

When Did Inmates Start School?

The Long-Run Criminal Consequences of Kindergarten Cutoff Dates

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Working Paper[†]

Abstract

I exploit exogenous variation in kindergarten entry cutoff dates based on date of birth and use administrative prison data within a regression discontinuity design (RDD) to measure their effect on adult crime. I draw on data from Florida and Illinois, two states that gradually moved their kindergarten eligibility cutoff dates earlier by one month each year during the 1980s. Kindergarten entry cutoffs create quasi-random variation in school starting age. Children born just after the cutoff are required to delay entry by a year, which reduces their required schooling since compulsory schooling laws are age based and alters their peer environment during formative years. Leveraging temporal variation in school entry cutoffs, I link school start age to incarceration outcomes observed decades later and find that delayed entry increases imprisonment among females by over 20% with this effect concentrated among White women.

JEL: I21, K42, J24

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[†]Last updated on October 24, 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. Kindergarten entry cutoffs, which determine when a child is eligible to start school, affect criminal activity through later educational attainment.

I exploit changes in kindergarten entry cutoff dates to examine how they affect, through human capital, lifetime criminal behavior. Florida and Illinois each moved their kindergarten entry cutoffs earlier in the school year during the 1980s while leaving unchanged the compulsory schooling laws that require school attendance up to a minimum age. The cutoff dates therefore provide the sole source of variation in required schooling among compliers, creating a discontinuity where the running variable is the individual’s date of birth.¹ As the cutoffs moved earlier in the year, children born just after the new cutoff were required to delay kindergarten entry by a full year but could drop out at the same age.

Several papers use compulsory schooling laws to understand how exogenous factors affecting education influence outcomes such as adult earnings (e.g., [Angrist and Krueger 1991](#), [Oreopoulos 2006](#), and [Brunello, Fort, and Weber 2009](#)), arrests and incarceration (e.g., [Lochner and Moretti 2004](#), [Hjalmarsson, Holmlund, and Lindquist 2015](#), and [Bell, Costa, and Machin 2022](#)), and other outcomes (e.g., [Lleras-Muney 2005](#) and [Clark and Royer 2013](#)). Fewer papers exploit kindergarten entry cutoff dates with [Cook and Kang \(2016\)](#) being the most similar to this paper. They exploit kindergarten entry cutoff dates to instrument for education and find that being born just after the cutoff decreases high school enrollment and increases convictions in early adulthood. However, data limitations prevented them from estimating any criminal effects of cutoff dates after the age of nineteen. [McAdams \(2016\)](#) uses a difference-in-differences model to measure the effect of being born after the cutoff on incarceration in adulthood. He finds those born in the quarter after the

¹Demographic groups with higher crime rates are also the most likely to be constrained by compulsory schooling laws ([Lochner and Moretti 2004](#)).

cutoff to have higher rates of incarceration in adulthood. Importantly, [McAdams \(2016\)](#) observes incarceration status of the individual during a single Census whereas I examine lifetime incarceration outcomes using exact dates of birth. In doing so, I provide new evidence on the long run criminal consequences of school entry cutoffs. This paper focuses on Florida and Illinois, where reforms to kindergarten entry cutoffs in the 1980s created exogenous variation in school entry.

In this paper, I use administrative prison population data from each state, which include exact dates of birth, and I use a regression discontinuity design (RDD) where the running variable is the distance to the cutoff.² I use this setup to measure the effect that being delayed kindergarten start by one year has on an individual’s lifetime criminal history. Particularly unique to my paper are the changing kindergarten entry cutoff dates, which occurred after conception. This eliminates parents’ ability to time their child’s birth relative to the cutoff date since their child was already conceived.

I find changes in the kindergarten cutoff date cause more crime among females who experience delayed entry. I observe no effect for males, but substantial heterogeneity among females. I estimate a more than 20% increase in female prisoners born on any given day after the cutoff with this effect concentrated in white females, who have a 31% increase. Importantly, I observe no difference in either the number of births or the characteristics of births on each side of the changing cutoffs, meaning the effect can be attributed to the change in cutoff dates rather than to changes in births.

II Institution

School entry cutoffs refer to the date used to determine whether a child starts school in that year or must wait until the following year.³ 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 in that same school year. Prior to 1979, 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 its inception in 1965 and 1895 for Florida and Illinois, respectively ([Florida Senate Committee on Education Innovation 1999](#) and [Education Week 1987](#)). Florida enacted the change in the cutoff from January 1 to September 1 over four years in

²My model is nearly identical to [Cook and Kang \(2016\)](#) and [Arenberg, Neller, and Stripling \(2024\)](#).

³I use kindergarten and school entry cutoffs synonymously.

June 1979 ([Florida Legislature 1979](#)). This initially moved the kindergarten cutoff date to December 1, rather than January 1, during the 1980–1981 school year and delayed the school entry for children born between December 2, 1975 and January 1, 1976 (inclusive). Each subsequent school year had a cutoff one month earlier than the previous one until the cutoff remained at September 1 beginning in the 1983–1984 school year ([Whaley 1985](#)).

Illinois had a similar change in its kindergarten cutoff caused by amendments to the Illinois School Code (105-ILCS 5) in September 1985 ([Education Week 1987](#)). This moved the cutoff by one month per year for three years. This first affected the cohort beginning kindergarten in the 1986–1987 school year, for whom the cutoff date moved from December 1 to November 1. This delayed school entry 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](#) and a more detailed version in [Table 3](#) of the [appendix](#).⁴ Once the cutoff date was moved to September 1, it remained unchanged and continues to serve as the kindergarten entry cutoff in both Florida and Illinois ([Education Commission of the States 2014](#)).

Importantly, neither Florida nor Illinois experienced changes in compulsory schooling laws during the sample period ([Florida Legislature 1970](#); [Illinois General Assembly 1970](#); [Illinois State Board of Education 1983](#); [Angrist and Krueger 1991](#); [Knapp et al. 2025](#)). Students in Florida and Illinois could not drop out until age sixteen and required a parent or guardian’s permission.⁵ Upon turning eighteen in either state, individuals are considered adults and can leave school without permission from a parent or guardian. Thus, all variation in required schooling for these individuals is determined solely by the time they can enroll in kindergarten.⁶

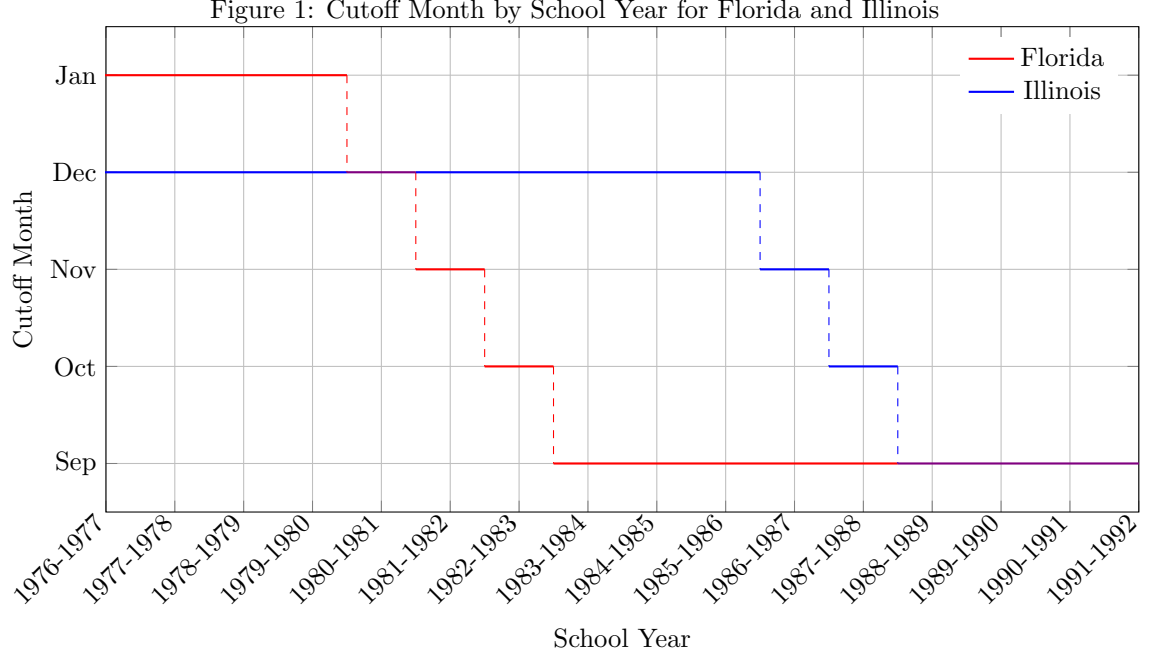
The combination of kindergarten entry cutoffs and compulsory schooling laws generates a discontinuity in required schooling based on a child’s date of birth. [Figure 2](#) illustrates this discontinuity, which arises solely from changes in kindergarten entry cutoffs in Florida and Illinois, as minimum

⁴Those delayed by these policies 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.

⁵Illinois has since moved the minimum dropout age to 17 for those born after August 31, 1989.

⁶This 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.

school leaving ages remained constant during this period.



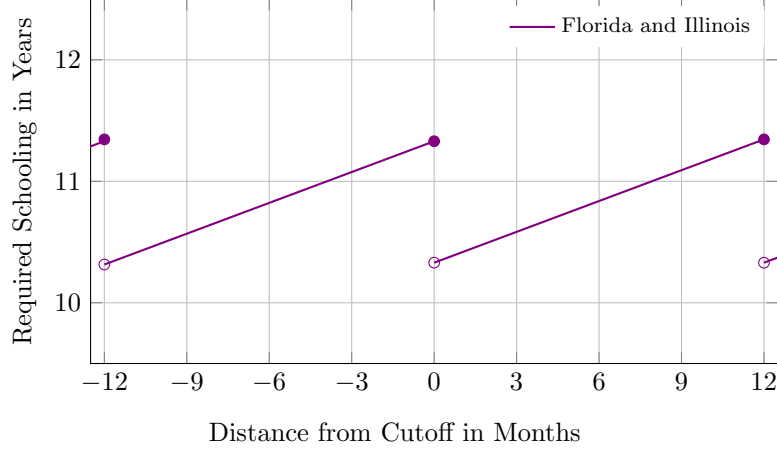
Note: The school cutoff always took place on the first of the month. Florida implemented the reform in the 1980–1981 school year while Illinois did so in the 1986–1987 school year. Based on information from [Florida Senate Committee on Education Innovation \(1999\)](#), [Florida Legislature \(1979\)](#), and [Education Week \(1987\)](#). A more detailed version is in table 3 in the [appendix](#).

III Theory

I model the setting in Florida and Illinois using a simple two period framework linking kindergarten entry cutoffs to later criminal behavior. This model is based on [Becker \(1968\)](#) and [Lochner \(2004\)](#), in which heterogeneous individuals receive schooling in the first period and make a decision between legal or illegal work in the second period. Their choice of labor supply is determined by the expected payoff of crime relative to their certain payoff of working.

Each individual i has two possible schooling outcomes: high schooling S_i^H and low schooling S_i^L , where $S_i^L < S_i^H$. Each individual has human capital $h_i = g_i(S_i, Z_i)$, where S_i denotes schooling and Z_i represents other factors uncorrelated with schooling which affect human capital. The function g_i

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.

is strictly increasing in schooling. An individual's human capital determines their legal wage offer $w_i(h_i)$, which is strictly increasing in their human capital h_i .

$$h_i = g_i(S_i, Z_i) : \frac{\partial g_i(S_i, Z_i)}{\partial S_i} > 0 \forall S_i \quad (1)$$

Every individual i has the opportunity to work either in the legal labor market or the illegal labor market. If one chooses the illegal labor market (i.e. crime) they receive a benefit B_i at the risk of apprehension and punishment. All individuals have an equal probability of apprehension $p \in (0, 1)$ and identical punishment F . This results in the total payoff from crime, π_i^C , which depends on the benefit and the expected punishment.⁷ Individuals choose to commit crime if their payoff, π_i^C , is strictly greater than the payoff from legal work, $w_i(h_i)$.⁸ When the individual chooses crime $c_i(h_i) = 1$.

⁷I assume the benefit of crime B_i is independent of schooling and human capital, consistent with the standard literature (e.g., [Becker 1968](#), [Ehrlich 1973](#), and [Lochner 2004](#)). This assumption can be relaxed without altering equation 7. It is sufficient that additional schooling raises legal earnings more than criminal returns, that is $\frac{dB_i(h_i)}{dS_i} \leq \frac{dw(h_i)}{dS_i}$ for all S_i .

⁸Here I assume individuals are risk averse at the inflection point.

$$c_i(h_i) := \begin{cases} 1 & \pi_i^C > w_i(h_i) \\ 0 & \text{otherwise} \end{cases} \quad (2)$$

Individuals' level of schooling is partially determined by their date of birth relative to the kindergarten entry cutoff date. Those born before the cutoff receive $S_i = S_i^H$ for certain whereas those born after receive S_i^L with a probability of q_i where $q_i \in (0, 1)$.⁹ Therefore the cutoff alone changes the distribution of schooling toward lower attainment than those individuals would have had if they were born before the cutoff, consistent with the literature (e.g., [Dobkin and Ferreira 2010](#), [Cook and Kang 2016](#), and [Kaplan, Spenkuch, and Tuttle 2025](#)).

$$Pr(S_i = S_i^L | \text{Born after cutoff}) = q_i, \quad Pr(S_i^H | \text{Born after cutoff}) = 1 - q_i, \quad q_i \in (0, 1) \quad (3)$$

Since the decision rule is determined by the realization of S_i and schooling only affects the legal wage offer through human capital, an individual who commits crime when they have high schooling, S_i^H would also have chosen to commit crime if they had low schooling, S_i^L . Thus, the implication below holds, but the reverse does not.

$$c_i(h_i^H) = 1 \Rightarrow c_i(h_i^L) = 1 \quad (4)$$

The probability of crime for those born before the cutoff is equal to $c_i(h_i^H)$.¹⁰ The probability of crime for individuals born after the cutoff is a convex combination of the two decision rules, $c_i(h_i^L)$ and $c_i(h_i^H)$, weighted by the probability of low education, q_i .¹¹ This relationship is expressed in equation 6.

$$Pr(c_i(h_i) = 1 | \text{Born before cutoff}) = c_i(h_i^H) \quad (5)$$

$$Pr(c_i(h_i) = 1 | \text{Born after cutoff}) = q_i c_i(h_i^L) + (1 - q_i) c_i(h_i^H) \quad (6)$$

⁹I assume individuals born before the cutoff receive their otherwise optimal amount of schooling and those born after receive less, on average, only due to the kindergarten entry cutoff. For example, there is nothing different about them or their environment which causes them to choose less education.

¹⁰This is because there is no uncertainty. An individual's probability of crime is 1 when their benefit from crime, $\pi_i(h_i^H)$, is greater than their wage, $w_i(h_i^H)$, and is zero otherwise.

¹¹The uncertainty in crime for those born after the cutoff comes from the random assignment of schooling, not from any uncertainty in individuals' decisions in the second period.

Implication 4 results in $c_i(h_i^L) \geq c_i(h_i^H)$. It follows that the probability of crime for those born after the cutoff is always at least as high as it would have been if they were born before the cutoff. This is shown in equation 7.

$$Pr(c_i(h_i) = 1 | \text{Born after cutoff}) \geq Pr(c_i(h_i) = 1 | \text{Born before cutoff}) \quad (7)$$

In total, my simple model demonstrates how cutoff-induced delays in school entry raise the probability of receiving less schooling, which reduces human capital and wages and in turn raises the likelihood of crime. The size of this effect depends on both the probability of reduced schooling, q_i , and the wage gap between high and low schooling outcomes, $w_i(h_i^H) - w_i(h_i^L)$. By shifting the distribution of schooling in a quasi-random manner, kindergarten entry cutoffs generate discontinuities in long run crime outcomes that I identify using a regression discontinuity design. Although my model omits other possible mechanisms such as peer effects or differences across crime types, it highlights the primary channel: reductions in required schooling lower human capital and increase criminal involvement.

IV Data

I use public data from the Florida Offender Based Information System (FOBIS) and the Illinois Department of Corrections (IDOC). FOBIS includes all Florida prisoners admitted since 1981, while IDOC includes those in Illinois since 2005.¹² Most importantly, both datasets include the date of birth for all prisoners admitted so that their eligibility for kindergarten is known.¹³ 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 of prisoners by demographics on a given date of birth. Each date of birth

¹²The 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 4 of the [appendix](#).

¹³I make the implicit assumption that all prisoners observed grew up in the same state they went to prison in. Similar assumptions are made in the literature ([Cook and Kang 2016](#) and [McAdams 2016](#)) and will attenuate my results since I do not have data on where prisoners were born nor where they started school.

is then assigned a distance to the nearest kindergarten entry cutoff. I restrict data to only cohorts conceived before the announcement of the policy change that were subject to the new kindergarten cutoffs. This leaves five cohorts per state.

I use natality data from the National Vital Statistics System (NVSS) of the National Center for Health Statistics and the American Community Survey (ACS) to test whether observed factors are smooth around the cutoff dates. From the NVSS data, I observe the race, age, and education of the mother and (sometimes) father.¹⁴ 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. The ACS data remain at the individual level, but the date of birth is reported only as the quarter of birth. ACS data report state of birth and current state of residence at the time of survey. Using these, I see whether someone moved into or out of either Florida or Illinois. I include more about how the NVSS and ACS datasets are used for validity checks in Section [V.B](#).

V Identification Strategy

V.A 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 the cutoff, which is perfectly determined by one's date of birth and location. 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 one 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 because these children were conceived before the policy was announced. Assuming that birthdays are as good as random around

¹⁴Generally, data on the father is only included in the NVSS when the father is either (i) married to the mother of the child and present at the birth or (ii) present at the birth and the father voluntarily provides the information.

the new cutoffs gives appropriate exogeneity supporting the use of a regression discontinuity design (RDD).¹⁵

I pool observations across the first five affected cohorts. I use the RDD in equation 8, where my preferred specification uses a triangular kernel and a twenty-nine day bandwidth.¹⁶

$$Y_{ds} = \beta_0 + \beta_1 \text{Delayed}_{ds} + \beta_2 (\text{DateOfBirth}_d - \text{Cutoff}_{ds}) + \beta_3 \text{Delayed}_{ds} \times (\text{DateOfBirth}_d - \text{Cutoff}_{ds}) + \mu_s + \epsilon_{ds} \quad (8)$$

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_{ds} 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 cutoff for their cohort. If Delayed_{ds} 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 because of the policy change.¹⁷ $\text{DateOfBirth}_d - \text{Cutoff}_{ds}$ represents the running variable which is the relative distance between the individual's birthday and their relevant kindergarten entry cutoff. Unobservable differences between Florida and Illinois create level differences, so I include state fixed effects, μ_s , to account for time invariant differences between states. Standard errors are clustered at the month-day level.

A statistically significant β_1 indicates a discontinuous change in incarceration rates among individuals born just after the cutoff compared to those born before. A positive estimate points to a higher likelihood of imprisonment among those born after the cutoff. Given the continuity of births around the cutoff, a positive estimate corresponds to an increase in the crime rate. Thus receiving up to one year less of education increased the likelihood of incarceration.

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

¹⁵This assumption is common in the literature (e.g., Angrist and Krueger 1991 and Cook and Kang 2016).

¹⁶Twenty-nine days is the largest bandwidth possible in order to keep the maximum amount of affected 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, 1988 cutoff in Illinois.

¹⁷This is not true for those in the initially affected cohorts who have a birth day more than a month after the cutoff. These observations are not included for my preferred specification where the bandwidth is only twenty-nine days. An alternative definition of Delayed_{ds} is that it is an indicator equal to one when $\text{DateOfBirth}_d - \text{Cutoff}_{ds}$ is positive.

would be the local average treatment effect (LATE) if I observed state of birth and education for all individuals as well as a monotonicity assumption. This assumption is that no parents who choose to delay school entry if their child is born before the cutoff would petition for their child to start early if they were born after the cutoff.

Lastly, I estimate equation 8, where the outcome is the individual’s age during their first offense for which they went to jail. I am able to construct this given the date of birth and date of conviction which is reported in both datasets. All other variables in the model remain unchanged.

V.B Validity Checks

V.B.1 Births

Random assignment of births around school entry cutoffs is a common assumption in empirical work (e.g., [Bédard and Dhuey 2006](#), [Black, Devereux, and Salvanes 2011](#), and [McCrary and Royer 2011](#)). However, I am the first to use variation in changing kindergarten entry cutoff dates as a source of exogenous variation. The timing 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 much more difficult for parents to plan around new kindergarten entry cutoffs. Here, I test that birth observables are smooth around the cutoff as my identification strategy requires that all other factors (observable and unobservable) evolve smoothly at the cutoff ([Lee and Lemieux 2010](#)).

I use equation 9 and NVSS data to test my assumption that all observable characteristics vary smoothly at the cutoff.

$$\begin{aligned}
 X_{ds} = & \alpha_0 + \alpha_1 \text{Delayed}_{ds} + \beta_2 (\text{DateOfBirth}_d - \text{Cutoff}_{ds}) \\
 & + \alpha_3 \text{Delayed}_{ds} \times (\text{DateOfBirth}_d - \text{Cutoff}_{ds}) + \mu_s + \nu_{ds}
 \end{aligned} \tag{9}$$

Here, X_{ds} is one of many observables at the time of birth on day d in state s . Delayed_{ds} and μ_s are the same as in equation 8. $\text{DateOfBirth}_d - \text{Cutoff}_{ds}$ is again the running variable being the relative distance between birth day and the applicable kindergarten entry cutoff. Standard errors

are again clustered at the month-day level.

I display the results of equation 9 where X_{ds} is either the average number of births, average birth weight, average mother characteristics, average father characteristics, percentage male, or percent born in hospitals in Figure 3 and report the results in Table 5 of the appendix. I find no discontinuity in any of these observables lending credence to my RDD being properly identified.

V.B.2 Moving

Due to the limitations of my data, a potential concern is individuals moving at differential rates which are correlated with their date of birth relative to the cutoff. The ACS has data on state of birth, state of residence when surveyed, and quarter of year of birth.¹⁸ The month by month change of each policy resulted in an October first cutoff for one year in both Florida and Illinois. This coincides with the quarter of birth for that year, 1977 in Florida and 1982 in Illinois, so I compare moving rates of individuals in the third and fourth quarters of those years.

I use variations of the difference-in-differences model in equation 10 to test for differences in moving between each side of the October first cutoff in Florida and Illinois.¹⁹

$$\begin{aligned} \text{Move}_{isq} = & \gamma_0 + \sum_r \gamma_{fr} (\text{Delayed}_{isq} \times \text{Female}_i \times \mathbb{1}\{\text{Race} = r\}) \\ & + \sum_r \gamma_{mr} (\text{Delayed}_{isq} \times \text{Male}_i \times \mathbb{1}\{\text{Race}_i = r\}) + Z'_i \delta + \mu_s + \nu_{isq} \end{aligned} \quad (10)$$

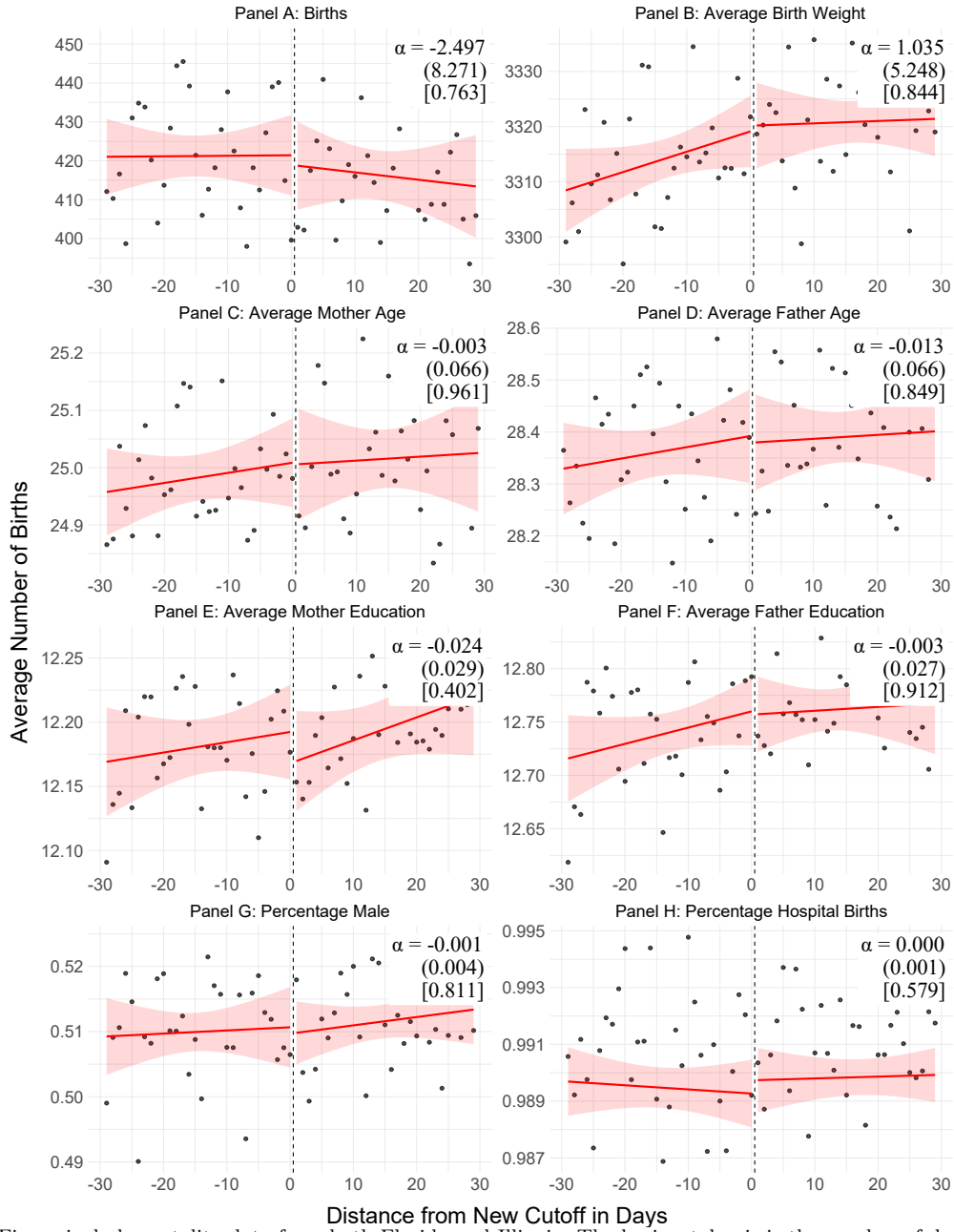
Move_{isq} is an indicator equal to one if individual i currently resides in a state different from where they were born. This definition of Move_{isq} represents either moving into Florida and Illinois or moving out of Florida and Illinois depending on the sample. Delayed_{isq} is similar to previous models where it is equal to one if the individual's quarter of birth was delayed in either Florida or Illinois of that year, otherwise it is zero.²⁰ Female_i and Male_i are indicators for the individual's gender and Race_i indicators for their race. Z_i is a vector of demographic controls including age, gender, and race. μ_s is a state fixed effect and standard errors are clustered at the household level.

¹⁸No version of the ACS has more specific dates of birth than quarter.

¹⁹The estimates of this model are biased by those born on October first although this represents only about 1% of the total treated population.

²⁰In practice, this is an indicator equal to one if the person was born in the fourth quarter of the year (i.e. after October 1) due to the restrictions made on the sample.

Figure 3: Natality Regression Discontinuity Plots



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 point wise 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]. Parents' education and the is not reported for children born in 1975 in Florida and place of birth is reported beginning in 1976 for both states. RD results from each shown outcome are displayed in Table 5 of the [appendix](#). Source: [National Center for Health Statistics \(1975-1986\)](#).

Equation 10 represents the most complex of my moving models in which I estimate the difference in moving by gender and race. Due to there being no omitted category, double interactions and Delayed_{isq} alone are not included. I estimate versions of this model in which Delayed_{isq} is alone and where Delayed_{isq} is only interacted with gender or race.

I report the results of equation 10 in Table 6 of the [appendix](#). Columns 1-4 correspond to samples restricted to only those born in Florida and Illinois, giving the dependent variable as an indicator for moving out. Columns 5-8 have the sample restricted to those who reside in Florida and Illinois at the time of survey and so the dependent variable is moving in to the state.

Overall, I find no evidence of differences in moving habits for all demographic groups other than White males. White males born after the kindergarten entry cutoff move out of Florida and Illinois about 3.3 percentage points more than those born before the cutoff. This represents about a 7% increase in moving among White men relative to the mean. This biases my estimate of adult crime downwards since the White men subject to these policies are more likely to have moved out of their respective states and likely commit any crimes in their new state of residence.²¹

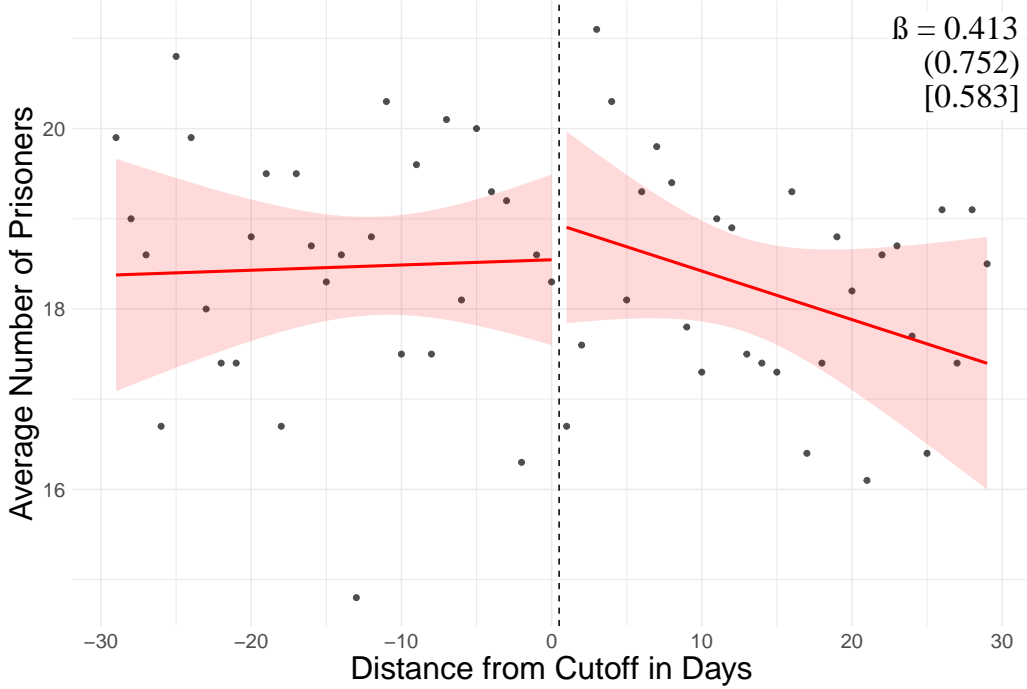
VI Results

VI.A Whole Sample

I first check for a discontinuity among all prisoners. Figure 4 displays the RDD plot of all prisoners relative to the cutoff. The first column of Panel A in Table 1 reports the results of the RDD from equation 8. I find no difference in the count of prisoners born on a given day due to changes in required schooling caused by the kindergarten cutoff date. No aggregate effect is common in the crime literature as most individuals are not on the margin of being a criminal (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.

²¹Approximately 90% of crimes are committed within a 20 minute drive of the criminal's home ([Kirchmaier 2024](#))

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 point-wise 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 8 is reported along with its standard error (in parentheses) and p-value [in brackets].

VI.B Heterogeneity Between Demographic Groups

I examine heterogeneity by gender, race, and gender-race pairs using equation 8 on demographic subsamples. Because my unit of observation is the date of birth, splitting by demographics reduces the number of prison stays but does not reduce statistical power since the bandwidth remains the same. I report the results by gender, race, and gender-race pairs in Table 1 and Figure 5 displays the corresponding RD plots.

I find being delayed school entry due to the cutoff to increase the number of female prisoners. The effect for females is concentrated among White females who see an increase in the average count of incarcerations by about 0.47 prisoners per day. This is a large effect relative to the mean,

representing about a 31% increase in crime for White females. 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 (Dobkin and Ferreira 2010 and Kaplan, Spenkuch, and Tuttle 2025).

Interestingly, I find no evidence of an effect on men. This is likely due to academic-redshirting in which parents prevent school entry for their child when they are eligible.²² Academic-redshirting is done to allow for another year of maturing and growing so that the child can be among the oldest in their cohort. The literature has grown to support the claim that older students perform better in school, especially early on, although it leads to no effect on educational attainment or labor market outcomes (e.g., Datar 2006; Elder and Lubotsky 2009; Dobkin and Ferreira 2010; Black, Devereux, and Salvanes 2011). Academic red-shirting is done primarily among those of above average socioeconomic status and is more common among Whites and males (Bédard and Dhuey 2006 and Deming and Dynarski 2008).²³ This correlates with demographic groups least likely to commit crime and end up in prison, biasing my estimate downward.

Estimates should be interpreted as lower bounds due to data limitations. Migration across states and the absence of birthplace and schooling data create misclassification of treatment status among some individuals. This attenuation implies that my intention to treat estimates understate the true effect of delayed school entry on incarceration.

VI.C Age Effects

I generally find no effect on the age at which a prisoner first ends up in prison, as reported in Table 8. I do find an effect for Hispanic women, but this effect is likely driven by just a few prisoners who were born after the cutoff.²⁴ One concern with this estimate is censoring due to Illinois's data only going back to 2005. Censoring results in not capturing the full population of prisoners born from December 1979 to September 1983 who went to prison before age 24 and leads in my average

²²Compulsory schooling laws in Florida and Illinois require all children to attend school at age of 6. This limits the amount of time a child can be redshirted.

²³Males are about twice as likely to be academically-redshirted than Females. White men are the demographic group academically-redshirted most often (Bédard and Dhuey 2006 and Deming and Dynarski 2008).

²⁴This is because for a given date of birth within my bandwidth, there is less than a 50% chance that I have any Hispanic females in my sample with that date of birth.

Table 1: Regression Discontinuity Results by Race and Gender

	All	White	Black	Hispanic	Other Race
<i>Panel A: Males and Females</i>					
Delayed	0.414 (0.752) {18.013}	0.049 (0.462) {8.115}	0.143 (0.468) {7.595}	0.217 (0.257) {2.193}	0.019 (0.042) {0.036}
<i>Panel B: Males Only</i>					
Delayed	-0.120 (0.738) {15.721}	-0.425 (0.394) {6.613}	0.027 (0.454) {7.003}	0.271 (0.267) {2.034}	0.004 (0.042) {0.034}
<i>Panel C: Females Only</i>					
Delayed	0.534** (0.213) {2.292}	0.474** (0.187) {1.502}	0.115 (0.140) {0.592}	-0.053 (0.057) {0.159}	0.014 (0.022) {0.002}
Bandwidth (Days)	29	29	29	29	29
N (Days of Birth)	590	590	590	590	590

Note: Standard errors in parentheses, clustered at the month-day level. Sample means, within the bandwidth, are reported in the braces. 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 because of the policy change. * significant at 10%, ** at 5%, *** at 1%.

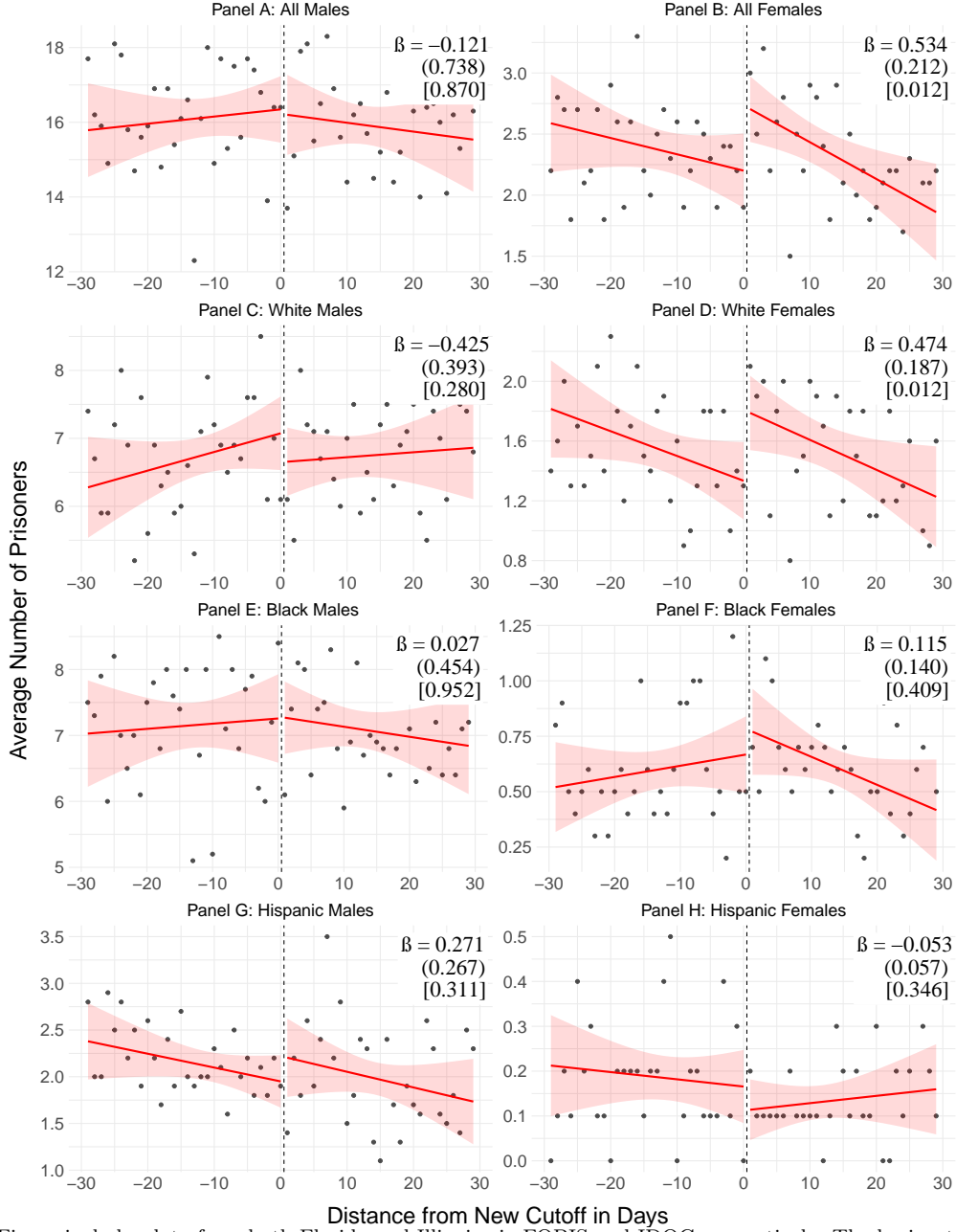
age of prisoner entry to be much higher than in reality, as reported in Table 4 in the [appendix](#).

VII Robustness

I check the robustness of my results by varying the bandwidth and checking both triangular and uniform kernels. I report these results in Table 2, along with my preferred specification being a twenty-nine day bandwidth and a triangular kernel.

I find my estimates to be robust to different bandwidths and kernels. In all specifications, I find an effect for females, concentrated among White females. I find modest evidence of effects for other demographic groups at smaller bandwidths, which quickly go away at larger bandwidths. Across all specifications, the smallest effect I find is about a 17% increase in White female crime due to delayed school entry.

Figure 5: Prisoner Regression Discontinuity Plots by Demographic Group



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 point wise confidence intervals around the fitted regression function, based on standard errors clustered at the month-day level. The estimated coefficient, β , from equation 8 is reported along with its standard error (in parentheses) and p-value [in brackets].

VIII Conclusion

This paper exploits quasi-random variation in the kindergarten entry cutoff date using policy changes in Florida and Illinois during the 1980s, which occurred after conception. I pair administrative prison data from each state, which include exact dates of birth, with a regression discontinuity design where the running variable is the distance between the date of birth and the cutoff date.

I find evidence of an effect on women, concentrated among White females, with delayed school entry increasing the number of White women who ever go to prison on affected dates of birth by 0.474 prisoners per day. I show, using NVSS birth certificate data, that the number of births and birth characteristics remained similar across the cutoff. This implies that the increase in prisoners reflects an increase in the crime rate rather than an increase in births. By comparing to the mean, I find delayed school entry increases the crime rate of White women by at least 17%, by even my most modest estimate.

Using first stage estimates from [Dobkin and Ferreira \(2010\)](#) and [Kaplan, Spenkuch, and Tuttle \(2025\)](#), which estimate the effect of delayed entry on school attainment, I calculate a back of the envelope local average treatment effect of education on lifetime crime. Using my most conservative estimate along with theirs, I find that a one percentage point increase in high school attainment decreases crime committed by White women by over 11%. Put another way, increasing education by one year decreases the probability that a White woman commits crime by over 5%.

Table 2: Results by Demographic Group, Bandwidth, and Kernel

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All	-0.708 (0.969)	0.413 (0.752)	0.459 (0.596)	0.407 (1.192)	0.465 (1.021)	0.483 (0.873)	0.362 (0.747)	0.215 (0.636)	0.485 (0.590)
Males	-1.902* (1.026)	-0.121 (0.738)	0.131 (0.571)	-0.272 (1.197)	-0.072 (0.994)	-0.168 (0.857)	-0.019 (0.698)	-0.061 (0.608)	0.176 (0.567)
Females	1.033*** (0.296)	0.534** (0.212)	0.327** (0.155)	0.679** (0.307)	0.538* (0.274)	0.650*** (0.238)	0.381* (0.216)	0.277* (0.161)	0.309** (0.156)
Whites	0.283 (0.538)	0.049 (0.462)	0.017 (0.364)	0.184 (0.720)	0.014 (0.602)	-0.122 (0.520)	0.013 (0.458)	-0.072 (0.404)	0.055 (0.362)
Blacks	-0.699 (0.674)	0.143 (0.468)	0.353 (0.343)	0.285 (0.782)	0.119 (0.604)	0.291 (0.528)	0.154 (0.444)	0.258 (0.354)	0.387 (0.345)
Hispanics	-0.506 (0.413)	0.217 (0.257)	0.077 (0.217)	-0.114 (0.407)	0.336 (0.350)	0.296 (0.295)	0.164 (0.235)	0.020 (0.228)	0.037 (0.215)
White Males	-0.767 (0.545)	-0.425 (0.393)	-0.275 (0.309)	-0.296 (0.610)	-0.475 (0.514)	-0.679 (0.452)	-0.377 (0.385)	-0.340 (0.348)	-0.196 (0.311)
Black Males	-1.115* (0.663)	0.027 (0.454)	0.256 (0.322)	-0.036 (0.752)	-0.005 (0.583)	0.127 (0.516)	0.089 (0.430)	0.181 (0.335)	0.287 (0.327)
Hispanic Males	-0.599 (0.449)	0.271 (0.267)	0.149 (0.211)	-0.068 (0.422)	0.377 (0.366)	0.374 (0.303)	0.256 (0.236)	0.100 (0.219)	0.098 (0.209)
White Females	1.080*** (0.246)	0.474** (0.187)	0.292** (0.144)	0.479* (0.285)	0.489** (0.242)	0.558*** (0.213)	0.390** (0.186)	0.269* (0.152)	0.250* (0.144)
Black Females	-0.107 (0.236)	0.115 (0.140)	0.097 (0.109)	0.320 (0.211)	0.124 (0.175)	0.164 (0.154)	0.065 (0.134)	0.077 (0.111)	0.101 (0.106)
Hispanic Females	0.200 (0.144)	-0.053 (0.057)	-0.072* (0.044)	-0.046 (0.090)	-0.041 (0.077)	-0.078 (0.063)	-0.092 (0.057)	-0.080* (0.044)	-0.061 (0.042)
IK Optimal?	Yes								
Triangular Kernel	Yes	Yes	Yes						
Uniform Kernel				Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth (Days)	4–9	29	60	10	15	20	29	40	60
N (Days of Birth)	90–190	590	1,089	210	310	410	590	729	1,089

Note: Standard errors in parentheses, clustered at the month-day level. 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. The coefficient of Delayed_{ds} is reported being the effect of delayed kindergarten entry on the number of prisoners with that date of birth. IK optimal represents optimal bandwidths suggested by [Imbens and Kalyanaraman \(2012\)](#) and rounded to the nearest whole number. Optimal bandwidths are reported in Table 7 of the [appendix](#). When the bandwidth is greater than twenty-nine days, those born in 1983 are excluded to account for potential confoundedness created by Medicaid expansion to kids. * significant at 10%, ** at 5%, *** at 1%.

IX Appendix

Table 3: 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 \(1987\)](#).

Table 4: 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.

Table 5: Natality Regression Discontinuity Results

Outcome	Delayed	Florida	Average	Bandwidth (Days)	N (Days of Birth)
Births	-2.497 (8.271)	-188.888*** (2.764)	396.549	29	590
Birth Weight	1.035 (5.248)	-35.814*** (2.639)	3,318.676	29	590
Mother's Age	-0.003 (0.066)	-1.461*** (0.025)	24.989	29	590
Father's Age	-0.013 (0.066)	-0.501*** (0.031)	28.363	29	590
Mother's Education	-0.024 (0.029)	-0.478*** (0.011)	12.155	29	590
Father's Education	-0.003 (0.027)	-0.344*** (0.011)	12.711	29	590
% Male	-0.0010 (0.0041)	-0.0011*** (0.0002)	0.510	29	590
% Hospital Births	0.0005 (0.0008)	-0.0054*** (0.0004)	0.991	29	413

Note: Standard errors in parentheses, clustered at the month-day level. The dependent variable is indicated in the outcome column. *Delayed* is a dummy indicating that people born in that state on that date of birth had their kindergarten start date moved because of the policy change. * significant at 10%, ** at 5%, *** at 1%. Parents' education and the is not reported for children born in 1975 in Florida and place of birth is reported beginning in 1976 for both states. Source: [National Center for Health Statistics \(1975-1986\)](#).

Table 6: Moving by Quarter of Birth (1977 and 1982 Birth Cohorts)

	Moving Out				Moving In			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delayed	-0.006 (0.008)				0.008 (0.005)			
Delayed×Male		-0.018 (0.012)				0.006 (0.007)		
Delayed×Female		0.006 (0.012)				0.008 (0.007)		
Delayed×White			-0.013 (0.010)				0.006 (0.008)	
Delayed×Black			-0.011 (0.019)				-0.000 (0.014)	
Delayed×Hispanic			0.027 (0.024)				0.012 (0.009)	
Delayed×Male×White				-0.033** (0.014)				-0.001 (0.010)
Delayed×Male×Black				0.002 (0.024)				0.013 (0.018)
Delayed×Male×Hispanic				0.022 (0.030)				0.018 (0.012)
Delayed×Female×White				0.009 (0.014)				0.014 (0.010)
Delayed×Female×Black				-0.023 (0.024)				-0.13 (0.018)
Delayed×Female×Hispanic				0.033 (0.030)				0.007 (0.012)
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Delayed Quarter	4	4	4	4	4	4	4	4
N	2,648,081	2,648,081	2,648,081	2,648,081	4,120,289	4,120,289	4,120,289	4,120,289

Note: Standard errors in parentheses, clustered at the household level. The moving out sample is restricted to those born in the third and fourth quarters of 1978, in Florida, and 1982, in Illinois. These individuals were affected by the policy change after conception and the new cutoff date (10/1) aligned with a quarter of birth, the most specific level of birth date information available in the ACS. The moving in sample is restricted to those born in the third and fourth quarters of 1978 and 1982 and currently reside in Florida or Illinois at the time of survey. The dependent variable is an indicator equal to one if the individual no longer lives in their state of birth. *Delayed* is a dummy indicating that individual was delayed school entry due to the policy change. * significant at 10%, ** at 5%, *** at 1%.

Table 7: Optimal Bandwidths

	Bandwidth Choice	
	IK	CCT
All	9	
Males	8	
Females	5	
Whites	7	
Blacks	8	
Hispanics	6	
White Males	6	
Black Males	7	
Hispanic Males	6	
White Females	5	
Black Females	5	
Hispanic Females	4	

Note: IK represents the optimal bandwidth suggested by [Imbens and Kalyanaraman \(2012\)](#) which have been rounded to the nearest whole number. CCT represents the optimal bandwidth suggested by [Calonico, Cattaneo, and Titiunik \(2014\)](#).

Table 8: Age Regression Discontinuity Results by Race and Gender

	All	White	Black	Hispanic
<i>Panel A: Males Only</i>				
Delayed	-0.569 (0.634) {30.034}	0.038 (0.859) {31.388}	-0.829 (1.125) {27.462}	0.353 (1.213) {28.777}
<i>Panel B: Females Only</i>				
Delayed	0.101 (1.000) {33.515}	-0.316 (1.213) {34.166}	2.209 (1.431) {31.630}	-5.089* (2.934) {30.239}
Bandwidth (Days)	29	29	29	29
<i>N</i> (Days of Birth)	590	590	590	590

Note: Standard errors in parentheses, clustered at the month-day level. Sample means, within the bandwidth, are reported in the braces. The dependent variable is the average age of first offense, for each demographic group, for which the prisoner went to prison, 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 because of the policy change. * significant at 10%, ** at 5%, *** at 1%.

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