Does Human Capital Influence Crime? Evidence from School Cutoff Dates in Florida and Illinois

Colin Adams*

Working Paper†

Abstract

I exploit exogenous variation in kindergarten entry cutoff dates based on date of birth and use administrative prison data with a regression discontinuity design (RDD) to measure the effect of human capital on adult crime. I use data from Florida and Illinois, two states that gradually moved their kindergarten eligibility cutoff dates earlier by one month each year during the 1980s. Due to the quasi-random assignment of school start age based on date of birth, children born just after the cutoff were required to delay kindergarten entry by a year, reducing their required schooling and altering peer interactions during formative years. Leveraging temporal variation in school entry cutoffs, I link school start age to incarceration outcomes observed decades later and provide new evidence on how school entry timing can shape long-run criminal outcomes using administrative data.

*Ph.D. Student in Economics, Florida State University (colinpadams.com | ca23a@fsu.edu) I am grateful to Carl Kitchens and Luke Rodgers for their invaluable advice and support on this project.

†Last updated on August 28, 2025. You can see the most recent version of this paper here.

I Introduction

A large body of literature connects human capital accumulation to criminal behavior, both theoretically (e.g. Becker 1968, Ehrlich 1973, and Lochner 2004) and empirically (e.g. Angrist and Krueger 1991; Lochner and Moretti 2004; Anderson 2014; Baron, Hyman, and Vasquez 2024). Education raises the opportunity cost of crime, making criminal activity less appealing and shifting the margin for those choosing between legal and illegal behavior. This paper focuses on Florida and Illinois, where reforms to kindergarten entry cutoffs in the 1980s created quasi-experimental variation in human capital.

Two mechanisms through which kindergarten cutoffs influence later outcomes are formal schooling and relative age. There is a vast literature on the effects of education through the K-12 system and college. Many papers, including mine, exploit compulsory schooling laws to find the effects of more education. Angrist and Krueger (1991) use quarter of birth as an instrument for education, showing that additional schooling leads to higher earnings. Lochner and Moretti (2004) use state-level changes in compulsory schooling laws and find that more education reduces arrests and incarceration. Hjalmarsson, Holmlund, and Lindquist (2015) reach similar conclusions using Swedish data.

The second human capital channel is one's age relative to peers, determined by their birthday in relation to the cutoff. The literature here is much smaller and less clear than the formal education channel. A child born just after the kindergarten entry cutoff becomes one of the oldest in their grade, while a child born just before is one of the youngest. This relative age can shape a child's academic performance and social development. Both Black, Devereux, and Salvanes (2011) and Fredriksson and Ockert (2013) find being older at school entry improves educational performance, although long-run consequences remain ambiguous. Dobkin and Ferreira (2010) find starting school earlier lowers test scores initially, increases schooling, and has mixed effects on adult wages and employment. Depew and Eren (2016) find that delayed school entry age reduces juvenile crime among black females and reduces the severity of their crimes. They find no evidence of an effect on males. Cook and Kang (2016) find being born just after the cutoff to cause more crime for individuals before they are nineteen in North Carolina while Landersø, Nielsen, and Simonsen (2015) find it to decrease juvenile crime in Denmark. Closest to my study, McAdams (2016) uses U.S. Census data and finds that earlier school entry cutoffs reduce adult incarceration overall, but increases incarceration for those whose start was delayed due to changes in the cutoff.

I use variation in required schooling to measure human capital's impact on crime. To do so, I look at Florida and Illinois who each changed their kindergarten cutoff entry ages in the 1980s while not changing their compulsory schooling laws. This causes the kindergarten cutoff entry date to be the sole source of variation in required schooling among compliers and a discontinuity in required schooling where the running variable is their birthday. This is ideal for this paper as demographic groups with high crime rates are also those most likely to exit before completing high school (Lochner and Moretti 2004). In the 1980s, both states gradually moved their kindergarten eligibility cutoffs earlier in the calendar year. This change caused children born just after the new cutoff date to delay kindergarten by a year.

Using prison population data from each state, which includes exact dates of birth, I am able to exploit this exogeneity to measure the combination of the formal education and relative age effects on adult incarceration. I do this with a regression discontinuity design (RDD) similar to Arenberg, Neller, and Stripling (2024); where the running variable is the distance to the cutoff which causes a jump in required schooling at the cutoff dates. What is particularly unique about my setting relative to other papers is that I use a change in the kindergarten entry cutoff date which affected children already born and conceived. This eliminates the ability for parents to time their child's birth relative to the cutoff date since their child was already conceived.

I find that the combination of less education and being older relative to their peers causes more crime for females as I observe about 17.4% more female prisoners born on any given day after the cutoff. However, I observe no effect for males. Within all females, there is significant heterogeneity in the effect. White women have the strongest effect at about 28.9%. The effect is opposite for Hispanic women of which I observe a 70% reduction in prisoners born on any given day after the cutoff. I find no effect of required education and relative age on Black females via being delayed kindergarten entry.

II Institution

School entry cutoffs refer to the date used to determine whether a child starts school that year or must wait until the following year.[1] In both Florida and Illinois, children must turn five on or before the cutoff date of the school year to be eligible to begin kindergarten that same school year. Prior to any variation I exploit, Florida had a kindergarten birthday cutoff of January 1 and Illinois had a cutoff of December 1.

Neither state had changed the cutoff date since their inception in 1965 and 1895 for Florida and Illinois, respectively (Florida Senate Committee on Education Innovation 1999 and Education Week 1987). Florida announced the change in the cutoff from January 1 to September 1 over four years in 1979 (Florida Legislature 1979). This initially moved the kindergarten cutoff date to December 1st, rather than January 1st, in the 1980–1981 school year and delayed the school start of children born between December 2, 1975 and January 1, 1976 (inclusive). Each subsequent school year had a cutoff one month earlier than the previous until the cutoff remained at September 1 beginning in the 1983–1984 school year (Whaley 1985).

Illinois had a similar change in kindergarten cutoff caused by changes in the Illinois School Code (105-ILCS 5) in 1985 (Education Week 1987). This also moved the cutoff by one month per year, but for only three years. This first affected the cohort beginning kindergarten in the 1986–1987 school year, whose cutoff date moved from December 1st to November 1st. This delayed the school start of children born between November 2, 1981 and December 1, 1981 (inclusive). I include a timeline of the change in cutoff dates for Florida and Illinois in figure 1 as well as a more detailed version in table 6 of the appendix.[2] These cutoffs remained constant in Florida and Illinois for decades following these changes (Education Commission of the States 2014).

Importantly, neither Florida nor Illinois experienced changes in the minimum age at which students could legally withdraw from school during the sample period (Florida Legislature 1970; Illinois General Assembly 1970; Illinois State Board of Education 1983; Angrist and Krueger 1991; Knapp

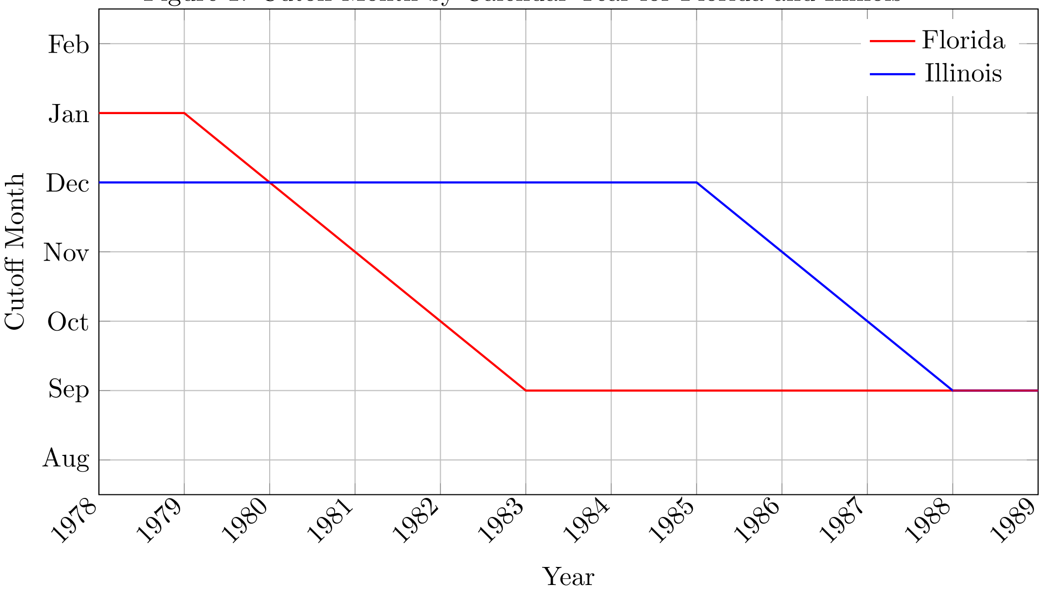
[1] I use kindergarten and school cutoffs synonymously.

[2] Those delayed include individuals born between the following dates (inclusive) in Florida: December 2, 1975 – January 1, 1976, November 2, 1976 – January 1, 1977, October 2, 1977 – January 1, 1978, September 2, 1978 – January 1 1979, and September 2, 1979 – January 1, 1980. The following were delayed in Illinois: November 2 – December 1, 1981, October 2 – December 1, 1982, September 2 – December 1, 1983, September 2 – December 1, 1984, and September 2 – December 1, 1985.

et al. 2025). Students in Florida could not drop out until the age of sixteen and required a parent or guardian's permission while those in Illinois were required to attend school until seventeen. Upon turning eighteen in either state, an individual is considered an adult and can leave school without permission from a parent or guardian. Thus all variation in the amount of required schooling for these individuals is solely determined by the time that they can enroll in kindergarten.[3]

The change in kindergarten school entry cutoffs in combination with compulsory schooling laws creates a discontinuity in the amount of required schooling and age relative to peers caused by a child's date of birth. I display this discontinuity in figure 2. This difference in required schooling is driven solely by the discontinuity in kindergarten entry caused by cutoffs in Florida and Illinois since minimum dropout ages remained constant through this entire period.

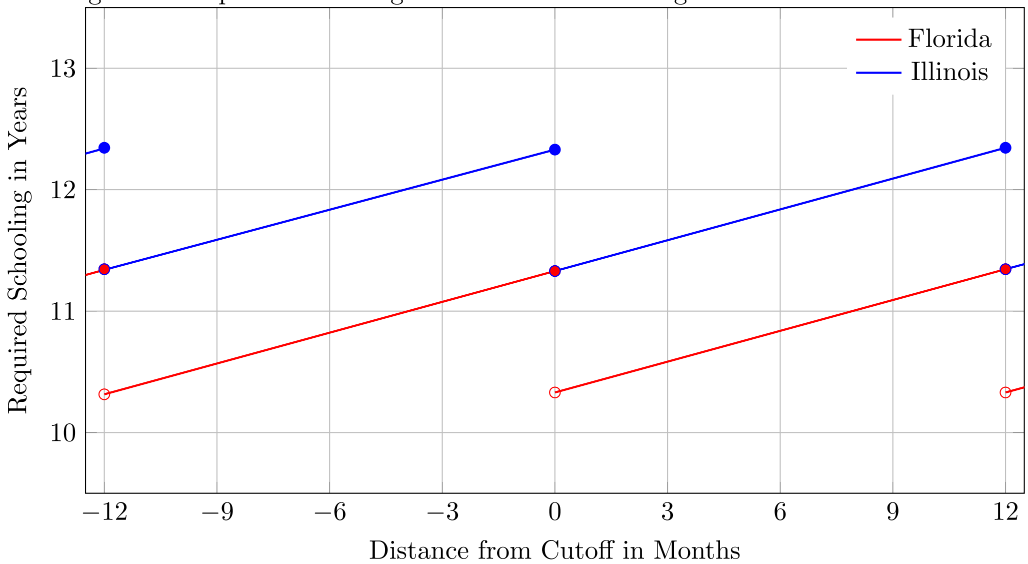
Figure 1: Cutoff Month by Calendar Year for Florida and Illinois



Note: The school cutoff always took place on the first of the month. Florida implemented the reform in the 1980–1981 school year while Illinois did so in the 1986–1987 school year. Based on information from Florida Senate Committee on Education Innovation (1999), Florida Legislature (1979), and Education Week (1987). A more detailed version is in table 6 in the appendix.

[3]This is under the assumption that parents always enrolled students in school when they were first eligible for kindergarten as compulsory schooling laws at the time allowed children to legally be out of school until six in Florida and seven in Illinois (Angrist and Krueger 1991; Florida Legislature 1970; Illinois General Assembly 1970). Failure to meet this assumption results in attenuation bias.

Figure 2: Required Schooling Relative to the Kindergarten Start Date Cutoff



Note: Required schooling is calculated as the difference in age between the age at which a child begins kindergarten, which is determined by the cutoff dates, and the age at which a child can opt out of schooling with permission. Students could opt out of schooling at the age of sixteen with a parent or guardian's permission during this period in Florida and at age seventeen in Illinois. A zoomed in version of this figure is included in figure 7 within the appendix.

III Data

I use public data from the Florida Offender-Based Information System (FOBIS) and Illinois Department of Corrections (IDOC). FOBIS includes all Florida prisoners admitted since 1981, while IDOC includes those in Illinois since 2005.[4] Most importantly, both datasets include the date of birth for all prisoners admitted so that their eligibility for kindergarten is known.[5] Other demographic information is available, including race, gender, and offense type.

Both FOBIS and IDOC data are reported at the prison-stay-level. I convert each of these datasets to be at the date-of-birth-level with each date of birth including the number of prisoners born on that day, as well as counts and proportions of demographics on a given date of birth. Each date of birth is then assigned a distance to the nearest kindergarten entry cutoff. I restrict data to only cohorts conceived by the announcement of the policy change who were subject to the new kindergarten

[4]The initially affected cohort in Illinois were twenty-four in 2005. This will attenuate the estimated effect in Illinois since I don't observe prison stays during early adult years for these individuals. This is shown in table 7 of the appendix.

[5]I make the implicit assumption that all prisoners observed grew up in the same state they went to prison in. This will attenuate my results since I do not have data on where prisoners were born nor where they started school.

cutoffs. This leaves five cohorts in each state.

Lastly, I use natality data from the National Vital Statistics System of the National Center for Health Statistics to test that births and other observables are smooth around the cutoff for the population. From these, I observe the race, age, and education of the mother and (sometimes) father. Other observables include: the child's race, prenatal care, and birth weight. These data are at the child-level which I convert to the date-of-birth-level and include counts as well as proportions of births in total and by demographic groups.

IV Identification Strategy

IV.A Births

Random assignment of births around school entry cutoffs is a common assumption in empirical economics (e.g. Bédard and Dhuey 2006, Black, Devereux, and Salvanes 2011, and McCrary and Royer 2011). However, the nature of the change in kindergarten school entry cutoffs in Florida and Illinois during this period make a stronger case for random assignment than if the cutoffs were constant. In both Florida and Illinois, these cutoffs changed after the affected cohorts were born or conceived making it impossible for parents to plan around new kindergarten entry cutoffs. This is important as any RDD requires that all other factors (observable and unobservable) evolve smoothly at the cutoff in order to identify the effect of treatment caused by the cutoff (Lee and Lemieux 2010).

I use equation 1 to test my assumption that all observable characteristics vary smoothly at the cutoff.

$$\begin{aligned} X_{ds} = \alpha_0 + \alpha_1\, \mathit{Delayed}_d + \beta_2\, (\mathit{DateOfBirth}_d - \mathit{Cutoff}_{ds}) \\ + \alpha_3\, \mathit{Delayed}_d \times (\mathit{DateOfBirth}_d - \mathit{Cutoff}_{ds}) + \mathit{Florida}_s + \nu_{ds} \end{aligned} \tag{1}$$

Here, X_{ds} is one of many observables including the number of births, average birth weight, average mother age, or average father age on day d in state s. $\mathit{Delayed}_d$ is an indicator equal to one if individuals born on day d in state s were delayed kindergarten entry a year due to the change in birthday cutoff for their cohort. If $\mathit{Delayed}_d$ equals one then individuals born on that day would have been subject to the original cutoff prior to the change, but are subject to the new cutoff due to the policy change. $DateOfBirth_d - Cutoff_{ds}$ represents the running variable which is the relative distance between the individual's birthday and their relevant kindergarten entry cutoff. $Florida_s$ is a state fixed effect equal to one for affected cohorts in Florida.

IV.B Adult Crime

Children in Florida and Illinois must turn five on or before the cutoff date of the school year in which they begin kindergarten. This creates a discontinuity where the running variable is the distance to this cutoff which is perfectly determined by one's date of birth. Children born on January 1 and January 2 in Florida are only one day apart, but the older child begins school a full year earlier. Additionally, because compulsory schooling laws are age-based, they become eligible to leave school only one day apart. Thus the cutoff alone causes those born after January 1 to receive a year less of required schooling. Similar examples hold for Illinois where the cutoff was initially December 1.

I exploit the discontinuity at each new cutoff within five years since all children were conceived before the policy was announced. This timing ensures that parents could not have timed births in response to the policy. Assuming that birthdays are as good as random around the new cutoffs gives appropriate exogeneity lending to a regression discontinuity design (RDD).[6]

I pool observations across the first five affected cohorts in each state since all children were already conceived when the change was announced. Unobservable differences in Florida and Illinois create level differences, so I include state fixed effects to account for time-invariant differences between states. I begin with a bandwidth of twenty-nine days around the cutoff and my RDD in equation 2.[7]

$$\begin{aligned} Y_{ds} = \beta_0 + \beta_1 \, Delayed_d + \beta_2 \, (DateOfBirth_d - Cutoff_{ds}) \\ + \beta_3 \, Delayed_d \times (DateOfBirth_d - Cutoff_{ds}) + Florida_s + \epsilon_{ds} \end{aligned} \tag{2}$$

Here, Y_{ds} is the number of prisoners born on date d who were admitted to prison at any time during the observation period. $Delayed_d$ and $Florida_s$ are the same as in equation 1. $DateOfBirth_d - Cutoff_{ds}$ is again the running variable being the relative distance between the individual's birthday

[6] This assumption is common in the literature (e.g. Angrist and Krueger 1991 and Cook and Kang 2016).

[7] Twenty-nine days is the largest bandwidth possible in order to keep the maximum amount of affected years for cohorts who were already conceived. This is because individuals born after September 30, 1983 had increased medicaid eligibility as studied in Arenberg, Neller, and Stripling (2024). A bandwidth of thirty or more leads to this confounding my identification in the window around the September 1, 1983 cutoff in Illinois.

and their relevant kindergarten entry cutoff.

β_1 captures the intent to treat (ITT) effect of being assigned a delayed kindergarten start based on date of birth and presumed state of schooling, proxied by state of incarceration. The ITT is identified under the assumption that births are as-good-as-random around the cutoff.

This effect would be the local average treatment effect (LATE) if I observed state of birth and education for all individuals as well as a strong monotonicity assumption. This assumption being that no parents choose to delay school entry if their child is born before the cutoff and would choose to petition for their child to start early if they were born after the cutoff. This assumption is inherently untestable, but if not met it would result in attenuation bias due to two-sided misclassification.

If β_1 is statistically significant, then there is a discontinuous change in the number of prisoners born just after the cutoff compared to before. A positive β_1 provides evidence of increased rates of incarceration due to being born after the cutoff. Thus the combination of being the oldest in your cohort receiving up to one year less of education increased the likelihood of incarceration.

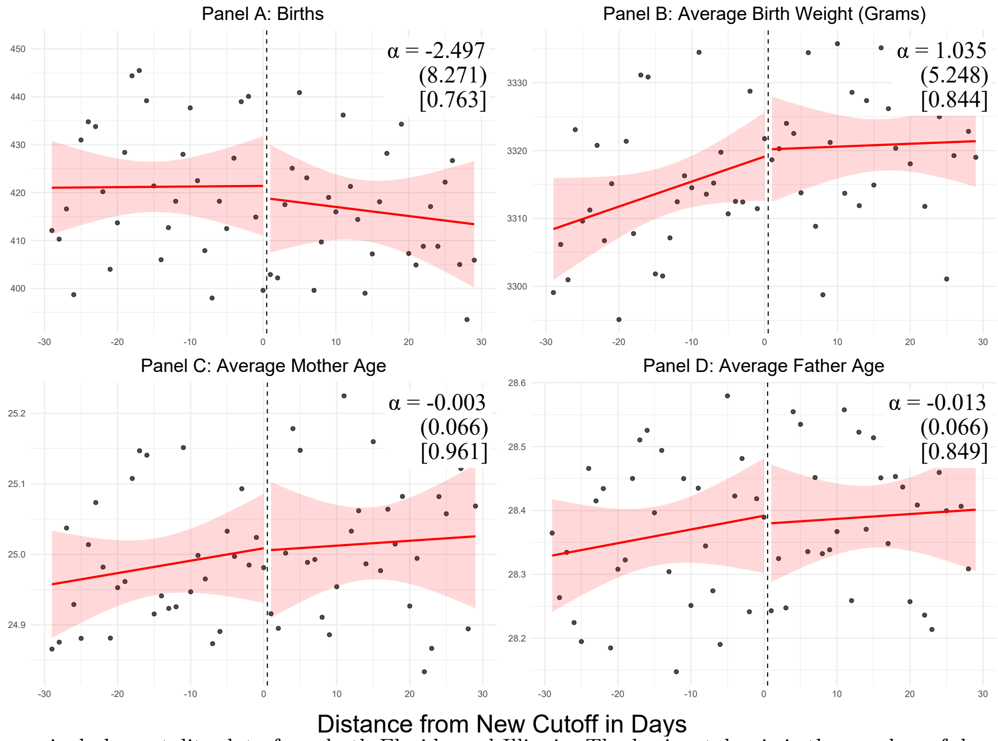
V Results

V.A Other Observables

I display the results of equation 1 where X_{ds} is either the average number of births, average birth weight, average mother age, or average father age in figure 3 and report the results in table 1. I find there to be no discontinuity in any of these observables which lends credence to my RDD being properly identified.

I also use the same equation 1 on the original cutoffs, that would have been used if not for the change, and report the results in figure 8 and table 8 in the appendix. Importantly, I do not observe the same smoothness of all observables around the original kindergarten entry cutoffs. I find mother and father's age to be different on each side of the cutoff. Children who were born just after the cutoff have younger mothers and fathers, on average. This gives an important confounder to the identification strategy if one were to use a cutoff which had been announced before conception.

Figure 3: Natality Regression Discontinuity Plot



Note: Figure includes natality data from both Florida and Illinois. The horizontal axis is the number of days to the new kindergarten entry cutoff and is normalized to zero. Individuals with a distance greater than zero from the cutoff were delayed in school entry due to the policy change. Zero corresponds to either December 1, 1975, November 1, 1976, October 1, 1977, September 1, 1978, or September 1, 1979 in Florida and either November 1, 1981, October 1, 1982, September 1, 1983, September 1, 1984, or September 1, 1985 in Illinois. The shaded area represents 95 percent pointwise confidence intervals around the fitted regression function, based on heteroskedasticity-robust standard errors clustered at the month-day level. The estimated coefficient is reported along with its standard error (in parentheses) and p-value (in brackets). RD results from each shown outcome are displayed in Table 1 with none being significant. Source: National Center for Health Statistics (1977-1985).

Table 1: Natality Regression Discontinuity Results

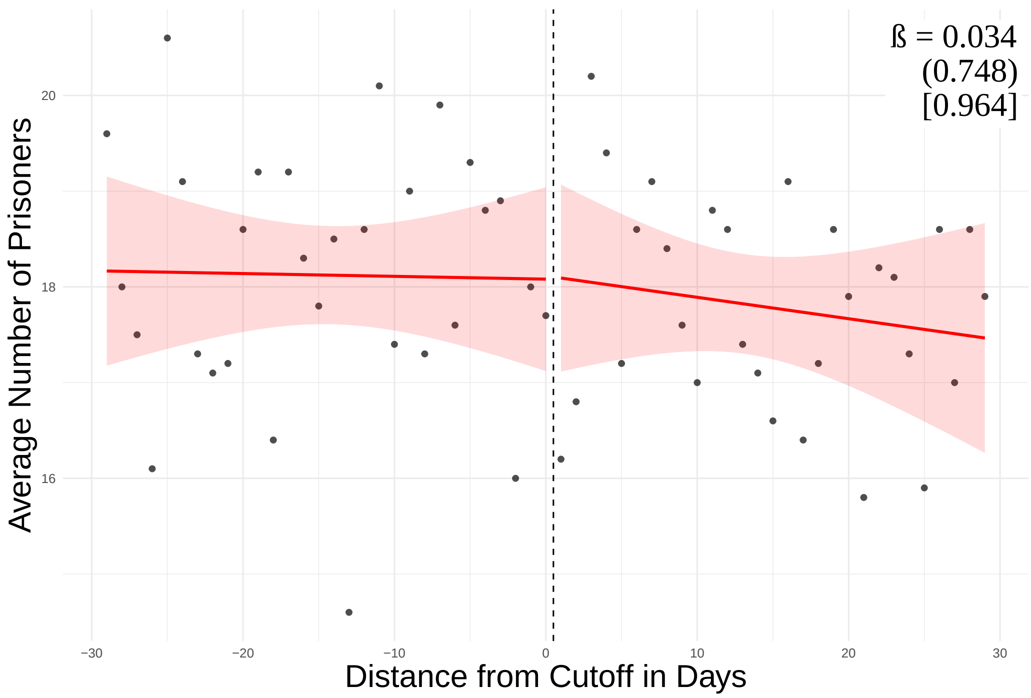
	Births	Birth Weight	Mother's Age	Father's Age
Delayed	-2.497	1.035	-0.003	-0.013
	(8.271)	(5.245)	(0.066)	(0.066)
Florida	-188.888***	-35.814***	-1.461***	-0.501***
	(2.764)	(2.639)	(0.025)	(0.031)
Bandwidth (Days)	29	29	29	29
N (Days of Birth)	590	590	590	590
Average	418.688	3,317.230	24.999	28.375

Note: Standard errors reported in parentheses are corrected for heteroskedasticity as presented in MacKinnon and White (1985). The dependent variable is indicated at the top of the column. *Delayed* is a dummy indicating that people born in that state on that date of birth had their kindergarten start date moved after they were born. The average of each outcome within the bandwidth is reported in the bottom row. *significant at 10%, **significant at 5%, ***significant at 1%. Source: National Center for Health Statistics (1977-1985).

V.B Whole Sample

I first check for a discontinuity among all prisoners. Figure 4 displays the RD plot of all prisoners relative to the cutoff. The first column of table 2 reports the results of the RDD from equation 2. I find no difference in the count of prisoners born on a given day due to the combination of education and relative age effects. Since it is common in the literature to find no aggregate effect (e.g. Landersø, Nielsen, and Simonsen 2015, Depew and Eren 2016, and McAdams 2016), I continue in the following section by examining heterogeneous effects among demographic groups.

Figure 4: Regression Discontinuity Plot of All Prisoners



Note: Figure includes data from both Florida and Illinois via FOBIS and IDOC, respectively. The horizontal axis is the number of days to the new kindergarten entry cutoff and is normalized to zero. Individuals with a distance greater than zero from the cutoff were delayed in school entry due to the policy change. Zero corresponds to either December 1, 1975, November 1, 1976, October 1, 1977, September 1, 1978, or September 1, 1979 in Florida and either November 1, 1981, October 1, 1982, September 1, 1983, September 1, 1984, or September 1, 1985 in Illinois. The shaded area represents 95 percent pointwise confidence intervals around the fitted regression function, based on heteroskedasticity-robust standard errors clustered at the month-day level. The estimated coefficient, β, from equation 2 is reported along with its standard error (in parentheses) and p-value (in brackets).

V.C Heterogeneity Between Demographic Groups

I examine heterogeneity by gender, race, and gender-race pairs using equation 2 on demographic subsamples. Because my unit of observation is the date of birth, splitting by demographics reduces the number of prison visits but does not reduce statistical power within the twenty-nine-day bandwidth. I report the results by gender in Table 2, race in Table 3, race for males in Table 4, and race for females in Table 5. Figures 5 and 6 display RD plots of males and females, respectively, by race.

I find the combination of one less year of required schooling and being among the oldest in the cohort, rather than one of the youngest, to increase the number of female prisoners born on a given day. I observe no statistically significant effect on men.

There is significant heterogeneity among females. White females see an increase in the average daily count of incarcerations by about 0.35 prisoners per birth date. This is a large effect relative to the mean ($\approx$ 28.9%). This finding is consistent with economic models of crime (Becker 1968 and Ehrlich 1973). White women born after the new kindergarten cutoff dates have less required schooling so they have less human capital and commit more crime due to lower opportunity costs than White women born just before the cutoff. While I cannot separate the peer-age and schooling mechanisms, the literature provides mixed evidence on which channel dominates for females.

Among Hispanic women, the combination of these human capital effects results in about a 0.1 fewer prisoners on a given date of birth. This is about a 70% reduction in incarcerations relative to the mean in Florida and Illinois. This follows most, although not all, of the literature which finds that starting school at an older age increases juvenile and adult crime (McAdams 2016, Cook and Kang 2016). However, it contrasts with seminal findings in the literature which show education to reduce crime and incarceration (e.g. Angrist and Krueger 1991 and Lochner and Moretti 2004). This result is likely driven by a relatively small subset of individuals born after the cutoff who went to prison.

All estimates should be interpreted as lower bounds due to data limitations. Migration across states and the absence of birthplace and schooling data mean that some individuals are misclassified with respect to treatment status. This attenuation implies that my intention-to-treat estimates understate the true effect of delayed school entry and reduced human capital on incarceration.

Table 2: Regression Discontinuity Results by Gender

	Everyone	Male	Female
Delayed	0.034	-0.350	0.383*
	(0.748)	(0.7000)	(0.212)
Florida	-7.814***	-7.664***	-0.149
	(0.328)	(0.323)	(0.119)
Bandwidth (Days)	29	29	29
N (Days of Birth)	590	590	590
Avg Prisoners	18.013	15.721	2.292

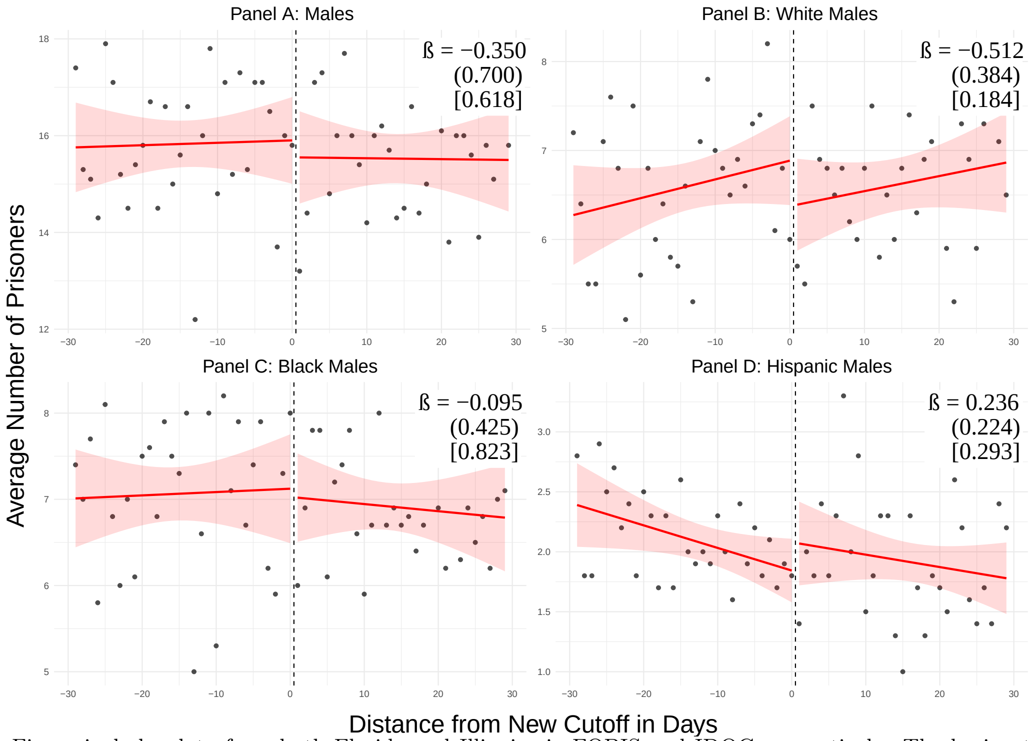
Note: Standard errors reported in parentheses are corrected for heteroskedasticity as presented in MacKinnon and White (1985). The dependent variable is the number of prisoners born a given number of days from the new cutoff, for each demographic group, in Florida and Illinois. *Delayed* is a dummy indicating that people born in that state on that date of birth had their kindergarten start date moved after they were born. The average number of prisoners born for a distance to the cutoff (within the bandwidth) is reported on the bottom row. *significant at 10%, **significant at 5%, ***significant at 1%.

Table 3: Regression Discontinuity Results by Race

	Everyone	White	Black	Hispanic	Asian	Native American
Delayed	0.034	-0.095	-0.045	0.129	0.019	-0.006
	(0.748)	(0.455)	(0.437)	(0.223)	(0.034)	(0.019)
Florida	-7.1814***	0.071	-5.264***	-2.522***	-0.064***	-0.020*
	(0.328)	(0.233)	(0.213)	(0.122)	(0.016)	(0.012)
Bandwidth (Days)	29	29	29	29	29	29
N (Days of Birth)	590	590	590	590	590	590
Avg Prisoners	18.013	8.115	7.595	2.193	0.036	0.020

Note: Standard errors reported in parentheses are corrected for heteroskedasticity as presented in MacKinnon and White (1985). The dependent variable is the number of prisoners born a given number of days from the new cutoff, for each demographic group, in Florida and Illinois. *Delayed* is a dummy indicating that people born in that state on that date of birth had their kindergarten start date moved after they were born. The average number of prisoners born for a distance to the cutoff (within the bandwidth) is reported on the bottom row. *significant at 10%, **significant at 5%, ***significant at 1%.

Figure 5: Regression Discontinuity Plots of Male Prisoners by Race



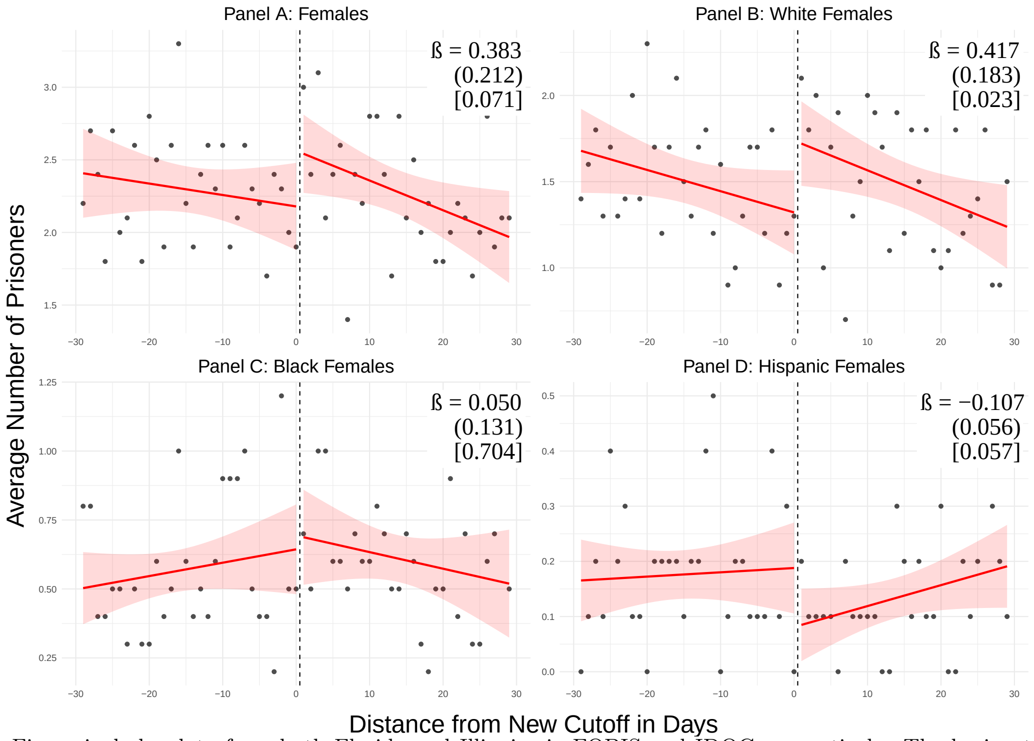
Note: Figure includes data from both Florida and Illinois via FOBIS and IDOC, respectively. The horizontal axis is the number of days to the new kindergarten entry cutoff and is normalized to zero. Individuals with a distance greater than zero from the cutoff were delayed in school entry due to the policy change. Zero corresponds to either December 1, 1975, November 1, 1976, October 1, 1977, September 1, 1978, or September 1, 1979 in Florida and either November 1, 1981, October 1, 1982, September 1, 1983, September 1, 1984, or September 1, 1985 in Illinois. The shaded area represents 95 percent pointwise confidence intervals around the fitted regression function, based on heteroskedasticity-robust standard errors clustered at the month-day level. The estimated coefficient, β, from equation 2 is reported along with its standard error (in parentheses) and p-value (in brackets). Panel A provides an RD plot of only male prisoners with panels B, C, and D representing subsets of the whole male sample.

Table 4: Male Regression Discontinuity Results by Race

	All Males	White	Black	Hispanic	Asian	Native American
Delayed	-0.350	-0.512	-0.095	0.236	0.017	-0.004
	(0.700)	(0.384)	(0.425)	(0.224)	(0.034)	(0.004)
Florida	-7.664***	-0.081	-5.092***	-2.437***	-0.068***	0.003
	(0.323)	(0.216)	(0.195)	(0.121)	(0.015)	(0.006)
Bandwidth (Days)	29	29	29	29	29	29
N (Days of Birth)	590	590	590	590	590	590
Avg Prisoners	15.721	6.613	7.003	2.034	0.034	0.005

Note: Standard errors reported in parentheses are corrected for heteroskedasticity as presented in MacKinnon and White (1985). The dependent variable is the number of prisoners born a given number of days from the new cutoff, for each demographic group, in Florida and Illinois. *Delayed* is a dummy indicating that people born in that state on that date of birth had their kindergarten start date moved after they were born. The average number of prisoners born for a distance to the cutoff (within the bandwidth) is reported on the bottom row. *significant at 10%, **significant at 5%, ***significant at 1%.

Figure 6: Regression Discontinuity Plots of Female Prisoners by Race



Note: Figure includes data from both Florida and Illinois via FOBIS and IDOC, respectively. The horizontal axis is the number of days to the new kindergarten entry cutoff and is normalized to zero. Individuals with a distance greater than zero from the cutoff were delayed in school entry due to the policy change. Zero corresponds to either December 1, 1975, November 1, 1976, October 1, 1977, September 1, 1978, or September 1, 1979 in Florida and either November 1, 1981, October 1, 1982, September 1, 1983, September 1, 1984, or September 1, 1985 in Illinois. The shaded area represents 95 percent pointwise confidence intervals around the fitted regression function, based on heteroskedasticity-robust standard errors clustered at the month-day level. The estimated coefficient, β, from equation 2 is reported along with its standard error (in parentheses) and p-value (in brackets). Panel A provides an RD plot of only female prisoners with panels B, C, and D representing subsets of the whole female sample.

Table 5: Female Regression Discontinuity Results by Race

	All Females	White	Black	Hispanic	Asian	Native American
Delayed	0.383*	0.417**	0.050	-0.107*	0.002	-0.002
	(0.212)	(0.183)	(0.131)	(0.056)	(0.002)	(0.018)
Florida	-0.149	0.153	-0.173***	-0.095***	0.003	-0.024**
	(0.119)	(0.097)	(0.060)	(0.030)	(0.003)	(0.010)
Bandwidth (Days)	29	29	29	29	29	29
N (Days of Birth)	590	590	590	590	590	590
Avg Prisoners	2.292	1.502	0.592	0.159	0.002	0.015

Note: Standard errors reported in parentheses are corrected for heteroskedasticity as presented in MacKinnon and White (1985). The dependent variable is the number of prisoners born a given number of days from the new cutoff, for each demographic group, in Florida and Illinois. *Delayed* is a dummy indicating that people born in that state on that date of birth had their kindergarten start date moved after they were born. The average number of prisoners born for a distance to the cutoff (within the bandwidth) is reported on the bottom row. *significant at 10%, **significant at 5%, ***significant at 1%.

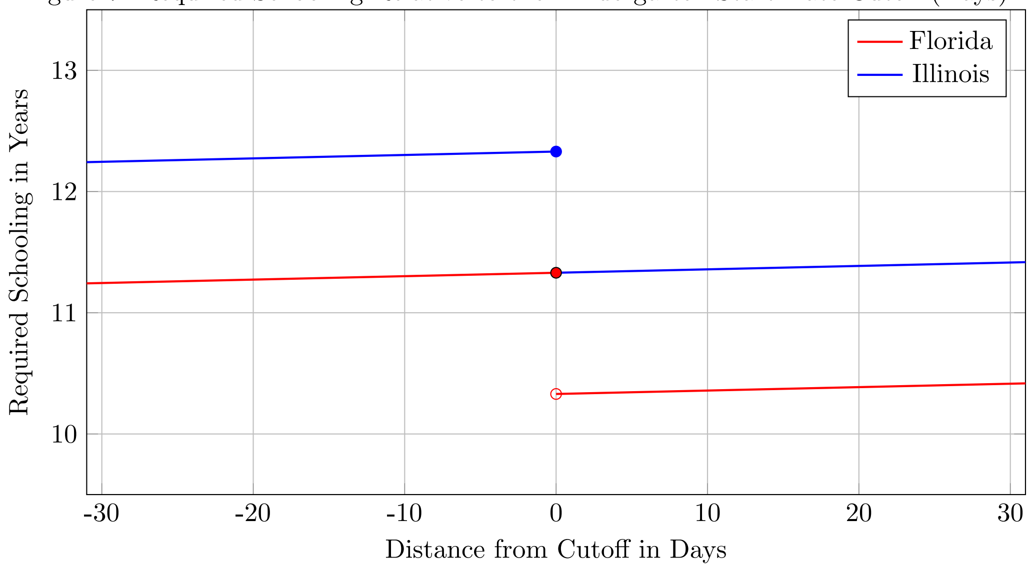
VI Appendix

Table 6: Kindergarten Cutoff Birthday by State and Year

State	Pre-1980	1980	1981	1982	1983	1984-1985	1986	1987	1988	Post-1988
Florida	Jan 1	Dec 1	Nov 1	Oct 1	Sept 1	Sept 1	Sept 1	Sept 1	Sept 1	Sept 1
Illinois	Dec 1	Dec 1	Dec 1	Dec 1	Dec 1	Dec 1	Nov 1	Oct 1	Sept 1	Sept 1

Note: Based on information from Florida Senate Committee on Education Innovation (1999), Florida Legislature (1979), and Education Week (1987).

Figure 7: Required Schooling Relative to the Kindergarten Start Date Cutoff (Days)



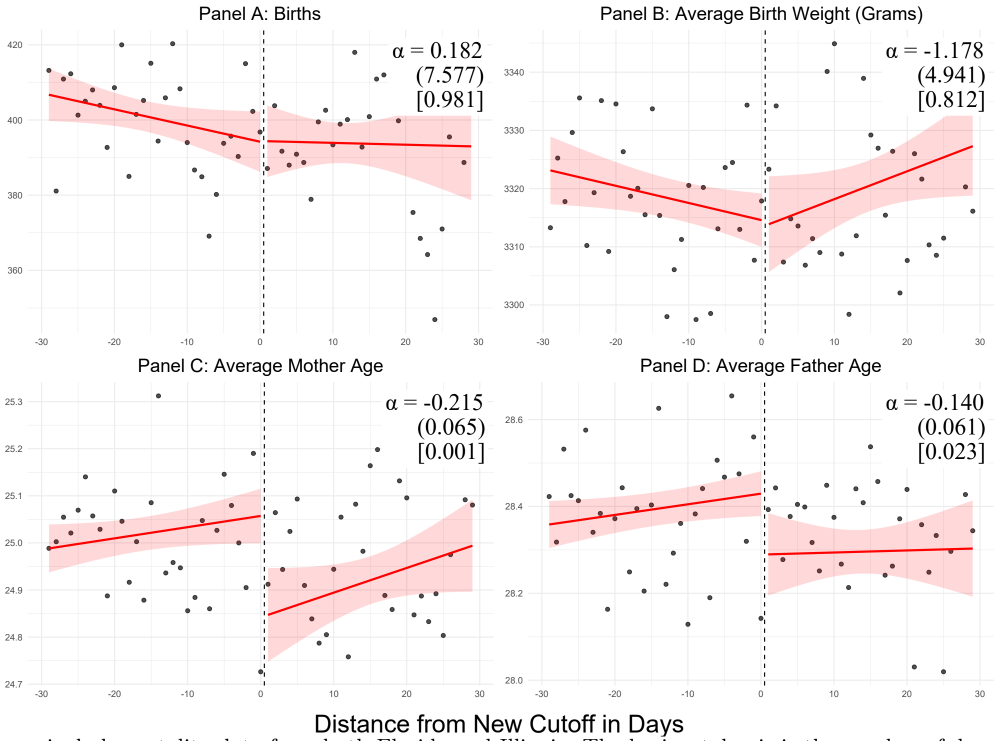
Note: The school cutoff always took place on the first of the month. Florida implemented the reform in the 1980 – 1981 school year while Illinois did so in the 1986 – 1987 school year. These school years serve as year 0 in the figure. Required schooling is calculated as the difference in age between the age at which a child begins kindergarten, which is determined by the cutoff dates, and the age at which a child can opt out of schooling with permission. Students could opt out of schooling at the age of sixteen with a parent or guardian's permission during this period in both Florida and Illinois.

Table 7: Prison Admissions Data Available in Illinois by Age

Birthday Cohort	20	21	22	23	24	25	26
Dec 2, 1979 – Dec 1, 1980						X	X
Dec 2, 1980 – Nov 1, 1981					X	X	X
Nov 2, 1981 – Oct 1, 1982				X	X	X	X
Oct 2, 1982 – Sept 1, 1983			X	X	X	X	X
Sept 2, 1983 – Sept 1, 1984		X	X	X	X	X	X
Sept 2, 1984 – Sept 1, 1985	X	X	X	X	X	X	X
Sept 2, 1985 – Sept 1, 1986	X	X	X	X	X	X	X

Note: Illinois Department of Corrections (IDOC) prison admissions data is only available from 2005 onward. As a result, the number of observed years of criminal history increases for later cohorts. I observe admissions data starting at age 24 for the first cohort affected by the policy, and gain an additional year for each subsequent cohort.

Figure 8: Natality Regression Discontinuity Plot Around Original Cutoffs



Note: Figure includes natality data from both Florida and Illinois. The horizontal axis is the number of days to the new kindergarten entry cutoff and is normalized to zero. Individuals with a distance greater than zero from the cutoff were delayed in school entry due to the policy change. Zero corresponds to either January 1, 1975, January 1, 1976, January 1, 1977, January 1, 1978, or January 1, 1979 in Florida and either January 1, 1981, January 1, 1982, January 1, 1983, January 1, 1984, or January 1, 1985 in Illinois. The shaded area represents 95 percent pointwise confidence intervals around the fitted regression function, based on heteroskedasticity-robust standard errors clustered at the month-day level. The estimated coefficient is reported along with its standard error (in parentheses) and p-value (in brackets). RD results from each shown outcome are displayed in Table 8. Source: National Center for Health Statistics (1977-1985).

Table 8: Natality Regression Discontinuity Results Around Original Cutoffs

	Births	Birth Weight	Mother's Age	Father's Age
Delayed	0.182	-1.178	-0.215***	-0.140***
	(7.576)	(4.941)	(0.065)	(0.061)
Florida	-162.728***	-36.891	-1.488	-0.539
	(2.861)	(2.762)	(0.030)	(0.033)
Bandwidth (Days)	29	29	29	29
N (Days of Birth)	590	590	590	590
Average	396.549	3,318.676	24.990	28.363

Note: Standard errors reported in parentheses are corrected for heteroskedasticity as presented in MacKinnon and White (1985). The dependent variable is indicated at the top of the column. *Delayed* is a dummy indicating that people born in that state on that date of birth had their kindergarten start date moved after they were born. The average of each outcome within the bandwidth is reported in the bottom row. *significant at 10%, **significant at 5%, ***significant at 1%. Source: National Center for Health Statistics (1977-1985).

References

Anderson, D. M. (2014). "In School and Out of Trouble? The Minimum Dropout Age and Juvenile Crime". *Review of Economics and Statistics* 96.2, pp. 318–331.

Angrist, J. D. and A. B. Krueger (1991). "Does Compulsory School Attendance Affect Schooling and Earnings?" *Quarterly Journal of Economics* 106.4, pp. 979–1014.

Arenberg, S., S. Neller, and S. Stripling (2024). "The Impact of Youth Medicaid Eligibility on Adult Incarceration". *American Economic Journal: Applied Economics* 16.1, pp. 121–156.

Baron, E. J., J. M. Hyman, and B. N. Vasquez (2024). "Public School Funding, School Quality, and Adult Crime". *Review of Economics and Statistics* 1.1, pp. 1–46. DOI: 10.1162/rest_a_01452.

Becker, G. S. (1968). "Crime and Punishment: An Economic Approach". *Journal of Political Economy* 76.2, pp. 169–217.

Bédard, K. and E. Dhuey (2006). "The Persistence of Early Childhood Maturity: International Evidence of Long-Run Age Effects". *Quarterly Journal of Economics* 121.4, pp. 1437–1472.

Black, S. E., P. J. Devereux, and K. G. Salvanes (2011). "Too Young to Leave the Nest? The Effects of School Starting Age". *Review of Economics and Statistics* 93.2, pp. 455–467.

Cook, P. J. and S. Kang (2016). "Birthdays, Schooling, and Crime: Evidence from a Regression Discontinuity Design". *Economic Journal* 126.593, pp. 107–134.

Depew, B. and Ö. Eren (2016). "Born on the Wrong Day? School Entry Age and Juvenile Crime". *Journal of Urban Economics* 96, pp. 73–90. DOI: 10.1016/j.jue.2016.01.005.

Dobkin, C. and F. Ferreira (2010). "Do School Entry Laws Affect Educational Attainment and Labor Market Outcomes?" *Economics of Education Review* 29.1, pp. 40–54.

Education Commission of the States (2014). "State Kindergarten Policies". *Education Commission of the States Reports* 2014. URL: https://www.ecs.org/clearinghouse/79/58/7958.pdf.

Education Week (June 1987). "Legislatures, Districts Move to Raise Age for Kindergarten". *Education Week*. URL: https://www.edweek.org/education/legislatures-districts-move-to-raise-age-for-kindergarten/1987/06.

Ehrlich, I. (1973). "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation". *Journal of Political Economy* 81.3, pp. 521–565.

Florida Legislature (1970). "Florida Statutes, Title XLVIII, Chapter 232: Compulsory School Attendance". *State of Florida Legislative Documents*. No amendments raising the dropout age from 1970 to 1999. URL: http://www.leg.state.fl.us/Statutes/.

Florida Legislature (1979). "Summary of General Legislation: 1979". *Florida State University Law Library Digital Collections*. URL: https://library.law.fsu.edu/Digital-Collections/FLSumGenLeg/FlSumGenLeg1979.pdf.

Florida Senate Committee on Education Innovation (1999). "Bill Analysis and Economic Impact Statement: House Bill 307". *Florida Senate Bill Analyses*. URL: https://www.flsenate.gov/Session/Bill/1999/307/Analyses/19990307HEDK_HB0307S1.EDK.pdf.

Fredriksson, P. and B. Ockert (2013). "Life-Cycle Effects of Age at School Start". *Economic Journal* 124.579, pp. 977–1004.

Hjalmarsson, R., H. Holmlund, and M. J. Lindquist (2015). "The Effect of Education on Criminal Convictions and Incarceration: Causal Evidence from Micro-data". *Economic Journal* 125.587, pp. 1290–1326.

Illinois General Assembly (1970). "School Code of Illinois: Compulsory Attendance Law (105 ILCS 5/26-1)". *State of Illinois Legislative Documents*. No amendments raising the dropout age from 1970 to 1999. URL: https://www.ilga.gov/legislation/ilcs/ilcs5.asp?ActID=1005&ChapterID=17.

Illinois State Board of Education (1983). "Compulsory Attendance Mandate Report and Preliminary Recommendations: The Age of Leaving School".

Knapp, D. et al. (2025). "Gateway Policy Explorer: USA Compulsory Schooling Policy Details, 1900–2024".

Landersø, R., H. S. Nielsen, and M. Simonsen (2015). "School Starting Age and the Crime-Age Profile". *Economic Journal* 127.602, pp. 1096–1118.

Lee, D. S. and T. Lemieux (2010). "Regression Discontinuity Designs in Economics". *Journal of Economic Literature* 48.2, pp. 281–355. DOI: 10.1257/jel.48.2.281.

Lochner, L. (2004). "Education, Work, and Crime: A Human Capital Approach". *International Economic Review* 45.3, pp. 811–843.

Lochner, L. and E. Moretti (2004). "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports". *American Economic Review* 94.1, pp. 155–189.

MacKinnon, J. G. and H. White (1985). "Some Heteroskedasticity-Consistent Covariance Matrix Estimators with Improved Finite Sample Properties". *Journal of Econometrics* 29.3, pp. 305–325. DOI: 10.1016/0304-4076(85)90158-7.

McAdams, J. M. (2016). "The Effect of School Starting Age Policy on Crime: Evidence from U.S. Microdata". *Economics of Education Review* 54, pp. 227–241.

McCrary, J. and H. 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, pp. 158–195. DOI: 10.1257/aer.101.1.158.

National Center for Health Statistics (1977-1985). *Natality Data Files, 1977–1985*. Accessed via the National Bureau of Economic Research (NBER). URL: https://www.nber.org/research/data/citing-nchs-data.

Whaley, M. (1985). "The Status of Kindergarten: A Survey of the States". *Illinois State Board of Education, Department of Planning, Research and Evaluation*. URL: https://files.eric.ed.gov/fulltext/ED260835.pdf.