Test-Based Promotion Policies, Dropping Out, and Juvenile Crime

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Abstract

Over the past decade, several states and school districts have implemented accountability systems that require students to demonstrate a minimum level of proficiency through standardized exams. With many states and school districts ending social promotion, policy makers and researchers have gained renewed interest in the role of grade retention and remedial education in US schools. This paper examines the effects of summer school and grade retention on high school completion and juvenile crime. To do so, we use administrative data from a number of state agencies in Louisiana and a regression discontinuity design to analyze Louisiana's statewide promotion policy administered to students in fourth and eighth grades. In general, our results indicate that grade retention, even at fourth grade, increases the propensity that a student drops out of school at a later point in time. In addition, eighth grade remedial education in the form of summer school appears to provide a positive benefit by decreasing the likelihood that a student later drops out. As for fourth grade students, however, we do not find any effect of summer school on the likelihood of dropping out. Additional evidence lends support to an explanation related to fading out of the summer school effect. Lastly, the regression discontinuity results provide tentative evidence that for eighth grade students, summer school decreases the likelihood of a student being convicted of a juvenile crime.

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1 Introduction

As part of the efforts to meet the expectations of the No Child Left Behind Act and school accountability systems, several states and school districts have enacted test-based promotion policies requiring students to demonstrate a minimum level of proficiency in various academic subject(s). The increase in the number of states and school districts employing these policies has rekindled the debate over social promotion, the practice of promoting students to the next grade regardless of their academic skills and performance.[1] Advocates assert that social promotion improves children's academic outcomes in the long run, decreases dropout rates and reduces risky behaviors that may arise due to stigmatization and disenfranchisement. Opponents of social promotion contend that the practice may frustrate children by advancing them to grades in which they are not ready for and therefore may hurt these children both in the short and long run, may send false signals to parents on their children's progress, and may improve efficiency and achievement by creating a more homogeneous classroom environment for the prepared students.

A test-based promotion policy typically uses standardized tests to determine whether a student should advance to the next grade. This high-stakes procedure usually comes after a remedial instruction period (e.g., Boston, Chicago, New York City, and Washington, DC). The premise for this practice is to preserve the incentive of the student to excel in the coursework and to limit grade retention by providing interventions in the form of additional instruction during or after the end of the school year (Jacob and Lefgren 2009, Xia and Kirby 2009, and Manacorda 2012).[2] Specifically, in many states and school districts, students failing to meet a predetermined promotional cutoff are assigned to a remedial instruction program such as a summer school. At the end of this program, students retake the same exams in the failed subject(s) and if they again fail to meet the established standards, they are required to repeat the grade.

Recent trends in test-based promotion policies along with accountability systems have attracted the

[1] As of 2012, 32 states have test-based promotional policies (Zinth 2005 and Rose 2012).

[2] A recent report by the Education Commission of the States find that there are more than 30 states providing summer remediation programs to students (Griffith and Zinth 2009).

interest of many researchers regarding the role of grade retention and remedial education on various outcomes. Several studies find positive and significant effects of grade retention on student achievement (see, for example, Jacob and Lefgren 2004, Greene and Winters 2012, Schwerdt and West 2012). These papers generally find that retention effects fade out as students advance to higher grades. In addition to achievement effects, a few other studies examine the effects of grade retention on high school completion and find large negative effects on higher grades (see, for example, Jacob and Lefgren 2009 and Manacorda 2012).[3] Turning to remedial education, the evidence in the field of economics is rather limited and existing studies find positive but short-lived effects of summer remedial education on student achievement (see, for example, Jacob and Lefgren 2004 and Matsudaira 2008).[4]

Grade retention is a very costly process. Considering an average per-pupil expenditure of $12,608 in 2012 (National Center of Education), the direct cost to society of retaining around 450,000 students per year exceeds $5.6 billion.[5] Although we do not have very reliable numbers on the exact costs of summer remediation programs, total available state funding for summer remediation programs in 2008 amounts to more than $500 million, $40 million and $30 million in California, Illinois and Kentucky, respectively (Griffith and Zinth 2009). Given these large costs, it is crucial to improve our understanding in the role of test-based policies on students' long-run educational and social outcomes.

In this paper, we extend the existing literature along four dimensions. First, using administrative data and exogenous variation derived from the accountability system adopted in Louisiana, we examine the net effects of summer remediation programs and grade retention for fourth and eighth grade students' propensity to later drop out of school. The Louisiana Public School (LPS) system tied summer school attendance and grade retention to predefined scores in standardized tests; thus generating two separate discontinuities from a March exam and a July exam. We use the March discontinuity to estimate the net

[3] Apart from these retention studies in economics, Holmes (1989), in his survey of 47 different empirical studies, find that retained students performed 0.19 to 0.31 standard deviation lower on various subjects than students who were not retained. In a more recent meta-analysis, Allen et al. (2009) show similar adverse retention effects on student achievement. There are also a few other studies looking at the association between grade retention and labor market outcomes (see, for example, Eide and Showalter 2001, and Babcock and Bedard 2011).

[4] See also Cooper at al. (2000) for a review of studies on summer remediation programs.

[5] The estimate for the number of retained students is taken from Warren and Saliba (2012) and covers grades 1 to 8.

effects of summer remedial education and grade retention. To the extent that unobserved characteristics are smooth around the cutoff, the estimates identify the net causal effects. Second, we estimate the effect of grade retention on the probability of dropping out of school using the July cutoff and students who took the July exam. Third, combining the effects from March and July discontinuities and a simple mathematical identity, we back out the summer school effects. Finally, we estimate the impact of summer school and grade retention on the likelihood of committing a juvenile crime for fourth and eighth grade students.

To the best of our knowledge, this paper is the first study to use a Regression Discontinuity Design (RDD) framework and examine (i) the effects (including net and separate effects) of summer school and grade retention on the dropout decision, and (ii) the effects (including net and separate effects) of summer school and grade retention on the probability of committing a juvenile crime.

We have three main findings. Summer school and grade retention in fourth grade have no net effect on the propensity to drop out of school at any point in time, while retention alone appears to increase the drop out probability for male students by more than 6 percentage points. As for summer school, we do not find any effect on the likelihood of dropping out for fourth grade students. Additional empirical evidence lends support to an explanation related to fading out of the summer school effect. Turning to eighth grade students, summer school and grade retention appear to have a strong net effect. Specifically, students who barely miss the March promotional cutoff, and thus qualify for summer remediation, are 2-2.5 percentage points less likely to drop out of school than students who barely meet the promotional cutoff. We also find that grade retention increases the propensity to drop out of school and these effects are more pronounced for female students. Combining the net and retention effects, we observe a large mediating impact of eighth grade summer school on the probability of dropping out. Finally, our results provide some tentative evidence that eighth grade summer school and grade retention have a net negative effect on the juvenile crime probability.

The remainder of the paper is organized as follows. Section 2 discusses the background of accountability system in Louisiana and describes the data. Section 3 describes the econometric methodology. Section 4 presents tests for potential sample selection biases, RDD validation tests, main results and several robustness checks. Conclusions and policy implications are provided in Section 5.

2 Background and Data

2.1 Background

The Louisiana School and District Accountability system, which predates the No Child Left Behind Act, was adopted by the state's Board of Elementary and Secondary Education (BESE) in June 1998. The state set ten and twenty year goals for all public schools and required schools to demonstrate progress toward these goals. As part of the accountability system, the BESE also ended the practice of passing students to the next grade regardless of their school performance. Under this new test-based promotion policy, students in fourth and eighth grades are required to score at predefined levels on the Louisiana Educational Assessment Program (LEAP) tests for both English Language Arts (ELA) and math to advance to the next grade.

LEAP tests are criterion-referenced tests and are designed to directly align with the state content standards. A student's LEAP test score can be expressed as either a continuous scale score ranging from 100 to 500 points or a discrete achievement level ranging from unsatisfactory to advanced. Students must score at least *Approaching Basic* in both subjects to advance to the next grade. This is equivalent to 263 (269) and 282 (296) scale points in ELA and math LEAP tests, respectively for fourth (eighth) grades.[6] In addition to LEAP tests, students in the "off-grades" (grades 3, 5, 6, 7 and 9) were also given Iowa Tests of Basic Skills (ITBS), a low-stakes norm-referenced test for which scores are compared to a national norm group. LEAP tests are administered in mid-March to all fourth and eighth grade students.[7]

[6] Raw scores are transformed to scaled scores in a three stage process. They are first mapped onto the Item Response Theory scale. They are then converted to a reporting scale and finally, they are equated to reflect the differences in item difficulty.

[7] ITBS tests are also administered on the same day.

Students who fail to achieve the promotional standards in March are required to retake the exams in the failed subject(s) in July. The school districts must offer, at no cost, a minimum 50 hours per subject of summer remediation in ELA and math to students who fail to meet the passing standards in March. The school districts are given the flexibility to determine curriculum used in summer remediation classes but the summer programs are monitored by the state on a regular basis. Evidence from the annual summer school remediation reports and monitoring visits suggest that teachers in summer schools are proficient in the content area in which they are teaching and are using a variety of creative teaching strategies including but not limited to small group instruction, use of hands-on materials and problem solving teams. Large classrooms in some summer programs (around 20 students per classroom) are a commonly addressed concern in annual reports (Pastorek 2010).

Students are not required to attend summer school programs to be eligible for the July testing. Those who pass the July exams move on to the next grade. Students who again fail are required to repeat the grade unless the student is permitted to move to the next grade through a waiver.

2.2 Data

The data for this study comes from the administrative records of the Louisiana Department of Education (LDOE) from 1999 through 2012. The administrative data includes basic information such as student's gender, race, free/reduced lunch and immigrant status, as well as all scores from the LEAP and ITBS tests. Unique state identification numbers allow us to track all the students through their tenure in the public school system and therefore, we are able to identify each school a student was enrolled in from the Fall of 1999 to the Spring of 2012.

Our sample consists of students who were enrolled in the fourth or eighth grade for the first time from the 1999-2000 school year to the 2002-2003 school year. We impose several restrictions on our research sample. First, we omit the first year of the program in order to control for prior achievement (ITBS scores from the third and seventh grades), although including the first year cohort yields very similar results to those presented in the text. Second, we restrict our attention to students who were enrolled in the fourth or eighth grade for the first time in the Fall of 2002 or earlier. Fourth grade students who were retained in the 2002-2003 school year would be enrolled in the twelfth grade by the Fall of 2011, assuming that they did not drop out of school and did not have any other retentions. This restriction allows us to observe even the one-time repeaters from the last cohort of fourth graders over at least three years after the start of high school. Third, we only keep fourth and eighth grade students who were subject to accountability system and took the March LEAP exams and those with nonmissing data on demographic characteristics and prior achievement scores.[8] Finally, students are dropped from our effective sample if they move out of state or exit the public school system prior to determining their drop out status, i.e., transferred to a private/home school. This type of restriction may lead to a selected sample and for that matter may bias the discontinuity estimates if attrition itself is correlated with the promotional standards. We address this issue in Section 3.1 with a detailed discussion on potential sample selection biases. Having imposed these restrictions, we end up with a total sample of 155,182 and 153,953 observations for fourth and eighth grades, respectively.

Table 1 presents the descriptive statistics for all students subject to accountability system from 2000 to 2003 and for subsamples of students who failed to meet the March standard and those students who were retained. Consistent with the state's demographics, the student body largely contains black and white students and they make more than 96 percent of the fourth and eighth grade samples. The fraction of students receiving free/reduced lunches is similar to national averages. Roughly 75 percent of the students who scored below the March cutoff are black in both fourth and eighth grades, they are more likely to come from disadvantaged families and not surprisingly, their prior achievement scores are lower than the state averages. Disproportionate representation of the black students and low prior test scores are even more pronounced for the sample of retained students.

Considering that the national average dropout rate over the last decade was approximately 13 percent,

[8] We dropped fourth and eighth grade students in special education programs and students with Limited English Proficiency if they were not administered the LEAP exams.

(Chapman et al. 2011), Panel B of Table 1 suggests that the dropout rates in Louisiana were significantly higher. Specifically, in Louisiana slightly more than 20 percent of both fourth and eighth graders ended up dropping out of school.[9] Around 23 percent of fourth graders and more than 24 percent of eighth graders failed to meet the March promotional standards between 2000 and 2003. Looking at the subject failure rates, it appears that the math test was more of an obstacle than the ELA test for promotion. Finally, even after considering that more than 10 percent of those who failed one or both of the spring LEAP exams were not retained due to the July waiver, we still observe 8.7 percent of the fourth graders and 11 percent of the eighth graders were retained.

As noted, for students who fail to meet the March standard, summer school participation is not mandatory and unfortunately, the administrative data does not include any information on summer school attendance. That being said, however, aggregate information from the annual summer remediation reports (Pastorek 2010) and our discussions with the LDOE administrators indicate that the participation rate was more than 90 percent among eligible students for both subjects and grades.

3 Empirical Methodology

To obtain the net effect of summer school and grade retention on the likelihood of dropping out of school, we rely on the exogenous variation generated by the accountability policy in Louisiana and estimate the following reduced form equation

$$DS_i = \gamma_0 + \gamma_1 FP_i + f(Index_i) + X_i'\gamma_2 + \epsilon_i \qquad (1)$$

where DS_i is an indicator variable that takes the value of one if student i later dropped out of school. $FP_i = 1\{Index_i\}$ is also an indicator variable that takes the value of one if student i scores below *Approaching Basic* in either of the March ELA or Math LEAP exams, i.e., FP_i takes the value of one if

[9] The dropout rate of 13 percent at the national level is based on the average of event and status dropout rates from 2000 to 2009.

the minimum of the difference between subject-specific March LEAP scores and their respective relevant cutoffs is negative. $Index_i$ denotes the minimum of the subject-specific distances from the respective cutoffs and is given by

$$Index_i = \min[P_{ij} - Cutoff_j]$$

where P_{ij} and $Cutoff_j$ are the LEAP score and the relevant cutoff in subject j ($j \in \{ELA, Math\}$), respectively. The functional form between $Index_i$ and dropping out of school is described by the polynomial function $f(\cdot)$. X_i is a vector of observed covariates and u_i is the error term.

The key identifying assumption underlying this framework is that the function $f(\cdot)$ is continuous through the March promotional cutoff, i.e., unobserved characteristics are smooth around the cutoff. Under this assumption, for students near the cutoff, the coefficient estimate of γ_1 can be interpreted as the net intent to treat (ITT) effect of summer school and grade retention. We also estimate a variant of equation (1) where we replace the March cutoff with July to obtain separate effects of grade retention, using the sample of students who were assigned to summer school and took the July exams.

4 Results

Prior to presenting any results, there are three estimation details to mention. First, in the main RDD specifications, we use a cubic spline as the functional form between the outcome variable and the index score. Graphical analysis of the raw data supports the choice of a cubic polynomial over other higher or lower ordered degrees. To test the robustness of the findings, we also show additional results using varying degrees of polynomials and local linear regressions. Second, we limit our attention to students who scored 100 points below and 100 points above the index score (roughly two standard deviations of the index), which corresponds to 93 (96) percent of our initial fourth (eighth) grade sample. That being said, we experiment with the full range of the index score and alternative bandwidths. Finally, all reported standard errors are clustered at the year by index score.

4.1 Threats to Identification

As noted in Section 2.2, our effective sample consists of students enrolled in fourth or eighth grades from 2000 to 2003 and who have stayed in the public school system until the dropout status is determined. By imposing this sample restriction, we assume that failing to meet the March promotional cutoff is uncorrelated with the likelihood of leaving the Louisiana public school system for any reason (e.g., moving out of state and transferring to private/home school). This may not be true in practice. Ignoring any potential differential attrition just below and just above the cutoff may yield biased estimates in the RDD framework. To check this potential contamination, we define an indicator variable that takes the value of one if the student is an attriter and examine the relationship between failing to meet the March cutoff and attrition outcome. In our sample, around 23 (17) percent of the fourth (eighth) grade students either moved out of state or transferred to a private/home school.[10] Table 2 presents the discontinuity estimates for three different outcomes: (i) moved out of state, (ii) transferred to a private or a home school, and (iii) either one of them. The coefficient estimates from this exercise are all uniformly small and are not statistically different from zero, suggesting equivalent attrition from the left and the right of the discontinuity for both fourth and eighth grade samples.

One other concern regarding the validity of a RDD is the manipulation of the index score. Given the complexity of the grading metric, it is not likely for students to strategically change their scores near the cutoff. As noted in Jacob and Levitt (2003) and Jacob and Lefgren (2004), however, teacher's manipulation under accountability system is more plausible. In the absence of any sorting and/or manipulation of the running variable, we would expect pre-determined characteristics to be smooth through the cutoff. Table 3 presents the discontinuity estimates for several pre-determined characteristics. Panel A reports the effects of failing to meet the March standards for fourth graders, while Panel B reports the effects for eighth graders. The coefficient estimates in both panels are small in magnitude and are highly imprecisely estimated.

[10] There are 202,459 (185,972) fourth (eighth) grade students with available LEAP tests information.

Overall, we do not find any evidence of a threat to the validity of the RDD framework.

4.2 Test Based-Promotion Policies and Dropping out of School

4.2.1 The Net Effect of Summer School and Grade Retention on Dropping Out of School

We begin with a graphical representation of the reduced form estimates. Figure 1 displays the net effect of summer school and grade retention on the probability of dropping out of school. We plot the unconditional means over a window of 100 index score points. Fitted values from a cubic spline are superimposed over these averages. Looking at Panel A of Figure 1, we observe a continuous decreasing trend in the probability of dropping out of school over the index score, suggesting no net effect of summer school and grade retention for fourth graders. As for eighth graders, however, there is a visible discontinuity at the March cutoff, suggesting evidence for a net adverse effect of summer school and grade retention near the cutoff on dropping out. It appears that students who barely miss the March cutoff are less likely to drop out of school.

Turning to regression results, Table 4 presents our main findings. For comparison purposes, we report the results from an OLS regression of failing to meet March cutoff on the probability of dropping out, by limiting our attention to our RDD sample (100 index score points) and controlling for student characteristics, prior ITBS achievement scores in ELA and math and March LEAP scores. We find that failing to meet the Spring standard significantly increases the propensity to drop out. The effect sizes are 6.6 and 9.9 percentage points for fourth and eighth grades, respectively (Column 1, Panels A and B, Table 4).

Columns 2-5 report the reduced form RDD results (ITT effect). The coefficient estimates are based on four different specifications. Column 2 presents the RDD estimates in the absence of any controls, other than the cohort fixed effects, Column 3 adds birth year dummies, Column 4 presents the results with additional control variables (indicators for student's gender, race, free/reduced lunch and immigrant status and prior (third or seventh grade) ELA and math ITBS achievement scores), and lastly Column

5 presents the most extensive specification including the school fixed effects at the time of March exams. Focusing first on fourth graders, it appears that summer school and grade retention have no net effect on the probability of dropping out (Columns 2-5, Panel A, Table 4). The coefficients are all imprecisely estimated and are virtually equal to zero in magnitude. Turning to eighth graders, summer school and grade retention have a net negative impact on dropping out. Specifically, students who barely miss the March promotional cutoff are 2.4 percentage points less likely to drop out of school than those who barely pass the threshold (Column 2, Panel B, Table 4). Assuming that a student's relative position in the vicinity of the cutoff is as good as random, the RDD estimates should be insensitive to the inclusion of any pre-determined controls. As is visible from Columns 3-5 in Panel B, adding these variables do not significantly alter the discontinuity estimates. Taking the average dropout rate in the control group (promoted eighth graders in March) as our benchmark (13 percent), the estimated effect from our preferred specification (Column 4) implies a net reduction of slightly more than 15 percent for those who barely fail to meet the promotional standard in March.

Comparing our RDD results with the naive estimates from Column 1 of Table 4, we observe evidence for non-negligible positive selection biases in the simple OLS estimations in both grades.

4.2.2 Robustness Checks and Heterogeneity Effects

We undertake two sensitivity checks to examine the validity of our discontinuity estimates. First, rather than using a cubic spline, we estimate the net effect of summer school and grade retention using a local linear regression. Local linear regression is known to be robust to trends away from the cutoff (Lee and Lemieux, 2010). Optimal bandwidths, (31 and 23 index score points for fourth and eighth grades, respectively), are obtained by applying the procedure in Calonico et al. (2014). The RDD estimates from the local linear regression are reported in the first column of Table 5. Second, in Columns 2-4, we provide evidence from a quartic spline using the full range of index score points, a quadratic spline where we limit the index score to 50 points below and 50 points above the March cutoff and finally, a linear spline with a bandwidth size of 25 index score points. The results from these alternative specifications are very similar to our main estimates from Table 4.

We also attempt to extend our analysis to see whether there are any heterogeneous effects along student's gender, race and family income. As reported in Table 6, the full sample results for fourth graders do not seem to mask any heterogeneity. The net effects of summer school and grade retention are consistently small and are imprecisely estimated among all subgroups of interest. Turning to eighth graders, we find the net effects to be more pronounced for male and black students, although none of the differences are statistically significant.[11]

4.2.3 The Effect of Grade Retention on Dropping Out of School

Thus far, we have focused on the net effect of summer school and grade retention. In this section, we take our analysis one step further to estimate the separate effect of grade retention on the propensity to drop out. As discussed further below, obtaining the effect of retention would also help us to tease out the summer school effect. Recall that students who fail to achieve the promotional standards in March are required to retake the exams in July in the failed subjects in order to advance to the next grade. The second discontinuity generated by the July cutoff allows us to estimate the impact of grade retention on the probability of dropping out. We proceed by estimating a variant of equation (1) where we replace the March cutoff with the one from July for the sample of students who took the summer exam.

Prior to presenting the RDD estimates for grade retention, we have to confirm that (i) attrition is not correlated with the July cutoff, and (ii) predetermined characteristics are smooth around the July cutoff. Tables A1 and A2 in the Appendix provide the empirical tests for threat to identification and we do not find evidence for any potentially confounding effects.

We again begin with a graphical representation. Figure 2 displays the effects of repeating a grade on the probability of dropping out of school. We plot the unconditional means over a window of 100 index

[11] We also examine the heterogeneity with respect to prior achievement. The net positive effect of summer school and grade retention is more pronounced for top achieving eighth grade students. These results are available upon request.

score points. Fitted values from a cubic spline are superimposed over these averages. Unlike the net effect RDD estimates, we observe a sharp discontinuity at the July cutoff for the fourth grade sample, suggesting evidence for a positive effect of grade retention on the probability of dropping out of school (Panel A, Figure 2). There is a similar discontinuity at the July cutoff for eighth graders as well (Panel B, Figure 2).

Similar to net effect estimates, we estimate four different specifications and report the OLS results from a regression of failing to meet the July cutoff on the probability of dropping out. Table 7 presents the results. Looking at the first row of Panel A of Table 7, we see that students who barely miss the July cutoff are around 3 percentage points more likely to drop out of school (Columns 2-5, Panel A, Table 7). Turning to eighth graders, we observe similar effect sizes to those obtained from the sample of fourth graders (Columns 2-5, Panel B, Table 7), although the coefficient estimates are less precisely estimated. We also examine the retention effects using local linear regression and varying degrees of polynomials with different bandwidths. The results are similar to those presented in the text and are reported in Table A3 in the Appendix.

Since we observe the actual retention status of the students in our sample, we can also run a fuzzy RDD by instrumenting the retention indicator with the July cutoff indicator. Doing so yields the Local Average Treatment Effect (LATE) and allows us to make a direct comparison with Jacob and Lefgren (2009). Column 6 of Table 7 provides the IV results. Taking the average dropout rate in the control group (promoted students in July) as our benchmark (33 percent), the estimated effect from Column 6 implies a 15 percent increase in the probability of dropping out among four graders. Looking at Panel B and taking the control group mean as our baseline (44 percent), we see that the estimated effect increases the eighth grade propensity to drop out by 7 percent.

Our RDD estimates for grade retention are somewhat different than those presented in Jacob and Lefgren (2009). Using a fuzzy RDD and a similar high-stakes testing policy implemented in Chicago Public School system in sixth and eighth grades, the authors find that grade retention has no effect on the likelihood of dropping out in sixth grade, while repeating eighth grade increases the propensity to drop out only among young eighth graders (ages 14.4 or less) by around 21 percent. They attribute the absence of any effect among sixth graders to the opportunities available to them in the short-run to catch up with their promoted peers. Indeed, they show that sixth grade retention reduced the likelihood of being in the eighth grade two years later by only 34.7 percent, two-thirds of repeaters seem to catch up. Even though the LPS system provides a variety of opportunities in the early grades, any potential differences in the set of opportunities created between the two public school systems may explain the discrepancy in the results.[12] To see this, we estimate the effect of retention on the likelihood that a fourth grade student was enrolled in the eighth grade four years later. The IV estimate from the fuzzy RDD is -0.335 (0.018), suggesting that students in the LPS have very similar catching up rates as students in the Chicago Public School system.[13] Therefore, unlike Jacob and Lefgren (2009), we find long lasting effects of early grade retention on the propensity to drop out.

In addition to this, we do not find any significant differences when we split the sample by younger and older eighth graders. Specifically, we find the IV estimates to be 0.020 (0.020) for young eighth graders, while the retention effect on the probability of dropping out is 0.047 (0.039) for older eighth graders.[14] This may stem from the fact that LPS has no clear defined age policy on eighth grade repeaters (Louisiana Administrative Code, 2005).

We also examine the heterogeneous effects in grade retention. The reduced form results are reported in Table 8. We do not observe considerable differences in the coefficient estimates of grade retention with respect to race and free/reduced lunch status (Panel B and C, Table 8). When we switch our attention to gender, however, we find some interesting results (Panel A). Specifically, the adverse effects of retention on the likelihood of dropping out are observed only for fourth grade male students. As for the eighth

[12] For example, in order to catch up with the original cohort, students are able to attend a fourth grade transitional program, which includes a combination of intensive fourth grade remedial work and fifth grade regular coursework. See Pupil and Progression Procedures of Louisiana for more details (Louisiana Administrative Code, 2005).

[13] In the absence of any catching up, we would expect the IV estimate to be equal to -1.

[14] We use 14.56 years old (average age of the July sample) to distinguish younger and older eighth graders. The results are not sensitive to the use of other thresholds (i.e., 14.4 years old).

grade sample, similar to Jacob and Lefgren (2009), female students seem to be much more affected from grade retention.

4.2.4 The Effect of Summer School

In this section, we will try to tease out the separate effect of summer school (ITT effect) from the net effect of summer school and grade retention. To do so, we follow a similar approach to Jacob and Lefgren (2004) and specify the following identity

$$\gamma_{\text{Summer}} = \gamma_{\text{Net}} - \gamma_{\text{Retain}} * P_{\text{Retain}} \tag{2}$$

where γ_{Summer} is the effect of summer school, γ_{Net} is the net effect of summer school and grade retention (Column 4, Table 4), γ_{Retain} is the effect of grade retention and P_{Retain} is the probability of retention. Since we are trying to back out an estimate for the summer school, it is important to use the full information from the March sample. We can obtain an estimate for P_{Retain} by simply replacing the dependent variable in equation (1) with the retention indicator and the coefficient on March cutoff indicator provides an estimate of the retention probability. Unfortunately, we do not have an estimate for the effect of grade retention that would be consistent with the March sample. To the extent that July sample is a lower ability group than the March sample, our summer school effect estimates would be biased from using the grade retention effects (Column 4, Table 7). Nevertheless, using the retention coefficients from July sample still provide some insights regarding the magnitude of summer school effect.

Table 9 presents the estimated effects of summer school for the full sample, as well as for subgroups. For each subgroup, we separately estimate P_{Retain}. Not surprisingly, we find the summer school effect on the likelihood of dropping out of school to be small and even to be positive for some subgroups (females and whites) in the fourth grade sample. Specifically, the estimated effect relative to control group's mean is only 2.7 percent for the full sample (Column 1, Table 9). As for eighth graders, however, the impact of summer school for the full sample is 2.4 percentage points. This roughly corresponds to more than an 18 percent decrease in the probability of dropping out for students who barely miss the March promotional cutoff over students who barely meet the standard. We observe similar results when the analysis is extended to the eighth grade subgroups.(Columns 2- 7, Table 9). Of course, these estimates hinge upon the assumption that the retention effect is the same for the March and July samples. The grade retention effect for the eighth grade March sample must be more than five times as large as the effect from the July sample to rule out any mediating effects of summer school on the probability to dropout, while an effect of the same size is enough to rule out any fourth grade summer school effect.

One reason why summer remediation programs among fourth graders do not have a large impact on schooling is that the effect of summer remediation programs may not be persistent and may have faded out by the time fourth graders have reached the legal dropout age (see, for example, Chetty et al. 2011 and Schwerdt and West 2013). To examine this hypothesis further, we estimate the net impact of summer school and grade retention on follow-up test scores in grades 5 to 7. Recall that students in grades 5, 6 and 7 were also given ITBS tests. Identical scaling in ITBS tests across grades allows us to evaluate the gains (or losses) over time. Note also that the net effect estimates based on a same grade comparison are likely to be confounded by retained students since most retained students were likely to be one year older and were in school for an additional year at the time the relevant test was administered. Nevertheless, there is still value in observing the trends in net effects over grades.[15] The results from this exercise are given in Table 10 and show that the net gains exhibit a downward trend as students advance to higher grades. This result is consistent with a fading out explanation.[16]

4.3 Test-Based Promotion Policies and Juvenile Crime

Even though the primary purpose of test-based promotion policies under an accountability system is to provide students with additional instruction prior to confronting more challenging academic material

[15] Ideally, we would like to back out the effect of summer school on achievement for grades 5-7. This, however, is not plausible since retention effects on a same grade comparison are likely to reflect age and additional schooling effects as well.

[16] Alternatively, it may also be the case that the impact of summer school interventions are age-specific and that older children benefit more from them.

and reinforce knowledge, summer remediation programs and/or grade retention may also have non-achievement effects. One such effect is on the propensity to be involved in delinquent behavior. There are various routes to why summer school and/or grade retention may affect juvenile crime. For one thing, test-based promotion policies may increase parental involvement in child and parental involvement may prevent juvenile delinquency. Summer schools may also inhibit delinquency by keeping idle youth occupied and leaving less time for crime, the so-called incapacitation effect (see, for example, Jacob and Lefgren 2003). Alternatively, summer school and grade retention may have adverse effects if test-based promotion policies lead to, say, lower socio-emotional outcomes and demoralize students because of stigmatization or disenfranchisement.

To examine the net effect of summer school and grade retention on juvenile crime, we estimate the following reduced form model

$$JC_i = \beta_0 + \beta_1 FP_i + f(Index_i) + X_i'\beta_2 + v_i \tag{3}$$

where JC_i is an indicator variable that takes the value of one if student i committed a post-March crime over the window in which juvenile crime is measured, v_i is the error term and all other variables are as previously defined.[17]

Our crime data come from the Louisiana Department of Public Safety and Corrections, Youth Services, Office of Juvenile Justice. By special permission, we obtain access to juvenile justice files that provide information on all entries occurring in the state for the period 1999-2012 in which the juvenile was found to be delinquent.[18] Thus, our juvenile crime data is likely to reflect the upper end of the crime involvement spectrum. The files include the type of crime the individual committed, the date the individual was admitted to the juvenile justice system, and the location of the offense. In addition, we are able to observe the same state identification number in the juvenile justice data that was also in the LPS which

[17] We take the post-LEAP test period to begin with March 30th of the year the LEAP test was taken.

[18] The upper age for juvenile court jurisdiction in the state of Louisiana is 17 years old.

allows us to merge these two data sets.

Figure 3 displays the net effect of summer school and grade retention on the probability of committing a crime, using the same index score range and the functional form. For both grades, the juvenile crime trend is decreasing in the index score and we observe a discontinuity at the March cutoff. Table 11 presents the regression results. As displayed in Column 1 of Table 11, the average post-exam juvenile crime rate is higher for fourth graders (4.5 percent) than for eighth graders (2.5 percent). Having a higher juvenile crime rate for fourth graders is not surprising given a longer post-exam spell. OLS estimates are given in Column 2 and are positive. However, the discontinuity estimates are consistently negative and similar in magnitude across specifications. Focusing on our preferred specification, reported in Column 5 of Table 11, we see that the coefficient estimates of the net effect of summer school and grade retention on juvenile crime are negative for both fourth and eighth grades, although the estimates are only marginally significant for eighth graders. Switching to a local linear regression and alternative degrees of polynomials with different bandwidths do not alter the findings (Table A4 in the Appendix).

To further isolate the channels of summer school and grade retention on juvenile crime, we estimate the effects of grade retention for students who took the summer tests. Specifically, we replace the dependent variable in equation (3) with an indicator function denoting a post-July crime and the March cutoff with the one from July.[19] Table 11 reports the effects of grade retention on juvenile crime. The discontinuity estimates for fourth graders are positive but imprecisely estimated. As for eighth grade sample, the effects of grade retention are all virtually equal to zero.

Overall, the lack of a strong statistical power does not allow us to make firm conclusions regarding the net effect of summer school and grade retention on the probability of committing juvenile crime. That being said, however, the robustness of the discontinuity estimates accompanied with some precision provide suggestive evidence that eighth grade students who barely miss the March cutoff are less likely to be involved in delinquency behavior than those students who barely meet the cutoff. Moreover, small

[19] We take the post-LEAP test period to begin with July 15th of the year the LEAP test was taken.

coefficient estimates from the July sample may suggest that the mediating effects of failing to meet the March cutoff are driven by summer school effect. Remedial education may indeed help preventing delinquent behavior.

5 Conclusion

Obtaining convincing estimates of the effects of test-based promotion policies on educational/social outcomes is a daunting task. The main problem stems from the fact that students are not randomly selected for summer remediation programs and grade retention. We overcome the challenges to identification using an exogenous variation generated by the accountability system adopted in Louisiana. Utilizing administrative data, we reach a number of policy relevant conclusions.

First, we do not find any net effect of summer remediation programs and grade retention on the likelihood of dropping out of school for fourth grade students, while fourth grade retention appears to have a long-lasting effect for male students and increases their probability of dropping out of school by more than 6 percentage points. Using a simple mathematical identity and backing out the effect of summer school, we generally find small effects of the fourth grade summer remediation programs on the likelihood of dropping out. Further examination of the association between test-based promotion policies and achievement from subsequent grades provides supportive evidence in favor of an explanation related to fading out of the summer school effect. Second, we find a strong net effect of summer remediation programs and grade retention on the propensity to drop out for eighth grade students. Students who barely miss the March promotional cutoff are 2-2.5 percentage points less likely to drop out of school than students who barely meet the promotional cutoff. As for grade retention, however, we find that retained students are more likely to drop out and grade retention effects are significantly more pronounced for females. Combining these two effects and using the same backing out strategy, we find a large decreasing effect of eighth grade summer school on the propensity to drop out. Third, even though the lack of strong statistical power prevents us from making firm conclusions, our results provide tentative evidence that eighth grade summer school and grade retention has a net negative effect on the probability of committing juvenile crime. Several robustness checks presented throughout the paper support our findings.

It may be premature to propose any policy recommendation on test-based promotion policies without fully considering any potential spillover effects to peers and/or private costs (e.g., delayed labor market entry due to retention). That being said, we noted a direct cost of $5.6 billion to society per year from grade retention. Given this large cost and adverse effects of retention coupled with the encouraging results from summer programs, enhancing remedial interventions both in summer and during the school year may be an exceptionally cost-effective way of producing optimum outcomes, at least for higher grades.

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Table 1: Summary Statistics

	Fourth Grade			Eighth Grade		
	Total	Failed March Promotional Cutoff	Retained	Total	Failed March Promotional Cutoff	Retained
	Mean (Standard Deviation)					
Panel A: Student Characteristics						
Black	0.477	0.756	0.858	0.440	0.746	0.841
	(0.499)	(0.429)	(0.348)	(0.496)	(0.434)	(0.365)
White	0.489	0.220	0.126	0.526	0.230	0.141
	(0.499)	(0.414)	(0.332)	(0.499)	(0.421)	(0.348)
Female	0.504	0.464	0.470	0.516	0.510	0.534
	(0.499)	(0.498)	(0.499)	(0.499)	(0.499)	(0.498)
Free/Reduced Lunch	0.487	0.646	0.684	0.382	0.502	0.489
	(0.499)	(0.477)	(0.464)	(0.486)	(0.499)	(0.499)
Age at Test	10.274	10.487	10.408	14.338	14.643	14.560
	(0.613)	(0.761)	(0.754)	(0.704)	(0.859)	(0.821)
Prior (ITBS) Math Test Score	182.71	164.93	162.33	235.20	204.87	200.11
	(19.61)	(13.25)	(11.07)	(37.38)	(45.30)	(37.07)
Prior (ITBS) ELA Test Score	189.29	169.63	166.79	243.33	214.62	211.21
	(22.26)	(15.21)	(13.06)	(40.40)	(48.18)	(44.36)
Panel B: Outcome and Accountability Measures						
Drop Out of School	0.211	0.440	0.476	0.221	0.497	0.533
	(0.408)	(0.496)	(0.499)	(0.415)	(0.499)	(0.498)
LEAP Math Test Score (March)	320.70	254.25	244.69	320.82	265.45	260.92
	(51.82)	(37.88)	(39.02)	(44.99)	(39.26)	(38.16)
LEAP ELA Test Score (March)	312.61	252.55	244.63	319.20	274.57	272.74
	(50.59)	(44.56)	(42.87)	(41.92)	(39.93)	(35.32)
Passed March Promotion Cutoff	0.769	0.000	0.003	0.751	0.000	0.002
	(0.421)	(0.000)	(0.057)	(0.432)	(0.000)	(0.047)
Failed ELA only (March)	0.032	0.141	0.092	0.014	0.059	0.033
	(0.177)	(0.348)	(0.289)	(0.120)	(0.236)	(0.181)
Failed Math only (March)	0.100	0.437	0.340	0.156	0.629	0.606
	(0.301)	(0.496)	(0.473)	(0.363)	(0.483)	(0.488)
Failed ELA and Math (March)	0.096	0.421	0.564	0.077	0.311	0.357
	(0.295)	(0.493)	(0.495)	(0.267)	(0.462)	(0.479)
March Waiver	0.022	0.096	0.000	0.029	0.116	0.000
	(0.147)	(0.295)	(0.000)	(0.168)	(0.321)	(0.000)
July Waiver	0.027	0.118	0.000	0.031	0.126	0.000
	(0.162)	(0.322)	(0.000)	(0.175)	(0.322)	(0.000)
Retained	0.087	0.380	1.000	0.110	0.443	1.000
	(0.283)	(0.485)	(0.000)	(0.313)	(0.496)	(0.000)
Sample Size	155,182	35,756	13,647	153,953	38,315	17,028

NOTES: The statistics above reflect our research sample, which consists of students enrolled in regular classes in grades 4 or 8 between 2000 and 2003, took March ELA and math LEAP exams, stayed in the Louisiana public school system with known dropout status as of Spring 2012. The variables are only a subset of those used in the analysis. The remainder are excluded in the interest of brevity. The full set of sample statistics are available upon request. See text for further details.

Table 2: The Net Effect of Summer School and Grade Retention on Moving Out of State and/or Transferring to Private/Home School

	Fourth Grade	Eighth Grade
Dependent Variables:	Coefficients (Standard Error)	
Panel A: Moving Out of State		
Failed March Promotion Cutoff	-0.006	0.003
	(0.004)	(0.004)
	[175,178]	[171,644]
Panel B: Transferring to Private/Home School		
Failed March Promotion Cutoff	0.005	-0.000
	(0.005)	(0.003)
	[171,635]	[169,278]
Panel C: Moving Out of State or Transferring to Private/Home School		
Failed March Promotion Cutoff	-0.000	0.002
	(0.005)	(0.005)
	[187,052]	[179,021]

NOTES: Grade-specific samples include students who moved out of state and/or students who transferred to private/home school before dropout status is determined, in addition to all observations from Table 1. Samples are restricted to students who scored within 100 points of the March index score. Standard errors are clustered at the year by index score. All specifications control for separate cubic trends in index score. Covariates include indicators for student's birth year, gender, race, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and indicators for cohort fixed effects. Sample sizes are reported in square brackets.

Table 3: Regression Discontinuity Validation Tests

	Female	Black	White	Free/Reduced Lunch	Prior Math Achievement
	Coefficients (Standard Error)				
	(1)	(2)	(3)	(4)	(5)
Panel A: Fourth Grade (N=143,873)					
Failed March Promotion Cutoff	0.014	-0.006	0.008	-0.001	-0.323
	(0.017)	(0.009)	(0.008)	(0.010)	(0.386)
Panel B: Eighth Grade (N=148,705)					
Failed March Promotion Cutoff	0.004	-0.012	0.007	-0.004	0.062
	(0.015)	(0.009)	(0.009)	(0.010)	(1.143)

NOTES: Grade-specific samples are restricted to students who scored within 100 points of the March index score. Standard errors are clustered at the year by index score. All specifications control for separate cubic trends in index score and indicators for cohort fixed effects. N represents sample sizes.

Table 4: OLS and Regression Discontinuity Estimates of the Net Effect of Summer School and Grade Retention on Dropping Out of School

	Coefficients (Standard Errors)				
	OLS				
	(1)	(2)	(3)	(4)	(5)
Panel A: Fourth Grade (N=143,873)					
Failed March Promotion Cutoff	0.066***	-0.004	0.000	0.000	0.001
	(0.004)	(0.010)	(0.008)	(0.008)	(0.008)
Panel B: Eighth Grade (N=148,705)					
Failed March Promotion Cutoff	0.099***	-0.024**	-0.020**	-0.020**	-0.020**
	(0.004)	(0.010)	(0.010)	(0.009)	(0.009)
Controls:					
Cohort Fixed Effects		Yes	Yes	Yes	Yes
Birth Year Effects		No	Yes	Yes	Yes
Covariates		No	No	Yes	Yes
School Fixed Effects		No	No	No	Yes

NOTES: Grade-specific samples are restricted to students who scored within 100 points of the March index score. Robust standard errors reported in column (1), while standard errors in columns (2)-(5) are clustered at the year by index score. Specifications in columns (2)-(5) control for separate cubic trends in index score. Covariates include indicators for student's birth year, gender, race, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and indicators for cohort fixed effects. In addition to these controls, OLS regressions in column (1) control for March ELA and math LEAP scores. N represents sample sizes.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5: Robustness Checks- Regression Discontinuity Estimates of the Net Effect of Summer School and Grade Retention on Dropping Out of School

	Local Linear Regression	Quartic Spline Full Sample	Quadratic Spline Index= [-50,50]	Linear Spline Index=[-25,25]
	Coefficients (Standard Error)			
	(1)	(2)	(3)	(4)
Panel A: Fourth Grade				
Failed March Promotion Cutoff	-0.002	-0.003	0.001	0.003
	(0.009)	(0.008)	(0.008)	(0.007)
	[155,182]	[155,182]	[90,825]	[46,420]
Panel B: Eighth Grade				
Failed March Promotion Cutoff	-0.017*	-0.025***	-0.020**	-0.014*
	(0.009)	(0.009)	(0.009)	(0.008)
	[153,953]	[153,953]	[108,860]	[59,810]
Controls:				
Cohort Fixed Effects	Yes	Yes	Yes	Yes
Birth Year Effects	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes

NOTES: Optimal bandwidths for local linear regression estimations in column (1) are 31 and 23 index points for fourth and eighth grades, respectively. Standard errors are clustered at the year by index score. All specifications in columns (2)-(4) control for separate trends in index score. Covariates include indicators for student's birth year, gender, race, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and indicators for cohort fixed effects. Sample sizes are reported in square brackets.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6: Regression Discontinuity Estimates of the Net Effect of Summer School and Grade Retention on Dropping Out of School for Subgroups

	Fourth Grade	Eighth Grade
	Coefficients (Standard Error)	
Panel A: Gender		
Male		
Failed March Promotion Cutoff	-0.010	-0.029**
	(0.011)	(0.013)
	[71,070]	[71,614]
Female		
Failed March Promotion Cutoff	0.011	-0.013
	(0.010)	(0.010)
	[72,803]	[77,091]
χ^2 test of equal coefficients (p-value)	0.16	0.34
Panel B: Race		
Black		
Failed March Promotion Cutoff	-0.005	-0.022**
	(0.009)	(0.011)
	[70,531]	[65,557]
White		
Failed March Promotion Cutoff	0.010	-0.015
	(0.014)	(0.013)
	[68,558]	[78,315]
χ^2 test of equal coefficients (p-value)	0.37	0.66
Panel C: Family Income		
Free/Reduced Lunch		
Failed March Promotion Cutoff	0.001	-0.020**
	(0.009)	(0.010)
	[72,047]	[57,426]
No Free/Reduced Lunch		
Failed March Promotion Cutoff	-0.001	-0.019
	(0.014)	(0.013)
	[71,826]	[91,279]
χ^2 test of equal coefficients (p-value)	0.87	0.95

NOTES: Grade-specific samples are restricted to students who scored within 100 points of the March index score. Standard errors are clustered at the year by index score. All specifications control for separate cubic trends in index score. Panel A controls for student's birth year, race, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and cohort fixed effects, Panel B controls for student's birth year, gender, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and cohort fixed effects and Panel C controls student's birth year, gender, race, immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and cohort fixed effects. Sample sizes are reported in square brackets.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 7: OLS and Regression Discontinuity Estimates of Grade Retention on Dropping Out of School

	Coefficients (Standard Error)					
	OLS					**Fuzzy RDD**
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Fourth Grade (N=31,173)						
Failed July Promotion Cutoff	0.094***	0.030*	0.033**	0.033**	0.032**	…..
	(0.005)	(0.018)	(0.015)	(0.015)	(0.014)	
Retained in Grade	0.078***	…..	…..	…..	…..	0.051**
	(0.005)					(0.023)
Panel B: Eighth Grade (N=32,450)						
Failed July Promotion Cutoff	0.155***	0.029*	0.021	0.024*	0.020	…..
	(0.057)	(0.015)	(0.014)	(0.013)	(0.013)	
Retained in Grade	0.116***	…..	…..	…..	…..	0.033*
	(0.005)					(0.018)
Controls:						
Cohort Year Effects		Yes	Yes	Yes	Yes	Yes
Birth Year Effects		No	Yes	Yes	Yes	Yes
Covariates		No	No	Yes	Yes	Yes
School Fixed Effects		No	No	No	Yes	No

NOTES: Grade-specific samples are restricted to students who scored within 100 points of the July index score. Robust standard errors are reported in column (1), while standard errors in columns (2)-(6) are clustered at the year by index score. Specifications in columns (2)-(6) control for separate cubic trends in index score. Covariates include indicators for student's birth year, gender, race, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and indicators for cohort fixed effects. In addition to these controls, OLS regressions in column (1) control for March ELA and math LEAP scores. N represents sample sizes.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8: Regression Discontinuity Estimates of Grade Retention on Dropping Out of School for Subgroups

	Fourth Grade	Eighth Grade
	Coefficients (Standard Error)	
Panel A: Gender		
Male		
Failed July Promotion Cutoff	0.066***	0.002
	(0.021)	(0.018)
	[16,371]	[15,263]
Female		
Failed July Promotion Cutoff	-0.005	0.041**
	(0.019)	(0.017)
	[14,802]	[17,187]
χ^2 test of equal coefficients (p-value)	0.01	0.12
Panel B: Race		
Black		
Failed July Promotion Cutoff	0.024	0.021
	(0.016)	(0.015)
	[23,869]	[24,403]
White		
Failed July Promotion Cutoff	0.058*	0.046
	(0.031)	(0.035)
	[6,611]	[7,313]
χ^2 test of equal coefficients (p-value)	0.32	0.52
Panel C: Family Income		
Free/Reduced Lunch		
Failed July Promotion Cutoff	0.028	0.036**
	(0.020)	(0.015)
	[20,129]	[16,297]
No Free/Reduced Lunch		
Failed July Promotion Cutoff	0.039*	0.010
	(0.023)	(0.018)
	[11,044]	[16,153]
χ^2 test of equal coefficients (p-value)	0.72	0.28

NOTES: Grade-specific samples are restricted to students who scored within 100 points of the July index score. Standard errors are clustered at the year by index score. All specifications control for separate cubic trends in index score. Panel A controls for student's birth year, race, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and cohort fixed effects, Panel B controls for student's birth year, gender, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and cohort fixed effects and Panel C controls student's birth year, gender, race, immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and cohort fixed effects. Sample sizes are reported in square brackets.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 9: The Effect of Summer School on Dropping Out of School

	Estimated Effect (% of Average March Control Group Dropout Rate)						
	Full Sample	**Male**	**Female**	**Black**	**White**	**Free Lunch**	**No Free Lunch**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fourth Grade	-0.004 (2.7%)	-0.018 (10.6%)	0.011 (9.3%)	-0.008 (4.4%)	0.004 (3.3%)	-0.003 (1.7%)	-0.005 (4.1%)
Eighth Grade	-0.024 (18.5%)	-0.029 (18.4%)	-0.021 (20%)	-0.027 (18.2%)	-0.021 (17.3%)	-0.027 (17.3%)	-0.021 (18.1%)

NOTES: The estimated summer school effects are computed by subtracting the adjusted retention coefficient estimates from the net effect estimates of summer school and grade retention. The adjusted coefficient estimate is obtained by multiplying the retention estimates with the probability of being retained for the March sample. Summer school effects taking the average March control group as our baseline are reported in parentheses.

Table 10: The Net Effect of Summer School and Grade Retention in Fourth Grade on ITBS Exams from Subsequent Grades

	ELA ITBS Exam	Math ITBS Exam
	Coefficients (Standard Error)	
Panel A: Fifth Grade (N=123,859)		
Failed March Promotion Cutoff	1.919**	2.308***
	(0.985)	(0.837)
[Mean ITBS, SD of ITBS]	[225.29, 35.71]	[216.16, 31.43]
Panel B: Sixth Grade (N=123,859)		
Failed March Promotion Cutoff	1.600**	1.919***
	(0.825)	(0.724)
[Mean ITBS, SD of ITBS]	[233.19, 40.60]	[225.38, 35.48]
Panel C: Seventh Grade (N=123,859)		
Failed March Promotion Cutoff	0.808	0.941
	(0.907)	(0.997)
[Mean ITBS, SD of ITBS]	[246.04, 44.10]	[239.39, 38.47]

NOTES: The sample restricted to students who scored within 100 points of the March index score and who took all fifth, sixth and seventh grade ITBS exams. Standard errors are clustered at the year by index score. Covariates include indicators for student's birth year, gender, race, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and indicators for cohort fixed effects. N represents sample sizes.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 11: OLS and Regression Discontinuity Estimates of the Net Effect of Summer School and Grade Retention on Juvenile Crime

	Mean	OLS	Coefficients (Standard Error)			
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Fourth Grade (N=143,873)						
Failed March Promotion Cutoff	0.045	0.001	-0.006	-0.005	-0.005	-0.005
	(0.207)	(0.002)	(0.006)	(0.006)	(0.006)	(0.006)
Panel B: Eighth Grade (N=148,705)						
Failed March Promotion Cutoff	0.025	0.005***	-0.006*	-0.006*	-0.006*	-0.006*
	(0.156)	(0.001)	(0.003)	(0.003)	(0.003)	(0.003)
Controls:						
Cohort Fixed Effects			Yes	Yes	Yes	Yes
Birth Year Effects			No	Yes	Yes	Yes
Covariates			No	No	Yes	Yes
School Fixed Effects			No	No	No	Yes

NOTES: Grade-specific samples are restricted to students who scored within 100 points of the March index score. Robust standard errors are reported in column (2), while standard errors in columns (3)-(6) are clustered at the year by index score. Specifications in columns (3)-(6) control for separate cubic trends in index score. Covariates include indicators for student's birth year, gender, race, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and indicators for cohort fixed effects. In addition to these controls, OLS regressions in column (2) control for March ELA and math LEAP scores. N represents sample sizes.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 12: OLS and Regression Discontinuity Estimates of Grade Retention on Juvenile Crime

	Mean	OLS	Coefficients (Standard Error)			
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Fourth Grade (N=31,173)						
Failed July Promotion Cutoff	0.075	0.009***	0.009	0.009	0.010	0.012
	(0.262)	(0.003)	(0.008)	(0.008)	(0.008)	(0.008)
Panel B: Eighth Grade (N=32,450)						
Failed July Promotion Cutoff	0.035	0.007***	-0.002	-0.002	-0.001	-0.000
	(0.183)	(0.002)	(0.005)	(0.005)	(0.005)	(0.004)
Controls:						
Cohort Fixed Effects			Yes	Yes	Yes	Yes
Birth Year Effects			No	Yes	Yes	Yes
Covariates			No	No	Yes	Yes
School Fixed Effects			No	No	No	Yes

NOTES: Grade-specific samples are restricted to students who scored within 100 points of the July index score. Robust standard are reported in column (2), while standard errors in columns (3)-(6) are clustered at the year by index score. Specifications in columns (3)-(6) control for separate cubic trends in index score. Covariates include indicators for student's birth year, gender, race, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and indicators for cohort fixed effects. In addition to these controls, OLS regressions in column (2) control for July ELA and math LEAP scores. N represents sample sizes.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Panel A: Fourth Grade Panel B: Eighth Grade

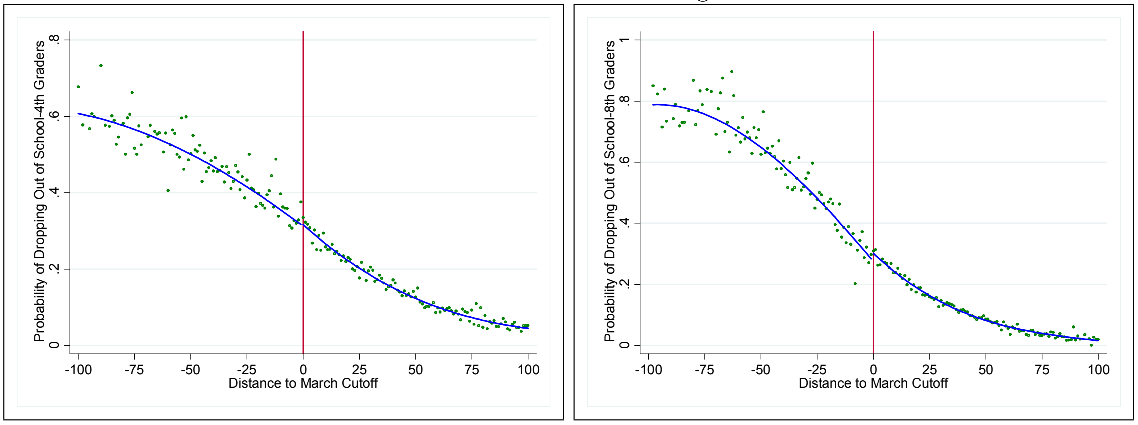

Figure 1: Probability of Dropping out of School and Distance to March Promotional Cutoff

NOTES: The vertical lines denote the March promotional cutoff. Each circle represents the unconditional mean of dropout rates, based on the distance to March cutoff. The solid lines are fitted values of probability of drop out from a cubic spline over an index score of 100 points.

Panel A: Fourth Grade

Panel B: Eighth Grade

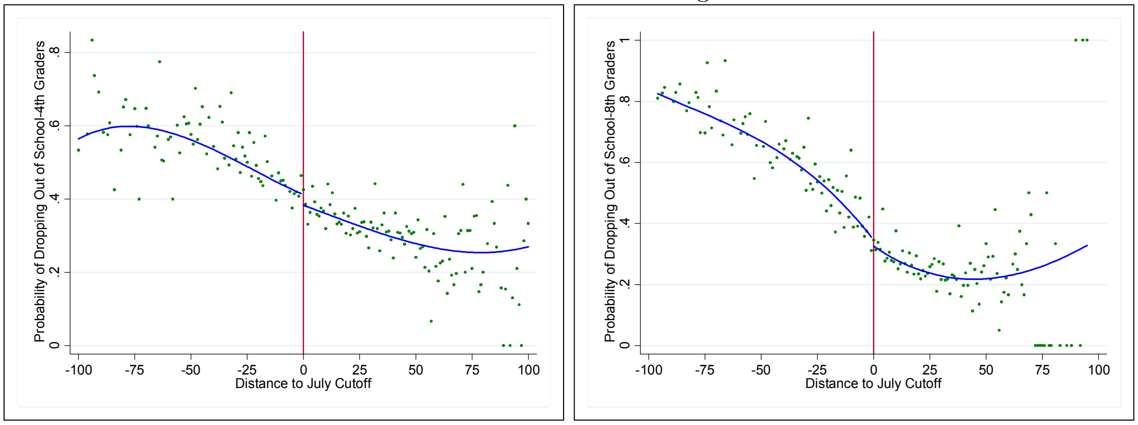

Figure 2: Probability of Dropping out of School and Distance to July Promotional Cutoff

NOTES: The vertical lines denote the July promotional cutoff for students who took the July exam. Each circle represents the unconditional mean of dropout rates, based on the distance to July cutoff. The solid lines are fitted values of probability of drop out from a cubic spline over an index score of 100 points.

Panel A: Fourth Grade

Panel B: Eighth Grade

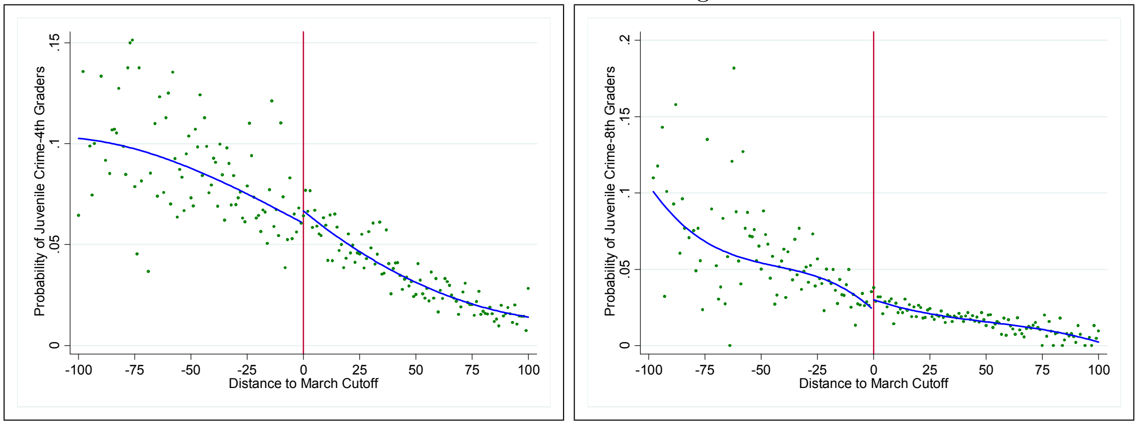

Figure 3: Probability of Juvenile Crime and Distance to March Promotional Cutoff

NOTES: The vertical lines denote the March promotional cutoff. Each circle represents the unconditional mean of juvenile crime committed after the LEAP exam, based on the distance to March cutoff. The solid lines are fitted values of probability of juvenile crime from a cubic spline over an index score of 100 points.

Appendix:

Appendix A1: The Effect of Grade Retention on Moving Out of State and/or Transferring to Private/Home School

	Fourth Grade	Eighth Grade
	Coefficients (Standard Error)	
Panel A: Moving Out of State		
Failed July Promotion Cutoff	-0.006	0.011
	(0.008)	(0.009)
	[41,911]	[40,877]
Panel B: Transferring to Private/Home School		
Failed July Promotion Cutoff	0.006	-0.002
	(0.006)	(0.005)
	[40,979]	[40,198]
Panel C: Moving Out of State or Transferring to Private/Home School		
Failed July Promotion Cutoff	-0.000	0.009
	(0.010)	(0.007)
	[44,396]	[42,474]

NOTES: Grade-specific samples include students who moved out of state and/or students who transferred to private/home school before dropout status is determined, in addition to all observations from Table 1. Samples are restricted to students who scored within 100 points of the July index score. Standard errors are clustered at the year by index score. All specifications control for separate cubic trends in index score. Covariates include indicators for student's birth year, gender, race, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and indicators for cohort fixed effects. Sample sizes are reported in square brackets.

Table A2: Regression Discontinuity Validation Tests-Grade Retention

	Female	Black	White	Free/Reduced Lunch	ITBS Math Score
	Coefficients (Standard Error)				
	(1)	(2)	(3)	(4)	(5)
Panel A: Fourth Grade (N=31,168)					
Failed July Promotion Cutoff	0.010	-0.005	0.000	0.002	-0.231
	(0.021)	(0.015)	(0.014)	(0.016)	(0.482)
Panel B: Eighth Grade (N=32,450)					
Failed July Promotion Cutoff	0.018	-0.003	0.003	-0.017	1.209
	(0.018)	(0.013)	(0.012)	(0.015)	(1.030)

NOTES: Grade-specific samples are restricted to students who scored within 100 points of the July index score. Standard errors are clustered at the year by index score. All specifications control for separate cubic trends and indicators for cohort fixed effects. N represents sample sizes.

Table A3: Robustness Checks- Regression Discontinuity Estimates of Grade Retention on Dropping Out of School

	Local Linear Regression	Quartic Spline Full Sample	Quadratic Spline Index= [-50,50]	Linear Spline Index=[-25,25]
	Coefficients (Standard Error)			
	(1)	(2)	(3)	(4)
Panel A: Fourth Grade				
Failed July Promotion Cutoff	0.025*	0.021	0.030**	0.025**
	(0.015)	(0.015)	(0.015)	(0.013)
	[32,381]	[32,381]	[25,914]	[16,165]
Panel B: Eighth Grade				
Failed July Promotion Cutoff	0.032**	0.023*	0.021	0.039***
	(0.014)	(0.014)	(0.013)	(0.012)
	[33,911]	[33,911]	[28,987]	[20,557]
Controls:				
Cohort Fixed Effects	Yes	Yes	Yes	Yes
Birth Year Effects	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes

NOTES: Optimal bandwidths for local linear regression estimations in column (1) are 30 and 23 index points for fourth and eighth grades, respectively. Standard errors are clustered at the year by index score. All specifications in columns (2)-(4) control for separate trends in index score. Covariates include indicators for student's birth year, gender, race, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and indicators for cohort fixed effects. Sample sizes are reported in square brackets.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table A4: Robustness Checks- Regression Discontinuity Estimates of The Net Effect of Summer School and Grade Retention on Juvenile Crime

	Local Linear Regression	Quartic Spline Full Sample	Quadratic Spline Index=[-50,50]	Linear Spline Index=[-25,25]
	Coefficients (Standard Error)			
	(1)	(2)	(3)	(4)
Panel A: Fourth Grade				
Failed March Promotion Cutoff	-0.006	-0.007	-0.001	-0.006
	(0.005)	(0.006)	(0.006)	(0.005)
	[155,182]	[155,182]	[90,825]	[46,420]
Panel B: Eighth Grade				
Failed March Promotion Cutoff	-0.004	-0.004	-0.005	-0.006
	(0.003)	(0.003)	(0.003)	(0.005)
	[153,953]	[153,953]	[108,860]	[59,810]
Controls:				
Cohort Fixed Effects	Yes	Yes	Yes	Yes
Birth Year Effects	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes

NOTES: Optimal bandwidths for local linear regression estimations in column (1) are 29 and 37 index points for fourth and eighth grades, respectively. Standard errors are clustered at the year by index score. All specifications in columns (2)-(4) control for separate trends in index score. Covariates include indicators for student's birth year, gender, race, free/reduced lunch and immigrant status, prior (3rd or 7th grade) ELA and math achievement scores and indicators for cohort fixed effects. Sample sizes are reported in square brackets.
* significant at 10%, ** significant at 5%, *** significant at 1%.