School Entry Policies and Skill Accumulation Across Directly and Indirectly Affected Men

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Abstract

During the past half century, there has been a trend towards increasing the minimum age a child must reach before entering school in the United States. States have accomplished this by moving the school entry cutoff date earlier in the school year: generally from January 1 towards September 1. The evidence presented in this paper shows that these law changes increased human capital accumulation and hence adult wages. More specifically, backing up the school entry cutoff by one month (i.e. from January 1 to December 1) increases average male hourly earnings by approximately 0.5 percent. Perhaps more importantly, the available evidence also shows that the majority of the cohort benefits from backing up the cutoff, not just those whose entry are delayed.

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1. Introduction

What is the optimal age to start formal schooling? On one hand, the earlier children enroll in school, the sooner they begin accumulating the skills taught there. On the other hand, enrolling a child before he or she is ready for the academic rigors of formal education may be less productive than waiting until that child is more mature. In addition, the presence of children who are not yet ready for school may have a negative impact on the rate of human capital accumulation among other students in the class, as teachers are forced to alter curriculum choices and/or redirect resources towards these children. Despite the potential for offsetting effects, the actions of policy makers suggest that they believe that students benefit from later school entry. Early in the 20th century, most states allowed children to enter school in the fall as long as their fifth birthday occurred before January 1 (Angrist and Krueger, 1991). Since the mid 1960s, 26 states have increased their minimum school entry age. In 1964, 18 states required children to turn five on or before October 1 and by 1986, 30 states had imposed this requirement (see Table 1).

State policymakers have enacted earlier school entry laws for a variety of reasons. First, states with earlier cutoff dates have a higher average cohort age, which may improve individual's school readiness. Second, and perhaps more important to policy makers, backing up the cutoff date means that cohorts are older when national assessments take place, which improves cross-state relative test score rankings. Third, backing up the cutoff date generates a temporary reduction in cohort size, and hence temporary cost savings. For example, a recent policy study suggests that moving California's cutoff date from December 2 to September 1 would save between $392 and

$700 million dollars per year for the thirteen years that the smaller cohort attends public schools (Cannon and Lipscomb, 2008).

While, to the best of our knowledge, there is no empirical research examining the overall impact of school entry policies on student outcomes (both direct and indirect effects), there has been a recent flurry of interest in the impact of school entry age on academic performance. The usual approach is to use birth date variation relative to state-level school entry laws to estimate the return on being relatively older within a cohort. Many studies find that children who are older at school entry score higher on several important margins, ranging from better performance on standardized achievement tests (Bedard and Dhuey, 2006; Datar, 2006; Elder and Lubotsky, 2009; Puhani and Weber, 2007; Smith, 2007; Crawford, Dearden, and Meghir, 2007), to higher college enrollment rates (Bedard and Dhuey, 2006), to a higher probability of becoming a high school leader (Dhuey and Lipscomb, 2008), to earning higher adult wages (Fredriksson and Öckert, 2006; Kawaguchi, 2006). However, not all studies find long-term wage effects (Dobkin and Ferreira, 2009; Fertig and Kluve, 2005; Black, Devereux, and Salvanes, 2008).

While parents are justifiably concerned about the impact of age at school entry, the optimal minimum school entry age cutoff is the broader policy concern. In contrast to relative age (or age at entry), which we have a limited ability to influence, given that all cohorts will have an age continuum,[1] we do have the ability to set public school minimum entry age laws. Increasing the minimum entry age, by moving the cutoff date earlier in the year, has three distinct effects.[2] First, it increases the absolute age of directly affected children who must wait an extra year before entering school due to the

[1] Although dual and multiple entry dates decreases relative age differences, these structures are rarely used.
[2] Most school entry cutoff laws changes have moved the cutoff to earlier in the year. However, there are three cases in which the cutoff has been moved to a later date (see Appendix Table 1).

change in cutoff date. Second, it thereby increases the average age of the entire cohort. Third, it increases the relative age of children who are directly affected by the policy change and decreases the relative age of children who are not. School entry law changes therefore have both a direct effect on the students affected by the policy change as well as spillover effects on their classmates who are not directly affected by the policy change.

To give a concrete example, in 1973, New Mexico changed their school entry cutoff date from January 1 to September 1. Before the law change, the youngest children entering kindergarten were 56 months old (4 years and 8 months). After the law change the youngest entry age increased to 60 months.[3] This entry law change therefore increased the average cohort entry age from approximately 61.5 months to 65.5 months. While only children born between September 1 and January 1 were directly affected by the policy change, children born during the remainder of the year were indirectly affected by the increase in average starting age of their cohort and the change in their location on the relative age scale. Our estimates encompass both the effects on the directly and indirectly affected subsamples and should therefore be interpreted as the average effect of the policy shift.[4]

Using a state of birth level repeated cross-section for 1959-1981 male birth cohorts from the 2000 U.S. Census and the 2001-2007 American Community Surveys combined with school entry laws from 1964-1986, we find that backing up the school entry cutoff by one month (i.e. from January 1 to December 1) increases male hourly earnings by approximately 0.5 percent. Given an 'average' school entry change of 3

[3] This assumes that all children enter when eligible. We discuss early and late entry in Section 4.1.
[4] In Section 5, we separately examine the effects for the directly and indirectly affected subsamples.

months,[5] this translates into a 1.5 percent increase in the average male hourly earnings. This is a sizeable increase and points to a substantial return to increased average age at school entry within the entry cutoff range represented in the data, from September 1 through January 1. As there are no school entry dates changes before September 1 during the period under study, it is an open question whether pushing the school entry cutoff date even farther back would have a positive, negative, or neutral impact.

2. The Impact of Minimum School Entry Age Laws

Since the innovative work of Angrist and Krueger (1991), who use quarter of birth as an instrument for educational attainment,[6] many researchers have used birth dates and school entry and exit laws in somewhat modified ways. Prominent examples include Lleras-Muney's (2005) examination of the impact of education on adult mortality using compulsory schooling and work laws to instrument for educational attainment. Oreopolous, Page and Stevens (2006) use the same IV strategy to estimate the impact of parental education on offspring schooling outcomes. In addition, McCrary and Royer (2006) use a regression discontinuity design in California and Texas to compare the fertility outcomes of women born just before and just after the school entry cutoff date. Finally, Oreopolous (2006) and Clark and Royer (2007) use the increase in the national compulsory law in the U.K. in 1947 to estimate the impact of educational attainment on earnings (Oreopolous, 2006) and health and mortality (Clark and Royer, 2007).

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[5] The unweighted mean school entry change is 2.7 months.

[6] See Bound, Jaeger and Baker (1995), Bound and Jaeger (2000), and Dobkin and Ferreira (2009) for detailed discussions of the pros and cons of using quarter of birth as an instrument for educational attainment.

However, the age at entry literature discussed in the introduction and recent papers by Dobkin and Ferreira (2009) and Mazumder (2007) draw into question the use of quarter of birth and compulsory schooling laws in instrumental variable, state panel, and regression discontinuity frameworks, at least in the U.S. context.[7] There are two key issues. First, if relative age within a cohort directly affects human capital accumulation as well as affecting educational attainment then it likely has a direct impact on other outcomes and is an invalid instrument. Second, if compulsory schooling laws change educational attainment we should expect discontinuities in educational attainment localized near the binding cutoffs, but instead they appear over a range of high school grades. This leads one to wonder if it is really the interaction between school entry and exit laws that are driving the observed educational attainment differences (see Dobkin and Ferreira, 2009).

None of this, however, changes the fact that school entry and/or exit laws may have an important impact on human capital accumulation. Rather, it suggests that there may be multiple interacting effects associated with such laws. Minimum school entry age (cutoff) laws impact student outcomes in at least two important ways. First, and most obviously, they determine age at school entry: A January 1 cutoff implies a school entry age range of 56 to 67 months,[8] while a September 1 cutoff implies an entry age range of 60 to 71 months. While only children born between September 2 – December 31 are forced to wait an extra year before entering school under the September 1 cutoff compared to the January 1 cutoff, the other children in each school entry cohort are

[7] There is no evidence to suggest that the compulsory schooling change(s) used by Oreopolous (2006) and Clark and Royer (2007) are invalid.
[8] For descriptive ease, we assume that all children enter when eligible, we will return to this issue.

indirectly affected by the increase in average starting age and a change in their relative age position within the cohort.

It is easiest to discuss the implications for the directly affected group first. Since this group waits an extra year before entering school, there are three inter-related age effects. First, they are a year older when they enter school, which may increase their level of school readiness (see Stipek, 2002, for a review of the literature). While the rhetoric surrounding cutoff date changes suggests that it is widely believed that children have more rapid human capital accumulation if they enter school at older ages, theoretically, the impact is ambiguous and it is therefore an empirical question. In addition to becoming absolutely older, directly affected children also become relatively older; they switch from being the youngest children in their cohort to being the oldest children in their cohort. The findings from the age at school entry literature suggest that this aspect of the entry law change will have a positive, or at worst zero, impact on directly affected children. Third, the entire cohort is now older. To the extent that younger school-unready classmates have a negative impact on the entire class, postponing the enrollment of these individuals by a year may have a positive impact on the entire cohort. Since all of these effects are non-negative, with the possible exception of the absolute age effect, one may expect a positive net effect for the directly affected subgroup. It is worth pointing out, however, that these effects are not separately identifiable because the relative age and the cohort age changes add up to the absolute age change for directly affected individuals.

In contrast, the net effect for the indirectly affected group is less complicated, but of ambiguous direction. Since school entry age is unchanged for this group, the net effect has only two components. Just as for the directly affected group, the entire cohort is older. As discussed above, this should have a positive impact. On the other hand, this group is now relatively younger. For example, children born in January switch from being the relatively oldest in their cohort under a January 1 cutoff to a more middle position in the relative age distribution under a September 1 cutoff. At the same time, children born in August move from the middle of the relative age distribution to the relatively young end. Since the relative age and average cohort age effects may go in opposite directions, the net effect is ambiguous for indirectly affected children.

While the net effect for certain subgroups within school entry cohorts are theoretically ambiguous, the mean net effect for the entire cohort is likely positive. The likely positive overall average reflects the fact that the relative age effects wash out on average. While different students may be relatively older or younger, there is always a 12-month age range.[9] This only leaves the likely positive absolute age effect for directly affected students and the likely positive average cohort age effect for the entire cohort.[10] While the overall effect is therefore likely to be positive, there is no unambiguous prediction, rendering both the sign and the magnitude an empirical question.

The primary objective of this paper is to estimate the overall policy impact. More specifically, we estimate the net, or average, effect of changing the minimum school entry age law. We do this using state of birth level repeated cross-section. Since age at school entry and the peer effects associated with cohort age composition can affect skill accumulation either directly through within grade human capital accumulation rates or

[9] Years in which the cutoff changes are exceptions: The age range is shorter in these years.
[10] Note that the two effects are not separately identifiable since they move together.

through educational attainment, the most natural way to think about estimating the impact of minimum school entry age laws on adult earnings is as follows:

$$W_{ibty} = \alpha_0 + \alpha_1 S_{bt} + X_{ibt}\alpha_2 + A_{ibty}\alpha_3 + B_b\alpha_4 + T_{bt}\alpha_5 + \varepsilon_{ibty} \quad (1)$$

where W_{ibty} denotes the ln adult wage, for individual *i* born in state *b* in year *t* observed in Census or American Community Survey year *y*, S_{bt} denotes the age at which the youngest member of the cohort is eligible for kindergarten in birth state *b* in birth year *t*, X_{ibt} is a vector of race indicators and birth state level controls,[11] A_{ibty} is a vector of age indicators, B_b is a vector of state of birth indicators, T_{bt} is a vector of census division of birth specific cohort indicators, and ε_{ibty} is the usual error term.[12] Notice that equation (1) does not hold educational attainment constant since school entry laws may change skill accumulation through either within grade human capital accumulation or through educational attainment. All models are population weighted and the standard errors are clustered at the state of birth level.

It is worth re-emphasizing that the reduced form estimate of the effect of the minimum school starting age on earnings described by equation (1) is the average effect of the policy on the entire birth cohort. In other words, it is the overall average impact of a change in the minimum school starting age, as opposed to the average impact of the policy on just those children whose school entry are delayed by the change in the minimum school starting age. It also is important to note that α_1 is also net of changes in parental decisions regarding early and late entry. If all parents simply enrolled their children as soon as they became eligible we would observe exactly the correct fraction of

[11] This includes kindergarten subsidization, pupil teacher ratio, relative teacher salaries, and compulsory school leaving age. See Section 3.3 for details.
[12] Alternative specifications are explored in Sections 4 and 5.

each month or quarter of birth enrolled in school at age five. However, some parents enroll their child a year early and some hold their child back and enroll them a year late (see Dhuey, 2009). To the extent that these decisions are sensitive to cutoff dates, the reduced form estimate is net of this. In particular, if backing up the cutoff date means that fewer children born in the fall are voluntarily held out of school for a year by their parents then $\hat{\alpha}_1$ will be smaller than might be expected since there is less change in cohort composition than predicted as these children were already "conforming" to the new cutoff even before it existed. In the same vein, backing up the cutoff may also induce some parents to switch from on-time entry to early entry, which will again reduce the estimated effect since it again amounts to no change in observed behavior. We will return to this issue in Section 4.1.

3. Data

3.1 *School Entry Laws*

In most states, a statewide statute or regulation mandates the age at which children are eligible to enter primary school. For example, a child can enter school in California as long as the child turns five by December 2 of that academic year. For descriptive ease, Table 1 reports the number of states by cutoff month in 1964 and 1986. For example, the first row reports the number of states that have a cutoff date of January 1 or February 1. This means that children need to reach age five before January 1 or February 1, respectively. The last two rows report the number of states that leave school entry to the discretion of local education authorities or have no school entry law, respectively. Table 1 reveals a clear pattern: states have been backing up their school entry laws over time forcing children to be older before entering the education system. In 1964, 8 states required children to be five by September but by 1986, 19 states had this requirement. The complete set of entry laws from 1964-1986 are reported in Appendix Table 1.

All school entry cutoff dates were collected from state statutes and corresponding historical state session laws and/or regulations. The current list of statutes with citations can be found in Appendix Table 2. In addition, Appendix Table 3 lists the cutoff date in each state in 1964 with its corresponding legal citation regarding this cutoff date in 1964. The table then lists the year of change, if any, what the new cutoff date is and the legal citation which indicates the changing of the cutoff date. A legislative history of each statute from 1964-1986 is available from the authors upon request.[13]

In order to simplify the coding of dates, all entry laws are coded as either the first of the month or mid-month. This avoids confusion between end of month and beginning of month differentiation and inconsequential law changes of one or two days.[14] States that do not have statutes or regulations regarding their entry law during a particular time period are reported as none during those years in Table 1 and Appendix Table 1 and are coded as missing in the data. States that leave school entry at the discretion of local authorities are also coded as missing in the data since we do not have sub-state level

[13] These cutoff dates have been cross-referenced with Angrist and Krueger (1992), Cascio and Lewis (2006), the Digest of Education Statistics (1972, 1973, and 1983), the Educational Research Service (1975), and information from the website of the Education Commission of the States (http://www.ecs.org). Some conflicting cutoff date information exists between sources. It is unclear why the dates differ but if our cutoff date differed from a previously published source, we re-checked the legislative history for the statute. If the dates differ, we list the date indicated in the statutes and corresponding historical state session laws for that particular year. See Appendix Table 3 for more details regarding citations.
[14] This simplification has no substantive effect. The estimates using exact date are available upon request.

information. Lastly, states requiring children to be five years old by the start of the school year in order to enroll are coded as a September 1 cutoff.[15]

Estimating equation (1) requires that we restrict attention to the subset of years reported in Appendix Table 1 (cohorts who are age five in 1964-1986). We use these cohorts because we need to calculate the age at which the youngest member of the cohort is eligible for school entry and link the cohort to adult wages later in life. As will be discussed in detail in the next section, the best available wage data come from the 2000 U.S. Census and the 2001-2007 American Community Surveys. Unless otherwise stated, all analyses use state school entry cutoffs from 1964-1986. This translates into using the entry cutoff dates for 1959-1981 birth cohorts.

3.2 *Wages*

In order to match people to the school cutoff in place when they were entering school, we assign cutoffs to individuals based on the law in place in the state of birth when their 'cohort' was 5 years old. Since quarter of birth is only available for the 2005-2007 ACSs – the census and the 2001-2004 ACS only report age as of April 1 – we assign people to age 5 cohorts using year–age+4. While this incorrectly assigns some people, it is the best that can be done without more detailed birth date data. However, in our particular case, incorrect cohort assignment is only a problem in years with policy changes. We therefore solve the problem by dropping observations for the year before a change, the year of a change, and the year after a change.[16]

[15] The one exception is Montana, which is coded as mid-September because they list September 10th beginning in 1979, and it does not appear that this was a change in policy from the previous regime.
[16] This approach also has four additional benefits. (1) It is appropriate in case where there are leads or lags in adoption. (2) It eliminates policy-timing difficulties associated with states in which school entry is grade

Ideally, one would restrict attention to individuals who have completed all major schooling and who are pre-retirement. For example, by restricting the sample to individuals ages 30-54 or 35-54. However, most of the cutoff changes occurred fairly recently – there are only seven cutoff changes between 1964 and 1975. Since it is important to use the most recent cohorts possible, we restrict the sample to U.S. born men from the 1959-1981 birth cohorts in the 2000 U.S. 5 percent Public Use Micro Census and the 2001-2007 American Community Survey (ACS). The ACS is a nationally representative annual 1 in 250-person sample of the United States. This choice of sample allows the use of 20 statewide cutoff changes. Using the ACS has two important advantages. First, it increases the available data for young cohorts surrounding cutoff changes. Second, the addition of a year of observation dimension allows us to control for age and birth cohort separately.

The drawback to focusing on the 1959-1981 birth cohorts is that wage observations are at younger than optimal ages for the later cohorts. The sample includes men aged 25-47 who reside in the 48 contiguous states.[17] This choice is a tradeoff between two factors. On one hand, we would prefer to focus on wages after age 30 when we are more confident that educational investments are largely complete. On the other, this would require excluding the 1976-1980 birth cohorts, which means losing a quarter of the cutoff changes as well as losing more than half the wage observations surrounding

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one rather than kindergarten. (3) Excluding these years also ensures that we are not confounding entry age law changes with changes in cohort size in the years immediately surrounding cutoff changes. If everyone enters on their legally defined date, every month that the school cutoff moves back implies a 1/12 reduction in the cohort size for the first year of the cutoff change. (4) It mitigates problems associated with the miss-match caused by allocating individuals to birth cohorts based on age rather than based on school year cohorts because the miss-allocation only miss-assigns school entry policies in the years directly surrounding changes. While cohort assignment based on school years rather than age does not suffer from the miss-allocation problem, it is impossible implement because we do not have specific birth dates.

[17] Observations with imputed data or missing education information are excluded from the sample.

another quarter of the cutoff changes (those for the 1972-1975 birth cohorts). Given these data limitations, we focus on young adult wages, age 25-47, and focus only on the wage effects for men who are employed.[18]

Table 2 summarizes the Census and ACS data. It reports summary statistics for U.S. born men under two different sample definitions. Column 1 reports the summary statistics for the sample of all men. This sample is used to examine the impact of cutoff changes on educational attainment. Column 2 is restricted to men who are not in school or prison and who report positive hours and weeks of work as well as positive wage income. This is the primary sample used in the majority of the analysis.

3.3 *Other Education Policy Controls*

The identification of the model comes from state-time variation in minimum school entrance ages induced by statewide school entry policy changes. If cutoff changes tend to be bundled with other policies that affect academic and hence labor market outcomes, it is important to control for these in equation (1). While we are aware of no evidence of other policies being bundled with cutoff date changes, we control for school exit laws, pupil-teacher ratios, teachers salaries, and the beginning of state subsidized kindergarten.

The pupil-teacher ratio is the number of students in each state divided by the number of teachers. Birth cohorts are assigned the average pupil-teacher ratio during their thirteen years of available public schooling. Relative teacher salaries are defined as the average wage of teachers divided by the average wage of 30-49 year old male BA holders in the 1950-2000 U.S. Censuses (inter-census years are linearly interpolated). The number of students, the number and the wage of teachers, and the oldest age required

[18] We focus on men to minimize selection issues related to labor market non-participation.

by compulsory schooling laws are from the *Digest of Education Statistics.* Information not provided by the *Digest of Education Statistics* regarding the oldest age required by compulsory schooling are from state statutes and corresponding historical session laws. The small number of cases with missing student and teacher counts and wages are linearly extrapolated. The beginning of state subsidized kindergarten is an indicator variable for whether states subsidized kindergarten with state revenue in a particular state for a particular birth cohort.[19]

4. Short-Run Effects of Minimum School Entry Age Laws

While our ultimate goal is to examine the impact of minimum school entry age laws on adult earnings, the existence of such effects depends on compliance with law changes. Before turning to the wage estimates, we therefore examine the available evidence on compliance with school entry date cutoff changes. Further, while cutoff changes can impact adult productivity, and hence wages, with or without changing educational attainment, it nonetheless important to examine the impact of this policy on educational attainment.

4.1 *Do Minimum School Entry Age Laws Change School Entry?*

Compliance with minimum school entry age law changes is imperfect because parents and/or educators can advance or delay school entry for specific children. Acceleration and deferral usually require petitioning the school or district for an exception. While in recent years it is rare for children to enter school early, it was more common in the past.

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[19] See Dhuey (2009) for information regarding collection of data on state subsidized kindergarten.

[20] For example, in 1980, 8 percent of children born in the fourth quarter of the year in Minnesota were enrolled in kindergarten even though the official cutoff date was September 1. At the same time, 5 percent of the children born in the first quarter of the year from the same cohort were not enrolled in kindergarten, even though according to the minimum school entry laws they were eligible. In contrast, in Maryland, 87 percent of fourth quarter children were enrolled in kindergarten in 1980, which means that 13 percent deferred entry given the January 1 cutoff date.

We estimate the impact of changes in minimum school entry age laws on school enrollment using data on six year olds[21] residing in the 48 contiguous states that have state-level minimum school entry laws the 1960-1980 Censuses[22] using the following modified version of equation (1).

$$E_{iry} = \beta_0 + \beta_1 S_{ry} + X_{iry}\beta_2 + R_r\beta_3 + Y_{ry}\beta_4 + \nu_{iry} \qquad (2a)$$

where E_{iry} is the enrollment status (1 = enrolled in first grade or higher)[23] of child *i* in state of residence *r* in census year *y*, S_{ry} denotes the age at which the youngest member of the cohort is eligible for school entry, X_{iry} is a vector of race indicators and an indicator for the availability of publically subsidized kindergarten, R_r is a vector of state of residence indicators, Y_{ry} is a vector of census division specific year indicators, and ν_{iry} is

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[20] Using data from the Early Childhood Longitudinal Study, Kindergarten Class of 1998-99, only 1.8 percent of children entered kindergarten early in the 1998 school year.

[21] We focus on enrollment in first grade rather than kindergarten for two related reasons: (1) kindergarten enrollment is not compulsory in most states during this time period, and (2) some states have low kindergarten enrollment rates during this period, at least partly due to kindergarten not being subsidized with state revenue.

[22] Cohorts are defined by age rather school year due to data limitations. As with all other estimates reported in this paper we exclude the year before, of, and after policy changes to ensure that we are not confounding other factors. All results are similar if we define cohorts based on the calendar year instead of age. See page 9 and footnote 16 for a detailed discussion of these issues.

[23] The results are almost identical if we define enrollment status as 1 if enrolled in grade 1.

the usual error term. All models are population weighted and the standard errors are clustered at the state level.

The equation (2a) results are reported in Table 3. The results for both males and females are reported in column 1 and the results for the male sub-sample are reported in column 3. Focusing on column 1, backing up the school cutoff date by one month decreases the fraction of six year olds enrolled in grade one or higher by 3.3 percentage points and backing it up by three months decreases enrollment by 9.9 percentage points. If the entire impact comes from those directly effect by the cutoff change, this implies a compliance rate of approximately 40 percent. These findings of imperfect compliance are consistent with Dobkin and Ferreira (2009). The large discrepancy reflects the fact that children near cutoffs are more likely to be accelerated or retained.

Unlike the majority of the available wage data, the 1960-1980 Census data includes quarter of birth information. This allows us to roughly check that changes in enrollment are driven by groups directly effected by the backing up of cutoff dates. More specifically, since all cutoff changes during this period occur between January 1 and September 1, most enrollment changes should occur amongst children born in the fourth quarter, with a small impact on third quarter children. We therefore generalize equation (2a) to allow differential effects across quarters.

$$\begin{aligned} E_{irqy} &= \beta_0 + \beta_1 S_{ry} + \beta_2 SQ^1_{rqy} + \beta_3 SQ^2_{rqy} + \beta_4 SQ^3_{rqy} + Q_q \beta_5 \\ &\quad + X_{iry}\beta_6 + RQ_{rq}\beta_7 + YQ_{qy}\beta_8 + Y_{ry}\beta_9 + \nu_{irqy} \end{aligned} \tag{2b}$$

where q denotes quarter of birth, SQ^1_{rqy}, SQ^2_{rqy}, and SQ^3_{rqy} denote the interaction of an indicator variable for birth quarter one, two, and three, respectively, with the age at which the youngest member of the cohort is eligible for school entry in birth state r, Q_q are indicators for birth quarter one, two, and three, RQ_{rq} is a vector of state by birth quarter indicators, and YQ_{qy} is a vector of cohort by birth quarter indicators. This specification allows the impact of cutoff changes to differ across birth quarters.

The equation (2b) results are reported in columns 2 and 4 in Table 3 for the males and females and the male sub-sample, respectively. Focusing on column 2, backing up the school cutoff date by three months decreases the fraction of fourth quarter six year olds enrolled in grade one or higher by 40.5 percentage points, reduces enrollment among third quarter babies by 5.7 percentage points, and has no measurable impact on enrollment for first and second quarter six year olds.

4.2 *Educational Attainment*

While it is not necessary for cutoff changes to affect educational attainment in order to have an impact on labor market outcomes, given that there can be a direct impact on skill accumulation, it is nonetheless important to examine the possible educational attainment effects before estimating the effect on wages. The most natural way to think about estimating the impact of school entry age on educational attainment follows directly from the specification of the basic wage model described by equation (1) in Section 2.

$$Ed_{ibty} = \pi_0 + \pi_1 S_{bt} + X_{ibt}\pi_2 + A_{ibty}\pi_3 + B_b\alpha_4 + T_{bt}\pi_5 + \omega_{ibty} \qquad (3)$$

where Ed_{ibty} denotes the attainment of a specified level of education, for individual *i* born in state *b* in year *t* observed in Census or ACS year *y*, S_{bt} denotes the age at which the youngest member of the cohort is eligible for school entry in birth state *b* in year birth year *t*, X_{bt} is a vector of individual and state of birth specific controls (including race, the availability of publicly provided kindergarten, the pupil teacher ratio, relative teacher salaries, and the compulsory school leaving age), A_{ibty} is a vector of current age indicators, B_b is a vector of state of birth indicators, and T_{bt} is a vector of census division of birth specific cohort indicators. All baseline models of this form are reported in column 1 in all subsequent tables.

While the most reduced form approach is to exclude later life controls, such as marital status and factors that depend on current residential location, as they may all be endogenous to the policy change of interest, we nonetheless check the robustness of our results to a variety of additional controls. All column 2 models include a vector of state of residence indicators, a vector of census division of residence specific age indicators, state of residence specific GDP and unemployment rates,[24] and marital status. All column 3 models further add a set of region of birth – region of residence interactions to control for selective migration. Heckman, Layne-Farrar, and Todd (1995) show that non-random migration across regions may confound education policy point estimates. We follow their approach and check the sensitivity of our results to including a matrix of region of birth and region of residence interactions that control for migration choices. In all cases, our point estimates are robust to their inclusion.

Table 4 reports the results for equation (3) for men aged 25-47 in the 2000 U.S. Census and the 2001-2007 ACSs. The first row reports the estimated impact of a one-month moving back of the school start date earlier in the year on the probability of graduating from high school. Rows 2 and 3 similarly report the probability of obtaining some college or more and obtaining an undergraduate degree or more. There is no

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[24] State GDP data are from the Bureau of Economic Analysis and are reported in 2007 dollars. State unemployment rates are from Bureau of Labor Statistics.

evidence that school start dates impact educational attainment at any level. As such, any impact on wages must be coming through changes in within grade skill accumulation.

5. The Long-Run Effect Minimum School Entry Age Laws on Adult Wages

The baseline equation (1) ln hourly wage estimate is reported in row 1 of column 1 in Table 5. The sample includes men aged 25-47 in 2000-2007 who are not in prison or school and who report positive income, usual hours of work, and weeks of work. Similar to Table 4, columns 2 and 3 include an expanded set of control variables. Column 2 controls for state of residence, cohort, census division of residence cohort specific indicators, state of residence GPD and unemployment rate, and marital status. Column 3 further adds an interaction between region of birth and region of residence. Depending on the specification, we estimate that a one-month increase in the minimum school starting age increases average hourly wages by 0.47 - 0.58 percent.[25] Using the mean cutoff change of 3 months, this range translates into a 1.41 - 1.74 percent increase in average male hourly wages. One might be concerned that these estimates are downward biased because the policy change implies a year less experience due to later school entry. It is true that those who are directly affected by the policy wait an extra year to begin school, and hence have one less year of work experience when observed in the wage data. However, for each month that the school start date is backed up, only one-twelfth of the population is directly affected. If we add back in the 2 percent per year average return to

[25] The point estimates are similar if ln weekly wages or total wages (including zeros) are used instead. The point estimates are also similar under a variety of sampling criteria. For example, restricting the sample to men who work at least 10 hours per week, or restricting the sample to white men. Alternative specifications are available from the authors upon request. While we can not restrict the sample to older men for the reasons described on page 9, the point estimates based on progressively younger samples are qualitatively similar: α_1 is 0.0052, 0.0054, and 0.0049, with standard errors of 0.0022, 0.0026, and 0.0024 for samples restricted to men aged 25-45, 25-40, and 25-35, respectively, using the specification reported in column 1 in Table 5.

experience lost to the directly affected month of men, the point estimate of 0.0047 would rise to approximately 0.0064. Another way to gauge this issue is to isolate a group for which there is no experience loss and measure the effect of the policy change on this group's wages. We follow this strategy in Table 6 using a subset of the ACS data.

In addition, when thinking about the magnitude of the policy effect it is also important to remember that students receive the treatment for the entire time they are in school; thirteen years for high school graduates. Furthermore, all students in the cohort may benefit from having an older cohort. In other words, the average effect is the sum of all direct and spillover effects. More specifically, in the following pages we show that indirectly affected children get a substantial wage benefit from backing up the school entry cutoff. This finding is important because a point estimate of 0.47 percent per month that comes only from directly affected children is clearly unreasonable, as it would imply a 5.64 percent effect for the directly affected month and zero for the other eleven months. As we show below, this is not the case – a substantial indirect effect is driving the estimate.

While the results reported in Table 5 clearly point to a substantial return to later school entry dates, they also raise the question of exactly who benefits from the policy change. Does the wage return largely reflect an increase for those whose school entry is directly affected, or are there indirect effects for other segments of the cohort as well? Our ability to examine this issue is limited by the relative lack of data regarding birth date. However, beginning in 2005 the ACS reports quarter of birth. The 2005-2007 ACS data can therefore be used to at least crudely examine the impact of cutoff date changes on specific segments of class cohorts. We use the term crude because quarter of birth does not allow for exact identification of school cutoff for all birth months.

For the cohorts included in this analysis, only men born between September 1-December 31 are directly affected by cutoff law changes (see Appendix Table 1). The aggregation of birthdays to the quarter of birth level in the ACS substantially complicates the analysis of who is affected. The problem arises because the policy change may affect children either directly or indirectly, despite their being from the same quarter of birth. For example, children born in December are directly affected when the cutoff is moved from January 1 to December 1, whereas children born in October and November are indirectly affected. As the ACS does not allow us to identify at any level more detailed than quarter of birth, we cannot separate the directly affected children from the indirectly affected children in this case. Unfortunately, these types of within quarter changes make up the majority of the cutoff changes during the sample period. The estimates for both third (July-September) and fourth (October-December) birth quarters are therefore difficult to interpret. This further means that it is impossible to cleanly estimate the minimum age entry effect for directly affected children.

Given the fact that all cutoff changes during the sample period occur between September 1-December 31, on the surface it therefore appears that the impact of cutoff changes for quarters one (January-March) and two (April-June) should be easily interpretable since all children in these groups are indirectly affected. While this is true for quarter two, the quarter one estimates should be interpreted with care due to possible non-random changes in voluntary school entry. As the cutoff is backed up from December 31 towards earlier in the fall, it is likely that fewer first quarter children enter school before they are legally eligible; parents stop enrolling their children in school early. As such, a small fraction of first quarter children are essentially directly affected by the policy change. The quarter one estimates cannot therefore be interpreted as purely indirect. This leaves us with quarter two. We can obtain a lower bound estimate of the cohort age effect using this sub-group. It is a lower bound because we cannot separate the positive cohort effect and the negative relative age effect.

Operationally, we modify equation (1) to allow cutoff changes to differentially impact birth quarters.

$$\begin{aligned} W_{ibtqy} = {} & \delta_0 + \delta_1 S_{bt} + \delta_2 SQ^1_{btq} + \delta_3 SQ^2_{btq} + \delta_4 SQ^3_{btq} + Q_q \delta_5 \\ & + X_{ibt}\delta_6 + A_{ibtqy}\delta_7 + BQ_{bq}\delta_8 + Y_{tq}\delta_9 + T_{bt}\delta_{10} + \varepsilon_{ibtqy} \end{aligned} \tag{4}$$

where q denotes quarter of birth, SQ^1_{btq}, SQ^2_{btq}, and SQ^3_{btq} denote the interaction of an indicator variable for birth quarter one, two, and three, respectively, with the age at which the youngest member of the cohort is eligible for school entry in birth state *b*, X_{ibt} is a vector of individual and birth state level controls (see Section 3.3), Q_q are indicators for birth quarter one, two, and three, BQ_{bq} is a vector of state of birth by birth quarter indicators, Y_{tq} is a vector of cohort of birth by birth quarter indicators, and T_{bt} is a vector of census division of birth specific cohort indicators.

Table 6 reports the impact of the minimum school starting age on ln hourly wages by birth quarter using the 2005-2007 ACS. For comparative purposes, Panel A (labeled pooled) reports $\hat{\alpha}_1$ for equation (1) using only the 2005-2007 ACS. The next four rows (Panel B) report the quarter of birth specific effect of backing up the cutoff by one month (from equation (4)): Row 1 reports $\hat{\delta}_1$ and its corresponding standard error and rows 2-4 report the interaction terms ($\hat{\delta}_2$, $\hat{\delta}_3$, and $\hat{\delta}_4$) and their appropriate standard errors.[26]

The point estimate for youngest legal entry age is positive and statistically significant and the interaction terms for quarters one and two are always small and statistically insignificant. In other words, we cannot reject the null hypothesis that backing up the cutoff by one month has the same effect on quarters one and two as it does on four. In contrast, the interaction term for quarter three is always negative and statistically significant, and in no case can we reject the null hypothesis that $\delta_1 + \delta_4 = 0$. These findings for quarter three likely reflect factors. First, the majority of this group becomes the relatively youngest in most cases, which is a negative effect. Second, similar to quarter four, the point estimate for quarter three is a mixture of direct and indirect effects.

As discussed above, the most interesting result reported in this table is the finding that the point estimate for second quarter children is not statistically distinguishable from those of fourth quarter children. As the second quarter only includes indirectly affected children, the point estimate is a mixture of the effect of having an older cohort along with the effect of being relatively younger in the age distribution. This point estimate is therefore a lower bound for the cohort age effect because the relative age effect is negative for this group. This finding is important for at least two reasons. First, it means that the average point estimates reported in Table 5 reflect both direct and indirect effects; backing up the school entry cutoff has positive spillover effects that benefit all or at least most of the cohort. In the absence of these spillovers the point estimates reported

[26] For completeness, Appendix Table 4 reports the corresponding results for educational attainment.

in Table 5 would be too large. Second, it allows us to separate lost labor market experience from the school entry age policy effect because the school entry timing of quarter two children is not altered by the policy change. As such, the concern that we are under-estimating the impact of the policy change does not apply in this case.

While the available data is not ideal, in the sense that we cannot perfectly separate directly and indirectly affected individuals, Table 6 still delivers a very important finding: Backing up the cutoff date has an economically significant positive effect for both directly and indirectly affected individuals.

6. Conclusion

This paper documents the statistically significant and economically important positive earning effect associated with backing up school cutoff dates. We find that increasing the minimum school entry age increases wages, but has no measurable effect on educational attainment. This implies that increases in within grade human capital acquisition are mostly responsible the for the estimated wage return. In particular, a one-month increase in the minimum school entry age increases wages by about 0.5 percent. In addition, we report preliminary evidence that suggests that minimum age entry law changes have a positive impact on the fraction of the cohort that is indirectly affected, not just children directly affected by the policy change.

While backing up cutoff dates is not costless – directly affected individuals are forced to enter elementary school and the labor market a year later[27] – it likely uses fewer

[27]Approximating lifetime costs and benefits requires assumptions about labor market entry age, retirement age, the return to experience, the starting wage, and the discount rate. For illustration purposes we assume the following: labor market entry at age 20 with a starting wage of $25,000 per year, quadratic experience growth ($0.02X-0.0003X^2$), retirement at age 64, a 2 percent discount rate, and an extra $5000 day care cost

public funds than many other interventions (class size reductions, for example). This policy is also likely popular in an era of national testing, since students in earlier cutoff states score higher. The key unanswered question is, of course, what is the optimal minimum entry age law? The estimates reported in this paper clearly show that there are gains associated with backing the cutoff up from January to September; they do not however tell us whether there would be gains or losses associated with backing it up even farther.

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for children who must wait an extra year to start school. Under these assumptions, directly affected individuals, those who must wait an extra year to enter school, suffer a lifetime wage loss if the policy effect is less than approximately 4 percent because they must spend an extra year in private cost childcare and they lose a year of employment (assuming that retirement age is unaffected). At the same time, there is an overall gain to the cohort at large as long as the policy effect is greater than approximately 0.35 percent. The gap between these estimates arise because 1/12 of the cohort pays the cost of policy change while 11/12 of the cohort only benefits from the skill accumulation gain. As the estimates in Table 6 suggest that the indirect effect is 1 percent and the direct effect is at least 1.6 percent, there appears to be a small loss to directly affected individuals and a substantial gain to the rest of the cohort.

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Table 1. Cutoff Date Distribution

	Number of States	
	1964	1986
January / February	13	8
December	5	3
November	5	3
October	10	11
September / Start of school year (SSY)	8	19
Local education authority (LEA)	3	5
None	6	1

Table 2. Census and ACS Summary Statistics

	All Men (1)	Employed Men (2)
Wages		
Ln hourly wage	--	2.96
		(0.70)
Education Outcomes		
High school dropout	0.10	0.08
	(0.30)	(0.28)
High school graduate	0.31	0.32
	(0.46)	(0.46)
Some college	0.31	0.30
	(0.46)	(0.46)
BA+	0.29	0.30
	(0.45)	(0.46)
School Start Date		
Age of youngest children (months)	57.67	57.66
	(1.50)	(1.50)
Other State Education Policies		
Kindergarten	0.84	0.84
	(0.36)	(0.36)
Pupil-teacher ratio	19.60	19.59
	(2.33)	(2.31)
Relative teacher salaries	0.64	0.64
	(0.07)	(0.07)
School leaving age	16.47	16.47
	(0.79)	(0.79)
Other Variables		
State of residence unemployment rate	5.10	5.09
	(1.30)	(1.30)
State of residence GDP (in millions)	527,886	521,122
	(459,229)	(451,975)
Black	0.11	0.10
	(0.31)	(0.30)
Hispanic	0.08	0.08
	(0.27)	(0.26)
Other	0.03	0.03
	(0.17)	(0.16)
Married	0.57	0.61
	(0.49)	(0.49)
Sample Size	1,676,264	1,351,799

Summary statistics are population weighted. Standard deviations in parentheses. All dollar values are reported in 2007 currency.

Table 3. The Impact of the Minimum School Starting Age on First Grade Enrollment

	Females and Males		Males Only	
	(1)	(2)	(3)	(4)
Age of youngest children (months)	**-0.033**	**-0.135**	**-0.040**	**-0.137**
	(0.008)	(0.025)	(0.007)	(0.026)
Youngest×(January-March)		**0.141**		**0.145**
		(0.022)		(0.023)
Youngest×(April-June)		**0.135**		**0.132**
		(0.024)		(0.027)
Youngest×(July-September)		**0.116***		**0.112***
		(0.028)		(0.031)
Sample size	212,888	212,888	108,255	108,255

The dependent variable is one if the individual is enrolled in grade one or higher and zero otherwise. All models are population weighted and clustered at the state of residence level. Heteroskedastic-consistent standard errors in parentheses. Bold coefficents are significant at the 5 percent level. Columns 1 and 3 include indicators for the existence of publically funded kindergarten, sex, race, state of residence, and census division specific year cohorts. Columns 2 and 4 further include birth quarter indicators and interactions between state of residence and birth quarter and year and birth quarter. The sample includes 6 year olds from the 1960, 1970, and 1980 U.S. Censuses. A star in column 2 or 4 indicates that youngest plus the specified interaction effect (i.e. $\beta_1+\beta_2$, $\beta_1+\beta_3$, or $\beta_1+\beta_4$) is non-zero at the 95% level.

Table 4. The Impact of the Minimum School Starting Age on Educational Attainment

	(1)	(2)	(3)
High school graduate or higher	0.0007	0.0008	0.0008
	(0.0017)	(0.0017)	(0.0017)
Some college or higher	0.0009	0.0013	0.0014
	(0.0018)	(0.0017)	(0.0016)
BA or higher	0.0004	0.0011	0.0012
	(0.0017)	(0.0016)	(0.0016)
Additional Controls:			
State of Residence	No	Yes	Yes
CD of Residence*Age	No	Yes	Yes
State of Residence GDP & UER	No	Yes	Yes
Marital Status	No	Yes	Yes
Region of Birth*Region of Residence	No	No	Yes
Sample Size	1,676,264	1,676,264	1,676,264

The sample includes men aged 25-47 in 2000-2007. All models are population weighted and clustered at the state of birth level. All models also include controls for kindergarten subsidization, pupil teacher ratio, relative salary of teachers, compulsory school leaving age, race, state of birth, age, and census division of birth specific cohort indicators. Heteroskedastic-consistent standard errors in parentheses. Bold coefficients are statistically significant at the 5% level and bold italics are statistically significant at the 10% level.

Table 5. The Impact of the Minimum School Starting Age on Ln Hourly Wages

	(1)	(2)	(3)
Age of youngest children (months)	**0.0047**	**0.0058**	**0.0058**
	(0.0021)	(0.0023)	(0.0023)
<u>Additional Controls:</u>			
State of Residence	No	Yes	Yes
CD of Residence*Age	No	Yes	Yes
State of Residence GDP & UER	No	Yes	Yes
Marital Status	No	Yes	Yes
Region of Birth*Region of Residence	No	No	Yes
Sample Size	1,351,799	1,351,799	1,351,799

The sample includes men aged 25-47 in 2000-2007 who are not in prison or school and who report positive income, usual hours of work, and weeks of work. All models are population weighted and clustered at the state of birth level. All models also include controls for kindergarten subsidization, pupil teacher ratio, relative salary of teachers, compulsory school leaving age, race, state of birth, age, and census division of birth specific cohort indicators. Heteroskedastic-consistent standard errors in parentheses. Bold coefficients are statistically significant at the 5% level and bold italics are statistically significant at the 10% level.

Table 6. The Impact of the Minimum School Starting Age on Ln Hourly Wages by Birth Quarter

	(1)	(2)	(3)
Panel A			
Age of youngest children (months)	**0.0058**	**0.0065**	**0.0066**
	(0.0028)	(0.0029)	(0.0029)
Panel B			
Age of youngest children (months)	**0.0101**	**0.0103**	**0.0103**
	(0.0040)	(0.0041)	(0.0041)
Youngest×(January-March)	-0.0012	-0.0009	-0.0008*
	(0.0058)	(0.0057)	(0.0056)
Youngest×(April-June)	0.0002*	-0.0012*	-0.0009*
	(0.0067)	(0.0070)	(0.0070)
Youngest×(July-September)	**-0.0155**	**-0.0130**	**-0.0128**
	(0.0044)	(0.0045)	(0.0045)
Additional Controls:			
State of Residence	No	Yes	Yes
CD of Residence*Age	No	Yes	Yes
State of Residence GDP & UER	No	Yes	Yes
Marital Status	No	Yes	Yes
Region of Birth*Region of Residence	No	No	Yes
Sample Size	551,476	551,476	551,476

The sample includes men aged 25-47 in 2005-2007 who are not in prison or school and who report positive income, usual hours of work, and weeks of work. All models are population weighted and clustered at the state of birth level. All models also include controls for kindergarten subsidization, pupil teacher ratio, relative salary of teachers, compulsory school leaving age, race, survey year, birth quarter, state of birth specific quarter of birth, and census division of birth specific cohort indicators. Heteroskedastic-consistent standard errors in parentheses. Bold coefficients are statistically significant at the 5% level and bold italics are statistically significant at the 10% level. Sample restricted to 2005-2007 ACS. A star in Panel B indicates that youngest plus the specified interaction effect (i.e. $\delta_1+\delta_2$, $\delta_1+\delta_3$, or $\delta_1+\delta_4$) is non-zero at the 90% level.

Appendix Table 1. School Entry Cutoff Dates (School Years 1964-1986)

	1964	1965	1966	1967	1968	1969	1970	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984	1985	1986
AL	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1
AK	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2	11.2
AZ	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	12.1	11.1	10.1	9.1	9.1	9.1	9.1	9.1
AR	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1
CA	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1
CO	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA
CT	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1
DE	9.1	9.1	9.1	9.1	9.1	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31
FL	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	12.1	11.1	10.1	9.1	9.1	9.1	9.1
GA	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	9.1	9.1
HI	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31
ID	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16
IL	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	11.1
IN	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none	none
IA	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15
KS	ssy	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
KY	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1
LA	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31
ME	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15
MD	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31
MA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA
MI	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1
MN	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
MS	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	12.1	11.1	10.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
MO	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	9.1
MT	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	9.10	9.10	9.10	9.10	9.10	9.10	9.10	9.10
NE	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15	10.15
NV	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	11.30	10.31	10.31	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30
NH	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30
NJ	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA
NM	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
NY	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1
NC	10.1	10.1	10.1	10.1	10.1	10.1	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16	10.16
ND	10.31	10.31	10.31	10.31	10.31	10.31	10.31	10.31	10.31	10.31	10.31	9.30	8.31	8.31	8.31	8.31	8.31	8.31	8.31	8.31	8.31	8.31	8.31
OH	none	10.31	10.31	10.31	10.31	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30
OK	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
OR	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	11.15	9.1	9.1	9.1	9.1
PA	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1	2.1
RI	none	none	none	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31	12.31
SC	none	none	none	none	none	none	none	none	none	none	none	none	none	none	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1
SD	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
TN	12.31	12.31	11.30	10.31	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30
TX	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy
UT	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy
VT	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1
VA	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	9.30	10.31	11.30	12.31	12.31	12.31	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA
WA	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA
WV	none	none	none	none	none	none	none	none	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	11.1	9.1	9.1	9.1	9.1
WI	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
WY	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15	9.15

Shading indicates a change in the school entry cutoff date. The number before the decimal is the month of cutoff, the number after the decimal is the day of the cutoff. LEA indicates that the local education authority sets the cutoff date, ssy indicates that the cutoff date is the start of school year, none indicates that the cutoff date is not listed in the state statutes. Al cutoff dates were collected from state statutes and corresponding historical state session laws. See Appendix Table 2 and 3 for more details.

Appendix Table 2. State Statutes Regarding School Entry

State	Statute
AL	ST § 16-28-4
AK	ST § 14.03.080
AZ	ST § 15-821
AR	ST § 6-18-207
CA	EDUC § 48000
CO	ST § 22-32-119
CT	ST § 10-15c
DE	ST TI 14 § 2702
FL	ST § 232.01; ST § 1003.21; ST § 232.04
GA	ST § 20-2-150
HI	ST § 302A-411
ID	ST § 33-201
IL	ST SCH 105 § 5/10-20.12
IN	ST § 20-8.1-3-17
IA	ST § 282.3
KS	ST § 72-1107(c)
KY	ST § 158.030
LA	R.S. 17:151.3 and 17:222
ME	ST T. 20-A § 5201
MD	EDUC § 7-101 & § 7-301 & COMAR 13A.08.01.02
MA	ST § 76-1
MI	ST 380.1147
MN	ST § 120A.20
MS	ST § 37-15-91
MO	ST 160.051
MT	ST 20-7-117
NE	ST § 79-214
NV	ST 392.040
NH	ST § 193:1
NJ	ST 18A:44-2
NM	ST § 22-13-3
NY	EDUC § 1712 § 3202
NC	ST § 115C-364)
ND	ST 15.1-06-01
OH	ST § 3321.01
OK	ST T. 70 § 1-114
OR	ST § 336.092
PA	ST 24 PS. § 5-503, §13-1304
RI	ST § 16-2-27
SC	ST § 59-63-20
SD	ST § 13-28-2
TN	ST § 49-6-201
TX	EDUC § 29.151
UT	ST § 53A-3-402
VT	ST T. 16 § 1073
VA	ST § 22.1-199 & 22-218.1
WA	ADC 180-39-010
WV	ST § 18-5-18
WI	ST 118.14
WY	ST § 21-4-302

Cutoff date changes in Appendix 1 were compiled from the state statutes listed above along with information collected from corresponding historical state session laws relating to the statute.

Appendix Table 3. State Statutes Regarding School Entry

	1964		Cutoff Date Change		
State	Cutoff Date	Citation	Year of Change	New Cutoff Date	Citation
AL	10.1	Acts 1950, 2nd Ex. Sess. No. 4, p. 24, §1	none		
AK	11.2	§ 1 ch. 98 SLA 1966	none		
AZ	1.1	Laws 1960, Ch. 127, §§17, 18	1978-1982	12.1, 11.1, 10.1, 9.1	Laws 1980, Ch 195, §1
AR	10.1	A.S.A. 1947, §80-1501.2	none		
CA	12.1	Stats. 1951, c. 362, p. 827 §1	none		
CO	LEA	Laws 1963, H.B. 17 §5, C.R.S. 1963, §123-20-5	none		
CT	1.1	CT statute: Title 10, Chapter 164, Sec. 10-15 1959	none		
DE	9.1	49 Del. Laws, c. 403, §§ 6,7	1969	12.31	1969 57 Del. Laws, c. 112
FL	1.1	Angrist & Krueger (1991) & Laws 1965, c. 65-239	1980-1983	12.1, 11.1, 10.1, 9.1	Laws 1979, c. 79-288 §§ 8, 11
GA	none	*Digest of Education Statistics* (1972). First year with cutoff is 1985, Code 1981, § 20-2-150.	1985	9.1	Code 1981, § 20-2-150, enacted by Ga. L. 1985, p. 1657, § 1
HI	12.31	am L 1961, c 39, section 1	none		
ID	10.16	1963 ch. 13 §24, p.27	none		
IL	12.1	Laws 1961, p. 31 § 10-20.12	1986	11.1	P.A. 84-126, Art IV, §2
IN	none	Angrist & Krueger (1991). First year with cutoff is 1989 (9.1 P.L.34-1991 Sec. 23)	none		
IA	10.15	Acts 1961 (59 G.A.) ch. 163, §§ 1, 2	1975	9.15	Acts 1974 (65 G.A.) ch. 1172, §77, effective July 1, 1975
KS	ssy	L. 1943, ch. 248 §39	1965	9.1	L. 1965, ch. 405 §1
KY	12.31	1952 c 145 §1	1979	10.1	1978 ch. 136, § 2, effective July 1, 1979
LA	12.31	Acts 1964, No. 109, § 2	none		
ME	10.15	Laws 1957, c. 364, §22	none		
MD	12.31	Bylaw 710 (Public School Laws 1967)	none		
MA	LEA	1950, 400	none		
MI	12.1	P.A. 1949, No. 315, § 1	none		
MN	9.1	Laws 1959, Ex. Sess., c. 71, art. 1, §6 & Laws 1967, c. 173, § 1	none		
MS	1.1	Laws 1953, 1st Ex. Sess, Ch. 24, §3	1977-1980	12.1, 11.1, 10.1, 9.1	Laws 1976, Cd. 390, § 1
MO	10.1	L. 1963, p. 200, §1-5	1986	9.1	L. 1984, H.B. Nos. 1456 & 1197, p. 439, § 1
MT	ssy	Mont. Rev. Code § 75-2004 (1947)	1979	9.10	amd. Sec. 3 Ch. 334, L. 1979
NE	10.15	Laws 1949, c. 258, § 1, p. 869 & Laws 1949, c. 256, § 83, p. 720	none		
NV	12.31	1956, p. 161; 1957, p. 304	1972-1973 & 1975	11.30, 10.31 & 9.30	1971, p. 170 & 1975, p. 49.
NH	9.30	RSA 193:1	none		
NJ	LEA	S: 18A:38-5 (1940) & L. 1967, c. 271 § 18A:44-2	none		
NM	1.1	Laws of NM, 1967, Ch. 16 § 181 & Angrist & Krueger (1991)	1973	9.1	L. 1973, Ch . 357, § 1
NY	12.1	Op. Counsel Educ. Dept. , 1952, 1 Educ. Dept. Rep. 775.	none		
NC	10.1	1955, c. 1372, art. 19, s.2	1970	10.16	1969, c. 1213, § 4
ND	10.31	S.L. 1959, ch. 172, § 1	1974-1975	9.30, 8.31	1973, ch. 158
OH	none	1943: 120 v 475	1965 & 1969	10.31 & 9.30	1965 vol. 131 pts 1 2 3 1965 & 1967-1968 vol. 132 pt. 1 1967
OK	11.1	Laws 1953, p. 374 §2	1980	9.1	Laws 1979, c. 204 § 1, eff. July 1, 1979
OR	11.15	1961 Oregon Revised Statutes & Laws 1965 ch. 100 Section 285	1983	9.1	Laws 1983, c. 193 § 1
PA	2.1	1965, Oct. 21, P.L. 601 § 32	none		
RI	none	P.L. 1966, ch 66, § 1	1967	12.31	P.L. 1966, ch 66, § 1
SC	none	1978 Act. No. 633 §1(1) & §4(3)	1978	11.1	1978 Act. No. 633 §1(1) & §4(3)
SD	11.1	SL 1955, ch 41, ch12, § 2	1979	9.1	SL 1979 ch 116 §4
TN	12.31	1957 Pub Acts, c. 9, §1	1966-1968	11.30, 10.31, 9.30	Acts 1965, ch. 303 §§ 1,2
TX	ssy	Acts 1961, 57th Leg., 1st C.S., p.132, ch. 29, sec. 1	none		
UT	ssy	U.C.A. 1953 §53A-3-402	none		
VT	1.1	1921, No. 51. G.L. §1243 & Amended 1971, No. 243 (Adj. Sess,), §1	none		
VA	9.30	1954 c. 638	1974-1976 & 1979	10.31, 11.30, 12.31 & LEA	1972 c. 245 & 1978 c. 518
WA	ssy	1909 c 97 p 261 § 1, part & 1969 ex.s. c 223 § 28A.58.190	1977	LEA	1977 ex.s. c 369 §14
WV	none	1959, c. 53	1971 & 1983	11.1 & 9.1	1971, c. 148 & 1983. c.61
WI	12.1	L. 1949, c. 151 & L. 1967, c. 92, § 17	1979	9.1	L. 1977, c. 429, §§1m, 2, 3
WY	9.15	Laws 1955, ch. 192, § 1	none		

Appendix Table 4. The Impact of the Minimum School Starting Age on Educational Attainment by Birth Quarter

	Specification (1)			Specification (2)			Specification (3)		
	HS +	Some Col +	BA +	HS +	Some Col +	BA +	HS +	Some Col +	BA +
Panel A									
Age of youngest children (months)	0.0002	-0.0002	-0.0007	0.0003	0.0002	0.0001	0.0004	0.0005	0.0004
	(0.0019)	(0.0027)	(0.0022)	(0.0019)	(0.0027)	(0.0021)	(0.0019)	(0.0026)	(0.0022)
Panel B									
Age of youngest children (months)	-0.0025	-0.0040	0.0025	-0.0023	-0.0037	0.0035	-0.0023	-0.0037	0.0035
	(0.0028)	(0.0039)	(0.0031)	(0.0028)	(0.0039)	(0.0031)	(0.0028)	(0.0040)	(0.0031)
Youngest×(January-March)	***0.0049***	0.0038	-0.0039	***0.0049***	0.0038	-0.0044	***0.0050***	0.0041	-0.0040
	(0.0025)	(0.0064)	(0.0043)	(0.0026)	(0.0065)	(0.0045)	(0.0026)	(0.0067)	(0.0046)
Youngest×(April-June)	***0.0042***	0.0053	-0.0046	***0.0037***	0.0041	-0.0059	0.0038	0.0048	-0.0052
	(0.0021)	(0.0047)	(0.0043)	(0.0020)	(0.0045)	(0.0039)	(0.0021)	(0.0045)	(0.0040)
Youngest×(July-September)	0.0018	0.0063	-0.0041	0.0020	0.0075	-0.0034	0.0021	***0.0077***	-0.0032
	(0.0025)	(0.0043)	(0.0028)	(0.0026)	(0.0044)	(0.0025)	(0.0026)	(0.0045)	(0.0026)
Additional Controls:									
State of Residence	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
CD of Residence*Age	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
State of Residence GDP & UER	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Marital Status	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Region of Birth*Region of Residence	No	No	No	No	No	No	Yes	Yes	Yes
Sample Size	685,212	685,212	685,212	685,212	685,212	685,212	685,212	685,212	685,212

The sample includes men aged 25-47 in 2005-2007. All models are population weighted and clustered at the state of birth level. All models also include controls for kindergarten subsidization, pupil teacher ratio, relative salary of teachers, compulsory school leaving age, race, survey year, birth quarter, state of birth specific quarter of birth, and census division of birth specific cohort indicators. Heteroskedastic-consistent standard errors in parentheses. Bold coefficients are statistically significant at the 5% level and bold italics are statistically significant at the 10% level. Sample restricted to 2005-2007 ACS. A star in Panel B indicates that youngest plus the specified interaction effect (i.e. δ1+δ2, δ1+δ3, or δ1+δ4) is non-zero at the 90% level.