Should We Care About the Age at Which Children Enter School? The Impact of School Entry Laws on Educational Attainment and Labor Market Outcomes*

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This paper examines how state laws regulating the age at which students can enter school effect children's progression through school, adult educational attainment and labor market outcomes. Using exact day of birth from the 2000 Long Form Decennial Census for the states of California and Texas we first show that students born right before the cut off date for school enrollment are 50-60 percentage points more likely to enroll in kindergarten a year earlier than similar students that were born right after the cut off date. This effect is significantly larger for minorities and the children of parents with less than a high school education. We also find that almost one third of the initial differences in enrolment rates disappear by 9th grade since the youngest children in a cohort are considerably more likely to be held back a grade during elementary school. Despite the striking differences in the timing of enrollment and the rates at which students are held back, we find only a modest impact on adult educational attainment, with individuals that enter school a year earlier having only an approximately one percentage point increase in the probability of completing high school We find slightly larger effects for Hispanics and older cohorts. However, the additional education resulting from early school entry does not result in differences in employment rates or wages. This may be due to other confounding mechanisms, such as the impact of school entry laws on the age relative to peers and the absolute age at which an individual is taught a particular material. These results suggest that using school entry laws as an instrument for educational attainment when trying to estimate the returns to education is problematic.

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1. Introduction

Parents and educators have expressed concerns about the effects of early school entry on children's education and wellbeing. The age at which a student enters school not only determines their physical and mental maturity at any given grade, but also how they compare in physical and mental maturity with their peer group in school. Since being the youngest child in a grade can adversely impact either the child's education or their interactions with their peers, a significant number of parents are choosing to delay the entry of their kids into school. But the choice of delaying school entry also implies that parents are giving up potential educational gains from early formal education. The importance of this trade-off is recognized by a number of studies in the education literature, where it is argued that kindergarten is the first critical step in children's school path, with potential subsequent impacts on learning and educational attainment.[1]

In this paper we provide quasi experimental evidence of the impact of early school entry on the educational path of children, their educational attainment and their labor market outcomes. A quasi experimental design is crucial to get causal estimates of the impact of early school entry since parents can self select their children into early formal schooling based on each child's age, maturity, ability or any other indicators of future school performance. In order to overcome this fundamental selection issue, our research design relies on state school entry laws that regulate school enrollment eligibility. For example, the state of California determines that a child must be at least 5 years old by December 2nd in order to enroll in kindergarten in a given academic year. Children born immediately before the school eligibility cut off date are very similar to children born right after it but they will differ in the age at which they enter school by one year. We can take advantage of this discontinuity to implement a regression discontinuity design (RDD) that will let us estimate the short run and long run consequences of early school entry by comparing children that are similar in all dimensions, but are different ages when they enter school.[2]

There is a literature leveraging the school entry and leaving laws as a source of variation in the timing of school entry and educational attainment. Angrist and Krueger (1991) used quarter of birth as an instrument for years of education, the idea being that

[1] A rich survey of this education literature can be found in Stipek (2002) and Datar (2005).
[2] Early applications of RDD can be found in Thistlethwaite and Campbell (1960) and Cook and Campbell (1979). We discuss the details of the RDD model and the most recent literature in the next section.

quarter of birth captures the interaction of early school entry laws and compulsory schooling laws.[3] More recently, Cascio and Lewis (2005) estimated the impact of schooling on AFQT performance using school entry dates as an instrumental variable. Since then, a number of researchers have applied similar RDD methodology to study the relationship between age based school entry laws and educational attainment. Bedard and Dhuey (2005) use month of birth and school entry laws in all OECD countries to show that the youngest members of a given cohort in grades four and eight have 2-12 percentile lower test scores than the oldest students in the same cohort. The authors also present evidence that the youngest members in a cohort in the United States are slightly less likely to enroll in pre-university academic track course. Elder and Lubotsky (2006) also use month of birth in the Early Childhood Longitudinal Study to estimate that a one year increase in the school entry age reduces the probably of grade retention by 10 to 18 percent in elementary school. Fredriksson and Ockert (2005) find similar effects using data on Swedish children. McCrary and Royer (2005) used the school entry laws to examine how maternal education affects birth outcomes.

Our contribution to the existing economics literature is twofold. First, we use the restricted access 15% Decennial Census Long Form Data for the states of California and Texas to precisely measure the impact of entry laws while the children are still in school, and then measure the effect of early school entry on long run outcomes, such as educational attainment, employment and wages.[4] Unlike the publicly available micro sample (PUMS), the restricted-access data has information on the *exact day of birth* for each individual. This is the first study we are aware of using a data set large enough get precise measures of the impact of the school entry on adult outcomes for the United States using an explicit RDD design.

Second, the paper gives some insight into the value of school entry laws as an instrument for educational attainment. Our large sample of repeated cross-sections makes it possible to evaluate the validity of this instrument by correctly estimating the first stage relationship between school entry laws and long term adult educational attainment. In addition, we document how other potentially important causal mechanisms, such as retention and relative age, are affected by the school entry laws.

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[3] For a detailed discussion of the benefits and shortcomings of quarter of birth as instrumental variables see Bound, Jaeger and Baker (1995), in addition to the original Angrist and Kruger paper.

[4] We only use those states due to the availability of data.

Our empirical results show that school entry laws have a very significant effect on the age at which students enroll in school. By age 5, the discontinuity in kindergarten enrollment is 53 percentage points in California and 62 percentage points in Texas. This difference is not 100% since some parents decide not to enroll their children in school as soon as they are eligible and some don't comply with the law. For example, only 76% (83%) of parents in California (Texas) enroll their children in kindergarten when they are eligible to do so. On the other hand, 18% (22%) of parents whose kids are still not eligible for enrollment still find a way to matriculate their kids in kindergarten.[5]

By ninth grade, the magnitude of the discontinuity in school enrollment is reduced to 40 percentage points (43 percentage points) in California (Texas). This convergence is largely due to the fact that children born just before the cut off date for school entry are considerably more likely to be held back a grade than their older peers. However, we find no evidence that the school entry laws result in differences in drop out rates.

The average effect on enrolment and drop back rates is not uniform by gender, race or parental education. Girls, minorities and the children of parents with less than a college education are disproportionately entering school as early as legally possible. The education literature suggests that the gender difference is due the relative maturity of girls, while the stronger compliance of minorities has also been documented in other studies.[6] Hispanics and children of parents with low educational attainment are considerably more likely to be held back a grade. However, black children are very likely to enroll in school as early as legally possible and are no more likely to be held back than other groups. As a result they are disproportionately the youngest students enrolled in a given grade, which potentially has negative consequences on learning and test scores.

Despite the striking differences in the timing of enrollment and retention rates, we find only a modest one percentage point increase in high school attainment for individuals born right before the cut off for school entry. The magnitude of the discontinuity is approximately half percentage point from grades nine to eleven. The discontinuities are considerably larger among 65 to 79 year olds in Texas and California. Adults born just before the cut off date are 1 to 1.8 percentage points more likely to complete 9th grade, 10th

[5] These numbers correspond to children born in the 30 days before and in the 30 days after the cut off dates.
[6] See Stipek (2002).

grade or 11th grade and about 2.2 percentage points more likely to complete high school or attend some college.

Since those positive estimates are spread over so many different levels of educational attainment, it suggests that the interaction between school entry laws and the laws that restrict when an individual can leave school are not causing the observed discontinuities in education achievement. If it were an interaction of these two laws causing the discontinuities, we would expect to see an impact for at most the one or two years at which the school leaving law is binding. A school leaving age of 16, for example, should have at most an indirect and minor impact on high school completion rates. One possible explanation for the observed patterns is that some individuals have a certain targeted age at which they wish to leave school and that those who enter school earlier will have completed one more year of school before they hit their target age.[7] Therefore, researchers should be careful when using the interaction of school entry laws and mandatory school attendance laws as an instrument for educational attainment as it may not be the school attendance laws that are driving the difference in educational attainment.

A second problem with using school entry laws as a source of variation in adult educational attainment is that they impact adult outcomes such as wages and employment thorough a variety of channels other than pure education achievement. First, individuals that start school early are exposed to a particular subject at a younger age then their classmates and may be less able to understand it. Bedard and Dhuey (2005) and Elder and Lubotsky (2006) showed that student's relative maturity can impact test scores. Second, we present evidence that the school entry laws result in a strong discontinuity in retention rates, so even when the laws induce little difference in the highest grade achieved they induce significant differences in the number of years of education. Third, other types of peer effects, such as relative height, might have an impact on student achievement and labor market outcomes. Persico, Postlewaite and Silverman (2004) showed that relative height at age 16 explains a significant portion of the variation in adult wages. This mechanism could have a substantial impact in this setting since the youngest kid in a class will be an inch, or more, shorter on average than their peers.

[7] Oreopolous (2005) shows that compulsory school attendance laws can have a large effect on educational attainment in some circumstances, such as in the 1950's in England. Also see Acemoglu and Angrist (2000) and Del Bono and Galindo-Rueda (2004).

Even though the existence of the multiple causal channels described above make the two-stage least squares estimates of the impact of years of education on adult outcomes hard to interpret, we can still get credible reduced form estimates of the impact of the school entry law program on wages and employment. These estimates are of interest to forward looking parents that are currently making decisions about when to enroll their children in school. Our approach will give them an unbiased estimate of the net return of that initial parental choice, without the obvious endogeneity problem that can arise from estimating the return to early school entry from a cross-section model.

Interestingly, we find no evidence that school entry laws and the additional education that results from early school entry leads to differences in employment rates or wages, or any of the other long run outcomes we have measures of. One possible explanation is that the impact of the law on these various causal channels is not sufficiently large to result in a detectable impact on adult outcomes. Another possibility is that the various causal channels are canceling each other out.

This paper is organized as follows: in section 2 we discuss the empirical model and data sources in detail. In section 3 we present estimates of the effects of school entry laws while children are still enrolled in school. In section 4 we examine the impact of school entry laws on adult outcomes. Section 5 concludes.

2. Econometric Methods

In this section we describe the regression discontinuity design (RDD) model we use to estimate the effect of school entry laws on the timing of school entry and a variety of outcomes. The seminal theoretical paper in this area, Hahn, Todd and Van der Klaauw (2001), focused on identification and non parametric estimation of the RDD when the discontinuity occurs on a continuous variable. Lee (2005), Lee and Card (2005) and Dinardo and Lee (2004) show that in the case where the variable on which the discontinuity occurs is not continuous the RDD can be estimated parametrically.[8] Because age is measured in days we use a parametric RDD rather than a nonparametric approach. The parametric model we estimate is a second order polynomial fully interacted with a dummy for being born after the cutoff date for school entry.

[8] The parametric approach also allows for straightforward hypothesis testing - see Lee and Card (2005).

The first outcome we examine is school enrollment for which we estimate the following equation;

$$(1)\ Enroll_i = \alpha + \beta_1 Cut_i + \beta_2 Bday_i + B_3 Cut_i * Bday_i + \beta_4 Bday_i^2 + \beta_5 Cut_i * Bday_i^2 + \Pi X_i + \varepsilon_i$$

where $Enroll_i$ is an indicator variable equal to 1 if individual i is enrolled in a given grade or higher, Cut_i is an indicator variable for being born after the cut off date, $Bday_i$ is the number of days from the individual's birthday to the cut off date, X_i is a set of covariates, and ε_i is an idiosyncratic error term. The coefficient β_1 is the estimate of how much the law decreases the probability that an individual born just after the cut off date for school entry will be enrolled in a particular grade. A coefficient β_1 equal to -1 would indicate full compliance with the law and a coefficient β_1 close to zero would indicate that the law has little or no impact on the timing of school entry.

For each outcome we create a figure, over the support of age, with the fitted model superimposed over the mean of the outcome. We compute the mean of the outcomes for blocks of age rather than single days of age so as to make the figure more interpretable.[9] The figures let us visually determine if there is a break in the outcome and also let us determine if the regression model is correctly specified. We experimented with higher order polynomials and found no visual evidence that the second order polynomial is under fitting the data. We also found no statistical evidence in favor of models with higher order polynomials, as the inclusion of such higher order terms did not improve the fit of the model.[10]

When we examine educational attainment of the adult population we estimate the following equation:

$$(2)\quad Educ_i = \alpha + \delta_1 Cut_i + \delta_2 Bday_i + \delta_3 Cut_i * Bday_i + \delta_4 Bday^2 + \delta_5 Cut_i * Bday^2 + \Psi \tilde{X} + \mu_i$$

[9] When we tried means by day the figures were very noisy. We adjust the number of days we use to compute the means depending on the sample size.

[10] See Porter (2003) for an additional discussion on the appropriate choice of polynomial order.

where *Educ* is based on the highest educational attainment of individual *i*, and $\tilde{X}$ represents the relevant set of covariates for the adult population. Highest educational attainment is defined by a set of indicator variables for having completed a particular number of years of education (or more).[11] The use of indicator variables for completed years of education make it possible to determine at what points in the distribution of educational attainment the school entry laws have their impact.[12] This will help us determine if it is the school entry laws interacting with school leaving laws that drive the differences we observe in educational attainment.[13]

Finally, we estimate the reduced form relationship between school entry laws (i.e., exact date of birth) and labor market outcomes with the following equation:

$$(3) \qquad Y_i = \alpha + \gamma_1 Cut_i + \gamma_2 Bday_i + \gamma_3 Cut_i * Bday_i + \gamma_4 Bday^2 + \gamma_5 Cut_i * Bday^2 + \Sigma \tilde{X} + \nu_i$$

where Y_i is an adult outcome, such as wages or employment. This reduced form equation provides estimates of the net effect of school entry laws on long run outcomes without the need to specify any structural relationship that includes the channels through which early entry in school affects the adult outcome. If educational attainment were the sole channel through which early school entry laws affected the adult outcome then γ_1 / δ_1 is the causal impact of educational attainment on the outcome for people who comply with the law.[14] As we document in the next section there are multiple causal channels via which early school entry laws affect adult outcomes so we do not compute this implied instrumental variables estimate.

One appealing property of the RDD is that it is possible to indirectly check for the possibility of selection bias. In order to get consistent estimates with the RDD all potential

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[11] For example, the 10th grade indicator variable is equal to 1 if the individual highest education achievement is 10th grader or higher, and zero otherwise.

[12] Another common way of defining education attainment in the literature of returns to education is by calculating the perceived number of years of formal schooling. Despite the measurement difficulties of calculation such variable given high retention rates, differences in college term, etc., we prefer to look at the distributional effects since we will not estimate any structural equation of returns to education.

[13] We leverage the fact that the if it is the interaction between school entry laws and school leaving laws we would expect to see the impact on educational attainment occur for particular attainment cut offs.

[14] For a detailed discussion of the Local Average Treatment Effect (LATE) please see Angrist, J., Krueger, A. and Imbens, G. (1995).

confounders must be constant across the discontinuity. If the observed characteristics do not change discretely at the school entry cut off date, this suggests that the unobservable characteristics are not changing around the threshold either and that therefore omitted bias is not a problem. We test for discontinuities in the observable variables by estimating a set of regressions of the form of equation (1), for each of the covariates in our data set.

All the regressions are estimated using the 2000 Decennial Census Long Form data for the states of California and Texas. Besides the large number of observations (approximately 15% of the population in each state), these restricted access data also have the exact date of birth for every individual in the sample. One limitation of the Census data is that we observe a series of cross-sections rather than the full educational path of each individual. This can lead to measurement error, since a high proportion of individuals are immigrants or migrants from other states. We deal this issue by restricting the sample to individuals born in California and Texas. Although it is possible that someone born in California attended school outside California, restricting the analysis to natives deals with most of the problem caused by immigration and migration between states.[15]

3. Effect of School Entry Laws on Student Achievement

In this section we start by examining the impact of school enrollment laws in California and Texas on school age children. We document that though compliance with the law is not perfect, the laws do induce a large discontinuity in the age at which children enter school. We then show that this discontinuity varies significantly by race, gender and parental education. Finally we examine two outcomes: dropping out of school and repeating a grade. We show that though children that are very young for their grade are much more likely to be held back a grade, they are no more likely to drop out by age 17 than their older peers.

3.1. Children's School Enrollment

In Figure 1A we present estimates of the proportion of individuals in California that are enrolled in kindergarten or a higher grade.[16] The proportion enrolled is plotted over the

[15] We also eliminate all census data allocations to date of birth, educational attainment and school enrollment, since those are the most important variables in our study

[16] Due to the limited categories for the grade enrolled question in the Census we are constrained to examining four cutoffs for school age individuals: kindergarten, first grade, fifth grade and ninth grade. For the adult

support of the number of days from an individual's birthday to the cut off date for entry into kindergarten in California.[17] To make the figures less noisy the proportion enrolled has been computed for nine day blocks rather than for individual days. The fitted values from the regression model specified in equation (1) are laid over the means.[18] The other figures described in this section have the same structure though the age group and outcomes examined vary.

The top left panel of Figure 1A reveals that there is less than perfect compliance with the law. About 20% of individuals born immediately after the cut off for school entry are enrolled one grade higher than they would be if compliance with the law was perfect. We also see that a considerable number of individuals born before the cut off date delay enrolling in kindergarten until the year after they are eligible. This phenomenon is most pronounced among children that are barely eligible for kindergarten. As can be seen in the figure this group has kindergarten enrollment rates considerably lower than their slightly older peers. However, the law still has a considerable effect and most individuals born before the cut off date for kindergarten enrollment are a grade ahead of individuals born just after the cut off date.

The remaining panels of Figure 1A reveal that the size of the gap induced by the discontinuity shrinks as children get older. In Figure 1C we present the fitted values from the four regressions in a single figure to facilitate comparing them. This figure reveals that the discontinuity in the proportion enrolled in the highest grade closes significantly between first grade and ninth grade. The changes we see across the four panels are consistent with the very youngest students in a particular grade being held back at rates higher than their older peers. The retention rates are considerable with 31% of the students born just before the threshold being retained at some point between kindergarten and ninth grade compared with 16% of those born 90 days before the cut off date.[19]

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population we are able to analyze the complete distribution of education attainment since there is no need to look at enrollment rates.

[17] For example, an individual born ten days before the kindergarten cut off date would have a relative age of -10 and would be eligible to enroll in kindergarten.

[18] To maximize the precision of the estimates the regression line is estimated from the enrollment proportions for each day rather than from the nine day means in the figure. The regressions in the figures do not include individual level covariates though the regressions in the tables do.

[19] The 31% retention rate was calculated by dividing the proportion of students enrolled in 9th grade at the relevant age right before the cut off date (55%) by the proportion of students enrolled in kindergarten at the relevant age right before the cut off date (80%). A similar calculation was done for students born 90 days

In Figure 1B we present the corresponding four figures for Texas. The pattern in Texas is similar to the one in California, though a larger proportion of those eligible to enter the higher grade do so in Texas. The figures reveal that the gap in enrollment for ninth graders is about two thirds the size of the gap at kindergarten. As seen in Figure 1D, the closing of the gap is due largely to the fact that the youngest in each class are being held back at considerably higher rates than their older peers.

Tables 1A and 1B present the regression results corresponding to Figures 1A, 1B, 1C and 1D. The first column of each pair presents the regression discontinuity estimates corresponding to the appropriate line in the figures. The second of each pair of columns contains the same regression run on the underlying micro data with covariates added. Table 1A reveals that the inclusion of the covariates has no statistically significant effect on the estimates for California. The estimates of the discontinuous change in grade enrollment at a particular age confirm what we saw in the figures. In California we find a difference of 52 percentage points in kindergarten, 50 percentage points in first grade, 41 percentage points in fifth grade, and 40 percentage points in ninth grade. Although parents in Texas are much more likely than parents in California to enroll their children in kindergarten as soon as they are eligible, by ninth grade the discontinuity in enrollment in Texas is approximately the same size as the discontinuity in California.

One concern about the results presented above is that some of the differences in the grade in which individuals are enrolled in may be due not to the legislation but to demographic factors that change abruptly at the school enrollment threshold. That we see similar patterns in California and Texas despite the fact that they have different cut offs for school enrollment and that the changes are as abrupt as they are suggests that the differences are due to the school entry laws. That adding of covariates to the regressions has no impact on the estimates also argues that the changes we observe at the discontinuity are due to the school enrollment law. However it is worth implementing a more direct examination of the continuity assumptions under which our regression discontinuity design gives us consistent estimates. In Table 2 we examine the data to see if there are discrete changes in any of the observable characteristics of the children. For there to be an abrupt change in children's characteristics at the threshold it would be necessary for some subpopulation to be timing

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before the cut off date. This calculation ignores the possibility that there are significant differences across cohorts.

their births so as to put them on one side of the threshold. This would likely result in a discernable difference in the number of individuals born on either side of the threshold. We see no evidence of this. Table 2 also reveals no evidence of abrupt changes in the gender or racial composition of our sample at the threshold for school enrollment in either Texas or California. The only difference we observe is that children born after the cut off for school entry are 7 (12) percentage points more likely to be enrolled in private kindergarten in California (Texas) when examined at age 5. One possibility is that that this difference is due to parents using private schools to work around the school entry laws, especially when they have to work[20]. However, this difference almost completely disappears by age 6 as many of the children enter the public school system for first grade.

3.2. Compliance with the Law

Figures 1A and 1B showed a significant amount of non-compliance, with some parents deferring enrolling their children in kindergarten by a year and other parents circumventing the law and enrolling their children a grade ahead of the one they should be in. In Tables 3A and 3B we examine the first part of this issue, by looking at the characteristics of children who were born 30 days before the school age cut off. The first three columns of Table 3A present the results for children that are eligible, though not required to enter, kindergarten in California. The first column contains the characteristics of the students that enter kindergarten. The second column presents the characteristics of students that defer entry into kindergarten for a year. The third column presents the t-statistic of the difference between the first two columns. The table reveals striking differences between the children who are entering school as the youngest in their cohort and students who defer entry for a year. The youngest students in their grade are disproportionately black and Hispanic, and also more likely to be female. Their parents have lower education attainment, lower incomes and less valuable houses. The remaining columns of Table 3A reveals that a large portion of these differences persist for the first, fifth and ninth grade cut off in California. In Table 3B we present the corresponding results for Texas. We find a similar pattern in Texas though the differences between the two groups are smaller. These differences suggest that estimates of the return to entering school

[20] Gelbach (2002) showed that working mothers are more likely to enroll their kids in private kindergarten when age based entry laws make their children ineligible for public kindergarten.

early that leverage cross-sectional variation in the timing of school entry are likely to suffer from significant omitted variables bias.

In Tables 4A and 4B we examine the characteristics of children that are born in the 30 days after the cut off for school entry in California and Texas respectively. According to the law these children are too young to enter school, though some parents get permission to from the governing board of the school district to enroll their children in kindergarten a year early. We see that minorities, less educated parents and parents of female children are significantly more likely to enroll their children in school early. The differences we observe suggest that the effect of the law will vary considerably by gender, race and parental education. We explore these differences in the following subsection.

3.3. School Enrollment, Retention, and Attendance by Gender, Race and Parental Education

Given the striking differences in compliance with the law by race, gender and parental education, we conduct a separate examination of the enrollment patterns for each group. In Figures 2A and 2B we present the grade boys in California and Texas are enrolled in for the four grade cut offs available in the Census. The figures reveal that the large gap that exists in kindergarten for males has shrunk considerably by ninth grade in both states. The decrease in the gap in grade enrolled in is due largely to the large number of boys born just before the school enrollment cut off date that are being held back a grade. The small proportion of the population born after the threshold that is enrolled a grade above the one they should be in are held back primarily as they transition from kindergarten to first grade, very few are held back after first grade.

Figures 3A and 3B present the corresponding results for females. As expected the figures reveal that the parents of girls are much more likely than parents of boys to enroll them in school as soon as the law allows. However, the figures reveal that girls that are just barely eligible for the grade they enroll in are being held back at much higher rates than the older girls in their class. As a result, by ninth grade there is a considerable closing of the gap in the grade in which the girls are enrolled, though the gap does not close as much as it did for the boys.

In Table 5A we present the corresponding regression results by gender for the state of California. The regression estimates are robust to the inclusion of covariates, and they confirm what we saw in the figures. The gap for girls in kindergarten is 57 percentage

points, and this gap drops to 46 percentage points by ninth grade. The gap for boys is 49 percentage points in kindergarten, and drops to 34 percentage points by ninth grade, which indicates a larger cumulative (and percentage) effect for boys than for girls. Table 5B shows that the pattern in Texas is very similar to the one in California.

In Figures 4A-4F we present the enrollment patterns in California and Texas separately for whites, blacks and Hispanics. In California, the gap for whites is 40 percentage points in kindergarten and drops to 28 percentage points in ninth grade. The gap for blacks drops from 49 to 39 percentage points and the gap for Hispanics drops from 63 to 51 percentage points. Similar numbers are found in Texas: between kindergarten and ninth grade the gap for whites drops from 60 percentage points to 40, for blacks it drops from 53 to 45 and for Hispanics it drops from 66 to 45. Those numbers are confirmed by Tables 6A and 6B, where we present the regression results corresponding to the figures.

The differences across the races in the rate at which students are held back a grade are less striking than the differences by gender. The main conclusion from examining Figures 4A-4F is that blacks and Hispanics are much more likely than whites to enroll in school as soon as they are eligible and are slightly less likely to be held back. By ninth grade the proportion of black and Hispanics that are enrolled in the highest grade they are eligible for is 25 percentage points higher than for whites. The pattern in Texas is similar though not as extreme. The net result is that black and Hispanic children are on average exposed to academic material at a considerably younger age than white children.

In Tables 7A and 7B we present the enrollment patterns in California and Texas for children of high and low education parents, where high education means college degree or more. For the children of high education parents in California the law induces a 40 percentage point discontinuity in kindergarten enrollment. This gap has dropped to 30 percentage points by ninth grade. Parents with less than a college education are considerably more likely to enroll their children in the highest grade possible. The gap in kindergarten enrollment for this group is 59 percentage points, and it declines to 45 percentage points by ninth grade. In Texas the gap in enrollment induced by the law is 52 percentage points in kindergarten for the children of high education parents but declines to 37 percentage points by ninth grade. For low education parents in Texas the initial gap is 66 percentage points but declines to 45 percentage points by ninth grade. It is unlikely that children with less educated parents are at a higher grade level than the children of parents with more education

because they are outperforming them. A more plausible explanation is that parents with lower educational attainment are less likely to make the decision to have a child that is struggling held back a grade.[21]

Finally we examine if the age at which a child enters school affects the probability that they will still be attending school (not dropped out of school) at a particular age. Tables 8A and 8B show estimates of the discontinuity in school attendance rates by gender, race and parental education, at ages 11 to 18 in California and Texas respectively. We find no evidence that early school entry affects the probability that a child will still be attending school at any of these ages.

4. Effect of School Entry Laws on Long Run Adult Outcomes

In this section we examine the effect of the school entry laws on adult educational attainment and labor market outcomes. We find that the school enrollment laws only induce small long-term differences in educational attainment for older cohorts and have no discernable effect on younger cohorts. We find no evidence that the laws had an impact on labor market outcomes such as employment rates and wages for any age group.

4.1. Adult Educational attainment

Even though we do not see evidence that school entry laws cause differences in drop out rates among people under 18 years old, it is still possible that there are differences in educational attainment among adults. These could occur either because the differences appear after high school or because school enrollment laws had a more pronounced effect on older cohorts.

In Figure 5A we present the profile of educational attainment by birthday for California natives between the age of 30 and 79. In each panel of the figure we plot the proportion of California natives born in a particular fifteen day period that have completed a particular grade or higher. As can be seen in the graph, adults that were just barely eligible to enter school appear to be slightly more likely to have completed 11th grade, 12th grade or

[21] It is also possible that the patterns we observe in retention rates are due to systematic differences in the schools these groups are attending, but unfortunately we are not able to test such alternative hypotheses because we are only allowed to use the restricted Census data with state level regressions.

received a H.S. diploma. The evidence of an impact of the school enrollment laws on college attendance is less compelling in part because the estimates are much less precise.

Regression estimates of the impact of school entry laws on the educational attainment of California natives are presented in Table 9A. Each regression is estimated off the micro data and includes the demographic characteristics.[22] The regression results are robust to the inclusion of covariates and confirm that the increases in grade completion probabilities that we observed in the figures are statistically significant. The school entry laws resulted in an increase in the proportion of adults completing 10^{th}, 11^{th}, 12^{th} grade and receiving a H.S. Diploma. The respective increases at the discontinuity are: 0.3%, 0.5%, 0.6% and 0.9%.

In Figure 5B we present the educational attainment for adults in Texas. Surprisingly, the figure reveals a very pronounced seasonality in educational attainment in Texas[23]. Despite the fact that the seasonality makes the figure harder to interpret, we see evidence of a seam in educational attainment in Texas that is very similar to the seam in California. Adults born just before the school entry cut off are slightly more likely to have completed 9^{th}, 10^{th} , 11^{th} ,12^{th} grade, or received a H.S. diploma than those just born after the cut off. As with California there is no compelling evidence of differences in rates of college entry or completion of an Associates degree.

These results are confirmed in Table 9B. The discontinuity estimates for 9^{th} grade, 10^{th} grade, 11^{th} grade, 12^{th} grade and H.S. Diploma are all statistically significant. The largest difference in educational attainment is for 12^{th} grade completion and it is slightly under a percentage point. The other effects are also slightly larger than the ones observed in California, particularly for the lower grades. That the effect sizes in the regressions conditioned on covariates are the same size as the ones from the unconditional regressions presented in the figures is indirect evidence that the observable characteristics are distributed smoothly across the discontinuity. As a more direct test of this we check for seams in the proportion that are male, white, black or Hispanic or lived in the same state 5 years ago. In Table 10 we see that these observable characteristics evolve smoothly through the cut off for school entry. This suggests that the unobservable characteristics are also likely to evolve

[22] The unconditional regressions used to plot the lines in Figure 5 have similar discontinuity estimates, and are available upon request.

[23] Such seasonality in birth outcomes in Texas can also be observed in McCrary and Royer (2005).

smoothly and that the differences in adult educational attainment that we documented above are not due to selection bias.

4.2. Adult Educational attainment by Gender, Race and Age Groups

In this section we examine how the differences in educational attainment induced by the school entry laws vary by gender, race and age. In Table 11A we present the results by gender and by race for individuals 30 to 79 years of age in California. The seams are slightly larger for females than males in California though most of the differences are not statistically significant. We present comparable results by gender for Texas in Table 11B. In Texas most of the effect sizes for men and women are fairly similar and for most levels of educational attainment slightly larger than the effects we saw in California.

The results by race for Texas and California show a considerably larger seam in highest grade attained for Hispanics than for whites. In both states the seam for 11th grade, 12th grade and H.S. diploma is on the order of 1.5 to 2 percentage points for Hispanics, which is 3 to 4 times the respective seam for whites. In both states some of the results for blacks have perverse signs and all of them are statistically insignificant, which may be due to the small sample sizes.

Next we turn to comparing educational attainment across age cohorts. In Table 12A we present the seam in educational attainment for California natives for 30-39, 40-49, 50-64 and 65-79 year olds respectively. The regressions reveal that the seams in enrollment around 12th grade are between 0.5% and 0.8% for the youngest cohorts, zero for the cohort of 50 to 64 year olds and almost 3% - for the 65-79 year olds. The corresponding results for Texas, presented in Table 12B, are very similar to the results from California. There are modest seams in educational attainment for the two youngest cohorts, slightly larger effects on the cohort of 65 to 79 year olds and no evidence of an effect on the cohort of 50 to 64 year olds.

Overall, we observe a small effect of school entry laws on adult educational attainment with the largest effects for women, Hispanics and 65 to 79 year olds. There are a number of possible causes of the difference in educational attainment observed around the school entry cut off. One possibility is that we are seeing an interaction between the school entry laws and the mandatory schooling laws. However if this were the case we would expect the seam in school attainment to be focused largely at particular cut offs and this does not appear to be the case. Another possibility is that there are peer effects that cause the

oldest members of a class to be more likely to drop out or the youngest member of a class to be less likely to drop out, although we do not observe such effect from 5th grade to 12th grade. A final possibility is that for some individuals the probability of dropping out is a function of biological age so that people who enter school early will on average get slightly more education.

4.3. Labor Market and Other Long Run Outcomes

There are a number of ways in which the early school entry laws could create a discontinuity in labor market outcomes. One mechanism is through the increase in educational attainment documented above. However the school entry laws also induce discontinuities in the rate at which students are held back, their age relative to their peers and the absolute age at which they are exposed to material.[24] It is worth noting that while the school entry laws increase educational attainment for only a small part of the population these other three mechanisms impact a larger proportion of the population. The first affects more than a third of the people near the discontinuity and the second and third impact everyone that complies with the law. In this section we estimate the net effect of all these mechanisms on labor market outcomes. The net effect is of interest to parents making decisions regarding when to enroll their children in school.

In Tables 13A and 13B we document the reduced form discontinuity estimates for labor market outcomes, such as employment and wages, and also on home ownership, house prices, rental values, house size, marriage rates, and household income. The discontinuity estimates in both states are very small in magnitude and with a few exceptions statistically insignificant. Figures 6A and 6B corroborate these zero discontinuity estimates. To take advantage of the significant differences in compliance with the law across different subgroups in Tables 14A and 14B we present the same estimates by gender, race and age groups. Again almost all the coefficients are small and statistically insignificant indicating that school entry laws have at most a modest impact on long run outcomes. This does not necessarily mean that none of these mechanisms have an impact. It is possible that the impacts are too small to detect or that they are canceling each other out.

[24] There is also a one year difference in potential labor market experience.

5. Conclusion

Our study credibly documented that school entry laws are an important determinant of when children enter school, and the children that are the youngest in their school cohort are considerably more likely to be held back than their peers. We also find that blacks are more likely to enroll in school as soon as legally possible, and that they are held back at lower rates than other groups. Unless black children, conditional on age, are significantly more able than their white and Hispanic peers, it is likely that they are disproportionately bearing the consequences of being the youngest students in an academic cohort.

We find that the school entry laws have only a modest impact on educational attainment and that these effects are strongest for the oldest cohort. That there are seams in highest grade completed up through high school suggest it is not the school leaving laws causing the seams because for most of the period we are examining the school leaving age in both California and Texas was 16. This in combination with the very pronounced seasonality in educational attainment we observed in Texas suggest that using quarter of birth as an instrument educational attainment may result in inconsistent estimates of the returns to education.

Though we do not find an effect of the school entry laws on labor market outcomes there are a few reasons we might expect the laws to have an impact. The one which comes to mind first is the increase in educational attainment due to the school entry laws. However, given how small an impact the school entry laws have on educational attainment the other causal channels via which the school entry laws effect labor market outcomes are likely to dominate it. The youngest students in an academic cohort are considerably more likely to be held back a grade which may stigmatize them though it also increases the total years of education they receive. The school entry laws also create a difference in the age at which people are exposed to particular course material. Bedard and Dhuey (2005) show evidence that older students in a class do better on standardized tests. This suggests that older students may be better able to understand and absorb the course material due to their greater development. Finally there is the peer group effect, such as the height effect found by Persico, Postlewaite and Silverman (2004).

Several of these mechanisms such as total years in school, and adult educational attainment are likely to positively impact labor market outcomes for the youngest students in

a cohort. Other mechanisms such as being exposed to material at a younger age and being smaller than their peers are likely to negatively effect labor market outcomes. Given that we have all these conflicting mechanisms at work, it is unclear which direction we should see labor market outcomes varying across the school enrollment threshold. We do not find any evidence of a net effect of school entry laws on labor market outcomes. This does not mean that the individual mechanisms described above have no impact, since it is possible that they partially cancel each other out. Finally, the existence of this large number of mechanisms combined with a small effect of school entry laws on adult education attainment undermines the validity and interpretation of econometric models that use these laws as an instrumental variable for educational attainment.

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Figure 1A. Grade enrolled by date of birth, California

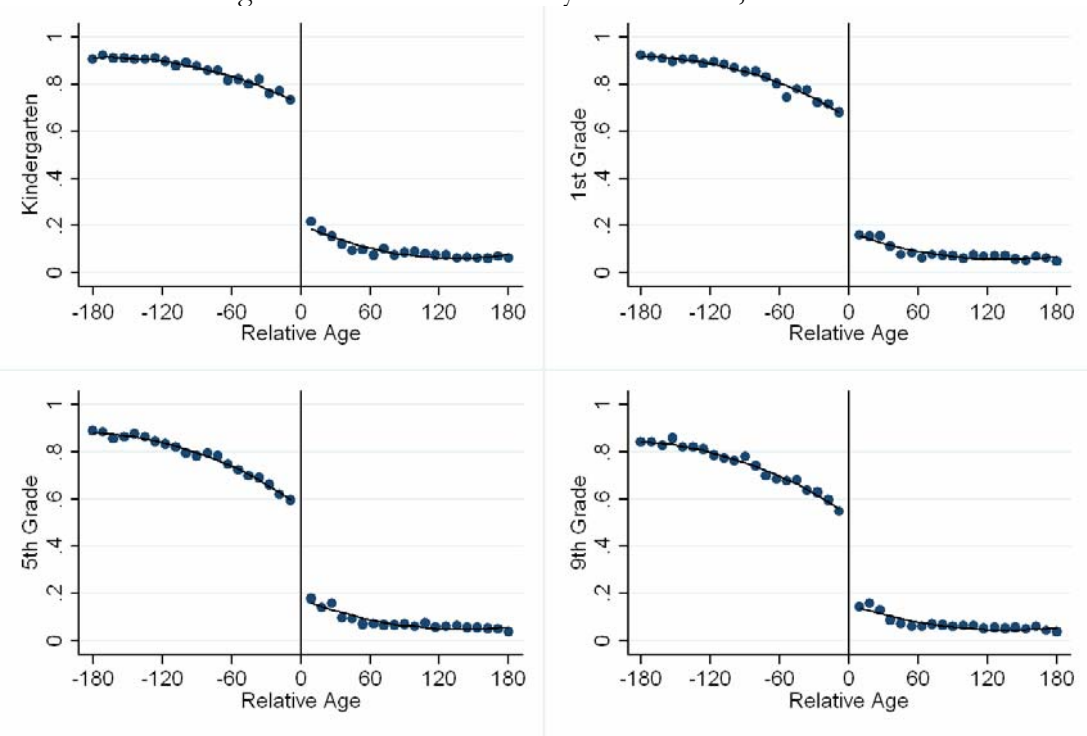
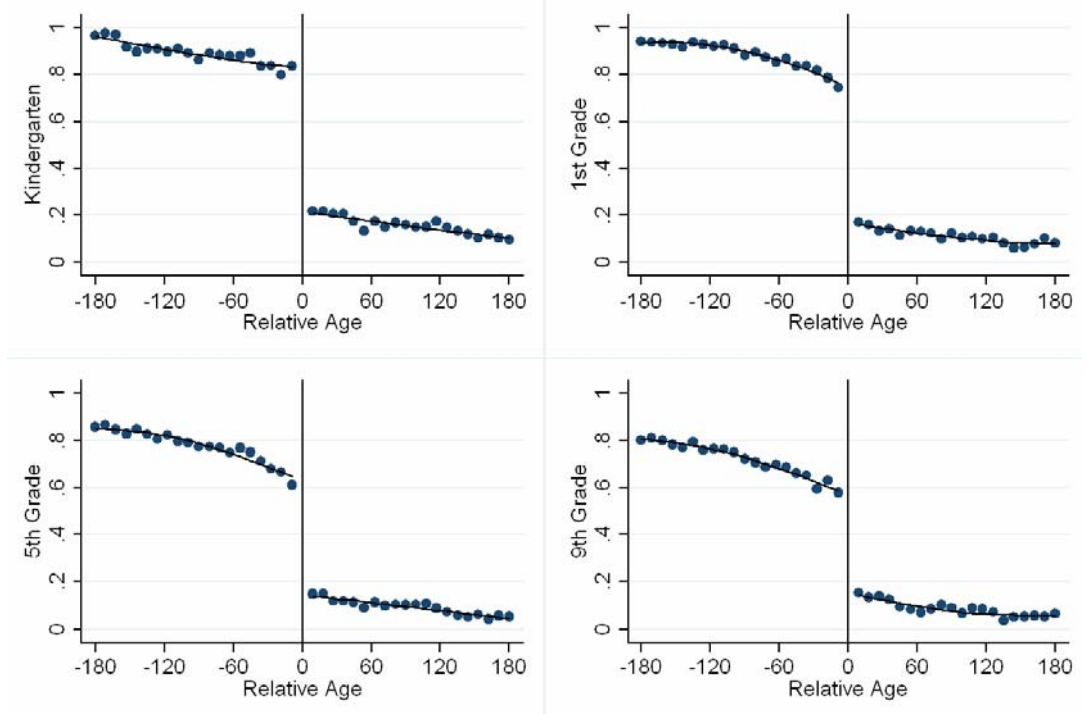


Figure 1B. Grade enrolled by date of birth, Texas



Notes: Figures 1A and 1B were estimated using the 2000 Decennial Long Form Census Data. Each figure shows the average enrollment for children of a certain age/grade group. For example, relative age equals to zero for kindergarten corresponds to a child of exact 5 years old (6 years old for 1st grade, 10 years old for 5th grade and 14 years old for 9th grade). Each dot represents the average enrollment by 9 day blocks of age. The solid line corresponds to an unconditional regression of school enrollment on relative age, relative age squared, a dummy for children born after the cutoff date and interactions of this dummy with relative age and relative age squared.

Figure 1C. Enrollment patterns for all grades in California

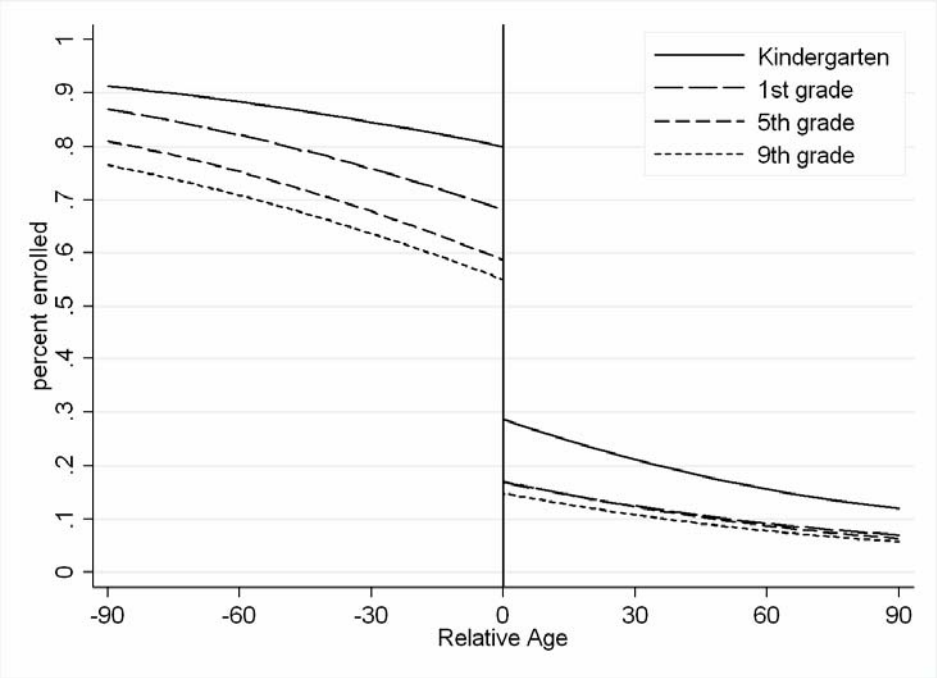
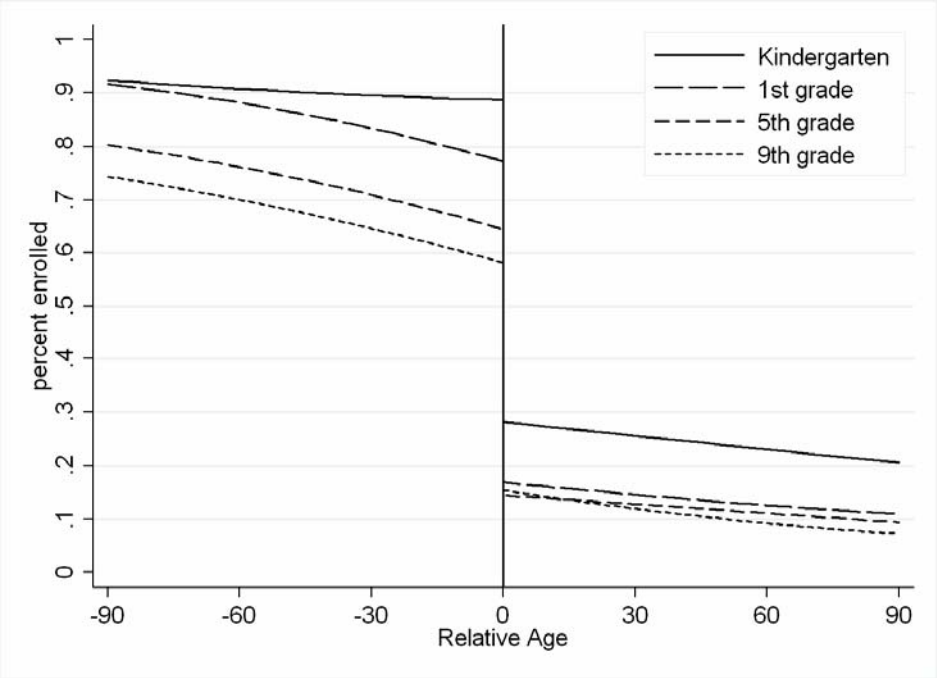


Figure 1D. Enrollment patterns for all grades in Texas



Notes: Figures 1C and 1D were created by stacking up the fitted lines presented in figures 1A and 1B

Figure 2A. Grade enrolled by date of birth for males, California

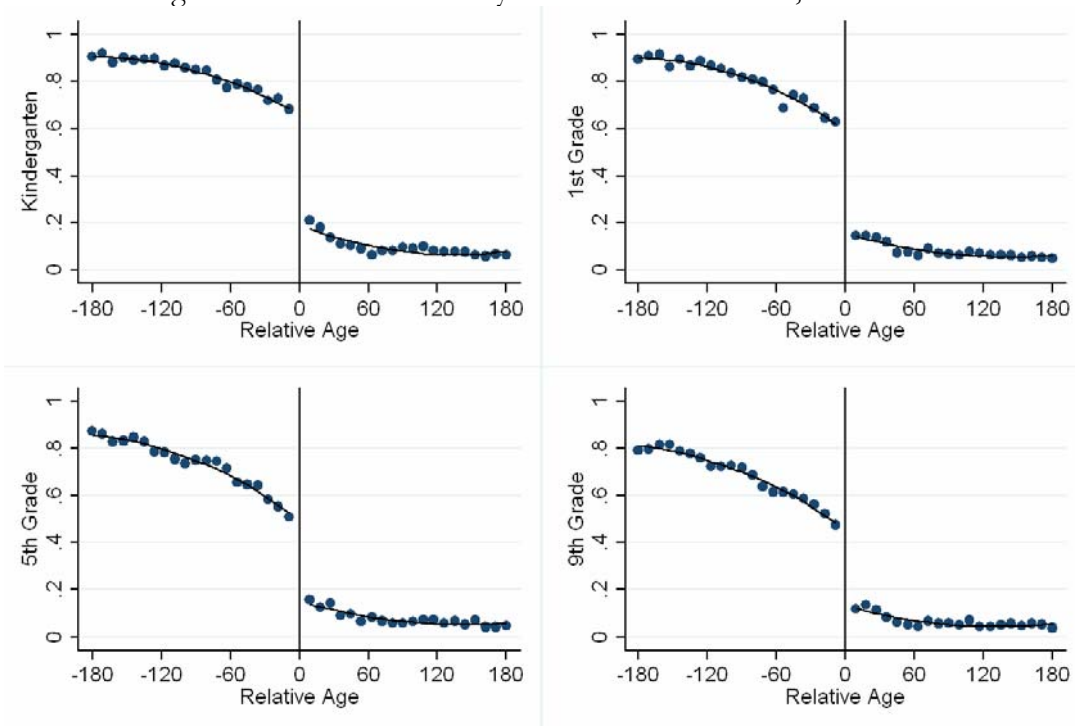
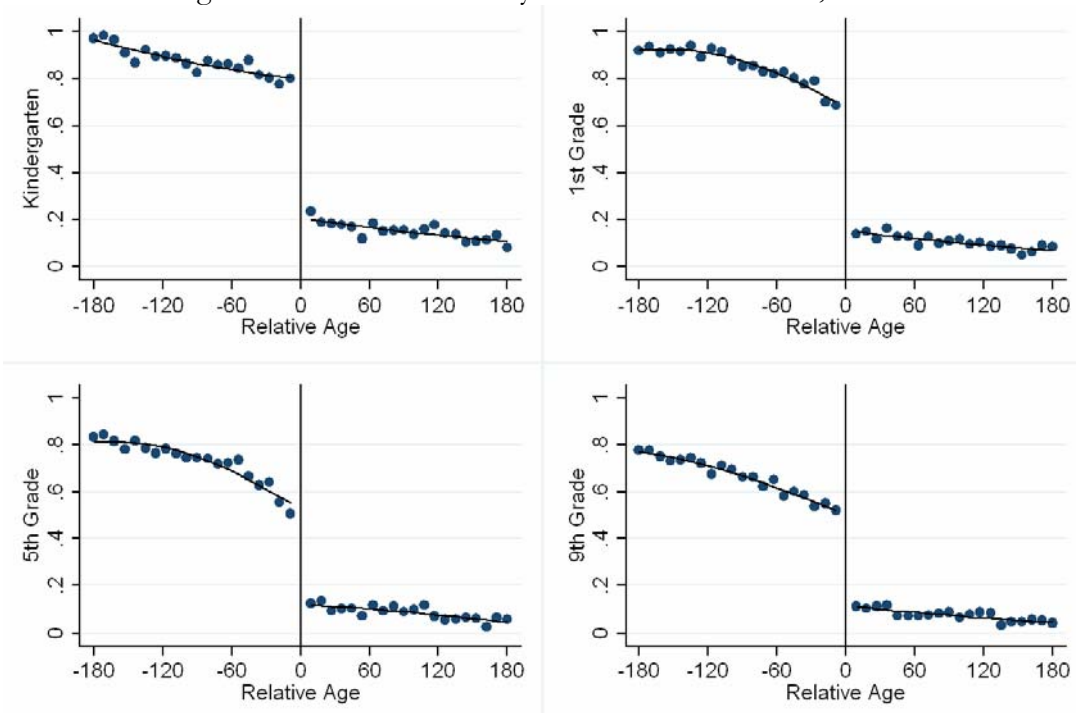


Figure 2B. Grade enrolled by date of birth for males, Texas



Notes: Figures 2A and 2B were estimated using the 2000 Decennial Long Form Census Data. Each figure shows the average enrollment for male children of a certain age/grade group. For example, relative age equals to zero for kindergarten corresponds to a child of exact 5 years old (6 years old for 1st grade, 10 years old for 5th grade and 14 years old for 9th grade). Each dot represents the average enrollment by 9 day blocks of age. The solid line corresponds to an unconditional regression of school enrollment on relative age, relative age squared, a dummy for children born after the cutoff date and interactions of this dummy with relative age and relative age squared.

Figure 3A. Grade enrolled by date of birth for females, California

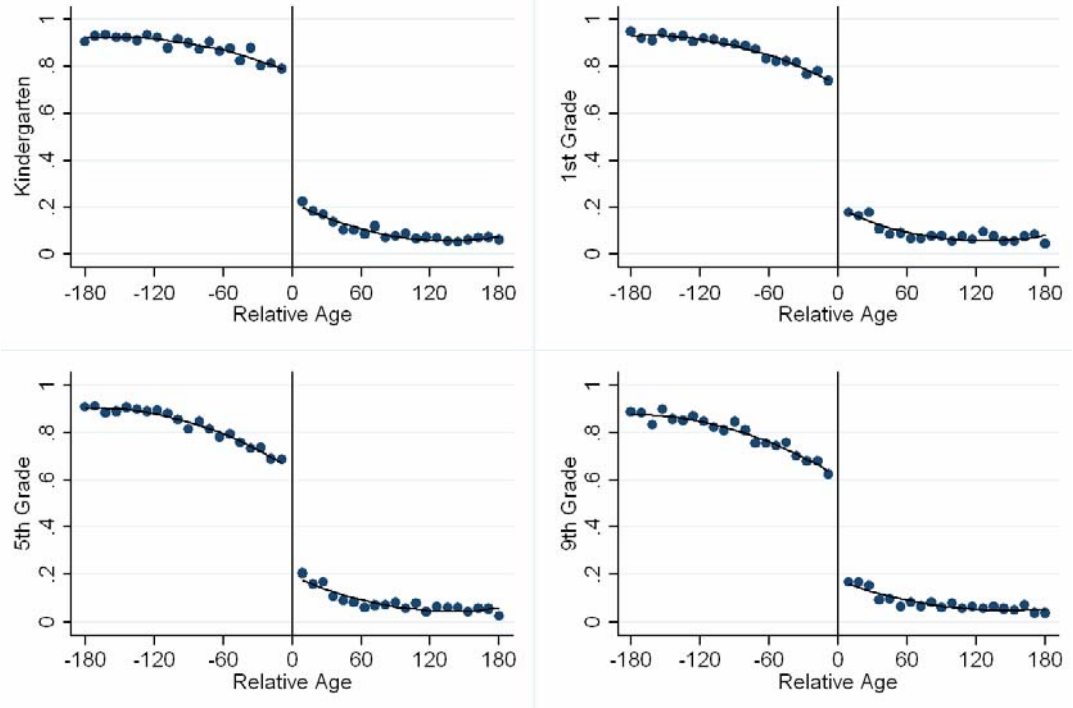
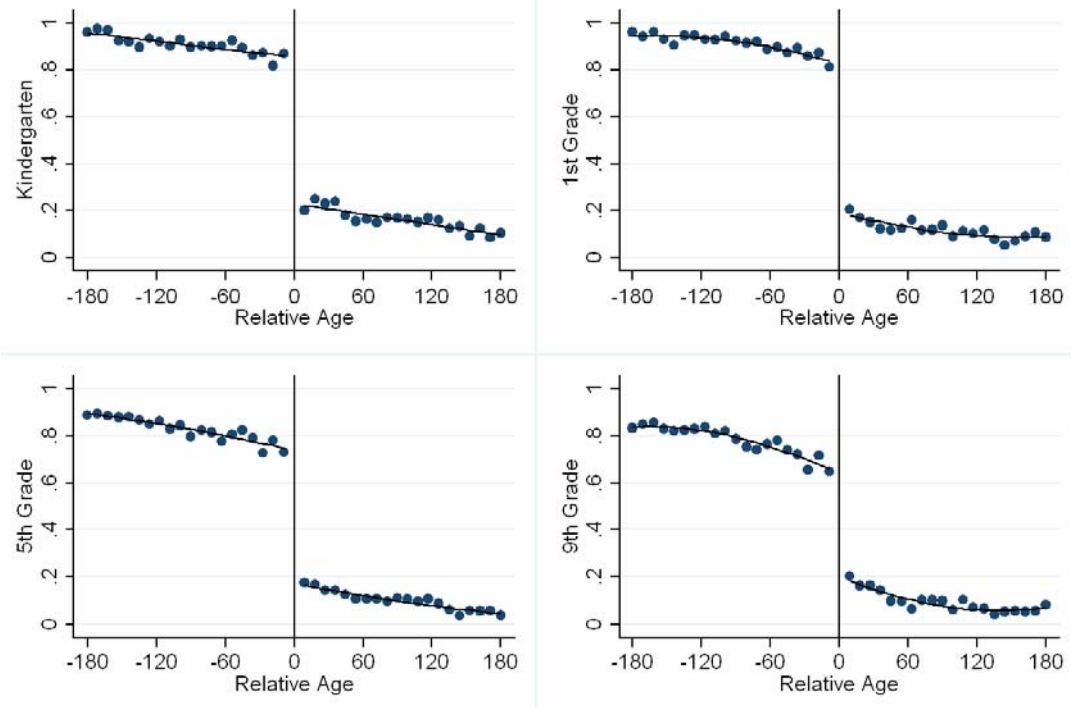


Figure 3B. Grade enrolled by date of birth for females, Texas



Notes: Figures 3A and 3B were estimated using the 2000 Decennial Long Form Census Data. Each figure shows the average enrollment for female children of a certain age/grade group. For example, relative age equals to zero for kindergarten corresponds to a child of exact 5 years old (6 years old for 1st grade, 10 years old for 5th grade and 14 years old for 9th grade). Each dot represents the average enrollment by 9 day blocks of age. The solid line corresponds to an unconditional regression of school enrollment on relative age, relative age squared, a dummy for children born after the cutoff date and interactions of this dummy with relative age and relative age squared.

Figure 4A. Grade enrolled by date of birth for whites, California

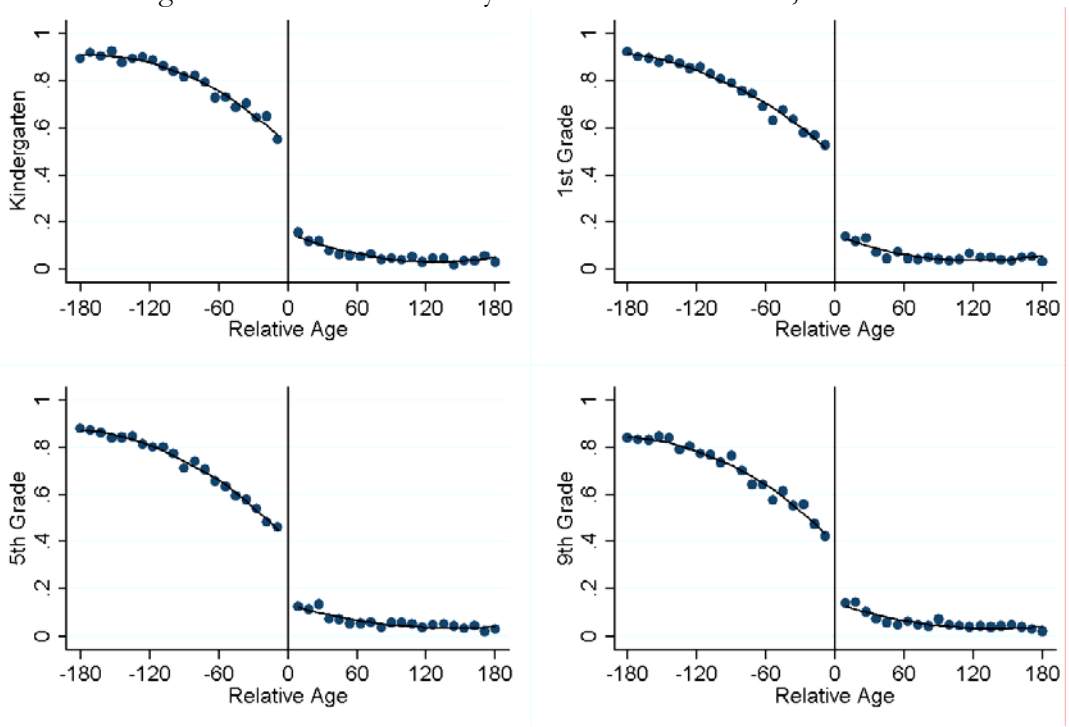
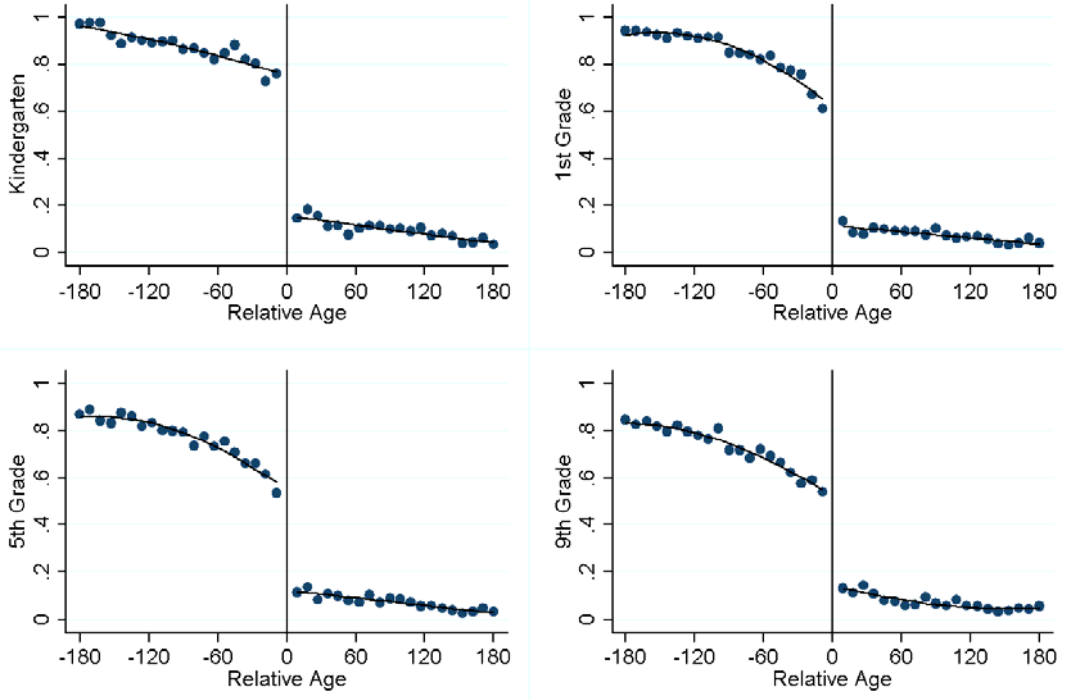


Figure 4B. Grade enrolled by date of birth for whites, Texas



Notes: Figures 4A and 4B were estimated using the 2000 Decennial Long Form Census Data. Each figure shows the average enrollment for white children of a certain age/grade group. For example, relative age equals to zero for kindergarten corresponds to a child of exact 5 years old (6 years old for 1st grade, 10 years old for 5th grade and 14 years old for 9th grade). Each dot represents the average enrollment by 9 day blocks of age. The solid line corresponds to an unconditional regression of school enrollment on relative age, relative age squared, a dummy for children born after the cutoff date and interactions of this dummy with relative age and relative age squared.

Figure 4C. Grade enrolled by date of birth for blacks, California

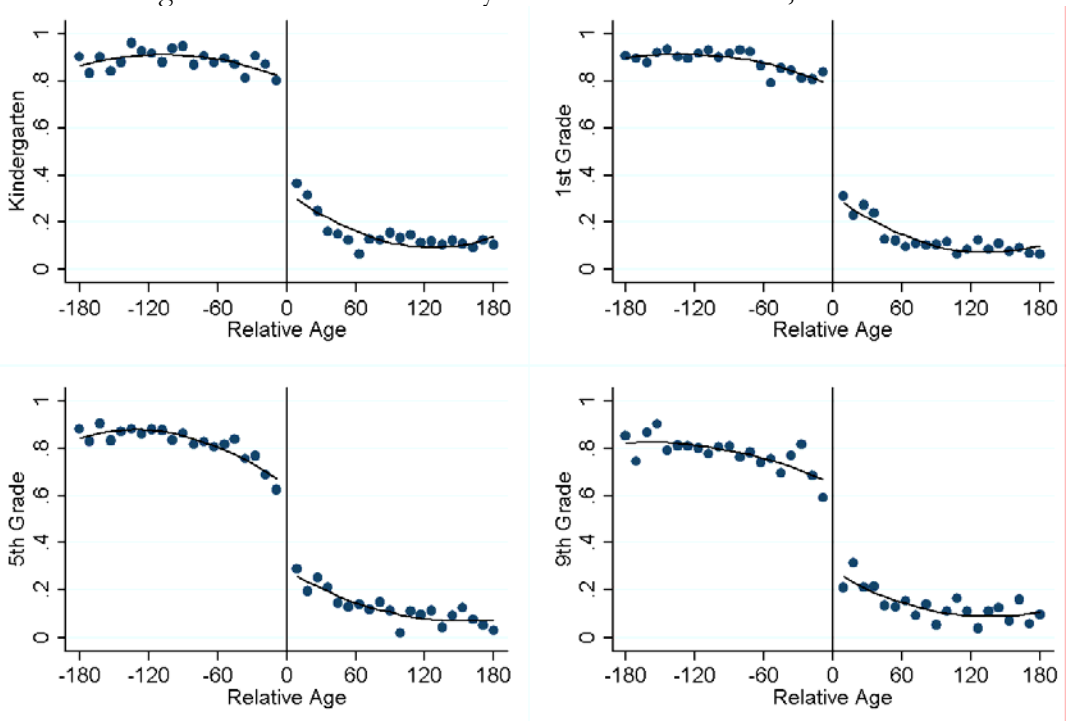
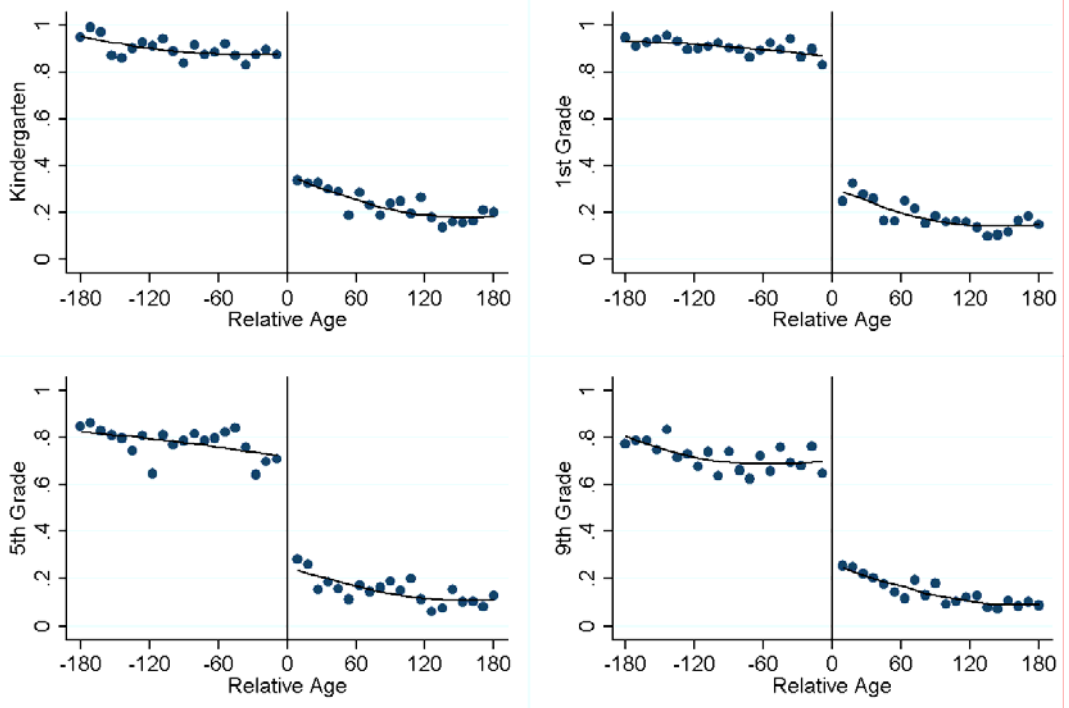


Figure 4D. Grade enrolled by date of birth for blacks, Texas



Notes: Figures 4C and 4D were estimated using the 2000 Decennial Long Form Census Data. Each figure shows the average enrollment for black children of a certain age/grade group. For example, relative age equals to zero for kindergarten corresponds to a child of exact 5 years old (6 years old for 1st grade, 10 years old for 5th grade and 14 years old for 9th grade). Each dot represents the average enrollment by 9 day blocks of age. The solid line corresponds to an unconditional regression of school enrollment on relative age, relative age squared, a dummy for children born after the cutoff date and interactions of this dummy with relative age and relative age squared.

Figure 4E. Grade enrolled by date of birth for Hispanics, California

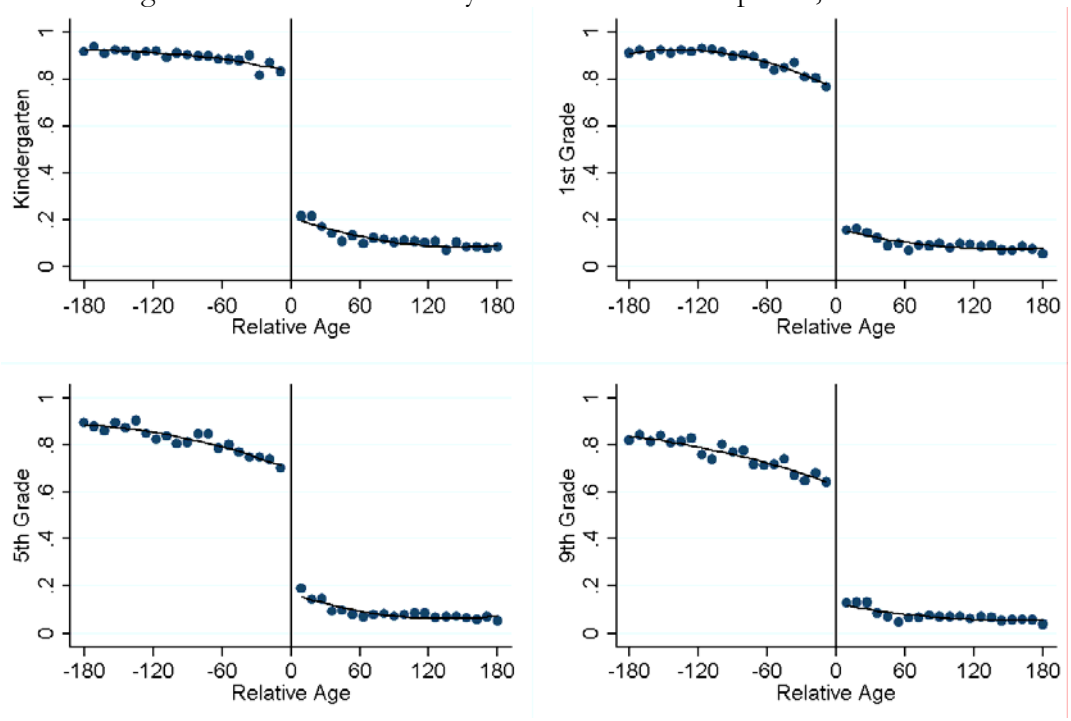
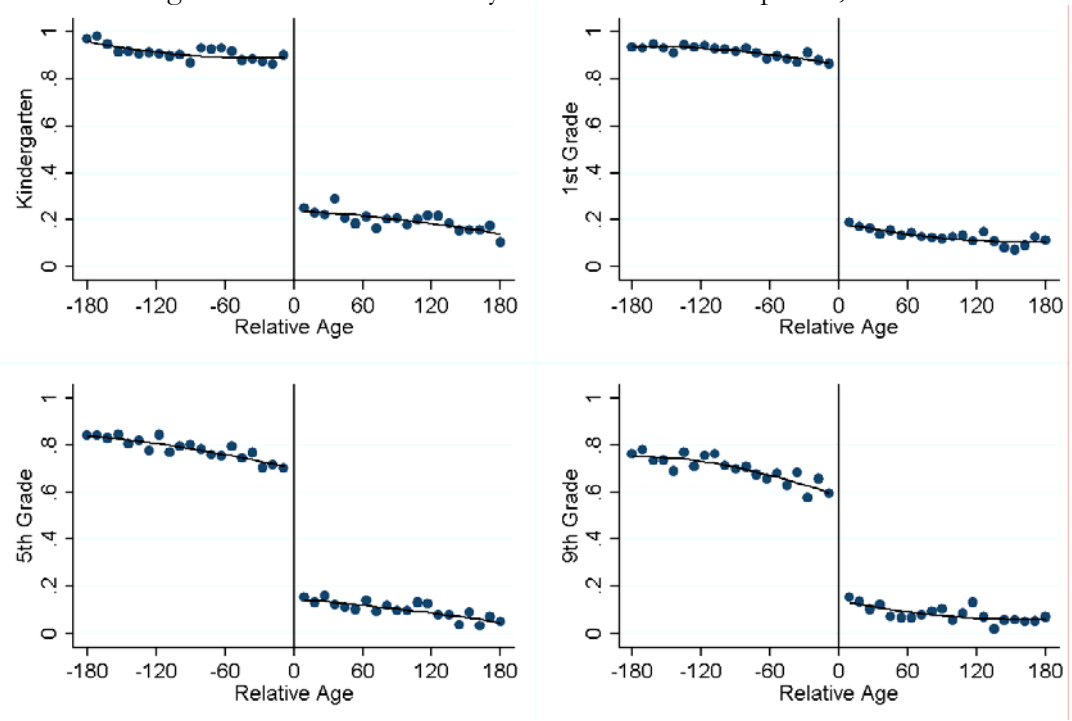
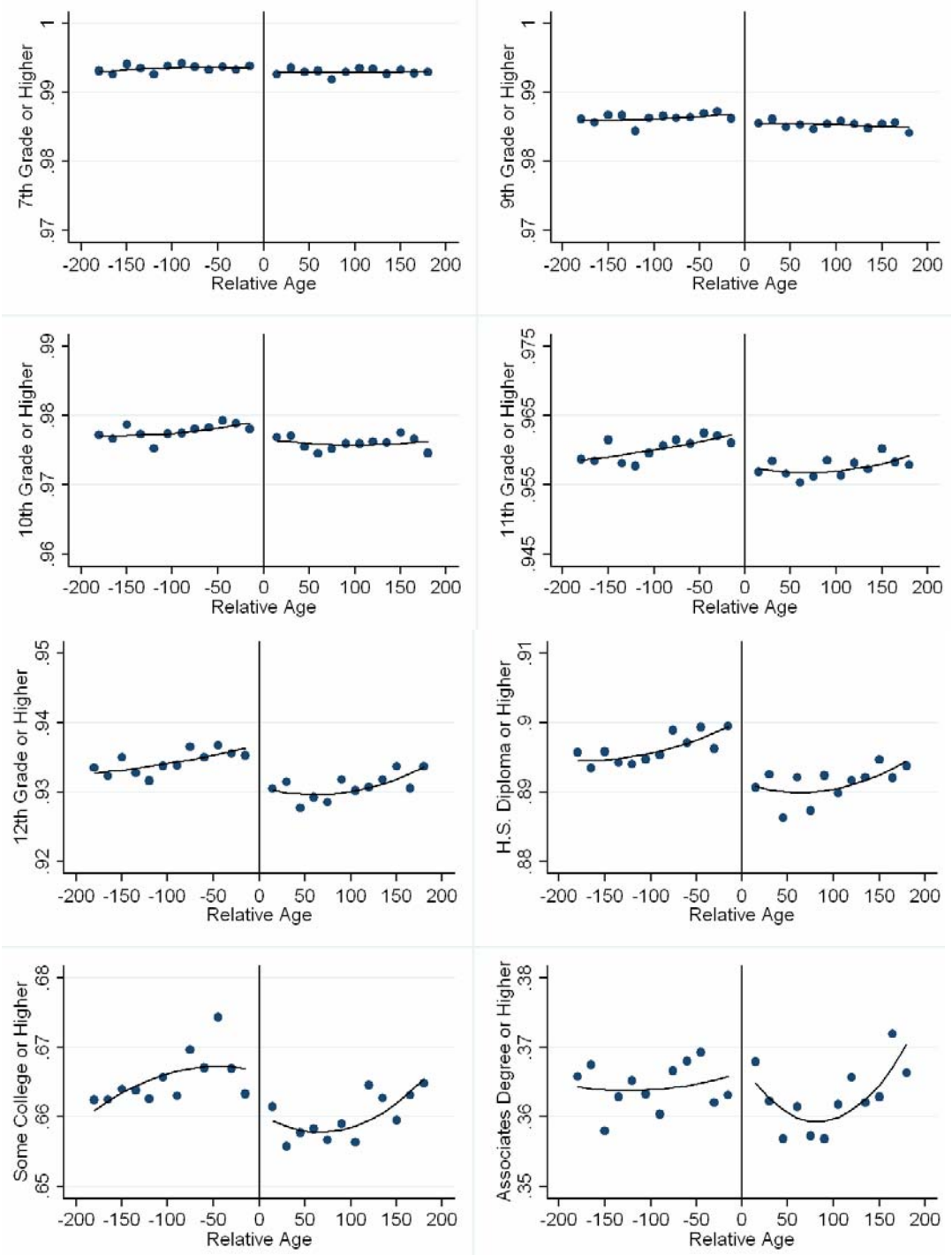


Figure 4F. Grade enrolled by date of birth for Hispanics, Texas



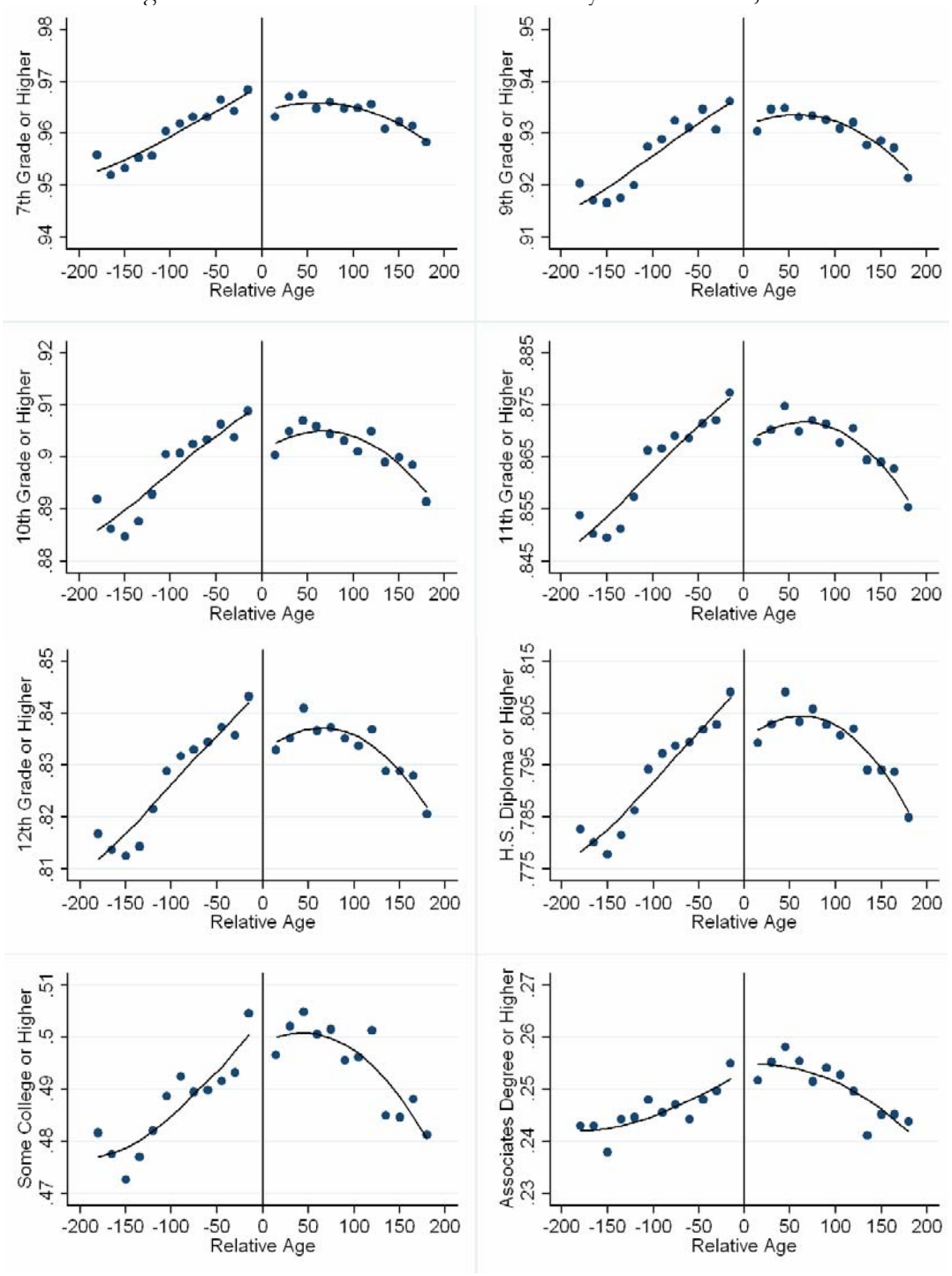
Notes: Figures 4E and 4F were estimated using the 2000 Decennial Long Form Census Data. Each figure shows the average enrollment for hispanic children of a certain age/grade group. For example, relative age equals to zero for kindergarten corresponds to a child of exact 5 years old (6 years old for 1st grade, 10 years old for 5th grade and 14 years old for 9th grade). Each dot represents the average enrollment by 9 day blocks of age. The solid line corresponds to an unconditional regression of school enrollment on relative age, relative age squared, a dummy for children born after the cutoff date and interactions of this dummy with relative age and relative age squared.

Figure 5A. Adult Educational attainment by Date of Birth, California



Notes: All panels in Figure 5A were estimated using the 2000 Decennial Long Form Census Data. Each figure shows the profile of average educational attainment for adults of with a certain educational attainment or higher. Each dot represents the average enrollment by 15 day blocks of age. The solid line corresponds to an unconditional regression of school attainment on relative age, relative age squared, a dummy for children born after the cutoff date and interactions of this dummy with relative age and relative age squared.

Figure 5B. Adult Educational attainment by Date of Birth, Texas



Notes: All panels in Figure 5B were estimated using the 2000 Decennial Long Form Census Data. Each figure shows the profile of average educational attainment for adults of with a certain educational attainment or higher. Each dot represents the average enrollment by 15 day blocks of age. The solid line corresponds to an unconditional regression of school attainment on relative age, relative age squared, a dummy for children born after the cutoff date and interactions of this dummy with relative age and relative age squared.

Figure 6A. Adult Long Run Outcomes, California

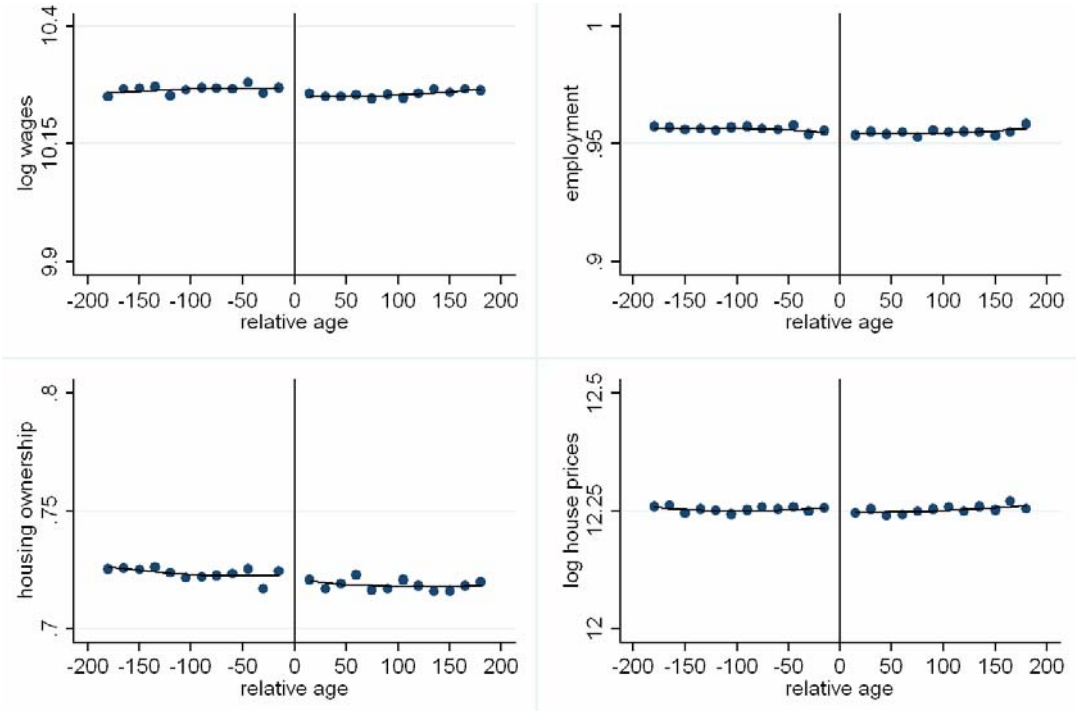
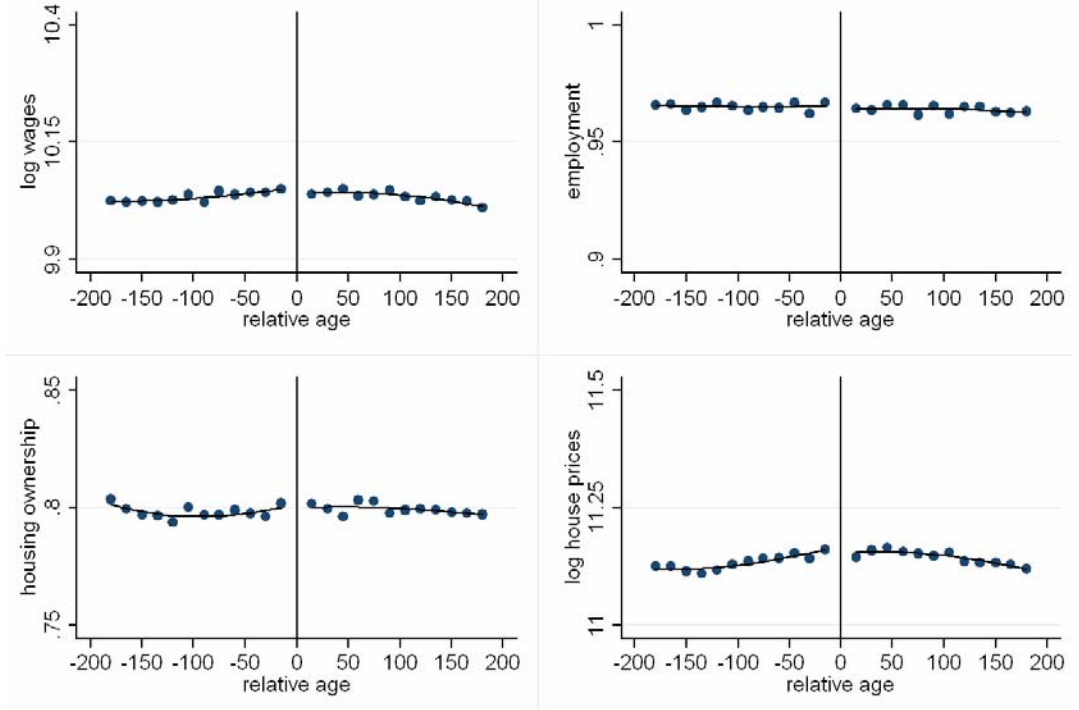


Figure 6B. Adult Long Run Outcomes, Texas



Notes: All panels in Figure 6A and 6B were estimated using the 2000 Decennial Long Form Census Data. Each figure shows a given outcome for 30-79 year olds in California and Texas. Each dot represents the average enrollment by 15 day blocks of age. The solid line corresponds to an unconditional regression of school attainment on relative age, relative age squared, a dummy for children born after the cutoff date and interactions of this dummy with relative age and relative age squared.

Table 1A: Impact of School Entry Laws on the Grade Students are Enrolled In, California

	Kindergarten		1st grade		5th grade		9th grade	
Discontinuity	-0.513	-0.527	-0.513	-0.503	-0.417	-0.417	-0.402	-0.399
	(0.014)	(0.011)	(0.012)	(0.012)	(0.014)	(0.014)	(0.014)	(0.013)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	NO	YES	NO	YES	NO	YES
Micro-data	NO	YES	NO	YES	NO	YES	NO	YES
Observations	46543		47792		50373		41842	

Table 1B: Impact of School Entry Laws on the Grade Students are Enrolled In, Texas

	Kindergarten		1st grade		5th grade		9th grade	
Discontinuity	-0.606	-0.618	-0.603	-0.591	-0.500	-0.497	-0.428	-0.425
	(0.016)	(0.014)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.013)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	NO	YES	NO	YES	NO	YES
Micro-data	NO	YES	NO	YES	NO	YES	NO	YES
Observations	31795		31946		33102		32494	

Notes: Tables 1A and 1b were estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on grade enrolled in at the state entry law cut off date. Each coefficent was estimated from a quadratic polynomial regression of enrollment in a certain school grade or higher on an indicator variable for people born after the cut off date, and on a set of date of birth controls that include: exact relative date of birth with respect to the cut off date, relative date of birth squared and interactions of the discontinuity dummy with relative date of birth and relative date of birth squared. The conditional regressions include the following covariates: gender, race, urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and lived in the same state 5 years ago. The unconditional regresions are estimated at the date of birth level of agregation, using 180 days of birth before and after each cut off date. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 2: Testing for Discontinuity in Observed Characteristics of Children and Their Families, California and Texas

	California				Texas			
	kindergarten	1st grade	5th grade	9th grade	kindergarten	1st grade	5th grade	9th grade
Observations per day	0.491	-1.552	2.775	1.008	-10.206	-1.297	-3.355	-0.288
	(5.751)	(6.061)	(5.675)	(3.982)	(6.208)	(5.834)	(5.254)	(4.419)
Male	0.000	0.012	0.001	-0.011	0.012	-0.027	-0.002	0.010
	(0.014)	(0.015)	(0.014)	(0.017)	(0.016)	(0.015)	(0.016)	(0.016)
White	0.000	-0.006	-0.036	0.010	0.009	-0.008	-0.005	0.009
	(0.013)	(0.013)	(0.014)	(0.015)	(0.018)	(0.020)	(0.018)	(0.018)
Black	0.003	0.008	0.008	-0.004	-0.001	0.022	0.005	0.005
	(0.007)	(0.008)	(0.007)	(0.008)	(0.011)	(0.012)	(0.012)	(0.011)
Hispanic	0.009	0.001	0.026	-0.002	-0.013	-0.020	0.006	-0.011
	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.017)	(0.016)	(0.016)
Log household income	0.049	0.013	-0.052	-0.036	0.036	-0.028	0.020	0.012
	(0.027)	(0.029)	(0.029)	(0.027)	(0.034)	(0.040)	(0.035)	(0.032)
Attending School	-0.209	0.000	-0.001	-0.001	-0.160	-0.005	-0.001	-0.002
	(0.011)	(0.004)	(0.003)	(0.002)	(0.011)	(0.004)	(0.003)	(0.003)
Private school	0.072	0.007	-0.005	-0.002	0.121	0.027	-0.001	0.001
	(0.013)	(0.010)	(0.008)	(0.009)	(0.013)	(0.010)	(0.009)	(0.007)
Lived in state 5 years ago	-0.004	-0.003	-0.003	-0.011	0.001	0.000	0.003	0.004
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Housing ownership	-0.011	0.017	0.001	0.005	0.006	0.024	0.029	-0.008
	(0.014)	(0.014)	(0.014)	(0.014)	(0.018)	(0.016)	(0.016)	(0.016)
Log house price	-0.029	0.011	-0.065	-0.034	0.010	-0.061	0.025	-0.070
	(0.032)	(0.032)	(0.029)	(0.026)	(0.047)	(0.038)	(0.034)	(0.033)
Observations	46543	47792	50373	41842	31795	31946	33102	32494

Notes: Table 2 was estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on the assigned variable at the state entry law cut off date Each coefficent was estimated from a quadratic polynomial regression of the assigned covariate on an indicator variable for people born after the cut off date, and on a set of date of birth controls that include: exact relative date of birth with respect to the cut off date, relative date of birth squared and interactions of the discontinuity dummy with relative date of birth and relative date of birth squared. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 3A: Demographic Characteristics of Those Children Born in the 30 days Before the Threshold by Grade Enrolled in, California

	Kindergarten		t-test	1st Grade		t-test	5th grade		t-test	9th grade		t-test
Enrolled in grade	Yes	No		Yes	No		Yes	No		Yes	No	
Male	0.476	0.603	-7.1	0.458	0.588	-7.7	0.444	0.600	-9.9	0.442	0.594	-9.0
White	0.305	0.609	-17.2	0.300	0.571	-16.3	0.333	0.565	-15.1	0.364	0.560	-11.7
Hispanic	0.504	0.281	13.1	0.513	0.319	11.8	0.485	0.310	11.6	0.424	0.326	5.9
Black	0.087	0.045	5.0	0.090	0.049	5.0	0.087	0.065	2.6	0.098	0.060	4.2
Log houshold income	10.582	10.954	-10.5	10.585	10.932	-10.6	10.685	10.891	-7.0	10.739	10.859	-3.6
Parental education	8.855	10.557	-14.2	8.845	10.172	-11.4	8.882	10.106	-11.1	9.168	9.955	-6.5
Private school	0.107	0.403	-17.8	0.075	0.227	-11.6	0.077	0.162	-8.0	0.075	0.124	-4.7
Housing ownership	0.498	0.657	-9.1	0.493	0.616	-7.3	0.551	0.641	-5.8	0.640	0.646	-0.4
Log house price	12.116	12.465	-9.4	12.039	12.410	-10.2	12.109	12.370	-8.3	12.141	12.302	-4.9
Observations	3,041	981		2,909	1,202		2,661	1,581		2,098	1,435	

Table 3B: Demographic Characteristics of Those Children Born in the 30 days Before the Threshold by Grade Enrolled in, Texas

	Kindergarten		t-test	1st Grade		t-test	5th grade		t-test	9th grade		t-test
Enrolled in grade	Yes	No		Yes	No		Yes	No		Yes	No	
Male	0.483	0.586	-4.4	0.486	0.662	-8.1	0.458	0.654	-10.7	0.460	0.612	-8.3
White	0.430	0.627	-8.5	0.408	0.693	-13.4	0.467	0.587	-6.4	0.513	0.584	-3.9
Hispanic	0.424	0.272	7.0	0.458	0.233	11.3	0.378	0.289	5.0	0.342	0.323	1.1
Black	0.134	0.097	2.5	0.123	0.070	4.4	0.135	0.113	1.8	0.130	0.088	3.6
Log houshold income	10.450	10.681	-4.5	10.461	10.648	-3.9	10.562	10.579	-0.4	10.629	10.566	1.8
Parental education	9.029	10.204	-7.8	9.023	10.134	-7.9	9.354	9.540	-1.5	9.436	9.193	2.0
Private school	0.088	0.316	-10.9	0.057	0.142	-5.8	0.054	0.094	-3.9	0.043	0.060	-2.1
Housing ownership	0.630	0.723	-4.3	0.629	0.699	-3.3	0.709	0.689	1.1	0.785	0.721	4.0
Log house price	11.046	11.414	-7.3	11.040	11.399	-6.9	11.079	11.246	-4.0	11.132	11.158	-0.7
Observations	2,551	534		2,325	618		2,064	1,051		1,822	1,200	

Notes: Table 3 was estimated using the 2000 Decennial Long Form Census Data.

Table 4A: Demographic Characteristics of Those Children Born in the 30 days After the Threshold by Grade Enrolled in, California

	Kindergarten		t-test	1st Grade		t-test	5th grade		t-test	9th grade		t-test
Enrolled in grade	Yes	No		Yes	No		Yes	No		Yes	No	
Male	0.489	0.519	-1.4	0.462	0.521	-2.7	0.457	0.519	-2.9	0.424	0.505	-3.3
White	0.270	0.401	-6.8	0.309	0.399	-4.4	0.311	0.412	-5.0	0.405	0.465	-2.5
Hispanic	0.495	0.446	2.3	0.439	0.447	-0.4	0.417	0.433	-0.7	0.349	0.384	-1.5
Black	0.124	0.065	4.4	0.152	0.074	5.2	0.138	0.079	4.1	0.132	0.066	4.1
Log houshold income	10.665	10.713	-1.2	10.706	10.713	-0.2	10.762	10.735	0.6	10.725	10.775	-1.1
Parental education	8.771	9.270	-3.1	9.079	9.307	-1.5	9.434	9.249	1.2	9.430	9.326	0.6
Private school	0.247	0.266	-1.1	0.187	0.122	3.9	0.130	0.096	2.4	0.113	0.099	0.9
Housing ownership	0.501	0.540	-1.8	0.586	0.542	2.0	0.571	0.599	-1.3	0.653	0.646	0.3
Log house price	12.183	12.191	-0.2	12.099	12.197	-2.2	12.251	12.151	2.4	12.202	12.193	0.2
Observations	677	3,060		611	3,262		652	3,500		479	2,937	

Table 4B: Demographic Characteristics of Those Children Born in the 30 days After the Threshold by Grade Enrolled in, Texas

	Kindergarten		t-test	1st Grade		t-test	5th grade		t-test	9th grade		t-test
Enrolled in grade	Yes	No		Yes	No		Yes	No		Yes	No	
Male	0.486	0.515	-1.3	0.449	0.505	-2.2	0.436	0.524	-3.4	0.413	0.537	-4.8
White	0.341	0.498	-7.0	0.308	0.482	-7.2	0.411	0.522	-4.2	0.491	0.547	-2.2
Hispanic	0.444	0.382	2.7	0.446	0.399	1.9	0.372	0.356	0.6	0.307	0.336	-1.2
Black	0.193	0.109	4.8	0.228	0.107	5.7	0.204	0.113	4.4	0.193	0.105	4.4
Log houshold income	10.445	10.525	-1.8	10.446	10.502	-1.0	10.513	10.602	-1.6	10.689	10.611	1.6
Parental education	9.029	9.449	-2.6	8.747	9.278	-3.0	9.397	9.267	0.8	9.828	9.216	3.8
Private school	0.264	0.231	1.6	0.194	0.088	5.4	0.122	0.057	3.8	0.075	0.052	1.8
Housing ownership	0.618	0.660	-1.9	0.647	0.667	-0.8	0.710	0.740	-1.3	0.743	0.748	-0.2
Log house price	11.060	11.121	-1.2	11.050	11.091	-0.7	11.151	11.116	0.7	11.123	11.091	0.6
Observations	595	2,146		439	2,421		411	2,550		424	2,574	

Notes: Table 4 was estimated using the 2000 Decennial Long Form Census Data.

Table 5A: Impact of School Entry Laws on the Grade Students are Enrolled In by Gender, Texas

	Kindergarten		1st grade		5th grade		9th grade	
Discontinuity for boys	-0.484	-0.485	-0.460	-0.456	-0.364	-0.362	-0.349	-0.341
	(0.018)	(0.014)	(0.014)	(0.014)	(0.017)	(0.017)	(0.017)	(0.017)
Discontinuity for girls	-0.540	-0.569	-0.565	-0.549	-0.472	-0.472	-0.462	-0.459
	(0.019)	(0.016)	(0.017)	(0.018)	(0.018)	(0.017)	(0.016)	(0.016)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	NO	YES	NO	YES	NO	YES
Micro-data	NO	YES	NO	YES	NO	YES	NO	YES
Observations - boys	23932		24339		25671		21398	
Observations - girls	22611		23453		24702		20444	

Table 5B: Impact of School Entry Laws on the Grade Students are Enrolled In by Gender, Texas

	Kindergarten		1st grade		5th grade		9th grade	
Discontinuity for boys	-0.597	-0.599	-0.556	-0.542	-0.429	-0.421	-0.391	-0.398
	(0.024)	(0.019)	(0.020)	(0.019)	(0.019)	(0.019)	(0.020)	(0.019)
Discontinuity for girls	-0.617	-0.636	-0.653	-0.645	-0.581	-0.579	-0.462	-0.457
	(0.023)	(0.020)	(0.015)	(0.015)	(0.017)	(0.016)	(0.018)	(0.017)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	NO	YES	NO	YES	NO	YES
Micro-data	NO	YES	NO	YES	NO	YES	NO	YES
Observations - boys	16206		16318		16927		16696	
Observations - girls	15589		15628		16175		15798	

Notes: Tables 5A and 5b were estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on grade enrolled in at the state entry law cut off date. Each coefficent was estimated from a quadratic polynomial regression of enrollment in a certain school grade or higher on an indicator variable for people born after the cut off date, and on a set of date of birth controls that include: exact relative date of birth with respect to the cut off date, relative date of birth squared and interactions of the discontinuity dummy with relative date of birth and relative date of birth squared. The conditional regressions include the following covariates: gender, race, urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and lived in the same state 5 years ago. The unconditional regresions are estimated at the date of birth level of agregation, using 180 days of birth before and after each cut off date. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 6A: Impact of School Entry Laws on the Grade Students are Enrolled In by Race, California

	Kindergarten		1st grade		5th grade		9th grade	
Discontinuity for whites	-0.429	-0.395	-0.370	-0.360	-0.303	-0.297	-0.275	-0.277
	(0.023)	(0.019)	(0.020)	(0.020)	(0.016)	(0.016)	(0.020)	(0.020)
Discontinuity for blacks	-0.502	-0.491	-0.507	-0.487	-0.389	-0.384	-0.394	-0.389
	(0.049)	(0.046)	(0.037)	(0.037)	(0.048)	(0.042)	(0.047)	(0.047)
Discontinuity for hispanics	-0.585	-0.632	-0.622	-0.612	-0.545	-0.540	-0.520	-0.509
	(0.017)	(0.014)	(0.014)	(0.013)	(0.021)	(0.019)	(0.018)	(0.017)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	NO	YES	NO	YES	NO	YES
Micro-data	NO	YES	NO	YES	NO	YES	NO	YES
Observations - whites	18333		18815		21516		19453	
Observations - blacks	3444		3709		4045		3335	
Observations - hispanics	20773		21182		20573		15501	

Table 6A: Impact of School Entry Laws on the Grade Students are Enrolled In by Race, Texas

	Kindergarten		1st grade		5th grade		9th grade	
Discontinuity for whites	-0.609	-0.603	-0.531	-0.524	-0.456	-0.448	-0.399	-0.399
	(0.023)	(0.018)	(0.020)	(0.019)	(0.020)	(0.019)	(0.019)	(0.018)
Discontinuity for blacks	-0.480	-0.534	-0.589	-0.568	-0.505	-0.478	-0.437	-0.445
	(0.041)	(0.039)	(0.038)	(0.037)	(0.041)	(0.036)	(0.042)	(0.039)
Discontinuity for hispanics	-0.643	-0.660	-0.698	-0.682	-0.569	-0.571	-0.459	-0.453
	(0.023)	(0.019)	(0.016)	(0.016)	(0.021)	(0.021)	(0.023)	(0.022)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	NO	YES	NO	YES	NO	YES
Micro-data	NO	YES	NO	YES	NO	YES	NO	YES
Observations - whites	15074		15404		17119		17610	
Observations - blacks	3939		3744		4055		3739	
Observations - hispanics	12371		12357		11511		10705	

Notes: Tables 6A and 6b were estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on grade enrolled in at the state entry law cut off date. Each coefficent was estimated from a quadratic polynomial regression of enrollment in a certain school grade or higher on an indicator variable for people born after the cut off date, and on a set of date of birth controls that include: exact relative date of birth with respect to the cut off date, relative date of birth squared and interactions of the discontinuity dummy with relative date of birth and relative date of birth squared. The conditional regressions include the following covariates: gender, race, urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and lived in the same state 5 years ago. The unconditional regresions are estimated at the date of birth level of agregation, using 180 days of birth before and after each cut off date. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 7A: Impact of School Entry Laws on the Grade Students are Enrolled In by Parental Education, California

	Kindergarten		1st grade		5th grade		9th grade	
Discontinuity for college or more	-0.416	-0.405	-0.384	-0.382	-0.285	-0.283	-0.302	-0.302
	(0.022)	(0.019)	(0.019)	(0.019)	(0.019)	(0.017)	(0.022)	(0.021)
Discontinuity for less than college	-0.564	-0.590	-0.579	-0.565	-0.487	-0.489	-0.456	-0.452
	(0.016)	(0.013)	(0.015)	(0.015)	(0.017)	(0.016)	(0.015)	(0.014)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	NO	YES	NO	YES	NO	YES
Micro-data	NO	YES	NO	YES	NO	YES	NO	YES
Observations - college or more	16217		16506		17720		15236	
Observations - less than college	30326		31286		32653		26606	

Table 7B: Impact of School Entry Laws on the Grade Students are Enrolled In by Parental Education, Texas

	Kindergarten		1st grade		5th grade		9th grade	
Discontinuity for college or more	-0.529	-0.524	-0.498	-0.482	-0.476	-0.464	-0.366	-0.366
	(0.031)	(0.027)	(0.023)	(0.022)	(0.024)	(0.024)	(0.022)	(0.021)
Discontinuity for less than college	-0.636	-0.660	-0.649	-0.636	-0.513	-0.513	-0.457	-0.453
	(0.017)	(0.015)	(0.015)	(0.015)	(0.016)	(0.015)	(0.017)	(0.016)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	NO	YES	NO	YES	NO	YES	NO	YES
Micro-data	NO	YES	NO	YES	NO	YES	NO	YES
Observations - college or more	10088		10152		10509		10310	
Observations - less than college	21707		21794		22593		22184	

Notes: Tables 7A and 7b were estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on grade enrolled in at the state entry law cut off date. Each coefficent was estimated from a quadratic polynomial regression of enrollment in a certain school grade or higher on an indicator variable for people born after the cut off date, and on a set of date of birth controls that include: exact relative date of birth with respect to the cut off date, relative date of birth squared and interactions of the discontinuity dummy with relative date of birth and relative date of birth squared. The conditional regressions include the following covariates: gender, race, urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and lived in the same state 5 years ago. The unconditional regresions are estimated at the date of birth level of agregation, using 180 days of birth before and after each cut off date. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 8A: Impact of School Entry Laws on School Attendance, California

	5th grade	6th grade	7th grade	8th grade	9th grade	10th grade	11th grade	12th grade
Discontinuity for females	-0.004	-0.005	0.001	0.004	-0.001	0.000	0.011	0.016
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)	(0.008)
Discontinuity for males	0.000	0.005	-0.003	0.000	-0.002	-0.001	0.001	-0.005
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.009)
Discontinuity for whites	0.002	0.001	0.003	0.004	-0.003	0.000	0.008	0.001
	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.004)	(0.005)	(0.007)
Discontinuity for blacks	-0.011	0.024	0.010	0.005	0.011	-0.005	-0.016	0.010
	(0.008)	(0.011)	(0.007)	(0.004)	(0.006)	(0.005)	(0.018)	(0.023)
Discontinuity for hispanics	-0.002	-0.002	-0.006	0.000	-0.003	0.000	0.007	0.013
	(0.004)	(0.003)	(0.004)	(0.003)	(0.005)	(0.006)	(0.007)	(0.011)
Discontinuity for college or more	-0.001	0.000	0.003	0.001	-0.001	-0.004	0.003	-0.004
	(0.004)	(0.003)	(0.003)	(0.004)	(0.002)	(0.003)	(0.006)	(0.006)
Discontinuity for less than college	-0.002	0.000	-0.003	0.002	-0.001	0.002	0.007	0.010
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)	(0.009)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES
Micro-data	YES	YES	YES	YES	YES	YES	YES	YES

Table 8B: Impact of School Entry Laws on School Attendance, Texas

	5th grade	6th grade	7th grade	8th grade	9th grade	10th grade	11th grade	12th grade
Discontinuity for females	-0.002	0.002	0.005	0.007	0.001	-0.003	0.003	0.039
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.005)	(0.007)	(0.014)
Discontinuity for males	0.003	-0.004	0.004	-0.001	-0.005	0.007	0.007	0.003
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.007)	(0.014)
Discontinuity for whites	0.001	-0.003	0.006	0.001	-0.003	0.001	0.015	0.015
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.006)	(0.012)
Discontinuity for blacks	0.015	0.005	0.003	-0.009	-0.015	0.012	0.018	0.060
	(0.007)	(0.008)	(0.007)	(0.006)	(0.007)	(0.009)	(0.015)	(0.023)
Discontinuity for hispanics	-0.006	0.002	0.002	0.010	0.004	0.000	-0.017	0.022
	(0.005)	(0.005)	(0.004)	(0.005)	(0.006)	(0.007)	(0.011)	(0.018)
Discontinuity for college or more	0.004	-0.006	0.004	-0.002	-0.003	-0.002	0.009	0.004
	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.004)	(0.007)	(0.013)
Discontinuity for less than college	-0.002	0.001	0.005	0.005	-0.002	0.004	0.003	0.031
	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.007)	(0.012)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES
Micro-data	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Tables 8A and 8b were estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on grade enrolled in at the state entry law cut off date. Each coefficent was estimated from a quadratic polynomial regression of attendance in a certain school grade or higher on an indicator variable for people born after the cut off date, and on a set of date of birth controls that include: exact relative date of birth with respect to the cut off date, relative date of birth squared and interactions of the discontinuity dummy with relative date of birth and relative date of birth squared. The conditional regressions include the following covariates: gender, race, urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and lived in the same state 5 years ago. The unconditional regresions are estimated at the date of birth level of agregation, using 180 days of birth before and after each cut off date. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 9A: Impact of School Entry Laws on Adult Education Attainment, California

	7th grade	9th grade	10th grade	11th grade	12th grade	high school	some college	college
Discontinuity	-0.0005	-0.0015	-0.0026	-0.0049	-0.0060	-0.0089	-0.0066	0.0000
	(0.0007)	(0.0009)	(0.0011)	(0.0016)	(0.0019)	(0.0023)	(0.0034)	(0.0036)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES
Observations	691219	691219	691219	691219	691219	691219	691219	691219

Table 9B: Impact of School Entry Laws on Adult Education Attainment, Texas

	7th grade	9th grade	10th grade	11th grade	12th grade	high school	some college	college
Discontinuity	-0.0034	-0.0042	-0.0068	-0.0084	-0.0088	-0.0077	-0.0015	0.0028
	(0.0015)	(0.0018)	(0.0018)	(0.0019)	(0.0022)	(0.0026)	(0.0031)	(0.0026)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES
Observations	767302	767302	767302	767302	767302	767302	767302	767302

Notes: Tables 9A and 9b were estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on education attainment at the state entry law cut off date. Each coefficent was estimated from a quadratic polynomial regression of attainment of a certain educational level or higher on an indicator variable for people born after the cut off date, and on a set of date of birth controls that include: exact relative date of birth with respect to the cut off date, relative date of birth squared and interactions of the discontinuity dummy with relative date of birth and relative date of birth squared. The conditional regressions include the following covariates: gender, race, urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 10A: Testing for Discontinuity in Observed Characteristics for 30 to 79 Year Olds, California

	1 if Male	1 if White	1 if Black	1 if Hispanic	1 if lived in state 5 years ago
Discontinuity	-0.002	0.002	-0.002	0.001	0.000
	(0.003)	(0.003)	(0.002)	(0.003)	(0.001)
Observations	691219	691219	691219	691219	691219

Table 10B: Testing for Discontinuity in Observed Characteristics for 30 to 79 Year Olds, Texas

	1 if Male	1 if White	1 if Black	1 if Hispanic	1 if lived in state 5 years ago
Discontinuity	-0.003	-0.005	0.000	0.006	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)
Observations	767302	767302	767302	767302	767302

Notes: Tables 10A and 10b were estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on the assigned Each coefficent was estimated from a quadratic polynomial regression of the assinged variable on an indicator variable for people born after the cut off date, and on a set of date of birth controls that include: exact relative date of birth with respect to the cut off date, relative date of birth squared and interactions of the discontinuity dummy with relative date of birth and relative date of birth squared. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 11A: Impact of School Entry Laws on Adult Education Attainment by Gender and Race, California

	7th grade	9th grade	10th grade	11th grade	12th grade	high school	some college	college
Discontinuity for females	-0.0003	-0.0011	-0.0023	-0.0069	-0.0066	-0.0107	-0.0119	-0.0046
	(0.0008)	(0.0011)	(0.0015)	(0.0020)	(0.0023)	(0.0028)	(0.0045)	(0.0049)
Discontinuity for males	-0.0008	-0.0019	-0.0028	-0.0028	-0.0052	-0.0070	-0.0009	0.0051
	(0.0010)	(0.0013)	(0.0014)	(0.0022)	(0.0028)	(0.0033)	(0.0050)	(0.0053)
Discontinuity for whites	-0.0004	-0.0010	-0.0020	-0.0031	-0.0052	-0.0074	-0.0075	-0.0026
	(0.0005)	(0.0007)	(0.0009)	(0.0015)	(0.0019)	(0.0024)	(0.0038)	(0.0046)
Discontinuity for blacks	0.0002	0.0009	0.0026	0.0024	0.0069	0.0046	0.0060	-0.0020
	(0.0023)	(0.0032)	(0.0040)	(0.0052)	(0.0070)	(0.0109)	(0.0141)	(0.0124)
Discontinuity for hispanics	-0.0016	-0.0056	-0.0073	-0.0149	-0.0144	-0.0207	-0.0024	0.0148
	(0.0026)	(0.0035)	(0.0045)	(0.0057)	(0.0065)	(0.0080)	(0.0086)	(0.0075)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES

Table 11B: Impact of School Entry Laws on Adult Education Attainment by Gender and Race, Texas

	7th grade	9th grade	10th grade	11th grade	12th grade	high school	some college	college
Discontinuity for females	-0.0023	-0.0043	-0.0059	-0.0077	-0.0100	-0.0077	-0.0027	0.0030
	(0.0019)	(0.0023)	(0.0026)	(0.0029)	(0.0032)	(0.0036)	(0.0049)	(0.0038)
Discontinuity for males	-0.0047	-0.0042	-0.0077	-0.0091	-0.0075	-0.0078	-0.0005	0.0024
	(0.0019)	(0.0027)	(0.0028)	(0.0031)	(0.0038)	(0.0042)	(0.0043)	(0.0040)
Discontinuity for whites	-0.0027	-0.0029	-0.0044	-0.0050	-0.0052	-0.0043	0.0024	0.0050
	(0.0009)	(0.0015)	(0.0019)	(0.0021)	(0.0024)	(0.0027)	(0.0035)	(0.0033)
Discontinuity for blacks	0.0040	0.0023	-0.0017	-0.0063	-0.0093	-0.0119	-0.0029	-0.0037
	(0.0035)	(0.0050)	(0.0056)	(0.0060)	(0.0081)	(0.0090)	(0.0097)	(0.0083)
Discontinuity for hispanics	-0.0099	-0.0124	-0.0173	-0.0207	-0.0202	-0.0165	-0.0123	-0.0009
	(0.0055)	(0.0065)	(0.0065)	(0.0068)	(0.0067)	(0.0071)	(0.0063)	(0.0050)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Tables 11A and 11b were estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on education attainment at the state entry law cut off date. Each coefficent was estimated from a quadratic polynomial regression of attainment of a certain educational level or higher on an indicator variable for people born after the cut off date, and on a set of date of birth controls that include: exact relative date of birth with respect to the cut off date, relative date of birth squared and interactions of the discontinuity dummy with relative date of birth and relative date of birth squared. The conditional regressions include the following covariates: gender, race, urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 12A: Impact of School Entry Laws on Adult Education Attainment by Age Groups, California

	7th grade	9th grade	10th grade	11th grade	12th grade	high school	some college	college
Discontinuity for 30-39 year olds	-0.0003	0.0004	-0.0012	-0.0047	-0.0058	-0.0064	-0.0068	0.0043
	(0.0008)	(0.0010)	(0.0016)	(0.0022)	(0.0029)	(0.0035)	(0.0053)	(0.0054)
Discontinuity for 40-49 year olds	-0.0004	-0.0014	-0.0030	-0.0055	-0.0069	-0.0082	-0.0065	-0.0006
	(0.0009)	(0.0011)	(0.0015)	(0.0022)	(0.0028)	(0.0035)	(0.0064)	(0.0067)
Discontinuity for 50-64 year olds	0.0000	-0.0007	-0.0013	-0.0008	0.0003	-0.0040	0.0005	-0.0039
	(0.0010)	(0.0014)	(0.0021)	(0.0030)	(0.0038)	(0.0042)	(0.0066)	(0.0062)
Discontinuity for 65-79 year olds	-0.0026	-0.0085	-0.0073	-0.0115	-0.0158	-0.0279	-0.0196	-0.0021
	(0.0037)	(0.0045)	(0.0054)	(0.0067)	(0.0076)	(0.0079)	(0.0115)	(0.0088)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES

Table 12B: Impact of School Entry Laws on Adult Education Attainment by Age Groups, Texas

	7th grade	9th grade	10th grade	11th grade	12th grade	high school	some college	college
Discontinuity for 30-39 year olds	0.0005	0.0001	-0.0018	-0.0043	-0.0043	-0.0011	0.0134	0.0096
	(0.0010)	(0.0017)	(0.0026)	(0.0030)	(0.0033)	(0.0041)	(0.0063)	(0.0052)
Discontinuity for 40-49 year olds	-0.0027	-0.0028	-0.0073	-0.0098	-0.0122	-0.0089	-0.0051	0.0011
	(0.0015)	(0.0023)	(0.0027)	(0.0030)	(0.0033)	(0.0040)	(0.0060)	(0.0051)
Discontinuity for 50-64 year olds	-0.0005	-0.0028	-0.0021	0.0001	0.0033	0.0012	0.0047	0.0054
	(0.0027)	(0.0035)	(0.0038)	(0.0041)	(0.0042)	(0.0040)	(0.0059)	(0.0056)
Discontinuity for 65-79 year olds	-0.0127	-0.0103	-0.0149	-0.0187	-0.0213	-0.0218	-0.0212	-0.0043
	(0.0042)	(0.0054)	(0.0055)	(0.0062)	(0.0069)	(0.0077)	(0.0074)	(0.0058)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Tables 12A and 12b were estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on education attainment at the state entry law cut off date. Each coefficent was estimated from a quadratic polynomial regression of attainment of a certain educational level or higher on an indicator variable for people born after the cut off date, and on a set of date of birth controls that include: exact relative date of birth with respect to the cut off date, relative date of birth squared and interactions of the discontinuity dummy with relative date of birth and relative date of birth squared. The conditional regressions include the following covariates: gender, race, urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 13A: Impact of School Entry Laws on Long Run Adult Outcomes, California

	1 if employed	not in labor force	log wages	log house income	house ownership	log house value	log rental price	number of rooms	1 if married
Discontinuity	0.0001	-0.0019	-0.0093	-0.0060	-0.0020	-0.0130	0.0020	-0.0307	0.0007
	(0.0018)	(0.0030)	(0.0084)	(0.0069)	(0.0030)	(0.0073)	(0.0072)	(0.0133)	(0.0024)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	499644	691219	479500	685956	691219	498332	183520	691219	691219

Table 13B: Impact of School Entry Laws on Long Run Adult Outcomes, Texas

	1 if employed	not in labor force	log wages	log house income	house ownership	log house value	log rental price	number of rooms	1 if married
Discontinuity	-0.0006	0.0063	0.0009	-0.0037	0.0016	-0.0064	0.0184	0.0066	0.0037
	(0.0015)	(0.0026)	(0.0075)	(0.0057)	(0.0024)	(0.0061)	(0.0078)	(0.0103)	(0.0019)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	504877	767302	496100	759276	767302	612831	137181	767302	767302

Notes: Tables 13A and 13b were estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on the assigned variable at the state entry law cut off date. Each coefficent was estimated from a quadratic polynomial regression of the assigned variable on an indicator variable for people born after the cut off date, and on a set of date of birth controls that include: exact relative date of birth with respect to the cut off date, relative date of birth squared and interactions of the discontinuity dummy with relative date of birth and relative date of birth squared. The conditional regressions include the following covariates: gender, race, urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 14A: Impact of School Entry Laws on Long Run Adult Outcomes by Gender, Race and Age Groups, California

	1 if employed	not in labor force	log wages	log house income	house ownership	log house value	log rental price	number of rooms	1 if married
Discontinuity for females	-0.0018	-0.0025	-0.0158	-0.0131	-0.0052	-0.0147	0.0158	-0.0593	0.0016
	(0.0027)	(0.0043)	(0.0153)	(0.0102)	(0.0042)	(0.0098)	(0.0099)	(0.0179)	(0.0029)
Discontinuity for males	0.0018	-0.0012	-0.0037	0.0016	0.0017	-0.0110	-0.0143	0.0004	-0.0004
	(0.0024)	(0.0037)	(0.0120)	(0.0092)	(0.0045)	(0.0105)	(0.0106)	(0.0199)	(0.0041)
Discontinuity for whites	-0.0021	-0.0012	-0.0063	-0.0064	-0.0001	-0.0165	-0.0014	-0.0433	-0.0012
	(0.0018)	(0.0034)	(0.0094)	(0.0076)	(0.0037)	(0.0082)	(0.0096)	(0.0164)	(0.0029)
Discontinuity for blacks	0.0130	0.0099	-0.0080	-0.0122	0.0082	0.0257	0.0087	0.0661	0.0087
	(0.0104)	(0.0134)	(0.0336)	(0.0331)	(0.0137)	(0.0264)	(0.0230)	(0.0569)	(0.0141)
Discontinuity for hispanics	0.0007	-0.0052	-0.0047	0.0109	-0.0104	-0.0006	0.0191	-0.0123	-0.0040
	(0.0056)	(0.0069)	(0.0174)	(0.0146)	(0.0085)	(0.0141)	(0.0145)	(0.0274)	(0.0058)
Discontinuity for 30-39 year olds	0.0022	-0.0090	-0.0048	0.0051	-0.0023	-0.0138	-0.0042	0.0009	0.0006
	(0.0031)	(0.0044)	(0.0150)	(0.0109)	(0.0062)	(0.0102)	(0.0075)	(0.0218)	(0.0057)
Discontinuity for 40-49 year olds	-0.0022	0.0010	-0.0184	-0.0124	-0.0035	-0.0184	0.0286	-0.0404	0.0001
	(0.0028)	(0.0052)	(0.0133)	(0.0113)	(0.0058)	(0.0117)	(0.0129)	(0.0241)	(0.0046)
Discontinuity for 50-64 year olds	-0.0025	0.0050	-0.0073	-0.0028	-0.0011	-0.0047	-0.0234	-0.0530	0.0015
	(0.0034)	(0.0070)	(0.0172)	(0.0129)	(0.0048)	(0.0142)	(0.0224)	(0.0254)	(0.0037)
Discontinuity for 65-79 year olds	0.0204	-0.0048	0.0176	-0.0308	0.0027	-0.0132	-0.0231	-0.0500	0.0012
	(0.0082)	(0.0087)	(0.0770)	(0.0206)	(0.0073)	(0.0189)	(0.0331)	(0.0366)	(0.0042)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES

Table 14B: Impact of School Entry Laws on Long Run Adult Outcomes by Gender, Race and Age Groups, Texas

	1 if employed	not in labor force	log wages	log house income	house ownership	log house value	log rental price	number of rooms	1 if married
Discontinuity for females	-0.0005	0.0043	-0.0147	-0.0008	0.0046	-0.0039	0.0250	0.0054	0.0025
	(0.0019)	(0.0037)	(0.0115)	(0.0083)	(0.0035)	(0.0071)	(0.0119)	(0.0153)	(0.0024)
Discontinuity for males	-0.0008	0.0088	0.0142	-0.0069	-0.0017	-0.0091	0.0106	0.0083	0.0052
	(0.0024)	(0.0034)	(0.0100)	(0.0074)	(0.0033)	(0.0101)	(0.0123)	(0.0157)	(0.0026)
Discontinuity for whites	0.0009	0.0073	0.0068	0.0014	0.0021	-0.0029	0.0205	0.0169	0.0028
	(0.0016)	(0.0030)	(0.0097)	(0.0073)	(0.0027)	(0.0077)	(0.0118)	(0.0118)	(0.0020)
Discontinuity for blacks	-0.0091	0.0086	-0.0213	-0.0105	0.0028	0.0152	-0.0079	0.0033	0.0089
	(0.0057)	(0.0087)	(0.0232)	(0.0207)	(0.0087)	(0.0183)	(0.0166)	(0.0367)	(0.0065)
Discontinuity for hispanics	-0.0014	0.0027	-0.0088	-0.0168	-0.0015	-0.0276	0.0336	-0.0310	0.0014
	(0.0042)	(0.0059)	(0.0156)	(0.0112)	(0.0064)	(0.0133)	(0.0141)	(0.0210)	(0.0050)
Discontinuity for 30-39 year olds	-0.0031	0.0070	0.0093	0.0007	0.0080	0.0122	0.0092	0.0326	0.0112
	(0.0027)	(0.0039)	(0.0123)	(0.0112)	(0.0052)	(0.0126)	(0.0119)	(0.0229)	(0.0044)
Discontinuity for 40-49 year olds	0.0006	0.0039	-0.0064	-0.0089	-0.0028	0.0118	0.0104	0.0063	0.0023
	(0.0025)	(0.0042)	(0.0139)	(0.0117)	(0.0051)	(0.0105)	(0.0152)	(0.0231)	(0.0036)
Discontinuity for 50-64 year olds	0.0018	0.0040	0.0006	0.0073	0.0001	-0.0221	0.0207	0.0002	-0.0020
	(0.0026)	(0.0059)	(0.0148)	(0.0106)	(0.0042)	(0.0111)	(0.0210)	(0.0215)	(0.0023)
Discontinuity for 65-79 year olds	-0.0058	0.0091	-0.0286	-0.0182	0.0015	-0.0285	0.0892	-0.0169	0.0030
	(0.0055)	(0.0063)	(0.0466)	(0.0144)	(0.0048)	(0.0145)	(0.0371)	(0.0241)	(0.0026)
Date of birth controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Tables 13A and 13b were estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on the assigned variable at the state entry law cut off date. Each coefficent was estimated from a quadratic polynomial regression of the assigned variable on an indicator variable for people born after the cut off date, and on a set of date of birth controls that include: exact relative date of birth with respect to the cut off date, relative date of birth squared and interactions of the discontinuity dummy with relative date of birth and relative date of birth squared. The conditional regressions include the following covariates: gender, race, urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.