figure A.1 replicates this result in the voter registration data. The absence of a discontinuity in this sample is noteworthy because it implies that early school entry has no impact on individuals' propensity to register to vote.[10]

We further explore registration effects in Appendix Table A.2. We do not find evidence of a discontinuity in the density of observations in either the L2 or the Census data. Moreover, we conduct formal "manipulation tests" following Cattaneo et al. (2020), using bandwidths selected based on the MSE of the density estimators. We fail to reject the null of no manipulation in both the L2 and Census samples, with p-values of 0.84 and 0.20, respectively.

5. RD Estimates

5.1. *First-Stage Effects on Education*

The upper panel of Figure 1 visualizes the impact of early school entry on years of schooling. We observe a small but clear discontinuity around the threshold. Individuals born just before the threshold have, on average, 0.034 more years of schooling than those born a few days afterwards ($p < 0.001$).[11]

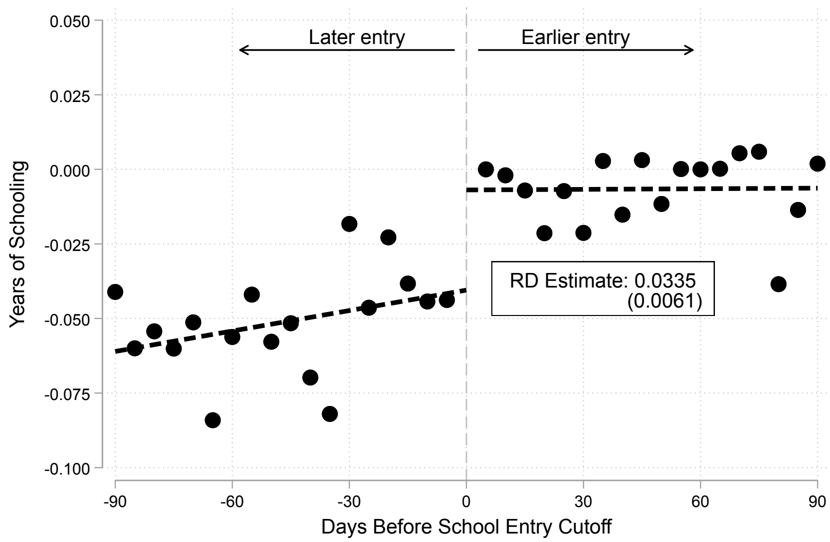
We examine the impact of early school entry on the *distribution* of educational attainment in the lower panel of Figure 1. To this end, we construct a set of indicator variables for whether an individual has completed at least x years of schooling. We then estimate the model in eq. (1) for each of these dependent variables. The results imply that early entry has the largest effect on completing 10–14 years of schooling—with a peak impact of about 0.5 p.p. on completing high school, followed closely by completing two years of college. We also estimate effects on completing exactly x years of schooling (see Appendix Figure A.2). Evidently, being eligible to enter school earlier makes it less likely that individuals drop out between grades 8–11, and more likely that they have completed high school, two, and, perhaps, even four years of college. As we demonstrate in Section 6, however, the positive estimates on college attainment are, at least in part, mechanical. Among college-going adults, early school entrants are one academic year ahead of those born just a few days after the cutoff.

[10]Both panels of Appendix Figure A.1 show large declines in the number of observations in roughly thirty-day intervals around the cutoff date. These declines are a mechanical consequence of the sample restrictions detailed in Section 3.

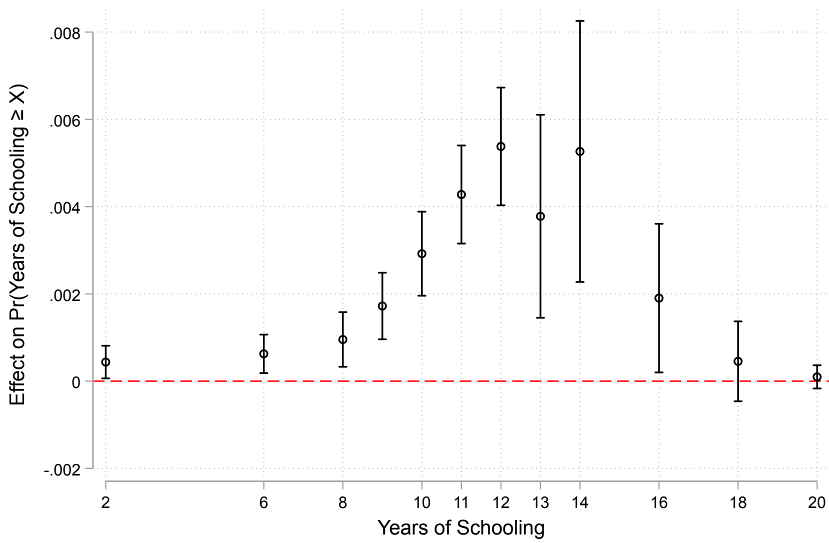
[11]Appendix Figure A.3 shows that this finding is robust to choice of bandwidth and functional form.

Figure 1: Educational Attainment

(a) Years of Schooling



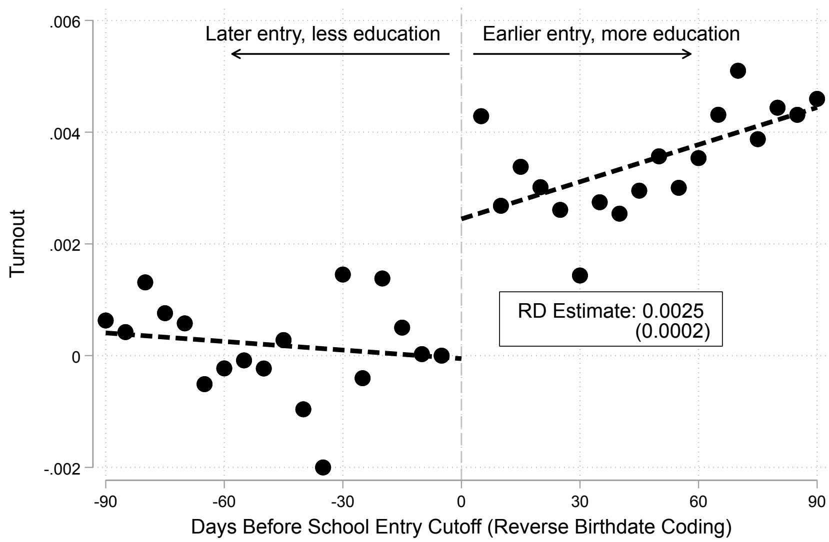
(b) Years of Schooling $\geq X$



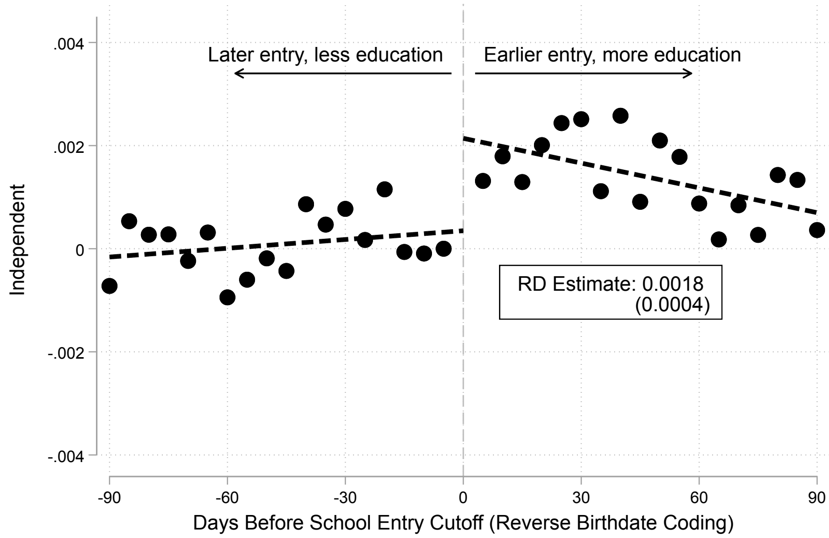
Notes: Panel (a) displays average educational attainment within five-day bins of the running variable, relative to the 0 to 5 day bin, for individuals within +/- 90 days of the relevant school-entry cutoff. The running variable is reverse-coded so that positive values indicate the individual is born *before* the cutoff, and thus will enter school in the current year. Panel (b) displays point estimates and 95% confidence intervals for the effect of early entry on having completed at least a certain number of years of schooling.

Figure 2: Voter Turnout and Partisanship

(a) Voter Turnout



(b) Registered Independent



Notes: Figure displays average outcomes within five-day bins of the running variable, relative to the 0 to 5 day bin, for individuals within +/- 90 days of the relevant school-entry cutoff. The running variable is reverse-coded so that positive values indicate the individual is born *before* the cutoff, and thus will enter school in the current year. Negative values indicate the individual is born after the cutoff, and thus will enter school in the following year. In panel (a), the outcome is voter turnout, pooled across all general elections from 2008–2020. In panel (b), the outcome is an indicator for independent party affiliation, meaning the individual is not registered as either Democrat or Republican. Only non-modeled states are used in panel (b).

5.2. *Reduced-Form Effects on Turnout and Partisanship*

Figure 2 presents our reduced-form estimates of the impact of early school entry on voter turnout and partisanship. The evidence therein shows that children born just before the cutoff date are about 0.25 p.p. more likely to vote later in life than individuals born just a few days later ($p < 0.001$). Although the former appear to be more politically engaged, they are 0.18 p.p. *less* likely to register with either of the two major parties ($p < 0.001$). In other words, early school entry increases voter turnout and decreases partisanship.[12]

Table 2 compares estimates for presidential and midterm elections (upper panel), and for all and non-modeled states (lower panel). In the latter set of states, voters' party affiliation is not imputed. We find similar effects of early school entry on turnout in presidential and midterm elections—although, for the latter, the effect size is larger relative to the mean.[13] We also obtain similar estimates when we compare the effect on partisanship across both sets of states, suggesting that imputation of party affiliation is not a major concern.

Although the estimates in the lower panel of Table 2 suggest that the observed increase in independents comes mainly at the cost of Republican registration, our robustness checks in Appendix Figures A.8 and A.9 show that this conclusion is sensitive to the choice of bandwidth. While we cannot say whether early school entry decreases Republican registration rates more than Democratic ones, we do observe a robust positive impact on registering as independent.

6. Heterogeneity by Age and the Quantity of Education

Early school entry can change political behavior because it alters the *quantity* of schooling or because it affects the *quality* of individuals' educational experiences (e.g., through school readiness or relative-age effects). In our setting, relative-age effects may increase participation through changes in pro-social attitudes or due to peer effects from having more older, voting-age friends. To isolate the quantity channel, we exploit the fact that, for young adults, the first-stage effect of early school entry varies with age. Among college-going adults, early school entrants are mechanically (but only temporarily) one academic year ahead of their peers born just a few days after the cutoff (see also McCrary and Royer 2011). As individuals complete their post-secondary education, this gap shrinks until it only reflects the permanent effect of early entry on educational attainment. Below, we show that if the quality effect of early school entry is approximately age-invariant by the time individuals reach adulthood, then the age gradient in the reduced form, scaled by the gradient in the first stage, isolates

[12] Appendix Figures A.4 and A.8 explore the robustness of these results to the choice of bandwidth and functional form.

[13] Appendix Figure A.5 presents results separately by election. We observe a larger point estimate for 2008, roughly constant effect sizes from 2010–2018, and a smaller impact in 2020.

Table 2: Effect of Early School Entry on Turnout and Partisanship

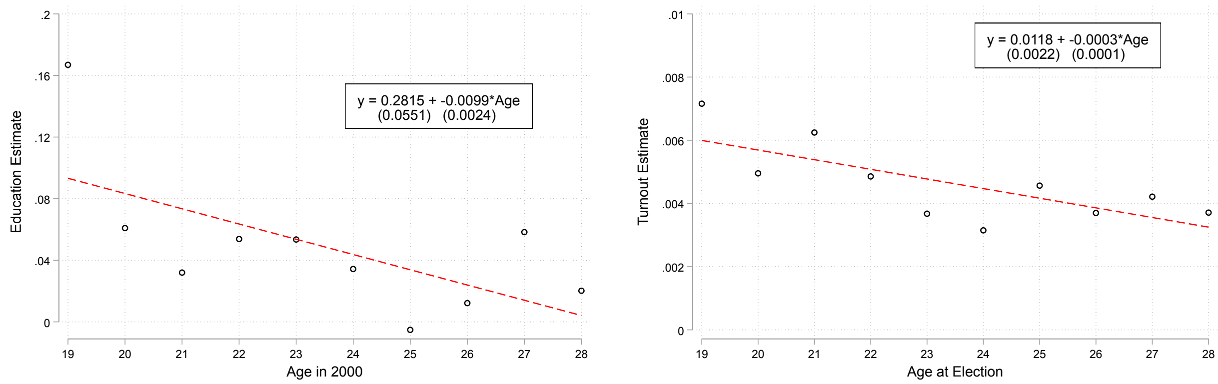
	(1)	(2)	(3)			
	General Election Turnout					
	All Years	Presidential	Midterms			
Born Before Cutoff	0.0025***	0.0025***	0.0024***			
	(0.0002)	(0.0002)	(0.0002)			
Constant	0.6041***	0.7112***	0.4551***			
	(0.0002)	(0.0001)	(0.0002)			
Dep. Var. Mean	0.6060	0.7132	0.4570			
R-squared	0.1340	0.0510	0.0958			
Observations	221,163,187	128,644,490	92,518,697			

	(1)	(2)	(3)	(4)	(5)	(6)
	Party Affiliation					
	All States			Non-modeled States		
	Democrat	Republican	Independent	Democrat	Republican	Independent
Born Before Cutoff	–0.0002	–0.0016***	0.0018***	–0.0007	–0.0011***	0.0018***
	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0004)
Constant	0.4178***	0.2843***	0.2978***	0.4072***	0.2765***	0.3163***
	(0.0003)	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0003)
Dep. Var. Mean	0.4169	0.2844	0.2987	0.4058	0.2775	0.3167
Observations	45,177,003	45,177,003	45,177,003	27,154,495	27,154,495	27,154,495
R-squared	0.0498	0.0702	0.0864	0.0376	0.0569	0.0604

Notes: Entries are point estimates and standard errors from estimating the regression discontinuity model in eq. (1), modified for the upper panel as described in Section 4, given that the turnout analysis is conducted at the individual-by-election level. In the upper panel, col. (1) estimates the effect on voter turnout in all general elections from 2008–2020. Col. (2) estimates the effect on voter turnout in presidential elections during that period (i.e., 2008, 2012, 2016, and 2020). Col. (3) estimates the effect on voter turnout in midterm elections during that period (i.e., 2010, 2014, 2018). These specifications include state-by-cohort-by-election fixed effects. In the lower panel, cols. (1)–(3) show estimates for all states, while cols. (4)–(6) show estimates solely for non-modeled states (i.e., states in which party affiliation is not imputed by L2). Cols. (1) and (4) estimate the effect on Democratic registration, cols. (2) and (5) estimate the effect on Republican registration, and cols. (3) and (6) estimate the effect on independent registration. These specifications include state-by-cohort fixed effects. Appendix Tables A.3 and A.4 show that the turnout and party results are robust to the exclusion of fixed effects. Standard errors are clustered at the state-by-cohort level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Figure 3: Heterogeneous Effects by Age

(a) Educational Attainment (b) Turnout



Notes: Figure shows regression discontinuity estimates of early school entry on educational attainment (panel (a)) and voter turnout (panel (b)) for individuals aged 19–28. All estimates are based on the specification in eq. (1), estimated separately for individuals in each age group. The figure also shows estimated linear-in-age trend lines, with bootstrapped standard errors.

a pure quantity effect. We develop this idea and use it to quantify how much of the turnout response is attributable to additional years of schooling.

6.1. *Heterogeneity by Age*

Estimating eq. (1) separately by age, the left panel of Figure 3 shows a steep decline in the discontinuity in educational attainment between the ages of 19 and 28.[14] At age 19, individuals born just before the cutoff have, on average, completed roughly 0.17 more years of schooling than those born just after. The gap narrows to approximately 0.06 years at age 20, and 0.04 years at age 24. It becomes even smaller thereafter. This pattern is largely mechanical. As early entrants start each grade sooner, they have completed more schooling during their late teens and early twenties; but the gap closes once individuals complete their post-secondary education.

Turnout shows a similar but less pronounced decline. The right panel of Figure 3 plots reduced-form estimates by age. The impact of early school entry on turnout declines from ages 19 to 28. It is roughly flat thereafter (see Appendix Figure A.10).[15] The slower decline in turnout relative to education is consistent with extant evidence on short-run persistence

[14] For estimates for all ages, see Appendix Figure A.10. Figure 3 excludes 18-year-olds to avoid mechanical eligibility and school-based voter registration issues in the turnout estimates.

[15] To test whether these effects are due to heterogeneity by age rather than across cohorts, Appendix Figure A.11 plots estimated effects on turnout by age for all elections from 2008 to 2020. Except for the last two of these elections, we always observe a quantitatively similar downward-sloping relationship between effect size and age, which suggests that our results capture heterogeneity by age.

in voting behavior following an initial impetus (Meredith, 2009; Coppock and Green, 2016).

In Appendix Figure A.12, we conduct a similar effect-by-age analysis for partisanship. Among college-aged adults—when the first stage is largest—we observe negative reduced-form effects on Republican registration with offsetting, positive effects on registering as independent and, perhaps, Democrat (see also Appendix Table A.8 and Appendix Figure A.13).[16]

6.2. *Decomposing Quality versus Quantity*

If the impact on the quality of educational experiences—say from being younger than classmates or from being not quite ready to enter school—is approximately stable by the time people reach adulthood, then we can use the age gradients in the figure above to infer a "pure quantity effect" of education.

Formally, let β_a^{FS} and β_a^{RF} denote the first-stage and reduced-form effects of early school entry for individuals of age a. Assume that each can be expressed as the sum of a constant quality component, μ, and an age-varying quantity component, γ_a:

$$\beta_a^{FS} = \mu^{FS} + \gamma_a^{FS}, \qquad \beta_a^{RF} = \mu^{RF} + \gamma_a^{RF}. \tag{2}$$

Pooling across age groups and interpreting our RD setup through the standard instrumental variables (IV) framework (Hahn et al. 2001), the probability limit of the usual Wald estimator for the local average treatment effect of education is given by

$$\text{plim } \beta^{IV} = \frac{\beta^{RF}}{\beta^{FS}} = \frac{\mu^{RF} + \gamma^{RF}}{\mu^{FS} + \gamma^{FS}},$$

which confounds the quantity and quality channels.

To see how we can isolate the quantity channel, note that

$$\frac{\Delta\beta_a^{RF}}{\Delta\beta_a^{FS}} = \frac{(\mu^{RF} + \gamma_{a+1}^{RF}) - (\mu^{RF} + \gamma_a^{RF})}{(\mu^{FS} + \gamma_{a+1}^{FS}) - (\mu^{FS} + \gamma_a^{FS})} = \frac{\gamma_{a+1}^{RF} - \gamma_a^{RF}}{\gamma_{a+1}^{FS} - \gamma_a^{FS}}. \tag{3}$$

The numerator of this expression corresponds to the change in the outcome of interest between ages a and $a + 1$. The denominator scales this difference by the corresponding change in the quantity of education. Their ratio thus corresponds to a Wald-type estimate

[16] To assess whether our findings are driven by the particular political climate of 2021, we have acquired L2 data from 2014 and use it to show partisan effects by age. Appendix Table A.10 demonstrates that, with all states included, early school entry reduces Republican registration and raises non-affiliation. Restricting to non-modeled states only, the shift toward independents remains, though it is now driven by lower Democratic rather than Republican registration.

of the pure quantity effect among individuals who have one extra year of schooling due to starting college earlier.

In practice, we do not directly take eq. (3) to the data. Rather, we estimate β_a^{FS} and β_a^{RF} separately by age, regress the resulting estimates on linear trends for ages 19–28, and use the ratio of slopes to recover the quantity effect. Our empirical implementation thus relies on the additional assumption that the first-stage and reduced-form age gradients are approximately linear over the relevant age range. This is a strong assumption, but it allows us to efficiently pool across multiple age groups with noisy first-stage and reduced-form results.[17]

Figure 3 shows the fitted lines. The slope of the first-stage estimates is about –0.0099, while the slope for the reduced form is about –0.0003. Their ratio implies that an additional year of schooling raises turnout by roughly 3 p.p., which accounts for about 40 percent of the pooled IV estimate of 7.46 p.p. (see Appendix Table A.12).

We compute standard errors for the quantity effect as well as the share of the pooled IV estimate that is explained by the quantity channel under the assumption of zero covariance between the first-stage and reduced-form slopes. The resulting standard errors are approximately 1.2 p.p. and 0.18, respectively, which allows us to reject the null of no quantity effect on political participation. Appendix Table A.13 shows results omitting 19-year-olds, for whom the reduced-form point estimates are especially large. If anything, excluding 19-year-olds raises the estimated share of the quantity effect on turnout.[18,19]

[17] More formally, our "ratio of slopes" estimator can be derived as follows. Let β_a^{FS} and β_a^{RF} be as defined in eq. (2). Using Slutsky's theorem to take the probability limit of the ratio of the linear-in-age trends in the reduced-form and first-stage estimates gives:

$$\text{plim } \beta^{QIV} = \frac{\sum_a \left[\left(\mu^{RF} + \gamma_a^{RF} - (\overline{\mu}^{RF} + \overline{\gamma}^{RF})\right)(a - \overline{a})\right] / \sigma_a^2}{\sum_a \left[\left(\mu^{FS} + \gamma_a^{FS} - (\overline{\mu}^{FS} + \overline{\gamma}^{FS})\right)(a - \overline{a})\right] / \sigma_a^2},$$

where $\overline{a}$ and σ_a^2 correspond to the mean and variance of the age variable, while $\overline{\mu}$ and $\overline{\gamma}$ denote the average quality and quantity components. Under the assumption that the quality component is fixed by the time individuals reach adulthood (i.e., $\mu = \overline{\mu}$), and imposing constant age gradients so that $\gamma_a = \gamma a$ for all relevant a, the expression above simplifies to:

$$\text{plim } \beta^{QIV} = \frac{\sum_a \left(\gamma^{RF} a - \gamma^{RF} \overline{a}\right)(a - \overline{a})}{\sum_a \left(\gamma^{FS} a - \gamma^{FS} \overline{a}\right)(a - \overline{a})} = \frac{\gamma^{RF}}{\gamma^{FS}}.$$

Again, the numerator of this expression corresponds to the expected change in the outcome of interest between individuals in adjacent age groups, while the denominator scales this difference by the corresponding change in the quantity of education. The ratio of the linear-in-age trends in the reduced-form and first-stage estimates thus isolates the pure quantity effect of education for the subpopulation of compliers.

[18] Since the first-stage standard errors did not undergo Census Bureau disclosure review, we cannot report uncertainty estimates for these robustness checks.

[19] In Appendix Table A.11, we present evidence that the quality effect is also important in certain contexts. Specifically, we find that individuals entering school earlier are more likely to be incarcerated as of the 2000 Census, and that this effect is particularly strong for Black and Hispanic individuals. These results from our nationwide sample align with prior work focused on North Carolina (see Cook and Kang, 2016).

An important assumption behind our approach to disentangle the quantity and quality channels is that the causal effect of an additional year of education does not vary with age.[20] For turnout, this assumption appears reasonable. Regardless of whether the instrument operates at the college margin for younger cohorts or at the high school margin for older cohorts, the evidence points to positive effects on political participation (see Appendix Figure A.10). For partisanship, however, college and high school have plausibly different consequences, which is why we do not apply our decomposition method to this outcome. We note, however, that the age gradients in Appendix Figure A.12 suggest negative effects of additional years college on Republican registration and positive effects on registering as independent.

7. Conclusion

In this paper, we present novel estimates of the effect of education on political preferences and turnout, using nationwide data in combination with a credible identification strategy based on school-entry cutoffs and exact date of birth. Our data allow us to estimate particularly well-powered RD models, which deliver very precise estimates of the first-stage effect of early school entry on educational attainment. On average, children born just before the school-entry cutoff complete an additional 0.034 years of schooling and are about 0.5 p.p. more likely to graduate from high school. Moving on to the reduced form, we find that early school entry leads to increases in voter turnout of about 0.25 p.p. and 0.18 p.p. decrease in the probability of registering with either of the two major parties.

Finally, we introduce a new method to disentangle how early school entry affects later-life outcomes through the quantity versus the quality of education. Our results imply that an additional year of schooling increases turnout by approximately 3 percentage points.

[20] A noteworthy implication of this assumption is that the local average treatment effect of an additional year of schooling is identical for individuals induced to complete an extra year of high school and for those who (temporarily) have an additional year of college.

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Appendix A: Supplemental Results

Appendix Table A.1: Covariate Balance

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Decennial Census Records						
	Female	White	Black	Hispanic	Asian	Other Race
Born Before Cutoff	0.0008	0.0008	0.0002	–0.0011	–0.0002	0.0000
	(0.0011)	(0.0009)	(0.0007)	(0.0007)	(0.0004)	(0.0002)
Constant	0.5121	0.6861	0.1101	0.1536	0.0450	0.0106
	(0.0007)	(0.0006)	(0.0005)	(0.0005)	(0.0003)	(0.0001)
R-squared	0.0005	0.1323	0.0550	0.1561	0.0604	0.0288
Observations	4,049,000	4,049,000	4,049,000	4,049,000	4,049,000	4,049,000
Panel B. Voter Registration Records						
	Female					
Born Before Cutoff	0.0007**					
	(0.0003)					
Constant	0.5234***					
	(0.0002)					
R-squared	0.0013					
Observations	44,944,003					

Notes: Entries are point estimates and standard errors from estimating the regression discontinuity model in eq. (1), using pre-treatment covariates as the dependent variables. The upper panel considers covariates available in the 2000 Decennial Census and the bottom panel considers the single pre-treatment covariate available in the voter records. All specifications include state-by-cohort fixed effects. Standard errors are clustered at the state-by-cohort level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.2: Density

	Log(Census Count)		Log(L2 Count)	
	(1)	(2)	(3)	(4)
Born Before Cutoff	–0.0141	–0.0305	0.0004	–0.0244
	(0.0163)	(0.0313)	(0.0140)	(0.0257)
Constant	10.0780***	10.0511***	12.4899***	12.4732***
	(0.0111)	(0.0220)	(0.0070)	(0.0163)
Bandwidth	± 29 days	± 90 days	± 29 days	± 90 days
Observations	56	178	56	178
R-squared	0.1019	0.0087	0.0995	0.0389

Notes: Entries are point estimates and heteroskedasticity robust standard errors from estimating the regression discontinuity model in eq. (1), using the natural log of Decennial Census and L2 Voter Registration observation counts as dependent variables. Cols. (1) and (2) estimate the effect on the natural log of Decennial Census observation counts. Cols. (3) and (4) estimate the effect on the natural log of L2 Voter Registration observation counts. Cols. (1) and (3) use a bandwidth of ± 29 days and cols. (2) and (4) use a bandwidth of ± 90 days. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.3: General Election Turnout, No State-Year-Election Fixed Effects

	All Years	Presidential	Midterms
	(1)	(2)	(3)
Born Before Cutoff	0.0032***	0.0029***	0.0031***
	(0.0003)	(0.0002)	(0.0003)
Constant	0.6032***	0.7107***	0.4541***
	(0.0040)	(0.0022)	(0.0039)
Dep. Var. Mean	0.6060	0.7132	0.4570
R-squared	0.0000	0.0000	0.0000
Observations	221,163,187	128,644,490	92,518,697

Notes: Entries are point estimates and standard errors from estimating the regression discontinuity model in eq. (1), modified as described in Section 4, given that the turnout analysis is conducted at the individual-by-election level. Col. (1) estimates the effect on voter turnout in all general elections from 2008–2020. Col. (2) estimates the effect on voter turnout in presidential elections during that period (i.e., 2008, 2012, 2016, and 2020). Col. (3) estimates the effect on voter turnout in midterm elections during that period (i.e., 2010, 2014, 2018). The specifications in this table do not contain state-by-cohort-by-election fixed effects. Standard errors are clustered at the state-by-cohort level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.4: Party Affiliation, No State-Year Fixed Effects

	All States			Non-modeled States		
	(1) Democrat	(2) Republican	(3) Independent	(4) Democrat	(5) Republican	(6) Independent
Born Before Cutoff	–0.0013***	–0.0011***	0.0024***	–0.0015***	–0.0016***	0.0031***
	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0004)	(0.0004)
Constant	0.4178***	0.2854***	0.2968***	0.4058***	0.2783***	0.3159***
	(0.0037)	(0.0044)	(0.0046)	(0.0041)	(0.0054)	(0.0043)
Dep. Var. Mean	0.4169	0.2844	0.2987	0.4058	0.2775	0.3167
Observations	45,177,003	45,177,003	45,177,003	27,154,495	27,154,495	27,154,495
R-squared	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Notes: Entries are point estimates and standard errors from estimating the regression discontinuity model in eq. (1). Cols. (1)–(3) show estimates for all states, while cols. (4)–(6) show estimates solely for non-modeled states (i.e., states in which party affiliation is recorded on the voter file). Cols. (1) and (4) estimate the effect on Democratic registration, cols. (2) and (5) estimate the effect on Republican registration, and cols. (3) and (6) estimate the effect on Independent registration. The specifications in this table do not include state-by-cohort fixed effects. Standard errors are clustered at the state-by-cohort level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.5: Effects on Turnout and Partisanship, Including Oct. 15-Nov. 17 Cutoffs

	(1)	(2)	(3)	(4)	(5)	(6)
	General Election Turnout					
	All Years	Presidential	Midterms			
Born Before Cutoff	0.0024***	0.0025***	0.0024***			
	(0.0002)	(0.0002)	(0.0002)			
Constant	0.6113***	0.7178***	0.4634***			
	(0.0002)	(0.0001)	(0.0002)			
Dep. Var. Mean	0.6130	0.7196	0.4651			
R-squared	0.1354	0.0530	0.0973			
Observations	238,974,061	138,931,332	100,042,729			
	Party Affiliation					
	All States			Non-modeled States		
	Democrat	Republican	Independent	Democrat	Republican	Independent
Born Before Cutoff	–0.0003	–0.0015***	0.0018***	–0.0007*	–0.0011***	0.0018***
	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0003)
Constant	0.4124***	0.2908***	0.2968***	0.3992***	0.2852***	0.3155***
	(0.0003)	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0003)
Dep. Var. Mean	0.4116	0.2908	0.2976	0.3980	0.2860	0.3160
Observations	48,334,369	48,334,369	48,334,369	29,612,310	29,612,310	29,612,310
R-squared	0.0499	0.0706	0.0842	0.0385	0.0594	0.0585

Notes: Entries are point estimates and standard errors from estimating the regression discontinuity model in eq. (1), modified for the upper panel as described in Section 4, given that the turnout analysis is conducted at the individual-by-election level. For this analysis, we include state-cohort cutoffs that fall between October 15 and November 17. In the upper panel, col. (1) estimates the effect on voter turnout in all general elections from 2008–2020. Col. (2) estimates the effect on voter turnout in presidential elections during that period (i.e., 2008, 2012, 2016, and 2020). Col. (3) estimates the effect on voter turnout in midterm elections during that period (i.e., 2010, 2014, 2018). These specifications include state-by-cohort-by-election fixed effects. In the lower panel, cols. (1)–(3) show estimates for all states while cols. (4)–(6) show estimates solely for non-modeled states (i.e., states in which party affiliation is recorded on the voter file). Cols. (1) and (4) estimate the effect on Democratic registration, cols. (2) and (5) estimate the effect on Republican registration, and cols. (3) and (6) estimate the effect on independent registration. These specifications include state-by-cohort fixed effects. Standard errors are clustered at the state-by-cohort level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.6: Effects on Turnout and Partisanship, Including RD Donut Region

	(1)	(2)	(3)	(4)	(5)	(6)
	General Election Turnout					
	All Years	Presidential	Midterms			
Born Before Cutoff	0.0026***	0.0026***	0.0026***			
	(0.0002)	(0.0002)	(0.0002)			
Constant	0.6040***	0.7112***	0.4551***			
	(0.0002)	(0.0001)	(0.0002)			
Dep. Var. Mean	0.6060	0.7132	0.4570			
R-squared	0.1340	0.0510	0.0958			
Observations	223,759,372	130,154,117	93,605,255			
	Party Affiliation					
	All States			Non-modeled States		
	Democrat	Republican	Independent	Democrat	Republican	Independent
Born Before Cutoff	–0.0003	–0.0014***	0.0017***	–0.0007	–0.0011***	0.0018***
	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0004)
Constant	0.4178***	0.2843***	0.2979***	0.4072***	0.2765***	0.3163***
	(0.0003)	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0003)
Dep. Var. Mean	0.4169	0.2844	0.2987	0.4058	0.2775	0.3167
Observations	45,707,878	45,707,878	45,707,878	27,472,575	27,472,575	27,472,575
R-squared	0.0498	0.0702	0.0864	0.0376	0.0569	0.0605

Notes: Entries are point estimates and standard errors from estimating the regression discontinuity model in eq. (1), modified for the upper panel as described in Section 4, given that the turnout analysis is conducted at the individual-by-election level. For this analysis, we include individuals born on the day of the school-entry cutoff, the day before, and the day after. In the upper panel, col. (1) estimates the effect on voter turnout in all general elections from 2008–2020. Col. (2) estimates the effect on voter turnout in presidential elections during that period (i.e., 2008, 2012, 2016, and 2020). Col. (3) estimates the effect on voter turnout in midterm elections during that period (i.e., 2010, 2014, 2018). These specifications include state-by-cohort-by-election fixed effects. In the lower panel, cols. (1)–(3) show estimates for all states while cols. (4)–(6) show estimates solely for non-modeled states (i.e., states in which party affiliation is recorded on the voter file). Cols. (1) and (4) estimate the effect on Democratic registration, cols. (2) and (5) estimate the effect on Republican registration, and cols. (3) and (6) estimate the effect on independent registration. These specifications include state-by-cohort fixed effects. Standard errors are clustered at the state-by-cohort level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.7: Effects on Turnout and Partisanship, States with High Exact DOB Coverage

	(1)	(2)	(3)	(4)	(5)	(6)
	General Election Turnout					
	All Years	Presidential	Midterms			
Born Before Cutoff	0.0025***	0.0026***	0.0023***			
	(0.0003)	(0.0002)	(0.0002)			
Constant	0.5943***	0.7049***	0.4397***			
	(0.0002)	(0.0001)	(0.0002)			
Dep. Var. Mean	0.5961	0.7067	0.4414			
R-squared	0.1329	0.0453	0.0915			
Observations	187,308,920	109,179,423	78,129,497			
	Party Affiliation					
	All States			Non-modeled States		
	Democrat	Republican	Independent	Democrat	Republican	Independent
Born Before Cutoff	0.0001	–0.0018***	0.0017***	–0.0005	–0.0011***	0.0016***
	(0.0004)	(0.0003)	(0.0003)	(0.0005)	(0.0004)	(0.0004)
Constant	0.4232***	0.2785***	0.2983***	0.4145***	0.2618***	0.3237***
	(0.0003)	(0.0002)	(0.0003)	(0.0004)	(0.0003)	(0.0003)
Dep. Var. Mean	0.4223	0.2786	0.2990	0.4131	0.2630	0.3239
Observations	39,176,604	39,176,604	39,176,604	24,391,778	24,391,778	24,391,778
R-squared	0.0501	0.0605	0.0830	0.0363	0.0448	0.0506

Notes: Entries are point estimates and standard errors from estimating the regression discontinuity model in eq. (1), modified for the upper panel as described in Section 4, given that the turnout analysis is conducted at the individual-by-election level. For this analysis, we focus on states in which date of birth is rarely imputed and include individuals born on the first day of the month. In the upper panel, col. (1) estimates the effect on voter turnout in all general elections from 2008–2020. Col. (2) estimates the effect on voter turnout in presidential elections during that period (i.e., 2008, 2012, 2016, and 2020). Col. (3) estimates the effect on voter turnout in midterm elections during that period (i.e., 2010, 2014, 2018). These specifications include state-by-cohort-by-election fixed effects. In the lower panel, cols. (1)–(3) show estimates for all states while cols. (4)–(6) show estimates solely for non-modeled states (i.e., states in which party affiliation is recorded on the voter file). Cols. (1) and (4) estimate the effect on Democratic registration, cols. (2) and (5) estimate the effect on Republican registration, and cols. (3) and (6) estimate the effect on independent registration. These specifications include state-by-cohort fixed effects. Standard errors are clustered at the state-by-cohort level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.8: Party Affiliation, Individuals Aged 20-23

	(1)	(2)	(3)	(4)	(5)	(6)
	All States			Non-modeled States		
	Democrat	Republican	Independent	Democrat	Republican	Independent
Born Before Cutoff	0.0026*	–0.0075***	0.0050***	0.0016	–0.0066***	0.0050***
	(0.0015)	(0.0012)	(0.0013)	(0.0016)	(0.0014)	(0.0015)
Constant	0.4489***	0.1965***	0.3546***	0.3874***	0.2097***	0.4028***
	(0.0008)	(0.0009)	(0.0009)	(0.0011)	(0.0011)	(0.0011)
Dep. Var. Mean	0.4486	0.1931	0.3583	0.3873	0.2070	0.4056
Observations	3,785,614	3,785,614	3,785,614	1,993,863	1,993,863	1,993,863
R-squared	0.1185	0.0464	0.1749	0.0620	0.0493	0.0897

Notes: Entries are point estimates and standard errors from estimating the regression discontinuity model in eq. (1), estimated solely on individuals aged 20–23. Cols. (1)–(3) show estimates for all states, while cols. (4)–(6) show estimates solely for non-modeled states (i.e., states in which party affiliation is recorded on the voter file). Cols. (1) and (4) estimate the effect on Democratic registration, cols. (2) and (5) estimate the effect on Republican registration, and cols. (3) and (6) estimate the effect on Independent registration. All specifications include state-by-cohort fixed effects. Standard errors are clustered at the state-by-cohort level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.9: Party Affiliation, 2014

	(1)	(2)	(3)	(4)	(5)	(6)
	All States			Non-modeled States		
	Democrat	Republican	Independent	Democrat	Republican	Independent
Born Before Cutoff	–0.0013***	–0.0017***	0.0030***	–0.0018***	–0.0008*	0.0025***
	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0005)	(0.0004)
Constant	0.4004***	0.2779***	0.3217***	0.4170***	0.2813***	0.3017***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Dep. Var. Mean	0.3981	0.2783	0.3236	0.4140	0.2828	0.3032
Observations	27,348,087	27,348,087	27,348,088	17,368,029	17,368,029	17,368,030
R-squared	0.0553	0.0531	0.1126	0.0368	0.0391	0.0723

Notes: Entries are point estimates and standard errors from estimating the regression discontinuity model in eq. (1), using a 2014 vintage of the L2 voter database. Cols. (1)–(3) show estimates for all states, while cols. (4)–(6) show estimates solely for non-modeled states (i.e., states in which party affiliation is recorded on the voter file). Cols. (1) and (4) estimate the effect on Democratic registration, cols. (2) and (5) estimate the effect on Republican registration, and cols. (3) and (6) estimate the effect on Independent registration. All specifications include state-by-cohort fixed effects. Table A.4 shows that the results are robust to the inclusion or exclusion of these fixed effects. Standard errors are clustered at the state-by-cohort level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.10: Party Affiliation, Individuals Aged 20-23, 2014

	(1)	(2)	(3)	(4)	(5)	(6)
	All States			Non-modeled States		
	Democrat	Republican	Independent	Democrat	Republican	Independent
Born Before Cutoff	–0.0010	–0.0032**	0.0042***	–0.0033*	0.0002	0.0031*
	(0.0017)	(0.0014)	(0.0013)	(0.0019)	(0.0014)	(0.0017)
Constant	0.3906***	0.2113***	0.3982***	0.3778***	0.2284***	0.3938***
	(0.0012)	(0.0010)	(0.0011)	(0.0013)	(0.0010)	(0.0012)
Dep. Var. Mean	0.3890	0.2109	0.4001	0.3751	0.2306	0.3943
Observations	3,014,677	3,014,677	3,014,677	1,866,861	1,866,861	1,866,861
R-squared	0.1376	0.0447	0.2208	0.0603	0.0370	0.0986

Notes: Entries are point estimates and standard errors from estimating the regression discontinuity model in eq. (1), estimated solely on individuals aged 20–23, using a 2014 vintage of the L2 voter database. Cols. (1)–(3) show estimates for all states, while cols. (4)–(6) show estimates solely for non-modeled states (i.e., states in which party affiliation is recorded on the voter file). Cols. (1) and (4) estimate the effect on Democratic registration, cols. (2) and (5) estimate the effect on Republican registration, and cols. (3) and (6) estimate the effect on Independent registration. All specifications include state-by-cohort fixed effects. Standard errors are clustered at the state-by-cohort level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.11: Incarceration

	Full Sample		Males	Males, Age≤30	Full Sample		Males	Males, Age≤30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Born Before Cutoff	0.00021***	0.00007	0.00008	0.00017	0.00022***	0.00002	–0.00002	0.00014
	(0.00008)	(0.00007)	(0.00013)	(0.00024)	(0.00006)	(0.00005)	(0.00009)	(0.00017)
Black/Hisp. x Born Before	.	0.00046**	0.00092**	0.00090	.	0.00074***	0.00138***	0.00162***
		(0.00019)	(0.00037)	(0.00061)		(0.00014)	(0.00026)	(0.00045)
Constant		0.00850***	0.01480***	0.01632***		0.00850***	0.01482***	0.01644***
		(0.00017)	(0.00033)	(0.00059)		(0.00016)	(0.00032)	(0.00057)
R-squared	0.00206	0.00731	0.01487	0.01564	0.00200	0.00730	0.01483	0.01575
Observations	45,480,000	45,480,000	22,550,000	8,646,000	90,100,000	90,100,000	44,720,000	17,180,000

Notes: Entries are point estimates and standard errors from estimating the regression discontinuity model in eq. (1). All specifications estimate the effect of early school entry on the likelihood that the individual is incarcerated in 2000, as measured by group quarters status in the Decennial Census. Col. (1) estimates this on the full sample of respondents to the short-form Census. Col. (2) also estimates this on the full sample, including an interaction term to examine heterogeneity for black and Hispanic individuals. Col. (3) restricts attention to male respondents, and col. (4) restricts attention to males below the age of 30. Cols. (5)–(8) do the same, but use a bandwidth of +/- 180 days instead of +/- 90 days. All specifications include state-by-cohort fixed effects. Standard errors are clustered at the state-by-cohort level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.12: Isolating the Quantity Effect on Turnout

	(1)	(2)	(3)
Panel A. Estimating Quantity Effect using Various Age Ranges			
Time-Varying Component (RF)	–0.0003***	–0.0005***	–0.0003***
	(0.0001)	(0.0002)	(0.00004)
Time-Varying Component (FS)	–0.0099***	–0.0196***	–0.0055***
	(0.0024)	(0.0040)	(0.0011)
Pure Quantity Effect	0.0308**	0.0254***	0.0498***
	(0.0120)	(0.0097)	(0.0127)
Age Range	19–28	19–25	19–35
Panel B. IV Estimate from Full Sample			
RD Reduced Form		0.0025***	
		(0.0002)	
RD First Stage		0.0335***	
		(0.0061)	
IV Estimate		0.0746	
		(0.0148)	
Age Range		19–61	
Panel C. Estimating Share of IV Explained by Quantity			
Ratio of Quantity to Total IV	0.4129**	0.3409**	0.6672***
	(0.1809)	(0.1463)	(0.2157)
Age Range	19–28	19–25	19–35

Notes: Entries are point estimates and Wald ratios that isolate the effect of education on voter turnout through the quantity channel, as explained in Section 6.2. Standard errors are computed under the assumption of no correlation between estimates, and are reported in parentheses. The first two rows in the upper panel report the slope coefficients from regressions of the age-specific reduced-form (first row) and first-stage (second row) RD estimates on a linear term in age, for individuals aged 19–28 (col. 1), 19–25 (col. 2), and 19–35 (col. 3). The third row reports the Wald ratio of the reduced-form and first-stage estimates. The middle panel shows reduced-form, first-stage, and IV estimates for the entire sample, while the lower panel reports the ratio of the pure quantity effect in the upper panel and the pooled IV estimate in the middle panel.

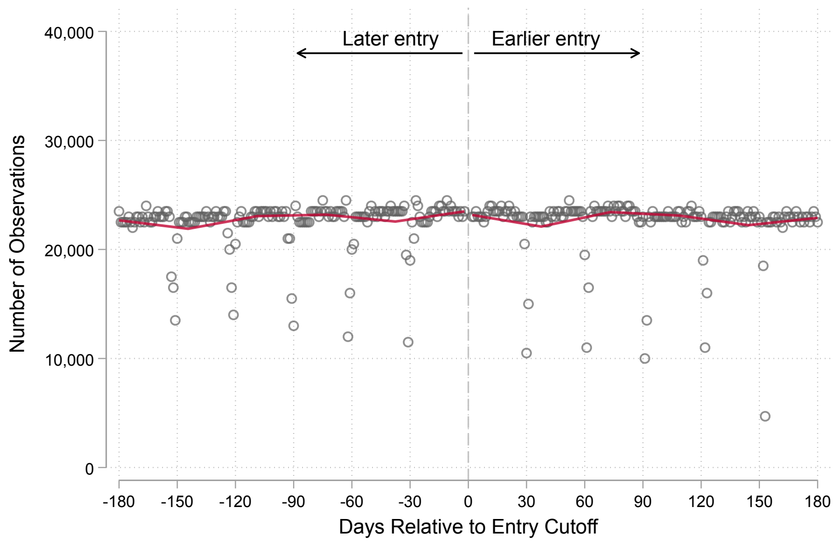
Appendix Table A.13: Isolating the Quantity Effect on Turnout

	(1)	(2)	(3)
Panel A. Estimating Quantity Effect using Various Age Ranges			
Time varying Component (RF)	–0.0002	–0.0004	–0.0002
Time-varying Component (FS)	–0.0038	–0.0093	–0.0033
Pure Quantity Effect	0.0552	0.0383	0.0736
Age Range	20–28	20–25	20–35
Panel B. IV Estimate from Full Sample			
RD Reduced Form		0.0025	
RD First Stage		0.0335	
IV Estimate		0.0746	
Age Range		19–61	
Panel C. Estimating Share of IV Explained by Quantity			
Ratio of Quantity to Total IV	0.7406	0.5137	0.9862
Age Range	20–28	20–25	20–35

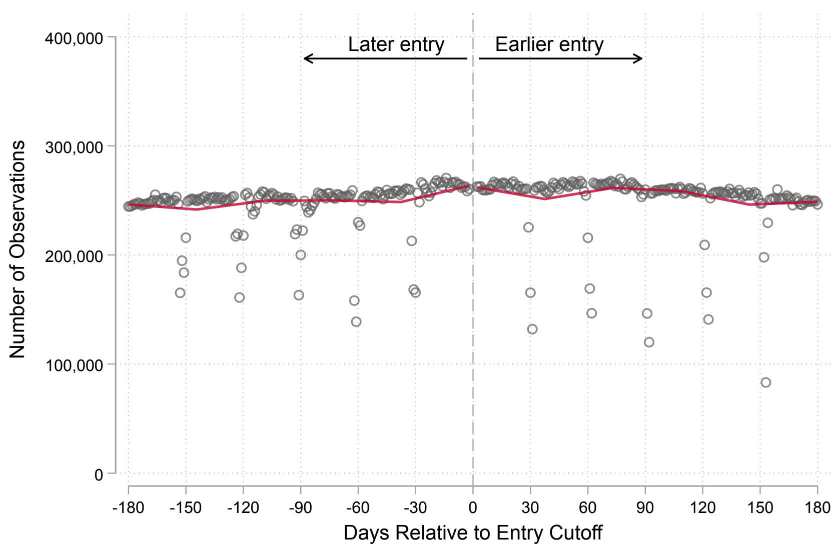
Notes: Entries are point estimates and Wald ratios that isolate the effect of education on voter turnout through the quantity channel, as explained in Section 6.2. Standard errors are reported in parentheses. The first two rows in the upper panel report the slope coefficients from regressions of the age-specific reduced-form (first row) and first-stage (second row) RD estimates on a linear term in age, for individuals aged 20–28 (col. 1), 20–25 (col. 2), and 20–35 (col. 3). The third row reports the Wald ratio of the reduced-form and first-stage estimates. The middle panel shows reduced-form, first-stage, and IV estimates for the entire sample, while the lower panel reports the ratio of the pure quantity effect in the upper panel and the pooled IV estimate in the middle panel.

Appendix Figure A.1: Density of Running Variable

(a) Decennial Census Records

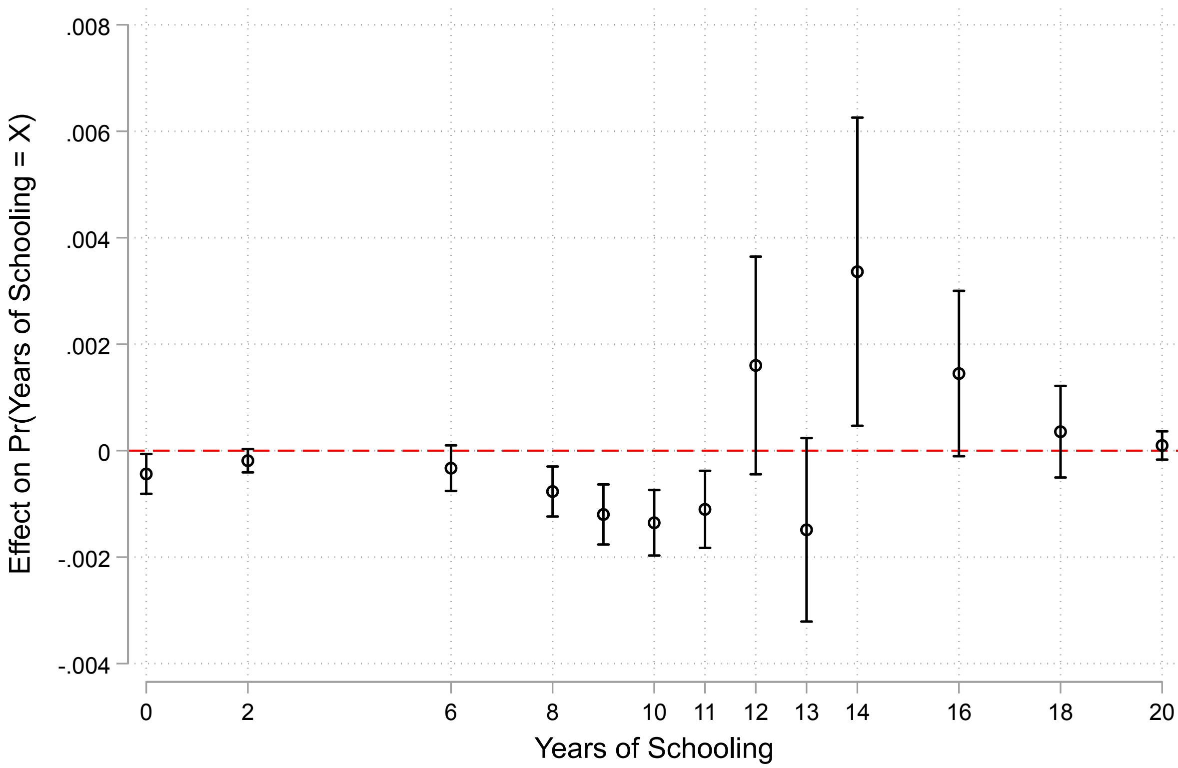


(b) Voter Registration Records



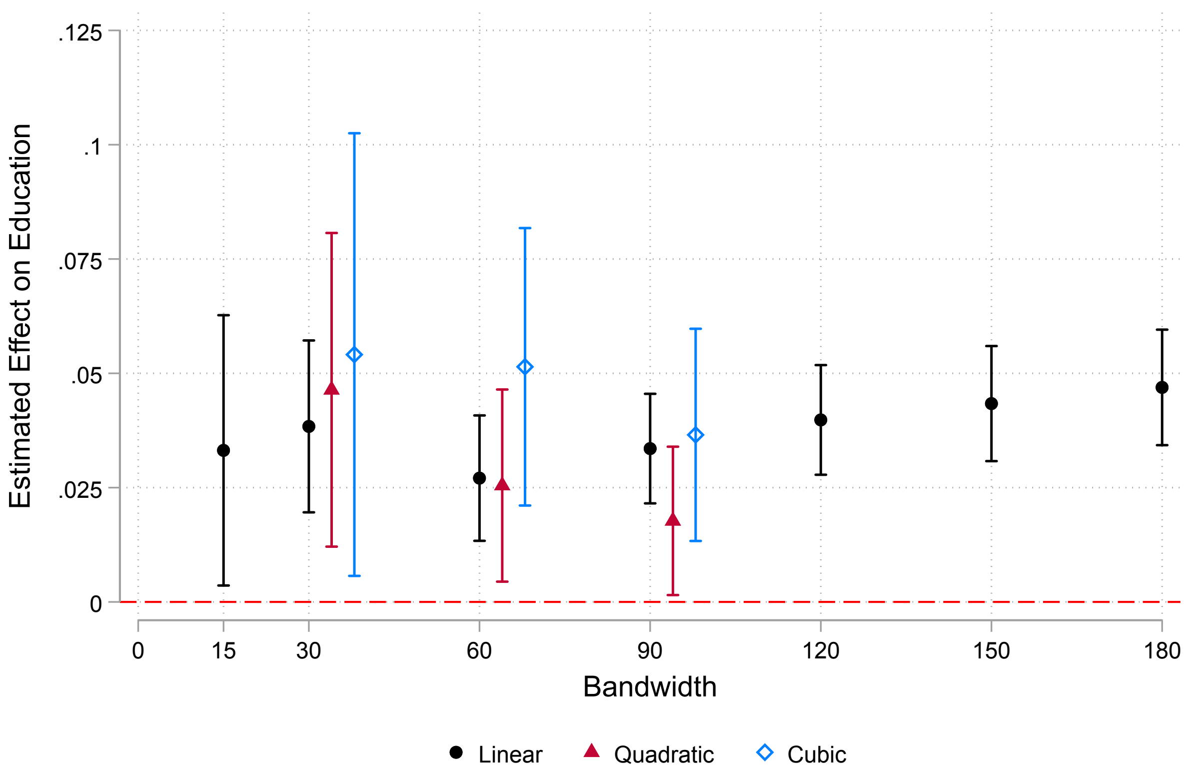
Notes: Figure displays the number of observations at each value of the running variable, centered at zero based on the relevant school-entry cutoff. We plot a smoothed local polynomial on each side based on the binned counts. Panel (a) shows this using the 2000 Decennial Census data and panel (b) shows this using the voter records.

Appendix Figure A.2: Effects along Attainment Distribution, Years of Schooling = X



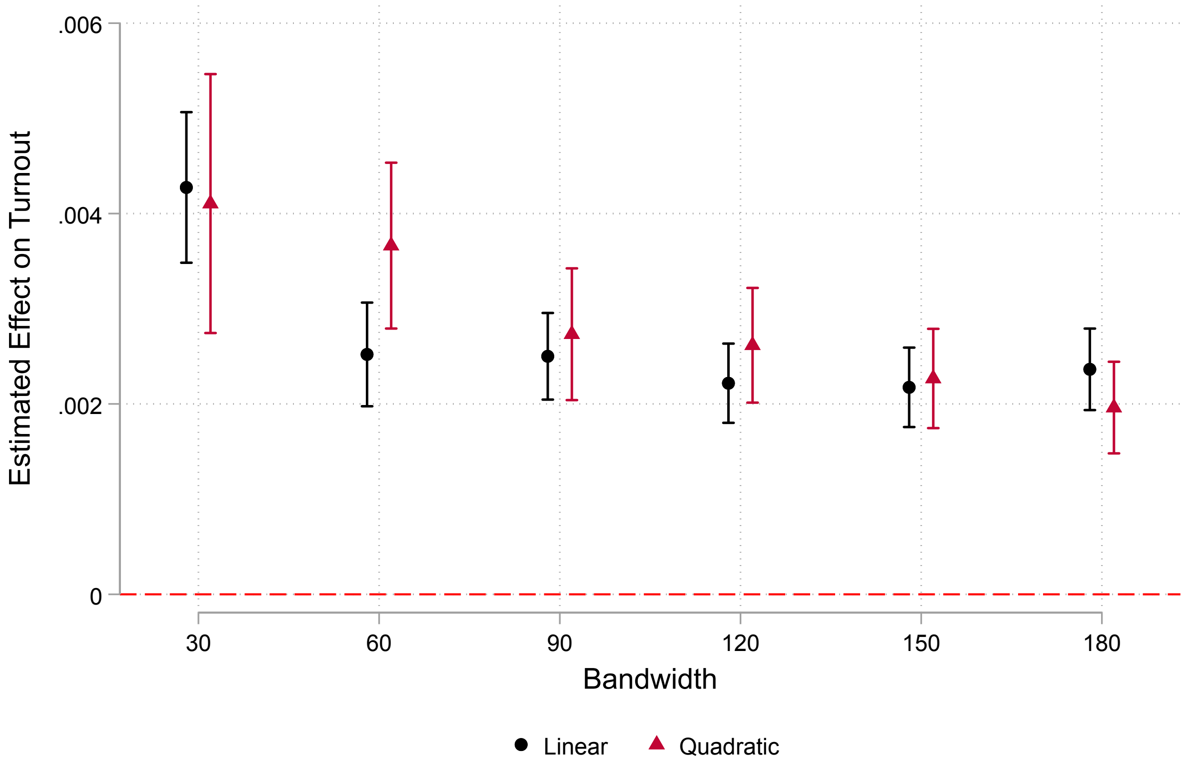
Notes: Figure displays estimates of the effect of early entry on exact years of completed schooling. We plot point estimates and 95% confidence intervals from each regression.

Appendix Figure A.3: Robustness of Education Effects



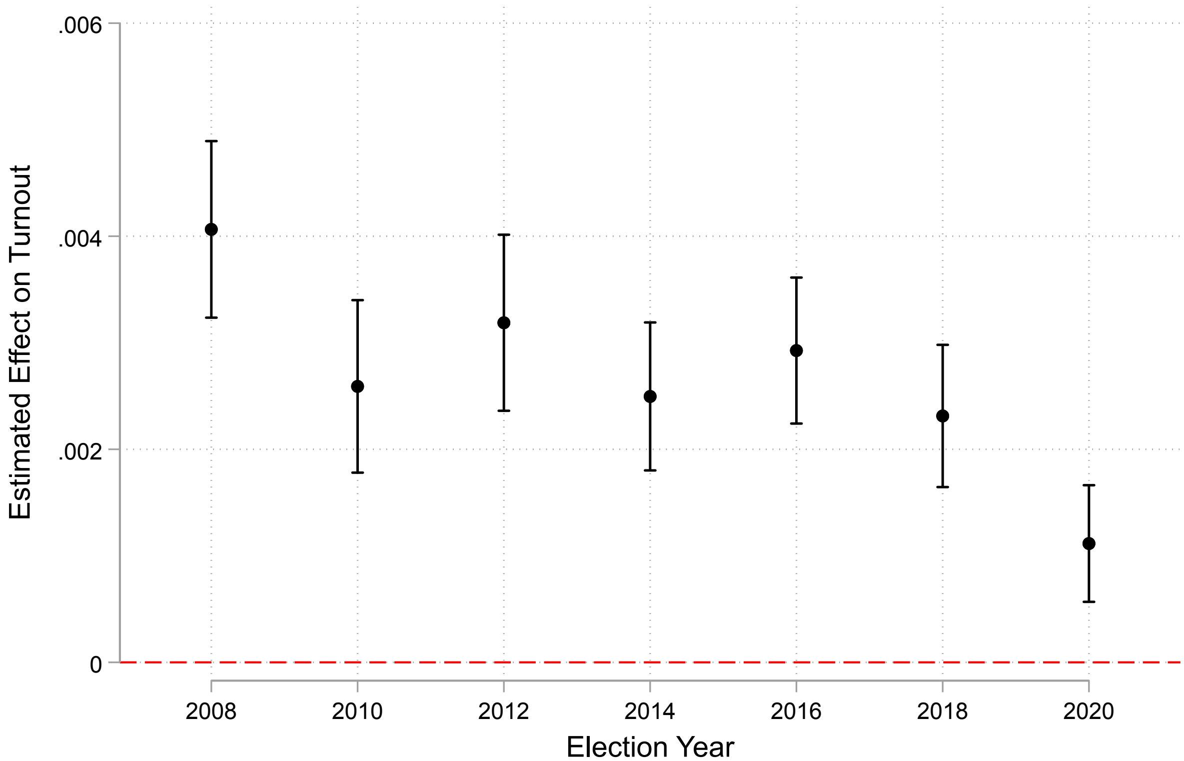
Notes: Figure displays coefficients and 95% confidence intervals for regression discontinuity specifications using different bandwidths and functional forms of the running variable. The black markers show the education estimates from specifications using a linear functional form and using bandwidths from +/- 15 days around the cutoff up to +/- 180 days. The red markers show results from specifications using a quadratic functional form and for bandwidths of +/- 30 days, +/- 60 days, or +/- 90 days. The blue markers show results from specifications using a cubic functional form and for bandwidths of +/- 30 days, +/- 60 days, or +/- 90 days.

Appendix Figure A.4: Robustness of Turnout Effects



Notes: Figure displays coefficients and 95% confidence intervals for regression discontinuity specifications using different bandwidths and functional forms of the running variable. The black markers show the estimates from specifications using a linear functional form and using bandwidths from +/- 30 days around the cutoff up to +/- 180 days. The red markers show the estimates from specifications using a quadratic functional form and using bandwidths from +/- 30 days around the cutoff up to +/- 180 days.

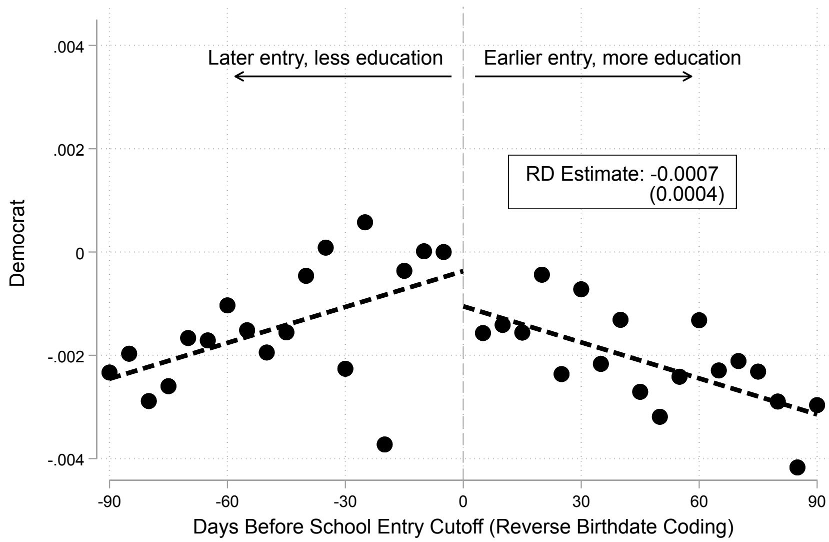
Appendix Figure A.5: Turnout Effects by Election



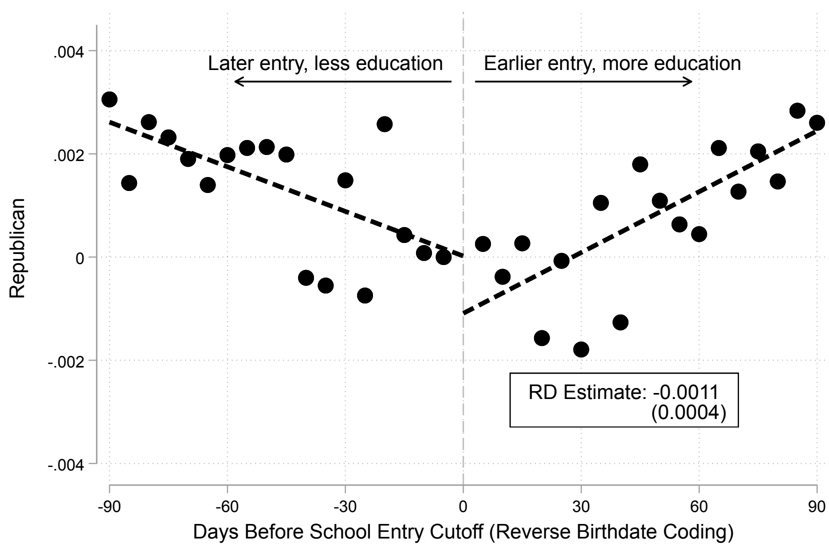
Notes: Figure displays coefficients and 95% confidence intervals for regression discontinuity specifications that estimate the effect of being born before the school entry cutoff on turnout by election, for all general elections from 2008-2020.

Appendix Figure A.6: Party Affiliation

(a) Democrat



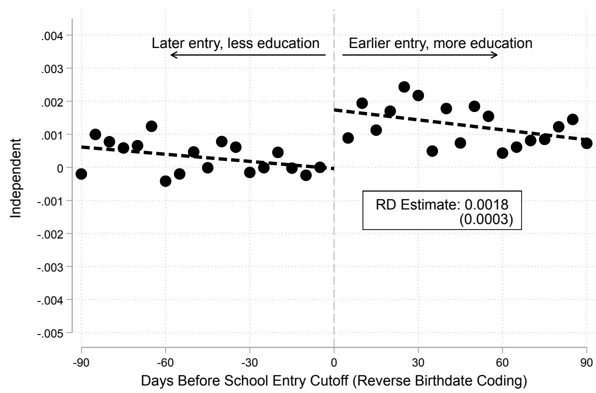
(b) Republican



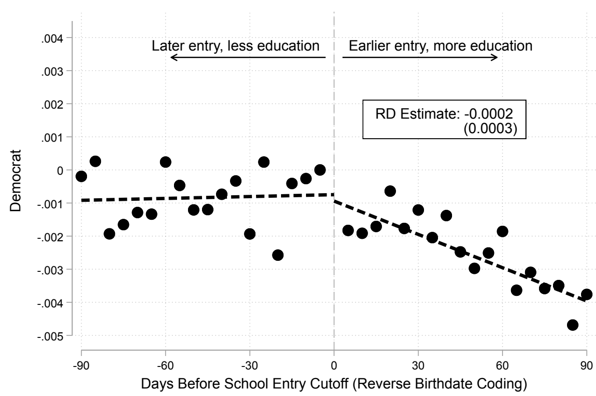
Notes: Figure displays average outcomes within five-day bins of the running variable, relative to the 0 to 5 day bin, for individuals within +/- 90 days of the relevant school-entry cutoff. The running variable is reverse-coded so that positive values indicate the individual is born *before* the cutoff, and thus will enter school in the current year. Negative values indicate that the individual is born after the cutoff, and thus will enter school in the following year. Panel (a) shows the result for Democratic registration, and panel (b) shows the result for Republican registration. Only non-modeled states are included in these figures.

Appendix Figure A.7: Party Affiliation, All States

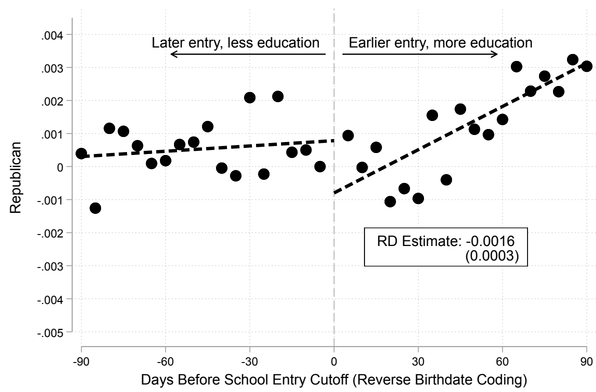
(a) Independent



(b) Democrat



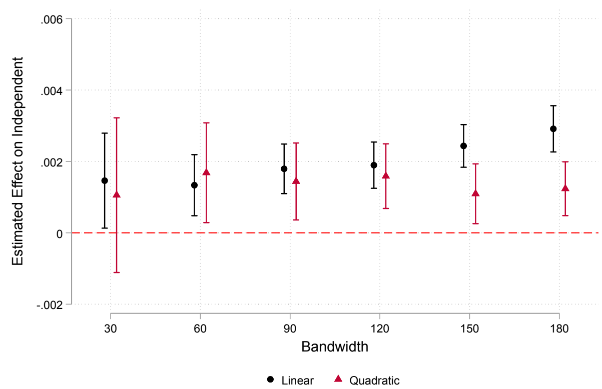
(c) Republican



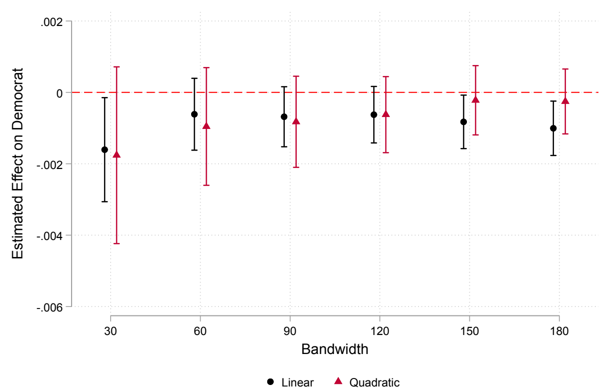
Notes: Figure displays average outcomes within five-day bins of the running variable, relative to the 0 to 5 day bin, for individuals within +/- 90 days of the relevant school-entry cutoff. The running variable is reverse-coded so that positive values indicate the individual is born *before* the cutoff, and thus will enter school in the current year. Negative values indicate that the individual is born after the cutoff, and thus will enter school in the following year. Panel (a) shows the result for independent registration, panel (b) shows the result for Democratic registration, and panel (c) shows the result for Republican registration. All states are included in these figures.

Appendix Figure A.8: Robustness of Party Affiliation Effects, Non-Modeled States

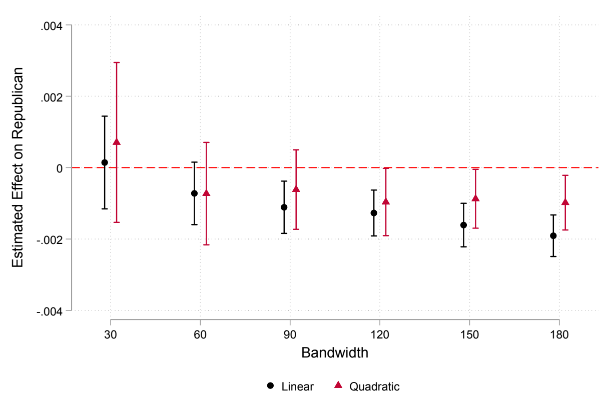
(a) Independent



(b) Democrat



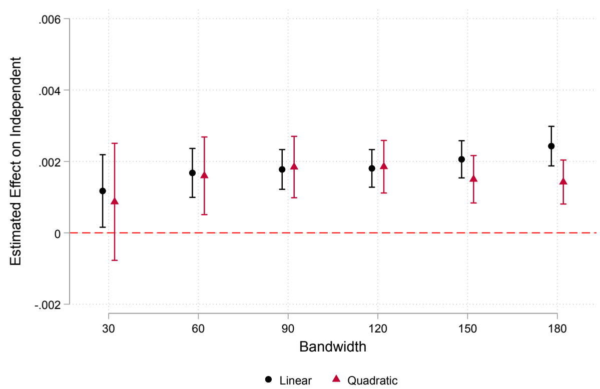
(c) Republican



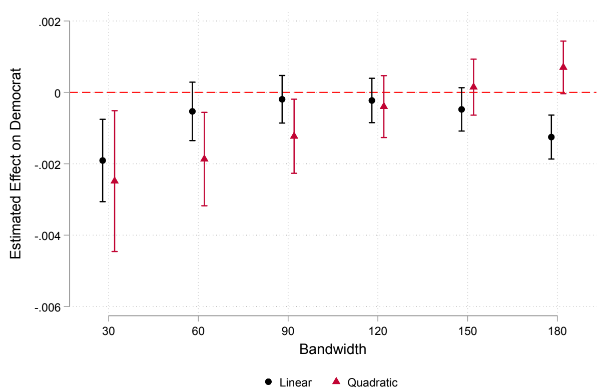
Notes: Figure displays coefficients and 95% confidence intervals for regression discontinuity specifications using different bandwidths and functional forms of the running variable. Panel (a) shows this for independent registration, panel (b) for Democratic registration, and panel (c) for Republican registration. Only non-modeled states are included in the results. The black markers show the estimates from specifications using a linear functional form and using bandwidths from +/- 30 days around the cutoff up to +/- 180 days. The red markers show the estimates from specifications using a quadratic functional form and using bandwidths from +/- 30 days around the cutoff up to +/- 180 days.

Appendix Figure A.9: Robustness of Party Affiliation Effects, All States

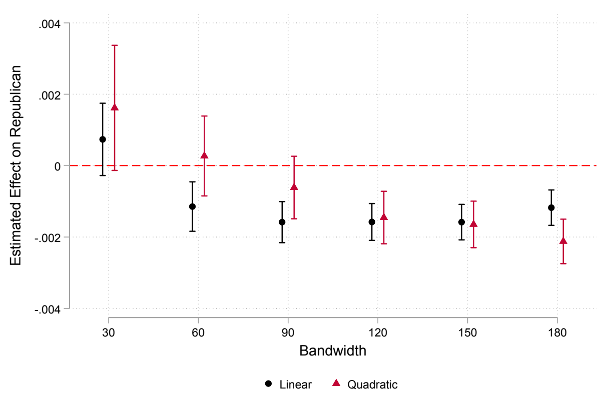
(a) Independent



(b) Democrat



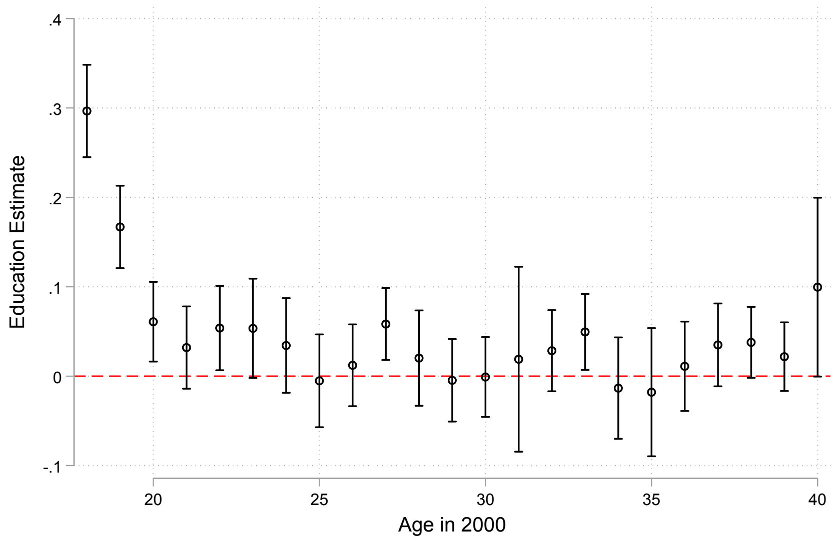
(c) Republican



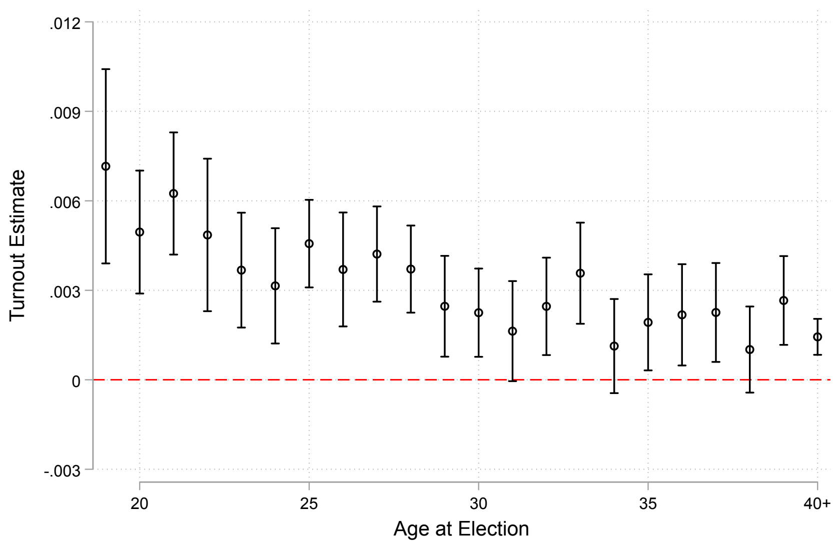
Notes: Figure displays coefficients and 95% confidence intervals for regression discontinuity specifications using different bandwidths and functional forms of the running variable. Panel (a) shows this for independent registration, panel (b) for Democratic registration, and panel (c) for Republican registration. All states are included in the results. The black markers show the estimates from specifications using a linear functional form and using bandwidths from +/- 30 days around the cutoff up to +/- 180 days. The red markers show the estimates from specifications using a quadratic functional form and using bandwidths from +/- 30 days around the cutoff up to +/- 180 days.

Appendix Figure A.10: Heterogeneous Effects by Age

(a) Educational Attainment



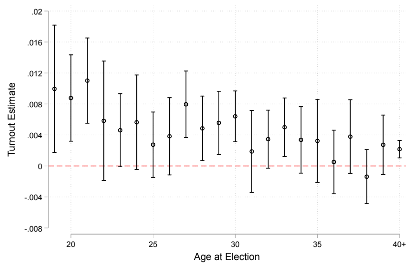
(b) Turnout



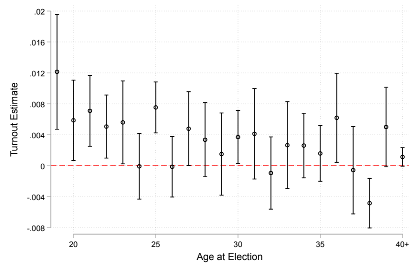
Notes: Figure displays estimates of the effect of early entry on educational attainment (panel (a)) and on voter turnout (panel (b)) by age. We estimate eq. (1) separately for each age (in years), and we plot point estimates and 95% confidence intervals from each regression.

Appendix Figure A.11: Heterogeneous Effects on Turnout by Age and Election

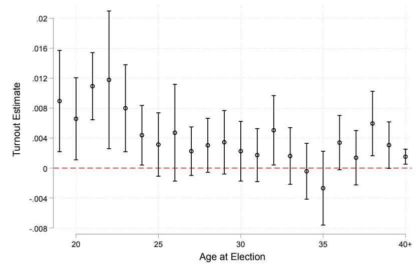
(a) 2008



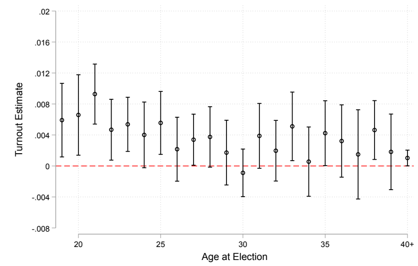
(b) 2010



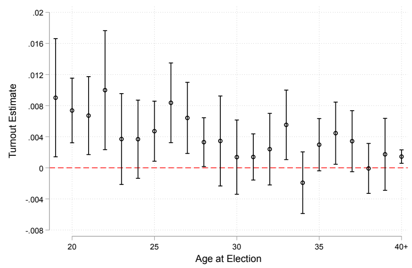
(c) 2012



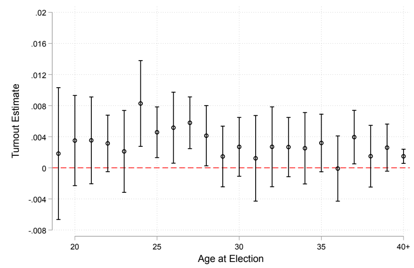
(d) 2014



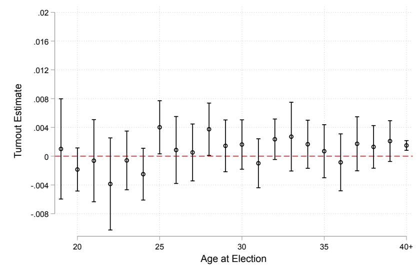
(e) 2016



(f) 2018



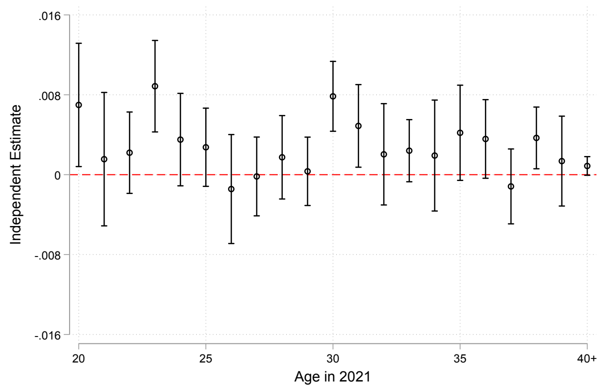
(g) 2020



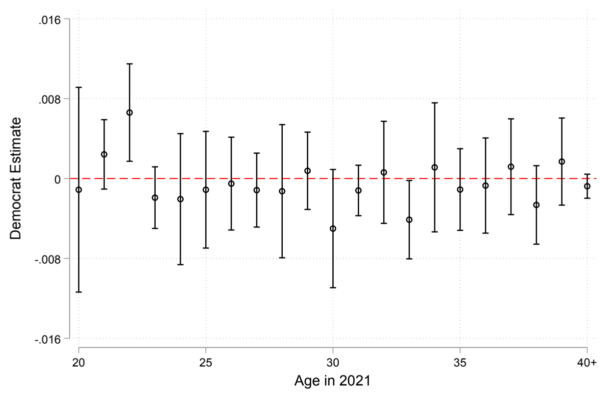
Notes: Figure displays estimates of the effect of early entry on voter turnout by age and by election. We estimate eq. (1) separately for each age (in years), and we do so separately for each election. We plot point estimates and 95% confidence intervals from each regression.

Appendix Figure A.12: Effects on Party Affiliation by Age

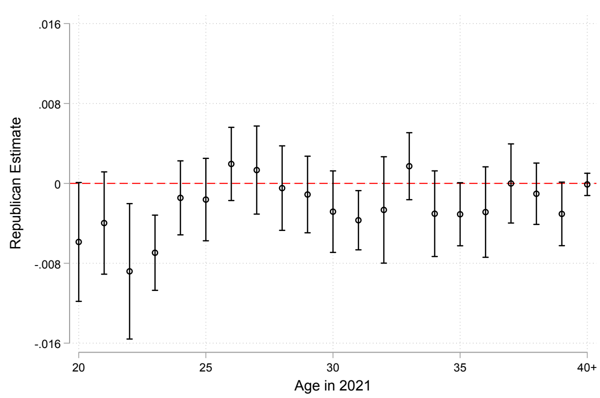
(a) Independent



(b) Democrat



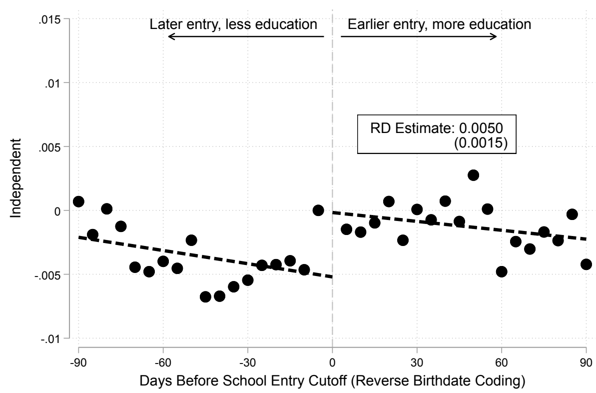
(c) Republican



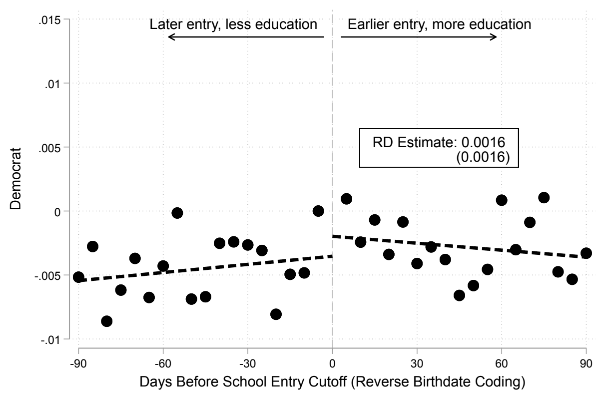
Notes: Figure displays estimates of the effect of early entry on independent registration (panel (a)), on Democratic registration (panel (b)), and on Republican registration (panel (c)) by age. We estimate eq. (1) separately for each age (in years), and we plot point estimates and 95% confidence intervals from each regression.

Appendix Figure A.13: Party Affiliation, Individuals Aged 20-23

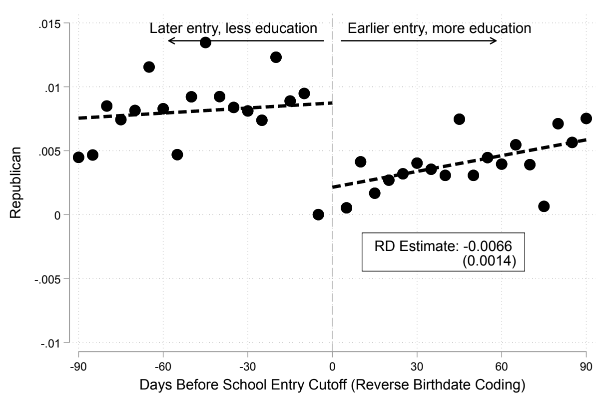
(a) Independent



(b) Democrat



(c) Republican



Notes: Figure displays average outcomes within five-day bins of the running variable, relative to the 0 to 5 day bin, for individuals within +/- 90 days of the relevant school-entry cutoff. The running variable is reverse-coded so that positive values indicate that the individual is born *before* the cutoff, and thus will enter school in the current year. Negative values indicate the individual is born after the cutoff, and thus will enter school in the following year. For this figure, we restrict attention to individuals who are 20-23 years old as of 2021. Panel (a) shows the result for independent registration, panel (b) shows the result for Democratic registration, and panel (c) shows the result for Republican registration. Only non-modeled states are included in these figures.