The Lifelong Criminal Consequences of Kindergarten Cutoff Dates

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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 their effects 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 because compulsory schooling laws are age based. Leveraging temporal variation in school entry cutoffs in Florida and Illinois, I link school start age to incarceration outcomes observed decades later and find that delayed entry increases crime among females by over 20%, with this effect concentrated among White women.

JEL: I21, K42, J24

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 individuals choosing between legal and illegal behavior.

In the U.S., required education is determined at the state level through compulsory schooling laws that mandate school attendance for specified ages.[1] Several papers exploit compulsory schooling laws to instrument for education and measure its effects on adult earnings (e.g., Angrist and Krueger 1991, Oreopoulos 2006, and Brunello, Fort, and Weber 2009), arrests and incarcerations (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).

Related work measures the effect of beginning school later due to kindergarten entry cutoff dates, which determine when a child is eligible to start school. Individuals born on each side of the cutoff are, on average, similar in all ways aside from when they start school. Those born before the cutoff start school at nearly five, while those born afterward delay entry by a year and start when they are almost six. These papers generally find delayed school entry increases early test scores which fade with time (Elder and Lubotsky 2009 and Lubotsky and Kaestner 2016), decrease educational attainment (Dobkin and Ferreira 2010 and Kaplan, Spenkuch, and Tuttle 2025), decrease earnings through age thirty (Black, Devereux, and Salvanes 2011), and has little effect on childbearing (McCrary and Royer 2011). Fewer papers estimate the criminal consequences of kindergarten entry cutoff dates.

The literature consistently finds delayed individuals to have less juvenile crime due to incapacitation (Cook and Kang 2016, Depew and Eren 2016, and Shepard 2024). Using a regression discontinuity design (RDD), Landersø, Nielsen, and Simonsen (2015) find delayed entry to decrease young adult crime, also due to incapacitation, in Denmark where compulsory schooling laws are attainment-based. Attainment-based compulsory schooling laws, prevent the discontinuity of required education based on school entry which occurs with age-based compulsory schooling laws common in the U.S. Cook and Kang (2016) uses a similar design to Landersø, Nielsen, and Simonsen (2015) but in the U.S. and finds delayed entry to increase crime by the age of nineteen. Only McAdams (2016) estimates the effect of delayed entry from the cutoff on adult crime. He does so using a difference-in-differences comparing those born in the quarter before the cutoff to

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[1]Failure to comply with compulsory schooling laws may result in fines and criminal prosecution of the parents possibly resulting in a misdemeanor and jail time (U.S. Department of Education 2024 and Florida Legislature 2025).

those in the quarter after the cutoff. Using the 1970 and 1980 decennial Censuses he finds delayed entry to increase one's propensity for crime by 3%. My paper builds on Cook and Kang (2016) and McAdams (2016) by being the first to exploit changes in the kindergarten entry cutoff date paired with state-level prison population data and a RDD to measure the effect on lifetime crime.

Florida and Illinois each moved their kindergarten entry cutoffs earlier in the school year 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 an individual's date of birth. I use administrative prison population data from each state, which include exact dates of birth, and a RDD to measure the effect that delayed school start has on an individual's lifetime criminal history.[2] Particularly unique to my paper are the changing kindergarten entry cutoff dates, which occurred after conception. This reduces potential manipulation around the cutoff due to parents timing their child's birth since their child was already conceived at the announcement of the cutoff change.

I find delayed entry, because of changes in the kindergarten cutoff date, cause more imprisonments among females. I observe no effect for males. Additionally, I find largely no differences in births, birth and parental characteristics, and moving behaviors among those near the cutoff. Together this implies the increase in female imprisonments is due to an increase in the crime rate, rather than the number of births.[3] In total I estimate women born just after the cutoff have at least a 20% higher crime rate, with this effect concentrated among White women.

II   Institution

School entry cutoff dates refer to the date used to determine whether a child starts school in the current year or must wait until the following year.[4] 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 its inception in 1965 and 1895 for Florida and Illinois, respectively (Florida House of Representatives 1999). 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, 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

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[2]My model is nearly identical to Landersø, Nielsen, and Simonsen (2015), Cook and Kang (2016), and Arenberg, Neller, and Stripling (2024).

[3]Assuming that the probability of arrest conditional on committing a crime and the probability of imprisonment conditional on arrest do not change discontinuously at the cutoff, as is common in the literature.

[4]I use kindergarten and school entry cutoffs synonymously.

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 for 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.[5] 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).

Figure 1: Cutoff Month by School Year for Florida and Illinois
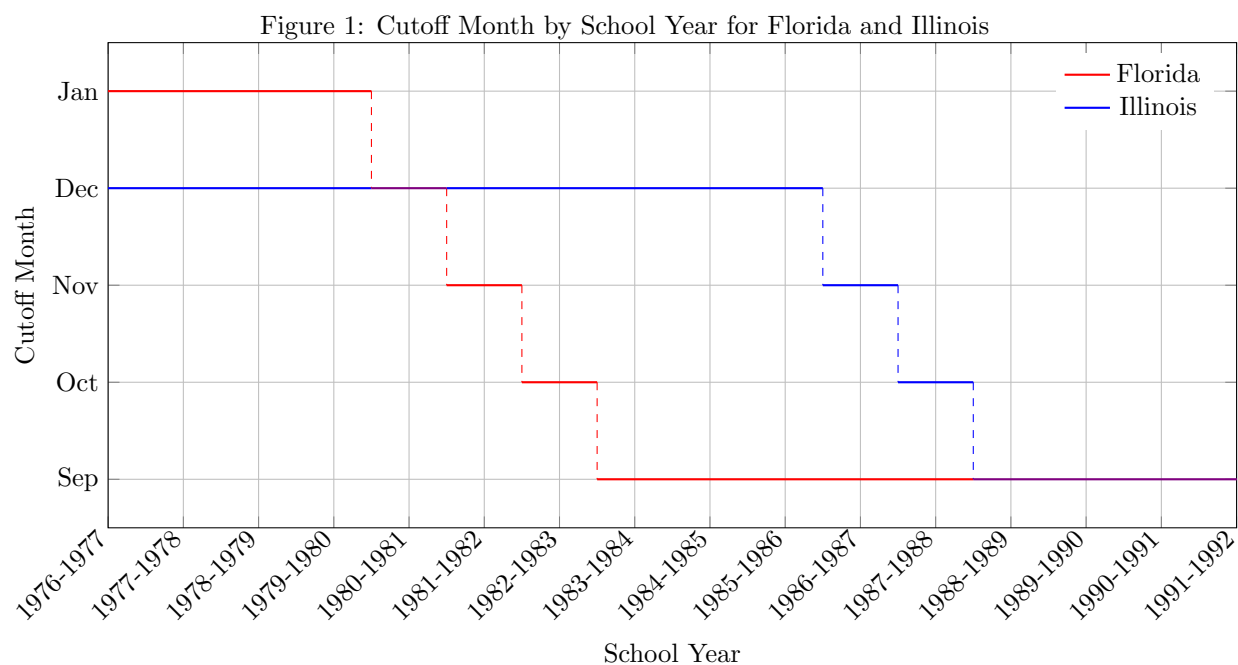
Note: The school cutoff always took place on the first of the month. Florida first implemented the reform in the 1980–1981 school year while Illinois did so in the 1986–1987 school year (Florida Legislature 1979; Education Week 1987; Florida House of Representatives 1999; Florida Senate Committee on Education Innovation 1999). Changes in the cutoff date were announced in June 1979 in Florida and September 1985 in Illinois. A more detailed version is in table 3 in the appendix.

Importantly, neither Florida nor Illinois changed their compulsory schooling laws during this period of changing kindergarten cutoff dates (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

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[5]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 (inclusive) 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.

could not drop out until age sixteen and required a parent or guardian's permission.[6] 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 those affected by changing cutoff dates is determined solely by the cutoff date itself.[7]

The combination of kindergarten entry cutoffs and compulsory schooling laws generates a discontinuity in required schooling based on a child's date of birth. Those born just before the cutoff have over eleven years of required schooling while those born just after have only ten years. Figure 2 illustrates this discontinuity, which arises solely from changes in kindergarten entry cutoffs in Florida and Illinois, as minimum school leaving ages remained constant during this period.

Figure 2: Required Schooling Relative to the Kindergarten Start Date Cutoff
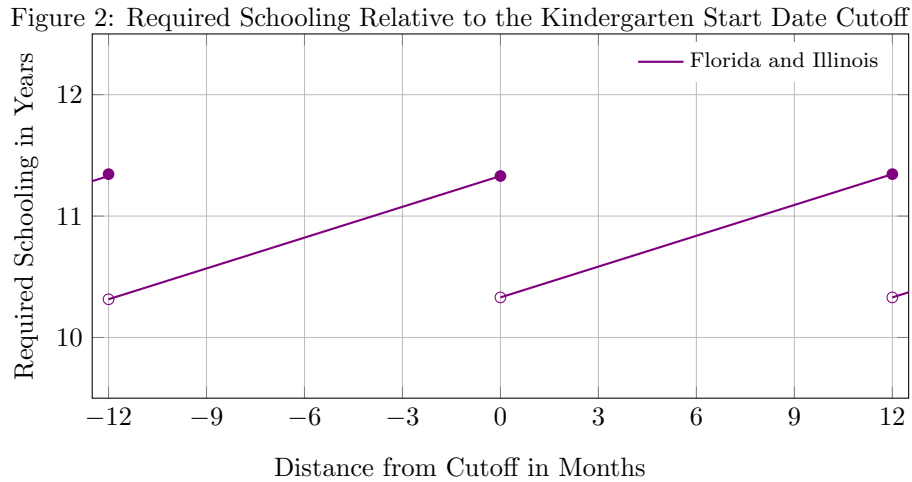
Note: Required schooling is calculated as the difference in age between the age at which a child begins kindergarten, which is determined by the cutoff dates, and the age at which a child can opt out of schooling with permission. Students could opt out of schooling at the age of sixteen with a parent or guardian's permission during this period in Florida and Illinois (Florida Legislature 1970; Illinois General Assembly 1970; Illinois State Board of Education 1983; Angrist and Krueger 1991; Knapp et al. 2025).

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 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.

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

[7]This is under the assumption that parents always enrolled students in school when they were first eligible for kindergarten, i.e. everyone is a complier, 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, and Illinois General Assembly 1970). Failure to meet this assumption results in attenuation bias.

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 all other factors that affect human capital. The function g_i 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.

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.[8] All individuals have an equal probability of apprehension $p \in (0,1)$ and punishment $F_i(h_i)$ is an increasing function of human capital due to rising opportunity costs from legal work, $w_i(h_i)$. This results in the total payoff from crime, π_i^C, which depends on the benefit and the expected punishment. Individuals choose to commit crime if their payoff, π_i^C, is strictly greater than the payoff from legal work, $w_i(h_i)$.[9] When the individual chooses crime, their decision rule, $c_i(h_i)$, equals one.

Individuals' level of schooling is partially determined by their date of birth relative to the kindergarten entry cutoff. Those born before the cutoff receive their high level of schooling, $S_i = S_i^H$, for certain, whereas those born after may receive low schooling, S_i^L, with a probability q_i where $q_i \in (0,1)$.[10] 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).

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

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. This makes the probability of committing crime with low human capital, $c_i(h_i^L)$, no less than with high human capital, $c_i(h_i^H)$.

$$c_i\left(h_i^H\right) = 1 \Rightarrow c_i\left(h_i^L\right) = 1$$
$$\Rightarrow c_i\left(h_i^L\right) \geq c_i\left(h_i^H\right) \qquad (2)$$

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[8]I assume the benefit of crime B_i is independent of schooling and human capital, consistent with the literature (e.g., Becker 1968, Ehrlich 1973, and Lochner 2004). This assumption can be relaxed without altering equation 3. It is sufficient that additional schooling raises legal earnings more than criminal returns, that is $\frac{d\,B_i(h_i)}{d\,S_i} \leq \frac{d\,w(h_i)}{d\,S_i}$ for all S_i.

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

[10]I assume individuals born before the cutoff receive their high 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.

The probability of crime for those born before the cutoff is equal to $c_i\left(h_i^H\right)$.[11] The probability of crime for individuals born after the cutoff is a 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, $q_i \in (0,1)$.[12] 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, shown in equation 3.

$$Pr\left(c_i\left(h_i\right) = 1|\text{ Born before cutoff}\right) \leq Pr\left(c_i\left(h_i\right) = 1|\text{ Born after cutoff}\right) \tag{3}$$

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 increases the likelihood of criminal activity. 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, $w_i\left(h_i^H\right) - w_i\left(h_i^L\right)$. By shifting the distribution of schooling in a quasi-random manner, kindergarten entry cutoffs generate discontinuities in long run crime outcomes that I estimate using a regression discontinuity design.[13]

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.[14] Both datasets include the date of birth for all prisoners admitted, so their eligibility for kindergarten is known.[15] 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 and crime type on a given date of birth. Each date of birth is then assigned a distance to the nearest kindergarten entry cutoff. I restrict the data to only cohorts affected

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[11]This is because there is no uncertainty in the amount of human capital. An individual's probability of crime is 1 when their expected 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.

[12]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.

[13]Additionally, the framework can be extended to allow individuals to choose between misdemeanor and felony offenses. Because my empirical outcome is imprisonment, a sanction rarely applied to misdemeanors, any shift from misdemeanors to felonies is empirically indistinguishable from a shift from nonoffending into felony crime. This extension can be made by allowing the punishment to be increasing in the severity of the crime such that the expected payoff of felony offenses falls more quickly with education than the payoff of misdemeanors.

[14]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 4 of the appendix.

[15]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 when and where they started school.

by the cutoff changes who were also conceived before their announcement to reduce endogeneity caused by parents planning their child's birth. This restriction leaves me with 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. NVSS data include information, such as the race, age, and education of the mother and (sometimes) father on all births in the affected cohorts in Florida and Illinois.[16] 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 of births in total and by demographic group. 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 data, I see whether individuals moved into or out of either Florida or Illinois. I provide more detail on 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 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 the new cutoffs and there are no confounders, a sharp regression discontinuity design (RDD) gives an unbiased estimate of delayed entry on lifetime crime.[17] I pool observations across the first five affected cohorts in each state.[18] I use the

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[16] 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.

[17] These assumptions are common in the literature (e.g., Angrist and Krueger 1991 and Cook and Kang 2016).

[18] The Illinois cohort born in 1983 is omitted for any results using a bandwidth of more than twenty-nine days. 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. I display results using both twenty-nine day and thirty day bandwidths in Table 2 to verify that the inclusion of 1983 does not drive my results.

RDD in equation 4, where my preferred specification uses a triangular kernel and a ten day bandwidth.

$$
\begin{aligned}
Y_{dts} = \beta_0 &+ \beta_1 \, Delayed_{dts} + \beta_2 \, (DateOfBirth_{dt} - Cutoff_{dts}) \\
&+ \beta_3 \, Delayed_{dts} \times (DateOfBirth_{dt} - Cutoff_{dts}) + \mu_s + \mu_t + \varepsilon_{dts}
\end{aligned}
\tag{4}
$$

Here, Y_{dts} is the number of individuals born on date d in year t in state s whom I ever observe in prison. $Delayed_{dts}$ is an indicator equal to one if individuals born on day d, year t, and in state s had their kindergarten entry delayed by a year due to the change in cutoff after conception. If $Delayed_{dts}$ 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.[19] $DateOfBirth_{dt} - Cutoff_{dts}$ represents the running variable, which is the relative distance between the individual's birthday and their kindergarten entry cutoff date.[20] Unobservable differences, different population sizes, and different birth rates between Florida and Illinois create level differences, so I include state fixed effects, μ_s, to account for time invariant differences between states. As previously mentioned, cohorts' crime in Illinois is observed differentially due to data limitations.[21] Cohorts are also exposed to changing policing and judicial policies over time. For these reasons, I include year-of-birth fixed effects, μ_t. Standard errors are clustered at the month-day level.

A statistically significant β_1 indicates a discontinuous change in incarcerations 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 crime rate for those born after the cutoff date.

β_1 captures the intent to treat (ITT) effect of being assigned a delayed kindergarten start based on the date of birth and 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 would be the local average treatment effect (LATE) using a fuzzy RDD if I observed state of birth and the realized school start date for all prisoners as well as a monotonicity assumption .[22]

Lastly, I estimate equation 4 with two other outcome variables. First, I use as an outcome the individual's age at their first conviction for which they went to prison. I am able to construct this given the date of birth and date of conviction, which are reported in both datasets. Secondly, I estimate heterogeneous effects of

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[19]This 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 ten days.

[20]An alternative definition of $Delayed_{dts}$ is that it is an indicator equal to one when the running variable, $DateOfBirth_{dt} - Cutoff_{dts}$, is positive.

[21]I show this in Table 4 of the appendix.

[22]The monotonicity assumption being 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.

delayed entry on different crime categories. I estimate the effect on violent, drug, property, sex, and other crimes. These are supplemental results, as it is possible that the overall crime rate does not change but the age distribution of criminals or the crime composition does. Such an effect is found by Lochner (2004), Lochner and Moretti (2004), and Machin, Marie, and Vujić (2011). All independent 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 makes a stronger case for random assignment than if the cutoffs were constant. Additionally, Barr, Eggleston, and Smith (2022) find confoundedness with January 1 cutoffs, used in Florida prior to the change, due to differences in public assistance at early ages.

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 5 and NVSS data to test my assumption that all observable characteristics vary smoothly at the cutoff.

$$
\begin{aligned}
X_{dts} = {} & \alpha_0 + \alpha_1\, Delayed_{dts} + \beta_2 \left(DateOfBirth_{dt} - Cutoff_{dts}\right) \\
& + \alpha_3\, Delayed_{dts} \times \left(DateOfBirth_{dt} - Cutoff_{dts}\right) + \mu_s + \mu_t + \nu_{dts}
\end{aligned}
\tag{5}
$$

Here, X_{dts} is one of many observables at the time of birth on day d of year t in state s. $Delayed_{dts}$, $Cutoff_{dts}$, μ_s, and μ_t are the same as in equation 4. $DateOfBirth_d - Cutoff_{ds}$ is again the running variable, being the relative distance between birthday and the nearest kindergarten entry cutoff date. Standard errors are again clustered at the month-day level. Due to limitations in the NVSS's demographic variables, I am only able to check smoothness for Whites and Blacks. This covers nearly ninety percent of the prisoner population in Florida and Illinois.

I display the results of equation 5 where X_{dts} is either the number of births, birth weight, mother characteristics, father characteristics, percentage male, percent born in hospitals, or prenatal visits in Figure 3 and report the results in Table 6 of the appendix. I find no discontinuity in any observables at birth, lending

credence to my RDD being properly identified.

Figure 3: Natality Regression Discontinuity Plots
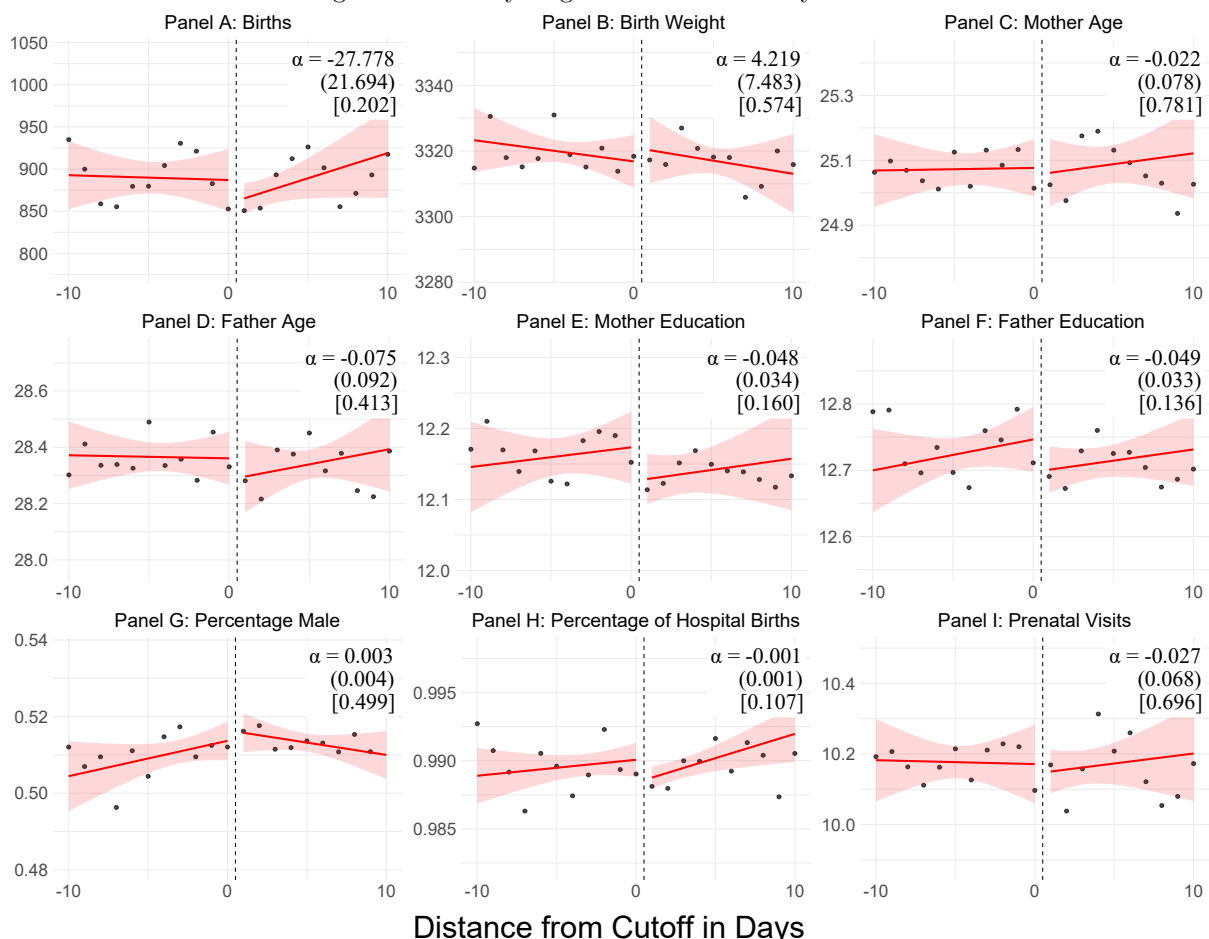
Note: Figure includes natality data from both Florida and Illinois. The horizontal axis is the number of days to the kindergarten entry cutoff and is normalized to zero. Individuals with a positive distance were delayed school entry due to the policy change. Zero corresponds to either December 1, 1975, 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 does not include the initial Florida cohort as it was not reported until 1976. RD results from each shown outcome are displayed in Table 6 of the appendix. Source: National Center for Health Statistics (1975-1985).

V.B.2 Moving

Since I don't observe current residence, a potential concern is individuals moving at differential rates which are correlated with their date of birth relative to the cutoff. The ACS includes state of birth, state of residence when surveyed, and quarter of year of birth.[23] The month by month change of each policy resulted in an October 1 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

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[23]No version of the ACS has more specific dates of birth than quarter.

fourth quarters of those years.

I use variations of the difference-in-differences model in equation 6 to test for differences in moving between each side of the October 1 cutoff in Florida and Illinois.[24]

$$
\begin{aligned}
\text{Move}_{isq} = \gamma_0 &+ \sum_r \gamma_{fr} \left(\text{Delayed}_{isq} \times \text{Female}_i \times \mathbb{1}\left\{ \text{Race} = r \right\} \right) \\
&+ \sum_r \gamma_{mr} \left(\text{Delayed}_{isq} \times \text{Male}_i \times \mathbb{1}\left\{ \text{Race}_i = r \right\} \right) + Z_i' \delta + \mu_s + \eta_{isq}
\end{aligned}
\tag{6}
$$

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.[25] Female_i and Male_i are indicators for the individual's gender, and Race_i is a variable representing i's race. Z_i is a vector of demographic controls including age, gender, and race. μ_s is a series of state fixed effects for the state the individual was born in and standard errors are clustered at the household level.

Equation 6 represents the most complex of my moving models in which I estimate the difference in moving by gender and race. There is no omitted category, so 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 and report all results in Table 7 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 3.3 percentage points less than those born before the cutoff. This represents about a 7% decrease in moving among White men relative to the mean. This biases my estimate of adult crime upwards since the White men delayed kindergarten entry are more likely to remain in their respective state and individuals are most likely to commit crime within their state of residence. [26]

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

[25]In practice, this is an indicator equal to one if the person was born in the fourth quarter of the year (i.e. after September 30) due to the restrictions made on the sample.

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

VI   Results

VI.A   Adult Crime

I first estimate equation 4 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 4. 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. The absence of an aggregate effect is common in the crime literature, as crime rates vary heavily by demographic group with few individuals 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.

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 positive distance were delayed 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 4 is reported along with its standard error (in parentheses) and p-value [in brackets].

VI.B   Heterogeneity By Demographic Group

I examine heterogeneity by gender, race, and gender-race pairs using my RDD 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 in Table 1 and Figure 5 displays the corresponding RD plots.

I find being delayed school entry increases the number of female prisoners. The effect for females is concentrated among White females who see an increase in the number of prisoners by 0.780 per day of birth delayed. This is a large effect relative to the mean, representing about a 50% increase in incarcerations for White females. White women born after the new kindergarten cutoff dates have less required schooling leading to less human capital, on average, and committing more crimes due to lower opportunity costs than those 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 high rates of academic redshirting where parents prevent school entry for their child when they are eligible.[27] 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 early on, although it has 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).[28] This correlates with demographic groups that are least likely to commit crime.

ITT estimates should be interpreted as lower bounds of the local average treatment effect (LATE) due to data limitations. Academic redshirting, migration across states, and the absence of birthplace and school start data create misclassification of treatment status among some individuals. This attenuation implies that my ITT estimates understate the true effect of delayed school entry on incarceration.

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[27]Compulsory 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.

[28]Males 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).

Figure 5: Prisoner Regression Discontinuity Plots by Demographic Group
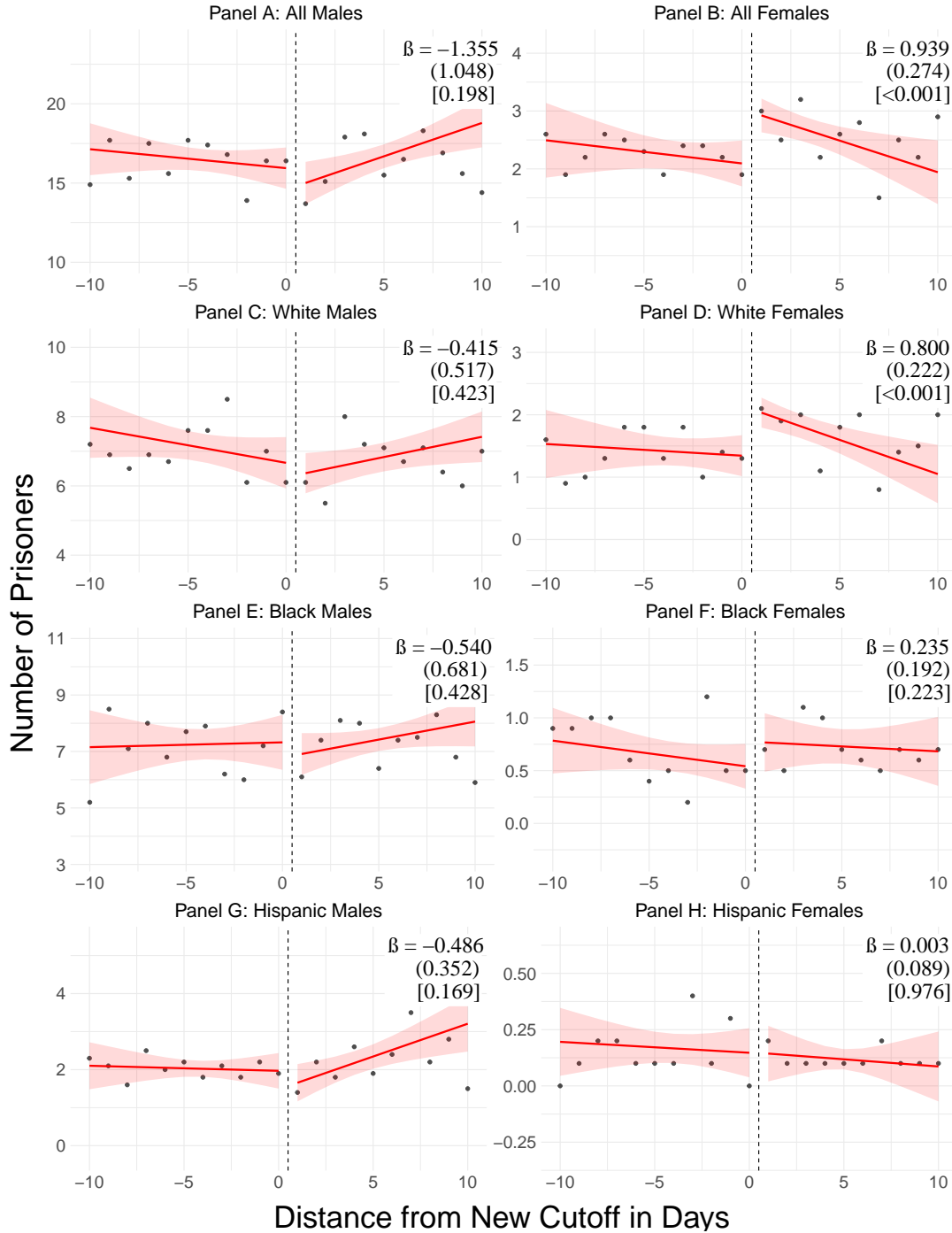
Note: Figure includes data from both Florida and Illinois via FOBIS and IDOC, respectively. The horizontal axis is the number of days to the new kindergarten entry cutoff and is normalized to zero. Individuals with a positive distance were delayed school entry due to the policy change. Zero corresponds 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 4 is reported along with its standard error (in parentheses) and p-value [in brackets].

Table 1: Regression Discontinuity Results by Race and Gender

	All	White	Black	Hispanic
Panel A: Males and Females				
Delayed	-0.416	0.384	-0.305	-0.483
	(0.998)	(0.592)	(0.697)	(0.341)
	{18.662}	{8.381}	{7.890}	{2.267}
Panel B: Males Only				
Delayed	-1.356	-0.415	-0.540	-0.486
	(1.048)	(0.517)	(0.681)	(0.352)
	{16.267}	{6.867}	{7.186}	{2.133}
Panel C: Females Only				
Delayed	0.939***	0.780***	0.235	0.003
	(0.274)	(0.222)	(0.192)	(0.089)
	{2.395}	{1.514}	{0.705}	{0.133}
Bandwidth (Days)	10	10	10	10
N (Days of Birth)	210	210	210	210

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$_{dts}$ 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. The estimated coefficient, β, from equation 4 is reported along with its standard error, clustered at the month-day level, (in parentheses) and outcome average {in braces}. * significant at 10%, ** at 5%, *** at 1%.

VI.C Age Effects and Crime Composition

I generally find no effect on the age at which a prisoner first ends up in prison, as reported in Table 9 in the appendix. 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 who went to prison before age 24 and leads in my average age of prisoner entry to be much higher than in reality. Similarly, I find no effect on crime composition as reported in Table 10 of 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 and Figure 6, with my preferred specification being a ten day bandwidth and a triangular kernel. I find my estimates to be robust to different bandwidths and kernels. I find a consistent effect on females that is concentrated among Whites. 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 a 16% increase in White female crime due to delayed school entry.

Figure 6: Robustness Check by Bandwidth Selection
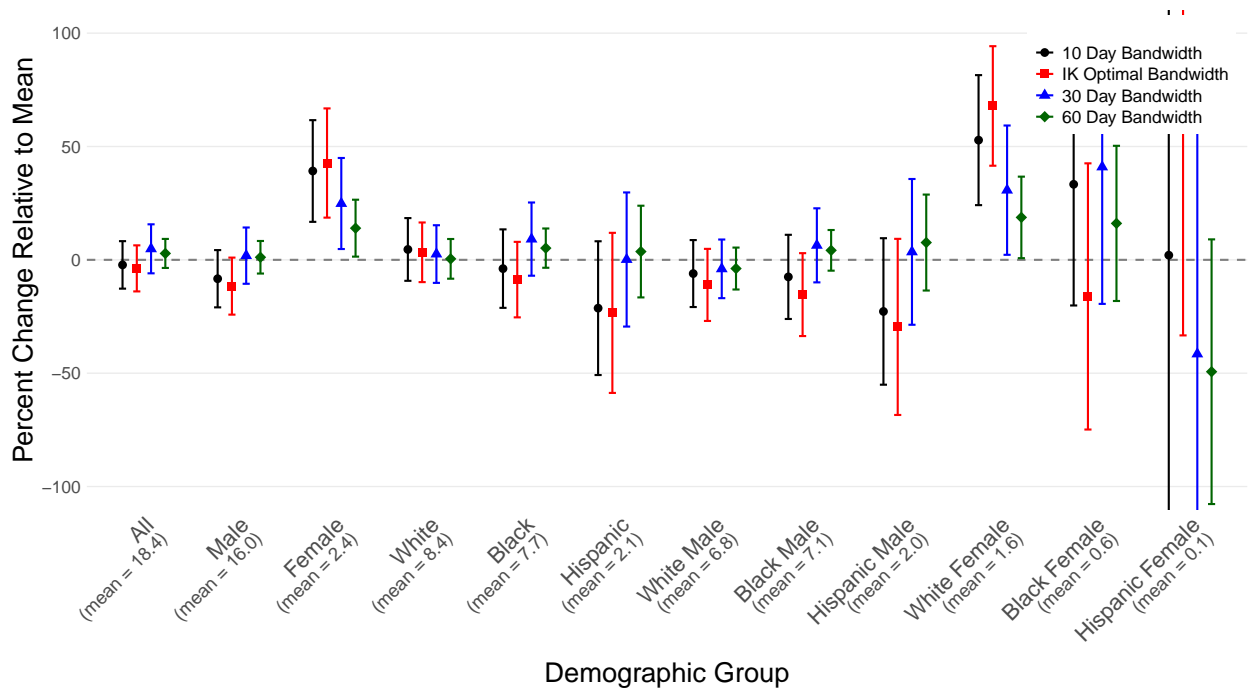
Note: Figure includes data from both Florida and Illinois via FOBIS and IDOC, respectively. The bars represent 95 percent confidence intervals, based on standard errors clustered at the month-day level. All estimates in this figure use a triangular kernel.

VIII   Conclusion

This paper is the first to exploit quasi-random variation in the kindergarten entry cutoff date using policy changes in Florida and Illinois during the 1980s, which occurred after conception, to measure the effect of kindergarten entry cutoff dates on lifetime crime. 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 that is 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 0.780 prisoners per day. I show, using NVSS birth certificate data, that the number of births and birth characteristics remained similar across the cutoff. Additionally, using ACS data, I show rates of moving both into and out of Florida and Illinois are similar on both sides of the cutoff for almost all demographic groups. Together, these imply that the increase in prisoners reflects an increase in the crime rate rather than an increase in births or the relocation of criminals. Compared to the mean, I find delayed school entry increases the crime rate of White women by at least 16%, even by my most modest estimate.

My estimates for White women are higher than those of the literature. Past literature finds delayed

enrollment to increase adult incarceration by over 3% and felony convictions, aged 17-19, by 14% (Cook and Kang 2016 and McAdams 2016). These differences can be explained by my much smaller window and data on the full population of Florida and Illinois prisoners from 1981 and 2005 to present, respectively.

Cook and Kang (2016) use a sixty day bandwidth, while McAdams (2016) does a difference-in-differences with a quarter of birth on either side of the cutoff. My preferred specification includes only those born within ten days of the cutoff. My narrower bandwidth makes both observable and unobservable factors more likely to be indistinguishable across the cutoff due to the seasonality of births and potential for parents to plan (Buckles and Hungerman 2013 and Clarke, Oreffice, and Quintana-Domeque 2019). Similar to Cook and Kang (2016), my data include the entire prison population in Florida and Illinois which prevents selection bias of prisoners on those serving the most time or who committed crime most recently to the survey period.

I find an effect only for women, which is consistent with prior evidence that the consequences of education for crime are often concentrated among women (Depew and Eren 2016 and Cano-Urbina and Lochner 2019). Women receive the highest returns to education, particularly during high school (e.g., Ashenfelter and Krueger 1994; Behrman, Rosenzweig, and Taubman 1994; Dougherty 2005; Blau and Kahn 2017). This matches my setting closely: delayed entrants face one fewer year of required schooling in high school, which allows them to leave school in tenth grade once they reach their sixteenth birthday rather than in eleventh grade. The resulting drop in educational attainment is therefore largest precisely where returns are the greatest.

Marriage market channels further reinforce this pattern. Becker (1991) predicts that increases in education raise the education and earnings of a spouse through assortative mating. Empirical evidence shows marriage market returns to be substantial for White women but smaller or absent for men and nonwhite women (Behrman, Rosenzweig, and Taubman 1994 and Chiappori, Iyigun, and Weiss 2009). These returns are also highest at lower education levels, exactly where compulsory schooling binds (Lefgren and McIntyre 2006 and McCrary and Royer 2011). These gains raise the opportunity cost of criminal activity in ways that do not operate through the labor market. Taken together, the concentration of both labor market and marriage market returns explain why delayed entry increases crime for only white women.

Lastly, I use 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 10%. Put another way, increasing education by one year decreases the probability that a White woman commits crime by over 4.71%.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All	-0.808	-0.417	0.414	0.680	0.510	0.407	0.362	0.169	0.546
	(0.974)	(0.998)	(0.764)	(0.757)	(0.589)	(1.210)	(0.731)	(0.708)	(0.570)
Males	-1.903*	-1.356	-0.120	0.127	0.178	-0.272	-0.019	-0.123	0.230
	(1.053)	(1.048)	(0.756)	(0.720)	(0.574)	(1.231)	(0.689)	(0.698)	(0.556)
Females	1.032***	0.939***	0.534***	0.553**	0.332**	0.680**	0.381*	0.293	0.315**
	(0.298)	(0.274)	(0.211)	(0.220)	(0.152)	(0.310)	(0.213)	(0.199)	(0.153)
Whites	0.283	0.384	0.049	0.092	0.036	0.184	0.013	-0.138	0.075
	(0.571)	(0.592)	(0.464)	(0.521)	(0.372)	(0.720)	(0.451)	(0.429)	(0.365)
Blacks	-0.699	-0.305	0.143	0.362	0.387	0.285	0.154	0.220	0.429
	(0.683)	(0.697)	(0.470)	(0.417)	(0.332)	(0.816)	(0.435)	(0.365)	(0.329)
Hispanics	-0.557	-0.483	0.217	0.186	0.076	-0.114	0.164	0.042	0.036
	(0.389)	(0.341)	(0.252)	(0.274)	(0.216)	(0.384)	(0.238)	(0.273)	(0.216)
White Males	-0.767	-0.415	-0.425	-0.479	-0.259	-0.296	-0.377	-0.393	-0.183
	(0.563)	(0.517)	(0.394)	(0.426)	(0.318)	(0.613)	(0.376)	(0.389)	(0.314)
Black Males	-1.116	-0.540	0.028	0.291	0.288	-0.036	0.089	0.111	0.327
	(0.679)	(0.681)	(0.457)	(0.410)	(0.314)	(0.786)	(0.422)	(0.357)	(0.315)
Hispanic Males	-0.599	-0.486	0.271	0.272	0.148	-0.068	0.256	0.112	0.099
	(0.401)	(0.352)	(0.261)	(0.267)	(0.210)	(0.391)	(0.240)	(0.274)	(0.209)
White Females	1.080***	0.780***	0.474***	0.570***	0.295**	0.479**	0.390**	0.255	0.258*
	(0.214)	(0.222)	(0.189)	(0.200)	(0.145)	(0.285)	(0.187)	(0.189)	(0.145)
Black Females	-0.107	0.234	0.116	0.071	0.099	0.320	0.065	0.109	0.103
	(0.199)	(0.192)	(0.139)	(0.150)	(0.108)	(0.215)	(0.134)	(0.124)	(0.105)
Hispanic Females	0.200	0.003	-0.054	-0.086	-0.072*	-0.046	-0.092	-0.070	-0.063
	(0.128)	(0.089)	(0.056)	(0.062)	(0.043)	(0.088)	(0.056)	(0.054)	(0.042)
IK Optimal?	Yes								
Triangular Kernel	Yes	Yes	Yes	Yes	Yes				
Uniform Kernel						Yes	Yes	Yes	Yes
Bandwidth (Days)	4–9	10	29	30	60	10	29	30	60
N (Days of Birth)	90–190	210	590	610	1,089	210	590	610	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$_{dts}$ 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 8 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: Prisoner Summary Statistics

Variable	Entire Affected Cohorts					Affected Cohorts w/in Bandwidth				
	Mean	SD	Min	Max	Total	Mean	SD	Min	Max	Total
Florida Prisoners	14.908	4.643	3	34	74.539	16.045	4.574	5	30	80.226
Illinois Prisoners	19.697	5.438	4	41	98.480	20.981	5.183	10	37	104.905
Males	15.026	5.293	2	38	150.268	15.979	5.170	4	34	159.791
Females	2.276	1.603	0	11	22.751	2.471	1.631	0	10	24.710
Whites	7.807	3.201	0	24	78.065	8.255	3.123	2	17	82.552
Blacks	7.349	3.609	0	26	73.488	7.884	3.482	1	19	78.840
Hispanics	2.028	1.796	0	11	20.278	2.176	1.805	0	8	21.763
White Males	6.311	2.770	0	19	63.116	6.643	2.649	1	15	66.427
Black Males	6.725	3.488	0	21	67.249	7.202	3.337	0	18	72.019
Hispanic Males	1.895	1.746	0	10	18.942	2.030	1.738	0	8	20.302
White Females	1.495	1.282	0	8	14.949	1.613	1.316	0	6	16.125
Black Females	0.624	0.811	0	6	6.239	0.682	0.832	0	5	6.821
Hispanic Females	0.134	0.371	0	3	1.336	0.146	0.380	0	2	1.462
Age	31.280	4.331	18.146	47.961	312.803	31.274	4.224	22.301	45.032	312.744
Violent Crimes	3.238	3.772	0	18	32.378	3.990	4.027	0	16	34.408
Drug Crimes	3.675	4.238	0	20	36.745	4.038	4.341	0	16	37.726
Property Crimes	1.988	2.529	0	14	19.875	2.510	2.652	0	12	20.046
Sex Crimes	0.718	1.130	0	7	7.185	0.881	1.209	0	7	7.865
Other Crimes	0.226	0.538	0	5	2.253	0.205	0.518	0	3	2.088

Note: Table includes the summary stats of observations at the date of birth level for cohorts in Florida and Illinois affected by the change in the kindergarten entry cutoff date after conception. This includes those born in Florida from 1975 to 1979 and in Illinois from 1981 to 1985. The right panel includes only dates of births within ten days of the new cutoff entry day. The demographic groups listed corresponds to prisoners of that demographic group observed in the combined FOBIS and IDOC data. Similarly, types of crimes listed correspond to prisoners who committed a crime of that category.

Table 6: Natality Regression Discontinuity Results

Outcome	Delayed	Average	Bandwidth (Days)	N (Days of Birth)
Births	-0.278	889.252	10	210
	(21.694)			
Male Births	-11.924	455.429	10	210
	(11.761)			
Female Births	-15.855	433.824	10	210
	(10.978)			
White Births	-22.872	665.105	10	210
	(19.979)			
Black Births	-5.805	210.129	10	210
	(5.079)			
White Male Births	-8.274	341.729	10	210
	(10.828)			
Black Male Births	-4.620	106.376	10	210
	(3.126)			
White Female Births	-14.598	323.376	10	210
	(10.181)			
Black Female Births	1.185	103.752	10	210
	(3.979)			
Birth Weight	4.219	3,318.204	10	210
	(7.483)			
Mother's Age	0.022	25.068	10	210
	(0.078)			
Father's Age	-0.075	28.344	10	210
	(0.092)			
Mother's Education	-0.048	12.152	10	210
	(0.034)			
Father's Education	-0.049	12.722	10	210
	(0.033)			
% Male	0.003	0.512	10	210
	(0.004)			
% Hospital Births	-0.001	0.990	10	210
	(0.001)			
Prenatal Visits	-0.027	10.167	10	210
	(0.068)			

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. The estimated coefficient, β, from equation 4 is reported along with its standard error, clustered at the month-day level, (in parentheses). Source: National Center for Health Statistics (1975-1985). * significant at 10%, ** at 5%, *** at 1%.

Table 7: 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)				0.000 (0.001)			
Delayed×Male		-0.018 (0.012)				-0.002 (0.002)		
Delayed×Female		0.006 (0.012)				-0.002 (0.003)		
Delayed×White			-0.013 (0.010)				-0.003 (0.002)	
Delayed×Black			-0.011 (0.019)				0.003 (0.0.003)	
Delayed×Hispanic			0.027 (0.024)				0.001 (0.005)	
Delayed×Male×White				-0.033** (0.014)				-0.003 (0.003)
Delayed×Male×Black				0.002 (0.024)				0.001 (0.003)
Delayed×Male×Hispanic				0.022 (0.030)				-0.002 (0.006)
Delayed×Female×White				0.009 (0.014)				-0.004 (0.004)
Delayed×Female×Black				-0.023 (0.024)				0.004 (0.004)
Delayed×Female×Hispanic				0.033 (0.030)				0.003 (0.007)
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	5,816,268	5,816,268	5,816,268	5,816,268

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 1977, 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 1977 and 1982 and 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 8: IK Optimal Bandwidths

Outcome Variable	Bandwidth
All Prisoners	9
Male Prisoners	8
Female Prisoners	5
White Prisoners	7
Black Prisoners	8
Hispanic Prisoners	6
White Male Prisoners	6
Black Male Prisoners	7
Hispanic Male Prisoners	6
White Female Prisoners	5
Black Female Prisoners	5
Hispanic Female Prisoners	4
All Prisoners' Age	8
Male Prisoners' Age	8
Female Prisoners' Age	8
White Prisoners' Age	7
Black Prisoners' Age	9
Hispanic Prisoners' Age	9
White Male Prisoners' Age	7
Black Male Prisoners' Age	9
Hispanic Male Prisoners' Age	9
White Female Prisoners' Age	8
Black Female Prisoners' Age	11
Hispanic Female Prisoners' Age	12
Violent Crimes	8
Property Crimes	7
Drug Crimes	7
Sex Crimes	5
Other Crimes	4

Note: Bandwidths represent the optimal bandwidth suggested by Imbens and Kalyanaraman (2012), for each outcome, which have been rounded to the nearest whole number.

Table 9: Age Regression Discontinuity Results by Race and Gender

	All	White	Black	Hispanic
Panel A: Males and Females				
Delayed	-0.620	-1.173	-0.232	-1.662
	(0.724)	(0.855)	(1.486)	(1.868)
	{30.833}	{32.223}	{28.802}	{28.281}
Panel B: Males Only				
Delayed	-1.233	-1.452	-1.044	-0.702
	(0.985)	(1.016)	(1.964)	(1.662)
	{30.313}	{31.721}	{28.119}	{28.070}
Panel C: Females Only				
Delayed	1.405	0.051	5.568***	-12.090**
	(1.139)	(1.382)	(1.869)	(4.836)
	{33.267}	{33.596}	{31.132}	{28.965}
Bandwidth (Days)	10	10	10	10
N (Days of Birth)	210	210	210	210

Note: Standard errors in parentheses, clustered at the month-day level. The dependent variable is the average age of first offense, resulting in imprisonment, 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. The estimated coefficient, β, from equation 4 is reported along with its standard error, clustered at the month-day level, (in parentheses) and outcome average {in braces}. * significant at 10%, ** at 5%, *** at 1%.

Table 10: Crime Category Regression Discontinuity Results

	Violent	Drug	Property	Sex	Other
Delayed	0.355	-0.140	-0.053	-0.073	0.078
	(0.502)	(0.468)	(0.428)	(0.150)	(0.108)
	{3.990}	{4.038}	{2.510}	{0.881}	{0.205}
Bandwidth (Days)	10	10	10	10	10
N (Days of Birth)	210	210	210	210	210

Note: The dependent variable is the number of prisoners born a given number of days from the new cutoff, who was convicted of the given crime type, 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. The estimated coefficient, β, from equation 4 is reported along with its standard error, clustered at the month-day level, (in parentheses) and outcome average {in braces}. * significant at 10%, ** at 5%, *** at 1%.

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