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Abstract

A growing number of American states require that students who do not demonstrate basic reading proficiency at the end of third grade be retained and provided with remedial services. We exploit a discontinuity in the probability of third grade retention under Florida's test-based promotion policy to study the causal effect of retention on student outcomes over time. Although conventional OLS estimates suggest negative effects of retention on achievement, regression discontinuity estimates indicate large positive effects on achievement and a reduced probability of retention in subsequent years. The achievement gains from test-based retention fade out over time, however, and are statistically insignificant after six years.

JEL-Code: H520, I210, I280.

Keywords: educational production, public schools, grade retention.

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1 Introduction

Fourteen states and the District of Columbia have recently enacted policies requiring that students who do not demonstrate basic reading proficiency at the end of third grade be retained and provided with remedial services (Rose, 2012). Similar policies are under debate in state legislatures across the nation. Although these policies aim to provide incentives for educators and parents to ensure that students meet performance expectations, they can also be expected to increase the incidence of retention in the early grades. Their enactment has therefore renewed a longstanding debate about retention's consequences for low-achieving students.

Roughly 10 percent of American students are retained at least once between kindergarten and eighth grade, with the incidence of retention concentrated among low-income students and traditionally disadvantaged minorities (Planty et al., 2009). Retaining students in the same grade is costly in terms of additional per pupil spending and foregone earnings, if students (as intended) spend an additional year in full-time public education as a result of being held back. Yet consensus is lacking as to whether retention yields benefits for students that could offset these costs and, if so, under what conditions.

Proponents of policies encouraging the retention of low-performing students contend that these students stand to benefit from an improved match of their ability to that of their peers, from the opportunity for additional instruction before confronting more challenging material, and from any additional services provided to students during the retention year. Critics, meanwhile, warn that retained students may be harmed by stigmatization, reduced expectations for their academic performance on the part of teachers and parents, and the challenges of adjusting to a new peer group. In fact, a large literature confirms that retained students achieve at lower levels, complete fewer years of school, and have worse social-emotional outcomes than observably similar students who are promoted.[1] Because retention decisions typically reflect student characteristics unobserved by the researcher, however, these studies are likely to suffer from severe selection bias.

In this paper, we use statewide administrative data covering all students in Florida public schools in grade 3 to 9 to study the causal effect of third grade retention and remediation on future student outcomes up to 6 years later. The Florida database has three key advantages for studying the consequences of grade retention. First, Florida since 2003 has required that schools retain third grade students failing to demonstrate basic proficiency on the state reading test unless the student is eligible for one of a specified set of exemptions. While similar policies elsewhere have led to non-linearities in the relationship between test scores and retention probabilities (e.g., Jacob and Lefgren, 2004, 2009), Florida's test-based promotion policy generates a true discontinuity in the probability of retention. We can therefore employ a standard

[1] Influential studies in this area include Jimerson (1999) and Jimerson et al. (2000, 2002), and McCoy and Reynolds (1999). A survey of 47 empirical studies conducted between by Holmes (1989) concluded that retained students performed 0.19 to 0.31 standard deviations worse on various measures of academic achievement than similar students who were not retained. A meta-analysis of post-1990 studies Allen et al. (2009) found that, although most studies indicated negative effects of retention, a subset with more rigorous designs yielded more positive evidence.

regression discontinuity design to overcome the selection issues plaguing most existing research on this topic.

Second, the Florida database contains vertically scaled test scores in reading and math that make it possible to compare the achievement of students tested in different grades. Making this comparison is essential because the counterfactual condition for students who are retained is to have been immediately promoted to the next grade. While often reported in the literature, same-grade comparisons conflate any effect of retention with the effect of being a year older and in school for an additional year at the time the relevant test is administered. As we demonstrate below, they will also be biased if students on the margin of retention have experienced prior retentions or other educational interventions with effects on achievement that fade out over time or are delayed.

Finally, the availability of annual test scores for 6 full years after the retention decision makes it possible to determine the extent to which any changes over time in the magnitude of the estimated effect of retention are driven by grade-specific effects on achievement. The average amount students learn may vary across grades for several reasons, including differences in teacher quality, the alignment of curricula with test content, and the share of students making school transitions. Because estimates of retention effects based on same-age comparisons capture these grade-specific effects along with the isolated effect of being retained, studies examining the outcomes of retained students after only 2 years (e.g., Jacob and Lefgren, 2004; Greene and Winters, 2007) are unable to determine whether any short-term effects of retention persist, fade out, or even grow larger over time.

It is important to note that the Florida policy requires that retained students be given the opportunity to attend a summer reading program prior to the next school year and that they be assigned to a "high-performing" teacher and receive intensive reading interventions during that year. Our estimates of the policy's impact will therefore capture the combined effect of retention and these additional measures and may not be directly comparable to those of some previous studies of retention. Requirements that retained students receive remedial interventions are typical of test-based promotion policies currently in use and under consideration in other settings, however, giving our results considerable policy relevance.

Due to the availability of exemptions for students scoring below the promotion cutoff, as well as to the voluntary retention of some higher-scoring students, our regression discontinuity design is fuzzy and yields estimates local to students who are retained as a result of the policy but would otherwise have been promoted (i.e., compliers). From a policy perspective, however, this local average treatment effect is arguably the most relevant parameter. Teachers granting a low-scoring student an exemption or recommending that a student with higher test scores be retained presumably do so because they have strong views as to whether retention would be beneficial for the student in question. In the case of compliers, in contrast, the fact that retention occurs only as a result of the test-based promotion policy implies that local educators are uncertain about whether retention is desirable. Moreover, because the retention policy is based on reading scores alone, we can exploit variation compliers' math achievement to provide

suggestive evidence that our estimates are generalizable to a broader population in terms of third grade achievement.

Our analysis confirms that students retained in third grade under Florida's test-based promotion policy experience substantial short-term gains in both math and reading achievement. On average over the first three years after being held back, retained students outperform their same-age peers who were promoted by 0.34 standard deviations in reading and by 0.26 standard deviations in math. These positive effects fade out over time, however, becoming statistically insignificant in both subjects within five years. We also find that test-based retention in third grade reduced the probability that a student would be retained in each of the four subsequent years. In contrast, we find no effects of third grade retention on student absences or special education placement rates.

These findings contribute to an emerging literature using quasi-experimental research designs to study the effects of retention in U.S. public schools.[2] Jacob and Lefgren (2004, 2009) exploit a non-linearity in the relationship between test scores and retention probabilities in third, sixth, and eighth grades to study the impact of retention on achievement and high school completion of Chicago students. They find that retention and mandatory summer school had a small positive short-term effect on achievement for third graders but not for sixth graders. They also find that retention increased dropout rates for eighth graders but not for sixth graders. In a prior study of the Florida policy, Greene and Winters (2007) find that third grade retention improved student achievement after two years.[3] Taken as a whole, this evidence suggests that retention in higher grade levels may have detrimental effects on future student outcomes, but that early grade retention may be more beneficial. We confirm that third grade retention in Florida improves student achievement in the short-run, while also showing that these initial benefits fade out over time.

Our evidence that third grade retention reduces the probability of retention in subsequent grades highlights an additional consequence of policies that increase retention rates in early grades and clarifies their costs. Specifically, we show that many of the students retained as third graders as a result of Florida's test-based promotion policy would otherwise have been retained in a subsequent grade. To the extent that later grade retention is in fact less beneficial, students who are retained earlier rather than later may particularly benefit from the policy. Overall, our results indicate that after six years students retained in third grade are, on average, only 0.74 grade levels behind their non-retained peers. The cost associated with increasing early grade retention for the individual student due to foregone earnings is therefore likely to be substantially less than a full year.

The paper proceeds as follows. Section 2 develops a statistical model of education produc-

[2]In addition to the studies discussed in the text, Eide and Showalter (2001) use variation in kindergarten entry ages across states as an instrument for retention and conclude that retention increases high school completion and earnings for white students, although their results are not statistically significant. In a comparative setting, Manacorda's (2012) regression discontinuity analysis finds that retention in junior high school increases dropout rates for Uruguayan students.

[3]Winters and Greene (2012) present additional evidence on the Florida policy based on same-grade comparisons but do not provide estimates that isolate the causal effect of retention and remediation.

tion with potential grade retention that motivates our approach to studying retention effects. In Section 3 we describe the Florida policy and our data. Section 4 presents our identification strategy and provides graphical evidence supporting its validity, while Section 5 presents our findings concerning the effects of third grade retention on student outcomes. Section 6 concludes.

2 A Statistical Model of Education Production with Grade Retention

To motivate our empirical approach to identifying the causal effect of grade retention, we incorporate retention effects into a simple education production function that may describe the process by which our data are generated:

$$Y_{iag} = \sum_{t=1}^{a} \alpha_t + \sum_{t=6}^{a} \sum_{h=1}^{g} (\lambda + \beta_h) G_{ith} + \sum_{t=6}^{a-1} \sum_{h=1}^{g-1} \tau_{h(a-t)} I_{ith} + \nu_{iag} \quad (1)$$

where Y_{iag} is a measure of the achievement of student i in grade g at age a that can be decomposed into the cumulative effects of age, α_t, schooling, $\lambda + \beta_h$, the isolated effects of grade retention, $\tau_{h(a-t)}$, and an error term, ν_{iag}, capturing individual heterogeneity and error in the measurement of the student's "true" achievement when tested at age a in grade g. Note that the effect of schooling consists of an average effect of a year of schooling, λ, that is constant across grade levels and a grade-specific deviation from this average effect, β_h. The latter is introduced to allow for differential learning gains across grades. The history of grade levels attended at any age between age 6 and age a is captured by the set of indicators, G_{ith}, that take the value one if student i attended grade h at age t. Similarly, I_{ith} indicates whether student i was retained in grade h at age t. Note that we allow the effects of being retained, $\tau_{h(a-t)}$, to vary by grade level and to fade out over time.

This model serves to clarify the choice between same-grade and same-age comparisons to study retention effects, a point of debate in the literature (see, e.g., Allen et al., 2009). For simplicity (and to correspond to our empirical analysis below), consider a study designed to estimate the effect of retention in grade 3 (i.e.,$I_{it3} = 1$) on student achievement. At least in theory, the outcome of interest can be defined as achievement when students first reach grade 4 (same-grade) or as achievement one year after potential grade 3 retention (same-age).

Consider first the same-grade comparison. The expected achievement in grade 4 at age A of students not retained in grade 3 is given by:

$$\begin{aligned} E[Y_{iag}|a = A, g = 4, I_{i,A-1,3} = 0] &= \sum_{t=1}^{A} \alpha_t + \beta_4 + \lambda + \sum_{t=6}^{A-2} \sum_{h=1}^{3} (\lambda + \beta_h) E[G_{ith}|\cdot] \\ &+ \sum_{t=6}^{A-2} \sum_{h=1}^{2} \tau_{h(A-t)} E[I_{ith}|\cdot]. \end{aligned} \quad (2)$$

If we assume that students retained in grade 3 cannot be required to repeat the grade twice, their expected achievement in grade 4 is:

$$\begin{aligned} E[Y_{iag}|a = A+1, g = 4, I_{i,A-1,3} = 1] &= \sum_{t=1}^{A+1} \alpha_t + \beta_4 + \beta_3 + 2\lambda + \sum_{t=6}^{A-2} \sum_{h=1}^{3} (\lambda + \beta_h) E[G_{ith}|\cdot] \\ &+ \sum_{t=6}^{A-2} \sum_{h=1}^{2} \tau_{h(A+1-t)} E[I_{ith}|\cdot] + \tau_{3(2)}. \end{aligned} \tag{3}$$

Differencing Equations (3) and (2) yields:

$$\Delta^{grade} = \alpha_{A+1} + (\beta_3 + \lambda) + \Theta + \tau_{3(2)} \tag{4}$$

where $\Theta = \sum_{t=6}^{A-2} \sum_{h=1}^{3} \tau_{h(A+1-t)} E[I_{ith}|\cdot] - \sum_{t=6}^{A-2} \sum_{h=1}^{3} \tau_{h(A-t)} E[I_{ith}|\cdot]$.

The first term in Equation (4) captures the effect of being of age $A+1$ instead of A, while the second term reflects the average effect of an additional year of schooling, λ, plus the grade 3-specific deviation from this average effect, β_3. The third term captures the effects of potential grade retention in grades 1 or 2, which is zero only if $\tau_{h(A-t)} = \tau_{h(A+1-t)}$. That is, any effects of prior retentions cancel out only if they do not fade out over time. Finally, the equation's last term, $\tau_{3(2)}$, represents the isolated effect of grade 3 retention on achievement two years later.

The same-grade comparison represented by Equation (4) therefore identifies the isolated effect of grade 3 retention, $\tau_{3(2)}$, only in the absence of any grade 3 specific year-of-schooling effect ($\beta_3 = -\lambda$) effect and age effect ($\alpha_{A+1} = 0$) and if any effects of prior grade retentions do not fade out. Although they are not explicitly modeled here, the potential implications of prior grade retentions readily extend to other interventions that affect student achievement prior to grade 3 and fail to persist fully over time. Even if the use of a (quasi-)experimental identification strategy ensures that the incidence of such interventions is orthogonal to grade 3 retention, the fact that outcomes are measured at different time points for retained and non-retained students would influence the estimates of retention effects. The decay of achievement impacts is a pervasive pattern in the literature on educational production, suggesting that empirical estimates of retention effects based on same-grade comparisons are likely to be poor proxies of the isolated effects of grade retention even in the absence of grade-specific year-of-schooling and age effects.

In contrast, the same-age approach compares the expected achievement at age A of students who were retained in grade 3 at age $A-1$ to that of students who were not retained. For non-retained students, this expectation is again given by Equation (2). For retained students, it is:

$$\begin{aligned} E[Y_{iag}|a = A, g = 3, I_{i,A-1,3} = 1] &= \sum_{t=1}^{A} \alpha_t + \beta_3 + \lambda + \sum_{t=6}^{A-2} \sum_{h=1}^{3} (\lambda + \beta_h) E[G_{ith}|\cdot] \\ &+ \sum_{t=6}^{A-2} \sum_{h=1}^{2} \tau_{h(A-t)} E[I_{ith}|\cdot] + \tau_{3(1)}. \end{aligned} \tag{5}$$

First-differencing equations (5) and (2) yields:

$$\Delta^{age} = \beta_3 - \beta_4 + \tau_{3(1)}. \quad (6)$$

Equation (6) shows that a same-age comparison identifies the isolated effect of retention in grade 3 on achievement in the following year plus any effect of having attended grade 4 rather than having attended grade 3 for a second time. Such a grade-specific effect could arise due to differences between grades 3 and 4 in curricula, instructional time, or average teacher quality. Attending grade 3 a second time rather than attending grade 4 in the following year is however a direct consequence of being retained. In other words, $\beta_3 - \beta_4$ is part of the desired treatment effect. Δ^{age} therefore represents a meaningful causal effect of grade retention despite the fact that the two terms on the right-hand-side are not separately identifiable.

Despite its clear advantages in terms of isolating the causal effect of grade retention, implementing the same-age comparison approach requires an achievement measure that places students tested in different grades on a common scale.[4] Our analysis exploits the fact that Florida is one of a small number of states that provides vertically equated developmental scale scores for students tested at each grade level included in its statewide accountability program.

We provide evidence below that the achievement gains made by typical students on this scale are not uniform across grades. Thus, our estimates of Δ^{age} may vary with the number of years since treatment for at least two reasons: true fade out of retention effects and grade-specific effects on achievement conditional on the number of prior years of schooling. For example, our estimates of the effects of grade retention in grade 3 may diminish over time if $\beta_h < \beta_{h+1}$ even if $\tau_{3(a)} = \tau_{3(a+1)}$. To back out an approximate estimate of the extent of true fade out of retention effects over time, we rescale the developmental scale scores under the assumption that achievement gains are uniform across grades 3 to 10. We explain this rescaling in more detail at the end of the next section.

3 Institutional Setting and Data

In 2002, Florida's legislature mandated that third grade students scoring below level two (of five performance levels) on the Florida Comprehensive Assessment Test (FCAT) reading test be retained and provided with remedial services unless they qualify for one of six "good cause exemptions."[5] The Florida policy's exclusive focus on third grade reading distinguishes it from test-based promotion measures in Chicago and New York City, which include retention gates based on reading and math achievement at multiple grade levels. This focus reflects a common belief among educators that acquiring basic reading proficiency by third grade is essential for subsequent performance across disciplines, as well as the fact that third grade is the lowest included in the state testing program.

[4] National longitudinal studies tracking a grade cohort of students over time typically meet this requirement, but often lack credibly exogenous sources of variation in the probability of retention.

[5] The description of the Florida program in this section is based on Office of Program Policy Analysis & Government Accountability (2006).

Students scoring below the level two cutoff may be granted an exemption from the policy if they fall into any of the following categories: students with disabilities whose Individualized Education Plan indicates that the state test is an inappropriate measure of their performance; students with disabilities who were previously retained in third grade; Limited English proficiency (LEP) students with less than two years of instruction in English; students who were retained twice previously; students scoring above the 51st percentile nationally on another standardized reading test; and students demonstrating proficiency through a portfolio of work.[6] In light of these exemptions, the term "test-based promotion policy" may be a misnomer. It would be more precise to say that, for students not in special education, a low test-score shifts the burden of proof such that educators need to make an affirmative case that the student should be promoted.

Even so, the policy sharply increased the number of students held back in third grade. The number of Florida third graders retained jumped to 21,799 (13.5 percent) as the policy was implemented in 2003, up from 4,819 (2.8 percent) the previous year. The number of Florida students retained in third grade fell steadily over the next five years, reaching 9,562 (5.6 percent) in 2008, primarily due to a reduction in the number of students failing to meet the promotion standard.

As noted above, the policy includes several provisions intended to ensure that retained students acquire the reading skills needed to be promoted the following year. First, retained students must be given the opportunity to participate in their district's summer reading camp. Schools must also develop an academic improvement plan for each retained student and assign them to a "high-performing teacher," as determined by satisfactory performance appraisals. Finally, while repeating third grade, retained students must receive intensive reading interventions including ninety uninterrupted minutes daily of research-based reading instruction.[7]

The data for our analysis are drawn from the Florida Department of Education's PK-20 Education Data Warehouse and contain information on all Florida students attending public schools in grades 3 to 9 from the 2000-01 through 2008-09 school years. We identify retained students based on the grade level of the state tests taken in adjacent years.[8] Our data extract includes the school each student attends and its location; student characteristics such as ethnicity, gender, special education classification, English proficiency, and free lunch eligibility; annual measures of absences; and annual FCAT math and reading test scores.

The first cohort to be impacted by the test-based promotion policy (which we will refer to as the 2003 cohort) entered third grade in the 2002-03 school year and can be followed for an additional six years after potential grade 3 retention, at which point students who were promoted to grade 4 and not retained in a later grade should have reached ninth grade. The five subsequent cohorts that we include in our analysis enter third grade in later years and can therefore be tracked for progressively shorter periods of time. Our primary analyses pool the

[6]Since the 2004-05 school year, retained students have also been given the opportunity for a midyear promotion to fourth grade if they demonstrate mastery of necessary skills at that time.

[7]Since 2004-05, the uninterrupted ninety minute reading block has been mandatory for all K-5 students.

[8]Students receiving mid-year promotions after 2004-05 will therefore be recorded as not being retained.

data on all cohorts for which the relevant outcome is available.

Table 1 provides summary statistics for the pooled sample covering the 2003-2008 cohorts used to study outcomes one year after potential retention. The first column reports mean characteristics (measured in third grade) for all students; columns 2 and 3 in turn include all students scoring below the cutoff and all students who were retained in third grade; and column 4 includes students who were retained in third grade despite exceeding the cutoff. The table shows that 8.3 percent of all Florida students in these cohorts were retained in grade 3. This includes almost half (47.8 percent) of students scoring below the promotion cutoff, as well as an additional 0.6 percent of students scoring above the cutoff.

Students' raw third grade test scores in reading and math have been standardized by subject and year to have a mean of zero and a standard deviation of one. Naturally, students scoring below the cutoff and retained students perform at low levels. For example, retained students score 1.43 standard deviations below the average student in reading and 1.22 standard deviations below the average student in math. Students scoring below the cutoff and retained students are quite similar with respect to their observable characteristics. In contrast, the relatively few voluntarily retained students are higher performing on average, more likely to be white, and substantially younger than the average retained student. They are also absent more frequently as third graders, perhaps suggesting the importance of behavioral indicators to voluntary retention decisions.

In addition to raw test scores, our data extract includes vertically equated Developmental Scale Scores (DSS) intended to support comparisons of student achievement across grade levels. During the 2000-01 school year, when the FCAT testing program was expanded to include reading and math in all grades three through ten, a special data collection scheme incorporated the use of common items administered to students across multiple grades. Specifically, operational items from each grade's test were also included on the test administered to the higher and lower adjacent grade. These common items permitted the use of Item Response Theory (IRT) methods to place results from each grade's test on a common scale.[9]

Figure 1 plots average DSS scores in reading and math by grade for all students in the pooled dataset. The DSS scores have an across-grade, student-level standard deviation of 364 points in reading and 305 points in math. The jagged trajectory evident in both subjects indicates that average achievement gains as measured by developmental scale scores vary considerably by grade. For example, math gains are very small in grade six while reading gains are particularly pronounced in grade four. This variation could reflect imperfections in the vertical scaling process. Alternatively, it could reflect true differences in the average rate of learning in Florida public schools across grades. For example, the small math gains in grade six likely reflect the fact that most Florida students transition into a middle school in grade six, which Schwerdt and West (2013) show has a negative impact on their achievement growth. To the extent that retention simply delays students from experiencing a grade in which their own achievement growth is likely to be smaller, policymakers arguably would want to incorporate this information

[9]See Hoffman et al. (2001) for technical details on the construction of the developmental scale scores.

into the metric used to compare their achievement to that of promoted students.

The variation in achievement gains by grade motivates our construction of an alternative vertical scaling of reading and math achievement, which is also plotted in Figure 1. Specifically, we subtract from each student's DSS score the grade-specific mean score and then add the predicted value for each grade from a linear regression of mean scores on grade level. These rescaled scores increase linearly from grades three to ten by construction. The estimated slope coefficients, which indicate the average annual rate of achievement growth between third and tenth grade, are 80 DSS points in reading and 83 DSS points in math.

Using these rescaled scores as the outcome measure when estimating the impact of retention on student achievement enables us to back out an approximate estimate of the extent of true fade out of retention effects. In terms of the statistical model presented in section 2, we treat the estimated slope parameters as a measure of λ and the difference the between average and predicted DSS score for grade h as an approximation of β_h. The assumption of linear achievement growth underlying the rescaling is admittedly arbitrary, and point estimates based on rescaled scores do not necessarily represent an unbiased estimate of the isolated retention effects, $\tau_{3(a)}$. Comparing estimates based on rescaled scores across years should nonetheless be informative about the rate at which retention effects fade out over time.

4 Empirical Strategy

Empirical strategies that rely on a selection-on-observables assumption will fail to provide unbiased estimates of the effect of early grade retention on future student outcomes if students are selected for retention based on factors unobserved by the researcher that influence educational outcomes. We address this concern by taking advantage of Florida's test-based promotion policy, which leads to a discontinuous relationship between third grade reading test scores and the probability of grade retention. This discontinuity generates plausibly exogenous variation in retention which we exploit to identify the causal effect of test-based retention on future outcomes.

4.1 Graphical Evidence

Our identification strategy hinges on the assumption that Florida's test-based promotion policy generates exogenous variation in third grade retention which we can use standard regression discontinuity methods to exploit. We first present graphical evidence of the existence of a discontinuity in the relationship between a student's third grade reading test scores and the probability of being retained. We then discuss potential threats to the validity of regression discontinuity studies and provide additional graphical evidence demonstrating that these threats are not applicable in this setting (c.f., Lee and Lemieux, 2010). Unless otherwise noted, all figures are based on the pooled data set of students in the 2003-2008 cohorts.[10]

Figure 2, which plots the share of students retained as a function of third grade reading

[10]Cohort-specific graphs are available from the authors upon request.

scores (measured relative to the test score cutoff), provides visual evidence of the discontinuity in retention probabilities. The data points represent the share of students retained for each possible score on the third grade reading test, with each marker's size proportional to the number of students receiving that score. The solid line represents predicted values from separate local linear regressions on either side of the cutoff. For students 30 or more points ($> .5$ standard deviations) below the cutoff, retention probabilities are relatively stable at just under 0.6. The probability of retention then declines as test scores increase, with retention probabilities immediately to the left of the cutoff approaching 0.3. Retention probabilities drop sharply to less than 0.05 at the cutoff, however, and approach zero 50 points above it.

Figure 3 displays the same relationship for the two cohorts of students in our data extract entering third grade immediately prior to the introduction of the test-based promotion policy. Note that the probability of retention for students in these cohorts rarely exceeds 20 percent, even for very low-scoring students. More importantly, the probability of retention is essentially continuous around the cutoff, indicating that the discontinuity evident in Figure 2 was in fact generated by the policy change.

While Figure 2 is based on the full distribution of third grade reading test scores, we limit our regression discontinuity analysis of the causal effects of retention to a narrower sample of students within a 10 test-score-point bandwidth on either side of the cutoff. Figure 4 illustrates the discontinuity within this more restricted sample, again plotting the fraction of students retained by third grade reading test scores measured relative to the cutoff. Local linear regressions on either side of the cutoff suggest an approximately linear relationship between test scores and retention probabilities in the cutoff region. However, the slope of this relationship clearly differs for students below and above the cutoff. We make use of this observation below when specifying the functional relationship between the forcing variable (reading test scores) and the retention indicator in our empirical model.

A common concern with regression discontinuity analyses is the possibility of precise manipulation of the forcing variable around the cutoff (c.f., Urquiola and Verhoogen, 2009). In this setting, for example, one might worry that teachers were able to manipulate students' reading scores to push them just above the promotion cutoff. The fact that the FCAT reading test is scored objectively without teacher input makes this possibility unlikely, however, and Figure 5 confirms that the overall distribution of reading test scores shows no evidence of a heaping of observations around the cutoff.

The regression discontinuity identification strategy also assumes that there are not discontinuities in other characteristics associated with student outcomes at the cutoff. Figure 6 addresses this issue by plotting the mean value of the observable student characteristics available in our data against third grade reading test scores. In addition to examining each characteristic individually, we also use a probit model to generate a predicted retention probability for each student based on all available background characteristics (except reading scores). The figure confirms the absence of discontinuities in observed student characteristics at the test-score cutoff used to inform retention decisions.

Finally, we confirm that attrition from the Florida database in subsequent years also does not vary discontinuously at the promotion cutoff. Even in the absence of sorting around the cutoff based on prior characteristics, differential attrition could occur if, for example, being retained in third grade made students more likely to leave the Florida public schools. Figure 7 therefore plots attrition rates against third grade reading scores around the cutoff. To enhance legibility, the figure plots attrition rates after two, four, and six years only; the patterns after three and five years are similar.[11] Attrition rates increase as expected with the number of years since potential third grade retention, but they appear to be unrelated to third grade reading scores and there is no evidence of a discontinuity at the promotion cutoff.[12]

4.2 Estimation

Because only a subset of students scoring below the cutoff in reading test scores were actually retained, our empirical analysis takes the form of a fuzzy regression discontinuity design which can be implemented via instrumental variables (IV) estimation. In our preferred specification we estimate the causal effect of test-based retention on future student outcomes in a two-stage least squares model. The first stage is given by the following equation:

$$\begin{aligned} retain &= \gamma_1 below + \gamma_2 below \times LEP + \gamma_3 below \times SpEd \\ &+ \gamma_4 below \times forcevar + \gamma_5 forcevar + \Gamma X + \epsilon, \end{aligned} \tag{7}$$

where *retain* indicates retention in grade 3, *below* indicates that the student scored below the promotion cutoff on the grade 3 reading test, LEP identifies students with limited English proficiency in grade 3, $SpEd$ indicates whether students are classified as special education students in grade 3, $forcevar$ measures student achievement on the grade 3 reading test, X is a vector of student demographic characteristics including the student's math achievement in grade 3, and ϵ is a standard zero-mean error term. Note that, based on the graphical evidence in Figure 4, we model the relationship between reading scores and the retention indicator as linear with a break in the trend at the cutoff.

The corresponding second stage of our 2SLS model is given by:

$$y = \delta_1 retained + \delta_2 below \times forcevar + \delta_3 forcevar + \Delta X + \eta, \tag{8}$$

where y denotes the student outcome of interest. We achieve identification of δ_1 by instrumenting for grade retention in grade 3 ($retained$) with the indicator for being below the cutoff

[11] Because we identify students as having been promoted or retained in third grade based on the grade in which they are observed the following year, attrition rates one year after potential retention are zero by construction. We can, however, examine the rate of attrition among all students tested in third grade regardless of whether we observe them in Florida public schools the following year. Appendix Table A-1 confirms that attrition rates after one year and subsequently do not vary discontinuously around the promotion cutoff.

[12] In addition to the graphical analyses in figures 6 and 7, we used each student characteristic and attrition in each year after potential third grade retention as the outcome variable in regressions with the same specification and bandwidth as our preferred regression discontinuity model. The results (available upon request) confirm the absence of any statistically significant breaks in the relationship between reading scores and these outcomes at the promotion cutoff.

for promotion to grade 4 (*below*) and the interactions with LEP and special education status. As noted above, we estimate the 2SLS model for the sample of students within ten test score points on either side of this cutoff. We select this bandwidth based on the optimal bandwidth algorithm developed by Imbens and Kalyanaraman (2009) and demonstrate the robustness of our results to alternative bandwidths below. In order to compare our preferred IV results with conventional estimates of the effects of retention based on a selection-on-observables assumption, we also estimate Equation (8) using OLS. To maximize comparability across the two designs, we limit the OLS specification to the regression discontinuity sample.

5 Results

Table 2 reports results from estimating the first-stage model in Equation (7) for each cohort of students separately and for the pooled sample. For purposes of comparison, we also present results for the two cohorts of students in our data that were not impacted by the policy. Note that all estimations are based on our preferred discontinuity sample within a 10 test-score-point bandwidth around the cutoff. Despite this narrow bandwidth, we still have between 9,981 and 15,687 students in each post-2002 cohort and a total of nearly 75,000 students in the pooled sample.

The first row of Table 2 presents estimates of the jump in the probability of retention at the promotion cutoff for non-special education, non-LEP students. Consistent with Figure 3, the first two columns confirm that there was essentially no such jump in the two years immediately preceding the policy's introduction.[13] In contrast, each of the cohort-specific estimates for students impacted by the policy is positive and highly statistically significant, with F-statistics on the excluded instruments exceeding 100. Point estimates of the jump in retention probabilities at the cutoff range from 0.22 to 0.40, with the largest estimate observed for the initial 2003 cohort and the two smallest estimates observed for the 2007 and 2008 cohorts. This suggests that enforcement of the retention requirement was relatively lax (a pattern which is arguably consistent with the availability of good cause exemptions) and appears to have declined over time. The overall first stage effect for the pooled sample nonetheless indicates an increase of 0.31 in the probability of retention for typical students scoring immediately below the cutoff, relative to students scoring one point higher. The results also confirm that the increase in retention probabilities for students just missing the cutoff was smaller for special education and, to a lesser extent, LEP students. This is as expected given that students in these groups were eligible for additional good cause exemptions from the retention requirement.

5.1 The Effect of Test-Based Retention on Student Achievement

We begin our discussion of the effects of grade retention on student outcomes with graphical evidence on the reduced form relationship between students' third grade reading test scores

[13] Although the results for the 2002 cohort show a statistically significant increase in the probability of retention for students scoring below the cutoff, the cohort-specific estimates while the policy was in place are all more than ten times as large.

and their future achievement. Figures 8 and 9 use local linear regressions estimated separately on each side of the promotion cutoff to depict the relationship between students' third grade reading test scores and their reading and math achievement up to six years after potential third grade retention. In both subjects, we observe students scoring below the promotion cutoff performing at higher levels in the first three years after potential third grade retention. However, these differences dissipate in later years and, in some cases, appear to turn slightly negative.

Table 3 presents same-age estimates of the effects of third grade retention on reading and math achievement over time. Columns 1 and 2 report OLS estimates from Equation (2) with and without covariates, while columns 3 and 4 report results from our preferred IV model exploiting the discontinuity. As expected, the inclusion of covariates does not notably influence the IV point estimates (although it modestly improves their precision) but substantially alters the OLS results.

Consistent with Figures 8 and 9, the IV estimates indicate that third grade retention substantially improves students reading and math achievement in the short run. Measured relative to the statewide standard deviation in third grade reading DSS scores, reading achievement improves by 26 percent of a standard deviation after one year and by as much as 50 percent of a standard deviation after two years. The estimated impact of retention on math achievement is 31 percent of a standard deviation after one year and grows to 36 percent of a standard deviation after three years. These initial benefits fade out in subsequent years, however. The effects of third grade retention on reading achievement are reduced in years three and four and become statistically insignificant in years five and six. In the case of math achievement, the estimated effects become slightly negative in years four and five but are statistically insignificant after six years. Appendix Table A-2, which presents the same year-by-year results separately for each cohort, confirms that this apparent fade out in the effects of third grade retention over time does not simply reflect smaller impacts of retention on the earliest cohorts whose outcomes we are able to observe for more years.

Relative to our preferred IV estimates, OLS estimates of the effects of third grade retention are always more negative and would suggest a statistically significant negative impact on reading and math achievement after 6 years. The differences across the two sets of results are substantial even after including performance and demographic covariates. In reading after one year, for example, the difference between the OLS and IV point estimates is more than one third of a standard deviation. This suggests that OLS estimates fail to control adequately for unobserved confounding factors and, thus, understate any benefits (and exaggerate any harms) of grade retention.

One unusual aspect of the results in Table 3 is the non-monotonic relationship between the size of the estimated impacts of retention and the time elapsed since the student was retained. The estimated impact is largest after two years in the case of reading achievement and after three years in math. Given the overall pattern of fade out and the fact that remedial services were required only in the year the student was retained, one would expect the impact

of retention to be largest at the end of that year. This pattern likely stems in part from the grade-to-grade variation in the average achievement gains of Florida public school students as measured by DSS scores. For example, Figure 1 shows that Florida students statewide experience particularly large gains in DSS reading achievement in fourth grade, which promoted students enter immediately and (most) retained students enter one year later. This difference in timing could explain the unexpected growth from year one to year two in the estimated impact of retention on DSS reading achievement. The alternative scaling of the DSS scores discussed above eliminates variation in average achievement gains across grades and thereby allows us to approximate the true rate of fade out over time.

Table 4 presents OLS and IV estimates of Equation (8) based on these rescaled DSS scores. In both reading and math, the magnitude of the estimated impacts now decreases monotonically with distance from treatment. In reading, the impacts based on the rescaled DSS scores are as large as 61 percent of a standard deviation after one year but fade to 14 percent of a standard deviation by year four and are statistically insignificant thereafter. In math, the impacts start at 43 percent of a standard deviation but are statistically insignificant by year four and become modestly negative after six years. Qualitatively, however, the results concerning achievement impacts of third grade retention do not depend on the test scaling. Both approaches show large positive initial impacts of retention that fade out completely over time.

On the other hand, the results of same-grade comparisons presented in Table 5 confirm the existence of substantial positive effects on reading and math achievement that persist through grade 8, the highest grade reached by the first cohort of students retained under the test-based promotion policy. Specifically, retained students scored 25 percent of a standard deviation higher than their promoted peers in reading and math when both groups of students are first tested in grade 8. This is roughly equivalent to the average annual achievement gain Florida students make between grades 3 and 8. As discussed in Section 2, these estimates conflate the effects of being a year older and having received an additional year of schooling with the isolated effect of retention; they may also be biased by differential fade out of interventions received prior to grade 3. Even so, they may be of interest to policymakers seeking evidence on how test-based promotion policies affect the performance of retained students measured relative to other students in the same grade.

5.2 The Effect of Test-Based Retention on Grade Progression, Absences, and Special Education Placement

We next present estimates of the effect of third grade retention on subsequent grade progression, absences from school, and special education placement rates. Grade progression is an important outcome to consider when evaluating test-based promotion policies for at least two reasons. First, it has direct implications for retention's costs to both the individual and society. If early grade retention influences the probability that students are retained at higher grade levels, the cost of early grade retention in terms of foregone earnings and additional educational expenditures could be well below a full school year. Second, the effects of retention on outcomes

such as student achievement and attainment could vary according to the grade level at which the student is retained. If retention in early grades is more beneficial to students than later retention, test-based promotion policies targeting early grades could benefit students who would eventually be retained by ensuring that they are retained at a younger age.

Figure 10 depicts the reduced form relationship between third grade reading test scores and retention probabilities in each of the next six years after their initial third grade year. The figure indicates that students below the promotion cutoff are substantially less likely to be retained each year from two to five years after potential third grade retention.

The top panel of Table 6 shows the corresponding estimates of the effect of third grade retention on future retention probabilities for the full sample.[14] The IV estimates confirm that third grade retention reduces the probability that the student will be in the process of repeating a grade two years later by 11 percentage points. The effect is smaller in subsequent years, but remains statistically significant and ranges from 3 to 4 percentage points in magnitude in years three to five. The bottom panel of Table 6 uses grade level as the outcome variable in Equation (8), thereby providing direct evidence on the differences in the grade progression of retained and promoted students. The IV estimates show that six years after being retained in third grade, students impacted by Florida's test-based promotion policy are only 0.74 grade levels behind comparable peers who were promoted.

The evidence in Table 6 confirms that third grade retention substantially reduced the probability that Florida students at the promotion cutoff would be retained in future grades. Could these differences in the subsequent grade progression of retained and promoted students explain the fade out of test score impacts evident in Table 4? To evaluate this possibility, we assume that (1) the effects of retention on student achievement after one year are in fact fully persistent and (2) that students retained in subsequent grades experience the same short-term benefits, regardless of the grade in which they were retained. We then ask how much of the observed fade out in test score impacts from year one to year two would be explained by the additional gains made by students retained in year two. The results suggest that differences in subsequent retention could account for no more than 35 percent of the observed fade out in reading effects after two years and 22 percent of the fade out in math effects.[15] Additional analyses also confirm that the test score impacts in both subjects fade out even when students who were subsequently retained are excluded from the sample.

Table 7 reports estimates of the effect of third grade retention on student absences and special education placement. The results generally confirm that retention had no impact on these outcomes for students with third grade reading scores at the promotion cutoff. The lone exception is absences after three years, when the modest improvement in attendance for

[14] Appendix Table A-3 provides estimates of the impact of third grade retention on future retention probabilities by cohort.

[15] For example, the simple calculation in terms of reading is as follows: True fade out in reading effects between year one and two is given by 225.8 - 154.6 = 71.2 DSS points (see column 4 of Table 4). Fade out resulting from a 11 percentage point reduction in the probability of being retained after two years (see column 4 of Table 6) is given by 0.11* 225.8 = 24.6. Thus, roughly 35 percent of the fade out in reading effects after two years could be explained by effects on future grade retention.

retained students likely reflects the fact that most of them had not yet made the transition to middle school.[16] Again in contrast to our preferred IV results, the OLS estimates with controls would suggest statistically significant increases in absences in four of six years and increased rates of special education classification in three years.

5.3 Robustness Analysis

Table 8 presents the results of alternative specifications of our analysis of the effects of test-based retention on student achievement and the probability of future retention. To consolidate presentation, we combine the data on each outcome across multiple years into two models intended to summarize short-term (after 1-3 years) and longer-term (after 4-6 years) impacts. The achievement results are based on the unadjusted DSS scores used in Table 3. The table's first row presents the results from our preferred specification in this summary format; we then examine whether plausible modifications to that specification influence these results.

The next four rows confirm that our preferred results are robust to the use of alternatives to the ten test-score-point bandwidth ranging from five to 25 points on either side of the cutoff.[17] Achievement impacts in both subjects are consistently more positive using wider bandwidths, but the differences are modest in size. No consistent pattern with respect to bandwidth choice is evident in the results for future retention. We next show that the results are not influenced by the exclusion of students at or within one test score point of the promotion cutoff, as could be the case if there were sorting on unobserved characteristics. The following row confirms that our results are essentially unchanged when we use school fixed effects to restrict comparisons to students attending the same school, thereby ruling out the possibility that the reported effects reflect differences in the quality of schools retaining more and fewer students. Finally, the table's last row shows that the results are also robust the use of quadratic terms in modeling the relationship between third grade reading scores and the probability of retention on either side of the cutoff.

One other potential concern with interpreting our results as the causal effect of test-based retention is the possibility of labeling effects (Papay et al., 2011). Students scoring above the cutoff are labeled as level 2 readers, while students below the cutoff are labeled as level 1 readers. Although there are no explicit consequences apart from the promotion decision of being a level 2 rather than a level 1 reader, these labels could alter the behavior of students, teachers, and parents in ways that affect students' subsequent achievement. To test whether labeling effects bias our estimates of test-based retention, we conduct a placebo test using the two cohorts of students in our data that entered third grade before 2003 and therefore were unaffected by the promotion policy. Table 9, which shows reduced form estimates of the effect of being labeled a level 1 reader confirms that being below the cutoff had no effect on future achievement for

[16] Schwerdt and West (2013) show that the modal Florida student enters middle school in grade six and experiences an increase in absences of roughly one day (relative to students attending K-8 schools) upon making this transition.

[17] These alternatives more than encompass the informal sensitivity test suggested by Nichols (2007) of using twice and half the preferred bandwidth.

these students. Labeling effects are thus unlikely to confound our estimates of retention effects.

5.4 Subgroup Results

Our analysis thus far has focused on the local average treatment effect of test-based retention for all students performing at the promotion cutoff. This approach could conceal important heterogeneities in local treatment effects across subgroups. It also raises the question of whether similar patterns would hold for higher-achieving students were they to be retained.

Table 10, which presents results for several key subgroups in the same format as Table 8, provides little evidence of systematic heterogeneity across subgroups based on gender, ethnicity, or free/reduced-price lunch eligibility. The short-term and longer-term achievement effects of retention appear to be modestly less positive for black students than for whites or Hispanics, a pattern which may warrant attention in future research on the adult outcomes of students retained in Florida. The achievement gains from retention also appear to be larger and more persistent for students who were absent from school more often in grade 3.[18] This suggests that test-based retention may be particularly beneficial for low-achieving students whose initial third grade year was disrupted by repeated absences.

The remaining rows in Table 10 examine whether our estimates of retention effects are local to students at a specific achievement level, exploiting the fact that there is considerable variation in the math achievement of Florida students who are retained on the basis of their reading test scores. Among students in our preferred bandwidth, 20,537 (27 percent) were classified as performing at level one (of five) based on the third grade math test, 26,357 (35 percent) performed at level two, and 29,253 (29 percent) performed at level three or higher. The first-stage results in column (1) show that the increase in the probability of retention at the promotion cutoff was more than twice as large for students performing at level one in math as for students performing at level three or above, suggesting that students' math performance influenced whether they were granted an exemption from the retention requirement. The estimated effects of grade retention on reading and math achievement are quite similar across all three groups, however, providing at least suggestive evidence that the short-term benefits of test-based retention are not limited to students achieving at a specific level.

Similar to the subgroup analysis by student characteristics, Table 11 examines whether the effects of retention vary according to the characteristics of the school attended in third grade. For simplicity, we split the discontinuity sample into two subgroups at the median of each available school characteristic. The results provides little evidence of systematic heterogeneity in the effects of test-based retention by pupil/teacher ratio, expenditure per student, average teacher experience, and average teacher salary. There is some evidence, however, that the positive effects of test-based retention are more pronounced in schools with below-median retention and failure rates. This could indicate that retained students receive more attention when there are fewer of them, which may reinforce any beneficial impact of test-based retention.

[18] Among students in our preferred bandwidth, 28 percent were absent 10 days or more and 47 percent were absent fewer than 5 days during their initial third grade year.

Although we find only limited evidence of heterogeneous retention effects, policymakers may nonetheless be interested in which students subgroups are most impacted by the introduction of a test-based promotion policy. Because some retained students (i.e., always-takers) would have been retained regardless of whether they scored below the promotion cutoff and other students (i.e., never-takers) would never be retained, students complying with the policy cannot be individually identified. We can, however, use the first-stage estimates of the effect of scoring below the promotion cutoff on retention probabilities for students with various characteristics to describe the distribution of these characteristics among compliers (c.f. Angrist and Pischke, 2009). A standard complier analysis based on the estimates reported in Column 1 of Table 10 suggests that compliers are on average not very different in their observable student characteristics from the average student in the discontinuity sample, with one notable exception. For students with different math achievement levels we see substantial differences in compliance rates. For example, a complier is 37 percent more likely to score at level one in math than the average student in the discontinuity sample.[19]

5.5 Potential Mechanisms

As discussed above, Florida requires that students retained under its test-based promotion policy receive remedial services intended to help them acquire the reading skills needed to be promoted the following year. These include the opportunity to attend a summer reading program prior to the next school year, assignment to a "high-performing" teacher, and intensive reading interventions during the retention year. Any of these program components could in theory account for part or all of the short-term academic gains we have documented for retained students.

Unfortunately, a lack of detailed information on the implementation and take-up of the policy's summer programming component makes it impossible to disentangle its separate effect. We note, however, that Matsudaira's (2008) regression discontinuity study of mandatory summer school for low-achieving grade 3-5 students in a large urban district finds average effects of 0.12 standard deviations in both reading and math. Jacob and Lefgren (2004) find that attending summer school after third grade improved the achievement of retained students in Chicago by 0.05 standard deviations in reading and 0.07 standard deviations in math after two years. Even if summer school attendance among students retained under Florida's policy were quite high, it is therefore unlikely that it accounts for more than a fraction of the overall gains we observe for retained students.

We do have information on the teachers to which roughly 60 percent of the retained students were assigned in both their initial and repeated third grade year. Because the evaluation systems Florida school districts used during this period rated very few teachers as ineffective, the requirement that retained students be assigned to a high-performing teacher did not meaningfully constrain classroom placements. Even so, our data indicate that 94 percent of retained students were assigned to a different teacher during their retention year. Average class sizes

[19]Table A-4 provides the results of a complier analysis across all available student characteristics.

for retained students also fell by almost two students, from 19.6 to 17.7, between their first and second years in the third grade.

In Table 12, we therefore use our regression discontinuity approach (same-grade comparison) to estimate the effect of being retained on two characteristics of the teachers to which students are assigned, as well as on their class size, in grades 3-5. The first row of Table 12 confirms that students retained in third grade are assigned to smaller classes compared to those non-retained students had experienced in third grade. Moreover, they are roughly 8 percentage points less likely to be assigned to a teacher with less than 2 years of experience. However, in grades 4 and 5 (rows 2 and 3) we no longer observe any significant differences with respect to class size or teacher experience. Nor do we find any evidence for systematic differences in grades 4 and 5 with respect to teacher quality as measured by value-added to student achievement.[20]

In sum, this evidence suggests that Florida schools did take steps to ensure that students were placed with different and possibly more effective teachers when repeating the third grade, but that any effects on teacher assignments were limited to that year. The lack of prior test scores for third grade students prevents us from constructing value-added estimates that would allow us to examine the effectiveness of third grade teachers directly. However, a recent review by Hanushek and Rivkin (2010) indicates that the within-school standard deviation of teacher value added to reading (math) test scores is, on average, 0.13 (0.17) standard deviations. Feasible improvements in teacher effectiveness during the retention year could therefore explain some of the short-term gains made by students retained under the Florida policy, but are unlikely in our view to be the only mechanism. Rather, it appears that the majority of the gains are attributable to the combination of a pure retention effect and whatever supplemental interventions students receive during the retention year.

6 Conclusion

Our analysis exploits a discontinuity in the probability of grade retention under Florida's test-based promotion policy to study the policy's effects on students retained in the third grade up to six years later. Based on same-age comparisons, we find evidence of substantial short-term gains in both math and reading achievement. However, these positive effects fade out over time and become statistically insignificant within five years. We also find that third grade retention and remediation substantially reduces the probability of being retained in later grades but has no clear impact on student absences or special education placement rates.

In sum, our analysis provides more favorable evidence on the effects of early grade retention than found in many previous studies - in particular those which do not rely on credible quasi-

[20]We construct a single value-added measure for each math and reading teacher who could be linked to students in grades 4-5 that combines value-added estimates from all available years, grades, tests, and subjects. During our analysis period, Florida administered both the Florida Comprehensive Achievement Test and the Stanford Achievement Test in math and reading in these grades. In a given year, a teacher in a self-contained elementary classroom therefore has up to four separate value-added estimates. The methods used to construct these value-added estimates and average them across subjects, tests, and years are described in detail in Chingos and West (2012). We follow their procedures exactly, except that we exclude estimates based on the year for which the teacher assignment is the outcome when calculating teachers' average effectiveness.

experimental methods to address unobserved selection into the retention treatment. We show that test-based retention has substantial positive effects on reading and math achievement in the short run, has no detrimental effects on the limited set of outcomes we can measure, and generates educational and opportunity costs well below a full year when subsequent grade progression is taken into account. To the extent that early grade retention is more beneficial than later grade retention (as suggested by the results of Jacob and Lefgren, 2004, 2009), students who were retained in third grade and would have been retained later clearly benefited from the introduction of the Florida policy. However, we also do not provide definitive evidence that test-based retention in early grades is beneficial for students in the long run, even when it is accompanied by the requirement that students receive additional services.

The fade out of test score impacts is a common pattern in the literature on educational interventions, including those which have been shown to generate lasting impacts on adult outcomes. For example, Chetty et al. (2011) show that kindergarten classroom quality improves college enrollment and adult earnings despite the complete fade out of short-term test score gains. The same appears to be true of early childhood interventions such as the Perry and Abecederian preschool demonstration projects and the Head Start program (see Almond and Currie [2011] for a review). Whether students retained in Florida will also experience long-run benefits remains uncertain. However, same-grade comparisons confirm that these students are performing at the same level as their promoted peers despite the fact that the latter are closer to expected graduation. To the extent that additional time in school (conditional on achievement) increases, for example, the probability of graduation or post-secondary enrollment, early grade retention could generate benefits that outweigh the opportunity costs. An analysis of the effects of test-based retention on educational attainment should be feasible in Florida within a few years.

The Florida policy we have analyzed in this paper has emerged as a model for policymakers in other states. Arizona, Indiana, Oklahoma, and Ohio enacted test-based promotion policies modeled on Florida's between 2010 and 2012, and similar bills have been introduced in the legislatures of several other states. In light of this interest, we should emphasize that the effects on retained students are only one component of a comprehensive analysis of these policies' merits. Test-based promotion policies also aim to provide incentives for educators and parents to improve the skills of low-performing students prior to third grade. There are also a variety of potential mechanisms, such as the creation of more homogenous grade cohorts, that could influence outcomes for higher-performing students. With few exceptions (e.g., Babcock and Bedard, 2011), the broader consequences of policies influencing retention rates have received little attention and deserve further scrutiny.[21]

[21]Using within-state variation in primary school retention rates from 1960 to 1980, Babcock and Bedard (2011) show that a one standard deviation increase in retention rates is associated with a 0.7 percent increase in mean earnings for adult males.

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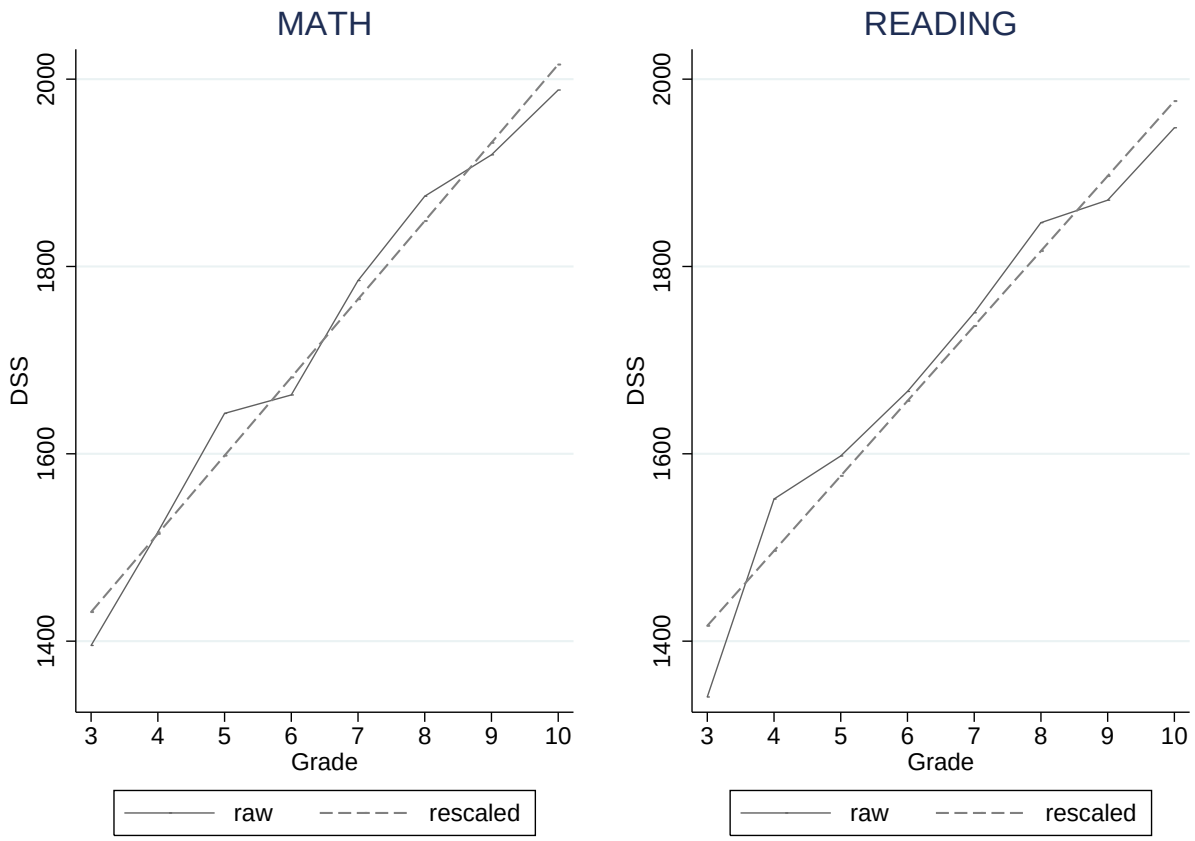
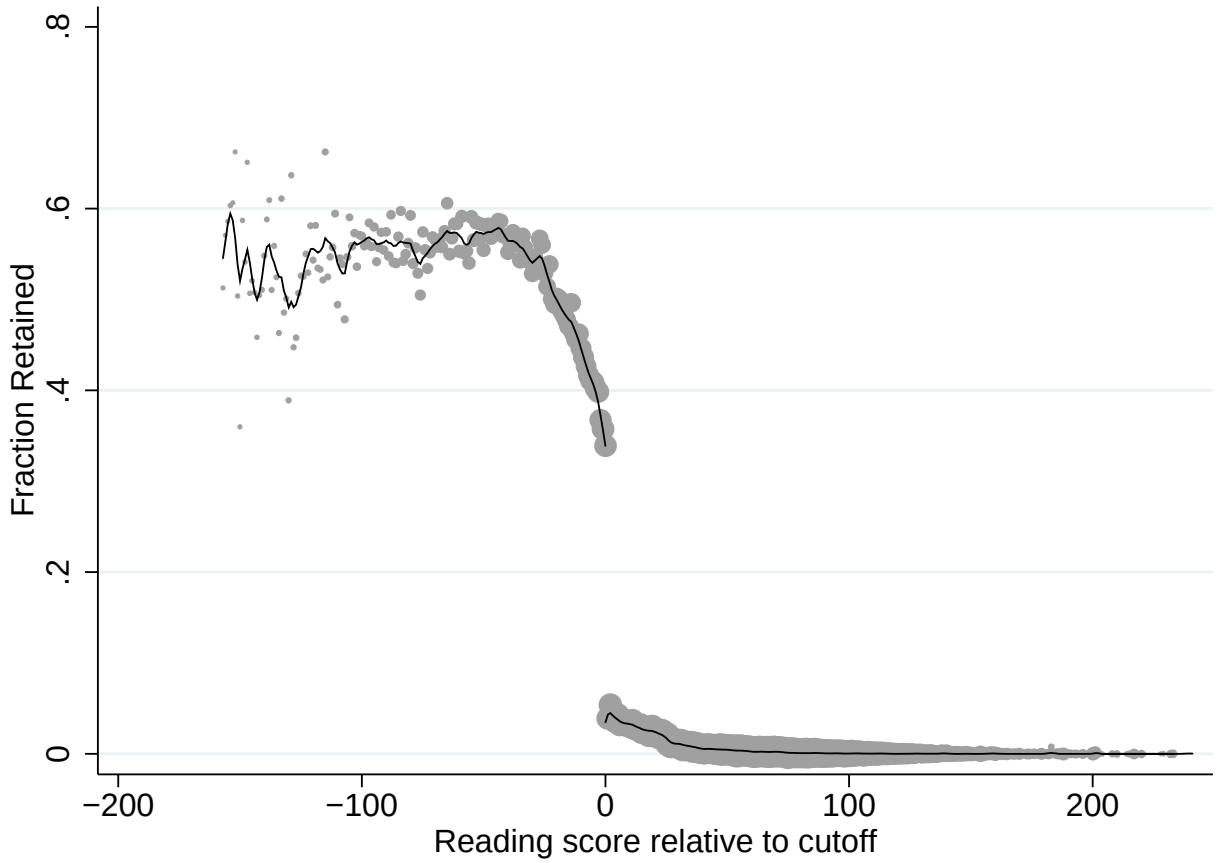
Figure 1: Average Developmental Scale Scores by Subject and Grade



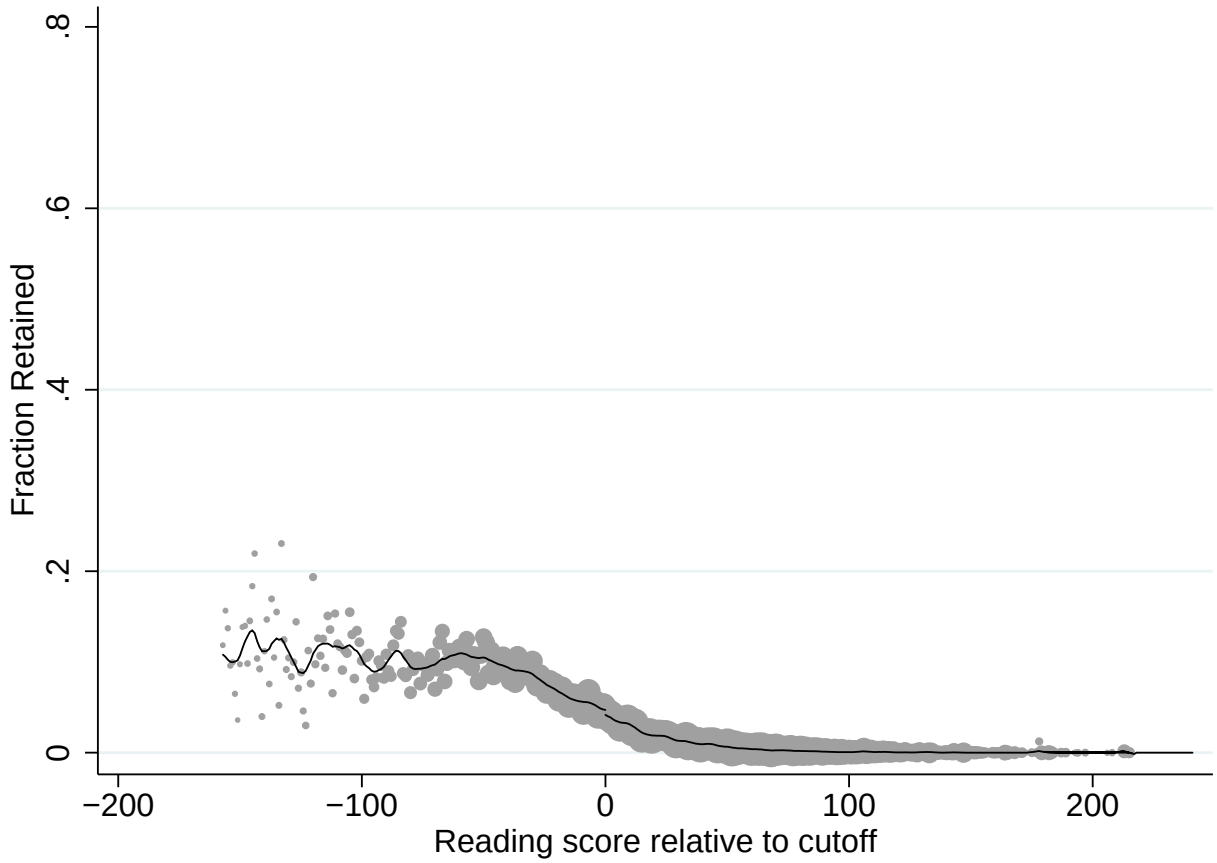
Note: Based on all students in grades 3 to 10 between 2002 and 2009. Rescaled scores stem from predicted values of a linear regression of developmental scale scores on grade levels.

Figure 2: The Relationship between Grade 3 Reading Scores and the Probability of Grade 3 Retention, 2003-2008



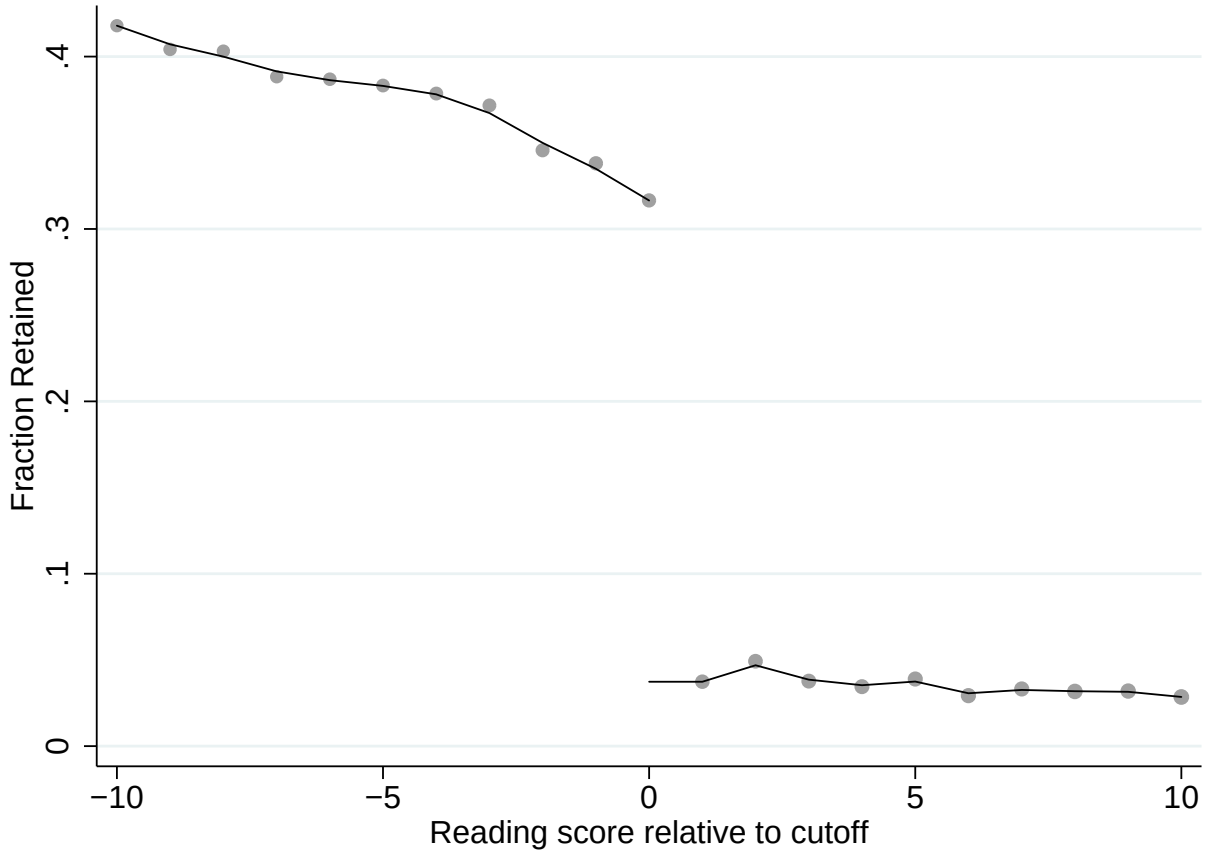
Note: Based on 2003-2008 cohorts. Full sample. Solid line represents predicted values from local linear regressions on both sides of the cutoff. Marker size represents relative group size.

Figure 3: The Relationship between Grade 3 Reading Scores and the Probability of Grade 3 Retention, 2001-2002



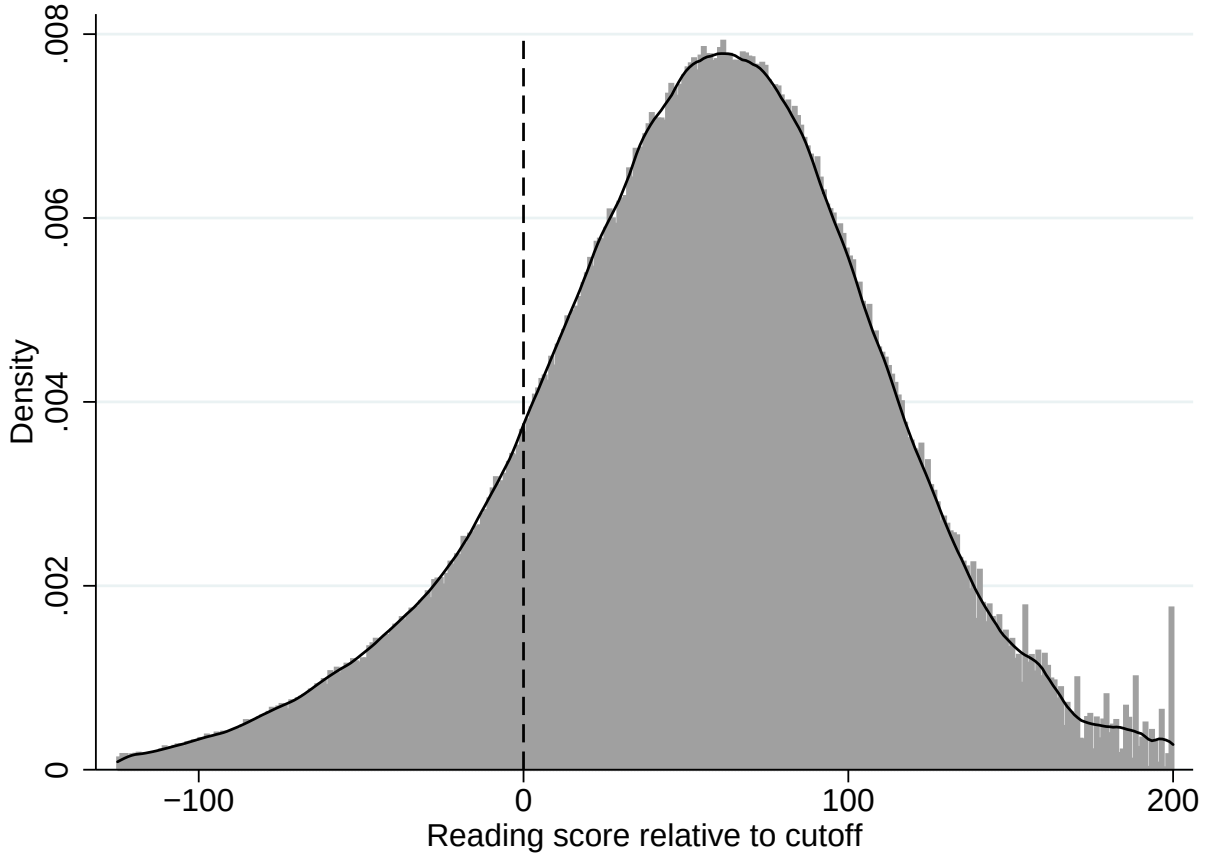
Note: Based on 2001-2002 cohorts. Full sample. Solid line represents predicted values from local linear regressions on both sides of the cutoff. Marker size represents relative group size.

Figure 4: The Relationship between Reading Scores and Grade Retention around the Cutoff



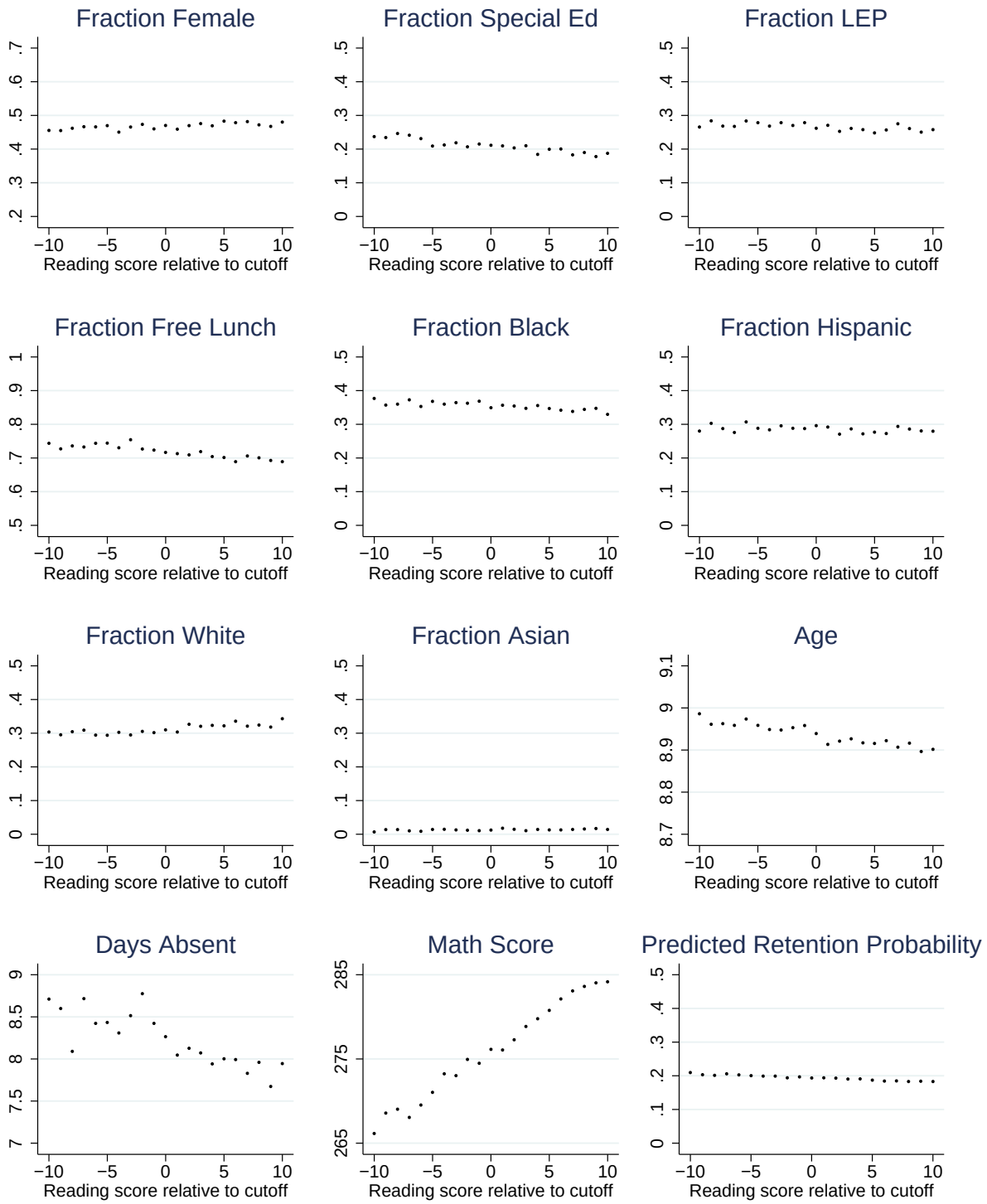
Note: Based on 2003-2008 cohorts. Discontinuity sample with 10-point bandwidth. Solid line represents predicted values from local linear regressions on both sides of the cutoff. Marker size represents relative group size.

Figure 5: Distribution of Reading Scores in Grade 3



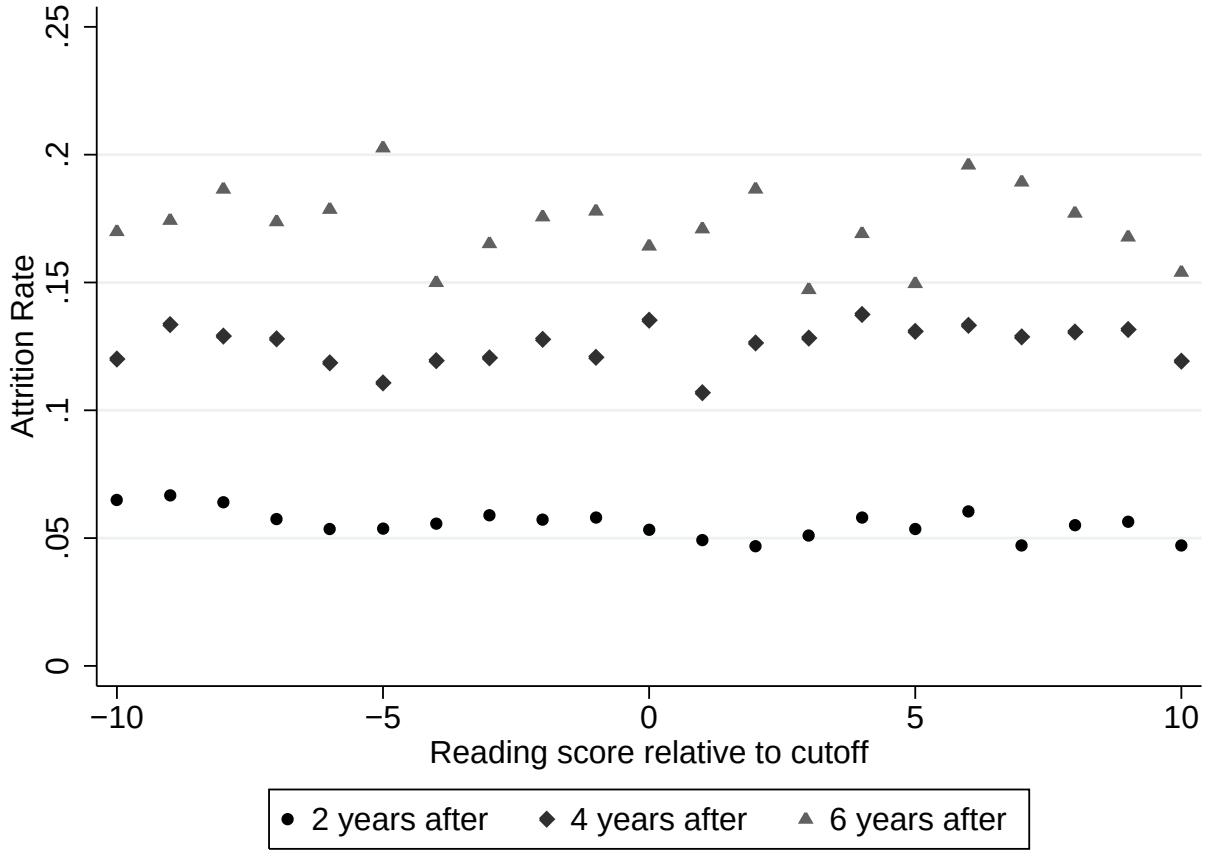
Note: Based on 2003-2008 cohorts. Full sample. Solid line represents kernel density estimates.

Figure 6: The Relationship between Reading Scores in Grade 3 and Student Characteristics



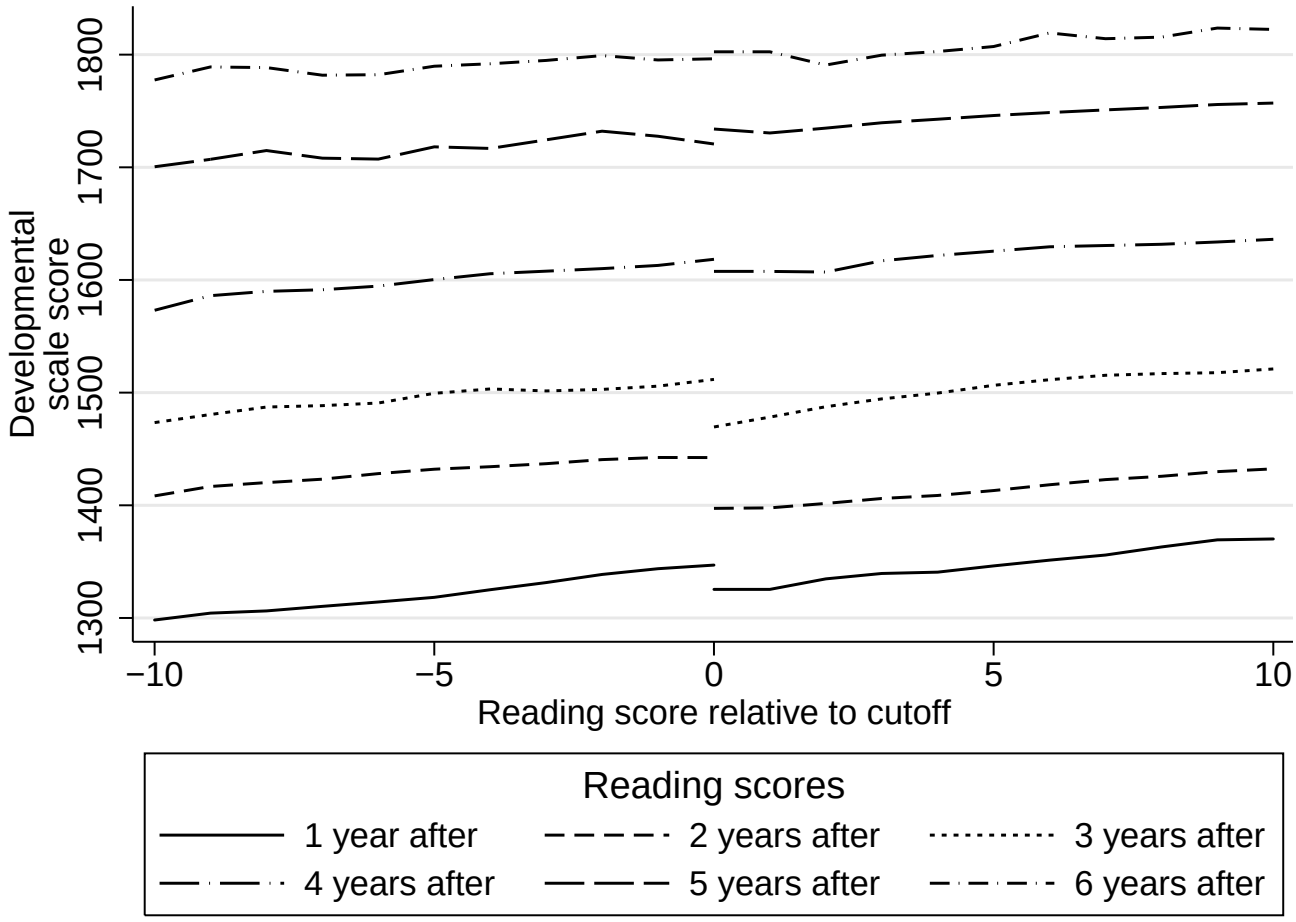
Note: Based on 2003-2008 cohorts. Discontinuity sample with 10-point bandwidth. Predicted retention probability displays predicted values after estimating a probit model that includes all student background variables except for reading scores as explanatory variables.

Figure 7: The Relationship between Reading Scores in Grade 3 and Subsequent Attrition from the Data around the Cutoff



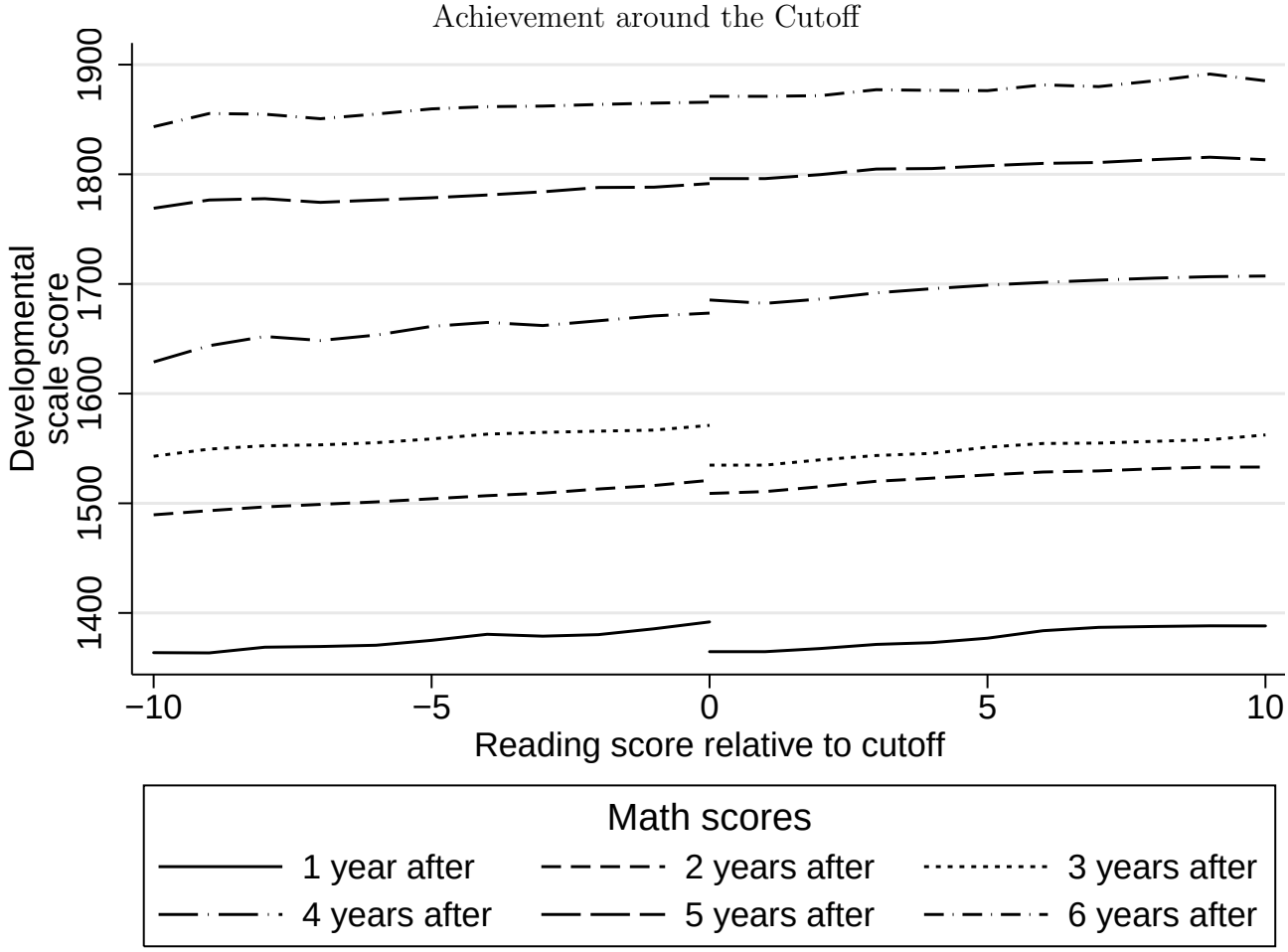
Note: Based on cohorts 2003-2008. Discontinuity sample with 10-point bandwidth.

Figure 8: The Relationship between Reading Scores in Grade 3 and Future Reading Achievement around the Cutoff



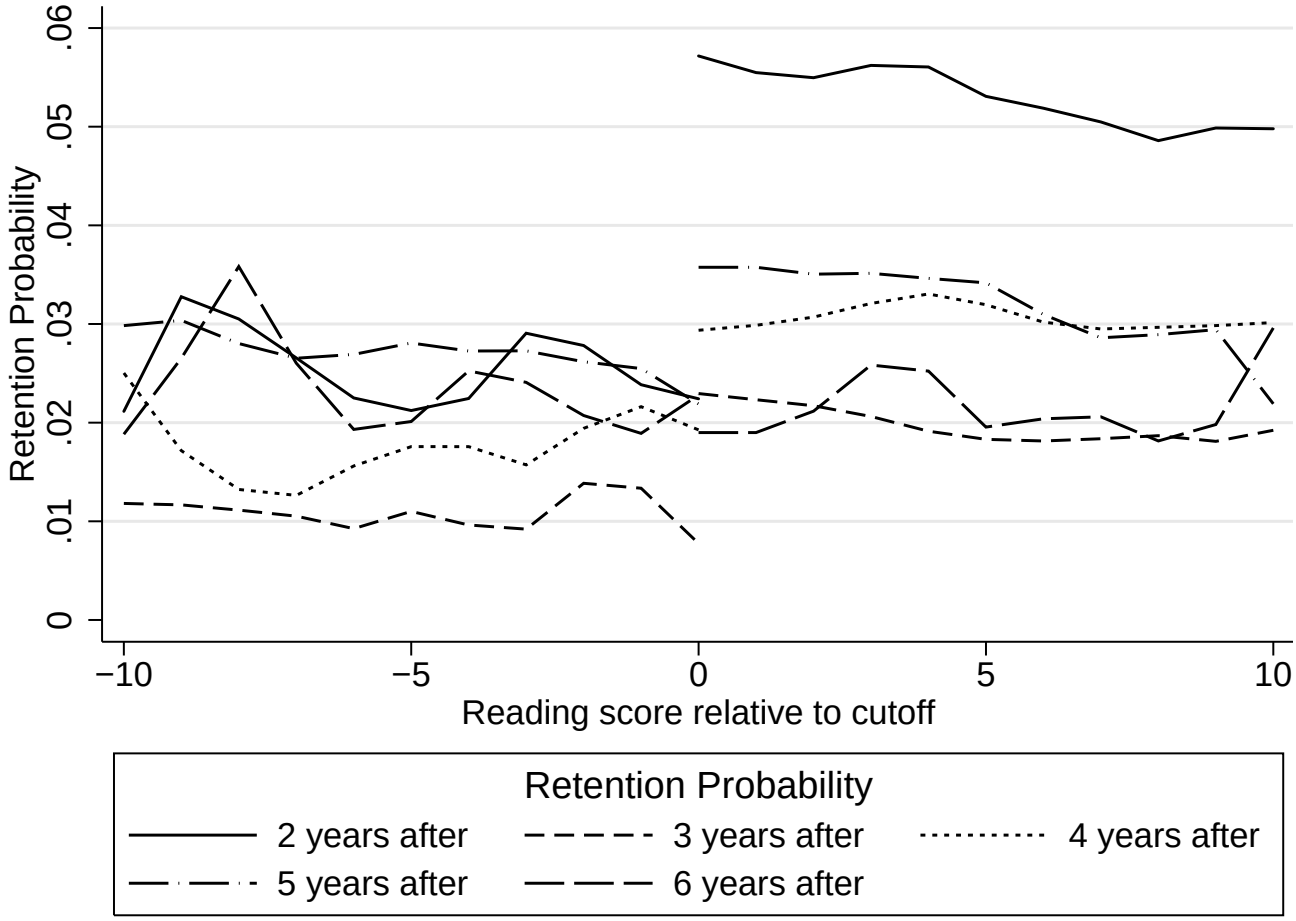
Note: Based on 2003-2008 cohorts. Discontinuity sample with 10-point bandwidth. Lines represent predicted values from local linear regressions on both sides of the cutoff.

Figure 9: The Relationship between Reading Scores in Grade 3 and Future Math Achievement around the Cutoff



Note: Based on 2003-2008 cohorts. Discontinuity sample with 10-point bandwidth. Lines represent predicted values from local linear regressions on both sides of the cutoff.

Figure 10: The Relationship between Reading Scores in Grade 3 and Future Grade Retention around the Cutoff



Note: Based on 2003-2008 cohorts. Discontinuity sample with 10-point bandwidth. Lines represent predicted values from local linear regressions on both sides of the cutoff.

Table 1: Summary Statistics

	Total	Failed Promotion Cuttoff	Retained	Retained, but above Cuttoff
FCAT Math	0.06	-1.13	-1.22	-0.83
FCAT Reading	0.07	-1.46	-1.43	-0.38
Female	0.49	0.42	0.42	0.46
Age	8.84	9.06	8.89	8.77
White	0.48	0.28	0.28	0.50
Black	0.22	0.38	0.40	0.29
Hispanic	0.24	0.31	0.29	0.15
Asian	0.02	0.01	0.01	0.01
Other	0.04	0.03	0.03	0.04
Free or reduced lunch	0.52	0.78	0.79	0.65
Limited English proficiency	0.19	0.30	0.29	0.11
Special Education	0.16	0.37	0.29	0.15
Days absent	7.46	9.10	9.28	10.13
Number of students	983,308	159,866	81,357	4,959

Note: Based on 2003-2008 cohorts. Full sample. Test scores in math and reading are standardized by subject, year, and grade to have a mean of zero and a standard deviation of one.

Table 2: Effect of Reading Performance on the Probability of Grade Retention in Grade 3

	Policy off					Policy on			
Cohorts	2001	2002	2003	2004	2005	2006	2007	2008	2003-2008
Below cutoff	0.006	0.020***	0.396***	0.298***	0.317***	0.372***	0.222***	0.234***	0.310***
	(0.006)	(0.007)	(0.013)	(0.015)	(0.014)	(0.017)	(0.013)	(0.014)	(0.006)
Below cutoff	–0.002	–0.004	–0.017	–0.048***	0.014	0.027	–0.029**	–0.002	–0.014**
× LEP	(0.007)	(0.007)	(0.014)	(0.015)	(0.015)	(0.017)	(0.013)	(0.014)	(0.006)
Below cutoff	–0.000	–0.003	–0.104***	–0.086***	–0.116***	–0.166***	–0.083***	–0.075***	–0.109***
× Special Education	(0.007)	(0.008)	(0.016)	(0.016)	(0.015)	(0.016)	(0.014)	(0.014)	(0.006)
Reading	–0.000	–0.001	–0.001	–0.000	–0.001	–0.001	–0.001	0.001	–0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Reading	0.000	0.000	–0.010***	–0.009***	–0.007***	–0.006**	–0.007***	–0.008***	–0.008***
× Below cutoff	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Additional covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	No	No	No	No	Yes
Students	17,676	16,516	15,687	12,040	12,435	9,981	12,995	11,536	74,674
R^2	0.018	0.020	0.299	0.209	0.233	0.261	0.161	0.171	0.230
F-statistic on instruments	0.33	2.71	314.47	142.74	184.60	187.64	106.80	104.94	1,014.72
Pr > F	0.80	0.04	0.00	0.00	0.00	0.00	0.00	0.00	0.00

* p<0.10, ** p<0.05, *** p<0.01

Note: Based on discontinuity sample with 10-point bandwidth. Dependent variable is a dummy indicating retention in grade 3 in all columns. Additional covariates (not reported) include special education status in grade 3, LEP status in grade 3, math scores, gender, age, race, and free or reduced-price lunch status in grade 3. Robust standard errors in parentheses.

Table 3: Effect of Grade Retention on Student Achievement
[Same Age Comparison]

	Specification			
	OLS		IV	
Dependent Variable	(1)	(2)	(3)	(4)
Reading (SD= 370)				
1 year (n = 74,443)	–60.68***	–41.19***	92.58***	94.58***
	(2.064)	(2.058)	(10.409)	(9.941)
2 years (n = 59,554)	58.18***	76.43***	183.6***	184.5***
	(2.287)	(2.263)	(11.653)	(11.179)
3 years (n = 45,175)	–4.555*	14.09***	98.24***	100.1***
	(2.691)	(2.650)	(12.462)	(11.989)
4 years (n = 35,001)	–53.18***	–35.85***	46.95***	48.29***
	(2.970)	(2.934)	(13.534)	(12.974)
5 years (n = 23,568)	–70.45***	–55.23***	–9.989	–6.667
	(3.180)	(3.135)	(13.842)	(13.201)
6 years (n = 12,912)	–30.21***	–14.74***	15.03	15.39
	(3.852)	(3.779)	(15.759)	(14.916)
Math (SD= 306)				
1 year (n = 74,327)	–1.454	47.85***	94.35***	95.43***
	(2.097)	(1.729)	(10.213)	(8.028)
2 years (n = 59,354)	–58.12***	–15.26***	29.00***	28.64***
	(2.100)	(1.789)	(10.064)	(8.048)
3 years (n = 45,093)	31.78***	73.82***	109.7***	111.5***
	(2.473)	(2.155)	(11.808)	(9.992)
4 years (n = 34,987)	–116.0***	–76.95***	–17.52	–19.26*
	(2.868)	(2.561)	(12.845)	(10.924)
5 years (n = 23,563)	–77.68***	–48.60***	–27.04**	–25.08**
	(2.800)	(2.473)	(12.148)	(10.344)
6 years (n = 12,905)	–57.20***	–31.37***	–4.812	–8.717
	(3.156)	(2.796)	(12.617)	(10.803)
Performance and demographic covariates	No	Yes	No	Yes

Note: Based on discontinuity sample with 10-point bandwidth. Dependent variables are unadjusted developmental scale scores in reading and math; reported standard deviations are for grade 3. All estimations control for special education status in grade 3, LEP status in grade 3, a linear function in grade 3 reading scores that allows for different trends at both sides of the cuttoff, and cohort dummies. Performance and demographic covariates include math scores, gender, age, race, and free or reduced-price lunch status in grade 3. Robust standard errors in parentheses.

Table 4: Effect of Grade Retention on Student Achievement (rescaled)
[Same Age Comparison]

	Specification			
	OLS		IV	
Dependent Variable	(1)	(2)	(3)	(4)
Reading (SD= 370)				
1 year (n = 74,443)	70.58***	90.07***	223.8***	225.8***
	(2.064)	(2.058)	(10.409)	(9.941)
2 years (n = 59,554)	26.13***	44.76***	153.7***	154.6***
	(2.286)	(2.261)	(11.660)	(11.175)
3 years (n = 45,175)	–14.99***	3.861	88.82***	90.71***
	(2.691)	(2.651)	(12.475)	(11.995)
4 years (n = 35,001)	–49.81***	–32.57***	49.82***	51.19***
	(2.970)	(2.934)	(13.522)	(12.963)
5 years (n = 23,568)	–57.34***	–42.45***	.764	4.004
	(3.170)	(3.125)	(13.743)	(13.123)
6 years (n = 12,912)	–73.64***	–56.91***	–21.77	–21.80
	(3.859)	(3.798)	(15.998)	(15.104)
Math (SD= 306)				
1 year (n = 74,327)	36.00***	85.30***	131.8***	132.9***
	(2.097)	(1.729)	(10.213)	(8.028)
2 years (n = 59,354)	–17.50***	24.89***	67.11***	66.77***
	(2.097)	(1.788)	(10.010)	(8.025)
3 years (n = 45,093)	–25.57***	17.44***	58.04***	59.84***
	(2.465)	(2.143)	(11.848)	(9.972)
4 years (n = 34,987)	–83.37***	–45.10***	10.81	9.156
	(2.854)	(2.542)	(12.647)	(10.757)
5 years (n = 23,563)	–71.11***	–42.27***	–21.46*	–19.60*
	(2.772)	(2.447)	(12.037)	(10.246)
6 years (n = 12,905)	–87.89***	–61.14***	–30.88**	–35.09***
	(3.185)	(2.836)	(12.931)	(11.080)
Performance and demographic covariates	No	Yes	No	Yes

Note: Based on discontinuity sample with 10-point bandwidth. Dependent variables are rescaled developmental scale scores in reading and math; reported standard deviations are for grade 3. All estimations control for special education status in grade 3, LEP status in grade 3, a linear function in grade 3 reading scores that allows for different trends at both sides of the cutoff, and cohort dummies. Performance and demographic covariates include math scores, gender, age, race, and free or reduced-price lunch status in grade 3. Robust standard errors in parentheses.

Table 5: Effect of Grade Retention on Student Achievement
[Same Grade Comparison]

	Specification			
	OLS		IV	
Dependent Variable	(1)	(2)	(3)	(4)
Reading (SD= 370)				
4th grade (n = 64,534)	116.5***	143.1***	267.2***	300.0***
	(2.205)	(2.424)	(11.545)	(13.608)
5th grade (n = 57,495)	87.18***	119.7***	229.5***	259.6***
	(2.576)	(2.786)	(13.872)	(15.816)
6th grade (n = 43,259)	63.28***	98.53***	184.0***	213.6***
	(2.951)	(3.149)	(15.061)	(16.849)
7th grade (n = 32,069)	38.39***	75.51***	130.0***	157.0***
	(3.207)	(3.443)	(17.291)	(19.364)
8th grade (n = 20,282)	18.63***	53.56***	70.00***	92.44***
	(3.298)	(3.506)	(17.653)	(19.748)
Math (SD= 306)				
4th grade (n = 64,423)	93.81***	145.9***	189.8***	197.6***
	(2.119)	(2.022)	(10.968)	(10.841)
5th grade (n = 57,266)	48.97***	110.4***	148.4***	163.5***
	(2.171)	(2.034)	(11.436)	(10.897)
6th grade (n = 43,225)	37.69***	110.4***	128.4***	155.6***
	(2.964)	(2.896)	(15.046)	(14.991)
7th grade (n = 32,080)	13.69***	72.32***	89.16***	106.9***
	(2.706)	(2.648)	(14.667)	(14.505)
8th grade (n = 20,262)	3.601	55.62***	56.84***	78.87***
	(2.821)	(2.739)	(15.403)	(15.332)
Performance and demographic covariates	No	Yes	No	Yes

Note: Based on discontinuity sample with 10-point bandwidth. Dependent variables are unadjusted developmental scale scores in reading and math. All estimations control for special education status in grade 3, LEP status in grade 3, a linear function in grade 3 reading scores that allows for different trends at both sides of the cuttoff, and cohort dummies. Performance and demographic covariates include math scores, gender, age, race, and free or reduced-price lunch status in grade 3. Robust standard errors in parentheses.

Table 6: Effect of Grade Retention in Grade 3 on Future Retention Probability and Grade Level

	Specification			
	OLS		IV	
Dependent Variable	(1)	(2)	(3)	(4)
Retention Probability				
2 years (n = 59,679)	−.0506***	−.0614***	−.110***	−.109***
	(.001)	(.002)	(.010)	(.010)
3 years (n = 44,271)	−.00833***	−.0124***	−.0295***	−.0295***
	(.001)	(.001)	(.007)	(.007)
4 years (n = 33,946)	−.0240***	−.0290***	−.0416***	−.0423***
	(.002)	(.002)	(.011)	(.011)
5 years (n = 22,746)	−.00226	−.00701**	−.0404***	−.0426***
	(.003)	(.003)	(.014)	(.014)
6 years (n = 12,384)	.00821**	.00525	−.00162	−.00205
	(.004)	(.004)	(.014)	(.014)
Grade Level				
2 years (n = 59,679)	−.944***	−.932***	−.878***	−.879***
	(.002)	(.002)	(.011)	(.011)
3 years (n = 44,271)	−.920***	−.902***	−.828***	−.829***
	(.003)	(.003)	(.014)	(.014)
4 years (n = 33,946)	−.885***	−.862***	−.755***	−.758***
	(.004)	(.004)	(.020)	(.020)
5 years (n = 22,746)	−.863***	−.835***	−.679***	−.685***
	(.006)	(.006)	(.030)	(.029)
6 years (n = 12,384)	−.857***	−.826***	−.734***	−.746***
	(.009)	(.009)	(.037)	(.036)
Performance and demographic covariates	No	Yes	No	Yes

Note: Based on discontinuity sample with 10-point bandwidth. Dependent variable is a dummy indicating grade retention in the top panel and the student's grade level in the bottom panel. All estimations control for special education status in grade 3, LEP status in grade 3, a linear function in grade 3 reading scores that allows for different trends at both sides of the cutoff, and cohort dummies. Performance and demographic covariates include math scores, gender, age, race, and free or reduced-price lunch status in grade 3. Robust standard errors in parentheses.

Table 7: Effect of Grade Retention on Student Absence and Special Education Placement

	Specification			
	OLS		IV	
Dependent Variable	(1)	(2)	(3)	(4)
Days absent				
1 year (n = 74,599)	.499***	.304***	–.309	–.374
	(.081)	(.081)	(.391)	(.384)
2 years (n = 59,597)	.499***	.326***	–.0630	–.147
	(.093)	(.092)	(.435)	(.426)
3 years (n = 45,267)	–.333***	–.485***	–1.267**	–1.422***
	(.110)	(.110)	(.497)	(.487)
4 years (n = 35,101)	.268*	.0313	–.654	–.785
	(.149)	(.148)	(.689)	(.673)
5 years (n = 23,659)	1.011***	.831***	1.331	.906
	(.207)	(.207)	(.939)	(.917)
6 years (n = 12,985)	1.735***	1.406***	–.608	–.867
	(.303)	(.302)	(1.167)	(1.140)
Special Ed Placement				
1 year (n = 74,674)	.0129***	.0122***	.0174	.0168
	(.003)	(.003)	(.012)	(.012)
2 years (n = 59,684)	.0197***	.0172***	.0179	.0169
	(.003)	(.004)	(.015)	(.015)
3 years (n = 45,299)	.0152***	.0114***	.00911	.00754
	(.004)	(.004)	(.017)	(.017)
4 years (n = 35,126)	.00608	.00139	.0119	.01000
	(.005)	(.005)	(.020)	(.020)
5 years (n = 23,681)	–.000179	–.00649	.0140	.00620
	(.006)	(.006)	(.024)	(.024)
6 years (n = 13,000)	.000190	–.00751	.0251	.0134
	(.007)	(.007)	(.028)	(.027)
Performance and demographic covariates	No	Yes	No	Yes

Note: Based on discontinuity sample with 10-point bandwidth. Dependent variable is the number of days absent in the school year in the top panel and a dummy indicating special education placement in the bottom panel. Performance and demographic covariates include math scores, gender, age, race, free or reduced-price lunch status in grade 3. Robust standard errors in parentheses.

Table 8: Robustness Checks

Outcomes:	1st Stage	Reading		Math		Retention	
Years:		1-3	4-6	1-3	4-6	2-3	4-6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Baseline	.310***	126.1***	19.37**	75.42***	–23.48***	–.0739***	–.0338***
	(.006)	(6.575)	(8.434)	(5.349)	(7.078)	(.007)	(.007)
Bandwidth 25	.314***	148.0***	32.21***	90.19***	–10.05**	–.0642***	–.0309***
	(.004)	(4.484)	(5.679)	(3.588)	(4.783)	(.004)	(.005)
Bandwidth 20	.311***	144.4***	35.77***	85.53***	–13.18**	–.0683***	–.0315***
	(.005)	(4.918)	(6.307)	(3.942)	(5.283)	(.004)	(.005)
Bandwidth 15	.314***	138.4***	28.26***	78.79***	–22.06***	–.0713***	–.0313***
	(.005)	(5.479)	(7.018)	(4.423)	(5.885)	(.005)	(.006)
Bandwidth 5	.302***	120.8***	15.18	74.50***	–29.78***	–.0750***	–.0254**
	(.008)	(9.493)	(12.037)	(7.735)	(10.005)	(.010)	(.010)
Bandwidth 1	.291***	102.2***	–3.826	79.82***	–34.52***	–.0805***	–.0266**
	(.011)	(11.044)	(13.565)	(8.892)	(11.462)	(.011)	(.011)
w/o cuttoff ±1	.321***	132.0***	14.49	67.72***	–24.21***	–.0679***	–.0389***
	(.007)	(8.487)	(11.043)	(6.969)	(9.208)	(.009)	(.010)
School fixed effects	.312***	128.1***	18.60**	76.66***	–25.21***	–.0737***	–.0336***
	(.006)	(6.527)	(8.318)	(5.284)	(6.948)	(.007)	(.007)
Quadratic	.313***	123.0***	18.50**	73.16***	–24.86***	–.0730***	–.0334***
	(.006)	(6.385)	(8.167)	(5.238)	(6.819)	(.007)	(.007)

Note: Top row indicates dependent variable. Second row indicates years after potential grade 3 retention. Column 1 shows first stage estimates, while columns 2-7 report the corresponding IV estimates. All estimations control for special education status in grade 3, LEP status in grade 3, a linear function in grade 3 reading scores that allows for different trends at both sides of the cutoff, cohort dummies, grade 3 math scores, gender, age, race, and free or reduced-price lunch status in grade 3. Estimated effects on achievement are based on unadjusted developmental scales scores. Robust standard errors in parentheses.

Table 9: Placebo Test: Reduced Form Estimates for 2001-2002 Cohorts

Panel A	Outcome: Reading Scores in					
	1 year	2 years	3 years	4 years	5 years	6 years
Below cutoff	–3.711	1.137	4.146	–2.623	0.799	–8.490*
	(4.248)	(4.395)	(4.605)	(4.600)	(4.039)	(4.406)
Reading	4.024***	3.677***	4.102***	3.110***	2.707***	1.864***
	(0.498)	(0.519)	(0.554)	(0.539)	(0.481)	(0.513)
Reading	–0.647	–0.278	–0.962	–0.648	–0.572	–0.397
× Below cutoff	(0.712)	(0.739)	(0.773)	(0.770)	(0.686)	(0.732)
Additional covariates	Yes	Yes	Yes	Yes	Yes	Yes
Students	34,028	31,800	30,237	29,713	28,937	26,804
R^2	0.109	0.088	0.108	0.105	0.120	0.109

Panel B	Outcome: Math Scores in					
	1 year	2 years	3 years	4 years	5 years	6 years
Below cutoff	0.049	–1.654	–3.357	–4.571	–4.419	–4.362
	(3.519)	(3.656)	(3.811)	(3.912)	(3.405)	(3.096)
Reading	1.322***	1.371***	0.746*	0.904**	0.621	0.934***
	(0.416)	(0.430)	(0.453)	(0.459)	(0.397)	(0.355)
Reading	–0.678	–0.901	–0.494	–0.765	–0.546	–1.591***
× Below cutoff	(0.589)	(0.610)	(0.637)	(0.652)	(0.571)	(0.510)
Additional covariates	Yes	Yes	Yes	Yes	Yes	Yes
Students	34,022	31,830	30,220	29,699	28,816	26,801
R^2	0.365	0.346	0.303	0.279	0.286	0.277

* p<0.10, ** p<0.05, *** p<0.01

Note: Based on discontinuity sample with 10-point bandwidth for the 2001-2002 cohorts. The table displays reduced form estimates for cohorts of students not affected by the policy. Dependent variables are unadjusted developmental scale scores in reading in panel A and math in panel B. The top row indicates the distance in years between the year the outcome is measured and the first time students attended third grade. Additional covariates include math scores, gender, age, race, free or reduced-price lunch status in grade 3. Robust standard errors in parentheses.

Table 10: Subgroup Results by Student Characteristics

Outcomes:	1st Stage	Reading		Math		Retention	
Years:		1-3	4-6	1-3	4-6	2-3	4-6
Subgroup	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Baseline	.310***	126.1***	19.37**	75.42***	–23.48***	–.0739***	–.0338***
	(.006)	(6.575)	(8.434)	(5.349)	(7.078)	(.007)	(.007)
Girls	.295***	127.7***	19.40	75.21***	–24.74**	–.0807***	–.0281***
	(.008)	(9.383)	(11.975)	(7.958)	(9.946)	(.009)	(.008)
Boys	.325***	124.5***	20.73*	75.23***	–24.15**	–.0686***	–.0391***
	(.008)	(9.123)	(11.782)	(7.161)	(9.944)	(.009)	(.011)
White	.289***	157.9***	53.77***	91.19***	–.553	–.0845***	–.0465***
	(.011)	(13.356)	(16.806)	(10.317)	(13.532)	(.013)	(.014)
Black	.328***	101.0***	–19.22	65.15***	–44.41***	–.0831***	–.0258**
	(.009)	(9.982)	(13.048)	(8.552)	(11.709)	(.011)	(.013)
Hispanic	.308***	127.4***	23.31	67.60***	–20.61*	–.0427***	–.0288**
	(.013)	(12.117)	(15.268)	(9.726)	(12.165)	(.010)	(.011)
Age 9 or above	.306***	130.0***	24.60**	80.61***	–14.73*	–.0631***	–.0382***
	(.007)	(7.716)	(10.017)	(6.241)	(8.441)	(.008)	(.009)
Age 8 or below	.319***	108.5***	8.066	65.48***	–43.01***	–.0984***	–.0234**
	(.012)	(11.337)	(14.141)	(8.910)	(10.926)	(.012)	(.012)
Free or reduced lunch	.337***	116.8***	12.72	73.53***	–18.32**	–.0739***	–.0386***
	(.007)	(7.176)	(9.289)	(5.957)	(7.988)	(.008)	(.009)
LEP Students	.305***	135.7***	29.15*	73.62***	–32.96***	–.0541***	–.0314***
	(.010)	(12.277)	(15.427)	(9.941)	(12.449)	(.010)	(.011)
Special Ed Students	.216***	116.2***	55.10**	60.91***	–22.74	–.0620***	–.0276
	(.011)	(21.432)	(28.096)	(17.969)	(23.189)	(.016)	(.019)
Days absent > 10	.328***	149.9***	77.06***	98.82***	15.82	–.0824***	–.0610***
	(.011)	(12.278)	(15.973)	(10.114)	(13.793)	(.014)	(.016)
Days absent $5 - 10$	.313***	135.2***	19.24	78.05***	–21.32	–.0638***	–.0292**
	(.012)	(12.899)	(16.939)	(10.443)	(14.184)	(.012)	(.014)
Days absent < 5	.296***	106.6***	–13.76	61.89***	–45.06***	–.0773***	–.0229**
	(.009)	(9.774)	(12.295)	(7.888)	(10.115)	(.009)	(.010)
Math Level 1	.423***	105.1***	7.832	66.45***	–39.14***	–.0964***	–.0200*
	(.012)	(9.732)	(12.783)	(8.985)	(11.994)	(.012)	(.012)
Math Level 2	.331***	129.0***	24.14*	86.49***	–22.94**	–.0711***	–.0426***
	(.010)	(10.306)	(13.096)	(8.144)	(10.804)	(.011)	(.012)
Math Level ≥ 3	.208***	149.1***	16.64	60.61***	–10.62	–.0495***	–.0377***
	(.009)	(15.846)	(19.639)	(11.468)	(14.535)	(.012)	(.014)

Note: Based on discontinuity sample with 10-point bandwidth. Top row indicates dependent variable. Second row indicates years after potential grade 3 retention. Column 1 shows first stage estimates. Columns 2-7 report IV estimates with performance and demographic covariates. Estimated effects on achievement are based on unadjusted developmental scales scores. Robust standard errors in parentheses.

Table 11: Subgroup Results by Third Grade School Characteristics

Outcomes:	1st Stage	Reading		Math		Retention	
Years:		1-3	4-6	1-3	4-6	2-3	4-6
Subgroup	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Baseline	.310***	126.3***	19.35**	75.53***	–23.49***	–.0739***	–.0338***
	(.006)	(6.573)	(8.435)	(5.348)	(7.078)	(.007)	(.007)
Pupil/teacher ratio							
≥ median	.338***	132.2***	25.08**	78.39***	–19.36**	–.0771***	–.0382***
	(.009)	(8.849)	(10.238)	(7.155)	(8.503)	(.009)	(.009)
< median	.288***	119.4***	6.959	72.62***	–31.45**	–.0702***	–.0263**
	(.008)	(9.735)	(14.624)	(7.969)	(12.495)	(.010)	(.013)
Expenditure/student							
≥ median	.322***	114.3***	17.32	68.14***	–29.58***	–.0729***	–.0370***
	(.008)	(8.825)	(11.227)	(7.313)	(9.512)	(.009)	(.010)
< median	.301***	138.0***	24.88*	80.59***	–12.48	–.0743***	–.0315***
	(.009)	(10.224)	(13.091)	(8.174)	(10.885)	(.010)	(.011)
Teacher experience							
≥ median	.295***	120.8***	10.12	68.89***	–34.16***	–.0733***	–.0316***
	(.008)	(9.453)	(12.107)	(7.707)	(10.171)	(.009)	(.010)
< median	.327***	128.7***	32.94***	82.36***	–12.49	–.0715***	–.0414***
	(.009)	(9.320)	(11.895)	(7.576)	(9.988)	(.009)	(.010)
Teacher salary							
≥ median	.326***	115.5***	7.921	68.84***	–42.44***	–.0620***	–.0270***
	(.009)	(8.760)	(11.043)	(7.114)	(9.150)	(.008)	(.009)
< median	.298***	136.4***	45.83***	84.97***	7.127	–.0885***	–.0437***
	(.008)	(10.125)	(13.121)	(8.254)	(11.162)	(.011)	(.012)
Retention rate							
≥ median	.387***	101.6***	8.457	68.72***	–24.93***	–.0663***	–.0332***
	(.009)	(7.482)	(9.000)	(6.193)	(7.685)	(.008)	(.008)
< median	.234***	162.8***	45.46**	86.46***	–24.81	–.0910***	–.0352**
	(.008)	(12.986)	(20.562)	(10.358)	(16.580)	(.013)	(.017)
Failure rate							
≥ median	.336***	109.5***	4.723	72.63***	–26.48***	–.0746***	–.0248***
	(.008)	(8.392)	(9.999)	(7.050)	(8.565)	(.009)	(.009)
< median	.289***	143.4***	38.79**	77.96***	–20.54	–.0736***	–.0512***
	(.009)	(10.517)	(15.462)	(8.275)	(12.537)	(.010)	(.012)

Note: Based on discontinuity sample with 10-point bandwidth. Top row indicates dependent variable. Second row indicates years after potential grade 3 retention. Column 1 shows first stage estimates. Columns 2-7 report IV estimates with performance and demographic covariates. Estimated effects on achievement are based on unadjusted developmental scales scores. Robust standard errors in parentheses.

Table 12: Mechanisms: IV estimates of the Effect of Retention in Grade 3 on Teacher Assignment and Class Size in Elementary School Grades

Outcome	Teacher quality estimates based on math scores	Teacher quality estimates based on reading scores	Teacher experience (in years)	Teacher with ≤ 2 years of experience	Class size
	(1)	(2)	(3)	(4)	(5)
Grade 3	*n.a.*	*n.a.*	.109	–.077**	–1.479***
			(.737)	(.035)	(.297)
Grade 4	–.005	–.001	.056	–.007	–.095
	(.015)	(.012)	(.777)	(.036)	(.324)
Grade 5	.006	.009	–.845	.007	.074
	(.017)	(.012)	(.903)	(.038)	(.363)

Note: Based on discontinuity sample with 10-point bandwidth. Dependent variable indicated in first row. Grade 3 refers to the retention year for students retained in grade 3. All IV estimations control for math scores, gender, age, race, free or reduced-price lunch status in grade 3. Robust standard errors in parentheses.

Table A-1: Attrition Analysis: Reduced Form Estimates for 2003-2008 Cohorts

Outcome	Missing Test Score Information in					
	1 year	2 years	3 years	4 years	5 years	6 years
Below cutoff	0.007	0.007	–0.003	0.004	–0.006	–0.014
	(0.004)	(0.005)	(0.006)	(0.007)	(0.009)	(0.012)
Reading	–0.001	–0.000	–0.000	0.000	–0.001	–0.002
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Reading	0.000	–0.001	0.000	–0.000	0.001	–0.000
× Below cutoff	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Additional covariates	Yes	Yes	Yes	Yes	Yes	Yes
Students	83,274	70,514	56,551	45,080	30,908	17,776
R^2	0.004	0.005	0.007	0.008	0.009	0.011

* p<0.10, ** p<0.05, *** p<0.01

Note: Based on discontinuity sample with 10-point bandwidth for the 2003-2008 cohorts. The table displays reduced form estimates for cohorts of students affected by the policy. Dependent variable is an indicator for missing test score information in a particular year. The top row indicates the distance in years between the year the outcome is measured and the first time students attended third grade. Additional covariates include math scores, gender, age, race, free or reduced-price lunch status in grade 3. Robust standard errors in parentheses.

Table A-2: Achievement Results by Cohort

Cohort	2003	2004	2005	2006	2007	2008
Reading						
1 year	58.989***	88.784***	114.329***	111.501***	172.555***	35.159
	(15.411)	(26.192)	(21.839)	(25.299)	(36.475)	(30.796)
2 years	221.823***	164.137***	119.808***	175.696***	206.972***	
	(17.783)	(28.983)	(25.748)	(23.915)	(34.191)	
3 years	81.107***	152.072***	84.745***	99.902***		
	(17.787)	(31.339)	(26.263)	(24.562)		
4 years	35.894**	55.503*	59.001**			
	(17.926)	(29.131)	(23.692)			
5 years	–26.040*	29.369				
	(15.470)	(24.150)				
6 years	15.386					
	(14.916)					
Math						
1 year	68.408***	116.292***	77.516***	112.458***	127.031***	83.242***
	(13.049)	(20.257)	(18.683)	(19.474)	(27.970)	(26.478)
2 years	1.024	21.355	22.700	42.381**	81.110***	
	(12.465)	(20.163)	(17.821)	(18.883)	(25.841)	
3 years	101.271***	108.067***	117.875***	137.378***		
	(15.006)	(27.577)	(20.503)	(20.207)		
4 years	–34.955**	–32.760	13.539			
	(16.416)	(22.038)	(19.578)			
5 years	–30.335**	–15.405				
	(12.270)	(18.708)				
6 years	–8.717					
	(10.803)					
Students	15,687	12,040	12,435	9,981	12,995	11,536

Note: Based on discontinuity sample with 10-point bandwidth. Dependent variables are unadjusted developmental scale scores in reading and math. The table displays IV estimates with performance and demographic covariates by cohort of students. A cohort is defined by the school year students attended third grade for the first time. The last row indicates the number of students by cohort in the first stage regression for outcomes after 1 year. Robust standard errors in parentheses.

Table A-3: Retention Results by Cohort

Cohort	2003	2004	2005	2006	2007
2 years	–.096***	–.185***	–.089***	–.085***	–.099***
	(.017)	(.031)	(.023)	(.023)	(.028)
3 years	–.027**	–.049**	–.031**	–.015	
	(.012)	(.020)	(.014)	(.011)	
4 years	–.049***	–.076***	–.002		
	(.015)	(.024)	(.018)		
5 years	–.038**	–.050**			
	(.016)	(.025)			
6 years	–.002				
	(.014)				
Students	15,687	12,040	12,435	9,981	12,995

Note: Based on discontinuity sample with 10-point bandwidth. Dependent variable is a dummy indicating grade retention. The table displays IV estimates with performance and demographic covariates by cohort of students. A cohort is defined by the school year students attended third grade for the first time. The first row shows first stage estimates. The last row indicates the number of students by cohort in the first stage regression for outcomes after 1 year. Robust standard errors in parentheses.

Table A-4: Characterizing Compliers

Variable	All students	Compliers	Relative likelihood that compliers have the characteristic indicated in each row
	(1)	(2)	(3)
Girl	0.47	0.45	0.95
Boy	0.53	0.56	1.05
White	0.32	0.30	0.93
Black	0.35	0.37	1.06
Hispanic	0.28	0.28	0.99
Age 9 or above	0.75	0.74	0.99
Age 8 or below	0.25	0.26	1.03
Free/reduced lunch	0.71	0.77	1.09
Days absent > 10	0.27	0.29	1.06
Days absent 5-10	0.25	0.26	1.01
Days absent < 5	0.47	0.45	0.95
Math Level 1	0.28	0.38	1.37
Math Level 2	0.35	0.37	1.07
Math Level ≥ 3	0.37	0.25	0.67

Note: The table reports an analysis of complier characteristics. Column 1 reports the shares of students with the characteristic indicated in each row among all students in the discontinuity sample. Column 2 reports the shares of students with the characteristic indicated in each row among compliers. Column 3 reports the ratio of the first stage estimate for individuals with that characteristic to the first stage for the discontinuity sample as a whole, a statistic which can be interpreted as the relative likelihood that compliers have this characteristic. Based on discontinuity sample with 10-point bandwidth for the 2003-2008 cohorts.