School Entry, Compulsory Schooling, and Human Capital Accumulation: Evidence from Michigan*

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Abstract

Extant research on school entry and compulsory schooling laws finds these policies to increase the high school graduation rate of relatively younger students, but weaken their academic performance. In this paper, we explore the postsecondary effects of the interaction of school entry and compulsory schooling laws in Michigan. We employ a regression discontinuity (RD) design using longitudinal administrative data to examine impacts on high school performance, college enrollment, choice, and persistence. On average, we find that children eligible to start school earlier persist for fewer postsecondary semesters, and are 2 percentage points more likely to first attend a 2-year (rather than 4-year) college, compared to their older peers. Relatively younger females are more likely to enroll in college, but this result does not hold for males. Finally, we illustrate that the increase in the high school graduation rate attributable to this set of laws is driven by impacts on economically disadvantaged students.

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I. Introduction

Across the United States, two types of policies work in tandem to structure the amount of time a student is legally required to spend in public K-12 schooling. The first dictates the age at which children can start school. The second sets a minimum age at which students can drop out of school. Depending on a child's birth date, this set of policies has different implications for the amount of time she is legally compelled to remain in school: For example, students who start school relatively young for grade reach the legal age of dropout after acquiring more formal time in school than their older peers. These two policies have spawned several lines of rich inquiry.

One line of research has focused on questions concerning the impacts of school starting age on short-run outcomes such at test scores (Datar, 2006; Dobkin & Ferreira, 2007; Elder & Lubotsky, 2009). The consensus from this line of inquiry is that relatively older students perform better than their younger counterparts. Recent work based in the United States and abroad argues that this performance advantage is not driven by the ability of relatively older students to "learn better," but rather by "age-at-test" effects (Black, Devereux, & Salvanes, 2011) and the stock of skills accumulated prior to kindergarten entry (Elder & Lubotsky, 2009).

A second line of research has exploited changes in the legal dropout age or the intersection of school starting age and compulsory schooling laws to examine net effects on long-run outcomes such as educational attainment and earnings (Angrist & Krueger, 1991; Cook & Kang, 2013; Dobkin & Ferreira, 2010). A consensus seems to be developing that there is little to no effect of this set of laws on labor market earnings (Dobkin & Ferreira, 2010; Black et al., 2011; Fredriksson & Ockert, 2005); yet, impacts on measures of longer-run educational attainment (especially among different subgroups of students) remain less clear.

In this paper, we use rich student-level administrative data from the state of Michigan covering ten years (2002-2003 through 2012-2013) to estimate the joint impact of school starting age and compulsory schooling laws on educational attainment through high school, academic performance in high school, postsecondary enrollment, choice, and persistence. In addition, we examine whether impacts on educational progression and achievement during the K-8 years found in past work appear in the Michigan context. We then describe how these earlier results necessarily shape later outcomes. We employ a regression discontinuity (RD) design, wherein we compare the educational experiences of students whose birth dates caused them to just barely miss the calendar cutoff for starting kindergarten in a given year to students who barely met this cutoff. The identifying assumption is that these two groups of students are similar in all ways except one, the age at which they are eligible to start public schooling. Therefore, any differences in later outcomes can be attributed to this set of laws and not to other personal, family, or school-level factors known to affect progression through school, performance, and educational attainment.

We aim to make several contributions: First, we examine the joint, long-run impacts of school entry and compulsory schooling laws on measures of postsecondary choice and persistence. These outcomes are new to the literature. Second, we examine effects of this same set of laws on educational attainment through the end of high school as well as on academic performance while in high school. Many of these outcomes have been examined in previous work. Yet, the policy settings (e.g., domestic versus international), data sets (e.g., national probability surveys versus state-level data), and data quality (e.g., self-reported versus administrative outcomes) vary quite a bit across this vast literature. In such cases, it is important to have evidence from different, unrelated sources of data (and policy contexts) on the same underlying outcome constructs of interest. To this end, we develop an analytic approach that estimates upper-bound effects on our high school and postsecondary outcomes of interest (i.e., what the intent-to-treat effects would become if all students complied with school entry and compulsory schooling laws). Third, our data allow us to test for heterogeneous effects of this set of laws across policy-relevant subgroups of students (e.g., economically disadvantaged) with substantially more precision than was possible in much of the prior literature. Finally, our paper adds to the few studies that are able to exploit administrative data sets that include exact birth dates of children to estimate impacts of school entry and compulsory schooling laws on various educational, social, and economic outcomes (i.e., Dobkin & Ferreira, 2010; Cook & Kang, 2013).

To preview results, we find that this set of laws in Michigan induces about 1.5 percent more students to graduate from high school. This effect is driven almost entirely by impacts on economically disadvantaged students (i.e., those eligible for free or reduced-price meals). Children eligible to start school relatively younger are slightly more likely to attend college than their older counterparts, but enroll in different types of institutions: Specifically, these students are 2 percentage points more likely to first attend a 2-year (rather than 4-year) college. In addition, we find evidence that students eligible to start kindergarten a year earlier also enroll in fewer semesters of college, within three years of expected on-time high school graduation.

The paper unfolds as follows: The next section summarizes the most salient extant literature and situates our contributions within this body of work. Section III describes the Michigan policy context in detail. In section IV, we describe our data and empirical approach. Section V presents our main findings. Section VI concludes with a discussion of the policy implications of our results.

II. Literature Review

Angrist and Krueger (1991) were the first to recognize the natural experiment created by the intersection of season of birth with school starting age and compulsory schooling laws. They concluded that about 25 percent of potential dropouts remained in school because of compulsory schooling laws. Since their seminal work, and adopting similar methodologies, many researchers have examined the impacts of school entry and compulsory schooling laws on educational, labor market, and socio-behavioral outcomes (Angrist & Krueger, 1992; Black et al., 2011; Cook & Kang, 2013; Dobkin & Ferreira, 2010).[1] On balance, the evidence suggests that students eligible to start school younger are more likely to graduate from high school and less likely to commit crime.[2] Yet, there seems to be little to no effect on long-run labor market outcomes, such as wages and employment.

One set of outcomes missing from this literature on long-run effects are richer measures of students' postsecondary decisions and experiences. While several prior studies examine impacts of these laws on college enrollment, to our knowledge, there is no exploration of impacts on college choice or persistence. In addition, recent policy changes across the United States stand to alter the context in which school entry and compulsory schooling laws operate. For example, in Michigan, the ACT became mandatory for 11th graders in 2007.[3] If one of the net effects of school entry and compulsory schooling laws is to increase the share of (younger) students

[1] Much of this literature has (by necessity) relied on self-reported outcome measures from the Current Population Survey (CPS), and Census data, as well as nationally representative surveys such as the National Longitudinal Surveys of Youth (NLSY) and the National Educational Longitudinal Survey (NELS; Angrist & Krueger, 1992; Dobkin & Ferreira, 2010; Elder & Lubotsky, 2009; Oreopoulos, 2007). Further, the data used in most of these analyses were representative of much earlier birth cohorts (i.e., mostly children born during the 1930s through the 1960s). We use data on much more recent birth cohorts (i.e., the 1990s) to explore how the impacts of school entry and compulsory schooling laws may have evolved over time.

[2] For girls, Black and colleagues (2011) find that starting school younger increases the likelihood of giving birth as a teenager.

[3] This policy change is not limited to Michigan. Since 2001, 11 states have implemented free and mandatory college entrance exams for high school juniors (Hyman, 2013).

persisting to 11th grade, we might expect effects on subsequent postsecondary outcomes to look different than in prior work. In such policy contexts, students induced to complete 11th grade are now also being compelled to take a test used for college entry.

Other scholars have examined the shorter-run academic achievement effects of starting kindergarten as either the relatively oldest or youngest student in a grade cohort. These studies consistently found that students who start younger have weaker short-run academic performance than their older peers. However, there is disagreement in the literature about how long this age-advantage lasts. For example, while Smith (2009) finds that older students consistently perform better on tests through grade ten, Elder and Lubotsky (2009) find that age-based differences in academic performance almost completely disappear by grade eight. We add to this line of inquiry by exploring the joint impact of school entry and compulsory schooling laws in Michigan on students' performance on the (mandatory) ACT in 11th grade.

Regardless of the duration of any advantages, parental perception that being relatively old for grade is academically (and athletically) beneficial for students has led to increases in the number of students starting school a year later than when they were eligible (i.e., "red-shirting"). This practice has been utilized more frequently with white boys from more economically advantaged households (Elder & Lubotsky, 2009), and least utilized by parents of black and Hispanic children, who are also often less financially well-off and may not be able to afford an additional year of childcare outside the public school system (Deming & Dynarski, 2008). Such variation in practices by gender, race, ethnicity, and income level underscores the need to better understand the impacts of school entry and compulsory schooling laws on these subgroups of students. We leverage our large, detailed, state-level administrative dataset to examine such subgroups effects with greater precision.

Two recent studies are the most similar to ours in terms of the research designs and data used to answer their research questions. Dobkin and Ferreira (2010) use restricted-access Decennial Census Long Form data for a sample of the population in California and Texas and find no evidence that school entry laws and the additional attainment that results from such policies leads to differences in employment rates or wages. Their null finding holds across student subgroups defined by age, gender, and race. Cook and Kang (2013) use student-level administrative data from North Carolina for students born in the late 1980s and early 1990s. They focus on outcomes related to crime and find that students born just after the entry cutoff date (i.e., relatively older students) are about 1 percentage point more likely to commit a crime between the ages of 17 and 19 than their younger counterparts.

Like Dobkin and Ferreira (2010), our primary interest lies in the net effect of school entry and compulsory schooling laws on long-run outcomes. We are particularly interested in how these laws affect students' decisions about college enrollment, choice, and persistence. Similar to Cook and Kang (2013), we exploit rich, student-level data with exact birth dates that enable us to track individual students over time and carefully examine measures leading up to these longer-run postsecondary outcomes that are common to prior literature: grade retention, educational attainment, and academic performance through the end of high school.

III. Policy Context

The state of Michigan provides fertile ground for addressing our research questions for several reasons. Until recently, Michigan had one of the latest school starting dates in the country: Students had to turn age five by December 1st of the kindergarten year. Twenty-six states require students to turn five by September 2nd or sooner, and 12 have start date cutoffs later in September or October (Bassok and Reardon, 2013). Accordingly, many more students in

Michigan have been eligible to begin kindergarten at age four than elsewhere. In fact, only two states (California and Connecticut) and the District of Columbia allow children to start school younger than does Michigan. The Michigan policy has meant that a child born on November 30 was still four when school began in September, while a child born just two days later on December 2 was a full year older, and closer to age six, when she began school the following year, providing a wide range of ages, and possible developmental differences within given kindergarten cohorts.

However, Michigan has recently changed its kindergarten entry policy. Beginning in the 2013-2014 school year, the statewide school entry age is set to roll back one month per year for three years, culminating in a school entry cutoff of turning age five by September 1st (at the beginning of the 2015-2016 school year). This change means that every student will have to be at least five by the first day of school.

Michigan is also in the process of changing its compulsory schooling laws. Beginning with the graduating class of 2016, students will not be allowed to drop out of high school until age 18, at least not without parental permission.

By investigating what has happened to students over time that began school under the December 1st school starting age policy, and who were eligible to dropout at age 16, we hope to provide insight into what outcomes Michigan may be able to expect as these new policy changes take effect.

IV. Data and Analytic Samples

For all analyses, we use individual student-level administrative data collected by the Michigan Department of Education (MDE) and the Center for Educational Performance and Information (CEPI), provided through the Michigan Consortium for Education Research

(MCER). These records contain detailed information on the characteristics and educational experiences of all students enrolled in public schools in Michigan, beginning in the 2002-2003 school year. Data include information on students' date of birth, city of birth, race, ethnicity, eligibility for free and reduced-price meals (FARM), and test scores. These data have also been matched to students' college enrollment records collected from the National Student Clearinghouse (NSC), allowing us to measure college-going for students through August of 2012. For each student that matches to the NSC's database (by name and birth date), a complete history of postsecondary enrollment experience is returned (Dynarski, Hemelt, & Hyman, 2013). [4]

In our analyses we use three samples of students: The first group is a set of birth cohorts that we can follow through at least three years of potential postsecondary experience. We refer to this group as the college sample. These students were born between June 4, 1989 and May 30, 1992. We then expand the number of birth cohorts in our analytic sample when we examine high school outcomes to those students born between June 4, 1989 and May 30, 1994. We refer to this group as our high school sample. Finally, to examine impacts on grade progression during elementary and middle school, we use students born between June 4, 1997 and May 30, 1999. We refer to this group as our K-8 sample. Our sample selection is driven by the desire to use data

[4] The earliest (on-time) high school graduates in our college sample could enter college is in the fall of 2007. Therefore, to ensure consistency in the operationalization of this key outcome across birth cohorts, we construct our college enrollment variable based solely on the stock of institutions that were reporting enrollment information to the NSC as of September 2007. In practice, this does not turn out to be a very binding restriction, as enrollment coverage in Michigan of the NSC data in 2007 is high overall (i.e., 84.4% for all institutions, public and private), and especially high among public 2-year and public 4-year institutions (i.e., 92% and 97%, respectively). Coverage only rises (across all types of colleges) in later years. See Dynarski, Hemelt, and Hyman (2013) for a comprehensive discussion of coverage rates of NSC data.

on the maximum number of students for each set of outcomes (college, high school, K-8).[5] Across all three samples, we exclude special education students.[6]

Although an ideal panel would allow us to follow the same students from kindergarten through college, our data span just about a decade. However, because these samples of students were all born within 10 years of each other, and during a time period in which Michigan did not change either its school entry policy or its dropout policy, we assume that the experiences of the younger cohorts of students we observe in primary grades is similar to the experience the older cohorts of students likely had in the primary grades, allowing us to reasonably use the findings from the K-8 cohort of students to inform our understanding of outcomes for the high school and college cohorts. In addition, we use the K-8 sample to explicitly test for differential rates of sample attrition. Finding no such evidence, we assuage concerns about beginning our high school and college analytic samples with students who we see somewhere in our Michigan data at ages 14 and 15.

Our main analyses focus on students in the high school and college samples, whom we first observe in 8th grade. We begin with 8th graders because this is the youngest group we can follow into higher education. The oldest of these students were eligible to begin 8th grade in 2002-2003, the first year for which we have data. The youngest students were eligible for 8th grade in 2005-2006.

One limitation with these two samples is that, even though we know students' birth dates and can calculate when they should have enrolled in kindergarten, we do not actually observe kindergarten entry. Thus, if a student is not on-time, we do not know if she entered school on-

[5] Our high school results are robust to estimations that use only the college sample. Results are available upon request from the authors.
[6] Specifically, within each sample, we exclude students who are educated in a special education classroom full time (and are therefore "ungraded"). We do not drop students with special education flags who appear in regular classrooms (with regular grade-level information).

time and was retained, or if she was red-shirted. We also do not know if she attended Michigan public schools prior to 8th grade. To at least control for the possibility that students did not start school in Michigan, but moved to the state from other places with different school entry policies, possibly inducing different levels of attainment prior to when we observe them, we restrict these samples to students who were born in Michigan. Although these students could have moved out of and back into Michigan between birth and 8th grade enrollment, this restriction reduces the likelihood that any results are driven by the mobility of students who enter the state later, and are likely different from those rooted in Michigan.

Table 1 presents descriptive statistics about the three analytic samples. Looking across these samples, it appears that Michigan did undergo some demographic changes between 1989 and 1999 (i.e., the range of birth years that encompasses our various cohorts). In particular, there was growth in the Hispanic population as a proportion of K-12 students, from approximately 2 percent to 6 percent, which in turn slightly shifted the overall racial composition of students across the state. In addition, the share of students eligible for free or reduced-price meals (FARM) is greater at the K-8 level (62 percent) than at the high school level (51 to 54 percent).

IV. Empirical Strategy

We use a regression discontinuity (RD) design to compare students on either side of the kindergarten enrollment cutoff, half of whom are eligible to start school a year prior to the other half. We construct the running variable such that the cutoff date of December 1st is equal to zero, and extend the variable 180 days in each direction. This means that the earlier birthdays are those between June 4th and December 1st, and the later birthdays are those that fall between December 2nd and May 30th. Students whose birthdays fall in the 5-day window between the tails of the running variable are dropped. In the first set of analyses, we define treatment as being eligible to start kindergarten at a relatively younger age, and refer to the group of students with birthdays before the enrollment cutoff as the treatment group, and the group of students with birthdays after the enrollment cutoff as the control group.

We first examine graphical evidence of any discontinuities around the birth date cutoff. We then progress to estimating parametric specifications of the following basic type:

$$Y_{ic} = \beta_0 + \beta_1 T_{ic} + \beta_2 f(R_{ic}) + \beta_3[T_{ic} * f(R_{ic})] + \beta_4 X_{ic} + \delta_c + \varepsilon_{ic} \quad (1)$$

where Y_{ic} is the outcome of interest (e.g., retained in K-8, graduate high school, enroll in college) for student *i* in birth cohort *c*. T_{ic} is a binary indicator equal to one if student *i* in birth cohort *c* was born on or before the cutoff date of December 1st, and $f(R_{ic})$ represents a flexible function of the distance between student *i*'s birthday and the cutoff date (in days). For example, R_{ic} is equal to zero for a student born on December 1st, 10 for a child born on December 11th, and -12 for a student born on November 18th. $T_{ic} * f(R_{ic})$ is an interaction between the flexible distance variable and the dummy indicating "treatment" (i.e., that the student was born before the cutoff date). This allows the relationship between birth date and the outcome to vary on either side of the cutoff value. X_{ic} is a vector of observable characteristics likely associated with the outcomes of interest, and includes information on students' gender, race and ethnicity, eligibility for free or reduced-price meals (FARM), and limited English proficiency status (LEP). Finally, δ_c is a vector of birth-cohort dummies and ε_{ic} is a stochastic error term. Within this parametric setup, β_1 gives the intent-to-treat (ITT) estimate of being eligible to start school a year earlier than the control group.

Since we find that a large portion of treatment students do not actually comply with the school enrollment policy as intended, we supplement these analyses with a fuzzy regression discontinuity design that allows us to better estimate effects on long-run outcomes for policy compliers. To do so, we use a two-stage least squares (2SLS) model in which the outcome in the first stage is an indicator equal to one if a student turns 13 by December 1st of her predicted, on-time 8th grade year. We then use the exogenous variation in the likelihood of remaining young for grade all the way through 8th grade (i.e., being fully exposed to the treatment) netted out of this first stage to estimate effects on our high school and college outcomes in the second stage. One way to think about these estimates is as an attempt to get at what the treatment effects of school entry and compulsory schooling laws working in tandem would be in Michigan if grade retention (and/or red-shirting) rates in earlier grades were not so large (i.e., as effects on compliers).

Students who comply with the school entry policy in kindergarten and remain on track through 8th grade must have turned 5 by December 1st of kindergarten, and would subsequently turn 13 by the same date in 8th grade. Thus, our two-stage model is as follows:

$$D_{ic} = \alpha_0 + \alpha_1 T_{ic} + \alpha_2 f(R_{ic}) + \alpha_3[T_{ic} * f(R_{ic})] + \alpha_4 X_{ic} + \delta_c + \omega_{ic} \quad (2)$$

$$Y_{ic} = \beta_0 + \beta_1 \widehat{D}_{ic} + \beta_2 f(R_{ic}) + \beta_3[T_{ic} * f(R_{ic})] + \beta_4 X_{ic} + \delta_c + \varepsilon_{ic} \quad (3)$$

where, in the first-stage, the outcome D_{ic} is a dummy variable equal to one if student *i* in cohort *c* is 13 by December 1 of their 8th grade year. We use the random allocation of students to either side of the school entry cutoff date by birth to extract the exogenous variation in remaining on-time through grade 8 (i.e., before the legal age of dropout occurs for any students in our samples). We then use this variation to estimate the effects of this set of laws for students who "fully complied" (i.e., students who progressed on-time up to 8th grade). In each equation, we include a vector of student-level covariates (X_{ic}), cohort fixed effects (i.e., δ_c, dummy variables that associate students with a particular yearly running variable). Finally, ω_{ic} and ε_{ic} are idiosyncratic error terms. In all models, we cluster the standard errors by exact birth date.

We begin with graphical analyses of all our main outcomes. We then progress to estimating both the parametric models specified above as well their non-parametric analogues. We see very similar results across parametric and non-parametric analyses, likely due to the quite linear relationship between our running variable and outcomes in the neighborhood of the discontinuity. Indeed, in RD work more broadly, estimates that use all the data and control for higher-order polynomials of the running variable tend to be quite similar to estimates that rely on a narrow band of data around the cutoff and control linearly for the running variable (Angrist & Pischke, 2009). For our parametric results, we present results from several windows of data, zeroing in on the smallest window for which we have adequate power to detect effects (i.e., within 30 days of the cutoff). This allows the reader to see and judge tradeoffs between precision and potential bias across a range of estimates. Other work in this area has used date ranges between 50 and 70 days, on either side of the birth date cutoff (Cook & Kang, 2013; McCrary & Royer, 2006). Our relatively large sample sizes allow us to use ranges of 30 and 45 days. We consider average impacts and explore potential heterogeneity in effects by student gender and eligibility for free or reduced-price meals (FARM).

V. Results

Although our primary interest lies in the high school and higher education outcomes, we begin by graphically examining a few particular outcomes based on the K-8 sample. We do so for two reasons: First, we can observe kindergarten entry and more clearly depict the identifying variation we use across all samples. Second, we test assumptions about sample attrition through grade 8 (i.e., when our high school and college samples begin).

Figure 1 illustrates that about 80 percent of students born just before the cutoff date start kindergarten a year younger than their counterparts born just after the cutoff date. This implies that the rate of noncompliance with kindergarten entry (or "red-shirting") in Michigan is about 20 percent for students born just before the cutoff.[7] It is the variation in age at kindergarten entry that later intersects with compulsory schooling laws and would compel an on-time student to remain in school longer than her older counterparts. The validity of the RD approach rests on the assumption that there is smoothness in the running variable through the cutoff determining treatment. In Figure 2, we illustrate that there is no manipulation or heaping of births to one side or the other of the school entry cutoff date by plotting the density of our running variable as recommended by (McCrary, 2008).[8] Finally, in Figure 3, we calculate the share of students that leave our K-8 sample (regardless of whether they later return to the Michigan data). If families of younger students were moving out of Michigan at differential rates, we might be worried about our choice to begin the high school and college samples with students we observe around age 14 in our data. But, we find no evidence of differential sample attrition for students eligible to begin school at a younger age.

[7] This is the same rate of noncompliance documented by Dobkin and Ferreira (2010) in California and Texas (p. 48).
[8] We use the "DCdensity" command developed by McCrary (2008), and find no statistically significant difference in the density of births at the cutoff.

A. Effects on High School Outcomes

We now examine effects of school entry eligibility on our high school attainment outcomes. Figure 4 presents the graphical depiction of these results while Table 2 reports the corresponding parametric estimates. On average, we find that students eligible to begin school a year younger are 1.6 percentage points more likely to enroll in 11th grade, about 1 percentage point more likely to enroll in 12th grade, and 1.5 percentage points more likely to graduate from high school than their older counterparts.[9] When we non-parametrically estimate the impacts on these same outcomes,[10] we obtain nearly identical point estimates – all of which are statistically significant at the 10 percent level or lower. This set of estimates represents the ITT effects of school entry and compulsory schooling laws on educational attainment through the end of high school. These effects are substantially muted by increased grade retention among relatively younger students because students who repeat a grade during elementary or middle school will no longer confront the legal age of dropout later than their original older counterparts.

In panel B of Table 2 we present estimates from our two-stage, fuzzy RD approach (2SLS) for this same set of high school outcomes. From the first-stage results, we see that being eligible to start kindergarten at a relatively younger age is associated with a 33 percentage point increase in the likelihood of remaining young for grade up until grade eight. This finding allows us to scale up our estimates of the impact of school entry and compulsory schooling laws for compliers: Indeed, among those who "fully comply," the compulsory schooling law increases the

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[9] These effects are slightly larger than those of Dobkin and Ferreira (2010): In Texas and California, they find effects on high school graduation of 0.8 and 0.9 percentage points, respectively (p. 43). Yet, our results are much closer to those of Dobkin and Ferreira (2010) than to Cook and Kang's (2013) finding of a 4 percentage point difference in rates of completing 12th grade in North Carolina (p. 26).

[10] We use the "rd" command developed by (Nichols, 2012). This command estimates local linear regressions of degree zero using a triangular kernel. In other words, these regressions estimate smoothed, weighted means of the outcome variable within a specified bandwidth (i.e., 30 days) – giving greater weight to observations close to the cutoff. In order to examine the statistical significance of any estimated gaps, we obtain standard errors by bootstrapping this process; re-sampling observations, with replacement, from the relevant sample 100 times.

likelihood of high school graduation by about 5 percentage points (relative to an outcome mean of 74 percent). We interpret this as evidence that the effect of school entry and compulsory schooling laws working in tandem would be up to three times as large if all students to the left of the school entry date cutoff were induced to begin school relatively younger and remain on-track as young-for-grade individuals through the point at which the compulsory schooling law would begin to bind (i.e., before age 16).

In addition to attainment, we can explore impacts of this set of laws on academic performance in high school. The ACT became mandatory for all 11th grade students in Michigan in 2007. Therefore, all but one of our birth cohorts were required to take this test in their penultimate year of high school. Table 3 presents results from ITT models identical to those above (but where the outcome is now composite ACT score rather than an attainment measure) and confirm that students eligible to begin school a year younger score about 0.4 points (i.e., 0.08 standard deviations) lower on the ACT composite (relative to their older counterparts).[11] This effect approaches 2 points for students who "fully complied" with the treatment (i.e., 0.39 standard deviations). These achievement results align with (Smith, 2009) and suggest that the underperformance of relatively younger students does persist beyond middle school into high school.[12]

B. Effects on College Outcomes

We next turn to effects of this same set of laws on measures of postsecondary attendance, choice, and persistence. Table 4 presents the ITT (panel A) and 2SLS (panel B) results for this set of outcomes. The 2SLS effects are generally two to three times larger than the analogous ITT

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[11] We obtain nearly identical results if we exclude the birth cohort of students for whom the ACT was not mandatory at the time they were in 11th grade (i.e., those born in 1989-1990) from our sample.

[12] Specifically, these achievement effects are at the lower end of the range of effects on 10th grade performance found by Smith (2009): By grade 10, he finds that older students' academic performance advantage is about 0.10σ in numeracy, 0.11σ in reading, and 0.19σ in writing (p. 13).

estimates. Figure 5 presents the corresponding graphs. We see evidence of a small positive (but statistically insignificant) effect on overall college-going of about 1 percentage point. Any small effect on overall college attendance is driven by increases in the likelihood that students first attend a 2-year (rather than 4-year) college. Impacts on outcomes that measure whether a student initially enrolls in a 2-year or 4-year college also suggest that school entry and compulsory schooling laws affect college choice. Specifically, students eligible to start school at a younger age are more likely to first attend a 2-year college and less likely to first attend a 4-year college.[13]

When we non-parametrically estimate the impact of school entry eligibility on this set of college outcomes,[14] we get slightly larger point estimates. For example, using a non-parametric approach, we estimate that students eligible to start school at a relatively younger age are 2.9 percentage points more likely to attend college (of any type) within three years of expected on-time high school graduation ($p < 0.05$) – and that this is almost entirely driven by an increased propensity to first attend a 2-year college.

Finally, we examine the impact of these laws on the total number of college semesters in which a student enrolls during the three years after her expected, on-time high school graduation. We estimate that students eligible to begin school at a relatively younger age enroll in about 0.4 fewer semesters of college (of any intensity) than their older counterparts.[15] This is an unconditional estimate (that is, those who did not attend college receive a value of zero semesters).

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[13] In addition, these students are no more likely than their older counterparts to ever attend a 4-year. So, the impact on the outcomes measuring whether a student first attends a 2-year college is not simply a proxy for later enrollment in a 4-year college. Results for the “ever 4-year college” outcome are omitted for parsimony and available from authors upon request.

[14] We conduct these non-parametric analyses using a bandwidth of 30 days and a triangular kernel.

[15] Our non-parametric ITT estimate of 0.25 fewer semesters ($p<0.01$) is similar to the parametric estimate.

We conclude that school entry and compulsory schooling laws have a small effect on college enrollment (driven by an increased propensity to attend 2-year institutions), and that this set of laws shifts the choices of relatively younger college-going students toward 2-year rather than 4-year colleges (relative to their older college-going peers). We also find this set of laws to foster lower levels of postsecondary persistence among relatively younger students. Perhaps the relative underperformance of younger students in high school accounts for some of these postsecondary effects. That is, such students are induced to remain in high school longer, underperform on (mandatory) college entrance exams, and enter relatively weaker (i.e., 2-year) postsecondary institutions. At least, this is one analytic story that is consistent with the joint results across our high school and college outcomes and with the Michigan policy context (i.e., the relatively recent adoption of a mandatory, free college entrance exam in 11^{th} grade).

As a test of the validity of our RD design, we use our student-level covariates (i.e., female, white, black, Hispanic, FARM, LEP) as outcomes and present graphical evidence in Figure 6 on the smoothness of these covariates through the cutoff of interest. In no case do we estimate a statistically significant discontinuity in any of these student-level covariates at the cutoff. In addition, we see this same pattern of null results for our other samples (i.e., the high school and K-8 samples).

C. Heterogeneity in Impacts by Gender and Economic Advantage

Given past evidence on the differences in impacts of school entry and compulsory schooling laws by gender and socio-economic advantage (e.g., Cook & Kang, 2013), we next explore heterogeneous effects on our high school and college outcomes by gender and eligibility for free or reduced-price meals (FARM). Table 5 presents these results.

The results in Table 5 are quite rich and provide considerable nuance to our basic high school and postsecondary findings. We focus mostly on the ITT effects in the discussion that follows. Relatively younger students across these four subgroups underperform relative to their older counterparts – but the magnitude of that underperformance is greater for boys and FARM students. While boys and girls see similar impacts on high school educational attainment of the offer to enter school a year earlier than their slightly older same-gender counterparts, we see substantial differences in the effect of this offer by FARM eligibility. Specifically, this set of laws induces about 3 percent more FARM students who are relatively young for grade to graduate from high school, compared to older FARM students (whereas the impact among non-FARM students is essentially zero). We therefore conclude that the overall positive impacts on educational attainment through the end of high school are being almost entirely driven by impacts on socio-economically disadvantaged students (of both genders).

When we look to heterogeneous effects of school entry and compulsory schooling laws along the college-going margin, we see that such laws appear to increase college-going among female and non-FARM students, by 2.7 and 3.1 percentage points respectively – and that these increases are primarily absorbed by increases within the 2-year sector. In contrast, we see little impact of these laws on the overall college enrollment rates of male and FARM students, but clearer impacts on the college choices of such students. Specifically, males and FARM students are 2.5 and 1.2 percentage points *less* likely to first attend a 4-year college (relative to a 2-year college). These heterogeneous effects allow us to see which groups of students account for the mix of college (attendance and choice) effects we saw across the full sample of students: We find increases in college-going among females and non-FARM students, where these results are primarily driven by an increased propensity to enroll in 2-year colleges. However, for collegeenrollees in all subgroups except females, we find greater proportions of younger students choosing 2-year rather than 4-year colleges. Taken together, these effects illustrate important differences in the impacts of school entry and compulsory schooling laws on the educational performance and attainment of students of different types.

D. Effects on Progression through Elementary and Middle School

Thus far, our analysis provides important new information about the net, long-range impacts of school starting age and compulsory schooling laws. Yet, our high school and college analytic samples begin with students we can observe in 8^{th} grade (at a variety of ages). We know from prior literature that school starting age laws also impact performance and progression in elementary and middle school. These impacts necessarily shape later outcomes as students age into the time period where compulsory schooling laws begin to bind. To better understand this phenomenon, we examine grade progression patterns for students in our K-8 sample who attended school in the same policy context, just a few years later.

Figure 3 confirmed that leaving the Michigan K-8 data is unrelated to the treatment (i.e., the offer to start school a year earlier than one's peers). Therefore, we drop students who leave our data from the K-8 analytic sample for this grade progression analysis. Figure 7 examines the relationship between grade repetition and the age at which children are eligible to start school. We see a large discontinuity in the likelihood of ever repeating a grade during the K-8 years at the cutoff. Specifically, students eligible to start school at a relatively younger age are between 35 and 40 percentage points more likely to repeat an elementary or middle school grade.

To explore the evolution of this overall impact, we estimate our ITT model using the sample that includes students born within 30 days of either side of the school entry cutoff date on outcomes that measure whether a student is on-time in each grade (K-8). Figure 8 presents these grade-specific estimates. An interesting pattern emerges – wherein the vast majority of the "ever repeat" effect is driven by an increased likelihood of repeating kindergarten or first grade. Specifically, treatment students (those born before the cutoff) are 15 percentage points less likely to enter kindergarten on-time, and by first grade this effect jumps to nearly 40 percentage points. By third and fourth grade, these students are close to 42 percentage points less likely to be on-time for grade, as predicted by their actual birth date.[16] Note that there is little change in the estimates between being on-time in grade 3 and being on-time in grade 4, indicating that virtually all of the grade retention occurs between kindergarten and grade 3. These results make clear that our ITT impacts on high school and college outcomes are indeed muted by the high rate of K-8 grade repetition among our "treatment" (i.e., relatively younger) students in Michigan. Our 2SLS impact estimates for the high school and postsecondary outcomes illustrate how large such effects could be were it not for such large early grade repetition (and/or red-shirting).

We consider the average rate of parental red-shirting to be the impact estimate for the on-time kindergarten outcome. These results reflect a given student's first year in kindergarten and so we assume that if they do not start kindergarten on-time, based on their birth date, that the decision was likely made solely or mostly by parents, prior to student involvement in the public school system. Although it is possible that some fraction of these students were held back because of discussion with school officials, it is nonetheless important to note that, at 15 percent, this average rate of kindergarten red-shirting is much higher than the 5 percent average found nationally (Bassok & Reardon, 2013). Although our general findings are in line with other

[16] Cook and Kang's (2013) finding that a relatively younger student is 6 percentage points more likely to repeat a grade between the ages of 11 an 15 (p. 18) is quite in line with our finding that the largest impacts on grade retention occur in early elementary school grades (i.e., kindergarten through 3rd grade), with small marginal increases to this likelihood in upper elementary school years and beyond. The magnitude of our early grade repetition findings is much larger than estimates from international contexts: For example, using data from Chile, McEwan and Shapiro (2008) find that a one-year delay in starting primary school decreases the probability of repeating first grade by 2 percentage points.

research, we speculate that the magnitude of these figures may be specific to Michigan because, at December 1, Michigan's school entry cut date is one of the latest in the nation. If the high rates of red-shirting and early grade retention we see here are due to absolute age and developmental maturity of the youngest students, we expect that, in moving the school entry date back to September 1, Michigan will likely see red-shirting and grade retention rates in the early grades fall in the coming years, and become more closely aligned with national averages.

VI. Conclusions and Policy Implications

In this paper, we exploit the discontinuity in expected legal exposure to K-12 schooling created by school entry date and students' exact birth dates to investigate the joint impact of school entry and compulsory schooling laws on a variety of educational outcomes along the K-20 pipeline. Using rich administrative data on all public school students in Michigan, we investigate the role of being offered kindergarten entry eligibility a year younger on high school performance, attainment, and college enrollment, choice, and persistence. To contextualize these findings, we also explore on-time grade progression from kindergarten through the 8th grade for one sample of students. Overall, our results provide more nuanced answers to policy questions about school starting age and compulsory schooling laws in several ways.

Similar to previous studies in this literature (e.g., Cook & Kang, 2013; Dobkin & Ferreira, 2010), we find that students eligible to start school a year earlier (i.e., at a relatively younger age) persist at greater rates into grades 11 and 12, and graduate at higher rates; yet, these same students demonstrate lower academic performance while in high school. Along the postsecondary margin, we find small to no average effect on college enrollment. However, we do find that relatively younger females and those who are younger but from more advantaged backgrounds are more likely to enroll in college. These effects are primarily driven by these same two subgroups of students enrolling in 2-year colleges. We also find this set of laws to foster lower levels of postsecondary persistence among students eligible to start school a year earlier than their slightly older counterparts. This finding is consistent with increased enrollment at 2-year colleges, where degree and certificate programs are by definition shorter than programs at 4-year colleges. While speculative, it is plausible that the relative underperformance of younger students in high school accounts for some of these postsecondary effects. Similar to Dobkin & Ferreira (2010), who argue that the net impact of lower academic performance and higher attainment explains null effects on adult earnings (p. 13), we argue that an additional net impact of these laws is a shift toward (and increased reliance on) potentially lower-quality postsecondary institutions and lower overall postsecondary persistence.

Yet, the increase in high school graduation generated by these laws is a net positive. The fact that this increase in high school graduation is concentrated among low-income, traditionally disadvantaged students suggests that compulsory schooling policies are a potentially manipulable policy lever that can be expected to affect students most at-risk for dropout. Moreover, the fact that some subgroups of relatively younger students are enrolling in college at higher rates than their older counterparts (e.g., females) suggests that inducing some students to remain in high school longer may also propel them to acquire some postsecondary education (that they might not obtained had they been able to drop out of school sooner).

Finally, we present evidence that average high school and college effects of school entry and compulsory schooling laws are muted by high rates of early grade retention for students in primary grades. Such impacts place students who are predicted to be the youngest in their cohort onto actual grade progression trajectories that mirror those of students born just to the right of the cutoff (i.e., those relatively old for grade). Accordingly, our 2SLS results can serve as upper-bounds on magnitudes of the high school and postsecondary impacts of this set of laws.

Our findings may help predict what Michigan can expect as a result of the dual changes the state has recently made to their school starting age and compulsory schooling laws. By moving the school start date back such that all students will have to be 5 years old on the first day of school, rates of red-shirting and early grade retention may fall, bringing those figures closer to national averages – particularly if these two mechanisms for placing students off-track are driven by students' absolute maturity, rather than relative-age position. If this occurs, then the state can reasonably expect that more students will comply with grade progression, which would likely raise the magnitude of the joint impact of school entry and compulsory schooling laws toward the size of our 2SLS estimates.

Impacts of moving the legal age of dropout from 16 to 18 are harder to predict, based on these findings. On the one hand, given that students who reach the dropout age later in their schooling careers are more likely to graduate high school, it seems plausible that moving the dropout age to 18 will increase high school graduation rates. Additionally, the fact that these increases are driven by impacts on low-income students suggests that as Michigan increases its legal age of dropout, low-income students may be the greatest beneficiaries of the change. However, some prior descriptive evidence suggests that changing the dropout margin from less than 18 to 18 has not impacted high school graduation rates in states that have made such a change (Whitehurst & Whitfield, 2012). Yet, other recent work exploiting changes in states' dropout ages over time provides cautiously optimistic evidence that more restrictive compulsory schooling laws may further reduce high school dropout and promote later employment (Oreopoulos, 2013). This line of research suggests one important avenue for future research.

Another important avenue is to explore impacts of this set of laws on what students compelled to remain in high school a bit longer than their slightly older peers do with their time. Of particular interest are any high school mechanisms that may be helping some groups of students reach farther toward attaining some postsecondary education, while failing to address issues faced by other groups, particularly males and economically disadvantaged students. Such results may help to further elucidate the mediators that shape the long-run joint impacts of school entry and compulsory schooling laws on postsecondary and labor market outcomes for various types of students.

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Figure 1. Age at Start of Kindergarten: K-8 sample

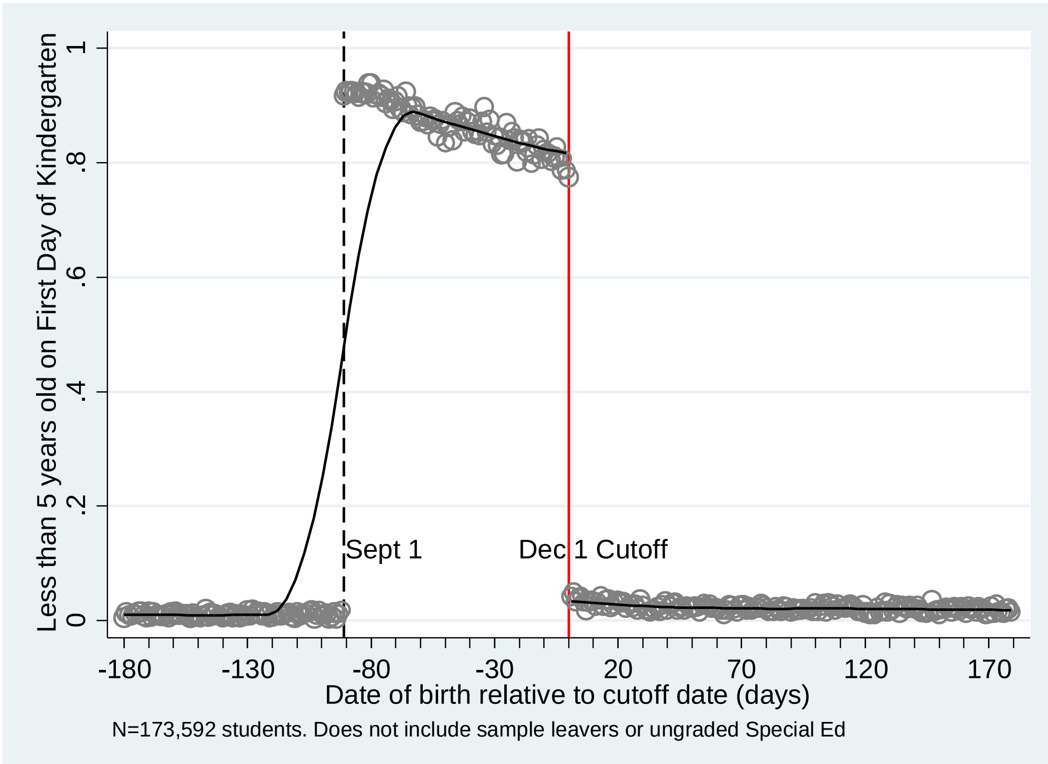
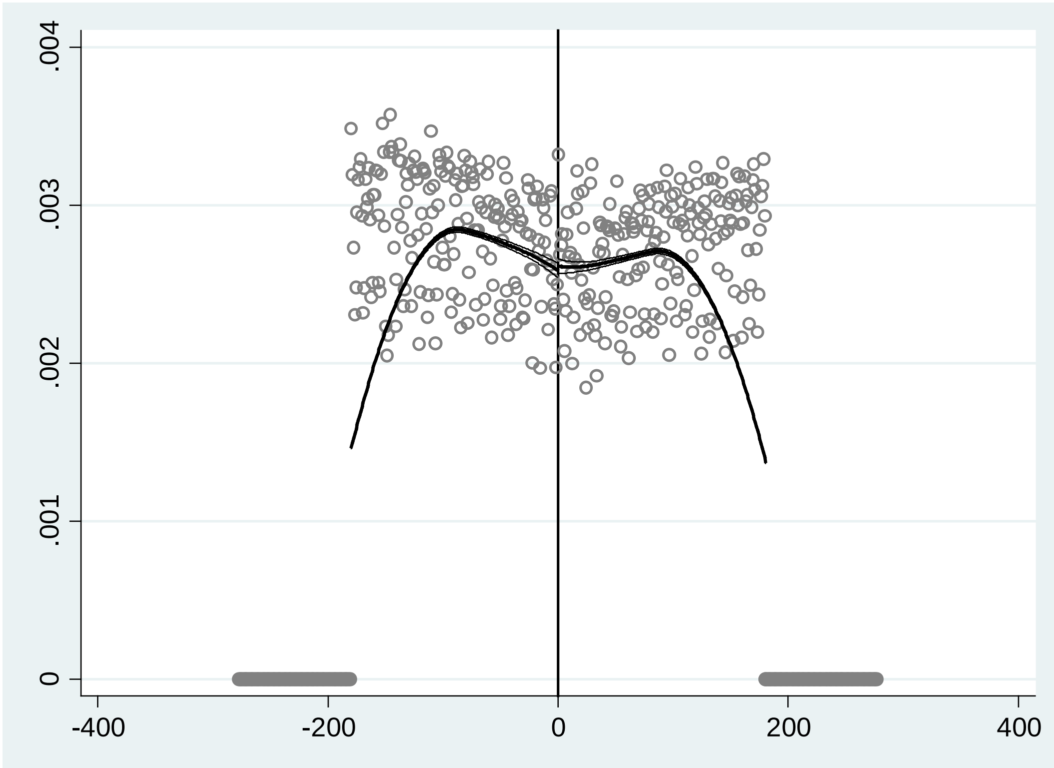
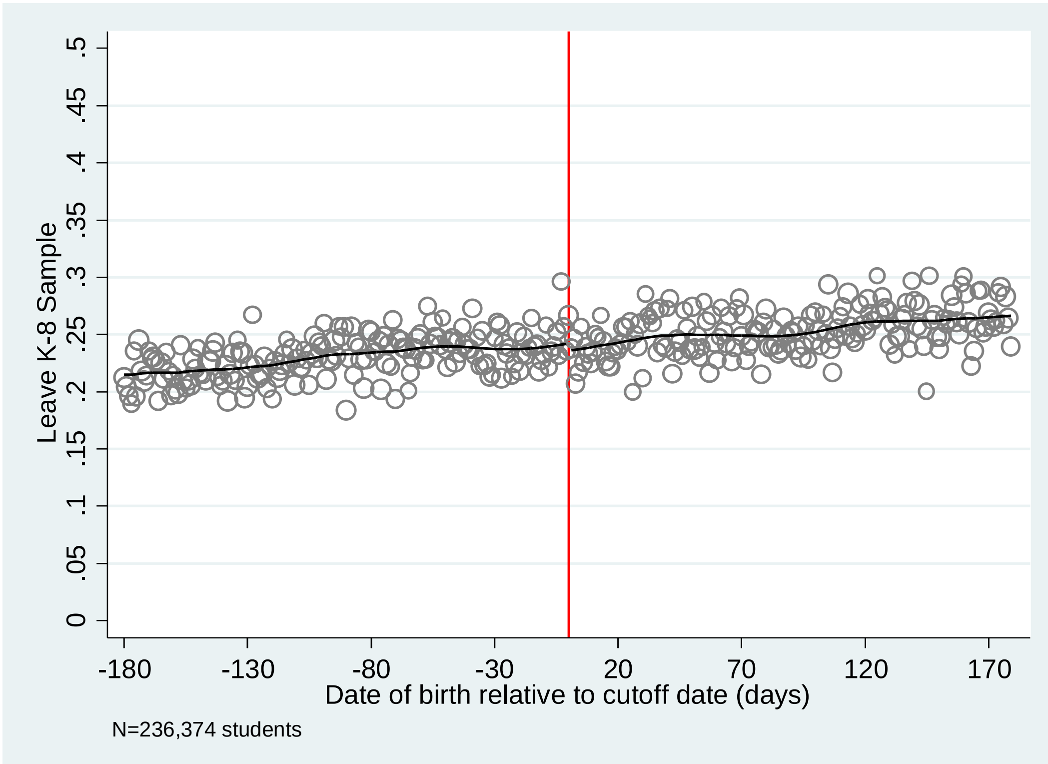


Figure 2. Density of Births by Days from School Entry Cutoff



Notes: Data are from the K-8 sample.

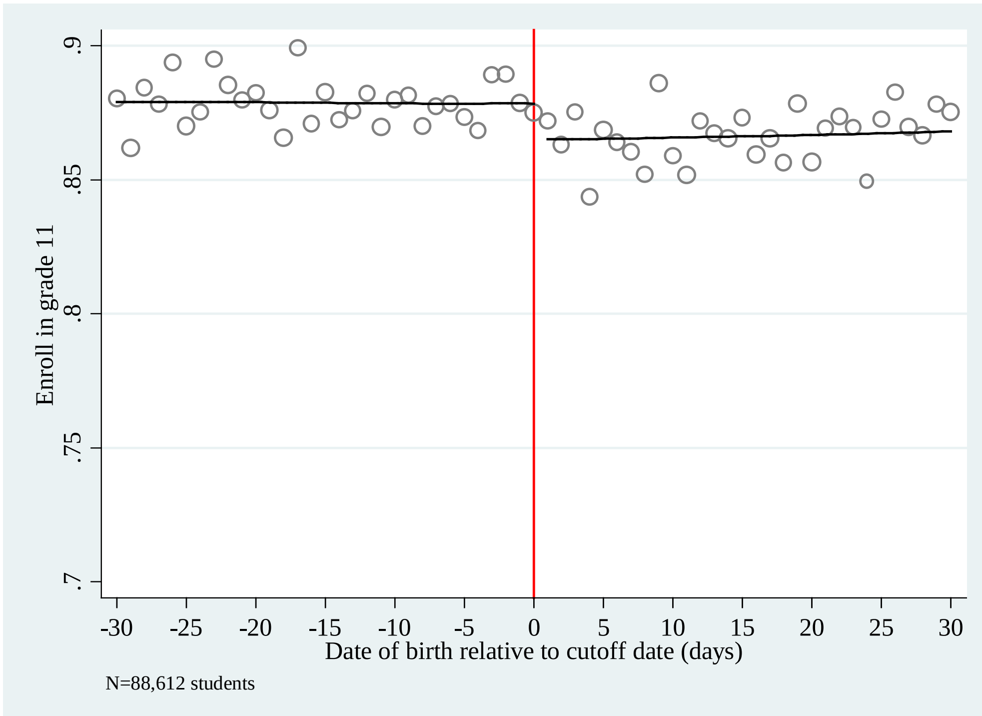
Figure 3. Sample Attrition: K-8 sample



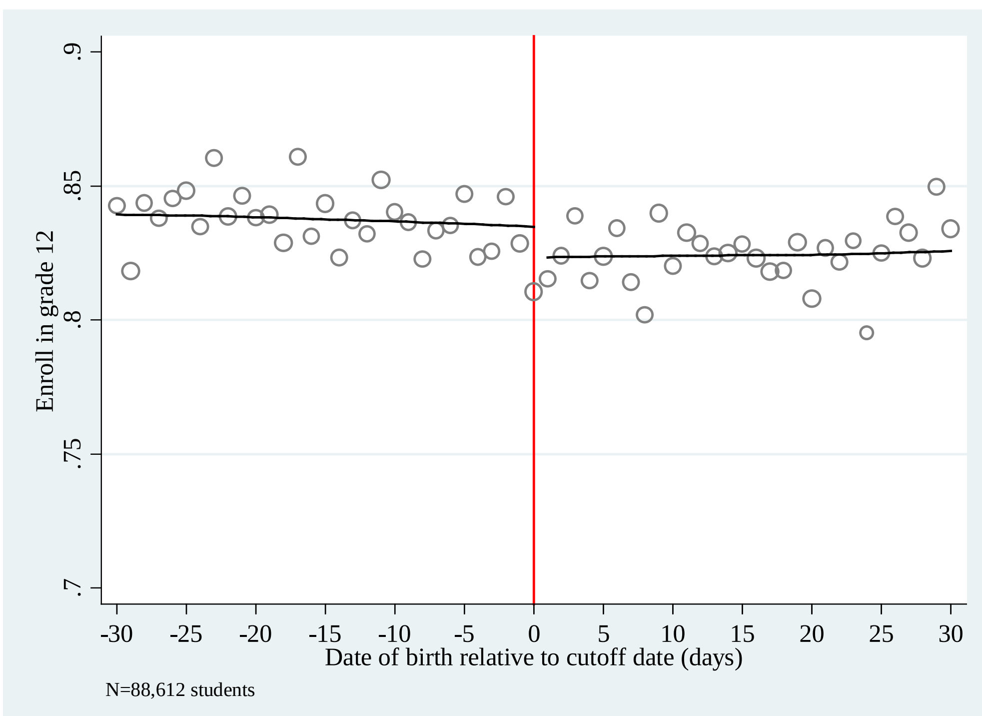
Notes: A student is coded as leaving the sample regardless of whether or not she reappears in later years.

Figure 4. Effects of School Entry Eligibility on Educational Attainment in Late High School

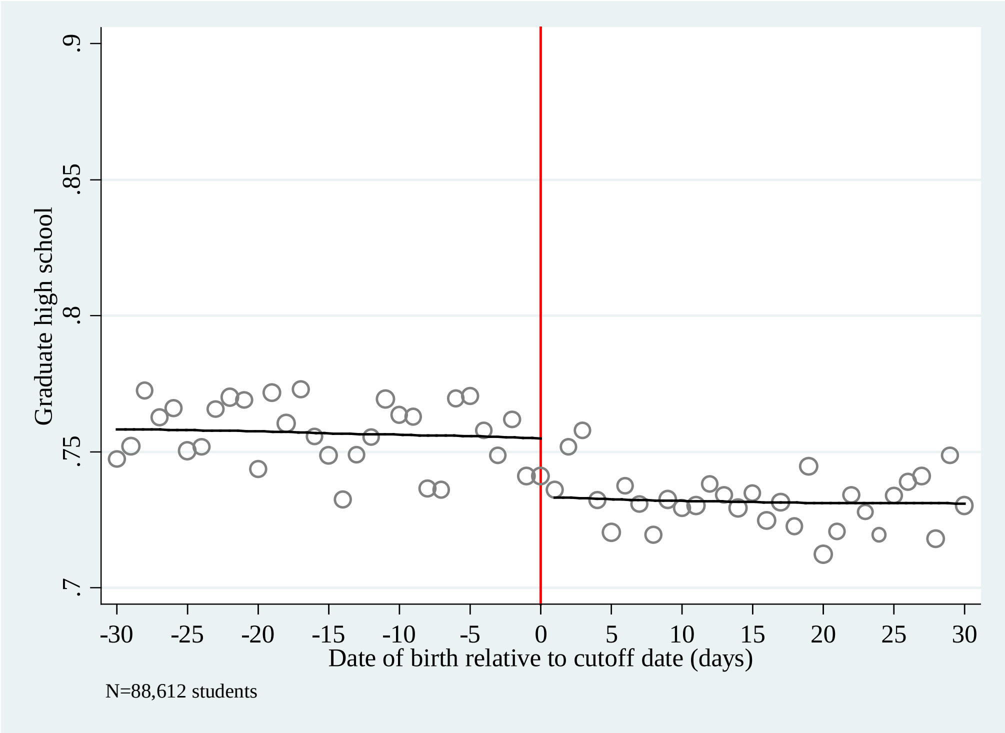
A. Enroll in 11th Grade



B. Enroll in 12th Grade



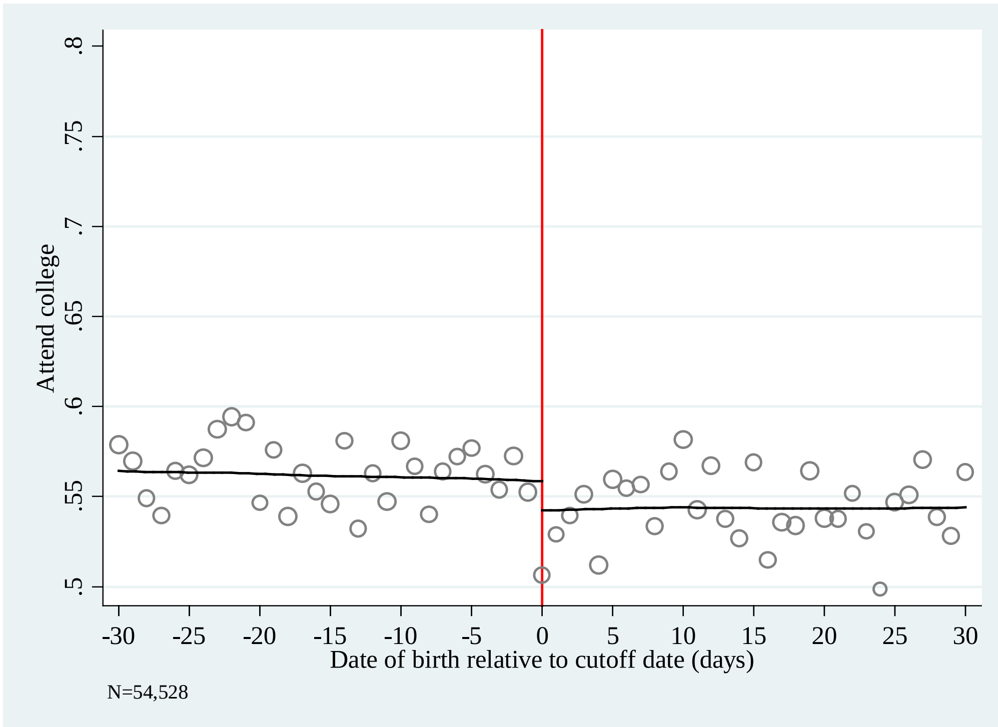
C. Graduate from High School



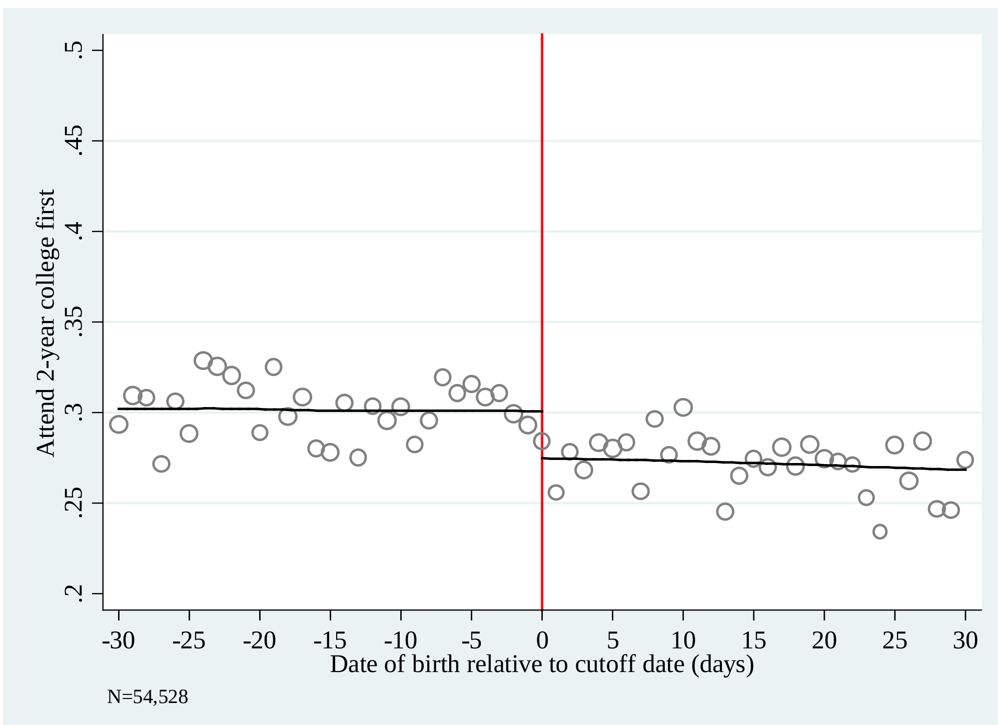
Notes: All figures use the high school sample. Weighted means are plotted for each day of birth. The weight is the number of children born on each day. A local polynomial regression of degree zero fits a line on each side of the cutoff using a triangular kernel.

Figure 5. Effects of School Entry Eligibility on College Attendance, Choice, and Persistence

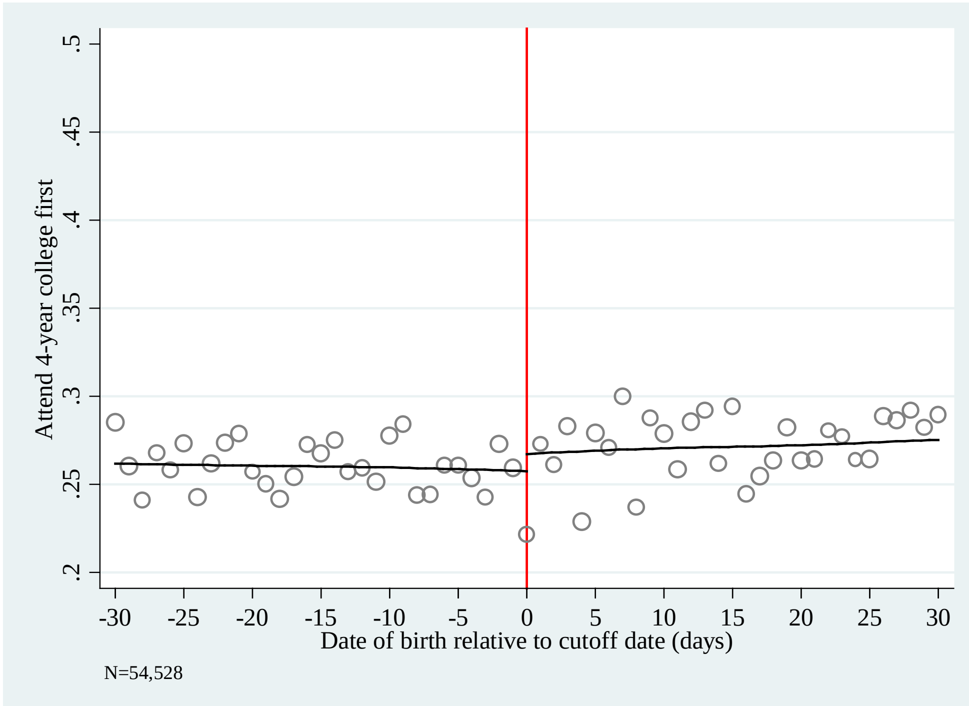
A. Attend college (within 3 years of expected on-time high school graduation)



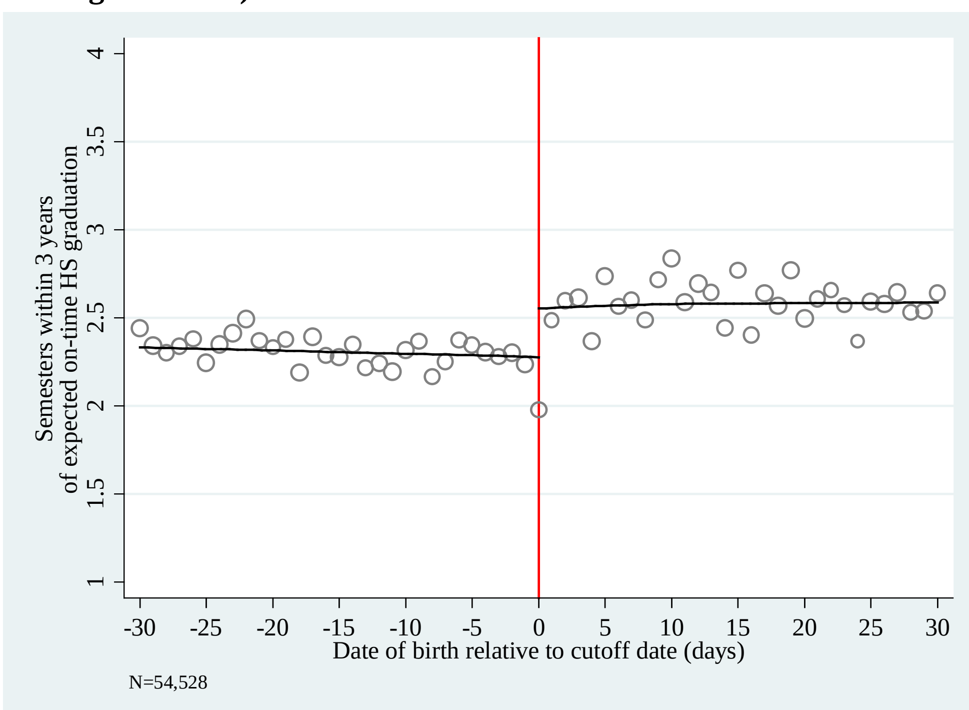
B. First attend a 2-year college



C. First attend a 4-year college



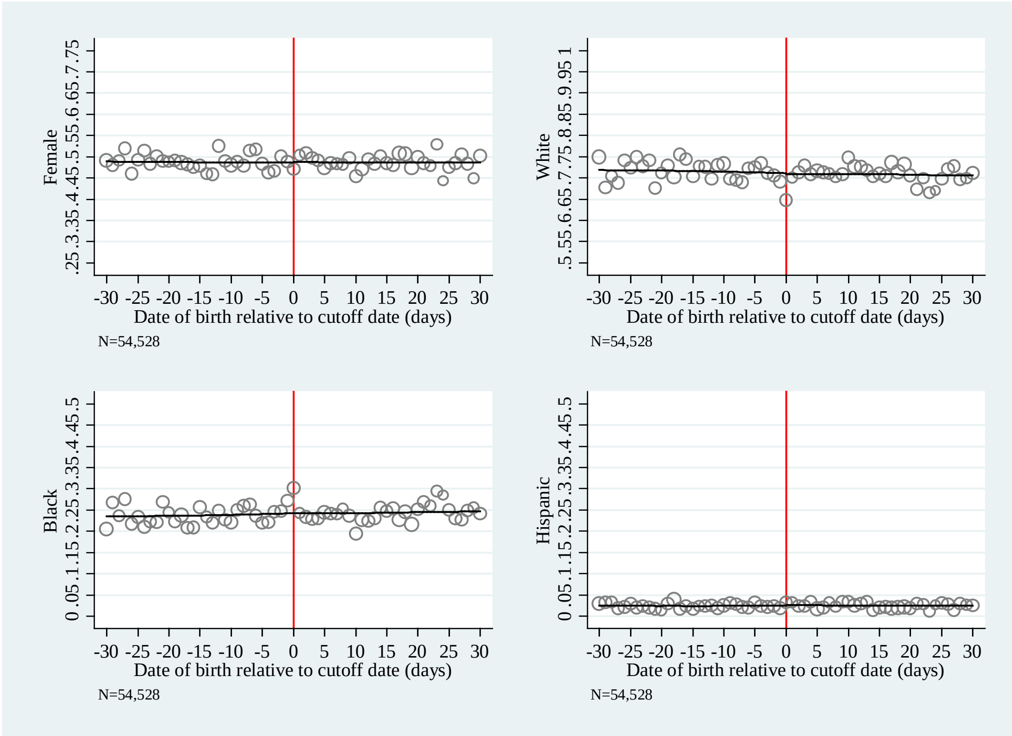
D. Number of semesters enrolled in college (within 3 years of expected on-time high school graduation)



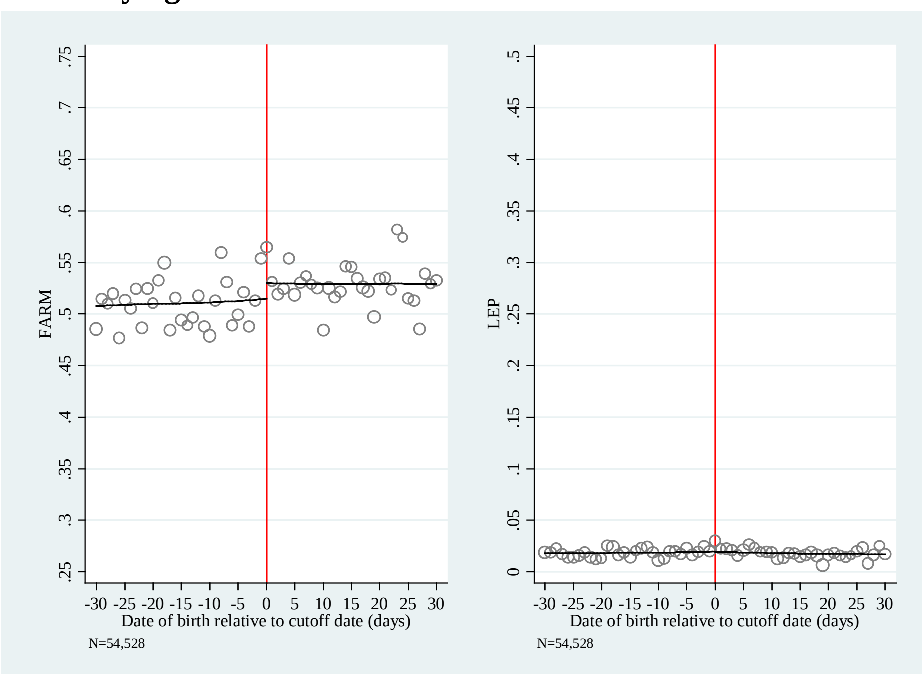
Notes: All figures use the college sample. Weighted means are plotted for each day of birth. The weight is the number of children born on each day. A local polynomial regression of degree zero fits a line on each side of the cutoff using a triangular kernel.

Figure 6. Balance of Covariates at Cutoff: College sample

A. Fixed Student Characteristics



B. Time-varying Student Characteristics



Notes: All graphs based on the college sample. Weighted means are plotted for each day of birth. The weight is the number of children born on each day. A local polynomial regression of degree zero fits a line on each side of the cutoff using a triangular kernel. The y-axes for the fixed student characteristics measure whether a student was ever identified as the outcome within our data.

Figure 7. Effect of School Entry Eligibility on Grade Repetition: K-8 sample

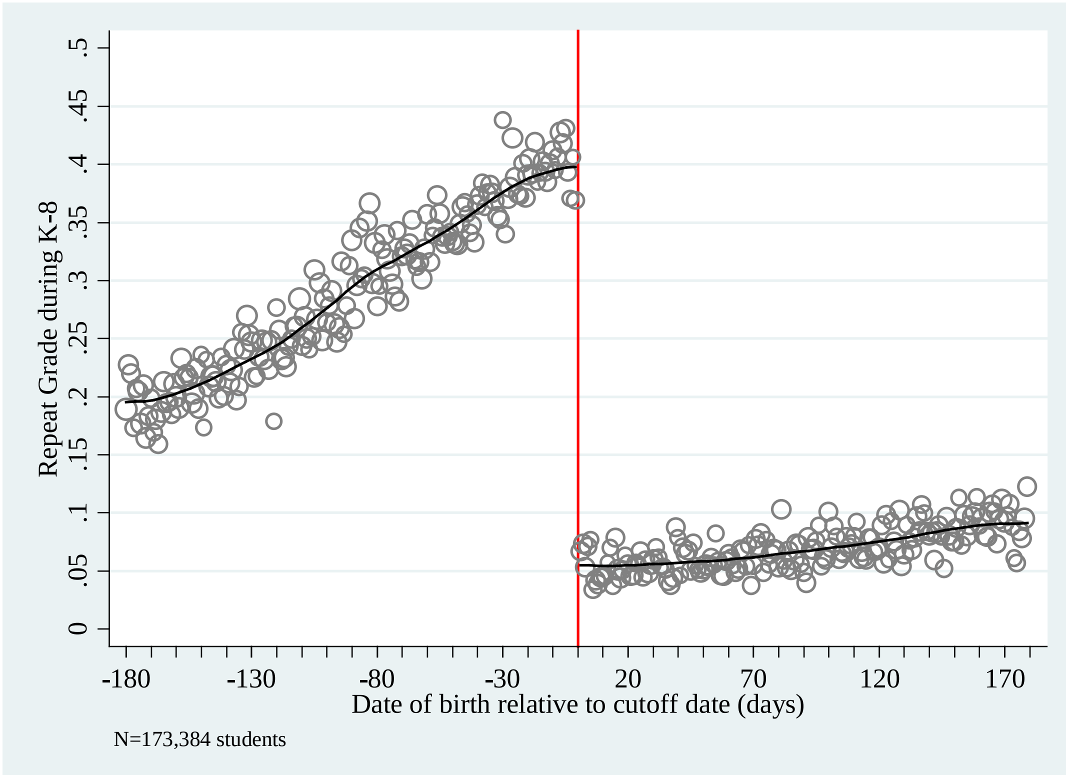
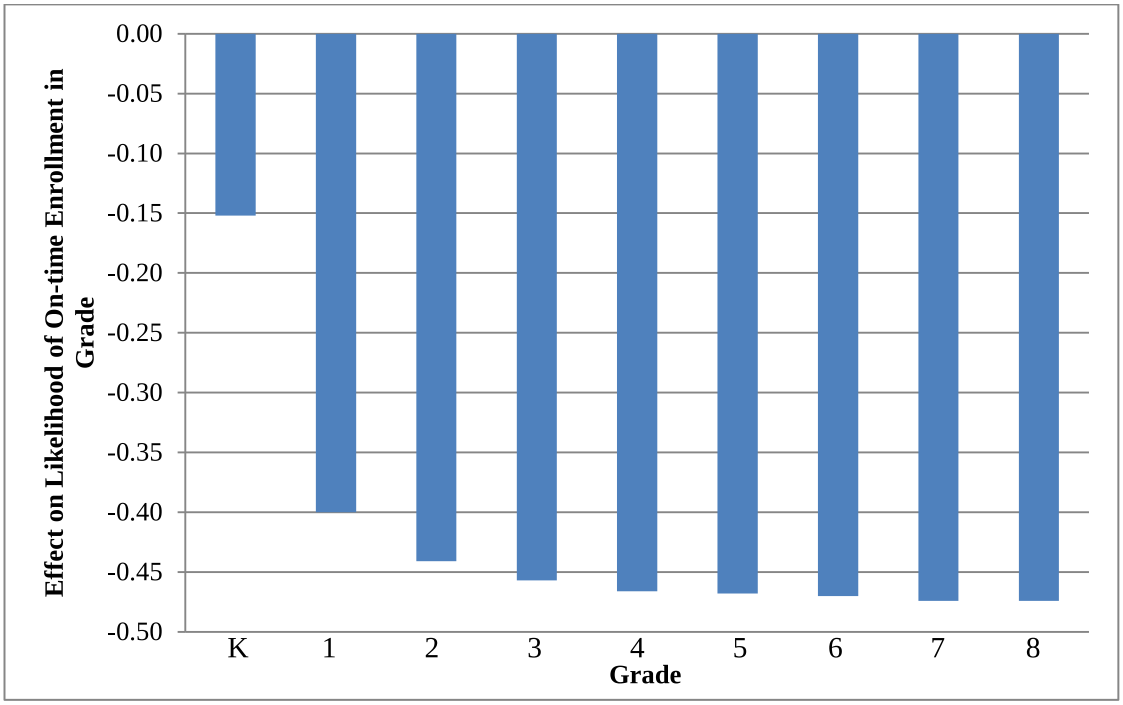


Figure 8. Effects of School Entry Eligibility on Timely Progression through Elementary and Middle School: K-8 sample



Notes: Sample excludes students who leave the sample and special education students; N = 28,049. Each bar represents a separate effect on grade-specific on-time enrollment.

Table 1. Descriptive Statistics by Sample

A. Postsecondary Sample (Born 1989 - 1992)

Data window	+/- 120 days		+/-45 days		+/- 30 days	
Variable	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Female	0.49	0.50	0.49	0.50	0.49	0.50
White	0.72	0.45	0.71	0.45	0.71	0.45
Black	0.24	0.42	0.24	0.43	0.24	0.43
Hispanic	0.02	0.16	0.02	0.16	0.02	0.15
Other	0.01	0.11	0.01	0.11	0.01	0.12
FARM	0.51	0.50	0.52	0.50	0.52	0.50
LEP	0.02	0.13	0.02	0.13	0.02	0.13
Age 13 in grade 8	0.28	0.45	0.23	0.42	0.22	0.42
N	224855		80971		54528	

B. High School Sample (Born 1989 - 1994)

Data window	+/- 120 days		+/- 45 days		+/- 30 days	
Variable	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Female	0.49	0.50	0.49	0.50	0.49	0.50
White	0.72	0.45	0.71	0.45	0.71	0.45
Black	0.23	0.42	0.24	0.43	0.24	0.43
Hispanic	0.03	0.16	0.03	0.16	0.03	0.16
Other	0.02	0.15	0.02	0.15	0.02	0.15
FARM	0.54	0.50	0.55	0.50	0.55	0.50
LEP	0.02	0.14	0.02	0.15	0.02	0.14
Age 13 in grade 8	0.28	0.45	0.23	0.42	0.22	0.42
N	363588		131341		88612	

C. K-8 Sample (Born 1997 - 1999)

Data window	+/- 120 days		+/- 45 days		+/- 30 days	
Variable	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Female	0.49	0.50	0.49	0.50	0.49	0.50
White	0.69	0.46	0.68	0.46	0.69	0.46
Black	0.20	0.40	0.20	0.40	0.20	0.40
Hispanic	0.06	0.24	0.06	0.24	0.06	0.24
Other	0.05	0.21	0.05	0.22	0.05	0.22
FARM	0.62	0.49	0.62	0.48	0.63	0.48
LEP	0.08	0.27	0.08	0.27	0.08	0.27
N	155893		56848		38220	

Notes: Samples exclude special education students educated exclusively in special education classrooms. See text for additional sample-specific construction details.

Table 2. Effects of School Entry and Compulsory Schooling Laws on Educational Attainment through High School

A. ITT Effects	Enroll in 11th grade				Enroll in 12th grade				Graduate high school			
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treat	0.015***	0.014***	0.016***	0.016***	0.011***	0.010***	0.010*	0.010**	0.021***	0.019***	0.016***	0.015***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Running	0.000*	0.000**	0.000	0.000	0.000	0.000	0.000	0.000	-0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treat*Running	-0.000**	-0.000**	-0.000	-0.000	-0.000	-0.000	-0.001**	-0.000	-0.000	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Data window	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days
Include student-level covariates?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	0.87	0.87	0.87	0.87	0.83	0.83	0.83	0.83	0.74	0.74	0.74	0.74
N	131341	131341	88612	88612	131341	131341	88612	88612	131341	131341	88612	88612
B. RDIV/2SLS Effects	*First stage*	Enroll in 11th grade	*First stage*	Enroll in 11th grade	*First stage*	Enroll in 12th grade	*First stage*	Enroll in 12th grade	*First stage*	Graduate high school	*First stage*	Graduate high school
Age 13 by Grade 8		0.043***		0.049***		0.031***		0.030**		0.057***		0.046***
		(0.011)		(0.014)		(0.012)		(0.014)		(0.014)		(0.017)
Treat	0.330***		0.334***		0.330***		0.334***		0.330***		0.334***	
	(0.004)		(0.005)		(0.004)		(0.005)		(0.004)		(0.005)	
Running	-0.001***	0.000***	0.001	0.000*	-0.001***	0.000	0.001	0.000	-0.001***	0.000	0.001	-0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Treat*Running	-0.002***	-0.000	-0.004***	-0.000	-0.002***	-0.000	-0.004***	-0.000	-0.002***	0.000	-0.004***	0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Data window	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days
Include student-level covariates?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome mean		0.87		0.87		0.83		0.83		0.74		0.74
N	131341	131341	88612	88612	131341	131341	88612	88612	131341	131341	88612	88612

Notes: All models include cohort fixed effects. Standard errors clustered on day of birth appear in parentheses: *** p<0.01, ** p<0.05, *p<0.1.

Table 3. Effects of School Entry and Compulsory Schooling Laws on Academic Achievement in High School

A. ITT Effects	ACT Composite Score			
Independent variable	(1)	(2)	(3)	(4)
Treat	-0.356***	-0.413***	-0.371***	-0.433***
	(0.081)	(0.070)	(0.097)	(0.087)
Running	-0.003	-0.002	0.001	-0.001
	(0.002)	(0.002)	(0.004)	(0.003)
Treat*Running	0.002	0.002	-0.006	-0.002
	(0.003)	(0.003)	(0.005)	(0.005)
Data window	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days
Include student-level covariates?	No	Yes	No	Yes
Outcome mean	18.73	18.73	18.75	18.75
N	60887	60887	41325	41325

B. RDIV/2SLS Effects	*First stage*	ACT Composite Score	*First stage*	ACT Composite Score
Age 13 by Grade 8		-1.565***		-1.982***
		(0.300)		(0.506)
Treat	0.264***		0.264***	
	(0.016)		(0.019)	
Running	-0.000	-0.002	-0.001	-0.004
	(0.000)	(0.002)	(0.002)	(0.006)
Treat*Running	-0.002***	-0.002	-0.002***	-0.005
	(0.001)	(0.003)	(0.001)	(0.008)
Data window	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days
Include student-level covariates?	Yes	Yes	Yes	Yes
Outcome mean		18.73		18.75
N	60887	60887	41325	41325

Notes: The ACT composite score outcome is from the student's first attempt. All models include cohort fixed effects. Standard errors clustered on day of birth appear in parentheses: *** p<0.01, ** p<0.05, *p<0.1.

Table 4. Effects of School Entry and Compulsory Schooling Laws on College Attendance, Choice, and Persistence

A. ITT Effects	Enroll in college (within 3 years of expected on-time high school graduation)				First enrollment is in a 2-year college				First enrollment is in a 4-year college				Number of semesters enrolled			
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Treat	0.014	0.012	0.008	0.007	0.026***	0.026***	0.019**	0.018**	-0.013**	-0.014**	-0.011	-0.011*	-0.343***	-0.354***	-0.375***	-0.378***
	(0.008)	(0.007)	(0.010)	(0.009)	(0.007)	(0.007)	(0.008)	(0.008)	(0.006)	(0.006)	(0.008)	(0.007)	(0.044)	(0.037)	(0.053)	(0.045)
Running	0.000	0.000	-0.000	0.000	-0.000	-0.000	-0.001	-0.001	0.000*	0.000**	0.001*	0.001**	0.001	0.002	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)
Treat*Running	-0.000	-0.000	-0.001	-0.000	0.000	0.000	0.000	0.000	-0.001**	-0.001***	-0.001**	-0.001*	-0.005***	-0.005***	-0.005*	-0.004*
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)	(0.001)	(0.003)	(0.002)
Data window	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days
Include student-level covariates?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	0.55	0.55	0.55	0.55	0.29	0.29	0.29	0.29	0.27	0.27	0.27	0.27	2.46	2.46	2.45	2.45
N	80971	80971	54528	54528	80971	80971	54528	54528	80971	80971	54528	54528	80971	80971	54528	54528
B. RDIV/2SLS Effects	*First stage*	Enroll in college	*First stage*	Enroll in college	*First stage*	First enroll 2-yr college	*First stage*	First enroll 2-yr college	*First stage*	First enroll 4-yr college	*First stage*	First enroll 4-yr college	*First stage*	Num sems enrolled	*First stage*	Num sems enrolled
Age 13 by Grade 8		0.035		0.021		0.079***		0.054**		-0.043**		-0.034*		-1.075***		-1.124***
		(0.022)		(0.027)		(0.020)		(0.025)		(0.018)		(0.020)		(0.118)		(0.139)
Treat	0.329***		0.336***		0.329***		0.336***		0.329***		0.336***		0.329***		0.336***	
	(0.005)		(0.007)		(0.005)		(0.007)		(0.005)		(0.007)		(0.005)		(0.007)	
Running	-0.001***	0.000	-0.001***	0.000	-0.001***	-0.000	-0.001***	-0.001	-0.001***	0.000**	-0.001***	0.001**	-0.001***	0.001	-0.001***	-0.001
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.002)
Treat*Running	-0.002***	-0.000	-0.002***	-0.001	-0.002***	0.000	-0.002***	0.001	-0.002***	-0.001***	-0.002***	-0.001**	-0.002***	-0.008***	-0.002***	-0.006**
	(0.000)	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.002)
Data window	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days	+/- 45 days	+/- 45 days	+/- 30 days	+/- 30 days
Include student-level covariates?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome mean		0.55		0.55		0.29		0.29		0.27		0.27		2.46		2.45
N	80971	80971	54528	54528	80971	80971	54528	54528	80971	80971	54528	54528	80971	80971	54528	54528

Notes: All models include cohort fixed effects. Standard errors clustered on day of birth appear in parentheses: *** p<0.01, ** p<0.05, *p<0.1.

Table 5. Heterogeneous Effects of School Entry and Compulsory Schooling Laws on High School Performance and Educational Attainment: by gender and poverty level

		Female		Male		FARM		Non-FARM	
Outcome	Estimate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High School Outcomes									
Enroll in 11th grade	ITT	0.010*	0.011*	0.018***	0.021***	0.024***	0.029***	0.003	0.001
		(0.005)	(0.007)	(0.005)	(0.006)	(0.006)	(0.008)	(0.003)	(0.004)
	2SLS	0.025*	0.028*	0.071***	0.081***	0.068***	0.082***	0.008	0.003
		(0.013)	(0.016)	(0.019)	(0.023)	(0.017)	(0.022)	(0.011)	(0.013)
Enroll in 12th grade	ITT	0.007	0.007	0.014**	0.014**	0.017***	0.018**	0.003	0.001
		(0.006)	(0.007)	(0.005)	(0.006)	(0.006)	(0.008)	(0.004)	(0.005)
	2SLS	0.018	0.017	0.052**	0.052**	0.048***	0.050**	0.008	0.003
		(0.015)	(0.018)	(0.021)	(0.024)	(0.018)	(0.021)	(0.013)	(0.016)
Graduate high school	ITT	0.022***	0.020**	0.016**	0.012	0.030***	0.026***	0.006	0.003
		(0.007)	(0.008)	(0.007)	(0.008)	(0.007)	(0.009)	(0.005)	(0.007)
	2SLS	0.055***	0.050**	0.062**	0.045	0.085***	0.074***	0.019	0.010
		(0.016)	(0.020)	(0.025)	(0.030)	(0.020)	(0.024)	(0.018)	(0.022)
ACT Composite Score	ITT	-0.284***	-0.349***	-0.423***	-0.514***	-0.487***	-0.404***	-0.325***	-0.457***
		(0.108)	(0.115)	(0.097)	(0.120)	(0.091)	(0.112)	(0.110)	(0.128)
	2SLS	-1.258***	-1.105***	-2.034***	-2.451***	-1.604***	-1.319***	-1.482***	-2.123***
		(0.321)	(0.393)	(0.490)	(0.612)	(0.331)	(0.396)	(0.516)	(0.645)
Postsecondary Outcomes									
Enroll in college	ITT	0.027**	0.027*	-0.003	-0.012	-0.002	-0.014	0.028***	0.031***
		(0.011)	(0.014)	(0.010)	(0.012)	(0.010)	(0.013)	(0.009)	(0.011)
	2SLS	0.066**	0.067*	-0.011	-0.046	-0.007	-0.038	0.093***	0.100***
		(0.027)	(0.034)	(0.037)	(0.045)	(0.029)	(0.036)	(0.030)	(0.037)
Enroll 2-yr college first	ITT	0.030***	0.024*	0.022**	0.013	0.011	-0.002	0.042***	0.041***
		(0.010)	(0.013)	(0.009)	(0.011)	(0.010)	(0.013)	(0.010)	(0.011)
	2SLS	0.075***	0.059*	0.083**	0.049	0.032	-0.005	0.141***	0.135***
		(0.025)	(0.032)	(0.034)	(0.039)	(0.027)	(0.034)	(0.032)	(0.037)
Enroll 4-yr college first	ITT	-0.004	0.003	-0.024***	-0.025***	-0.014**	-0.012*	-0.014	-0.011
		(0.009)	(0.011)	(0.008)	(0.009)	(0.006)	(0.007)	(0.010)	(0.012)
	2SLS	-0.009	0.008	-0.094***	-0.095***	-0.039**	-0.033*	-0.048	-0.035
		(0.023)	(0.027)	(0.029)	(0.035)	(0.017)	(0.020)	(0.032)	(0.038)
Number of semesters enrolled	ITT	-0.295***	-0.330***	-0.412***	-0.425***	-0.172***	-0.202***	-0.546***	-0.569***
		(0.060)	(0.075)	(0.048)	(0.057)	(0.043)	(0.051)	(0.053)	(0.062)
	2SLS	-0.732***	-0.807***	-1.589***	-1.591***	-0.485***	-0.556***	-1.813***	-1.858***
		(0.152)	(0.187)	(0.196)	(0.224)	(0.124)	(0.143)	(0.186)	(0.215)
Data window		+/- 45 days	+/- 30 days	+/- 45 days	+/- 30 days	+/- 45 days	+/- 30 days	+/- 45 days	+/- 30 days
N (high school sample)		64214	43240	67127	45372	71893	48389	59448	40223
N (college sample)		39437	26585	41534	27943	42125	28286	38846	26242

Notes: All models include cohort fixed effects and student-level covariates. Standard errors clustered on day of birth appear in parentheses: *** p<0.01, ** p<0.05, *p<0.1.