Do Schooling Cutoff Dates Influence Crime? Evidence from Florida and Illinois

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

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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 empirical work (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. In this paper, I exploit variation in birthday cutoffs to start kindergarten. These cutoffs determine whether a child starts kindergarten at age four or five, depending on their date of birth.

I use variation in school starting age that comes from policy reforms in Florida and Illinois during the 1980s. Both states gradually moved their kindergarten eligibility cutoffs earlier in the calendar year. This change caused children born just after the new cutoff date to delay kindergarten by a year. Using data which includes exact dates of birth, I am able to exploit this exogeneity to measure its effects on adult incarceration.

Two human capital channels may explain how school entry cutoffs affect crime. The first relates to compulsory schooling laws which, at this time, are based solely on age rather than grade level or achievement. In most states, including Florida and Illinois, students can leave school at sixteen or seventeen with parental permission or at eighteen without it. Several papers exploit this structure. Angrist and Krueger (1991) use quarter of birth as an instrument for education, showing that additional schooling leads to higher earnings. Lochner and Moretti (2004) use state-level changes in compulsory schooling laws and find that more education reduces arrests and incarceration. Hjalmarsson, Holmlund, and Lindquist (2015) reach similar conclusions using Swedish data. Variation in cutoffs is particularly important in my context, as demographic groups with high crime rates are also those most likely to exit before completing high school (Lochner and Moretti 2004). The second channel is a peer and maturity channel. A child born just after the cutoff becomes one of the oldest in their grade, while a child born just before is one of the youngest. This relative age can shape a child's academic performance and social development. Both Black, Devereux, and Salvanes (2011) and Fredriksson and Ockert (2013) find being older at school entry improves educational performance, although long-run consequences remain ambiguous. Dobkin and Ferreira (2010) find that starting school earlier lowers test scores initially, increases schooling, and has mixed effects on adult wages and employment. I measure the combined effect of these channels on crime by exploiting variation in kindergarten school cutoff dates.

Depew and Eren (2016) find that delayed school entry age reduces juvenile crime among black females and reduces the severity of their crimes. They find no evidence of an effect on males. Other papers, such as Cook and Kang (2016) in North Carolina and Landersø, Nielsen, and Simonsen (2015) in Sweden, find being born just after the cutoff to cause more crime for individuals before they are nineteen. Closest to my study, McAdams (2016) uses U.S. Census data and finds that earlier school entry cutoffs reduce adult incarceration overall, but increases incarceration for those whose start was delayed due to changes in the cutoff. None of the papers in the literature link school starting age cutoffs and delayed school entry to long-run criminal activity using administrative data.

A similar identification strategy appears in Arenberg, Neller, and Stripling (2024), who use Medicaid eligibility cutoffs based on date of birth to estimate the long-run effects of childhood healthcare access on incarceration. Using Florida prison data, they find that Medicaid access reduces incarceration rates for Black youth, particularly for financially motivated offenses. The relationship between schooling and crime is partly driven by incapacitation effects, but these are less relevant when examining the impact of school entry cutoffs on lifetime crime. Anderson (2014) finds that when the legal dropout age is lowered, juvenile crime increases. Bell, Costa, and Machin (2022) finds evidence of both a short-term and long-term incapacitation effect caused by compulsory schooling, with the long-term effect stemming from individuals' reduced likelihood of exposure to criminal environments during adolescence.

This paper contributes to the literature by using Florida and Illinois administrative prison data to exploit the gradual change of school entry cutoffs in the 1980s. Because the reforms were enacted after the affected cohorts were conceived, this, in combination with compulsory schooling laws, generates plausibly exogenous variation in school start age and required schooling. I estimate the causal effect of being born after the cutoff on prison admissions using a sharp regression discontinuity design.

II Institution

School entry cutoffs refer to the date used to determine whether a child starts school that year or must wait until the following year.[1] In both Florida and Illinois, children must turn five on or before the cutoff date of the school year to be eligible to begin kindergarten that same school year. Prior to any variation I exploit, Florida had a kindergarten birthday cutoff of January 1 and Illinois had a cutoff of December 1. Thus, children in Florida must turn five on or before January 1 of the school year in which they begin kindergarten. This creates a discontinuity where the running variable is the distance to this cutoff which is perfectly determined by one's date of birth. Children born on January 1 and January 2 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.

Prior to the 1980 – 1981 school year, 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). In 1978, Florida House Bill 1036 was proposed in Florida to move the kindergarten birthday cutoff by one month per year for four years. This policy ultimately moved the cutoff from January 1 to September 1 over four years. Florida House Bill 1036 was enacted in 1979 and the school cutoff date became December 1st, rather than January 1st, in the 1980–1981 school year. This initially delayed the school start of children born between December 2, 1975 and January 1, 1976 (inclusive) (Florida Legislature 1979).

Illinois had a similar change in kindergarten cutoff proposed in 1984, with the Illinois General Assembly enacting it in 1985 (Education Week 1987). This law also moved the cutoff by one month per year, but for only three years. Illinois's cutoff was already a month earlier than Florida's. 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 relative to the initial year in figure 1 as well as a more detailed version

[1] I use kindergarten and school cutoffs synonymously.

in table 6 of the appendix. These cutoffs remained constant in Florida and Illinois for decades following these changes (Education Commission of the States 2014).

In addition to the initial birthdays delayed which I mention above, each subsequent year, an additional month of birthdays was delayed until each state had a cutoff of September 1.[2] This creates additional discontinuities, which I pool since subsequent cutoffs should not be influenced by earlier changes. I include observations within a month bandwidth for each change in the cutoff.

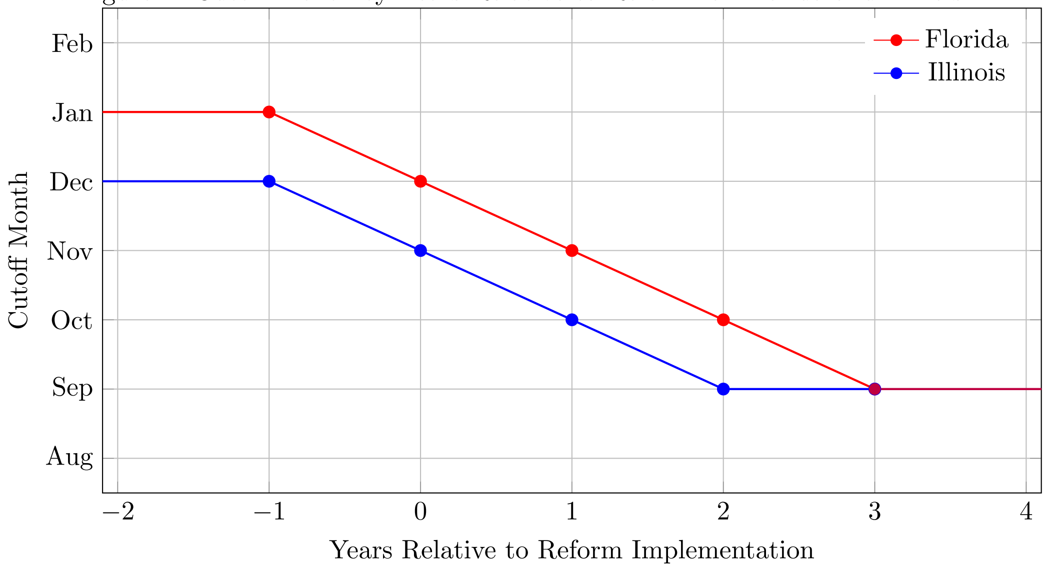
Importantly, neither Florida nor Illinois experienced changes in the minimum age at which students could legally withdraw from school during the sample period (Florida Legislature 1970; Illinois General Assembly 1970; Illinois State Board of Education 1983; Angrist and Krueger 1991; Knapp et al. 2025). Students in Florida could not drop out until the age of sixteen and required a parent or guardian's permission. While those in Illinois were required to attend school until seventeen. Only then could they leave with a parent or guardian's permission. Upon turning eighteen in either state, an individual is considered an adult and can leave school without any permission from a parent or guardian. Thus all variation in the amount of required schooling for these individuals is solely determined by the time that they can enroll in kindergarten.[3]

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

[2]Those delayed include individuals born between the following dates (inclusive) in Florida: November 2, 1976 – January 31, 1977, October 2, 1977 – January 1, 1978, and September 2, 1978 – January 1 1979. The following were also delayed in Illinois: November 1 – December 1, 1981, October 2 – December 1, 1982, and September 2 – December 1, 1983.

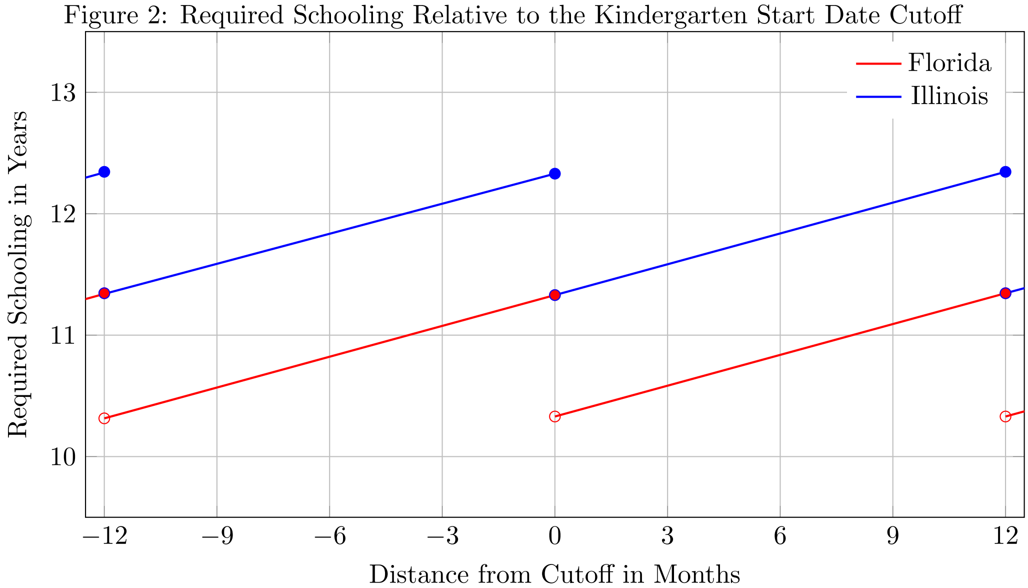
[3]This is under the assumption that parents always enrolled students in school when they were first eligible for kindergarten as compulsory schooling laws at the time allowed children to legally be out of school until six in Florida and seven in Illinois (Angrist and Krueger 1991; Florida Legislature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

Figure 1: Cutoff Month by Years Relative to Reform 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. These school years serve as year 0 in the figure. Based on information from Florida Senate Committee on Education Innovation (1999), Florida Legislature (1979), and Education Week (1987). A more detailed version of this is included in table 6 within the appendix.

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



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

III Data

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

Both FOBIS and IDOC data are reported at the prison-stay-level. I convert each of these datasets to be at the date-of-birth-level with each date of birth including the number of prisoners born on that day, as well as counts and proportions of demographics on a given date of birth. Each date of birth is then assigned a distance to the nearest kindergarten entry cutoff. This entry cutoff is the previous entry cutoffs for non-affected cohorts, and is the new entry cutoff for cohorts affected by Florida House Bill 1036 and the 1985 Illinois General Assembly. I define my running variable, $Distance_{ds}$, as in equation 1.

$$Distance_{ds} = DateOfBirth_d - Cutoff_{ds} \tag{1}$$

Here, $Distance_{ds}$ is the number of days between the date of birth, $DateOfBirth_d$, and its corresponding cutoff date, $Cutoff_{ds}$, in state s. $Distance_{ds}$ can take both positive and negative values. $Cutoff_{ds}$ is the nearest kindergarten entry cutoff in effect in state s for the individuals born on date d.[6]

Lastly, I use natality data from the National Vital Statistics System of the National Center for Human Statistics to test that births are smooth around the cutoff. From these, I observe the race, age, and education of the mother as well as the father, when reported. Other observables include: the child's race, prenatal care, and birth weight. These data are at the mother-level which

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

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

[6]This is either January 1, December 1, November 1, October 1, or September 1 in Florida and December 1, November 1, October 1, or September 1 in Illinois.

I convert to the date-of-birth-level and include counts as well as proportions of births in total and by demographic groups.

IV Identification Strategy

IV.A Regression Discontinuity Design

Each law was enacted in the year prior to taking effect. Since the full roll-out in each state took fewer than five years, I can exploit the discontinuity at each new cutoff since all children were born before the policy was proposed. This timing ensures that parents could not have timed births in response to the policy. Assuming that birthdays are as good as random around the new cutoffs gives appropriate exogeneity lending to a regression discontinuity design (RDD).[7]

Since the cutoff moved up one month per year for four years in Florida and three in Illinois, I pool observations across both states and each cutoff change. Figures 8 and 9 in the appendix display prisoner counts by state for days around the cutoff. 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 one month around the cutoff. This includes individuals born up to thirty days before or after the cutoff. This restricts my sample to date of birth observations with a $Distance_{ds}$ value between -30 and 30 (inclusive). I show my RDD in equation 2.

$$\begin{aligned} Y_{ds} = \beta_0 + \beta_1\, Delayed_d + \beta_2\, Distance_{ds} \\ + \beta_3\, Delayed_d \times Distance_{ds} + Florida_s + \epsilon_d \end{aligned} \tag{2}$$

Here, Y_{ds} is either the number of prisoners born on date d who were admitted to prison at any time during the observation period or the total number of prison admissions of those born on that date of birth. These specifications for Y_{ds} give me an estimate for both the extensive and intensive margins. $Delayed_d$ is an indicator equal to one if $Distance_{ds}$ is greater than 0, meaning that individuals born on day d in state s were delayed kindergarten entry a year due to the change in birthday cutoff for their cohort. $Florida_s$ is a state fixed effect equal to one for Florida observations.

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

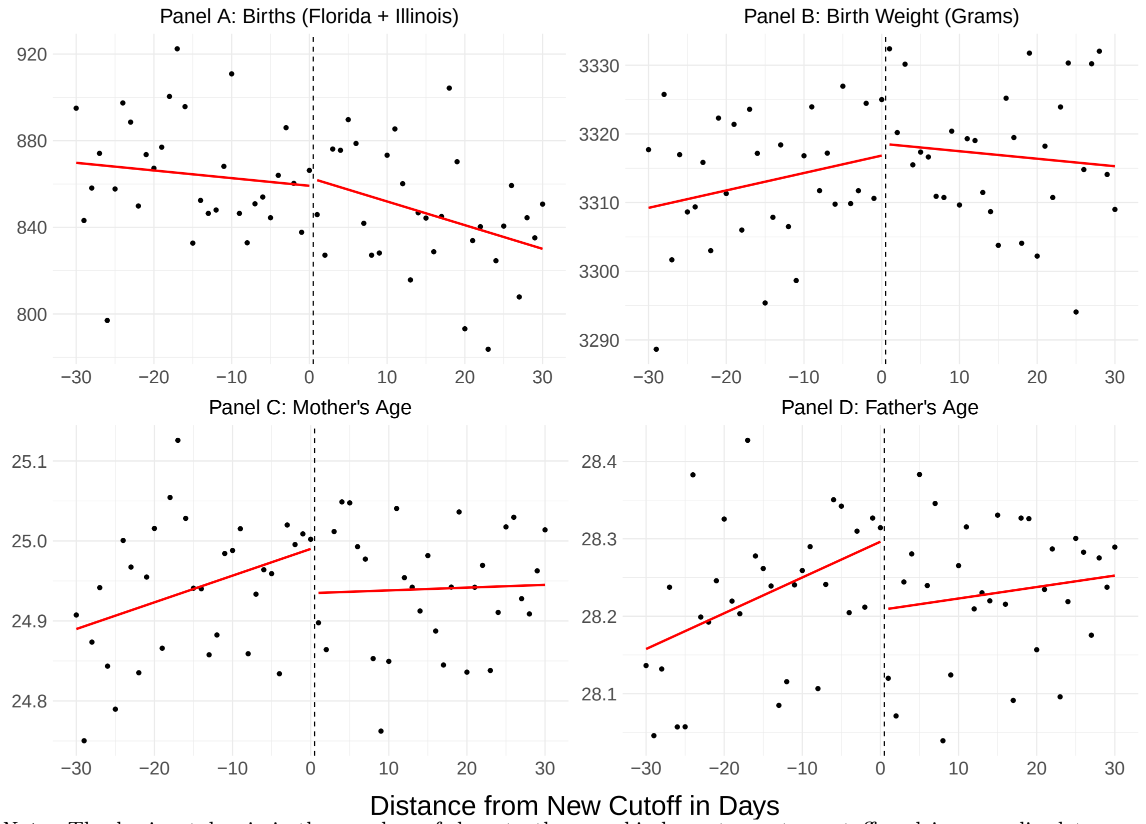
β_1 is the local average treatment effect (LATE) of a delayed kindergarten start date on the number of prisoners born on a given day. This LATE is identified under two assumptions: (1) that births are as-good-as-random around the cutoff, and (2) a monotonicity assumption. I test the first assumption in section IV.B. The monotonicity 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. If β_1 is statistically significant, then there is a discontinuous change in the number of prisoners born just after the cutoff compared to before. A positive β_1 provides evidence of increased rates of incarceration due to being born after the cutoff. Thus the combination of being the oldest in your cohort and being required to have up to one year less of education increased the likelihood of incarceration.

IV.B Births Around the Cutoff

Random assignment of births around school entry cutoffs is a common assumption in empirical economics (e.g. Depew and Eren 2016, Cook and Kang 2016, and McAdams 2016). 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 initial few cohorts were born (and conceived) making it impossible for parents to plan in any way around new kindergarten entry cutoffs. This is important as any RDD requires that all other factors (observable and unobservable) evolve smoothly at the cutoff in order to identify the effect of treatment caused by the cutoff (Lee and Lemieux 2010).

I use natality data from the National Vital Statistics System of the National Center for Health Statistics to look at births around the new kindergarten entry cutoffs. I display the results of equation 2 where Y_{ds} is either the number of births, birth weight, mother's age, or father's age in figure 3 and report the results in table 1. I find there to be no discontinuity in births, birth weight, mother's age, or father's age at the new cutoffs which lends credence to my RDD being properly identified.

Figure 3: Natality Regression Discontinuity Plot



Note: 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, or September 1, 1978 in Florida and either November 1, 1981, October 1, 1982, or September 1, 1983 in Illinois. RD results from each shown outcome are displayed in table 1 with none being significant. Source: National Center for Health Statistics (1977-1984).

Table 1: Natality Regression Discontinuity Results

	Births	Birth Weight	Mother's Age	Father's Age
Delayed	5.298	-32.174	-0.065	-0.190
	(9.926)	(45.796)	(0.108)	(0.150)
Florida	-220.433***	-13.788	-0.533***	0.360***
	(5.006)	(23.095)	(0.055)	(0.075)
Bandwidth (Days)	30	30	30	30
N (Days of Birth)	427	427	427	427
Average	389.279	3,293.373	27.941	31.262

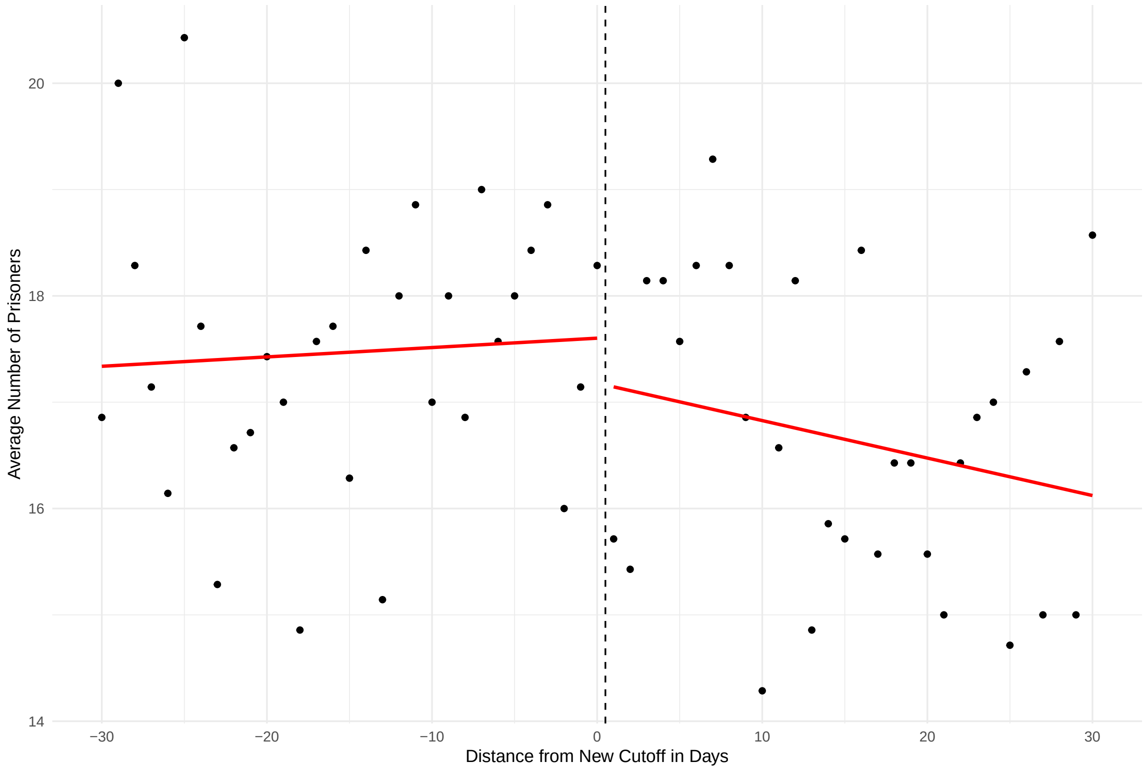
Note: Standard errors reported in parenthesis. The dependent variable is indicated at the top of the column. *Delayed* is a dummy indicating that people born in that state on that date of birth had their kindergarten start date moved after they were born. The average of each outcome within the bandwidth is reported in the bottom row. *significant at 10%, **significant at 5%, ***significant at 1%. Source: National Center for Health Statistics (1977-1984).

V Results

V.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 table 2 reports the results of the RDD from equation 2. I find no difference in the count of prisoners born on a given day due to the kindergarten entry cutoff. It is common in the literature to find no effect in aggregate (e.g. Landersø, Nielsen, and Simonsen 2015, Depew and Eren 2016, and McAdams 2016), so I continue in the following section by examining heterogeneous effects among demographic groups.

Figure 4: RD Plot of Florida and Illinois Prisoners Relative to Cutoff



Note: 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, 1982, November 1, 1983, October 1, 1984, or September 1, 1985 in Florida and either November 1, 1986, October 1, 1987, or September 1, 1988 in Illinois.

V.B Heterogeneity Between Demographic Groups

I examine heterogeneity by gender, race, and gender-race pairs using model 2 on subsets of my sample. This decreases the number of prison observations in each result, but does not affect my statistical power. This is because my unit of observation is at the date-level, so my RDD is only restricted by the bandwidth which is the number of days away from the cutoff. I report the results by gender in table 2, race 3, race for males in 4, and race for females in table 5. Figures 5 and 6 display RD plots of males and females, respectively, by race.

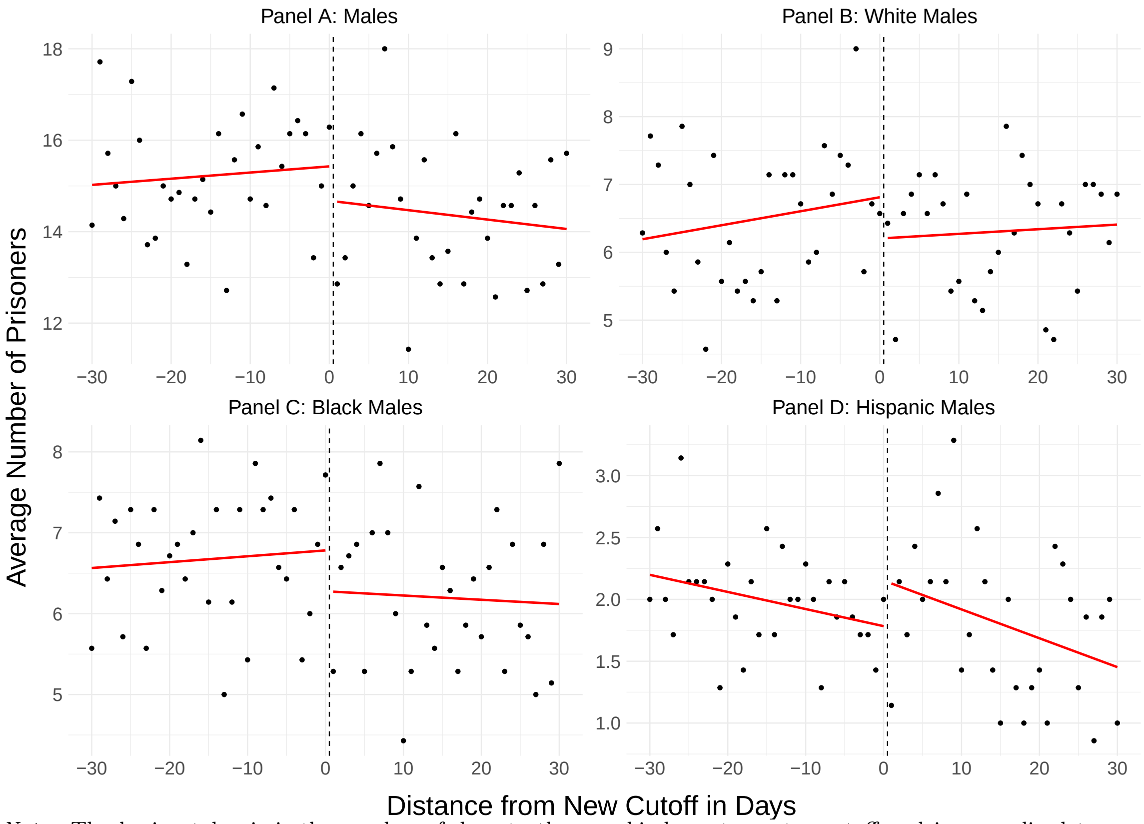
There is large variation in the number of prisoners born based on gender and race, but I again find no effect of the kindergarten entry cutoff affecting the number of prisoners born.

Table 2: Regression Discontinuity Results by Gender

	All	Male	Female
Delayed	-0.422	-0.752	0.330
	(0.863)	(0.804)	(0.279)
Florida	-8.007***	-7.821***	-0.186
	(0.435)	(0.406)	(0.279)
Bandwidth (Days)	30	30	30
N (Days of Birth)	427	427	427
Avg Prisoners	17.059	14.800	2.260

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

Figure 5: RD Plot of Male Florida and Illinois Prisoners Relative to Cutoff without FEs



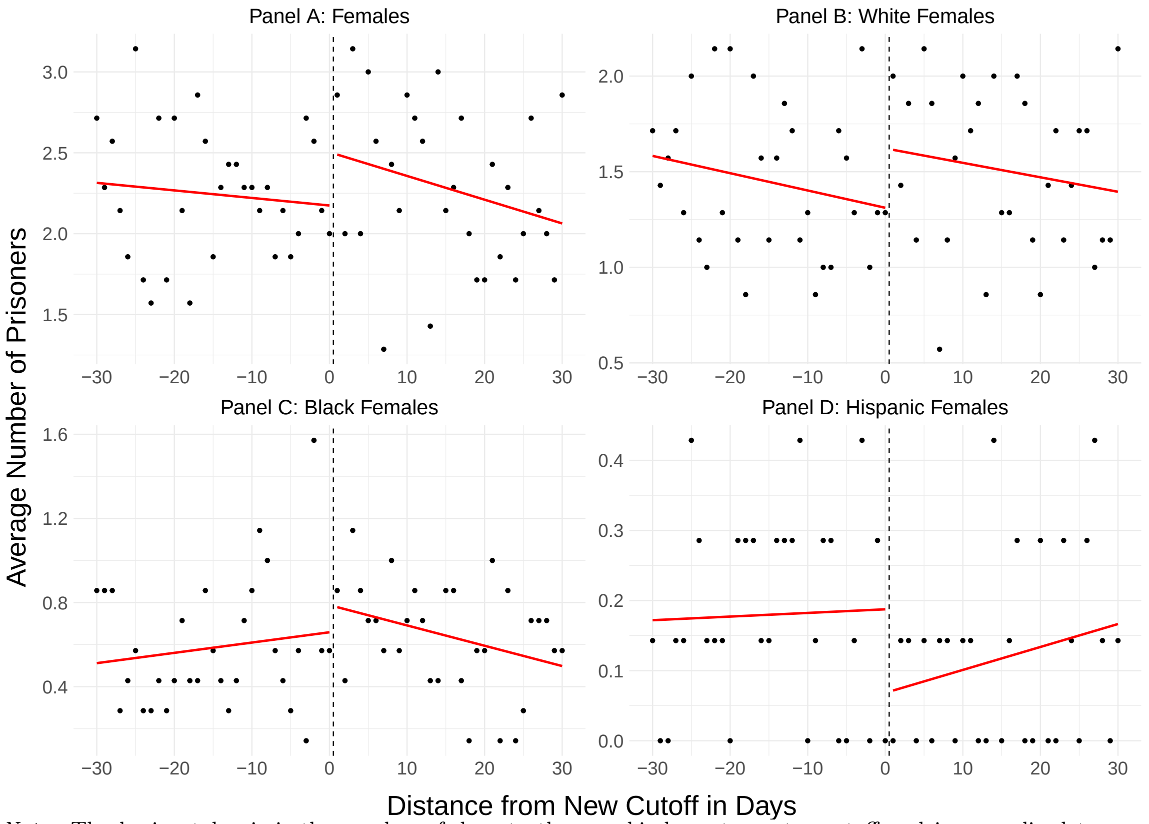
Note: The horizontal axis is the number of days to the new kindergarten entry cutoff and is normalized to zero. Individuals with a distance greater than zero from the cutoff were delayed in school entry due to the policy change. Zero corresponds to either December 1, 1982, November 1, 1983, October 1, 1984, or September 1, 1985 in Florida and either November 1, 1986, October 1, 1987, or September 1, 1988 in Illinois. Panel A provides an RD plot of only male prisoners with panels B, C, and D representing subsets of the whole male sample.

Table 4: Male Regression Discontinuity Results by Race

	All	White	Black	Hispanic	Asian	Native American
Delayed	-0.752	-0.610	-0.505	0.369	-0.014	-0.000
	(0.804)	(0.517)	(0.476)	(0.282)	(0.033)	(0.013)
Florida	-7.821***	-0.167	-5.115***	-2.512***	-0.060***	0.000
	(0.406)	(0.260)	(0.240)	(0.142)	(0.017)	(0.000)
Bandwidth (Days)	30	30	30	30	30	30
N (Days of Birth)	427	427	427	427	427	427
Avg Prisoners	14.800	6.407	6.438	1.892	0.026	0.005

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

Figure 6: RD Plot of Female Florida and Illinois Prisoners Relative to Cutoff without FEs



Note: 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, 1982, November 1, 1983, October 1, 1984, or September 1, 1985 in Florida and either November 1, 1986, October 1, 1987, or September 1, 1988 in Illinois. Panel A provides an RD plot of only female prisoners with panels B, C, and D representing subsets of the whole female sample.

Table 5: Female Regression Discontinuity Results by Race

	All	White	Black	Hispanic	Asian	Native American
Delayed	0.330	0.311	0.129	-0.119	0.002	0.017
	(0.279)	(0.230)	(0.153)	(0.072)	(0.009)	(0.019)
Florida	-0.186	0.143	-0.202**	-0.092*	0.004	-0.022*
	(0.140)	(0.116)	(0.077)	(0.037)	(0.005)	(0.009)
Bandwidth (Days)	30	30	30	30	30	30
N (Days of Birth)	427	427	427	427	427	427
Avg Prisoners	2.260	1.475	0.611	0.150	0.002	0.010

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

Table 3: Regression Discontinuity Results by Race

	All	White	Black	Hispanic	Asian	Native American
Delayed	-0.422	-0.297	-0.376	0.249	-0.011	0.017
	(0.863)	(0.567)	(0.518)	(0.288)	(0.344)	(0.023)
Florida	-8.007***	-0.023	-5.317***	-2.604***	-0.056**	-0.014
	(0.435)	(0.286)	(0.261)	(0.145)	(0.017)	(0.012)
Bandwidth (Days)	30	30	30	30	30	30
N (Days of Birth)	427	427	427	427	427	427
Avg Prisoners	17.059	7.883	7.049	2.042	0.028	0.014

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

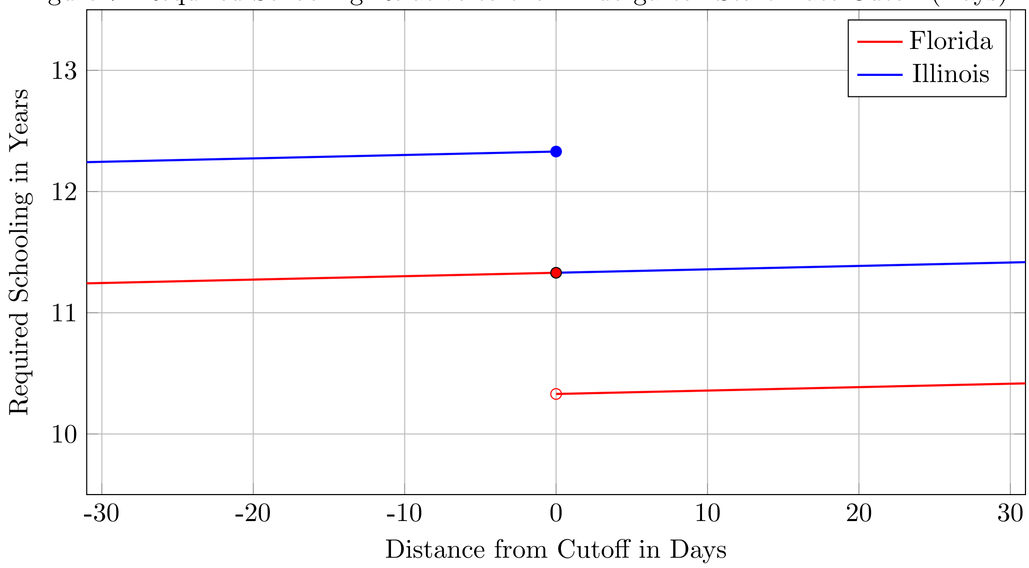
VI Appendix

Table 6: Kindergarten Cutoff Birthday by State and Year

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

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

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



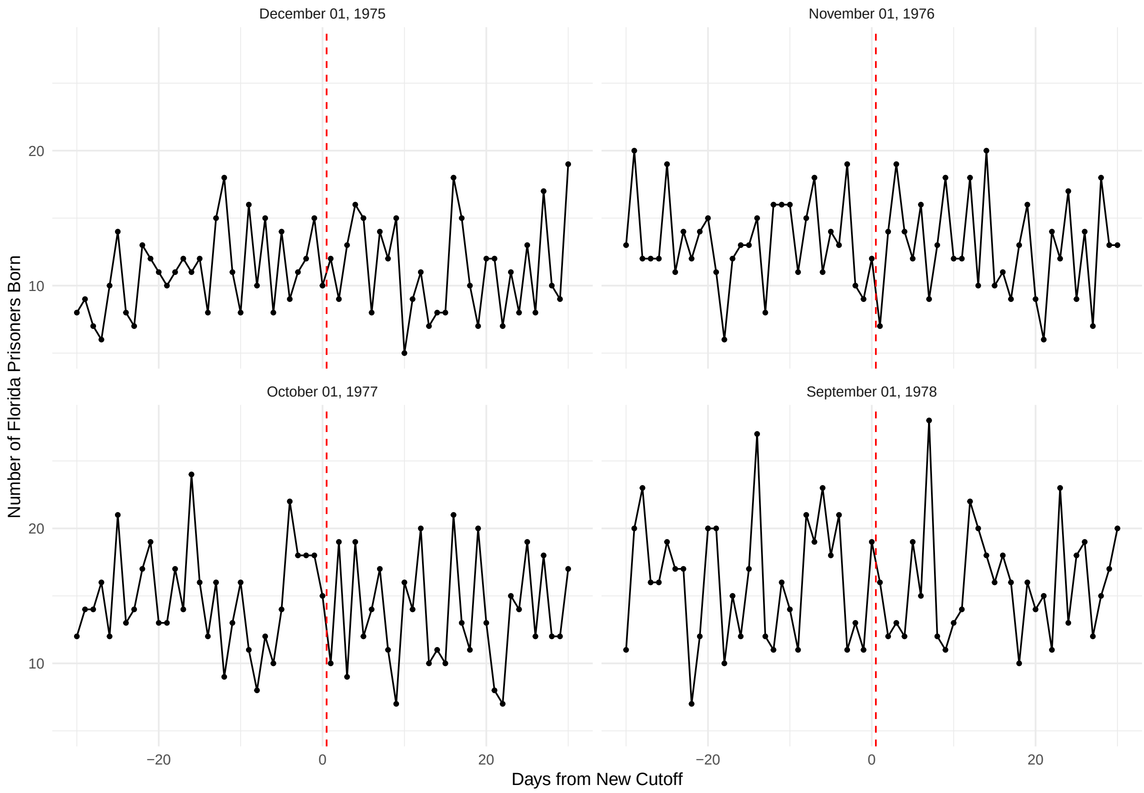
Note: The school cutoff always took place on the first of the month. Florida implemented the reform in the 1980 – 1981 school year while Illinois did so in the 1986 – 1987 school year. These school years serve as year 0 in the figure. Required schooling is calculated as the difference in age between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

Table 7: Prison Admissions Data Available in Illinois by Age

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

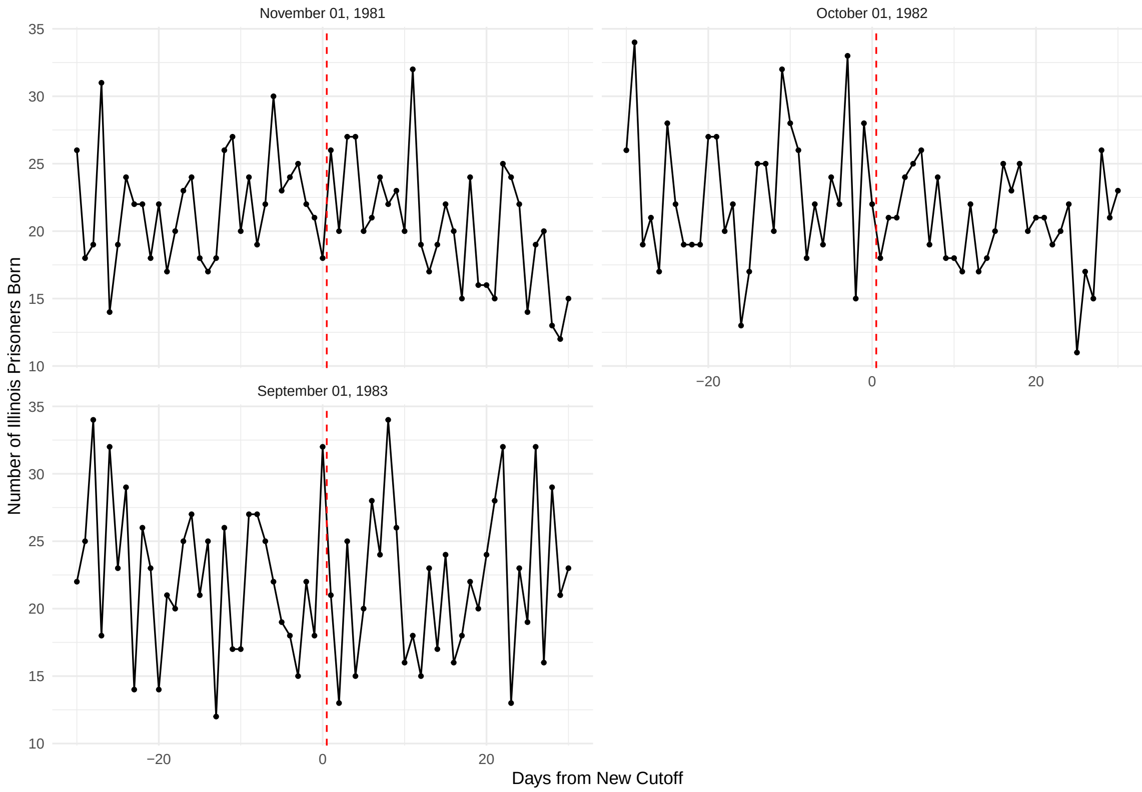
Note: Illinois Department of Corrections (IDOC) prison admissions data is only available from 2005 onward. As a result, the number of observed years of criminal history increases for later cohorts. I observe admissions data starting at age 24 for the first cohort affected by the policy, and gain an additional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

Figure 8: Number of Prisoners Born in Florida Relative to All New Cutoffs



Note: Figure is based on FOBIS data and includes the number of prisoners born within thirty days of each new kindergarten entry cutoff age.

Figure 9: Number of Prisoners Born in Illinois Relative to All New Cutoffs



Note: Figure is based on IDOC data and includes the number of prisoners born within thirty days of each new kindergarten entry cutoff age.

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