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Do school entry laws affect educational attainment and labor market outcomes?

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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.

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. Several papers have documented the adverse effects on academic performance of being the youngest student in a classroom in the United States. 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 cutoff dates to document that children that start kindergarten later get higher test scores.[1] Elder and Lubotsky (forthcoming) used the Early Childhood Longitudinal Study to document that a 1 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 approximately 13%. They also find differences in test scores, but this outcome is largely driven by accumulation of skills prior to kindergarten and declines rapidly as children age. Studies focused on other countries found more mixed effects.[2] Overall, these findings have lead to substantial concern among both parents and educators about the effect of age based school entry laws, and legislators in several U.S. states have changed their school entry dates in order to increase the age at which children enter kindergarten.[3]

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[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 oldest boys in a cohort in Canada are more likely to thrive in professional sports, and Fredriksson and Öckert (2006) found a negative impact on wages for the youngest individuals in a cohort in Sweden. However, Black, Devereux, and Salvanes (2008) find that being relative older at the start of kindergarten has no effect on educational attainment and earnings in Norway.

[3] See Bedard and Dhuey (2007).

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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 as noted by Angrist and Krueger (1991).[4] This suggests that there is an important tradeoff to consider. This paper provides estimates of the net *long run* impact of these opposing mechanisms on labor market outcomes in the United States. In addition to getting at the net effect of the tradeoff described above, labor market outcomes of adults are arguably of greater interest than the intermediate outcomes, such as academic performance, that are typically considered in the literature.

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. 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. 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. We take advantage of these threshold dates to implement a regression discontinuity (RD) design. The RD approach lets us estimate the 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.[6]

The analysis focuses on adult outcomes of individuals over the age of 30 as they are more likely to have completed their education. We find that the school entry laws have a modest effect on educational attainment: adults born right before the cutoff for school entry in Texas and California are about 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. Evidence from contemporary cohorts shows that though school entry laws have a very pronounced effect on the timing of school entry, a substantial part of the difference is undone through retention. Data from recent cohorts also show that youngest students have lower academic performance, as measured by retention rates.

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 outcomes we observe in the Census, such as family income, house ownership, house value and marital status. We find no evidence that early school entry has an impact on adult outcomes for any of the age, gender and race subgroups we examine, not even for Hispanics who have the largest difference in educational attainment of any of the contemporary cohorts.

The rest of the paper is organized as follows: in Section 2 we discuss the empirical model and data sources. In Section 3 we examine the impact of school entry laws on educational attainment. In Section 4 we present evidence on adult labor market outcomes. In Section 5 we show educational attainment and labor market outcomes by subpopulation. Section 6 presents estimates of the impact of school entry laws on contemporary cohorts. Section 7 concludes.

2. Econometric methods and data

In this section we describe the regression discontinuity design model we use to estimate the effect of school entry laws on adult educational attainment and labor market outcomes. A complete review of the RD method can be found in Imbens and Lemieux (2008); and Lee and Lemieux (2009). Here we just focus on the econometric specification used to estimate the parameters of interest. Following Lee (2008) and Lee and Card (2008), we use a parametric rather than a nonparametric approach since the threshold for school entry laws is based on the discrete variable age, which is measured in days.[7]

The first outcome we examine is educational attainment in the adult population. We estimate the impact of the school entry laws on this outcome by fitting the following equation:

$$Educ_i = \delta_0 + \delta_1 Cut_i + \delta_2 Bday_i + \delta_3 Cut_i \times Bday_i + \delta_4 Bday^2 + \delta_5 Cut_i \times Bday^2 + \Psi X_i + \varepsilon_i \quad (1)$$

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 10th grade indicator variable is equal to 1 if the individual has completed at least 10th grade, and zero otherwise. We run separate regressions for each possible level of educational attainment between 7th grade and college completion. 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

[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 previous 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.

[5] We use those states because of their large and diverse population, and due to the availability of data.

[6] Cascio and Lewis (2006) used a similar design to estimate the impact of schooling on AFQT performance. Early applications of RD design can be found in Thistlethwaite and Campbell (1960) and Cook and Campbell (1979). We discuss the details of the RD model and the most recent literature in the next section.

[7] See Hahn, Todd, and Van der Klaauw (2001) for non-parametric estimation of the RD model when the discontinuity occurs on a continuous variable.

have their impact.[8] The variable Cut_i is an indicator variable for being born after the cutoff date, $Bday_i$ is the number of days from the individual's birthday to the cutoff date, X_i is a set of covariates, and ε_i is an idiosyncratic error term. We run separate regressions for each cutoff between 7th grade and college. The primary parameter of interest in Eq. (1) is δ_1 which is the size of the discrete change in the outcome $Educ_i$ at the cutoff date for the school entry laws.

For each outcome we create a figure, over the support of age, with the fitted model from Eq. (1) 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 from Eq. (1) 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.

Finally, we estimate the relationship between school entry laws and labor market outcomes using the following equation:

$$Y_i = \gamma_0 + \gamma_1 Cut_i + \gamma_2 Bday_i + \gamma_3 Cut_i \times Bday_i + \gamma_4 Bday^2 + + \gamma_5 Cut_i \times Bday^2 + \Sigma X_i + \nu_i \quad (2)$$

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.[9] We do not construct this statistic because the school entry laws result in differences in relative age, retention rates and test scores. These are all very likely to affect adult wages which violates the exclusion restriction under which the instrumental variable estimate is identified.[10]

One appealing property of the RD strategy is that it is possible to assess the probability of an omitted variables problem fairly directly. All potential confounders must evolve smoothly across the discontinuity for the RD to generate consistent estimates. We test for discontinuities in the observable variables by estimating a set of regressions of the form of Eq. (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 cutoff 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 the sample 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). 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 on educational attainment and labor market outcomes using exact day of birth in the United States.

One limitation of the Census data is that we do not know what state an individual lived in when their parents were facing the school enrollment decision. This is important as the school enrollment cutoff date varies across states. We deal with this issue by restricting the sample to individuals born in California and Texas who are still living in their state of birth. 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 cutoff 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 adult educational attainment

In this section we examine the effect of school entry laws on adult educational attainment. The two states we focus our analysis on have different cutoff dates 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. The State of California has used the cutoff date of December 2nd since 1987. Between 1951 and 1987 the statute read 'be 4 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 cutoff date prior to 1951 which makes it impossible to ascertain which cutoff date people faced without knowing which school they attended, something

[8] 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. However, the high retention rates observed in the data makes this variable difficult to interpret.

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

[10] 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. See Bound et al. (1995).

we do not observe in the Census.[11] 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 also for various cohorts in both states.

In the figures we deal with the variation in the cutoff date for school entry by setting the cutoff date for each cohort at 0 and measuring the number of days from the individual's birthday to the cutoff date that was in force when they were 5 years old. For example, an individual born in California on November 22nd, 1975, would have a relative age of −10. We then plot the proportion with a particular level of educational attainment over the support of this running variable. To make the figures less noisy the proportion enrolled has been computed for 15-day blocks rather than for individual days. The fitted values from the regression model specified in Eq. (1) are laid over the means.[12]

In Fig. 1 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 15-day period that have completed a particular grade or higher. Surprisingly, the figure reveals that there is pronounced seasonality in educational attainment.[13] 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 cutoff are slightly more likely to have completed 9th, 10th, 11th, 12th grade, and received a H.S. diploma than those just born after the cutoff. We do not find compelling evidence that the school enrollment laws increase college attendance though the estimates are fairly imprecise.

In Table 1A 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.[14] 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 9th, 10th, 11th, 12th 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%.

In Fig. 2 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 11th grade, 12th 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 Associate degree. These results are confirmed in Table 1B. The discontinuity estimates for 10th grade, 11th grade, 12th 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 1 percentage point. The other effects are slightly smaller than the ones observed in Texas, particularly for the lower grades.

The fact 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. To test this more directly, we check to make sure that there are no abrupt changes in the proportion of the population that is male, white, black, Hispanic, or that immigrated to the state in the last 5 years. We present the results of this exercise in Tables 2A and 2B which reveal that these observable characteristics evolve smoothly through the cutoff for school entry.

There are a couple of possible explanations for the abrupt jump in educational attainment observed around the school entry cutoff. 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 (from 9th grade through High School completion). 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. In either case, on average the students that enroll in school at a younger age stay in school longer.

4. Effect of school entry laws on labor market and other long run outcomes

In this section we examine the impact of the school entry laws on employment rates, wages, and several other outcomes available in the Census including family income, house ownership, house value and marital status. 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 indi-

[11] Prior to 1951 not everyone in California faced a December 1st cutoff 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. See Cos (2001) for more details.

[12] To maximize the precision of the estimates, the regression line is estimated from the day level data rather than from the 9-day means in the figure. The regressions in the figures do not include individual level covariates though the regressions in the tables do.

[13] This seasonality is also observed for birth outcomes—see Lam and Miron (1991).

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

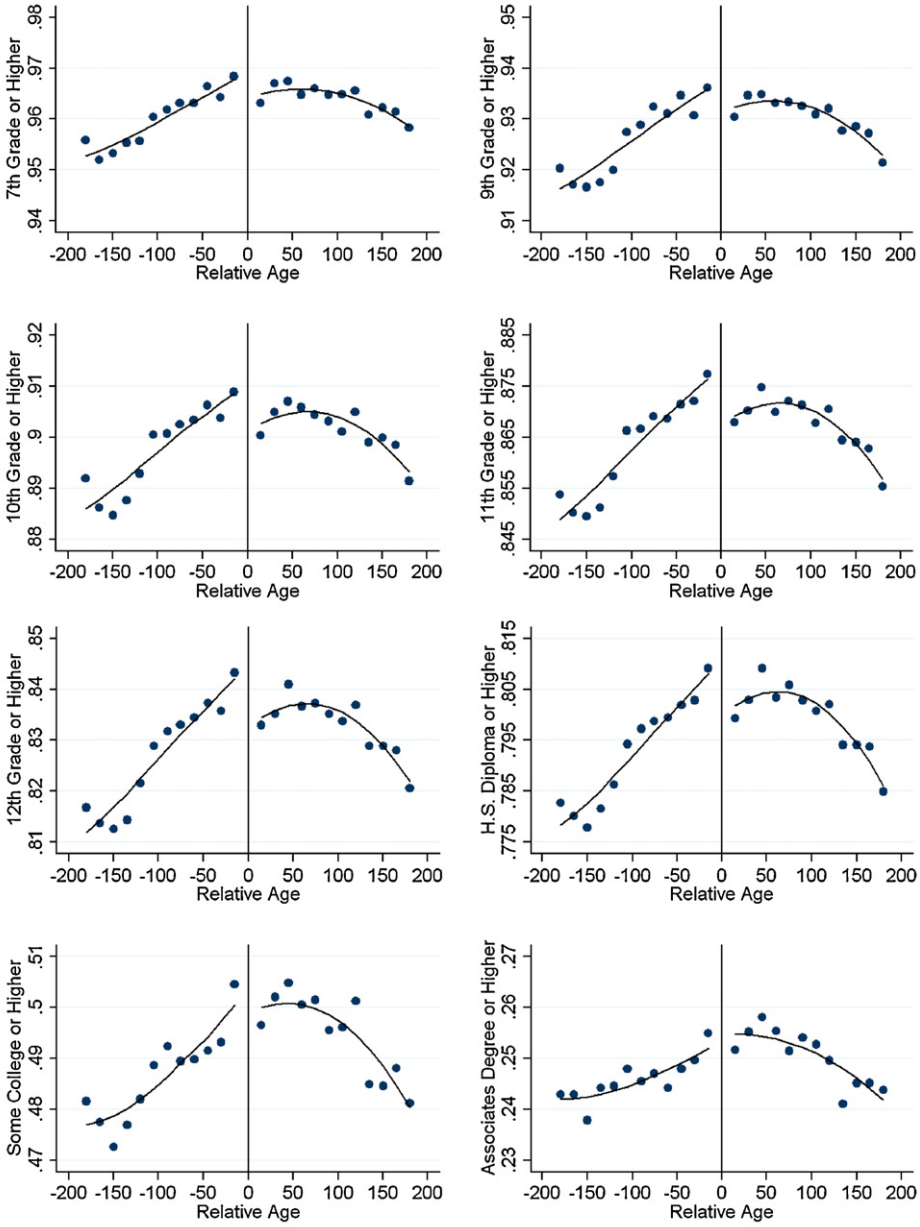

Fig. 1. Adult educational attainment by date of birth, Texas. *Notes*: all panels in figure were estimated using the 2000 Decennial Long Form Census Data. Each figure shows the profile of average educational attainment for adults of a certain educational attainment or higher. Each dot represents the average educational attainment by 15-day blocks of age, where relative age 0 is the age entry cutoff date for the state. 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.

viduals because, as has been extensively documented in the literature, the youngest students in a class on average have poorer academic performance.[15] We estimate the net long run effect of these opposing mechanisms by comparing the labor market outcomes of individuals born right before the cutoff date with the outcomes of individuals born right after the cutoff date for school entry.[16] We find no evidence that the laws had a net impact on

[15] Our sample corroborates this, as we observe higher retention rates for the youngest students in a cohort. We show these calculations in Section 6.

[16] 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.

Table 1A
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	−0.008	−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	767,302	767,302	767,302	767,302	767,302	767,302	767,302	767,302

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on educational attainment, relative to being born just before the cutoff. Each coefficient was estimated separately, from a quadratic polynomial regression variable as specified in Eq. (1) in the text. The 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 a dummy for whether the household lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 1B
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	691,219	691,219	691,219	691,219	691,219	691,219	691,219	691,219

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on educational attainment, relative to being born just before the cutoff. Each coefficient was estimated separately, from a quadratic polynomial regression variable as specified in Eq. (1) in the text. The 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 a dummy for whether the household lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 2A
Testing for discontinuity in observed characteristics for 30–79 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 (0.003)	−0.005 (0.003)	0.000 (0.003)	0.006 (0.003)	−0.001 (0.001)
Observations	767,302	767,302	767,302	767,302	767,302

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on the assigned variables, relative to being born just before the cutoff. Each coefficient was estimated separately, from a quadratic polynomial regression variable as specified in Eq. (1) in the text, but without the inclusion of covariates. Standard errors shown in parentheses are clustered at the exact date of birth.

labor market outcomes such as employment rates and wages, or on other outcomes such as the probability of homeownership.

In Fig. 3 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 that the laws have an effect on any of those outcomes. RD estimates for Texas are presented in Table 3A 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 cutoff date is only 0.0009, while the change in the probability of employment is −0.0006. The results for California displayed in Table 3B and Fig. 4 show similar patterns, although we should be cautious about those estimates given the uncertainty related to the school entry cutoff 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 strong first stage relationship between the school entry laws and the timing of school entry that we document in Section 6 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 extensive literature documenting the substantial adverse impact on

Table 2B
Testing for discontinuity in observed characteristics for 30–79 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 (0.003)	0.002 (0.003)	−0.002 (0.002)	0.001 (0.003)	0.000 (0.001)
Observations	691,219	691,219	691,219	691,219	691,219

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on the assigned variables, relative to being born just before the cutoff. Each coefficient was estimated separately, from a quadratic polynomial regression variable as specified in Eq. (1) in the text, but without the inclusion of covariates. Standard errors shown in parentheses are clustered at the exact date of birth.

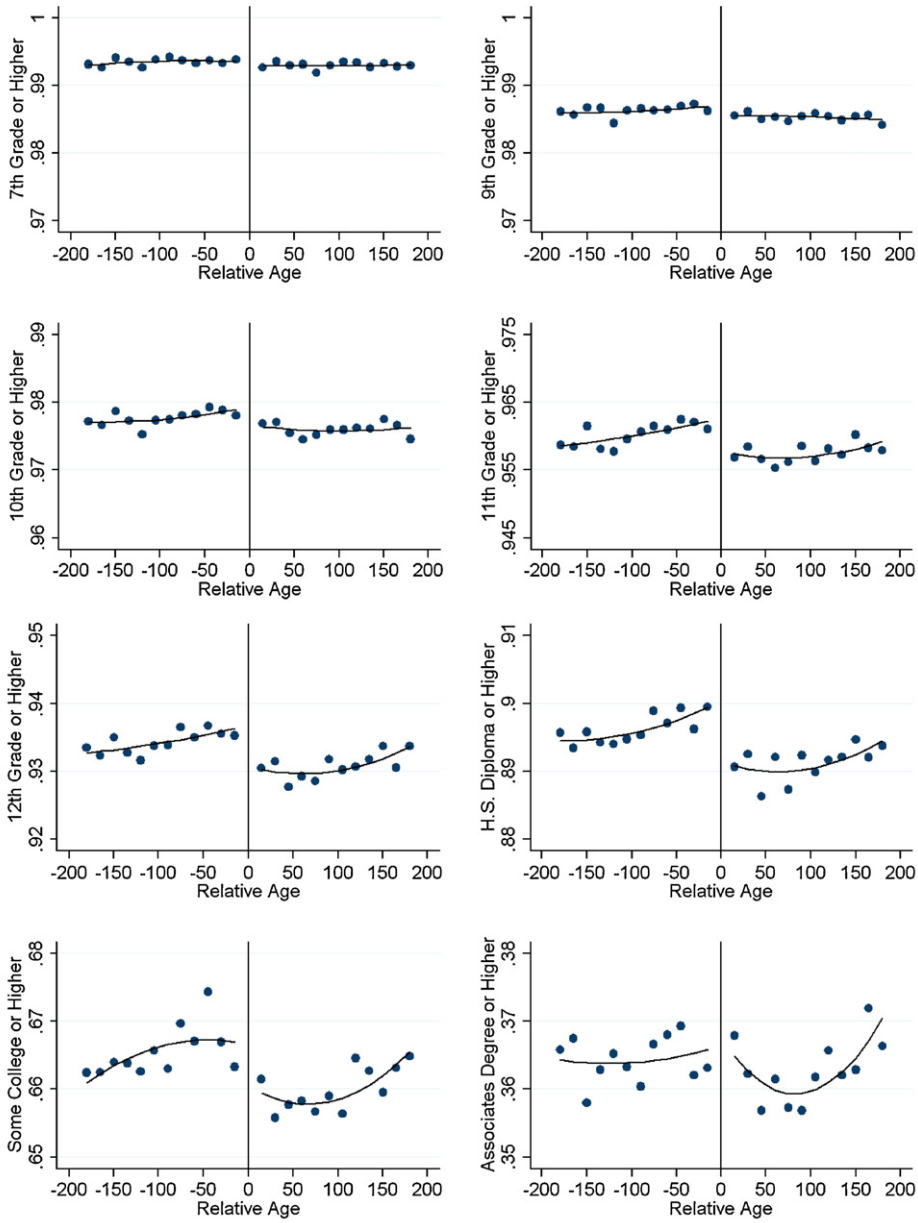

Fig. 2. Adult educational attainment by date of birth, California. *Notes*: all panels were estimated using the 2000 Decennial Long Form Census Data. Each figure shows the profile of average educational attainment for adults with a certain educational attainment or higher. Each dot represents the average educational attainment by 15-day blocks of age, where relative age 0 is the age entry cutoff date for the state. 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 3A
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 (0.0075)	−0.0006 (0.0015)	−0.0037 (0.0057)	0.0016 (0.0019)	−0.0064 (0.0061)	0.0037 (0.0019)
Date of birth controls	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	496,100	504,877	759,276	767,302	612,831	767,302

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on adult outcomes, relative to being born just before the cutoff. Each coefficient was estimated separately, from a quadratic polynomial regression variable as specified in Eq. (2) in the text. The 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 a dummy for whether the household lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

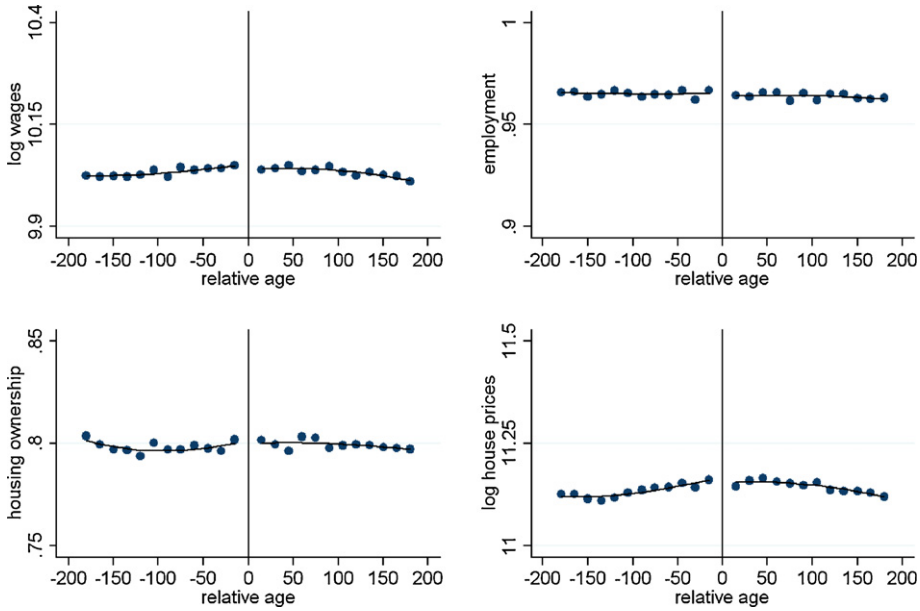

Fig. 3. Adult long run outcomes, Texas. *Notes*: all panels in figure were estimated using the 2000 Decennial Long Form Census Data. Each figure shows a given outcome for 30–79 year olds in Texas. Each dot represents the average of the long run outcome by 15-day blocks of age, where relative age 0 is the age entry cutoff date for the state. 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 3B
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 (0.0084)	0.0001 (0.0018)	−0.0060 (0.0069)	00.00 (0.0030)	−0.0130 (0.0073)	0.0007 (0.0024)
Date of birth controls	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	479,500	499,644	685,956	691,219	498,332	691,219

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on adult outcomes, relative to being born just before the cutoff. Each coefficient was estimated separately, from a quadratic polynomial regression variable as specified in Eq. (2) in the text. The 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 a dummy for whether the household lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

academic performance of being the youngest student in a cohort.

5. Variation in effect of school entry laws by gender, race and age

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 the laws have a larger impact on some subgroups than others. In Tables 4A and 4B we present the educational attainment results by gender and by race for individuals 30–79 years of age in Texas and California 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 1.5–2 percentage points for Hispanics, which is 3–4 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. 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, 0 for the cohort of 50–64 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 65–79 year olds and no evidence of an effect on the cohort of 50–64 year olds. Overall, these results indicate that the school entry laws have smaller effects on the educational attainment of younger cohorts.

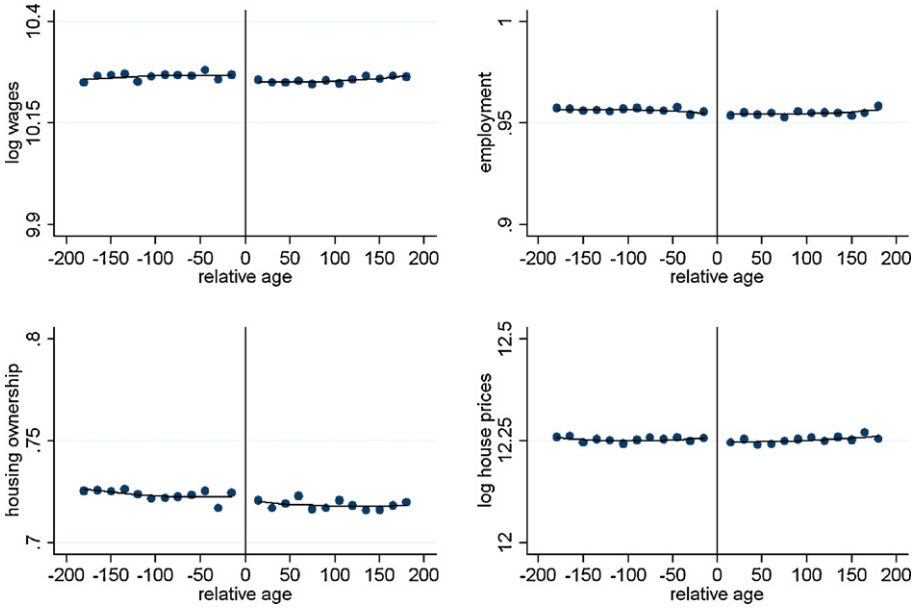

Fig. 4. Adult long run outcomes, California. *Notes*: all panels in figure were estimated using the 2000 Decennial Long Form Census Data. Each figure shows a given outcome for 30–79 year olds in California. Each dot represents the average of the long run outcome by 15-day blocks of age, where relative age 0 is the age entry cutoff date for the state. 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.

The corresponding labor market and long run outcome estimates for all the subgroups are presented in Tables 5A and 5B. 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.

6. School entry law effects on contemporary cohorts

In this section we examine the impact of school enrollment laws on school age children of contemporary cohorts. Because the data are not available, we are unable to directly estimate the effect of school entry laws on the timing of school entry for the adult cohorts we examined above. However, the impact of school entry laws on more recent cohorts sheds light on the magnitude of this treatment, and how it varies by race, gender and parental education.

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. In four panels of Fig. 5A and B we present estimates of the proportion of individuals in Texas and California who are enrolled in public kindergarten, first grade, fifth grade or ninth grade.[17] The top left panels (kindergarten panels) for both states reveal that there is less than perfect compliance with the law: about 20% of individuals born immediately after the cutoff 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 cutoff 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 cutoff date for kindergarten enrollment are a grade ahead of individuals born just after the cutoff date.

The remaining panels reveal that the size of the gap induced by the school entry laws shrinks as children get older. The gap in enrollment for ninth graders is about two-thirds the size of the gap at kindergarten. This is consistent with the very youngest students in a particular grade being held back more often than their older peers. In fact, implied retention rates vary substantially, while 31% of the students born just before the cutoff date are retained at some point between kindergarten and ninth grade only 11% of those born 180 days before the cutoff date are retained.[18]

[17] 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. We restricted the sample to public school enrollment because the cutoff dates are only supposed to be enforced by public schools. However, estimates that also include private schools do not present very different results, in part because less than 10% of the students are enrolled in private schools.

[18] The 31% retention rate was calculated by dividing the proportion of students enrolled in 9th grade at the relevant age right before the cutoff date (55%) by the proportion of students enrolled in kindergarten at the relevant age right before the cutoff date (80%). A similar calculation was done for students born 180 days before the cutoff date. This calculation ignores the possibility that there are significant differences across cohorts.

Table 4A
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 (0.0019)	−0.0043 (0.0023)	−0.0059 (0.0026)	−0.0077 (0.0029)	−0.0100 (0.0032)	−0.0077 (0.0036)	−0.0027 (0.0049)	0.0030 (0.0038)
Discontinuity for males	−0.0047 (0.0019)	−0.0042 (0.0027)	−0.007 (0.0028)	−0.0091 (0.0042)	−0.0075 (0.0038)	−0.0078 (0.0042)	−0.0005 (0.0043)	0.0024 (0.0040)
Discontinuity for whites	−0.0027 (0.0009)	−0.0029 (0.0015)	−0.0044 (0.0019)	−0.0050 (0.0027)	−0.0052 (0.0024)	−0.0043 (0.0027)	0.0024 (0.0035)	0.0050 (0.0033)
Discontinuity for blacks	0.0040 (0.0035)	0.0023 (0.0050)	−0.0017 (0.0056)	−0.0063 (0.0060)	−0.0093 (0.0081)	−0.0119 (0.0090)	−0.0029 (0.0097)	−0.0037 (0.0083)
Discontinuity for Hispanics	−0.0099 (0.0055)	−0.0124 (0.0065)	−0.0173 (0.0065)	−0.017 (0.0068)	−0.0202 (0.0067)	−0.0165 (0.0071)	−0.0123 (0.0063)	−0.0009 (0.0050)
Discontinuity for 30–39 year olds	0.0005 (0.0010)	0.0001 (0.0017)	−0.0018 (0.0026)	−0.0043 (0.0030)	−0.0043 (0.0033)	−0.0011 (0.0041)	0.0134 (0.0063)	0.0096 (0.0052)
Discontinuity for 40–49 year olds	−0.0027 (0.0015)	−0.0028 (0.0023)	−0.0073 (0.0027)	−0.0098 (0.0027)	−0.0122 (0.0030)	−0.0089 (0.0040)	10.0011 (0.0060)	0.0011 (0.0051)
Discontinuity for 50–64 year olds	−0.0005 (0.0027)	−0.0028 (0.0035)	−0.0021 (0.0038)	0.0001 (0.0040)	0.0033 (0.0042)	0.0012 (0.0042)	0.0047 (0.0059)	0.0054 (0.0056)
Discontinuity for 65–79 year olds	−0.0127 (0.0042)	−0.0103 (0.0054)	−0.0149 (0.0055)	−0.0187 (0.0077)	−0.0213 (0.0069)	−0.0218 (0.0077)	−0.0212 (0.0074)	−0.0043 (0.0058)
Date of birth controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on educational attainment, relative to being born just before the cutoff. Separate samples were created by gender, race, and age cohorts. Each coefficient was estimated separately, from a quadratic polynomial regression variable as specified in Eq. (1) in the text. The regressions include the following covariates: urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and a dummy for whether the household lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 4B
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 (0.0008)	−0.0011 (0.0011)	−0.0023 (0.0015)	−0.0069 (0.0020)	−0.0066 (0.0023)	−0.0107 (0.0028)	−0.0119 (0.0045)	−0.0046 (0.0049)
Discontinuity for males	−0.0008 (0.0010)	−0.0019 (0.0013)	−0.0028 (0.0014)	−0.0028 (0.0022)	−0.0052 (0.0028)	−0.0070 (0.0033)	−0.0009 (0.0050)	0.0051 (0.0053)
Discontinuity for whites	−0.0004 (0.0005)	−0.0010 (0.0007)	−0.003 (0.0009)	−0.0031 (0.0024)	−0.0052 (0.0019)	−0.0074 (0.0024)	−0.0075 (0.0038)	−0.0026 (0.0046)
Discontinuity for blacks	0.0002 (0.0023)	0.0009 (0.0032)	0.0026 (0.0040)	0.0024 (0.0109)	0.0069 (0.0070)	0.0046 (0.0109)	0.00460 (0.0141)	−0.0020 (0.0124)
Discontinuity for Hispanics	−0.0016 (0.0026)	−0.0056 (0.0035)	−0.0073 (0.0045)	−0.0149 (0.0057)	−0.0144 (0.0035)	−0.0207 (0.0080)	−0.0024 (0.0086)	0.0148 (0.0075)
Discontinuity for 30–39 year olds	−0.0003 (0.0008)	0.0004 (0.0010)	−0.0012 (0.0016)	−0.0047 (0.0022)	−0.0058 (0.0029)	−0.0064 (0.0035)	−0.0068 (0.0053)	0.0043 (0.0054)
Discontinuity for 40–49 year olds	−0.0004 (0.0009)	−0.0014 (0.0011)	−0.0030 (0.0015)	−0.0055 (0.0022)	−0.0069 (0.0028)	−0.0082 (0.0035)	−0.0065 (0.0064)	−0.006 (0.0067)
Discontinuity for 50–64 year olds	0.00 (0.0010)	−0.0007 (0.0014)	−0.0013 (0.0021)	−0.0008 (0.0030)	0.0003 (0.0038)	−0.00 (0.0021)	0.0005 (0.0066)	−0.0039 (0.0062)
Discontinuity for 65–79 year olds	−0.0026 (0.0037)	−0.0085 (0.0045)	−0.0073 (0.0054)	−0.0115 (0.0067)	−0.0158 (0.0067)	−0.0279 (0.0079)	−0.0196 (0.0115)	−0.01 (0.0088)
Date of birth controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on educational attainment, relative to being born just before the cutoff. Separate samples were created by gender, race, and age cohorts. Each coefficient was estimated separately, from a quadratic polynomial regression variable as specified in Eq. (1) in the text. The regressions include the following covariates: urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and a dummy for whether the household lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 5A
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 (0.0115)	−0.0005 (0.0019)	−0.0008 (0.0083)	0.0046 (0.0035)	−0.0039 (0.0071)	0.0025 (0.0024)
Discontinuity for males	0.0142 (0.0100)	−0.0008 (0.0024)	−0.0069 (0.0074)	−0.0017 (0.0026)	−0.001 (0.0101)	0.0052 (0.0026)
Discontinuity for whites	0.0068 (0.0097)	0.0009 (0.0016)	0.0014 (0.0073)	0.0021 (0.0027)	−0.0029 (0.0073)	0.0028 (0.0077)
Discontinuity for blacks	−0.0213 (0.0232)	−0.0091 (0.0057)	−0.0105 (0.0057)	0.0028 (0.0065)	0.0152 (0.0183)	0.0089 (0.0065)
Discontinuity for Hispanics	−0.0088 (0.0156)	−0.0014 (0.0042)	−0.0168 (0.0112)	−0.0015 (0.0050)	−0.0276 (0.0133)	0.0014 (0.0050)
Discontinuity for 30–39 year olds	0.0093 (0.0123)	−0.0031 (0.0027)	0.0007 (0.0112)	0.0080 (0.0052)	0.0122 (0.0126)	0.0112 (0.0044)
Discontinuity for 40–49 year olds	−0.0064 (0.0139)	0.0006 (0.0025)	−0.0089 (0.0117)	−0.0028 (0.0117)	0.0118 (0.0025)	0.0023 (0.0036)
Discontinuity for 50–64 year olds	0.0006 (0.0148)	0.0018 (0.0026)	0.0073 (0.0026)	0.0001 (0.0042)	−0.0221 (0.0111)	−0.0020 (0.0023)
Discontinuity for 65–79 year olds	−0.0286 (0.0466)	−0.0058 (0.0055)	−0.0182 (0.0144)	0.0015 (0.0048)	−0.0285 (0.0145)	0.0030 (0.0026)
Date of birth controls	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on adult outcomes, relative to being born just before the cutoff. Separate samples were created by gender, race, and age cohorts. Each coefficient was estimated separately, from a quadratic polynomial regression variable as specified in Eq. (2) in the text. The regressions include the following covariates: urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and a dummy for whether the household lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 5B
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 (0.0153)	−0.0018 (0.0027)	−0.0131 (0.0027)	−0.0052 (0.0042)	−0.0147 (0.0098)	0.0016 (0.0098)
Discontinuity for males	−0.0037 (0.0120)	0.0018 (0.0024)	0.0016 (0.0092)	0.0017 (0.0045)	80.00160 (0.0045)	−0.0004 (0.0105)
Discontinuity for whites	−0.0063 (0.0094)	−0.0021 (0.0018)	−0.0064 (0.0076)	−0.0021 (0.0029)	−0.0165 (0.0082)	−0.0012 (0.0029)
Discontinuity for blacks	−0.0080 (0.0336)	0.0130 (0.0104)	−0.0122 (0.0104)	0.0082 (0.0137)	0.0257 (0.0264)	0.0087 (0.0331)
Discontinuity for Hispanics	−0.0047 (0.0174)	0.0007 (0.0056)	0.0109 (0.0056)	−0.0104 (0.0085)	−0.0006 (0.0146)	−0.0040 (0.0085)
Discontinuity for 30–39 year olds	−0.0048 (0.0150)	0.0022 (0.0031)	0.0051 (0.0109)	−0.0023 (0.0062)	−0.0138 (0.0062)	0.0006 (0.0057)
Discontinuity for 40–49 year olds	−0.0184 (0.0133)	−0.0022 (0.0028)	−0.0124 (0.0113)	−0.0035 (0.0046)	−0.0184 (0.0117)	0.0001 (0.0046)
Discontinuity for 50–64 year olds	−0.0073 (0.0172)	−0.0025 (0.0034)	−0.0028 (0.0129)	−0.0011 (0.0048)	−0.0047 (0.0129)	0.0015 (0.0037)
Discontinuity for 65–79 year olds	0.0176 (0.0770)	0.0204 (0.0082)	−0.0308 (0.0206)	0.0027 (0.0042)	−0.0132 (0.0189)	0.0012 (0.0042)
Date of birth controls	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on adult outcomes, relative to being born just before the cutoff. Separate samples were created by gender, race, and age cohorts. Each coefficient was estimated separately, from a quadratic polynomial regression variable as specified in Eq. (2) in the text. The regressions include the following covariates: urban area, housing ownership, number of people in the house, number of rooms, household income, parental education and a dummy for whether the household lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

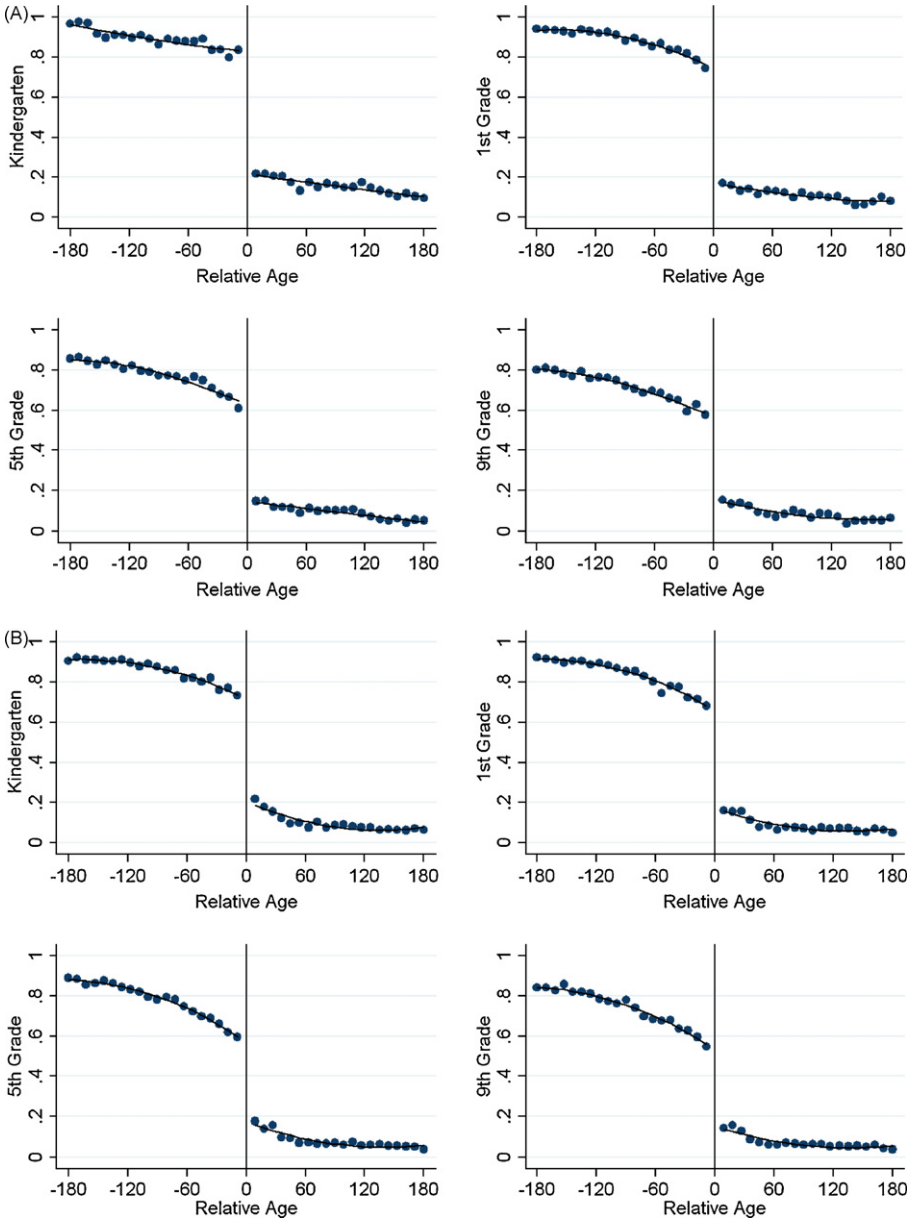

Fig. 5. (A) Grade enrolled by date of birth, Texas. (B) Grade enrolled by date of birth, California. *Notes*: A and B were estimated using the 2000 Decennial Long Form Census Data. Each figure shows the average enrollment for children of a certain age/grade group. For example, relative age equals to zero for kindergarten corresponds to a child exactly 5 years old on the day of the cutoff for school entry (6 years old for 1st grade, 10 years old for 5th grade and 14 years old for 9th grade). A relative age of −10 corresponds to a person born 10 days before the cutoff. 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.

In Tables 6A and 6B 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 6A reveals that the inclusion of the covariates has no statistically significant effect on the estimates for Texas. The regression estimates of the discontinuous change in grade enrollment induced by the school entry laws confirm what we saw in the figures. In Texas we find a difference of 62 percentage points in kindergarten, 59 percentage points in first grade, 50 percentage points in fifth grade, and 43 percentage points in ninth grade. Table 6B shows that though the discontinuity in enrollment in kindergarten is smaller in California than in Texas, 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

Table 6A
Impact of school entry laws on the grade students are enrolled, Texas.

	Kindergarten		1st Grade		5th Grade		9th Grade	
Discontinuity	−0.606 (0.016)	−0.618 (0.014)	−0.603 (0.013)	−0.591 (0.013)	−0.500 (0.014)	−0.497 (0.014)	−0.428 (0.014)	−0.425 (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	31,795		31,946		33,102		32,494	

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on grade enrolled, relative to being born just before the cutoff. Each coefficient was estimated separately from a quadratic polynomial regression variable as specified in Eq. (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 cutoff date. The conditional regressions use the microdata, and include individuals born 180 days before and after the cutoff date. For example, relative age equals to zero for kindergarten corresponds to a child exactly 5 years old on the day of the cutoff for school entry (6 years old for 1st grade, 10 years old for 5th grade and 14 years old for 9th grade). A relative age of −10 corresponds to a person born 10 days before the cutoff. These 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 a dummy for whether the household lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 6B
Impact of school entry laws on the grade students are enrolled, California.

	Kindergarten		1st Grade		5th Grade		9th Grade	
Discontinuity	−0.513 (0.014)	−0.527 (0.011)	−0.513 (0.012)	−0.503 (0.012)	−0.417 (0.014)	−0.417 (0.014)	−0.402 (0.014)	−0.399 (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	46,543		47,792		50,373		41,842	

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on grade enrolled, relative to being born just before the cutoff. Each coefficient was estimated separately from a quadratic polynomial regression variable as specified in Eq. (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 cutoff date. The conditional regressions use the microdata, and include individuals born 180 days before and after the cutoff date. For example, relative age equals to zero for kindergarten corresponds to a child exactly 5 years old on the day of the cutoff for school entry (6 years old for 1st grade, 10 years old for 5th grade and 14 years old for 9th grade). A relative age of −10 corresponds to a person born 10 days before the cutoff. These 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 a dummy for whether the household lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

abruptly at the school enrollment threshold rather than the legislation. That adding covariates to the regressions has no impact on the estimates implies that the changes we observe at the discontinuity are due to the school enrollment laws. In addition, we examined the data to see if there are discrete changes in any of the observable characteristics of the children at the cutoff date for early school entry. We see no evidence of this for a range of variables, such as gender, race, household income, and house ownership. Moreover, the variable state of residence in 1995 is also continuous around the threshold, indicating that selective migration is not a problem.[19]

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 Table 7A we show how the discontinuity in school enrollment in Texas evolves as children age. We do the analysis separately by gender, race and parental education. 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 the size of the 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 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 California are displayed in Table 7B and show similar patterns, although the differences between groups seem to be slightly more pronounced.[20]

The main conclusion from examining the tables and 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

[19] These estimates are available upon request. The only discrete change we observe is that children born after the cutoff 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. This difference almost completely disappears by age 6 as most of the children in private kindergartens enter the public school system for first grade.

[20] The pattern of retention rates within a cohort also varies substantially by race. Minorities are much less likely to be held back; by ninth grade blacks and Hispanic born right before the cutoff data are only 16 percentage points more likely to be retained then those born 180 days before the cutoff date. Though we also observe differences by gender and parental education, the most striking differences are across race. These calculations are available upon request.

Table 7A
Impact of school entry laws on the grade students are enrolled in by gender, race and parental education, Texas.

	Kindergarten	1st Grade	5th Grade	9th Grade
Discontinuity for boys	−0.599 (0.019)	−0.542 (0.019)	−0.421 (0.019)	−0.398 (0.019)
Discontinuity for girls	−0.636 (0.020)	−0.645 (0.015)	−0.579 (0.016)	−0.457 (0.017)
Discontinuity for whites	−0.603 (0.018)	−0.524 (0.019)	−0.448 (0.019)	−0.399 (0.018)
Discontinuity for blacks	−0.534 (0.039)	−0.568 (0.037)	−0.478 (0.036)	−0.445 (0.039)
Discontinuity for Hispanics	−0.660 (0.019)	−0.682 (0.016)	−0.571 (0.021)	−0.453 (0.022)
Discontinuity for college or more	−0.524 (0.027)	−0.482 (0.022)	−0.464 (0.024)	−0.366 (0.021)
Discontinuity for less than college	−0.660 (0.015)	−0.636 (0.015)	−0.513 (0.015)	−0.453 (0.016)
Date of birth controls	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on grade enrolled, relative to being born just before the cutoff. Separate samples were created by gender, race, and parental education. Each coefficient was estimated separately from a quadratic polynomial regression as specified in Eq. (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 cutoff date. The conditional regressions use the microdata, and include individuals born 180 days before and after the cutoff date. For example, relative age equals to zero for kindergarten corresponds to a child exactly 5 years old on the day of the cutoff for school entry (6 years old for 1st grade, 10 years old for 5th grade and 14 years old for 9th grade). A relative age of −10 corresponds to a person born 10 days before the cutoff. These 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 a dummy for whether the household lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

Table 7B
Impact of school entry laws on the grade students are enrolled in by gender, race and parental education, California.

	Kindergarten	1st Grade	5th Grade	9th Grade
Discontinuity for boys	−0.485 (0.014)	−0.456 (0.014)	−0.362 (0.017)	−0.341 (0.017)
Discontinuity for girls	−0.569 (0.016)	−0.549 (0.018)	−0.472 (0.017)	−0.459 (0.016)
Discontinuity for whites	−0.395 (0.019)	−0.360 (0.020)	−0.297 (0.016)	−0.277 (0.020)
Discontinuity for blacks	−0.491 (0.046)	−0.487 (0.037)	−0.384 (0.042)	−0.389 (0.037)
Discontinuity for Hispanics	−0.632 (0.014)	−0.612 (0.013)	−0.540 (0.019)	−0.509 (0.017)
Discontinuity for college or more	−0.405 (0.019)	−0.382 (0.019)	−0.283 (0.017)	−0.302 (0.021)
Discontinuity for less than college	−0.590 (0.013)	−0.565 (0.015)	−0.489 (0.016)	−0.452 (0.014)
Date of birth controls	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes

Notes: Table was estimated using the 2000 Decennial Long Form Census Data. Each coefficient estimates the impact of being born just after the state entry law cutoff day on grade enrolled, relative to being born just before the cutoff. Separate samples were created by gender, race, and parental education. Each coefficient was estimated separately from a quadratic polynomial regression as specified in Eq. (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 cutoff date. The conditional regressions use the microdata, and include individuals born 180 days before and after the cutoff date. For example, relative age equals to zero for kindergarten corresponds to a child exactly 5 years old on the day of the cutoff for school entry (6 years old for 1st grade, 10 years old for 5th grade and 14 years old for 9th grade). A relative age of −10 corresponds to a person born 10 days before the cutoff. These 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 a dummy for whether the household lived in the same state 5 years ago. Standard errors shown in parentheses are clustered at the exact date of birth.

age than white children. It is unlikely that blacks, Hispanics and children with less educated parents are on average 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.[21] However, as shown in Sections 4, there is no discernable effect on labor market outcomes, at least for older cohorts.

7. Conclusion

In this paper we documented that, though students that enter school at a younger age due to the school entry laws have poorer academic performance, on average they also have slightly higher educational attainment. When we examine the net impact of this tradeoff on adult outcomes we find no evidence that the timing of school entry affects wages or any of the other outcomes that we observe in the Census. Though the educational attainment of Hispanics is substantially more affected by the school entry laws than any of the other groups we find no evidence of any effect on labor market outcomes even in 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 at a younger age or that variation in academic performance that is due purely to relative age, and not adjusted away through retention, does not affect labor market performance.

We also found that contemporary cohorts of students born right before the cutoff date for school enrollment are significantly more likely to enroll in kindergarten a year earlier than similar students who were born right after the cutoff date. One third of these initial differences disappear by 9th grade since the youngest children in a cohort are

[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. It should also be noted that we do not examine other out-of-school alternatives available for minority children, some of which may be worse than attending school as the youngest student in a cohort.

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 make up a disproportionate share of the youngest students in a cohort.

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