Does Schooling Create Democratic Voters? Turnout and Partisan Consequences of Additional Education

By Ethan Kaplan, Jörg Spenkuch, and Cody Tuttle*

We estimate the impact of education on voter turnout and on partisanship using a regression discontinuity design based upon school entry age and exact date of birth. Using the universe of voter registration data from the year 2021, we find that individuals who enter school one year earlier are more likely to vote and more likely to register as Independent. These reduced form effects may be driven by changes in the quantity of educational attainment or by differences in the quality of the educational experience. We use data from the 2000 Decennial Census to show that individuals who enter school one year earlier end up higher educational attainment, on average. Finally, we develop a new method that leverages heterogeneity in these effects by age to separate out the impact of quantity from quality. We show that the majority of the turnout effect is a quantity effect. *Keywords: Education, Turnout, Partisan, Voting*

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We thank people for voting.

I. Introduction

Educational differences across people are responsible for daunting contemporary economic cleavages (Goldin and Katz, 2010). It is unsurprising that these economic cleavages are correlated with political cleavages. Even more, political polarization along educational lines has increased in recent decades (Piketty et al., 2018). Not only do those with more education vote differently than those with less education, they also participate more in the political process (Bonica et al., 2013).[1] Motivated by these stark correlations and the potential importance of education on politics, we ask: what is the causal effect of education on turnout and partisanship in the United States?

We provide new evidence on this question by using the universe of voter registration records from 2021 and variation in education induced by school entry cutoffs. Starting school at a younger age, by virtue of being born before the school entry cutoff date, increases educational attainment and changes the quality of one's educational experience (Angrist and Krueger, 1991, 1992; Dobkin and Ferreira, 2010).[2] We begin by estimating the combined, reduced form effect of these changes on voter behavior. Since the voter registration records contain exact date of birth, we use a regression discontinuity design to compare otherwise similar individuals who start school one year apart. This one year difference in school starting age has lasting effects on turnout and partisanship. Individuals who enter school one year earlier are more likely to vote and more likely to register as Independent.

After documenting the combined effect of these changes in the quantity and quality of education on voter behavior, we turn to mechanisms. First, we use the 2000 Decennial Census to estimate the quantity effect of early school entry. We find that starting one year earlier increases educational attainment by 0.03 years, on average. Next, we point out a new fact in this literature regarding heterogeneity in effect size by age. Namely, the discontinuity in educational attainment at the school entry cutoff is the result of a mechanical and temporary one-year difference among college-going individuals and a persistent difference in attainment among individuals who have completed their education. We show that the discontinuity in attainment is much larger for individuals of college-age. Furthermore, the discontinuity in turnout is also larger for younger individuals. We then develop a novel method that leverages this heterogeneity by age to disentangle the quantity and quality mechanisms.

[1] In the early 20^{th} century, some political scientists were concerned that those with more education would be more likely to realize that the individual costs of voting were not trivial but the individual benefits minuscule and thus that they would systematically as a group refrain from voting. This generation of political scientists were thus concerned that only the 'less informed' would vote. Instead, precisely the opposite problem has been of concern. Those with less education are vastly under-represented in the vote.

[2] Although we frequently refer to the reduced form results as the effect of "starting school one year earlier" or "early school entry", compliance with the cutoffs is not 100%. Using data from Texas and California, Dobkin and Ferreira (2010) estimate that individuals born just before the entry cutoff are 62 percentage points more likely to enter school a year early than individuals born just after the cutoff.

We contribute to a sizable literature which estimates the impact of education on voter turnout and a smaller literature which studies the impact upon partisanship. Much of the earlier literature matches individuals based upon observable covariates and compares those with more education to those with less education (Tenn, 2007; Kam and Palmer, 2008; Berinsky and Lenz, 2011). (Milligan, Moretti and Oreopoulos, 2004; Marshall, 2016) study turnout effects by using variation in education across cohorts due to changes in compulsory schooling laws. These papers find that education has no effect on voter turnout. However, these studies rely on self-reported data on turnout, focus primarily on older cohorts of voters, and use research designs that potentially capture long run trends across cohorts in voting behavior.[3] Sondheimer and Green (2010) leverage three experimental education interventions that increased the probability of high school completion to study the effect of educational attainment on turnout. Among the small sample of students involved in these experiments, they find that the treatment that increases high school completion also increases voter turnout.

Two recent papers estimate impacts on partisanship (Marshall, 2016, 2019). Marshall (2019) looks at the United States and uses state-by-year variation from changes in school dropout laws. He finds that high school education leads to a very large decrease in Democratic turnout, on the order of 15 percentage points per each additional year. Marshall (2016) looks at the UK and uses a one-time increase in the school-leaving age that impacted half the country. He finds that this increased votes for the Conservative Party by 12%. Both studies use self-reported survey data on turnout; one concern is that education may increase social desirability bias surrounding voter turnout and thus those with more education may over-report turnout. Also, if the implementation of these policies is correlated with independent political trends across cohorts, then the estimates will capture those broader cross-cohort differences.

We expand upon the prior literature in four key ways. First, we use a regression discontinuity design based on exact date of birth around school entry cutoffs. This obviates concerns about cross-cohort differences and strengthens our claim to causal identification since we compare people who received different schooling yet were born mere days apart. We apply this design to study the impact of education on both turnout and partisanship. For this analysis, we use administrative records on all registered voters as of April, 2021. This circumvents concerns of biased reporting common with survey data. In addition, the large sample increases our statistical power over much of the prior literature and simultaneously bolsters the external validity of our estimates on turnout and partisanship.

Second, we document the first stage effect on educational attainment and the reduced form effect on earnings using a nationwide sample. For this exercise, we rely on the 2000 Census long-form survey, which is administered randomly

[3]Also, to the degree that there is time variation in the effect of education on voting, a recent literature in the econometrics of difference-in-differences estimation shows that two way fixed effects models will yield biased estimatesGoodman-Bacon (2021).

to approximately one in six households. Prior work studying age-of-entry and education in the US has used nationwide data with quarter-of-birth Angrist and Krueger (1991, 1992) or state-level data with exact date of birth Dobkin and Ferreira (2010). Our paper is the first to leverage both the clean identification afforded by exact date of birth and a large sample of individuals across all US states. Again, since the 2000 Census is a random sample of one in six households in the US, our estimates on education and labor market outcomes are also highly powered and externally valid.

Third, we point out a heretofore unrecognized fact in the literature on school entry cutoffs (Angrist and Krueger, 1991, 1992; Dobkin and Ferreira, 2010). The discontinuity in educational attainment coming from discontinuities in school age-of-entry laws combines effects from two different subgroups: (1) college students and (2) adult individuals of all ages. Among college students, there is a mechanical and temporary one-year difference in attainment. For example, a college student born before their elementary school entry cutoff will enter sophomore year of college one year earlier than a college student born after their elementary school entry cutoff. Both individuals may go on to complete a college degree, but while they are in college, there will be a temporary one-year difference in their education. Among all adult individuals, there is a persistent difference for those who were induced to increase their high school educational attainment due to entering school one year earlier. We document this important heterogeneity by age in the first stage and in our voter analysis.

Our fourth core contribution uses this heterogeneity by age to disentangle the changes in the quantity of educational attainment induced by entering school one year earlier from the changes in the quality of the educational experience of entering school on year earlier. The intuition of our approach is... We formalize this and provide a new econometric technique...

Overall, we find turnout increases by approximately 3-4 percentage points per each additional year of education. We estimate that two-thirds of this effect is due to increases in the quantity of educational attainment induced by early school entry. We also find a persistent shift away from both major parties with increases in high school attainment leading to increases in registration as an Independent. Finally, we discuss suggestive evidence that college education has negative effects on Republican registration. These results provide new evidence using a credible, high-powered design and administrative data that education increases turnout and has a causal effect on partisanship.

Our paper is structured as follows. In Section II, we discuss the voter data and the Census data in more detail. Then, in Section III, we proceed with our reduced form analysis of the effect of entering school one year earlier on turnout and partisanship. We discuss our baseline methods, standard regression discontinuity validity tests, and present results on our two main outcomes. In Section IV, we explore the potential mechanisms underlying these reduced form results. First, we estimate the effect of entering school one year earlier on educational attainment.

Then, we document the stark heterogeneity in the first stage effects by age. In Section IV.D we describe a new method that uses this heterogeneity to disentangle the quantity and quality mechanisms, and we apply that method to our data. In Section V, we conclude.

II. Data

Our data comes from three sources. First, we use a snapshot of administrative voter registration records from April 30, 2021 to estimate the effect of early school entry on turnout and partisanship. Second, we use data from the 2000 Census to estimate the impact of early school entry on educational attainment. For both of these analyses, we use annual information on school entry cutoff dates by state from Bedard and Dhuey (2012). Bedard and Dhuey (2012) collected these dates for the years 1964 through 2005.

The voter snapshot is provided by L2, a company that aggregates voter registration files across all US states. The underlying state-level voter files are collected by state-level Secretaries of State. The aggregated dataset includes sex, reported or imputed race, exact date of birth, voter turnout history from 2008 to 2020, and reported or imputed partisan affiliation. The race variable is self-reported in many Southern states. Where the race variable is not reported, it is imputed by L2 using.... States maintain records of turnout history within state but do not follow individuals across state lines. In the process of aggregating the state records, L2 attempts to match people as they move, even when they move across states. To do this, L2 uses data from the US Postal Service as well as data from bank accounts and magazine subscriptions to track people as they change addresses.

Partisan registration data is obtained from states in a couple of ways. In some states, voters have the option to register with a party when they register to vote. In other states, L2 models partisanship based on primary participation as well as attributes including gender, race, age, and zip code. Throughout the paper, we show results using partisanship data both from all states as well as from states where partisanship is self-reported (henceforth "non-modeled states") rather than imputed. When we show partisanship results from all states, we are mixing data on actual party of registration with imputed data on partisanship provided by L2.

The census data contains the universe of those who responded to the 2000 Census long form survey. The long form is distributed to roughly one sixth of the U.S. population. We obtain exact date of birth, state of residence, sex, race, whether the individual lives in group quarters[4], educational attainment in years, and annual wage income.

We restrict both the registration data and the census data in three ways. First, voter registration offices frequently fill in blank birth dates with January 1^{st} if

[4]In the 2000 Census, those living in group quarters mainly include the incarcerated as well as those living in nursing homes, mental health facilities, military barracks, shelters, missions and college dormitories.

month and day are missing. If only day is missing, they often fill in the day as the first of the month. As a result, we see a large spike in recorded birth dates on January 1^{st} and smaller spikes on the first day of every other month in the voter registration data. The spike on the first of the month does not come from nearby dates, further suggesting that these are filled in missing values rather than intentionally timed births. Since we cannot determine which voters with a recorded birthday on the first were actually born on the first, we omit them from the voter registration data. To estimate the first stage and reduced forms on similar samples, we also omit them from the census data.[5]

The second data restriction that we impose is to drop all individuals with state-year cutoffs between October 15^{th} and November 17^{th}. Here, we slightly abuse the term state-year. What we mean is a cohort of people within a given state who were within 90 days of the age cutoff for school entry in a given year.[6] We do this because it is well established in the literature that political campaigns target recent adults during election season mobilization. This leads to differences in registration and voting rates between those born just before versus after a general election which can persist Meredith et al. (2009), in some cases over decades Coppock and Green (2016).

The third restriction that we impose is to drop those who were born on the day of, the day after, or the day before the school entry cutoff for a given state-year. There is lower compliance with the policy for those born on these three dates. However, we show that those born two days before the cutoff are still comparable to those born only three days later. By dropping individuals born on those three days, we increase our instrument strength without sacrificing the exogeneity of our research design.

[Insert paragraph(s) detailing summary stats from voter data and census data]

III. Main Results

In this section, we present our reduced form results on the impact of early school entry on voter turnout and partisanship. We begin by detailing the methods used for this analysis. Then, we discuss standard regression discontinuity validity tests. Finally, we present our main results and explore heterogeneity in those results by age.

[5]We did estimate our results using Census data including those born on the first of the month and the results are similar.

[6]For example, in 1979, a child must be five years old by December 31, 1979 in order to start kindergarten in the 1979-1980 school year. For our purposes, the "1979 cohort" includes those born within 90 days of December 31, 1979 despite the fact that half of them were born in 1974 and half in 1975 and that half of them start school in 1979-1980 and half in 1980-1981. They were all within the bandwidth of starting in 1979.

A. Methods

We estimate a regression discontinuity model in which the running variable is birth date relative to the school entry cutoff date. Negative values of the running variable indicate that the person was born before the school entry cutoff and thus should begin school in the current school year. Positive values of the running variable indicate that the person was born after the school entry cutoff and thus should begin school in the next school year. We regress our political outcomes on a binary indicator of being born after the cutoff, the running variable, and the interaction between the indicator and the running variable.

A long literature, starting with Angrist and Krueger (1991), uses small differences in birth date around school entry cutoffs as an instrument to estimate the effect of education on wages. However, since starting school at an earlier age can lead to differing quantity and quality of education, we first document the reduced form effect of early school entry. In IV, we directly estimate the effect of early school entry on the quantity of education and develop a method to disentangle the quantity and quality effects.

In our baseline estimates, we restrict our sample to those born within 90 days of the cutoff. For example, in New York in 1975, a child was required to have turned 5 by December 1 in order to start kindergarten. A five year old in 1975 would have been 30 in 2000. Thus for those in New York in 2000, we include in our sample everyone who was born within 90 days of December 1, 1970 where everyone born before December 1, 1970 within that cohort is treated and everyone born after is not. Our estimation equation for the reduced form is given by:

$$O_{d,s,y} = \alpha + \beta T_{d,s,y} + f(d) + T_{d,s,y} \times f(d) + \gamma_{s,y} + \epsilon_{d,s,y} \quad (1)$$

where $O_{d,s,y}$ is an individual's outcome (i.e., turnout or partisanship for the reduced form analysis) as a function of their day of birth relative to the relevant cutoff for school entry in their state and year. We do not observe the individual's state of birth in the L2 voter data. We assume, both for reduced form and for first stage estimation, that the current state of residence is the state of birth.[7] $T_{d,s,y}$ is the treatment variable which takes on a value of 1 if an individual was born after the cutoff date for school entry and takes on a value of 0 if an individual was born before the cutoff date. We also control for trends in birth date separately below ($f(d)$) and above ($T_{d,s,y} \times f(d)$) the cutoff with a linear polynomial. We include state-by-year fixed effects ($\gamma_{s,y}$). For the main analysis, we focus on individuals born within 90 days of the school entry cutoff. We show robustness to our choice of bandwidth and polynomial in the appendix.

[7]If people migrate to states with different school entry cutoffs for their cohort, this will attenuate both our first stage and reduced form estimates. Since the first stage is estimated fewer years after graduation, we think that the first stage is likely to be less attenuated than the reduced form.

We estimate the equation above on the following outcomes: voter turnout, registration as a Democrat, registration as a Republican, and registration as an Independent. Standard errors are clustered at the level of treatment, namely at the state-year level. Again, for our purposes, year is the school year for which an individual is within 90 days of the entry cutoff.

B. RD Validity Tests

We now turn to the validation of our estimates. The identifying assumption of our design is that potential outcomes are a continuous function of birth date at the school entry cutoff. A potential concern is that people born just before the cutoff are systematically different from those born just afterwards. This is unlikely, especially since school entry cutoffs can change from one year to the next, giving parents limited control over the cutoff their child will face at age 5 even if they can precisely manipulate birth timing. However, since our effects are small, a small fraction of parents delaying birth until just after the cutoff could account for our results if said parents also pushed their children to complete more education. Despite having no a priori reason to expect that this is the case, we validate our estimates in two separate ways. First, we show that there is no sizable or statistically significant difference across the entry cutoffs in pre-treatment covariates. Second, we plot the distribution of the number of individuals by birth date relative to the cutoff and formally perform a McCrary density test. We find no evidence for a systematic difference in those born just before versus just after the school entry cutoff dates. This lends credence to the underlying assumption that potential outcomes are continuous at the entry cutoffs.

We are limited in the covariates we can look at to those available in the 2000 Census. However, we test for discontinuities in race and gender variables at the cutoff. All of the coefficients are extremely small; moreover, all of our t-statistics are below 1 except for the coefficient on Hispanic where the t-statistic is slightly greater than 1. These estimates are shown in Table 2 below.

We also show, using the 2000 Census data, that there aren't more people systematically just below or just above the cutoff. For disclosure purposes, the Census requires us to report a rounded number of observations. As can be seen in 1, the number of observations does not vary around the cutoff and does not vary much overall with the exception of days in roughly thirty day intervals of the cutoff date. This is because we remove the first day of the month from the sample and the first day of a month is often thirty days away from school entry cutoff dates. This varies slightly depending on the month of the cutoff date.

In addition to the density plot, we also conduct a formal McCrary density test. In our main analysis, we employ a bandwidth of +/- 90 days around the cutoff and a uniform kernel. Under those choices, we cannot reject the null that the density is smooth through the cutoff (p-value = 0.25). We also conduct the density test using the MSE-optimal bandwidth of +/- 23 days and a triangular kernel. Again,

Table 2: Covariate Balance - Census

	(1) Female	(2) White	(3) Black	(4) Hispanic	(5) Asian	(6) Other Race
Born After 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***	0.04503***	0.0106***
	(0.0007)	(0.0006)	(0.0000)	(0.0005)	(0.0003)	(0.0001)
Number of Obs (Rounded)	4,049,000	4,049,000	4,049,000	4,049,000	4,049,000	4,049,000
R-Squared	0.0005	0.1323	0.0550	0.1561	0.0604	0.0288

Notes: Covariate balance for selected variables from the US 2000 Census. Results from estimating equation 1 for each variable. Running variable is equal to 0 for the school entry day. Data from L2 for states with registered partisanship and states with modelled partisanship. We drop all individuals with state-year cutoffs between October 15th and November 17th. We also drop individuals with date of birth January 1st from the sample. *** p<0.01, ** p<0.05, * p<0.1

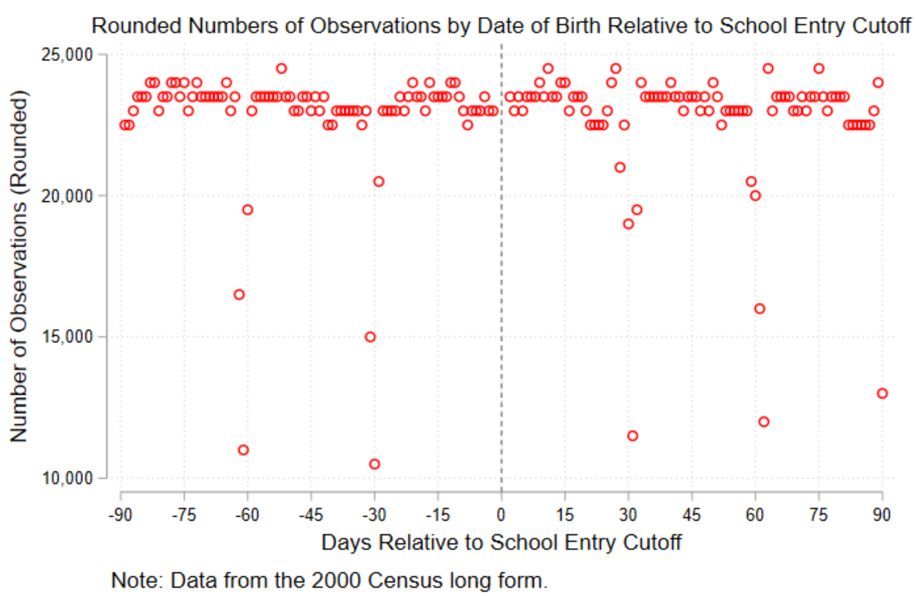


Figure 1. Number of Observations by Birth Date Relative to the Cutoff

we cannot reject the null of a continuous density at the cutoff (p-value = 0.20).[8] The results are presented in Table 3 below.

[8]Although we can only disclose a rounded number of observations, per Census rules, the density test is conducted on the actual underlying data, not the rounded data.

Table 3: McCrary Density Test for Change in Density at the Threshold

BW: Type	BW: Left	BW: Right	P-Value	# Obs (Rounded)	Kernel
Optimal	23	23	0.1957	1,097,000	Triangular
User-Chosen	90	90	0.2518	4,049,000	Uniform

Notes: Results from performing the density test for manipulation of the running variable as described in McCrary (2008). Running variable is equal to 0 for the school entry day. Data from L2 for states with registered partisanship and states with modelled partisanship. We drop all individuals with state-year cutoffs between October 15th and November 17th. We also drop individuals with date of birth January 1st from the sample. *** p<0.01, ** p<0.05, * p<0.1

C. Turnout

We now discuss the impact of early school entry on voter turnout and partisanship. Since we expect that early entry impacts both quantity of educational attainment and the quality of one's educational experience, we do not interpret these results as operating solely through the quantity of education channel. In subsequent sections, we directly estimate the quantity effect and we develop a method that separately identifies the quantity and quality channels.

For the reduced form analysis, we begin with our results on voter turnout. We limit our sample to those between 39 and 60 years old so that the individuals are from the same cohorts as those we study in the 2000 Census. We focus on turnout in the 2016 general presidential election, pooling estimates across state-year cohorts. We find that early entry into school causes a 0.2 percentage point increase in turnout.[9] The estimate is very precise and statistically differentiable from no effect at well below the 1% level. Our results are below in Figure 2. In the appendix, we show that our results are robust to choice of bandwidth and kernel. In addition, we find similar results across all election years going back to 2008, including midterm elections.

D. Partisanship

We now turn to estimates of the impact upon partisanship. We separately estimate impacts upon three different measures of partisanship: Democrat registration, Republican registration and independent registration. We can measure partisanship in closed primary states where registration with a party is necessary for voting in that party's primary[10], those states where parties can choose to allow independents to vote in their party primaries[11], states where only unaffiliated voters can choose which primary to participate in[12] and one state with party registration for the purposes of presidential primaries, county central committee and local offices only[13]. We define independents as those not registered for one of

[9]Since the indicator variable is equal to one for people born after the cutoff, the coefficient represents the effect of late entry. We multiply by -1 to discuss in terms of early entry.
[10]The states with closed primaries include DE, FL, KY, MD, NV, NM, NY, OR, and PA
[11]CT, ID, NC, OK, SD, UT
[12]AZ, CO, KS, MA, ME, NH, NJ, RI and WV
[13]CA.

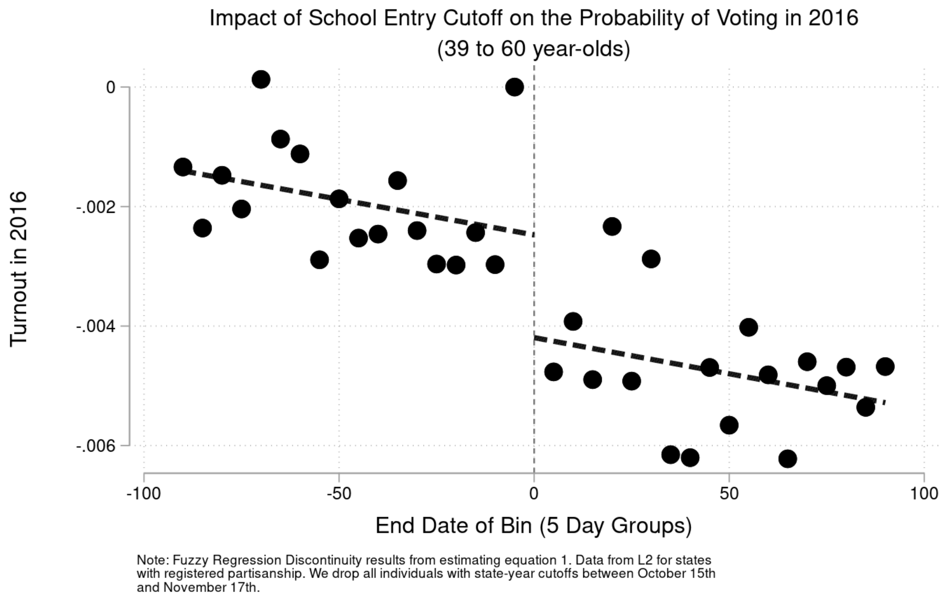


Figure 2. Voter Turnout in the 2016 General Election by Birth Date Relative to the Cutoff

the two main parties. This includes both those not registered with a party and a relatively small number of others who are registered with a small party (i.e. the Green Party, the Libertarian Party, and a number of other smaller parties).

We separate our estimates as we did with turnout into two types: those broken down by age for younger voters (19-40 year olds) and aggregate estimates for those 39-60 years old who correspond to the 19-40 year olds in the 2000 Population Census.

We find no sizable or statistically significant average impact in the long run (i.e. for 39-60 year olds) on either the Republican or Democrat registration share Appendix Figures A3 and A2. However, we do find that early entry into school does increase the fraction of independent voters. We show our estimates below.

We also break down our estimates by age for 19-40 year olds and we show that estimates are larger for those in college but do persist at least through the late 30s. Unfortunately, we do not estimate the impact of early school entry for these cohorts of individuals since the 2000 census was the last census to offer the long form and the American Community Survey has a sample size that is too small to obtain precise estimates. We break down estimates by age in years in Figure (4).

Finally, we show that during college-aged years, those who entered early and thus have an extra year of college completed are less likely to register Republican and more likely to register Democrat. However, these estimates only exist for

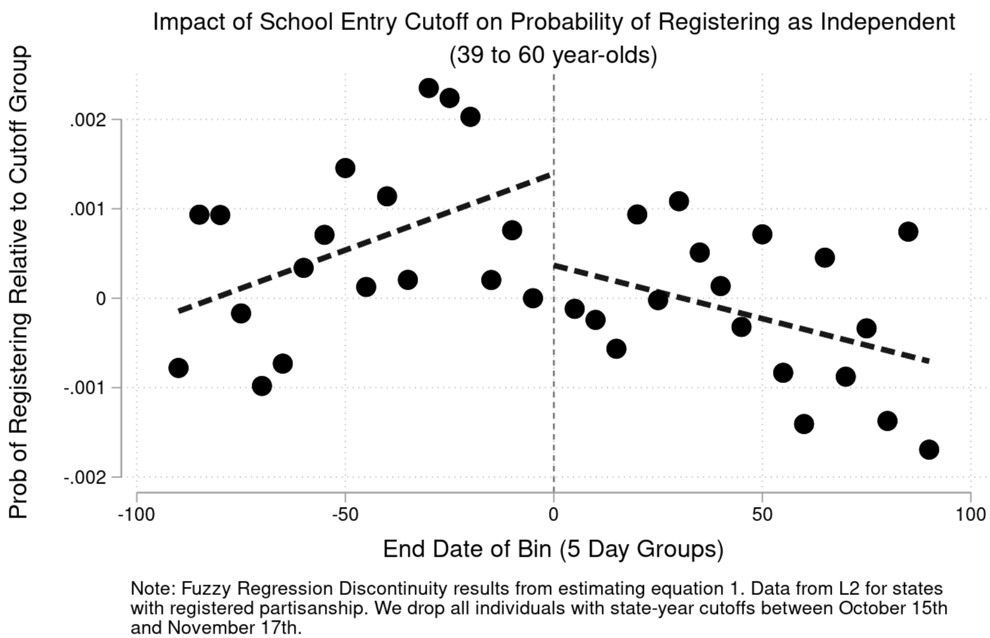


Figure 3. Long run Impacts of Late Entry on the Republican Vote Share

those of college-age and likely reflect a college impact but we see no evidence of a similar effect on politics for those of high school age (see Appendix Figures (A4) and (A5)). Since our instrument only impacts college attainment for those in the late teens and early 20s, we have no way of assessing whether these sizable shifts in political identification persist.

IV. Mechanisms

A. *Methods*

B. *Quantity Effects*

We begin by showing estimates of the impact of birth after the threshold on educational attainment in Figure 5 below. There is a very clear though small discontinuity in educational attainment at the birth date cutoff for school entry. Individuals who are just a few days younger (born a few days later) and just after the cutoff for entry into school in a given year on average have 0.0335 fewer years of education. We show these results in Table 1.

We also show that these increases in educational attainment translate into

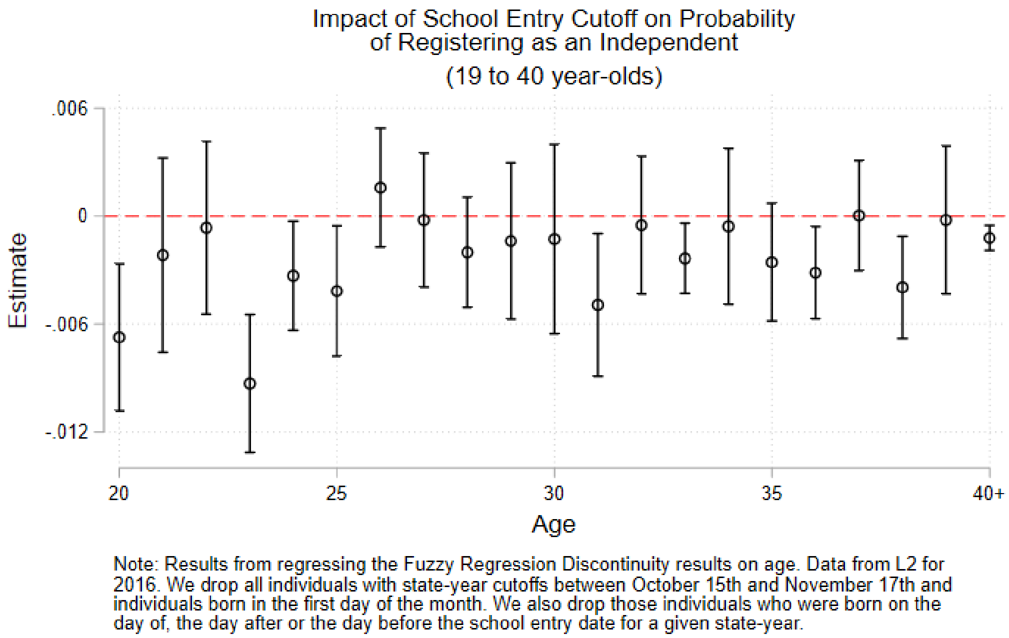


Figure 4. Impacts Upon the Political Independent Share by Age

higher average wage earnings. Estimates on the impact on income are slightly noisier though statistically significant at a 1% level. This is unsurprising since a higher fraction of the variation in wages is measurement error than the variation in education. On average a worker born just after a school entry cutoff earns \$326 less than a worker born just before the discontinuity.

Looking at the figure where we bin observations into 18 groups to the left and 18 to the right of the discontinuity, only two of the 18 observations on the right are above any of the observations on the left and even these two observations are below 15 of the 18 observations on the left. Moreover, there is little slope to the left of the discontinuity. The differences across the discontinuity are clearly due to the discontinuity itself.

What are the sources of the discontinuity in educational attainment? Why does starting earlier increase long run educational attainment. Underappreciated in prior literature, we show that there are actually two separate reasons. First, there is the reason which the prior literature has focused upon (Angrist and Krueger (1991), Dobkin and Ferreira (2010). Stephens Jr and Yang (2014)). High school graduation is not mandatory in the United States. States have individual minimum high school dropping out ages. If one person starts school a year early, they will have a year more of schooling before they are eligible to drop out. Much

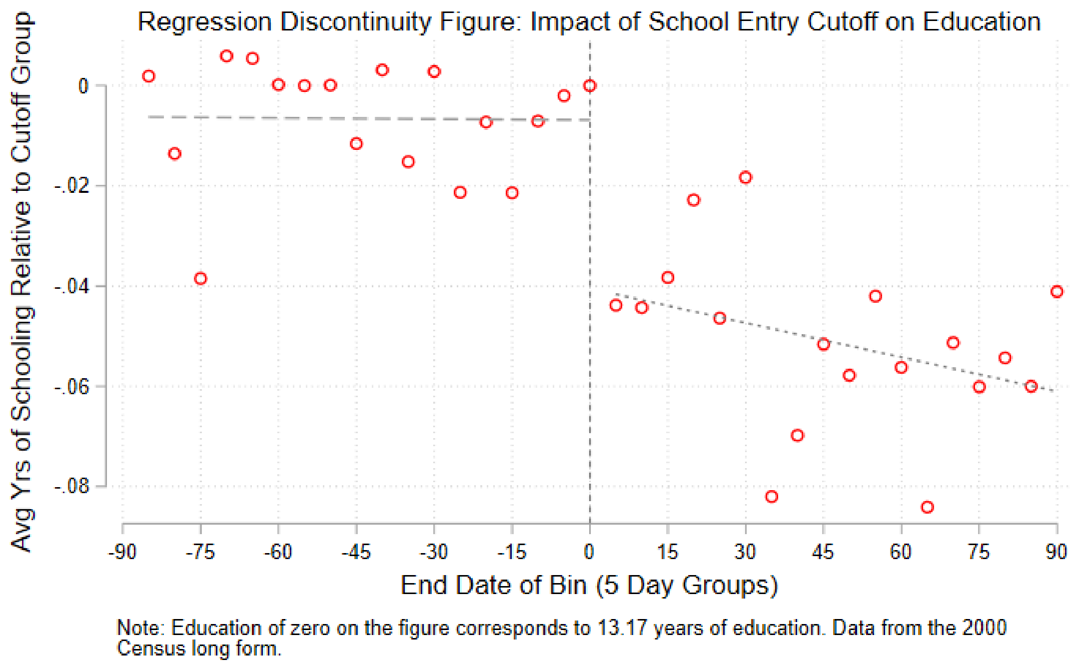


Figure 5. Education Impacts of Age Discontinuities From School Entry Laws

of the variation across the discontinuity, therefore, comes from the bottom of the education distribution.

Additionally, however, there is variation for those in their late teens and earlier 20s from those who are contemporaneously in college. Mechanically, if Sarah starts school a year earlier than John because she is born a few days earlier, and if both Sarah and John go straight to college, then when both are 20 years old and Sarah is a Junior in college, John is a sophomore and has one fewer years of education. We provide evidence for the existence of these two separate channels in Figure 6 where we estimate the impact of being born just before versus after the school entry cutoff date separately for each age (in years)[14]. We see that the estimates are substantially larger in magnitude for 18 year olds than for 19 year olds and for 19 year olds than for 20 year olds. For 18 year olds, those born just after the cutoff have on average -0.2966 years less education than those

[14]We note that not everyone is sampled by the Census on the same date. Thus, it is possible that the variation in educational attainment partially reflects not only actual differences in educational attainment but also differences in sampling date. Given that these differences in educational attainment reporting are random based upon date of sampling, this should widen the standard errors for our estimates but should otherwise be inconsequential.

Table 1: Effects of Birth After Cutoff on Education and Income

	(1) Years of Schooling	(2) Total Income
Born After Cutoff	-0.0335***	-325.90***
	(0.0061)	(67.69)
Constant	13.17***	24080***
	(.003935)	(42.81)
Number of Obs (Rounded)	4,049,000	4,049,000
R-Squared	0.03015	0.07086

Notes: Results from estimating equation 1 for years of schooling in column (1) and total income in US dollars (2) in column 2. Running variable is equal to 0 for the school entry day. Data from L2 for states with registered partisanship and states with modelled partisanship. We drop all individuals with state-year cutoffs between October 15th and November 17th. We also drop individuals with date of birth January 1st from the sample. *** p<0.01, ** p<0.05, * p<0.1

born just before. The standard errors are very tight and we can easily reject a coefficient even of -0.2450. For 19 year olds, this coefficient drops in magnitude to -0.1669 and for 20 year olds to -0.0609. These coefficients are all statistically distinguishable from each other at conventional levels of statistical significance. Afterwards the rate of decline in the magnitude of the coefficients becomes much smaller and by age 25, it shows no systematic trend whatsoever.

Why is there such a sharp decline in the magnitude of the coefficients in the late teens and early twenties? The reason is that age becomes less predictive of college educational attainment with each additional year. Some students defer college while others enroll and then drop out. This attenuates the birth discontinuity estimates. Eventually, many students also graduate from college. By the time people reach their early to mid 20s, birth discontinuities are no longer predictive of how much college education an individual has. The coefficients stabilize at slightly more (in magnitude) than -0.03 years for being born just after a birth cutoff.

We additionally show the impacts of being born after the cutoff on minimal educational attainment. We construct variables for having a minimum of 2, 6, 8, 9, 10, 11, 12, 13, 14, 16, 18 and 20 years of education respectively. We then estimate our regression discontinuity with each of these variables individually and plot the results in Figure 7 below. We find that starting early has its largest impact on completing 10^{th} grade through the first year of college. The peak impact is for 12 years of education (11^{th} grade completion) followed by 14 years of education (first year of college completion). In both cases, the impact is slightly more than 0.5 percentage points. Birth just after the cutoff reduces the probability

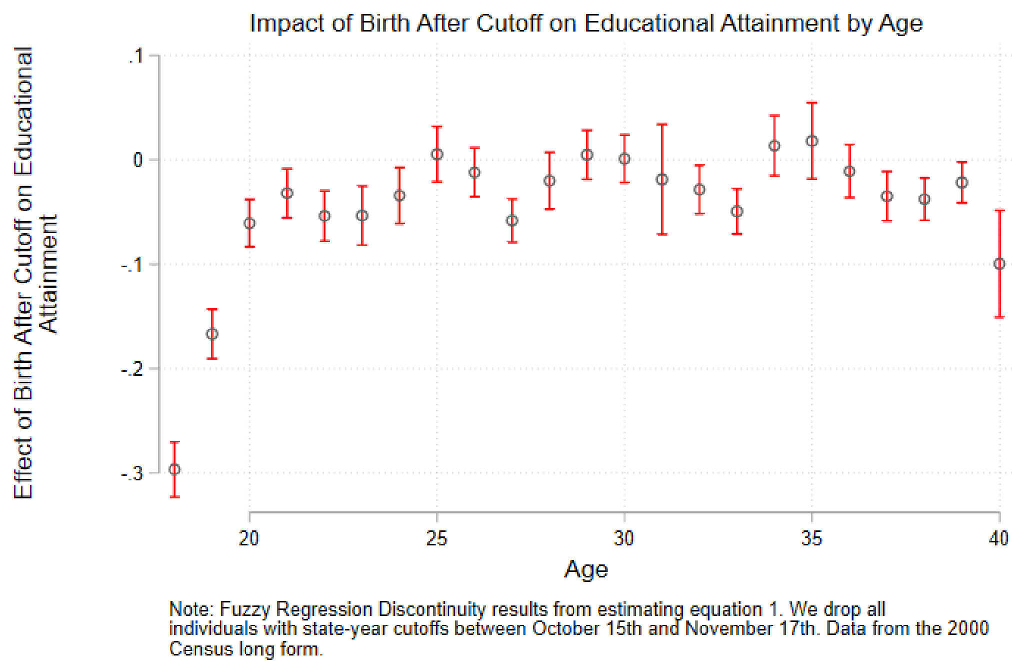


Figure 6. Educational Discontinuities From School Entry Laws by Age

of completing 12 years of education by 0.54 percentage points and reduces the probability of completing 14 years of education by 0.53 percentage points.

C. Heterogeneity by Age

Next, we break our estimates down by age and find substantial heterogeneity in effects. We display separate estimates for each age from 19 to 39 in Figure 8 and a pooled estimate for anyone aged 40 or above. The figure displays a piecewise linear pattern–we find the effect of early entry is decreasing in age from ages 19 to 28 but that the effect is similar for all ages above 29 and above.[15]. Later, we explore similar heterogeneity by age in the effect of early school entry on educational attainment.

From cursory inspection, the decline in the magnitude of the impact upon turnout appears longer-lasting than the decline in the impact on education. However, this is consistent with an initial impact of education on turnout and a dy-

[15]We drop the estimated effect for age 18. The estimates for 18 year olds are an outlier. They are positive (i.e., being born after the cutoff is associated with higher not lower participation). We view this as mechanical given that those who are born after the cutoff are substantially more likely to be in high school and to be registered at their parents' house through programs in their local high school

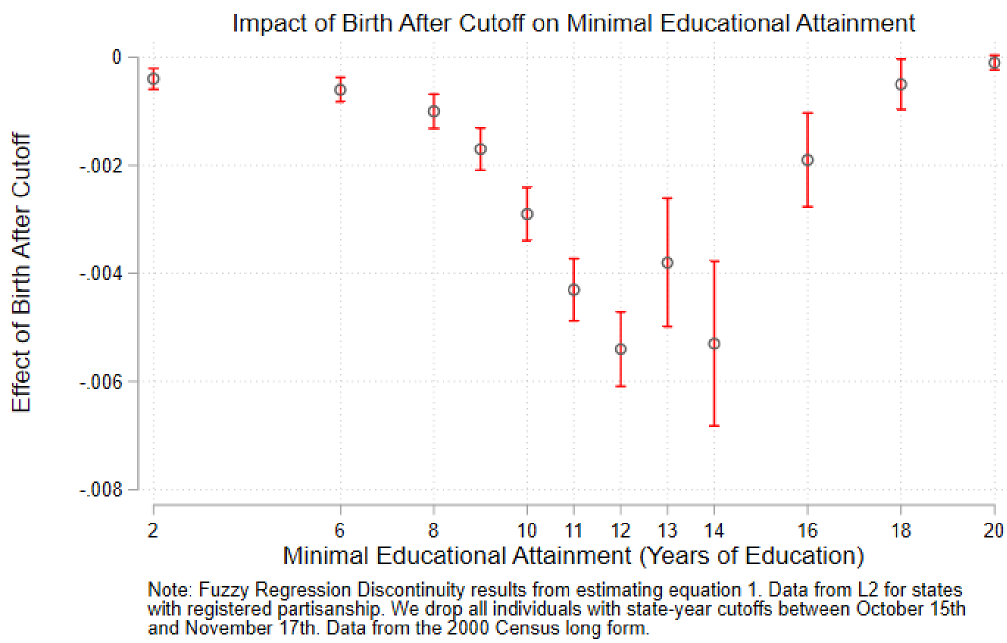


Figure 7. Estimates of Effects on Minimal Educational Attainment

namic impact of turnout itself. There is evidence from the United States of a short run dynamic impact of voter turnout on turnout in the subsequent election (Meredith et al. (2009)) and mixed evidence for a longer run dynamic effect (Coppock and Green (2016)). In the context of the United States, these dynamic effects have two potential sources. First, there are fixed costs to voting (namely registration and learning where your your polling station is. Second, voting may increase the taste for future voting. Our evidence of a longer-lasting effect on turnout than on educational quantity is consistent with an impact of educational attainment on turnout in the short run and short-run dynamic impact of earlier turnout on later turnout.

D. *Decomposing Quality versus Quantity*

In this section, we try to cohesively interpret our estimates and show how we can isolate the pure impact of the expansion of the quantity of education.

We will present IV estimates. We first note that though technically, we are estimating using a two sample instrumental variable estimate, the sample for our reduced form contains the universe of registered voters in 2021 and our first stage is the universe of people in the 2000 census. Thus, there is a very substantial

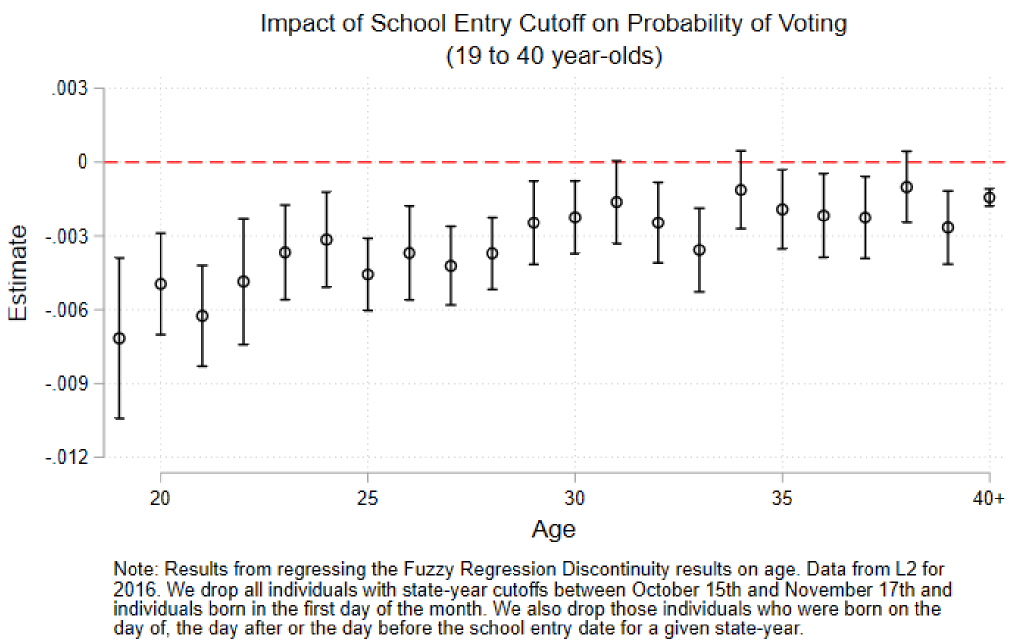


Figure 8. Estimates of Early Entry Impact of Schooling on Turnout by Age

overlap across the two samples. Nonetheless, our sample size is sufficiently large and our first stage sufficiently predictive that we are not worried about small sample IV bias.

We first note that the first stage coefficients on educational attainment and the reduced form coefficients on turnout both are linear in age for those in their late teens and 20s. We now show how we can isolate the pure impact of quantity of education from the IV of the impact of education on turnout. Isolating the quantity component of the effect of education on turnout is something that the prior literature has so far been unable to do.

We note that the decline in the magnitude of the coefficient of age on birth after the discontinuity is due to greater predictive power for the discontinuity with age. That quantity component, however, dies off linearly over time. In contrast, the effect of the quality component (the experience of the having started school early) is likely to be the same for 19 year olds as for 20 year olds. There will likely be a persistent effect of having started earlier on educational attainment. However, that is not likely to differ for the set of 19 year olds versus 22 year olds.

The time invariance of the quality component for young adults in contrast with the time variation in the quantity effect allows us to extract the quantity component. We can regress separately the reduced form and the first stage. The quality component will be in the constant coefficient and the slope coefficient will contain only the quality component. We can thus take the ratio of the reduced form slope coefficient to the first stage slope coefficient and that should give us the effect of education on turnout through increased educational quantity.

We now show this analytically. Let $\beta_{s,c}$ be the structural coefficient of the impact of education upon an outcome for a given stage s and cohort c where cohorts are delineated by their age (i.e. 18, 19, 20). The IV estimate of the effect of education for a given cohort upon turnout is thus $\frac{\beta(c)_{rf}}{\beta(C)_{fs}}$. If $\beta(c)_s = \alpha(c)_s + \gamma(c)_s t$ where α is the quality (or rank-order) effect and $\gamma(c)$ is the quantity effect, you claim is that $\beta(c)_s)$ has the particular form of $\alpha + \gamma(c)$. Thus whereas the overall IV is $\frac{\beta(c)_{rf}}{\beta(c)_{fs}} = \frac{\alpha_{rf}+\gamma(c)_{rf}}{\alpha_{fs}+\gamma(c)_{fs}}$ and thus not interpretable as the effect of having more education (because the quality effect is confounding), we can first difference both the first stage (fs) and reduced form (rf) separately and then take the ratio (using Slutsky) to get:

$$(2) \qquad \frac{\Delta\beta_{rf}}{\Delta\beta_{fs}} = \frac{\alpha_{rf} + \gamma(c+t)_{rf} - \alpha_{rf} - \gamma(c)_{rf}}{\alpha_{fs} + \gamma(c+t)_{fs} - \alpha_{fs} - \gamma(c)_{fs}} = \frac{\gamma(c+t)_{rf} - \gamma(c)_{rf}}{\gamma(c+t)_{fs} - \gamma(c)_{fs}}$$

With one additional assumption: linearity in coefficients as a function of age, we can get the desired result. Assume that $\gamma(c+t)_s = t\gamma(c)_s$. In this case, we get the first difference ratio IV to be $\frac{(1-t)\gamma(c)_{rf}}{(1-t)\gamma(c)_{fs}} = \frac{\gamma(c)_{rf}}{\gamma(c)_{fs}}$ which is what is desired: the IV estimate for the quantity effect. This is true if the rank-order effect is constant and if the γ effect scales linearly with cohort (or actually just proportionately across the first and second stage which would be a weaker assumption).

We now show linear extrapolations of both our first stage and of our reduced form. We regress our estimates on age for three different age ranges: 19-35, 19-28 and 19-25. As we see, our estimates from a 0.0055 to a 0.0196 years in education increase in the size of the discontinuity per year of education (i.e. reduction of the magnitude) as we reduce the size of the age range. These are presented in Table 5:

We choose the middle estimate (19-28 year olds) and present the figure for the corresponding estimate of turnout:

As we see, these are upwards sloping as well. We now combine the first stage and reduced forms into an instrumental variables estimate which captures the pure quantity impact of education. As we can see, the numerator of the IV extracts the pure quantity effect on turnout and the denominator extracts the pure quantity effect on education. The ratio of these two are 0.0303. The IV estimate of the reduced form to the first stage for the total effect is 0.0448. These two estimates are remarkably similar and not statistically differentiable. We see the similarity between the ratio of the linear trends in age of the reduced form to the IV coefficients to the actual IV coefficient as, in part, validation of our

Table 5: Linear Trend Estimates of Effects by Age: Education

	(1)	(2)	(3)
Model Description	**Education**	**Education**	**Education**
Linear Estimate	0.0055***	0.0099***	0.0196***
	(0.0011)	(0.0024)	(0.0040)
Constant Coefficient	-0.1814***	-0.2815***	-0.4872***
	(0.0288)	(0.0552)	(0.0868)
Number Observ.	3132000	1759000	1239000
R-Squared	0.3759	0.3348	0.5515
Age Range	19-35	19-28	19-25

Running variable is equal to 0 for the school entry day. Data from L2 for states with registered partisanship and states with modelled partisanship. We drop all individuals with state-year cutoffs between October 15th and November 17th and individuals born in the first day of the month. We also drop those individuals who were born on the day of, the day after or the day before the school entry date for a given state-year. *** p<0.01, ** p<0.05, * p<0.1

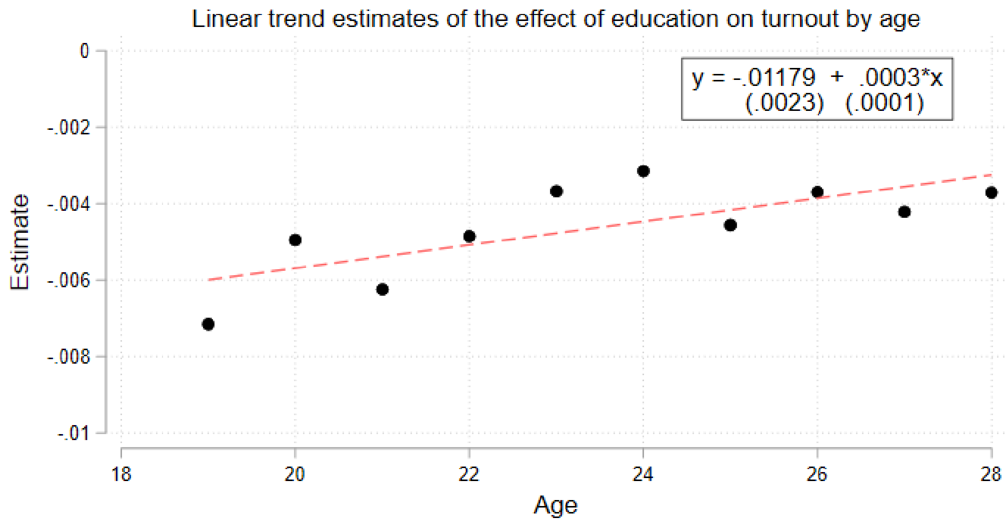
approach. We present our results in Table 7:

We do not do this for partisanship because the time paths are not linear and thus it is not possible to extract the quality component of the impact of education on partisanship. However, we see our methods for isolating quantity and quality components of educational attainment increases as broadly applicable throughout many areas of economics.

V. Conclusion

In conclusion, we present new estimates of the effect of education on politics using school entry age discontinuities. We obtain our estimates from the sixth of the United States that answered the long form of the 2000 Population Census along with the universe of registered voters in 2021. We further present new methods to separate out the pure quantity effect of education from the quality effect. Our findings show that early entry increases turnout by 4.5 percentage points. Three percentage points or two thirds can be attributed to a pure quantity effect and we cannot reject that 100% of the effect is due to quantity. We also find short run negative effects of Republican registration from college education and a shift towards registration as an independent from additional high school years.

We do not separate out quality versus quantity effects of education on partisanship. Nor do we estimate long run impacts on college education. Finally, education can impact voting behavior through multiple channels including by



Note: Results from regressing the Fuzzy Regression Discontinuity results on age. Data from L2 for 2016. We drop all individuals with state-year cutoffs between October 15th and November 17th and individuals born in the first day of the month. We also drop those individuals who were born on the day of, the day after or the day before the school entry date for a given state-year.

Figure 9. Linear Trend Estimates of Effects on Turnout by Age

raising income. Since we could not match individual voting records to individual census records, this was beyond the scope of our current paper. However, we think these would all be excellent avenues for future research.

Table 7. Isolating the Quantity Effect: Turnout

Description	(1) Coeff.
Time Varying Component (RF)	0.0003
Time Varying Component (FS)	0.0099
Pure Quantity Effect	0.0303
IV Reduced Form	-0.0015
IV First Stage	-0.0335
IV Estimate	0.0448
Ratio of Quantity to Total IV	0.6768

Notes: Results from estimating equation 2 to capture the pure quantity effect of education. We restrict the sample to the 19-28 age range.

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Appendix

A1. Supplemental Results

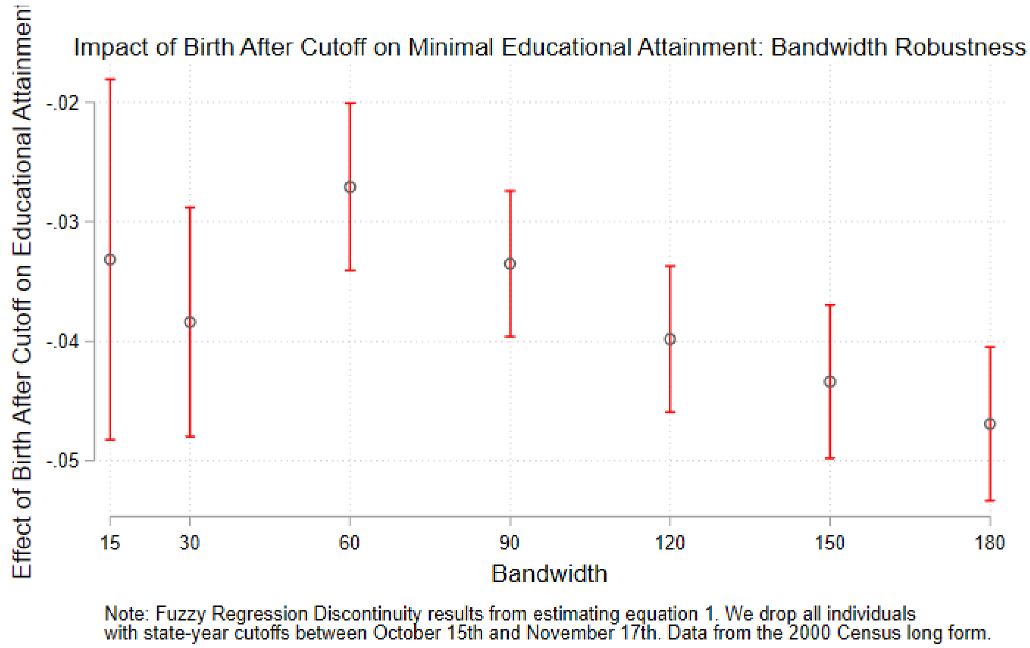

Figure A1. Impact of School Entry Cutoffs on Educational Attainment: Bandwidth Robustness

A2. Incarceration Effects

We would like to use the differences in educational attainment across the discontinuity as the first stage of an instrumental variables estimation strategy to estimate the causal impact of the quantity of educational attainment on political outcomes. However, there are interpretational problems in doing so. Anders, Barr and Smith (2022) show that early entry into school also increases incarceration. They estimate impacts in the state of North Carolina. We replicate their results for the entire U.S. population and find similar effects.

It is somewhat surprising that a reduction in educational attainment is associated both with an average reduction in wage income and in incarceration. This is surprising since one would have expected the reduction in educational quantity to increase the incarceration rate. The resolution of this apparent paradox is that birth *after* the discontinuity on the one hand *reduces* the quantity of educational attainment but also *increases* the quality of education as experienced by the individual. There is a volume of research (Dhuey et al. (2019) which shows that early entry into school leaves kids behind academically and socially. The combination of the reduction in quantity and increase in quality from birth after the discon-

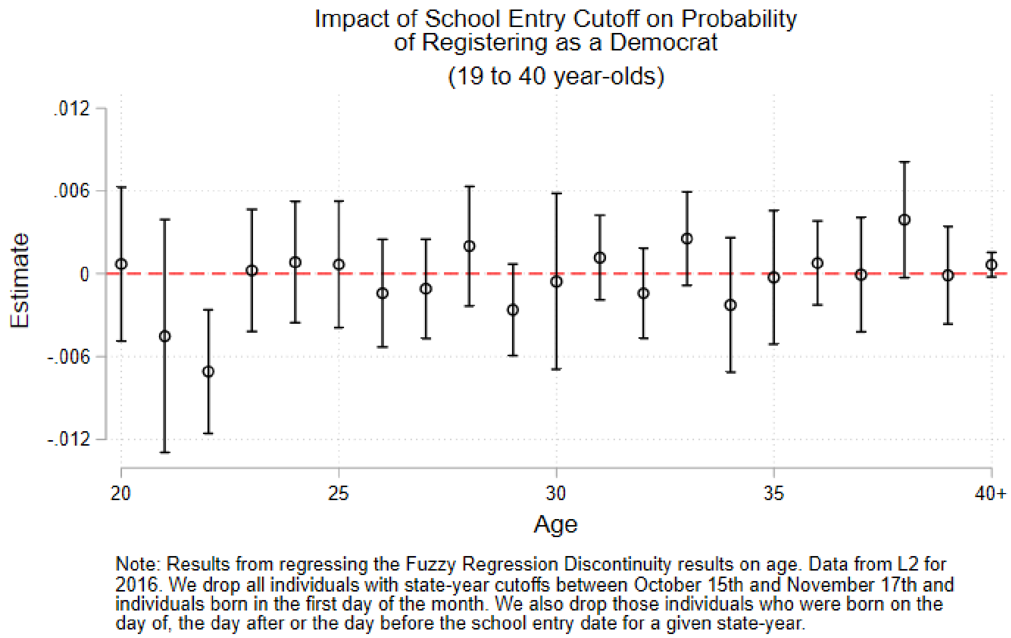

Figure A2. Long run Impacts of Late Entry on the Democratic Vote Share

tinuity is what is responsible for the combination of the decline in wage income but the increase in incarceration. For some individuals the effect of the extra education dominates; for a small minority, however, the effect of reduced quality dominates and incarceration results. This creates a problem in interpretation for the IV estimates of educational attainment on politics using the sharp discontinuities in birth date due to school age of entry laws. To be clear the IV estimates are still interpretable as an effect of education. However, it is not clear whether the effect is attributable to greater quantity or worse quality of education. In the next section we will look at the impact of early school entry on voter turnout and political partisanship. We will present estimates and then show how we can, in the case of voter turnout isolate the pure impact of the quantity as opposed to quality of education.

In sum our main findings on birth discontinuities and educational attainment are thus (1.) birth just after a discontinuity reduces average educational attainment by 0.0335 years, (2.) estimates are substantially larger for those in their late teens and early 20s because these estimates combine a long-run effect on high school completion along with a short run mechanical effect of contemporaneous college attendance, (3.) these estimates are cleanly estimated and highly robust, and (4.) early entry into school both increases the quantity of educational

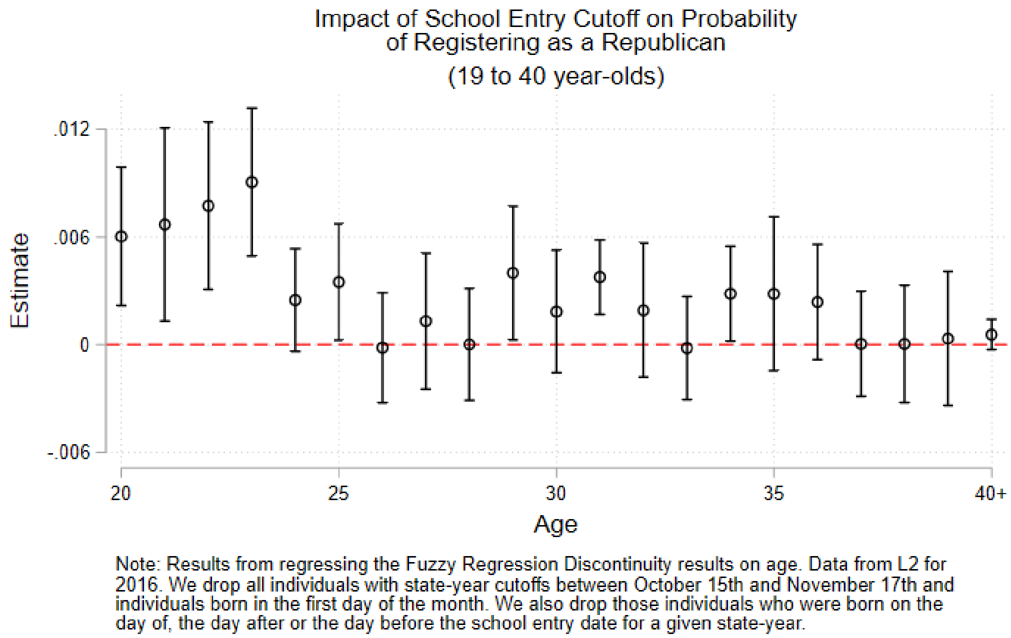

Figure A3. Long run Impacts of Late Entry on the Republican Vote Share

attainment and also reduces educational quality.

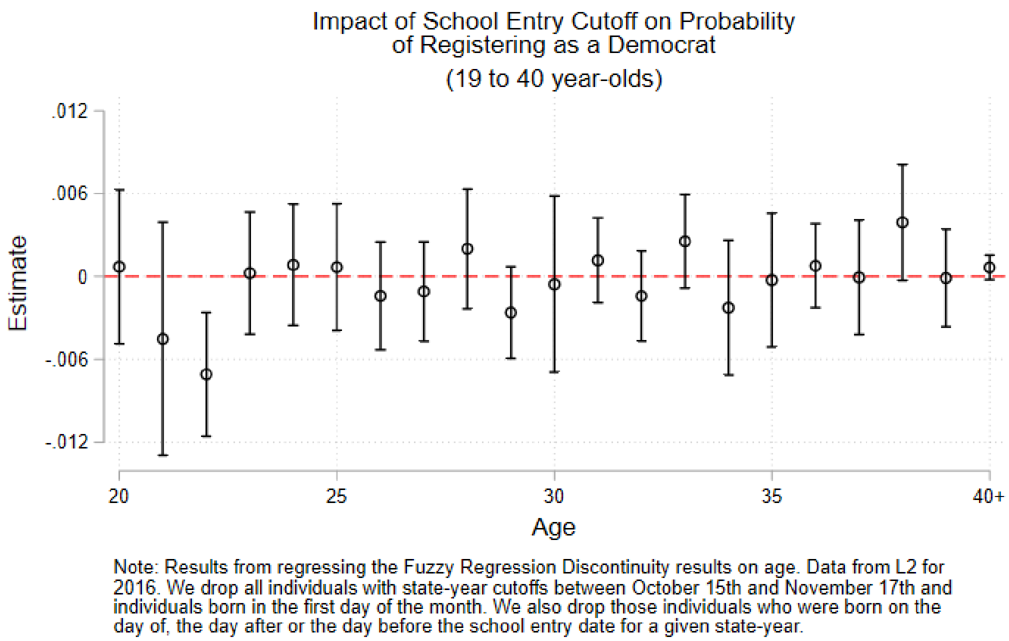

Figure A4. Impacts of Late Entry on the Democratic Vote Share by Age

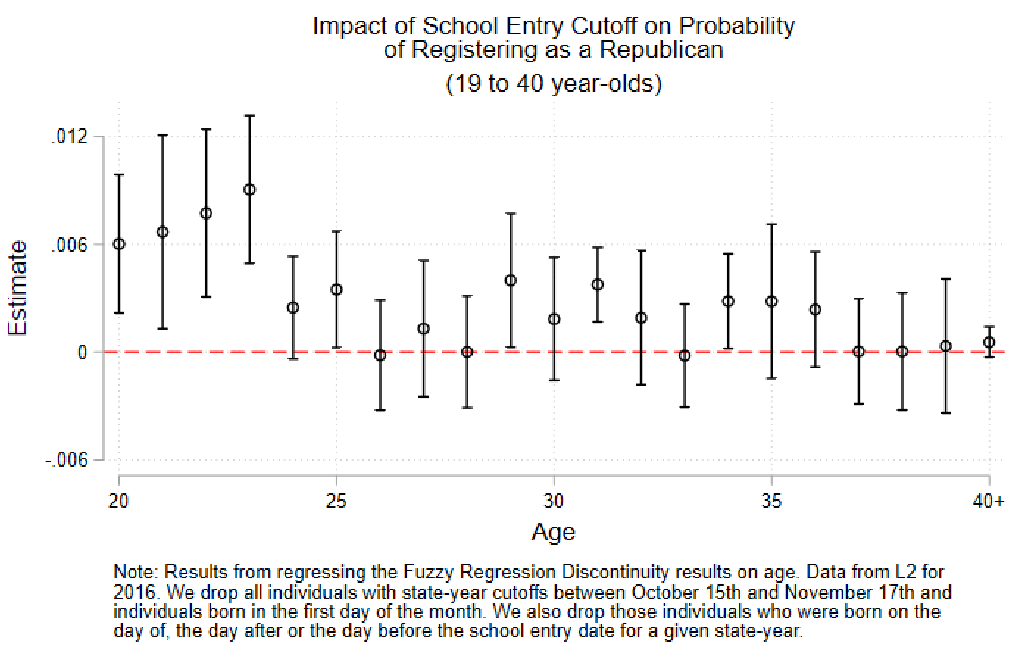

Figure A5. Impacts of Late Entry on the Republican Vote Share by Age

Table A1: Effects of Birth After Cutoff on Education by Gender

	Years of School	Income
	(1)	**(2)**
Birth After Cutoff	-0.0288***	-359.9***
	(0.0082)	(110.9)
Birth After X Female	-0.0087	47.6
	(0.0107)	(135.4)
Constant	13.01***	30370***
	(0.0069)	(212.9)
Number of Observations	4049000	4049000
R-Squared	0.0332	0.1038

Notes: Results from estimating equation 1 by gender for years of schooling in column (1) and total income in US dollars (2) in column 2. Running variable is equal to 0 for the school entry day. *** p<0.01, ** p<0.05, * p<0.1. Running variable is equal to 0 for the school entry day. Data from L2 for states with registered partisanship and states with modelled partisanship. We drop all individuals with state-year cutoffs between October 15th and November 17th. We also drop individuals with date of birth January 1st from the sample. *** p<0.01, ** p<0.05, * p<0.1