School-Entry Policies and Skill Accumulation Across Directly and Indirectly Affected Individuals

Kelly Bedard
Elizabeth Dhuey

ABSTRACT

During the past half-century, there has been a trend toward 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. Backing up the cutoff by one month increases average male hourly earnings by approximately 0.6 percent. The evidence also suggests that the majority of the cohort benefits from backing up the cutoff, not just those who must delay entry.

I. 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 toward these children. Despite the potential for offsetting effects, the ac-

Kelly Bedard is a professor of economics at the University of California, Santa Barbara. Elizabeth Dhuey is an assistant professor of economics at the University of Toronto. *They thank David Card, Peter Kuhn, David Autor, Philip Babcock, and seminar participants at the University of Calgary, UCLA, the University of Washington, UCSD, UC Merced, the University of Manitoba, Tilberg University, Wilfrid Laurier University, the University of Waterloo and the Society of Labor Economist Annual meetings for helpful comments. Elizabeth Dhuey gratefully acknowledges the financial support of the Social Sciences and Humanities Research Council of Canada. The data used in this article can be obtained beginning January 2013 through December 2016 from Elizabeth Dhuey, 121 St. George Street, Toronto, ON, M5S2E8, Canada, elizabeth.dhuey@utoronto.ca.*
[Submitted September 2010; accepted September 2011]
ISSN 022-166X E-ISSN 1548-8004

Table 1
Cutoff Date Distribution

	Number of States	
	1964	1988
January/February	12	7
December	5	3
November	4	1
October	10	11
September/start of school year (SSY)	8	20
July	0	1
Local education authority (LEA)	3	4
None	6	1

tions of policymakers 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, by 1988, 32 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 school readiness rates. Second, and perhaps more important to policymakers, 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. 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 13 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 to 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 2009; Crawford, Dearden, and Meghir 2007), to higher university 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 2008; Kawaguchi 2011). However, not all studies find long-term wage effects (Dobkin and Ferreira 2010; Fertig and Kluve 2005; Black, Devereux, and Salvanes 2011).

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 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 (four years and eight 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, therefore, should be interpreted as the average effect of the policy shift.[4]

Using a state of birth level repeated cross-section for 1959–83 birth cohorts from the 2000 U.S. Census and the 2001–2007 American Community Surveys combined with school-entry laws from 1964–88, we find that backing up the school-entry cutoff by one month (for example, from January 1 to December 1) increases male hourly earnings by approximately 0.6 percent but has little impact on average female hourly earnings. Given an "average" school-entry change of about three months,[5] this translates into a 1.8 percent increase in the average hourly earnings of males. This is a sizeable increase and points to a substantial return for males 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

1. Although dual and multiple entry dates decrease 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, in three cases, the cutoff has been moved to a later date (see Appendix Table A1).
3. This assumes that all children enter when eligible. We discuss early and late entry in Section IV.A.
4. In Section V, we separately examine the effects for the directly and indirectly affected subsamples.
5. The unweighted mean school-entry change is 2.7 months.

before September 1 during the period under study,[6] it is an open question whether pushing the school-entry cutoff date even farther back would have a positive, negative, or neutral impact.

In an era in which backing up school-entry dates is a popular potential education policy, both because it involves short-term cost savings and because it is thought to improve interstate test score comparisons, it is important to point out that an average gain does not necessarily imply that everyone gains. Directly affected individuals are forced wait a year to enter elementary school and the labor market. Based on the male estimates reported in this paper, we can approximate lifetime costs and benefits if we are willing to make strong assumptions about labor market entry age, retirement age, the return to experience, the starting wage, and the discount rate. For illustration purposes let us 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 $5,000 daycare cost for children who must wait an extra year to start school. Under these assumptions, the 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 arises 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. The estimates reported in this paper suggest that the indirect effect is approximately 1 percent, therefore there appears to be a small loss to directly affected individuals and a substantial gain for the majority of the cohort.

II. 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,[7] 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. Oreopoulos, 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 (2011) use a regression discontinuity design in

6. The cutoff date in Missouri becomes August 1 in 1987 and July 1 in 1988. Since class sizes also change in years with policy changes, our specification includes state-specific indicators for policy-change years. Given this specification and the fact that the only July and August policy change dates occur at the very end of the sample period, with no cohorts following the policy change, our coefficient estimates for cutoff changes only reflect cutoff changes between September and January. See Section II.

7. See Bound, Jaeger, and Baker (1995), Bound and Jaeger (2000), and Dobkin and Ferreira (2010) for detailed discussions of the pros and cons of using quarter of birth as an instrument for educational attainment.

California and Texas to compare the fertility outcomes of women born just before and just after the school-entry cutoff date. Finally, Oreopoulos (2006) and Clark and Royer (2010) use the increase in the national compulsory law in the United Kingdom in 1947 to estimate the impact of educational attainment on earnings (Oreopoulos 2006) and health and mortality (Clark and Royer 2010).

However, the results from the literature using school-entry laws referenced in the introduction along with the literature on school exit laws draws 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.[8] There are two key issues. First, if relative age within a cohort directly affects human capital accumulation as well as affects educational attainment, it likely has a direct impact on other outcomes. Therefore, using school-entry laws as instruments is invalid. Second, if compulsory schooling laws change educational attainment, discontinuities in educational attainment should be localized near the binding cutoffs. However, they appear over a range of high school grades, which 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 2010).

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,[9] while a September 1 cutoff implies an entry age range of 60 to 71 months. Second, while only children born between September 2 and 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 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. Because this group waits an extra year before entering school, there are three interrelated 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). Although 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

8. There is no evidence to suggest that the compulsory schooling change(s) used by Oreopoulos (2006) and Clark and Royer (2010) are invalid.

9. For descriptive ease, we assume that all children enter when eligible, we will return to this issue.

have a positive impact on the entire cohort. Since all of these effects are nonnegative, 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. Because 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.

Although the net effect for certain subgroups within school-entry cohorts is theoretically ambiguous, the average cohort effect is likely positive. We conjecture this because of the likely positive absolute age effect for directly affected students, the likely positive average cohort age effect for the entire cohort,[10] and the fact that the relative age effects wash out on average.[11] 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. We do this using a state of birth level repeated cross-section. Because 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 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:

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

where W_{ibty} denotes the ln adult wage, for individual i born in state b in year t observed in Census or 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_{ibty} is a vector of race indicators, state of birth-specific indicators denoting that it is the year of, before or after an entry age policy change,[12] and in

10. Note that the first two effects are not separately identifiable since they move together.

11. While different students may be relatively older or younger, there is always a 12-month age range. Years in which the cutoff changes are exceptions: The age range is shorter in these years.

12. This indicator allows for the fact that years surrounding entry age law changes also have cohort size 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. As leads and lags are often associated with law changes, it is also possible that the years preceding and following changes also experience cohort fluctuations. Including controls for these years ensures that we do not confound cohort size and entry age effects.

most specifications birth state-specific education institution and quality controls,[13] 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 region of birth-specific cohort indicators, and ε_{ibty} is the usual error term.[14] 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 reemphasizing 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 each month or quarter of birth enrolled in school at age five. However, some parents enroll their child a year early and some hold them back and enroll them a year late. 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 IV.A.

III. Data

A. 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 1988. 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

13. This includes kindergarten subsidization, pupil teacher ratio, relative teacher salaries, and compulsory school leaving age. See Section IIIC for details.

14. Alternative specifications are explored in Sections IV and V.

entering the education system. In 1964, eight states required children to be five by September but by 1988, 21 states had this requirement. The complete set of entry laws from 1964–88 are reported in Appendix Table A1.

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 A2. In addition, Appendix Table A3 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 1964–88 is available from the authors upon request.[15]

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.[16] 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 A1 and are coded as missing in the data. States that leave school entry to the discretion of local authorities are also coded as missing in the data since we do not have substate 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.[17]

Estimating Equation 1 requires that we restrict attention to the subset of years reported in Table A1 (cohorts who are age five in 1964–88). 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 1964–88. This translates into using the entry cutoff dates for 1959–83 birth cohorts.

B. 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 five years old. Because quarter of birth is only available for the 2005–2007 ACSs—the census and the 2001–2004 ACS only report age

15. These cutoff dates have been cross-referenced with Angrist and Krueger (1992), Cascio and Lewis (2006), National Center for Education Statistics (1972, 1973, and 1983), the Education 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 rechecked 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 Tables A2 and A3 for more details regarding citations.
16. This simplification has no substantive effect.
17. The one exception is Montana, which is coded as mid-September because they list September 10 as the school-entry cutoff date beginning in 1979, and it does not appear that this was a change in policy from the previous regime.

as of April 1—we assign people to age five cohorts using year − age + 4. While this incorrectly assigns some people, it is the best that we can do without more detailed birth date data.[18]

Ideally, one would restrict attention to individuals who have completed all major schooling and who are preretirement. For example, by restricting the sample to individuals aged of 30–54. However, most of the cutoff changes occurred recently—there are only seven cutoff changes between 1964 and 1975. Because it is important to use the most recent cohorts possible, we restrict the sample to U.S.-born individuals from the 1959–83 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 one 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 cohorts who were eligible to enter kindergarten from 1964 through 1988 is that wage observations are at younger than optimal ages for the later cohorts. The sample includes people aged 23–45 who reside in the 48 contiguous states.[19] 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 hand, this would require excluding the post-1976 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. Given these data limitations, we focus on employed young adults, ages 23–45.

Table 2 summarizes the Census and ACS data. It reports summary statistics for U.S.-born men and women under the two different sample definitions used in this analysis. Column 1 and 3 report the summary statistics for all males and females. These samples are used to examine the impact of cutoff changes on educational attainment. Columns 2 and 4 are restricted to males and females who are not in school or prison, who work for wages (are not self-employed), and report positive income. These are the primary samples used in the majority of the analysis.

18. In our case, incorrect cohort assignment is only a serious problem in years with policy changes. The inclusion of state of birth-specific policy year indicators is therefore important. We also check the robustness of our results by dropping observations for the year before, of, and after a change in our base specification. The results are similar in all cases. Our specification has four additional benefits. (1) It is appropriate in cases where there are leads or lags in adoption. (2) It eliminates policy-timing difficulties associated with states in which school entry is Grade 1 rather than kindergarten. (3) As discussed in footnote 12, it ensures that we are not confounding entry age law changes with changes in cohort size in the years immediately surrounding cutoff changes. (4) It mitigates problems associated with the mismatch caused by allocating individuals to birth cohorts based on age rather than based on school year cohorts because the misallocation only misassigns 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 to implement because we do not have exact birth dates.

19. Observations with imputed education, wage, sex, race, or place of birth data or missing education information are excluded from the sample.

Table 2
Census and ACS Summary Statistics

	Males		Females	
	All (1)	Employed (2)	All (3)	Employed (4)
Wages				
Ln hourly wage	—	2.89	—	2.68
		(0.67)		(0.67)
Other outcomes				
In school	0.08	0.00	0.11	0.00
	(0.27)	(0.00)	(0.31)	(0.00)
In the labor force	0.91	0.97	0.77	0.93
	(0.29)	(0.18)	(0.42)	(0.26)
Employed	0.85	0.92	0.73	0.89
	(0.36)	(0.27)	(0.45)	(0.32)
Full-time	0.71	0.82	0.52	0.67
	(0.46)	(0.38)	(0.50)	(0.47)
Education outcomes				
High school graduate	0.39	0.41	0.34	0.35
	(0.49)	(0.49)	(0.47)	(0.48)
Some college	0.26	0.25	0.29	0.28
	(0.44)	(0.43)	(0.45)	(0.45)
BA +	0.27	0.28	0.31	0.32
	(0.45)	(0.45)	(0.46)	(0.47)
School start date				
Age of youngest children (months)	57.76	57.75	57.75	57.75
	(1.51)	(1.51)	(1.51)	(1.51)
Other state education policies				
Kindergarten	0.85	0.86	0.85	0.85
	(0.35)	(0.35)	(0.36)	(0.36)
Pupil-teacher ratio	19.36	19.35	19.38	19.36
	(2.40)	(2.38)	(2.39)	(2.37)
Relative teacher salaries	0.64	0.64	0.64	0.64
	(0.07)	(0.07)	(0.07)	(0.07)
School leaving age	16.51	16.51	16.50	16.50
	(0.82)	(0.82)	(0.82)	(0.82)
Other variables				
Birth cohort size	160,837	160,232	160,736	159,221
	(115,216)	(114,426)	(114,731)	(113,622)
State of residence GDP (in millions)	517,010	510,986	515,154	506,591
	(452,925)	(445,922)	(448,356)	(439,585)

(continued)

Table 2 *(continued)*

	Males		Females	
	All (1)	Employed (2)	All (3)	Employed (4)
State of residence unemployment rate	5.09 (1.31)	5.09 (1.32)	5.09 (1.28)	5.07 (1.28)
Age	33.58 (6.32)	33.70 (6.21)	33.60 (6.31)	33.76 (6.26)
Black	0.11 (0.32)	0.11 (0.31)	0.14 (0.08)	0.14 (0.34)
Hispanic	0.08 (0.27)	0.08 (0.27)	0.27 (0.27)	0.08 (0.26)
Other	0.03 (0.17)	0.03 (0.17)	0.03 (0.17)	0.03 (0.17)
Married	0.53 (0.50)	0.56 (0.50)	0.55 (0.50)	0.54 (0.50)
Sample size	2,022,544	1,530,618	2,182,211	1,473,400

Note: Sample includes individuals aged 23–45 in 2000–2007. Summary statistics are population weighted. Standard deviations are in parentheses. All dollar values are reported in 2007 currency. Columns 2 and 4 are restricted to men and women, respectively, who are not in school or prison and who work for wages (are not self-employed), and who report positive income.

C. 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 13 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 (intercensus years are linearly interpolated). The number of students, the number and the wages 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 states subsidized kindergarten with state revenue in a particular state for a particular birth cohort.[20]

IV. 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.

A. 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.[21] 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[22] residing in the 48 contiguous states that have state-level minimum school-entry laws in the 1960, 1970 and 1980 U.S. Censuses,[23] using the following slightly simplified version of Equation 1.

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

where E_{iry} is the enrollment status (1 = enrolled in first grade or higher)[24] 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 region-specific year

20. See Dhuey (2011) for information regarding collection of data on state-subsidized kindergarten.
21. 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.
22. 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.
23. Cohorts are defined by age rather school year due to data limitations. All results are similar if we define cohorts based on the calendar year instead of age.
24. The results are almost identical if we define enrollment status as one if enrolled in grade 1.

indicators, and ν_{iry} is the usual error term. All models are population weighted and the standard errors are clustered at the state level.

The Equation 2 results are reported in Table 3. The results for males are reported in Columns 1–4 and the results for females are reported in Columns 5–8. Focusing on Column 1 and 5, backing up the school cutoff date by one month decreases the fraction of six-year-olds enrolled in Grade 1 or higher by 3.4 percentage points and backing it up by three months decreases enrollment by 10.2 percentage points, for both males and females. If the entire impact comes from those directly affected by the cutoff change, this implies a compliance rate of approximately 40 percent. These findings of imperfect compliance are consistent with Dobkin and Ferreira (2010). The large discrepancy reflects the fact that children with birthdates near cutoffs are more likely to be accelerated or retained.

Unlike the majority of the available wage data, the 1960, 1970, and 1980 Census data includes quarter of birth information. This allows us to see whether changes in enrollment are driven by groups directly affected by cutoff date changes. More specifically, since all cutoff changes during this period occur between January 1 and September 1, most of the enrollment change should be driven by children born in the fourth quarter, with a small impact on third-quarter children. We therefore generalize Equation 2 to allow differential effects across quarters. In particular, indicators for first, second and third quarter births and their interactions with *S*, *R*, and *Y* are added to the model. This specification allows the impact of cutoff changes to differ across birth quarters. The results for the generalized model are reported in Columns 2 and 6 in Table 3 for males and females, respectively. Focusing on these columns, backing up the school cutoff date by three months decreases the fraction of fourth quarter six-year-old males (females) enrolled in Grade 1 or higher by 39.6 (42.3) percentage points, reduces enrollment among third-quarter babies by 5.7 (5.1) percentage points, and has no measurable impact on enrollment for first and second quarter six-year-olds. As a final specification check, Columns 3, 4, 7, and 8 add linear state trends. The results are robust to the inclusion of this trend specification in all cases.[25]

B. Educational Attainment

While it is not necessary for cutoff changes to effect educational attainment in order to have an impact on labor market outcomes, given the possible direct impact on skill accumulation, it is nonetheless useful 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 II.

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

25. Appendix Table A4 generalizes the Table 3 models by adding interactions between race/ethnicity and S_{bt}. This exercise reveals some evidence that black and Hispanic students are less likely to reduce enrolment in response to a law change.

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

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age of youngest children (months)	**−0.034**	**−0.132**	**−0.059**	**−0.180**	**−0.034**	**−0.141**	**−0.046**	**−0.175**
	(0.005)	(0.023)	(0.008)	(0.013)	(0.008)	(0.021)	(0.010)	(0.011)
Youngest×(January–March)		**0.142**		**0.162**		**0.136**		**0.145***
		(0.021)		(0.028)		(0.021)		(0.013)
Youngest×(April–June)		**0.128**		**0.166***		**0.138**		**0.184***
		(0.024)		(0.010)		(0.020)		(0.013)
Youngest×(July–September)		**0.113***		**0.160***		**0.124***		**0.160**
		(0.030)		(0.015)		(0.023)		(0.030)
Includes state-specific linear trend	No	No	Yes	Yes	No	No	Yes	Yes
Sample size	102,958	102,958	102,958	102,958	99,534	99,534	99,534	99,534

Note: The dependent variable is one if the individual is enrolled in Grade 1 or higher and zero otherwise. All models are population weighted and clustered at the state of residence level. Heteroskedastic-consistent standard errors are in parentheses. Bold coefficients are significant at the 5 percent level. Columns 1, 3, 5, and 7 include indicators for the existence of publicly funded kindergarten, sex, race, state of residence, and census-division-specific year cohorts. Columns 2, 4, 6, and 8 further include birth quarter indicators and interactions between state of residence and birth quarter and year and birth quarter. The sample includes six-year-olds from the 1960, 1970, and 1980 U.S. Censuses. An asterisk in Column 2, 4, 6, or 8 indicates that youngest plus the specified interaction effect (i.e., $\beta_1 + \beta_2$, $\beta_1 + \beta_3$, or $\beta_1 + \beta_4$) is nonzero at the 10 percent level.

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 birth year t, 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 region of birth-specific cohort indicators. In the baseline specification (Columns 1 and 6 in Tables 4 and 5), X_{ibty} includes race and state of birth-specific indicators for it being the year of, before, or after a policy change.[26] Columns 2 and 7 expand X_{ibty} to include the availability of publicly subsidized kindergarten, the pupil teacher ratio, relative teacher salaries, and the compulsory school leaving age.

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. The model reported in Columns 3 and 8 add a vector of state of residence indicators, a vector of census region of residence-specific age indicators, cohort size defined by state of birth, state of residence-specific GDP and unemployment rates,[27] and marital status. The model in Columns 4 and 9 further adds a set of region of birth–region of residence interactions to control for selective migration. Heckman, Layne-Farrar, and Todd (1996) show that nonrandom 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. Finally, Columns 5 and 10 add state of birth-specific linear cohort trends.

Table 4 reports the educational attainment results using Equation 3 for men and women aged 23–45 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 little evidence that school start dates impact educational attainment at any level. As such, any substantive impact on wages must be coming through changes in within grade skill accumulation.

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

The baseline Equation 1 ln hourly wage estimate for men is reported in Row 1 of Column 1 in Panel A of Table 5. The sample includes men aged 23–45 in 2000–2007 who are not in prison or school, who work for wages, and who report positive income. Similar to Table 4, Columns 2–5 progressively expands the

26. The policy year indicators are state of birth-specific because states changed school-entry dates by different amounts.

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

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

	Males					Females				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
High school graduate or higher	−0.0001	0.0001	0.0005	0.0006	0.0005	−0.0007	−0.0006	−0.0002	−0.0002	−0.0002
	(0.0013)	(0.0014)	(0.0015)	(0.0015)	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0009)
Some college or higher	−0.0003	−0.0007	0.0004	0.0007	0.0011	−0.0009	−0.0012	−0.0003	−0.0001	0.0009
	(0.0015)	(0.0015)	(0.0015)	(0.0015)	(0.0015)	(0.0012)	(0.0012)	(0.0010)	(0.0010)	(0.0013)
BA or higher	−0.0011	−0.0015	−0.0001	0.0002	0.0004	−0.0016	−0.0016	0.0000	0.0002	0.0011
	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0016)	(0.0012)	(0.0013)	(0.0011)	(0.0010)	(0.0015)
Additional controls										
Other educational variables	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
State of residence	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Region of residence × age	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
State of residence GDP & UER	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Marital status	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Birth state cohort size	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Region of birth × region of residence	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Linear state of birth Specific cohort trends	No	No	No	No	Yes	No	No	No	No	Yes
Sample size	2,022,544	2,022,544	2,022,544	2,022,544	2,022,544	2,182,211	2,182,211	2,182,211	2,182,211	2,182,211

Note: The sample includes individuals aged 23–45 in 2000–2007. All models are population weighted and clustered at the state of birth level. All models include race, state of birth, age, state of birth-specific indicators for years surrounding cutoff law changes, and census region of birth-specific cohort indicators. Other education controls include kindergarten subsidization, pupil teacher ratio, relative salary of teachers, and compulsory school leaving age. Heteroskedastic-consistent standard errors are in parentheses. Bold coefficients are statistically significant at the 5 percent level and bold italics are statistically significant at the 10 percent level.

set of control variables for the male models. Column 2 adds a set of education quality variables. Column 3 adds state of residence, census region of residence-specific age indicators, cohort size defined by state of birth, state of residence-specific GDP and unemployment rates, and marital status. Column 4 further adds an interaction between region of birth and region of residence and Column 5 adds linear state of birth-specific cohort trends. Depending on the specification, we estimate that a one-month increase in the minimum school starting age increases average male hourly wages by 0.55–0.68 percent. Using the mean cutoff change of three months, this range translates into a 1.65–2.04 percent increase in average male hourly wages.

The same set of results for women is reported in Columns 6–10. While the female point estimates are generally smaller, the male-female difference is not always statistically significant. More specifically, the results indicate that a one-month increase in the minimum school starting age is associated with a 0.21–0.47 increase in the average female hourly wage, which translates into a 0.63–1.41 percent increase for a three-month entry age change. There are two important caveats. First, not all female coefficients are precisely estimated: We cannot reject the null hypothesis of no effect even at the 10 percent level for the specifications reported in Columns 6 and 7. Second, the female results are not robust to sample definition changes. We will return to this issue shortly.

One might be concerned that the estimates reported on Panel A 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 the 2 percent per year average return to experience lost to the directly affected month of people, a point estimate of 0.0055 would rise to approximately 0.0072. 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.

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; 13 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 0.6 percent per month that comes only from directly affected children is clearly unreasonable, as it would imply a 7.2 percent effect for the directly affected month and zero for the other eleven months. As we show below, this is not the case—substantial indirect effect is driving the male estimates.

On the surface, it may seem like the estimates reported in this paper contradict those reported in some recent papers in the age at entry literature. For instance, Black, Devereux, and Salvanes (2011) estimate short-run small negative wage effects for those starting school older at older ages and Fredriksson and Öckert (2008) estimate positive educational attainment effects. However, it is important to keep in

Table 5
The Impact of the Minimum School Starting Age

	Males					Females				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Ln Hourly Wages										
Age of youngest children (months)	**0.0059**	**0.0055**	**0.0066**	**0.0068**	**0.0056**	0.0022	0.0021	***0.0033***	***0.0034***	**0.0047**
	(0.0020)	(0.0020)	(0.0022)	(0.0022)	(0.0025)	(0.0020)	(0.0018)	(0.0018)	(0.0018)	(0.0018)
Panel B: Ln Hourly Wages (Excluding States with No Policy Changes)										
Age of youngest children (months)	0.0037	0.0031	***0.0038***	***0.0038***	***0.0049***	0.0015	0.0017	0.0020	0.0020	**0.0046**
	(0.0022)	(0.0021)	(0.0021)	(0.0021)	(0.0027)	(0.0017)	(0.0017)	(0.0017)	(0.0017)	(0.0022)
Panel C: Ln Hourly Wages (Restricted to Full-Time Workers)										
Age of youngest children (months)	**0.0045**	***0.0039***	**0.0048**	**0.0049**	0.0004	**0.0044**	**0.0040**	**0.0051**	**0.0054**	**0.0038**
	(0.0019)	(0.0019)	(0.0020)	(0.0020)	(0.0024)	(0.0016)	(0.0013)	(0.0015)	(0.0015)	(0.0015)
Panel D: Ln Hourly Wages (Restricted to Age 30 +)										
Age of youngest children (months)	0.0044	0.0042	**0.0061**	**0.0062**	***0.0095***	−0.0027	−0.0022	−0.0006	−0.0004	0.0021
	(0.0030)	(0.0033)	(0.0030)	(0.0030)	(0.0052)	(0.0025)	(0.0025)	(0.0025)	(0.0026)	(0.0027)
Panel E: Other Outcomes										
In school	−0.0014	−0.0009	−0.0009	−0.0008	0.0012	−0.0007	−0.0001	−0.0002	−0.0002	0.0005
	(0.0014)	(0.0013)	(0.0012)	(0.0012)	(0.0007)	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0008)
In labor force	**0.0012**	**0.0011**	**0.0011**	**0.0012**	**0.0011**	−0.0012	−0.0001	0.0001	0.0001	0.0011
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0008)	(0.0015)	(0.0011)	(0.0010)	(0.0010)	(0.0009)
Employed	**0.0024**	**0.0022**	**0.0023**	**0.0023**	**0.0031**	−0.0014	−0.0003	0.0000	−0.0001	0.0003
	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0012)	(0.0018)	(0.0013)	(0.0011)	(0.0011)	(0.0011)
Fulltime	**0.0030**	**0.0028**	**0.0028**	**0.0028**	0.0015	−0.0014	−0.0002	0.0002	0.0001	0.0006
	(0.0011)	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0020)	(0.0017)	(0.0015)	(0.0015)	(0.0019)

Additional controls										
Other educational variables	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
State of residence	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Region of residence × age	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
State of residence GDP & UER	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Marital status	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Birth state cohort size	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Region of birth × region of residence	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Linear state of birth Specific cohort trends	No	No	No	No	Yes	No	No	No	No	Yes
Sample size Panel A	1,530,618	1,530,618	1,530,618	1,530,618	1,530,618	1,473,400	1,473,400	1,473,400	1,473,400	1,473,400
Sample size Panel B	678,152	678,152	678,152	678,152	678,152	657,242	657,242	657,242	657,242	657,242
Sample size Panel C	1,273,719	1,273,719	1,273,719	1,273,719	1,273,719	983,047	983,047	983,047	983,047	983,047
Sample size Panel D	1,087,237	1,087,237	1,087,237	1,087,237	1,087,237	1,037,827	1,037,827	1,037,827	1,037,827	1,037,827
Sample size Panel E (in school)	1,993,724	1,993,724	1,993,724	1,993,724	1,993,724	2,177,920	2,177,920	2,177,920	2,177,920	2,177,920
Sample size Panel E (all other outcomes)	1,828,161	1,828,161	1,828,161	1,828,161	1,828,161	1,949,337	1,949,337	1,949,337	1,949,337	1,949,337

Note: The samples in Panels A-C include individuals who are age 23–45 in 2000–2007, are not in school, are not in prison, work for wages (are not self-employed), and report positive income. The sample in Panel D is the same as Panels A–C but includes only individuals who are age 30–45. The full-time sample in Panel C is restricted to employees who report at least $2000 in annual earnings, 30 or more usual weekly hours, and at least 46 weeks of work. The sample in Panel E includes individuals who are aged 23–45 in 2000–2007, are not in prison (for outcome “in school”), and who are not in school (for all other outcomes in Panel E). All models are population weighted and clustered at the state of birth level. All models include race, state of birth, age, state of birth-specific indicators for years surrounding cutoff law changes, and census region of birth-specific cohort indicators. Other education controls include kindergarten subsidization, pupil teacher ratio, relative salary of teachers, and compulsory school leaving age. Heteroskedastic-consistent standard errors are in parentheses. Bold coefficients are statistically significant at the 5 percent level and bold italics are statistically significant at the 10 percent level.

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

	Males				Females			
	Ln Hourly Wage (1)	Employed (2)	Full-Time Ln Hourly Wage (3)	Age 30 + Ln Hourly Wage (4)	Ln Hourly Wage (5)	Employed (6)	Full-Time Ln Hourly Wage (7)	Age 30 + Ln Hourly Wage (8)
Panel A								
Age of youngest children (months)	**0.0082**	0.0013	**0.0049**	**0.0085**	0.0000	0.0010	**0.0038**	0.0026
	(0.0021)	(0.0010)	(0.0019)	(0.0031)	(0.0021)	(0.0011)	(0.0018)	(0.0028)
Panel B								
Age of youngest children (months)	**0.0111**	0.0018	**0.0088**	***0.0128***	0.0036	0.0034	***0.0079***	0.0073
	(0.0039)	(0.0025)	(0.0032)	(0.0060)	(0.0039)	(0.0025)	(0.0041)	(0.0082)
Youngest×(January–March)	−0.0014*	−0.0008	−0.0011*	−0.0001*	−0.0031	−0.0003*	−***0.0089***	−0.0078
	(0.0045)	(0.0028)	(0.0048)	(0.0092)	(0.0051)	(0.0030)	(0.0044)	(0.0117)
Youngest×(April–June)	0.0001*	−0.0021	−0.0040	−0.0080	−0.0057	−**0.0054**	−0.0077	0.00034
	(0.0073)	(0.0026)	(0.0048)	(0.0119)	(0.0047)	(0.0026)	(0.0046)	(0.0089)
Youngest×(July–September)	−***0.0091***	0.0009	−**0.0099**	−0.0084	−0.0053	−0.0039	−0.0003*	−0.0103
	(0.0047)	(0.0032)	(0.0049)	(0.0091)	(0.0051)	(0.0033)	(0.0047)	(0.0125)

Additional Controls								
Other educational variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State of residence	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region of residence × age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State of residence GDP & UER	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marital status	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth state cohort size	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region of birth × region of residence	No	No	No	No	No	No	No	No
Linear state of birth Specific cohort trends	No	No	No	No	No	No	No	No
Sample size	587,153	704,624	485,225	437,669	564,438	742,180	376,638	418,973

Note: The sample in Columns 1 and 5 includes individuals who are aged 23–45 in 2005–2007, are not in school or prison, work for wages (are not self-employed), and report positive income. The samples in Columns 2 and 4 remove the works for wages and positive income restrictions, and the remaining columns restrict the Column 1 and 5 samples to include only full-time employees or age 30+ individuals as defined. Full-time is defined as reporting at least $2,000 in annual salary income, 30 usual weekly hours of work, and 46 weeks of work. All models are population weighted and clustered at the state of birth level. All models include race, state of birth, age, state of birth-specific indicators for years surrounding cut off law changes, and census region of birth-specific cohort indicators. Other education controls include kindergarten subsidization, pupil teacher ratio, relative salary of teachers, and compulsory school leaving age. Heteroskedastic-consistent standard errors are in parentheses. Bold coefficients are statistically significant at the 5 percent level and bold italics are statistically significant at the 10 percent level. An asterisk in Panel B indicates youngest plus the specified interaction effect (i.e. $\delta_1 + \delta_2$, $\delta_1 + \delta_3$, or $\delta_1 + \delta_4$) is nonzero at the 10 percent level.

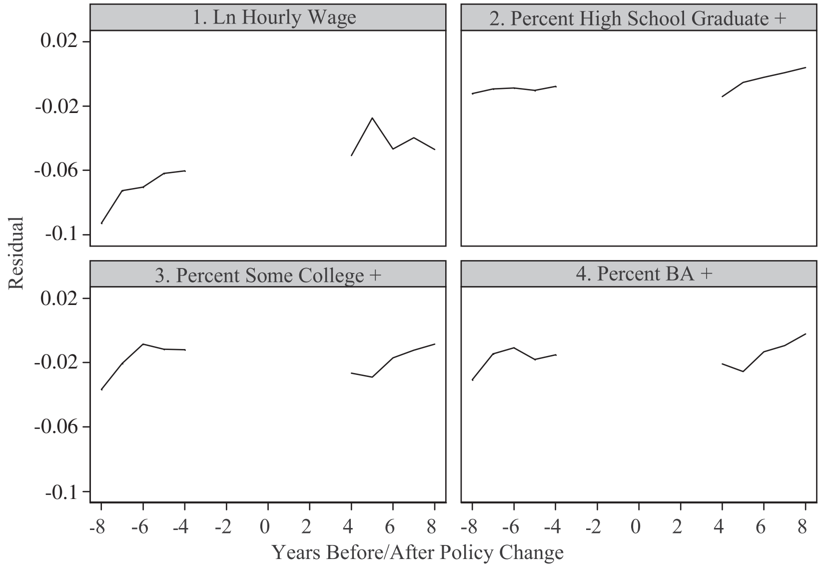

Figure 1
Pre- and Post-Entry Age Changes for Males

mind that we are examining the impact of changing the cutoff for the entire cohort rather the effect of individuals being relatively young or old within the cohort. As such, the results are not directly comparable.

Before focusing on the spillover effects, it is worth pausing to probe the robustness of estimates reported in Panel A.[28] While excluding states that do not experience a policy change from the sample changes the trend against which deviations are compared, it is worth checking that the results are not driven by these states. This seems particularly worth checking because we already exclude states in years in which school starting age is a local decision. These results are reported in Panel B. Although the magnitudes are not generally statistically different from those in Panel A, four of the five female point estimates become statistically insignificant. As alluded to above, this is a common theme—the male results are generally robust across samples and specifications but the female results are not. The next two panels reveal the root cause of the instability in the female point estimates.

Before probing the estimates further, it is worth pausing to display the results for states that experience a school-entry age policy change graphically. Figures 1 and 2

28. Appendix Table A5 reports the results for a placebo check of the randomization of the changes in the minimum school-entry age across demographic characteristics and education policy controls. There is little evidence that entry age law changes are correlated with the available controls. Only one coefficient is statistically significant at the five percent level—other race. This result reflects the fact that a few have unusually large increases in their Asian population that occurred roughly during the same period as changes in entry age laws.

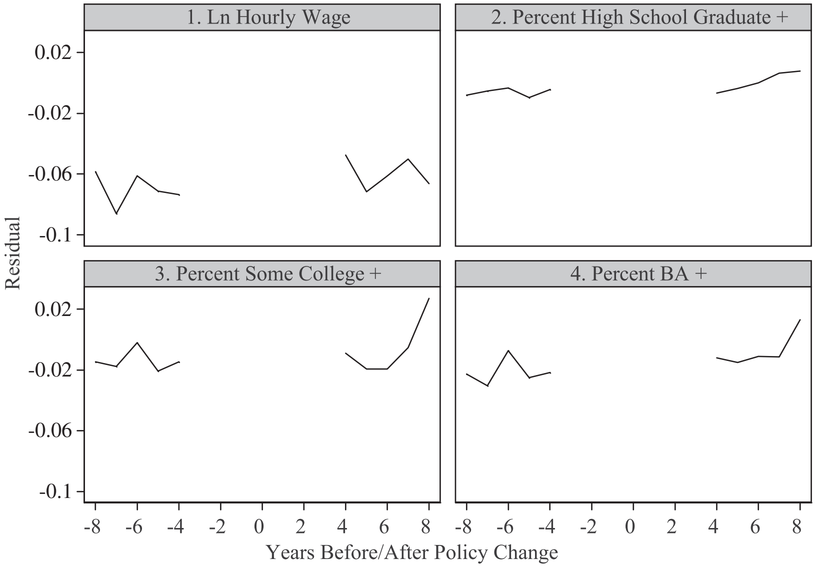

Figure 2
Pre- and Post-Entry Age Changes for Females

display the wage and education data for men and women, respectively. These figures plot the mean residuals before and after cutoff changes[29] from regressions that include only age controls.[30] A strong positive pattern emerges for ln hourly wages for males in Figure 1.1—average ln hourly wages are higher after the cutoff date changes. However, similar to the results reported in Tables 4 and 5, no other clear pattern emerges for education for either sex or for women's ln hourly wages.

Panel C in Table 5 restricts the sample to full-year/full-time workers defined as reporting employment of at least 30 usual hours per week, with at least 46 weeks of work, and $2000 in annual earnings. This sample is included to help shed light on the pattern of female point estimates reported in the remainder of the Tables 5 and 6. Once we focus on women who are firmly attached to the labor market, there are few differences between the male and female point estimates. The lone exception is the last specification, which falls to zero for men. In contrast, the point estimates for the subsample age 30 or more (Panel D) fall to zero for women but remain similar to the base specification for men. While this subsample has the benefit of ensuring that education is largely complete, it also puts more weight on women

29. We plot the residuals for four to eight years before the first cutoff change and four-eight years after the last cutoff change. The long gaps in the figures are necessary because many states slowly changed their cutoff dates over a period of several years (See Appendix Table A1).

30. Figures 1.1 and 2.1 only include individuals who are not in school, not in prison, work for wages (are not self-employed) and report positive income.

during child bearing/rearing years when labor market attachment is often weaker. The difference in male-female labor market attachment is easily seen by comparing sample sizes at the bottom of the table.

The point estimates for wages naturally lead one to wonder about the impact on labor force participation. Panel E examines this.[31] Whether we measure labor market participation as being in the labor force, being employed or being full-time/full-year we find that backing up the school starting age increases male labor market participation, but has no impact on women. More specifically, measured by in the labor force, the male point estimates range from a 0.11–0.12 percentage point increases in the labor force reports per month that the school starting age is backed up. As such, the positive male wage effects are not the result of driving men at the bottom of the earnings distribution out of the labor market. In contrast, there is no discernable labor force participation effect for women, regardless of the definition used. The sample sizes at the bottom of the table suggest that female selection into and out of the labor market is the most likely reason for this difference. More specifically, a large fraction of women are choosing to either stay out of the labor market or only partially participate at different ages and in different years (likely due to fertility and marriage) in large enough numbers and in heterogeneous enough ways that it is impossible to detect the small types of effect we are looking for.

Although the results reported in Table 5 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 members of the cohort as well? Our ability to examine this issue is limited by the relative scarcity of birth date data. However, beginning in 2005 the ACS reports quarter of birth. The 2005–2007 ACS data can therefore be used to examine, at least crudely, 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 people born between September 1–December 31 are directly affected by cutoff law changes (see Appendix Table A1). 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

31. Panel E also includes whether the individual is currently in school as an outcome variable. We find no effect of the minimum school starting age on this outcome.

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 2, the Quarter 1 estimates should be interpreted with care due to possible nonrandom changes in voluntary school entry. As the cutoff is backed up from December 31 toward 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 is essentially directly affected by the policy change. The Quarter 1 estimates cannot therefore be interpreted as purely indirect. This leaves us with Quarter 2. We can obtain a lower bound estimate of the cohort age effect using this subgroup. 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 individuals born in different birth quarters. More specifically, we add indicators for first, second, and third quarters of birth, and interact these indicators with the state of birth-specific age at which the youngest member of the cohort is eligible to enter school. We also interact quarter of birth with state of birth, region of birth-specific cohort indicators, state of birth-specific indicators for years surrounding cutoff changes.

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 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 effects of backing up the cutoff by one month (from the quarter of birth interacted model). For descriptive ease, denote the coefficient on S as δ_1 and the coefficients on S interacted with the birth Quarter 1, two, and three indicators as δ_2, δ_3, and δ_4, respectively for the quarter of birth interacted model. 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.[32]

We begin by focusing on men in Columns 1–4. To facilitate the presentation of results on a single page, all columns use the Column 3 specification from Tables 4 and 5, our preferred specification. Column 1 reports the results for ln hourly wages. The point estimates for youngest legal entry age are positive and statistically significant and the interaction terms for Quarters 1and 2 are 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 3 is always negative and statistically significant at conventional levels, and we cannot reject the null hypothesis that $\delta_1 + \delta_4 = 0$. The findings for Quarter 3 likely reflect the following factors. First, the majority of this group becomes the relatively youngest in most cases, which is a

32. For completeness, Appendix Table A6 reports the corresponding results for educational attainment for males and females, respectively.

negative effect. Second, similar to Quarter 4, the point estimate for Quarter 3 is a mixture of direct and indirect effects. As discussed above, the most interesting result reported in Column 1 is the finding that the point estimate for second quarter men is not statistically distinguishable from that of fourth quarter men. 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 reflects 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 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 2 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.

Columns 2–4 run several specification checks. The employment effects reported in Column 2 are smaller and less precise for the 2005–2007 ACS subsample. As a result, we cannot reject no effect for any birth quarter. In contrast, the wage effect estimates for the full-time/full-year subsample in Column 3 and the age 30 or over subsample in Column 4 are similar to the base specification for men. The one caveat is that the second quarter interaction is too large given the degree of precision to rule out a zero effect.

The results for the same specifications for women are reported in Columns 5–8. As in Table 5, the only nonzero point estimates are for full-time/full-year women in Column 7. In this case, the point estimate with no quarter of birth interactions (Panel A) is similar to that in Table 5, both in magnitude and in the fact that it is substantially smaller that its male counterpart. However, the patterns of results reported in the quarter of birth-specification (Panel B) differ somewhat from the male results. In particular, we cannot reject a zero result for either the first or the second quarter, but we can for the third quarter. We are not entirely sure what to make of the third-quarter results for women. While it is possible this is a real effect, it is also possible that it is an anomaly since the fourth quarter estimates in Columns 5 and 8 are much larger, but very imprecise.

The finding that those indirectly affected benefit from later school entry suggests that children benefit from having older peers in the classroom. However, very little literature exists regarding the effect of having older peers. Both Leuven and Rønning (2011) and Sandgren and Strøm (2005) find that students in Norway benefit from sharing the classroom with older peers. Leuven and Rønning (2011) conclude that the students in multigrade classrooms perform better than students in single-grade classrooms perform and attribute this to students benefiting from sharing the classroom with older peers. Sandgren and Strøm (2005) examine whether students with older peers achieve higher achievement levels in math and reading in fourth grade. They find a positive effect on achievement for male students but not for females. However, Argys and Rees (2008) find that females with older peers are more likely to use marijuana, alcohol, and tobacco versus females with younger peers but find no effect for males.

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.

VI. 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 for the estimated wage return. In particular, a one-month increase in the minimum school-entry age increases wages by about 0.6 percent for males. In addition, we report evidence showing that minimum entry age law changes have an 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—it likely uses fewer public funds than many other interventions (class size reductions, for example). This policy is also popular in an era of national testing because students in earlier cutoff states score higher. Despite the positive rhetoric, the optimal minimum entry cutoff remains unclear. While the estimates reported in this paper show that there are gains associated with backing up the cutoff from January to September, they do not tell us whether there would be gains or losses associated with backing it up farther.

The results reported in this paper also suggest that caution is required when interpreting results from models that use school starting policies to instrument for completed education because these policies have complex effects. We find no systematic relationship between changes in school starting age rules and completed education for males or females for any quarter of birth while at the same time finding adult wages effects. This leads us to interpret the results as coming through increased human capital accumulation within education categories. The finding of spillover effects on quarters of birth not directly impacted by policy changes is also important in this regard because positive human capital effects for these groups do not reflect changes in the timing of school entry.

Appendix Table A1
School-entry Cutoff Dates (School Years 1964–1988)

	1964	1965	1966	1967	1968	1969	1970	1971	1972	1973	1974	1975
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
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
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
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
CO	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
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
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
GA	none	none	none	none	none	none	none	none	none	none	none	none
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
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
IN	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
KS	ssy	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
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
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
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
MA	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
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
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
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

MT	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy
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
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
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
NJ	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
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
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
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
OH	none	10.31	10.31	10.31	10.31	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
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
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
RI	none	none	none	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
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
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
TX	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
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
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
WA	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy	ssy
WV	none	none	none	none	none	none	none	none	11.1	11.1	11.1	11.1
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
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

Note: 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 cutoff dates were collected from state statutes and corresponding historical state session laws and regulations. See Appendix Tables A2 and A3 for more details.

Appendix Table A1 *(continued)*
School-entry Cutoff Dates (School Years 1964–1988)

	1976	1977	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988
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
AZ	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
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
CA	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.2	12.2
CO	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
DE	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	12.1	11.1	10.1	9.1	9.1	9.1	9.1	9.1	9.1
GA	none	none	none	none	none	none	none	none	none	9.1	9.1	9.1	9.1
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
IL	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	12.1	11.1	10.1	9.1
IN	none	none	none	none	none	none	none	none	none	none	none	none	none
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
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
KY	12.31	12.31	12.31	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	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
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
MA	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
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
MS	1.1	12.1	11.1	10.1	9.1	9.1	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	9.1	8.1	7.1

MT	ssy	ssy	ssy	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
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
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
NJ	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
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
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
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
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
OK	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
OR	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
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
SC	none	none	11.1	11.1	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	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
TX	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	9.2
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
VA	12.31	12.31	12.31	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA	LEA
WA	ssy	8.31	8.31	8.31	8.31	8.31	8.31	8.31	8.31	8.31	8.31	8.31	8.31
WV	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
WI	12.1	12.1	12.1	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

Note: 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 cutoff dates were collected from state statutes and corresponding historical state session laws and regulations. See Appendix Tables A2 and A3 for more details.

Appendix Table A2
State Statutes Regarding School Entry

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

Note: Cutoff date changes in Appendix A1 were compiled from the state statutes listed above along with information collected from corresponding historical state session laws and regulations relating to the statute.

Appendix Table A3
State Statutes Regarding School Entry (School Years 1964–1988)

State	1964		Cutoff Date Change		
	Cutoff Date	Citation	Year of Change	New Cutoff Date	Citation
AL	10.1	Acts 1950, 2nd Ex. Sess. No. 4, p. 24, §1	none		
AZ	1.1	Laws 1960, Ch. 127, §§17, 18	1978–82	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,	none		§123-20-5
CT	1.1	CT statute: Title 10, Chapter 164,	none		Sec. 10-15 1959
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	1980–83	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
ID	10.16	1963 ch. 13 §24, p.27	none		
IL	12.1	Laws 1961, p. 31 § 10-20.12	1986–88	11.1, 10.1, 9.1	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

(continued)

Appendix Table A3 *(continued)*

	1964		Cutoff Date Change		
State	Cutoff Date	Citation	Year of Change	New Cutoff Date	Citation
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	1977–80	12.1, 11.1, 10.1, 9.1	Laws 1976, Cd. 390, § 1
MO	10.1	L. 1963, p. 200, §1-5	1986–88	9.1. 8.1, 7.1	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,	none		1 Educ. Dept. Rep. 775.
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	1975–76	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 & 1967–68 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	1986	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	1966–68	11.30, 10.31, 9.30	Acts 1965, ch. 303 §§ 1,2
TX	ssy	Acts 1961, 57th Leg., 1st C.S.,	none		p.132, ch. 29, sec. 1
UT	ssy	U.C.A. 1953 §53A-3-402	1988	9.2	Laws 1988, c. 2, §56
VT	1.1	1921, No. 51. G.L. §1243 &	none		Amended 1971, No. 243 (Adj. Sess,), §1
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	8.31	1977 ex.s. c 369 §14 & WAC 392-335-025
WV	none	1959, c. 53	1972 & 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	
WY	9.15	Laws 1955, ch. 192, § 1	none		

Appendix Table A4

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

	Males		Females	
	(1)	(2)	(3)	(4)
Age of youngest children (months)	**−0.0585**	**−0.1323**	**−0.0469**	**−0.1417**
	(0.0080)	(0.0235)	(0.0110)	(0.0213)
Youngest × (January–March)		**0.1411**		**0.1363**
		(0.0204)		(0.0204)
Youngest × (April–June)		**0.1287**		**0.1386**
		(0.0243)		(0.0204)
Youngest × (July–September)		**0.1134***		**0.1240***
		(0.0311)		(0.0236)
Black interactions				
Age of youngest children × Black	−0.0030*	0.0009*	0.0013*	0.0027*
	(0.0044)	(0.0053)	(0.0062)	(0.0050)
Youngest × (January–March) × Black		0.0005		**0.0011**
		(0.0003)		(0.0003)
Youngest × (April–June) × Black		**−0.0023**		**−0.0018**
		(0.0003)		(0.0002)
Youngest × (July–September) × Black		**−0.0023***		**−0.0015***
		(0.0003)		(0.0002)
Hispanic interactions				
Age of youngest children × Hispanic	**0.0003***	**0.0011***	−0.0001*	0.0002*
	(0.0001)	(0.0003)	(0.0002)	(0.0004)
Youngest × (January–March) × Hispanic		0.0002		0.0005
		(0.0004)		(0.0003)
Youngest × (April–June) × Hispanic		**−0.0016**		**−0.0012**
		(0.0005)		(0.0004)
Youngest × (July–September) × Hispanic		**−0.0012***		**−0.0012***
		(0.0004)		(0.0004)
Other race interactions				
Age of youngest children × other race	0.0050*	0.0082*	0.0061*	**0.0104***
	(0.0101)	(0.0116)	(0.0064)	(0.0049)
Youngest × (January–March) × other race		−0.0005		−0.0006
		(0.0005)		(0.0004)
Youngest × (April–June) × other race		**−0.0024**		**−0.0014**
		(0.0004)		(0.0004)
Youngest × (July–September) × other race		**−0.0018**		**−0.0019**
		(0.0004)		(0.0005)
Sample size	102,958	102,958	99,534	99,534

Note: The dependent variable is one if the individual is enrolled in Grade 1 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 coefficients are significant at the 5 percent level. Columns 1 and 3 include indicators for the existence of publicly 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 six-year-olds from the 1960, 1970, and 1980 U.S. Censuses. An asterisk indicates that youngest plus the specified interaction effects are nonzero at the 10 percent level.

Appendix Table A5
Randomization Across Background Demographics and Education Policy Controls

Female	−0.0005
	(0.0006)
Black	−0.0014
	(0.0013)
Hispanic	−0.0041
	(0.0027)
Other race	**−0.0019**
	(0.0009)
Kindergarten subsidization	0.0149
	(0.0262)
Pupil teacher ratio	−0.0842
	(0.0881)
Relative salary of teachers	***0.0035***
	(0.0020)
Compulsory school leaving age	−0.0076
	(0.0606)
Sample size	4,204,755

Note: The sample includes males and females aged 23–45 in 2000–2007. All models are population weighted and clustered at the state of birth level. All models include state of birth, age, state of birth-specific indicators for years surrounding cutoff law changes, and census region of birth-specific cohort indicators. Heteroskedastic-consistent standard errors in parentheses. Bold coefficients are statistically significant at the 5 percent level and bold italics are statistically significant at the 10 percent level.

Appendix Table A6
The Impact of the Minimum School Starting Age on Educational Attainment by Birth Quarter

	Males			Females		
	HS grad or higher (1)	Some college or higher (2)	BA or higher (3)	HS grad or higher (4)	Some college or higher (5)	BA or higher (6)
Panel A						
Age of youngest children (months)	0.0000	0.0008	−0.0001	−0.0011	−0.0002	0.0002
	(0.0013)	(0.0016)	(0.0013)	(0.0010)	(0.0012)	(0.0010)
Panel B						
Age of youngest children (months)	−0.0013	−0.0020	0.0005	***−0.0020***	−0.0027	−0.0001
	(0.0021)	(0.0025)	(0.0020)	(0.0012)	(0.0025)	(0.0022)
Youngest×(January–March)	***0.0034***	0.0019	−0.0011	0.0003	**0.0079***	0.0024
	(0.0018)	(0.0029)	(0.0025)	(0.0016)	(0.0036)	(0.0028)
Youngest×(April–June)	0.0022	0.0046	−0.0004	0.0019	−0.0002	−0.0004
	(0.0014)	(0.0028)	(0.0028)	(0.0021)	(0.0031)	(0.0024)
Youngest×(July–September)	−0.0003	0.0045	−0.0012	0.0009	0.0019	−0.0004
	(0.0019)	(0.0040)	(0.0025)	(0.0018)	(0.0031)	(0.0027)

Additional Controls						
Other educational variables	Yes	Yes	Yes	Yes	Yes	Yes
State of residence	Yes	Yes	Yes	Yes	Yes	Yes
Region of residence × age	Yes	Yes	Yes	Yes	Yes	Yes
State of residence GDP & UER	Yes	Yes	Yes	Yes	Yes	Yes
Marital status	Yes	Yes	Yes	Yes	Yes	Yes
Birth state cohort size	Yes	Yes	Yes	Yes	Yes	Yes
Region of birth × region of residence	No	No	No	No	No	No
Linear state of birth Specific cohort trends	No	No	No	No	No	No

Note: The sample includes males aged 23–45 in 2005–2007. All models are population weighted and clustered at the state of birth level. All models include race, state of birth, age, state of birth-specific indicators for years surrounding cutoff law changes, and census region of birth-specific cohort indicators. Other education controls include kindergarten subsidization, pupil teacher ratio, relative salary of teachers, and compulsory school leaving age. Heteroskedastic-consistent standard errors in parentheses. Bold coefficients are statistically significant at the 5 percent level and bold italics are statistically significant at the 10 percent level. An asterisk in Panel B indicates youngest plus the specified interaction effect (i.e., $\delta_1 + \delta_2$, $\delta_1 + \delta_3$, or $\delta_1 + \delta_4$) is nonzero at the 10 percent level.

References

Angrist, Joshua, and Alan Krueger. 1991. "Does Compulsory School Attendance Affect Schooling and Earnings?" *Quarterly Journal of Economics* 106(4):979–1014.

——— 1992. "The Effect of Age at School Entry on Educational Attainment: An Application of Instrumental Variables with Moments from Two Samples." *Journal of the American Statistical Association* 87(418):328–36.

Argys, Laura M., and Daniel I. Rees. 2008. "Searching for Peer Group Effects: A Test of the Contagion Hypothesis." *Review of Economics and Statistics* 90(3):442–58.

Bedard, Kelly, and Elizabeth Dhuey. 2006. "The Persistence of Early Childhood Maturity: International Evidence of Long-Run Age Effects." *Quarterly Journal of Economics* 121(4):1437–72.

Black, Sandra, Paul Devereux, and Kjell Salvanes. 2011. "Too Young to Leave the Nest? The Effects of School Starting Age." *Review of Economics and Statistics* 93(2):455–67.

Bound, John, David A. Jaeger, and Regina Baker. 1995. "Problems with Instrumental Variables Estimation When the Correlation Between the Instrument and the Endogenous Explanatory Variable Is Weak." *Journal of the American Statistical Association* 90(430): 443–50.

Bound, John, and David A. Jaeger. 2000. "Do Compulsory School Attendance Laws Alone Explain the Association Between Quarter of Birth and Earnings?" *Research in Labor Economics* 19:83–108.

Cascio, Elizabeth, and Ethan Lewis. 2006. "Schooling and the Armed Forces Qualifying Test: Evidence from School Entry Laws." *Journal of Human Resources* 41(2):294–318.

Cannon, Jill, and Stephen Lipscomb. 2008. "Changing the Kindergarten Cutoff Date: Effects on California Students and Schools." Public Policy Institute of California Occasional Paper.

Clark, Damon, and Heather Royer. 2010. "The Effect of Education on Adult Health and Mortality: Evidence from Britain." NBER Working Paper 16013.

Crawford, Claire, Lorraine Dearden, and Costas Meghir. 2007. "When You Are Born Matters: The Impact of Date of Birth on Child Cognitive Outcomes in England." Institute for Fiscal Studies Research Paper.

Datar, Ashlesha. 2006. "Does Delaying Kindergarten Entrance Give Children a Head Start?" *Economics of Education Review* 25(1):43–62.

Dhuey, Elizabeth. 2011. "Who Benefits from Kindergarten? Evidence from the Introduction of State Subsidization." *Educational Evaluation and Policy Analysis* 33(1):3–22.

Dhuey, Elizabeth, and Stephen Lipscomb. 2008. "What Makes A Leader? Relative Age and High School Leadership." *Economics of Education Review* 27(2):173–83.

Dobkin, Carlos, and Fernando Ferreira. 2010. "Do School Entry Laws Affect Educational Attainment and Labor Market Outcomes." *Economics of Education Review* 29(1):40–54.

Education Research Service. 1975. "Kindergarten and First Grade Minimum Entrance Age Policies." *ERS Informant* 1–38.

Elder, Todd, and Darren Lubotsky. 2009. "Kindergarten Entrance Age and Children's Achievement: Impacts of State Policies, Family Background, and Peers." *Journal of Human Resources* 44(3):641–83.

Fertig, Michael, and Jochen Kluve. 2005. "The Effect of Age at School Entry on Educational Attainment in Germany." IZA Discussion Paper Number 1507.

Fredriksson, Peter, and Björn Öckert. 2008. "The Effect of School Starting Age on School and Labor Market Performance." Stockholm University Working Paper.

Heckman, James, Anne Layne-Farrar, and Petra Todd. 1996. "Does Measured School Quality ReallyMatter? An Examination of the Earnings-Quality Relationship." In *Does*

Money Matter? The Effect of School Resources on Student Achievement and Success, ed. Gary Burtless, 192–289. Washington, D.C.: Brookings Institution Press.

Kawaguchi, Daiji. 2011. "Actual Age at School Entry, Educational Outcomes, and Earnings." *Journal of the Japanese and International Economies* 25(2):64–80.

Lleras-Muney, Adriana. 2005. "The Relationship Between Education and Adult Mortality in the U.S." *Review of Economic Studies* 72(1):189–221.

Leuven, Edwin, and Marte Rønning. 2011. "Classroom Grade Composition and Pupil Achievement." École Nationale de la Statistique et de l'Administration Économique Working Paper.

McCrary, Justin, and Heather Royer. 2011. "The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Policies Using Exact Date of Birth." *American Economic Review* 101(1):158–95.

National Center for Education Statistics. 1972. *Digest of Education Statistics*. Washington, D.C.: U.S. Department of Health, Education, and Welfare, Office of Education.

——— 1973. *Digest of Education Statistics*. Washington, D.C.: U.S. Department of Health, Education, and Welfare, Office of Education.

——— 1983. *Digest of Education Statistics*. Washington, D.C.: U.S. Department of Health, Education, and Welfare, Office of Education.

Oreopoulos, Philip. 2006. "Estimating Average and Local Average Treatment Effects of Education when Compulsory Schooling Laws Really Matter." *American Economic Review* 96(1):152–75.

Oreopoulos, Philip, Marianne Page, and Ann Stevens. 2006. "Does Human Capital Transfer from Parent to Child? The Intergenerational Effects of Compulsory Schooling." *Journal of Labor Economics* 24(4):729–60.

Puhani, Patrick, and Andrea Weber. 2007. "Does the Early Bird Catch the Worm? Instrumental Variable Estimates of Early Educational Effects of Age of School Entry in Germany." *Empirical Economics* 32(2):359–86.

Sandgren, Sofia, and Bjarne Strøm. 2005. "Peer Effects in Primary School: Evidence from Age Variation." Norwegian University of Science and Technology Working Paper.

Smith, Justin. 2009. "Can Regression Discontinuity Help Answer and Age-old Question in Education? The Effect of Age on Elementary and Secondary School Outcomes." *The B.E. Journal of Economic Analysis and Policy (Topics)* 9(1): Article 48.

Stipek, Deborah. 2002. "At What Age Should Children Enter Kindergarten? A Question for Policymakers and Parents." *Social Policy Report: Giving Child and Youth Development Knowledge Away* 16(2):3–17.