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 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. 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 delayed entry to increase female imprisonment by 17.4% with this effect concentrated among White females.

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†Last updated on September 22, 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 affect criminal activity through educational attainment and relative age among peers, but I cannot separately identify the impact of each.

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). Relative age, determined by whether a child is among the oldest or youngest in their grade, has been shown to shape educational and social outcomes (Black, Devereux, and Salvanes 2011, Fredriksson and Ockert 2013, and Dobkin and Ferreira 2010) and to influence juvenile delinquency and adult crime, though findings are mixed across contexts (Depew and Eren 2016, Cook and Kang 2016, and Landersø, Nielsen, and Simonsen 2015).

While Cook and Kang (2016) measure crime only through age 19 and McAdams (2016) observes incarceration status during a single Census, I examine lifetime incarceration outcomes using exact dates of birth, providing 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 quasi-experimental variation in human capital.

I exploit variation in required schooling to measure the impact of human capital on crime. Florida and Illinois each shifted their kindergarten entry cutoffs earlier during the 1980s while leaving compulsory schooling laws unchanged. 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. Demographic groups with higher crime rates are also the most likely to be constrained by compulsory schooling laws (Lochner and Moretti 2004). As the cutoffs moved earlier in the calendar year, children born just after the new cutoff were required to delay kindergarten entry by a full year.

In this paper I use prison population data from each state, which includes exact dates of birth, and a regression discontinuity design (RDD) where the running variable is the distance to the cutoff.[1] I use this setup to measure the effect that being delayed kindergarten start by one year has on an individual's lifetime criminal history. 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 changes in the kindergarten cutoff date causes more crime for females who are delayed entry. I observe no effect for males. Within all females, there is significant heterogeneity in the effect. I estimate a 17.4% increase in female prisoners born on any given day after the cutoff with this effect concentrated in white females, who have a 28.9% increase. Importantly, I observe no difference in the number of births nor the characteristics of those born on either side of the changing cutoffs.

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.[2] 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 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 their 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 June 1979 (Florida Legislature 1979). This initially moved the kindergarten cutoff date to December

[1]My model is nearly identical to Cook and Kang (2016) and Arenberg, Neller, and Stripling (2024).
[2]I use kindergarten and school entry cutoffs synonymously.

1, rather than January 1, 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 September, 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 2 of the appendix.[3] 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 could not drop out until the age of sixteen and required a parent or guardian's permission while students 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 their 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.[4]

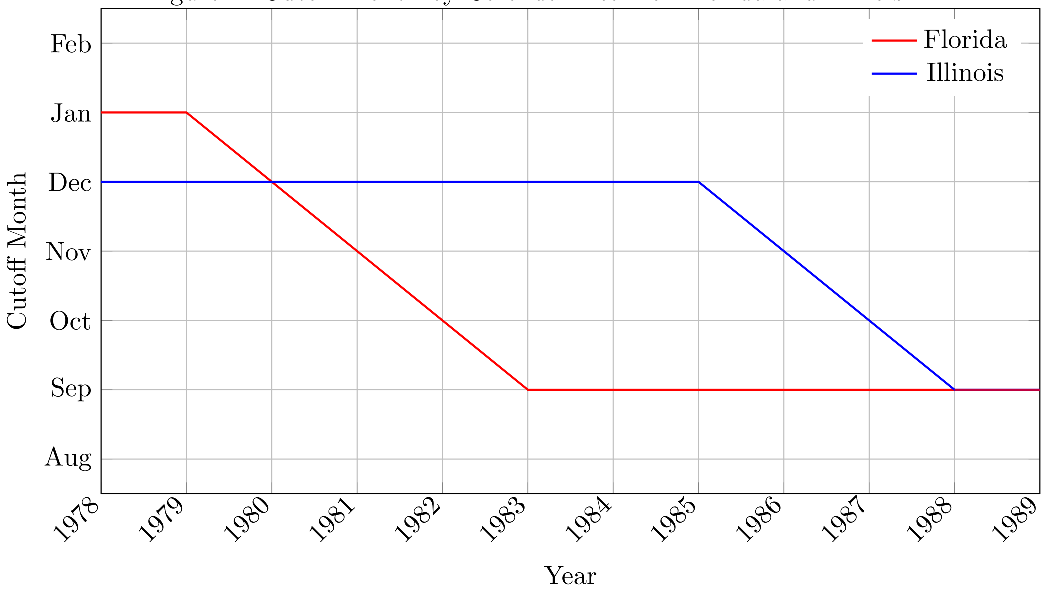
The change in kindergarten school entry cutoffs in combination with compulsory schooling laws creates a discontinuity in the amount of required schooling and age relative to peers caused by a child's date of birth. I display this discontinuity in figure 2. This difference in required schooling

[3]Those 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.

[4]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.

is driven solely by the discontinuity in kindergarten entry caused by cutoffs in Florida and Illinois since minimum dropout ages remained constant through this period.

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



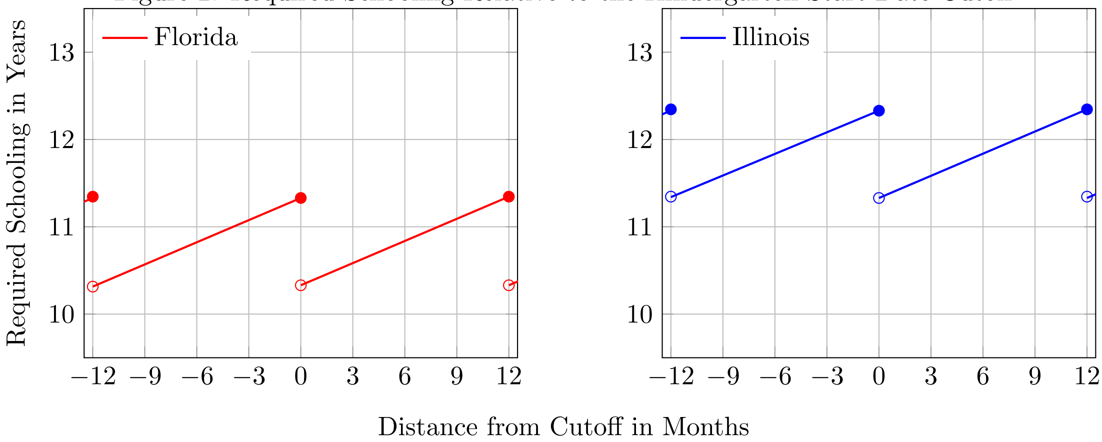
Note: The school cutoff always took place on the first of the month. Florida implemented the reform in the 1980–1981 school year while Illinois did so in the 1986–1987 school year. Based on information from Florida Senate Committee on Education Innovation (1999), Florida Legislature (1979), and Education Week (1987). A more detailed version is in table 2 in the appendix.

III Theory

I model the setting in Florida and Illinois using a simple two period framework that links 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 over 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 other factors uncorrelated with schooling. The function g_i is strictly increasing in schooling.

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.

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

$$h_i = g_i\left(S_i, Z_i\right) : \; \frac{\partial\, g_i\left(S_i, Z_i\right)}{\partial\, S_i} > 0 \; \forall \; S_i \tag{1}$$

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 punishment F. This results in the total payoff from crime, π_i^C, which depends on their benefit and the expected punishment.[5] Individuals will only choose to commit crime if their payoff, π_i^C, is (strictly) greater than the payoff from legal work, $w_i\left(h_i\right)$.[6] When the individual chooses crime, their decision rule, $c_i\left(h_i\right) = 1$.

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

[5] I 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, i.e. $\frac{d\, B_i(h_i)}{d\, S_i} \leq \frac{d\, w(h_i)}{d\, S_i} \; \forall \; S_i$.

[6] Here I assume individuals are risk averse at the inflection point.

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 $q_i \in (0,1)$.[7] 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.

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

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 had high schooling, S_i^H would certainly have chosen to commit crime if they had low schooling, S_i^L. Thus, the implication below holds, but the reverse does not.

$$c_i\left(h_i^H\right) = 1 \Rightarrow c_i\left(h_i^L\right) = 1 \tag{4}$$

The probability of crime for those born before the cutoff is equal to $c_i\left(h_i^H\right)$.[8] Whereas the probability of crime for individuals born after the cutoff is the convex combination of the two decision rules, $c_i\left(h_i^L\right)$ and $c_i\left(h_i^H\right)$, weighted by the probability of low education, q_i.[9] This relationship is expressed in equation 6.

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

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

Implication 4 implies $c_i\left(h_i^L\right) \geq c_i\left(h_i^H\right)$. It follows that the probability of crime for those born after the cutoff is always at least as high as it would be if they were born before the cutoff. This is

[7] I 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.

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

[9] The 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.

shown in equation 7.

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

In total, my model shows that 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, $\left(w_i\left(h_i^H\right)-w_i\left(h_i^L\right)\right)$.

By shifting the distribution of schooling in a quasi-random manner, kindergarten entry cutoffs generate discontinuities in long run crime outcomes that can be identified with a regression discontinuity design. While 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.[10] Most importantly, both datasets include the date of birth for all prisoners admitted so that their eligibility for kindergarten is known.[11] 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 before the announcement of the policy change and were subject to the new kindergarten

[10] 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 3 of the appendix.

[11] I 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.

cutoffs. This leaves five cohorts per state.

Lastly, I use natality data from the National Vital Statistics System (NVSS) 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.[12] 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.

V Identification Strategy

V.A Births and Observables

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 to identify the treatment effect caused by the cutoff (Lee and Lemieux 2010).

I use equation 8 to test my assumption that all observable characteristics vary smoothly at the cutoff.

$$\begin{aligned} X_{ds} = \alpha_0 + \alpha_1\, \mathit{Delayed}_{ds} + \beta_2\, (\mathit{DateOfBirth}_d - \mathit{Cutoff}_{ds}) \\ + \alpha_3\, \mathit{Delayed}_{ds} \times (\mathit{DateOfBirth}_d - \mathit{Cutoff}_{ds}) + Florida_s + \nu_{ds} \end{aligned} \tag{8}$$

Here, X_{ds} is one of many observables including the number of births, average birth weight, mother characteristics, and father characteristics on day d in state s. $\mathit{Delayed}_{ds}$ 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 $\mathit{Delayed}_{ds}$ equals one then individuals born on that day would

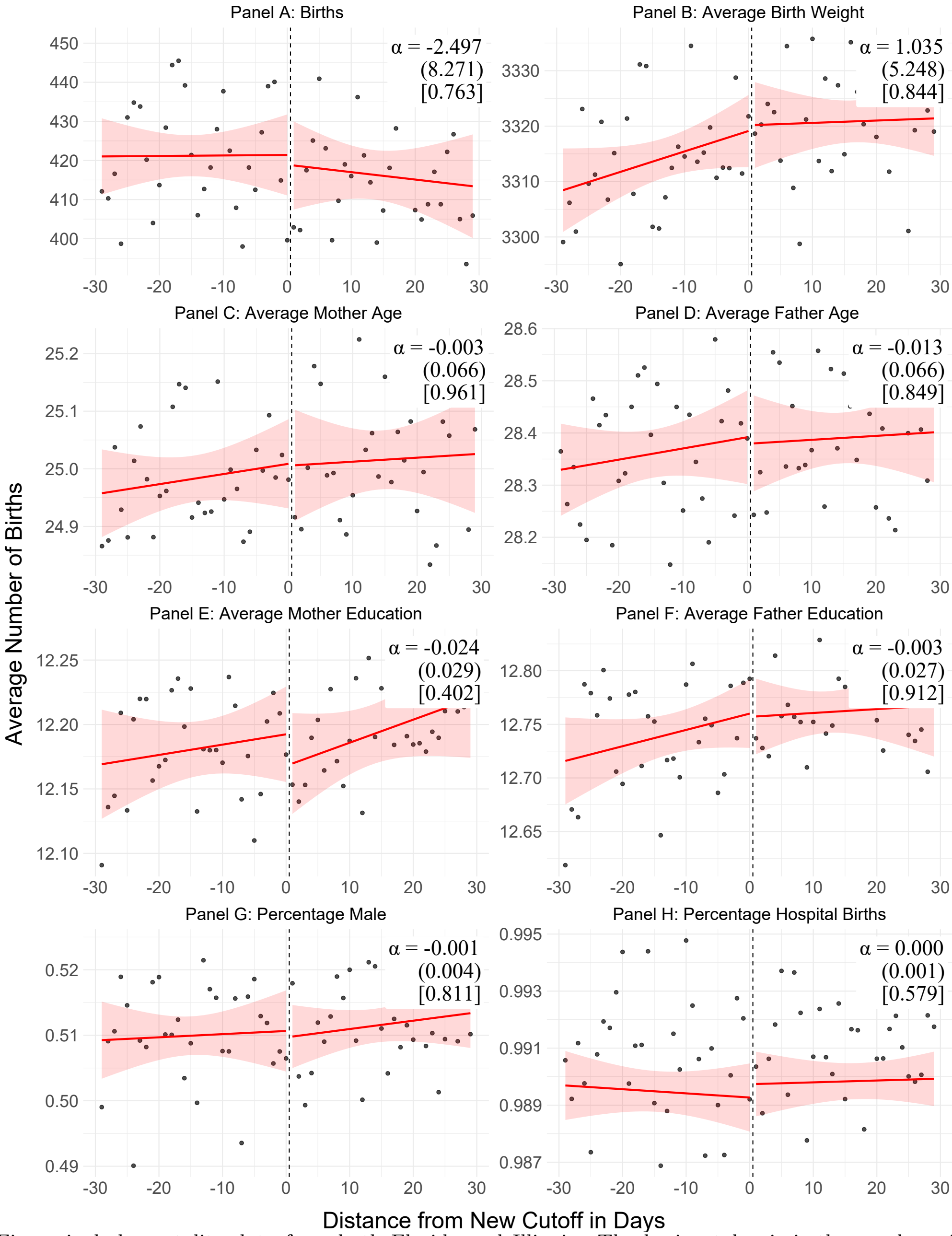
[12]Generally, data on the father is only included in the NVSS when the father is either (i) married to the mother of the child and present at the birth or (ii) present at the birth and the father voluntarily provides the information.

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.[13] $Florida_s$ is a state fixed effect equal to one for affected cohorts in Florida.

I display the results of equation 8 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 4 of the appendix. I find no discontinuity in any of these observables which lends credence to my RDD being properly identified.

[13]An alternative definition of $Delayed_{ds}$ is that it is an indicator equal to one when $DateOfBirth_d - Cutoff_{ds}$ is positive.

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 4 of the appendix. Source: National Center for Health Statistics (1975-1986).

V.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 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 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. Assuming that birthdays are as good as random around the new cutoffs gives appropriate exogeneity supporting the use of a regression discontinuity design (RDD).[14]

I pool observations across the first five affected cohorts. 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 9.[15]

$$\begin{aligned} Y_{ds} = \beta_0 + \beta_1\, \mathit{Delayed}_{ds} + \beta_2\, (\mathit{DateOfBirth}_d - \mathit{Cutoff}_{ds}) \\ + \beta_3\, \mathit{Delayed}_{ds} \times (\mathit{DateOfBirth}_d - \mathit{Cutoff}_{ds}) + \mathit{Florida}_s + \epsilon_{ds} \end{aligned} \tag{9}$$

Here, Y_{ds} is the number of prisoners born on date d who were admitted to prison at any time during the observation period. $\mathit{Delayed}_{ds}$ and $\mathit{Florida}_s$ are the same as in equation 8. $\mathit{DateOfBirth}_d - \mathit{Cutoff}_{ds}$ is again the running variable being the relative distance between the birthday and the applicable kindergarten entry cutoff.

A statistically significant β_1 indicates a discontinuous change in incarceration among individuals born just after the cutoff compared to those born before. A positive estimate points to a higher

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

[15] Twenty-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.

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 the combination of being the oldest in your cohort 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 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. β_1 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.

VI Results

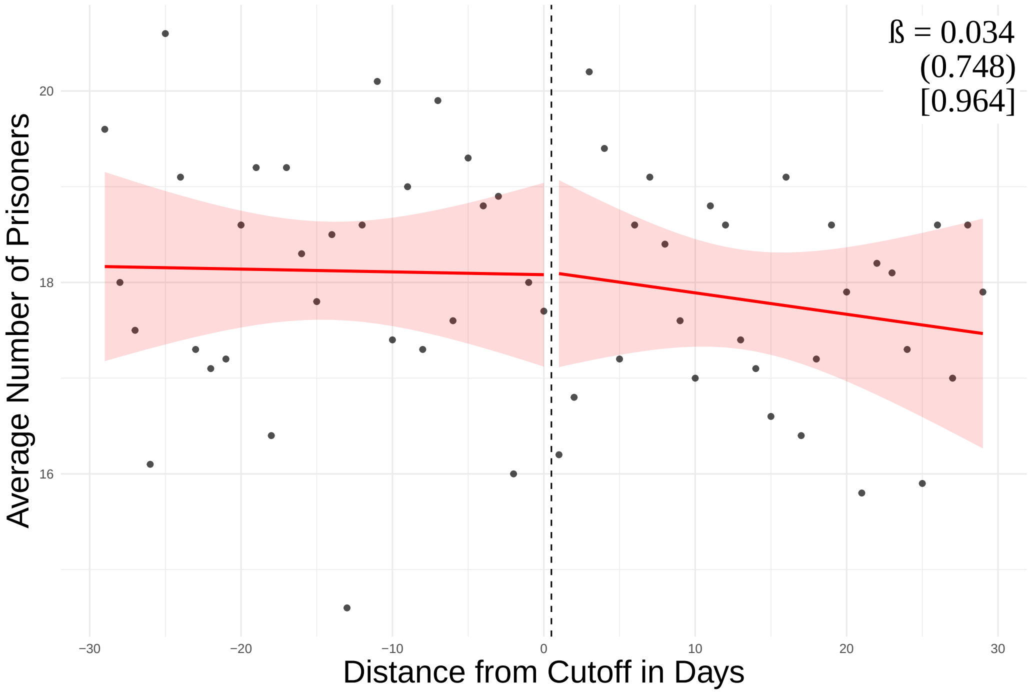
VI.A Whole Sample

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 Panel A in Table 1 reports the results of the RDD from equation 9. I find no difference in the count of prisoners born on a given day due to the combination of education and relative age effects. No aggregate effect is common in the literature (e.g. Landersø, Nielsen, and Simonsen 2015, Depew and Eren 2016 and McAdams 2016) and I continue in the following section by examining heterogeneous effects among demographic groups.

VI.B Heterogeneity Between Demographic Groups

I examine heterogeneity by gender, race, and gender-race pairs using equation 9 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 bandwidth. I report the results by gender, race, gender-race pairs in Table 1, and Figure 5 displays the corresponding RD

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

plots.

I find being delayed school entry due to the cutoff to increase the number of female prisoners born on a given day. I observe no statistically significant effect on men. The effect for females is concentrated among white females who 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.

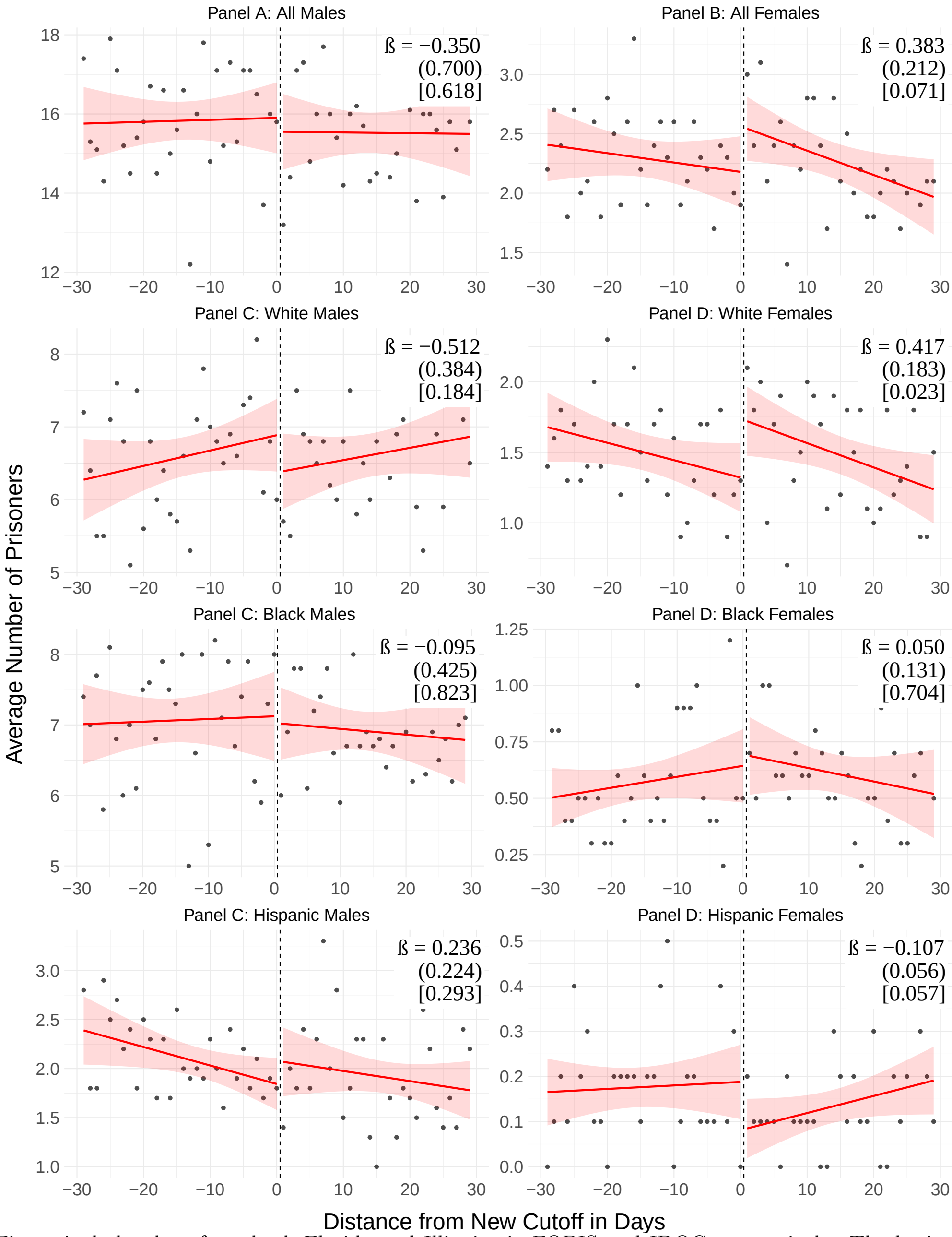
Estimates should be interpreted as lower bounds due to data limitations. Migration across states and the absence of birthplace and schooling data mean that some individuals are misclassified with respect to treatment status. This attenuation implies that my intention-to-treat estimates understate the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

Table 1: Regression Discontinuity Results by Race and Gender

	All	White	Black	Hispanic	Asian	Native American
Panel A: Males and Females						
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)
	{18.013}	{8.115}	{7.595}	{2.193}	{0.036}	{0.020}
Panel B: Males Only						
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)
	{15.721}	{6.613}	{7.003}	{2.034}	{0.034}	{0.005}
Panel C: Females Only						
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)
	{2.292}	{1.502}	{0.592}	{0.159}	{0.002}	{0.015}
Bandwidth (Days)	29	29	29	29	29	29
N (Days of Birth)	590	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%.

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 heteroskedasticity-robust standard errors clustered at the month-day level. The estimated coefficient, β, from equation 9 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.

VII Appendix

Table 2: 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 3: 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 4: Natality Regression Discontinuity Results

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

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).

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