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The effect of school starting age policy on crime: Evidence from U.S. microdata[☆]

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ABSTRACT

Does school starting age policy have an impact on the propensity of individuals to commit crime as adults? Using microdata from the U.S. Census, we find that a higher school starting age cutoff leads to lower rates of incarceration among both those directly affected by the laws and those only indirectly affected. However, the reduction in incarceration among those directly affected is smaller in magnitude, implying that the delay itself was harmful with respect to crime outcomes. These findings provide further support for early childhood interventions influencing future criminal activity.

1. Introduction

Does school starting age (SSA) policy have an impact on the propensity of individuals to commit crime as adults? States set regulations on the minimum age at which a child can be enrolled in the public school system. The legislation specifies a day before which an individual must be born in order to enter school in a given year. Moving the cutoff earlier in the year (increasing the cutoff) forces the youngest individuals within a cohort to delay their school entry to the following year. This can influence their skill accumulation since they will be older upon school entry, and older children may be more mature, cognitively developed, and ready for instruction; on the other hand, it may crowd out time spent in school or labor market experience. School starting age policy may also affect children for whom the laws were not binding. Cohorts that are older on average may suffer from fewer behavioral disruptions and positive peer effects from higher average cognitive development than their younger counterparts.[1] Changing the age distribution that a given individual faces within the classroom may also change the nature of his or her social interactions, for instance from greater exposure to those who are older or younger.

There is little existing evidence on the relationship between school starting age and later criminal activity.[2] A potentially important mechanism through which school starting age is expected to operate is through human capital accumulation, via the standard economic model of crime (Becker, 1968; Lochner, 2004). The vast literature on school starting age, though, does not give a clear indication that

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1 We refer throughout the paper to the average age within a cohort, though clearly other aspects of the age distribution may be equally or more important.

2 Two working papers contemporaneous with ours (Cook & Kang, 2013; Landersø, Nielsen, & Simonsen, 2013) examine the impact of school starting age on crime using a regression discontinuity framework. By design, this can only isolate the impact on those students who were directly induced to delay school entry. We find that the indirect effect of the law is even larger than the direct effect.

the laws influence educational attainment or labor market outcomes. However, a number of prominent early childhood educational interventions show a sizable reduction in crime despite little impact on labor market outcomes.[3] This has motivated some recent research on the importance of non-cognitive factors in explaining adult criminal activity (Agan, 2011; Heckman, Pinto, & Savelyev, 2013). Relatedly, emerging evidence suggests that peer interactions within the classroom, including exposure to older average cohorts or distinct peer networks, may be an important factor in the determination of criminal activity.[4] While the Becker model of crime has received extensive support in the empirical literature, far less work has explored the importance of these alternative channels, a gap that this paper addresses.

We first demonstrate that those with birthdays near the school starting age cutoff do delay their entry into school in response to changes to the cutoff, while those born far away from the cutoff do not. In a difference-in-differences (DD) model with data from the 1960 and 1970 U.S. Censuses, we show that changes in the SSA cutoff lead to a delay in entry into public kindergarten and first grade among those with third and fourth quarter birthdays. A three-month change in the cutoff, from January 1 to October 1, is predicted to reduce kindergarten attendance among whites with a fourth quarter birth by 81 percent, and blacks with a fourth quarter birth by 54 percent. Children whose eligibility was unaffected by the law changes, those born in the second quarter of the year, do not alter their school entry decision. These children, therefore, satisfy the key identifying assumption necessary for separating out the direct and indirect effects of the laws.

Then, in a difference-in-differences framework using individual-level data from the 1970 and 1980 U.S. Censuses, we estimate the impact of increasing the minimum school entry age on incarceration for males who were directly and indirectly affected by the law. Among directly affected white males, a one-month increase in SSA leads to a 0.06 percentage point reduction in the annual probability of incarceration, which is a 10 percent reduction relative to the sample mean. Among those for whom the laws were not binding, we find that a one-month increase in the school starting age cutoff leads to a 0.08 percentage point, or 13 percent, reduction in incarceration among white males. This means that individuals tend to commit less crime when they are exposed to an older average cohort while in school, consistent with fewer disruptions, positive peer effects, or improved social interactions.

Both directly and indirectly affected individuals benefit with respect to crime outcomes from a higher minimum starting age. But the gap between the laws' effect on those two groups implies that those who delayed their school entry were harmed by the delay itself (i.e., the reduction in incarceration was smaller than it otherwise would have been).[5] This finding is consistent either with children learning less in their redshirt year compared to those enrolling on time, experiencing less within-grade learning throughout school, or with late entry crowding out final educational attainment. It may appear surprising that the majority of the reduction in crime comes through the indirect effect of the laws. But any benefits from an older average cohort accrue over an individual's entire time spent in elementary and secondary school, and so could be substantial. We do not find an impact of school starting age policy on black males, though the estimates are not precise enough to draw strong inferences. Using a novel measure of sentence length from standard Census microdata, we also show that school starting age policy affects the incidence of both short- and long-sentence crimes.

This paper adds to the growing literature on the role that education policy, including early childhood interventions, has in shaping criminal activity.[6] The large social costs associated with crime make it an important outcome of study. For example, state and local expenditures in the U.S. on policing and corrections were $170 billion in 2011 (U.S. Census Bureau, 2011). Given that many of the social costs to crime are external, our findings provide further support for one of the canonical justifications—the existence of positive externalities—for providing public support for education. The minimum school starting age is an attractive policy tool since many of the other early education programs known to be effective for crime, like publically subsidized kindergarten and Head Start, have already been implemented. Little empirical evidence exists to address the importance of peer effects and social interactions on criminal behavior, and our findings offer some suggestive support that these mechanisms are important.

The remainder of the paper is organized as follows. Section 2 discusses the mechanisms through which school starting age policy may affect crime, and reviews the relevant literature. Section 3 reports estimates of the impact of school starting age policy on school entry behavior, and Section 4 on its effect on incarceration. Section 5 concludes.

2. School starting age and crime

School starting age laws specify the minimum age a child must be to enroll in public school. The laws typically consist of a birthday cutoff—a date by which the child must have reached a certain age in order to be eligible to enroll in kindergarten or first grade in a given year. For example, in a state with a school starting age cutoff of December 1, a student would have to reach age 5 before December 1 in the year in which he or she were to enroll in kindergarten. This corresponds to a minimum school entry age of 4.75 years (or 57 months), when the school year commences on September 1. In the U.S., these cutoffs usually fall sometime in the September to January months, although several occur earlier or later. Codifying SSA cutoffs into state law became more prevalent in the years following World War II in an effort to provide more uniformity across localities in the ages of students within a given cohort. These regulations only mandate a *minimum* age at which a child can be enrolled. States also set a mandatory entry age, which is not until 6, 7, or 8, which

[3] Consider, for example, programs like the Perry PreSchool Project (Heckman, Moon, Pinto, Savelyev, & Yavitz, 2010), Head Start (Garces, Thomas, & Currie 2002), and publically subsidized kindergarten (Cascio 2009).

[4] See Glaeser, Sacerdote, and Scheinkman (1996) on social interactions, and Bayer, Hjalmarsson, and Pozen (2009) on peer effects.

[5] The gap is the difference-in-difference-in-differences (DDD) parameter, measuring the change in incarceration among the directly affected in response to changes in the SSA cutoff *relative* to the change among the indirectly affected.

[6] See Lochner (2011) for a survey.

gives parents some latitude in deciding when to enroll their child.

Through what mechanisms will school starting age policy affect individuals' human capital accumulation? Children who are induced to delay entry in response to the law may enter school more mature, knowledgeable, ready to learn, or willing to follow directions.[7] However, early differences in achievement may either fade as students progress through school, or late entry may crowd out later educational attainment. Empirical evidence suggests that students entering school older due to school starting age cutoffs tend to perform better on standardized tests during kindergarten and primary school (Datar, 2004; McEwan & Shapiro, 2008), have lower rates of grade repetition (Dobkin & Ferreira, 2010), and incur fewer diagnoses of learning disabilities (Elder & Lubotsky, 2009). However, the impact of school starting age on educational attainment is mixed, with some studies suggesting crowd out (Angrist & Krueger, 1992) and some finding no effect (Dobkin & Ferreira, 2010; McCrary & Royer, 2011). Bedard and Dhuey (2012) explain their positive effect of school starting age on adult wages, despite no impact on years of schooling, in terms of greater within-grade skill accumulation. This explanation is consistent with the other studies finding improved test scores for older entrants.

There also may be spillovers on those who are not induced to change the timing of their entry. A cohort that is older on average may benefit from fewer disruptions, leading to greater learning per year of schooling (Lazear, 2001). Elder and Lubotsky (2009) find that exposure to an older average cohort improves test scores, consistent with positive peer effects from the average student being more mature. It is expected that these types of effects will be persistent, since a cohort which is older on average in one grade will likewise be older for later grades as well. Student outcomes may also be affected by where an individual falls within their cohort's age distribution. For example, Elder and Lubotsky (2009) document that those who are relatively young are more likely to repeat a grade and be diagnosed with ADHD, presumably because they appear to teachers to lag behind their peers developmentally. Bedard and Dhuey (2006) find that the relatively young have a lower trajectory of test scores and are less likely to attend college. Note that indirectly affected individuals experience both an older average cohort as well as a change in their relative position within their cohort, and so it is not possible to disentangle the two effects. But, distinguishing between the direct and indirect effects of school starting age policy is undertaken in the empirical section, which is important because it provides evidence that a particular type of peer effect matters for students' academic achievement.

Human capital accumulation—either through changing educational attainment or the within-grade rate of accumulation—is one channel through which we expect school starting age to operate on later crime. The standard economic theory of crime predicts that individuals with higher levels of human capital will commit fewer crimes either due to better licit opportunities relative to illicit ones (Becker, 1968; Lochner, 2004). Relatedly, if school starting age policy affects time spent in school, then it may also affect criminal activity through an incapacitation effect (Jacob & Lefgren, 2003; Landersø, Nielsen, & Simonsen, 2013). However, several other channels may be operating beyond the human capital channels. First, school starting age policy may also affect one's social network by changing the composition of peers to which they are exposed. Bayer, Hjalmarsson, and Pozen (2009) find not only that social networks matter for crime, but also the age distribution of one's peers. This is relevant because our policy variation causes changes in the age distribution within schooling cohorts. Second, differences in the classroom age distribution may affect the accumulation of non-cognitive skills, which consist of personality traits like respect for authority, socialization, aggression, and impulse control. Black, Devereux, and Salvanes (2011), for example, find that a higher age-at-entry is associated with better mental health, which is likely to be correlated with these types of non-cognitive skills. Agan (2011) and Heckman, Pinto, and Savelyev (2013) show that the development of non-cognitive skills is important for explaining criminal activity later in life.

This paper makes contributions toward understanding school starting age laws' impact on both school enrollment decisions as well as long-term incarceration outcomes. Regarding school enrollment, both Dobkin and Ferreira (2010) and Bedard and Dhuey (2012) find that SSA laws do induce changes in the timing of school entry, but our results differ in several respects. Dobkin and Ferreira (2010) analyze Census data from California and Texas in 2000. It is unclear how these results would generalize to cohorts who were born decades earlier, when the outside option facing children was substantially different. Moreover, they do not test whether children substituted toward private schools (which are generally not bound by SSA cutoffs) in response to becoming ineligible for public school entry. Such a shift would mitigate the long-term impact of school starting age regulations, and so it is important to assess its importance in the data. Bedard and Dhuey (2012) use Census data from 1960, 1970, and 1980, but only look at first grade enrollment and not kindergarten. It is possible that demand for kindergarten may also fall in response to an increase in the SSA cutoff—for instance, parents may forego kindergarten instead of delaying it a year if they deem the benefits small enough. If attending kindergarten has any positive impact on long-run outcomes, then it will be important to estimate the impact of SSA on kindergarten attendance in order to understand any long-run impact of the legislation. Despite these differences, though, our results are similar to both papers' in that we find that the laws induce sizable changes in the timing of school entry.

Only two other studies directly examine the impact of school starting age policy on crime. Both use individuals' exact date of birth and exploit the sharp change in school entry eligibility around SSA cutoffs in a regression discontinuity design. Using administrative data from North Carolina, Cook and Kang (2013) find that those born just after the school starting age cutoff are more likely to drop out of high school

[7] The causal effect of delayed entry will depend on the relative rate of accumulation in formal schooling as compared to other forms of childcare like preschool or a parent's care, as well as the availability of public kindergarten and Head Start. Existing evidence suggests that the rate of learning in formal schooling is about twice the rate of informal arrangements (Fitzpatrick, Grissmer, & Hastedt, 2011). This figure is derived from comparing test score growth over the course of a school year with its growth over the summer months.

before graduation and are 0.9 percentage points more likely to have a felony conviction at age 19, compared to those born just before the cutoff. With administrative data, Landersø, Nielsen, and Simonsen (2013) provide evidence that a higher school starting age leads to fewer convictions among Danish teenagers. They find that the reduction in crime among boys occurs primarily for those who are of school age and for crimes committed during the week, consistent with school attendance acting as an incapacitation mechanism. The regression discontinuity framework used in these and other SSA papers offers a clean, quasi-experimental estimate of the laws' impact on those who are directly induced to delay school entry, and provides confirmatory evidence that school starting age policy matters for crime. Nevertheless, the approach is not well-suited to estimating the laws' impact on indirectly affected individuals, nor in separating the direct effect of late entry and the indirect effect of exposure to an older average cohort throughout primary and secondary school.

3. The effect of school starting age policy on school attendance

An increase in the school starting age cutoff will push children who are no longer eligible to enroll in school in a given year to delay their entry until the following year. This proposition is tested empirically using data on school attendance, school starting age, and birthday timing. Beyond verifying that the laws had their intended effect, we will investigate two forms of strategic behavior by parents. First, since school starting age policy only affects public school enrollment, parents may substitute into private school when the law makes their children ineligible for public school. Second, parents may delay the entry of an eligible child in order to ensure their child is relatively old within his or her school cohort. Understanding the change in attendance patterns in response to changes in the law will help in interpreting the reduced form effect of the laws on crime. Depending on the behavior of parents, the "treatment" of being forced to delay school entry will either consist of children staying at home for a year, attending private school, or some other arrangement.

3.1. Data

Data on school attendance come from the Public-Use Micro Sample (PUMS) of the 1960 and 1970 U.S. Censuses (Ruggles et al., 2008). Both are flat, one-percent samples. The 1960 and 1970 Censuses are chosen so as to roughly align the birth cohorts within the school attendance and the crime samples. The sample includes white and black children between the age of 5 (with a quarter 4 birth) and 7 (with a quarter 1 birth).[8] The sample is then divided into children who are of approximately kindergarten age (5 and 6) and of first grade age (6 and 7). A child is considered to be in attendance at school if he or she attended school at any point between February 1 and April 1. School attendance can be broken down further by whether the student attended a public or private/parochial institution. School starting age cutoffs are derived from historical state statutes.[9] We convert those cutoffs to the minimum age (in months) that a child would need to be in order to enroll in kindergarten. To abstract from one or two day changes, SSA cutoffs are rounded to the nearest half-month. States with no SSA cutoff over the entire sample period are excluded, leaving 41 states in the sample. Fig. 1 graphs the minimum entry age over time, and Table 1 tabulates the changes which occur within the sample.

Table 1
States changing their school starting age cutoff, 1945–1969.

State	Year	Cutoff change (in months)
School attendance sample		
Delaware	1969	60–56
Florida	1966	57–56
Iowa	1962	57.5–58.5
Iowa	1963	58.5–59.5
Kansas	1966	56–57
Kansas	1967	57–58
Kansas	1968	58–59
Kansas	1969	59–60
Ohio	1969	58–59
Tennessee	1966	56–57
Tennessee	1967	57–58
Tennessee	1968	58–59
Crime sample		
Alabama	1948	59–55.5
Alabama	1951	55.5–59
Alaska	1947	55–58
California	1951	54–57
Florida	1966	57–56
Iowa	1962	57.5–58.5
Iowa	1963	58.5–59.5
Kansas	1966	56–57
Kansas	1967	57–58
Kentucky	1947	59–56
Nebraska	1950	60–58.5
North Dakota	1959	56–58
South Dakota	1956	60–58
Tennessee	1966	56–57
Tennessee	1967	57–58

Notes: Changes do not include states instituting an SSA cutoff. 56 months corresponds to a January 1 cutoff, 59 months corresponds to an October 1 cutoff.

In the empirical analysis, we control for several measures of family background (family income quartile, the household head's educational attainment), other education policies (the presence of publically subsidized kindergarten, the mandatory enrollment age), and measures of school quality (education expenditure per capita, student–teacher ratio). The mandatory entry age comes from Acemoglu and Angrist (2001) and the timing of public kindergarten subsidization is from Dhuey (2011), Cascio (2009), and Hunt (1969). Average per pupil expenditure and the student–teacher ratio

[8] The data only contain information on the quarter of birth. Ideally, we would have access to individuals' day of birth and so could observe heterogeneity in response to SSA across the calendar year. This would allow us to observe redshirting, for example, and see whether those with a birthday just before the cutoff are more likely to delay their school entry. Unfortunately, our Census data only include information on respondents' quarter of birth, and so we can only observe individuals' response to the laws at this coarser level.

[9] Further information about the statutes appears in Appendix A.

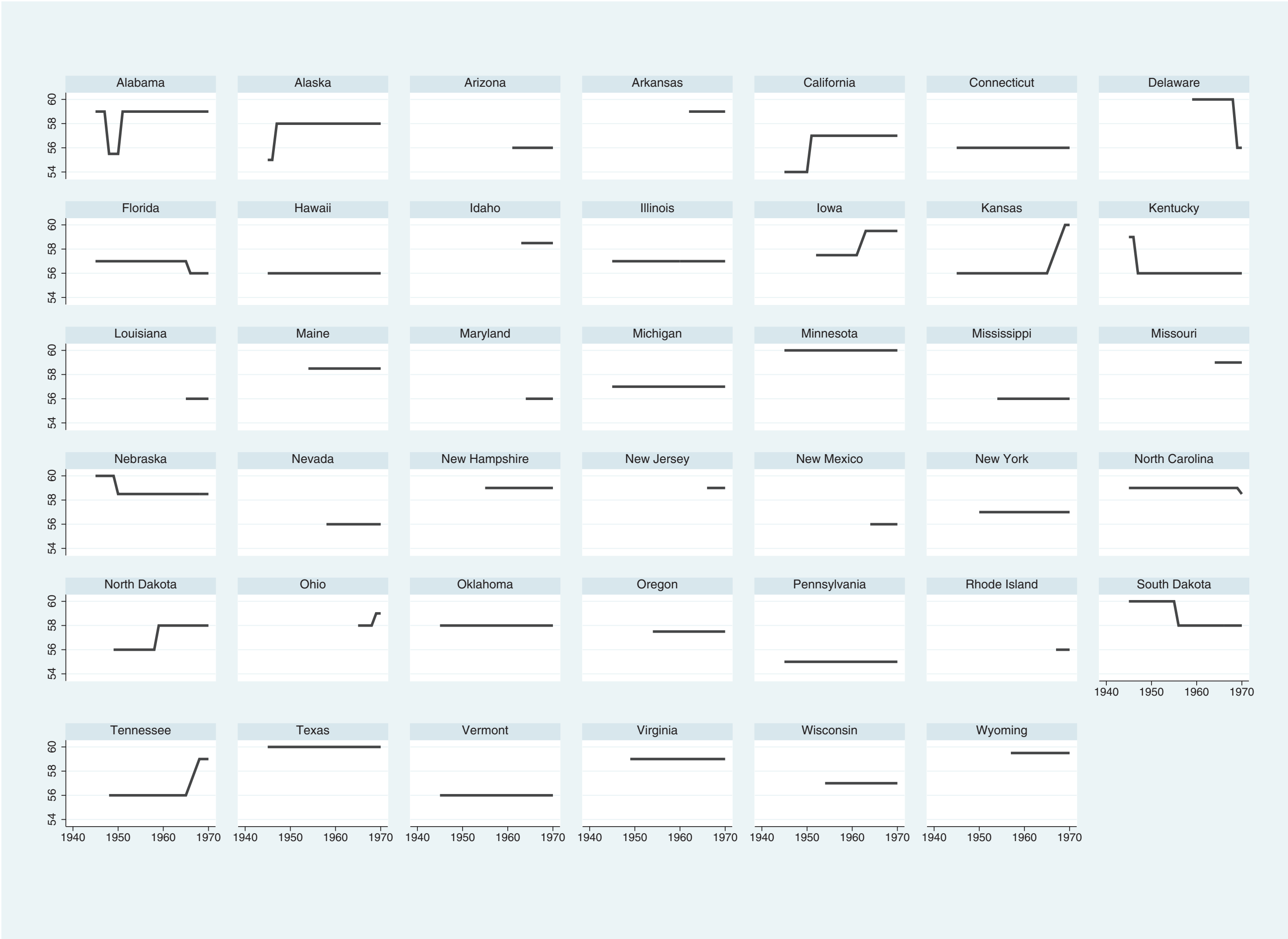

Fig. 1. Minimum school starting age (in months) over time, 1945–1970. Nine states did not have a school starting age defined over the sample period: Colorado, Georgia, Indiana, Massachusetts, Montana, South Carolina, Utah, Washington, and West Virginia.

Table 2.
Summary statistics for school attendance sample.

Variable	White sample Mean	White sample SD	Black sample Mean	Black sample SD
School starting age (SSA, in months)	57.47	1.43	57.56	1.45
Attending school	0.78	0.41	0.71	0.45
Public school	0.67	0.47	0.67	0.47
Private school	0.12	0.32	0.04	0.19
Not attending school	0.22	0.41	0.29	0.45
Age	5.74	0.66	5.73	0.66
Female	0.49	0.50	0.50	0.50
Family income ($, 1999)	44,712	27,027	24,898	19,520
Household head's years of schooling	11.64	3.26	9.62	4.01
Subsidized kindergarten	0.73	0.44	0.56	0.50
Mandatory entry age	6.96	1.02	6.53	1.86
Expenditure per pupil	0.58	0.24	0.54	0.25
Student–teacher ratio	23.25	2.43	24.06	2.75
Sample size	91,143		14,835	

Table 3.
Age-specific public kindergarten attendance rates (in percent), by SSA cutoff.

Age, QOB	White January 1	White October 1 or earlier	Black January 1	Black October 1 or earlier
Age 6, Q4	5.1	32.7	1.9	18.4
Age 6, Q1	34.5	42.9	30.0	24.7
Age 5, Q2	37.8	43.7	29.2	25.7
Age 5, Q3	37.1	36.2	28.0	22.8
Age 5, Q4	36.2	11.4	27.8	11.7

are derived from the Digest of Education Statistics (various years).

Table 2 provides summary statistics for the sample. There is a modest gap in school attendance between white and black children (78 percent vs. 71 percent). But this masks a much larger difference in kindergarten attendance, with 58 percent of white children attending kindergarten compared to only 40 percent of black children. The average school starting age cutoff is relatively close between the two groups: whites face a cutoff of 57.47 months while blacks face a cutoff of 57.56 months.

As a first pass at evaluating the effect of SSA policy on attendance, Table 3 shows age-specific public kindergarten attendance rates by SSA cutoff (January 1 vs. October 1 or earlier). As expected, we observe lower attendance rates among quarter 4 births in states with an October 1 or earlier cutoff, since being born after the cutoff makes them ineligible to enroll. Among white children, for example, we see that 36.2 percent of those who are age 5 (with fourth quarter birth) attend public kindergarten in states with an SSA equal to 56, a figure which drops to 11.4 percent in states in which such students are ineligible to attend. There is a corresponding uptick in attendance of those age 6 (with fourth quarter birth) in states with an SSA cutoff of October 1 or earlier, reflecting the fact that students with fourth quarter births who are ineligible at age 5 become eligible the following year. Among black children, this difference is somewhat narrower, with public kindergarten attendance falling from 27.8 percent to 11.7 percent. Thus, it appears that changes in the SSA cutoff do in fact induce changes in the timing of public kindergarten attendance. Examining first-grade attendance rates reveals a similar delay in the timing of school entry.[10] There is some level of non-compliance with the laws, though it is not particularly large. In states with a cutoff of October 1 or earlier, slightly over 11 percent of individuals born in the fourth quarter are attending kindergarten despite being ineligible.[11]

3.2. Econometric model and estimation results

Although suggestive, the summary statistics on school attendance by SSA cutoff are limited by the inability to control for observed and unobserved heterogeneity across states and years in assessing the impact of school starting age legislation on school attendance. To control for these differences, consider the following model for the school attendance of individual i in state s and year t:

$$\mathrm{SCHOOL}_{ist} = \alpha + \beta \mathrm{SSA}_{st} \times \mathrm{AgeQ}_{ist} + \theta X_{ist} + \mu_s + \gamma_t + \varepsilon_{ist}, \tag{1}$$

where SSA is the school starting age cutoff (in months) and AgeQ is a vector of age-specific indicators for i's quarter of birth.[12] The vector X contains individual controls (including the main effects for AgeQ, gender, family income quartile, and dummies for the household head's highest degree attained) and state education controls (an indicator for publically subsidized kindergarten, the mandatory enrollment age, student–teacher ratio, and expenditure per pupil). The terms μ_s and γ_t are fixed effects for state of residence and Census year, respectively. Eq. (1) is estimated as a linear probability model, with identification of β coming from changes in *SSA* within states over time. Standard errors are clustered at the *state* level in order to permit arbitrary correlation in the error terms over time among individuals residing in the same state (Bertrand, Duflo, & Mullainathan, 2004).

Changes in school enrollment behavior that are directly driven by changes in eligibility will occur among individuals born in quarters that contain variation in the school starting age cutoff (quarters 3 and 4, in the present sample). For parents who desire to enroll their child in school at age 5, becoming ineligible should lead to an increase in non-attendance among individuals age 5 who were born in quarters 3 or 4 (depending on where the new cutoff ended up). Among parents who wish to enroll their child in school at age 6, a shift in the SSA cutoff will lead some children to delay their entry into school, and some children to substitute toward kindergarten enrollment instead.

Estimates of Eq. (1) appear in Table 4 under several different specifications and samples. The left panels are for the

[10] As expected, the starting age laws interact with other education policies. For example, the drop off in attendance between the two sets of SSA cutoffs is substantially larger in states with publicly subsidized kindergarten compared to those without.

[11] This may reflect legal exemptions to the law, or a provision of the statute that allows for midyear entry.

[12] For kindergarten attendance, AgeQ consists of indicators for being (in ascending order) age 5 (with quarter of birth within quarter 4 [Q4]), age 5 (Q3), age 5 (Q2), and age 6 (Q1). For first grade attendance, AgeQ consists of indicators for being age 6 (Q4), age 6 (Q3), age 6 (Q2), age 7 (Q1).

Table 4.
The effect of school starting age policy on school attendance.

	White sample			Black sample		
Regressor	Public kindergarten	Private kindergarten	Not attending school	Public kindergarten	Private kindergarten	Not attending school
SSA x (age 6, QOB1)	−0.006	0.000	0.020	0.012	0.013*	−0.007
	[0.019]	[0.003]	[0.026]	[0.015]	[0.007]	[0.042]
SSA x (age 5, QOB2)	−0.019	−0.003	0.015	−0.003	0.003	−0.017
	[0.019]	[0.003]	[0.025]	[0.015]	[0.007]	[0.040]
SSA x (age 5, QOB3)	−0.045**	−0.003	0.037	−0.020	0.009	−0.007
	[0.020]	[0.003]	[0.028]	[0.015]	[0.007]	[0.038]
SSA x (age 5, QOB4)	−0.084***	−0.016***	0.097***	−0.034**	0.010	0.026
	[0.028]	[0.003]	[0.032]	[0.015]	[0.007]	[0.041]
Mean of dependent variable	0.50	0.08	0.36	0.36	0.04	0.47
Observations	45,665	45,665	45,665	7532	7532	7532
R-squared	0.29	0.07	0.30	0.26	0.03	0.28
Regressor	First grade (public)	First grade (private)	Not attending school	First grade (public)	First grade (private)	Not attending school
SSA x (age 7, QOB1)	0.025***	0.007	−0.010	0.034**	0.001	−0.025***
	[0.007]	[0.004]	[0.009]	[0.017]	[0.005]	[0.007]
SSA x (age 6, QOB2)	0.011	−0.001	−0.009	0.012	0.008	−0.015*
	[0.007]	[0.004]	[0.009]	[0.017]	[0.006]	[0.008]
SSA x (age 6, QOB3)	−0.015	0.001	−0.001	−0.004	0.005	−0.012
	[0.009]	[0.004]	[0.006]	[0.017]	[0.005]	[0.009]
SSA x (age 6, QOB4)	−0.131***	−0.001	0.060***	−0.123***	−0.002	0.066***
	[0.013]	[0.004]	[0.017]	[0.017]	[0.005]	[0.010]
Mean of dependent variable	0.65	0.12	0.07	0.66	0.02	0.10
Observations	45,478	45,478	45,478	7303	7303	7303
R-squared	0.12	0.05	0.12	0.08	0.05	0.12

Notes: Each column of each panel represents a separate regression. Standard errors are clustered at the *state* level. Each model includes fixed effects for state, Census year, and quarter of birth-specific age, and controls for gender, public kindergarten subsidization, mandatory enrollment age, head of household's education, family income quartile, student–teacher ratio, and expenditure per pupil.
* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

sample of white children, and the right panels are for the sample of black children. Among both white and black children, there is strong evidence of delayed entry into kindergarten due to the changes in eligibility. For white children who are age 5 and born in quarter 4, a one-month increase in SSA reduces the probability of attending public kindergarten by 8.4 percentage points. Thus, a change in the SSA cutoff from January 1 to October 1, which would make all children born in quarter 4 ineligible to attend school, reduces the likelihood of attendance by 25 percentage points. To put this figure in perspective, the 1960 attendance rate of this group—in states with an SSA cutoff of January 1 or later—is 31 percent. Moving up the cutoff from January 1 to October 1 would be predicted to reduce the attendance rate from 31 percent to 6 percent, an 81 percent reduction. Among white children who are age 5 and born in quarter 3, the effect of a one-month increase to the SSA cutoff is to reduce attendance by 4.5 percentage points. The smaller effect on this group of individuals most likely reflects the fact that only a subset of changes to SSA cutoffs (those moved to earlier than October 1) had a direct impact on their eligibility.

Among black children, the reduction in public kindergarten attendance is smaller. Among black children who are age 5 and born in the fourth quarter, a one-month increase in SSA leads to a 3.4 percentage point reduction in the likelihood of attendance. Using a similar calculation as above, moving the SSA cutoff from January 1 to October 1 would be predicted to reduce kindergarten attendance by 54 percent. Among quarter 3 births, the impact of ineligibility is also to reduce attendance at public kindergarten, although the point estimate is not statistically significant. The smaller effect of school starting age policy on the kindergarten enrollment behavior of black students most likely reflects lower access to publicly subsidized kindergarten: 56 percent of black students had access compared to 73 percent of white students. For a given law change, a smaller fraction of the black sample experienced a change in eligibility. The differential effect on the kindergarten entry behavior of white and black students suggests that the longer-term effect on crime may also differ across the two groups. In contrast, the point estimates for the effect of SSA on first grade attendance are very close between white and black students, reflecting comparable (universal) access to first grade.

Could other education policies or family background characteristics explain the estimated relationship between school starting age laws and school attendance? This is highly unlikely, for two reasons. First, any potentially confounding policy would have to affect attendance rates only among those born in quarters three and four, not just overall attendance rates. It is hard to conceive of such a policy. Second, the inclusion of the education and family background controls has little effect on the estimated coefficients on the SSA variable.

This suggests that school starting age policy is not being set in a fashion that is systematically related to several other education policies, the distribution of household income, or average parental education within a state. This provides some support for the exogeneity of the laws in the crime regressions in the next section.

Overall, then, school starting age laws do in fact “work” in the sense that they induced reductions in quarter-of-birth specific attendance rates. How did parents respond to the SSA-induced ineligibility? We see that it led to a shift toward time spent at home rather than private school attendance. Among those age 5 and born in quarters 3 and 4, there is almost a one-to-one shift from attendance at public school to non-attendance among both white and black children. Among those age 6 and born in quarters 3 and 4, the shift away from public school attendance is divided almost equally between non-attendance and a shift toward kindergarten attendance (estimates for the latter outcome are not shown). There is no evidence of substitution away from public school toward private school in response to SSA-induced ineligibility. Among white students who are age 5 and born in quarter 4, private kindergarten attendance actually declined in response to an increase in the SSA cutoff. This is unexpected given that private kindergartens are generally not bound by school starting age cutoffs.[13] In any case, the overall pattern suggests that increasing the school starting age cutoff does in fact lead to a delay in the timing of an individual’s enrollment in formal schooling.

There is little evidence of strategic redshirting in response to changes in school starting age policy, at least at the (coarse) quarter of birth level for which we are able to classify individuals.[14] Under such a scenario, we would expect to see unusually low rates of attendance among students who barely qualify under the starting age cutoff. For example, increasing the school starting age cutoff from January 1 to September 1 would lead parents of some children born in the summer months to delay their child’s school entry. We see that there is no response among quarter 2 births to changes in SSA. There is some evidence of a decline in attendance of those born in quarter 3, but this is most likely driven by those changes in SSA that occurred within the third quarter. There is no longer a statistically significant effect on quarter 3 births if states that changed their SSA to within the third quarter are dropped from the sample.

The lack of an effect of school starting age policy on the school enrollment behavior of those whose eligibility was not affected (those born far from the cutoff) is important for our ability to identify the indirect effect of school starting age on crime outcomes. Since these individuals did not alter their entry behavior, the only channel through which SSA policy affected their outcomes is through a change in the age composition of their schooling cohort. Individuals born in quarter 2 clearly satisfy this criterion.

[13] South Dakota, for example, is an exception to this rule in that private schools are also bound by school starting age laws. It is possible that private schools decided to harmonize their own admissions policies with state policy for public schools, or that parents sending their children to private school decided to redshirt them in anticipation of enrolling them in public school in the future.

[14] This is consistent with the finding in Barua and Lang (2012).

4. The effect of school starting age policy on incarceration

4.1. Data

Data on incarceration come from the Public-Use Micro Sample of the 1970 and 1980 U.S. Censuses (Ruggles et al., 2008). The PUMS data are a 1 percent sample (in 1970) and 5 percent sample (in 1980) of individual-level responses from the decennial U.S. Census. The sample consists of U.S.-born males, ages 18–30.[15] Our choice of sample and time period is based on data availability.[16] Since state of residence at school entry is not in the data, school starting age is assigned based on the year in which an individual turned 5 in his state of birth. The state-level measures of school quality (expenditure per pupil and student–teacher ratio) are averaged over the years in which a given individual would have been age 5–14 in his state of birth.

Incarceration status is derived from whether the respondent resides in a correctional institution, which includes federal prisons, state prisons, and local jails. As a measure of crime, incarceration status has several advantages over self-reported data. Incarceration is not subject to misreporting. It represents a market outcome, to which a significant fraction of expenditure on the criminal justice system is directed. That being said, incarceration is the result of a complex process of arrest, trial, and conviction, and so any impact of school starting age on incarceration could come via an impact on the probability of arrest, the probability of conviction given trial, etc. The number of incarcerated will undercount the number of individuals engaged in crime.[17] Table 5 reports summary statistics for the crime sample. Among whites, 0.61 percent of individuals are incarcerated, compared to 4.09 percent of blacks.

4.2. Econometric model and identification

We estimate both the impact of school starting age policy on incarceration for an entire cohort, as well as its differential impact by season of birth in order to shed light on the underlying mechanisms at work. Consider first the effect of the minimum SSA on incarceration status (INCARC) for individual i born in state s in year c:

$$\text{INCARC}_{isc} = \alpha + \beta \text{SSA}_{sc} + \theta X_{isy} + \mu_s + \gamma_c + \varepsilon_{isc}, \quad (2)$$

where the vector X contains controls for age, age squared, indicators for quarter of birth, Census year, and state of

[15] Females are excluded from the analysis because their incarceration rates are very low.

[16] Individuals younger than 18 are often incarcerated in juvenile detention facilities, and these cannot be identified in the Census data. School starting ages were rarely codified before 1940, and so it is not possible to include individuals in the 1970 Census older than 30. Later Censuses do not separately identify correctional institutions. In particular, the 1990 and 2000 Censuses and 2001 to 2005 ACS surveys identify group quarters status but not the type of group quarters; the 2006–2012 ACS surveys list a coarse version of group quarters type, but do not separately identify correctional institutions. Thus, for comparability reasons, we do not use these later data.

[17] This can be seen by comparing victimization surveys (like the National Crime Victimization Survey) with arrest reports (as aggregated in the FBI’s Uniform Crime Reports). Fewer than half of crimes are reported (Levitt, 1998).

Table 5.
Summary statistics for crime sample.

Variable	White sample Mean	White sample SD	Black sample Mean	Black sample SD
School starting age (SSA, in months)	57.31	1.43	57.55	1.46
Incarcerated (%) (full sample)	0.61	7.77	4.09	19.81
Incarcerated (%) (migration sample)	0.59	7.69	3.99	19.57
0–4 years (%)	0.51	7.12	3.16	17.50
5 or more years (%)	0.08	2.91	0.83	9.05
Age	23.48	3.75	23.08	3.76
Age squared	565.29	178.68	546.80	178.33
Subsidized kindergarten	0.64	0.48	0.38	0.49
Mandatory enrollment age	7.03	0.94	6.67	1.66
Drop-out age	16.05	2.10	14.96	4.42
Average expenditure per pupil	1133.36	1050.83	1014.71	1007.54
Average student–teacher ratio	23.45	2.36	24.33	2.54
Educational attainment (ED) ≥ 11 (%)	89.44	30.73	78.60	41.01
Educational attainment (ED) ≥ 12 (%)	81.24	39.04	64.61	47.82
Educational attainment	12.68	2.34	11.69	2.29
Year = 1970	0.10	0.30	0.08	0.27
Year = 1980	0.90	0.30	0.92	0.27
Sample size (full sample)	787,183		109,552	
Sample size (migration sample)	434,876		58,913	

Notes: Only half of the 1980 sample was asked the migration question, and so the reported statistics for the length of incarceration only apply to that reduced sample.

residence, the presence of publically subsidized kindergarten, the mandatory enrollment age, dropout age, average expenditure per pupil, and average student–teacher ratio. The terms μ_s and γ_c are fixed effects for state of birth and year of birth, respectively. Eq. (2) is a difference-in-differences model, with the coefficient β measuring the effect of SSA policy on incarceration for the average individual in a given cohort. Identification of β comes from changes in the SSA cutoff within states over time. Eq. (2) is estimated as a linear probability model with standard errors clustered at the *state of birth* level to guard against arbitrary correlation in the error term among those born in the same state in different years (Bertrand, Duflo, & Mullainathan, 2004).

For β to be estimated consistently, the school starting age cutoff set by a state must be uncorrelated with other determinants of incarceration. This is a plausible assumption considering that the laws are set many years before crime outcomes are realized. However, that does not rule out that states adopting different levels of the minimum school starting age differ on some observed or unobserved dimension that influences crime, like school quality or aspects of the state criminal justice system. To mitigate the influence of potentially confounding factors, Eq. (2) includes a variety of observable covariates (individual characteristics, state education policies, and school quality measures) and controls for unobserved heterogeneity (fixed effects for state of birth and residence, and birth cohort). For example, state of residence fixed effects will control for cross-state heterogeneity in spending on policing or prisons. The inclusion of the average student–teacher ratio during the student's career will control for any direct effect of SSA on class size, which may independently affect adult outcomes. Encouragingly, the inclusion of state education policy and school quality controls has little impact on the estimated impact of SSA, which is evidence in favor of the laws' exogeneity.

While β is a policy relevant parameter, we can gain further insight into the underlying mechanisms at play by exploiting any differential effect across individuals born at different times of the year. As shown in Section 3, individuals born in quarters 1, 3, and 4 (the "directly affected") were induced to delay school entry in response to becoming ineligible based on changes in the SSA cutoff. Those born in quarter 2 (the "indirectly affected") did not alter their entry behavior, as the cutoffs were never moved so as to change their eligibility. Thus, moving beyond the impact of SSA regulations on the entire cohort, consider a model that allows the effect of SSA to differ by quarter of birth:

$$\begin{aligned}\text{INCARC}_{isc} = \alpha + \beta_1 \text{SSA}_{sc} + \beta_2 \text{SSA}_{sc} \\ \times \text{QOB}_{isc} + \theta X_{isy} + \mu_s + \gamma_c + \varepsilon_{isc},\end{aligned} \tag{3}$$

where the QOB is an indicator for birth quarters 1, 3, and 4 (the directly affected), making the comparison group those born in quarter 2 (the indirectly affected). Eq. (3) is estimated as a linear probability model, with standard errors clustered at the *state of birth* level.

Eq. (3) is a difference-in-difference-in-differences model. The coefficient β_1 measures the impact of SSA policy on the probability of incarceration for indirectly affected individuals (those born in quarter 2). As in Eq. (2), identification comes from changes in SSA cutoffs within states over time. Since those born in quarter 2 did not change the timing of their school entry in response to changes in the SSA cutoff, any effect of changes in SSA on this group must come indirectly through changes in the age distribution, and so β_1 captures the indirect effect of school starting age policy. The coefficient β_2 measures the impact of SSA policy on those born in quarters 1, 3, and 4 *relative* to those born in quarter 2. Identification of β_2 comes from within-state changes in incarceration over time in response to changes in SSA, relative to the within-state changes in incarceration of quarter-two-born individuals. Thus, it captures the direct effect of delayed entry, where this direct effect is net of the impact of changes in the age distribution. Whether directly affected individuals are made better or worse off from the laws depends on the combined direct and indirect effects, $(\beta_1 + \beta_2)$.

The interpretation of β_2 as the direct effect requires the strong but necessary assumption (given the available data) that the two effects are additively separable from each other.[18] Since the estimated effect comes from the differential effect on the law on individuals born at different times of the same year, any potentially confounding shock that affects the entire birth cohort, like changes in other education policies, will be netted out. Thus, estimates of β_2 are particularly robust to omitted variable bias. It is important to note that we are not comparing individuals across different quarters of birth, but rather are using within-quarter variation across different levels of school starting age. Consequently, the

[18] This would be violated if, for example, students who delayed entry tended to benefit more from an older average cohort. In this case, the interaction between the two effects would load onto β_2.

Table 6.
The effect of school starting age policy on incarceration status.

Regressor	Sample: White males					
	[1]	[2]	[3]	[4]	[5]	[6]
SSA	−0.065**	−0.064**	−0.065**	−0.081**	−0.079**	−0.081***
	[0.030]	[0.031]	[0.030]	[0.030]	[0.031]	[0.030]
SSA × (QOB = 1,3,4)				0.021**	0.021**	0.020**
				[0.009]	[0.009]	[0.009]
School quality controls			X			X
Education policy controls		X	X		X	X
Mean of dependent variable	0.61	0.61	0.61	0.61	0.61	0.61
Observations	787,183	787,183	787,183	787,183	787,183	787,183
R-squared	0.001	0.001	0.001	0.001	0.001	0.001
Regressor	Sample: Black males					
	[1]	[2]	[3]	[4]	[5]	[6]
SSA	0.322	0.308	0.314	0.381	0.366	0.374*
	[0.223]	[0.226]	[0.203]	[0.231]	[0.236]	[0.214]
SSA × (QOB = 1,3,4)				−0.076	−0.075	−0.077
				[0.077]	[0.076]	[0.077]
School quality controls			X			X
Education policy controls		X	X		X	X
Mean of dependent variable	4.09	4.09	4.09	4.09	4.09	4.09
Observations	109,552	109,552	109,552	109,552	109,552	109,552
R-squared	0.006	0.006	0.006	0.006	0.006	0.006

Notes: Each column represents a separate regression. Estimates are in percentage points. Standard errors are clustered at the *state of birth* level. Each model includes fixed effects for state of birth, year of birth, state of residence, Census year, and quarter of birth, and controls for age and age squared. Education policy controls include an indicator for public kindergarten subsidization, the mandatory enrollment age, and dropout age. School quality controls include average expenditure per pupil, and average student–teacher ratio.
* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

well-known differences in individual and family background characteristics by birth timing (Buckles & Hungerman, 2013) will not confound estimates of the parameters of interest.[19]

4.3. Main results

Estimates of the effect of school starting age policy on incarceration from Eqs. (2) and (3) appear in Table 6. Columns 1–3 show the DD specification and columns 4–6 show the DDD specification. In the DD specifications for the white sample, we see that a higher school starting age cutoff is associated with a lower likelihood of incarceration: a one-month increase in SSA leads to a 0.065 percentage point ($p < 0.05$) decline in incarceration (in column 3), which is 11 percent of the sample mean. The inclusion of education policy and school quality controls has little impact on the estimates. Thus, on average, the cohort as a whole benefits from a higher minimum entry age, at least along the dimension of incarceration.

Because individuals born at different times of the year may be affected differently, we turn to the DDD specification for the white sample. The indirect effect of a one-month increase in SSA is to reduce the probability of incarceration by 0.081 percentage points ($p < 0.01$) in the most complete specification (column 6). This estimate amounts to a 13 percent reduction in incarceration relative to the sample mean. In economic terms, this means that individuals tend to commit less crime when they are exposed to an older average cohort while in school, consistent with positive peer effects. The inclusion of school quality and education policy controls has almost no impact on the coefficient estimates. Turning to the direct effect of the laws, we see that a one-month increase in SSA leads to a 0.020 percentage point ($p < 0.05$) increase in the likelihood of incarceration during adulthood. Thus, the direct effect of entering school later is to increase one's odds of becoming incarcerated as an adult. This is consistent either with children learning less in their redshirt year compared to those enrolling on time, less within-grade learning throughout school, or with late entry crowding out final educational attainment. Note that the *overall* effect of a higher SSA cutoff is to reduce incarceration among directly affected individuals, since the indirect effect dominates: a one-month increase in SSA leads to a 0.061 percentage point ($p < 0.05$) reduction among those born in quarters 1, 3, and 4.

Relative to other successful early childhood education programs and education policies, school starting age policy has a comparable level of effectiveness in reducing criminal behavior. The indirect effect is about 60 percent as large as the estimated effect on incarceration of an additional year of schooling induced by compulsory schooling laws (Lochner & Moretti, 2004). Participation in Head Start is estimated to

[19] Dickert-Conlin and Elder (2010) find no evidence of differences in infants' health or mothers' characteristics around the SSA cutoffs using detailed birth records.

have reduced the likelihood of being booked or charged with a crime by 8.5 percentage points (Garces, Thomas, & Currie, 2002), and participation in the Perry Preschool Project led to a 20 percentage point reduction in those having been arrested or charged with a crime (Gramlich, 1986).[20]

Turning to the black sample, the impact of minimum school starting age laws is generally indistinguishable from zero in both the DD and DDD specifications in Table 6. Only one estimate in the panel is statistically significant, and then only marginally (at 10 percent). The coefficient estimates are opposite in sign to those found for the white sample. A higher minimum school starting age is associated with a higher likelihood of incarceration for the overall cohort (columns 1–3) and for indirectly affected individuals (columns 4–6), and is associated with lower incarceration among directly affected individuals (columns 4–6). Note that those born in quarter two are becoming relatively younger within their cohort, and several papers find that being relatively young within a cohort is associated with worse outcomes. For example, Bedard and Dhuey (2006) find that the relatively young have lower test scores and are less likely to attend college, and Elder and Lubotsky (2009) document that the relatively young are more likely to repeat a grade and be diagnosed with attention deficit hyperactivity disorder (ADHD). These relative age effects may explain why incarceration is higher among indirectly affected individuals in the black sample, though it is not obvious why this effect should dominate for blacks but not whites.

Although the differing effects for the black sample are consistent with theory, some care is necessary in interpreting the estimates given their imprecision. Recall that less access to public kindergartens meant that fewer black students were dissuaded from attending in response to changes in the cutoff, and so black students experienced less (though still some) treatment from the laws. Also, the black sample is about one-seventh the size of the white sample. These two factors likely contribute to the lack of precision for the black sample. If schools were fully integrated, though, we might still expect to find an indirect effect of the laws through spillover from affected white students. However, Boozer, Krueger, Wolkon, Haltiwanger, and Loury (1992) show that very little desegregation occurred until 1964, despite Brown v. Board of Education making segregation unconstitutional 10 years earlier. Almost all of the black sample had already entered formal schooling by 1964—with some portion having already completed school by then—and so it is not surprising that we do not see the delayed entry among whites spilling over to black individuals.

4.4. Further results and specification checks

One of the hypothesized ways in which school starting age policy affects crime is through its impact on educational attainment. Since educational attainment is observable in our data, we now test whether SSA policy has an impact on 11th and 12th grade completion rates using Eq. (3). Finding a decrease in grade-specific completion rates for the direct effect would be consistent with crowd-out, since those entering school later would reach the dropout age sooner than otherwise. Since many 18 year olds are still attending high school, we restrict the sample to individuals who are 19 years or older. The results from modeling grade completion rates using Eq. (3) appear in Table 7. For both the white and black samples, the effect of school starting age policy on grade completion rates is statistically indistinguishable from zero, and sufficiently precisely estimated to rule out small effects.

Table 7.
The effect of school starting age policy on grade completion rates.

	White sample (age ≥ 19)		Black sample (age ≥ 19)	
Regressor	ED ≥ 11	ED ≥ 12	ED ≥ 11	ED ≥ 12
SSA	−0.526	−0.556	−0.397	−0.744
	[0.350]	[0.377]	[0.514]	[0.661]
SSA x (QOB = 1,3,4)	−0.001	−0.035	0.206	0.385
	[0.084]	[0.090]	[0.164]	[0.244]
Mean of dependent variable	90.21	85.30	80.33	69.09
Observations	710,562	710,562	96,742	96,742
R-squared	0.03	0.03	0.03	0.04

Notes: Each column represents a separate regression. Estimates are in percentage points. Standard errors are clustered at the *state of birth* level. Each model includes fixed effects for state of birth, year of birth, state of residence, Census year, and quarter of birth, and controls for age, age squared, an indicator for public kindergarten subsidization, the mandatory enrollment age, dropout age, average expenditure per pupil, and average student–teacher ratio.
* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

Another test of crowd-out would be to add high school graduation status as an additional regressor to Eq. (3). The direct and indirect effects are now net of any intermediate effect on high school graduation.[21] The results of doing so appear in column 3 of Table 8, with the sample again limited to 19 year olds and older; column 2 shows the baseline model restricted to this age range for comparison. The estimated indirect effect of the law is barely changed, but the direct effect shrinks by 26 percent and becomes statistically indistinguishable from zero. This latter finding is consistent with the direct effect of SSA policy on incarceration operating through a crowd-out mechanism.

The main regression results are fairly robust to several alternative model specifications, which are explored in Table 8. For reference, column 1 reproduces the results from the most complete model in Table 6. We first control for several additional forms of unobserved heterogeneity that could drive the results. Columns 4–6 show the results from adding state of residence × year effects, state of birth × year effects, and both sets of effects, respectively, to the model. These effects control for time varying, state-specific changes in the educational

[20] Relative to the literature on school starting age, Bedard and Dhuey (2012) find that a one-month increase in SSA is associated with a 0.6 percent increase in wages. Combining this with Grogger's (1998) estimate of the wage elasticity of crime suggests that a one-month increase in SSA would be expected to reduce crime by 11 percent via the wage channel. This calculation should not be confused to imply that wages account for the entire effect of SSA laws. The direction of causality could very well run from higher crime causing lower wages via reduced labor market experience, stigma, etc.

[21] Of course, high school graduation status is likely to be endogenous in this regression, and so its coefficient should not be interpreted as a causal estimate of the returns to a high school degree.

Table 8.
Further results and specification tests.

	Sample: White males								
Regressor	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
SSA	−0.081***	−0.091***	−0.092***	−0.065***	−0.069**	−0.070**	−0.115**	−0.020	−0.046
	[0.030]	[0.032]	[0.032]	[0.023]	[0.033]	[0.034]	[0.045]	[0.049]	[0.046]
SSA × (QO B = 1,3,4)	0.020**	0.019**	0.014	0.020**	0.020**	0.020**	0.064	0.021**	0.020**
	[0.009]	[0.009]	[0.010]	[0.009]	[0.009]	[0.009]	[0.062]	[0.009]	[0.009]
ED ≥ 12			−0.016***						
			[0.001]						
SSA($c+1$)									−0.041
									[0.043]
State of residence × year effects				X		X			
State of birth × year effects					X	X			
Quarter of birth × state of birth effects						X			
State-specific trend								X	
Mean of dependent variable	0.61	0.61	0.61	0.61	0.61	0.61	0.61	0.61	0.61
Observations	787,183	787,183	787,183	787,183	787,183	787,183	787,183	787,183	787,183
R-squared	0.001	0.006	0.007	0.001	0.001	0.001	0.001	0.001	0.001

Notes: Each column represents a separate regression. Estimates are in percentage points. Standard errors are clustered at the *state of birth* level. Each model includes fixed effects for state of birth, year of birth, state of residence, Census year, and quarter of birth, and controls for age, age squared, an indicator for public kindergarten subsidization, the mandatory enrollment age, dropout age, average expenditure per pupil, and average student–teacher ratio. Columns 2 and 3 restrict the sample to those ages 19–30.
* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

and criminal justice environment that could otherwise confound the relationship between school starting age and incarceration. For instance, states may shift resources between education, policing, and prisons over time; since educational reforms may be packaged, this could induce a correlation between changes in school starting age policy and incarceration rates. We see in Table 8 that including one or both of these effects leads to some attenuation in the indirect effect of SSA. When including both effects, the magnitude of the indirect effect falls by 14 percent to −0.070 percentage points per one-month change to SSA, though it remains statistically significant. There is no change in the estimated direct effect of the laws across the three specifications. These results suggest that other time varying, state-specific factors are not driving the observed relationship between school starting age policy and incarceration. It is possible that the quarter of birth effects differ across birth states due to quarter-of-birth-specific differences in home environment or state policies. To address this, column 7 shows the results from adding quarter of birth × state of birth effects. With this addition, the indirect effect remains negative and statistically significant; the interaction of SSA and quarter of birth, though remaining positive, is no longer significant.

Another concern is that changes in school starting age cutoffs are relatively infrequent, and so the results may reflect longer-run trends in incarceration rates. Adding a state-specific trend term (column 8) has almost no effect on the direct effect of the laws, though it does lead to sizeable attenuation and loss of precision for the indirect effect. This latter finding is not surprising given that state-specific trends absorb a considerable amount of variation in the data.

If changes in the minimum school starting age are driving changes in the probability of incarceration and not vice versa, then there should not be any effect of future laws on current incarceration status, conditional on the current level of SSA. Here, future laws refer to the starting age regulations in place after the individual had already reached school age. To test this proposition, we add a one-year lead of the SSA variable to Eq. (3). The estimated coefficient on the lead variable is not statistically significant, as seen in column 9. The direct effect is unchanged, although the indirect effect shrinks in size and becomes statistically insignificant.

A final concern is that individuals who reach age 5 in a year in which there is a change to the cutoff may be assigned an incorrect value of SSA. However, excluding individuals reaching age 5 the year before, the year of, and the year after a change to the minimum school entry age has no effect on the estimated parameters.[22]

4.5. Sentence length

In this section, we investigate the impact of school starting age policy on the length of time for which an individual has been incarcerated. More serious crimes tend to result in longer sentences. For instance, in 1980 the average sentence length among federal crimes was 10.5 years for violent offenses, 3.9 years for drug offenses, 2.4 years for property crimes, and 2 years for public-order offenses (Bureau of Justice Statistics, 1989). Understanding the impact of the laws on sentence length is important because it sheds light on the total amount of harm that has been reduced, an important element in any cost-benefit analysis.

Though the data convey no explicit information about the type of offense or length of sentence, it is possible to derive a measure of sentence length using information on migration. The 1980 Census files contain a migration variable that identifies whether the respondent currently lives in the same

[22] Results are available upon request.

Table 9.
The effect of school starting age on sentence length (S) [average marginal effects from ordered probit.]

Regressor	White sample			Black sample		
	$\Pr(S = 0)$	$\Pr(0 < S < 5)$	$\Pr(S \geq 5)$	$\Pr(S = 0)$	$\Pr(0 < S < 5)$	$\Pr(S \geq 5)$
SSA	0.087***	−0.072***	−0.015***	−0.356	0.262	0.094
	[0.030]	[0.025]	[0.005]	[0.299]	[0.220]	[0.079]
SSA x (QOB = 1,3,4)	−0.018	0.015	0.003	0.063	−0.046	−0.017
	[0.011]	[0.009]	[0.002]	[0.129]	[0.095]	[0.034]
Frequency of category (%)	99.41	0.51	0.08	96.01	3.16	0.83
Observations		434,876			58,913	
Pseudo r-squared		0.02			0.02	

Notes: Each panel represents a separate regression. Estimates are in percentage points. Standard errors are clustered at the *state of birth* level. Each model includes fixed effects for state of birth, year of birth, state of residence, Census year, and controls for age, age squared, an indicator for public kindergarten subsidization, mandatory enrollment age, dropout age, average student–teacher ratio, and average expenditure per pupil.
* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

residence as he did five years ago. This makes it possible to identify individuals who are serving a sentence at least five years long. The 1970 Census contains an analogous categorical variable for the number of years the respondent has lived in their current residence; this variable combines the categories for four and five years ago. When pooling the 1970 and 1980 data, the categories of zero to five years in 1980 will be combined with the category for zero to four-to-five years in 1970. This variable will be used as a measure of sentence length.[23] Controlling for Census year in the regression analysis will mitigate differences in the variable definition between 1970 and 1980. Among the migration sample, we see in Table 5 that 0.51 percent of whites are incarcerated for between 0 and 4 years, and 0.08 percent for 5 or more years, while 3.16 percent of blacks are incarcerated for less than 4 years and 0.83 percent for 5 or more.

To determine the effect of school starting age on sentence length (S), we estimate an ordered probit model with linear index given in Eq. (2). The dependent variable is a categorical variable for not incarcerated, incarcerated for 0–4 years, and incarcerated 5 or more years. Table 9 reports the average marginal effects. Among whites, a higher SSA cutoff leads to a reduction in incarceration for both shorter ($0 < S < 5$) and longer sentences ($S \geq 5$). Only the indirect effect is statistically significant. A one-month increase in the SSA cutoff is associated with a 0.072 percentage point reduction in shorter sentences, and a 0.015 percentage point reduction in longer sentences. That more of the reduction in crime occurs among those with shorter sentence lengths is not surprising given that the bulk of the sentences are shorter than 5 years, and the sample consists of a relatively young population. Although the direct effect of the laws is not statistically significant, it remains the same sign as before. Once again, neither of the effects is statistically significant for the black sample.

5. Conclusions

Raising the minimum age at which children can enroll in school affects both those who are induced to delay entry and those who are not. Those who do not change their entry age end up committing less crime, which is consistent with positive peer effects or improved social interactions from an older average cohort while in school. Individuals who do delay entry also end up committing less crime, but more crime than they otherwise would have had their entry not been delayed. As states continue to debate raising their minimum school starting age, the potential savings from crime reduction should be factored in to their cost-benefit calculus. But considering that the benefits are larger for some individuals than others, policymakers should be cognizant of the distribution of gains in setting school starting age regulations.

Appendix A

School starting age cutoffs were compiled by the author from state statutes and legislative codes using the sources in Appendix Table A.1. The cutoffs were checked against the data in Angrist and Krueger (1992) and Bedard and Dhuey (2012). Several exceptions to how school starting age regulations generally operate are worth noting. First, completion of kindergarten is usually sufficient grounds for a child to enter first grade even if he or she does not meet the first grade cutoff. Such a situation would arise if a student transferred between states with different starting age regulations, or attended a private kindergarten. Second, some states permit exemptions from the cutoff based on a successful petition of the parents to the principal and/or local school board which argues that the student in question is sufficiently mature and intellectually ready for school. Third, several states permit students who were born after the SSA cutoff to enter kindergarten during the second semester of the year. These states include: Alabama, California, Florida, Illinois, Kentucky, Michigan, Ohio, Oregon, Pennsylvania, Tennessee, Vermont, and Virginia. In these states, there is a second cutoff defined

[23] This measure has three limitations. The first is that it cannot identify prisoners who were in the corrections system five years ago but in a different facility, and so will tend to undercount those serving long sentences. Second, it is censored, as the only information known is whether the prisoner served at least five years, but not how many more. The third limitation is that prisoners who were released but recidivated will be marked as serving a contiguous sentence, and thus to some extent the variable measures recidivism.

Table A1.
Legislative sources for school starting age cutoffs.

State	Legislative source
Alabama	C.A. 1940, 1953, and 1958, §52-11-298; C.A. 1991, §16-28-4
Alaska	C.L.A. 1933, §91-97-29; C.L.A. 1949, §37-1-11; A.S. 1962, §14-03-070; A.S. 1991, § 14-03-080
Arizona	Laws 1960, Ch 127, §17-18; A.R.S. 1956 (1975 supplement), §15-302
Arkansas	A.R.S. 1947 (1960 replacement), §80-1501; A.S.A. 1947 (1980 replacement), §80-1654
California	Deering's C. Ed. C.A. 1943 (1952 supplement), §8404; West's A.C.C. 1993, §4800
Colorado	C.R.S. 1953 and 1963, §123-21-15; C.R.S. 1973, §22-32-119
Connecticut	C.S.A. 1935, Ch 146, §303; G.S.C. 1949, §1349; C.G.S.A. 1958 (1977 revision), §10-15
Delaware	49 Del. Laws, Ch. 403, §§6,7; 1969 57 Del. Laws, c. 112; D.C. 1991, Title 14, §2702
Florida	F.S. 1941, §232.04; West's F.S.A. 1943 (1977 revision), §232.04
Georgia	C.G.A. 1933 (1976 revision), 32-604a
Hawaii	R.L.H. 1945, §1823; H.R.S. 1968 (1976 supplement), §298-4
Idaho	I.C. 1963 and 1991, §33-201
Illinois	I.R.S. 1945, §6-25; Smith-Hurd I.R.S. 1961 School Code (1989 revision), §10-20.12
Indiana	Burns I.S.A. 1991, §20-8.1-3-17
Iowa	C.I. 1958 and 1962, §282.3; W.I.C.A 1988, §282.3
Kansas	G.S.K.A. 1949, §72-1107; K.S.A. 1964 (1980 supplement), §72-1107
Kentucky	K.R.S. 1942, §158.040; K.R.S. 1948, §158.030-158.040
Louisiana	Acts 1964, No. 109, §2; L.R.S. 1951 (1981 revision), Ch. 1, §17:222
Maine	Mitchie R.S.M. 1954, Ch 41, §44; M.R.S. 1991, Title 20-A, §5201
Maryland	Bylaw 710 (Public School Laws 1967); COMAR 13A.08.01.02
Massachusetts	M.A.L. 1992, Ch. 76, §1
Michigan	Pub Acts 1927, No. 319, Part II, ch 2, §9; Op Atty Gen, Nov 22, 1958, No. 3135; M.S.A., 1979 revision, §15.41147
Minnesota	M.S. 1945, §132.01; M.S.A. 120A.20.1 (2011 revision)
Mississippi	Codes 1942, §6225-03; Laws, 1953, Ex Sess, ch. 24; M.C.A. 1972, §37-15-9
Missouri	V.A.M.S. 1959 (1965 revision), §160.051
Montana	R.C.M. 1961, §75-2004; M.C.A. 1979, §20-7-117
Nebraska	R.S.N. 1943 and 1943 (1958 supplement), §79-414; R.S.N. 1943 (1991 supplement), §79-444
Nevada	N.R.S. 1957 (1965, 1971, 1975 editions), §392.040
New Hampshire	N.H.R.S.A 1955, §193-1; N.H.R.S.A 1955 (1963 and 1977 supplements), §193-1
New Jersey	R.S.N.J. 1937, §18:15-1; N.J.S.A. 1937 (1968 and 1988 revisions), §18A:38-5
New Mexico	N.M.S. 1953 (1967 revision), §77-11-2; N.M.S.A. 1978, §22-1-2
New York	McKinney's C.L.N.Y.A. 1947 (1952 supplement), §3202; McKinney's C.L.N.Y.A. 1953 (1970 revision), §3202
North Carolina	G.S.N.C. 1943 and 1965, §115-371; N.C.G.S. 1991, §115C-364
North Dakota	N.D.R.C. 1943 (1957 supplement), §15-4701; N.D.C.C. 1981, §15-47-01
Ohio	1943: 120 v 475; 1965: vol. 131, pts. 1-3; 1965 & 1967-1968: vol. 132, pt. 1; O.R.C.A. 1996, 3321.01
Oklahoma	O.S. 1941, §70-21-650e; O.S. 1941 (1949 supplement), §70-1-16; O.S. 1991, §70-1-114
Oregon	O.R.S. 1953, §336.070; O.R.S. 1965, §339.115; O.R.S. 1992, §336.092
Pennsylvania	Purdon's P.S.A. 1950 (Public School Code of 1949), §13-1304; Purdon's P.S.A. 1962 and 1992, §13-1304
Rhode Island	G.L.R.I. 1956 (1969 and 1981 revisions), §16-2-27
South Carolina	Code 1976, §59-65-10
South Dakota	S.D.C. 1939, §15.2032; S.D.C. 1939 (1960 supplement), §15.3002; S.D.C.L. 1967 (1975 revision), §13-28-1,3
Tennessee	Williams T.C.A. 1934 (1952 supplement), §2472.1; T.C.A. 1955 (1966 and 1977 editions), §49-1702
Texas	V.A.R.C.S. 1950, Art 2902. 2894-2900; V.C.S.A. 1965, 2902-2903; V.T.C.A. 1955 (1987 edition), 21.031
Utah	U.C.A. 1953 (1970 supplement), §53-19-1
Vermont	P.L.V. 1933, §4242; V.S.A. 1974, §4295
Virginia	C.V. 1950, §22-218; Mitchie's C.V. 1950 (1969 revision), §22-218.2; C.V. 1950 (1980 replacement), §22.1-199
Washington	Pierce's C.W., 1939, Ch. 19, §5254; R.C.W.A. 1970, §28A.35.010; West's RCWA 2011, 28A.225.160
West Virginia	W.V.C.A. of 1961, §1776; W.V.C. 1971, §18-5-18
Wisconsin	W.S. 1953, 1963, and 1977, §40.44
Wyoming	Laws 1955, Ch 192, §1; Laws 1957, Ch 28, §1; W.S. 1991, § 21-4-302

for entry during the middle of the year. For example, a state may set both January 1st and March 1st as cutoffs for entry in September and January, respectively. In these cases, the SSA cutoff is coded as the earlier one. Fourth, South Dakota is an exception in that it explicitly applies the SSA cutoff to private and parochial schools (S.D.C. 1939 [1960 supplement], §15.3002), whereas in other states the cutoff only applies to public schools.

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