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Essays on Education Policy and Criminal Behavior

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ESSAYS ON EDUCATION POLICY AND CRIMINAL BEHAVIOR

A dissertation submitted in partial satisfaction of the requirements for the degree of

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by

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Abstract

Essays on Education Policy and Criminal Behavior

by

Asha Shepard

This dissertation studies the effect of education policies on the behavior of individuals—particularly the criminal behavior of juveniles.

The first chapter of this dissertation, "School Entry and Criminal Behavior", examines the effect of an education policy that affects when children are allowed to begin school on their likelihood of committing crime later in life. Children whose birthdays fall just before the school entry cutoff are the youngest in their cohort for their entire education. A large literature documents that, among other things, the youngest students in a classroom have lower test scores, are more likely to be held back, and are more likely to be diagnosed with ADHD. This paper investigates if being the youngest in a cohort has an impact on an individual's propensity to commit crime. I use records for over 4 million arrests spanning a 20-year period in California to assess if being the youngest in a school cohort increases the likelihood of being arrested at any point between the early teen and young adult years. Overall, I find no effect on the probability of arrest for serious crimes and no persistent effect for less serious crimes. However, the youngest students in a cohort have a higher risk of arrest for certain offenses at age 14, corresponding to the age at which they would transition to high school. This may reflect

the influence of school setting, peer group composition, and monitoring standards on behavior and the probability of arrest.

The second chapter of this dissertation, "School Quality, Student Performance, and Behavior", examines the effect of an education policy that places low-performing schools on probation on the school's academic progress and student misbehavior. School districts and state education boards across the U.S. have implemented school accountability systems in order to identify which schools may need to be improved. I use an accountability system implemented by Chicago Public Schools (CPS) which placed schools on probation if they failed to reach a certain performance level based on a combination of test scores, attendance, and student growth. Schools that are placed on probation and do not show improvement may face sanctions that include principal removal, staff turnaround, or school closure. I find that schools that receive low enough performance ratings to be placed on probation in one year show slightly more improvement in their performance in the following year relative to schools that just miss being placed on probation. That is, schools that received low performance ratings increased their rating in the next year by 3 more percentage points than schools that were not placed on probation. However, this difference was generally not large enough or sustained for long enough for many schools to get off of probation. Given the nature of these schools, this is potentially a resource issue as many of these schools are in poor, urban settings. From a behavior standpoint, I do not find evidence that receiving poor performance ratings has any effect on changes in student misconduct across the probation threshold.

The third chapter of this dissertation, "The Effect of School Year Length on

Juvenile Crime", examines the effect of a shortened school year on juvenile crime. As a result of the Great Recession during the late 2000s and early 2010s, many public sector employees were subject to mandatory work furloughs due to budgetary shortfalls. In order to alleviate the effects of the recession, public schools in California instituted work furloughs, which in effect decreased the number of days students would attend school. Specifically, schools were allowed to shorten their school year by five to twelve days. Given that a shorter school year implies that students will be in school less often, there may be changes in their behavior stemming from the incapacitation effect of attending school. Literature has shown that juveniles are less likely to commit property crime while in school due to a higher probability of detection and more likely to commit violent crime due higher concentrations of juveniles on school grounds. This paper examines the effect of a shortened school year on juvenile criminal behavior. I find that decreasing the amount of school days in the school year has no significant impact on juvenile arrests. This result may be explained by the overall decrease in juvenile crime over the past 20 years, as opposed to school year length changes having no effect on juvenile criminal behavior.

To my parents, brother, sisters,

and all the little ones.

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Chapter 1

School Entry and Criminal Behavior

1.1 Introduction

Every school year, children with birthdays just before the school entry cutoff date are able to enter kindergarten one year earlier than their similarly-aged peers born just after the cutoff. Those born before the cutoff will be the youngest students in their cohort if they enter school as soon as they are eligible, while those born after the cutoff will be the oldest members of their cohort when they begin school. However, parents of those born before the cutoff have the option to delay their child's entry into school by a year if they so choose—thereby shifting their children from being one of the youngest members of their cohort to one of the oldest. The decision to hold a child back a year, or "redshirt", continues to be a highly debated topic among parents, school administrators, and policymakers given the attention the issue has received in both the academic and popular press.[1]

Academic redshirting has become increasingly popular in recent years, particularly among whites and children with parents with at least a college degree (Deming and Dynarski 2008, Dobkin and Ferreira 2010). Starting with Angrist and Krueger (1991), previous literature has identified that there can be significant differences between the youngest and oldest students in a cohort based on their school starting age. Particularly, students who start school younger due to the cutoff display lower test scores and are more likely to repeat a grade than the older members of their school cohort (Bedard and Dhuey 2006, Datar 2006, Elder and Lubotsky 2009). It has also been found that those who begin school younger are at greater risk of being diagnosed with Attention Deficit/Hyperactivity disorder (ADHD) and similar mental health issues (Elder and Lubotsky 2009, Dee and Sievertsen 2015). In contrast to these negative consequences, it has been shown that the youngest in a cohort are more likely to graduate from high school (Dobkin and Ferreira 2010, Cook and Kang 2016). Also, Dobkin and Ferreira (2010) and Black, Devereux, and Salvanes (2011) find being the youngest in a cohort has no effect on long-term earnings.

These findings help illustrate the competing mechanisms at play. During the initial stages of their education, those who start school younger relative to their similarly-aged peers will face the challenge of competing against their older cohort mates. However, those who start when they are older are eligible to drop out of school earlier due to reaching the school drop-out age a grade sooner. These two competing treat-

[1] New York Times (Weil 2007, Paul 2010, Wang and Aamodt 2011, Kohn 2015), Washington Post (Strauss 2015), US News (Hansen 2016)

ments are important to consider when discussing how the age at which someone begins school affects their life path. While being the youngest of a cohort can place someone at higher risk of falling behind in school and developing mental health problems, having a slightly higher probability of finishing high school may make up, on average, for some of the deleterious effects of starting school younger.

This paper uses the school entry cutoff as the source of variation to estimate the effect of being the youngest in a school cohort on criminal behavior. The data set used here contains information on approximately 4.6 million arrests over a 20-year period in California for individuals between the ages of 13 and 24. Each record contains detailed information on the arresteeincluding the date of birth, date of arrest, the type of crime, and level of offense—with the types of crime being either property or violent and the levels of offense being either felony, misdemeanor, or status.[2] This will allow me to estimate various crime-age profiles around the school entry cutoff, providing evidence on how criminal behavior changes as people age both within and across different types and levels of crime. Having the exact date of birth of each individual makes it possible to determine how far an individuals birthday is from the school entry cutoff date and assign them to one of two groupsthose who begin school as the youngest in a cohort due to being born before the cutoff and those who begin school as the oldest in a cohort due to being born after the cutoff. Creating these groups allows for the use of a regression discontinuity (RD) design framework, where I am able to compare arrest rates of those born prior to the cutoff to begin school to those born after. The of use

[2] Status offenses are offenses for which only juveniles can be charged, which include truancy, loitering/breaking curfew, running away, and incorrigibility.

arrest rates as the measure of criminal activity stems from the seasonality of births that would make comparing arrest frequencies based on where individuals birthdays fall inaccurate—given that more people are born during certain times of year. To correct for this, I use population data and birth certificate records, in conjunction with the arrest records, to create arrest rates corresponding to where birthdays fall relative to the school entry cutoff.

This suggests that the transition from middle school to high school can have important impacts on student behavior. Upon reaching the 9th grade, students move from being the oldest in middle school to being the youngest in high school, are given more autonomy from parents and teachers, and are more likely to skip school (Reyes et. al 1994, Weiss and Bearman 2007, Benner 2011). A key factor influencing these changes is the shift in peer group composition upon entering high school (Schiller 1999). Transitioning to high school introduces students to a different peer group with which they may interact with on a daily basis, which has been shown to increase the potential criminal network of certain types of juveniles (Billings et al. 2016). Also, high school students likely face a higher probability of crime detection than middle school students due to the larger number of school security staff and School Resource Officers present in high schools relative to middle schools (U.S. Department of Education 2015).

Although there is a change in arrest rates at the cutoff for 14 year olds for certain offenses, it is important to note that the difference in criminal activity goes to zero when both groups are in high school. Also, even though the youngest members of a cohort exhibit a higher arrest rate for certain offenses at age 14, they show no sign of escalation into more serious criminal behavior past that age relative to their similarly-aged peers who began school older.

This paper closely relates to three recent papers that have investigated the effect of the school starting age on crime. Landers, Nielsen and Simonsen (2017) found that in Denmark those who are the oldest in their cohort are less likely to commit a crime before the age of 18 by 1.5 percentage points for females and 4 percentage points for males. With administrative data from Louisana, Depew and Eren (2016) showed that late school entry by one year decreased juvenile crime incidence for young black females through the age of 17 by approximately 3 percentage points—particularly for those who live in high crime areas. Cook and Kang (2016) used data from North Carolina and found that those who began school as the oldest in their cohort are less likely to be delinquent between the ages of 13 and 15 by 3 percentage points, but are more likely to be convicted of a felony between the ages of 17 and 19 by nearly 1 percentage point.

The results of these three papers indicate that those who begin school older than their similarly-aged peers are less likely to commit crime at younger ages by similar magnitudes. When compared on a similar scale, the change in arrest rates at age 14 between those that are the youngest and oldest in their cohorts that I find here for status and misdemeanor offenses is less than 1 percentage point. An important difference between this paper and the previous literature mentioned above is that the longitudinal nature of their data sets allows them to track if an individual ever committed a crime during a particular age window (i.e. before the age of 18, between the ages of 13 and 15, etc.). Though my data set is not longitudinal due to the lack of unique identifiers, I am able to estimate the probability of arrest among the population at each age due to the large number of observations. Thus, while the signs of my estimates are equivalent, the magnitude of my estimates are smaller, as I measure criminal activity one age at a time.

The rest of the paper is organized as follows: Section 1.2 reviews relevant literature. Section 1.3 provides a description of the data. Section 1.4 outlines the identification strategy and empirical model. Section 1.5 presents the results. Section 1.6 concludes.

1.2 Literature Review

1.2.1 Differences Across School Starting Age

An extensive literature examines how an individual's school starting age can impact various outcomes. This body of literature began with Angrist and Krueger (1991), which found that individuals born in the first quarter of the year have lower educational attainment levels than those who are born later in the year. This finding is likely driven by the fact that those born earlier in the year are typically forced to wait an additional year to enroll in kindergarten than those born later in the year due to compulsory schooling laws regarding school entry age. This translates to the older students being exposed to the dropout age sooner than the younger students since they started school a year older.

Academically, at younger ages those who began school later typically fare better in terms of test scores than those who begin younger. Datar (2006) find that students that start kindergarten when they are one year older have higher test scores at kindergarten entry and show stronger improvements in test scores during the first two years of school. Bedard and Dhuey (2006) show that the youngest students in a cohort have lower standardized test scores than the oldest students during fourth and eighth grade. Results from Black, Devereux, and Salvanes (2011) and Crawford, Dearden, and Meghir (2010) suggest that these differences in test scores may be due to age-at-test effects. That is, the relative age difference stemming from being a year younger relative to other same-grade peers may be responsible for this gap in academic performance. Elder and Lubotsky (2009) find that being a year younger at school entry raises the probability of repeating a grade between kindergarten and 2nd grade by 13 percentage points.

Outside of academic consequences, Elder and Luboksky (2009), along with Dee and Sievertsen (2015) also, find that being younger at the start of schooling can increase the probability of being diagnosed with ADHD. However, despite these negative outcomes for those who start school younger, Dobkin and Ferreira (2010) and Cook and Kang (2016) find that those who begin school younger have a higher probability of graduating from high school relative to those who start older due to the school entry cutoff.

1.2.2 School-Crime Relationship

Another related of literature explores the school-crime relationship, where a particular mechanism is identified through which school affects crime.

There have generally been two approaches through which researchers have investigated the effect of school on crimethe short-term effect and the long-term effect. The short-term approach focuses on the effect of being at school on criminal activity. That is, criminal outcomes are compared for juveniles when school is in session to when school is not in session due to some exogenous shock that is unrelated to factors affecting criminal activitythereby causing students to not attend school. This is often referred to as the incapacitation effect of school, where juveniles are unable to commit crime on the street because they are at school. Jacob and Lefgren (2003) exploit variation generated by teacher in-service days to show that on school days juveniles commit less property crime due to higher monitoring and incapacitation but more violent crime due to higher concentration and interaction levels. Luallen (2006) and Akee, Halliday and Kwak (2014) use teacher strike days and teacher furloughs as their source of variation, respectively, and find similar results.

The long-term approach focuses on how additional or better education affects criminal outcomes later in life. A fundamental problem in estimating the effect of education on criminal activity is that the unobservable characteristics that potentially contribute to a person's choice of schooling are likely to be correlated with unobservable characteristics that are attributed to a person's choice to commit crime. Thus, in order to control for this endogeneity concern, many studies have used an identification strategy that entails the use of some policy change that has some direct impact on schooling decisions. Lochner and Moretti (2004) utilize compulsory schooling laws as an instrument for years of schooling to estimate the effect of education on crime using data from the U.S. Census, Federal Bureau of Investigation and the National Longitudinal Survey of Youth, where they find a negative relationship between education and crime. Anderson (2014) finds that increasing the minimum dropout age from 16 to 18 reduces property and violent crime arrests for juveniles aged 16 to 18. Deming (2011) finds that those who were randomly selected to attend a high-ranking school through lottery are arrested less often and are incarcerated less frequently seven years after the assignment. The implication is that more or better education decreases the probability of committing crime.

1.2.3 School Starting Age and Crime

Recent literature has sought to examine the school-crime relationship by exploring how the school starting age affects criminal behaviorwhere investigators seek to establish if school entry laws governing the school starting age have any causal effect on an individuals propensity to commit crime at various ages.

Cook and Kang (2016) use administrative data from North Carolina and found that those born after the cutoff to start kindergarten are less likely to be involved in juvenile delinquency between the ages of 13 and 15, but are more likely to be convicted of a felony between the ages of 17 and 19with a significant portion occurring specifically at age 19.[3] The authors suggest that even though those born after the cutoff are less likely to be juvenile delinquents, being able to drop out of school sooner than those born before the cutoff outweighs the better academic performance and low juvenile delinquency mechanisms.[4] Landers, Nielsen and Simonsen (2017) utilize Danish register-based data and find that being older at school start lowers an individuals propensity to commit crime between the ages of 15 and 19. The authors argue that this result is due, in part, to incapacitationstemming from Denmarks school compulsory laws and crime occurring during the weekday. Depew and Eren (2016) use administrative data from Louisiana to show that for young black females, late school entry by one year lowers juvenile criminal activity—particularly for those who live in high crime areas. The authors propose that these findings may be potentially explained by age-related differences in human capital accumulation between those who begin school one year apart.

While the results of these three papers find that those who delay school entry by a year are less likely to commit crime at younger agesLanders, Nielsen and Simonsen (2017) and Cook and Kang (2016) find differing results as individuals reach adulthood.[5,6] Landers, Nielsen and Simonsen (2017) find the difference in criminal behavior between those on either side of the cutoff goes to zero as individuals age, while Cook and Kang (2016) find those born after the cutoff are more likely to be convicted of a felony in their young adult years. A direct comparison may be misleading, however,

[3] Juveniles are treated as adults in the North Carolina criminal justice system starting at age 16.

[4] The minimum school leaving age in North Carolina is 16 and those born after the cutoff have a longer window in which they can legally dropout compared to those born before the cutoff.

[5] Depew and Eren (2016) do not examine adult criminal behavior.

[6] McAdams (2016) finds that a higher school starting age cutoff leads to lower rates of incarceration for adults.

given the institutional differences between Denmark and the U.S. In the U.S., school is compulsory until a certain age, ranging from 16 to 18 depending on the state. However, in Denmark, school is compulsory from age 7 until an individual completes the 9th grade, regardless of age. Further, there is no juvenile court in Denmark and individuals cannot be arrested for, charged with, or incarcerated for a crime until the age of 15.[7]

1.3 Data

1.3.1 Arrests Data

Data on arrests comes from the Monthly Arrest and Citation Register (MACR) database from the California Department of Justice. The MACR database contains individual arrest records that have detailed information about the arrestee including date of birth, date of arrest, age of offender, type of crime, and level of offense. These records cover all reported juvenile and adult arrests in California over a time frame of 20 yearsfrom 1986 to 2006. The data set contains records on approximately 4.6 million arrests of individuals between the ages of 13 and 24. Each record contains only one offense; therefore, if someone is arrested for more than one crime only the most serious offense is recorded. That is, if an individual is arrested for multiple offenses at once, the data set only records the most serious of those offenses.

The key feature of the arrest records is that it contains an arrestees exact date of birth. With this information I am able to calculate how far an individuals birthday

[7]According to Landers, Nielsen and Simonsen (2017), incarcerated individuals between the ages of 15 and 17 are to be housed separately from adult prisoners.

is from the relevant cutoff date to begin school. This distance, measured in days, allows me to create two separate groupsthose born prior to the cutoff to begin school and those born after the cutoff. A more in-depth discussion of using the RD design in this context can be found in the next section.

All people in the MACR database have already been arrested; therefore, in order to create an outcome variable, I aggregated the individual arrest records at the relative date of birth level. That is, I created one arrest count for each group of individuals whose birthdays fall on the same relative distance away from the cutoff to begin school. For example, the year they turn five, all individuals born on November 30th when the cutoff was December 1st and all individuals born on December 1st when the cutoff was December 2nd are included in the arrest frequency count of those born one day before the cutoff.

1.3.2 Constructing Arrest Rates

Comparing arrest frequencies across the cutoff would not yield an accurate comparison since there are more people born at certain times of the year than others. Figure 1.1 displays birth counts by day of year over a 10-year period using birth certificate data from the California Department of Public Health from 1989 to 1998.[8] This figure shows that more people are born in the fall than other times of year. Therefore, comparing frequencies based on how far a birthday is from the cutoff would indicate that those born in the fall commit more crime than people born at other times when,

[8]This range of years is based on data availability.

in fact, this stems from higher birth rates during that time of year. Thus, I create arrest rates using annual population data from the California Department of Finance and birth certificate data from the California Department of Public Health. To calculate arrest rates for each relative distance from the cutoff I use data from 10 years of birth certificate records from 1989 to 1998 to determine the average percentage of people born on a particular day during the year. I then multiply these percentages by the annual population numbers over the 20-year arrest sample to get the approximate number of people in the population whose birthday falls a certain amount of days from the cutoff. Arrest frequency counts at each relative distance from the cutoff were then divided by this number and multiplied by 100,000 to obtain an arrest rate per 100,000 people.

1.3.3 Data Limitations

Before discussing the identification strategy, I first address some of the limitations of the data. A potential concern is that the data set does not have information on when each arrested person actually started school. If academic redshirting is prevalent among those born before the cutoff, then comparing them to those born after could invalidate the research design given they would begin school at the same time. To address this concern, I will be relying on findings from other sources that show that those born before the cutoff tend to be a grade ahead of those born after and that redshirting is not a serious issue (Dobkin and Ferrira 2010, Bassok and Reardon 2013, U.S. Department of Education 2013). I discuss this in more detail in the following section. Further, related to this concern, I do not have information on the arrestees state of birth. If those arrested in California were not born in California, then they may have been subject to a different school entry cutoff from a different state or country. While this an important consideration, data from the U.S. Census Bureau does show that of all residents of California aged 24 and younger, 80% were born in California (U.S. Census Bureau 2005). The arrest records do not have unique identifierswhich prevents me from identifying how prominent recidivism is within this universe of arrest records. Recidivism measures how often individuals released from prison are either rearrested, reconvicted, or sent back to prison within a certain time frame. Thus, it is likely that certain individuals appear more than once in the data set. Also, using arrests as the criminal outcome measure has the potential to underestimate or overestimate the true level of criminal activity. Arrests will underestimate the true level of criminal activity since every crime committed does not end in a subsequent arrest. However, this research design would not be possible to implement using crime counts, as the age of perpetrators of unsolved criminal activity cannot be observed. Arrests may also overestimate the true level of criminal activity because all arrested persons are not formally charged or convicted. This is likely not a major concern as 75% of juveniles and 90% of adults in the sample will proceed to the next step of the criminal justice process.[9]

[9] The MACR database shows where a person is sent after arrest, but not what happens to them after that step. For juveniles, this indicates that they are sent to the probation department for further processing and for adults this means that a formal complaint was sought.

1.4 Identification Strategy and Empirical Model

1.4.1 Institutional Setting

During the time frame for which the individuals in my data set were born, between the years of 1951 and 1993, students must have turned 5 by either December 1st or 2nd in order to start kindergarten that year. Specifically, for those born between 1951 and 1987, the cutoff date was December 1st, and for all others the cutoff date was December 2nd.[10] The goal here is to compare criminal outcomes across the threshold to begin school. That is, I investigate if those born after the cutoff commit more or less crime than those born before. As detailed in the previous section, the key to creating these groups is having the exact date of birth of the arrestee and calculating how far their birthday is from the relevant cutoff. In order to estimate the effect of school entry on criminal activity I utilize an RD design model.

1.4.2 Empirical Model

The goal of using an RD design is to emulate a randomized experiment where the only difference between the two groups on either side of the threshold is that one group receives some treatment while the other does not. In this case, those born after the cutoff will have to delay entry to school by one year. While it is a non-experimental technique, an RD design can closely resemble a locally randomized experiment as long as individuals do not have precise control over the assignment variable (Lee and Lemieux

[10]The California Education Code regarding school entry age prior to 1987 read that individuals must be 4 years and 9 months old by September 1, which is equivalent to turning 5 years old by December 1.

2010). In this case, that would mean parents do not have precise control of when their child is born such that there is no manipulation of birthdates around the cutoff to begin school. Dickert-Conlin and Elder (2010) use data from the U.S. Vital Statistics Natality Detail Files to present evidence that this potential manipulation is not prominent. Specifically, they find that the U.S. population of births around the relevant cutoff to begin school does not exhibit any statistically significant discontinuities.[11]

I estimate the effect of school entry cutoff on crime by using the following equation:

$$Y_i = \beta_0 + \beta_1 Cut_i + \beta_2 Distance_i + \beta_3 Cut_i x Distance_i + \beta_4 Distance_i^2 + \beta_5 Cut_i x Distance_i^2 + \epsilon_i \quad (1.1)$$

where Y_i represents the arrest rate for those whose birthdate is i days away from the cutoff, Cut_i represents an indicator variable that takes on a value of 1 for those born after the cutoff, and $Distance_i$ represents the number of days a birthdate is from the relevant cutoff. For example, a person whose birthday is on November 30th would get a value of -1 if their cutoff was December 1st. The inclusion of the interaction terms allows for the function to differ on either side of the cutoff (Lee and Lemieux 2010). The coefficient of interest here is β_1, which represents the magnitude of the discontinuity in arrest rates at the cutoff.

The sample consists of those arrested between the ages of 13 and 24with analyses conducted for juveniles (13-17 year olds) and adults (18-24 year olds) separately.[12]

[11] Shigeoka (2015) does find evidence of manipulation of births around the cutoff in Japan, where certain parents delay the birth of their child to just after the cutoff. However, in Japan the school entry cutoff is binding and the option of delaying entry is not available as it is in the U.S.

[12] I choose this particular age range for adults because the peak years of criminal activity are during the late teens and early 20s (Gottfredson and Hirschi 1983).

Additionally, I provide estimates for each individual age in the sample. I also present estimates by type of crimefocusing on property and violent crime individually.[13] These two major categories of crime are comprised of the most serious offenses and may exhibit different trends due to the varied nature of the sub-categories of crime they consist of. I also estimate Equation (1.1) by the severity of offensewhich can be felony, misdemeanor, or status. Status offenses include truancy, loitering/breaking curfew, running away, and incorrigibility. Individuals of any age can be arrested for felonies and misdemeanors, while only juveniles can be arrested for status offenses. Table 1.1 shows the distribution of arrests across the different types and levels of crime for each age group. Misdemeanor offenses make up more than half of all arrests for both groups, approximately one-third of arrests are for felony offenses, and thirteen percent of juvenile arrests are for status offenses. Table 1.1 also shows that property crime is more prevalent than violent crime for both age groups.[14] Lastly, I estimate Equation (1.1) by time of yearwhere I compare estimates during the school year to those in the summer.

Since the assignment variable here is discrete, I use a parametric approach as opposed to a nonparametric approach (Lee and Card 2008). Regardless of the chosen method, RD estimates can be sensitive to the choice of bandwidth. I use the optimal bandwidth calculation procedure created by Calonico, Cattaneo, and Titunik (2014a). This procedure calculates the optimal bandwidth of a given dataset using three dif-

[13]Property crime consists of burglary, larceny, auto theft and arson. Violent crime consists of murder, manslaughter, rape, robbery, and aggravated assault.

[14]The percentages shown do not equate to 100 percent as property crime and violent crime are subsets of felony and misdemeanor offenses.

ferent methodologies from the RD literature.[15] The results of this procedure indicate that the optimal bandwidth falls within a broad range, depending on the methodology used—varying from approximately 60 days to 120 days for the different types and levels of crime.[16] Given this range, Equation (1.1) will be estimated using the smallest bandwidth of 60 days to capture the effect of starting school younger for those who are most affected by the cutoff.

I do not have information on the actual school starting age for the individuals in the MACR database. That is, with these individuals I cannot explicitly show that those born prior to the cutoff started school earlier than those born after for these particular arrestees. It is likely that there is some percentage of individuals on both sides of the cutoff that did not adhere to the particular deadline. Those born before the cutoff may take part in academic redshirting, where parents delay school entry for a year so that their children will be a year older when they start school. Also, there may be those born after the cutoff who begin school a year earlier if they begin at a private school, where the cutoff is not mandatory, or gain some exception to start school early.[17] Although these concerns can invalidate this research design if the effects are strong enough, Bassok and Reardon (2013) use data from the Early Childhood Longitudinal Survey to show that between 4%- 5.5% of children in the U.S. delay school entry. Also, data from the U.S. Department of Education shows that 87% of kindergartners start on time and

[15]The procedure implements optimal bandwidth calculations proposed by Calonico, Cattaneo and Titiunik (2014b), Imbens and Kalyanaraman (2012), and Ludwig and Miller (2007).

[16]Table A.1 in the appendix shows the various optimal bandwidth calculations.

[17]The California Department of Education allows districts to decide early admissions, with the admission criteria being generally based on test results, maturity of the child, or preschool records.

that 6% delay entry (U.S. Department of Education 2013). Lastly, Dobkin and Ferreira (2010) used restricted access Decennial Census Long Form Data from 2000 to show that, though compliance with the cutoff is not perfect, those born prior to the cutoff tend to be a grade ahead of those born after the cutoff in California. Specifically, the school entry cutoff induces approximately 80% of those born prior to the cutoff to begin kindergarten as soon as they are eligible.

1.5 Results

1.5.1 Results for Juveniles and Adults

Figure 1.2 displays RD plots according to the corresponding type or level of crime using a 60-day bandwidth on either side for juveniles, while Figure 1.3 shows the same for adults. Each panel contains a scatterplot of points that represent 6-day local averages of arrest rates and fitted lines from Equation (1.1) for each side of the cutoff. Local averages of arrest rates are used in order to reduce the noise of the plots. The horizontal axis of each plot represents the relative date of birth—how many days away a birthdate is from the relevant cutoff to begin school, with negative values representing those born before and positive values representing those born after. Visual evidence from Figures 1.2 and 1.3 suggests that there is likely not any difference in arrest rates across the school entry cutoff for the various types and levels of crime.

Table 1.2 gives estimates of the discontinuity at the cutoff for the different crime categories and age ranges using a 60-day bandwidth corresponding to Figures 1.2

and 1.3. All of the estimates shown here are insignificant across the various types and levels of crime.

1.5.2 Results by Age

Given the results of the previous section, the age groupings of juveniles and adults may be too large to see any real effect; thus, examining the effect of the cutoff at each individual age may give a better picture of how the school entry cutoff affects criminal behavior. Figure 1.4 shows discontinuity estimates using a 60-day bandwidth for each individual age using age-specific arrest rates across the types and levels of crime. Estimating Equation (1.1) by age will show how criminal behavior of those born after the cutoff may change relative to those born before as both groups age. Overall, this figure indicates that there is little to no difference in arrest rates across the cutoff as individuals age. The only change occurs at age 14, where those born after the cutoff are less likely to commit a status or a misdemeanor offense. Specifically, those born after the cutoff are approximately 9% less likely to commit a status offense and 6% less likely to commit a misdemeanor than those born before the cutoff at age 14.[18] This figure also indicates that as individuals age there is no statistically significant difference in criminal behavior across the cutoff. Additionally, there is no sign of escalation in criminal behavior for either group relative to the other as they ageas any differential effect of the school entry cutoff is no longer present past the age of 14. Tables 1.3 and 1.4 show estimates for the individual crimes that constitute status offenses and some of the

[18]Figure A.3 in the appendix shows individual RD plots for status offenses and misdemeanor offenses for 14 year olds.

more frequent misdemeanor offenses for 14 year olds, respectively.[19] These tables show that at age 14, those born after the cutoff are nearly 30% less likely to be arrested for truancy, 9% less likely to be arrested for petty theft, and 11% less likely to be arrested for vandalism.

A potential explanation for this pattern is that on either side of the cutoff 14 year olds may be subjected to differential institutional settings. Given that individuals born before the school entry cutoff begin kindergarten as soon as they are eligible, those born prior to the cutoff will turn 14 during high school while those born after the cutoff will turn 14 during middle school. This may suggest that the result for 14 year olds here is capturing the effect of being in different environments as opposed to how starting school younger differentially affects criminal behavior compared to those who begin school older.

A more accurate comparison would be to examine how those who start school younger differ in arrest rates when they are part of the same population as those who began school older. The results in Figure 1.4 would indicate that after the age of 14, when both sets of people are most likely part of the same school setting and peer composition, there are no changes at the cutoff in criminal behavior. For example, at ages 15 and 16 when those who start school younger and those who start school older are both in high school, there are no longer any statistically significant differences in arrest rates at the cutoff. Even into adulthood, the results here do not indicate that past the age of 14 that there is any difference in criminal behavior between those who

[19]There are several more types of misdemeanor offenses, the ones shown here are the most prevalent based on arrest volumes.

begin school at different ages.[20] To investigate if this result is being driven by school setting, I re-estimate Equation (1.1) by time of year.

1.5.3 Results by Time of Year

Upon entering high school, their peer group composition will change through the increase in the number of peers they may interact with regardless of age on a daily basisincluding those who are in higher grades and are older by several years. That is, they are simply around more people during the school year. Also, within a given community, juveniles from various middle schools will become integrated into one high school upon entering the 9th grade. This in turn will increase the amount of same-aged peers that a juvenile may interact with on any given school day. It has been shown that peer group composition can have a significant impact on juvenile behavior. Billings et al. (2016) finds that increasing the amount of similar age, grade, and race peers in an individuals neighborhood who also attend the same school increases the probability of committing a crime. That is, increasing the amount of people similar to a juvenile in a given neighborhood that also attend the same school may increase their potential criminal network. Additionally, it terms of crime, being in high school likely increases the probability of being arrested due simply to the increased amount of resources dedicated to security relative to middle school (U.S. Department of Education

[20] Figure 1.4 shows that for 19 and 20 year olds estimates for property crime are nearly significant at the 95% level and that those who begin school older are slightly more likely to be arrested. This is also likely driven by a mechanical effect that captures the populations in different stages in life from transitioning out of school. Those who start school younger completed high school at a younger age while those who began school older may be making a slower transition out of high school and into the workforce. Comparing both populations when both are in their early to mid-20s and are more likely to be in a similar stage shows no difference in arrest rates at the cutoff.

2015). Thus, those who enter high school first due to starting school younger will be faced with different environmental conditions at age 14 compared to those who are in middle school due to starting school older.

Figure 1.5 shows status offense and misdemeanor arrest counts by day of arrest over the 20-year sample. The red lines demarcate the approximate school year in the state of Californiawhere the spaces inside each set of red lines correspond to Thanksgiving, Christmas, and summer breaks.[21] This figure would indicate that there is a marked decrease in arrests during the summer months and when school is not in session due to breaks.[22] As a comparison, Figure 1.6 shows misdemeanor arrest counts for the adults in the sample and there is no such change in arrests during the summer.[23] While there are likely several explanations as to why there is a drop in arrests when school is not in session, including differential monitoring standards during the school year, the purpose of this figure is to show that estimating the effect of the cutoff on criminal behavior by time of year may provide some suggestive evidence that being part of separate populations at age 14 is driving the change at the cutoff for status and misdemeanor offenses.

Figure 1.7 shows discontinuity estimates using a 60-day bandwidth for each

[21] I checked various school district websites and contacted district offices in California to get an approximate school year based on historical school calendars, though the years do not exactly match the time frame of my arrest data. I also use truancy arrests as a guide to the school year since it is an offense that only occurs during the school year as seen in Figure A.1where the arrest trend closely matches what I limit the school year to in my estimates. Thanksgiving break was approximated to be between November 23 and November 28, Christmas break was approximated to be between December 20 and January 4, and summer break was approximated to be between June 15 and September 1

[22] Figure A.2 in the appendix also shows a similar trend for each juvenile age and for all juveniles combined. Also, arrest frequency trends for the other types and levels of crime show a similar trend.

[23] Arrest frequency trends across other types and levels of crime show a similar trend.

individual age using age-specific arrest rates for status offenses and misdemeanors by time of year. Though only those of school age should be affected here, this figure also includes non-school ages as a comparison. During the approximate school year, those born after the cutoff are less likely to commit a status offense at age 14 than those born before the cutoff by approximately 9%. Also, during the summer those born after the cutoff are less likely to commit a misdemeanor at age 14 than those born before the cutoff by 11%.[24] Though the results stemming from misdemeanor offenses during the summer may not initially agree with the separate population argument, it is likely that those who began school younger would have already established their high school peer group during the prior school year as they would be 14 during the summer heading into the 10th grade—speaking to the effect peer group has on behavior as discussed earlier. Figure 1.8 also shows that after age 14 there is no difference in arrest rates across the cutoff during the school year or the summer when those born on either side of the cutoff are part of the same population and environment. Figure 1.8 further breaks the school year out into weekday and weekend arrest rates by age. This figure would indicate that those born after the cutoff are less likely to commit a status offense during the week at age 14 than those born before the cutoff by 13%.[25] It also shows that during the weekend there is no statistically significant difference in status offense arrest rates across the cutoff. Tables 1.5 and 1.6 show estimates for the individual crimes that constitute status offenses and some of the more frequent misdemeanor offenses by time of year,

[24]Figure A.4 in the appendix shows individual RD plots for 14 year olds for status and misdemeanor offenses during the school year and summer.

[25]Figure A.5 in the appendix shows individual RD plots for 14 year olds for status and misdemeanor offenses during the school year during the week and the weekend.

respectively. These results provide suggestive evidence that varied institutional settings may be driving the difference in arrest rates at the cutoff at age 14.

1.6 Conclusion

In this paper, I study the effect of starting school earlier than similarly-aged peers due to the school entry cutoff on criminal behavior using a large universe of arrest records. I do not find evidence of a significant overall effect of the school entry cutoff on juvenile or adult crime across different types and levels of crime including property crime, violent crime, felony offenses, misdemeanor offenses, and status offenses. When estimating this effect by each individual age in the sample using age-specific arrest rates, I find this result also holds for individuals age 15 and above. That is, between the ages of 15 and 24 I do not find any change in arrest rates at the cutoff across the types and levels of crime. However, I do find that those born after the cutoff are less likely to be arrested for a status or a misdemeanor offense at age 14. This result is likely driven by the fact that at age 14 those born on either side of the cutoff generally find themselves in different environments at that age—as those born just before the cutoff will turn 14 during their first year in high school compared to those born after the cutoff that will turn 14 during middle school.

While being in varied school settings may affect arrest rates through several channels, I posit that the environment in which those born on either side of the cutoff find themselves in is what is driving the results here. First, the peer group juveniles are exposed to during their first year of high school is likely different to that of middle schoolers. Also, there are differences in monitoring that likely affect the probability of being detected in high school compared to those in middle school. Further, I find no evidence that those born before the cutoff commit more crime past the age of 14 in any of the categories of crime or crime overallindicating that they do not exhibit any signs of criminal escalation relative to their peers born after the cutoff as they age. That the effect is limited to one age and only for a particular subset of less serious offenses would indicate that starting school younger has no persistent effect on criminal behavioras any changes at the cutoff do not exist past the age of 14 and are also not present before.

Given the nature and limited range of significant findings, the results here do not lend themselves to concluding that starting school younger has a lasting effect on criminal behavior. Compared to similar papers that have investigated the effect of school entry timing on crime, this generally null result may seem to contradict some of their findings. It is important to note, however, that the nature of this question may lead to different conclusions depending on the setting in which it is answered. That is, measuring criminal activity as it relates to school starting age involves two sets of regulations that likely vary across jurisdictions. As it relates to criminal activity, the stage at which a crime is measured may affect the outcome of this question, as an arrest may not capture the same level of criminality as a conviction. Also, compulsory schooling laws dictating the school starting age may differ by time of year, thereby capturing the effect for people born at different times of year.

While Landers, Nielsen and Simonsen (2017) does find a similar result that those who start school later due to the cutoff are less likely to commit crime at younger ages, they also find that the effect does persist until age 19 for boys in Denmark. However, given how different compulsory schooling laws are between the U.S. and Denmark, as well as how law enforcement differs for juveniles, a direct comparison of results may not be accurate. Closer comparisons can be found in Cook and Kang (2016) and Depew and Eren (2016). Both papers found that those born just after the cutoff are less likely to commit crime at younger ages, while Cook and Kang (2016) found that at age 19 those who born just after the cutoff are more likely to be convicted of a felony.[26] Comparing criminal activity across jurisdictions, especially across states, may not yield proper counterfactuals for one another given that criminal statutes and enforcement can vary widely across geographies. While certain findings here may be applicable in California, the same may not be true in other states. Future studies may be aided by utilizing a sample that includes individuals from various states across the country.

[26] Depew and Eren (2016) do not examine adult criminal behavior.

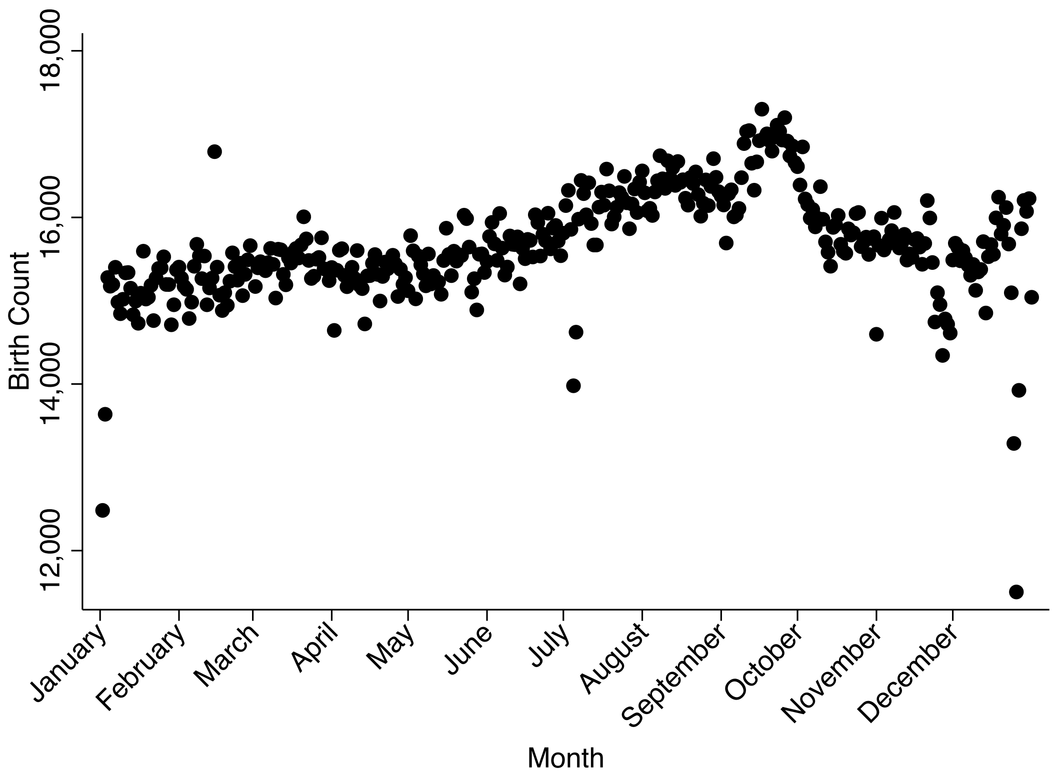
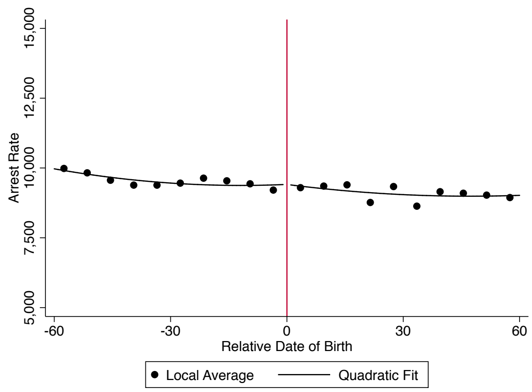
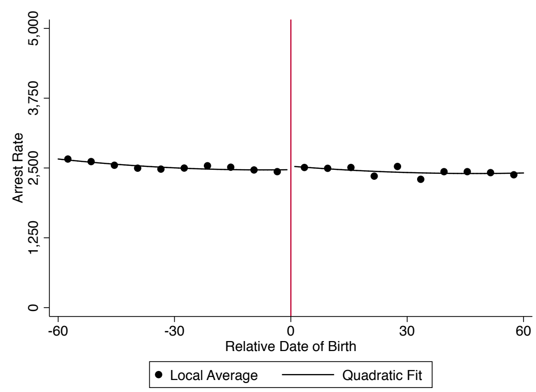


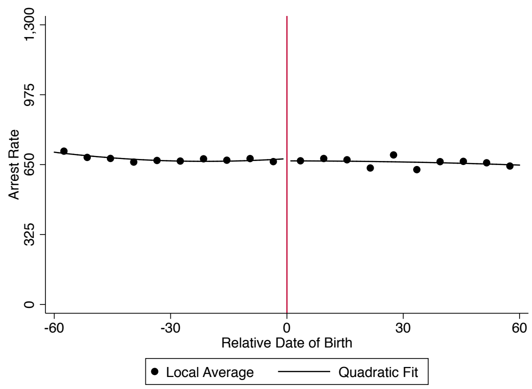
Figure 1.1: Birth Counts By Day of Year. Note: This figure displays birth counts by day of year over a 10-year period using birth certificate data from the California Department of Public Health from 1989 to 1998.



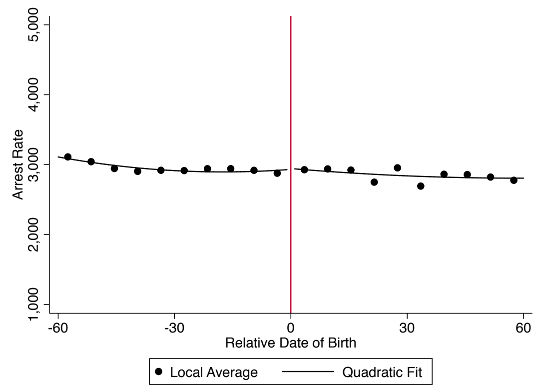
(a) All Crime



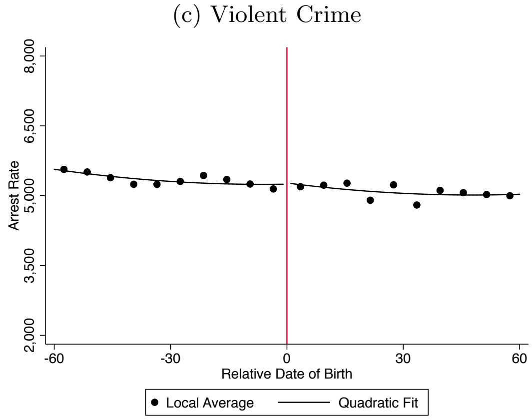
(b) Property Crime



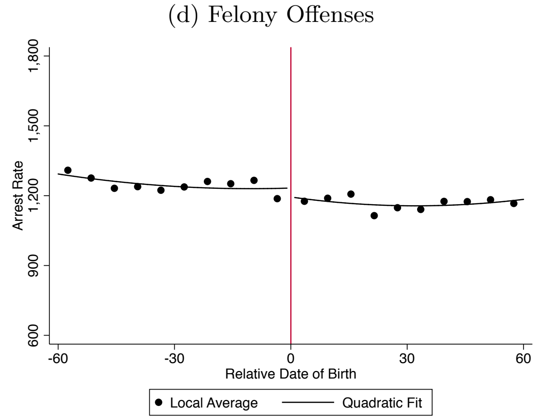
(c) Violent Crime



(d) Felony Offenses

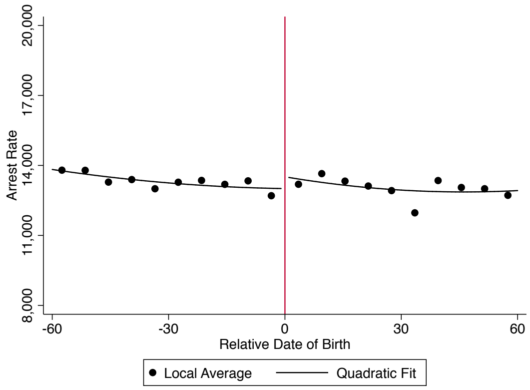


(e) Misdemeanor Offenses

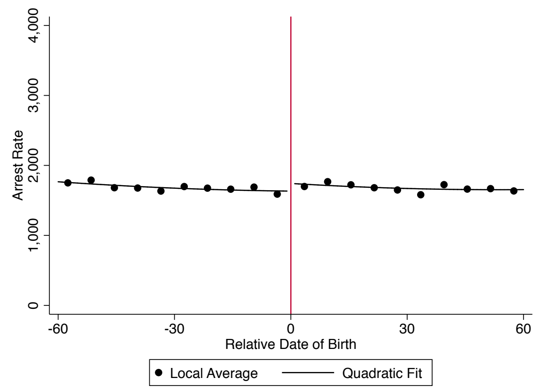


(f) Status Offenses

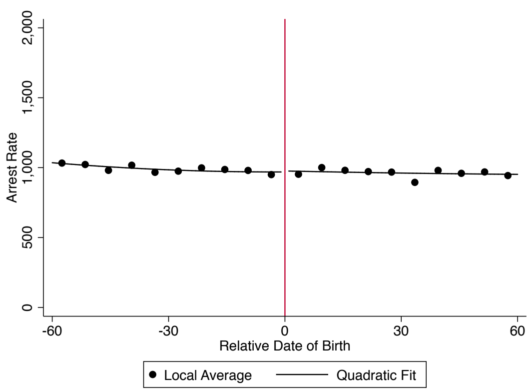
Figure 1.2: RD Plots By Crime Type - Juveniles. Note: Each panel represents the estimated discontinuity in arrest rates per 100,000 people at the cutoff to begin school for juveniles between the ages of 13 and 17 using a 60-day bandwidth on each side. Each dot represents the average arrest rate by 6-day blocks of relative age. Each line represents the fitted values of Equation 1.1 for each side of the cutoff.



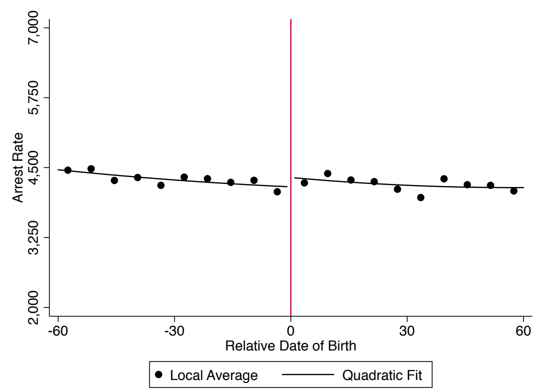
(a) All Crime



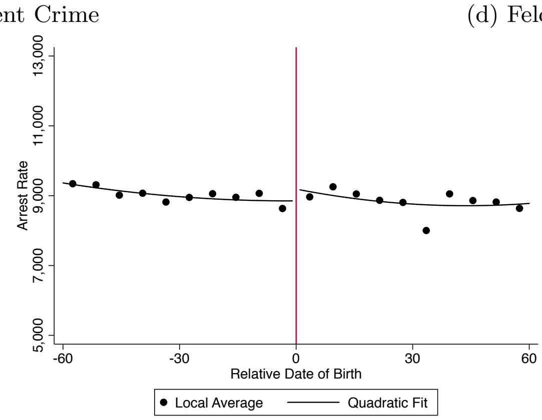
(b) Property Crime



(c) Violent Crime

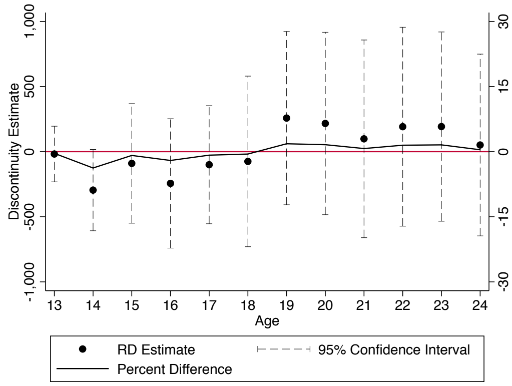


(d) Felony Offenses

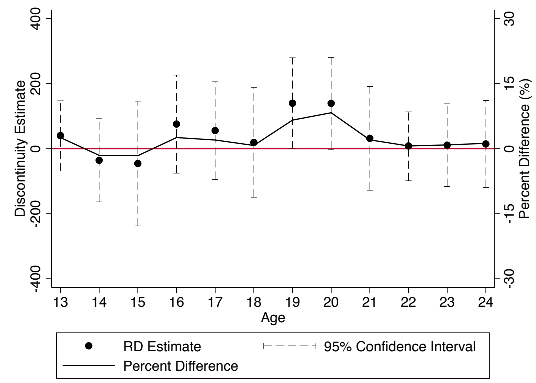


(e) Misdemeanor Offenses

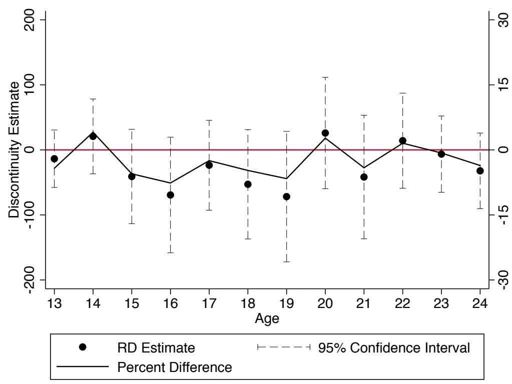
Figure 1.3: RD Plots By Crime Type - Adults. Note: Each panel represents the estimated discontinuity in arrest rates per 100,000 people at the cutoff to begin school for adults between the ages of 18 and 24 using a 60-day bandwidth on each side. Each dot represents the average arrest rate by 6-day blocks of relative age. Each line represents the fitted values of Equation 1.1 for each side of the cutoff.



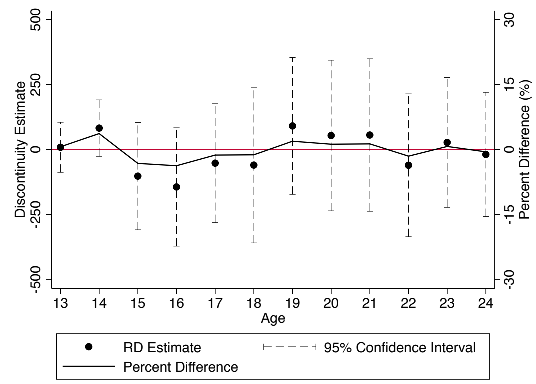
(a) All Crime



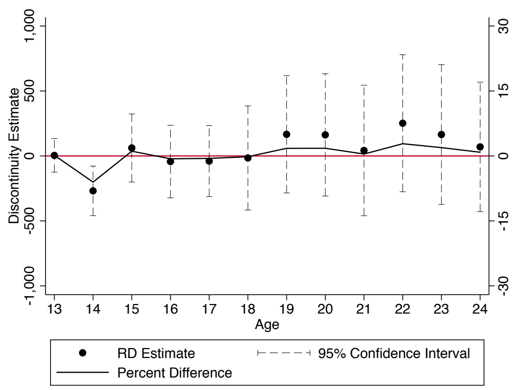
(b) Property Crime



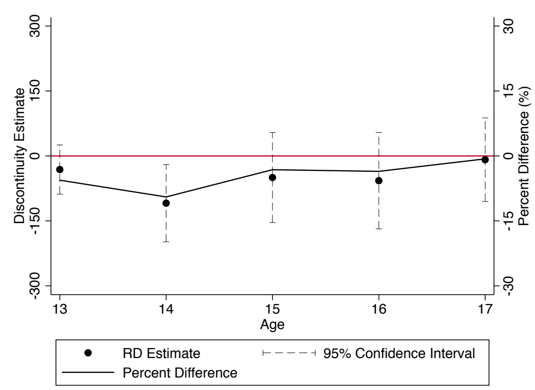
(c) Violent Crime



(d) Felony Offenses

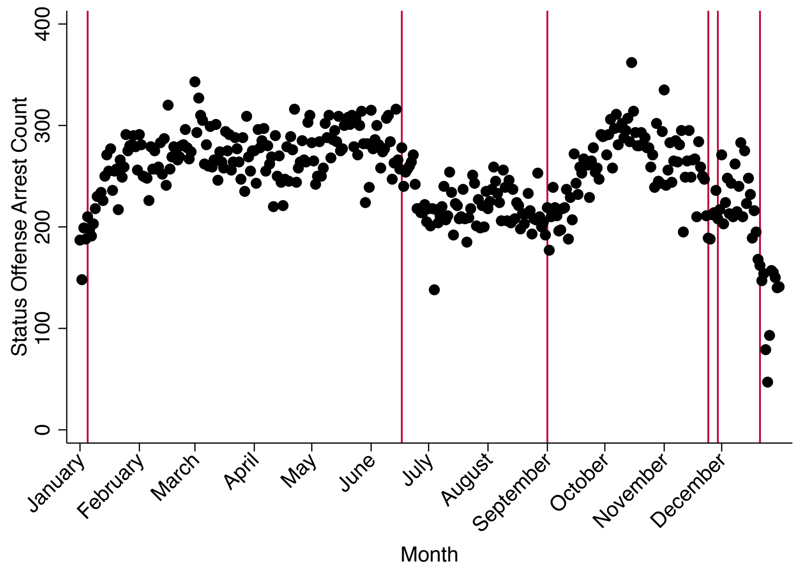


(e) Misdemeanor Offenses

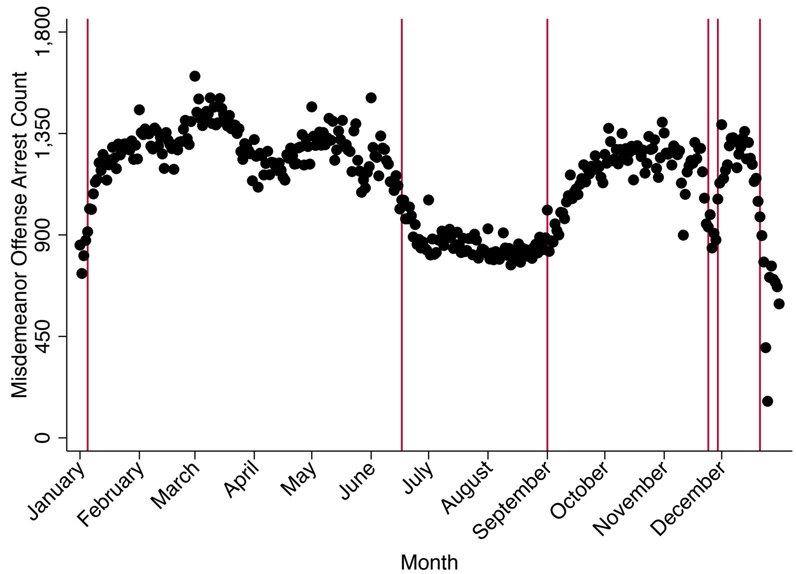


(f) Status Offenses

Figure 1.4: RD Plots By Crime Type By Age. Note: Each dot in each panel shows estimated discontinuities in arrest rates per 100,000 people at the cutoff to begin school for each age shown using a 60-day bandwidth for the corresponding type or level of crime. Dashed lines represent 95% confidence intervals. The solid line represents the percentage difference at the cutoff for each age.



(a) Status Offenses



(b) Misdemeanor Offenses

Figure 1.5: Arrest Frequency By Day of Year: 14 Year Olds. Note: Each panel represents the total arrest count for each day of the year over the 20-year sample for the corresponding level of crime for 14 year olds. Areas inside of the red lines indicate when school is not in session due to Thanksgiving and Christmas holidays and summer break. Arrest counts for status offenses do not include truancy arrests.

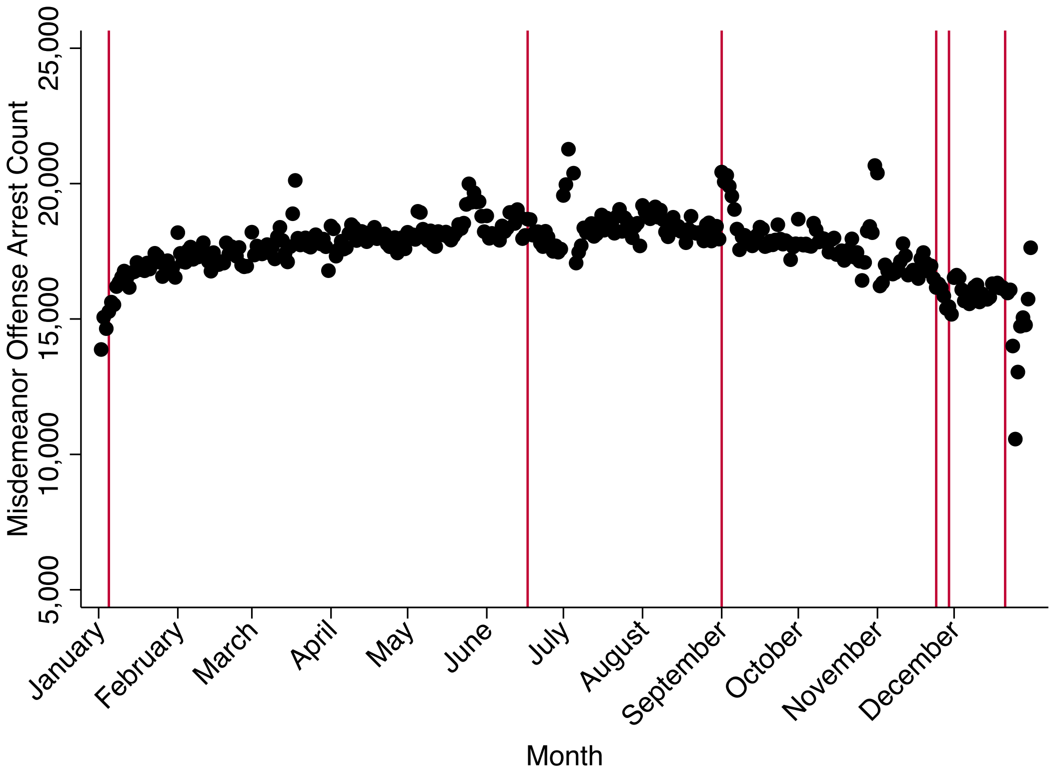
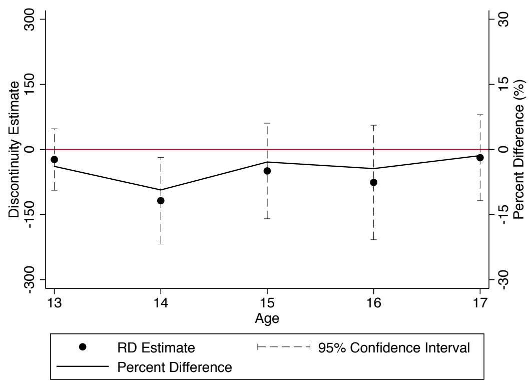
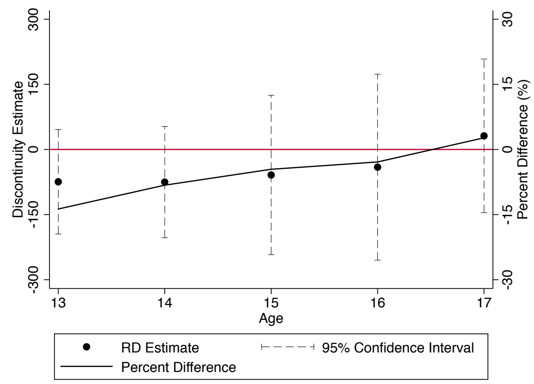


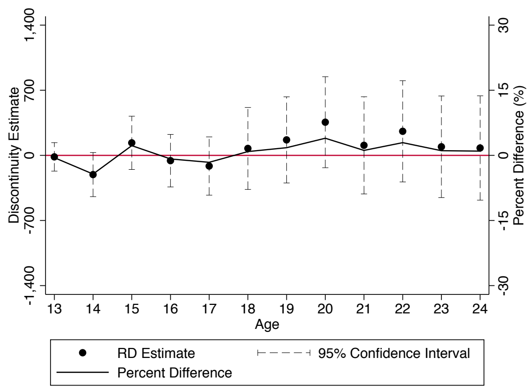
Figure 1.6: Arrest Frequency By Day of Year: Misdemeanor Offenses. Note: This figure represents the total arrest count for each day of the year over the 20-year sample for misdemeanor arrests for all adults between the ages of 18 and 24. Areas inside of the red lines indicate when school is not in session due to Thanksgiving and Christmas holidays and summer break.



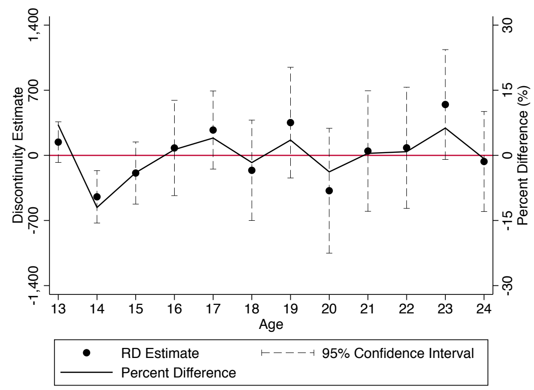
(a) Status Offense - School Year



(b) Status Offense - Summer

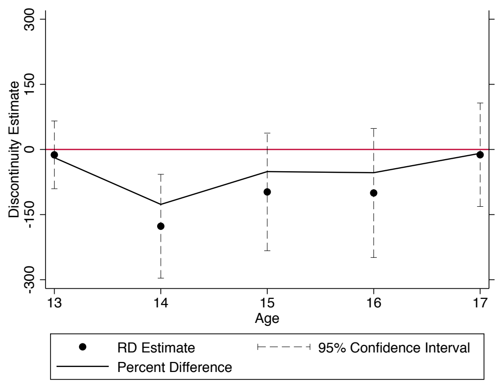


(c) Misdemeanor Offense - School Year

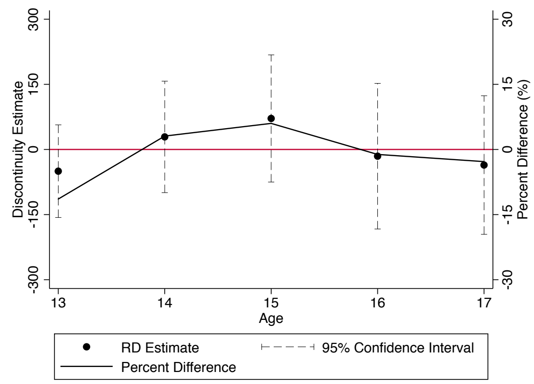


(d) Misdemeanor Offense - Summer

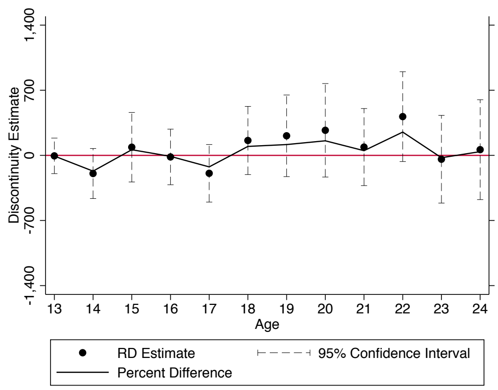
Figure 1.7: RD Plots By Age By Time of Year. Note: Each panel shows a figure of estimated discontinuities in arrest rates per 100,000 people at the cutoff to begin school for each age shown using a 60-day bandwidth for the corresponding level of crime and time of year. Each dot represents the estimated discontinuity at the cutoff. Dashed lines represent 95% confidence intervals. The solid line represents the percentage difference at the cutoff for each age.



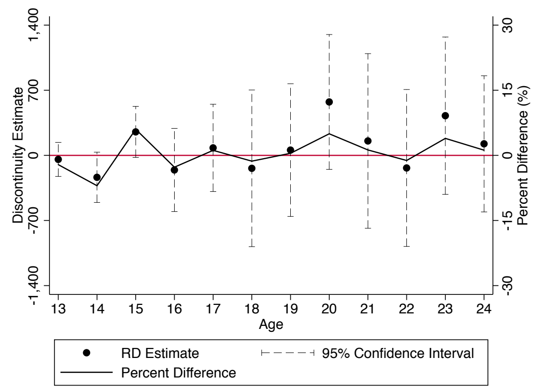
(a) Status Offense - School Year: Week



(b) Status Offense - School Year: Weekend



(c) Misdemeanor Offense - School Year: Week



(d) Misdemeanor Offense - School Year: Weekend

Figure 1.8: RD Plots By Age By Time of Year By Time of Week. Note: Each panel shows a figure of estimated discontinuities in arrest rates per 100,000 people at the cutoff to begin school for each age shown using a 60-day bandwidth for the corresponding level of crime and time of year and time of week during the school year. Each dot represents the estimated discontinuity at the cutoff. Dashed lines represent 95% confidence intervals. The solid line represents the percentage difference at the cutoff for each age.

Table 1.1: Arrest Distribution

	Juveniles (1)	Adults (2)	All (3)
Property Crime	397,580	402,876	800,726
	(26.61)	(12.77)	(17.22)
Violent Crime	107,184	233,858	341,042
	(7.17)	(7.41)	(7.34)
Felony Offenses	465,024	1,016,735	1,481,759
	(31.12)	(32.23)	(31.87)
Misdemeanor Offenses	835,554	2,138,237	2,973,791
	(55.92)	(67.77)	(63.96)
Status Offenses	193,615		
	(12.96)		
Total Arrests	1,494,193	3,154,972	4,649,165

Note: Each column represents the number of arrests within each type or level of crime for the corresponding row for each age group for those born within 60 days of the cutoff. Juveniles refer to individuals between the ages of 13 and 17, while adults refer to individuals between the ages of 18 and 24. Percentages of each measure are in parentheses. Property crime arrests are for burglary, larceny, auto theft, and arson. Violent crime arrests are for murder, manslaughter, rape, robbery, and aggravated assault. A felony arrest can result in the arrestee being sentenced to state prison if convicted and a misdemeanor arrest can result in the arrestee being sentenced to county jail, paying a fine, restitution, or probation. Only juveniles can be arrested for status offenses, which include loitering/breaking curfew, incorrigibility, truancy, and being classified as a runaway. The percentages shown do not equate to 100 percent as property crime and violent crime are subsets of felony and misdemeanor offenses.

Table 1.2: Effect of School Entry on Criminal Behavior

	All Crime	Property Crime	Violent Crime	Felony Offenses	Misdemeanor Offenses	Status Offenses
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Juveniles						
Estimate	-81.26	36.33	-20.46	-20.08	-18.88	-42.31
	(173.07)	(57.76)	(21.23)	(70.31)	(90.42)	(26.36)
Constant	9,491.91	2,495.86	687.86	2,963.99	5,291.34	1,236.58
	(119.69)	(37.77)	(13.52)	(45.21)	(61.39)	(20.50)
Pct. Difference (%)	-0.86	1.46	-2.97	-0.68	-0.36	-3.42
Panel B: Adults						
Estimate	337.74	75.71	-8.44	84.69	253.05	
	(394.40)	(59.59)	(30.07)	(136.80)	(265.50)	
Constant	13,174.67	1,666.72	984.55	4,238.78	8,935.88	
	(235.37)	(41.98)	(20.28)	(96.90)	(143.92)	
Pct. Difference (%)	2.56	4.54	-0.86	2.00	-2.83	

Note: Estimates for juveniles refer to those aged 13 to 17, while estimates for adults refer to those aged 18 to 24. Each estimate represents the β_1 coefficient from Equation 1.1—the difference in arrest rates for those born after the cutoff relative to those born before. The dependent variable is the arrest rate per 100,000 people for the corresponding age range and type or level of crime based on the row and column, respectively. The percent difference is the percent difference in arrest rates at the cutoff, which was calculated by dividing the RD estimate by the constant—where the constant term also represents the mean arrest rate for those born before the cutoff since the running variable, the distance in days of an individual's birthday from the school entry cutoff, is re-centered around zero. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.3: Status Offenses - 14 Year Olds

	Truancy	Loitering/ Curfew Viol.	Runaway	Incorrigible
	(1)	(2)	(3)	(4)
Estimate	-49.21***	-30.69	-4.27	-10.24
	(12.77)	(21.78)	(27.66)	(11.80)
Constant	168.78	463.94	331.57	108.02
	(9.61)	(15.46)	(21.46)	(7.32)
Pct. Difference (%)	-29.16	-6.62	-1.29	-9.48

Note: Each column shows estimates for 14 year olds for the corresponding status offense using a 60-day bandwidth. Each estimate represents the β_1 coefficient from Equation 1.1—the difference in arrest rates for those born after the cutoff relative to those born before. The dependent variable is the arrest rate per 100,000 people for the corresponding status offense. The percent difference is the percent difference in arrest rates at the cutoff, which was calculated by dividing the RD estimate by the constant—where the constant term also represents the mean arrest rate for those born before the cutoff since the running variable, the distance in days of an individual's birthday from the school entry cutoff, is re-centered around zero. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.4: Misdemeanor Offenses - 14 Year Olds

	Petty Theft	Assault & Battery	Vandalism	Marijuana
	(1)	(2)	(3)	(4)
Estimate	-113.09***	-13.30	-54.56**	-4.05
	(38.25)	(32.09)	(26.97)	(16.08)
Constant	1,250.11	727.23	494.70	297.44
	(23.75)	(25.81)	(13.53)	(9.47)
Pct. Difference (%)	-9.05	-1.83	-11.03	-1.36

Note: Each column shows estimates for 14 year olds for the corresponding misdemeanor offense using a 60-day bandwidth. These crimes represent the more prevalent misdemeanors in terms of arrest volume. Each estimate represents the β_1 coefficient from Equation 1.1—the difference in arrest rates for those born after the cutoff relative to those born before. The dependent variable is the arrest rate per 100,000 people for the corresponding misdemeanor offense. The percent difference is the percent difference in arrest rates at the cutoff, which was calculated by dividing the RD estimate by the constant—where the constant term also represents the mean arrest rate for those born before the cutoff since the running variable, the distance in days of an individual's birthday from the school entry cutoff, is re-centered around zero. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.5: Status Offenses by Time of Year - 14 Year Olds

	School Year		Summer
	Week	Weekend	
	(1)	(2)	(3)
Truancy	-97.85***		
	(23.53)		
Before Cutoff Mean	311.08		
	(18.64)		
Pct. Difference (%)	-31.45		
Loitering/Curfew Violation	-50.01	45.90	-31.09
	(32.01)	(43.20)	(40.48)
Before Cutoff Mean	497.15	449.12	461.19
	(22.47)	(30.95)	(28.87)
Pct. Difference (%)	-10.06	10.22	-6.74
Incorrigible	-14.62	8.12	15.29
	(17.03)	(25.79)	(17.11)
Before Cutoff Mean	128.74	94.96	67.61
	(11.03)	(14.61)	(10.63)
Pct. Difference (%)	-11.36	8.55	22.61
Runaway	6.03	-10.43	-36.92
	(34.12)	(40.03)	(41.03)
Before Cutoff Mean	364.45	321.11	304.36
	(24.71)	(25.47)	(24.28)
Pct. Difference (%)	1.65	-3.25	-12.13

Note: Each column shows estimates for 14 year olds for the status offense given in each row during the relevant time of year using a 60-day bandwidth. Each estimate represents the β_1 coefficient from Equation 1.1—the difference in arrest rates for those born after the cutoff relative to those born before. The dependent variable is the arrest rate per 100,000 people for the corresponding status offense and time of year based on the row and column, respectively. The percent difference is the percent difference in arrest rates at the cutoff, which was calculated by dividing the RD estimate by the constant—where the constant term also represents the mean arrest rate for those born before the cutoff since the running variable, the distance in days of an individual's birthday from the school entry cutoff, is re-centered around zero. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.6: Misdemeanor Offenses by Time of Year - 14 Year Olds

	School Year		Summer
	Week	Weekend	
	(1)	(2)	(3)
Petty Theft	-57.08	-158.64*	-196.50**
	(49.64)	(83.82)	(81.60)
Before Cutoff Mean	1,226.41	1,333.70	1,274.18
	(36.74)	(61.79)	(46.89)
Pct. Difference (%)	-4.65	-11.90	-15.34
Assault & Battery	-26.73	-15.58	-41.72
	(52.39)	(40.44)	(52.52)
Before Cutoff Mean	1,022.44	829.30	492.26
	(42.12)	(32.65)	(42.22)
Pct. Difference (%)	-2.61	-1.88	-8.48
Vandalism	-64.84*	-78.03	-3.61
	(36.40)	(48.25)	(46.11)
Before Cutoff Mean	575.83	436.92	412.04
	(22.04)	(38.84)	(22.92)
Pct. Difference (%)	-11.26	-17.86	-0.88
Marijuana	0.63	11.52	-30.26
	(33.74)	(21.57)	(25.23)
Before Cutoff Mean	464.72	80.41	150.48
	(16.86)	(12.25)	(19.00)
Pct. Difference (%)	0.14	14.33	-20.11

Note: Each column shows estimates for 14 year olds for the misdemeanor offense given in each row during the relevant time of year using a 60-day bandwidth. These crimes represent the more prevalent misdemeanors in terms of arrest volume. Each estimate represents the β_1 coefficient from Equation 1.1—the difference in arrest rates for those born after the cutoff relative to those born before. The dependent variable is the arrest rate per 100,000 people for the corresponding misdemeanor offense and time of year based on the row and column, respectively. The percent difference is the percent difference in arrest rates at the cutoff, which was calculated by dividing the RD estimate by the constant—where the constant term also represents the mean arrest rate for those born before the cutoff since the running variable, the distance in days of an individual's birthday from the school entry cutoff, is re-centered around zero. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Chapter 2

School Quality, Student Performance, and Behavior

2.1 Introduction

As a way of measuring school performance, school districts and state education boards have enacted school accountability systems in order to determine the schools that may need to be improved. These accountability systems give educators and parents a picture of school quality. These systems place schools in different categories of quality based on a series of metrics ranging from test scores, attendance, graduation rates, and the improvement students make from one period to the next. Certain accountability systems reward schools that achieve certain ratings, while others may punish those who fail to meet certain standards, or some combination of the two. An important question stemming from these systems is what impact do these rating systems have on student performance and behavior. This paper will analyze the effect of a school accountability system implemented by CPS, the school district which governs over the public schools in the city of Chicago, has on student performance and behavior.

Starting in the 2007-2008 school year, CPS introduced the Performance, Remediation and Probation Policy (PRPP) as the District's school accountability system. Under this policy, schools receive a performance rating based on a combination of metrics including test scores, attendance, drop-out rates, graduation rates, student outcomes in advanced placement courses, and gains students make from one period to the next. Depending on where their performance rating falls, schools are placed in one of three categories—Level 1 (excellent standing), Level 2 (good standing), or Level 3 (probation). Schools that receive a Level 3 rating are placed on probation and are required to develop a plan on how they will improve their school's performance. In order to be removed from probation status, schools must increase their performance rating and achieve a Level 1 or 2 rating. Chronically low-performing schools that remain on probation for several years will face the possibility of principal removal, staff replacement, and school closure. This paper will compare outcomes of schools that fall on either side of the probation threshold by using a regression discontinuity (RD) design.

Other related papers have generally find that schools that receive poor ratings under locally- or state-based accountability systems show greater academic improvements relative to schools that receive slightly higher ratings (Figlio and Rouse 2006, Rebeck 2008, Chiang 2009, Rockoff and Turner 2010, Rouse et al. 2013, Deming et al. 2016), though Dee and Dizon-Ross (2017) do not find any difference in academic performance between schools separated by a performance threshold.

Despite the extensive literature that has examined the effect of accountability systems on academic performance, little work has been done related to this question as it pertains to non-academic consequences. Chiang (2009) finds no evidence of accountability pressure affecting absenteeism or disciplinary incidents and Holbein and Ladd (2017) find that schools that failed to make adequate yearly progress under No Child Left Behind leads to better attendance but also increases in student misbehaviors.

I find that schools that receive low performance ratings in year t show larger gains in their performance rating in year t+1 relative to schools that receive ratings just above the probation threshold in year t. However, though the schools that are placed on probation show slightly higher gains in the next year, they tend to remain on probation—usually for one of two reasons. Either the increase in rating was not large enough to get over the probation threshold or the school had been on probation for more than one year and has not earned a high enough performance rating for at least two years in a row. Also, I do not find evidence that receiving low performance ratings has any significant impact on student misconduct in the following school year.

These results are similar to the ones found by Jacob and Lefgren (2004), as they found under an older accountability system used by CPS, that increasing academic achievement in high-poverty schools is quite difficult. During the sample period of this paper, schools within CPS are still generally considered to be quite poor, with approximately 86% of students being eligible for free or reduced lunch.

The rest of the paper is organized as follows: Section 2.2 gives an overview of the PRPP. Section 2.3 provides a description of the data and the identification strategy. Section 2.4 presents the results. Section 2.5 concludes.

2.2 Background

The PRPP replaced an accountability system that CPS introduced in 1996 that relied solely on test scores. Under this system, schools with less than 15 percent of their students meeting national standards on standardized tests were placed on probation and given additional resources to help spurn improvement. Measuring the effect of this system on student performance, Jacob and Lefgren (2004) find that increases in teacher in-service training due to poor performance had no effect on student achievement.[1]

Under PRPP elementary schools in CPS receive a performance rating based on a series of eight metrics including: 1) the percent of students meeting/exceeding standards on the Illinois Standards Achievement Test (ISAT) reading section, 2) the percent of students meeting/exceeding standards on the ISAT mathematics section, 3) the percent of students meeting/exceeding standards on the ISAT science section, 4) the percent of students exceeding standards on the ISAT composite score, 5) the percent of students exceeding standards on the ISAT composite score for the school's highest grade level, 6) attendance rate, 7) value-added reading, 8) value-added mathematics.[2] Performance ratings for high schools in CPS are based on a series of eleven metrics including: 1) av-

[1]Under this old accountability system, schools that received poor performance ratings were given additional resources to improve their school quality, including schools that were not on probation but still considered low-performing since they were near the probation threshold. Instead, their results can be interpreted as the effect of gaining additional resources on student performance.

[2]Value-added measures the school's impact on student academic growth on the ISAT from one year to the next.

erage ACT score, 2) one year drop-out rate, 3) freshman on-track rate, 4) the percent of students meeting/exceeding standards on the Prairie State Achievement Examination (PSAE) reading section, 5) the percent of students meeting/exceeding standards on the PSAE mathematics section, 6) the percent of students meeting/exceeding standards on the PSAE science section, 7) attendance rate, 8) enrollment in advanced placement (AP) courses, 9) AP success, 10) gains in student performance on the EXPLORE, PLAN, ACT Series (EPAS) reading section, 11) gains in student performance on the EPAS mathematics section. [3]

Schools can receive a maximum of 42 performance points with each metric granting 0-3 points. Schools gain performance points through three different measures of the metrics listed previously—current status, trend, and student growth. Current status points are given for a metric based on the last two years' average. Trend points are given for a metric based on how the current year compares to the average of the previous three years. Student growth points are given by the value-added metrics for elementary schools, and by the EPAS gains metrics for high schools.

Elementary schools earn current status and trend points for the test score and attendance metrics, and high schools earn current status and trend points for the test score, drop-out rate, freshman on-track rate, and attendance metrics. High schools also earn trend points from the AP enrollment and AP success metrics. Elementary schools

[3] The freshman on-track rate measures how many first-time freshmen are, by the end of their first year, on track to graduate from high school within four years. The gains in EPS measure represents the percentile rank of the school among CPS schools in terms of the percentage of students making expected gains on the EPAS from one test to another. High school students take the EXPLORE exam during the 9th grade, the PLAN exam during the 10th grade, and the ACT exam during the 11th grade.

can earn student growth points for the value-added metrics, and high schools can earn student growth points for the EPAS gains metrics. Tables 2.1 and 2.2 further describe the details of how schools receive performance points for each of the metrics.

Based upon the percentage of the 42-point maximum schools receive, they are placed into three different levels. For elementary schools, schools that receive 71% or more of the maximum receive a Level 1, or excellent standing, rating, schools that receive between 50% and 70.9% of the maximum receive a Level 2, or good standing, rating, and schools that receive 50% or less of the maximum receive a Level 3, or probation, rating. For high schools, schools that receive 66.7% or more of the maximum receive a Level 1, schools that receive between 44% and 66.6% of the maximum receive a Level 2 rating, and schools that receive 44% or less of the maximum receive a Level 3 rating. Schools that receive a Level 3 rating are placed on probation in the following academic year. Elementary schools with less than 50% of their students meeting/exceeding standards on the ISAT composite score and high schools with less than 10% of their students meeting/exceeding standards on the PSAE composite score are automatically placed on probation regardless of their overall performance policy rating.

Being placed on probation requires schools to create a plan which outlines how they intend on improving the quality of their school. This plan could include changes to the school budget, curriculum, staff, or other options that are aimed at improving the school's performance rating. In order to get a sense of the specific changes schools made in order to improve the quality of their school, I examined the Continuous Improvement Work Plans (CIWPs) that schools submitted to CPS in 2011. These CIWPs outline the strategic priorities each school will focus on for the next three academic years. I find that in these CIWPs, schools attempt to make a wide range of changes in order to improve the quality and performance of their schools. For example, many schools implemented new curricula that involved aligning their teaching philosophy with Common Core Standards, providing support and additional training for teachers, improving school climate by implementing non-cognitive training for students and teachers, and employing programs that help prepare students for college and beyond.

In order to be removed from probation, schools must improve their performance rating to Level 2 or Level 1. If a school has been on probation for only one year, then achieving a Level 2 or Level 1 rating will remove them from probation; however, if a school has been on probation for more than one year, then it must achieve a higher level rating for two consecutive years in order to be removed from probation status.

2.3 Data and Identification Strategy

2.3.1 Data

Data on PRPP points and school behavior comes directly from the CPS website and is publicly available. Data on PRPP points and the underlying metrics spans from 2008 to 2013, while data on in-school behavior spans the 2012 and 2013 school years. The dataset contains information on each of the metrics for 474 schools—with 398 being elementary schools and 76 being high schools. Data on in-school behavior includes the number of student misconducts per 100 students, the number of in-school suspensions per 100 students, the number of out-of-school suspensions per 100 students, the number of police notifications per 100 students, and the number of expulsions per 100 students. As defined by CPS, the number of student misconducts are the total number of times in a school year that a school reports behaviors that violate the Student Code of Conduct. This is an overarching measure of student misbehavior and can include a wide range of behaviors both serious and non-serious. Thus, I use the suspension rates as a proxy for less serious student misbehavior and the police notification and expulsion rates for more serious student misbehavior. The police notifications rate measures the rate at which school administrators contacted police to respond to a disciplinary incident on school grounds. A description of the PRPP points and each of the metrics can be found in an earlier section of the paper.

Table 2.3 gives descriptive statistics regarding demographic information and student misconduct for all schools in the dataset. The information in Table 2.3 would indicate that schools in CPS have a predominantly African-American and Hispanic population, where nearly 90% of students are eligible for free/reduced lunch—indicating that many of the students in these schools come from low-income households. Also, the rate of student misconducts per 100 students is much higher for high schools than elementary schools. In their CIWPs, many high schools noted the need to foster a better school climate through decreasing the number of student misconducts.

2.3.2 Identification Strategy

The aim of this paper is to compare outcomes of schools on either side of the probation threshold by using an RD design. The use of an RD design allows for the comparison of two groups separated by some threshold where one group receives a treatment and the other does not—in lieu of being able to conduct a randomized experiment. In this particular case, the treatment is that schools that fail to reach a certain performance rating on placed on probation and face certain consequences if the school does not improve. As long as schools do not have precise control over the assignment variable, where their performance rating falls relative to the probation threshold, an RD design can be similar to conducting a locally randomized experiment (Lee and Lemieux 2010). Figure 2.1 shows the histogram of the performance ratings received by schools under PRPP—with the ratings re-centered around the probation threshold. Figures 2.2 and 2.3 show histograms for elementary and high schools, respectively. These figures indicate that there is no discrete change of the running variable, the performance rating relative to the probation threshold, at the probation threshold—which is evidence that there is likely no manipulation of the performance rating.

I estimate the effect of being placed on probation due to a school's relative performance rating by using the following equation:

$$Y_{i,t+1} = \beta_0 + \beta_1 Prob_{i,t} + \beta_2 PRR_{i,t} + \beta_3 Prob_{t,i} x PRR_{t,i} + \beta_4 PRR^2_{t,i} + \beta_5 Prob_{t,i} x PRR^2_{t,i} + \epsilon_{t,i} \quad (2.1)$$

where $Y_{i,t+1}$ represents the outcome of interest for all schools i percentage points away from the threshold in time t+1. $Prob_{i,t}$ represents an indicator variable that takes a value of 1 if a school's performance rating is above the probation threshold in time t. $PRR_{i,t}$ represents the performance rating a school receives relative to the probation threshold in time t. For example, an elementary school that receives a rating that is 51% of the performance rating maximum would get a value of +1, and a high school that receives a rating that is 43% of the performance rating maximum would receive a value of -1. The inclusion of the interaction terms allows for the function to differ on either side of the cutoff (Lee and Lemieux 2010). The coefficient of interest here is β_1, which represents the difference in the outcome of interest at the probation threshold.

I estimate two types of outcomes using Equation (2.1). The first type of outcome is academic-related and tied to the performance rating, and the second type is non-academic and unrelated to performance rating. The academic outcomes consist of the change in each of the metrics of the performance rating and the performance rating itself. That is, for the academic outcomes I estimate Equation (2.1) where the outcome is each of the eight metrics for elementary schools and each of the eleven metrics for high schools, as well as the overall performance rating. Further, when estimating Equation (2.1) for each of the metrics, I also separate the change in each metric by the three measures on how schools receive performance points—through current status points, trend points, and growth points. The non-academic outcomes are related to student misconducts, which have no bearing on the performance rating that schools receive. While these behavioral outcomes are not directly related to the performance rating a school receives, many schools stated in their CIWP that part of improving the quality of their school involved creating a more positive school climate and reducing student misconduct. This is especially true for high schools, as evidenced by Table 2.3 that shows that student misconducts are quite frequent relative to elementary schools.

2.3.3 First Stage Estimates

In order for this identification strategy to be viable, schools must be assigned to probation status when the next school year starts based on where there performance rating falls. That is, if a school is assigned to Level 3, then they would need to be placed on probation in the next school year. Figure 2.4 displays the probability of a school receiving probation status in the next school year based on their performance rating in the current year. All schools to the left of the threshold in this figure are placed on probation because their performance rating was below the probation threshold and many of the schools to the right of the threshold are not on probation due to achieving a Level 2 or 1 rating. This figure shows that, based on the performance rating they receive in year t, schools to the left of the probation threshold are approximately 60% more likely to be on probation in year t+1 relative to schools to the right of the threshold. At first glance, this may seem to indicate that there are schools receiving high enough performance ratings, yet still being placed on probation—which may seem to violate the policy. However, as stated previously, under PRPP if a school has been on probation for more than one year it must earn a Level 1 or 2 rating for two consecutive years in order to be removed from probationary status. Given there are still some schools that receive probation status despite being to the right of the threshold, I split schools into two categories—those on probation in year t and those not on probation in year t. Figures 2.5 and 2.6 display the probability of a school being placed on probation based on their performance rating for schools that were not on probation in year t and for schools that were on probation in year t, respectively. These figures would indicate that the assignment to probation status has been implemented correctly according to the policy. From Figure 2.5, for schools that were not on probation in year t, the only schools to be placed on probation in year t+1 are schools with performance ratings below the probation threshold, while no schools above the threshold were placed on probation. Figures 2.7 through 2.12 display the assignment to probation status for elementary and high schools separately. Table 2.4 gives RD estimates corresponding to Figures 2.4 through 2.12.

2.4 Results

2.4.1 Academic Outcomes

For schools that are placed on probation, the goal is to improve their school's performance rating in the next year. The first set of estimates I present here examine how much the performance rating of schools change from one year to the next for schools on either side of the probation threshold. Figure 2.13 displays how schools relative performance rating changes in the next year. Figures 2.14 and 15 display the same for elementary schools and high schools, respectively. Table 2.5 gives the RD estimates for these figures. Figure 2.13 shows that schools that fall just below the probation threshold in year t show slightly more improvement in year t+1 relative to schools just over the threshold. Schools that were just below the probation threshold increase their relative performance rating by approximately 3.2 more percentage points in the following year relative to schools just above the threshold.

From Figures 2.14 and 2.15 it can be seen that the majority of the improvement in relative performance rating from probation schools can largely be attributed to gains experienced by elementary schools. Figure 2.14 shows that elementary schools just below the probation threshold in year t increase their relative performance rating by approximately 4 more percentage points in the following year relative to schools just above the threshold. Figure 2.15 shows that high schools just below the probation threshold in year t increase their relative performance rating by approximately 1 more percentage point in the following year relative to schools just above the threshold, though this difference is statistically insignificant and from visual inspection seems to be nearly zero.

Given that the schools just to the left of the probation threshold show a modest improvement in their performance rating relative to the schools just to the right of the threshold, I next examine if there are particular ares in which these schools are improving among the metrics that determine their performance rating. Under PRPP, schools receive performance points based on three measures of each of the metrics—current status, trend, and growth points. Examining the change in raw scores of the metrics may not give an appropriate picture of how the metrics affect the performance rating a school receives. Thus, I estimate Equation (2.1) for the change in each metric associated with the performance rating as it pertains to each particular measure.

Tables 2.6 and 2.7 show how each of the metrics contributed to the change in performance rating based on current status, trend, and growth points earned for elementary schools and high schools, respectively. As stated earlier, the increase in performance rating as seen in Figure 2.13 can be mostly attributed to gains experienced by elementary schools. RD estimates from Table 2.6 show that elementary schools that received performance ratings below the probation threshold increased their performance rating in the next year by gaining more current status points from the ISAT composite score metric, gaining more trend points from the ISAT science score metric, and gaining more growth points from the value-added mathematics score relative to schools that just missed being put on probation. RD estimates from Table 2.7 show that high schools on either side of the probation threshold are not much different in the change from one period to the next. Schools that received low performance ratings did gain more current status points from their PSAE science metric relative to those above the threshold; however, the difference and overall score change is nearly zero.

Increasing by a certain amount of percentage points is not going to automatically remove a school from probationary status. This increase must be sufficient enough for a school to move into a Level 2 or Level 1 rating. For example, if a school's performance rating falls 10 percentage points below the threshold and they only improve by 9 percentage points in the following year, then, despite their increase, they would still receive a Level 3 rating. Approximately 27 percent of the time, schools that received a Level 3 rating in year t, increased their performance rating enough to achieve at least a

Level 2 rating in year t+1. Figure 2.16 illustrates this graphically, where the schools to the left of the probation threshold and above the diagonal line increased their rating in the next period to be above the threshold.

If a school on probation were to increase their performance rating enough to receive a Level 2 rating, they may still remain on probation if they had been on probation for more than one year and the Level 2 rating had not been achieved for two consecutive years. Of the cases when a school increases their performance rating in the next period enough to lift their probation status, in 70% of instances schools are still on probation two periods later.[4] Also, it is more likely that elementary schools that fit this criteria are less likely to be on probation two periods later compared to high schools. That is, for elementary schools that increase their performance rating in the next school year enough to be taken off of probation, in 68% of cases are schools still on probation two years later; whereas for high schools, that number is 87%. High schools tend to have more trouble improving the quality of their school compared to elementary schools, as nearly 40% of the high schools in the sample were on probation for the entire sample period compared to 18% for elementary schools.

In some cases, schools remain on probation because they did not meet the minimum requirement and some were on probation under the previous accountability system and had not achieved a high enough rating for two consecutive years. While there is some evidence here of improvement from year to year, many schools remain on probation despite increasing their performance rating.

[4] The performance rating a school achieves in time t+1 determines its probation status at time t+2.

2.4.2 Behavioral Outcomes

Though not tied to the performance rating, it was the aim of some schools to improve the school climate and decrease student misconducts through non-cognitive training and relationship building between teachers and students. By fostering a positive school environment, they hope to create a better space for students to focus on improving academically. This is especially true for high schools, as the student misconduct rate is quite high as seen in Table 2.3.

Table 2.8 shows how some non-academic outcomes may change across the probation threshold. The estimates display the effect of receiving a low performance rating on in-school behavior. Similar to the academic outcomes, I examine what happens to student behavior in the year after a school receives their performance rating. Specifically, how the number of student misconducts per 100 students, the number of in-school and out-of-school suspensions per 100 students, the number of police notifications per 100 students, and the number of expulsions per 100 students change following receiving a certain performance rating. Overall, these estimates show that student behavior is not affected by a school being on either side of the probation threshold. Although Column (5) does show that schools above the probation threshold that received satisfactory performance ratings in year t have fewer expulsions relative to schools below the threshold in year t+1, the magnitude is quite small and nearly zero. From Column (1), though insignificant, the magnitude of the estimate for the student misconduct rate for high schools is rather large—particularly when compared to elementary schools. This speaks to why many high schools outlined in their CIWPs that curbing student misconduct is a priority in improving their school's overall quality.

Given these results it is important to note here that I only have two years of behavior data; thus, yielding a smaller sample size relative to the academic outcomes and making these estimates less precise.

2.5 Conclusion

In this paper, I examine the effect of a school accountability system implemented by CPS on academic and behavioral outcomes. This accountability system placed schools on probation if they failed to meet a certain performance threshold. In order to be removed from probation, schools must raise their performance rating to be above this threshold. Further, if a school has been on probation for more than one year, then it must raise and maintain a performance rating above the probation threshold for at least two years to be removed from probation. First stage estimates show that under PRPP, schools were properly assigned to probation status based on where their performance rating falls relative to the probation threshold. Using an RD design, I find that schools that fail to reach this probation threshold increase their performance rating to a slightly higher degree relative to schools who just miss being placed on probation. However, in many cases despite improving the quality of their school by raising their performance rating, many schools remain on probation due to either not improving enough to reach over the probation threshold or they had been on probation for more than one year and had not improved their performance for at least two years. Also, I do not find evidence that receiving a performance rating below the probation threshold has any meaningful impact on in-school student misconduct, though these estimates are much less precise due to a lack of observations.

The results found here follow closely with those found by Jacob and Lefgren (2004), who found that under the previous accountability system CPS used, increases in teacher in-service training due to poor performance ratings had no effect on student performance. They argue that increasing student performance in high-poverty areas, such as the one that encompasses CPS, is difficult and modest investments into staff training may not be enough to significantly impact student's academic achievement. It could be that there are significant gains being made by the worst students, but that would not be seen with the data here that is at the school level. Further studies may uncover more of an impact with data for individual students.

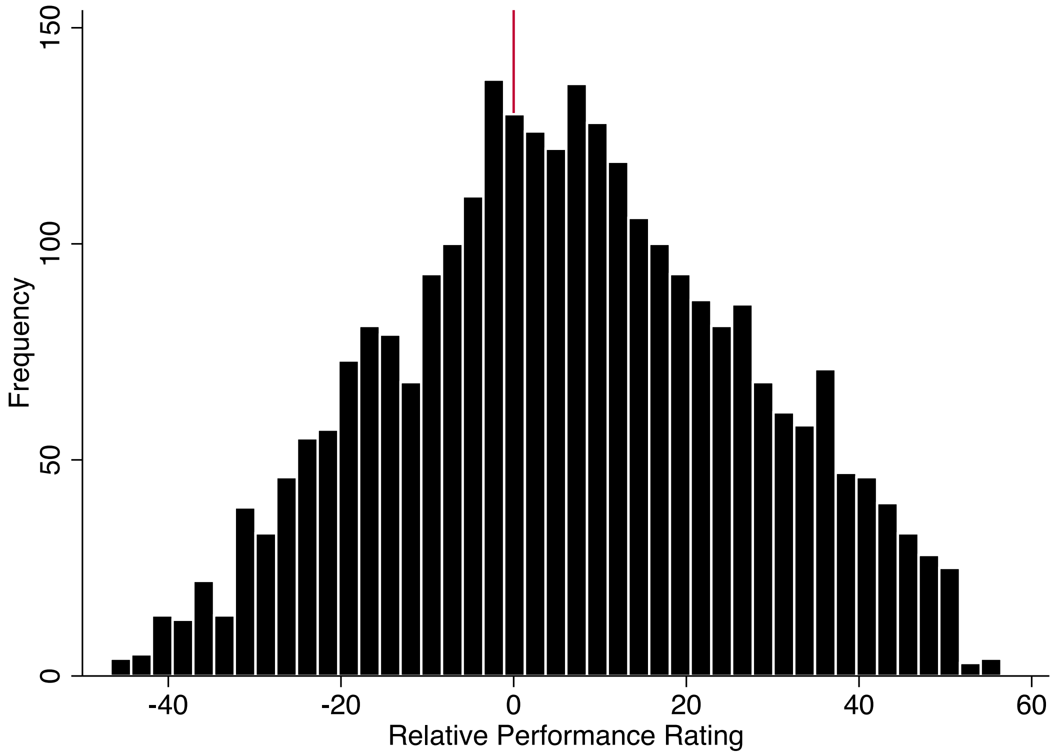


Figure 2.1: Histogram of relative performance rating for all schools. Note: This figure re-centers all performance ratings around the probation threshold, where schools that fall to the left of the threshold receive a Level 3 rating and schools that fall to the right receive a Level 2 or Level 1 rating. Bin width is 2.4 relative performance rating points.

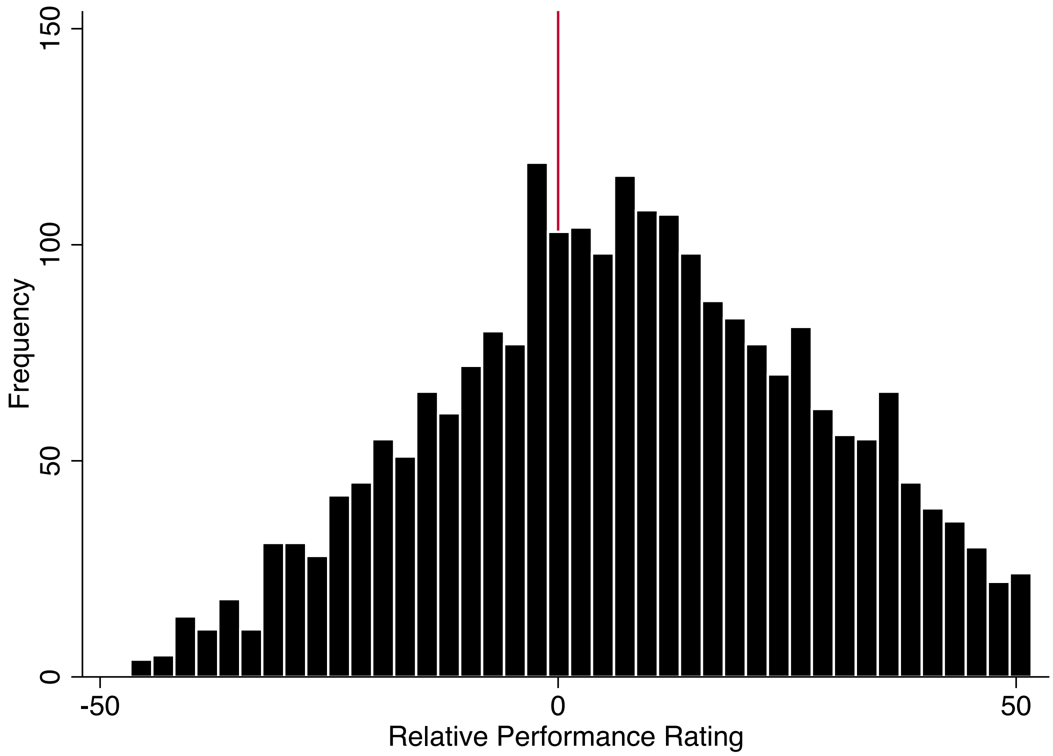


Figure 2.2: Histogram of relative performance rating for elementary schools. Note: This figure re-centers all performance ratings around the probation threshold, where schools that fall to the left of the threshold receive a Level 3 rating and schools that fall to the right receive a Level 2 or Level 1 rating. Bin width is 2.4 relative performance rating points.

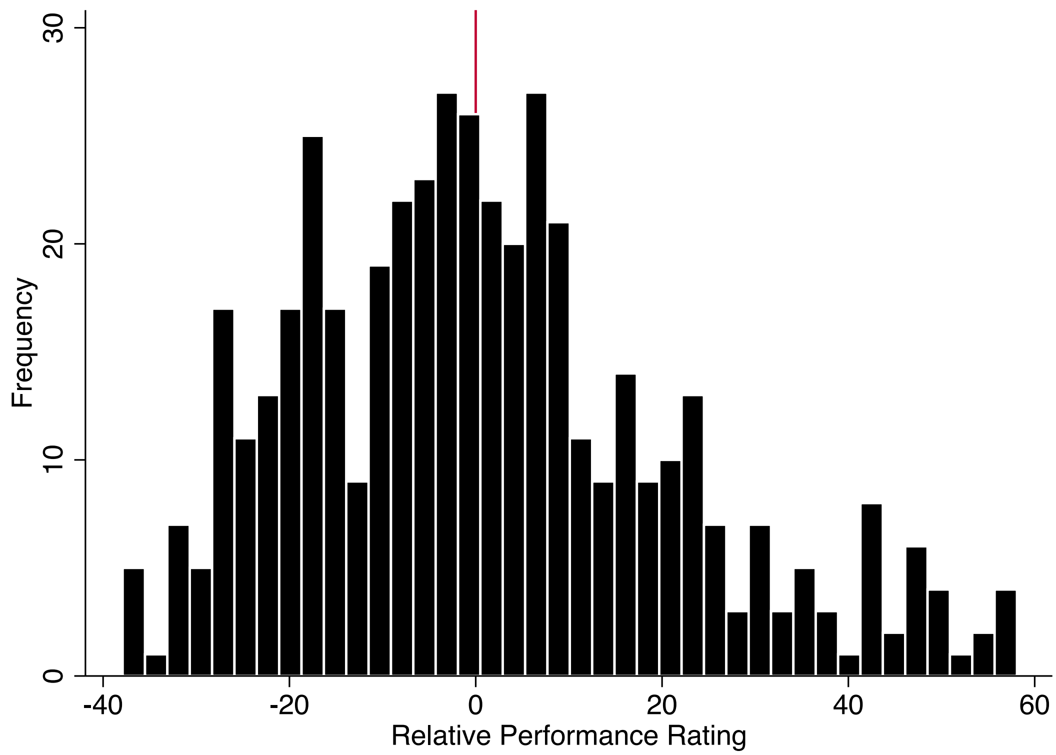


Figure 2.3: Histogram of relative performance rating for high schools. Note: This figure re-centers all performance ratings around the probation threshold, where schools that fall to the left of the threshold receive a Level 3 rating and schools that fall to the right receive a Level 2 or Level 1 rating. Bin width is 2.4 relative performance rating points.

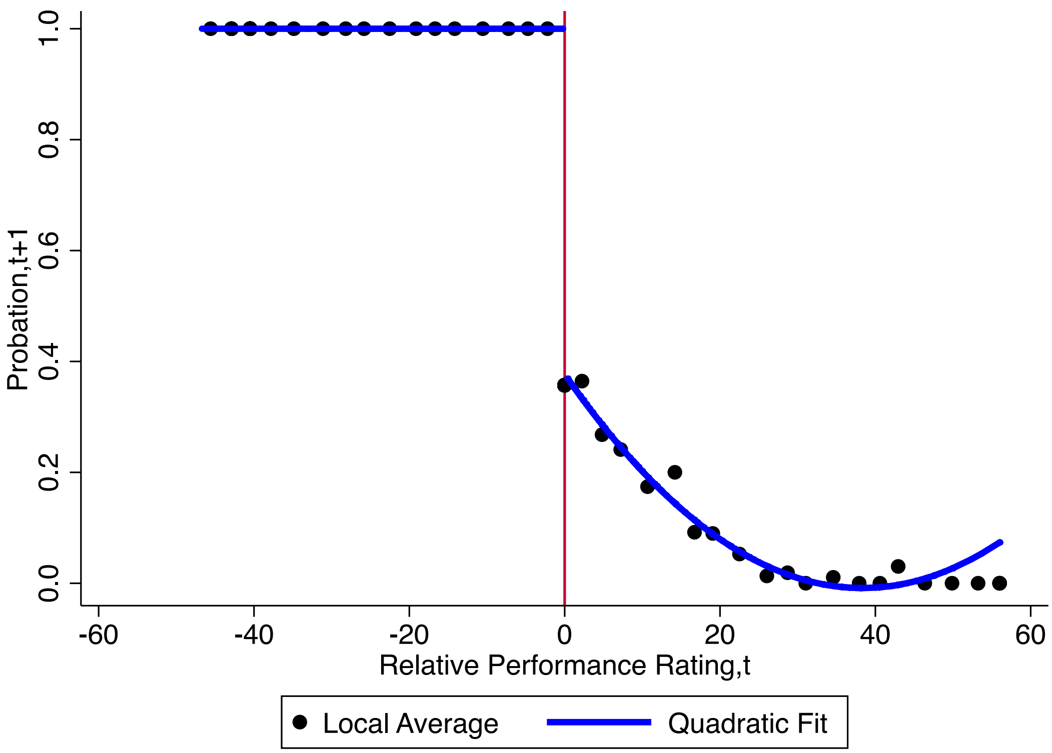


Figure 2.4: Probation Status in Time t+1 by Performance Rating in Time t. Note: Each dot represents the average probationary status in 3-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold. Each line represents the fitted values of Equation 1 for each side of the probation threshold.

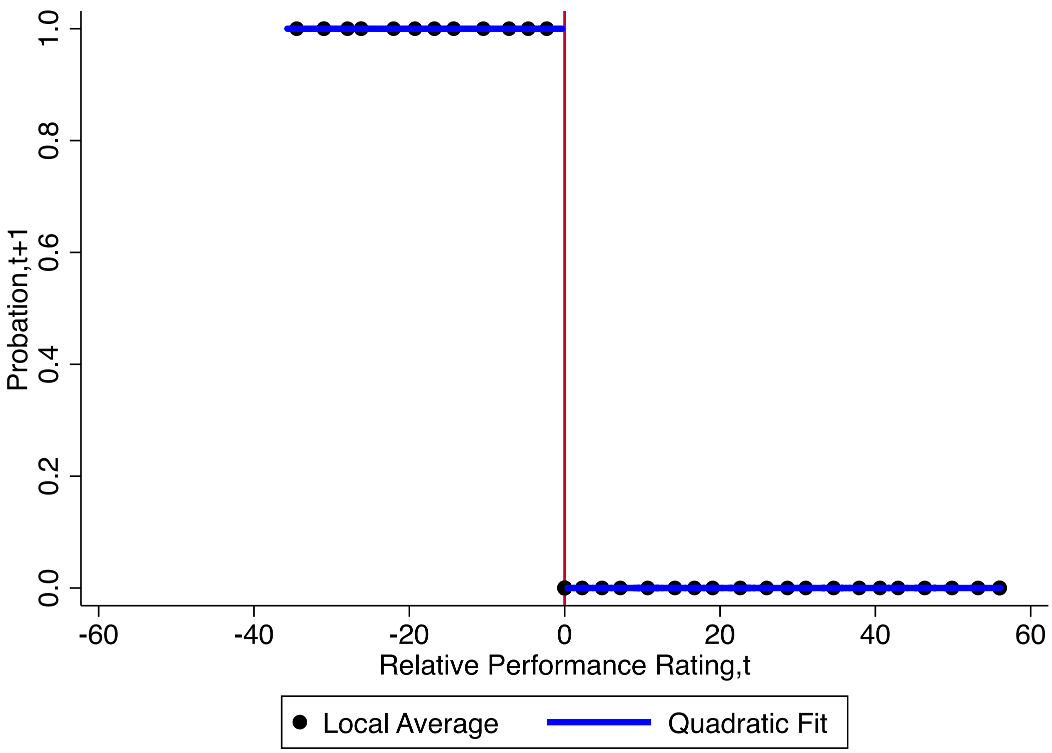


Figure 2.5: Probation Status in Time t+1 by Performance Rating in Time t for Schools Not on Probation in Time t. Note: Each dot represents the average probationary status in 3-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold. Each line represents the fitted values of Equation 1 for each side of the probation threshold.

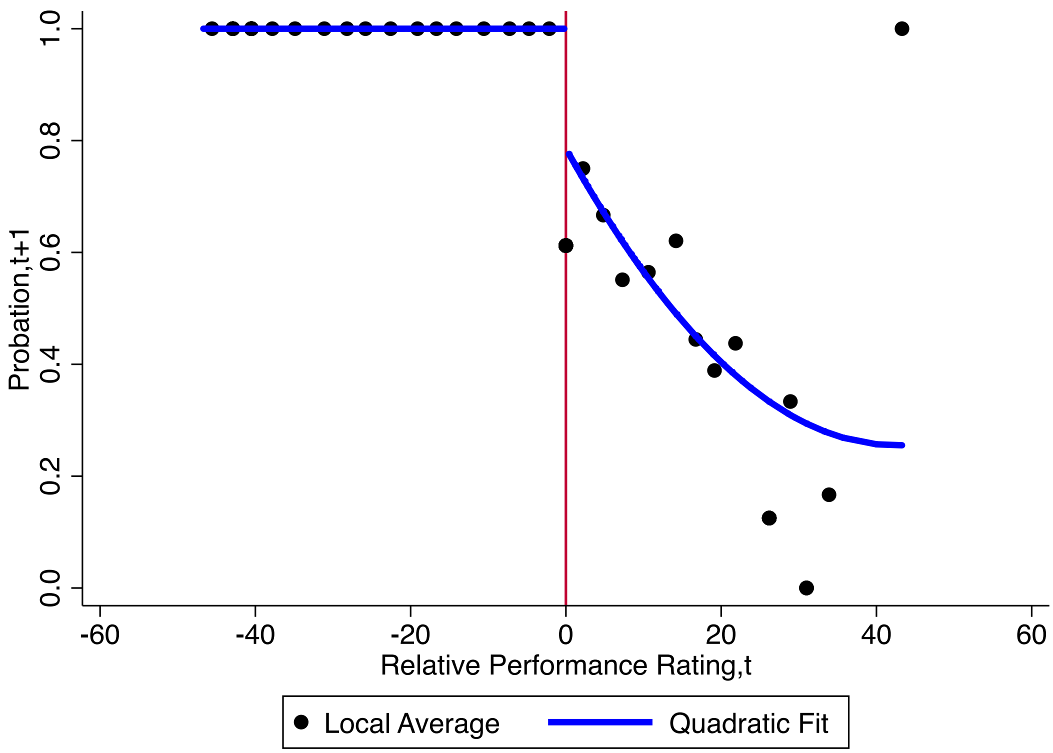


Figure 2.6: Probation Status in Time t+1 by Performance Rating in Time t for Schools on Probation in Time t. Note: Each dot represents the average probationary status in 3-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold. Each line represents the fitted values of Equation 1 for each side of the probation threshold.

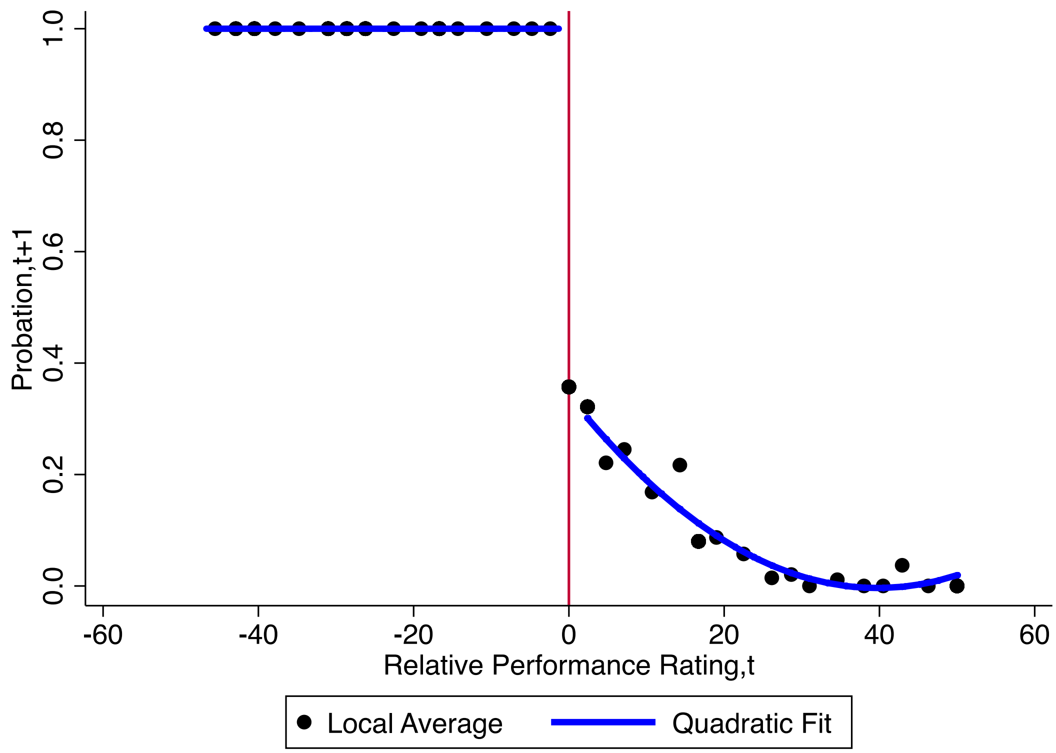


Figure 2.7: Probation Status in Time t+1 by Performance Rating in Time t—Elementary Schools. Note: Each dot represents the average probationary status in 3-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold. Each line represents the fitted values of Equation 1 for each side of the probation threshold.

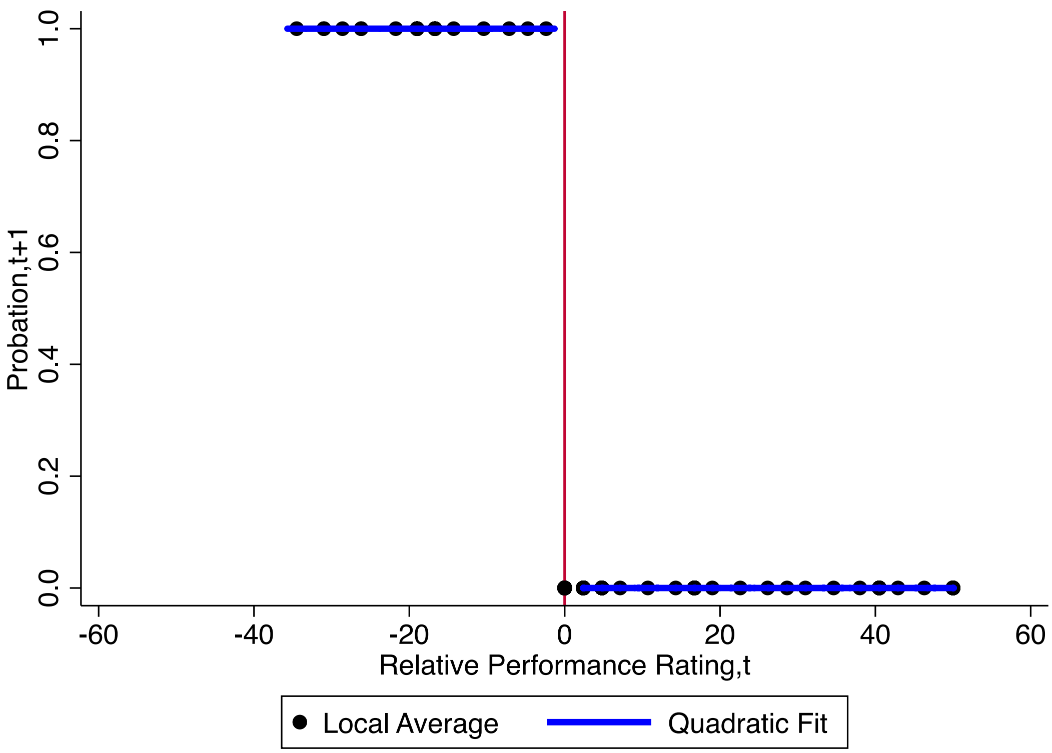


Figure 2.8: Probation Status in Time t+1 by Performance Rating in Time t for Schools Not on Probation in Time t—Elementary Schools. Note: Each dot represents the average probationary status in 3-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold. Each line represents the fitted values of Equation 1 for each side of the probation threshold.

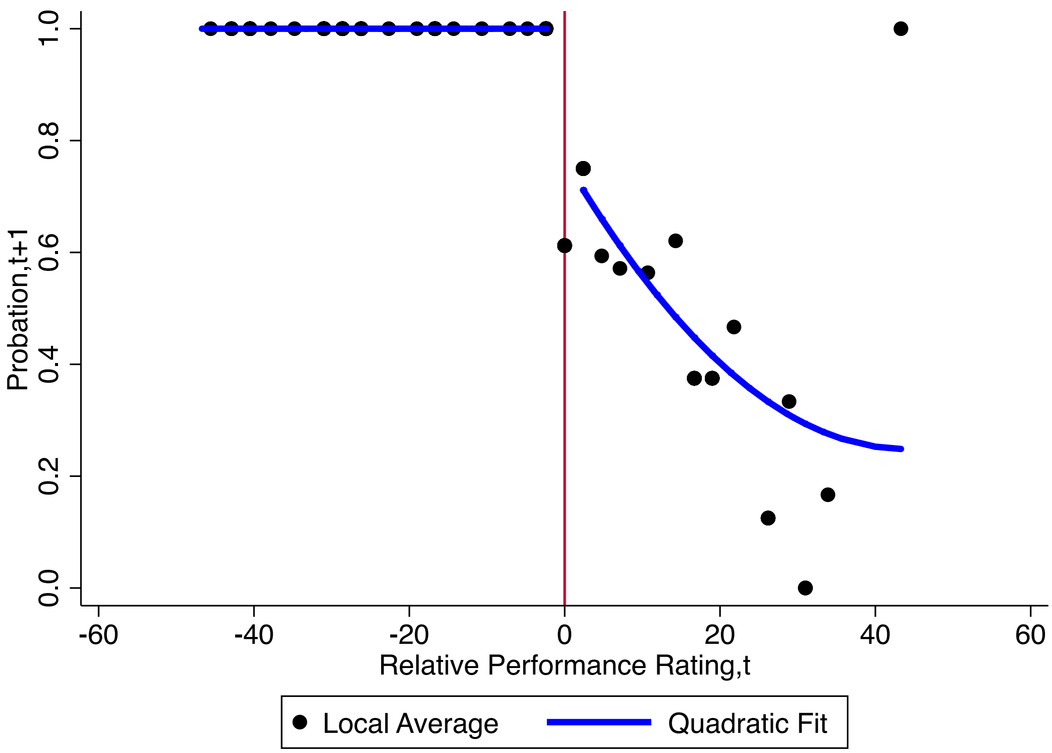


Figure 2.9: Probation Status in Time t+1 by Performance Rating in Time t for Schools on Probation in Time t—Elementary Schools. Note: Each dot represents the average probationary status in 3-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold. Each line represents the fitted values of Equation 1 for each side of the probation threshold.

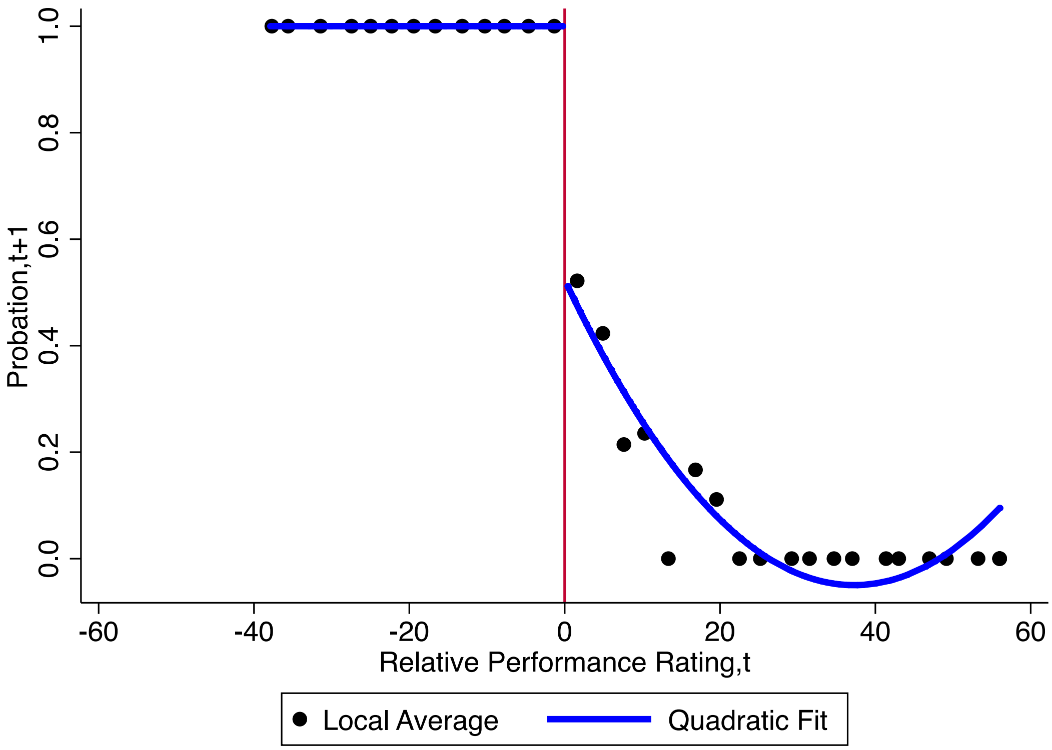


Figure 2.10: Probation Status in Time t+1 by Performance Rating in Time t—High Schools. Note: Each dot represents the average probationary status in 3-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold. Each line represents the fitted values of Equation 1 for each side of the probation threshold.

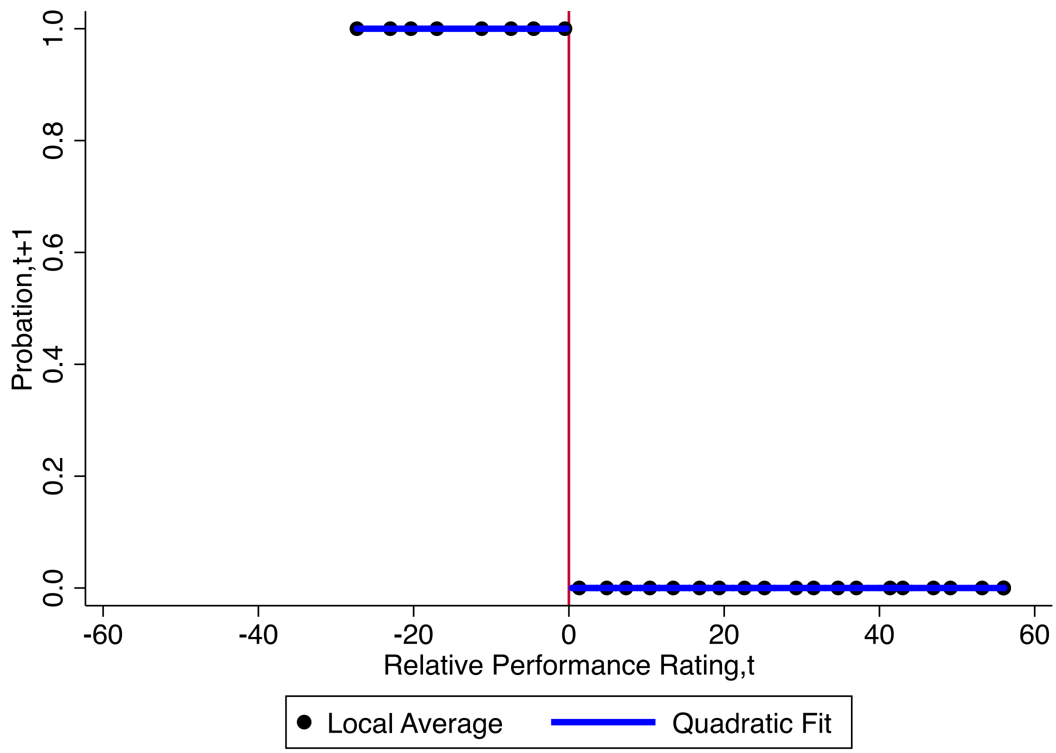


Figure 2.11: Probation Status in Time t+1 by Performance Rating in Time t for Schools Not on Probation in Time t—High Schools. Note: Each dot represents the average probationary status in 3-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold. Each line represents the fitted values of Equation 1 for each side of the probation threshold.

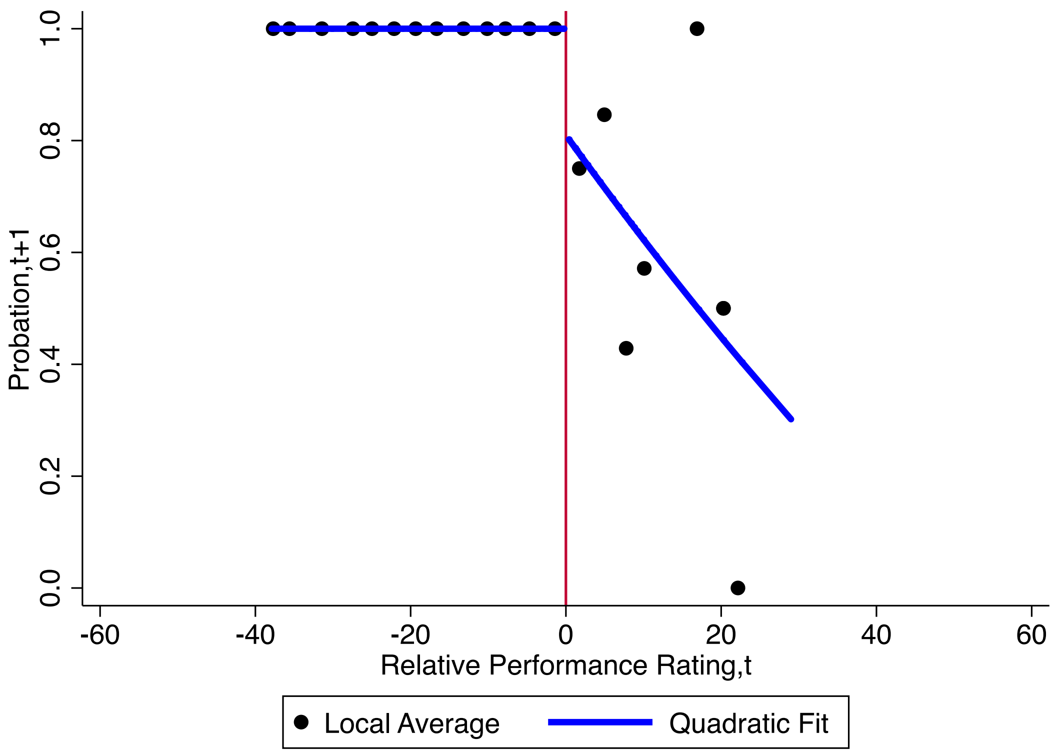


Figure 2.12: Probation Status in Time t+1 by Performance Rating in Time t for Schools on Probation in Time t—High Schools. Note: Each dot represents the average probationary status in 3-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold. Each line represents the fitted values of Equation 1 for each side of the probation threshold.

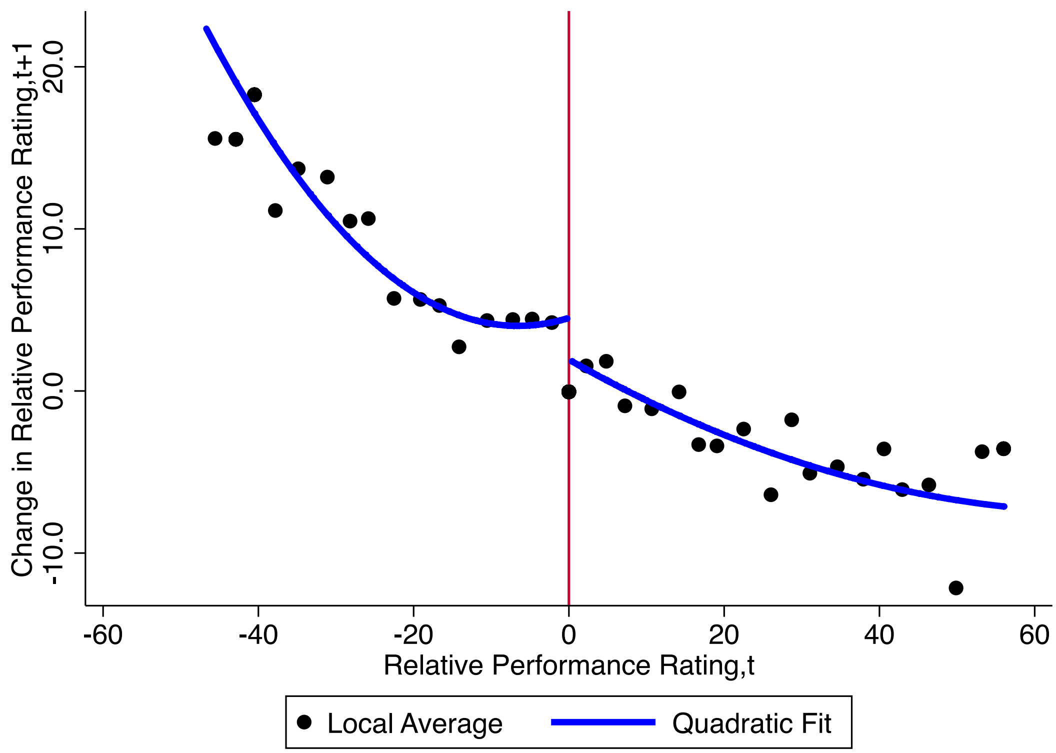


Figure 2.13: Change of Performance Rating in Time t+1 from Time t. Note: Each dot represents the average change in performance rating in 3-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold. Each line represents the fitted values of Equation 1 for each side of the probation threshold.

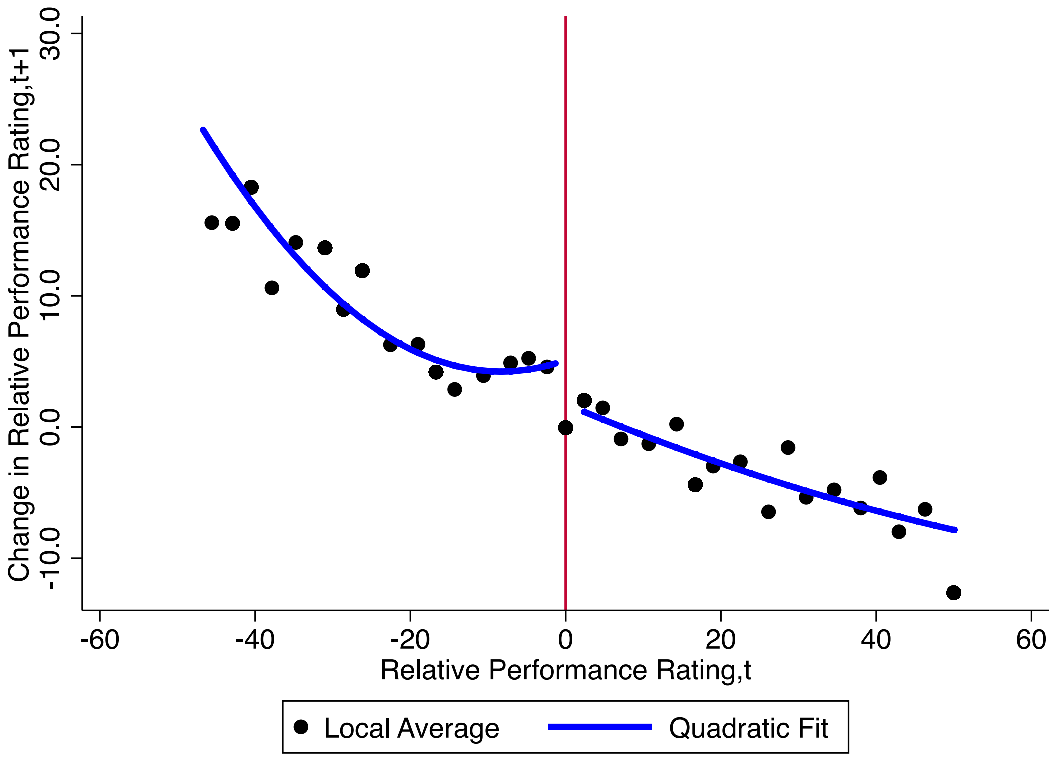


Figure 2.14: Change of Performance Rating in Time t+1 from Time t—Elementary Schools. Note: Each dot represents the average change in performance rating in 3-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold. Each line represents the fitted values of Equation 1 for each side of the probation threshold.

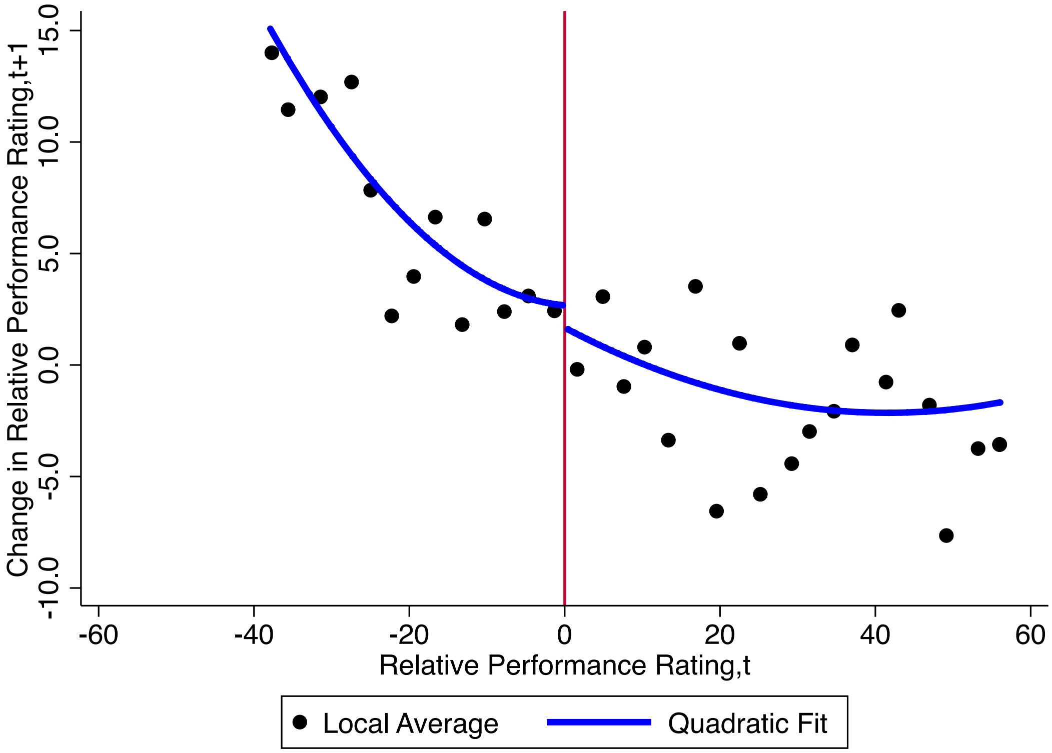


Figure 2.15: Change of Performance Rating in Time t+1 from Time t—High Schools. Note: Each dot represents the average change in performance rating in 3-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold. Each line represents the fitted values of Equation 1 for each side of the probation threshold.

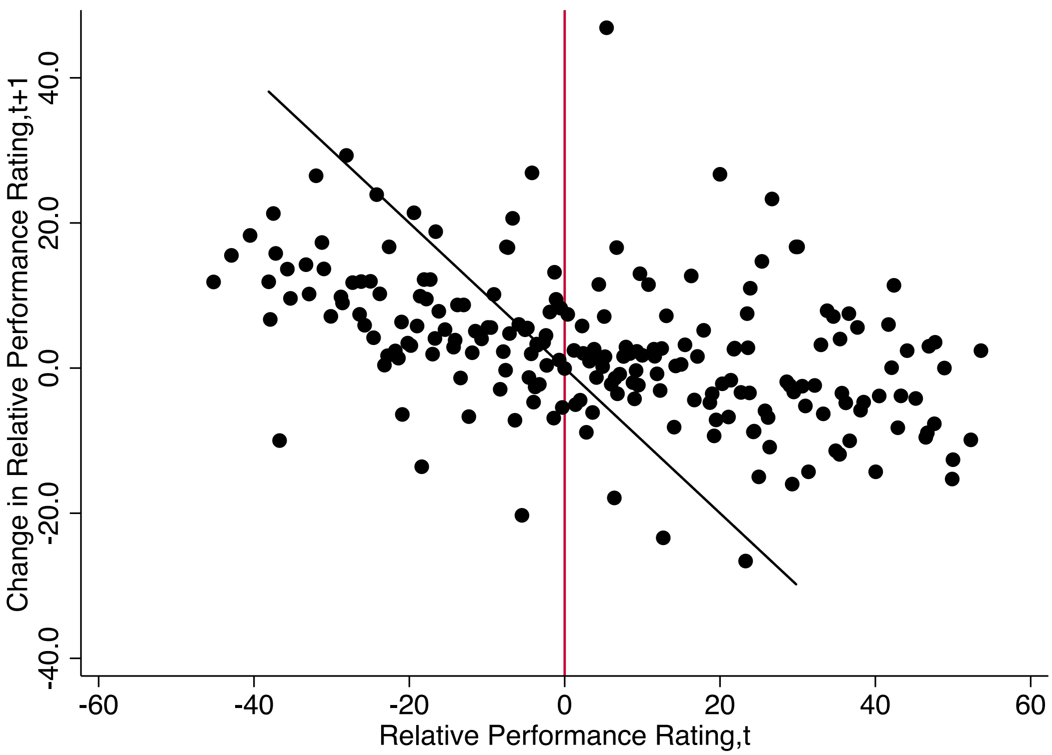


Figure 2.16: Change of Performance Rating in Time t+1 from Time t—Note: Each dot represents the average change in performance rating in 0.5-percent blocks of relative performance rating—how far a school's performance rating is from the probation threshold.

Table 2.1: Elementary School Rating Description

Metric	Current Status		Trend		Growth	
	Metric Range	Points Received	Metric Range	Points Received	Metric Range	Points Received
	(1)	(2)	(3)	(4)	(5)	(6)
ISAT Meet/Exceed	Less than 50.0%	0	Less than 0.1	0		
	50.0% to 69.9%	1	0.1 to 2.9	1		
	70.0% to 79.9%	2	3.0 to 5.9	2		
	80.0% or more	3	6.0 or more	3		
ISAT Exceed	Less than 5.0%	0	Less than 0.1	0		
	5.0% to 14.9%	1	0.1 to 2.9	1		
	15.0% to 24.9%	2	3.0 to 5.9	2		
	25.0% or more	3	6.0 or more	3		
Attendance	Less than 90.0%	0	Less than 0.1	0		
	90.0% to 92.9%	1	0.1 to 0.4	1		
	93.0% to 94.9%	2	0.5 to 0.9	2		
	95.0% or more	3	1.0 or more	3		
Value-Added Reading					Less than -2.2	0
					-2.2 to -0.1	1
					0.0 to 2.1	2
					2.2 or more	3
Value-Added Mathematics					Less than -2.7	0
					-2.7 to -0.1	1
					0.0 to 2.6	2
					2.7 or more	3

Source: Chicago Public Schools
Note: ISAT Meet/Exceed metric score ranges and points possible for current status and trend are the same for each section of the test—reading, mathematics, and science. The ISAT Exceed metric score ranges and points possible for current status and trend are the same for the exceeds and exceeds for the highest grade level metrics.

Table 2.2: High School Rating Description

Metric	Current Status		Trend		Growth	
	Metric Range	Points Received	Metric Range	Points Received	Metric Range	Points Received
	(1)	(2)	(3)	(4)	(5)	(6)
	Less than 16.0	0	Less than 0.1	0		
Average	16.0 to 17.9	1	0.1 to 0.4	1		
ACT	18.0 to 19.9	2	0.5 to 0.9	2		
	20.0 or more	3	1.0 or more	3		
One Year	More than 10.0%	0	Less than -0.1	0		
Drop-Out	6.1% to 10.0%	1	-0.1 to -0.9	1		
Rate	2.1% to 6.0%	2	-1.0 to -2.9	2		
	2.0% or less	3	-3.0 or more	3		
Freshman	Less than 45.0%	0	Less than 0.1	0		
On-Track	45.0% to 59.9%	1	0.1 to 2.4	1		
Rate	60.0% to 79.9%	2	2.5 to 4.9	2		
	80.0% or more	3	5.0 or more	3		
	Less than 85.0%	0	Less than 0.1	0		
Attendance	85.0% to 89.9%	1	0.1 to 0.4	1		
	90.0% to 94.9%	2	0.5 to 0.9	2		
	95.0% or more	3	1.0 or more	3		
	Less than 30.0%	0	Less than 0.1	0		
PSAE	30.0% to 49.9%	1/3	0.1 to 2.4	1/3		
Meet/Exceed	50.0% to 69.9%	2/3	2.5 to 4.9	2/3		
	70.0% or more	1	5.0 or more	1		
			Less than 0.1	0		
AP			0.1 to 2.4	1		
Enrollment			2.5 to 4.9	2		
			5.0 or more	3		
			Less than 0.1	0		
AP			0.1 to 0.9	1		
Success			1.0 to 2.9	2		
			3.0 or more	3		
					Less than 15th percentile	0
EPAS					15th to 49th percentile	1
Gains					50th to 84th percentile	2
					85th percentile or more	3

Source: Chicago Public Schools

Note: PSAE Meet/Exceed metric score ranges and points possible for current status and trend are the same for each section of the test—reading, mathematics, and science. The EPAS gains metric score ranges and points possible are the same for the reading and mathematics metrics.

Table 2.3: Descriptive Statistics

	All Schools	Elementary	High School
	(1)	(2)	(3)
% African-American	44.16	42.34	48.25
% White	8.60	8.83	8.07
% Hispanic	42.24	43.89	38.52
% Asian	3.48	3.36	3.75
% Bilingual	14.00	17.89	5.25
% Special Education	12.41	11.45	14.56
% Free/Reduced Lunch	86.26	86.58	85.56
Misconducts Per 100 Students	38.06	20.47	120.71
# of Schools	474	398	76

Source: Chicago Public Schools
Note: Data on racial/ethnic composition is based on school years between 2008 and 2013 and data on the percent bilingual, special education, and free/reduced lunch is based on school years between 2010 and 2013. Misconducts per 100 students is based on the 2012 and 2013 school years.

Table 2.4: First Stage Estimates

	All	On Probation, t	Not on Probation, t
	(1)	(2)	(3)
Panel A: All Schools			
Estimate	-0.63***	-0.30***	-1.00***
	(0.03)	(0.05)	(0.00)
Constant	1.00	1.00	1.00
	(0.00)	(0.00)	(0.00)
Observations	2,349	1,065	1,284
Panel B: Elementary			
Estimate	-0.65***	-0.32***	-1.00***
	(0.03)	(0.05)	(0.00)
Constant	1.00	1.00	1.00
	(0.00)	(0.00)	(0.00)
Observations	1,975	832	1,143
Panel C: High School			
Estimate	-0.48***	-0.19	-1.00***
	(0.08)	(0.15)	(0.00)
Constant	1.00	1.00	1.00
	(0.00)	(0.00)	(0.00)
Observations	374	233	141

Note: Each estimate represents the β_1 coefficient from Equation 1—which in this case, measures the difference in the probability of being on probation in time t+1 for schools with relative performance ratings above the probation threshold in time t relative to schools below in time t. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.5: Change in Relative Performance Rating Estimates

	All Schools	Elementary	High School
	(1)	(2)	(3)
Relative Performance	-3.21**	-4.02**	-1.00
Rating Change	(1.42)	(1.67)	(2.73)
Constant	4.51	5.10	2.67
	(1.18)	(1.42)	(2.00)
Observations	2,370	1,990	380

Note: Each estimate represents the β_1 coefficient from Equation 1—which in this case, measures the difference in the change in relative performance rating in time t+1 for schools with performance ratings above the probation threshold in time t relative to schools below in time t. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.6: Change in Performance Rating Components Estimates - Elementary Schools

Metric	ISAT Reading Meet/Exceed	ISAT Math Meet/Exceed	ISAT Science Meet/Exceed	ISAT Comp. Exceed	ISAT Comp. Hi. Gr. Exceed	Attendance	Value-Added Reading	Value-Added Math
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Current Status Points	-0.06	-0.02	-0.06	-0.10**	-0.05	-0.05		
	(0.05)	(0.05)	(0.06)	(0.05)	(0.07)	(0.05)		
Constant	0.11	0.21	0.21	0.16	0.14	0.09		
	(0.04)	(0.05)	(0.05)	(0.04)	(0.06)	(0.04)		
Trend Points	-0.08	0.01	-0.44**	-0.16	-0.26	-0.10		
	(0.18)	(0.15)	(0.20)	(0.11)	(0.17)	(0.17)		
Constant	-0.01	-0.09	0.62	0.26	0.37	-0.27		
	(0.15)	(0.13)	(0.17)	(0.09)	(0.14)	(0.15)		
Growth Points							0.00	-0.26**
							(0.14)	(0.13)
Constant							0.09	0.25
							(0.12)	(0.11)

Note: Current Status Points, Trend Points, and Growth Points represent the β_1 coefficient from Equation 1—which in this case, measures the difference in the points gained in relevant category in time t+1 for schools with relative performance ratings above the probation threshold in time t relative to schools below in time t. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.7: Change in Performance Rating Components Estimates - High Schools

Metric	Average ACT	1-Year Drop-Out Rate	Freshman On-Track Rate	Attendance	PSAE Reading Meet/Exceed	PSAE Math Meet/Exceed	PSAE Science Meet/Exceed	AP Enrollment	AP Success	EPAS Gains Reading	EPAS Gains Math
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Current Status Points	0.04	-0.17	-0.01	-0.04	0.01	-0.02	-0.05**				
	(0.07)	(0.17)	(0.12)	(0.09)	(0.02)	(0.01)	(0.02)				
Constant	0.07	0.36	0.13	0.14	-0.01	0.01	0.01				
	(0.04)	(0.12)	(0.09)	(0.06)	(0.01)	(0.01)	(0.01)				
Trend Points	-0.10	0.23	0.64*	-0.43	-0.04	0.01	0.11	-0.16	-0.68		
	(0.29)	(0.40)	(0.35)	(0.45)	(0.17)	(0.15)	(0.17)	(0.33)	(0.42)		
Constant	0.12	-0.02	-0.36	-0.06	0.14	-0.02	-0.05	-0.00	0.55		
	(0.19)	(0.31)	(0.25)	(0.34)	(0.12)	(0.10)	(0.11)	(0.24)	(0.33)		
Growth Points										-0.00	-0.09
										(0.18)	(0.16)
Constant										-0.02	0.05
										(0.14)	(0.12)

Note: Current Status Points, Trend Points, and Growth Points represent the β_1 coefficient from Equation 1—which in this case, measures the difference in the points gained in relevant category in time t+1 for schools with relative performance ratings above the probation threshold in time t relative to schools below in time t. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.8: Change in Behavior Estimates

	Misconducts Per 100	In-School Suspensions Per 100	Out of School Suspensions Per 100	Police Notifications Per 100	Expulsions Per 100
	(1)	(2)	(3)	(4)	(5)
Panel A: All Schools					
Estimate	2.60	-11.69	4.63	0.25	-0.02
	(17.16)	(10.22)	(4.53)	(0.95)	(0.09)
Constant	0.80	11.74	-0.74	-0.43	0.01
	(16.80)	(10.17)	(4.29)	(0.91)	(0.09)
Observations	465	465	465	465	465
Panel B: Elementary					
Estimate	9.11	-2.48	2.93	-0.07	-0.09**
	(7.24)	(1.90)	(3.28)	(0.37)	(0.04)
Constant	-4.12	2.43	-0.16	0.05	0.10
	(7.01)	(1.84)	(3.09)	(0.31)	(0.04)
Observations	389	389	389	389	389
Panel C: High School					
Estimate	53.08	8.65	29.91*	-1.09	0.04
	(67.22)	(34.03)	(17.35)	(4.49)	(0.37)
Constant	-62.35	-6.48	-15.44	-0.48	-0.23
	(58.34)	(32.91)	(14.41)	(3.96)	(0.35)
Observations	76	76	76	76	76

Note: Each estimate represents the β_1 coefficient from Equation 1—which in this case, measures the difference in the change in the rate for the corresponding behavior metric in time t+1 for schools with relative performance ratings above the probation threshold in time t relative to schools below in time t. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Chapter 3

The Effect of School Year Length on Juvenile Crime

Due to the ramifications of The Great Recession, budget cuts were prevalent throughout the U.S. during the late 2000s and early 2010s—especially in the public sector. A primary example of these budget cuts was the institution of mandatory furloughs for public sector workers. Being that public schools are publicly funded, they were susceptible to such regulatory changes.

Prior to the 2009-2010 school year, the minimum number of instructional days school districts in California were required to offer was reduced by five days.[1] Before this policy was enacted, school districts in California were required to offer 180 days of instruction each school year.[2] Additionally, prior to the 2011-2012 school year, school

[1] California Education Code Section 46201.2

[2] Certain districts were allowed to offer less than 180 instructional days in schools that operated as a multi-track year-round school.

districts were permitted to reduce the school year by an additional seven days.[3]

This paper investigates the effect of school on crime through the mechanism of school year length. Specifically, how a shorter school year affects the criminal outcomes of juveniles. As was the case in California, policy changes of this nature are generally triggered by budgetary concerns. The effect of a policy change of this nature will cause students to be in school less often. Thus, it is important to understand if there are any deleterious effects on juvenile behavior. Specifically, if students are committing more crime because they are not in school as often.

Estimating the effect of school on crime has been looked at through various channels including years of schooling (Lochner and Moretti 2004), changes in the high school drop-out age (Anderson 2014), quality of schooling (Deming 2011), and school starting age (Cook and Kang 2016; Landersø, Nielsen, and Simeonsen 2017; Depew and Eren 2016). This list of papers highlights how criminal behavior can be affected in many different ways through an individual's school experience. While there are numerous channels through which the criminal behavior of juveniles is affected, it is important to distinguish that this paper will focus solely on how juvenile behavior is affected through the mechanism of changes in the length of the school year, particularly by decreasing the number of school days.

The focus of this paper can also be framed as measuring the incapacitation effect of school on crime. The incapacitation effect arises from the fact that students cannot commit crime on the street if they are busy at school. That is, their being in

[3] California Education Code Section 46201.3

school prevents them from engaging in delinquent behavior outside of school during the day. This concept follows from Jacob and Lefgren (2003), who show that juveniles are less likely to commit property crime and more likely to commit violent crime on school days relative to non-school days using teacher in-service days as their source of variation. The decline in property crime is attributed to the incapacitation effect of being in school where juveniles face a higher probability of detection, and the increase in violent crime is associated with a larger concentration of juveniles on school days. Using teacher strike days as the source of variation, Luallen (2006) finds a similar result, and that the increase in violent crime on school days is driven largely by repeat juvenile offenders. Billings and Phillips (2017) also confirm the results of Jacob and Lefgren (2003), while also finding that the decrease in crime on teacher in-service days is larger for schools that have more students who are high-crime risks. In accordance with these papers, Billings et al. (2016) finds that increasing the number of similar age, grade, and race peers that attend a juvenile's same school and live in the same neighborhood increases the probability that a juvenile will commit crime.

I find that decreasing the number of school days in the school year has no effect on juvenile criminal behavior. The null result found here may be due to a few attributing factors. Trends in juvenile crime show that juvenile criminal behavior has been declining since peaking in the early 1990s, and any effect captured may be due to crime falling and not necessarily to the fact that students are in school less often. Also, the geographic level and measure of the crime data used here may not be precise enough to determine if the decrease in school days has a significant impact on juvenile crime.

The paper closest to the question examined here is Akee, Halliday, and Kwak (2014), who use teacher furloughs as their source of variation to examine the relationship between student time in school and juvenile arrests in Hawaii. They find that on furlough days when school is not is session there are fewer juvenile arrests for assault and drug-related crimes, where the decrease in assaults is largest in poorer areas and the decrease in drug-related crimes is largest in wealthier regions.

The rest of the paper is organized as follows: Section 3.2 provides a description of the identification strategy. Section 3.3 outlines the data used for the paper. Section 3.4 presents the results. Section 3.5 concludes.

3.1 Identification Strategy

As stated earlier, in California starting in the 2009-2010 school year, school districts, county offices of education, and charter schools were granted permission to reduce the school year by five days of instruction, or the equivalent number of instructional minutes, without facing punishment.[4] Also, during the 2011-2012 school year, those same jurisdictions were granted permission to reduce the school year further by an additional seven days. Though the law change took effect in 2009-2010 school year, the most significant reduction of class days generally occurred during the 2010-2011 school year across a substantial portion of school districts in California.

Table 3.1 represents the percentage of California school districts that offered a

[4]The policy came into effect on July 1, 2009

particular number of school days during the school year that ended in the year displayed. From column (7), before 2010 nearly all school districts offered 180 days of instruction. In 2010, there was an approximately 10 percentage point decline in that figure and in 2011 that percentage fell another 20 percentage points. From columns (2)-(6), many school districts began to decrease the number of instructional days starting in 2010. In particular, column (2) shows the percentage of school districts offering 175 school days (the minimum allowed by the policy change in 2010)—which shows the opposite pattern as column (7). That is, there was a slight increase in the percentage of school districts with 175 days of instruction in 2010, with a more dramatic increase in 2011. It also seems that the majority of districts did not choose to cut the permissible seven extra days off of the school year during the 2011-2012 school year.

Figure 3.1 shows trends in the number of school days offered —those offering the previous minimum of 180 days and those offering between 175 and 179 days. Nearly all school districts fall into one of these two categories. The purpose of this graph is to show that before the policy change offering 180 instructional days was the norm in California school districts and it was not until this change was put into effect that there were any substantial changes in the school year throughout the state. As stated earlier, the most significant changes occurred during the 2010-2011 school year. After the 2010-2011 school year, however, the number of school days across California school districts remained mostly constant up to the most current year of data. Given these trends, it will be easier to determine the timing of the treatment effect of decreasing the number of days in the school year. If districts were frequently changing the number of school days prior to the 2009-2010 school year, then pinning down the effect of the policy change would have been difficult. Figure 3.1 also shows that after the policy change took effect the decrease in districts offering 180 days was matched by the increase in districts offering between 175 and 179. This verifies that those districts that decreased from 180 school days did so within the allowable 5 day maximum.

Looking specifically at how many districts decreased their number of school days, 313 school districts in California began to implement shorter school years between 2010 and 2012—with 63 districts doing so in 2010, 187 in 2011, and 63 in 2012. That is, in 2010, 63 school districts reduced their school year below 180 days, 187 different school districts did so in 2011, and 63 other school districts did so in 2012.

The policy change was made available for all school districts, county offices of education, and charter schools in California that wished to reduce their number of school days without penalty. Thus, all jurisdictions in California were eligible to implement a reduced school year. However, not all jurisdictions chose to reduce their school year below the previous 180-day minimum. This allows for the creation of two different groups from which outcomes can be compared. The first group being those areas that remained at or above the 180-day school year and the second group being areas that chose to reduce the number of school days below 180. The first group will be the control group and the second group will be the treatment group. Though both groups were eligible to change the number of school days in the school year, I use the terms treatment and control merely as a way to define each of the groups based on their post-2010 number of school days.

Ideally, I would like to classify the month which days were lost in each school district in order to examine juvenile criminal activity in months that lost days of instruction. This is an important detail as the timing and intensity of the treatment varies from one district to another. This would allow me to more clearly define pre- and post-treatment periods in order to increase the precision of the estimates of the model. However, due to data limitations, I was only able to classify pre- and post-treatment periods at the school year level.

Data from individual school districts suggests that days of instruction lost due to furloughs and budgetary concerns were spread out throughout the year and not localized in one month. This was determined by examining various school district calendars during the appropriate year or by contacting the school district and asking for the information. For example, in the Santa Monica-Malibu Unified School District in Los Angeles County, the actual days of school lost during the 2010-2011 school year stemming from budget cuts were November 12, March 14, May 27, June 23, and June 24.

This fact is also true for the 20 largest school districts as well. I also examined school calendars for the 20 largest school districts in California based on student population and found a similar pattern of of widely dispersed furlough days throughout several different months, which makes pinning down the treatment to one particular month not feasible in many cases.[5] Of these 20, 10 decreased their number of school days below 180 at some point after the policy change was put into effect. Collectively,

[5]The California Department of Education ranks district size based on annual enrollment.

these 20 districts represented nearly 30% of all California students during the 2013-2014 school year.

Therefore, I defined the pre-treatment period as the months prior to July of the year when the city decreased the school year below 180 days and the post-treatment as all months thereafter. I use July because the school year in California starts on July 1 and ends on June 30 of the following year—which is based on the California fiscal year. It is important to note that using this particular scheme imposes a uniform timing and intensity of treatment for each of the treatment years. That is, all of the districts that decreased their number of school days in a particular year will all be assigned the same intensity and timing of the treatment under the main specification. I also present estimates using an alternative specification that allows for the intensity of the treatment to vary. Specifically, I only compare cities that lost a certain number of school days, between 1 and 5, to cities that adhered to the 180-day norm.

3.2 Data

Juvenile arrest data was gathered from the Federal Bureau of Investigation's Uniform Crime Reports. School year data was gathered from the California Department of Education.

A 2013 bulletin published by the U.S. Department of Justice showed that in 2011 juveniles were arrested most often for committing certain offenses, including larceny, drug violations (including sale and possession), and disorderly conduct (Puzzanchera 2013). This bulletin also reported that juvenile arrests accounted for 11% of all arrests in the U.S. that year. Also, juveniles were involved in approximately 20% of arrests for the offenses of robbery, burglary, larceny, and auto theft and female juvenile arrests accounted for 29% of all juvenile arrests that year.

Table 3.2 displays California arrests for juveniles sorted by Part I and Part II offenses for the 2011-2012 school year. Part I offenses are those that constitute Violent (Murder, Manslaughter, Rape, Robbery, Aggravated Assault) and Property (Burglary, Larceny, Auto Theft, Arson) Crimes. Part II offenses are all others.[6] Juveniles account for 100% of arrests for the offenses of Curfew/Loitering and Runaways since only juveniles can be charged with such crimes. Similar to the national estimates for juvenile arrests, juveniles in California accounted for 11% of all arrests in California and females accounted for 28% of all juvenile arrests in California. Of the Part I offenses, the most prevalent crimes were larceny, burglary, and assault.

The data regarding school days is collected at the district level. Arrest data is collected at the police agency level. The arrest data that I use here consists of police agencies that cover entire cities.[7] Thus, the school district data can be matched to the arrest data at the city level.

In order to merge the school day data with the arrest data, I matched the school district to the city where the district headquarters is located according the California Department of Education. Districts that could not be clearly matched to a correspond-

[6]When an individual is arrested for multiple offenses, only the most serious offense is counted.

[7]Examples of other types of police agencies include university campus police and highway patrol jurisdictions.

ing city were dropped from the sample. In most cases, these districts that could not be matched were in unincorporated communities that are not covered by city-level police agencies. Also, for the police agencies that cover more than one school district, the average number of school days among those districts is used for that city. After this matching process, I have 216 agencies, of which 134 belong to the treatment group.

Aggregating the districts up to the relevant cities will help to reduce measurement error compared to aggregating the districts at the county level, which was done in a previous iteration of this paper. Though in some cases there is only one district for a particular city-level police agency, there are certain jurisdictions that contain more than one district—which will reduce the precision of the estimates slightly. While this is a concern, I am limited by the level at which the arrest data that I am using is collected.

In the next section I will be estimating the following equation:

$$c_{it} = \alpha + \beta(m_i \cdot t_t) + \gamma_i + \delta_t +_{it} \tag{3.1}$$

where c_{it} is the arrest rate in city i in month t, m_i is a dummy variable that equals 1 if city i is in the treatment group and 0 otherwise, t_t is a dummy variable that equals 1 in the post-period and 0 otherwise, and κ_i and λ_t are city and time fixed effects, respectively.

3.3 Results

The sample consists of data from 216 California cities—with 82 cities belonging to the control group and 134 belonging to the treatment group. The results of the panel fixed effects estimation can be found in Table 3.3. Violent crime arrests consist of arrests relating to murder, manslaughter, rape, robbery, and aggravated assault. Property crime arrests consist of arrests relating to burglary (breaking and entering), larceny (non-auto theft), auto theft, and arson. The dependent variable in this model is arrest rate per 10,000 juveniles. In order to calculate the arrest rate, population data was gathered from the U.S. Census Bureau's American Community Survey.

The estimates in Table 3.3 represent the estimated β coefficient from Equation (3.1). This represents the post-change difference in arrest rates between cities that decreased their number of school days to those that did not. Columns (1), (3), and (5) display estimates of Equation (3.1) with both city and month fixed effects. The results in these columns suggest that the effect of this policy change on juvenile arrest rates is statistically no different from zero. In order to further investigate these estimates and the effect of a shorter school year on juvenile arrests, I added a linear city-specific time trend to the model as an additional control.

Estimates in Columns (2), (4), and (6) were found by adding a linear city-specific time trend to the model. I add this term to control for any underlying trends in arrest rates that differ across cities. Including the linear city-specific time trend causes the estimates to become more negative for all types of crime (Row 1) and for property crime separately (Row 2)—with the biggest change occurring for male juveniles. From Row 1, Column (2), though the estimates become negative, adding this trend term increases the magnitude of the estimates in absolute value. In Column 6, the magnitude is much larger in absolute value and the estimates become quite negative for males. These results indicate that long-term trends in arrests are important when determining the impact of a shorter school year on arrest rates. That is, based on the model presented here, these underlying trends are primarily responsible for any post-change differences in arrest rates. From Row 2, the estimates for property crime follow a similar pattern, though to a smaller magnitude. The estimates for female juveniles alone are not statistically different from zero in either case. From Table 3.2, given that female juveniles commit less than 30% of juvenile crime, it is not surprising that this policy change had no effect on their arrest rate. Estimates from Row 3 suggest that this policy change had no effect on violent crime arrests for both genders.

In Table 3.4, I present estimates for Equation (3.1) where I allow the intensity of the treatment to vary by the number of days a city decreased the school year. Panels A through E show estimates where the treatment group is all cities that decreased the number of school days to be between 175 and 179, respectively. Breaking out the treatment by the number of school days offered shows a similar trend to the results in Table 3.3. In general, the estimates become more negative when adding the city-specific time trend. Estimates for female juveniles when the treatment group consists of cities with school years of 175 and 178 days shows that the change in violent crime is negative and significant both with and without the city-specific time trend. However, the magnitudes of these estimates are quite small and are not present for any other level of treatment intensity.

A potential explanation of the results of Tables 3.3 and 3.4 is that crime in the United States has been trending downward for the past 20 years. In the late 1980s and early 1990s crime and arrests were reaching all-time highs in the U.S. for juveniles and adults—particularly juvenile violent crimes. Juvenile arrest trends in California closely mirrored the trends occurring at the national level—with both seeing large jumps during the late 1980s into the early 1990s where it began to taper off and follow its current downward trajectory.

Though the results from Tables 3.3 and 3.4 do not indicate that this policy change had any differential impact on juvenile arrest rates outside of the already declining crime trend, I present results of a falsification test for a different population that should be unaffected by the policy change in Table 3.5. The purpose of doing this analysis is to test the model to see if the results hold for a different population who should be unaffected by the policy change. I use arrest rates for the population aged 20 to 24 as the alternate population. I chose this group because they are close to juveniles in age, but old enough that changes in school policy should have no effect on them as they are no longer in school. The desired result of this test would be to show that this policy change had zero effect on the arrest rate for this particular population.

The results in Row 1 of Table 3.5 indicate that a shorter school year led to a lower arrest rate for 20 to 24 year olds based on the model presented here. Property crime estimates yield similar, yet smaller results. This placebo test yielding non-zero estimates indicates that this identification strategy may need to be adjusted in future work.

3.4 Conclusion

In this paper, I find that a shorter school year had no significant effect on juvenile arrest rates. Comparing cities that instituted a school year below the previous 180-day norm to those that had at least a 180-day school year, the model presented here did not find a significant causal relationship between having fewer school days in the school year and juvenile arrest rates. I extended the model by adding a linear city-specific time trend, which resulted in the estimates becoming negative—especially for male juveniles. Estimates for female juveniles and violent crime were not statistically different from zero both with and without the city-specific time trend. The results found here suggest that long-term trends in arrests are primarily responsible for any post-change differences in arrest rates and a shorter school year had little to no effect on juvenile criminal behavior.

A falsification test using a different age group found non-zero estimates when using 20 to 24 year olds as the affected population. Since this age group is no longer in school, a shorter school year should not affect their criminal behavior. However, the model presented here found a significant relationship between a shorter school year and arrests for this age group. This results suggests that the identification strategy may need to be refined in order to get a better estimate of the school-crime relationship.

Given the results found in this paper, it is important to discuss some potential data issues that, once addressed, may yield better results in future research on this topic. One of these concerns is the geography used here. I stated earlier that I could not match the juvenile arrest data to the school year data at the district level due to the arrest data not being calculated at the district level. I elected to use city-level arrest data and aggregated the school year data up to the city level in order to merge the two sets of data. In future research on this topic, I hope to match the arrest data at a smaller level of variation—namely at the district level if possible. Another concern is the timing of the treatment. The model imposes a uniform timing of the treatment by having the post period begin in the same month at the beginning of each treatment year. For example, areas that reduced their school days starting in the 2010-2011 school year all have their post-period begin in July 2010. The reason for doing this is because the lost school days for most school districts occurred during different months; thus, pinning down a particular month as the treatment month would not have been feasible. A remedy for this would be to have data at the daily level and have different treatment days for the districts. A better matching of the geography and timing of the treatment would better pin down the effect of a shorter school year on juvenile criminal behavior.

The model presented here is a simple reduced-form relationship between school and crime, relying solely on post-treatment comparisons. There are likely several other factors unaccounted for here that may affect juvenile criminal behavior. Stemming from the classical economic model of crime, individuals who decide to take part in illegal activities must weigh the benefits and costs beyond economic gain, and must also consider being caught, arrested, and incarcerated (Becker 1968, Ehrlich 1973). Also, in this paper, I ignore deterrence measures. Studies that have looked into deterrence measures have found that certain deterrence measures can be effective in decreasing crime (Freeman 1996, Garrett and Ott 2002, Levitt 1998).

An additional concern is that, as I stated previously, both violent and property crime have been steadily declining in the U.S. since the 1990s and this declining trend has been attributed to several different factors. Evidence of this can be seen with the addition of city-specific time trends to the model. While I do not attempt to establish causality here, this trend makes establishing any causal relationship regarding crime difficult.

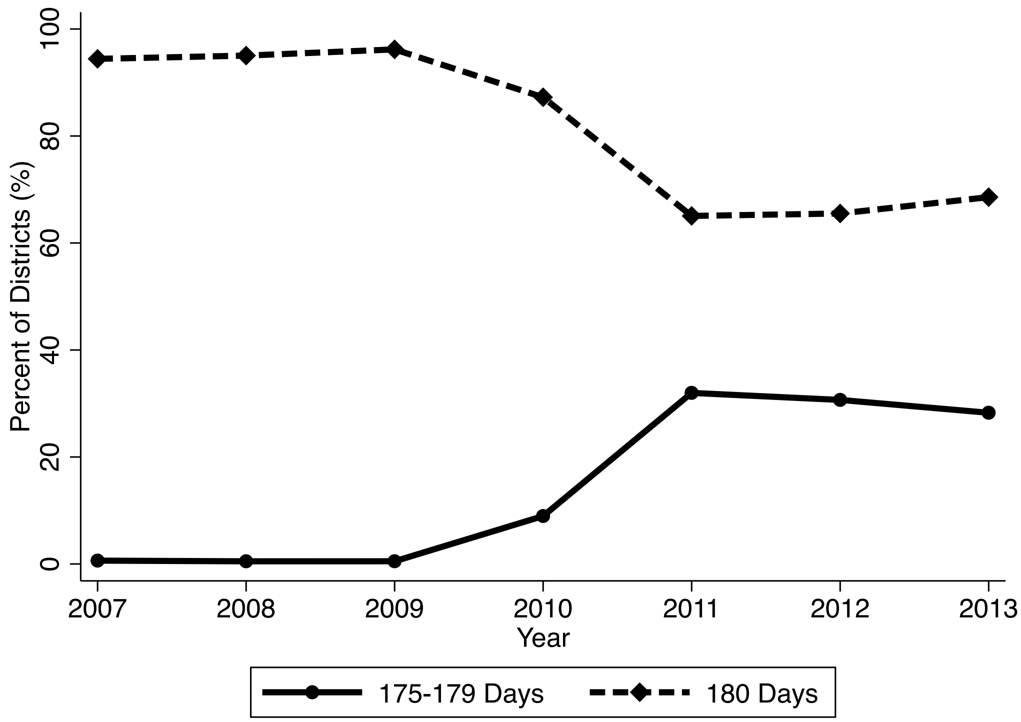


Figure 3.1: Days of School Offered in California School Districts

Table 3.1: Days of School Offered in California School Districts

School Year	# School Districts	<175	175	176	177	178	179	180	>180
2005	808	0.0	0.6	0.0	0.1	0.1	0.1	94.8	4.2
2006	825	0.0	0.4	0.0	0.1	0.1	0.2	94.7	4.5
2007	806	0.0	0.4	0.0	0.0	0.2	0.0	94.4	5.0
2008	805	0.0	0.4	0.0	0.1	0.0	0.0	95.0	4.5
2009	787	0.0	0.4	0.0	0.0	0.0	0.1	96.2	3.3
2010	815	0.0	3.8	1.1	1.2	1.6	1.2	87.2	3.8
2011	810	0.2	16.9	4.0	4.6	4.4	2.1	65.1	2.7
2012	812	0.2	15.3	4.2	4.1	3.9	3.2	65.5	3.6

Note: The numbers in each column represent percentages.

Table 3.2: 2011-2012 School Year Juvenile Arrests in California

Offense	Arrests	Juvenile Percent of All Arrests	Female Percent of Juvenile Arrests
Total	131,212	11	28
Part I Offenses			
Violent Crimes	10,142	10	19
Murder	115	7	3
Manslaughter	7	4	14
Rape	152	9	1
Robbery	3,831	23	14
Assault	6,037	7	22
Property Crimes	30,351	21	38
Burglary	10,084	19	24
Larceny	17,997	22	49
Auto Theft	1,799	15	16
Arson	471	45	11
Part II Offenses			
Against Family/Child	4	2	100
Curfew/Loitering	7,143	100	29
DUI	817	0	26
Disorderly Conduct	6,156	60	40
Drug Possession	8,866	6	21
Drug Sales/Manu.	2,050	7	12
Drunkeness	2,361	3	29
Embezzlement	15	1	40
Forgery/Counterfeit	110	2	24
Fraud	294	4	29
Gambling	23	5	4
Liquor	3,206	21	30
Other Assault	13,596	17	34
Other Non-Traffic	25,688	9	23
Prostitution	297	3	89
Runaways	3,576	100	50
Sex Related	1,480	13	13
Stolen Property	2,155	12	13
Vagrancy	252	4	9
Vandalism	7,214	37	11
Weapons Related	5,416	20	9

Note: The arrest numbers presented here represent the number of arrests that occurred during the 2011-2012 school year, which took place from July 2011 to June 2012. Arrests listed under Other Non-Traffic include violations of state or local laws not specifically identified as Part I or Part II offenses, except traffic violations.

Table 3.3: Panel FE Estimates

	Total		Females		Males	
	(1)	(2)	(3)	(4)	(5)	(6)
All	0.294	-0.746**	0.372	-0.202	0.207	-1.181**
	(0.585)	(0.378)	(0.420)	(0.292)	(0.832)	(0.582)
Property	-0.193	-0.259*	-0.101	-0.029	-0.275	-0.474***
	(0.142)	(0.134)	(0.174)	(0.175)	(0.171)	(0.172)
Violent	-0.001	-0.039	-0.054	-0.044	0.053	-0.026
	(0.048)	(0.052)	(0.034)	(0.048)	(0.084)	(0.084)
City-Specific Time Trend		x		x		x

Note: Standard errors clustered at the city level in parentheses. The dependent variable is number of arrests per 10,000 juveniles for the corresponding crime category and gender.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Panel FE Estimates by Treatment Intensity

	Total		Females		Males	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 175 Days						
All	0.314	-0.959	0.608	-0.444	0.168	-1.325
	(0.954)	(0.681)	(0.646)	(0.572)	(1.372)	(0.994)
Property	-0.111	-0.072	-0.100	0.205	-0.109	-0.331
	(0.243)	(0.298)	(0.330)	(0.384)	(0.287)	(0.345)
Violent	-0.028	-0.066	-0.114**	-0.103*	0.075	-0.012
	(0.065)	(0.086)	(0.050)	(0.061)	(0.120)	(0.145)
Panel B: 176 Days						
All	2.302**	-0.127	1.313*	0.319	3.587***	-0.225
	(0.942)	(0.984)	(0.689)	(0.848)	(1.289)	(1.218)
Property	-0.281	-0.980**	-0.510	-0.530***	-0.039	-1.398**
	(0.423)	(0.437)	(0.314)	(0.196)	(0.620)	(0.691)
Violent	-0.059	0.043	0.091	0.252	-0.182	-0.154
	(0.210)	(0.247)	(0.291)	(0.381)	(0.235)	(0.157)
Panel C: 177 Days						
All	2.705***	-0.384	0.962	-0.600	4.238***	-0.259
	(0.860)	(0.679)	(0.635)	(1.106)	(1.530)	(1.322)
Property	-0.066	-0.678***	-0.837**	-0.867	0.630	-0.551
	(0.273)	(0.228)	(0.338)	(0.783)	(0.532)	(0.500)
Violent	0.020	-0.015	-0.104	-0.171	0.124	0.096
	(0.223)	(0.234)	(0.088)	(0.112)	(0.349)	(0.359)
Panel D: 178 Days						
All	-0.745	-0.529	0.154	0.407	-1.738	-1.363
	(1.196)	(0.845)	(0.813)	(0.903)	(1.800)	(1.298)
Property	-0.380	0.016	-0.147	0.215	-0.616	-0.188
	(0.282)	(0.217)	(0.274)	(0.315)	(0.396)	(0.320)
Violent	-0.163	-0.155	-0.150**	-0.095**	-0.182	-0.204
	(0.102)	(0.116)	(0.069)	(0.045)	(0.180)	(0.212)
Panel E: 179 Days						
All	-1.228	-1.027	-0.873	-0.639	-1.555	-1.133
	(1.344)	(1.092)	(0.871)	(0.619)	(1.947)	(1.938)
Property	-0.507*	0.078	-0.257	0.361	-0.734**	-0.158
	(0.275)	(0.240)	(0.339)	(0.360)	(0.353)	(0.350)
Violent	0.043	0.160	-0.021	0.056	0.112	0.300
	(0.119)	(0.123)	(0.063)	(0.088)	(0.241)	(0.259)
City-Specific Time Trend		x		x		x

Note: Standard errors clustered at the city level in parentheses. The dependent variable is number of arrests per 10,000 juveniles for the corresponding crime category and gender.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Falsification Test (20-24 Year Olds)

	Total		Females		Males	
	(1)	(2)	(3)	(4)	(5)	(6)
All	-6.596**	-5.926***	-11.357***	-6.050***	-9.396**	-9.923***
	(2.899)	(1.699)	(4.333)	(2.298)	(4.182)	(2.549)
Property	-0.643*	-0.661*	-0.263	-0.027	-0.876*	-1.040**
	(0.379)	(0.349)	(0.570)	(0.442)	(0.475)	(0.472)
Violent	-0.274	-0.301	-0.007	-0.055	-0.389	-0.498
	(0.198)	(0.196)	(0.154)	(0.161)	(0.394)	(0.321)
City-Specific Time Trend		x		x		x

Note: Standard errors clustered at the city level in parentheses. The dependent variable is number of arrests per 10,000 individuals aged 20 to 24 for the corresponding crime category and gender.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

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Appendix A

Appendix for Chapter 1

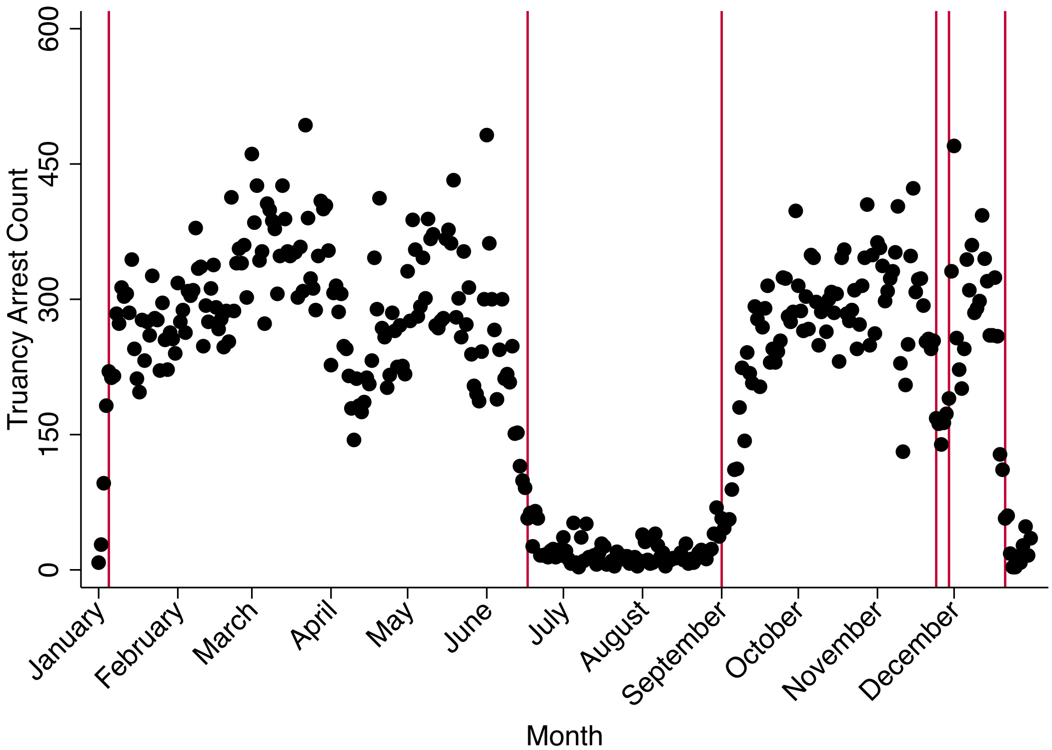
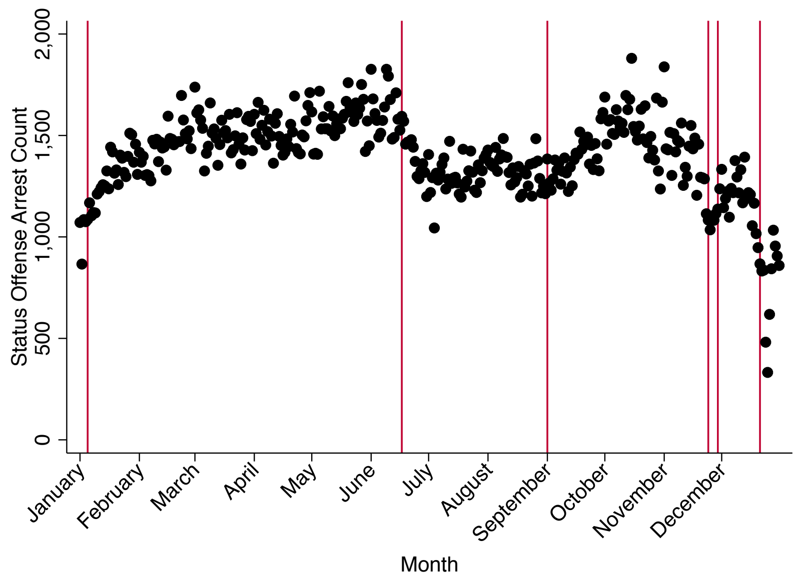
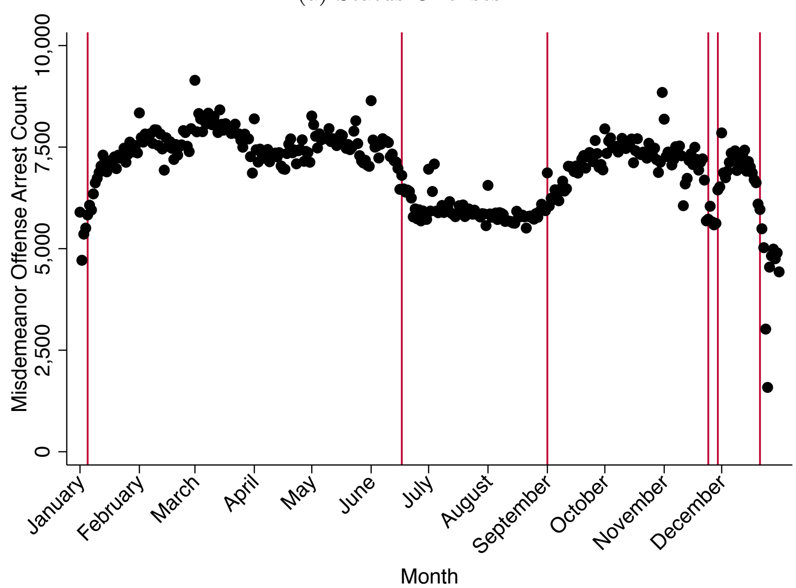


Figure A.1: Arrest Frequency By Day of Year: Truancy Arrests. Note: This figure represents the total arrest count for each day of the year over the 20-year sample for truancy arrests for all juveniles between the ages of 13 and 17. Areas inside of the red lines indicate when school is not in session due to Thanksgiving and Christmas holidays and summer break.

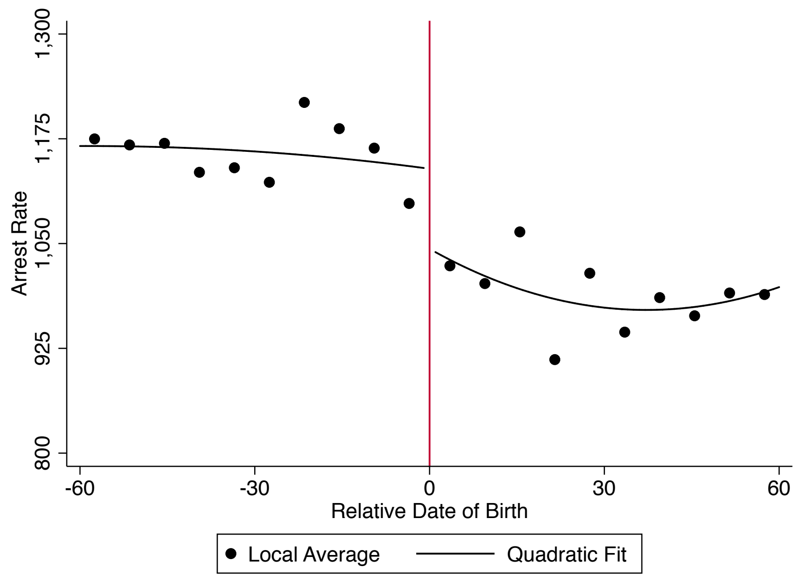


(a) Status Offenses

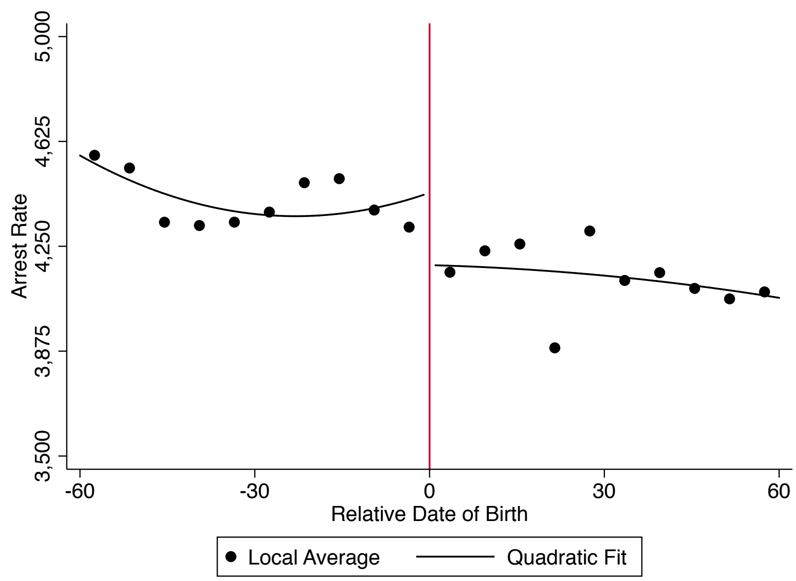


(b) Misdemeanor Offenses

Figure A.2: Arrest Frequency By Day of Year. Note: Each panel represents the total arrest count for each day of the year over the 20-year sample for the corresponding level of crime for all juveniles between the ages of 13 and 17. Areas inside of the red lines indicate when school is not in session due to Thanksgiving and Christmas holidays and summer break. Arrest counts for status offenses do not include truancy arrests.

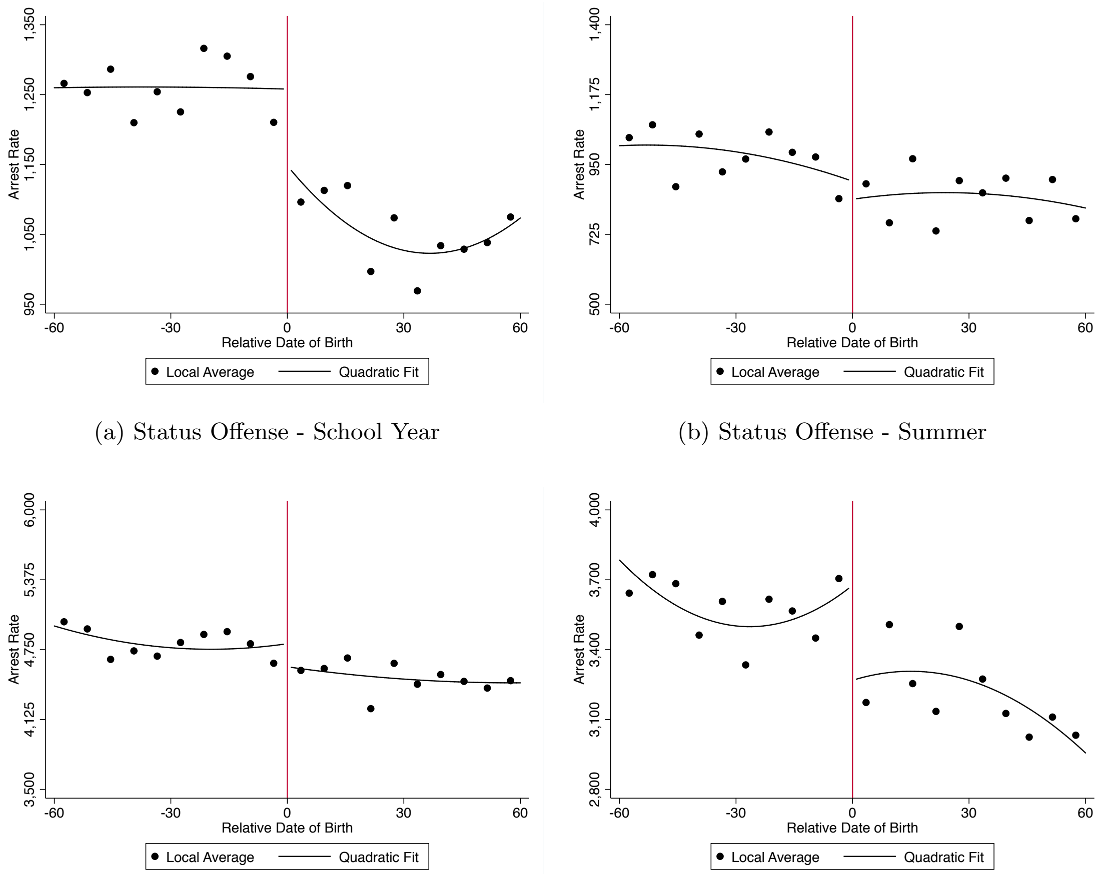


(a) Status Offenses



(b) Misdemeanor Offenses

Figure A.3: RD Plots By Offense: 14 Year Olds. Note: Each panel represents the estimated discontinuity in arrest rates per 100,000 people at the cutoff to begin school for 14 year olds using a 60-day bandwidth on each side. Each dot represents the average arrest rate by 6-day blocks of relative age—how far a birthdate is from the cutoff. Each line represents the fitted values of Equation 1.1 for each side of the cutoff.



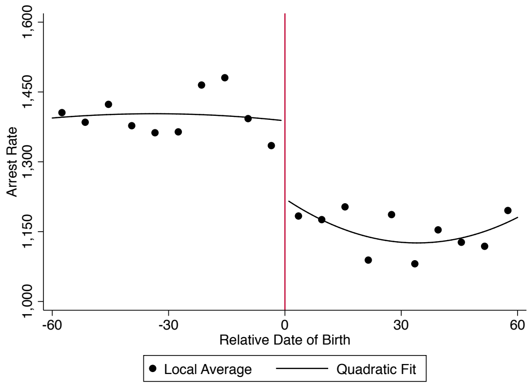
(a) Status Offense - School Year

(b) Status Offense - Summer

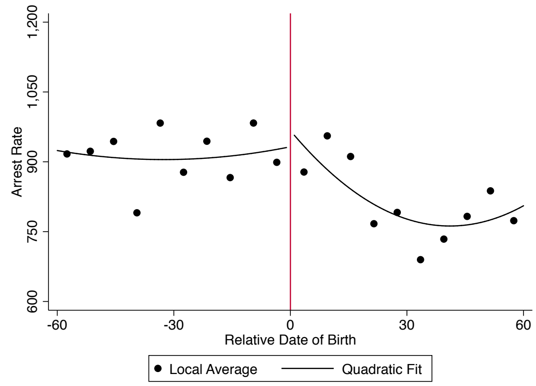
(c) Misdemeanor Offense - School Year

(d) Misdemeanor Offense - Summer

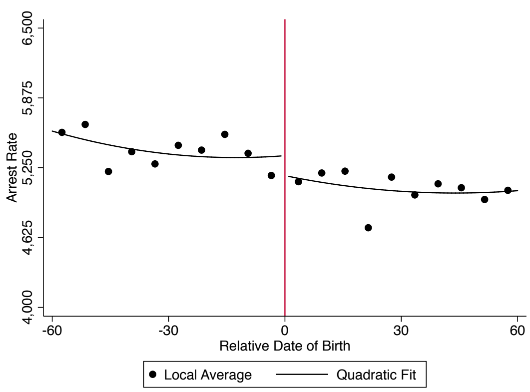
Figure A.4: RD Plots By Offense and Time of Year: 14 Year Olds. Note: Each panel represents the estimated discontinuity in arrest rates per 100,000 people at the cutoff to begin school for 14 year olds using a 60-day bandwidth on each side. Each dot represents the average arrest rate by 6-day blocks of relative age—how far a birthdate is from the cutoff. Each line represents the fitted values of Equation 1.1 for each side of the cutoff.



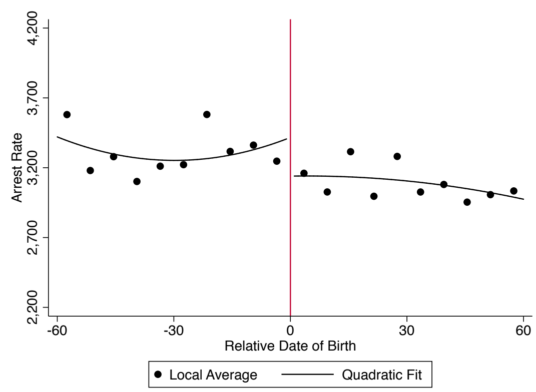
(a) Status Offense - School Year: Week



(b) Status Offense - School Year: Weekend



(c) Misdemeanor Offense - School Year: Week



(d) Misdemeanor Offense - School Year: Weekend

Figure A.5: RD Plots By Offense By Time of Year and Time of Week: 14 Year Olds. Note: Each panel represents the estimated discontinuity in arrest rates per 100,000 people at the cutoff to begin school for 14 year olds using a 60-day bandwidth on each side. Each dot represents the average arrest rate by 6-day blocks of relative age—how far a birthdate is from the cutoff. Each line represents the fitted values of Equation 1.1 for each side of the cutoff.

Table A.1: Optimal Bandwidths

	Juveniles		Adults	
	CCT	IK	CCT	IK
	(1)	(2)	(3)	(4)
All Crime	45	106	50	103
Property Crime	46	95	49	96
Violent Crime	62	94	59	97
Felony Offenses	52	112	50	97
Misdemeanor Offenses	42	141	52	112
Status Offenses	38	86		

Note: Columns labeled CCT represent optimal bandwidth calculations as proposed by Calonico, Cattaneo, and Titiunik (2014a) and columns labeled IK represent optimal bandwidth calculations as proposed by Imbens and Kalyanaraman (2012) for the corresponding age group. Juveniles refer to individuals between the ages of 13 and 17, while adults refer to individuals between the ages of 18 and 24. Percentages of each measure are in parentheses. Property crime arrests are for burglary, larceny, auto theft, and arson. Violent crime arrests are for murder, manslaughter, rape, robbery, and aggravated assault. A felony arrest can result in the arrestee being sentenced to state prison if convicted and a misdemeanor arrest can result in the arrestee being sentenced to county jail, paying a fine, restitution, or probation. Only juveniles can be arrested for status offenses, which include loitering/breaking curfew, incorrigibility, truancy, and being classified as a runaway.