



Born on the wrong day? School entry age and juvenile crime[☆]



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ABSTRACT

Kindergarten entry age is known to impact schooling outcomes. Less is known, however, about the role of school starting age on economic outcomes outside of the classroom. In this paper we use administrative data from Louisiana to analyze the effect of school starting age on juvenile crime. We find that late school entry by one year reduces the incidence of juvenile crime for young black females, particularly in high crime areas. The mediating effects of late school entry for this subgroup appear to be driven by reductions in non-felony offenses. We propose age related differences in human capital accumulation as a potential explanation for our findings.

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1. Introduction

Delaying a child's entry into kindergarten has become more popular over time. In 2008, 83% of six-year-old children were enrolled in first grade, compared to 91% of children in 1995. The increase in average school starting age is mostly driven by the choices of parents and the law changes at the state level (Deming and Dynarski 2008). Delayed entry may provide a child with a competitive edge through increased cognitive, emotional and physical development. Teachers may also encourage late entry because mature children are likely to be more amenable and cause less distraction in the classroom (Aamodt and Wang 2012).¹

Recent trends in delayed entry coupled with non-negligible variation in school entry laws across states have attracted the in-

terest of many researchers regarding the role of school entry age on various outcomes.² Several studies find strong and positive associations between entry age and achievement. These papers generally attribute the observed effects to differences in the rate of learning between older and younger entrants (see, for example, Bedard and Dhuey 2006; Datar 2006; and Barua and Lang 2008). Another set of research indicates that the estimated effects of school starting age partly reflect the endowment differences between students when they start school and they find little evidence that older entrants learn more in school (see, for example, Cascio and Schanzenbach 2007; and Elder and Lubotsky 2009).

In addition to achievement effects, research has also examined the relationship between school entry age and longer-run outcomes. Most commonly studied has been the effect of school entry age on educational attainment and labor market outcomes (Dobkin and Ferreira 2010; Black et al. 2011; McCrary and Royer 2011; Bedard and Dhuey 2012; and Fredriksson and Öckert 2014). Black et al. (2011) also find that starting school at a younger age increases the probability of teen pregnancy in Sweden. In a study of U.S. mothers, McCrary and Royer (2011) show that starting school

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¹ Deming and Dynarski (2008) show that two-thirds of this increase in starting age is explained by parents and teachers choosing to keep younger students out of kindergarten and first grade. The other one-third is the result of states increasing the legal entrance age of kindergarten.

² Forty-three states have set their minimum school entry age at age five. The remaining states do not have a uniform law and regulations regarding kindergarten entry are at the local education agency's discretion. Within those with a uniform law, twenty-seven have September age cutoff, nine have August age cutoff and the rest of the states have other months of the year as their cutoff ranging from January 1st to December 31st (National Center for Education Statistics, 2014, Table 5.3).

at a younger age improves the quality of a woman's mate without any significant effect on fertility and infant health.

Until recently, the relationship between school starting age and crime had been overlooked. However, given the robust results of school starting age on student achievement, it is plausible that children who are younger when they start school are more likely to pursue non-educational activities as they fail to be competitive and fall behind in the classroom. Crime is one alternative activity and is one of the most damaging avenues a youth can be diverted to. Research has shown that children that become involved in criminal activity are significantly less likely to graduate from high school (Hjalmarsson, 2008).

Two recent studies have analyzed the link between school start age and crime. Using detailed register-based Danish data, Landersø et al. (2013) find that late school entry by one year lowers the propensity to commit crime before the age of 18. The other recent study uses administrative data from North Carolina (Cook and Kang, 2016). They find that individuals born immediately after the school starting date cutoff (oldest in their cohort) are less likely to receive a formal juvenile complaint between the ages of 13–15, but more likely to receive an adult felony conviction between the ages 17 and 19. Cook and Kang (2016) suggest that crime reducing effects of late school entry observed at early ages are driven by better school performance while they attribute the increase in adult crime for this same group at later ages to a higher propensity to drop out of high school. As suggested by Cook and Kang (2016), at first glance, the findings of Landersø et al. (2013) and their own findings on felony convictions for 17–19 year-olds seem to be at odds with one another. However, since compulsory schooling is tied to age (legally allowed to drop out at 16) in North Carolina and grade level (completed 9th grade) in Denmark, they suggest the studies may actually reinforce each other.

Given that there are only two previous studies analyzing school starting age and crime, that the two studies are in different countries with different institutional features, and that the findings do not exactly align, additional evidence from different settings is beneficial to policymakers and researchers. Using administrative data from the Louisiana public school system, we build on the work of the two previous studies by further investigating the association between school start age and crime. The diverse population of students in Louisiana schools provides us with an excellent opportunity to study the potential effects by race and gender. Given the increasing trends in juvenile crime involvement over the last three decades, a separate discussion by race and gender may be warranted.³

To obtain the effect of school start age on juvenile crime, we implement a similar identification strategy as in Cook and Kang (2016). Specifically, a child must be five years old by September 30 in order to start kindergarten in the state of Louisiana. We compare children born just before and just after the school entry cutoff to identify the effects of school entry age. Like the previous two studies, we cannot completely purge out years of schooling effects from entry effects, given students with late entry are likely to accumulate fewer years of schooling by the end of the window over which juvenile crime is measured (i.e., early entrants

are likely to graduate by 17, while late entrants are still in twelfth grade). We have three distinct contributions in the U.S. context.

First, our data set allows us to observe the complete juvenile delinquency history of all children in the state through age 17. The majority of the juvenile offenses, at least in the state of Louisiana, occur between ages 15–17 and thus we can get a complete picture of the juvenile crime. Second, detailed information on convictions allow us to classify juvenile crime into broad categories (i.e., felony vs. non-felony), and estimate the school entry age effects by severity and types of crimes. Finally, we can estimate both the reduced form and two stage least squares (2SLS) effects due to availability of the actual age of entry into public kindergarten in the data. The effect for compliers is an equally interesting parameter (Imbens and Wooldridge, 2009). Apart from these empirical contributions, we also have a methodological contribution. Specifically, we introduce Lee's (2009) bounds estimator for sample selection in the regression discontinuity design (RDD) framework.

Viewing the complete set of results, we have the following findings. Late school entry age by one year seems to statistically reduce the incidence of juvenile crime among young black females. This effect on young black females is more prominent in high crime areas. We also find strong evidence that the mediating effects of late school entry for black females is driven by reductions in non-felony offenses, i.e., less serious offenses. Potential contamination of the estimated effects for white females due to attrition does not allow us to make firm conclusions in our main set of analysis. For males, however, we do not find any effect of late school entry on the propensity to commit juvenile crime. Several robustness checks, placebo regressions using false entry cutoffs and bounds estimators support our findings. To further explore the potential channels leading to these heterogeneous effects, we analyze test scores in English and Math at eighth grade. Our findings from this exercise similarly show that late school entry significantly increases test scores for females, but not males. We propose age related differences in human capital accumulation as a potential explanation for crime reducing effects of school starting age.

The remainder of the paper is as follows. Section 2 discusses the identification strategy and RDD. Section 3 discusses the institutional settings and the data used in the analysis. Section 4 tests for potential sample selection, presents results on juvenile crime, provides additional robustness checks to the main results, presents additional analysis and results that account for sample selection, and discusses potential mechanisms. Section 5 concludes.

2. Empirical methodology

To estimate the effect of school entry age on juvenile crime, we begin by presenting the following model,

$$JC_i = \beta_0 + \beta_1 Late_i + X_i' \beta_2 + u_i, \quad (1)$$

where JC_i is an indicator variable that takes the value of one if child i commits a crime over the window in which juvenile crime is measured. The variable of interest, $Late_i$, is an indicator variable that takes on the value of one if child i enters the school late, i.e., begins school at age six rather than age five. X_i is a vector of observed covariates and u_i is an unobserved term. The coefficient β_1 represents the effect of late school entry on juvenile crime.

Straightforward estimation of Eq. (1) via OLS will provide an unbiased coefficient estimate of β_1 if school starting age is exogenously determined. However, there are many potential unobserved factors that affect juvenile crime that are also correlated with the school starting age of a child (e.g., parental motivation, child's maturity). Ignoring these factors in the estimation of Eq. (1) will likely yield a biased coefficient estimate of the impact of school entry age on juvenile crime.

To address these potentially confounding effects, we rely on the exogenous variation generated by the school entry policies in

³ In 2011, courts with juvenile jurisdiction handled 3400 delinquency cases per day, compared to 1100 delinquency cases in 1960 (Hockenberry and Puzanchera 2014). In addition to this upward trend, there have been also some remarkable changes in gender- and race-specific juvenile crime involvement. Although males comprise a majority of cases, female involvement in juvenile crime grew considerably over the last three decades. Specifically, between 1985 and 2011, the number of delinquency cases involving females increased 55%, as opposed to a decrease of 5% for males. Turning to racial profile of juvenile crime, in 2011, black youth made up 16 percent of the U.S. population under juvenile court jurisdiction, but approximately 33% of all delinquency cases involved black youth. Unlike white youth, involvement in juvenile crime for black youth has displayed a constant increase from 30% in 2001 to 33% in 2011.

Louisiana. Specifically, we exploit the fact that the year in which a child starts school is a discontinuous function of the child's date of birth. As noted, a child must be five years old by September 30 in order to start kindergarten in the state of Louisiana.⁴ To the extent that children born near in time are similar in observed and unobserved dimensions, we can take advantage of the state's entry policy to identify the effect of late school entry on juvenile crime through a RDD framework. Specifically, we use a parametric fuzzy RDD and implement the estimation by the following two equation system:

$$Late_i = \pi_0 + \pi_1 Cut_i + g(BD_i) + X_i' \pi_2 + v_i, \quad (2)$$

$$JC_i = \gamma_0 + \gamma_1 Late_i + f(BD_i) + X_i' \gamma_2 + \epsilon_i. \quad (3)$$

In this model, $Cut_i = 1\{BD_i > 0\}$ is an indicator variable that takes the value of one if a child's birthday is after the state's defined official school entry threshold date. BD_i is the number of days from child i 's birthday to the state's official entry date.⁵ The functional form between BD_i and the outcome variables in the model, $Late_i$ and JC_i , are described by the polynomial functions $g(\cdot)$ and $f(\cdot)$, respectively. v_i and ϵ_i are the unobserved terms for the first and the second stage equations, respectively. All other variables in Eqs. (2) and (3) are as previously defined.

The key identifying assumption underlying this framework is that the functions $g(\cdot)$ and $f(\cdot)$ are continuous through the school entry date, i.e., a child's date of birth near the school entry cutoff is as good as random. Under this assumption, the 2SLS estimate of γ_1 , using the school entry cutoff indicator as instrument, produces a consistent coefficient estimate of late school entry on juvenile crime for those whose school entry decision are causally affected by the state's entry policy, the so-called *compliers*. Needless to say, it is not necessary for all families to follow the school entry laws. For example, parents of children with developmental difficulties may voluntarily delay school entry and/or some other parents may obtain exceptions to accelerate school entry. In either case, there will be noncompliance with the school entry policies and depending on the degree of noncompliance, the set of compliers do not necessarily need to represent the entire population. The instrumental variable estimator from Eq. (3) can be interpreted as the Local Average Treatment Effect (LATE) (see, for example, Imbens and Angrist, 1994) under the identifying assumption of monotonicity. In its simplest form, monotonicity would be violated if a parent would choose to delay school entry if his child were born before the school start cutoff, but would choose to petition the school district to allow his child to begin early if he was born after the school start cutoff. This kind of behavior is unlikely.

As a complement to 2SLS, one can also estimate a reduced form version of Eq. (3), which is given by

$$JC_i = \lambda_0 + \lambda_1 Cut_i + h(BD_i) + X_i' \lambda_2 + \epsilon_i, \quad (4)$$

where similarly, $h(\cdot)$ is a polynomial in BD_i , ϵ_i is the unobserved term, and all other variables are as previously defined. In this setup, for individuals near the school entry cutoff, the coefficient estimate λ_1 can be interpreted as the effect of receiving school entry eligibility almost a year later.

3. Institutional settings and data

3.1. Institutional settings

In this section, we briefly summarize the juvenile justice system in the state of Louisiana. Children begin the intake process in

the juvenile justice system when they are accused of committing a crime and arrested or referred by police to a juvenile court.⁶ Having received a formal complaint from a local law officer, the District Attorney's (DA) Office must decide whether or not to petition the case. Prosecutors may choose not to do so because of lack of sufficient evidence. In this case, the child will not appear in the juvenile justice system. Rather than filing a petition, the DA's Office may choose to enter into an informal agreement (diversion program) to prevent incarceration. An informal adjustment agreement occasionally entails a child to participate in community service, restitution, or treatment and comply with certain behavioral requirements such as school attendance (*Louisiana Children's Code CHC 631*). Finally, prosecutors may proceed with a petition. If so, the file then moves towards a formal hearing in which case the adjudication and disposition outcomes must be determined by a juvenile court judge (*Louisiana Children's Code CHC 650–675*).

3.2. Data

The data for this study come from two different sources. The first one is the administrative records from the Louisiana Department of Education from 1997 through 2012. The administrative data include basic information such as student's gender, race, free lunch status and exact date of birth.⁷ Unique state identification numbers allow us to track all the students through their tenure in the public school system, including charter schools. Thus, we are able to identify each school a student is enrolled in from 1997–2012. Furthermore, the data contain information on each public school student's Math and English proficiency in eighth grade. In Louisiana, state administered tests, known as Louisiana Educational Assessment Program (LEAP), have been given to eighth graders for the subjects of English Language Arts (ELA) and Mathematics, since 1999.

Our crime data come from the Louisiana Department of Public Safety and Corrections, Youth Services, Office of Juvenile Justice. By special permission, we obtain access to juvenile justice files that provide information on all entries occurring in the state for the period 1997–2012 unless the DA's Office (or disposition judge) dismissed the case due to lack of evidence. Our data consists of all juveniles that are adjudicated delinquent, including both juveniles who are incarcerated and juveniles who are placed on probation. With respect to Cook and Kang (2016), who observe juveniles that receive a formal complaint between the ages of 13–15, our data is likely to reflect the upper end of the crime involvement spectrum since we observe delinquent juveniles and not juveniles who had the charges dropped. The files include the type of crime the individual committed, the date the individual was admitted to the juvenile justice system, and the location of the offense. In addition, we are able to observe the same personal identification number in the juvenile justice data that was also in the Louisiana public school data. Thus, we are able to merge these two data sets to obtain our primary research sample.

Our main outcome of interest throughout the paper is the indicator variable, JC_i , which can be observed for all children through the age of 17. We also classify juvenile crimes by severity: felony vs. non-felony, which are provided in the data.⁸

⁶ The lower age for juvenile court jurisdiction is not specified in the state of Louisiana; the upper age is 17 years old.

⁷ Our data set treats Hispanics as a race identifier rather than an ethnicity identifier.

⁸ Following the National Incident-Based Reporting System (NIBRS) guidelines, we are also able to classify juvenile crimes into the following three broad categories: (i) crimes against persons (i.e., murder, assault, sex offenses), (ii) crimes against property (i.e., theft, robbery, burglary), and (iii) crimes against society (i.e., disorderly conduct, drug violations, weapon law violations). Information regarding the details

⁴ New Orleans parish is an exception; a child must be five years old by December 31 in order to start kindergarten.

⁵ For example, BD_i takes the value of -10 for a child with a birthday on September 20, while it takes the value of 10 for a child with a birthday on October 10.

Table 1
Summary statistics.

	Mean (Standard error)				
	Full sample (1)	White females (2)	Black females (3)	White males (4)	Black males (5)
School entry eligibility (1=Yes)	0.747 (0.434)	0.749 (0.433)	0.740 (0.438)	0.751 (0.432)	0.747 (0.434)
Late school entry (1=Yes)	0.277 (0.447)	0.270 (0.444)	0.271 (0.444)	0.289 (0.453)	0.275 (0.446)
Any crime	0.050 (0.218)	0.018 (0.133)	0.036 (0.187)	0.049 (0.217)	0.101 (0.302)
Felony	0.015 (0.121)	0.003 (0.054)	0.004 (0.059)	0.018 (0.132)	0.036 (0.186)
Rural	0.448 (0.497)	0.495 (0.499)	0.395 (0.488)	0.500 (0.500)	0.391 (0.488)
Free lunch	0.518 (0.499)	0.297 (0.457)	0.768 (0.421)	0.297 (0.457)	0.766 (0.423)
Female	0.489 (0.499)
White	0.519 (0.499)
Black	0.462 (0.498)
Hispanic	0.007 (0.087)
Sample size	132,930	33,274	30,658	35,759	30,854

Notes: The statistics above reflect our analysis sample, which consists of children born between 1992 and 1995 and those who had enrolled public kindergarten in Louisiana and who had stayed in the state from kindergarten through high school (public or private). The sample excludes parishes that are known to be most affected from Hurricanes Katrina and Rita. See text for further details. The sum of observations from columns 2–5 do not add up to column 1 because of the small proportion of *other race/ethnicity* students.

We impose several restrictions on our research sample. First, we focus on children born between 1992 and 1995. Because administrative records date back to 1997, the birth cohort of 1992 is the first year in which we can observe the actual public kindergarten enrollment.⁹ Similarly, we choose the birth cohort of 1995 as our youngest cohort in the research sample since this would be the last cohort in which we can fully observe juvenile incidents over the juvenile age range. Second, we restrict our attention to children who had enrolled in public kindergarten in Louisiana and who had stayed in the state through high school (public and private).¹⁰ Therefore, individuals are dropped from our sample if they only moved out of state. This type of restriction may lead to a selected sample and for that matter may bias the discontinuity estimates if attrition itself is correlated with birth dates near the school entry cutoff (see, for example, McCrary and Royer 2011). To address this, we provide: (i) a detailed discussion on potential sample selection biases in Section 4.1, and (ii) sharp bounds estimates (Lee 2009) in a RDD framework in Section 4.2.4. Third, in order to circumvent any potential confounding effects that may arise due to Hurricanes Katrina and Rita, we exclude parishes that are known to be most affected from the hurricanes in 2005 (see, for example, Sacerdote 2012).¹¹ Having imposed these restrictions, we end up with a total sample of 132,930 unique observations.

Table 1 presents the descriptive statistics for the full sample and for several subpopulations of interest. Not surprisingly, we find

that roughly 75% of all the children were born prior to September 30 school entry cutoff. In our data, the proportion of late school entry is nearly 28% and among subgroups of interest, white males have the highest rate of late school entry (29%), which is consistent with the existing studies (see, for example, Dobkin and Ferreira 2010). Turning to juvenile crime statistics, the data suggest that black males have the highest rates of juvenile crime (10.1%), followed by white males (4.9%), black females (3.7%) and lastly white females (1.8%). Non felony crimes occur at a much higher rate than felony crimes. For males, the ratio of nonfelony offenses to felony offenses is approximately 2:1, and for females, the ratio is approximately 7:1. We also find that black students are more than twice as likely to receive free/reduced lunch (77%). Consistent with the state's demographics, the student body largely contains black and white students and they make more than 98% of the sample. Fig. A1 in Appendix A displays the age distribution of individuals when they were admitted to the juvenile system. As is visible from the figure, age ranges from 8 to 17, with most offenses occurring between ages 15 to 17.

Prior to continuing, an important caveat regarding the interpretation of the results is warranted: absent of any grade retention and/or skipping a grade, the relationship between school entry age (SA), chronological age (A) and years in school (YS) form a mathematical identity ($A=SA+YS$) for children enrolled in compulsory schooling. Controlling for anyone of these does not allow one to identify the effects of the other two separately. As such, a same grade comparison not only captures the differences in school entry age but also differences in chronological age. Similarly, a same age comparison captures the differences in school entry age and differences in years of schooling. Therefore, we do not make “same age” or “same grade” comparisons in the regression analyses. Instead, we concentrate on the incidence of being arrested for a juvenile crime anytime through the age of 17. Even so, we cannot completely purge out years of schooling effects from our entry age effects since students with late entry are likely to accumulate less years of schooling by the end of the window over which juve-

on offense types is missing for around 10% of the total incidents. These juvenile crimes are left as unclassified.

⁹ The recent statistics show that more than 95% of the student population in Louisiana enroll in public kindergarten.

¹⁰ Charter schools are included as public schools in the analysis. Approximately, 1.6% of students in our effective sample attended a charter school in eighth grade.

¹¹ These parishes are located in southeast Louisiana and include Jefferson, Lafourche, Orleans, Plaquemines, St. Bernard, St. Tammany and Terrebonne. During the school years from 2005 to 2007, these parishes experienced large outflows to other states (i.e., Texas). It is conceivable to argue that grade level (and therefore school entry cutoff) is correlated with geographical mobility.

Table 2
Regression discontinuity validation tests.

Dependent variable (1 = yes; 0 = no):	Coefficients (Standard error)				
	Stayer	Free-Lunch	Urban	Late entry (First stage)	
	(1)	(2)	(3)	(4)	(5)
Panel A: White females					
School entry cutoff	0.075*** (0.024) [6874]	−0.020 (0.027) [5895]	0.010 (0.036) [5895]	0.839*** (0.022) [5895]	0.840*** (0.021) [5895]
Panel B: Black females					
School entry cutoff	0.022 (0.022) [6331]	0.124 (0.039) [5615]	−0.023 (0.034) [5615]	0.910*** (0.017) [5615]	0.911*** (0.017) [5615]
Panel C: White males					
School entry cutoff	0.021 (0.028) [7363]	−0.002 (0.035) [6352]	0.012 (0.037) [6352]	0.782*** (0.023) [6352]	0.781*** (0.023) [6352]
Panel D: Black males					
School entry cutoff	0.024 (0.027) [6349]	0.014 (0.033) [5558]	0.047 (0.045) [5558]	0.854*** (0.023) [5558]	0.853*** (0.023) [5558]
Controls:				No	Yes

Notes: Standard errors clustered at the date of birth level are reported. All specifications include separate quadratic trends in the number of days from child's birthday to the state's official entry date on each side of the discontinuity. The bandwidth size is equal to 30 days. Dependent variables, noted above the specification numbers in each column, are indicators that take the value of 1 if yes, and 0 if no. Stayer is an indicator if the child had stayed in the state from kindergarten through high school (public or private). Covariates for the specification reported in column 5 include birth year controls and indicators for free/reduced lunch eligibility and rural/urban status of the kindergarten. Sample sizes are reported in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%.

nile crime is measured (i.e., early entrants finish high school by the age of 17, while late entrants are still at twelfth grade). Under the assumption of a negative association between years of schooling and crime (see, for example, Lochner 2004; and Lochner and Moretti 2004), negative (positive) coefficient estimates on *Late* and *Cut* from Eqs. (3) and (4) indicate understatement (overstatement) of the true school entry effects.

4. Results

There are three estimation details to mention before we present any results. First, in the main RDD estimations, we specify a quadratic spline as the functional form between the outcome variable and the number of days an individual's birthday is from the entry threshold. Recent work by Gelman and Imbens (2014) suggest that RDD estimates should be based on quadratic polynomials or local linear. In later sections we show additional results using local linear regressions and varying orders of polynomials.¹² Second, an important element of the RDD framework is the choice of bandwidth. In our main analysis we employ a bandwidth of 30 days around the cutoff. We obtained this measure by applying the procedure described in Calonico et al. (2014), which suggested that the optimal bandwidth is roughly 30 days for all subpopulations.¹³ With that being said, we also show results with different bandwidth values. Finally, all reported standard errors are clustered by date of birth.

¹² We found the results to robust to alternative modeling specifications. For example, the marginal effects of the main results are nearly identical if we run a probit rather than a linear probability model.

¹³ The optimal bandwidth value is 26 days for white females, 32 days for black females, 34 days for white males, and 37 days for black males.

4.1. Empirical tests for threats to identification

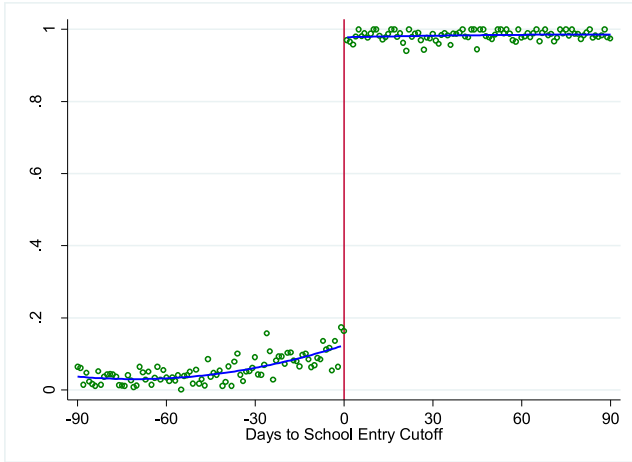
As noted, our effective sample consists of children who had enrolled public kindergarten in Louisiana and who had stayed in the state through high school. By imposing this sample restriction, we implicitly assume that the state's school entry policy is independent of the probability of an individual leaving out of state for any reason. This assumption suggests that children born on each side of the school entry date form an equivalently selected sample which may not be true in practice. Specifically, if parents are more likely to relocate children, say, born just before the cutoff, then the applied sample restriction is problematic. Ignoring this potential endogenous correlation may yield biased estimates in the RDD framework. To check this potential threat, we define an indicator variable that takes on the value one if we observe the child in the data through high school. We then examine the relationship between school entry laws and this attrition outcome.

The first column of Table 2 presents the RDD estimates from the following reduced form regression equation:

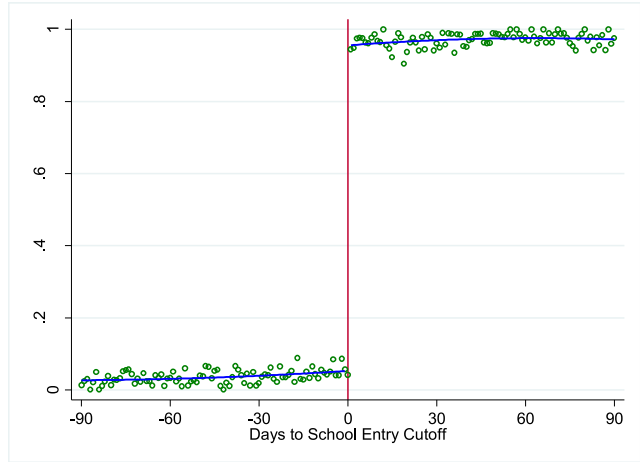
$$Stayer_i = \alpha_0 + \alpha_1 Cut_i + z(BD_i) + \phi_i. \quad (5)$$

$Stayer_i$ is an indicator variable for individual i , as described above. $z(\cdot)$ is a second degree polynomial that varies on both sides of the threshold and ϕ_i is an unobserved term. The coefficient estimates on school entry policies, presented in column (1), are small in magnitude and they are highly imprecisely estimated for black females, white males and black males. Therefore, it is likely that there is equivalent sample selection from the right and the left of the discontinuity for these three groups. However, for white females, we find a statistically significant coefficient estimate on the age cutoff. This result suggests that there is a discontinuous change in the likelihood of attrition around the entry policy for white female students. Given this attrition problem, we present the results for white females in the tables, but do not discuss them in the

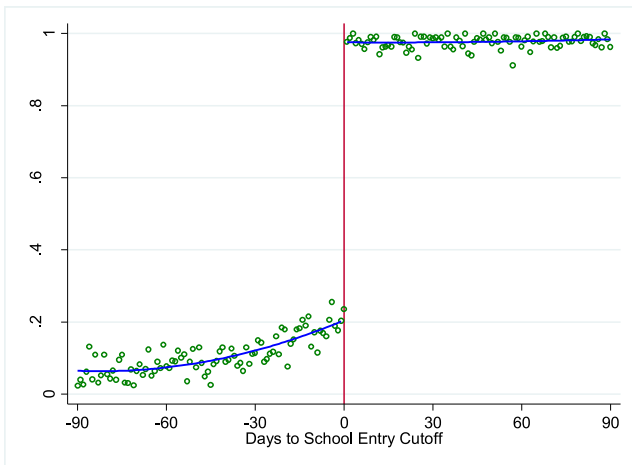
Panel A: White Females



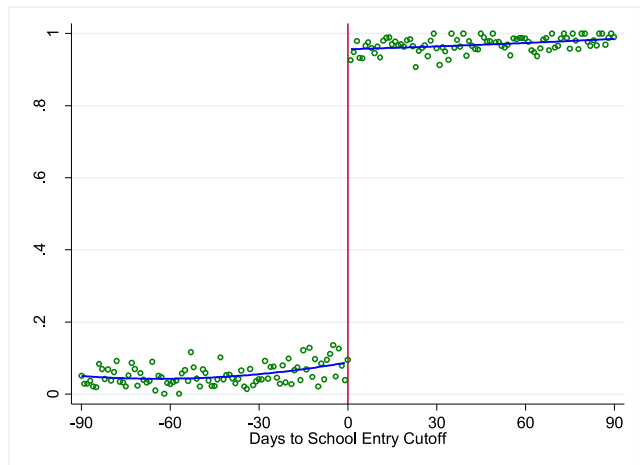
Panel B: Black Females



Panel C: White Males



Panel D: Black Males

**Fig. 1.** Probability of Delayed School Entry.

Notes: The horizontal axis presents the number of days to the school entry cutoff. The vertical lines denote the school entry cutoff of September 30 (normalized to zero). Each circle represents the fraction of children with late school entry, based on the number of days from birthday to the state's official entry date. The solid lines are fitted values of late school entry from a quadratic spline over a window of 90 days.

text.¹⁴ We examined the sensitivity of the attrition results to different orders of polynomials (i.e., local polynomial) and to varying bandwidth sizes. Our inference on attrition remains intact and these additional estimates are available upon request. As discussed further below, we also present sharp bound estimates of school entry in the RDD framework that account for endogenous selection through student attrition.

One other concern regarding the validity of a RDD is the manipulation of the running variable—the birth date of the child. It is not very likely for parents to strategically plan the exact date of their child's birth and there is substantial evidence supporting this argument (see, for example, Black et al. 2011; McCrary and Royer 2011; and Fredriksson and Öckert 2014). That being said, in the

absence of any manipulation, we would expect observable characteristics that are not affected by school entry laws to be similar for children born just before and just after the cutoff. Following the framework described in Eq. (5), we report the effect of the entry cutoff on free/reduced lunch and attending an urban kindergarten in the second and third columns of Table 2, respectively. The coefficients from this exercise are all imprecisely estimated and close to zero in magnitude. This finding along with other sensitivity checks (discussed below) offers some assurance on the validity of the RDD. Finally, it is important to note that we are able to circumvent concerns on season of birth effects by our focus on children born within a month of the September 30 cutoff (Bound et al. 1995).

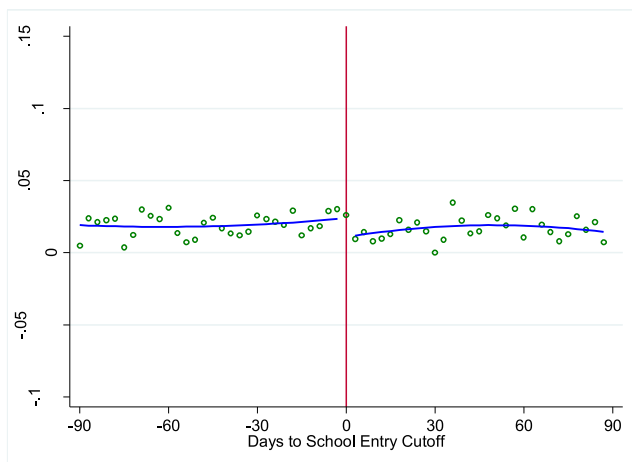
4.2. School starting age and crime

4.2.1. Graphical results

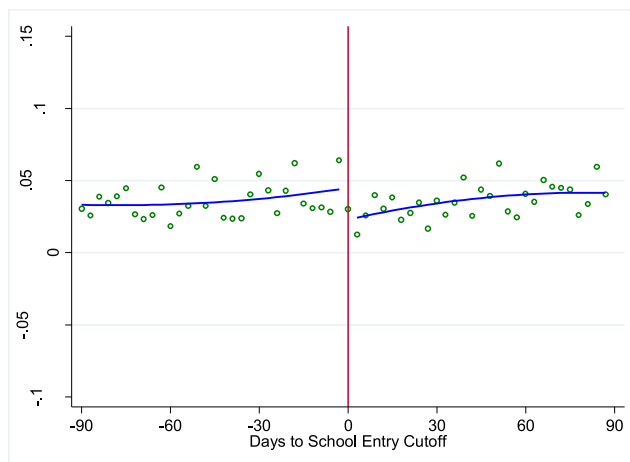
We begin with a graphical representation of our results. In all graphs, the running variable (BD_i) has been normalized so that September 30 is time zero. To have a clear visual inspection, we plot the unconditional means over a window of 90 days on each side of the threshold. Fitted values from a quadratic spline are superimposed over these averages. Fig. 1 displays the fraction of children who comply with the school entry policies. As is visible from the figures, compliance is very high among black females and black

¹⁴ Although we do not have a definitive explanation for endogenous sample selection observed for white females, the sample period coincides with many court-order desegregation plans for several parishes across the state (i.e., East Baton Rouge, West Carroll and Tangipahoa). It is widely acknowledged that desegregation efforts in Louisiana over this period had led to non-negligible white flight. The grade level (and therefore school entry cutoff) may potentially be correlated with the decision of internal migration from one state to another. Under the assumption that wealthier (white) parents of daughters are more sensitive to changes in student body experienced at the classroom or school, desegregation efforts experienced in early and mid-2000's may explain the discontinuous attrition observed for white females.

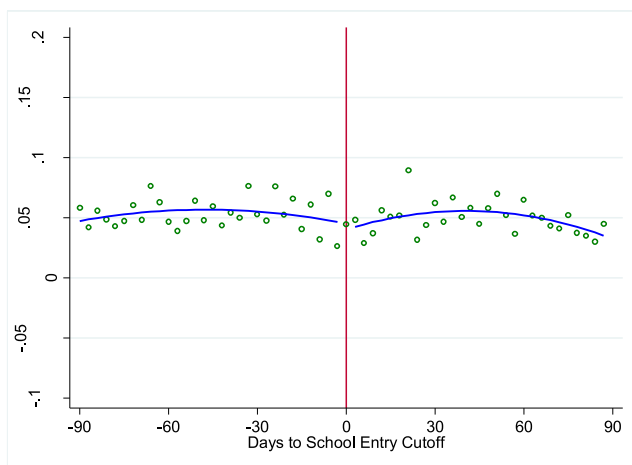
Panel A: White Females



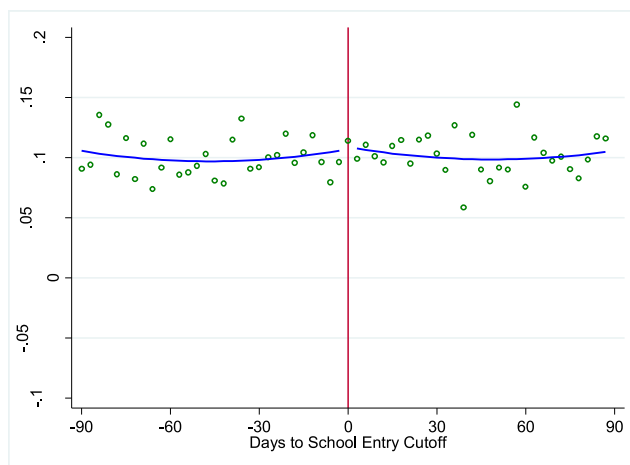
Panel B: Black Females



Panel C: White Males



Panel D: Black Males

**Fig. 2.** Probability of Any Juvenile Crime and Days to School Entry.

Notes: The horizontal axis presents the number of days to the school entry cutoff. The vertical lines denote the school entry cutoff of September 30 (normalized to zero). Each circle represents the unconditional means of juvenile crime in three-day bins, based on the number of days from birthday to the state's official entry date. The solid lines are fitted values of juvenile crime entry from a quadratic spline over a window of 90 days.

males born on each side of the entry law. (Panel B and D, Fig. 1). However, for white males, the data show that a considerable number of children who were born before the school entry cutoff delay their enrollment to kindergarten until the year after they are eligible. Specifically, for white males, the probability of noncompliance steadily increases and reaches a values of 0.20 at the entry threshold. White males born after the entry cutoff, on the other hand, have a compliance rate near 100% (Panel C).

Fig. 2 displays the reduced form models of school entry policy on the probability of committing juvenile crime. In order to reduce the noise in the data, unconditional means of committing crime are presented in three-day bins. The jumps at the cutoffs correspond to reduced form estimates for a bandwidth of 90 days. Looking at Panel B of Fig. 2, we observe a sharp jump at the cutoff (around 2 percentage points) indicating that black females born just after the school entry cutoff are significantly less likely to commit juvenile crime. This sharp jump at the threshold, however, does not extend over a large number of birth days. Specifically, the probability of juvenile crime is very similar for individuals that are more than roughly 60 days from the threshold. This suggests that the potential comparative advantage of being relatively older in school diminishes as children move away from the cutoff.

Panels C and D of Fig. 2 display the reduced form models for white and black males, respectively and the panels do not indicate

any compelling evidence on juvenile crime for white and black males.

4.2.2. Regression results

We now turn to the discussion of regression results. Columns 4–5 of Table 2 present the discontinuity estimates of school entry laws on late entry, i.e. the first stage effects. The coefficient estimates are based on two different specifications. Column 4 presents the RDD estimates in the absence of any controls and Column 5 presents the RDD estimates with birth year controls and additional covariate (free/reduced lunch eligibility and rural/urban status of the kindergarten). For each of the subpopulations of interest, the first stage estimates are highly significant, very large in magnitude, and robust across the specifications.

Table 3 presents our main results. For comparison purposes, we report the OLS estimates of late school entry on juvenile crime in the first column of Table 3. Columns 2–3 report the reduced form RDD results and columns 4–5 report the fuzzy RDD results from 2SLS estimation. Similar to the first stage regressions, we estimate two different specifications. Focusing first on black females, it appears that children born right after the school entry cutoff are around 3 percentage points less likely to commit a juvenile crime (column 2, Panel B, Table 3). Assuming that a child's date of birth that is near the school entry cutoff is as good as random, the

Table 3
Regression discontinuity estimates of school entry on juvenile crime dependent variable is an indicator for juvenile crime (1 = yes; 0 = no).

	Coefficients (Standard error)				
	OLS Estimates	Reduced Form RDD Estimates		Fuzzy RDD Estimates	
	(1)	(2)	(3)	(4)	(5)
Panel A: White females					
School entry cutoff		–0.013 (0.010) [5895]	–0.013 (0.010) [5895]
Late school entry	–0.001 (0.001) [33,274]	–0.015 (0.012) [5895]	–0.015 (0.011) [5895]
Panel B: Black females					
School entry cutoff		–0.032** (0.014) [5615]	–0.032** (0.015) [5615]
Late school entry	0.001 (0.002) [30,658]	–0.035** (0.016) [5615]	–0.035** (0.016) [5615]
Panel C: White males					
School entry cutoff		–0.000 (0.015) [6352]	–0.000 (0.015) [6352]
Late school entry	–0.004 (0.002) [35,759]	–0.000 (0.020) [6352]	–0.000 (0.020) [6352]
Panel D: Black males					
School entry cutoff		0.014 (0.021) [5558]	0.012 (0.022) [5558]
Late school entry	0.001 (0.003) [30,854]	0.016 (0.025) [5558]	0.014 (0.025) [5558]
Controls:	Yes	No	Yes	No	Yes

Notes: Standard errors clustered at the date of birth level are reported. All specifications include separate quadratic trends in the number of days from child's birthday to the state's official entry date on each side of the discontinuity. The bandwidth size is equal to 30 days. The dependent variable is an indicator that takes the value of 1 if the juvenile commits any juvenile crime. Covariates for the specifications reported in columns 1, 3 and 5 include birth year controls and indicators for free/reduced lunch eligibility and rural/urban status of the kindergarten. Naive OLS results reported in the first column are obtained using all subgroup-specific observations available in the effective sample. Sample sizes are reported in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%.

RDD estimates should be insensitive to the inclusion of other controls. Otherwise, the validity of the natural experiment generated by the discontinuity is likely compromised. As Column 3 indicates in Panel B of Table 3, adding control variables do not alter the discontinuity coefficient estimate. Considering that the average crime rate among black females is 3.6%, the estimated effect is large. It is important to note that children that are not close enough to school cutoff may experience little to no causal effect of school starting age. The effect just around the cutoff may be different than the average effect for the entire population. Relatedly, a 3 percentage point decrease does not imply that school starting age discontinuity explains a large fraction of crime among black females. Instead, individuals immediately to the right of the cutoff are 3 percentage points less likely to commit crime than individuals immediately to the left of the cutoff. Our point estimates for black females are similar in magnitude to that of Cook and Kang (2016) who find a 2.8 percentage point effect for children between the ages of 13 and 15. Similarly, Landersø et al. (2013) suggests that females with late entry are 1.5 percentage points less likely to receive criminal charges by their 18th birthday.

Turning to males, we find the point estimates for white males to be virtually equal to zero in magnitude (Columns 2–3, Panel C, Table 3). As for black males, the estimated effects of school entry

laws are positive but imprecisely estimated (Columns 2–3, Panel D, Table 3).

Not surprisingly, given the high compliance rate, the 2SLS estimates are very similar in magnitude to the reduced form effects. Late kindergarten entry or more precisely being a year older when entering kindergarten decreases the propensity to commit juvenile crime by 3.5 percentage points among black females who comply with the school entry laws (Columns 4–5, Panel B, Table 3). Turning to males, we continue to find no statistically significant impact of starting school one year later on juvenile crime, irrespective of race (Columns 4–5, Panels C and D, Table 3).

4.2.3. Heterogeneous effects-severity of crime and geographic aspects of school

In this section, we attempt to extend our analysis to see whether there are any differential effects of school starting age on juvenile crime. We explore the heterogeneity along the dimensions of severity of the offense committed as well as on different geographic aspects of the school the student was initially enrolled in.¹⁵

¹⁵ The results are similar if we use information from the school the student was enrolled in at the time of eighth grade instead of the school the student was enrolled at the time of kindergarten.

Table 4
Regression discontinuity estimates of school entry on types of juvenile offenses.

Dependent variable (1 = yes; 0 = no):	Coefficients (Standard error)			
	Felony Offense (1)	Non-Felony Offense (2)	Felony Offense (3)	Non-Felony Offense (4)
Panel A: White females				
School entry cutoff	0.000 (0.002) [5895]	−0.013 (0.009) [5895]
Late school entry	0.000 (0.003) [5895]	−0.015 (0.011) [5895]
Panel B: Black females				
School entry cutoff	−0.004 (0.005) [5615]	−0.028** (0.013) [5615]
Late school entry	−0.005 (0.006) [5615]	−0.030** (0.015) [5615]
Panel C: White males				
School entry cutoff	−0.000 (0.011) [6352]	0.000 (0.012) [6352]
Late school entry	−0.000 (0.014) [6352]	0.000 (0.015) [6352]
Panel D: Black males				
School entry cutoff	0.010 (0.012) [5558]	0.001 (0.019) [5558]
Late school entry	0.012 (0.015) [5558]	0.001 (0.022) [5558]
Controls:	Yes	Yes	Yes	Yes

Notes: Standard errors clustered at the date of birth level are reported. All specifications include separate quadratic trends in the number of days from child's birthday to the state's official entry date on each side of the discontinuity. The bandwidth size is equal to 30 days. The dependent variable in columns 1 and 3 is an indicator for felony offense. The dependent variable in columns 2 and 4 is an indicator for non-felony offense. Covariates include birth year controls and indicators for free/reduced lunch eligibility and rural/urban status of the kindergarten. Offense classifications (felony and non-felony) are based on the Louisiana Office of Juvenile Justice categorization. Sample sizes are reported in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4 reports the effect of school entry laws on the severity of the offense by analyzing whether there is a discontinuous jump in felony and non-felony offenses at the September 30th cutoff. The results, reported in Table 4, suggest that among black females, the effect of school start age on crime is driven by non-felony offenses, and not by felony offenses. Specifically, black females born just after the school entry cutoff are 2.8 percentage less likely to be delinquent of a non-felony offense (Column 2, Panel B) than black females born just before the school entry cutoff. However, there is no distinguishable effect on felony offenses (Column 1, Panel B). Furthermore, we find no relationship between the school entry cutoff and felony or non-felony offenses for males.^{16,17}

It is also widely recognized that crime is more of a concern in densely developed areas (see, for example, Glaeser and Sacerdote, 1999; and Rosenthal and Ross 2010). To address this poten-

tial geographic heterogeneity, we examine the effects of school entry laws by focusing on rural/urban status of the school. Table A1 in Appendix A presents basic descriptive statistics of late school entry and juvenile crime for subpopulations which are further disaggregated by urban status of the school. As is visible from the table, black students are more likely to attend schools located in non-rural areas but we do not observe any discernible patterns for school entry or juvenile crime among subpopulations. Turning to Table 5, although the discontinuity estimates are larger in magnitude for black female students in rural areas, they are only marginally significant for those enrolled in non-rural areas (Columns 1–4).

Finally, we look at the effects based on high versus low crime areas. We use juvenile crime information over the sample period at the district level to calculate the geographical density of crime. As such, an area is assumed to be high crime area if the district level juvenile crime rate for given cohorts is more than the average in the data. We continue to observe no impact of school entry cutoff for males, irrespective of the crime density of the district. For black females, we find that the estimated effects of school entry cutoff are considerably larger in high crime areas (Columns 5–6, Table 5).

4.2.4. Robustness checks

We undertake several sensitivity checks to examine the validity of our discontinuity estimates. First, rather than using a

¹⁶ We also further examined the effect of school starting age on many of the most common observed offenses. We found that the estimated effects for black females are more pronounced for offenses including disturbing the peace and simple battery; both of which are considered to be non-felony offenses.

¹⁷ When we categorized the data by types of crime: i) crimes against persons, ii) crimes against property, and iii) crimes against society, the lack of precision did not allow us to make firm conclusions. That being said, the estimated effects of school entry cutoff are more pronounced for crimes against persons for black females. This finding is consonant with Landersø et al. (2013) who find the mediating effects of school starting age for girls to operate through reductions in violent crimes.

Table 5

Regression discontinuity estimates of school entry on juvenile crime using different aspects of community type dependent variable is an indicator for juvenile crime (1 = yes; 0 = no).

Subsample:	Coefficients (Standard error)							
	Urban/Suburban (1)	Rural (2)	Urban/Suburban (3)	Rural (4)	High crime (5)	Low crime (6)	High crime (7)	Low crime (8)
Panel A: White females								
School entry cutoff	-0.015 (0.011) [3993]	-0.010 (0.020) [1902]	-0.011 (0.015) [2775]	-0.014 (0.011) [3120]
Late school entry	-0.018 (0.013) [3993]	-0.012 (0.024) [1902]	-0.014 (0.018) [2775]	-0.016 (0.014) [3120]
Panel B: Black females								
School entry cutoff	-0.027* (0.016) [4634]	-0.050 (0.041) [981]	-0.071*** (0.025) [2784]	0.004 (0.015) [2831]
Late school entry	-0.029* (0.017) [4634]	-0.058 (0.047) [981]	-0.077*** (0.026) [2784]	0.004 (0.017) [2831]
Panel C: White males								
School entry cutoff	0.017 (0.017) [4288]	-0.037 (0.025) [2064]	-0.012 (0.023) [2981]	0.009 (0.020) [3371]
Late school entry	0.022 (0.022) [4288]	-0.047 (0.032) [2064]	-0.015 (0.029) [2981]	0.012 (0.027) [3371]
Panel D: Black males								
School entry cutoff	0.026 (0.026) [4553]	-0.052 (0.059) [1005]	0.013 (0.037) [2740]	0.011 (0.025) [2818]
Late school entry	0.030 (0.030) [4553]	-0.062 (0.069) [1005]	0.015 (0.043) [2740]	0.012 (0.029) [2818]
Controls:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors clustered at the date of birth level are reported. All specifications include separate quadratic trends in the number of days from child's birthday to the state's official entry date on each side of the discontinuity. The bandwidth size is equal to 30 days. The dependent variable is an indicator for any juvenile crime. Covariates include birth year controls and an indicator for free/reduced lunch eligibility. An area is assumed to be high crime area if the district level crime rate over the sample period for given cohorts is more than the average in the data. Sample sizes are reported in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%.

quadratic spline, we employ a local linear regression, which is known to be more robust to trends away from the cutoff (Lee and Lemieux, 2010). The RDD estimates from the local linear regression are presented in columns 1 and 4 of Table 6. Second, keeping the well-known trade-off between the order of the polynomial and the bandwidth size in mind (Lee and Lemieux, 2010), we present in Table 6 linear spline estimates with a bandwidth size of 15 days, in columns 2 and 5, and cubic spline estimates with a bandwidth size of 60 days, in columns 3 and 6. The results from these alternative specifications are very similar to our main estimates.

Third, we pool the data (23,420 observations) and run a model where we interact school entry cutoff and quadratic trends of the running variable with gender and race dummies. The coefficient estimates for subgroups from this exercise are very similar to those presented in Table 3. Fourth, we run a series of placebo tests. Specifically, we assign a false school entry date and estimate the reduced form equation as if this false date was the actual school entry cutoff. In order to preserve the specifications used in the main analysis which includes birth year fixed effects and a bandwidth size of 30 days, we run placebo tests from the beginning of February until the end of November. In total, we run around 300 placebo regressions for each subpopulation of interest. Each panel of Fig. 3 plots the distribution of placebo estimates along with the true discontinuity value for each subpopulation. As is visible from Panel B, the actual coefficient estimate of school entry laws for black females lies at the far left tail of the placebo effects distribution. The location of the actual estimate relative to the distribution

of the placebo estimates indicates that the likelihood of finding an effect as large as we do merely due to randomness is very unlikely. Turning to Panels C and D of Fig. 3, the true estimates of school entry policies for white and black males are not unusually large relative to the corresponding placebo distributions. As an additional placebo test we restrict our analysis to Orleans parish where the school entry cutoff is set at December 31st, rather than September 30th. Using Orleans parish students, we test for a discontinuity at the September 30th cutoff and find no evidence of an increase or decrease in the likelihood of juvenile crime. This placebo analysis further suggests that our main results are likely not a systematic artifact caused by spurious factors around the September 30th cutoff.

Fifth, we added back the parishes that are known to be most affected from the Hurricanes Katrina and Rita into the effective sample and doing so does not alter our findings. Sixth, we run the specifications using all children who had enrolled in public kindergarten in Louisiana, including children who moved out of the state (non-stayers from Eq. 5). The results from this exercise are similar to those presented in the text. Finally, we examine the effect of school entry laws on age of conviction using various age cut-offs (e.g. less than 15 years old and 15 or more). The point estimates suggest that the crime reducing effects of school entry laws are more pronounced at later ages. This may also reinforce the importance of observing the complete delinquency history of the children. All these additional estimations are available upon request.

Table 6
Robustness checks- regression discontinuity estimates of school entry on juvenile crime dependent variable is an indicator for juvenile crime (1 =yes; 0=no).

	Coefficients (Standard error)					
	Local Linear 30 Days (1)	Linear Spline 15 Days (2)	Cubic Spline 60 Days (3)	Local Linear 30 Days (4)	Linear Spline 15 Days (5)	Cubic Spline 60 Days (6)
Panel A: White females						
School entry cutoff	-0.010 (0.008) [5895]	-0.012 (0.008) [3069]	-0.007 (0.009) [11,408]
Late school entry	-0.014 (0.011) [5895]	-0.014 (0.010) [3069]	-0.008 (0.010) [11,408]
Panel B: Black females						
School entry cutoff	-0.020* (0.011) [5615]	-0.033** (0.014) [2789]	-0.022 (0.014) [11,049]
Late school entry	-0.028* (0.015) [5615]	-0.036** (0.016) [2789]	-0.024 (0.016) [11,049]
Panel C: White males						
School entry cutoff	0.003 (0.011) [6352]	0.000 (0.013) [3302]	0.003 (0.014) [12,271]
Late school entry	0.004 (0.017) [6352]	0.000 (0.017) [3302]	0.004 (0.018) [12,271]
Panel D: Black males						
School entry cutoff	0.018 (0.018) [5558]	0.010 (0.019) [2809]	-0.000 (0.020) [10,891]
Late school entry	0.027 (0.026) [5558]	0.012 (0.022) [2809]	-0.001 (0.024) [10,891]
Controls:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors clustered at the date of birth level are reported. All specifications include separate trends in the number of days from child's birthday to the state's official entry date on each side of the discontinuity. The dependent variable is an indicator for any juvenile crime. Covariates include birth year controls and indicators for free/reduced lunch eligibility and rural/urban status of the kindergarten. Sample sizes are reported in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%.

4.2.5. Sharp bounds on school entry laws in a regression discontinuity framework

In this section we provide further evidence on the potential role of sample attrition on our school entry estimates. To do so, we extend the bounding approach developed in Lee (2009) to RDD framework. Specifically, Lee (2009) proposes a trimming procedure for bounding any treatment effect in the presence of sample selection bias. The bounding approach relies on the assumptions of randomness of treatment and monotonicity. The latter assumption in this setup is slightly different than the one for LATE. Here, the monotonicity assumption implies that treatment assignment can affect sample selection in one direction, i.e., school entry eligibility cannot induce some eligible students to leave the state while others to stay. The method amounts to first identifying the excess number of observations who are induced to be selected (number of stayers in our case) because of treatment (school entry cutoff) and then trimming the upper and lower tails of the outcome distribution by this number. The intuition behind the bounds estimator is to trim the sample of treated or the control observations such that the share of observations with observed outcome is equal for both groups (see, Lee 2009 for further details). In practice, the trimming procedure involves no covariates and requires a continuous outcome variable. The details of the bounds estimator are given in Appendix B.

To the best of our knowledge, this is the first study to apply sharp bounds in a RDD framework. In order to implement the

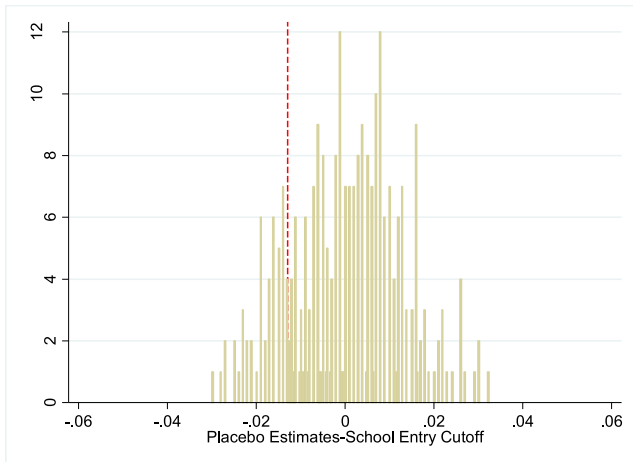
bounding approach properly in the current context, we first keep observations that are only in the very close neighborhood of the school entry cutoff (i.e., 3 days). This allows us to make a simple mean comparison absent of any polynomials. Next, within this close neighborhood, we use the residualized outcome values in which we obtain from a regression of juvenile crime on control variables. By virtue of RDD, this transformation does not lead to any loss of generality other than converting the discrete outcome variable to a continuous one.

Table 7 presents the bounds estimator results along with the trimming proportions for students born after the school entry cutoff (treatment group). To serve as a benchmark, we report the point estimates from a regression of residualized crime on school entry cutoff absent of any trimming in the first column of Table 7. The second and third columns give the lower and upper bound estimates, respectively, while the final column reports the Imbens and Manski (2004) confidence interval at the 90% level. Looking at the last column of Table 7, it is only for black females that one can rule out a zero effect of school entry laws on juvenile crime (Column 4, Table 7).

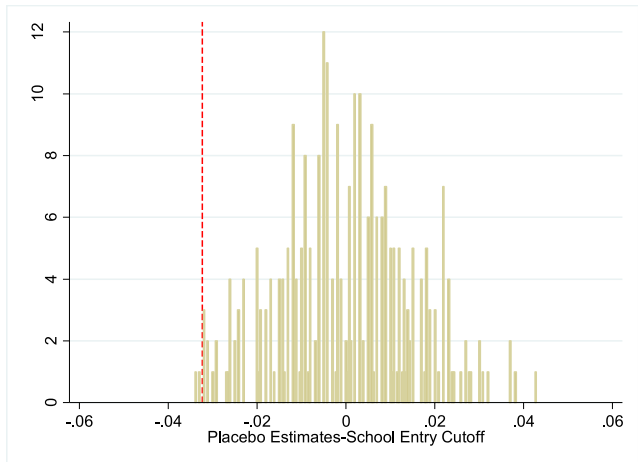
4.3. Discussion of the potential mechanisms

We observe crime reducing effects of late entry for female students only. This may be explained by a large literature documenting fundamental differences between females and males starting in

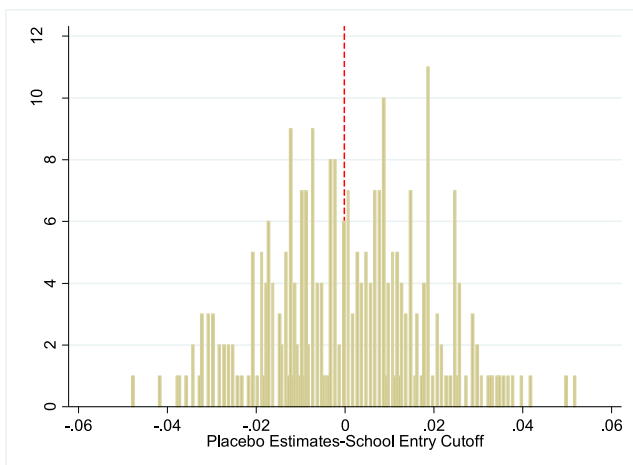
Panel A: White Females



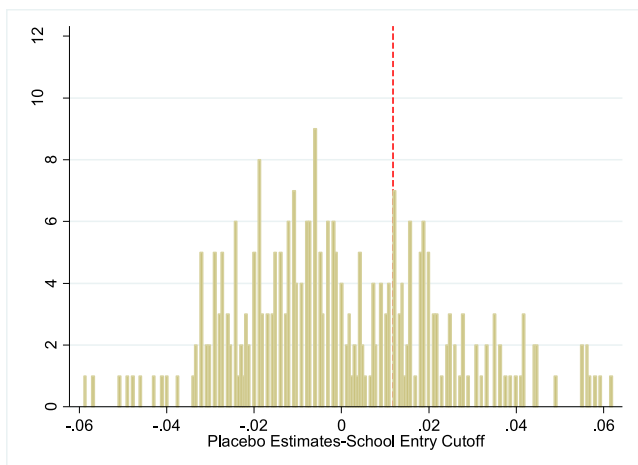
Panel B: Black Females



Panel A: White Males



Panel D: Black Males

**Fig. 3.** Placebo Estimates of School Entry on Juvenile Crime.

Notes: Each placebo estimate assigns a false school entry date (from the beginning of February until the end of November) and then uses a reduced form equation to estimate the effect of school entry laws on committing a juvenile crime. All estimates are obtained from a quadratic spline for a bandwidth of 30 days. The vertical dashed lines denote the actual estimates.

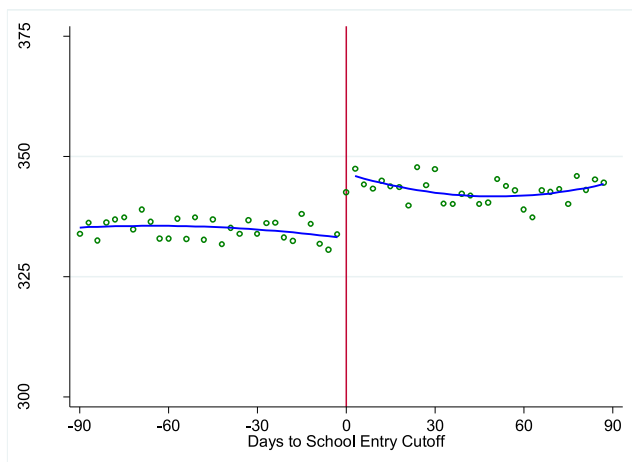
Table 7

Bounds on school entry laws for juvenile crime dependent variable is an indicator for juvenile crime (1=yes; 0=no).

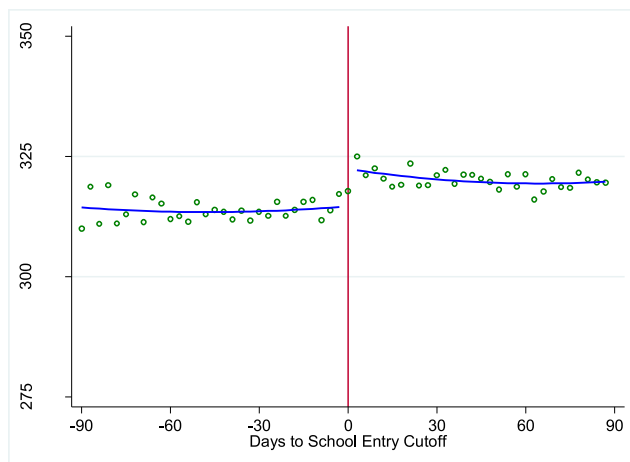
	Mean difference no trimming (Standard error) (1)	Lower bound (Standard error) (2)	Upper bound (Standard error) (3)	Imbens and Manski confidence interval (4)
Panel A: White females School entry cutoff ($p=0.07$)	-0.012 (0.011) [697]	-0.028 (0.008) [808]	-0.009 (0.011) [808]	[-0.039, 0.005]
Panel B: Black females School entry cutoff ($p=0.03$)	-0.038** (0.016) [642]	-0.054 (0.011) [719]	-0.030 (0.014) [719]	[-0.069, -0.011]
Panel C: White males School entry cutoff ($p=0.04$)	0.016 (0.014) [723]	-0.027 (0.031) [847]	0.021 (0.016) [847]	[-0.068, 0.042]
Panel D: Black males School entry cutoff ($p=0.03$)	0.013 (0.025) [664]	-0.013 (0.036) [763]	0.013 (0.025) [763]	[-0.063, 0.049]

Notes: The dependent variable is an indicator for any juvenile crime. Analytical standard errors from Lee (2009) are reported for the bounds. Mean differences and bounds are computed for children born within a three day neighborhood of the school cutoff. The outcome variable is the residualized juvenile crime, obtained from a regression of juvenile crime on control variables (birth year fixed effects and indicators for free/reduced lunch eligibility and rural/urban status of the kindergarten). p denotes the trimming proportion for the treatment group. Sample sizes for point and bound estimates are reported in square brackets. Imbens and Manski confidence interval is reported at the 90% level. * significant at 10%, ** significant at 5%, *** significant at 1%.

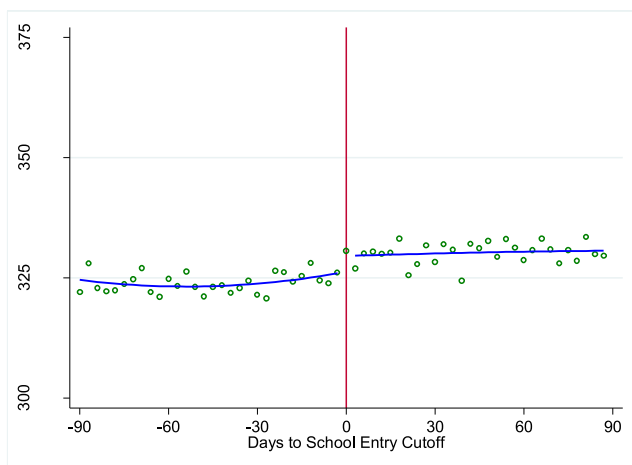
Panel A: White Females



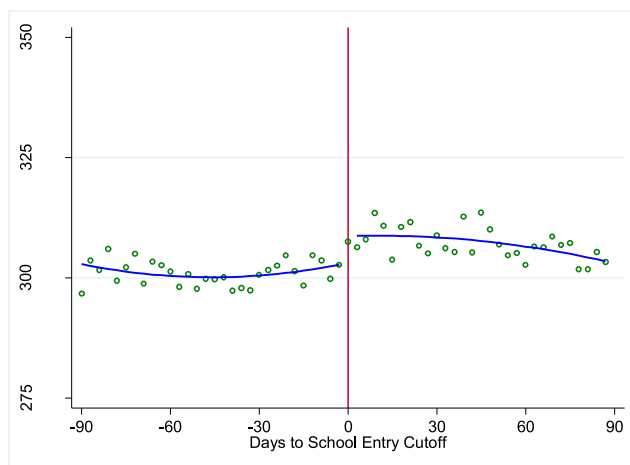
Panel B: Black Females



Panel C: White Males



Panel D: Black Males

**Fig. 4.** LEAP ELA Test Scores and Days to School Entry.

Notes: The horizontal axis presents the number of days to the school entry cutoff. The vertical lines denote the school entry cutoff of September 30 (normalized to zero). Each circle represents the unconditional means of ELA test scores in three-day bins, based on the number of days from birthday to the state's official entry date. The solid lines are fitted values of ELA test scores from a quadratic spline over a window of 90 days.

very early ages. It has been shown that females have a lower desire to seek challenges (Niederle and Yestrumskas 2008), they are more sensitive to social environment and they are also more averse to competition and risk (see, for example, Croson and Gneezy 2009 and Bertrand 2011 for survey reviews). Moreover, differential effects by gender are not unique to school entry policies. A growing body of research provides evidence for significant differences in gender-specific responses to policy interventions implemented during early childhood or adolescence (see, for example, Kling et al. 2007, Anderson 2008 and Rodríguez-Planas 2012).

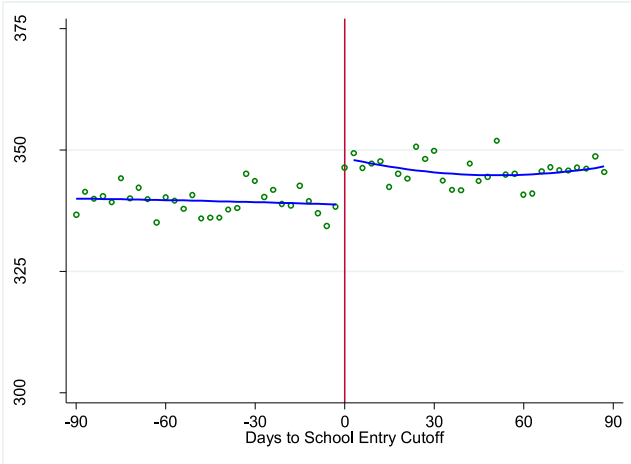
In this section, we explore the effects of school starting age on student achievement. Given that our study is the first to find that school starting age does not affect juvenile crime of males, it is expedient to further explore other outcomes that may provide evidence of the mechanisms at play. The failure to find an effect for males may simply be due to a type II error, or, that males are largely impervious to the effects of school starting age, at least within the realm of our study.

There are two competing explanations on the effects of late entry into kindergarten. Because children with late entry start school being one year older than the youngest children in their cohort, parents may have more time to invest in their child's preschool skill set. As such, late entry may generate different levels of human capital accumulation at the time a child enters kindergarten.

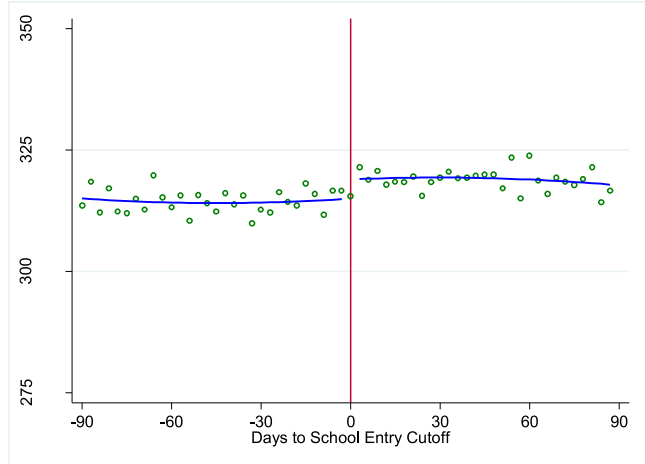
Elder and Lubotsky (2009) suggest that preschool endowment differences resulting from late entry provide a comparative advantage to older children without affecting their pace of learning. There are two important implications of this hypothesis: (i) endowment differences are driven by additional parental investments and therefore, one would expect the effects of school entry age to be more pronounced for wealthier families, and (ii) since late entry does not affect the rate or pace of learning, skill gap differences stemming from preschool investments tend to dissipate over time as they come to represent a smaller fraction of children's overall stock of knowledge. Elder and Lubotsky (2009) provide evidence in favor of the *differences in endowment* hypothesis in explaining school starting age effects on achievement. Specifically, the authors show that late school entry improves standardized reading and math test scores in the early years of schooling with the effects being more significantly pronounced for children from wealthier parents. Positive effects of school starting age, however, do not appear to be long lasting and these effects largely fade away as children reach eighth grade.

In contrast to this explanation is the hypothesis of age related *differences in human capital accumulation* (see, for example, Bedard and Dhuey 2012). It is conceivable that older children are more likely to have the required skills (i.e., brain development) and the maturity to succeed in school. School readiness affects the rate

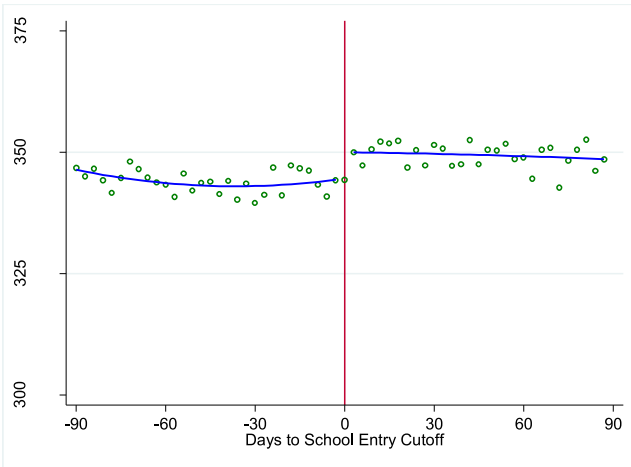
Panel A: White Females



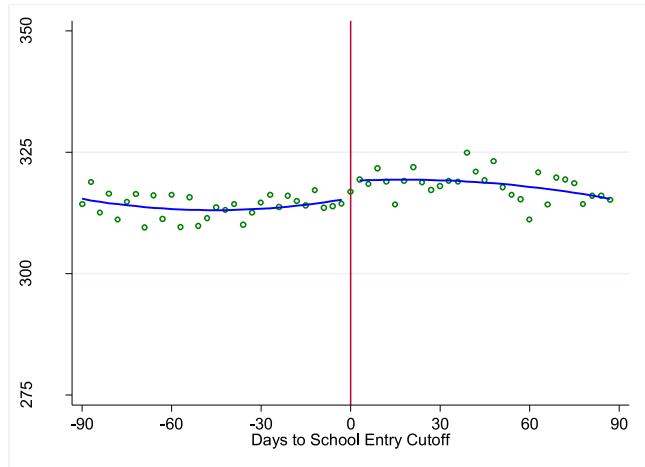
Panel B: Black Females



Panel C: White Males



Panel D: Black Males

**Fig. 5.** LEAP Math Test Scores and Days to School Entry.

Notes: The horizontal axis presents the number of days to the school entry cutoff. The vertical lines denote the school entry cutoff of September 30 (normalized to zero). Each circle represents the unconditional means of math test scores in three-day bins, based on the number of days from birthday to the state's official entry date. The solid lines are fitted values of math test scores from a quadratic spline over a window of 90 days.

of learning, and rate of learning affects human capital accumulation. Under this hypothesis, differences in capital accumulation persist over time, as changes in the rate of learning are persistent. As such, there are no prior predictions on the impact of school starting age for children from various parental backgrounds. School starting age related differences in human capital may reduce crime at later ages (i) through its incapacitation effects—high achieving students are more engaged and devote more time to school, thus devoting less time and attention to crime (see, for example, [Jacob and Lefgren \(2003\)](#), [Luallen \(2006\)](#), and [Landersø et al. \(2013\)](#)), and (ii) by the opportunity cost of crime resulting from the negative relationship that exists between education and crime ([Lochner and Moretti 2004](#)).

To evaluate these two competing potential explanations further and to see whether our results follow a similar pattern across gender as our results did for crime, we replace the juvenile crime outcomes with the eighth grade English and Math LEAP test scores and rerun the reduced form and the 2SLS equations. Unlike juvenile crime, test scores are measured at a given grade level and hence the impact of school entry age on test scores from these specifications not only captures the differences in kindergarten entrance ages but also the differences in their chronological ages at

the time of the test. Nonetheless, observed school starting age effects on achievement can still provide important insights. Since our examination of channels utilizes eighth grade test scores, we restrict our effective sample to include only public school students with non-missing test scores (test score information is available for public school students). [Table A2](#) in [Appendix A](#) presents the descriptive statistics (including LEAP scores).¹⁸ [Figs. 4](#) and [5](#) display the reduced form models of school entry policy on eighth grade ELA and Math test scores, respectively.

Columns 1 and 4 and columns 7 and 10 of [Table 8](#) presents the discontinuity estimates on reading and math test scores from the reduced form and the 2SLS regressions, respectively. [Table 8](#) also presents the discontinuity estimates by free lunch status (proxy for family wealth). Looking at the table, it is hard to see a consistent pattern in the age effects by free lunch status across the subpopulations of interest. Moreover, unlike [Elder and Lubotsky \(2009\)](#), we observe non-negligible age effects on achievement even at the eighth grade level for females. For example, late kindergarten entry increases ELA test scores of free lunch eligible black female stu-

¹⁸ The findings on crime from [Tables 2](#) and [3](#) carry over to our restricted sample. All these additional results are available upon request.

Table 8
Regression discontinuity estimates of school entry on ELA and math LEAP test scores.

Dependent variable:	Coefficients (Standard error)											
	LEAP ELA test score			LEAP math test score			LEAP ELA test score			LEAP math test score		
	Full sample (1)	Free lunch eligible (2)	Non eligible (3)	Full sample (4)	Free lunch eligible (5)	Non eligible (6)	Full sample (7)	Free lunch eligible (8)	Non eligible (9)	Full sample (10)	Free lunch eligible (11)	Non eligible (12)
Panel A: White females												
School entry cutoff	13.180*** (2.750) [4994]	14.220** (6.256) [1469]	12.748*** (2.858) [3525]	11.628*** (3.205) [4994]	11.578** (5.491) [1469]	11.497*** (3.546) [3525]
Late school entry	15.152*** (3.216) [4994]	15.286** (6.665) [1469]	15.080*** (3.410) [3525]	13.368*** (3.768) [4994]	12.446** (5.779) [1469]	13.600*** (4.338) [3525]
Panel B: Black females												
School entry cutoff	10.089*** (2.759) [4658]	9.539*** (3.387) [3590]	11.711* (6.275) [1068]	7.571*** (2.669) [4658]	6.046* (3.361) [3590]	12.802** (6.248) [1068]
Late school entry	10.850*** (2.922) [4658]	10.278*** (3.625) [3590]	12.494* (6.759) [1068]	8.142*** (2.844) [4658]	6.514* (3.614) [3590]	13.658** (6.716) [1068]
Panel C: White males												
School entry cutoff	4.382* (2.660) [5218]	−0.064 (7.199) [1464]	6.217** (2.592) [3754]	5.664 (3.827) [5218]	−0.254 (10.060) [1464]	8.168** (3.443) [3754]
Late school entry	5.533* (3.372) [5218]	−0.072 (8.050) [1464]	8.250** (3.413) [3754]	7.152 (4.825) [5218]	−0.286 (11.249) [1464]	10.838** (4.518) [3754]
Panel D: Black males												
School entry cutoff	4.857 (3.703) [4139]	5.729 (4.099) [3117]	1.341 (7.095) [1022]	7.364* (4.208) [4139]	9.391** (4.437) [3117]	1.264 (7.610) [1022]
Late school entry	5.550 (4.234) [4139]	6.239 (4.440) [3117]	1.828 (9.623) [1022]	8.414* (4.807) [4139]	10.226** (4.813) [3117]	1.724 (10.343) [1022]
Controls:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors clustered at the date of birth level are reported. All specifications include separate quadratic trends in the number of days from child's birthday to the state's official entry date on each side of the discontinuity. The bandwidth size is equal 30 days. Covariates include birth year controls and indicators for free/reduced lunch eligibility and rural/urban status of the kindergarten. For columns 1–3 and 7–9, the dependent variable is the LEAP ELA test score. For columns 4–6 and 10–12, the dependent variable is the LEAP Math test score. For the full sample, the mean ELA and Math test score is 322.4 and 331.5, respectively. See Table A2 for additional descriptive statistics. Sample sizes are reported in square brackets. * significant at 10%, ** significant at 5%, *** significant at 1%.

dents by roughly one-fourth of a standard deviation (Column 2, Panel B, Table 8).

Overall, we do not find strong compelling evidence in favor of the endowment gap hypothesis; rather, it appears that for all females, differences in human capital accumulation hypothesis is more consistent with our sample. Furthermore, these findings are consistent with the findings for juvenile crime as both sets of estimates show that females, rather than males, are affected.¹⁹

5. Conclusion

Although the effects of school entry age on student achievement continues to draw the interest of researchers, there has recently been increased enthusiasm to understand the consequences of school entry age on other economic outcomes. Two recent studies in North Carolina and Denmark have shown that school starting age affects a child's involvement in future crime. Plausibly due to institutional differences between the research settings, the findings in the two studies are not straightforwardly congruent, suggesting that additional research on the topic is warranted. Using administrative data from a number of state agencies in Louisiana, we find that late school entry age by one year appears to reduce the incidence of juvenile crime among black females and that these effects seem to be driven by reductions in non-felony offenses. We also find more pronounced effects for black females in high crime areas (school districts). Potential contamination due to endogenous sample selection does not allow us to make a firm conclusion for white females. For males, however, we do not find any effect of late school entry on juvenile crime. We propose age related differences in human capital accumulation as a potential explanation for our findings. In the light of the results from the two recent studies, our findings suggest that the effect of school entry laws on juvenile crime may depend significantly on the setting and institutions of where it occurs.

From a policy point of view, it is not clear whether earlier school entry law enactments would produce Pareto optimum outcomes. On one hand, it appears that extending the school entry cutoff dates reduce the incidents of juvenile crime, at least for some groups. However, there is also some empirical evidence that earlier school entry laws lower educational attainment through increases in rate of high school dropout (Cook and Kang 2016, Dobkin and Ferreira 2010). In the absence of a detailed cost-benefit analysis, it is challenging to make a firm conclusion.

Fixing state compulsory school laws to a grade rather than to years of age coupled with earlier school cutoff dates may be an optimal policy. With most research on school starting age focusing on the outcomes of school aged children or young adults, it would also be beneficial for future work to consider longer-run outcomes (Black et al., 2011) and whether the impact on crime persists into adulthood.

Appendix A

(Table A1, A2 and Fig. A1).

¹⁹ Another potential explanation for our findings pertains to peer effects. Note that, in addition to becoming absolutely older, children with delayed entry generally become relatively older at a given grade. To the extent that having older peers trigger engagement in risky behaviors, age composition of the peers may explain our findings. To examine the potential extent of this mechanism, we include average age of peers in student's school at the end of eighth grade as an additional control variable. Doing so, however, does not alter our results on juvenile crime.

Table A1
Selected summary statistics by community type.

	Mean (Standard deviation)	
	Urban/Suburban (1)	Rural (2)
Panel A: White females		
Late school entry (1=Yes)	0.269 (0.443)	0.273 (0.445)
Any crime	0.019 (0.136)	0.016 (0.127)
Sample size	22,688	10,586
Panel B: Black females		
Late school entry (1=Yes)	0.269 (0.443)	0.279 (0.449)
Any crime	0.037 (0.188)	0.034 (0.182)
Sample size	25,210	5448
Panel C: White males		
Late school entry (1=Yes)	0.291 (0.454)	0.286 (0.452)
Any crime	0.048 (0.214)	0.053 (0.224)
Sample size	24,323	11,436
Panel D: Black males		
Late school entry (1=Yes)	0.276 (0.447)	0.272 (0.445)
Any crime	0.102 (0.303)	0.098 (0.297)
Sample size	25,400	5454

Notes: The statistics above reflect children born between 1992 and 1995 and those who had enrolled public kindergarten in Louisiana and who had stayed in the state from kindergarten through high school (public or private). The sample excludes parishes that are known to be most affected from Hurricanes Katrina and Rita. See text for further details.

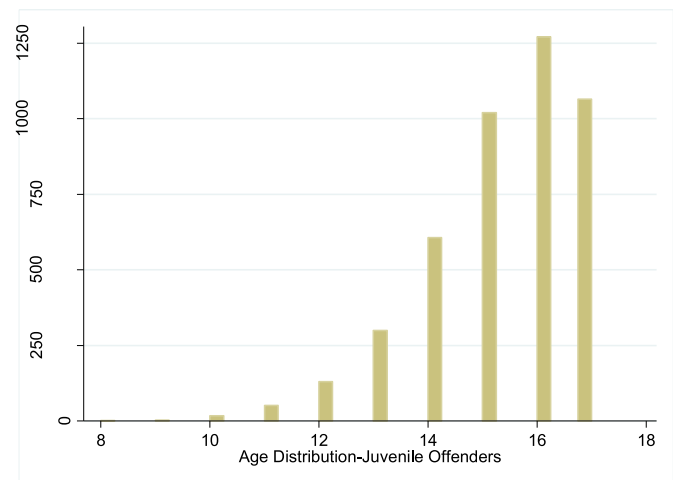


Fig. A1. Age Distribution of Juvenile Offenders (1997–2012).

Appendix B. Bounds Estimator

Let Y denote a continuous outcome variable, D be a treatment indicator such that $D \in \{0, 1\}$ and S_D denotes a selection indicator that takes the value of one if the outcome is non-missing and zero otherwise (i.e., attriter). Suppose that the following assumptions hold: (i) D is randomly assigned, and (ii) $Pr[S_1 \geq S_0] = 1$. Let

Table A2
Summary statistics-public school sample only.

	Mean (Standard deviation)				
	Full sample (1)	White females (2)	Black females (3)	White males (4)	Black males (5)
School entry eligibility (1=Yes)	0.746 (0.434)	0.748 (0.434)	0.738 (0.439)	0.749 (0.433)	0.750 (0.432)
Late school entry (1=Yes)	0.270 (0.444)	0.265 (0.441)	0.267 (0.442)	0.282 (0.450)	0.261 (0.439)
Any crime	0.040 (0.196)	0.016 (0.125)	0.031 (0.173)	0.042 (0.201)	0.078 (0.269)
Felony	0.011 (0.107)	0.003 (0.050)	0.003 (0.052)	0.016 (0.125)	0.027 (0.163)
Rural	0.456 (0.498)	0.499 (0.500)	0.404 (0.490)	0.503 (0.499)	0.403 (0.490)
Free lunch	0.505 (0.499)	0.292 (0.454)	0.769 (0.420)	0.280 (0.449)	0.756 (0.429)
LEAP ELA test score	322.427 (39.215)	338.643 (34.163)	316.443 (36.023)	326.348 (36.641)	303.379 (41.896)
LEAP math test score	331.451 (42.382)	342.257 (40.006)	316.272 (36.291)	345.761 (42.665)	315.625 (39.446)
Female	0.505 (0.499)
White	0.530 (0.499)
Black	0.451 (0.497)
Hispanic	0.007 (0.085)
Sample size	108,044	28,235	25,500	29,089	23,310

Notes: The statistics above reflect our analysis sample, which consists of children born between 1992 and 1995 and those who had enrolled public kindergarten in Louisiana and who had stayed in the public school system through high school. The sample excludes parishes that are known to be most affected from Hurricanes Katrina and Rita. See text for further details. The sum of observations from columns (2)–(5) does not add up to column (1) because of the small proportion of *other* race/ethnicity students.

a_T and a_C denote the shares of non-missing observations in the treatment and control groups, respectively and given by

$$a_T = \frac{\sum S \cdot D}{\sum D}$$

$$a_C = \frac{\sum S \cdot (1 - D)}{\sum (1 - D)}$$

Let's also assume that sample attrition is less likely for the treatment group ($a_T > a_C$).²⁰ Then

$$p = \frac{a_T - a_C}{a_T}$$

where p is the trimming proportion. p along with $(1 - p)$ determine the quantiles at which the treatment group's outcome distribution is trimmed to exclude extreme values from the analysis. Finally, the lower (Δ^{LB}) and upper (Δ^{UB}) bounds are calculated as follows

$$\Delta^{LB} = \frac{\sum Y \cdot S \cdot D \cdot 1[Y \leq y_{1-p}]}{\sum S \cdot D \cdot 1[Y \leq y_{1-p}]} - \frac{\sum Y \cdot S \cdot (1 - D)}{\sum S \cdot (1 - D)}$$

$$\Delta^{UB} = \frac{\sum Y \cdot S \cdot D \cdot 1[Y \geq y_{1-p}]}{\sum S \cdot D \cdot 1[Y \geq y_{1-p}]} - \frac{\sum Y \cdot S \cdot (1 - D)}{\sum S \cdot (1 - D)}$$

where $1[Y \leq y]$ is an indicator function and $y_q = \min\{y : \frac{\sum S \cdot D \cdot 1[Y \leq y]}{\sum S \cdot D} \geq q\}$.

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²⁰ All arguments hold symmetrically for the opposite case where $a_T < a_C$.

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