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Briggs Depew
Louisiana State University

Ozkan Eren
Louisiana State University

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Department of Economics
Louisiana State University
Baton Rouge, LA 70803-6306
http://www.bus.lsu.edu/economics/

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Briggs Depew

Louisiana State University

Ozkan Eren*

Louisiana State University

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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 tests. 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 potential 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 potential 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 assignment 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 assignment on the likelihood of dropping out. Finally, for eighth graders, we find that the test-based promotion policies decrease the probability of committing serious juvenile offenses.

JEL: I21, I28, J13, C21
Keywords: Summer Remediation Programs, Grade Retention, Dropping Out, Juvenile Crime, Regression Discontinuity.

*Corresponding author, E. J. Ourso College of Business, Department of Economics, Louisiana State University, Baton Rouge, LA 70803. Tel. (225)-578-3900. Fax. (225)-578-3807. Email: oeren@lsu.edu. The data used in this study is provided by the Louisiana Department of Education and the Louisiana Office of Juvenile Justice. The authors thank the Office of Social Service Research and Development (OSSRD) in the College of Human Sciences and Education at the Louisiana State University in their guidance with the data. We also thank Louis-Philippe Beland, Brian Jacob, Lars Lefgren and Naci Mocan and seminar participants at the Society of Labor Economists Meeting in Montreal for valuable comments and suggestions.

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 ending social promotion may improve efficiency and achievement by creating a more homogeneous classroom environment for the prepared students.

A test-based promotion policy is usually implemented in two stages and typically uses standardized tests to determine whether a student should advance to the next grade. In many states and school districts, students who fail 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 exams in the failed subject(s) and if they again fail to meet the established standards, they are required to repeat the grade (e.g., Boston, Chicago, New York City, and Washington, DC). The premise for promotion policies 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]

Recent trends in test-based promotion policies along with accountability systems have attracted the interest of many researchers regarding the role of grade retention and remedial education on various outcomes.

[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).

Several studies find that grade retention significantly increases student achievement with the effects generally fading out as students advance to higher grades (see, for example, Jacob and Lefgren 2004 and Greene and Winters 2012). A few other studies show that grade retention in higher grades decrease high school completion (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 remedial education on student achievement (see, for example, Jacob and Lefgren 2004 and Banerjee et al. 2007).[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 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 the test-based promotion policies (summer remediation program assignment and potential grade retention) on fourth and eighth grade students' propensity to later drop out of school. The Louisiana Public School (LPS) system tied summer school assignment 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 effects of the test-based promotion policies. To the extent that unobserved characteristics are smooth around the cutoff, the estimates identify the net causal effects.

[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.

Second, we estimate the effect of potential 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 effect of summer school assignment. In doing so, we follow Jacob and Lefgren (2004) and exploit the fact that the March and July exams have the same promotional cutoff. Finally, we estimate the impact of the test-based promotion policies on the likelihood of committing a juvenile crime for fourth and eighth grade students.[6]

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 assignment and potential grade retention on the dropout decision, and (ii) the effects (including net and separate effects) of summer school assignment and potential grade retention on the probability of committing a juvenile crime.

We have three main findings. Summer school assignment and potential grade retention in fourth grade have no net effect on the propensity to drop out of school at any point in time, while potential 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 impact of summer school assignment. Turning to eighth grade students, the test-based promotion policies 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 potential grade retention increases the propensity to drop out of school and that this effect is more pronounced for female students. Combining the net and retention effects, we observe a large mediating impact of eighth grade summer school assignment on the probability of dropping out. Finally, our results provide evidence that the eighth grade test-based promotion policies decrease the likelihood of committing a juvenile crime. Our further examination of juvenile crime by broad

[6] Throughout the paper, we use "summer school assignment" to refer to students who fail to achieve the March promotional standards (actual summer school attendance is not observable) and "potential grade retention" to refer to students who took the July exam and fail to meet the July cutoff (students who fail the cutoff may still be allowed to move to the next grade through a waiver).

categories indicates that the net effect of the test-based promotion policies is most salient for serious offenses.

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.[7] 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.[8]

[7] 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.

[8] 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 they 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 the accountability system (i.e., took the March LEAP exams) and who had nonmissing data on demographic characteristics and prior achievement scores.[9] Finally, students are dropped from our effective sample if they moved out of state or exited 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 three mutually inclusive groups: all students in our sample, students who failed to meet the March proportional standards, and students who were retained. Consistent with the state's demographics, the student body largely contains black and white students and a fairly equal proportion of black and white students in both fourth and eight grade. The fraction of students receiving free/reduced lunch is similar to national averages. Students who scored below the March cutoff are more likely to be black, come from disadvantaged families and not surprisingly, have lower prior achievement scores. 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, (Chapman et al. 2011), Panel B of Table 1 suggests that the dropout rates in Louisiana were significantly higher.[10] Specifically, in Louisiana slightly more than 20 percent of both fourth and eighth graders ended up

[9] 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.

[10] 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.

dropping out of school.[11] 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 failure rates by subject, it appears that the math test was more of an obstacle for promotion than was the ELA test. Finally, even after considering that around 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 assignment and potential grade retention (the net effect of the test-based promotion policies) 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

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

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 scored below *Approaching Basic* in either of the March ELA or Math LEAP exams, i.e., FP_i takes the value of one if the minimum of the difference between subject-specific March LEAP scores and their respective relevant cutoffs is negative.

[11] The LDOE requires the use of three school years (the previous, current and the following) to identify the dropout status of a student. Therefore, a dropout flag is not complete until after the dropout correction period of the following year. See Pastorek (2011) for identification details of the dropout flag in Louisiana.

$Index_i$ 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 (or the net effect of test-based promotion policies). We also estimate a variant of equation (1) where we replace the March cutoff with July to obtain separate effects of potential 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 local linear regressions and varying degrees of polynomials. 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 had stayed in the public school system until the dropout status was 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 for this type of 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 the 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.[12] 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 they 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. To further circumvent any concerns on sample selection bias, we estimate all the specifications presented throughout the paper by excluding parishes that are known to be most affected from Hurricanes Katrina and Rita. The results from this exercise are almost identical to those presented in the paper and they are available upon request.

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 (gender, race, free or reduced

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

lunch status, and prior Math and ELA achievement scores measured on a scale from 100 to 500 points). 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 they are highly imprecisely estimated.

Finally, Panels A and B of Figure A1 in the Appendix display the March test score distributions (centered on the promotion cutoff) for fourth and eighth graders, respectively. The figures do not indicate any unusual bunching or jumps that would compromise the RDD strategy.

4.2 Test Based-Promotion Policies and Dropping out of School

4.2.1 The Net Effect of the Test-Based Promotion Policies 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 assignment and potential 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 that for fourth graders the test-based promotion policies have no net effect on later dropping out of school. As for eighth graders, however, there is a visible discontinuity at the March cutoff, suggesting that the test-based promotion policies decrease the probability of dropping out, i.e., students who marginally fail the March exam have a lower probability of dropping out than students who marginally pass the March exam.

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 the 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 is associated with an increased propensity to drop out. The coefficient estimates are 0.066 (0.004) and 0.099 (0.004) 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 by additionally including school fixed effects at the time of the March exams. Focusing first on fourth graders, it appears that summer school assignment and potential 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 they are virtually equal to zero in magnitude. Turning to eighth graders, we find that the test-based promotion policies decrease the likelihood of later dropping out of school. 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 not be sensitive 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 of promoted eighth graders in March as our benchmark (13 percent), the estimated effect from our preferred specification (Column 4) implies that students who barely fail to meet the March standards are 15 percent less likely to drop out of school.

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. It is also important to note that the OLS coefficient estimates are positively correlated with the range of the index score points. As such, lowering the range decreases the extent of biases from the naive estimates.

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 assignment and potential grade retention using local linear regressions. Local linear regression is known to be robust to trends away from the cutoff

(Lee and Lemieux, 2010). Optimal bandwidths are obtained by applying the procedure in Calonico et al. (2014) and Imbens and Kalyanaraman (2012).[13] The RDD estimates from the local linear regressions are reported in the first two columns of Table 5. Second, in Columns 3-5, 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 the test-based promotion policies are consistently small and they 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.[14]

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 assignment and potential grade retention. In this section, we take our analysis one step further to estimate the separate effect of potential grade retention on the propensity to drop out. As discussed further below, obtaining the effect of retention would also allow us to provide insights regarding the effect of summer school assignment. Recall that students who fail to achieve the promotional standards in March are required to retake the exam(s) in July in the failed subject(s) in order to advance to the next grade. The second discontinuity generated by the July cutoff allows us to estimate the impact of potential 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

[13] These two procedures yield very similar bandwidth values. Optimal bandwidth values are roughly equal to 31 and 23 index score points for fourth and eighth grades, respectively.

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

sample of students who took the summer exam.

Prior to presenting the RDD estimates for potential grade retention, it is important 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 Panels A and B of Figure A2 display the density of students around the July cutoff for fourth and eighth graders, respectively. We do not find evidence for any potentially confounding effects.

We begin with a graphical representation. Figure 2 displays the effect of potentially repeating a grade 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. Unlike the RDD estimates of the net effect, we observe a sharp discontinuity at the July cutoff for the fourth grade sample, suggesting evidence that potential grade retention increases the probability of dropping out of school (Panel A, Figure 2). There is an analogous discontinuity at the July cutoff for eighth graders (Panel B, Figure 2).

Similar to the analysis of estimating the net effect, we run four different specifications and report the OLS estimates from the 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). As for 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 potential retention effects using local linear regressions and varying degrees of polynomials with different bandwidths. The results are similar to those presented in the text and they 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 of 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.[15] To see this, we estimate the effect of actual 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.[16] Therefore, unlike Jacob and Lefgren (2009), we find long lasting effects of early grade retention on the propensity to drop out. These results suggest that the effect of grade retention on dropping out may depend significantly on the setting and institutions of where it occurs.

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 estimate 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.[17] This may stem from the fact that LPS has no clear defined age policy on eighth grade repeaters (Louisiana Administrative Code, 2005).

[15] 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).

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

[17] 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).

We also examine potential 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 potential 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 potential retention on the likelihood of dropping out are observed only for fourth grade male students. As for the eighth grade sample, similar to Jacob and Lefgren (2009), female students seem to be much more affected from grade retention.[18]

4.2.4 The Effect of Summer School Assignment on Dropping Out of School

In this section, we will try to tease out the separate effect of summer school assignment (ITT effect) from the net effect of the test-based promotion policies. Borrowing from Jacob and Lefgren (2004), we 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 assignment, γ_{Net} is the net effect of the test-based promotion policies (Column 4, Table 4), γ_{Retain} is the effect of potential grade retention and P_{Retain} is the probability of potential retention. Prior to moving forward, it is important to note that this backing out strategy may produce misleading results if students under the accountability system had to face different promotional standards in the March and July exams (thresholds would then fall at different points of the index score distribution and prevent a straightforward comparison). However, the promotional standards in Louisiana for both the March and July exams require students to meet the same exact threshold and this allows us to

[18] We also examine whether differential catching up rates by gender among fourth graders can potentially explain the gender specific effect of potential grade retention in fourth grade. However, it appears that male students are more likely to be enrolled in the eighth grade four years later. The IV estimates from the fuzzy RDD are -0.270 (0.025) and -0.401 (0.028) for male and female students, respectively. Hence a catching up hypothesis may not serve as a potential explanation for the gender gap in retention effects for fourth grade students.

There is a large literature documenting fundamental differences between females and males starting in very early ages (see, for example, Croson and Gneezy 2009 and Bertrand 2011 for survey reviews). The impact of an adverse shock such as grade retention may be age- and gender-specific. Due to later maturation, boys may be more sensitive to early shocks and change of environment (e.g., new peers). During adolescence, however, females may experience more stress and disenfranchisement in situations involving a negative outcome (see, for example, Silverman and Kumka 1987, Fujita et al. 1991, and Spigner et al. 1993).

use the identity from equation (2). Of course, we may still end up overestimating the true effect if students hypothetically perform better on the July exam than the March exam absent of intervention.

Since we are trying to back out an estimate for the effect of summer school assignment, 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 an indicator for failing the July standards and the coefficient on March cutoff indicator provides an estimate of the potential retention probability. Unfortunately, we do not have an estimate for the effect of potential 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 assignment effect estimates would be biased from using the potential grade retention effects (Column 4, Table 7). Nevertheless, using the retention coefficients from July sample still provide important insights regarding the effect of summer school assignment.

Table 9 presents the estimated effects of summer school assignment for the full sample, as well as for subgroups. For each subgroup, we separately estimate P_{Retain}. Not surprisingly, we find the summer school assignment 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. As for eighth graders, however, the impact of summer school assignment for the full sample is -2.5 percentage points. This roughly corresponds to more than a 19 percent decrease in the probability of dropping out for students who barely miss the March promotional cutoff over students who barely meet the standards. We observe similar results when the analysis is extended to the eighth grade subgroups (Columns 2- 7, Table 9). These estimates hinge upon the assumption that the potential retention effect is the same for the March and July samples. The potential 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 assignment on the probability to dropout, while an effect of the same size is enough to rule out any fourth grade summer school assignment 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 the test-based promotion policies 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. However, estimates of the net effect based on a same grade comparison are likely to be confounded by retained students as 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. Since we are not primarily interested in the effect sizes, there is still value in observing the trends in net effects over grades.[19] The trend analysis would have been less informative if confounding effects were to vary over grades but there is no strong prior reason to expect differential confounding effects over time. Having said that, 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.[20]

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 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 how summer school and/or grade retention may affect juvenile crime. For one, test-based promotion policies may increase parental involvement with the 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.

[19] Ideally, we would like to back out the effect of summer school on achievement for grades 5-7. However, the estimates will be biased since retention effects on a same grade comparison are likely to reflect age and additional schooling effects as well.

[20] 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.

To examine the net effect of the test-based promotion policies 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 (March 30th of the year the LEAP tests were taken) through age 17 (upper bound for juvenile court jurisdiction in Louisiana), v_i is the error term and all other variables are as previously defined.

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. 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 (conviction date), and the location of the offense. In addition, we are able to merge the juvenile justice data with the LPS data because we observe the same state identification number in both data sets.

Figure 3 displays the net effect of summer school assignment and potential 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 corresponding 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 they 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 the test-based promotion policies on juvenile crime are negative for both fourth and eighth grades, although the estimates are only marginally significant for eighth graders. It is worth noting that local linear regressions and alternative degrees of polynomials with different bandwidths do not alter the findings (see Table A4 in the Appendix).

We also look at the net effect of the test-based promotion policies on broad crime categories. Specifically, our data includes the Office of Juvenile Justice classification of crimes based on the severity of the offense as follows: (i) felony, (ii) misdemeanor, and (iii) other crimes. Table 12 presents the results from this exercise. It appears that the test-based promotion policies decrease the probability of committing serious crimes for eighth graders (Column 1, Panel B, Table 12). Students who barely miss the March promotional cutoff are 0.4 percentage points less likely to be convicted of a felony. All other discontinuity estimates based on juvenile crime classifications are not statistically different from zero while we find the coefficient estimates on the net effect of test-based promotion policies from Columns 1 to 3 to be jointly significant (p-value=0.02).

To further isolate the channels of summer school and grade retention on juvenile crime, we estimate the effect of potential grade retention for students who took the summer tests. Specifically, we replace the dependent variable in equation (3) with an indicator variable denoting a post-July crime (July 15th of the year the LEAP test was taken through age 17) and the March cutoff with the one from July. Table 13 reports the ITT effects of grade retention on juvenile crime. The discontinuity estimates for fourth graders are positive but imprecisely estimated. As for eighth grade sample, the coefficient estimates of potential grade retention are all virtually equal to zero. Moving on to broad crime categories, Table 14 provides tentative evidence that potential grade retention at fourth grade increases the likelihood of being convicted of a misdemeanor, although, the lack of statistical significance at the conventional level does not allow us to draw a firm conclusion.[21]

Overall, the robustness of the discontinuity estimates accompanied with strong results from serious crimes provide 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 coefficient estimates from the July sample may suggest that the mediating effects of failing to meet the March cutoff are driven

[21] We also fail to reject the test of joint significance for coefficient estimates on the effect of potential grade retention on different crime categories in both fourth and eighth grade samples.

by the summer school impact. Remedial education may indeed help preventing delinquent behavior. Our results are also consistent with a growing body of research on summer youth employment programs which are designed to keep youth involved in socially productive activities. Recent evidence show that summer programs reduce incarceration and youth violence (see, for example, Heller 2014 and Gelber et al. 2015).[22]

5 Conclusion

Credible estimates of the effects of test-based promotion policies on educational/social outcomes are limited. 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 the test-based promotion policies on the likelihood of dropping out of school for fourth grade students, while potential 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 assignment, we generally find small effects of fourth grade summer remediation assignment on the likelihood of dropping out. Further examination of the association between the 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 impact. Second, we find a strong net effect of the test-based promotion policies 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. We also find that potential grade retention increases the probability of dropping out and 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 assignment on the propensity to drop out. Third,

[22] See also Cook and Kang (2014) for a discussion on dropping out and its effects on crime.

our results provide evidence that the eighth grade test-based promotion policies decrease the probability of committing serious juvenile offenses. 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
White	0.489	0.220	0.126	0.526	0.230	0.141
Female	0.504	0.464	0.470	0.516	0.510	0.534
Free/Reduced Lunch	0.487	0.646	0.684	0.382	0.502	0.489
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
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
Failed ELA only (March)	0.032	0.141	0.092	0.014	0.059	0.033
Failed Math only (March)	0.100	0.437	0.340	0.156	0.629	0.606
Failed ELA and Math (March)	0.096	0.421	0.564	0.077	0.311	0.357
March Waiver	0.022	0.096	0.000	0.029	0.116	0.000
July Waiver	0.027	0.118	0.000	0.031	0.126	0.000
Retained	0.087	0.380	1.000	0.110	0.443	1.000
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. Standard deviations are reported for only continuous variables. 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 the Test-Based Promotion Policies 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.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 3: Regression Discontinuity Validation Tests

	Female	Black	White	Free/Reduced Lunch	Prior Math Achievement	Prior ELA Achievement
	Coefficients (Standard Error)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Fourth Grade (N=143,873)						
Failed March Promotion Cutoff	0.014	-0.006	0.008	-0.001	-0.323	0.256
	(0.017)	(0.009)	(0.008)	(0.010)	(0.386)	(0.600)
Panel B: Eighth Grade (N=148,705)						
Failed March Promotion Cutoff	0.004	-0.012	0.007	-0.004	0.062	0.061
	(0.015)	(0.009)	(0.009)	(0.010)	(1.143)	(1.076)

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.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4: OLS and Regression Discontinuity Estimates of the Net Effect of the Test-Based Promotion Policies 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 the Test-Based Promotion Policies on Dropping Out of School

	Local Linear Regression (CCT)	Local Linear Regression (IK)	Quartic Spline Full Sample	Quadratic Spline Index= [-50,50]	Linear Spline Index=[-25,25]
			Coefficients (Standard Error)		
	(1)	(2)	(3)	(4)	(5)
Panel A: Fourth Grade					
Failed March Promotion Cutoff	-0.002	-0.001	-0.003	0.001	0.003
	(0.007)	(0.007)	(0.008)	(0.008)	(0.007)
	[55,616]	[56,943]	[155,182]	[90,825]	[46,420]
Panel B: Eighth Grade					
Failed March Promotion Cutoff	-0.016**	-0.017**	-0.025***	-0.020**	-0.014*
	(0.008)	(0.007)	(0.009)	(0.009)	(0.008)
	[52,490]	[54,844]	[153,953]	[108,860]	[59,810]
Controls:					
Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Birth Year Effects	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes

NOTES: Optimal bandwidths for local linear regression estimations in columns (1) and (2) are obtained using the procedures in Calonico et al. (2014) and Imbens and Kalyanaraman (2012), 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 the Test-Based Promotion Policies 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 Potential and Actual 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 Potential 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 Assignment 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%)	-0.010 (5.9%)	0.006 (5.0%)	-0.007 (3.9%)	0.005 (4.2%)	-0.003 (1.7%)	-0.002 (1.6%)
Eighth Grade	-0.025 (19.2%)	-0.029 (18.3%)	-0.023 (21.9%)	-0.028 (18.9%)	-0.022 (18.1%)	-0.028 (17.9%)	-0.021 (17.9%)

NOTES: The estimated summer school assignment effects are computed by subtracting the adjusted potential retention coefficient estimates from the net effect estimates of the test-based promotion polices. The adjusted coefficient estimates are obtained by multiplying the effect of potential retention estimates with the probability of being retained for the March sample. Summer school assignment effects taking the relevant average dropout rate of promoted students in March as our baseline are reported in parentheses.

Table 10: The Net Effect of the Test-Based Promotion Policies 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 the Test-Based Promotion Policies 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	-0.005	-0.005	-0.005	-0.005
	(0.208)	(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: Regression Discontinuity Estimates of the Net Effect of the Test-Based Promotion Policies on Juvenile Crime Categories

	Felony	Misdemeanor	Other
		Coefficients (Standard Error)	
Panel A: Fourth Grade (N=143,873)			
Failed March Promotion Cutoff	-0.000	-0.000	-0.004
	(0.003)	(0.003)	(0.003)
[Mean Crime]	[0.014]	[0.013]	[0.016]
Panel B: Eighth Grade (N=148,705)			
Failed March Promotion Cutoff	-0.004***	-0.002	0.000
	(0.001)	(0.002)	(0.002)
[Mean Crime]	[0.007]	[0.007]	[0.009]

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. 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.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 13: OLS and Regression Discontinuity Estimates of Potential 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***	0.010	0.010	0.011	0.012*
	(0.262)	(0.003)	(0.008)	(0.008)	(0.008)	(0.007)
Panel B: Eighth Grade (N=32,450)						
Failed July Promotion Cutoff	0.039	0.007***	0.000	0.000	0.000	0.000
	(0.193)	(0.002)	(0.005)	(0.005)	(0.005)	(0.005)
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

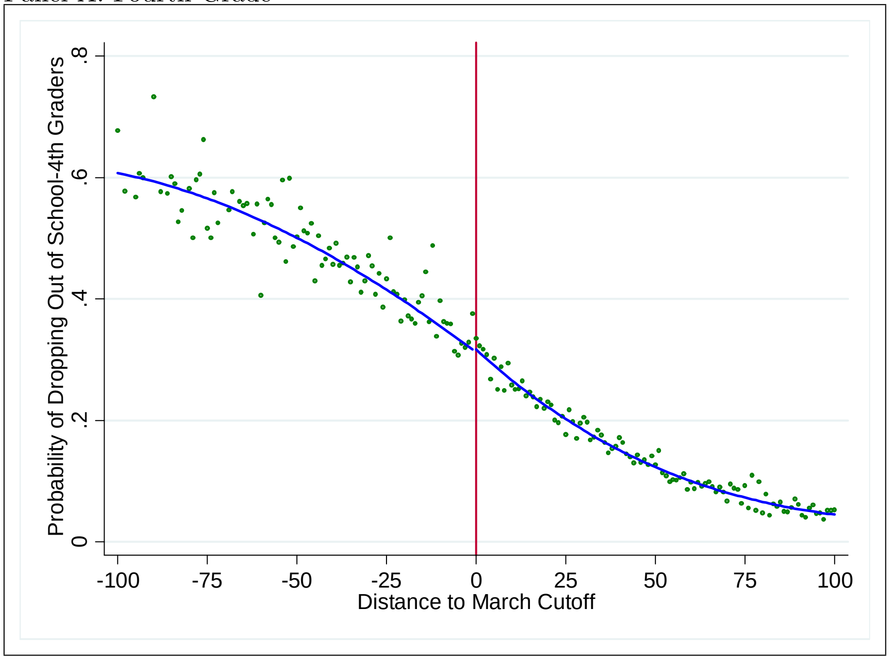
NOTES: Grade-specific samples are restricted to students who scored within 100 points of the July index score. Robust standard are reported in column (2), while standard errors in columns (3)-(6) are clustered at the year by index score. Specifications in columns (3)-(6) control for separate cubic trends 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%.

Table 14: Regression Discontinuity Estimates of Potential Grade Retention on Juvenile Crime Categories

	Felony	Misdemeanor	Other
		Coefficients (Standard Error)	
Panel A: Fourth Grade (N=31,173)			
Failed March Promotion Cutoff	-0.000	0.007*	0.004
	(0.004)	(0.004)	(0.004)
[Mean Crime]	[0.023]	[0.022]	[0.027]
Panel B: Eighth Grade (N=32,450)			
Failed March Promotion Cutoff	0.001	-0.004	0.003
	(0.002)	(0.003)	(0.003)
[Mean Crime]	[0.010]	[0.011]	[0.013]

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. 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.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Panel A: Fourth Grade



Panel B: Eighth Grade

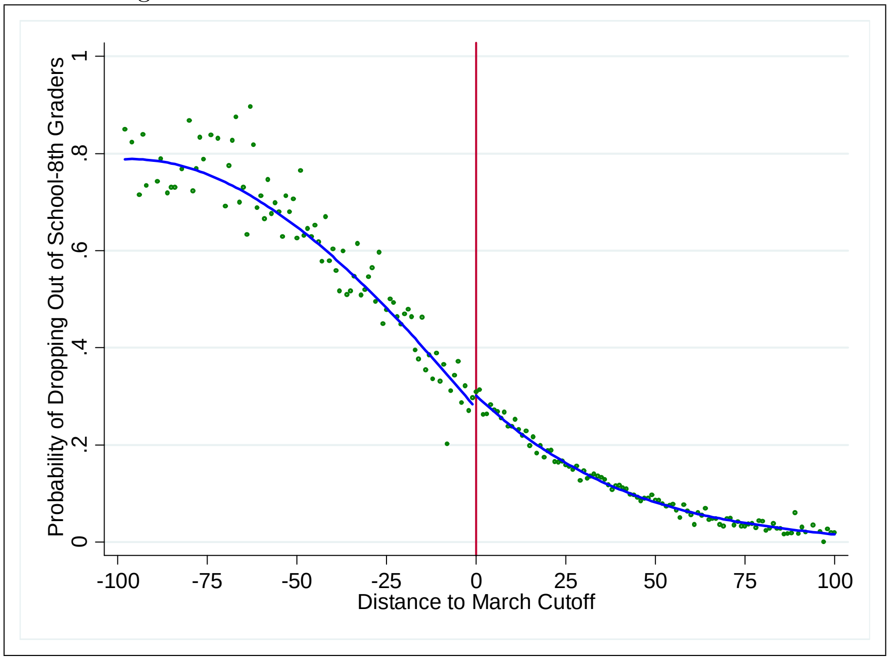
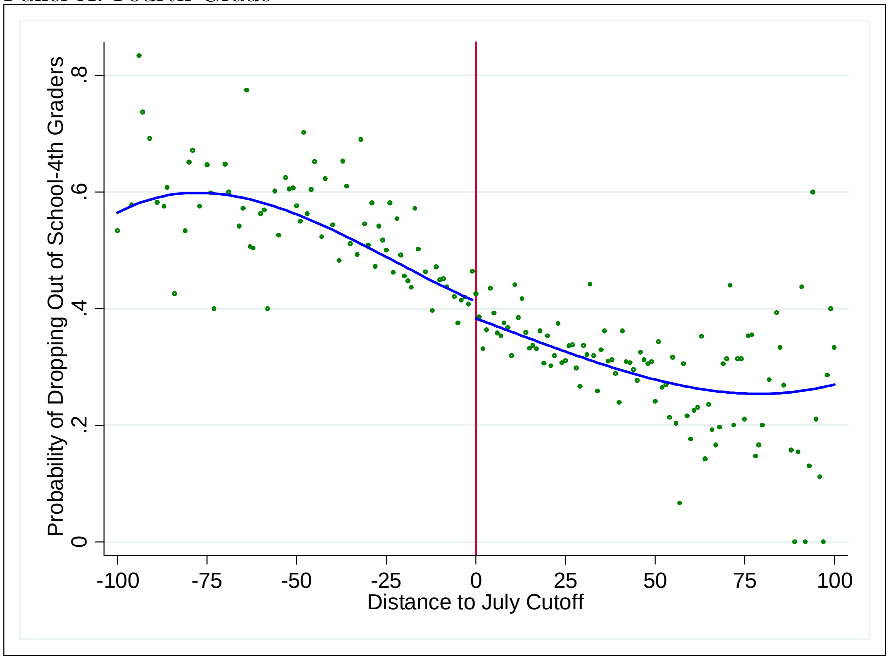


Figure 1: Probability of Dropping out of School and Distance to the March Promotional Cutoff

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

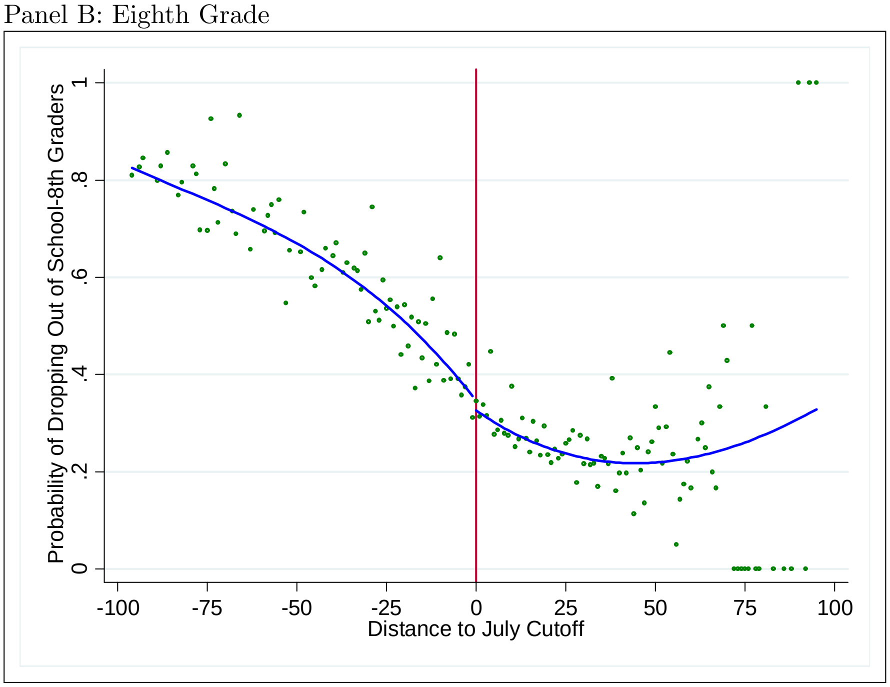
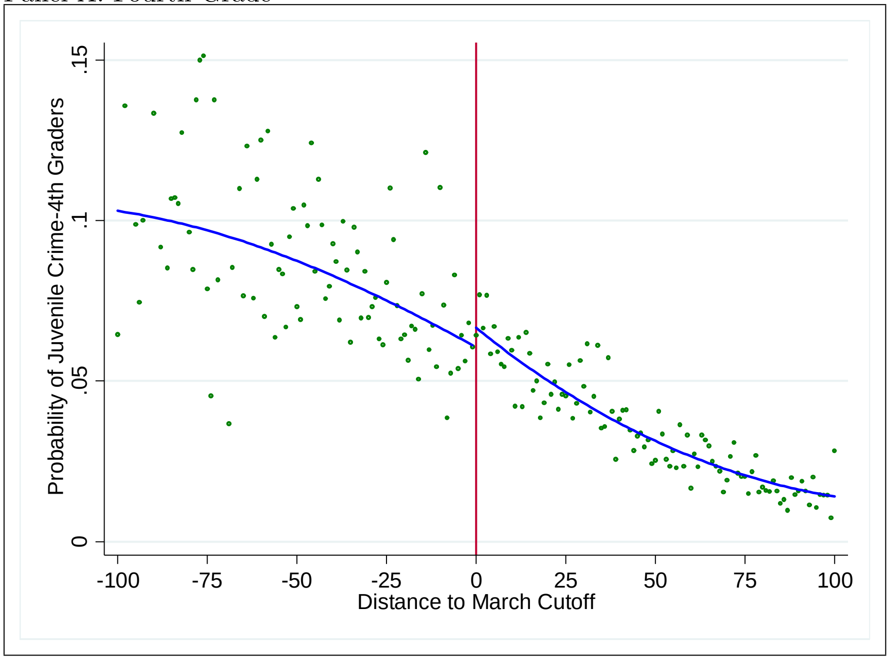


Figure 2: Probability of Dropping out of School and Distance to the July Promotional Cutoff

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

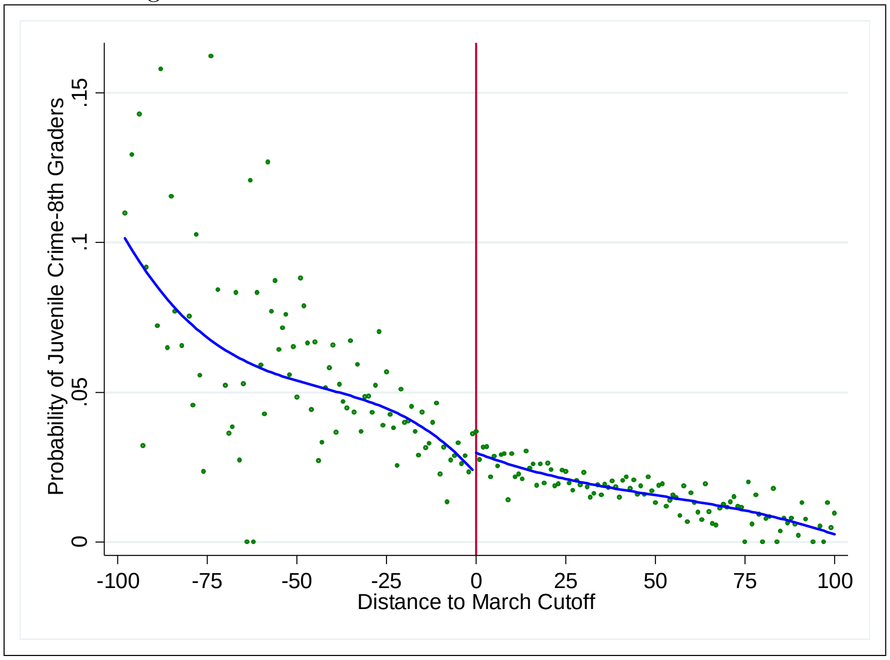


Figure 3: Probability of Juvenile Crime and Distance to the March Promotional Cutoff

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:

Table A1: The Effect of Potential 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.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table A2: Regression Discontinuity Validation Tests-Potential Grade Retention

	Female	Black	White	Free/Reduced Lunch	Prior Math Achievement	Prior ELA Achievement
	Coefficients (Standard Error)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Fourth Grade (N=31,168)						
Failed July Promotion Cutoff	0.010	-0.005	0.000	0.002	-0.231	-0.635
	(0.021)	(0.015)	(0.014)	(0.016)	(0.482)	(0.654)
Panel B: Eighth Grade (N=32,450)						
Failed July Promotion Cutoff	0.018	-0.003	0.003	-0.017	1.209	2.785*
	(0.018)	(0.013)	(0.012)	(0.015)	(1.030)	(1.635)

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.
* significant at 10%, ** significant at 5%, *** significant at 1%.

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

	Local Linear Regression (CCT)	Local Linear Regression (IK)	Quartic Spline Full Sample	Quadratic Spline Index= [-50,50]	Linear Spline Index=[-25,25]
			Coefficients (Standard Error)		
	(1)	(2)	(3)	(4)	(5)
Panel A: Fourth Grade					
Failed July Promotion Cutoff	0.025*	0.027**	0.021	0.030**	0.025**
	(0.013)	(0.012)	(0.015)	(0.015)	(0.013)
	[18,514]	[22,401]	[32,381]	[25,914]	[16,165]
Panel B: Eighth Grade					
Failed July Promotion Cutoff	0.032**	0.034***	0.023*	0.021	0.039***
	(0.013)	(0.012)	(0.014)	(0.013)	(0.012)
	[18,725]	[21,180]	[33,911]	[28,987]	[20,557]
Controls:					
Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Birth Year Effects	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes

NOTES: Optimal bandwidths for local linear regression estimations in columns (1) and (2) are obtained using the procedures in Calonico et al. (2014) and Imbens and Kalyanaraman (2012). 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 the Test-Based Promotion Policies on Juvenile Crime

	Local Linear Regression (CCT)	Local Linear Regression (IK)	Quartic Spline Full Sample	Quadratic Spline Index=[-50,50]	Linear Spline Index=[-25,25]
			Coefficients (Standard Error)		
	(1)	(2)	(3)	(4)	(5)
Panel A: Fourth Grade					
Failed March Promotion Cutoff	-0.004	-0.003	-0.006	-0.001	-0.006
	(0.004)	(0.003)	(0.006)	(0.006)	(0.005)
	[66,857]	[125,606]	[155,182]	[90,825]	[46,420]
Panel B: Eighth Grade					
Failed March Promotion Cutoff	-0.004	-0.002*	-0.004	-0.005	-0.006
	(0.003)	(0.001)	(0.003)	(0.003)	(0.005)
	[80,525]	[127,231]	[153,953]	[108,860]	[59,810]
Controls:					
Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Birth Year Effects	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes

NOTES: Optimal bandwidths for local linear regression estimations in columns (1) and (2) are obtained using the procedures in Calonico et al. (2014) and Imbens and Kalyanaraman (2012). 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%.

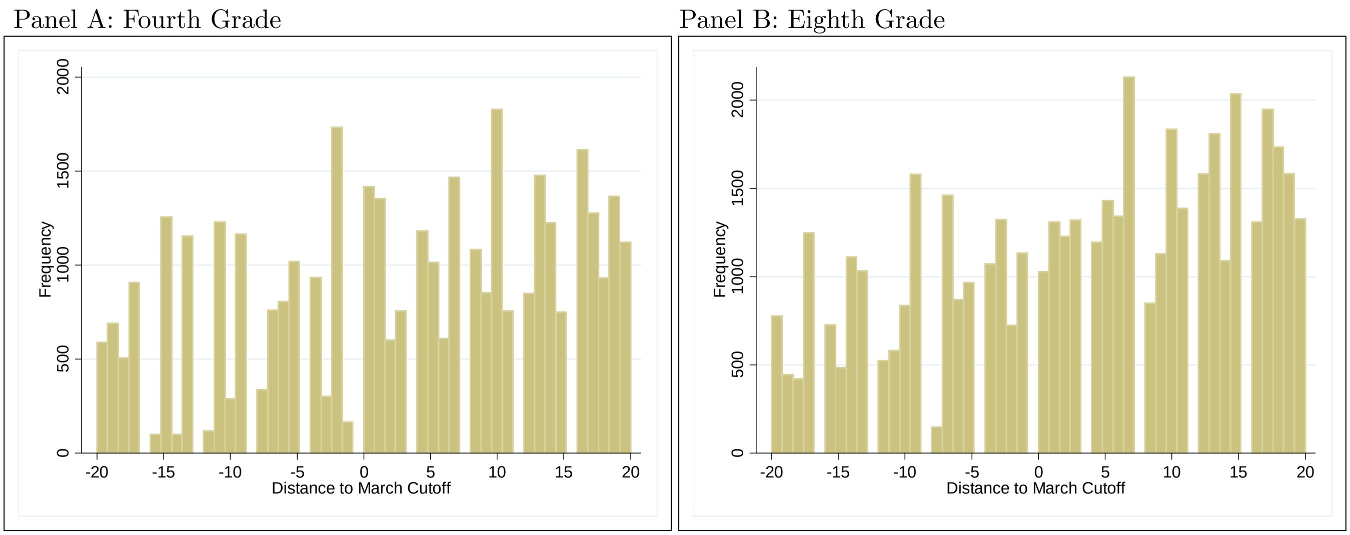


Figure A1: Distributions of Students around the March Promotional Cutoff

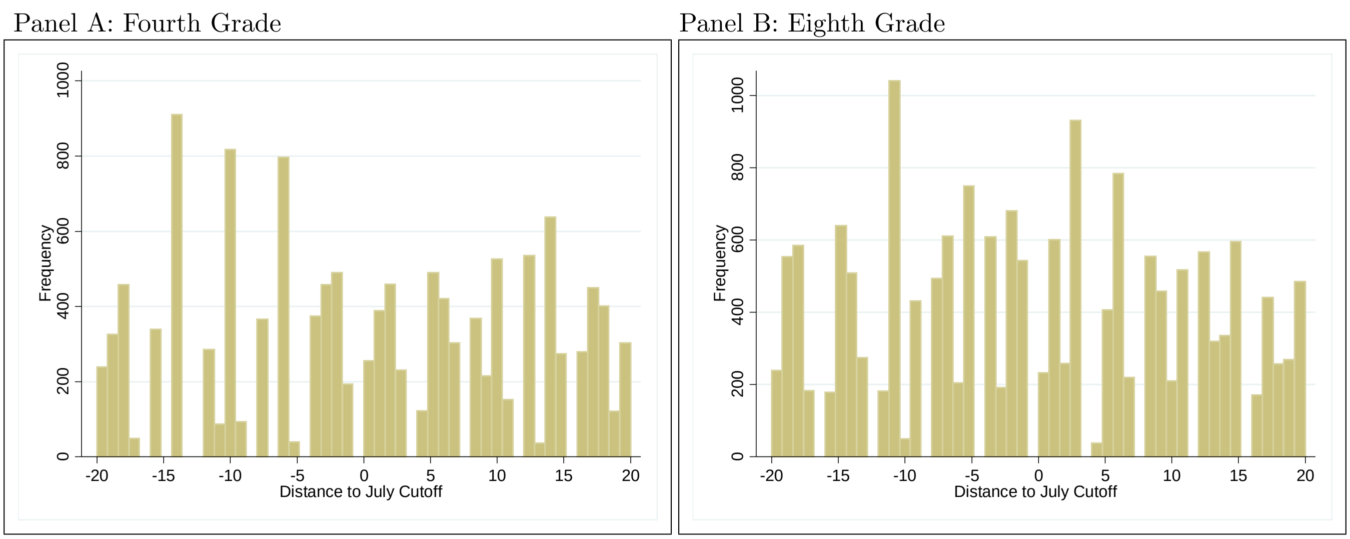


Figure A2: Distributions of Students around the July Promotional Cutoff