Student Age and the Collegiate Pathway

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Abstract: Using a rich data set of all SAT test takers from the 2004 through 2008 high school graduation cohorts, we investigate the impact of state-specific school age-of-entry laws on students' pathways into and through college. We document that these laws do not impact the probability that a student takes the SAT; however, we find strong evidence that students who are expected to be the oldest in their school cohorts based on their state residency and birthdays have a greater probability of taking an Advanced Placement (AP) exam and tend to take more AP exams. We also find that relatively younger students are more likely to attend two-year colleges before attending four-year colleges and are less likely to have earned bachelor's degrees four years beyond high school graduation, but eventually catch up to their older peers six years beyond high school graduation.

JEL Classifications: I20; C26; C51

1. INTRODUCTION

For decades, states have established guidelines specifying a minimum age for school entry. These policies help to ensure that students entering kindergarten are close in age to each other and that they are mature enough to function in a formal schooling environment. A natural result of these age-of-entry policies is that two students who are born a single day apart can be in different grades, forcing one to be among the youngest in his class and the other among the oldest in her class.

Over the past 50 years, the average age of students entering kindergarten has been steadily increasing (Deming and Dynarski, 2008). Part of this “graying of kindergarten” is attributable to state-specific policy changes in school age-of-entry dates, such as the recent Kindergarten Readiness Act of 2010 in California (SB 1381). By specifying that students must have reached age 5 by September 1st of the current school year to enroll in kindergarten, this policy change endeavored to reduce the number of 4 year olds in California who were entering kindergarten under the previous and longstanding age-of-entry date of December 2nd.

Since the mid 1960’s, more than half of all states have undertaken policy adjustments similar to that in California, and recent evidence suggests that these mandates to increase the age of kindergarteners have translated into real benefits. For example, Bedard and Dhuey (2012) link these changes in age-of-entry policies to increases in hourly earnings. Specifically, they find that moving the kindergarten age-of-entry cutoff date back one month increases average hourly earnings by 0.6 percent among males ages 30-54. Based on Bedard and Dhuey’s estimates, California’s recent policy, which moved the kindergarten age-of-entry cutoff date back three months, might be expected to increase hourly adult earnings among affected individuals by 1.8 percent.

In addition to statewide initiatives to increase the age of kindergarten cohorts, the parental practice of delaying a child's entry into kindergarten, also known as "redshirting," has become an increasingly common phenomenon.[1] Parents often claim that delaying entry into school is advantageous for kids who otherwise might be the youngest in their classrooms, citing maturity and physical size as motivators behind their decisions (Wang and Aamodt, 2011; Flapan, 2012). The choice of delayed school entry has actually accounted for a greater share of the "graying of kindergarten" over the past several decades than the statewide school age-of-entry policy changes (Deming and Dynarski, 2008).

The numerous statewide school age-of-entry policy changes combined with the proliferation of redshirting have compelled researchers to investigate whether there are a variety of advantages to entering school at a later age. Our paper unearths the contents of the black box of longer term outcomes by examining how state age-of-entry laws impact progress towards a college degree. Specifically, we look at differences in the probabilities of SAT and AP test-taking, SAT and AP scores, enrollment in two-year versus four-year colleges, quality of enrolled college, distance traveled to college, and degree attainment rates. Our primary results indicate that relatively older students within a high school cohort are equally likely to take the SAT, but more likely to take an AP exam, than relatively younger students. Also, relatively younger students are more likely to attend two-year colleges before attending four-year colleges, which likely contributes to their longer time to bachelor's degree completion.

To estimate the school age-of-entry effect on the aforementioned outcomes, we use student-level data on *all* SAT test takers and their exact dates of birth in all 43 states that imposed school age-of-entry laws between 1991 and 1995- the time period during which the

[1] In the college athletics parlance, "redshirting" refers to prolonging athletic eligibility by not competing for a season. Students who are redshirted may continue to practice with their teams, and this additional practice presumably gives them a leg-up when they return as competitive participants.

2004-2008 high school graduation cohorts would have been entering school.[2] The data on approximately 1.5 million SAT test-takers per year are combined with information from the National Student Clearinghouse (NSC), which indicates if and where students enroll in college and the date and college at which the students complete a degree. Along with the analyses incorporating all 43 states, we also present results using data from the 17 states in which the preponderance of four-year college-aspiring students take the SAT rather than the ACT as well as the subset of 14 SAT-dominant states that did not change their age-of-entry policies between 1991 and 1995. We show that results are insensitive to the composition of states in the sample.

We adopt a similar strategy and nomenclature as Bedard and Dhuey (2006), who carefully describe that naively running OLS regressions and comparing the differences in later outcomes between older and younger students in the same cohort can lead to biased estimates.[3] The bias stems from strong associations between drivers of non-compliance to the state policies (e.g. IQ) and the examined outcomes (e.g. college completion). This is why we exploit the exogenous school-entry birth date cutoffs in the 43 states that legalized such cutoffs. Because of these cutoffs, each student in our sample has an "assigned" age: the age at which she should be observed to complete high school, based on her birth day relative to the state age-of-entry cutoff date. This assigned age will differ from the "observed" age when the student fails to comply with the state's age-of-entry policy or, in some circumstances, repeats or skip grades.

[2] Seven states had age-of-entry policies that were determined by individual school districts (or local education agencies) and thus were excluded. Specifics on state age-of-entry laws were assembled from Bedard and Dhuey (2007), Elder and Lubotsky (2009), and Colasanti (2011). See Appendix Table A for a complete listing of the statewide laws or lack thereof. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

[3] Throughout the paper, "relative" age refers to being relatively old or young in the cohort. The same students with the lowest relative age also have the youngest absolute age. The importance of relative age versus absolute age is investigated in Cascio and Schanzenbach (2013).

Students whose birthdays fall immediately before the age-of-entry dates are, on average, expected to be identical to those whose birthdays fall just after just after these dates. Therefore, we can evaluate students' outcomes based on their assigned ages, just before and after the cutoffs. As Bedard and Dhuey (2006) note, these reduced form estimates measure the impacts of relative age "net of grade repetition and delayed entry", and it is these estimates that will shed light on whether the school age-of-entry policies impact later outcomes. We then account for non-compliance by using the student's assigned age, that is, the age she should be at high school graduation, as an instrument for the student's observed age at high school graduation. This allows us to estimate the causal effect of being one year older at high graduation relative to the student's peers. Formally, this is a fuzzy regression discontinuity design in which the forcing variable is the distance from the state birth date cutoff.[4]

In practice, the age-of-entry policies offer two potential strategies to perform our analyses. Using the first strategy, we would compare two students born one day apart who appear in different graduation cohorts as a result of state-specific age-of-entry policies and compare their later outcomes. However, using this approach, it would be impossible to disentangle the relative age effect from the high graduation cohort effect.[5] In addition, we do not observe *all* SAT test-takers with birth dates in the *assigned*, rather than the observed, 2004 thru 2008 cohorts.

The second strategy, which we favor, is to take two students, who are born 364 days apart and are in the *same* high school graduation cohort. Again, one student is relatively young and

[4] Barua and Lang (2012) describe the monotonicity problem associated with quarter of birth instruments. These issues are mitigated in regression discontinuities with small bandwidths, although not necessarily fully solved. Hence, we also rely on reduced-form (intent-to-treat) estimates, which evaluate the policies and do not suffer from the same criticism.

[5] We find strong evidence of a graduation cohort effect. For example, in California, the percentage of SAT test-takers attending a four-year college within 4 years of high school graduation dropped from 73.2 percent in 2006 to 71.9 percent in the 2007.

one is relatively old. The benefit of this strategy is that there is no high school graduation cohort effect. The possible limitation of this strategy is that it is impossible to disentangle the relative age effect from the *birth* cohort effect. We find that students in different birth cohorts are similar on observable characteristics and consequently argue that the birth cohort is of little concern.

We start our analysis by showing that the assigned age at high school graduation is unrelated to the probability of taking the SAT. For example, in California, where a child must have turned 5 on or before December 2nd to begin kindergarten, students who are born on December 2nd and tend to be the youngest in their cohorts are equally likely to take the SAT as students who are born on December 3rd and tend to be the oldest in their cohorts. This result serves two purposes. First, continuous density in the number of students taking the SAT around the age-of-entry cutoffs is a necessary condition of the running variable in all of this paper's regression discontinuity design analyses.[6] Second, whether being relatively old results in an increased probability of taking a college admissions assessment test is an interesting research question in and of itself, especially since sitting for an assessment test has been shown to increase four-year college enrollment (Klasik, 2013; Hurwitz et al. 2014). In fact, this question has previously been explored by Cascio and Schanzenbach (2013) who found that, among students in the Tennessee STAR experiment, a student's age relative to her peers in kindergarten bore no statistically significant relationship with whether that student ultimately took an ACT or SAT.

We then turn our attention to college preparation, college choice, and bachelor's attainment to find the following results. We find that students slated to graduate from high school one year older than their observationally similar peers:

[6] Had there been a failure of this McCrary-like test, then a selection problem may have existed such that the SAT test-takers on each side of the age-of-entry threshold may have differed in important ways related to the examined outcomes.

- achieve similar SAT scores;
- are approximately 4 – 5 percentage points more like to take an AP exam by high school graduation (and they take more exams and achieve more passing scores);
- are approximately 2.5 – 3 percentage points less likely to attend a two-year college;
- are equally as likely to ever enroll in a four-year college;
- attend similar quality colleges, as measured by average SAT, conditional on attending a four-year college;
- travel similar distances to attend college, regardless of college sector;
- are approximately 2 – 2.5 percentage points more likely to attain a bachelor's degree four years beyond high school graduation, but equally likely to obtain a bachelor's degree six years beyond high school graduation.
- are 1 percentage point less likely to have earned an associate's degree

This study has several advantages relative to previous work on school age-of-entry policies, which also generally implement regression discontinuity designs. First and foremost, our rich data set allows us to examine not only degree attainment as an outcome, but also new and finer details of the college degree pathway. There exists no consensus in the body of literature addressing whether school age-of-entry policies harm or benefit relatively younger students, and through this study, we hope to add clarity to this topic. In addition, using season of birth as an instrument has come under scrutiny because of differences in maternal characteristics, by birth season (Buckles and Hungerman, 2013). Fortunately, our sample size and precise information on day of birth allow us to compare students born just before and just after the state-specific cutoff dates, such that students on either side of the cutoff share identical sociodemographic characteristics. Finally, data on the student's expected high school graduation year, as reported by the student on the SAT registration forms, in addition to her date of birth, allow us to account for sources of policy non-compliance, such as redshirting, advanced school entry, and across-state migration.

2. LITERATURE REVIEW

Recent interest in the long-term consequences of school age-of-entry laws has dovetailed with the growing body of empirical research relating early childhood shocks to adult outcomes (Almond and Currie, 2011; Dynarski, Hyman, and Schanzenbach, 2013).[7] Though the term "shock" carries a clear negative connotation, in the context of this paper, a shock might be thought of as an interruption in a child's daily routine like the transition from day care or home care to a formal schooling environment. Such a shock need not be negative, but might prove harmful to a young child lacking the maturity to enter school. Children whose birthdays make them eligible to enter school systems earlier than their peers may be particularly susceptible to these negative shocks. A student's lack of maturity might be confused with a lack of academic ability, and the student might be tracked into an inappropriately unchallenging curriculum as a result. In other domains, early childhood shocks have been shown to reverberate well into the future, particularly for young people from disadvantaged backgrounds (e.g. Currie and Hyson, 1999). The influential role of early childhood experiences on later outcomes puts pressure on both parents and policymakers "to get it right" when it comes to the transition into formal schooling and a key piece of this decision involves the timing of this transition.

To date, the literature on the impacts of age-of-entry policies focuses on several outcomes, ranging from short term to long term. Many of these studies employ a regression

[7] Almond and Currie (2011) also discuss the theoretical framework of early childhood investment.

discontinuity design- similar to the one in which we implement in this paper. Despite the similar empirical strategies in these papers, the conclusions reached are composed of a mixed set of positive, negative, and null effects of relative maturity.

We start by discussing studies that find largely positive effects of being the relatively older student. Using the entire population of Swedes born between 1941 and 1982, Fredriksson and Ockert (2006) find that children who are among the older students in their cohort perform better academically and are more likely to graduate from college than their younger cohort peers. Similarly, Puhani and Weber (2005) find that German students entering school at age 7 outperform similar students entering school at age 6 in terms of test performance and years of secondary schooling. Bedard and Dhuey (2006) draw their data from the internationally administered Trends in Mathematics and Science Study, supplementing this main data set with some additional data sets including ECLS and NELS, which focus only on American students. They find that younger members of a cohort score more poorly than their older peers on the TIMSS in both 4^{th} and 8^{th} grade as well as experience a decreased likelihood of college attendance.

The news isn’t all good for students whose birthdays should place them among the oldest in their school cohorts. For example, McCrary and Royer (2011) use Department of Health natality data from California and Texas, two states which have cutoff dates for school entry, and find that mothers who should be among the youngest in their school cohorts tend to have more education at motherhood. They also find that mothers from their California sample with the youngest assigned ages, and therefore are expected to be the youngest in their cohorts based on their birth days, are less likely to be smokers.

Still other studies addressing whether age at school entry has any notable longer term outcomes find mixed results. Using Decennial Census Long Form data, Dobkin and Ferreira (2010) use a regression discontinuity design to document that while the youngest students within a school cohort suffer from weaker academic performance, they also tend accumulate more years of schooling, particularly at the high school level. College completion rates in their study do not appear to be related to age of entry. Also harnessing variation in school age resulting from state mandates and also finding mixed results, Elder and Lubotsky (2009) draw their conclusions from the Early Childhood Longitudinal Study- Kindergarten Cohort and the National Educational Longitudinal Study of 1988. They show that students who have relatively older classmates enjoy improved test scores, but also face higher probabilities of grade repetition and learning disability diagnoses. Finally, Black, Devereux and Salvanes (2011) exploit school starting age mandates in Norway to find that beginning school at a younger age has a small positive impact on IQ scores at age 18. By contrast, the absolute age at which students take the IQ test overshadows any effects associated with age of school entry, with age at test positively related to IQ scores. They also find higher teenage pregnancy rates among students who begin school younger, and no effect on educational attainment.

3. DATA

Our sample includes the universe of all SAT takers in the 2004 thru 2008 high school graduation cohorts. In addition to the student's reported year of high school graduation, the SAT registration form asks for the student's date of birth and state of residence.[8] Not only does our SAT data set include information on the student's test scores, it contains historical information

[8] Students may have started school in states with different birth date cutoff policies. While we cannot determine who this pertains to, the fuzzy regression discontinuity design also accounts for noncompliance due to cross-state migration.

on all AP participation during the student's high school career, the student's race/ethnicity, gender, parental education and income, and high school attended.[9] This dataset is then merged to the National Student Clearinghouse (NSC) data. The NSC collects data from about 3,300 participating colleges and universities, which covers 94 percent of enrolled students across the country. The NSC tracks individual students through their postsecondary education career through 2012, indicating the college attended, enrollment spells and, for many postsecondary institutions, whether or not the student ultimately received a degree and the date of this degree. We link students' postsecondary institutions with college-level data including institutional selectivity and distance between the student's home and her chosen college.[10]

In these analyses, we include all 43 states with school age-of-entry laws between 1991 and 1995- the time period during which students from the 2004-2008 high school graduation cohorts would have been entering elementary school. Historical information on these laws is reported in Bedard and Dhuey (2007), Elder in Lubotsky (2009) and Colasanti (2011), and we confirm their existence by identifying clear state-level discontinuities in age at high school graduation around these birthdays. We also present results from analyses that exclude states in which the preponderance of four-year college-aspiring students take the ACT, rather than the SAT exam (Clark, Rothstein and Schanzenbach , 2008), and separately by excluding states that changed their age-of-entry policies between 1991 and 1995. We show that all results are robust to these exclusion choices. Appendix Table A lists all 43 states' age-of-entry policies and the seven states where these policies rest with the local education agencies (LEA), and whether the state is an SAT-dominant state. Six states altered their policies over this time period, and thus we

[9] Data on demographic characteristics are missing for some students and are coded as such instead of dropping these observations.
[10] This information comes from the Integrated Postsecondary Education Data System (IPEDS).

assigned students from the 2004 cohort the 1991 policies, students from the 2005 cohort the 1992 policy, and so on.[11]

3.1. Descriptive Statistics

In Table 1, we present basic descriptive statistics on all students in our data who were born within 10 days of the state-specific age-of-entry cutoff, across all 43 sampled states for the 2004 thru 2008 high school graduation cohorts.[12] The first thing to note is that students born after the cutoff date are, on average, 0.33 years older than students born before the cutoff date. If there were perfect compliance with the state policy, the difference should be close to a full year. Non-compliance through delayed or advanced kindergarten entry and grade retention or skipping reduces this gap.

<<Insert Table 1>>

In this main analytic sample consisting of students residing in the 43 states from the 2004 thru 2008 high school cohorts, who were born within 10 days of the state-specific age-of-entry cutoff, approximately 81 percent of students attended public high schools, and about 60 percent self-identified as White. Nearly three-quarters of the sample attended at least one four-year college and slightly more than 40 percent attended a two-year college. Among students first attending a four-year postsecondary institution, the average SAT score of that institution was approximately 1115 and students, on average, traveled 200 miles from home to attend their first college.[13] Among all sampled students, 29 percent received bachelor's degrees within four years

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[11] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

[12] We show the sensitivity of our results to bandwidth selection in the appendix. However, the main results use this narrow bandwidth.

[13] This is calculated from IPEDS data using as the average of 25th and 75th percentiles on the critical reading section plus the average of the 25th and 75th percentiles on the math section. For colleges not reporting SAT scores, an average ACT is calculated as the average of the ACT composite 25th and 75th percentiles. The average composite ACT score is then converted into a composite SAT score using the College Board's concordance table

of high school graduation and 48 percent received bachelor's degrees within six years of high school graduation.

4. ECONOMETRIC METHODOLOGY

We adopt the strategy and language of Bedard and Dhuey (2006), with slightly different notation, and start with a simplistic model of the relationship between student outcomes and observed age:

$$\mathrm{Outcome}_{ist} = \alpha_0 + \alpha_1 \mathrm{A}_{ist} + \alpha_2 \mathrm{DOB}_{ist} + \alpha_3 \mathrm{X}_{ist} + \mathrm{State}_s + \mathrm{Cohort}_t + \varepsilon_{ist} \qquad (1)$$

where $\mathrm{Outcome}_{ist}$ is an outcome, such as enrollment in a four-year college, for student *i* in state *s* in cohort *t*. A_{ist} is the observed age for a student at the point of expected high school graduation. X_{ist} is a vector of student controls which includes the student's high school type (public versus private), student race, gender, family income and parental education, as well as a dummy variable indicating whether the student was born on a weekend day. *State* and *Cohort* are vectors of state and high school graduation cohort fixed effects and DOB_{ist} is the distance in days between the student's birthday and the state-specific threshold age-of-entry cutoff day, also known as the running variable.

Within a narrow range of birthdays centered at the state-specific age-of-entry cutoffs, students should differ in age by close to one year, and the coefficient α_1 would identify the effect of being relatively old on the outcome of interest if the student's observed age were entirely determined by these plausibly exogenous age-of-entry policies. Non-compliance makes such an assumption impossible.

(http://research.collegeboard.org/sites/default/files/publications/2012/7/researchnote-2009-40-act-sat-concordance-tables.pdf)

As with the most recent studies that rely on school age-of-entry policies to draw causal inferences, we employ a regression discontinuity design (RDD) using the student's date of birth to draw inferences. We first identify the net effect of the age-of-entry policies by estimating parameter δ_1 in the reduced-form regression discontinuity equation (Equation 2). In this first step, we consider the student's *assigned* age based on the month and day of the student's birthdate and state residency. Students with birthdays just before the state-specified cutoffs have assigned ages of 17 ($Boundary_{ist}=0$), and students with birthdays at or after these cutoffs have assigned ages of 18 ($Boundary_{ist}=1$). The regression parameter δ_1 identifies the direct impact of these age-of-entry laws on student outcomes.

$$\mathrm{Outcome}_{ist} = \delta_0 + \delta_1 \mathrm{Boundary}_{ist} + \delta_2 \mathrm{DOB}_{ist} + \delta_3 \mathrm{Boundary}_{ist} * \mathrm{DOB}_{ist} + \delta_4 \mathrm{X}_{ist} + \mathrm{State}_s + \mathrm{Cohort}_t + \sigma_{ist} \qquad (2)$$

Knowing the student's year of birth in addition to the month and day of birth allows us to take our analyses one step further and directly address the impact of age at high school graduation, as indicated on the student's SAT registration form, on student outcomes. We employ the standard econometric model for a fuzzy RDD (Lee and Lemieux, 2010), which is represented by the two-stage least squares (2SLS) regression shown in equation (3) and equation (4), where A_{ist} is student *i's* observed age on June 30 of the student's expected high school graduation year:

$$\mathrm{A}_{ist} = \beta_0 + \beta_1 \mathrm{Boundary}_{ist} + \beta_2 \mathrm{DOB}_{ist} + \beta_3 \mathrm{Boundary}_{ist} * \mathrm{DOB}_{ist} + \beta_4 \mathrm{X}_{ist} + \mathrm{State}_s + \mathrm{Cohort}_t + \zeta_{ist} \qquad (3)$$

$$\text{Outcome}_{ist} = \gamma_0 + \gamma_1 \widetilde{\text{A}}_{ist} + \gamma_2 \text{DOB}_{ist} + \gamma_3 \text{Boundary}_{ist} * \text{DOB}_{ist} + \gamma_4 \text{X}_{ist} + \text{State}_s + \text{Cohort}_t + \upsilon_{ist} \qquad (4)$$

Equation (3) is the first stage where student *i*'s observed age at high school graduation is regressed on the dummy variable, Boundary_{ist}. Equation (4) is the second stage regression that uses the predicted observed age, $\widetilde{\text{A}}_{ist}$, from equation (3).[14]

As is typical in all RD designs, the choice of bandwidth is particularly important when using local linear regression because bandwidths too large can introduce bias. Hence, we follow the bandwidth selection strategy recommended by Imbens and Kalyanaram (IK) (2012). We use the optimal IK bandwidth for the first stage equation of 10.11, which is generally smaller than when we estimate the IK-estimated bandwidths for this paper's outcome variables, such as four-year college-going. In our main specifications, we use a triangular kernel that places more weight on observations that are closer to the threshold. Appendix Tables B and C demonstrate that all parameter estimates obtained from our main specifications are robust to bandwidth, the use of a rectangular instead of a triangular kernel, and higher order expressions of the forcing variable. Finally, as is the standard in RDD type analyses, we offer evidence both graphically and empirically (Schochet et al., 2010).[15]

5. RESULTS

5.1. Taking the SAT

[14] Lee and Card (2008) recommend clustering standard errors on the forcing variable. Because of the small bandwidth used in these analyses, the number of clusters (≈ 20) is small enough that clustered standard errors often produce smaller standard errors than the standard heteroskedasticity robust errors. Therefore, we rely on the latter to maintain a conservative approach to our inferences.

[15] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

We first address whether school age-of-entry policies impact the probability of taking the SAT. As an outcome, the SAT is a requirement for admission to many four-year institutions, and any impact of age-of-entry policies on this outcome would logically extend into observed impacts on four-year college-going. In addition, finding a relationship between age-of-entry policies and SAT test-taking would complicate future analyses because these SAT test-takers are the foundation of our sample and a gap in the number of test takers across the age-of-entry thresholds might suggest other important student-level differences across these boundaries.

We demonstrate that age-of-entry laws do not impact SAT test-taking by showing that no difference in density of test-takers exists around the age-of-entry threshold. We then test whether covariates are balanced around the cutoff. Fortunately, both of these tests indicate that the sub-sample of SAT test-takers, rather than the universe of all students, is adequate to answer the research questions addressed in this paper.

Figure 1 shows the density of students for each of the three sets of sample states, by distance from the state-specific age-of-entry threshold. Students represented by the red bars have the youngest assigned ages based on their month and day of birth and state residency, meaning that under policy compliance, they should be the youngest students in their school cohorts. Students represented by the blue bars have the oldest assigned ages, and are expected to be the oldest in their cohorts based on month and day of birth. Alongside these histograms, we show the discontinuities in average age at high school graduation, by the student's distance from the state-specified thresholds. Two clear patterns emerge. First, the average age at high school graduation increases by 0.3 to 0.4 years across the state specific thresholds, which, as we will show, translates into a strong first stage. Second, in each of the sets of sampled states, there are no visible differences in the total number of students across the threshold, indicating that the day of birth is exogenous to SAT test-taking.

<<Insert Figure 1>>

It is important to emphasize that the state in which the student took the SAT is not necessarily the state in which the student was born. This type of across-state migration is another source of non-compliance, in addition to redshirting and advanced school entry, which is captured in Figure 1. If parents were strategically moving their children to states where they would be among the oldest, based on day of birth, our ability to draw causal inferences would be compromised. The plausibility of this scenario is doubtful, as uprooting a family just to reposition one child's relative age seems unnecessarily drastic. Moreover, this type of migration would generate visible discontinuities in number of observations around the age-of-entry thresholds, which are not evident.

In the spirit of McCrary (2008), we formally test whether density of observations differs across the state-specific thresholds. To accomplish this, we aggregate counts of students by state, year, whether the student's birthday occurred on a weekend, and distance from the state-specified age-of-entry threshold before estimating equation (5)

$$\mathrm{StudentCount}_{\mathrm{dst}} = \vartheta_0 + \vartheta_1 \mathrm{Boundary}_{\mathrm{st}} + \vartheta_2 D_{\mathrm{dst}} + \vartheta_3 \mathrm{Boundary}_{\mathrm{st}} * D_{\mathrm{dst}} + \mathrm{State}_{\mathrm{s}} + \mathrm{Cohort}_{\mathrm{t}} + \mathrm{Weekend}_{st} + \varepsilon_{\mathrm{dst}} \qquad (5)$$

where D_{dst} is the distance from the state-specific cutoff. As before, we are interested in the coefficient on $\mathrm{Boundary}_{\mathrm{st}}$, which indicates whether the number of students taking the SAT is affected by the age-of-entry assignment.

The first row in Table 2 provides the estimate of ϑ_1 parameter at varying bandwidths, and for the three main subsamples of states. These parameter estimates indicate the magnitude of the discontinuity in the numbers of sampled students across the age-of-entry thresholds. All density discontinuities are both trivial in magnitude and lack statistical significance, confirming what is visually evident in Figure 1.

<<Insert Table 2>>

Despite the fact that no clear differences in density of SAT takers exist around the state-specific thresholds, it is possible that the composition of test-takers differs in meaningful ways that have the potential to mislead us into making false conclusions about the impacts of age-of-entry laws. In Figure 2, we plot the percentage of students falling into selected non-academic covariate categories against day of birth relative to the state-specified threshold. Of the eight panels in Figure 2, none of the covariates experience any pronounced visual discontinuities around the age-of-entry cut date.

<<Insert Figure 2>>

We formally test for balanced covariates by fitting equation (6), where the value of π_1 in this equation represents the discontinuity in selected covariates at the age-of-entry cutoff date.

$$\mathrm{Covariate}_{\mathrm{ist}} = \pi_0 + \pi_1 \mathrm{Boundary}_{\mathrm{ist}} + \pi_2 D_{\mathrm{ist}} + \pi_3 \mathrm{Boundary}_{\mathrm{ist}} * D_{\mathrm{ist}} + \mathrm{State}_{\mathrm{s}} + \mathrm{Cohort}_{\mathrm{t}} + \cdots$$
$$\ldots + \mathrm{Weekend}_{\mathrm{ist}} + \varepsilon_{\mathrm{ist}} \qquad (6)$$

In Table 3, we present these π_1 parameters for selected covariates at various bandwidths for the three sets of states. At the preferred bandwidth of approximately 10 days, most parameter estimates of *Boundary* are small and generally not statistically significant. We do observe a small, statistically significant difference in the percentage of sampled students with one or more parents having received a bachelor's degree or higher across the age-of-entry threshold. Though this difference of less than one percentage point is statistically significant, it is too small in magnitude to drive the estimates presented in this paper. If any sample bias exists, it would suggest that students who should be the youngest in their cohort (with the youngest assigned ages) would experience more favorable outcomes. As we soon show, the opposite is generally true, meaning this paper's estimates may slightly *understate* the negative consequences of school age-of-entry laws faced by the youngest students within a grade. For all outcomes in this paper, we estimate the impact of the age-of-entry policies including and excluding these covariates. Their inclusion increases the precision of estimation, but does not change the actual values of any parameter estimates.

<<Insert Table 3>>

5.2. First-Stage Estimates

In Table 4, we show the parameter estimates of β_1 in equation (3), the effect of the age-of-entry policies on observed age (biological age). These estimates demonstrate the extent of policy adherence and are ultimately used to adjust our reduced-form estimates to account for non-compliance. We present first-stage parameter estimates for the three sets of states including (right-hand panels) and excluding (left-hand panels) the covariates in Table 3.[16] The 2004-2008 cohort parameter estimates in panel 3 correspond to first-stage of the 2SLS estimation for all test-taking and college-going outcomes as well as four-year graduation rates. Panel 2 presents the first-stage estimates relevant to the five-year graduation rate outcomes, and Panel 1 presents the first-stage estimates relevant to the six-year graduation rate outcomes. On average, students to the right of the state-specific age-of-entry thresholds are about 0.34 to 0.37 years older than those

[16] State-specific first-stage parameter estimates and the accompanying F-statistics are presented in Appendix Table A. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

to the left.[17] The first-stage F-statistics for the entire sample of students across all 43 states are in the vicinity of 2500.

<<Insert Table 4>>

5.3. SAT Scores and AP Exams

In this section, and ones to follow, we present the main results of our analyses. This includes the naïve OLS estimates (equation 1), as well as the reduced form estimates, which reveal the net effect of the age-of-entry policies (equation 2), and the 2SLS estimates which identify the impacts attributable to graduating from high school one year older (equation 4). Table 5 shows the results of these analyses.[18]

As alluded to above, policy non-compliance severely biases OLS estimates in Table 5, which misleadingly suggest that relatively older students fare worse than their observationally similar, but relatively younger peers, in all measures of academic success. This is due to the fact that youngest students in our sample, whose assigned ages suggest that they should be the oldest in the sample (grade skippers or students who enter school early), are particularly high achievers.[19]

[17] The estimates are insensitive to bandwidth and kernel selection and whether higher-order polynomials are used for the running variable. See Appendix Table B for results, which include the 2004-2008 cohort data. The 2SLS and IV estimates for bachelor's completion data exclude students who last attended four-year institutions failing to accurately report completion data to the NSC, while all students are included in Table 4's parameter estimates. Of the 270,105 sampled students in the full 43 state sample, less than 14,000 students were impacted by this issue. In column 1, the first-stage estimates (standard errors) excluding these students are 0.343 (0.006), 0.340 (0.005), and 0.341 (0.005) for the 2004-2006, 2004-2007 and 2004-2008 cohorts, respectively.

[18] Appendix Figure A plots the corresponding graphical analysis and supports the results of Table 5. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

[19] Of the four groups of students based on the compliance/assigned age interaction, the youngest non-compliers consistently demonstrate the best academic outcomes. For example, in the SAT-dominant sampled states, these students have SAT scores 6 points higher than the red-shirted students, who are about one year older at high school graduation and 18 points higher than the oldest compliers. These young non-compliers who started school a full-year before they should have or who skipped a grade also took 25 percent more AP exams by high school graduation than did the redshirted students and were 2 percentage points more likely to attend a four-year college than any other group of students.

Reduced form estimation demonstrates that, depending on the sample of states included, the age-of-entry policies introduce a 1.3 to 1.7 percentage point gap in AP test-taking between students who should be the oldest and youngest in their school cohorts, net of compliance with the policies. By high school graduation, the students who should be the oldest based on their state residency and month and day of birth (oldest assigned age) will have taken about 0.08 (approximately 6 percent) more AP examinations and will have achieved 0.043 (approximately 5 percent) more passing scores of 3 or greater (on a 1 to 5 scale). The 2SLS estimates reveal that students one year older at high school graduation are between 3.9 and 4.8 percentage points more likely to have taken one or more AP exams by high school graduation than observationally similar peers, and these same students will have accumulated about 0.22 more exams and 0.12 more passing scores by high school graduation. These results are robust to choice of bandwidth, kernel, and to higher order expressions of the forcing variable, as shown in Appendix Tables C and D.[20]

<<Insert Table 5>>

5.4. College-Going Outcomes

Table 6 present results indicating whether relatively older students are more likely to attend two- and four-year colleges than their younger peers.[21] Results suggest that age-of-entry laws do not impact the probability that a student will ever attend a four-year college. However, relatively older and younger students have different trajectories into the four-year postsecondary pipeline, with relatively older students more likely to begin at a four-year, rather than a two-year institution. Specifically, students who should be the oldest in their school cohorts (oldest

[20] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

[21] Appendix Figure B plots the corresponding graphical analysis and supports the results of Table 6. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

assigned age) are about 1 percentage point less likely to attend two-year colleges and between 1.1 and 1.3 percentage points less likely to begin their four-year postsecondary careers at two-year colleges, compared to similar students with the youngest assigned ages. While those estimates are the effect of the policies net of compliance (the intent-to-treat), the 2SLS estimates (the treatment on the treated) show that students one year older at high school graduation are between 2.6 and 3.1 percentage points less likely to attend two-year colleges.

<<Insert Table 6>>

5.5. College Choice

Ample evidence supports the claim that attending a more selective college is in the student's best interest (Daniel, Black and Smith, 1997; Hoxby, 1998; Rouse, 1999; Long, 2008; Hoekstra, 2009; Cohodes and Goodman, 2014; Smith, 2013), and age-of-entry laws may impact the student's future outcomes by restricting college choice. Older students might have the most flexibility to choose more selective colleges, or colleges that best match their academic and non-academic interests. Such flexibility might stem from heightened maturity or academic success by high school graduation or it might be reflective of lower transportation barriers.[22] Therefore, restrictions in the college choice process have the potential to yield significant long-term ramifications for younger students.

Table 7 reveals that the age-of-entry policies have little impact on the college choice process, beyond the two versus four year decision.[23] On average, students who are supposed to be the oldest in their school cohorts based on their month and day of birth attend four-year institutions where the average SAT scores are about 3 points higher than observationally

[22] Not only will younger students have had less time to receive driver's licenses, they are more likely to be subject to restrictive driving regulations, which limit driving hours. See http://www.allstatefoundation.org/teen-driving-laws.
[23] Appendix Figure C plots the corresponding graphical analysis and supports the results of Table 7. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

identical peers who have the lowest assigned ages and travel about 7 miles farther to their first college. Though the estimates on average SAT scores of first college attended are statistically significant, in large part because of our sample size, they are substantively trivial. Redefined as effect sizes, the impact on distance traveled to first college and average institutional SAT score are both only about 0.02 standard deviation units.

<<Insert Table 7>>

5.6. College Attainment Rates

The different paths into the four-year college pipeline between older and younger students shown in Table 6 suggest that younger students may be penalized, in terms of bachelor's degree completion, by entering the four-year college pipeline through the two-year college route (Long and Kurlaender, 2009). To some extent, this anticipated finding is borne out in the data. Table 8 shows that students who are supposed to be the oldest in their cohort (oldest assigned ages) are somewhat more likely to receive bachelor's degrees within 4 years of high school graduation.[24] Reduced form estimates indicate that students with the oldest assigned ages are slightly less than one percentage point more likely to have obtained a bachelor's degree within four years of graduation, and the 2SLS estimates suggest that students one year older at high school graduation are between 2 and 2.5 percentage points more likely to hold this degree four years out from high school. By contrast, five and six years after high school completion, the causal effects of age-of-entry laws on bachelor's completion appear to have vanished. While these laws may have a minor, yet detectable impact on time-to-degree, they do not appear to influence ultimate degree completion.

[24] Appendix Figure D plots the corresponding graphical analysis and supports the results of Table 8. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

Two-year completion rates among sampled students are low compared to bachelor's completion rates. Only about 6 percent of students sampled earned a degree from a two-year college by 2012. Nevertheless, Table 8 offers some evidence that students who should be the youngest in their cohorts (the youngest assigned ages) are more likely to earn two-year college degrees, an unsurprising finding considering that these students are more likely to attend two-year colleges than their older peers. The last column in Table 8 shows that students one year older at high school graduation are about 1 percentage point less likely to have earned two-year college degrees.

<<Insert Table 8>>

5.7. Heterogeneous Impacts

In Table 9, we present the 2SLS estimates for the impact of age-of-entry on all of this paper's outcomes, by state-level compliance to the policy, student gender, and student race.[25] Splitting the sample results in less precise estimates, yet this type of heterogeneity analysis does reveal some differences in the impacts of age-of-entry policies on subgroups. Perhaps the most striking is the differential impact of age-of-entry laws on AP participation by gender. On these outcomes, it appears that women who are supposed to be the youngest in their cohorts suffer much greater penalties than do men with the youngest assigned ages. For example, women one year older at high school graduation will have taken 0.274 more AP exams by high school graduation, compared to a non-statistically significant estimate of 0.121 for men.

There also exists some heterogeneity in the impacts of age-of-entry laws on completion. Female students one year older at high school graduation than similar peers are more than 3

[25] We split states into two groups based on the magnitude of the first-stage parameter estimates in Appendix Table A. Low compliance states include those with first-stage parameter estimates at or below the median of 0.257. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

percentage points, or about 10 percent, more likely to have obtained a bachelor's degree four years after graduating from high school, compared to a non-statistically significant estimate of 0.9 percentage points for men. While the overall estimates of age-of-entry laws on six-year bachelor's completion rates are approximately zero, these laws do appear to impact this outcome for Hispanic students. Members of this ethnic group who graduate from high school one year older than observationally similar peers are about 4 percentage points more likely to hold bachelor's degrees four years beyond high school graduation and more than 6 percentage points more likely to hold bachelor's degrees after six years has elapsed.

<<Insert Table 9>>

6. DISCUSSION

In these analyses, we have shown that the state age-of-entry policies do not influence SAT test-taking behavior, but *do* influence AP test-taking behavior. While more research is required to fully understand this result, perhaps it is a consequence of early and persistent student tracking. That is, if relatively young students are placed into lower-level academic tracks during kindergarten or elementary school, these same students may need to overcome more academic obstacles to enroll in rigorous AP courses. Similarly, these age-of-entry policy shift students' trajectories into the four-year pipelines, with students expected to be the youngest in their cohorts based on their birth days more likely to begin their four-year college careers at two-year postsecondary institutions. These students who are supposed to be the youngest in their cohorts based on their birthdays are also less likely to hold bachelor's degrees four years beyond high school graduation than their peers with the oldest assigned ages – a difference that disappears six years beyond high school completion.

Our paper points to the fact that relatively older students experience favorable educational outcomes compared to their younger peers, but what can be done to address this issue? States have many policy tools at their disposal (Berger and Toma, 1994), but adjusting state age-of-entry laws will just change which students tend to be the youngest based on their birth days, not the relative age distributions of students within grades. An often exercised option for parents with children whose birthdays fall just before the state-specified age-of-entry cutoff is delayed school entry. Through such a delay, students who should be among the youngest in their school cohorts are instead among the oldest. However, entering kindergarten one year older may translate into greater childcare costs for parents, and the consequences of such a delay may outweigh any benefits, particularly in the absence of high quality early childcare programs (Gormley, 2007). Another possible cost of delayed school entry is a penalty of fewer years in the labor market. Are the added costs of delayed school entry worth the modest benefits presented in this paper?

Year-round schooling presents one possible solution of reducing heterogeneity in ages among children in early grades, and possibly improving outcomes among students who ordinarily would be the youngest in their cohorts. This type of school calendar has been presented as an alternative to the traditional model of 180 school days followed by a lengthy summer break. Conceived partially as a response to school overcrowding and concerns about knowledge loss over summer, year-rounding schooling creates several tracks of students who remain in school all year with shorter, staggered breaks. Evidence on the efficacy of year-round schooling is mixed, with some research supporting the positive effects of year-round schooling for students from disadvantaged backgrounds and other studies casting doubt on its power to attenuate summer knowledge losses (McMullen and Rouse, 2012; Graves, McMullen and Rouse, 2013). In some states like Illinois with standard age-of-entry policies for schools adhering to the traditional calendar, modifications exist for year-round schools which specify that students must turn 5 within 30 days of a term's commencement, rather than September 1.[26] States utilizing both traditional and year-round schooling models are well positioned to exam the possibility that this latter approach improves outcomes for the would-be youngest students by comparing outcomes for August-born students who enter traditional schools with those who enter year-round schools.

Even among relatively young students without access to year-round schooling who comply with their states' age-of-entry laws, there exists another juncture at which maturity can be gained. Increasingly, "gap" years, in which students take a break from formal schooling after completing high school, are being used as a measure to improve postsecondary success. Unfortunately, there is no consensus as to whether taking a gap year increases a student's maturity to the point where she will be more likely to succeed in college (Kempner and Kinnick, 1990; Horn, Forest-Cataldi, and Sikora, 2005; Bozick and DeLuca, 2007; Birch and Miller, 2007). And while this paper does not address the benefits or drawbacks of gap years explicitly, it does comment on a student's relative maturity.

Rather than advocating for sweeping changes in school age-of-entry policies or potentially costly approaches of increasing student maturity, neither of which are defensible based on our results, we return to our most powerful result which indicates that older students are more likely to participate and succeed in AP courses. We propose as a first step a deeper investigation into the root cause underlying the very strong relationship between AP participation and relative age. Are younger students within grades discouraged from taking their high schools' most rigorous course offerings or are they self-selecting out of these courses? Such differences

[26] See (http://illinoisearlylearning.org/faqs/kinderentryage.htm) for the full details on the age-of-entry laws in Illinois.

in AP test-taking rates by relative age are particularly concerning in light of the miniscule differences in SAT scores between students with the oldest relative ages and those with the youngest relative ages, which suggest no real differences in academic preparation between these two sets of students. If disparities in these longer term outcomes, like four year graduation rates, can be corrected through parity in course-taking behavior at the high school level, much more expensive approaches like delayed progression through the educational pipeline may be unnecessary.

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Table 1: Descriptive Statistics for 43 sampled states with bandwidth +/-10 days

	Youngest assigned age			Oldest assigned age		
	Obs	Mean	Std	Obs	Mean	Std
Observed age at graduation	162168	18.17	0.562	161063	18.5	0.504
Male	162168	0.46	0.498	161063	0.46	0.498
Public	162168	0.81	0.391	161063	0.81	0.392
White	162168	0.6	0.49	161063	0.6	0.49
Hispanic	162168	0.12	0.33	161063	0.12	0.33
Black	162168	0.12	0.326	161063	0.12	0.33
Family Income <$30,000	162168	0.13	0.336	161063	0.13	0.341
Family Income >$50,000	162168	0.4	0.49	161063	0.4	0.489
Parent HS Grad	162168	0.83	0.378	161063	0.82	0.381
Parent BA/BS grad	162168	0.47	0.499	161063	0.46	0.498
SAT Composite (M+CR)	162168	1014	210	161063	1018	209
AP Taker	162168	0.4	0.49	161063	0.42	0.493
Total AP exams	162168	1.25	2.113	161063	1.32	2.155
Total AP(3+) scores	162168	0.77	1.727	161063	0.81	1.759
Attends four-year college	162168	0.75	0.431	161063	0.76	0.429
Attends two-year college	162168	0.41	0.492	161063	0.41	0.492
Attends four-year college first	162168	0.65	0.477	161063	0.66	0.475
Attends two-year college first	162168	0.26	0.436	161063	0.25	0.432
Distance from home of first college (miles)	146373	195	433	145322	202	444
Average SAT of four-year college	99250	1116	128	99659	1118	128
BA/BS attainment within 4 years of HS	152660	0.29	0.452	151579	0.29	0.455
BA/BS attainment within 5 years of HS	120059	0.43	0.494	119942	0.43	0.495
BA/BS attainment within 6 years of HS	88635	0.48	0.5	89397	0.48	0.5
2-Yr College Degrees	162168	0.06	0.235	161063	0.06	0.231

Notes: Students with the youngest assigned ages are born within 10 days prior to the state-specific cutoff and those with the oldest assigned ages are born within 10 days after the cutoff. The average SAT of the four-year college indicates the average SAT or average ACT (converted to the SAT scale) of the first college (only four-year) attended by the student. There are approximately 38 AP exams in which a student could have participated. Percentage of SAT takers with <$30,000 and >$50,000 family incomes and parental education are calculated using all students, including those who did not respond to the prompt. HS graduation is assigned as June 30 for each cohort. Bachelor's attainment rates are conditional on the student having last attended a postsecondary institution reporting accurate graduation data to NSC. Students never attending a four-year institution are classified as non-bachelor's recipients. Degrees must be attained by August four, five and six years beyond high school graduation.

Table 2- Density of observations at boundary

	43 states with age-of-entry policies			17 SAT states with age-of-entry policies			14 SAT states with stable age-of-entry policies between 1991 and 1995		
Bandwidth	+/- 10	+/-30	+/-50	+/- 10	+/-30	+/-50	+/- 10	+/-30	+/-50
Boundary	-2.630	-0.164	-0.048	-5.147	-0.750	-0.101	-5.074	-0.066	0.836
	(2.799)	(1.627)	(1.270)	(5.814)	(3.392)	(2.651)	(6.855)	(3.996)	(3.126)
Distance	0.213	-0.026	0.008	0.451	-0.032	0.012	0.514	-0.034	0.021
	(0.342)	(0.065)	(0.031)	(0.711)	(0.137)	(0.063)	(0.838)	(0.160)	(0.074)
Distance x Boundary	0.069	0.057	-0.024	0.052	0.108	-0.042	0.095	0.125	-0.069
	(0.461)	(0.094)	(0.044)	(0.965)	(0.196)	(0.092)	(1.138)	(0.231)	(0.108)
Observations	6,462	18,777	30,970	2,961	8,528	14,139	2,465	7,113	11,788
R-squared	0.551	0.552	0.545	0.502	0.502	0.495	0.498	0.497	0.490

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: Robust standard errors appear in parantheses. Fixed effects for cohort state and weekend appear in all models.

Table 3- Covariate balancing of selected student characteristics (coefficients on boundary)

	43 states with age-of-entry policies			17 SAT states with age-of-entry policies			14 SAT states with stable age-of-entry policies between 1991 and 1995		
Binary outcome:	+/- 10	+/-30	+/-50	+/- 10	+/-30	+/-50	+/- 10	+/-30	+/-50
Male	-0.002	0.002	0.003*	0.004	0.004	0.005***	0.004	0.003	0.004*
	(0.004)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)
Public	0.001	0.002	-0.000	0.000	0.001	-0.000	0.000	0.001	0.001
	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)
White	0.000	0.003*	0.002	0.001	0.003	0.001	0.001	0.002	-0.002
	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.003)
Hispanic	-0.002	0.000	-0.001	-0.003	-0.000	-0.001	-0.003	-0.000	-0.001
	(0.003)	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)
Black	0.004	0.005***	0.003**	0.004	0.005***	0.003***	0.005	0.005***	0.004***
	(0.003)	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)
Family Income <$30,000	0.004	0.003**	0.002*	0.006**	0.004**	0.003**	0.005	0.004**	0.003**
	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)
Family Income >$50,000	0.001	-0.001	-0.000	0.001	0.001	0.000	0.000	0.001	-0.000
	(0.004)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)
Parent HS Grad	-0.003	-0.002	-0.001	-0.003	-0.001	-0.001	-0.002	-0.001	-0.001
	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)
Parent BA/BS grad	-0.008**	-0.009***	-0.007***	-0.008**	-0.010***	-0.007***	-0.009**	-0.010***	-0.008***
	(0.004)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors appear in parantheses. Fixed effects for cohort, state and weekend appear in all models. An interaction between *Boundary* and the forcing variable appears in all models.

Table 4 - First-stage estimates (dependent variable = observed age)

	43 states with age-of-entry policies	17 SAT states with age-of-entry policies	14 SAT states with stable age-of-entry policies between 1991 and 1995	43 states with age-of-entry policies	17 SAT states with age-of-entry policies	14 SAT states with stable age-of-entry policies between 1991 and 1995
	Cohorts 2004-2006 (Panel 1)					
Boundary	0.341***	0.363***	0.370***	0.339***	0.361***	0.369***
	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)
Covariates	NO	NO	NO	YES	YES	YES
Observations	159,923	134,155	122,595	159,923	134,155	122,595
R-squared	0.135	0.140	0.139	0.176	0.179	0.178
	Cohorts 2004-2007 (Panel 2)					
Boundary	0.339***	0.360***	0.367***	0.338***	0.358***	0.366***
	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)
Covariates	NO	NO	NO	YES	YES	YES
Observations	215,008	181,211	165,412	215,008	181,211	165,412
R-squared	0.139	0.144	0.142	0.179	0.183	0.180
	Cohorts 2004-2008 (Panel 3)					
Boundary	0.339***	0.359***	0.367***	0.339***	0.357***	0.365***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Covariates	NO	NO	NO	YES	YES	YES
Observations	270,105	228,784	209,098	270,105	228,784	209,098
R-squared	0.143	0.149	0.145	0.183	0.187	0.184

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors appear in parantheses. All regressions use a triangular kernel with an IK-estimated optimal bandwidth of 10.11. Fixed effects for cohort, state and weekend are included in all models. Covariates include high school type (e.g. public versus private); gender; race dummies, income and parental education. In graduation rate outcome analyses, students who attended institutions reporting incomplete bachelor's attainment data are removed. In the full 43 state sample from the 2004-2008 cohorts, this only impacted about 14,000 students and their removal yields identical a nearly indentical first-stage estimate of 0.341 (s.e. 0.005).

Table 5 - OLS, reduced-form, and 2SLS estimates of the impact of age on SAT Scores and AP Exams.

	Student SAT Scores			Probability of taking an AP exam			Total AP Exams			Total AP Exams (3+)		
	OLS	RF	2SLS	OLS	RF	2SLS	OLS	RF	2SLS	OLS	RF	2SLS
	Panel 1: 43 states with age-of-entry policies											
Age (Observed or Assigned)	-1.314***	0.454**	1.337**	-0.053***	0.013***	0.039***	-0.217***	0.073***	0.215***	-0.127***	0.041***	0.120***
	(0.093)	(0.178)	(0.524)	(0.002)	(0.004)	(0.013)	(0.009)	(0.019)	(0.056)	(0.007)	(0.015)	(0.045)
Observations	270,105	270,105	270,105	270,105	270,105	270,105	270,105	270,105	270,105	270,105	270,105	270,105
R-squared	0.049	0.048	0.044	0.039	0.036	0.029	0.044	0.042	0.034	0.037	0.035	0.032
	Panel 2: 43 states with age-of-entry policies with covariates											
Age (Observed or Assigned)	-3.402***	0.485***	1.432***	-0.053***	0.014***	0.041***	-0.239***	0.076***	0.224***	-0.178***	0.043***	0.128***
	(0.082)	(0.154)	(0.457)	(0.002)	(0.004)	(0.012)	(0.009)	(0.018)	(0.053)	(0.007)	(0.015)	(0.043)
Observations	270,105	270,105	270,105	270,105	270,105	270,105	270,105	270,105	270,105	270,105	270,105	270,105
R-squared	0.289	0.282	0.274	0.121	0.118	0.112	0.136	0.133	0.124	0.129	0.126	0.121
	Panel 3: 17 SAT states with age-of-entry policies											
Age (Observed or Assigned)	-1.278***	0.473**	1.319**	-0.054***	0.017***	0.048***	-0.211***	0.085***	0.236***	-0.115***	0.043***	0.120***
	(0.102)	(0.195)	(0.545)	(0.002)	(0.005)	(0.013)	(0.010)	(0.020)	(0.057)	(0.008)	(0.016)	(0.046)
Observations	228,784	228,784	228,784	228,784	228,784	228,784	228,784	228,784	228,784	228,784	228,784	228,784
R-squared	0.007	0.006	0.003	0.033	0.029	0.021	0.036	0.034	0.025	0.019	0.018	0.014
	Panel 4: 14 SAT states with stable age-of-entry policies between 1991 and 1995											
Age (Observed or Assigned)	-1.173***	0.435**	1.186**	-0.056***	0.016***	0.043***	-0.221***	0.086***	0.234***	-0.119***	0.044**	0.121**
	(0.107)	(0.206)	(0.562)	(0.002)	(0.005)	(0.013)	(0.010)	(0.022)	(0.060)	(0.008)	(0.018)	(0.048)
Observations	209,098	209,098	209,098	209,098	209,098	209,098	209,098	209,098	209,098	209,098	209,098	209,098
R-squared	0.007	0.006	0.003	0.029	0.025	0.018	0.031	0.028	0.019	0.016	0.015	0.011

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors appear in parantheses. OLS and 2SLS use observed and predicted observed age while reduced-form uses assigned age, a binary instrument indicating whether the birthdate occurs after the state-specific cutoff. Fixed effects for cohort, state and weekend are included in all models. Covariates include high school type (e.g. public versus private), gender, race dummies, income and parental education. All regressions use a triangular kernel with an IK-estimated optimal bandwidth of 10.11.

Table 6 - OLS, reduced-form, and 2SLS estimates of the impact of age on college-going rates.

	Attends a four-year college			Attends a two-year college			Attends a four-year college first conditional on attending four-year college		
	OLS	RF	2SLS	OLS	RF	2SLS	OLS	RF	2SLS
	Panel 1: 43 states with age-of-entry policies								
Age (Observed or Assigned)	-0.029***	-0.001	-0.004	-0.014***	-0.009**	-0.026**	-0.000	0.011***	0.033***
	(0.002)	(0.004)	(0.011)	(0.002)	(0.004)	(0.012)	(0.002)	(0.003)	(0.010)
Observations	270,105	270,105	270,105	270,105	270,105	270,105	203,524	203,524	203,524
R-squared	0.022	0.021	0.021	0.071	0.071	0.071	0.036	0.036	0.034
	Panel 2: 43 states with age-of-entry policies with covariates								
Age (Observed or Assigned)	-0.032***	-0.001	-0.002	-0.006***	-0.009**	-0.026**	-0.001	0.011***	0.034***
	(0.002)	(0.004)	(0.011)	(0.002)	(0.004)	(0.012)	(0.002)	(0.003)	(0.010)
Observations	270,105	270,105	270,105	270,105	270,105	270,105	203,524	203,524	203,524
R-squared	0.083	0.081	0.081	0.095	0.095	0.094	0.051	0.051	0.049
	Panel 3: 17 SAT states with age-of-entry policies								
Age (Observed or Assigned)	-0.032***	-0.002	-0.005	-0.013***	-0.010**	-0.027**	-0.000	0.013***	0.038***
	(0.002)	(0.004)	(0.012)	(0.002)	(0.005)	(0.013)	(0.002)	(0.004)	(0.011)
Observations	228,784	228,784	228,784	228,784	228,784	228,784	167,551	167,551	167,551
R-squared	0.006	0.005	0.005	0.053	0.053	0.053	0.025	0.025	0.022
	Panel 4: 14 SAT states with stable age-of-entry policies between 1991 and 1995								
Age (Observed or Assigned)	-0.032***	-0.002	-0.004	-0.014***	-0.011**	-0.031**	-0.001	0.013***	0.035***
	(0.002)	(0.004)	(0.012)	(0.002)	(0.005)	(0.013)	(0.002)	(0.004)	(0.012)
Observations	209,098	209,098	209,098	209,098	209,098	209,098	152,539	152,539	152,539
R-squared	0.006	0.005	0.005	0.049	0.049	0.049	0.022	0.022	0.019

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors appear in parantheses. OLS and 2SLS use observed and predicted observed age while reduced-form uses assigned age, a binary instrument indicating whether the birthdate occurs after the state-specific cutoff. Fixed effects for cohort and state and weekend are included in all models. Covariates include high school type (e.g. public versus private), gender, race dummies, income and parental education. All regressions use a triangular kernel with an IK-estimated optimal bandwidth of 10.11.

Table 7 - OLS, reduced-form, and 2SLS estimates of the impact of age on college-going rates.

	Average SAT score of first college			Average distance travelled to first college (miles)		
	OLS	RF	2SLS	OLS	RF	2SLS
	Panel 1: 43 states with age-of-entry policies					
Age (Observed or Assigned)	9.163***	2.566*	8.090*	20.746***	6.291	19.702
	(0.730)	(1.376)	(4.333)	(2.672)	(5.049)	(15.804)
Observations	165,494	165,494	165,494	174,320	174,320	174,320
R-squared	0.062	0.061	0.062	0.126	0.125	0.126
	Panel 2: 43 states with age-of-entry policies with covariates					
Age (Observed or Assigned)	-0.749	3.048**	9.601**	10.112***	7.371	23.065
	(0.689)	(1.269)	(4.000)	(2.718)	(4.971)	(15.551)
Observations	165,494	165,494	165,494	174,320	174,320	174,320
R-squared	0.204	0.204	0.202	0.151	0.151	0.151
	Panel 3: 17 SAT states with age-of-entry policies					
Age (Observed or Assigned)	10.655***	3.440**	10.210**	24.079***	6.687	19.670
	(0.826)	(1.575)	(4.666)	(3.011)	(5.785)	(17.009)
Observations	133,527	133,527	133,527	140,569	140,569	140,569
R-squared	0.030	0.028	0.030	0.138	0.137	0.138
	Panel 4: 14 SAT states with stable age-of-entry policies					
Age (Observed or Assigned)	11.200***	2.837*	8.261*	26.051***	6.675	19.265
	(0.880)	(1.684)	(4.896)	(3.302)	(6.358)	(18.341)
Observations	120,335	120,335	120,335	126,917	126,917	126,917
R-squared	0.023	0.021	0.023	0.133	0.132	0.133

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors appear in parantheses. OLS and 2SLS use observed and predicted observed age while reduced-form uses assigned age, a binary instrument indicating whether the birthdate occurs after the state-specific cutoff. Fixed effects for cohort and state and weekend are included in all models. Covariates include high school type (e.g. public versus private), gender, race dummies, income and parental education. All regressions use a triangular kernel with an IK-estimated optimal bandwidth of 10.11. SAT scores of first college are restricted to students first attending a four-year college.

Table 8 -OLS, reduced-form, and 2SLS estimates of the impact of age on degree attainment rates.

	Completion of bachelor's degree within 4 years of HS graduation			Completion of bachelor's degree within 5 years of HS graduation			Completion of bachelor's degree within 6 years of HS graduation			Completion of 2-yr college degree		
	OLS	RF	2SLS	OLS	RF	2SLS	OLS	RF	2SLS	OLS	RF	2SLS
Panel 1: 43 states with age-of-entry policies												
Age (Observed or Assigned)	0.007***	0.008*	0.023*	-0.004	0.004	0.012	-0.015***	-0.001	-0.004	-0.002**	-0.004*	-0.011*
	(0.002)	(0.004)	(0.012)	(0.002)	(0.005)	(0.014)	(0.003)	(0.006)	(0.017)	(0.001)	(0.002)	(0.006)
Observations	256,346	256,346	256,346	202,652	202,652	202,652	149,925	149,925	149,925	270,105	270,105	270,105
R-squared	0.032	0.032	0.032	0.030	0.030	0.030	0.028	0.027	0.027	0.019	0.019	0.019
Panel 2: 43 states with age-of-entry policies with covariates												
Age (Observed or Assigned)	0.005***	0.009**	0.025**	-0.009***	0.006	0.019	-0.019***	0.001	0.003	-0.001	-0.004*	-0.011*
	(0.002)	(0.004)	(0.011)	(0.002)	(0.005)	(0.014)	(0.003)	(0.005)	(0.016)	(0.001)	(0.002)	(0.006)
Observations	256,346	256,346	256,346	202,652	202,652	202,652	149,925	149,925	149,925	270,105	270,105	270,105
R-squared	0.115	0.115	0.115	0.126	0.126	0.125	0.121	0.121	0.121	0.028	0.028	0.027
Panel 3: 17 SAT states with age-of-entry policies												
Age (Observed or Assigned)	0.007***	0.008*	0.022*	-0.005**	0.005	0.013	-0.018***	-0.002	-0.006	-0.001	-0.004*	-0.012*
	(0.002)	(0.004)	(0.012)	(0.003)	(0.005)	(0.015)	(0.003)	(0.006)	(0.017)	(0.001)	(0.002)	(0.006)
Observations	216,950	216,950	216,950	170,584	170,584	170,584	125,651	125,651	125,651	228,784	228,784	228,784
R-squared	0.012	0.012	0.012	0.008	0.008	0.008	0.007	0.007	0.007	0.026	0.026	0.026
Panel 4: 14 SAT states with stable age-of-entry policies between 1991 and 1995												
Age (Observed or Assigned)	0.008***	0.007*	0.020*	-0.006**	0.006	0.016	-0.017***	-0.002	-0.005	-0.002	-0.004	-0.010
	(0.002)	(0.004)	(0.012)	(0.003)	(0.006)	(0.015)	(0.003)	(0.007)	(0.018)	(0.001)	(0.002)	(0.007)
Observations	199,726	199,726	199,726	157,104	157,104	157,104	116,229	116,229	116,229	209,098	209,098	209,098
R-squared	0.013	0.013	0.013	0.009	0.009	0.008	0.007	0.006	0.006	0.018	0.018	0.017

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: Robust standard errors appear in parantheses. OLS and 2SLS use observed and predicted observed age while reduced-form uses assigned age, a binary instrument indicating whether the birthdate occurs after the state-specific cutoff. Fixed effects for cohort and state and weekend are included in all models. Covariates include high school type (e.g. public versus private), gender, race dummies, income and parental education. All regressions use a triangular kernel with an IK-estimated optimal bandwidth of 10.11. Students never attending a four-year institution are classified as non-bachelor's recipients. Degree attainment is by August four, five and six years beyond high school graduation. Six-year bachelor's attainment rates are estimated for the 2004-2006 cohorts only. Five-year bachelor's attainment rates are estimated for the 2004-2007 cohorts only. The completion of a two-year college degree measure tracks students through 2012. Bachelor's attainment rates are conditional on the student having last attended a postsecondary institution reporting accurate graduation data to NSC.

Table 9 - Heterogeneous 2SLS estimates of the impact of age on scores, college-choice and completion.

	Student SAT Scores	Probability of taking an AP exam	Total AP Exams	Total AP Exams (3+)	Attends a four-year college	Attends a two-year college	Attends a four-year college first | attending a four-year college	Average SAT score of first college	Average distance travelled to first college (miles)	Completion of bachelor's degree within 4 years of HS graduation	Completion of bachelor's degree within 5 years of HS graduation	Completion of bachelor's degree within 6 years of HS graduation	Completion of associate's degree
Low Compliance	1.848	0.029	0.418***	0.297**	-0.002	0.010	0.019	-0.634	76.357**	0.031	0.006	-0.012	-0.020
	(1.640)	(0.039)	(0.162)	(0.139)	(0.032)	(0.037)	(0.025)	(12.091)	(31.394)	(0.040)	(0.046)	(0.053)	(0.016)
Observations	72,341	72,341	72,341	72,341	72,341	72,341	58,093	50,256	65,161	69,061	55,281	40,934	72,341
High Compliance	1.238**	0.041***	0.176***	0.085*	-0.005	-0.033**	0.036***	10.120**	13.446	0.021*	0.013	-0.002	-0.009
	(0.539)	(0.013)	(0.059)	(0.047)	(0.012)	(0.013)	(0.012)	(4.541)	(12.780)	(0.011)	(0.015)	(0.017)	(0.006)
Observations	197,764	197,764	197,764	197,764	197,764	197,764	145,431	115,238	179,189	187,285	147,371	108,991	197,764
Male	-0.088	-0.003	0.121	0.046	-0.011	-0.040*	0.023	2.554	30.064	0.009	0.012	0.000	-0.014
	(0.974)	(0.023)	(0.105)	(0.088)	(0.020)	(0.022)	(0.019)	(8.166)	(21.455)	(0.020)	(0.026)	(0.031)	(0.010)
Observations	123,969	123,969	123,969	123,969	123,969	123,969	91,917	74,489	111,149	117,817	93,187	68,973	123,969
Female	2.104***	0.066***	0.274***	0.163***	0.001	-0.017	0.039***	11.527**	20.930	0.031**	0.014	-0.003	-0.009
	(0.600)	(0.015)	(0.063)	(0.050)	(0.013)	(0.014)	(0.012)	(4.972)	(14.062)	(0.014)	(0.017)	(0.020)	(0.007)
Observations	145,884	145,884	145,884	145,884	145,884	145,884	111,490	90,919	133,018	138,282	109,285	80,952	145,884
Public	1.279**	0.048***	0.239***	0.121***	-0.001	-0.028**	0.034***	5.134	21.540*	0.028**	0.018	-0.001	-0.011*
	(0.534)	(0.013)	(0.058)	(0.047)	(0.011)	(0.013)	(0.011)	(4.379)	(11.230)	(0.012)	(0.015)	(0.017)	(0.006)
Observations	219,963	219,963	219,963	219,963	219,963	219,963	164,313	132,367	199,160	208,893	164,579	120,584	219,963
White	0.835	0.038*	0.209**	0.139*	-0.010	-0.026	0.035**	4.163	29.956*	0.026	0.015	-0.020	-0.010
	(0.742)	(0.020)	(0.087)	(0.074)	(0.016)	(0.019)	(0.015)	(5.837)	(17.642)	(0.019)	(0.023)	(0.026)	(0.009)
Observations	158,163	158,163	158,163	158,163	158,163	158,163	125,927	105,823	145,859	149,280	118,895	88,545	158,163
R-squared	0.049	0.034	0.044	0.040	0.021	0.069	0.035	0.078	0.091	0.031	0.030	0.029	0.018
Hispanic	2.818***	0.042*	0.268***	0.142**	0.003	-0.018	0.059**	28.250***	-9.358	0.038**	0.039	0.061**	-0.029**
	(0.902)	(0.024)	(0.101)	(0.064)	(0.024)	(0.024)	(0.026)	(9.656)	(20.778)	(0.017)	(0.025)	(0.030)	(0.013)
Observations	36,315	36,315	36,315	36,315	36,315	36,315	22,498	16,343	31,262	34,967	26,737	19,153	36,315
Black	2.030**	0.050**	0.276***	0.102**	0.015	-0.021	-0.014	10.982	33.190	0.008	-0.008	-0.055	0.003
	(1.032)	(0.025)	(0.080)	(0.047)	(0.026)	(0.028)	(0.024)	(11.113)	(23.594)	(0.021)	(0.029)	(0.035)	(0.013)
Observations	33,686	33,686	33,686	33,686	33,686	33,686	23,654	18,257	29,878	31,980	24,871	18,301	33,686

Notes: Robust standard errors appear in parantheses. Fixed effects for cohort and state and weekend are included in all models. All regressions use a triangular kernel with an IK-estimated optimal bandwidth of 10.11. Low compliance states include those with first-stage parameter estimates at or below the median of 0.257 and include: MO, OK, ID, WI, KY, CT, MT, TN, LA, AL, MI, NH, NE, AZ, VA, SC, ME, OH, IA, WY, RI and KS. High compliance states include: UT, WV, AR, DE, IN, AK, IL, NV, NM, OR, CA, NC, MN, FL, TX, GA, MD, MS, SD, HI, ND.

Figure 1: Sample size, by distance from state-specific date of birth threshold

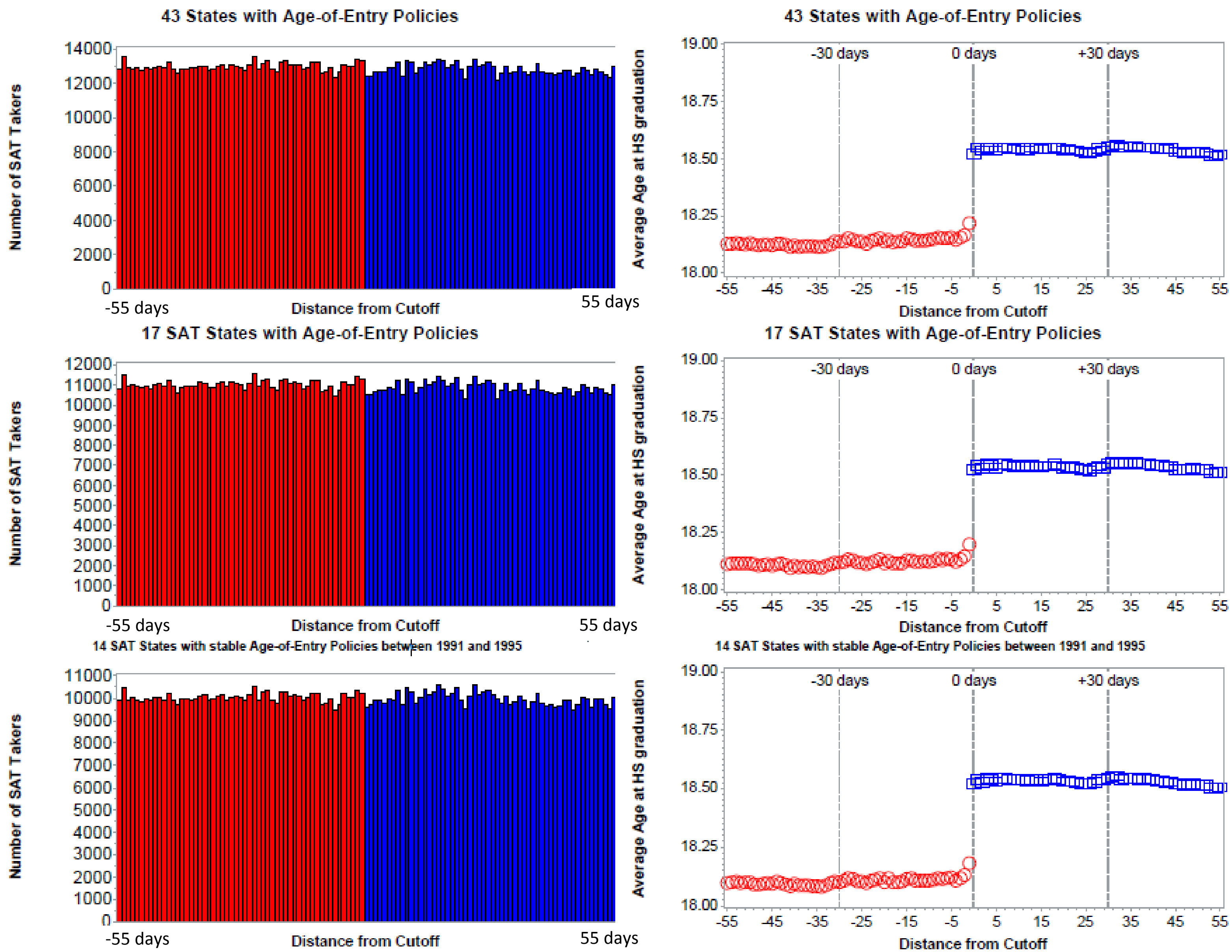
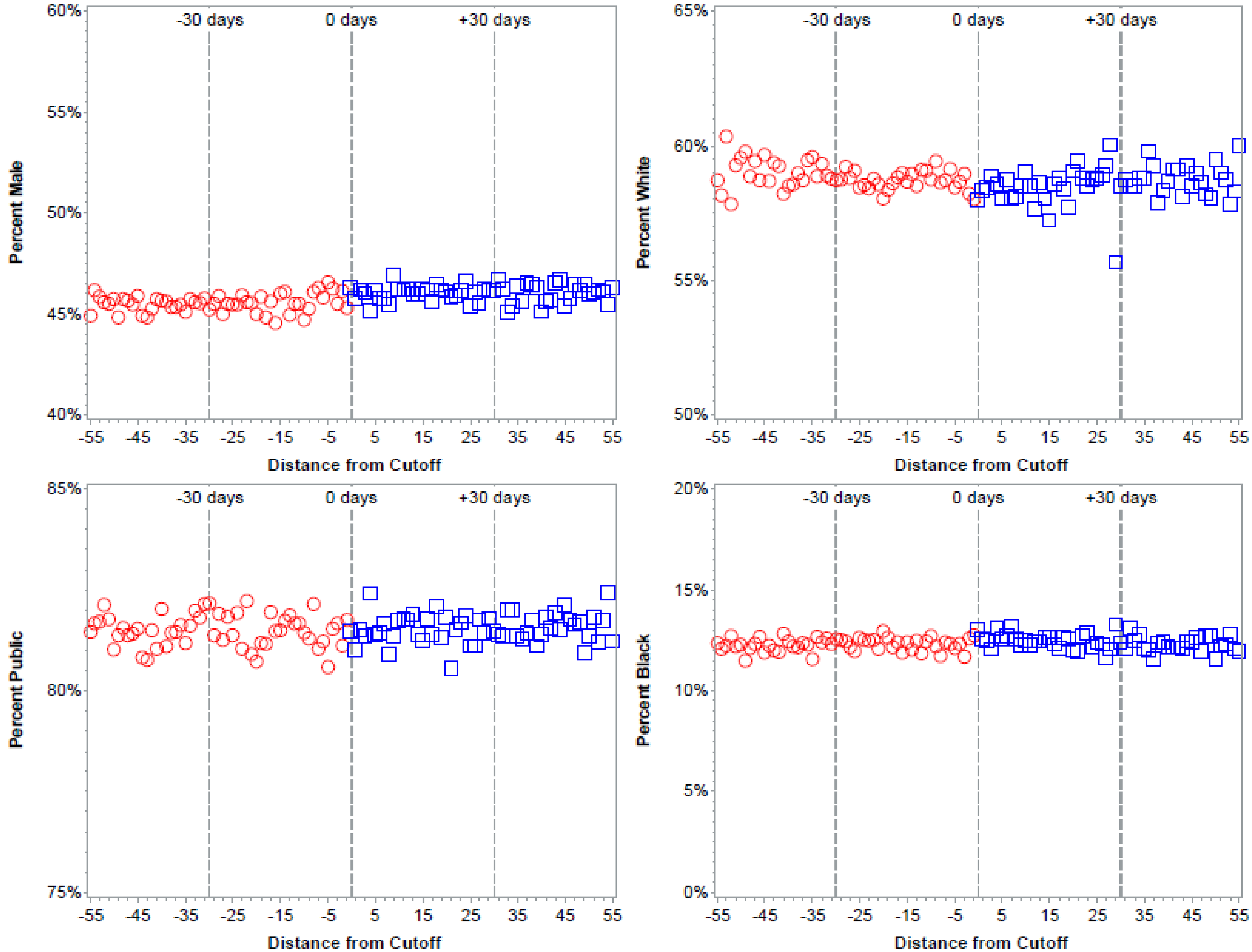
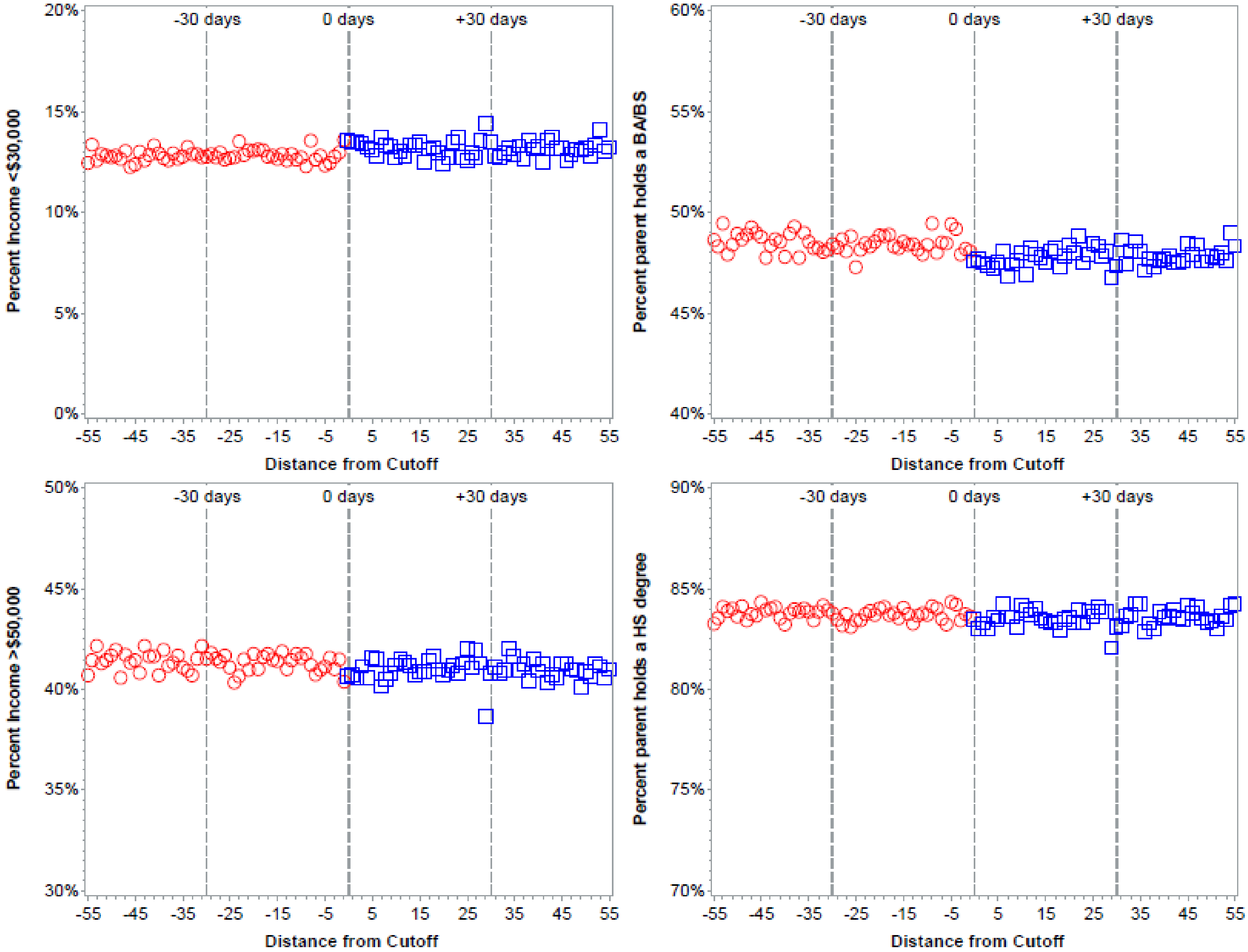


Figure 2: Percentage of sampled students in selected covariate categories, by distance from state-specific age-of-entry cutoff date.





Percent Income <$30,000
20%
15%
10%
5%
0%
-30 days
0 days
+30 days
-55 -45 -35 -25 -15 -5 5 15 25 35 45 55
Distance from Cutoff
Percent parent holds a BA/BS
60%
55%
50%
45%
40%
-30 days
0 days
+30 days
-55 -45 -35 -25 -15 -5 5 15 25 35 45 55
Distance from Cutoff
Percent Income >$50,000
50%
45%
40%
35%
30%
-30 days
0 days
+30 days
-55 -45 -35 -25 -15 -5 5 15 25 35 45 55
Distance from Cutoff
Percent parent holds a HS degree
90%
85%
80%
75%
70%
-30 days
0 days
+30 days
-55 -45 -35 -25 -15 -5 5 15 25 35 45 55
Distance from Cutoff

Appendix Table A: State level school age-of-entry dates, age discontinuities and corresponding F-statistics (for students entering Kindergarten between 1991 and 1995)

	Policy	+/- 10 days		+/- 30 days		+/- 50 days		
		Estimate	F-stat	Estimate	F-stat	Estimate	F-stat	SAT State
AL	Sep 1 (on or before)	0.21	2.387	0.33	7.41	0.342	10.362	
AK	Aug 15 (on or before)	0.312	4.165	0.32	7.623	0.314	9.764	X
AZ	Sep 1 (before)	0.224	7.016	0.301	16.679	0.324	23.518	
AR	Oct 1 (on or before)	0.279	2.352	0.307	4.751	0.312	6.313	
CA	Dec 2 (on or before)	0.359	34.818	0.415	72.148	0.417	95.27	X
CO	LEA choice							
CT	Jan 1 (on or before)	0.174	7.072	0.275	20.182	0.291	27.714	X
DE	Varies over years	0.296	4.762	0.247	6.714	0.203	7.177	X
FL	Sep 1 (on or before)	0.406	27.857	0.429	52.223	0.429	67.998	X
GA	Sep 1 (on or before)	0.429	23.229	0.452	43.334	0.447	56.085	X
HI	Dec 31 (on or before)	0.537	11.732	0.556	22.101	0.548	28.803	X
ID	Varies over years	0.081	0.944	0.159	3.39	0.209	5.878	
IL	Sep 1 (on or before)	0.323	7.856	0.369	16.344	0.385	22.432	
IN	Varies over years	0.305	14.309	0.326	26.552	0.332	35.476	X
IA	Sep 15 (on or before)	0.241	2.168	0.194	3.208	0.195	4.275	
KS	Varies over years	0.257	3.11	0.285	6.107	0.305	8.62	
KY	Oct 1 (on or before)	0.16	2.553	0.237	6.658	0.241	8.764	
LA	Dec 31 (on or before)	0.186	2.457	0.131	3.293	0.119	4.099	
ME	Oct 15 (on or before)	0.234	5.932	0.283	13.044	0.314	18.829	X
MD	Dec 31 (on or before)	0.429	18.507	0.475	38.292	0.482	51.393	X
MA	LEA choice							X
MI	Dec 1 (on or before)	0.214	5.163	0.249	10.196	0.242	12.889	
MN	Sep 1 (on or before)	0.384	6.623	0.318	9.672	0.323	12.893	
MS	Sep 1 (on or before)	0.453	3.335	0.426	5.271	0.452	7.396	
MO	Jul 1 (on or before)	0.004	0.059	0.137	3.201	0.136	4.141	
MT	Sep 10 (on or before)	0.177	2.234	0.117	2.766	0.158	4.836	
NE	Oct 15 (on or before)	0.217	2.243	0.11	1.946	0.112	2.557	
NV	Sep 30 (on or before)	0.333	6.905	0.373	13.425	0.348	16.116	
NH	Sep 30 (on or before)	0.216	5.097	0.29	12.601	0.289	16.35	X
NJ	LEA choice							X
NM	Sep 1 (before)	0.352	3.982	0.399	7.791	0.399	10.114	
NY	LEA choice							X
NC	Oct 16 (on or before)	0.38	19.236	0.363	33.242	0.382	45.773	X
ND	Aug 31 (on or before)	0.891	3.939	0.136	0.917	0.174	1.385	
OH	Sep 30 (on or before)	0.238	11.16	0.262	21.871	0.255	27.756	
OK	Varies over years	0.042	0.69	0.023	0.628	0.04	1.341	
OR	Sep 1 (on or before)	0.358	10.699	0.374	20.213	0.398	28.278	X
PA	LEA choice							X
RI	Dec 31 (on or before)	0.255	4.791	0.316	10.878	0.343	15.542	X
SC	Varies over years	0.227	7.239	0.241	14.206	0.245	18.911	X
SD	Sep 1 (on or before)	0.47	1.668	0.45	2.833	0.423	3.441	
TN	Sep 30 (on or before)	0.185	3.564	0.231	8.126	0.199	9.067	
TX	Sep 1 (on or before)	0.427	34.721	0.468	68.455	0.472	90.532	X
UT	Sep 2 (on or before)	0.268	2.59	0.325	5.66	0.328	7.321	
VT	LEA choice							X
VA	Sep 30 (on or before)	0.226	11.277	0.238	21.205	0.22	25.351	X
WA	LEA choice							X
WV	Sep 1 (before)	0.268	3.604	0.363	8.922	0.375	12.088	
WI	Sep 1 (on or before)	0.132	1.796	0.265	6.566	0.307	10.043	
WY	Sep 15 (on or before)	0.252	1.401	0.071	0.547	0.134	1.377	

Notes: Age of entry policies are collected from Elder and Lubotsky (2009), Bedard and Dhuey (2007) and Colasanti (2011). Discontinuities in age are obtained by regressing a student's age at graduation on the forcing variable, an indicator variable for whether the student's birthday falls on or after threshold and an interaction of these two terms. All regressions use a triangular kernel and include fixed effects for cohort, state and weekend. LEA choice means that the age-of-entry policies are established by districts or local education agencies. Age at high school graduation is calculated as the difference between June 30 on the student's year of high school graduation and the student's date of birth.

Appendix Table B - First-stage estimates (dependent variable = observed age)

	Bandwidth:		
	+/-10.11	+/-30 days	+/-50 days
	Triangular kernel		
Boundary *(Linear forcing variable)*	0.339***	0.376***	0.379***
	(0.005)	(0.003)	(0.002)
R-squared	0.143	0.156	0.164
	Rectangular kernel		
Boundary *(Linear forcing variable)*	0.354***	0.384***	0.386***
	(0.004)	(0.002)	(0.002)
R-squared	0.150	0.160	0.175
Boundary *(fourth-order forcing variable)*	0.227***	0.324***	0.340***
	(0.017)	(0.006)	(0.005)
R-squared	0.151	0.161	0.175
Observations	270,105	791,237	1,303,247

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors appear in parantheses. Fixed effects for cohort and state and weekend are included in all models. Regressions include students from 43 sampled states with age-of-entry policies between 1991 and 1995.

Appendix Table C - Robustness checks for reduced-form, and 2SLS estimates of the impact of age on various outcomes. (rectangular kernel)

	Student SAT Scores (M+CR)						Probability of Taking an AP					
	+/-10.11 days		+/-30 days		+/-50 days		+/-10.11 days		+/-30 days		+/-50 days	
	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS
Age at HS graduation *(Linear forcing variable)*	0.344**	0.970**	0.378***	0.985***	0.484***	1.255***	0.018***	0.050***	0.019***	0.048***	0.019***	0.049***
	(0.159)	(0.448)	(0.092)	(0.240)	(0.071)	(0.185)	(0.004)	(0.011)	(0.002)	(0.006)	(0.002)	(0.004)
R-squared	0.048	0.046	0.048	0.046	0.048	0.045	0.036	0.028	0.035	0.027	0.035	0.027
Age at HS graduation *(Fourth-order forcing variable)*	1.312**	5.786**	0.563**	1.736**	0.364**	1.071**	0.023	0.101	0.015***	0.047***	0.013***	0.039***
	(0.613)	(2.732)	(0.242)	(0.749)	(0.182)	(0.535)	(0.015)	(0.065)	(0.006)	(0.018)	(0.004)	(0.013)
R-squared	0.048	0.018	0.048	0.044	0.048	0.046	0.036	0.013	0.035	0.028	0.035	0.029
Observations	270,105	270,105	791,237	791,237	1,303,247	1,303,247	270,105	270,105	791,237	791,237	1,303,247	1,303,247
	Total AP exams						Total AP exams (3+)					
Age at HS graduation *(Linear forcing variable)*	0.079***	0.224***	0.072***	0.189***	0.077***	0.200***	0.042***	0.118***	0.037***	0.098***	0.040***	0.105***
	(0.017)	(0.048)	(0.010)	(0.026)	(0.008)	(0.020)	(0.014)	(0.039)	(0.008)	(0.021)	(0.006)	(0.016)
R-squared	0.042	0.033	0.042	0.036	0.042	0.035	0.035	0.031	0.036	0.033	0.036	0.033
Age at HS graduation *(Fourth-order forcing variable)*	0.119*	0.525*	0.090***	0.279***	0.069***	0.202***	0.065	0.289	0.051**	0.156**	0.032**	0.093**
	(0.064)	(0.288)	(0.026)	(0.079)	(0.019)	(0.057)	(0.053)	(0.233)	(0.021)	(0.065)	(0.016)	(0.046)
R-squared	0.042	0.014	0.042	0.031	0.042	0.035	0.035	0.022	0.036	0.031	0.036	0.034
Observations	270,105	270,105	791,237	791,237	1,303,247	1,303,247	270,105	270,105	791,237	791,237	1,303,247	1,303,247
	Attends a four-year college						Attends a two-year college					
Age at HS graduation *(Linear forcing variable)*	-0.001	-0.003	0.003*	0.009*	0.005***	0.012***	-0.010***	-0.027***	-0.006***	-0.016***	-0.005***	-0.014***
	(0.003)	(0.009)	(0.002)	(0.005)	(0.001)	(0.004)	(0.004)	(0.010)	(0.002)	(0.006)	(0.002)	(0.004)
R-squared	0.020	0.020	0.020	0.019	0.020	0.019	0.070	0.070	0.069	0.069	0.069	0.069
Age at HS graduation *(Fourth-order forcing variable)*	0.005	0.022	-0.005	-0.016	-0.002	-0.005	-0.001	-0.003	-0.010*	-0.032*	-0.008*	-0.024*
	(0.013)	(0.057)	(0.005)	(0.016)	(0.004)	(0.011)	(0.014)	(0.063)	(0.006)	(0.018)	(0.004)	(0.013)
R-squared	0.020	0.017	0.020	0.021	0.020	0.020	0.070	0.070	0.069	0.069	0.069	0.069
Observations	270,105	270,105	791,237	791,237	1,303,247	1,303,247	270,105	270,105	791,237	791,237	1,303,247	1,303,247

Appendix Table C - Continued

	Attends a four-year college first conditonal on attending four-year college						Average SAT score (M+CR) of first college					
	+/-10.11 days		+/-30 days		+/-50 days		+/-10.11 days		+/-30 days		+/-50 days	
	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS
Age at HS graduation *(Linear forcing variable)*	0.012***	0.034***	0.007***	0.018***	0.008***	0.020***	3.178***	9.632***	2.907***	8.071***	3.187***	8.755***
	(0.003)	(0.009)	(0.002)	(0.005)	(0.001)	(0.004)	(1.234)	(3.734)	(0.717)	(1.988)	(0.556)	(1.526)
R-squared	0.035	0.033	0.035	0.035	0.035	0.034	0.060	0.062	0.061	0.062	0.061	0.062
Age at HS graduation *(Fourth-order forcing variable)*	0.003	0.012	0.014***	0.044***	0.012***	0.037***	8.618*	39.697*	3.254*	10.773*	1.927	6.122
	(0.012)	(0.056)	(0.005)	(0.015)	(0.004)	(0.011)	(4.745)	(21.887)	(1.876)	(6.201)	(1.412)	(4.482)
R-squared	0.035	0.035	0.035	0.031	0.035	0.033	0.060	0.048	0.061	0.062	0.061	0.062
Observations	203,524	203,524	596,843	596,843	983,087	983,087	165,494	165,494	484,844	484,844	799,029	799,029
	Average distance travelled to first college (miles)						Completion of bachelor's degree within 4 years of HS graduation					
Age at HS graduation *(Linear forcing variable)*	8.418**	23.799**	7.385***	19.202***	7.236***	18.669***	0.007*	0.018*	0.007***	0.018***	0.007***	0.019***
	(3.603)	(10.181)	(2.104)	(5.469)	(1.635)	(4.216)	(0.004)	(0.010)	(0.002)	(0.005)	(0.002)	(0.004)
R-squared	0.093	0.094	0.091	0.092	0.092	0.092	0.032	0.032	0.032	0.032	0.032	0.031
Age at HS graduation *(Fourth-order forcing variable)*	-6.083	-27.512	7.550	23.339	5.106	15.070	0.027**	0.118**	0.004	0.013	0.008**	0.024**
	(13.771)	(62.413)	(5.476)	(16.920)	(4.123)	(12.165)	(0.014)	(0.059)	(0.005)	(0.016)	(0.004)	(0.012)
R-squared	0.093	0.091	0.091	0.092	0.092	0.092	0.032	0.016	0.032	0.032	0.032	0.031
Observations	244,350	244,350	716,381	716,381	1,179,861	1,179,861	256,346	256,346	751,080	751,080	1,237,044	1,237,044
	Completion of bachelor's degree within 5 years of HS graduation						Completion of bachelor's degree within 6 years of HS graduation					
Age at HS graduation *(Linear forcing variable)*	0.005	0.013	0.005**	0.013**	0.007***	0.018***	-0.000	-0.001	0.003	0.007	0.005**	0.012**
	(0.004)	(0.012)	(0.003)	(0.007)	(0.002)	(0.005)	(0.005)	(0.015)	(0.003)	(0.008)	(0.002)	(0.006)
R-squared	0.030	0.030	0.029	0.029	0.029	0.028	0.027	0.027	0.026	0.026	0.026	0.026
Age at HS graduation *(Fourth-order forcing variable)*	0.035**	0.155**	0.002	0.006	0.004	0.011	0.026	0.119	-0.004	-0.011	-0.002	-0.007
	(0.017)	(0.076)	(0.007)	(0.020)	(0.005)	(0.015)	(0.020)	(0.093)	(0.008)	(0.023)	(0.006)	(0.017)
R-squared	0.031	0.003	0.029	0.029	0.029	0.029	0.027	0.008	0.026	0.026	0.026	0.026
Observations	202,652	202,652	592,960	592,960	975,927	975,927	149,925	149,925	439,114	439,114	722,127	722,127
	Completion of associate's degree											
Age at HS graduation *(Linear forcing variable)*	-0.003*	-0.009*	-0.002*	-0.005*	-0.003***	-0.007***						
	(0.002)	(0.005)	(0.001)	(0.003)	(0.001)	(0.002)						
R-squared	0.018	0.018	0.019	0.019	0.019	0.019						
Age at HS graduation *(Fourth-order forcing variable)*	-0.008	-0.033	-0.005*	-0.016*	-0.003	-0.010						
	(0.007)	(0.031)	(0.003)	(0.009)	(0.002)	(0.006)						
R-squared	0.018	0.014	0.019	0.018	0.019	0.019						
Observations	270,105	270,105	791,237	791,237	1,303,247	1,303,247						

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors, clustered by distance from boundary, appear in parantheses. Fixed effects for cohort and state and weekend are included in all models. A rectangular kernel is used in all models. 2SLS estimates use predicted observed age while reduced-form uses assigned age, a binary instrument indicating whether the birthdate occurs after the state-specific cutoff. Regressions include students from 43 sampled states with age-of-entry policies between 1991 and 1995.

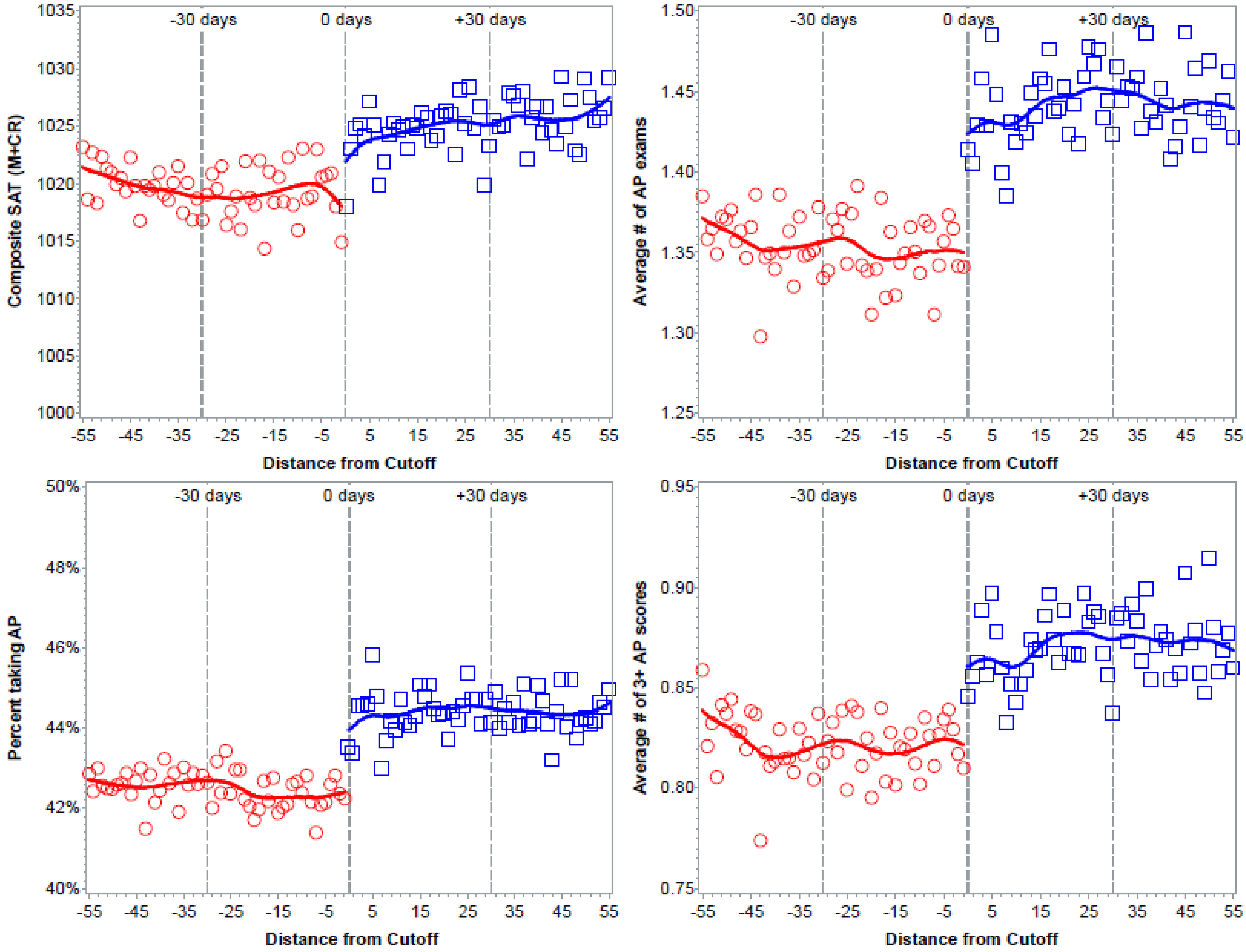
Appendix Table D - Robustness checks for reduced-form, and 2SLS estimates of the impact of age on various outcomes. (triangular kernel)

	Student SAT Scores						Probability of Taking an AP					
	+/-10.11 days		+/-30 days		+/-50 days		+/-10.11 days		+/-30 days		+/-50 days	
	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS
Age at HS graduation	0.454**	1.337**	0.321***	0.856***	0.403***	1.064***	0.013***	0.039***	0.017***	0.046***	0.018***	0.049***
	(0.178)	(0.524)	(0.102)	(0.271)	(0.079)	(0.208)	(0.004)	(0.013)	(0.002)	(0.006)	(0.002)	(0.005)
R-squared	0.048	0.044	0.048	0.046	0.048	0.046	0.036	0.029	0.036	0.028	0.035	0.027
Observations	270,105	270,105	765,583	765,583	1,277,969	1,277,969	270,105	270,105	765,583	765,583	1,277,969	1,277,969
	Total AP exams						Total AP exams (3+)					
Age at HS graduation	0.073***	0.215***	0.070***	0.186***	0.073***	0.192***	0.041***	0.120***	0.033***	0.087***	0.036***	0.094***
	(0.019)	(0.056)	(0.011)	(0.029)	(0.008)	(0.022)	(0.015)	(0.045)	(0.009)	(0.024)	(0.007)	(0.018)
R-squared	0.042	0.034	0.042	0.036	0.042	0.035	0.035	0.032	0.036	0.034	0.036	0.033
Observations	270,105	270,105	765,583	765,583	1,277,969	1,277,969	270,105	270,105	765,583	765,583	1,277,969	1,277,969
	Attends a four-year college						Attends a two-year college					
Age at HS graduation	-0.001	-0.004	0.002	0.005	0.004**	0.010**	-0.009**	-0.026**	-0.007***	-0.019***	-0.006***	-0.015***
	(0.004)	(0.011)	(0.002)	(0.006)	(0.002)	(0.004)	(0.004)	(0.012)	(0.002)	(0.006)	(0.002)	(0.005)
R-squared	0.021	0.021	0.020	0.020	0.020	0.019	0.071	0.071	0.069	0.069	0.069	0.069
Observations	270,105	270,105	765,583	765,583	1,277,969	1,277,969	270,105	270,105	765,583	765,583	1,277,969	1,277,969
	Attends a four-year college first conditonal on attending four-year college						Average SAT score of first college					
Age at HS graduation	0.011***	0.033***	0.009***	0.024***	0.007***	0.020***	2.566*	8.090*	2.522***	7.181***	2.983***	8.382***
	(0.003)	(0.010)	(0.002)	(0.005)	(0.002)	(0.004)	(1.376)	(4.333)	(0.791)	(2.251)	(0.612)	(1.719)
R-squared	0.036	0.034	0.035	0.034	0.035	0.035	0.061	0.062	0.061	0.062	0.061	0.062
Observations	203,524	203,524	577,486	577,486	963,903	963,903	165,494	165,494	469,210	469,210	783,400	783,400
	Average distance travelled to first college (miles)						Completion of bachelor's degree within 4 years of HS graduation					
Age at HS graduation	8.063**	23.781**	7.718***	20.541***	7.820***	20.564***	0.008*	0.023*	0.008***	0.020***	0.007***	0.017***
	(4.019)	(11.847)	(2.320)	(6.171)	(1.798)	(4.727)	(0.004)	(0.012)	(0.002)	(0.006)	(0.002)	(0.005)
R-squared	0.094	0.094	0.091	0.092	0.091	0.092	0.032	0.032	0.032	0.032	0.032	0.032
Observations	244,350	244,350	693,192	693,192	1,157,013	1,157,013	256,346	256,346	726,726	726,726	1,213,127	1,213,127
	Completion of bachelor's degree within 5 years of HS graduation						Completion of bachelor's degree within 6 years of HS graduation					
Age at HS graduation	0.004	0.012	0.005*	0.013*	0.005**	0.015**	-0.001	-0.004	0.001	0.003	0.003	0.007
	(0.005)	(0.014)	(0.003)	(0.008)	(0.002)	(0.006)	(0.006)	(0.017)	(0.003)	(0.009)	(0.003)	(0.007)
R-squared	0.030	0.030	0.030	0.029	0.029	0.029	0.027	0.027	0.026	0.026	0.026	0.026
Observations	202,652	202,652	573,944	573,944	956,685	956,685	149,925	149,925	424,995	424,995	707,516	707,516
	Completion of associate's degree											
Age at HS graduation	-0.004*	-0.011*	-0.003**	-0.007**	-0.002***	-0.007***						
	(0.002)	(0.006)	(0.001)	(0.003)	(0.001)	(0.002)						
R-squared	0.019	0.019	0.019	0.019	0.019	0.019						
Observations	270,105	270,105	765,583	765,583	1,277,969	1,277,969						

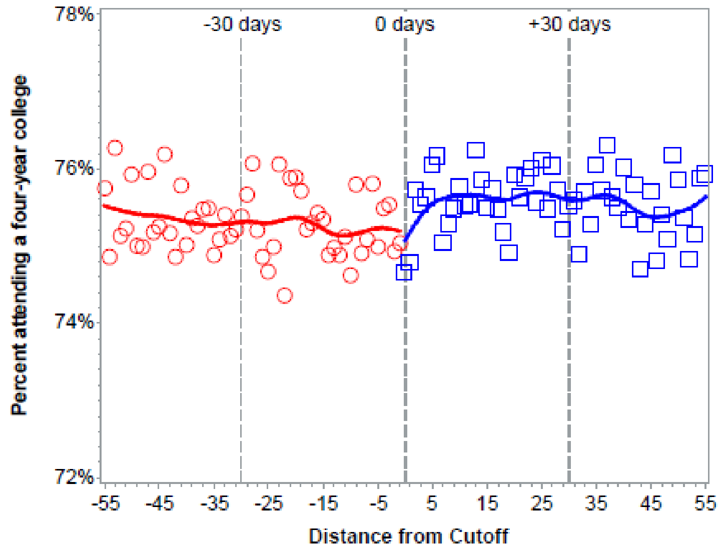
*** p<0.01, ** p<0.05, * p<0.1

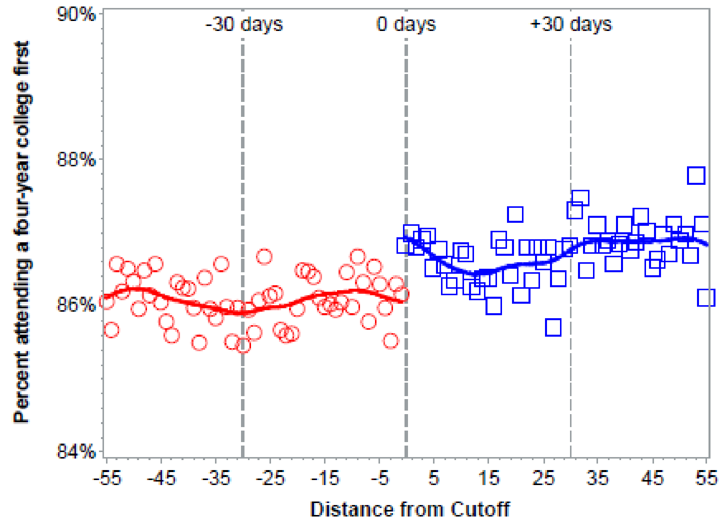
Notes: Robust standard errors appear in parantheses. Fixed effects for cohort and state and weekend are included in all models. A triangular kernel is used in all models. 2SLS estimates use predicted observed age while reduced-form uses assigned age, a binary instrument indicating whether the birthdate occurs after the state-specific cutoff. Regressions include students from 43 sampled states with age-of-entry policies between 1991 and 1995.

Appendix Figures A: Standardized test taking and performance, by distance from state-specific age-of-entry cutoff date.

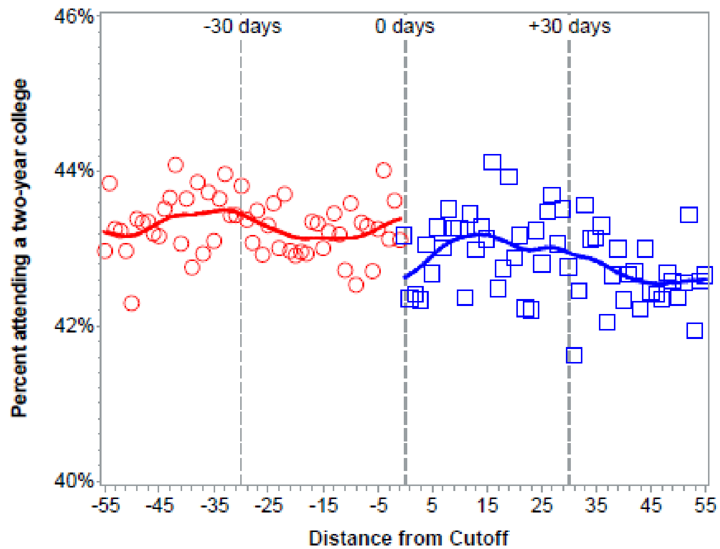


Appendix Figures B: Two- and four-year college-going rates, by distance from state-specific age-of-entry cutoff date.

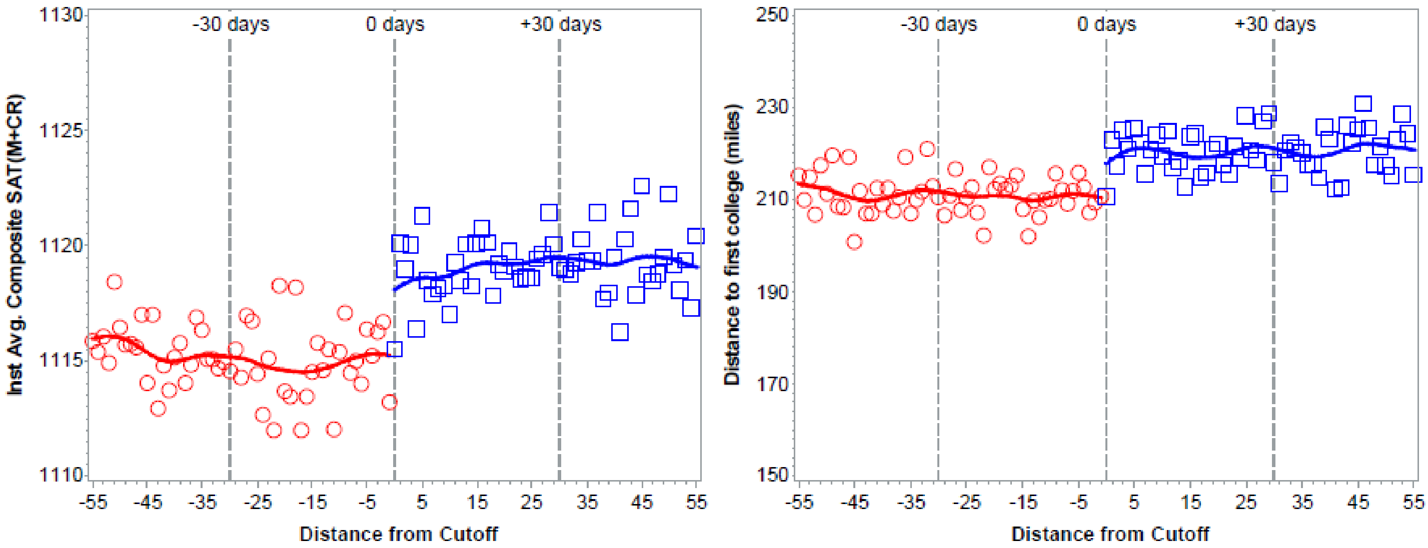




*Conditional on ever attending a four-year college



Appendix Figures C: College choice, by distance from state-specific age-of-entry cutoff date.



Appendix Figures D: Bachelor's completion, by distance from state-specific age-of-entry cutoff date.