# Is September Better than January?
# The Effect of Minimum School Entry Age Laws on Adult Earnings

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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. The evidence presented in this paper shows that these law changes increased human capital accumulation and hence adult wages. More specifically, shifting the school entry cutoff up by one month (i.e. from January 1 to December 1) increases average white male hourly earnings by approximately one percent. We further show that the increased human capital accumulation that generates this wage increase comes from within grade skill accumulation rather than increased educational attainment. Perhaps most importantly, there is some preliminary evidence that the entire cohort of children benefits from the cutoff change, not just those whose entry is delayed by a year.

## 1. Introduction

What is the optimal age to start formal schooling? The earlier children enroll in school, the sooner they begin accumulating the skills taught in school. However, enrolling a child in formal schooling before he or she is ready for the academic rigors of school may be less productive than waiting until that child is ready. In addition, the presence of children 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 redirect resources towards these children. Since most children enter school as soon as they are eligible, finding the optimal minimum age to start formal schooling amounts to finding the optimal minimum school entry age law. While, to the best of our knowledge, there has never been a rigorous empirical analysis of the effect of minimum school entry age laws, 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 (see Angrist and Krueger 1991). Since the mid 1960s, 26 states have increased their minimum school entry age. For example, by 1964, 18 states required children to turn five on or before October 1 and by 2005, 37 states made this a requirement (see Table 1).

Increasing the minimum entry age, by backing up the cutoff date, has three distinct effects.[1] First, it increases the average age of the entire cohort (we will refer to this as the *cohort* age effect). Second, it increases the absolute age of directly affected children who must wait an extra year before entering school (we will refer to this as the *absolute* age effect). Finally, 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 directly affected (we will refer to these as *relative* age effects).

In contrast to the absence of research examining the effect of minimum age at school entry on student outcomes, there has been a recent flurry of interest in the impact of relative age on academic performance. The usual approach is to use birth-date variation relative to state-level school entry laws to estimate the return to being relatively old in your cohort. Bedard and Dhuey (2006), Datar (2006), Elder and Lubotsky (2006), Fredriksson and Öckert (2004), and Puhani and Weber (2005) all find that relatively older students score higher than relatively

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[1] Most changes to the school entry cutoff laws have been to change them to be earlier in the year, which therefore increases the age of the directly affected children. However, there are five instances of the cutoff date has been changed to be later in the year (see appendix 1), which decreases the age of these children at school entry.

younger students during elementary and high school. Bedard and Dhuey (2006) further find that relatively older students are also more likely to attend university. However, except right at the beginning of formal schooling, relative and absolute ages are tied together in ways that are difficult to disentangle, i.e. relatively old students are also absolutely old (see Elder and Lubotsy, 2006). As such, the age effect estimates reported in the aforementioned papers cannot separate absolute and relative age from each other.

While it is clearly important that we understand the extent to which relatively older students within a cohort outperform their relatively younger counterparts, and ultimately the extent to which they acquire differential amounts of human capital, it is also important to examine the impact of minimum entry age. We use changes in state-level school entry laws over the past thirty years to estimate the impact of minimum entry age laws on long-run outcomes – adult wages. More specifically, we estimate the overall effect of changes in the minimum entry age for the entire school cohort, those directly and indirectly affected by cutoff changes. As we will discuss in section 2, this turns out to be a combination of the absolute age effect for directly affected individuals and the cohort effect for all individuals – the relative age effect washes out for the cohort as a whole.

To give you a flavor of the types of changes we are using, in 1972, 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. This entry law change therefore increased the average cohort age from approximately 61.5 months to 65.5 months. While only children born between September 2 and January 1 are directly affected by the policy change, children born during the remainder of the year may be 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 subsamples and should therefore be interpreted as the average effect of the policy shift.[2]

Using a state of birth level repeated cross-section for 1959-1980 white male birth cohorts from the 2000 Census and the 2000-2005 American Community Surveys combined with school entry laws from 1964-1985, we find that backing up the school entry cutoff by one month (i.e. from January 1 to December 1) increases white male hourly earnings by approximately 1

[2] In Section 5, we separately examine the effects for the directly and indirectly effected subsamples.

percent. Given an 'average' school entry change of 3 months[3], this translates into a 3 percent increase in the average hourly wage of young men.[4] This is a substantial increase and points to substantial return on increased average age at school entry.

Who is driving this result? Is it those who are directly affected by the law change – those who must wait an extra year before entering – or is there also an indirect effect on the majority of children who are not directly impacted? Unfortunately, our ability to answer this question is impeded by data limitations. However, the available data point to a substantial effect for both directly and indirectly affected groups (see Section 5). This is important since it indicates that the policy change not only affects the children who are forced to wait an additional year to enter school but that it additionally affects their classmates, who benefit from the increased average age of the cohort.

## 2. The Impact of Minimum School Entry Age Laws

Since the innovative work of Angrist and Krueger (1991),[5] who use quarter of birth as an instrument for educational attainment, many other researchers have used birth dates and school entry and exit laws in somewhat modified ways. Prominent U.S. 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).

However, the relative age effect literature discussed in the introduction[6] and recent papers by Dobkin and Ferreira (2006) and Mazumder (2007) draw into question the use of quarter of

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[3] The unweighted mean school entry change is 2.7 months.
[4] Our sample includes males ages 25-44 year olds.
[5] See Bound, Jaeger and Baker (1995), Bound and Jaeger (2000), and Dobkin and Ferreira (2006) for detailed discussions of the pros and cons of using quarter of birth as an instrument for educational attainment.
[6] Bedard and Dhuey (2006), Datar (2006), Elder and Lubotsky (2006), Fredriksson and Öckert (2004), and Puhani and Weber (2005).

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 problems. 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 2006).

None of this, however, changes the fact that school entry and/or exit laws may have an important impact on human capital accumulation and/or educational attainment. Rather, it suggests that there may be multiple interacting effects associated with such laws. This paper focuses on changes in school entry laws, although all specifications also control for school exit laws. 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 due under the September 1 cutoff compared to the January 1 cutoff, the other children in each school entry cohort are indirectly affected by the increase in average starting age and a change in their relative age.

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 (they are *absolutely* older), which may increase their level of school readiness (see Stipek (2002) for a review of the literature). While the backing up of cutoff dates in recent years in many states, and the rhetoric surrounding these 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. Since the relative age studies listed in footnote 6 find that relatively

[7] There is no evidence to suggest that the compulsory schooling change(s) used by Oreopolous (2005) and Clark and Royer (2007) are invalid.
[8] For descriptive ease, we assume that all children enter as soon as they are eligible, we will return to this issue.

older children have higher academic achievement compared to their younger counterparts, this aspect of the entry law change will have a positive impact on directly affected children. Third, on average the entire cohort is now older. To the extent that younger, not ready for school, classmates have a negative impact on the entire class, postponing their enrollment by a year will have a positive impact on the entire cohort. Since all of these effects are positive, with the possible exception of the absolute age effect, one would expect a positive net effect for the directly affected subgroup. It is worth point 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 effect group is less complicated, but of ambiguous direction. Since school entry age is unchanged for this group, the net effect only has two components. Just as for the directly affected group, the average age of the 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 on the relative age ladder 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 go in opposite directions, the net effect is ambiguous for the indirectly affected children.

While the net effect for certain subgroups within each school entry cohort are theoretically ambiguous, the mean net effect for the cohort as a whole is positive. The unambiguousness reflects the fact that the relative age effects wash out on average. While different students may be relatively older and younger, there is always a 12-month age range of relative age within each cohort. This only leaves the absolute age effect for directly affected students and the average cohort age effect for the entire cohort, both of which are positive, unless the absolute age effect for the directly effected group is strongly negative, which seems very unlikely. Again, however, the two effects are not separately identifiable since they move together.

Since age at school entry and the peer effects associated with cohort age composition might affect long run outcomes through educational attainment or human capital accumulation (holding educational attainment constant), the most natural way to think about estimating the impact of minimum school entry age laws on adult earnings is as follows:

$$W_{ibty} = \alpha_0 + \alpha_1 S_{bt} + X_{ibty}\alpha_2 + A_{ibty}\alpha_3 + B_b\alpha_4 + T_t\alpha_5 + \varepsilon_{ibty} \tag{1}$$

where $W_{ibty}$ denotes the ln adult wage, 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 kindergarten in birth state *b* in birth year *t*, *X* is a vector of personal characteristics and state of residence controls, $A_{ibty}$ is a vector of current age indicators, $B_b$ is a vector of state of birth indicators, $T_t$ is a vector of year of birth indicators, and $\varepsilon$ is the usual error term.[9] Notice that equation (1) does not hold educational attainment constant since school entry laws may change skill accumulation either through 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 and cohort level. Although we cannot include unrestricted age, year of birth, and observation year indicators (we have excluded the year indicators), we control for local labor market conditions by including state-specific unemployment rates.[10]

It is worth 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 children whose school entry is changed by the change in the minimum school starting age. It is important to note that $\alpha_1$ is 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 right fraction of 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, 2007). To the extent that these decisions are sensitive to cutoff dates, the reduced form estimate is net of this. More specifically, 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

[9] Section 5 checks the robustness of the results to different time trend controls. More specifically, we allow for region of birth specific time controls and state of birth specific linear time trends. In all cases, the results are similar.
[10] This data was obtained from the Bureau of Labor Statistics.

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 (see section 4.1).

Estimates from equation (1) may also be sensitive to the time-trend specification. As defined in equation (1), our most basic model includes a set of national-level birth cohort indicators. However, it seems reasonable to assume that underlying education trends and policies may be changing differentially across regions during the period. Therefore, we also report results for specifications that allow the birth cohort indicators to vary across census regions. Ultimately, one may be concerned that there are underlying state of birth specific trends. While we cannot include state of birth specific birth indicators, we can control for state-specific linear birth year trends. We estimate all wage models using all three birth-cohort control specifications.

## 3. Data

### 3.1 *School Entry Laws*

In most states, a statewide statute mandates the age at which children are eligible to enter school. For example, a child can enter school in California as long as the child turns five by December 2 of the relevant academic year. For descriptive ease, Table 1 reports the number of states by cutoff month in 1965, 1985, and 2005. 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 and 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 1965, 8 states required children to be five during September. In 1985, 17 states had this requirement, and by 2005, this had risen to 36. The complete set of entry laws from 1964-2005 are reported in Appendix Table 1.

All school entry cutoff dates were collected from historical information regarding state statutes and corresponding historical state session laws (see Appendix 2 for list of statute numbers). These cutoff dates have been cross-referenced with Angrist and Krueger (1992), Cascio and Lewis (2005), 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.[11] 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.[12] States that do not have statutes 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 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.[13]

Estimating equation (1) requires that we restrict attention to a subset of the years reported in Appendix Table 1 since calculating the age at which the youngest member of the cohort is eligible for kindergarten requires knowledge of the school entry cutoff date. As will be discussed in detail in the next section, the best available wage data come from the 2000 Census and the 2000-2005 American Community Surveys (ACS). Unless otherwise stated, the analyses will use state school entry cutoffs from 1964-1985. We assign cutoffs to individuals based on the law in place in the state of birth when they were 5 years old. This means that we have entry cutoff dates for the 1959-1980 birth cohorts. Table 2 reports the sample means.

### 3.2 *Wages*

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 five cutoff changes from 1964-1975. Since it is important to use the most current cohorts possible, we restrict the sample to U.S. born white men from the 1959-1980 birth cohorts in the 2000 U.S. 5 percent Public Use Micro Census and the 2000-2005 ACS. The ACS is a new nationally representative annual 1 in 250-person sample of the United States. This choice of sample allows the use of 16 statewide cutoff changes. Using the ACS has two important advantages. First, it

[11] If conflicting cutoff dates were found in different sources, the cutoff dates listed in state statutes and corresponding historical state session laws were used.
[12] This simplification has no substantive effect. The estimates using exact date are available upon request.
[13] 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.

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 1959-1980 birth cohorts is that wage observations are at younger than optimal ages for the later cohorts. The sample includes men aged 25-44 who reside in the 48 contiguous states.[14] 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 another quarter of the cutoff changes (for the 1972-1975 birth cohorts). Given these serious data limitations, we focus on young adult wages, age 25-44, and check the sensitivity of our results to various employment and education sample and specification definitions.

Table 2 summarizes the Census and ACS data. It reports the summary statistics for white U.S. born men who are not currently enrolled in school, report positive earnings, usually work at least 30 hours a week and worked 40 or more weeks in the survey period. Given the relative youth of some members of the sample, it helps to limit attention to men who have completed their education and are attached to the labor market.[15] The first two rows report ln hourly and ln weekly. While all analysis is carried out using both measures, we focus on the ln hourly wage specifications. The ln weekly specifications can be found in Appendix Table 4.

### 3.3 *Other Education Policy Controls*

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

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[14] Observations with imputed data or missing education information are excluded.

[15] All wage analysis was also done on two other samples: (1) men who are not currently enrolled in school, report positive earnings, usually work at least 10 hours a week and worked 20 or more weeks in the survey period, and (2) on the sample of men who are not currently enrolled in school and report positive earnings, usual hours of work and weeks of work. Results are similar for all three samples and are available upon request. The most notable difference is the increase in imprecision for the employed sample. This likely reflects the fact that we are unable to control for differential rates of labor market attachment among relatively young men when all workers are included, regardless of their hours and weeks of work, and measure wages at the weekly.

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. Each birth cohort was assigned the average pupil-teacher ratio during their thirteen years of available public schooling. The relative teacher salaries are defined as the average wage of teachers divided by the average wage of male 30-49 year old BA holders in the 1950-2000 Censuses (inter-census years are linearly interpolated). The number of students, the number and the wage of teachers, and the oldest age required 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 publicly subsidized kindergarten existed in a particular state for a particular birth cohort.[16]

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

While our ultimate goal is to examine the impact of minimum entry age laws on adult earnings, the existence of such effects depends on compliance with law changes and changes in within grade human capital accumulation or changes in educational attainment. Before turning to the wage estimates, we therefore examine these issues using available data.

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

Only seven states[17] and the District of Columbia require students to attend kindergarten, in all other states enrollment is only mandatory the following year. Despite not being mandatory in all states, most states have a legislated school entry cutoff date which dictates when a child is eligible to attend school. However, it is generally possible for parents and/or educators to advance or delay school entry for particular children, especially at the kindergarten level. In states with mandatory kindergarten, acceleration and deferral usually only requires petitioning the school or district for an exception.

[16] See Dhuey (2007) for information regarding collection of data on publicly subsidized kindergarten.
[17] These include Arkansas, Connecticut, Delaware, Maryland, New Mexico, Oklahoma, South Carolina, and Virginia.

While in recent years it is rare for children to enter school early,[18] it was more common in the past. For example, in 1980, 8 percent children born in the fourth quarter 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 from the same cohort were not enrolled in kindergarten, even though according to the cutoff rules they were all 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.

Examining the impact of cutoff law changes on on-time school entry rates is further complicated by the steep state-specific increases in kindergarten enrollment from 1950-1990. Ideally we would use the age five enrollment rate for a group of children who are completely unaffected by cutoff age changes to control for state-specific time trends. While it is impossible to identify a group of children that satisfy this requirement perfectly, we can get quite close by using the 1960-1980 Censuses because they report quarter of birth. Since all cutoff changes during this period fall between September 1 and January 1, only children born from September 1 – January 1 are directly affected. We therefore use children born in the first and second quarter (January – June) to flexibly control for state-specific time trends. We exclude children born in the third quarter since children born in September may be directly affected by cutoff changes in some states. This approach assumes that delayed or accelerated entry decisions for children are not indirectly altered by cutoff changes. While this is not a perfect assumption, it is clear from Table 3a that this is a much better detrending approach than simply using the national average or assuming within state linearity.

For descriptive ease, we begin by focusing on the states that change cutoffs between 1960 and 1980 (see Table 3). Pairs of years with cutoff changes are shaded. For example, in Arizona during 1970, all children born in the fourth quarter were eligible for kindergarten, but by 1980 the cutoff had moved to November 1, making two-thirds of the fourth quarter children ineligible. In an environment with 100 percent enrollment eligibility compliance and where all states offered state financed kindergarten throughout the sample period, we could simply compare changes in the percentage of ineligible children (columns 4-6) to the enrollment rate (columns 7-9) in Table 3a. However, this simple comparison is confounded in an environment with a state-

[18] Using data from the Early Childhood Longitudinal Study of kindergarten students in 1998-1999, only 1.8 percent of children entered kindergarten early.

specific upward trend in kindergarten enrollment. We therefore difference out the state-specific time trend using the enrollment rate of children born in the first and second quarter who are not directly affected by school cutoff changes during this period. Columns 13-14 report the across year difference-in-difference estimates that difference out within state time trends before comparing across cutoff changes. The shaded entries in columns 13-14 highlight the year pairs with cutoff changes (they match the shading in columns 1-3) and bold difference-in-differences are statistically significant at the 5 percent level.

Generally speaking, the difference-in-difference estimates reported in columns 13-14 are consistent with the fraction ineligible changes reported in columns 4-6. As the fraction of fourth quarter children who are ineligible for school entry rises, the enrollment rate falls. However, consistent with Dobkin and Ferreira (2006) we also find that compliance is imperfect. On average, a one month increase in the cutoff age (e.g. from January 1 to December 1) decreases the fourth quarter enrollment rate by 13 percent, compared to the 33 percent that we would observe under perfect compliance. The large discrepancy reflects the fact that children near cutoffs are more likely to be accelerated or retained.

Since enrollment in kindergarten is not mandatory in most states, we replicate Table 3a for six year olds enrolled in grade one. The results are reported in Table 3b. The drawback to this sample is that children who entered kindergarten on time but failed to advance to grade one appear as not enrolled. Nonetheless, the results are very similar to the kindergarten specification.

Table 4 reports a similar set of results in a regression framework. For the same reasons described above, the sample excludes children born in quarter 3.

$$E_{iry} = \beta_0 + \beta_1 G_{ry} + Y_{ry}\beta_2 + \beta_3 Q_{iry} + \nu_{iry} \qquad (2)$$

where $E_{iry}$ is the enrollment status (1 = enrolled in school) of child *i* in state of residence *r* in census year *y*, $G_{ry}$ denotes the fraction of children in individual *i*'s birth quarter who are ineligible for school entry (notice that this is always zero for children born in quarters 1 and 2), $Y_{ry}$ is a vector of state-specific year indicators, and $Q_{iry}$ is an indicator for being born in the fourth quarter. Under the assumption that in the absence of cutoff changes the enrollment trend of children born in the first half of the year provides a valid counterfactual for children born in the fourth quarter, this approach is differences-in-differences. For comparability with later results, the sample is restricted to white, fourth quarter, U.S. born children residing in the 48 contiguous states.

Consistent with the results reported in Table 3, a 100 percent increase in the percentage of fourth quarter children who are ineligible for kindergarten according to the cutoff , a switch from January 1 to October 1 or earlier, decreases the kindergarten enrollment rate for children born in the fourth quarter by 38 percentage points and the first grade enrollment rate by 57 percentage points. The larger estimate for first grade likely reflects the fact that first grade enrollment is mandatory while kindergarten enrollment is not.

### 4.2 *Test Scores*

Even if school entry cutoffs do not affect educational attainment, which we explore in the next section, they can still influence human capital accumulation through within grade skill accumulation. We examine skill accumulation at the fourth and eight grade levels using publicly available state aggregate mathematics and reading scores from the 1990-2005 National Assessment of Educational Progress (NAEP). Fourth graders were tested in math in 1992, 2000, 2003 and 2005, and reading in 1992, 1994, 1998, 2002, 2003, and 2005. Eighth graders were tested in math in 1990, 1992, 2000, 2003 and 2005, and reading in 1998, 2002, 2003, and 2005. Appendix Table 3 lists the participation of states in each subject, grade, and test year.

Similar to the wage equation described by equation (1), Table 5 reports the results for the net impact of a 1-month increase in the minimum starting age using a standard state panel model. As we do not have access to micro-level data, we cannot separate students by birth dates. However, even if we had the data to do this, we would not use children born in the first and second quarter to control for time trends because it is entirely possible that their test scores are impacted by cutoff changes that change the composition of their cohort. In other words, the impact on these groups is part of the effect that we are trying to estimate. Table 5 displays the results from the following regression:

$$M_{ry} = \theta_0 + \theta_1 S_{ry} + R_r\theta_2 + Y_y\theta_3 + \upsilon_{ry} \tag{3}$$

where $M_{ry}$ denotes the average math or reading score for state *r* in year *y*, $S_{ry}$ denotes the youngest age at which children in state *r* in year *y* could have entered school, $R_r$ is a vector of state indicators, and $Y_y$ is a vector of year indicators. Notice that *S* is the age in months that the youngest child in the cohort is eligible to enter kindergarten and not the fraction of children born in the fourth quarter who are ineligible to enter school. We switch to this specification because we are now examining the impact of the cutoff on all children rather than only for children born in the fourth quarter.

Table 5 reports the results. Panel A reports the reading results and Panel B reports the math results. Columns 1-6 report the results for grade four and columns 7-12 report the results for grade eight. We begin with grade four. The sample used in columns 1-3 is restricted to states with school entry cutoff changes during the sample period. Column 1 is unweighted, column 2 weights by the inverse sampling variance reported by NAEP, and column 3 is weighted by the number of children enrolled in fourth grade in each state. The estimates reported in columns 1 and 2 are similar for reading: a one month increase in the minimum school entry age increases the average test score by approximately 3 points on a test with a mean of 100 and a standard deviation of 10. This translates into a 9 point increase for a three month increase in the cutoff date (e.g. changing from January 1 to October 1), or slightly smaller than a one standard deviation increase. When fourth grade enrollment is used to weight the regression, the point estimate decreases to 1.6, which translates into a 4.8 point or approximately half a standard deviation increase for a three month change in the cutoff date. Columns 4-6 repeat columns 1-3 including all states that participated in the tests, regardless of whether or not they changed their cutoff during the sample period. The inclusion of non-changing states reduces the point estimates under the first two weighting schemes and renders the point estimate using fourth grade enrollment as the weight indistinguishable from zero. The imprecise zero reported in column 6 likely reflects the fact that no large state changed its cutoff during the sample period. The fourth grade math results are reported in columns 1-6 in Panel B. In all cases, these estimates are of the expected sign and magnitude but are quite noisy. This likely reflects the small number of years in which the math test was given – four years for math compared to six years for reading.

Columns 7-12 report the same set of results for grade eight. The sample size problem is reversed for grade eight; there are five test years for math but only four for reading. The precision of the estimates again reflects the small sample sizes. In this case, there is an even greater discrepancy between math and reading sample sizes with 18 cutoff changes for the math sample and only 9 for the reading sample. Focusing on the more precisely estimated mathematics results, when the model is weighted by the NAEP sampling variance and the sample is restricted to states with law changes during the sample period, a three-month increase in the minimum entry age results in a 3.3 point increase in the average test score, which translates into approximately a third of a standard deviation.

Overall, the fourth grade reading score effects and the eighth grade math score effects clearly suggest that later school starting dates lead to higher average scores on NAEP tests. Further, the effects appear to be quite large, with a three-month cutoff increase increasing average test scores by one standard deviation at the fourth grade level and half a standard deviation at the eighth grade level. While it is tempting to interpret these results as showing the impact of the cutoff age is diminishing across grades, it is important to remember that the subject is different and the set of states with cutoff changes is also different.

### 4.3 *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 their direct impact on skill accumulation as measured by test scores 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.

$$Ed_{ibty} = \pi_0 + \pi_1 S_{bt} + X_{ibty}\pi_2 + A_{ibty}\pi_3 + B_b\alpha_4 + T_t\pi_5 + \omega_{ibty} \tag{4}$$

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 kindergarten in birth state *b* in year birth year *t*, *X* is a vector of personal characteristics and state of residence controls, $A_{ibty}$ is a vector of current age indicators, $B_b$ is a vector of state of birth indicators, and $T_t$ is a vector of year of birth indicators.

Table 6 reports the results for equation (4) using the sample of all white men aged 25-44 in the 2000 Census and the 2000-2005 ACSs. In contrast to the wage sample discussed in section 3.2, the education sample is not restricted to individuals who are out of school and/or employed. Column 1 reports the estimates for the baseline model that includes a set of national year of birth indicators, column 2 generalizes the specification to include region of birth specific year of birth indicators, and column 3 includes state of birth specific linear year of birth trends. The first row reports the estimated impact of a one-month backing up of the school start date on the probability of graduating from high school. Rows 2 and 3 similarly report the probability of having some college or more and an undergraduate degree or more. Regardless of the education level or trend specification, there is no evidence of an impact of school start dates on educational attainment. This leads to the conclusion that if there is an impact of school start dates on earnings, it must come through within grade skill accumulation rather than increased educational attainment.

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

The baseline equation (1) wage estimates are reported in column 1 in Table 7. Column 2 reports the same results allowing for region of birth specific cohort controls, and column 3 reports the results using state of birth specific linear cohort trends. Depending on the trend specification, we estimate that a one-month increase in the minimum school starting age increases average hourly wages by 0.9-1.0 percent. Using the mean cutoff change of 3 months, this translates into approximately a 3.0 percent increase in average hourly wages. To put this in context this is approximately about one-quarter of the return to an additional year of education.[19]

Equation (1) explicitly excludes education controls because school entry laws may change skill accumulation through either within grade human capital accumulation or educational attainment. That not withstanding, it may be of interest to know whether the school entry coefficient changes if education controls are included. The estimates are reported in row 2. They are similar to the base specification in all cases.

While we find no evidence that later minimum school entry ages increase or decrease educational attainment, it is possible that school cutoff changes differentially benefit individuals from different parts of the education distribution. Rows 3-6 explore this by allowing the cutoff coefficient to differ across education designations. For descriptive ease, we focus on the sample with state of birth specific linear cohort trends (column 3). The point estimates are as follows. A three-month increase in the minimum school entry age increases the average hourly wage of full time high school drop, high school graduate, some college, and BA+ workers by 5.4, 4.9, 4.0, and 0.5 percent, respectively. With the first three estimates being statistically different from

[19] Compared to the OLS linear education coefficient.

zero, but not statistically different from each other, and the BA+ coefficient being statistically different from the other three point estimates, but not distinguishable from zero. In other words, backing up the school entry cutoff date by three months increases average male hourly earnings by 4.0-5.4 percent for workers with less than an undergraduate degree, but has no discernable benefit for highly educated workers.

Columns 4-6 in replicate columns 1-3 excluding men with undergraduate degrees. This is done because this is the group that is most likely to be in school or to go back to school after age twenty-five. As such, they are the most likely to have a selected sample and the most likely to have incomplete educational attainment given the relative youth of our sample. In all cases, the results are very similar. We also replicate table 7 for weekly earning. These results are reported in Appendix Table 4. Again, the results are similar.

While the results reported in Table 7 clearly point to a substantial return to later school entry dates, they also raise the question of exactly who benefits from the 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 absence of data regarding birth date. However, beginning in 2005 the ACS reports quarter of birth. Therefore, the 2005 ACS data can be used to at least crudely examine the impact of cutoff date changes on specific segments of class cohorts.

The aggregation of birthdays to the quarter of birth level substantially complicates the analysis of this question. This is due to the fact that birth months directly affected by cutoff law changes during this period fall from September 1-December 31. The problem, of course, is that some fourth quarter children will only be indirectly affected by some cutoff changes, a change from January 1 to December 1 for example. In addition, some children born in the third quarter will conversely be directly affected by some law changes (i.e. changes that move the cutoff into September). As such, it is impossible to cleanly estimate the minimum age entry effect for directly affected children. However, as first and second quarters are always indirectly affected by the cutoff changes during this period,[20] we can obtain a lower bound estimate of the cohort age effect using these sub-groups. It is a lower bound because we can not separate the positive cohort effect and the negative relative age effect.

[20] In reality, there is one change from that effects of children born in the first quarter in Florida. Their February 1 cutoff changes to September 1 in 1985. All results are similar if we exclude the youngest Florida cohort.

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

$$W_{ibty} = \delta_0 + \delta_1 S_{bt} + \delta_2 SQ1_{bt} + \delta_3 SQ2_{bt} + \delta_4 SQ3_{bt} + X_{ibty}\delta_5 + BQ_b\delta_6 + TQ_t\delta_7 + \varepsilon_{ibty} \qquad (5)$$

where $SQ1_{bt}$, $SQ2_{bt}$, $SQ3_{bt}$ denotes 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 kindergarten in birth state, $BQ_b$ is a vector of state of birth by birth quarter indicators and $TQ_t$ is a vector of year of birth by birth quarter indicators.

Table 8 reports the impact of the minimum school starting age on ln hourly wages by birth quarter using the 2005 ACS.[21] For comparative purposes, Panel A (labeled pooled) reports $\hat{\alpha}_1$ for equation (1) using only the 2005 ACS. The next four lines (Panel B) report the quarter of birth specific effect of backing up the cutoff by one month; the last three rows in this panel add the level and interaction effects (i.e. $\delta_1 + \delta_2$, $\delta_1 + \delta_3$, and $\delta_1 + \delta_4$) and their appropriate standard errors. The estimates for birth quarter one, two, and four, are significantly different from zero but not significantly different from each other. While one might expect the fourth quarter estimates to be bigger than the first and second quarter estimates, it is important to remember that there are both directly and indirectly affected individuals in this group. With the indirectly affect within quarter four generally becoming some of the relatively youngest within the cohort. Birth quarter three is not significantly different from zero. The lack of precision my reflect noise due to data constraints inherent in quarter aggregation or a very large negative relative age effect for members of this quarter. While our ability to examine directly and indirectly affected individuals and separate cohort, absolute and relative age effects is limited, Table 8 still delivers an important finding: Backing up the cutoff date has a positive affect for directly affected individuals and at least a majority of indirectly affected individuals.

## 6. Conclusion

This paper documents the large and statistically significant positive earning effect associated with backing up school cutoff dates. We find that increasing the minimum school entry age increases both test scores and wages, but has no affect on educational attainment. This implies that increases in within grade human capital acquisition are responsible the for the estimated

[21] Estimates for ln weekly wages are in Appendix Table 5.

wage return.  In particular, a one month increase in the minimum school entry age increases wages by about 1 percent.  In addition, we report preliminary evidence that suggests that minimum age entry law changes have a positive impact on a majority of the cohort, not just children directly affected by the policy change.

While backing up cutoff dates is not entirely costless – directly affected individuals enter the labor market a year later – it likely uses fewer public funds than many other interventions (i.e. class size reductions).  This policy is also likely popular in an era of nation 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 to backing it up even farther.  And within this context, what the tradeoffs are between earlier skill accumulation and improved school readiness.

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Table 1. Cutoff Date Distribution

| | Number of States | | |
|---|---|---|---|
| Cutoff Month | 1964 | 1985 | 2005 |
| January / February | 13 | 8 | 2 |
| December | 5 | 4 | 2 |
| November | 5 | 3 | 0 |
| October | 10 | 13 | 5 |
| September / Start of school year (SSY) | 8 | 17 | 32 |
| July / August | 0 | 0 | 2 |
| Local education authority (LEA) | 3 | 4 | 7 |
| None | 6 | 1 | 0 |

Table 2. Census and ACS Summary Statistics

| | Full Time<br>(1) | All Men<br>(2) |
|---|---|---|
| Wages | | |
| Ln hourly wage | 2.96 | 2.93 |
| | (0.64) | (0.69) |
| Ln weekly wage | 6.77 | 6.71 |
| | (0.66) | (0.74) |
| Education Outcomes | | |
| High school dropout | 0.07 | 0.09 |
| | (0.26) | (0.29) |
| High school graduate | 0.32 | 0.32 |
| | (0.47) | (0.47) |
| Some college | 0.25 | 0.25 |
| | (0.43) | (0.43) |
| BA+ | 0.36 | 0.34 |
| | (0.48) | (0.47) |
| School Start Date | | |
| Age of youngest children (months) | 57.59 | 57.60 |
| | (1.54) | (1.54) |
| Other State Education Policies | | |
| Kindergarten | 0.84 | 0.84 |
| | (0.37) | (0.37) |
| Pupil-teacher ratio | 19.48 | 19.48 |
| | (2.27) | (2.30) |
| Relative teacher salaries | 0.64 | 0.65 |
| | (0.07) | (0.07) |
| School leaving age | 16.42 | 16.43 |
| | (0.75) | (0.76) |
| Other Variables | | |
| State of residence unemployment rate | 5.05 | 5.07 |
| | (1.09) | (1.09) |
| Married | 0.67 | 0.62 |
| | (0.47) | (0.49) |
| Sample Size | 782,516 | 1,084,264 |

Summary statistics are population weighted. Standard deviations in parentheses. Sample size for ln hourly and ln weekly wages is 957,716 in column (2).

Table 3a. Effect of Changes in Cutoff Date on Fraction of 5 year olds Enrolled in Kindergarten

| | Cutoff | | | Fraction Ineligible Birth Quarter 4 | | | Fraction Enrolled Birth Quarter 4 | | | Fraction Enrolled Birth Quarters 1 & 2 | | | Diff-in-Diff | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1960 (1) | 1970 (2) | 1980 (3) | 1960 (4) | 1970 (5) | 1980 (6) | 1960 (7) | 1970 (8) | 1980 (9) | 1960 (10) | 1970 (11) | 1980 (12) | 70-60 (13) | 80-70 (14) |
| AZ | Jan 1 | Jan 1 | Nov 1 | 0.00 | 0.00 | 0.33 | 0.27 | 0.50 | 0.47 | 0.34 | 0.56 | 0.89 | 0.00 | **-0.36** |
| | | | | | | | [67] | [148] | [354] | [136] | [262] | [709] | (0.09) | (0.05) |
| DE | Sept 1 | Dec 31 | Dec 31 | 1.00 | 0.00 | 0.00 | 0.24 | 0.78 | 0.82 | 0.29 | 0.79 | 0.88 | 0.04 | -0.05 |
| | | | | | | | [17] | [18] | [60] | [45] | [38] | [144] | (0.18) | (0.12) |
| IA | Nov 15 | Oct 15 | Sept 15 | 0.50 | 0.83 | 1.00 | 0.35 | 0.14 | 0.06 | 0.88 | 0.83 | 0.93 | **-0.16** | **-0.18** |
| | | | | | | | [168] | [189] | [512] | [278] | [361] | [937] | (0.05) | (0.03) |
| KY | Dec 30 | Dec 31 | Oct 1 | 0.00 | 0.00 | 1.00 | 0.04 | 0.20 | 0.30 | 0.06 | 0.24 | 0.78 | -0.03 | **-0.44** |
| | | | | | | | [138] | [182] | [631] | [303] | [314] | [1181] | (0.08) | (0.04) |
| MS | Jan 1 | Jan 1 | Sept 1 | 0.00 | 0.00 | 1.00 | 0.17 | 0.40 | 0.31 | 0.17 | 0.33 | 0.63 | 0.08 | **-0.40** |
| | | | | | | | [72] | [87] | [314] | [139] | [161] | [551] | (0.09) | (0.07) |
| NV | Dec 31 | Dec 31 | Sept 30 | 0.00 | 0.00 | 1.00 | 0.73 | 0.53 | 0.14 | 0.84 | 0.79 | 0.88 | -0.16 | **-0.48** |
| | | | | | | | [15] | [38] | [110] | [31] | [53] | [211] | (0.17) | (0.09) |
| NM | Jan 1 | Jan 1 | Sept 1 | 0.00 | 0.00 | 1.00 | 0.22 | 0.33 | 0.14 | 0.12 | 0.37 | 0.84 | -0.13 | **-0.66** |
| | | | | | | | [58] | [57] | [208] | [113] | [169] | [327] | (0.10) | (0.07) |
| NC | Oct 1 | Oct 16 | Oct 16 | 1.00 | 0.83 | 0.83 | 0.05 | 0.16 | 0.31 | 0.11 | 0.36 | 0.90 | **-0.15** | **-0.39** |
| | | | | | | | [164] | [298] | [708] | [342] | [569] | [1318] | (0.05) | (0.03) |
| ND | Oct 31 | Oct 31 | Oct 1 | 0.67 | 0.67 | 1.00 | 0.08 | 0.15 | 0.09 | 0.25 | 0.55 | 0.77 | -0.24 | **-0.29** |
| | | | | | | | [48] | [20] | [129] | [69] | [49] | [193] | (0.13) | (0.11) |
| SD | Nov 1 | Nov 1 | Sept 1 | 0.67 | 0.67 | 1.00 | 0.19 | 0.38 | 0.13 | 0.63 | 0.84 | 0.86 | -0.02 | **-0.28** |
| | | | | | | | [43] | [29] | [108] | [72] | [68] | [206] | (0.13) | (0.09) |
| TN | Dec 31 | Oct 31 | Oct 31 | 0.00 | 0.67 | 0.67 | 0.09 | 0.22 | 0.38 | 0.13 | 0.35 | 0.87 | -0.09 | **-0.35** |
| | | | | | | | [154] | [217] | [674] | [324] | [495] | [1254] | (0.05) | (0.04) |
| WI | Dec 1 | Dec 1 | Sept 1 | 0.33 | 0.33 | 1.00 | 0.39 | 0.30 | 0.30 | 0.61 | 0.86 | 0.92 | **-0.34** | **-0.07** |
| | | | | | | | [242] | [370] | [790] | [419] | [704] | [1468] | (0.04) | (0.03) |

Table 3b. Effect of Changes in Cutoff Date on Fraction of 6 year olds Enrolled in First Grade

| | Cutoff | | | Fraction Ineligible Birth Quarter 4 | | | Fraction Enrolled Birth Quarter 4 | | | Fraction Enrolled Birth Quarters 1 & 2 | | | Diff-in-Diff | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1960 (1) | 1970 (2) | 1980 (3) | 1960 (4) | 1970 (5) | 1980 (6) | 1960 (7) | 1970 (8) | 1980 (9) | 1960 (10) | 1970 (11) | 1980 (12) | 70-60 (13) | 80-70 (14) |
| AZ | Jan 1 | Jan 1 | Nov 1 | 0.00 | 0.00 | 0.33 | 0.80 | 0.78 | 0.58 | 0.88 | 0.84 | 0.87 | 0.01 | **-0.23** |
| | | | | | | | [61] | [178] | [342] | [114] | [271] | [686] | (0.07) | (0.05) |
| DE | Sept 1 | Dec 31 | Dec 31 | 1.00 | 0.00 | 0.00 | 0.79 | 0.83 | 0.74 | 0.91 | 0.82 | 0.90 | 0.13 | -0.16 |
| | | | | | | | [19] | [29] | [82] | [35] | [34] | [128] | (0.14) | (0.11) |
| IA | Nov 15 | Oct 15 | Sept 15 | 0.50 | 0.83 | 1.00 | 0.40 | 0.07 | 0.07 | 0.87 | 0.86 | 0.89 | **-0.31** | -0.03 |
| | | | | | | | [146] | [201] | [468] | [253] | [432] | [897] | (0.05) | (0.03) |
| KY | Dec 30 | Dec 31 | Oct 1 | 0.00 | 0.00 | 1.00 | 0.76 | 0.75 | 0.55 | 0.84 | 0.80 | 0.90 | 0.02 | **-0.30** |
| | | | | | | | [155] | [197] | [604] | [279] | [322] | [1220] | (0.05) | (0.04) |
| MS | Jan 1 | Jan 1 | Sept 1 | 0.00 | 0.00 | 1.00 | 0.87 | 0.81 | 0.16 | 0.91 | 0.84 | 0.92 | 0.01 | **-0.72** |
| | | | | | | | [68] | [88] | [292] | [117] | [156] | [552] | (0.07) | (0.05) |
| NV | Dec 31 | Dec 31 | Sept 30 | 0.00 | 0.00 | 1.00 | 1.00 | 0.69 | 0.18 | 0.95 | 0.84 | 0.87 | -0.20 | **-0.54** |
| | | | | | | | [9] | [29] | [122] | [20] | [50] | [203] | (0.17) | (0.10) |
| NM | Jan 1 | Jan 1 | Sept 1 | 0.00 | 0.00 | 1.00 | 0.84 | 0.77 | 0.19 | 0.88 | 0.84 | 0.88 | -0.03 | **-0.62** |
| | | | | | | | [69] | [57] | [153] | [107] | [163] | [339] | (0.08) | (0.07) |
| NC | Oct 1 | Oct 16 | Oct 16 | 1.00 | 0.83 | 0.83 | 0.31 | 0.26 | 0.24 | 0.88 | 0.87 | 0.90 | -0.04 | -0.05 |
| | | | | | | | [150] | [302] | [764] | [315] | [623] | [1407] | (0.05) | (0.03) |
| ND | Oct 31 | Oct 31 | Oct 1 | 0.67 | 0.67 | 1.00 | 0.72 | 0.44 | 0.11 | 0.93 | 0.83 | 0.89 | -0.18 | **-0.38** |
| | | | | | | | [32] | [34] | [118] | [61] | [60] | [212] | (0.12) | (0.08) |
| SD | Nov 1 | Nov 1 | Sept 1 | 0.67 | 0.67 | 1.00 | 0.45 | 0.37 | 0.15 | 0.94 | 0.90 | 0.85 | -0.05 | -0.16 |
| | | | | | | | [42] | [30] | [120] | [82] | [61] | [199] | (0.11) | (0.09) |
| TN | Dec 31 | Oct 31 | Oct 31 | 0.00 | 0.67 | 0.67 | 0.75 | 0.34 | 0.35 | 0.82 | 0.81 | 0.89 | **-0.42** | -0.07 |
| | | | | | | | [166] | [208] | [613] | [282] | [421] | [1259] | (0.05) | (0.04) |
| WI | Dec 1 | Dec 1 | Sept 1 | 0.33 | 0.33 | 1.00 | 0.50 | 0.30 | 0.35 | 0.88 | 0.86 | 0.89 | -0.19 | 0.03 |
| | | | | | | | [205] | [365] | [712] | [382] | [713] | [1418] | (0.04) | (0.03) |

Table 4. The Effect of Changes in Cutoff Date on Enrollment

| | Age 5 - Kindergarten | Age 6 - 1st Grade |
|---|---|---|
| | (1) | (2) |
| Fraction ineligible | **-0.384** | **-0.570** |
| | (0.058) | (0.034) |
| Quarter 4 | **-0.084** | **-0.084** |
| | (0.026) | (0.026) |
| Sample size | 128,175 | 127,690 |

Heteroskedastic-consistent standard errors in parentheses. All models are population weighted and clustered at the state-year level. Bold coefficents are significant at the 5 percent level. All models also include state-specific year indicators.

Table 5. The Impact of School Start Dates on NAEP Scores

| | Grade 4 | | | | | | Grade 8 | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Panel A | | | | | | | | | | | | |
| Reading score | **2.966** | **2.717** | **1.606** | **1.827** | **1.629** | -0.010 | 1.194 | 1.020 | 0.055 | 1.536 | 1.325 | 0.433 |
| | (0.821) | (0.829) | (0.779) | (0.662) | (0.664) | (0.636) | (1.228) | (1.102) | (0.715) | (1.162) | (0.998) | (0.619) |
| R-Squared | 0.85 | 0.84 | 0.87 | 0.89 | 0.90 | 0.92 | 0.84 | 0.83 | 0.91 | 0.93 | 0.93 | 0.92 |
| Sample Size | 43 | 43 | 43 | 233 | 233 | 233 | 24 | 24 | 24 | 158 | 158 | 158 |
| Panel B | | | | | | | | | | | | |
| Math score | 0.853 | 0.670 | 0.965 | 0.795 | 0.722 | 0.305 | **1.195** | **1.091** | **0.916** | **0.655** | **0.601** | 0.312 |
| | (0.757) | (0.858) | (0.923) | (0.537) | (0.600) | (0.701) | (0.376) | (0.385) | (0.302) | (0.311) | (0.302) | (0.248) |
| R-Squared | 0.94 | 0.93 | 0.93 | 0.95 | 0.95 | 0.96 | 0.92 | 0.94 | 0.95 | 0.93 | 0.93 | 0.92 |
| Sample Size | 27 | 27 | 27 | 162 | 162 | 162 | 66 | 66 | 66 | 192 | 192 | 192 |
| Restricted to states with law changes | Yes | Yes | Yes | No | No | No | Yes | Yes | Yes | No | No | No |
| No weights | Yes | No | No | Yes | No | No | Yes | No | No | Yes | No | No |
| Weighted by NAEP provided variance | No | Yes | No | No | Yes | No | No | Yes | No | No | Yes | No |
| Population weighted | No | No | Yes | No | No | Yes | No | No | Yes | No | No | Yes |

Heteroskedastic-consistent standard errors in parentheses. Bold coefficents are significant at the 5 percent level. All models include state and year fixed effects.

Table 6. School Start Dates and Educational Attainment

| | (1) | (2) | (3) |
|---|---|---|---|
| High school graduate or higher | 0.0016 | 0.0019 | -0.0007 |
| | (0.0015) | (0.0014) | (0.0019) |
| Some college or higher | -0.0011 | 0.0014 | -0.0018 |
| | (0.0019) | (0.0020) | (0.0031) |
| BA or higher | -0.0029 | -0.0007 | -0.0021 |
| | (0.0018) | (0.0018) | (0.0028) |
| Sample size | 1,084,264 | 1,084,264 | 1,084,264 |
| Birth Cohort Controls* | NBC | RBC | SLBC |

* NBC stands for national birth cohort controls, RBC stands for regional birth cohort controls, and SLBC stands for linear state of birth controls. All models are population weighted and clustered at the state-year level. All models also include controls for kindergarten subsidization, pupil teacher ratio, relative salary of teachers, compulsory school leaving age, census year level unemployment rates, marital status, birth state, and state of residence.

Table 7. The Impact of the Minimum School Starting Age on ln Hourly Wages

| | Full Sample | | | Excluding BA+ | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| No education controls | **0.0099** | **0.0099** | **0.0091** | **0.0135** | **0.0134** | **0.0108** |
| | (0.0028) | (0.0029) | (0.0041) | (0.0029) | (0.0030) | (0.0043) |
| With education controls | **0.0098** | **0.0089** | **0.0113** | **0.0128** | **0.0125** | **0.0106** |
| | (0.0027) | (0.0027) | (0.0037) | (0.0030) | (0.0030) | (0.0042) |
| By education group | | | | | | |
| High school dropouts | **0.0166** | **0.0157** | **0.0180** | **0.0160** | **0.0156** | **0.0140** |
| | (0.0042) | (0.0042) | (0.0049) | (0.0044) | (0.0044) | (0.0053) |
| High school graduates | **0.0151** | **0.0143** | **0.0164** | **0.0139** | **0.0135** | **0.0117** |
| | (0.0031) | (0.0031) | (0.0040) | (0.0033) | (0.0033) | (0.0045) |
| Some college | **0.0123** | **0.0115** | **0.0133** | **0.0103** | **0.0100** | 0.0079 |
| | (0.0030) | (0.0029) | (0.0039) | (0.0033) | (0.0032) | (0.0044) |
| BA or higher | 0.0006 | -0.0002 | 0.0014 | | | |
| | (0.0031) | (0.0031) | (0.0040) | | | |
| Sample size | 782,516 | 782,516 | 782,516 | 516,775 | 516,775 | 516,775 |
| Birth Cohort Controls* | NBC | RBC | SLBC | NBC | RBC | SLBC |

* NBC stands for national birth cohort controls, RBC stands for regional birth cohort controls, and SLBC stands for linear state of birth controls. All models are population weighted and clustered at the state-year level. All models also include controls for kindergarten subsidization, pupil teacher ratio, relative salary of teachers, compulsory school leaving age, census year level unemployment rates, marital status, birth state, and state of residence.

Table 8. The Impact of the Minimum School Starting Age on ln Hourly Wages by Birth Quarter

| | |
|---|---|
| <u>Panel A</u> | |
| Pooled | **0.0106** |
| | (0.0033) |
| <u>Panel B</u> | |
| January-March | **0.0196** |
| | (0.0073) |
| April-June | **0.0158** |
| | (0.0075) |
| July-September | -0.0057 |
| | (0.0073) |
| October-December | **0.0153** |
| | (0.0071) |
| Birth Cohort Controls* | NBC - Q |
| Sample Size | 128,836 |

* NBC - Q stands for quarter of birth specific national birth cohort controls. Model is population weighted and clustered at the state-year level. Model also include controls for kindergarten subsidization, pupil teacher ratio, relative salary of teachers, compulsory school leaving age, marital status, state of residence, and birthquarter * birth state fixed effects.

Appendix Table 1. School Entry Cutoff Dates (School Years 1964-1985)

| | 1964 | 1965 | 1966 | 1967 | 1968 | 1969 | 1970 | 1971 | 1972 | 1973 | 1974 | 1975 | 1976 | 1977 | 1978 | 1979 | 1980 | 1981 | 1982 | 1983 | 1984 | 1985 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 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 |
| 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 |
| 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 |
| 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 |
| 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 |
| CO | 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 |
| 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 |
| FL | 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 | 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 |
| 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 |
| 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 |
| 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 |
| IN | 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 | 10.15 | 10.15 | 10.15 | 10.15 | 9.15 | 9.15 | 9.15 | 9.15 | 9.15 | 9.15 | 9.15 |
| KS | 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 |
| 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 |
| 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 |
| 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 |
| MA | 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 |
| 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 |
| 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 |
| 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 |
| 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 |
| 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 |
| NV | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 11.31 | 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 |
| 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 |
| NJ | 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 |
| 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 |
| 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 |
| ND | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.1 | 10.1 | 10.1 | 10.1 | 8.31 | 8.31 | 8.31 | 8.31 |
| OH | none | none | none | none | none | none | none | none | none | 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 | 11.2 | 11.2 | 11.2 | 11.2 | 11.2 | 11.2 | 11.2 | 11.2 | 11.2 | 11.2 | 11.2 |
| 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 |
| 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 |
| 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 |
| SC | 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 |
| TN | 12.31 | 12.31 | 11.30 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 10.31 | 9.30 |
| TX | 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 |
| UT | 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 |
| VA | 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 |
| WA | ssy | ssy | ssy | ssy | ssy | ssy | ssy | ssy | ssy | ssy | ssy | ssy | ssy | 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 |
| 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 | 9.1 | 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 |

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. All dates were collected from historical state statutes and state session laws. Exceptions are listed here: DE 1964-1981 are from Angrist & Krueger (1992) and Cascio & Lewis (2005); ND 1964-1968 are from Angrist & Krueger (1992), 1968-1972 are from Digest (1972) and Digest (1973), 1972-1978 are from ERS (1975), and 1978-1981are from Digest (1983); OH 1965-1972 are from Digest (1972), Digest (1973) and Digest (1983); UT 1964-1985 are from Digest (1972), Digest (1973) and Digest (1983).

Appendix Table 1b. School Entry Cutoff Dates (School Years 1986-2005)

| | 1986 | 1987 | 1988 | 1989 | 1990 | 1991 | 1992 | 1993 | 1994 | 1995 | 1996 | 1997 | 1998 | 1999 | 2000 | 2001 | 2002 | 2003 | 2004 | 2005 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| AL | 10.1 | 10.1 | 10.1 | 10.1 | 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 |
| AK | 11.2 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 8.15 | 9.1 | 9.1 |
| AZ | 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 |
| 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 | 9.15 | 9.15 | 9.15 | 9.15 | 9.15 |
| CA | 12.1 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 | 12.2 |
| CO | 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 |
| DE | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 11.30 | 10.31 | 9.30 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 |
| FL | 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 |
| GA | 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 |
| 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 |
| ID | 10.16 | 10.16 | 10.16 | 10.16 | 9.16 | 8.16 | 8.16 | 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 |
| IL | 11.1 | 10.1 | 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 |
| IN | none | none | none | 9.1 | 8.1 | 7.1 | 6.1 | 6.1 | 6.1 | 6.1 | 6.1 | 6.1 | 6.1 | 6.1 | 6.1 | 7.1 | 7.1 | 7.1 | 7.1 | 7.1 |
| IA | 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 |
| KS | 9.1 | 9.1 | 9.1 | 9.1 | 9.1 | 9.1 | 9.1 | 9.1 | 9.1 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 |
| KY | 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 |
| LA | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 12.31 | 9.30 | 9.30 | 9.30 | 9.30 | 9.30 | 9.30 | 9.30 | 9.30 | 9.30 | 9.30 |
| 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 |
| 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 | 11.30 | 10.31 | 9.30 |
| MA | 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 |
| 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 |
| MS | 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 |
| MO | 9.1 | 8.1 | 7.1 | 7.1 | 7.1 | 7.1 | 7.1 | 7.1 | 7.1 | 7.1 | 8.1 | 8.1 | 8.1 | 8.1 | 8.1 | 8.1 | 8.1 | 8.1 | 8.1 | 8.1 |
| MT | 9.10 | 9.10 | 9.10 | 9.10 | 9.10 | 9.10 | 9.10 | 9.10 | 9.10 | 9.10 | 9.10 | 9.10 | 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 |
| NV | 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 |
| 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 |
| NJ | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA |
| NM | 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 |
| 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 | LEA | LEA | LEA | LEA | LEA |
| NC | 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 |
| ND | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 8.31 | 9.1 | 9.1 | 9.1 | 9.1 | 9.1 | 9.1 | 9.1 |
| OH | 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 |
| OK | 11.2 | 11.2 | 11.2 | 11.2 | 11.2 | 11.2 | 11.2 | 11.2 | 11.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.1 | 9.1 | 9.1 | 9.1 | 9.1 | 9.1 |
| OR | 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 |
| 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 | LEA | LEA |
| RI | 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 | 9.1 | 9.1 |
| SC | 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 | 9.1 | 9.1 | 9.1 | 9.1 | 9.1 |
| SD | 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 |
| TN | 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 |
| TX | 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 |
| UT | ssy | ssy | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 | 9.2 |
| VT | 1.1 | 1.1 | 1.1 | 1.1 | 1.1 | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA |
| VA | 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 |
| WA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA | LEA |
| WV | 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 |
| WI | 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 |
| 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 |

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. All dates were collected from historical state statutes and state session laws. Exceptions are listed here: MD 2002-2005 are from the Education Commision of the States (http://www.ecs.org/); UT 1986-1987 are from Digest (1973) and Digest (1983). The cutoff date for OH during 2001-2005 is September 30 or by the first day of the term if that occurs in August or September.

Appendix Table 2. State Statutes Regarding School Entry

| | 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 |
| 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 § 7-301 |
| 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 |
| NC | ST § 115C-364) |
| ND | ST 15.1-06-01 |
| OH | ST § 3321.01 |
| OK | ST T. 70 § 18-108 |
| OR | ST § 336.092 |
| PA | ST 24 PS. § 5-503 |
| 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 |
| 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. Exceptions to this are noted in Appendix 1.

Appendix Table 3. History of State Nonparticipation in NAEP Testing

| | Math | | | | | | | | | Reading | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 4th Grade | | | | 8th Grade | | | | | 4th Grade | | | | | | 8th Grade | | | |
| | 1992 | 2000 | 2003 | 2005 | 1990 | 1992 | 2000 | 2003 | 2005 | 1992 | 1994 | 1998 | 2002 | 2003 | 2005 | 1998 | 2002 | 2003 | 2005 |
| AL | | | | | | | | | | | | | | | | | | | |
| AK | X | X | | | X | X | X | | | X | X | X | X | | | X | X | | |
| AZ | | | | | | | | | | | | | | | | | | | |
| AR | | | | | | | | | | | | | | | | | | | |
| CA | | | | | | | | | | | | | | | | | | | |
| CO | | X | | | | | X | | | | | | X | | | | X | | |
| CT | | | | | | | | | | | | | | | | | | | |
| DE | | X | | | | | X | | | | | | | | | | | | |
| FL | | X | | | | | X | | | | | | | | | | | | |
| GA | | | | | | | | | | | | | | | | | | | |
| HI | | | | | | | | | | | | | | | | | | | |
| ID | | | | | | | | | | | X | X | | | | X | | | |
| IL | X | | | | | X | | | | X | X | X | X | | | X | X | | |
| IN | | | | | | | | | | | | X | | | | X | | | |
| IA | | | | | | | | | | | | | | | | X | | | |
| KS | X | | | | X | X | | | | X | X | | | | | | | | |
| KY | | | | | | | | | | | | | | | | | | | |
| LA | | | | | | | | | | | | | | | | | | | |
| ME | | | | | X | | | | | | | | | | | | | | |
| MD | | | | | | | | | | | | | | | | | | | |
| MA | | | | | X | | | | | | | | | | | | | | |
| MI | | | | | | | | | | | X | | | | | X | | | |
| MN | | | | | | | | | | | | | | | | | | | |
| MS | | | | | X | | | | | | | | | | | | | | |
| MO | | | | | X | | | | | | | | | | | | | | |
| MT | X | | | | | X | | | | X | | | | | | | | | |
| NE | | | | | | | | | | | | X | | | | X | | | |
| NV | X | | | | X | X | | | | X | X | | | | | | | | |
| NH | | X | | | | | X | | | | | | X | | | X | X | | |
| NJ | | X | | | | | X | | | | | X | X | | | X | X | | |
| NM | | | | | | | | | | | | | | | | | | | |
| NY | | | | | | | | | | | | | | | | | | | |
| NC | | | | | | | | | | | | | | | | | | | |
| ND | | | | | | | | | | | | X | | | | X | | | |
| OH | | | | | | | | | | | X | X | | | | X | | | |
| OK | | | | | | | | | | | X | | | | | | | | |
| OR | X | | | | | X | | | | X | X | | | | | | | | |
| PA | | X | | | | | X | | | | | X | | | | X | | | |
| RI | | | | | | | | | | | | | | | | | | | |
| SC | | | | | X | | | | | | | | | | | | | | |
| SD | X | X | | | X | X | X | | | X | X | X | X | | | X | X | | |
| TN | | | | | X | | | | | | | | | | | | | | |
| TX | | | | | | | | | | | | | | | | | | | |
| UT | | | | | X | | | | | | | | | | | | | | |
| VT | X | | | | X | X | | | | X | X | X | | | | X | | | |
| VA | | | | | | | | | | | | | | | | | | | |
| WA | X | X | | | X | X | X | | | X | | | | | | | | | |
| WV | | | | | | | | | | | | | | | | | | | |
| WI | | X | | | | | X | | | | | | X | | | | X | | |
| WY | | | | | | | | | | | | | | | | | | | |

Appendix Table 4. The Impact of the Minimum School Starting Age on ln Weekly Wages

| | Full Sample | | | Excluding BA+ | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| No education controls | **0.0095** | **0.0094** | 0.0084 | **0.0126** | **0.0128** | **0.0101** |
| | (0.0031) | (0.0031) | (0.0045) | (0.0031) | (0.0031) | (0.0045) |
| With education controls | **0.0094** | **0.0084** | **0.0104** | **0.0120** | **0.0118** | **0.0099** |
| | (0.0029) | (0.0029) | (0.0040) | (0.0031) | (0.0031) | (0.0045) |
| By education group | | | | | | |
| High school dropouts | **0.0159** | **0.0150** | **0.0168** | **0.0149** | **0.0147** | **0.0130** |
| | (0.0045) | (0.0045) | (0.0052) | (0.0045) | (0.0046) | (0.0055) |
| High school graduates | **0.0159** | **0.0149** | **0.0166** | **0.0137** | **0.0136** | **0.0117** |
| | (0.0033) | (0.0033) | (0.0044) | (0.0034) | (0.0035) | (0.0047) |
| Some college | **0.0115** | **0.0106** | **0.0120** | **0.0086** | **0.0085** | 0.0064 |
| | (0.0032) | (0.0031) | (0.0042) | (0.0034) | (0.0033) | (0.0046) |
| BA or higher | -0.0006 | -0.0015 | -0.0003 | | | |
| | (0.0033) | (0.0033) | (0.0043) | | | |
| Sample size | 782,516 | 782,516 | 782,516 | 578,449 | 578,449 | 578,449 |
| Birth Cohort Controls* | NBC | RBC | SLBC | NBC | RBC | SLBC |

* NBC stands for national birth cohort controls, RBC stands for regional birth cohort controls, and SLBC stands for linear state of birth controls. All models are population weighted and clustered at the state-year level. All models also include controls for kindergarten subsidization, pupil teacher ratio, relative salary of teachers, compulsory school leaving age, census year level unemployment rates, marital status, birth state and state of residence.

Appendix Table 5. The Impact of the Minimum School Starting Age on ln Weekly by Birth Quarter

| | |
|---|---|
| <u>Panel A</u> | |
| Pooled | **0.0115** |
| | (0.0036) |
| <u>Panel B</u> | |
| January-March | **0.0221** |
| | (0.0076) |
| April-June | ***0.0149*** |
| | (0.0080) |
| July-September | -0.0031 |
| | (0.0075) |
| October-December | **0.0145** |
| | (0.0070) |
| Birth Cohort Controls* | NBC - Q |
| Sample Size | 128,836 |

* NBC - Q stands for quarter of birth specific national birth cohort controls. Model is population weighted and clustered at the state-year level. Model also include controls for kindergarten subsidization, pupil teacher ratio, relative salary of teachers, compulsory school leaving age, marital status, state of residence, and birthquarter * birth state fixed effects.