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Abstract
Age based school entry laws force parents and educators to consider an important tradeoff: though students who are the youngest in their school cohort typically have poorer academic performance, on average, they have slightly higher educational attainment. In this paper we document that for a large cohort of California and Texas natives the school entry laws increased educational attainment of students who enter school early, but also lowered their academic performance while in school. However, we find no evidence that the age at which children enter school effects job market outcomes, such as wages or the probability of employment. This suggests that the net effect on adult labor market outcomes of the increased educational attainment and poorer academic performance is close to zero.

Keywords
Educational attainment, earnings

Disciplines
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Do School Entry Laws Affect Educational Attainment and Labor Market Outcomes?*

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Age based school entry laws force parents and educators to consider an important tradeoff. Though students who enter school early typically have poorer academic performance, on average, they stay in school longer. The popular and academic press has focused primarily on the former effect which has lead to substantial concern and strategic behavior on the part of both parents and educators. In this paper we document that the school entry laws have a very large impact on the age at which students enroll in school and that the youngest students in a class are substantially more likely to be held back a grade. However, we also find that the youngest students in an academic cohort have slightly higher educational attainment than their older peers. To estimate the net effect of this tradeoff we examine a broad range of labor market outcomes and find that early school entry has no effect on any of them. We also document that school entry laws are a poor instrument for educational attainment in wage regressions because they affect wages by numerous causal pathways, several of which have a substantially stronger first stage relationship with the school entry laws than educational attainment does.

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1. Introduction

Recently there has been substantial interest in the choice that parents face as they decide at what age to enroll their children in kindergarten. There have been flurry of papers documenting the adverse effects on academic performance of being the youngest in a classroom. In one of the better known articles in the literature Bedard and Dhuey (2006) use data from OECD to show that the youngest members of fourth and eighth grade classes have standardized test scores that are 2-12 percentiles lower than the oldest students in the same cohort. Similarly, Datar (2006) used variation in school entry cut off dates to document that children that start kindergarten later get higher test scores.[1] In another recent paper, Elder and Lubotsky (2006) used the Early Childhood Longitudinal Study to document that a one year increase in the age at which an individual enters school reduces the probability they will be held back a grade at some point in elementary school by 10 to 18 percent. Finally, some studies focused on other countries find a negative impact on adult outcomes.[2] These findings have lead to substantial concern among both parents and educators about the effect of the school entry laws. Legislators in several states have changed their school entry dates in order to increase the age at which children enter kindergarten.[3]

All these results from the literature suggest that enrolling children in kindergarten as soon as they are eligible may be adversely affecting them. However, as we document in this paper, there is at least one positive effect of enrolling in kindergarten at the earliest age possible. The youngest students in a class complete high school at higher rates than their older peers.[4] This suggests that there is an important tradeoff to consider. The first contribution of this paper is to provide estimates of the net *long run* impact of these opposing mechanisms on labor market outcomes. We believe that in addition to getting at the net

[1] A current debate in the education literature tries to understand if the cause of this academic disadvantage for young kids is due to their relative age to peers or due to their absolute age at which they are exposed to a material. For a review of this debate see Stipek (2002).
[2] Allen and Barnsley (1993) report that boys born after the cut off date for school eligibility in Canada are more likely to thrive in professional sports, and Fredriksson and Ockert (2005) found a negative impact on wages for the youngest individuals in a cohort in Sweden.
[3] See Bedard and Dhuey (2007).
[4] Angrist and Krueger (1991) originally showed that individuals born in the 1st quarter of the year have lower education attainment than individuals born in the 4th quarter of the pervious year. Such difference was arguably due to the interaction of compulsory schooling laws with school entry laws, which makes individuals born in the first quarter more likely to start school later and therefore more likely to quit formal education before completing a high school degree. Angrist and Krueger (1992) also pointed out that they only used quarter of birth because a large data set with both exact day of birth and education attainment did not exist at that time, and therefore they could not explicitly examine the impact of school entry laws on education attainment.

effect of the tradeoff described above, the adult outcomes we focus on are of greater interest than the intermediate outcomes, such as academic performance, that are typically considered in the literature. The second contribution of this paper is to reexamine the value of the school entry laws as an instrument for adult educational attainment.

To conduct our analysis we use the restricted access Decennial Census Long Form Data for the states of California and Texas.[5] Unlike the publicly available micro sample (PUMS), the restricted-access data has the *exact day of birth* for each individual for a 15% random sample of the population of each state. This is the first study we are aware of using a data set large enough get precise measures of the impact of the timing of school entry on adult outcomes for the United States. Our research design uses state school entry laws that regulate the minimum age at which students are eligible to enroll in school as a source of exogenous variation in the timing of school entry.[6] We take advantage of these threshold dates to implement a regression discontinuity design (RDD). The RDD lets us estimate the short run and long run consequences of early school entry, by comparing individuals who are similar on all dimensions, but enter school at different ages on account of the school entry laws.[7]

We find that the school entry laws have a very significant effect on the age at which students enroll in school. Five year olds in California who are born just before the cut off date for early school entry are 53 percentage points more likely to enroll in kindergarten than five year olds born just after the cut off date. This difference is not 100 percentage points because some parents decide not to enroll their children in school as soon as they are eligible and some do not comply with the law. However by ninth grade, the magnitude of the discontinuity in school enrollment is reduced to 40 percentage points. This partial 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.

Compliance with the school entry laws varies substantially by gender, race and parental education. Girls, minorities and the children of parents with less than a college

[5] We use those states because of their large and diverse population, and due to the availability of data.
[6] The state of Texas requires that a child must be at least 5 years old by September 1st in order to enroll in kindergarten that academic year, while the threshold date is December 2nd in California for most of the age groups we examine.
[7] Cascio and Lewis (2006) used a similar design to estimate the impact of schooling on AFQT performance. 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.

education are disproportionately entering school as early as legally possible. Moreover, minorities and children of parents with low educational attainment are considerably less likely to be held back a grade. As a result they are disproportionately the youngest students enrolled in a given grade, which means they bear an excess share of the negative consequences on learning that arise from early school entry.

The large difference in educational attainment that exists while children are still in school does not persist in its entirety into adulthood. The *permanent* impact of school entry laws on education attainment can only be estimated for age groups that have completed their education.[8] For this reason when examining adult educational attainment we restrict our sample to individuals over the age of 30. Our figures show that adults born right before the cut off for school entry in Texas and California are almost a percentage point more likely to complete high school. They are also about a half percentage point more likely to complete 9th, 10th and 11th grades.

Interestingly, we find no evidence that school entry laws and the additional education that results from them leads to differences in employment rates, wages, or in any of the other long run outcomes we observe in the Census, such as family income, house ownership, house value and marital status.[9] We find no evidence that early school entry has an impact on adult outcomes for any of the subgroups we examine. Not even the minority groups that are most effected by the law. This suggests that some of the concern on the parts of parents and educators with respect to the poor academic performance of the youngest kids in a cohort may be misplaced.

Our second contribution is to give some insight into the value of school entry laws an instrumental variable for educational attainment.[10] As we document there are multiple causal channels other than educational attainment through which the school entry laws can

[8] McCrary and Royer (2006) for example estimated the impact of temporary differences in educational attainment on the birth outcomes of young mothers using exact day of birth and school entry laws.
[9] One important caveat explained in section 4 is that while in Texas the school entry cut off date has been September 1st since 1915, the December 2nd threshold may have changed for the state of California prior to 1951, which makes the long run estimates less compelling for that state. Also, our analyses are based on a cross-section of all cohorts in the year 2000, where exact day of birth is available. Therefore, we cannot link individual academic performance while in school with individual education attainment and labor market outcomes over time. The large panel dataset necessary to conduct this analysis does not exist for the United States.
[10] Bound, Jaeger and Baker (1995) demonstrated some of the problems with using quarter of birth as instrumental variable. Most of the issues raised in this paper concern the more general school entry law instrument.

affect adult labor market outcomes. Though the children entering school at a younger age due to the school entry laws get slightly more education they are also much more likely to repeat a grade. They are also younger than their peers and are being taught particular material at a younger biological age. These three causal mechanisms affect an order of magnitude more students than are affected by the additional education which is the focus of the returns to education literature. An additional concern is that there is substantial variation in compliance by race and gender and much of the original "assignment" is undone by retention before a student completes high school. This suggests that any estimate using this instrument tells us little about the general population because the "compliers" are clearly a highly selected group. One final problem peculiar to the quarter of birth version of this instrument is that as we document there is substantial seasonality in educational attainment which will swamp the effect of the school entry laws on educational attainment.

The rest of the paper is organized as follows: in section 2 we discuss the empirical model and data sources. 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, adult educational attainment and labor market 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. Because age is measured in days we use a parametric rather than a nonparametric approach.

The first outcome we examine is school enrollment for which we estimate the following second order polynomial equation:

$$(1)\ Enroll_i = \beta_0 + \beta_1 Cut_i + \beta_2 Bday_i + B_3 Cut_i * Bday_i + \beta_4 Bday_i^2 + \beta_5 Cut_i * Bday_i^2 + \Pi X_i + \varepsilon_i$$

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 parameter β_1 reveals 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. An estimate of β_1 equal to -1 would indicate full compliance with the law and an estimate 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 unconditional means of the outcome. The figure lets us visually check to be sure that there is a discrete break in the outcome and that 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.[11]

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

$$(2)\ Educ_i = \delta_0 + \delta_1 Cut_i + \delta_2 Bday_i + \delta_3 Cut_i * Bday_i + \delta_4 Bday^2 + \delta_5 Cut_i * Bday^2 + \Psi \tilde{X}_i + \mu_i$$

where $Educ_i$ is an indicator variable that takes on a value of 1 if individual i has completed more than a particular number of years of education. For example, the 10^{th} grade indicator variable is equal to 1 if the individual has completed at least 10^{th} grade, and zero otherwise. We run separate regressions for each cut off between 7^{th} grade and college. The use of indicator variables for completed years of education makes it possible to determine at what points in the distribution of educational attainment the school entry laws have their impact.[12]

[11] See Porter (2003) for an additional discussion on the appropriate choice of polynomial order.
[12] Another common way of defining education attainment in the literature on the returns to education is by calculating the number of years of formal schooling. Given that high retention rates make this variable difficult to interpret and that for reasons we document below we will not be estimating any structural equations for the return to education we prefer to focus on the impact of the school entry laws on the distribution of educational attainment.

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

$$(3)\ Y_i = \gamma_0 + \gamma_1 Cut_i + \gamma_2 Bday_i + \gamma_3 Cut_i * Bday_i + \gamma_4 Bday^2 + \gamma_5 Cut_i * Bday^2 + \Sigma \tilde{X} + \nu_i$$

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 school entry 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 would be an unbiased estimate of the impact of educational attainment on the outcome for people who comply with the law.[13] We do not construct this statistic because the school entry laws result in differences in retention rates and test scores that are very likely to affect adult wages and this violates the assumptions under which the instrumental variable estimate is identified.[14]

One appealing property of the RDD is that it is possible of to assess the probability of specification error fairly directly. All potential confounders must evolve smoothly across the discontinuity for the RDD to generate consistent estimates. 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. Though of course it is not possible to check the unobservable characteristics directly it is likely that if the observable characteristics do not change discretely at the school entry cut off date, then the unobservable characteristics are not changing discretely at the threshold either and that therefore omitted variables bias is not a problem. We have the additional advantage that in this setting most kinds of selection would result in a sorting of births around the discontinuity. To make sure that this is not occurring we check that the number of individuals born on a given day does not change discretely at the threshold for school entry.

All equations above are estimated using the 2000 Decennial Census Long Form data for the states of California and Texas (approximately 15% of the population in each state).

[13] For a detailed discussion of the Local Average Treatment Effect (LATE) see Angrist and Imbens (1994).

[14] We do not estimate the instrumental variable returns to education in this paper since there cannot be any other direct association between day of birth and labor market outcomes for day of birth to be a legitimate instrument for education attainment.

In addition to all the variables available in the IPUMS, these restricted access data also have the exact date of birth for every individual in the sample. To the best of our knowledge this is the first study that precisely estimates the impact of school entry laws and early school entry on long run adult outcomes using exact day of birth. One limitation of the Census data is that we observe a cross-section for each age group rather than the full educational path of each individual.[15]

Another limitation relates to the high proportion of individuals who are immigrants or migrants from other states. We deal this issue by restricting the sample to individuals born in California and Texas who are still living in the state they were born in. Although it is possible that someone born in California attended school outside California and then returned to California this is probably not a very common occurrence. Another more plausible concern is that there is selective migration by people on one side of the discontinuity or the other. To make sure neither of these is a significant problem we check that to be sure that neither the migration rate nor the population count changes discretely at the cut off date for school enrollment. We also eliminate all Census records where date of birth, educational attainment or school enrollment are imputed, since measurement error in the first variable will result in attenuation bias and measurement error in the other two variables will reduce the precision of our estimates.

3. Effect of School Entry Laws on Student Achievement

In this section we examine the impact of school enrollment laws on school age children. We start by documenting 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 children who are very young for their grade are much more likely to be held back during elementary school. Finally we document that compliance with the law and retention rates vary significantly by race, gender and parental education, and that school entry laws have no impact on drop out rates.

3.1. Children's School Enrollment

[15] Though a large panel dataset of US residents would make for a more powerful analysis this dataset does not exist.

In Figure 1A we present estimates of the proportion of individuals in California who are enrolled in kindergarten or a higher grade around age 5.[16] The proportion enrolled is plotted over the support of the number of days from an individual's birthday to the cut off date for entry into kindergarten in California. For example, an individual born on November 23rd would have a relative age of -10 and would be eligible to enroll in kindergarten. 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. The other figures described in this section have the same structure though the age group and outcomes examined vary.[17]

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. As can be seen from the figure this phenomenon is most pronounced among children who are barely eligible for kindergarten. Nonetheless, 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 school entry laws shrinks as children get older. In Figure 1B 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 more often than their older peers. The retention rates vary substantially, while 31% of the students born

[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 (age 5), first grade (age 6), fifth grade (age 10) and ninth grade (age 14). For the adult population we are able to analyze the complete distribution of education attainment since there is no need to look at enrollment rates.

[17] 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.

just before the cut off date are retained at some point between kindergarten and ninth grade only 11% of those born 180 days before the cut off date are retained.[18]

In Figure 1C we present the corresponding four figures for Texas. The pattern in Texas is similar to the one in California, though parents in Texas are more likely than parents in California to enroll their children in school as soon as it is legally possible. The figures reveal that in Texas 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 students in each class are being held back at considerably higher rates than their older peers. In fact, 35% of the students born just before the cut off for school entry, who enroll in kindergarten as soon as they are legally allowed, are held back at some point between kindergarten and ninth grade while only 19% of the students born 180 days before the cut off date are held back.

In Tables 1A and 1B 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 regression estimates of the discontinuous change in grade enrollment induced by the school entry laws 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. Table 1B shows that though the discontinuity in enrollment in kindergarten is much larger in Texas than in California, 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 to demographic factors that change abruptly at the school enrollment threshold rather than the legislation. That we see large discontinuities around the threshold date in both California and Texas despite the fact that they have different cut offs for school enrollment suggests that the differences we observe

[18] 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 180 days before the cut off date. This calculation ignores the possibility that there are significant differences across cohorts.

are due to the school entry laws. That adding 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 laws. 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 at the cut off date for early school entry. For there to be an abrupt change in children's characteristics at the threshold it would be necessary for some subpopulation to be timing their births so as to put them on one side of the threshold or for there to be selective migration. 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 shows that gender, race, household income and house ownership evolve smoothly through the cut off date for early school entry. The variable state of residence in 1995 is also continuous around the threshold, indicating that selective migration is not a problem. The only discrete change 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 this difference is due to parents using private schools to work around the school entry laws.[19] This difference almost completely disappears by age 6 as most of the children in private kindergartens enter the public school system for first grade.

3.2. Compliance with the Law

The figures above revealed that there is a significant amount of non-compliance with the law, with some parents enrolling their children in school earlier than the law permits. In addition, some parents defer enrolling their children in kindergarten by a year.[20] In Tables 3A and 3B we examine the second of these issues, by looking at the characteristics of children who were born 30 days before the cut off date for school entry. The first three columns of Table 3A present the results for children who are eligible to enroll in kindergarten that year in California. The first column contains the characteristics of the students who actually enter

[19] 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.
[20] This does not require working around the law as the law does not require parents to enroll their children in kindergarten as soon as they are eligible.

kindergarten. The second column presents the characteristics of students who 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 they are 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 and find a similar pattern though the differences between the two groups are smaller.

In Tables 4A and 4B we examine the characteristics of children who 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 from the governing board of the school district to enroll their children in kindergarten a year early. As noted above, some parents are enrolling their child in private kindergartens which are not governed by the laws and then moving their child into the public schools for first grade. These two tables reveal that minorities, less educated parents and the 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 Drop Out Rates by Gender, Race and Parental Education

On account of how much compliance with the law varies across demographic groups, we conduct a separate examination of the enrollment patterns for each group. In Figure 2A we show how the discontinuity in school enrollment in California evolves as children age. We do the analysis separately by gender, race and parental education.[21] The first noticeable pattern is that all groups experience a large reduction in the estimated discontinuity as they get older, which is consistent with the results observed for the whole population. Second, the ranking of groups by size of discontinuity is preserved across grades: whites and children of parents with more than a college degree have the lowest compliance rate and therefore the smallest discontinuities in the grade they are enrolled in. Hispanics and

[21] The tables with underlying estimates and standard errors are available upon request.

parents with less than a college degree have the largest discontinuities. Girls also comply with the law at higher rates than boys, but the largest difference is between whites and Hispanics. Results for Texas are displayed in Figure 2B and show similar patterns, although the differences between groups are smaller.

The pattern of retention rates within a cohort also varies substantially by race. In Figure 3A we plot the cumulative differences in grade retention between the youngest students in a cohort – born right before the cut off date - and the students born 180 days prior to the cut off date in California. For example, the youngest whites in 1st grade are approximately 17 percentage points more likely to be held back than white children in the same cohort who are six months older. By 9th grade, this difference is 25 percentage points for whites. Minorities are much less likely to be held back; by ninth grade blacks and Hispanic born right before the cut off data are only 16 percentage points more likely to be retained then those born 180 days before the cut off date. Though we also observe differences by gender and parental education, the most striking differences are across race. The results for Texas presented in Figure 3B are very similar to the results for California though the retention rates are considerably lower.

The main conclusion from examining the figures above is that blacks and Hispanics are much more likely than whites to enroll in school as soon as they are eligible, and they are also less likely to be held back. The net result is that black and Hispanic children are on average exposed to academic material at a considerably younger age than white children. It is unlikely that blacks, Hispanics and children with less educated parents are at a higher grade level than the children of whites and of parents with more education because they are outperforming them. A more plausible explanation is that black and Hispanic parents are less likely to make the decision to have a child who is struggling held back a grade.[22] This suggests that this is the population for which the school entry laws are most likely to have an effect on labor market outcomes.

Finally we examine if the age at which a child enters school affects the probability that they will still be attending school (not have dropped out of school) by a particular age. Tables 5A and 5B show estimates of the discontinuity in school attendance rates by gender, race and parental education, at ages 10 to 17 in California and Texas respectively. We find no

[22] It is also possible that the patterns we observe in retention rates are due to systematic differences in the schools these groups are attending.

evidence that early school entry affects the probability that a child will still be attending school at any of these ages. This result is not surprising since the current compulsory schooling laws in Texas and California require that children attend school until they are 18 years old.

Overall, we found that students born right before the cut off date for school enrollment are significantly more likely to enroll in kindergarten a year earlier than similar students who were born right after the cut off date. One third of these initial differences disappear by 9^{th} grade since the youngest children in a cohort are held back more often than their older classmates. Minorities are more likely to comply with the law than whites and they are held back less frequently, therefore they are disproportionately bearing the burden of being the youngest student in a cohort. All these results indicate that school entry laws are important for young cohorts, and suggest that they may have an impact on adult outcomes. In the next section we will estimate the impact of these laws on educational attainment and labor market outcomes.

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 induce long-term differences in educational attainment that are much smaller than the discontinuities observed while individuals are still enrolled in school. Surprisingly the laws increase the probability of completing everything from 9^{th} grade through High School. However, we find no evidence that the laws had a net impact on labor market outcomes such as employment rates and wages, or in other outcomes such as the probability of homeownership.

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 17 years old, it is still possible that there are differences in educational attainment among adults. This could occur either because the differences appear after children turn 17 or because school enrollment laws had a more pronounced effect on older cohorts. Although the data from the 2000 Census does not allow us to examine the enrollment patterns of older cohorts when they were attending school, the evidence from the literature on compulsory schooling indicates that the laws had a larger impact on older cohorts.[23]

There is variation both across states and in California over time in the cut off date for school entry. The Texas Education Agency informed us that the September 1st threshold was first implemented in 1915 and it has remained the same since then. However, the State of California has only used the cut off date of December 2nd since 1987. Between 1951 and 1987 the statute read 'be four years and 9 months of age on or before September 1st', which in practice means a threshold date of December 1st. Because of this in California we eliminate people born December 2nd from our estimates, and compare individuals born December 1st with those born December 3rd. Finally, there is some variation in the cut off date prior to 1951 which makes it impossible to ascertain which cut off date people faced without knowing which school they attended, something we do not observe in the Census.[24] Given this ambiguity for older cohorts in California, we first present the results for all adults in Texas, and then complement the analysis with estimates for California and for various age groups in both states.

In Figure 4B we present the profile of educational attainment by birthday for Texas natives between the age of 30 and 79. In each panel of the figure we plot the proportion of Texas natives born in a fifteen day period that have completed a particular grade or higher. Surprisingly, the figure reveals a very pronounced seasonality in educational attainment.[25] This seasonality will cause substantial problems for instrumental variables for educational attainment that are based on quarter of birth, and further reinforce the importance of using exact day of birth to focus directly on the discontinuity caused by the school entry laws. Despite the fact that the seasonality makes the figure harder to interpret, we see evidence of a seam in educational attainment in Texas. Adults born just before the school entry cut off are slightly more likely to have completed 9th, 10th, 11th, 12th grade, and received a H.S.

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[23] The compulsory schooling age was also lower than 18 years for older cohorts.

[24] Prior to 1951 not everyone in California faced a December 1st cut off date. In 1917 the Political Code Ch 552 Sec 9 states that to enroll in first grade children had to be 6 years old at the end of the third month of the school term (in this period the focus was on first grade because kindergarten enrollment was very low). This is likely to fall near December 1st. But in 1941, section 3.122 of the School Code was amended so that in schools with one term children had to have their birthday by March 1st. In schools with two terms they had to have their birthday by December 1st to be admitted for the first term, and by May 1st to be admitted to the Second term. In 1945 the education code was amended so that children in schools with either one term or two terms had to turn five by March 1st to be eligible for kindergarten. In 1951 the Education Code was amended so that Children who had turned five by December 1st were eligible for Kindergarten.

[25] This seasonality was also observed in other contexts, such as birth outcomes - see Lam and Miron (1991).

diploma than those just born after the cut off. We do not find compelling evidence that the school enrollment laws increase college attendance though the estimates are fairly imprecise.

In Table 6B we present regression estimates of the impact of school entry laws on the educational attainment of Texas natives. Each regression is estimated off the micro data and includes each individual's demographic characteristics.[26] 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 9^{th}, 10^{th}, 11^{th}, 12^{th} grade and receiving a H.S. diploma. The respective increases at the discontinuity are: 0.4%, 0.7%, 0.8%, 0.9% and 0.8%. These seams are much smaller than the discontinuities observed for school age cohorts.

In Figure 4A we present the profile of educational attainment for adults in California. As can be seen in the graph, adults who were just barely eligible to enter school are slightly more likely to have completed 11^{th} grade, 12^{th} grade or received a H.S. diploma. As with the results for adults in Texas there is no compelling evidence of differences in rates of college entry or completion of an Associates degree. These results are confirmed in Table 6A. The discontinuity estimates for 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 H.S. diploma and it is slightly under a percentage point. The other effects are slightly smaller than the ones observed in Texas, particularly for the lower grades.

There are a couple of possible causes of the abrupt jump 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 school attendance laws. That we do not observe an impact on the probability of attending college is consistent with this story, but it is not clear why the laws would generate discontinuities at so many points in the distribution of educational attainment. Another possible explanation 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. One final 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.

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

That the coefficients 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 to make sure that there are no abrupt changes in the proportion of the population that is male, white, black, Hispanic or a recent immigrant to the state. We present the results of this exercise in Tables 7A and 7B which reveal that these observable characteristics evolve smoothly through the cut off for school entry.

4.2. Labor Market and Other Long Run Outcomes

There are a couple of ways in which the early school entry laws could have an impact on labor market outcomes. One mechanism is through the increase in the educational attainment documented above, which would have a *positive* impact on wages for individuals born right before the threshold date. However the school entry laws could also have a *negative* impact on the wages of these individuals because being the youngest in their class adversely affects their academic performance as evidenced by their higher retention rates during elementary school. We estimate the net long run effect of these opposing mechanisms by comparing the labor market outcomes of individuals born right before the cut off date with the outcomes of individuals born right after the cut off date for school entry.[27]

In Figure 5B we document the effect of the school entry laws on labor market outcomes, such as employment and wages, and also on home ownership and house prices. We find no evidence the laws have an effect on any of those outcomes. RDD estimates are presented in Table 8B and corroborate what we observe in the figures. In the tables we also include the results for household income and marital status for which we also find no effect. The estimates for all outcomes are statistically and practically insignificant. For example, the change in log wages at the cut off date is only 0.0009, while the change in the probability of employment is -.0006. The results for California displayed in Table 8A and Figure 5A show similar patterns, although we should be cautious about those estimates given the uncertainty

[27] One caveat is that early school enrollment could also lead to differences in the number of years of labor market experience if we had full compliance with the law and no differences in retention rates. Given that we do not observe full compliance and that retention rates are much larger for the youngest students, the differences in potential labor market experience are very small at the time of high school completion. In addition for the age groups we examine the returns to an additional fraction of a year of experience is likely to be quite modest.

related to the school entry cut off for older cohorts in that state. Overall, these results indicate that the net impact of school entry laws on labor market outcomes is negligible. Given the very strong first stage relationship between the school entry laws and the timing of school entry it is clear that being the youngest in ones class has no discernable long-term effect on labor market outcomes. This null finding is striking given the substantial adverse impact on academic performance of being the youngest student in a cohort.

4.3. Long Run Outcomes by Gender, Race and Age Groups

In this section we examine how the differences in educational attainment and labor market outcomes induced by the school entry laws vary by gender, race and cohort. Though splitting the sample into subgroups reduces the precision of the estimates it is worth pursuing because as documented above the school entry laws have a larger impact on some subgroups than others. In Tables 9A and 9B we present the educational attainment results by gender and by race for individuals 30 to 79 years of age in California and Texas respectively. The seams are slightly larger for females than males in California though the differences are not statistically significant. 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 also 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. Though in both states some of the results for blacks have perverse signs all of the coefficients are statistically insignificant.

Next we turn to comparing educational attainment across the age cohorts 30-39, 40-49, 50-64 and 65-79 year olds respectively. The regressions for California reveal that the seams in education attainment around 12th grade are between 0.6% and 0.8% for the youngest cohorts, zero for the cohort of 50 to 64 year olds, and between 1.6 and 2.8% for the 65-79 year olds. The corresponding results for Texas are very similar. 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, these results indicate that the school entry laws have smaller effects on the educational attainment of younger cohorts.

The corresponding labor market and long run outcome estimates for all the subgroups are presented in Tables 10A and 10B. Although there is some variation in the magnitude and sign of the coefficients they are typically fairly small and statistically insignificant. Overall these results are consistent with the results for the full sample, i.e., school entry laws do not lead to statistically significant differences in adult outcomes other than educational attainment.

5. Conclusion

In the paper we documented that school entry laws are an important determinant of when children enter school, and that though the children who are the youngest in their school cohort are considerably more likely to be held back than their peers they also have slightly higher educational attainment on average. When we examine the net effect of this tradeoff on adult outcomes we find no evidence that the timing of school entry effects wages or any of the other outcomes for adults that we observe in the Census. Though minorities are substantially more affected by the school entry laws we find no evidence of any effect on labor market outcomes even on this subpopulation. These result suggests either that the increase in educational attainment induced by the school entry laws is offsetting the poorer academic performance of children who start school early or that variation in academic performance that is due purely to relative age, and not adjusted away through retention, does not effect labor market performance.

Finally, our findings suggest that for several reasons school entry laws are a poor instrument for educational attainment in wage regressions. First, the school entry laws have the potential to impact wages via several causal mechanisms and some of them such as retention affect a much larger percent of the population than the attainment mechanism. Second that a high percentage of the students that enter school early are held back suggests that the small percent of the population that gets an additional year of education is so heavily selected that results based on it are likely to tell us very little about the population at large. A final problem peculiar to the quarter of birth version of this instrument is that there is very substantial seasonality in educational attainment that has the potential to induce significant bias in first stage estimates of the effect of quarter of birth on educational attainment.

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

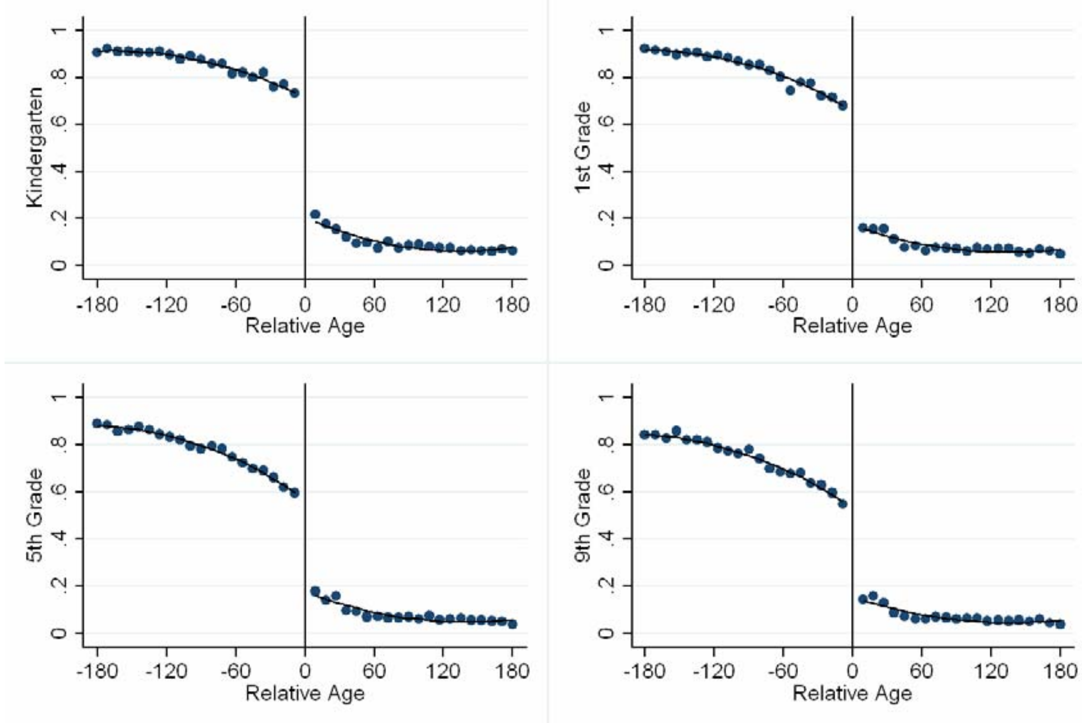
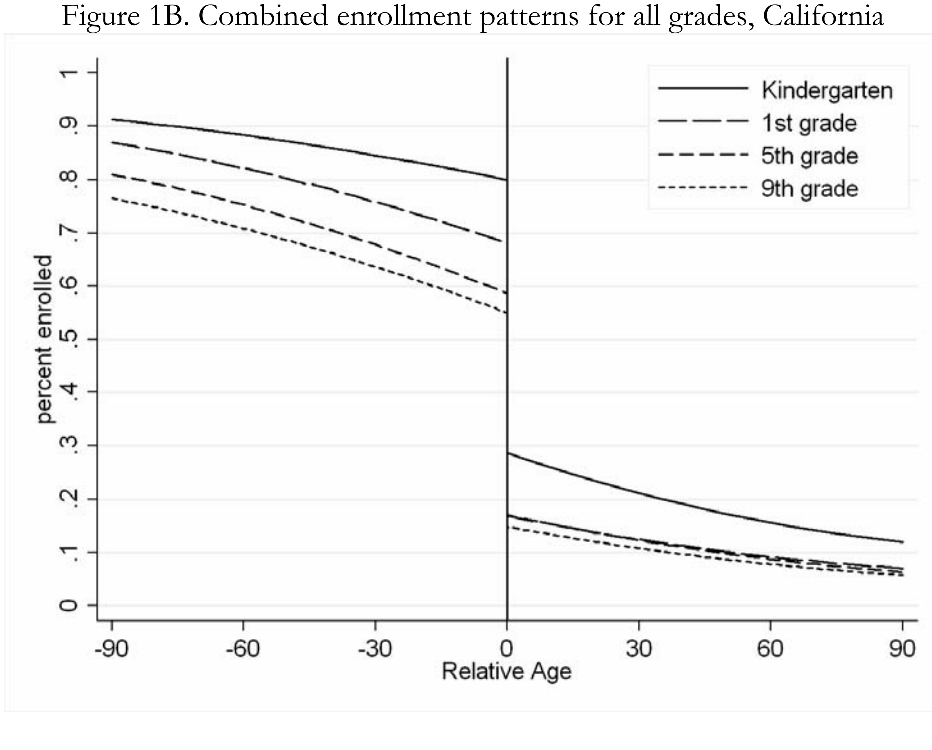


Figure 1B. Combined enrollment patterns for all grades, California



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. Grade enrolled by date of birth, Texas

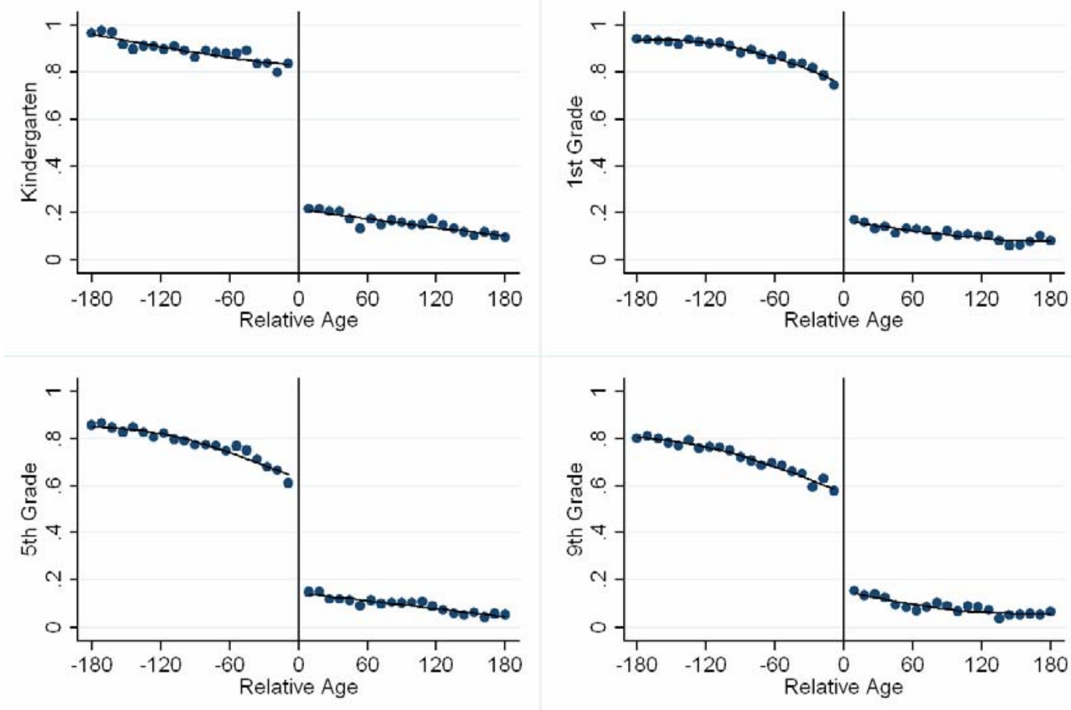
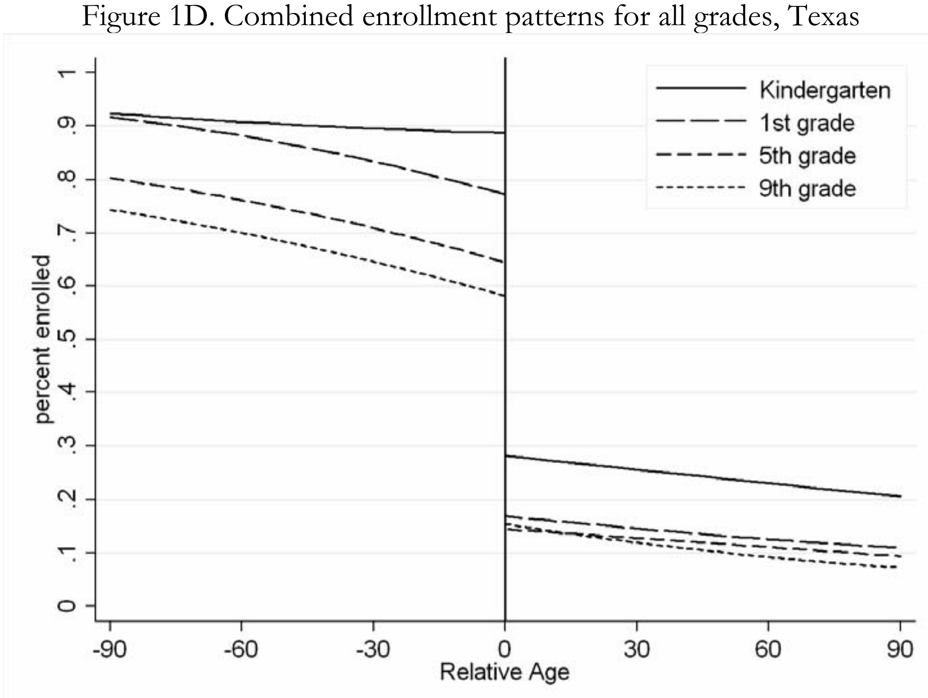


Figure 1D. Combined enrollment patterns for all grades, Texas



Notes: Figures 1C and 1D 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 2A. Estimated Discontinuities in Enrollment by Groups, California

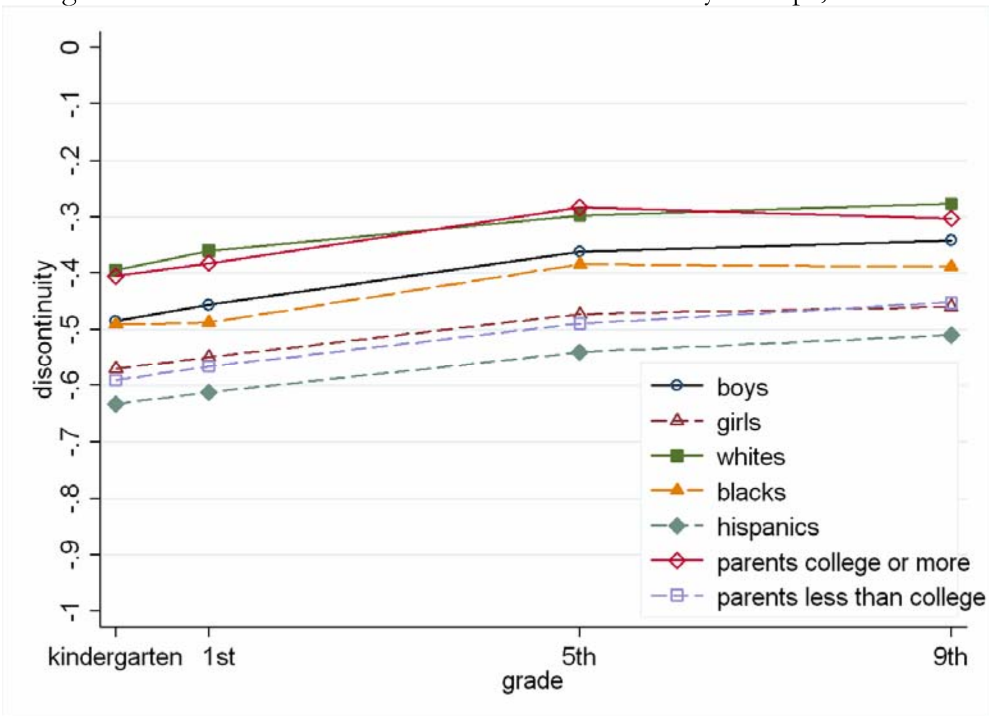
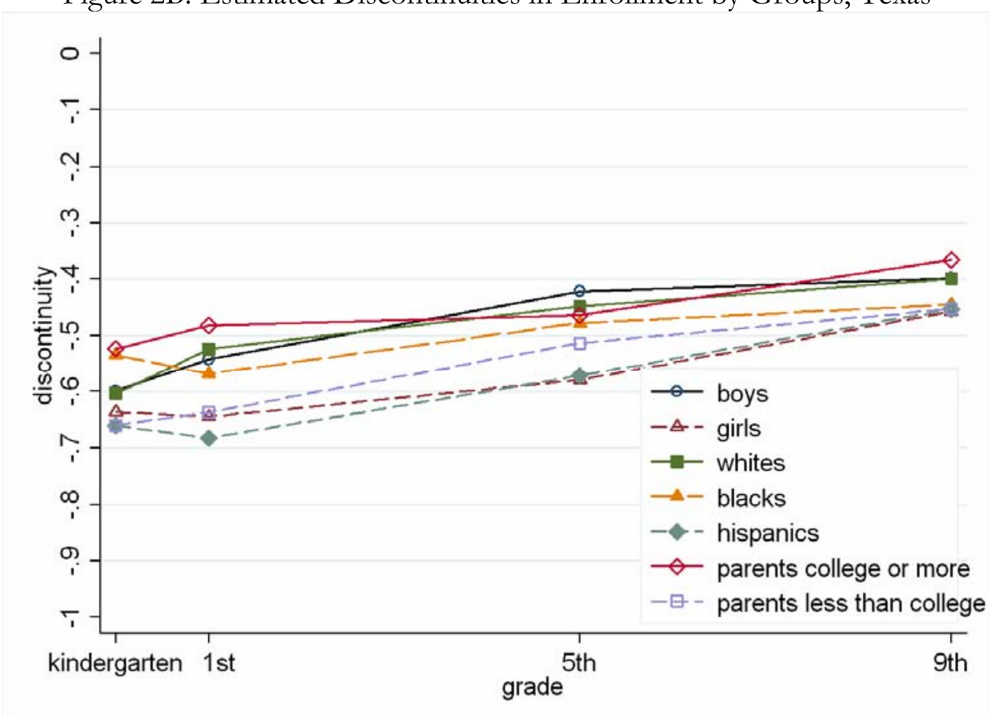


Figure 2B. Estimated Discontinuities in Enrollment by Groups, Texas



Notes: Figures 2A and 2B were estimated using the 2000 Decennial Long Form Census Data. Each figure shows the regression discontinuity estimates of the impact of school entry laws on student enrollment for children of a certain age/grade group. Each dot was estimated separately for each grade group and gender, race and parental education groups.

Figure 3A. Differences in Retention Rates by Groups, California

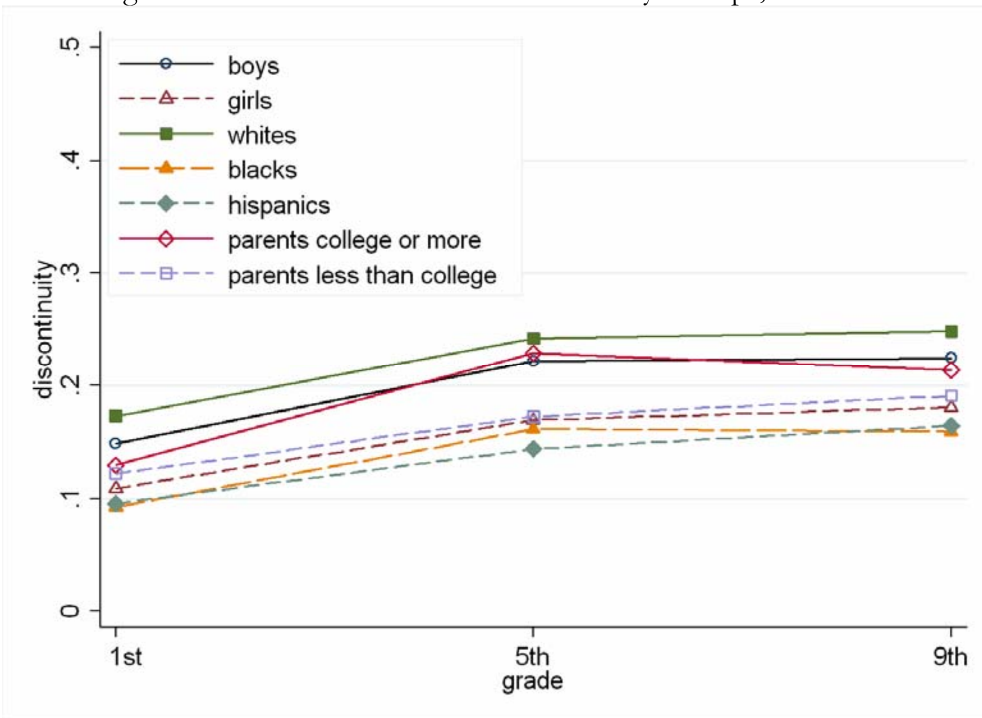
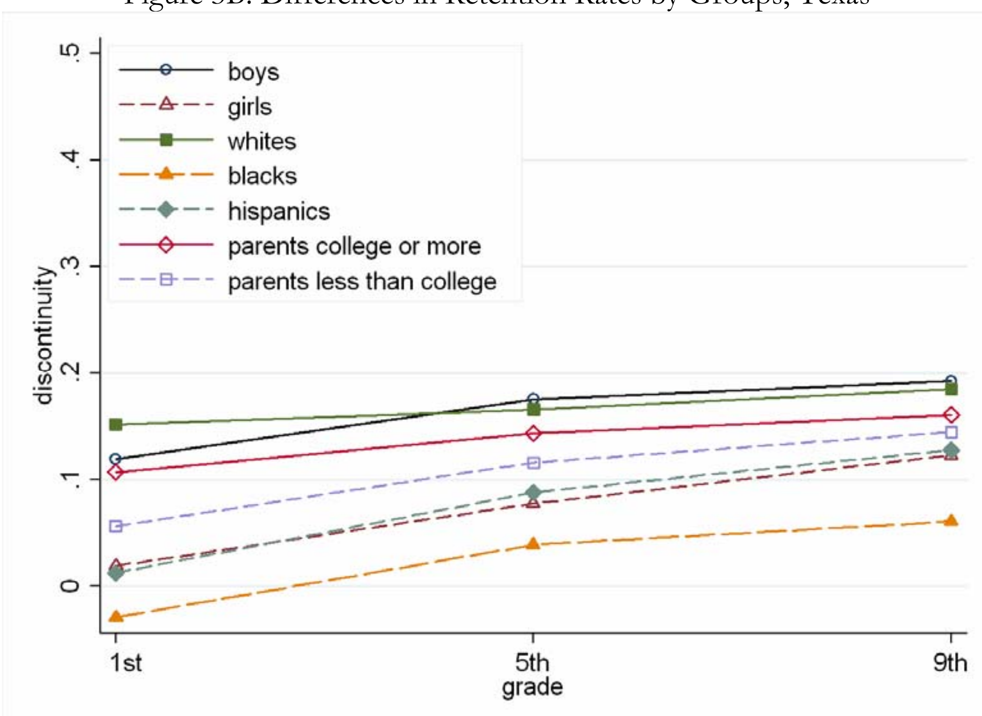
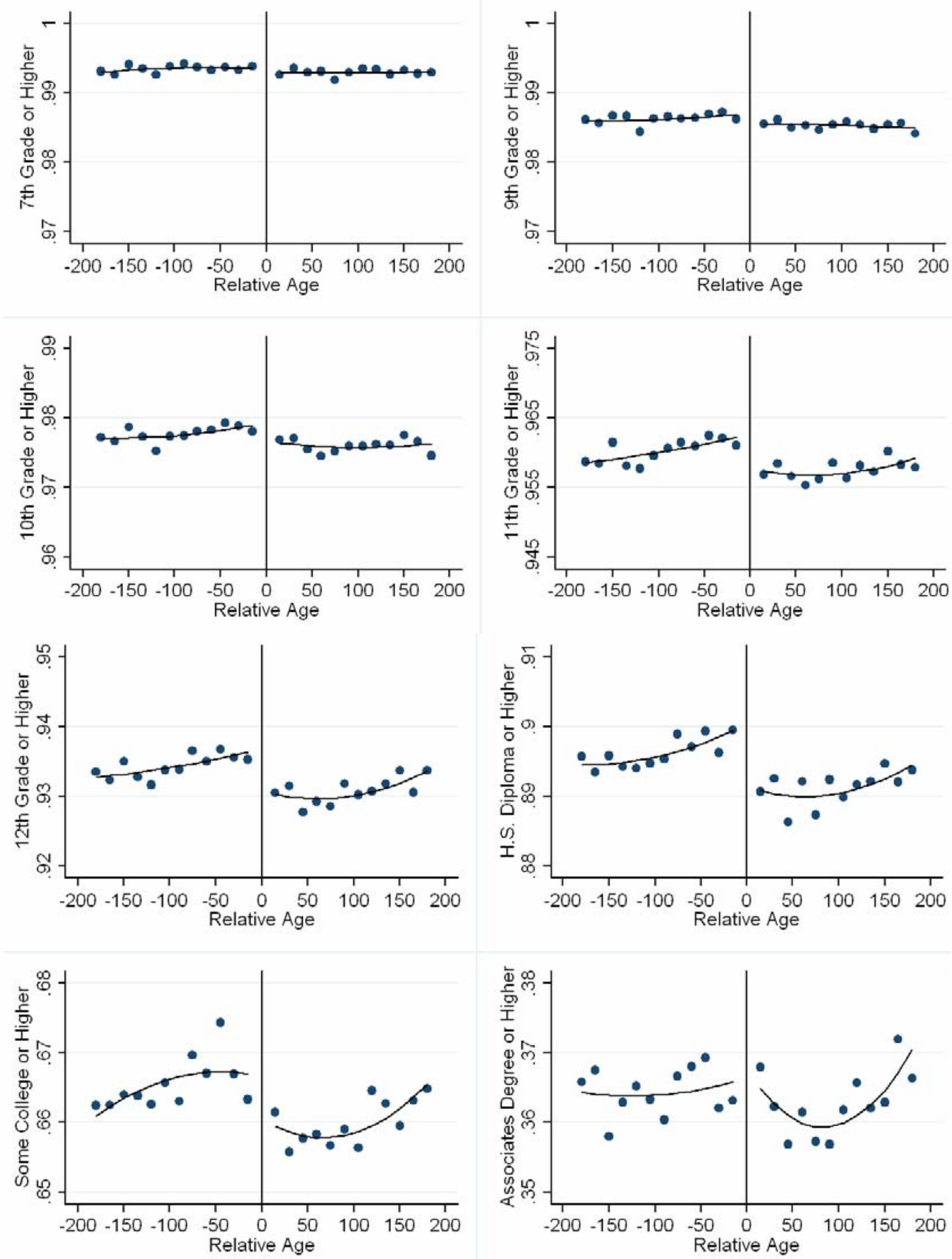


Figure 3B. Differences in Retention Rates by Groups, Texas



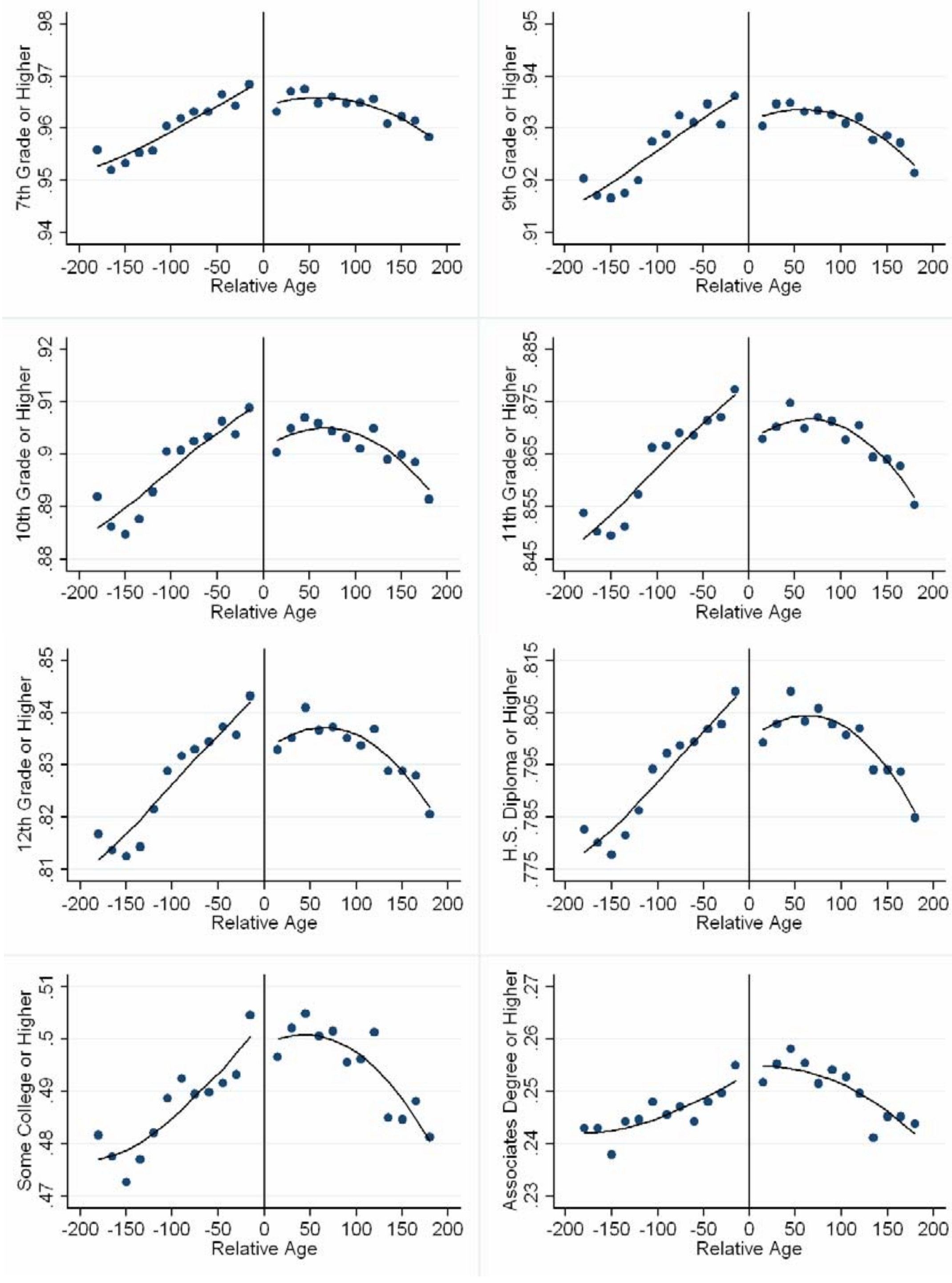
Notes: Figures 3A and 3B were calculated using the 2000 Decennial Long Form Census Data. Each dot in the figures show the proportional differences in implied cumulative retention rates of children born right before the cut off date and children born 180 days prior to the same cut off date. Each dot was calculated separately for each grade group and gender, race and parental education groups.

Figure 4A. Adult Educational attainment by Date of Birth, California



Notes: All panels in Figure 4B 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 4B. Adult Educational attainment by Date of Birth, Texas



Notes: All panels in Figure 4A 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 5A. Adult Long Run Outcomes, California

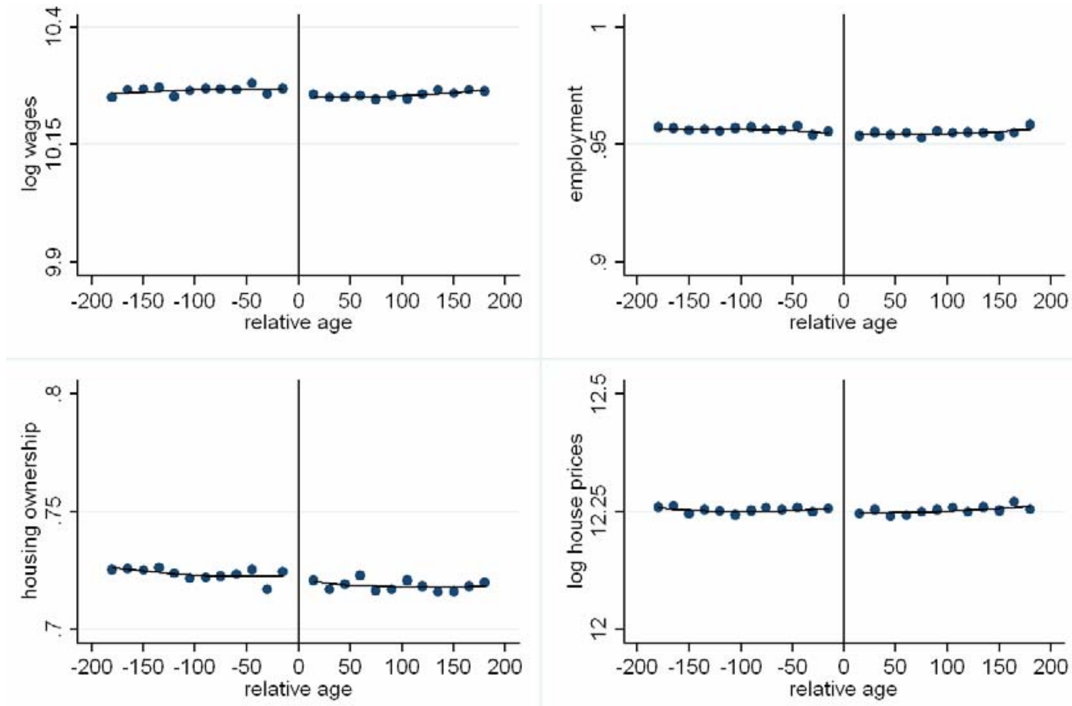
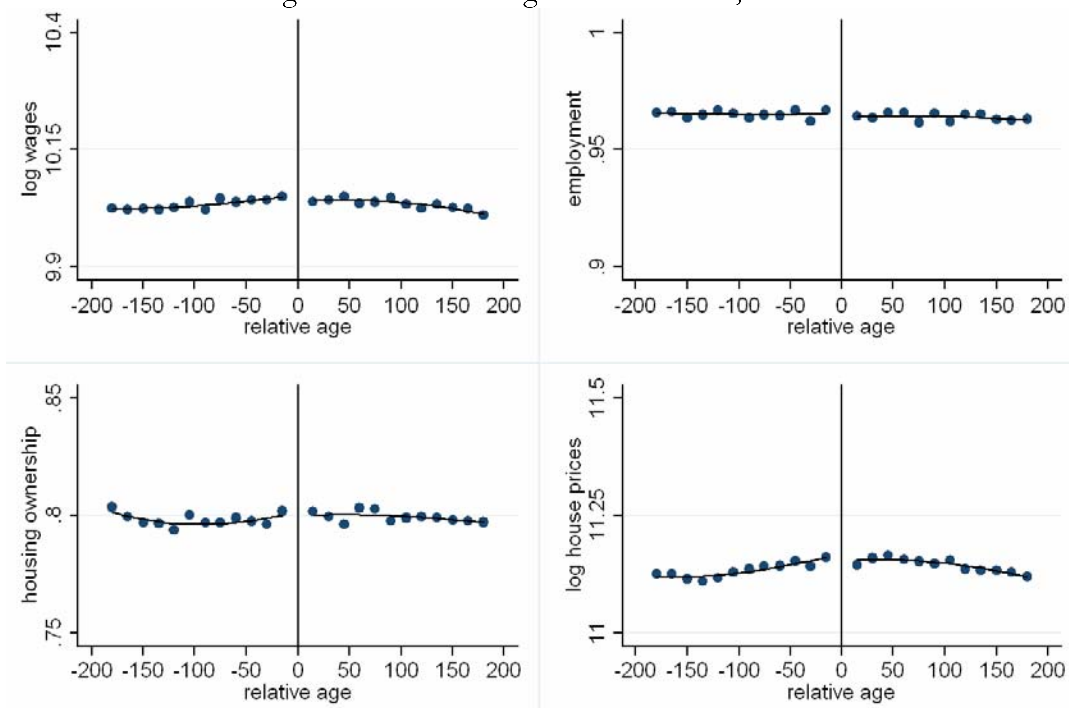


Figure 5B. Adult Long Run Outcomes, Texas



Notes: All panels in Figure 5A and 5B 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, 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, 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 coefficient was estimated from a quadratic polynomial regression, as specified in equation (1) in the text. The unconditional regressions are estimated at the date of birth level of aggregation, using 180 days of birth before and after each cut off date. 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 2: Discontinuity in Observed Characteristics of Children and Their Families, California and Texas

	California				Texas			
	kinder-garten	1st grade	5th grade	9th grade	kinder-garten	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)
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 st 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 coefficient 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: Tables 3A and 3B were estimated using the 2000 Decennial Long Form Census Data. The columns show averages of demographic characteristics of those children born in the 30 days before the cut off date for school entry by grade enrolled in. The t-test column presents the standard test for difference in means of children enrolled versus children not enrolled in the proper grade given their age.

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: Tables 4A and 4B were estimated using the 2000 Decennial Long Form Census Data. The columns show averages of demographic characteristics of those children born in the 30 days after the cut off date for school entry by grade enrolled in. The t-test column presents the standard test for difference in means of children enrolled versus children not enrolled in the proper grade given their age.

Table5A: Impact of School Entry Laws on School Attendance, California

	age 10	age 11	age 12	age 13	age 14	age 15	age 16	age 17
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 5B: Impact of School Entry Laws on School Attendance, Texas

	age 10	age 11	age 12	age 13	age 14	age 15	age 16	age 17
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 5A and 5b were estimated using the 2000 Decennial Long Form Census Data. Each estimated coefficient represents the discontinuity on school attendance (not dropping out of school) at the state entry law cut off date. Each coefficient was estimated from a quadratic polynomial regression, as specified in equation (1) in the text. The unconditional regressions are estimated at the date of birth level of aggregation, using 180 days of birth before and after each cut off date. 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 6A: 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 6B: 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 6A and 6b were estimated using the 2000 Decennial Long Form Census Data. Each coefficient represents the discontinuity on the assigned variable at the state entry law cut off date, and they were estimated from a quadratic polynomial regression variable as specified in equation (2) in the text. 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 7A: 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 7B: 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 7A and 7b were estimated using the 2000 Decennial Long Form Census Data. Each coefficient represents the discontinuity on the assigned variable at the state entry law cut off date, and they were estimated from a quadratic polynomial regression variable as specified in equation (3) in the text., but without any other covariate. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 8A: Impact of School Entry Laws on Long Run Adult Outcomes, California

	log wages	1 if employed	log house income	house ownership	log house value	1 if married
Discontinuity	-0.0093	0.0001	-0.0060	-0.0020	-0.0130	0.0007
	(0.0084)	(0.0018)	(0.0069)	(0.0030)	(0.0073)	(0.0024)
Date of birth controls	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES
Observations	479500	499644	685956	691219	498332	691219

Table 8B: Impact of School Entry Laws on Long Run Adult Outcomes, Texas

	log wages	1 if employed	log house income	house ownership	log house value	1 if married
Discontinuity	0.0009	-0.0006	-0.0037	0.0016	-0.0064	0.0037
	(0.0075)	(0.0015)	(0.0057)	(0.0024)	(0.0061)	(0.0019)
Date of birth controls	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES
Observations	496100	504877	759276	767302	612831	767302

Notes: Tables 8A and 8b were estimated using the 2000 Decennial Long Form Census Data. Each coefficient represents the discontinuity on the assigned variable at the state entry law cut off date, and they were estimated from a quadratic polynomial regression variable as specified in equation (3) in the text. 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 9A: Impact of School Entry on Adult Education Attainment by Gender, Race and Age Groups, 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)
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 9B: Impact of School Entry on Adult Education Attainment by Gender, Race and Age Groups, 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)
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 9A and 9b were estimated using the 2000 Decennial Long Form Census Data. Each coefficient represents the discontinuity on the assigned variable at the state entry law cut off date, and they were estimated from a quadratic polynomial regression variable as specified in equation (2) in the text. 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: Impact of School Entry on Adult Outcomes by Gender, Race and Age Groups, California

	log wages	1 if employed	log house income	house ownership	log house value	1 if married
Discontinuity for females	-0.0158	-0.0018	-0.0131	-0.0052	-0.0147	0.0016
	(0.0153)	(0.0027)	(0.0102)	(0.0042)	(0.0098)	(0.0029)
Discontinuity for males	-0.0037	0.0018	0.0016	0.0017	-0.0110	-0.0004
	(0.0120)	(0.0024)	(0.0092)	(0.0045)	(0.0105)	(0.0041)
Discontinuity for whites	-0.0063	-0.0021	-0.0064	-0.0001	-0.0165	-0.0012
	(0.0094)	(0.0018)	(0.0076)	(0.0037)	(0.0082)	(0.0029)
Discontinuity for blacks	-0.0080	0.0130	-0.0122	0.0082	0.0257	0.0087
	(0.0336)	(0.0104)	(0.0331)	(0.0137)	(0.0264)	(0.0141)
Discontinuity for hispanics	-0.0047	0.0007	0.0109	-0.0104	-0.0006	-0.0040
	(0.0174)	(0.0056)	(0.0146)	(0.0085)	(0.0141)	(0.0058)
Discontinuity for 30-39 year olds	-0.0048	0.0022	0.0051	-0.0023	-0.0138	0.0006
	(0.0150)	(0.0031)	(0.0109)	(0.0062)	(0.0102)	(0.0057)
Discontinuity for 40-49 year olds	-0.0184	-0.0022	-0.0124	-0.0035	-0.0184	0.0001
	(0.0133)	(0.0028)	(0.0113)	(0.0058)	(0.0117)	(0.0046)
Discontinuity for 50-64 year olds	-0.0073	-0.0025	-0.0028	-0.0011	-0.0047	0.0015
	(0.0172)	(0.0034)	(0.0129)	(0.0048)	(0.0142)	(0.0037)
Discontinuity for 65-79 year olds	0.0176	0.0204	-0.0308	0.0027	-0.0132	0.0012
	(0.0770)	(0.0082)	(0.0206)	(0.0073)	(0.0189)	(0.0042)
Date of birth controls	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES

Table 10B: Impact of School Entry on Adult Outcomes by Gender, Race and Age Groups, Texas

	log wages	1 if employed	log house income	house ownership	log house value	1 if married
Discontinuity for females	-0.0147	-0.0005	-0.0008	0.0046	-0.0039	0.0025
	(0.0115)	(0.0019)	(0.0083)	(0.0035)	(0.0071)	(0.0024)
Discontinuity for males	0.0142	-0.0008	-0.0069	-0.0017	-0.0091	0.0052
	(0.0100)	(0.0024)	(0.0074)	(0.0033)	(0.0101)	(0.0026)
Discontinuity for whites	0.0068	0.0009	0.0014	0.0021	-0.0029	0.0028
	(0.0097)	(0.0016)	(0.0073)	(0.0027)	(0.0077)	(0.0020)
Discontinuity for blacks	-0.0213	-0.0091	-0.0105	0.0028	0.0152	0.0089
	(0.0232)	(0.0057)	(0.0207)	(0.0087)	(0.0183)	(0.0065)
Discontinuity for hispanics	-0.0088	-0.0014	-0.0168	-0.0015	-0.0276	0.0014
	(0.0156)	(0.0042)	(0.0112)	(0.0064)	(0.0133)	(0.0050)
Discontinuity for 30-39 year olds	0.0093	-0.0031	0.0007	0.0080	0.0122	0.0112
	(0.0123)	(0.0027)	(0.0112)	(0.0052)	(0.0126)	(0.0044)
Discontinuity for 40-49 year olds	-0.0064	0.0006	-0.0089	-0.0028	0.0118	0.0023
	(0.0139)	(0.0025)	(0.0117)	(0.0051)	(0.0105)	(0.0036)
Discontinuity for 50-64 year olds	0.0006	0.0018	0.0073	0.0001	-0.0221	-0.0020
	(0.0148)	(0.0026)	(0.0106)	(0.0042)	(0.0111)	(0.0023)
Discontinuity for 65-79 year olds	-0.0286	-0.0058	-0.0182	0.0015	-0.0285	0.0030
	(0.0466)	(0.0055)	(0.0144)	(0.0048)	(0.0145)	(0.0026)
Date of birth controls	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES

Notes: Tables 10A and 10b were estimated using the 2000 Decennial Long Form Census Data. Each coefficient represents the discontinuity on the assigned variable at the state entry law cut off date, and they were estimated from a quadratic polynomial regression variable as specified in equation (3) in the text. 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.