# Early Grade Retention Harms Adult Earnings

Jiee Zhong *

Miami University

## Abstract

This paper provides new causal evidence on the effects of grade retention on educational attainment, behavioral outcomes, and labor market performance by analyzing Texas's reading test-based retention policy. Using a fuzzy regression discontinuity design, I find that third-grade retention significantly reduces annual earnings at age 26 by $3,477 (19%). While temporarily improving test scores, retention increases absenteeism, violent behavior, and juvenile crime, and reduces the likelihood of high school graduation. Moreover, retained students exhibit higher community college enrollment but lower public university attendance, though neither estimate is statistically significant.

**Keywords:** Third-Grade Retention, Educational Attainment, Behavioral Outcomes, Labor Market Performance

# 1 Introduction

One in ten public school students in the United States has been retained at least once between kindergarten and twelfth grade, with retention rates rising to 21% among Black students and 12% among Hispanic students (National Center for Education Statistics, 2010). The implementation of grade retention has been the subject of debate, with proponents citing its potential to improve the academic performance of low-achieving students through additional time to acquire essential knowledge. Conversely, opponents argue that grade retention can negatively impact student outcomes by creating a sense of punishment and stigma. Given the prevalence and growth of grade retention practices and their disproportionate impacts on disadvantaged populations, it is imperative to examine the effects of this policy.

The existing literature on grade retention presents a mixed view of its intermediate effects. Research focusing on third-grade retention policy shows improvements in reading and math scores (Jacob and Lefgren, 2004; Schwerdt et al., 2017; Figlio and Özek, 2020). These gains, however, tend to diminish in the subsequent years and do not lead to lasting improvements in educational attainment, such as high school graduation or college enrollment (Schwerdt et al., 2017). In contrast, Özek (2015) finds that the third-grade retention policy in Florida harms economically disadvantaged students by increasing disciplinary incidents. Furthermore, retention policies implemented in later grades tend to produce more deleterious outcomes. Studies (Jacob and Lefgren, 2009; Manacorda, 2012; Eren et al., 2022) indicate that eighth-grade retention substantially increases the risk of school dropout and violent crime conviction. Despite these conflicting results, the impacts of grade retention on long-term outcomes remain insufficiently examined.

This study addresses a critical gap in the literature by providing the first causal evidence on the effects of early grade retention on labor market outcomes. It also offers new causal evidence on the impact of grade retention on behavioral incidents and educational attainment, with a particular focus on college enrollment and graduation, areas that have received limited attention in prior research.[1] To do so, I utilize a reading test-based third-grade retention policy in Texas. This policy

---

[1]Schwerdt et al. (2017) provides suggestive evidence on the impact of Florida's third-grade retention policy on

requires third graders to pass the promotion cutoff on the state standardized reading test within three attempts to advance to fourth grade. This cutoff varies by the test difficulty and is undisclosed to students before assessment, creating a quasi-experimental environment. Students whose third-attempt reading scores are close to this cutoff are comparable in their characteristics, differing primarily in their likelihood of retention in third grade. Leveraging the discontinuity in retention probability at the cutoff, this study employs a fuzzy regression discontinuity design (fuzzy RDD) to estimate the causal effects of third-grade retention on educational attainment, behavioral issues, and labor market outcomes.

The results of this study demonstrate that while third-grade retention temporarily improves test scores, it increases absenteeism, violent behaviors, and even juvenile crime. These conflicting findings on short-term test scores and behavioral outcomes are consistent with the findings shown in prior works. Extending the analyses to long-term outcomes, I find that third-grade retention further lowers the likelihood of graduating from high school by 14%. Retention is also associated with higher community college enrollment but lower public university attendance, though neither estimate is statistically significant. After entering the labor market, retained students earn $3,477 less in annual earnings at age 26, representing a 19% reduction compared to the mean of peers who barely pass the cutoff. This earnings loss likely stems from a decreased likelihood of obtaining high-paying jobs and reduced labor force participation. The 19% decline is more significant than the 9% earnings reduction at age 28 reported by Meulen (2023), a working paper studying the effect of repeating the twelfth grade in the Netherlands. The disparity suggests that repeating third grade is more detrimental to long-term economic success than retention in later grades. This is likely due to prolonged exposure to the stigma and disengagement effects associated with retention, as shown by the increases in absenteeism, violence, and juvenile crime observed in this study. Furthermore, the subgroup analysis reveals higher retention rates for Hispanic, female, and low-income students than their counterparts. However, when examining the effects of third-grade re-

college enrollment but is limited by data constraints, including partial cohort coverage, exclusion of out-of-state enrollments, and lack of graduation data. This study overcomes these limitations by incorporating nationwide college enrollment data for all cohorts and detailed graduation data from Texas."

tention on earnings, no statistically significant differences emerge across demographic subgroups, including comparisons by race, gender, income level, and English proficiency status.

This study contributes to the literature on grade retention policies by providing a new causal analysis of the effect of early grade retention on educational attainment, behavioral incidents, and labor market outcomes. The findings offer important insight into the current debate on whether grade retention harms students. Previous studies present mixed findings, showing temporary gains in test scores alongside an increase in behavioral incidents (Eren et al., 2022; Figlio and Özek, 2020; Schwerdt et al., 2017; Eren et al., 2017; Özek, 2015; Jacob and Lefgren, 2009). Beyond examining intermediate outcomes, this study provides new causal evidence that third-grade retention has long-term adverse effects on low-achieving students by reducing their likelihood of high school graduation, shifting enrollment from public universities to community colleges, and lowering earnings in early adulthood.

Despite variations in policy design and sample populations across states and countries, this study—along with Jacob and Lefgren (2009), Eren et al. (2022), and Meulen (2023)—consistently finds that grade repetition alone harms students by increasing behavioral issues, reducing the likelihood of high school completion, and lowering earnings outcomes. These studies contrast with those examining Florida's third-grade retention policy (Figlio and Özek, 2020; Schwerdt et al., 2017; Özek, 2015; Greene and Winters, 2007), which estimate the combined effects of grade repetition, a summer program, intensive 90-minute daily reading instruction in the subsequent year, and assignment to high-quality teachers. However, the short-term results are consistent, as Florida's retention policy also leads to temporary gains in test scores and increased disciplinary incidents (Schwerdt et al., 2017; Özek, 2015). The gap emerges when examining long-term educational attainment, as Schwerdt et al. (2017) finds that Florida's retention policy does not impact the likelihood of high school graduation. One potential explanation for this null effect is that the initial benefits of remediation diminish over time, potentially offset by the unintended consequences of academic tracking (Figlio and Ozek, 2024).

This study also complements a growing body of literature on education interventions (Chetty

3

et al., 2011; Lindqvist and Vestman, 2011; Deming, 2011; Heckman et al., 2006; Heckman and Rubinstein, 2001), which demonstrates lasting impacts on adult outcomes despite fade-out gains in test scores. This study documents a similar pattern under Texas's third-grade retention policy. The findings of these studies underscore the critical role of non-test score outcomes in predicting long-term education and economic success, particularly for low-achieving students.

The policy implications of this study are particularly relevant to students who are marginally affected by Texas's third-grade retention policy. These students are also more likely to rank at the lower end of the ability distribution and are predominantly from low-income and racial minority groups. The results of this study reveal that instead of aiding these struggling students academically, third-grade retention exacerbates behavioral problems, reduces educational attainment, and further lowers their earnings upon entering the labor market. The disproportionately high retention rates among disadvantaged groups, coupled with lower high school graduation and earnings outcomes, suggest that retaining low-achieving students in third grade further deepens educational and income inequalities.

## 2 Background

The No Child Left Behind Act of 2001 led to the widespread adoption of test-based accountability measures, including a test-based grade retention policy to improve the test scores of low-achieving students by replacing social promotion with proficiency-based advancement. Texas implemented a test-based grade retention policy beginning with third-grade reading in the 2002-03 school year and later extended to fifth-grade reading and math in 2004-05, followed by eighth-grade reading and math in 2007-08.[2] Third grade serves as a critical intervention point, marking the transition from learning to read to reading to learn. Accordingly, this study examines the impact of Texas's third-grade retention policy. Moreover, the estimate of the effect of third-grade retention policy is less likely to be confounded by other retention policies applied after third grade.

---

[2]Texas stopped implementing the third-grade reading test-based retention policy in the 2009-10 school year.

Under the reading test-based retention policy, third-grade students must pass the Texas Assessment of Knowledge and Skills (TAKS) reading test to advance to the fourth grade. The TAKS test is a standardized assessment used to measure student performance in Texas public schools. Students have three chances to pass the reading test. The TAKS reading tests are typically administered in February or March, again in April, and then in either June or July. If the students fail the reading test, schools offer accelerated instruction to help them catch up before taking the next reading test. Each teacher is assigned a maximum of ten students in an accelerated instruction group. If a student struggles and fails twice, the district must establish a grade placement committee (GPC) of the school principal, parent, and English teacher to determine what accelerated instruction the student should receive before the third test attempt. Students who fail the reading test on their third attempt are automatically retained in third grade for the following academic year.

However, exemptions to the standard procedure exist. After a student fails the third try of the reading test, the school will formally notify the student's parent or guardian regarding the retention decision. Parents or guardians can then appeal this decision by submitting a written request to the GPC. If a parent appeals the retention and the GPC reaches a unanimous decision, the student will be promoted to the fourth grade. The GPC's decision is based on multiple academic factors, such as teacher recommendations and past academic performance. However, these criteria are not standardized across school districts.

Texas's grade retention policy shares similarities with retention frameworks in Chicago, Florida, and Louisiana, though key distinctions emerge in the tested subjects, exemption criteria, remediation timing, and instructional intensity. In the 1996-97 school year, Chicago introduced a test-based retention policy requiring third, sixth, and eighth graders to meet predefined proficiency thresholds in reading and math to advance to the next grade. Students who fail to meet these standards in the spring must attend a six-week summer school program and retake the exams. Those who pass the August retest advanced to the next grade, while those who fail are retained (Jacob and Lefgren, 2009). Louisiana's policy, initiated in 1998-99, applies a comparable framework to fourth and eighth graders, offering a minimum of 50 instructional hours per subject during summer remedi-

ation. However, participation is optional and conducted in more extensive class settings, raising concerns about instructional efficacy (Eren et al., 2017, 2022). Because the summer program is implemented before the final test, these studies isolate the effect of grade retention, separate from the influence of the summer program.

Implemented in the 2002–03 school year, Florida's third-grade retention policy mandates retention for students who fail a single reading test. Retained students receive intensive remedial support, including access to a summer program, 90 minutes of uninterrupted daily reading instruction, and assignments to high-performing teachers. Unlike Texas, Chicago, and Louisiana, where remediation occurs before the final retake, Florida integrates intensive support after retention, making it difficult to disentangle the effects of remediation from those of grade repetition. Florida also exempts students with limited English proficiency who have received less than two years of English instruction from retention, a provision absent in Texas. These structural differences underscore the need for caution when comparing empirical findings across states.

# 3 Data

This paper uses administrative data from the University of Houston Education Research Center (2024) covering comprehensive records for every student enrolled in Texas public schools. Specifically, the Education Research Center (ERC) compiles pre-kindergarten through the twelfth-grade educational records from Texas Education Agency (2022), post-secondary education records within Texas from Texas Higher Education Coordinating Board (2021), post-secondary education records from other states via National Student Clearninghouse (2019), and the Texas employment earnings data from Texas Workforce Commission (2023). Each dataset incorporates a uniquely generated identifier, denoted as ID2. This ID2 serves as a unique substitute for Social Security Numbers (SSNs), enabling longitudinal tracking of a student across these diverse datasets. To test the accuracy of the longitudinal tracking across datasets using ID2, I merged the main sample from the TEA data with the community college enrollment file in 2014. I then examine the gender

match rates between these two datasets. The matching rate for gender using ID2 is 99%. The remaining 1% discrepancy may reflect manual entry errors. The introduction of each dataset and the definitions for key outcome variables used in this study are documented below. More details about the data, data linkages, and definitions or attrition for each outcome variable are available in the Appendix section A.1.

**Texas Education Agency (TEA) 1994-2022**

One of the key variables from TEA is students' performance on state standardized assessment, the TAKS, which was administered from 2003 to 2011. Students in grades 3 to 11 must take reading and math tests, with the raw scores reflecting the number of correctly answered multiple-choice questions. I can observe the TAKS reading and mathematics scores up to six years after grade three or until eighth grade for the third-graders between the 2002-03 and 2004-05 school years. I use the raw reading score, subtracting the promotion cutoff, as the running variable. The TEA files also contain a scale score, which quantifies student performance relative to the passing standards or proficiency levels, allowing direct comparisons of student performance from different test administrations (Texas Education Agency). In this study, I use the raw and scale scores as outcome variables to test the robustness of retention's effects on test scores. Additionally, test scores are standardized to have mean zero and standard deviation one, stratified by subject, grade (or year), and cohort.

High school graduation is another critical measure of educational achievement. In this analysis, I define "ever graduating from high school" as obtaining a high school diploma (excluding a GED) between 2009 and 2022. Additionally, I categorize high school graduation based on its timing relative to the expected schedule. For example, graduating on time refers to graduating from high school by the ninth year following grade three. I also measure delayed graduation outcomes as graduating one, two, three, or four more years after the on-time benchmark. Dropout is closely related to high school graduation. Since the 2005-06 school year, Texas has adopted the National Center for Education Statistics (NCES) definition of a dropout: a student in grades 7-12 who leaves school and does not re-enroll the following fall without being expelled, graduating, earning

7

a GED, transferring to non-Texas public schools, starting college, or passing away. According to this definition, graduating from high school and dropping out are not mutually exclusive.

The TEA data also detail the reasons and actions for student disciplinary incidents and indicate whether a disciplinary incident is classified as a crime or violent behavior, as listed in Table A5 and A6, respectively. This study uses the violence and crime variables to estimate the impacts of retention on severe behavior outcomes.

### Texas Higher Education Coordinating Board (THECB) 2010-2021 and National Student Clearinghouse (NSC) 2011-2019

The post-secondary outcomes are combined from THECB and NSC, focusing on college enrollment, graduation, and the types of institutions attended. I construct two enrollment measures: one capturing any enrollment between 2010 and 2021 and another assessing on-time enrollment, defined as enrollment within nine years after third grade (otherwise coded as zero). As for college graduation outcomes, these are limited to graduations from Texas colleges, as NSC data from 2011 to 2016 lack specific graduation dates and statuses. Furthermore, I explore the effects on the types of institutions where students enroll or from which they graduate, categorizing these as community colleges in Texas, public universities in Texas, or out-of-state institutions.

### Texas Workforce Commission Wages Data 2007-2023

The primary outcome variables in this study are wages from 2007 through 2023 reported by the Texas Workforce Commission (TWC), which captures most wages earned within Texas. In the dataset, missing earnings records primarily reflect individuals who are outside the labor force, unemployed, self-employed, or employed outside Texas. The US Census Bureau (2023) reports that approximately 35% of Texans over age 16 are out of the labor force. Low labor force participation is more pronounced among young adults, which likely explains a significant share of missing wages among individuals between the ages of 23 and 26 observed in this study. The unemployment rate in Texas is around 3%. Additionally, Hipple and Hammond (2016) reports that the nationwide self-employment rate among young adults is just 1.9%, underscoring its rarity. Out-of-state employment is minimal: the Internal Revenue Service (2022) indicates that only 1.8% of

Texans relocated and paid tax outside of Texas in 2021–22. These statistics reveal that missing wages mainly reflect labor force disengagement and unemployment rather than data attrition due to self-employment and employment outside of Texas. As a result, this study codes missing wage records as zero to reflect labor market disengagement.

The outcomes of interest are annual earnings at each age from 23 to 26 and the average earnings between ages 23 and 25 and 23 and 26. I also generate variables indicating having positive earnings at each age from 23 through 26 and averaging across these years. Another set of earnings outcomes measures the annual wages earned from the $8^{th}$ to the $11^{th}$ year post-grade nine and the average earnings during this period. Utilizing data on earnings outcomes post-grade nine, rather than age-specific earnings, helps to circumvent the timing discrepancies in labor market entry attributable to the extra year for repeating third grade.

This study's primary sample consists of third-graders from the 2002-03 to the 2004-05 school years who barely pass or fail the third-attempt reading test. The 2002-03 group of third-graders represents the initial cohort impacted by Texas's test-based third-grade retention policy. As of 2023, when I can observe their most recent earnings, the youngest cohort in the analysis—students in third grade during the 2004-2005 school year—would be approximately 26 years old. These three cohorts are selected to ensure a sufficient sample size for conducting fuzzy regression discontinuity analysis and to enable the observation of earnings data up to the age of 26. Details for the calendar year corresponding to the age for each cohort are available in Appendix Table A2.

Table 1 presents summary statistics for three groups of third-grade students: (i) column (1) displays the entire cohort of third-graders from 2002–03 to 2004–05 school years; (ii) column (2) represents the main study sample—students whose third-attempt reading scores fall within zero to eight points of the promotion cutoff; and (iii) column (3) shows students who fail the initial reading test.[3] The main sample disproportionately comprises low-income Hispanic students with limited English proficiency relative to both comparison groups. Additionally, students in the main sample perform worse on the first attempt of third-grade reading and math tests compared to the 2003-05

---

[3]The bandwidth, determined by the model, varies across regressions; eight points is the maximum.

cohorts. They are also more likely to drop out and less likely to graduate from high school or enroll in any college. Their public university enrollment rate is notably lower at 8% compared to 26% for the 2003-05 cohorts. Upon entering the labor market, these students earn less than their peers in the broader population.

Table 1: Summary statistics

|  | (1) 2003-05 cohorts | (2) Main sample | (3) Fail first test | (4) Diff.(2)-(1) | (5) t-stat (4) | (6) Diff. (2)-(3) | (7) t-stat(6) |
|---|---|---|---|---|---|---|---|
| **Student Characteristics** | | | | | | | |
| Age | 8.16 | 8.26 | 8.35 | 0.10 | 35.5 | -0.09 | -25.8 |
| Eligible for Free Meals | 0.34 | 0.62 | 0.57 | 0.28 | 88.5 | 0.05 | 14.5 |
| Male | 0.51 | 0.54 | 0.59 | 0.03 | 7.9 | -0.06 | -16.9 |
| Limited English Proficiency | 0.27 | 0.38 | 0.28 | 0.11 | 36.3 | 0.09 | 29.6 |
| Bilingual Program | 0.07 | 0.17 | 0.13 | 0.10 | 56.5 | 0.05 | 19.0 |
| Migrant | 0.02 | 0.04 | 0.03 | 0.02 | 18.5 | 0.00 | 1.7 |
| Special Education | 0.12 | 0.07 | 0.46 | -0.06 | -26.4 | -0.39 | -118.6 |
| Hispanic | 0.45 | 0.60 | 0.50 | 0.15 | 44.7 | 0.10 | 28.1 |
| Black | 0.14 | 0.26 | 0.21 | 0.12 | 50.8 | 0.05 | 16.4 |
| White | 0.37 | 0.12 | 0.27 | -0.25 | -79.1 | -0.14 | -47.6 |
| First Attempt Third-Grade Reading Scores | 5.18 | -6.64 | -13.76 | -11.82 | -175.6 | 7.12 | 123.6 |
| First Attempt Third-Grade Math Scores | 7.02 | -3.20 | -1.36 | -10.22 | -214.2 | -1.84 | -30.0 |
| **Educational and Behavioral Outcomes** | | | | | | | |
| Absenteeism | 64.43 | 78.63 | 80.09 | 14.20 | 39.7 | -1.46 | -3.3 |
| Dropout | 0.07 | 0.16 | 0.13 | 0.10 | 57.9 | 0.04 | 14.9 |
| High School Graduation | 0.74 | 0.61 | 0.64 | -0.12 | -43.1 | -0.03 | -9.0 |
| Any College Enrollment | 0.57 | 0.37 | 0.34 | -0.19 | -59.7 | 0.03 | 8.5 |
| Public University Enrollment | 0.26 | 0.08 | 0.07 | -0.18 | -63.8 | 0.00 | 0.8 |
| Community College Enrollment | 0.48 | 0.32 | 0.30 | -0.17 | -50.6 | 0.02 | 5.4 |
| **Labor Market Outcomes** | | | | | | | |
| Av.Earnings btw Ages 23 to 25 | 19273 | 15368 | 13981 | -3905 | -25.0 | 1387 | 10.9 |

Notes: This table provides summary statistics for three groups of samples: all third-graders from 2002-2003 to 2004-2005 school years, the main study sample—students whose third-attempt reading scores fall within zero to eight points of the promotion cutoff, and those who fail the initial third-grade reading test.

These demographic differences between the primary study sample and the broader cohorts raise concerns about the external validity of the findings, specifically, whether the adverse effects of grade retention observed here extend to other student populations. However, two factors mitigate this concern. First, demographic composition of the main sample closely reflects statewide trends in recent years: 53% of Texas third-graders are Hispanic, and 63% are from low-income households, aligning with the characteristics of students near the retention threshold. Second, subgroup analyses in Section 5.4 indicate that retention consistently reduces academic achievement and long-term earnings across socioeconomic and linguistic groups. While further research is needed to assess the policy's effects under alternative retention criteria, these findings suggest that

the negative consequences of retention are not confined to the specific demographic group.

# 4    Empirical Strategy

The existing exemption in the retention process allows students who fail their third-attempt reading test to advance to the next grade upon successful parental appeal and approval by the grade placement committee members. This introduces a selection bias, complicating the estimation of retention's causal impacts, as exemption decisions are likely influenced by parental involvement and socioeconomic status, which vary across the retained and promoted students. For example, Appendix Table A7, which separates the primary sample by test performance and retention status, shows that among students who fail the third-attempt reading test, those who are retained tend to be younger. This pattern aligns with findings from Schwerdt et al. (2017) on Florida's third-grade retention policy. Additionally, retained students are more likely to be Hispanic and exhibit weaker reading proficiency than their peers promoted through exemptions. This contrasts with Schwerdt et al. (2017), where limited English proficient students were more likely to be exempted from retention. Among students who pass the third-attempt reading test, those retained also tend to be younger, less likely to be White, and demonstrate lower reading proficiency.

To circumvent the selection issue, this study exploits the variation generated at the promotion cutoff of the third-attempt reading test as a tool for causal identification. Texas's test-based retention policy mandates that third graders surpass a specified reading score cutoff to progress to the fourth grade. This cutoff, determined annually by educational experts based on test difficulty and undisclosed to students before assessment, creates a quasi-experimental environment. Students near the cutoff are comparable in their characteristics except for their probability of being retained in third grade. Despite the potential for exemptions, the likelihood of retention increases discontinuously for students just below the cutoff. This provides a clear division for employing a fuzzy regression discontinuity design, leveraging the quasi-random variation at the cutoff to estimate third-grade retention's short-term and long-term causal effects.

11

For identification, I employ the local linear regression within a bandwidth defined around the promotion cutoff, adhering to the recommended practices from Gelman and Imbens (2019) and Imbens and Lemieux (2008). The model controls for the running variable (reading scores minus cutoff) and accommodates different slopes on either cutoff side. A triangular kernel is employed, which places the highest weight on students close to the promotion cutoff. The optimal bandwidth is selected by optimizing the mean squared error based on the method in Calonico et al. (2017). Specifically, the analysis is implemented through a two-stage instrumental variables framework, detailed as follows:

$$Retention_i = \theta_0 + \theta_1 I\{score_i < C\} + \theta_2(score_i - C) + \theta_3 I\{score_i < C\} * (score_i - C) + \gamma X_i + \varepsilon_i \quad (1)$$

$$Y_i = \beta_0 + \beta_1 \hat{Retention}_i + \beta_2(score_i - C) + \beta_3 I\{score_i < C\} * (score_i - C) + \eta X_i + \mu_i \quad (2)$$

where $Y_i$ denotes the outcomes of interest for student $i$, including educational achievement, behavior incidents, and earnings outcomes; $Retention_i$ is an indicator that takes the value one if a student was retained in third grade; $score_i$ is the reading raw score, representing the number of questions answered correctly; $C$ is the promotion cutoff of the reading test score; $I\{score_i < C\}$ is an indicator, indicating scoring below the promotion cutoff, which is also the instrumental variable for third-grade retention; $X_i$ is a vector of control variables, including demographic characteristics like gender, eligibility for free meals, race, and cohort fixed effects. This study uses the robust standard error, but the results align closely with those clustering the standard error by school. The parameter of interest, $\beta_1$, measures the effect of third-grade retention on student outcomes. The regression discontinuity estimate of $\beta_1$ can be interpreted causally if the expected potential outcomes are smooth across the cutoff. While this assumption cannot be tested directly—since we only observe the realized potential outcome—it can be indirectly assessed. Evidence that baseline characteristics and the distribution of the running variable are continuous at the cutoff supports the validity of the smoothness assumption for potential outcomes.

Figure 1 visually illustrates the balance test by displaying baseline characteristics, such as eli-

gibility for free meals, gender, race, participation in special education, limited English proficiency, and math scores, as a function of third-attempt reading scores near the promotion cutoff. This graphical evidence confirms that these baseline characteristics are smooth across the cutoff, suggesting no systematic differences between students on either side of the threshold.

Complementing the graphical analysis, Table 2 offers statistical evidence, estimating the impact of scoring just below the cutoff on the same baseline characteristics before taking the third-attempt reading test. The results indicate that scoring below the cutoff does not significantly affect math scores, eligibility for free meals, racial composition, special education participation, or English proficiency level. Nevertheless, there is a marginally significant effect on gender at the 10% level. To ensure robustness, the primary analyses include gender as a control variable to mitigate any potential bias from this slight imbalance.

Further supporting the validity of the continuity assumption, Figure 2 shows that the distribution of the third attempt reading scores is continuous at the cutoff, with the P-value of 0.4382 from the continuity test in Cattaneo et al. (2020). Moreover, failing the third-attempt reading test significantly increases the likelihood of third-grade retention. As illustrated in Figure 3, students who score below the promotion cutoff have a 35.5 percentage point higher probability of repeating third grade, a strong relationship underscored by the F-statistic with a value larger than 100.

Table 2: Balance Test

| Outcome Variable | (1)<br>Free meals | (2)<br>Male | (3)<br>White | (4)<br>Special Education | (5)<br>Math Scores | (6)<br>Limited English Proficiency |
|---|---|---|---|---|---|---|
| Below Cutoff | 0.029 | 0.049 | -0.016 | 0.003 | -0.007 | 0.008 |
| | (0.018) | (0.026) | (0.012) | (0.010) | (0.032) | (0.018) |
| Above cutoff mean | 0.615 | 0.523 | 0.135 | 0.062 | 0.263 | 0.352 |
| Effect size | 4.72% | 9.37% | -11.85% | 4.84% | -2.66% | 2.27% |
| Observations | 14599 | 11646 | 17404 | 14536 | 17253 | 17404 |

Notes: This table shows estimates of the effects of scoring below the reading score cutoff on characteristics observed before retention. Math scores are standardized to have mean zero and standard deviation one by cohort. These estimates are obtained by replacing "Retention" on the left-hand side of equation (1) with students' characteristics. Standard errors are in parentheses. Variation in bandwidths across outcomes explains the differences in sample sizes. Standard errors are in parentheses.

Figure 1: Balance Test
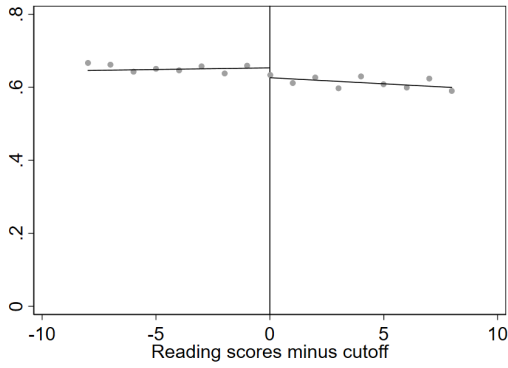


(a) Eligible for Free Meals
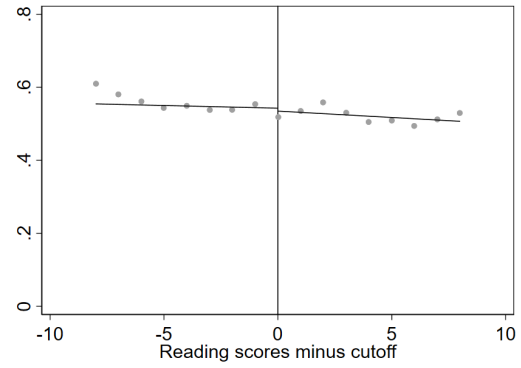
(b) Male
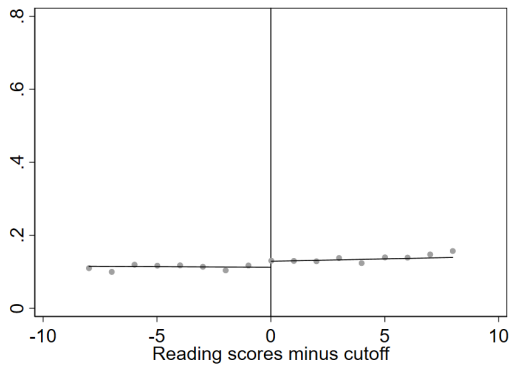
(c) White
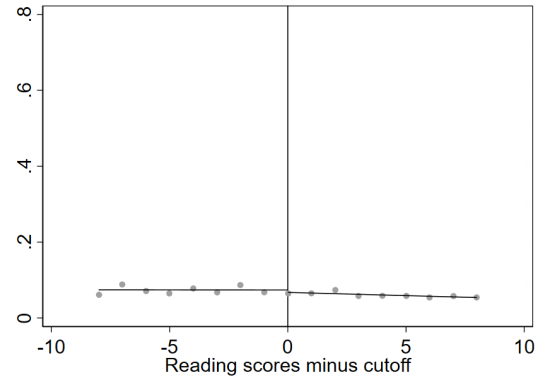
(d) Special Education Participation
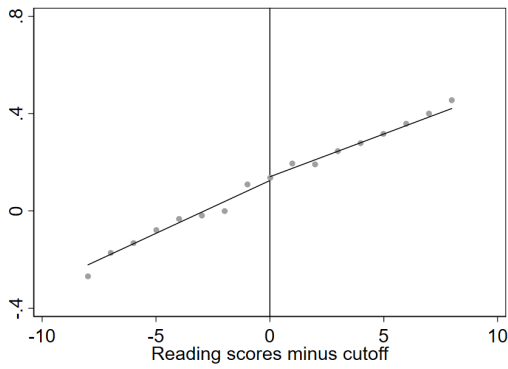
(e) Math Scores
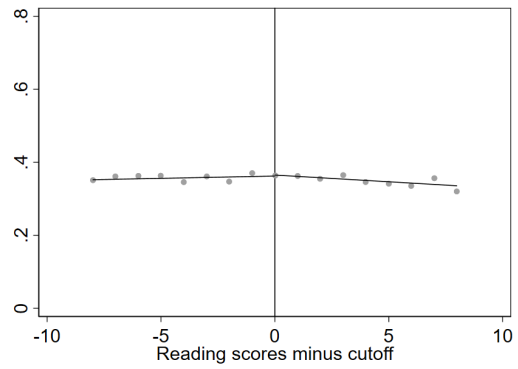
(f) Limited English Proficiency

Notes: These figures display baseline characteristics, measured before retention, as a function of third-attempt reading scores near the promotion cutoff. Math scores are standardized to have mean zero and standard deviation one by cohort.

Figure 2: The Distribution of the Third-Attempt Reading Test Scores



Continuity test at the cutoff: p-value=0.4382
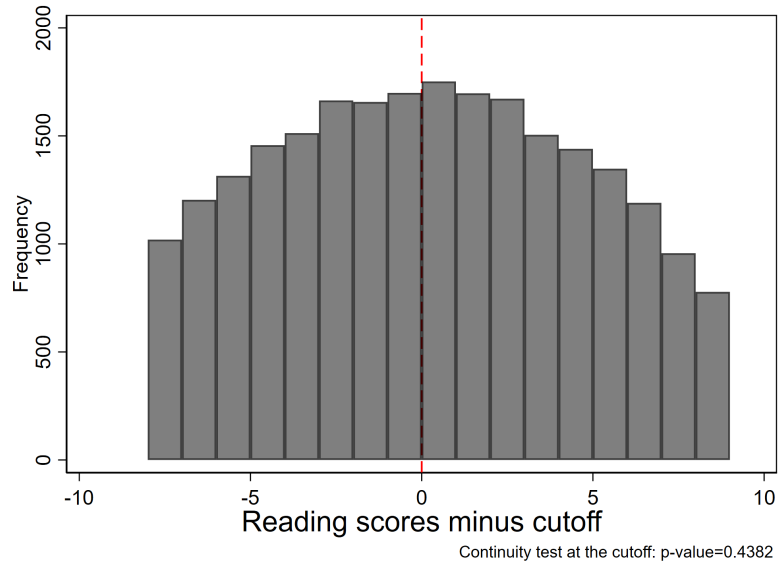
Notes: This figure displays the distribution of the third-attempt reading test scores within a bandwidth of zero to eight points to the cutoff. This is the maximum bandwidth from all regressions in the main results. The P-values of the continuity test come from the rddensity package, a method introduced in Cattaneo et al. (2020).

Figure 3: Failing Third-Attempt Reading Test Increases the Probability of Third-Grade Retention



First stage estimate: 0.355
Standard error: (0.013)
F-statistic: 1,355
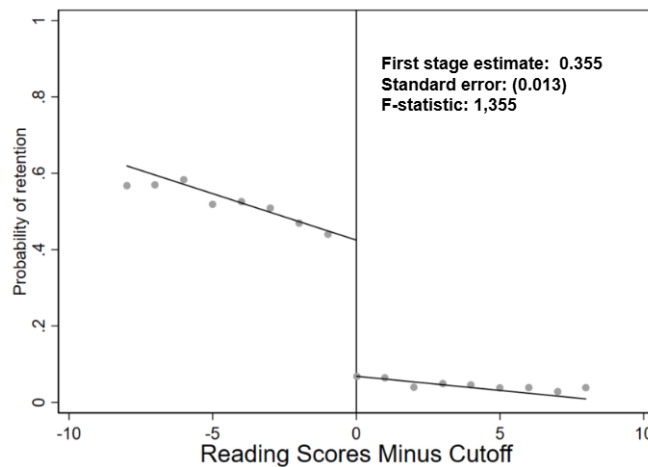
Notes: This figure shows the likelihood of repeating third grade for students who narrowly fail or pass the promotion cutoff on the third-attempt reading test. The bandwidth is set at eight points from the cutoff, the maximum bandwidth obtained from the main regressions.

# 5 Results

## 5.1 Effects on Earnings Outcomes

Failing the third-attempt reading test reduces young adult earnings. Figure 4 visually illustrates this decline, showing that students who barely fail the test earn $1,682 (11.3%) less at age 23 than those who barely pass. The gap narrows slightly in later years, with a reduction of $1,243 at age 24, $1,399 at age 25, and $1,338 at age 26. The modest attenuation in later years coincides with COVID-19 labor market disruptions, suggesting that the earnings gap may be even larger under stable economic conditions.[4] Over the four years from ages 23 to 26, students who barely fail the test experience an average decline in earnings of $1,508 (9.06%) compared to their counterparts. Appendix Table A8 displays these estimates, supporting the declines shown in Figure 4.

Repeating third grade harms young adult earnings. Table 3 presents estimated effects of third-grade retention on earnings outcomes using a fuzzy regression discontinuity design, which accounts for imperfect compliance with the retention policy. The results reveal significant income losses: by age 25, retained students earn $3,278 less than their promoted peers. By age 26, the gap widens to $3,477, a 19% reduction relative to the mean of those who barely score above the cutoff. Averaging across ages 23 to 26, retained students experience a $3,518 reduction in average earnings relative to those who advance to fourth grade. This estimate is statistically significant at the 5% confidence level, representing a 21% decline compared to the mean of students scoring above the cutoff. Appendix Table A9 reinforces the robustness of the results by presenting the effects of retention on earnings outcomes across cohorts.

The 19% earnings loss at age 26 is larger than the 9% decrease at age 28 reported in Meulen (2023), a working paper examining the effect of repeating twelfth grade due to failing high school exit exams in the Netherlands. This gap suggests that repeating third grade is more detrimental in the long run than repeating twelfth grade. This is likely due to prolonged exposure to the stigma and school disengagement associated with early grade retention, as evidenced by the increased school

---

[4]Appendix Table A2 shows that the 2002-2003 and 2003-2004 cohorts entered key earning years during the pandemic.

# Figure 4: Failing the Third Attempt Reading Test Reduces Earnings
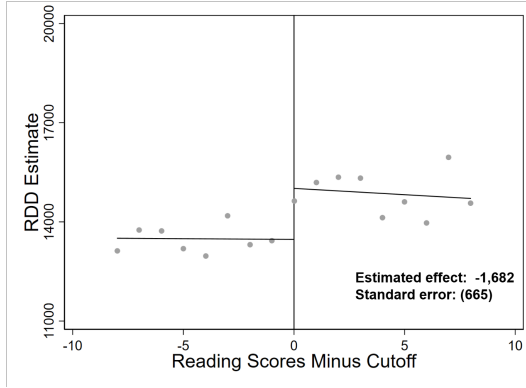


(a) Earnings at Age 23
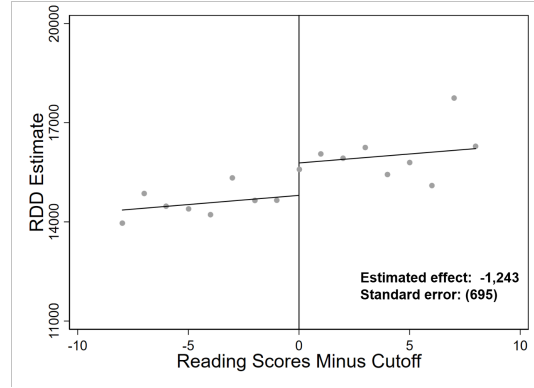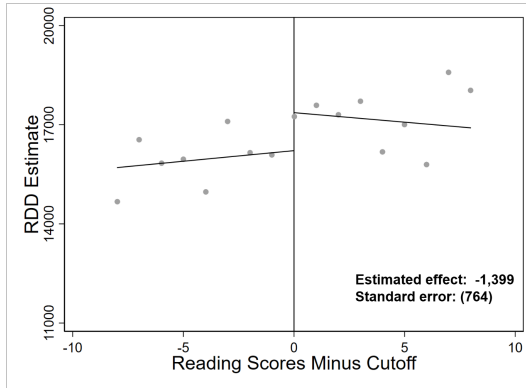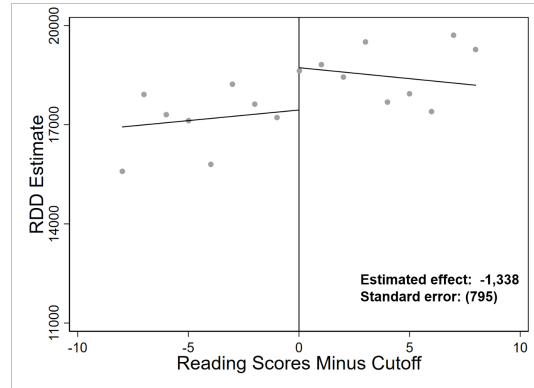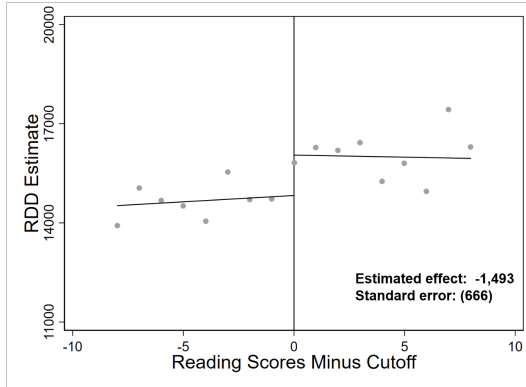
Estimated effect: -1,682
Standard error: (665)

(b) Earnings at Age 24

Estimated effect: -1,243
Standard error: (695)

(c) Earnings at Age 25

Estimated effect: -1,399
Standard error: (764)

(d) Earnings at Age 26

Estimated effect: -1,338
Standard error: (795)

(e) Average Earnings Between Ages 23 and 25

Estimated effect: -1,493
Standard error: (666)

(f) Average Earnings Between Ages 23 and 26

Estimated effect: -1,508
Standard error: (675)
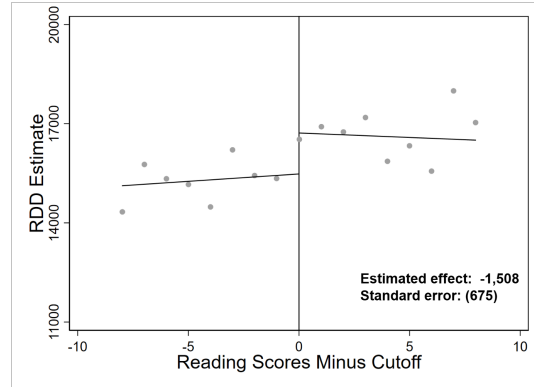
Notes: This figure shows reduced-form estimates of the effects of failing the third-attempt reading test on annual earnings at ages 23 to 26 and average earnings across ages 23–25 and 23–26. These estimates are derived by replacing "Retention" with earnings outcomes in equation (1), identified by $\theta_1$. The dots represent cell means, and the lines are fitted using a first-order polynomial regression with triangular weights. The bandwidth is eight points to the cutoff. Standard errors are in parentheses.

17

Table 3: Effect of Grade Retention on Earnings between Ages 23 and 26

| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Annual Earnings at Each Age from 23 to 26 | | | | Average Earnings | |
| | Age23 | Age24 | Age25 | Age26 | Ages23-25 | Ages23-26 |
|---|---|---|---|---|---|---|
| **Retention** | -4,324 | -2,921 | -3,278 | -3,477 | -3,516 | -3,518 |
| | (1535) | (1583) | (1753) | (1843) | (1468) | (1493) |
| Above cutoff mean | 14,864 | 15,948 | 17,221 | 18,557 | 16,012 | 16,645 |
| Effect size | -29.09% | -18.32% | -19.03% | -18.74% | -21.96% | -21.14% |
| Observations | 19784 | 19784 | 19782 | 19731 | 19784 | 19784 |

Notes: This table displays fuzzy RDD estimates of the effects of third-grade retention on annual earnings at each age from 23 to 26, as well as on average earnings between ages 23–25 and 23–26. Missing earnings are coded as zero. The "Above cutoff mean" indicates the average earnings of students whose reading scores are zero to eight points above the cutoff. The bandwidth is determined using the method outlined in Calonico et al. (2017) and may vary by outcome. Standard errors are in parentheses.

absences and criminal activity observed in this study. In contrast, such effects are not captured in Meulen (2023).

I explore whether repeating third grade affects labor market engagement and the likelihood of securing higher-paying jobs to better understand how retention reduces earnings. I find that repeating third grade is associated with a reduced likelihood of earning a positive income in young adulthood. The results are shown in Table 4, which examines the impact of third-grade retention on the likelihood of earning any income at each age from 23 to 26 and averaging across these periods. At age 23, retention significantly reduces the possibility of earning positive wages by 11 percentage points (16%). The estimate is statistically significant at a 5% level. This estimated effect weakens over time, with declines of 1.9%, 10%, and 4% at ages 24, 25, and 26, respectively. Moreover, these estimates are not statistically significant at the conventional levels. When averaging across ages 23–26, retention lowers the probability of earning positive wages by 5 percentage points (6.54%), though the estimate is not statistically significant.

Notably, the TWC excludes earnings from self-employment (without an employee) and out-of-state employment. This raises a concern that the negative effect of retention on having positive earnings may reflect attrition due to self-employment and out-of-state employment rather than genuine labor force disengagement. To address this concern, I reference external labor force statistics,

which suggest that attrition is unlikely to drive the results. The US Census Bureau (2023) reports that 35% of Texans over age 16 are out of the labor force, and 3% are unemployed, closely aligning with the 34% of individuals without positive earnings, as indicated by the above cutoff mean at age 26 in Table 4. Additionally, Hipple and Hammond (2016) shows that the national self-employment ratio remains rare among younger individuals: only 1.9% of young adults are self-employed. Out-of-state employment is minimal: the Internal Revenue Service (2022) indicates that only 1.8% of Texans relocated and paid tax outside of Texas in 2021-22. These statistics further suggest that the observed declines in positive earnings primarily reflect labor market disengagement and unemployment, rather than data attrition due to self-employment and employment outside Texas.

Table 4: Effect of Grade Retention on Having Positive Earnings by Age

|  | (1) Age 23 | (2) Age 24 | (3) Age 25 | (4) Age 26 | (5) Average Across Ages 23-26 |
|---|---|---|---|---|---|
| Having positive earnings | -0.112 | -0.013 | -0.069 | -0.028 | -0.051 |
|  | (0.042) | (0.043) | (0.042) | (0.042) | (0.038) |
| Above cutoff mean | 0.68 | 0.67 | 0.66 | 0.66 | 0.78 |
| Effect size | -16.47% | -1.94% | -10.45% | -4.24% | -6.54% |
| Observations | 19909 | 19909 | 19909 | 19909 | 19909 |

Notes: This table displays fuzzy RDD estimates of the effects of third-grade retention on having positive earnings at each age from 23 to 26 and averaging across this period. Missing earnings are coded as zero, typically indicating labor force disengagement, unemployment, self-employment, or out-of-state employment. Standard errors are in parentheses.

Furthermore, third-grade retention reduces the likelihood of attaining high-paying positions. Figure 5 shows estimated effects of third-grade retention on the probability of earning above each percentile, starting from the bottom of the distribution. Retained students are less likely to surpass each earnings threshold than their promoted peers. The gaps are more pronounced at higher percentiles, suggesting that retention reduces their likelihood of securing higher-paying jobs.

Figure 5: Effect of Grade Retention on the Average Earnings Between Ages 23-25
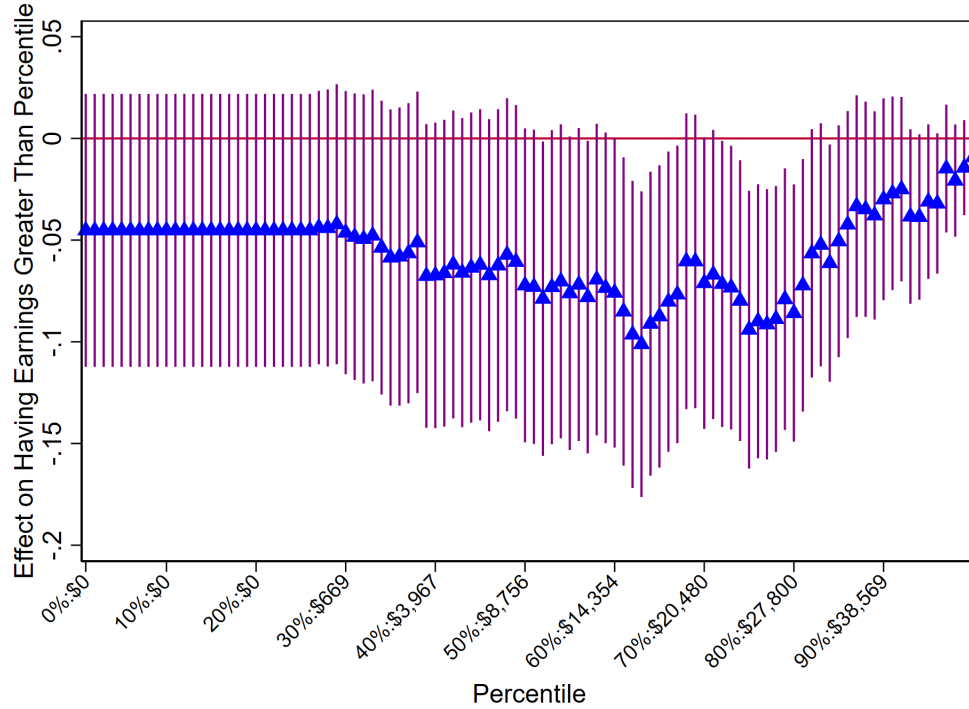


Notes: This figure displays estimates of the impacts of third-grade retention on the likelihood of earning more than each percentile from 0 to the 99$^{th}$ percentile of the average earnings between ages 23 and 25. The blue triangles show estimates of the effects of third-grade retention on the likelihood of earning more than each percentile. The purple lines represent the 95% confidence interval.

## 5.2 Effects on Educational Attainment

This section examines the long-term effects of third-grade retention on educational attainment. Table 5 presents estimated effects on college enrollment outcomes, disaggregated by institution type. I examine two primary outcomes: ever enrolling in college, defined as enrollment at any time between 2010 and 2021; and on-time enrollment, defined as enrolling within nine years of third grade (coded as 1 if on time, 0 otherwise). Column (1) shows that third-grade retention increases the likelihood of ever enrolling in college by 1.6 percentage points—a 3.95% increase relative to the 40.5% baseline among students just above the promotion cutoff. However, this estimate is not statistically significant at conventional levels.

Disaggregating by institution type reveals that the modest overall increase in enrollment primar-

Table 5: Effect of Retention on College Enrollment and Timing

|  | (1)<br>Ever enrolling in a college | (2)<br>On-time enrollment |
|---|---|---|
| **Any college or university** | 0.016 | -0.030 |
|  | (0.041) | (0.027) |
| Above cutoff mean | 0.405 | 0.145 |
| Effect size | 3.95% | -20.69% |
| Observations | 19909 | 19909 |
| **Community college in Texas** | 0.015 | -0.019 |
|  | (0.041) | (0.025) |
| Above cutoff mean | 0.348 | 0.121 |
| Effect size | 4.31% | -15.70% |
| Observations | 19909 | 19909 |
| **Public university in Texas** | -0.014 | -0.008 |
|  | (0.022) | (0.009) |
| Above cutoff mean | 0.093 | 0.019 |
| Effect size | -15.05% | -42.11% |
| Observations | 22070 | 22070 |
| **College outside Texas** | 0.009 | 0.003 |
|  | (0.009) | (0.003) |
| Above cutoff mean | 0.012 | 0.001 |
| Effect size | 75% | 300% |
| Observations | 22070 | 19909 |

Notes: This table presents fuzzy RDD estimates of the effects of third-grade retention on college enrollment outcomes. "Ever enrolling" indicates enrollment in any college between 2010 and 2021. "On-time enrollment" indicates college entry within nine years following third grade (coded as 1 if enrolled on time, 0 otherwise). Standard errors are in parentheses.

ily stems from community colleges. Retention is associated with a 1.5 percentage point (4.31%) increase in Texas community college enrollment relative to the above cutoff mean of 34.8% and a 1.4 percentage point (15.05%) decline in Texas public university enrollment compared to the above cutoff mean of 9.3%. Neither estimate is statistically significant at the 5% level. It is worth noting that students who barely pass or fail the third-attempt reading test already exhibit lower baseline academic performance, making them less likely to enroll in public universities than the general student population. This is reflected in their 8% enrollment rate in public universities versus 32%

in community colleges, as shown in Table 1. The results of Column (1) in Table 5 suggest that third-grade retention further widens this gap by shifting college enrollment patterns away from public universities toward community colleges. Additionally, enrollment in out-of-state colleges increases by 0.9 percentage points—a 75% increase relative to a low baseline rate of 1.2%—though this estimate is not statistically significant.

Results in Column (2) indicate that third-grade retention delays college entry, although these estimates are not statistically significant. Retention corresponds to a 3 percentage point (20.69%) decline in on-time enrollment across all colleges, a 1.9 percentage point (15.7%) decrease for Texas community colleges, and a 0.8 percentage point (42.11%) reduction for Texas public universities. In contrast, on-time enrollment outside Texas increases by 0.3 percentage points, a 300% rise relative to a minimal baseline rate of 0.1%, though the estimate is not statistically significant.

Table 6: Effect of Retention on College Graduation Outcomes

|  | (1) Any College in Texas | (2) Community College | (3) Public University |
|---|---|---|---|
| **Panel A: Ever graduated** | | | |
| Retention | 0.017 | 0.010 | -0.003 |
|  | (0.025) | (0.022) | (0.015) |
| Above cutoff mean | 0.106 | 0.076 | 0.046 |
| Effect size | 16.04% | 13.16% | -6.52% |
| Observations | 19909 | 19909 | 19909 |
| **Panel B: On-time graduation** | | | |
| Retention | 0.003 | 0.008 | -0.009 |
|  | (0.020) | (0.014) | (0.011) |
| Above cutoff mean | 0.067 | 0.028 | 0.029 |
| Effect size | 4.48% | 28.57% | -31.03% |
| Observations | 19909 | 19909 | 19909 |

Notes: This table presents fuzzy RDD estimates of the effects of third-grade retention on college graduation outcomes in Texas. "Ever graduated" indicates degree completion from any Texas college between 2010 and 2021. "On-time graduation" refers to completion by age 22 for public universities and any Texas college, and by age 20 for community colleges. Due to missing graduation records (2011-2016) in the National Student Clearinghouse data, out-of-state graduation results are not reported.

Panel A of Table 6 extends the analysis to college completion, indicating that third-grade retention increases the likelihood of community college graduation between 2010 and 2021 by 1

percentage point (13.16%), while decreasing public university graduation by 0.3 percentage points (6.52%). Overall, retained students are 1.7 percentage points (16.04%) more likely to graduate from any Texas college, though none of these estimates are statistically significant. Similarly, Panel B indicates that retention increases the likelihood of on-time community college graduation (by age 20) but decreases on-time public university graduation (by age 22), with neither estimate statistically significant at the 10% level.

Extending the analyses to earlier educational milestones, I find that third-grade retention significantly reduces the likelihood of graduating from high school and is associated with a higher likelihood of dropping out. The results are shown in Table 7. Notably, Texas adopted the NCES definition for dropout, classifying students in grades 7–12 as dropouts if they leave public school without graduating, earning a GED, pursuing further education, enrolling in college, or passing away. This study defines a dropout as a student who left Texas public schools between 2004 and 2018 under the NCES definition. The results of Column (1) suggest that retention leads to a 2.1 percentage point increase in the likelihood of dropping out, a 14% rise relative to the baseline of 15% for students just above the promotion cutoff. However, this estimate is not statistically significant.

Table 7: Effect of Retention on Dropout and High School Graduation Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | | | High School Graduation | | | | |
| | Ever dropout | Ever graduated | On time | 1+ years | 2+ years | 3+ years | 4+ years |
| Retention | 0.021 | -0.091 | -0.366 | 0.277 | 0.005 | -0.013 | -0.010 |
| | (0.033) | (0.043) | (0.037) | (0.037) | (0.014) | (0.006) | (0.004) |
| Above cutoff mean | 0.150 | 0.642 | 0.457 | 0.185 | 0.020 | 0.005 | 0.003 |
| Effect size | 14.00% | -14.17% | -80.09% | 149.73% | 25.00% | -260.00% | -333.33% |
| Observations | 19909 | 19909 | 19909 | 19909 | 19909 | 19909 | 19909 |

Notes: This table presents fuzzy RDD estimates of the effects of third-grade retention on dropout, high school graduation (ever graduated), and on-time high school graduation outcomes. A dropout is defined as a student in grades 7 through 12 who does not return to public school the following fall and is neither expelled, a graduate, a GED recipient, enrolled in a non-public school, attending college, nor deceased. This study classifies any student who exited the Texas public school system between 2004 and 2018 under this definition as having ever dropped out. The "ever graduated" outcome captures whether a student obtained a high school diploma between 2009 and 2022. Notably, this definition excludes students who only earned a General Educational Development (GED) certificate. On-time graduation means earning a high school diploma within nine years of third grade. The outcomes in columns (4) to (7) indicate whether a student graduated one to four more years later than the on-time benchmark.

Column (2) further shows that third-grade retention significantly reduces the probability of ever graduating from high school, which is defined as ever obtaining a high school diploma between 2009 and 2022. Retained students are 9.1 percentage points less likely to graduate from high school, representing a 14.17% decline compared to the 64.2% graduation rate among students just above the cutoff. This estimate is statistically significant at the 5% level.

The gap in the effects on dropout and high school graduation might be due to two institutional factors. First, Texas's compulsory schooling law requires students to remain in school until they graduate or reach age 19, which reduces dropout rates compared to other states with a lower legal dropout age. Second, students who leave the public school system for alternative education pathways, such as earning a GED or attending private, home, or out-of-state schools, are neither classified as dropouts nor high school graduates in the dataset, further deepening the discrepancy in the impacts observed on dropout and high school graduation.

Column (3) examines the effect of third-grade retention on on-time high school graduation, defined as graduating within nine years of third grade (coded as 1 if graduated on time and 0 otherwise). The results indicate that third-grade retention reduces on-time graduation by 36.6 percentage points, an 80% decline relative to the 45.7% baseline for students just above the cutoff. This estimate is statistically significant at the 5% level. Column (4) consistently shows that third-grade retention increases the likelihood of graduating from high school one or more years later than the on-time schedule by 27.7 percentage points — a 150% increase relative to the mean for students just above the cutoff. However, Columns (5) through (7) demonstrate that third-grade retention does not raise the likelihood of graduating more than one year late. This suggests that retained students catch up and graduate just one year behind their expected timeline.

## 5.3 Effects on Short-Term Outcomes

In this section, I provide new causal evidence on how third-grade retention impacts test scores and behavioral outcomes, whereas prior research has found mixed results. Estimating the causal effects of third-grade retention on test scores presents challenges due to the different tests adminis-

tered to retained students and their promoted peers post-third grade. To address this, I estimate the impacts of third-grade retention on the reading and math scores administered in the same academic year and grade level following third grade.

Figure 6 displays estimated impacts of third-grade retention on the reading scale scores from the first to the fifth year after third grade.[5] Considering the difficulty level of the tests, these scale scores allow direct comparisons of student performance across different test administrations. I further standardize the scale scores to have mean zero and standard deviation one by cohort and test year. The results of Figure 6 indicate that while third-grade retention initially boosts reading scores, this advantage diminishes by the fifth year. A similar trend is observed in the standardized mathematics scores, with initial improvements fading in the subsequent years.

The fadeout effect is also observed by comparing reading and math scores when retained students and their promoted peers reach the same grade level. [6] Although retention appears to enhance reading and math scores in the fourth grade, these gains are not sustained, dissipating by the eighth grade. These results confirm that the initial increases in test scores are transient. Nevertheless, these temporal improvements are associated with a lower likelihood of repeating a grade post-third grade, as demonstrated in Appendix Table A13. These temporary gains, combined with a lower probability of retention beyond third grade, closely align with the findings of Schwerdt et al. (2017).

In contrast, third-grade retention increases behavioral issues. Table 8 displays estimated impacts of third-grade retention on total, average, and annual incidents of absenteeism, violence, and juvenile crime from one through nine years post-third grade. Panel A shows that retention in third grade increases total and average absenteeism by 6.9 and 0.767 days, respectively. However, neither of the estimates is statistically significant at the 10% confidence level. Annual analysis reveals that retention increases absenteeism in the first and second years after third grade by 1.249 and 1.376 days, respectively. These estimates are statistically significant at the 5% level, representing increases of 23% and 25% relative to the mean of students who barely pass the cutoff.

---

[5]The effects of retention on raw test scores by year are shown in Appendix Figure A1.
[6]The effect of retention on raw scores by grades is available in Appendix Figure A2.

Figure 6: Effects of Third-Grade Retention on Test Scores
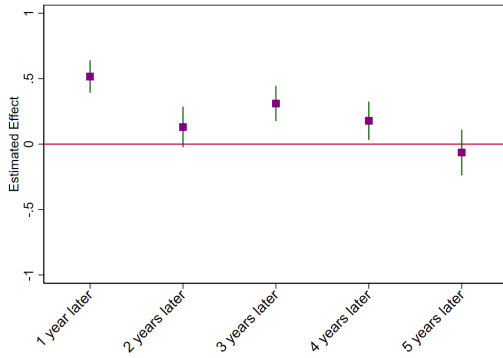


(a) Reading Scale Scores by Years
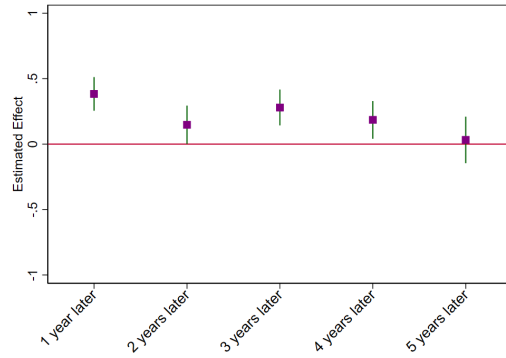
(b) Math Scale Scores by Years
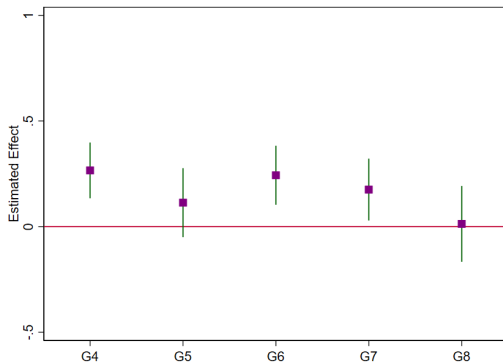
(c) Reading Scale Scores by Grade
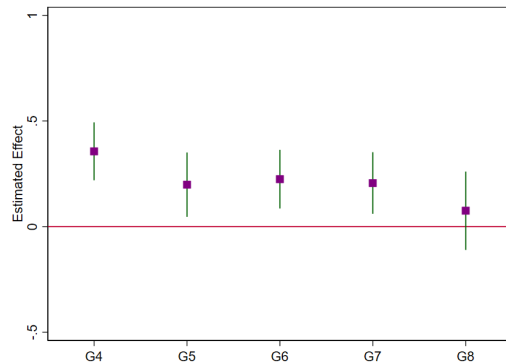
(d) Math Scale Scores by Grade

Notes: These figures display estimates of the effects of third-grade retention on reading and math scale scores by years and grades. The scale score quantifies a student's performance relative to the passing standards or proficiency levels, allowing direct comparisons of student performance between specific sets of test questions from different test administrations. The scale score is further standardized to have mean zero and standard deviation one by subject-year/grade-cohort.

Table 8: Effects of Third-Grade Retention on Absenteeism, Violence, and Crime

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 to 9 Years Post-Grade 3 | | Annual Outcomes: Years Post-Grade 3 | | | | | | | | |
| | Total | Average | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 |
| **Panel A: Absenteeism** | | | | | | | | | | | |
| | 6.907 | 0.767 | 1.249 | 1.376 | 0.458 | 0.803 | 1.678 | 1.577 | 1.757 | -1.074 | -1.030 |
| | (5.909) | (0.657) | (0.561) | (0.603) | (0.714) | (0.892) | (1.043) | (1.298) | (1.345) | (1.298) | (1.263) |
| Above cutoff mean | 76.081 | 8.453 | 5.448 | 5.535 | 6.426 | 7.506 | 8.820 | 9.983 | 10.900 | 11.142 | 10.320 |
| Effect Size | 9.08% | 9.07% | 22.93% | 24.86% | 7.13% | 10.70% | 19.02% | 15.79% | 16.12% | -9.64% | -9.98% |
| Obs. | 17,404 | 17,404 | 17,404 | 17,404 | 17,404 | 17,404 | 17,404 | 17,404 | 17,404 | 19,989 | 19,989 |
| **Panel B: Violent Behaviors** | | | | | | | | | | | |
| Violence | 0.598 | 0.066 | 0.043 | 0.037 | 0.060 | 0.159 | 0.138 | 0.044 | 0.033 | 0.034 | 0.026 |
| | (0.184) | (0.020) | (0.021) | (0.028) | (0.044) | (0.052) | (0.051) | (0.052) | (0.050) | (0.042) | (0.027) |
| Above cutoff mean | 0.907 | 0.085 | 0.028 | 0.056 | 0.085 | 0.116 | 0.127 | 0.129 | 0.114 | 0.077 | 0.030 |
| Effect size | 65.93% | 77.65% | 153.57% | 66.07% | 70.59% | 137.07% | 108.66% | 34.11% | 28.95% | 44.16% | 86.67% |
| Obs. | 17404 | 17404 | 19909 | 19909 | 19909 | 19909 | 19909 | 19909 | 19909 | 19909 | 19909 |
| **Panel C: Juvenile Crime** | | | | | | | | | | | |
| Crime | 0.237 | 0.026 | 0.003 | 0.013 | 0.013 | 0.062 | 0.062 | 0.002 | 0.036 | 0.011 | 0.006 |
| | (0.113) | (0.013) | (0.007) | (0.013) | (0.025) | (0.032) | (0.036) | (0.037) | (0.041) | (0.035) | (0.021) |
| Above cutoff mean | 0.375 | 0.038 | 0.003 | 0.010 | 0.022 | 0.041 | 0.061 | 0.064 | 0.068 | 0.049 | 0.020 |
| Effect size | 63.20% | 68.42% | 100.00% | 130.00% | 59.09% | 151.22% | 101.64% | 3.13% | 52.94% | 22.45% | 30.00% |
| Obs. | 17404 | 17404 | 19909 | 19909 | 19909 | 19909 | 19909 | 19909 | 19909 | 19909 | 19909 |

Note: This table displays fuzzy RDD estimates of the effects of third-grade retention on aggregated, average, and annual numbers of absenteeism, violence, and crime from one to nine years following grade three. The bandwidths vary across outcomes, explaining the differences in sample observations.

The results in Panel B show that repeating third grade increases violent behaviors listed in Table A5. Specifically, third-grade retention increases the total and average number of violent incidents from the first through the ninth year following grade three by 0.598 and 0.066, respectively. Both estimates are statistically significant at the 1% confidence level, representing increases of 66% and 78% relative to the mean among students slightly above the cutoff. Moreover, Column (3) indicates that third-grade retention increases violent incidents in the first year after grade three by 0.043. This estimate is statistically significant at the 5% level, representing a 154% increase relative to the mean of 0.028 among their initial peers slightly above the promotion cutoff. This adverse effect is also observed in the fourth and fifth years following grade three, with increases of 0.159 (137%) and 0.138 (109%), respectively. Both estimates are statistically significant at the 1% confidence level.

Furthermore, third-grade retention increases severe criminal activities, such as those listed in Table A6. Results are shown in Panel C, revealing that retention significantly increases the total number of crimes committed within nine years post-grade three by 0.237 and the average number

of crimes committed during this period by 0.026. Both estimates are statistically significant at the 5% level, representing increases of 63% and 68% relative to the control group mean. Examining annual effects after third grade, I find that the adverse effect on crime is particularly pronounced in the fourth and fifth years post-third grade. However, these estimates are statistically significant only at the 10% level.

## 5.4 Effects on Subgroups

Research on grade retention shows that its effects vary across student demographic groups. Jacob and Lefgren (2009) indicates that the adverse effect of eighth-grade retention on the likelihood of dropping out is more pronounced among African American students. Özek (2015) finds that Florida's third-grade promotion policy disproportionately increases disciplinary actions among low-income male students. Furthermore, Figlio and Özek (2020) finds that third-grade retention in Florida enhances English proficiency for English learners more than for non-English learners. Motivated by the heterogeneous effects observed across student demographic groups in prior research, this study examines how the probability of retention and the effects of third-grade retention on short- and long-term outcomes vary across student demographic groups.

Table 9 shows that failing the third-attempt reading test significantly increases the likelihood of retention by 29.4 percentage points (101%) for Black students, 40 percentage points (138%) for Hispanic students, and 25 percentage points (125%) for White students, with all estimates statistically significant at conventional levels. The difference in these estimates is statistically significant (p-values < 0.01). Among students who fail the reading test, the retention risk is significantly higher for female students (38.4 percentage points) and low-income students (35.1 percentage points) than their male and higher-income peers. For English-proficient and limited-English-proficient students, failing the reading test increases the likelihood of retention by 34 and 38 percentage points, respectively. The difference in these effect sizes is not statistically significant (p-value = 0.124).

The variations in the estimated effects of third-grade retention on adult earnings by demo-

Table 9: Effect of Failing the Third Reading Test on Retention Probability by Demographic Groups

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | **Outcome: Retention in Third Grade** | | | | | |
| | All | Black | Hispanic | White | Female | Male | High Income | Low Income | EP | LEP |
| **Fail Reading Test** | 0.355 | 0.294 | 0.400 | 0.250 | 0.384 | 0.329 | 0.329 | 0.351 | 0.340 | 0.380 |
| | (0.013) | (0.028) | (0.016) | (0.035) | (0.023) | (0.017) | (0.020) | (0.021) | (0.016) | (0.022) |
| **Mean** | 0.27 | 0.29 | 0.29 | 0.20 | 0.28 | 0.51 | 0.26 | 0.28 | 0.26 | 0.29 |
| **Effect Size** | 131% | 101% | 138% | 125% | 137% | 65% | 127% | 125% | 131% | 131% |
| **P-values for Equal Effects** | | By race: 0.000 | | | By Gender: 0.007 | | By income: 0.067 | | By English Proficiency: 0.124 | |
| **Observations** | 19,784 | 4,382 | 13,243 | 2,712 | 8,042 | 11,753 | 8,054 | 9,187 | 12,726 | 7,058 |

**Notes:** This table shows first-stage estimates of the effects of failing the third-attempt reading test on retention probability overall and by demographic groups. "EP" indicates English proficient, while "LEP" indicates limited English proficient. The bandwidths are 0 to 8 test-score points to the cutoff. Standard errors are reported in parentheses.

graphic groups are also not statistically significant. As shown in column (1) of Panel A in Table 10, retention reduces the average earnings between ages 23 and 25 by $6,015 for Black students, $2,611 for Hispanic students, and $3,603 for White students. Although the reduction is most prominent for Black students and statistically significant at the 5% level, difference in earnings losses across racial groups is not statistically distinguishable (p-value = 0.535). Panel B examines the effect of retention on earnings by gender. Retention reduces earnings for both men and women, with women experiencing a larger decline of $4,122 compared to $3,033 for men. Although the effect for women is statistically significant at the 1% level, the difference in the effect sizes by gender is not. Panel C reports the earnings effects by family income, measured by eligibility for free meals. Among low-income students, retention reduces earnings by $3,119. The estimate is statistically significant at the 5% level. For high-income students, retention reduces earnings by $3,630, though this estimate is not statistically significant at the conventional levels. Panel D compares the impacts of retention on earnings by English proficiency. Retention reduces earnings by $4,322 for English-proficient students and $2,056 for students with limited English proficiency. Although the earnings loss appears larger for English-proficient students, the difference between the two groups is not statistically significant. Column (2) presents the effects of third-grade retention on college enrollment across demographic groups. While the effect sizes vary, none is statistically significant. Differences in effect sizes within each demographic group are not statistically significant.

Table 10: Effect of Grade Retention on Short- and Long-term Outcomes by Demographic Groups

| | (1) Average Wages 2325 | (2) College Enroll. | (3) HS Grad. | (4) Ever Dropout | (5) Ever Out State | (6) Absenteeism | (7) Crime |
|---|---|---|---|---|---|---|---|
| **Panel A: By Race** | | | | | | | |
| **Black** | -6,015 | -0.087 | -0.176 | 0.007 | -0.017 | 22.189 | 0.037 |
| | (2,646) | (0.090) | (0.091) | (0.069) | (0.035) | (12.123) | (0.210) |
| Observations | 5574 | 5620 | 5620 | 5620 | 5620 | 5620 | 5620 |
| **Hispanic** | -2,611 | 0.020 | -0.006 | -0.010 | 0.022 | -1.726 | 0.135 |
| | (1,607) | (0.044) | (0.044) | (0.034) | (0.024) | (5.765) | (0.117) |
| Observations | 13243 | 13315 | 13315 | 13315 | 13315 | 13315 | 13315 |
| **White** | -3,603 | 0.195 | -0.438 | 0.213 | 0.142 | 18.298 | 0.103 |
| | (6,848) | (0.157) | (0.170) | (0.109) | (0.083) | (20.911) | (0.251) |
| P-value for Equal Effects | 0.535 | 0.266 | 0.019 | 0.180 | 0.195 | 0.168 | 0.921 |
| Observations | 2712 | 2727 | 2727 | 2727 | 2727 | 2727 | 2727 |
| **Panel B: By Gender** | | | | | | | |
| **Male** | -3,033 | 0.001 | -0.074 | 0.007 | 0.047 | 1.232 | 0.126 |
| | (2,327) | (0.056) | (0.057) | (0.044) | (0.029) | (7.619) | (0.169) |
| Observations | 11753 | 11827 | 11827 | 11827 | 11827 | 11827 | 11827 |
| **Female** | -4,122 | 0.013 | -0.100 | 0.035 | -0.000 | 11.305 | 0.122 |
| | (1,415) | (0.052) | (0.052) | (0.038) | (0.025) | (6.724) | (0.081) |
| P-value for Equal Effects | 0.662 | 0.869 | 0.728 | 0.632 | 0.217 | 0.316 | 0.996 |
| Observations | 10184 | 10243 | 10243 | 10243 | 10243 | 10243 | 10243 |
| **Panel C: By Income** | | | | | | | |
| **Low income** | -3,119 | 0.011 | -0.092 | 0.040 | 0.026 | 7.880 | 0.116 |
| | (1,580) | (0.045) | (0.047) | (0.036) | (0.023) | (6.185) | (0.114) |
| Observations | 13883 | 13971 | 13971 | 13971 | 13971 | 13971 | 13971 |
| **High income** | -3,630 | 0.014 | -0.059 | -0.026 | 0.019 | -0.142 | 0.145 |
| | (2,675) | (0.071) | (0.069) | (0.049) | (0.034) | (8.810) | (0.170) |
| P-value for Equal Effects | 0.904 | 0.971 | 0.670 | 0.267 | 0.866 | 0.437 | 0.878 |
| Observations | 8054 | 8099 | 8099 | 8099 | 8099 | 8099 | 8099 |
| **Panel D: By English Proficiency** | | | | | | | |
| **Limited English Proficient** | -2,056 | 0.021 | -0.023 | -0.024 | 0.061 | 2.419 | 0.135 |
| | (2,201) | (0.059) | (0.059) | (0.043) | (0.036) | (7.292) | (0.148) |
| Observations | 7832 | 7861 | 7861 | 7861 | 7861 | 7861 | 7861 |
| **English Proficient** | -4,322 | -0.004 | -0.125 | 0.049 | 0.000 | 8.443 | 0.128 |
| | (1,770) | (0.050) | (0.051) | (0.039) | (0.021) | (6.818) | (0.123) |
| P-values for Equal Effects | 0.434 | 0.740 | 0.193 | 0.211 | 0.152 | 0.556 | 0.974 |
| Observations | 14105 | 14209 | 14209 | 14209 | 14209 | 14209 | 14209 |

Notes: This table presents fuzzy RDD estimates of the effects of third-grade retention on students' short-term and long-term outcomes by demographic groups. Standard errors are reported in parentheses.

However, third-grade retention reduces the likelihood of high school graduation, with effects varying across racial groups. As detailed in column (3), White students experience a sharp 43.8 percentage point decline in high school graduation probability, higher than the reductions for Black (17.6 percentage points) and Hispanic students (0.6 percentage points). The variation in effect sizes across racial groups is statistically significant (p-value = 0.019). Retention also significantly reduces the likelihood of high school graduation for certain groups, with declines of 10 percentage points for female students, 9.2 percentage points for low-income students, and 12.5 percentage

points for English-proficient students. These estimates are statistically significant at the 10% level, while the ones for their counterparts are not. Moreover, the differences in these estimated effects by gender, income, and English proficiency are not statistically significant.

Columns (4) and (5) suggest that third-grade retention increases the likelihood of dropping out and attending an out-of-state school by 21.3 and 14.2 percentage points for White students, respectively. Both estimates are marginally significant at the 10% level, explaining the negative impact observed on the likelihood of high school graduation among White students. Nevertheless, this pattern is not observed for other students. Moreover, the differences in these estimated effects by race, gender, income, and English proficiency groups are not statistically distinguishable from zero.

Column (6) shows that third-grade retention increases cumulative absenteeism over the nine years following third grade by 22 days for Black students and 11 days for female students. Both estimates are marginally significant at the 10% level. The estimated effects for other groups are not statistically significant. Moreover, differences in effect sizes by race, gender, income, and English proficiency groups are not statistically significant. Finally, while third-grade retention is associated with increased criminal behavior, the estimate is not statistically significant for any demographic group.

# 6 Robustness Checks

## 6.1 Timing Analyses

Retaining students in third grade for an additional year naturally delays their educational progression, prompting concerns about whether the observed adverse effects on earnings stem from delayed entry into the labor market. To investigate this issue, I analyze the effect of third-grade retention on timely progression from grades 4 through 12, high school graduation, college enrollment and graduation, and labor market entry.

Appendix Figure A3 presents estimates of the effects of third-grade retention on the timing

31

of progression through subsequent grades. The results demonstrate that retention significantly reduces the probability of timely advancement from grades 4 to 12 relative to initially promoted peers. Nevertheless, the timing gap begins narrowing from grade 6 onwards. Despite these initial delays, retained students do not experience delays exceeding one year beyond the typical progression schedule. This pattern aligns closely with the timing of high school graduation. Although third-grade retention initially reduces the likelihood of on-time high school graduation (as detailed in Table 7), the resulting delays do not extend beyond one year from the expected graduation timeline. Tables 5 and 6 explore the effects of retention on timely college enrollment and graduation. Findings indicate that third-grade retention is associated with a lower likelihood of enrolling in college on time but a higher likelihood of graduating on schedule. However, neither of these estimates reaches conventional levels of statistical significance. Furthermore, third-grade retention does not significantly affect the likelihood of college attendance at any age between 18 and 26, as reported in Table A14. Additionally, approximately 86% of students near the passing threshold on the third-grade reading test attend two-year community colleges, typically completing their degrees by age 20. Consequently, the findings indicate that third-grade retention does not substantially prolong post-secondary enrollment in a way that delays labor market entry by age 26.

Moreover, third-grade retention does not significantly impact the age at which individuals enter the labor market. Appendix Table A10 estimates the effects of retention on the age at first wage earned (from age 18 onward), initial wage amounts, and total accumulated work experience between ages 21 and 26. Column (1) shows a small and statistically insignificant increase (0.085 years, or 0.46%) in the age at first wage. In contrast, retention significantly decreases initial wages by $1,649, representing a 23% decline relative to the mean for students just above the cutoff. Furthermore, retention is associated with a reduction in total work experience of 0.359 years (8.96%) between ages 21 and 26, although this estimate is not statistically significant.

To isolate the negative earnings effects from potential labor market entry delays related to repeating third grade, I further analyze the effects of retention on annual income during the eighth

to eleventh years following ninth grade, as well as average earnings across these years.[7] Results shown in Appendix Table A11 indicate that third-grade retention reduces average earnings during this period by $2,425, a 16% decrease relative to the mean above the cutoff. Although this reduction is somewhat smaller than the 21% decline observed in average earnings from ages 23 to 26, it underscores the consistent negative impact of retention on earnings outcomes across different analytical measures.

## 6.2   Attrition Analyses

The Texas ERC data includes only the educational records of students enrolled in public schools. This may introduce bias to the estimated educational effects if retention causes students to leave public schools and attain higher degrees elsewhere. Similarly, the TWC data excludes income from students working outside Texas. If retention significantly increases the likelihood of retained students earning higher wages outside Texas, this could lead to an overestimation of the negative effects on earnings outcomes reported in this study. To address these concerns, I first analyze the attrition rates for students with third-grade reading scores slightly above and below the promotion cutoff. Subsequently, I examine the impact of third-grade retention on the likelihood of students leaving the Texas public school system and assess how these attrition issues affect the estimated impacts on high school graduation and earnings outcomes.[8]

In Appendix Table A16, I compare the attrition rates among students slightly above and below the promotion cutoff. On average, students below the cutoff are more likely to leave the public school for private or home school than those above the cutoff. However, there is no significant difference in attending school outside Texas, and retained students are less likely to participate in college outside Texas. Since the Texas ERC data does not include high school graduation outside the public school system, I define high school graduation attrition as leaving the public school

---

[7]Using ninth grade as the baseline is advantageous because retention does not significantly affect attendance in grade nine, and fewer than 1.5% of initially retained students fail to reach ninth grade compared to their initially promoted peers.

[8]Attrition should not exist for college outcomes since the Texas ERC data covers nationwide college enrollment records.

without graduation to attend school elsewhere. It is important to note that this definition likely represents an upper limit of high school graduation attrition, since not all leavers will complete high school elsewhere. The statistics indicate that students scoring below the cutoff are 1.4% more likely to leave the public school system without graduating and attend school elsewhere starting from grade ten than those barely passing the cutoff.

I further analyze whether third-grade retention disproportionately increases the attrition probability. Appendix Table A18 presents the results of third-grade retention on the likelihood of enrolling in private or home schools and the probability of attending schools outside Texas. Column (1) indicates that third-grade retention marginally increases the likelihood of attending schools outside Texas by 4.2 percentage points. This estimate is marginally significant at the 10% level, representing a 71% increase relative to the above cutoff mean of 5.9%. Conversely, column (2) shows that third-grade retention has a negligible and statistically insignificant negative impact on the likelihood of attending private or home schools.

To address concerns that third-grade retention may lead students to complete their high school education outside the Texas public school system, I examine the potential impact of retention on high school graduation attrition, as shown in Appendix Table A17. The findings indicate that third-grade retention has a minimal and statistically insignificant effect on the likelihood of students leaving the public school system without graduating and enrolling in schools outside of Texas public schools. Additionally, the results show that this effect is consistent across different grades at which students leave the public school.

Next, I explore how the increase in the likelihood of attending school or college outside of Texas influences the estimated effects of retention on labor market outcomes. Particularly, if third-grade retention increases the likelihood of having higher wages outside of Texas, the negative effect of retention on earnings would be overestimated. To address this issue, I estimate the impacts of third-grade retention on earnings attrition and imputed earnings outcomes in Appendix Table A12. Earnings attrition, as shown in column (1), is defined as attending school or college outside Texas and subsequently having missing wage records for all ages from 23 through 26 in the TWC data.

The results indicate that third-grade retention has a negligible impact on the likelihood of attending out-of-state educational institutions and earning zero wages in Texas during the specified age range. The estimate is not statistically significant.

To further refine the analysis, columns (3) to (5) of Table A12 incorporate imputed earnings for those who attended schools or colleges outside Texas. Here, zero wage records are replaced with three scenarios: the average positive earnings of peers who are initially promoted and have identical reading scores, this average minus one standard deviation, and this average plus one standard deviation. The results from these imputed earnings scenarios align closely with those observed without imputation, as detailed in column (2). These consistent findings, alongside the negligible effect of third-grade retention on earnings attrition, indicate that attrition does not alter the estimated impacts on earnings outcomes.

## 6.3   Additional Analyses

To further assess the robustness of the main findings, I conduct additional robustness checks using the minimum standard cutoff from the first math test in third grade as a quasi-experimental threshold. Administered in April—two months before the third-attempt reading test and not a direct criterion for the reading test-based retention policy—this math test provides a counterfactual to assess whether the negative effect on earnings is specific to failure on the third-attempt reading test.

The reduced form estimates shown in Appendix Table A19 reveal that scoring below the math cutoff has no significant impact on earnings at each age from 23 to 26 for those marginally failing the math test. The results suggest that the math test threshold itself does not inherently influence earnings potential. Furthermore, fuzzy RDD estimates, which use scoring below the math cutoff as an instrument for third-grade retention, indicate that retention caused by failing the math cutoff does not lead to statistically significant changes in earnings outcomes. These results reinforce the conclusion that the negative earnings impacts are linked explicitly to retention resulting from failing the third-attempt reading test.

**Table 11: Comparison of the Effects of Grade Retention Across Studies**

| Policy | Study | Grade | Reading ($\delta$) | HS Grad. (%) | Any College/Public University (%) | Dropout (%) | Absence (%) | Crime (%) | Earnings (%) |
|---|---|---|---|---|---|---|---|---|---|
| Texas | This Study | 3 | +0.52 | -14 | +4/-15 | +14 | +9 | +63 (juvenile) | -19 |
| Louisiana | Eren et al. (2022) | 8 | - | - | - | +16 | +6 | +58 (adult) | - |
| | Eren et al. (2017) | 4 | - | - | - | +11 | - | +1.5 (juvenile) | - |
| | Eren et al. (2017) | 8 | - | - | - | +10 | - | -4.4 (juvenile) | - |
| Chicago | Jacob and Lefgren (2009) | 8 | - | -24 | - | +21 | - | - | - |
| Florida | Schwerdt et al. (2017) | 3 | +0.58 | -0.4 | +0.9/- | - | - | - | - |
| | Figlio and Özek (2020) | 3 | +0.085 | - | - | - | 0 | - | - |
| Netherlands | Meulen (2023) | 12 | - | - | 0/-5 | - | - | - | -9 |

**Notes:** This table compares results observed in this study with those from the literature studying grade retention policy in Chicago, Louisiana, Florida, and the Netherlands. Findings of studies using Florida's policy reflect the joint effect of retention and remediation, whereas others isolate the impact of grade retention alone. "Reading" indicates the reading scores one year after third grade. $\delta$ denotes effect size in standard deviations. All percentages reflect relative changes.

The robustness of the negative impact of retention on earnings is further supported by employing various identification strategies. Appendix Table A20 displays local linear regression estimates from the primary analysis, alongside bias-corrected and robust estimates from (Calonico et al., 2017). All estimations provide consistent and similar results, reinforcing the reliability of the adverse effects of retention on earnings outcomes.

# 7 Discussion and Conclusion

While test-based retention policies aim to provide struggling students additional academic support, their structure varies significantly across states, leading to divergent student outcomes. Existing studies of test-based retention policies present mixed findings on short- and medium-term academic outcomes, showing temporary improvements in test scores but null effects on high school graduation (Schwerdt et al., 2017). A growing body of research highlights unintended consequences, including increased disciplinary incidents, dropout, and criminal activity (Jacob and Lefgren, 2009; Özek, 2015; Eren et al., 2017, 2022). These conflicting results fuel debates on whether grade retention policies, which are widely adopted internationally, ultimately harm students who struggle academically. This study contributes to this discussion by providing new causal evidence on the effects of third-grade retention on adult earnings and postsecondary educational attainment, filling a critical gap in the literature. This study also examines how repeating a grade

affects academic and behavioral outcomes differently, which helps to explain the inconsistencies observed in earlier studies.

This study finds that while third-grade retention temporarily increases test scores, it leads to a 9% increase in school absences and a 63% rise in criminal activities, ultimately harming students' long-term outcomes. Retained students are 14% less likely to graduate from high school and experience a 19% reduction in annual earnings at age 26. Repeating third grade is associated with reduced public university enrollment, though it correlates with increased community college enrollment. Furthermore, the subgroup analysis shows that Hispanic, female, and low-income students are more likely to be retained than their peers. However, the differences are not statistically significant when examining the impacts of third-grade retention on earnings outcomes by race, gender, income, and English proficiency.

To place these results in context, Table 11 compares the findings of this paper to those from leading studies on grade retention policies in Chicago, Florida, Louisiana, and the Netherlands. Texas's reading test-based third-grade retention policy, examined in this paper, closely resembles those in Chicago and Louisiana, where students receive multiple opportunities to pass the test and are provided with remedial support before the final retake. As a result, studies using these policies isolate the effects of grade repetition from remedial support by focusing on students who take the final attempt test. They identify unintended consequences of grade retention, including increases in dropout and crime conviction (Jacob and Lefgren, 2009; Eren et al., 2017, 2022). In line with the findings of these studies, this paper also finds that third-grade retention increases absenteeism, dropout (not statistically significant at the 10% level), and juvenile criminal activity.

Florida's third-grade retention policy is distinctive in several respects. The policy mandates grade retention for third graders who fail a single reading test. School districts provide intensive remedial support post-retention, including a summer school program, daily reading instruction for 90 minutes throughout the subsequent academic year, and assignments to high-performing teachers. This post-retention remediation makes it challenging to isolate the effects of remediation from those of grade retention. As a result, research on Florida's policy can only reveal the

combined impact of grade retention and remediation. Prior works studying Florida's retention policy highlight short-term improvements in test scores, a quicker transition to English proficiency, and increased disciplinary incidents (Özek, 2015; Schwerdt et al., 2017; Figlio and Özek, 2020). Yet, they find no significant impact on absenteeism, high school graduation, and college enrollment. However, no causal evidence documents how grade repetition alone affects third-graders in Florida. Figlio and Özek (2020) indicates that the summer school program alone does not improve third-graders' academic outcomes. However, their analysis cannot further isolate the effects of other remedial supports, like daily reading instruction and assignments to high-quality teachers, from those of repeating a grade. Figlio and Ozek (2024) further raises concern about the effectiveness of remediation. Their results uncover unintended tracking consequences of remediation in middle school. While students scoring below proficiency thresholds receive additional educational resources, they are often placed in classrooms segregated by race, socioeconomic status, and prior academic achievement. Tracking further reduces students' likelihood of enrolling in advanced courses and yields no significant improvements in high school graduation rates or other non-test outcomes.

While Florida's policy combining grade retention and remediation showed no impact on high school graduation, this study finds that grade retention alone decreases graduation likelihood by 14%, even with initial test score gains. This study further shows that third-grade retention is associated with reduced enrollment in a public university but an increase in community college, though the estimates lack statistical significance. A study by Meulen (2023) on twelfth-grade retention in the Netherlands shows a similar pattern in college or university enrollment.

The adverse effects of grade retention on labor market outcomes are more concerning. This paper estimates that third-grade retention reduces annual earnings at age 26 by 19%. This effect is larger than the 9% earnings reduction at age 28 reported by Meulen (2023). The disparity suggests that repeating third grade is more detrimental to long-term economic success than retention in later grades. This is likely due to prolonged exposure to the stigma and disengagement effects associated with early grade retention, as shown by increased absenteeism and criminal activity observed in

this study. In contrast, such effects are not captured in Meulen (2023).

While differences in policy structure and sample population exist across states and countries, this paper—along with Jacob and Lefgren (2009), Eren et al. (2022), and Meulen (2023)—finds that repeating a grade harms students. Grade retention worsens behavioral issues, including absenteeism, dropout, and involvement in crime, reducing the likelihood of high school graduation and lowering adult earnings. Notably, the findings of this study apply to students who are marginally affected by the third-grade retention policy—students who are often at the lower end of the ability distribution and disproportionately from low-income and racial minority backgrounds. Determining whether these adverse effects extend to the general student population is essential for future research.

# References

Calonico, S., Cattaneo, M. D., Farrell, M. H., and Titiunik, R. (2017). rdrobust: Software for regression-discontinuity designs. *The Stata Journal*, 17(2):372–404.

Cattaneo, M. D., Jansson, M., and Ma, X. (2020). Simple local polynomial density estimators. *Journal of the American Statistical Association*, 115(531):1449–1455.

Chetty, R., Friedman, J. N., Hilger, N., Saez, E., Schanzenbach, D. W., and Yagan, D. (2011). How does your kindergarten classroom affect your earnings? evidence from project star. *The Quarterly journal of economics*, 126(4):1593–1660.

Deming, D. J. (2011). Better schools, less crime? *The Quarterly Journal of Economics*, 126(4):2063–2115.

Eren, O., Depew, B., and Barnes, S. (2017). Test-based promotion policies, dropping out, and juvenile crime. *Journal of Public Economics*, 153:9–31.

Eren, O., Lovenheim, M. F., and Mocan, H. N. (2022). The effect of grade retention on adult crime: Evidence from a test-based promotion policy. *Journal of Labor Economics*, 40(2):361–395.

Figlio, D. and Özek, U. (2020). An extra year to learn english? early grade retention and the human capital development of english learners. *Journal of Public Economics*, 186:104184.

Figlio, D. and Ozek, U. (2024). The unintended consequences of test-based remediation. *American Economic Journal: Applied Economics*, 16(1):60–89.

Gelman, A. and Imbens, G. (2019). Why high-order polynomials should not be used in regression discontinuity designs. *Journal of Business & Economic Statistics*, 37(3):447–456.

Greene, J. P. and Winters, M. A. (2007). Revisiting grade retention: An evaluation of florida's test-based promotion policy. *Education Finance and Policy*, 2(4):319–340.

Heckman, J. J. and Rubinstein, Y. (2001). The importance of noncognitive skills: Lessons from the ged testing program. *American Economic Review*, 91(2):145–149.

Heckman, J. J., Stixrud, J., and Urzua, S. (2006). The Effects of Cognitive and Noncognitive Abilities on Labor Market Outcomes and Social Behavior. *Journal of Labor Economics*, 24(3):411–482. Publisher: The University of Chicago Press.

Hipple, S. F. and Hammond, L. A. (2016). Bls spotlight on statistics: Self-employment in the united states.

Imbens, G. W. and Lemieux, T. (2008). Regression discontinuity designs: A guide to practice. *Journal of econometrics*, 142(2):615–635.

Internal Revenue Service (2022). SOI Tax Stats - Migration Data 2021–2022 - State-to-State Outflow [dataset]. Accessed via Internal Revenue Service [distributor], January 21, 2025.

Jacob, B. A. and Lefgren, L. (2004). Remedial education and student achievement: A regression-discontinuity analysis. *Review of economics and statistics*, 86(1):226–244.

Jacob, B. A. and Lefgren, L. (2009). The effect of grade retention on high school completion. *American Economic Journal: Applied Economics*, 1(3):33–58.

Lindqvist, E. and Vestman, R. (2011). The labor market returns to cognitive and noncognitive ability: Evidence from the swedish enlistment. *American Economic Journal: Applied Economics*, 3(1):101–28.

Manacorda, M. (2012). The cost of grade retention. *Review of Economics and Statistics*, 94(2):596–606.

Meulen, S. t. (2023). Long-term effects of grade retention.

National Student Clearninghouse (2010-2019). National Student Clearninghouse Data [dataset]. Accessed via University of Houston Education Research Center [distributor], March 11, 2021.

Özek, U. (2015). Hold back to move forward? early grade retention and student misbehavior. *Education Finance and Policy*, 10(3):350–377.

Schwerdt, G., West, M. R., and Winters, M. A. (2017). The effects of test-based retention on student outcomes over time: Regression discontinuity evidence from florida. *Journal of Public Economics*, 152:154–169.

Texas Education Agency (1994-2022). Texas Education Agency Data [dataset]. Accessed via University of Houston Education Research Center [distributor], March 11, 2021.

Texas Higher Education Coordinating Board (2010-2021). Texas Higher Education Coordinating Board Data [dataset]. Accessed via University of Houston Education Research Center [distrib-

utor], March 11, 2021.

Texas Workforce Commission (2009-2023). Texas Workforce Commission Wages Data [dataset].
Accessed via University of Houston Education Research Center [distributor], March 11, 2021.

University of Houston Education Research Center (1994-2024). Texas Education Research Center
Data [dataset]. Accessed via University of Houston Education Research Center [distributor],
March 11, 2021.

US Census Bureau (2023). American Community Survey (ACS): Public Use Microdata Sample (PUMS), 2019–2023. Distributed by Inter-university Consortium for Political and Social
Research, Ann Arbor, MI. Accessed January 21, 2025.

# A Appendix

## A.1 Data and Definitions for Outcome Variables

### A.1.1 Texas ERC Data

The Texas Education Research Center (ERC) compiles administrative data from various sources for students in Texas public schools. The Texas ERC dataset encompasses Pre-Kindergarten to twelfth grade (PK-12) educational records from the Texas Education Agency (TEA), post-secondary outcomes from Texas institutions via the Texas Higher Education Coordinating Board (THECB), national post-secondary outcomes from the National Student Clearinghouse (NSC), and employment and earnings records from the Texas Workforce Commission (TWC). This section outlines the specific contributions of each dataset to this study.

**TEA Data (1994-2022)**

The TEA provides comprehensive student-level educational records from Texas public schools at the PK-12 level. In this study, the TEA data span from the 1994-1995 school year to the 2022-2023 school year. This study utilizes demographic information, including age, gender, race, limited English proficiency, free meal eligibility, special education participation, and annual school attendance. Attendance records are mainly used to identify grade retention, which is defined as a student repeating the same grade in the subsequent year. Absenteeism data is also used to assess the impact of retention on school absences. The dataset also includes high school graduation statuses and other exit information, such as dropout rates, private or home school attendance, out-of-state schooling, expulsions, or death.

The TEA provides student-level test scores from the Texas Assessment of Knowledge and Skills (TAKS), which was administered annually from 2002-2003 to 2011-2012. This study examines the impact of third-grade retention on reading and math test scores for third to eighth-grade

students, or one to five years after third grade. The TEA data includes both raw scores, reflecting the number of correct responses, and scale scores, which facilitate direct comparisons across different test administrations. In this analysis, raw reading scores adjusted by the promotion cutoff serve as the running variable. Both raw and scale scores are standardized across subject-grade/year-cohort to have a mean of zero and a standard deviation of one, separately.

### THECB Data (2010-2021)

The THECB data used in this study spans from 2010 through 2021, including detailed records on post-secondary education in Texas. The data further categorizes institutions into five types: public universities, Community colleges, Independent colleges and universities, Health Institutions, and Career schools or colleges. This study examines the impact of third-grade retention on college outcomes across these institution types.

### NSC Data (2011-2019)

The NSC data from 2011 through 2019 supplements THECB by providing enrollment records for Texas students attending out-of-state colleges, covering about 96% of U.S. higher education enrollment. However, the NSC data from 2011 through 2016 do not include graduation dates or completion statuses.

### TWC Wages Data (2007-2023)

The wage data used in this study come from the Texas Workforce Commission (TWC) and span from 2007 to 2023. These data provide quarterly wage records for individuals employed in Texas. TWC mandates that all liable employers report Unemployment Insurance (UI) wages and pay UI taxes electronically quarterly. Failure to comply with these reporting requirements may result in penalties under Sections 213.023 and 213.024 of the Texas Unemployment Compensation Act (TUCA).

Texas liable employers include a wide range of entities, such as sole proprietorships, partnerships, limited liability companies, limited partnerships, professional corporations, professional associations, corporations, foundations, trusts, estates, banking institutions, political subdivisions, and governmental agencies.

**Employers become liable once they meet at least one of the following criteria:**

1. **Federal Liability:** The employing unit is subject to the Federal Unemployment Tax Act (FUTA) and pays wages to employees in Texas.

2. **General Wage or Employment Threshold:** The employing unit either:

   - Pays at least $1,500 in total gross wages within a single calendar quarter, **or**

   - Has at least one employee (full-time or part-time) for twenty separate weeks during a calendar year.

   *(The employee does not need to be the same individual for all twenty weeks, and these weeks do not need to be consecutive.)*

3. **Acquisition of Liable Business:** The employing unit acquires all or part of another organization's trade, business, or workforce, provided the other entity was a liable employer at the time of acquisition.

4. **Nonprofit Organizations:** The employing unit is a 501(c)(3) nonprofit organization employing at least four individuals (full-time or part-time) for twenty separate weeks during a calendar year.

   *(These employees need not be the same individuals, and the weeks do not have to be consecutive.)*

5. **Voluntary Liability:** The employing unit voluntarily elects to become liable even if none of the above requirements are met.

6. **Political Subdivisions:** All political subdivisions of Texas, including municipalities, counties, utility districts, and public educational institutions, are automatically liable.

7. **Domestic Employment:** The employing unit pays at least $1,000 in cash wages within any calendar quarter for domestic services.

8. **Farm or Ranch Employment:** The employing unit engages in farm or ranch labor and either:

   - Employs at least three workers (full-time or part-time) for twenty or more weeks within a calendar year, **or**

   - Pays total gross wages of at least $6,250 within a single calendar quarter.

   *(These weeks do not need to be consecutive, and workers do not have to be the same individuals throughout. Seasonal and migrant agricultural workers, including those employed on truck farms, orchards, and vineyards, or hired by labor agents, are included.)*

**Exemptions: types of employment are excluded from TWC wage reporting, including:**

1. Services performed for a church or an organization primarily operating for religious purposes.

2. Services performed by a minor child (under 21 years old), spouse, or parent of a sole proprietor. A sole proprietor is an individual who owns and operates their own business.

3. Work done as part of an unemployment work-relief or work-training program funded or supported by a federal or state agency.

4. Employment of a student by a school, college, or university, where the student is enrolled, attending classes regularly, and earning academic credit for their work.

In brief, TWC wage data exclude wage records for individuals who are not participating in the labor force, are unemployed, are employed outside of Texas, or are self-employed without employees. Therefore, missing wage records in the dataset likely indicate labor market disengagement, unemployment, self-employment, or employment outside the state.

I reference external labor force statistics to identify the primary causes of missing wage records. The US Census Bureau (2023) reports that approximately 35% of Texans over age 16 are out of the labor force. Low labor force participation is more pronounced among young adults, which

likely explains a significant share of missing wages among individuals between the ages of 23 and 26 observed in this study. The unemployment rate in Texas is around 3%. Additionally, Hipple and Hammond (2016) reports that the nationwide self-employment rate among young adults is just 1.9%, underscoring its rarity. Out-of-state employment is minimal: the Internal Revenue Service (2022) indicates that only 1.8% of Texans relocated and paid tax outside of Texas in 2021–22. These statistics reveal that missing wages mainly reflect labor force disengagement and unemployment rather than data attrition due to self-employment and employment outside of Texas. As a result, this study codes missing wage records as zero to reflect labor market disengagement. Table A4 provides detailed definitions for all earnings-related outcome variables.

Table A1: Employment Status From American Community Survey

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Total Class of Worker | Employed | Not in Labor Force | Unemployed | Unincorporated self-employed | Working w/o pay in family business |
| 23,027,304 | 12,958,586 | 8,009,820 | 765,803 | 1,235,292 | 57,803 |
| 100% | 56.3% | 34.8% | 3.3% | 5.4% | 0.3% |

Notes: This table presents Texas employment status based on the American Community Survey 5-Year Estimates Public Use Microdata Sample (2023). The sample excludes individuals younger than 16.

### A.1.2   Data Linkage and Cleaning

Each dataset includes a unique student identifier (ID2) as a substitute for Social Security Numbers (SSNs), enabling longitudinal tracking across datasets. Students without valid SSNs receive state-assigned ID2 numbers. These identifiers are regularly verified and updated across the TEA, THECB, and TWC datasets, reflecting changes such as the assignment of valid SSNs. Further details regarding the matching process and crosswalk are provided here. To verify the accuracy of longitudinal tracking using ID2, I merge the primary sample from TEA data with community college enrollment data from 2014 and examine gender matching rates across these datasets. The resulting gender match accuracy using ID2 is approximately 99%.

The primary analysis sample comprises three cohorts of first-time third-graders from the 2002–03 through 2004–05 school years. Third-graders from the 2002–03 cohort were the first affected by the Texas test-based third-grade retention policy and are approximately 28 years old in 2023. Stu-

dents from the final cohort (2004–05) will be around 26 in 2023. Table A2 presents a detailed timeline for each cohort. These three cohorts were selected to ensure a sufficient sample size for the fuzzy regression discontinuity analysis and to facilitate the observation of earnings outcomes through age 26.

Additionally, I restrict the sample to third-graders whose ID2 is available in the TEA test-score files.[9] This restriction is necessary because employment and earnings data from the TWC can only be matched using ID2. Nearly all records from the TWC dataset contain valid ID2 entries, as SSNs are required for employment reporting.

Table A2: Cohorts 2003 to 2005 Timeline Without Retention

| Calendar Year | Cohort 2002-2003 Grade | Age | Cohort 2003-2004 Grade | Age | Cohort 2004-2005 Grade | Age |
|---|---|---|---|---|---|---|
| 2003 | 3 | 8 | 2 | 7 | 1 | 6 |
| 2004 | 4 | 9 | 3 | 8 | 2 | 7 |
| 2005 | 5 | 10 | 4 | 9 | 3 | 8 |
| 2006 | 6 | 11 | 5 | 10 | 4 | 9 |
| 2007 | 7 | 12 | 6 | 11 | 5 | 10 |
| 2008 | 8 | 13 | 7 | 12 | 6 | 11 |
| 2009 | 9 | 14 | 8 | 13 | 7 | 12 |
| 2010 | 10 | 15 | 9 | 14 | 8 | 13 |
| 2011 | 11 | 16 | 10 | 15 | 9 | 14 |
| 2012 | 12 | 17 | 11 | 16 | 10 | 15 |
| 2013 | | 18 | 12 | 17 | 11 | 16 |
| 2014 | | 19 | | 18 | 12 | 17 |
| 2015 | | 20 | | 19 | | 18 |
| 2016 | | 21 | | 20 | | 19 |
| 2017 | The 8$^{th}$ year post-grade 9 | 22 | | 21 | | 20 |
| 2018 | The 9$^{th}$ year post-grade 9 | 23 | The 8$^{th}$ year post-grade 9 | 22 | | 21 |
| 2019 | The 10$^{th}$ year post-grade 9 | 24 | The 9$^{th}$ year post-grade 9 | 23 | The 8$^{th}$ year post-grade 9 | 22 |
| 2020 | The 11$^{th}$ year post-grade 9 | 25 | The 10$^{th}$ year post-grade 9 | 24 | The 9$^{th}$ year post-grade 9 | 23 |
| 2021 | The 12$^{th}$ year post-grade 9 | 26 | The 11$^{th}$ year post-grade 9 | 25 | The 10$^{th}$ year post-grade 9 | 24 |
| 2022 | The 13$^{th}$ year post-grade 9 | 27 | The 12$^{th}$ year post-grade 9 | 26 | The 11$^{th}$ year post-grade 9 | 25 |
| 2023 | The 14$^{th}$ year post-grade 9 | 28 | The 13$^{th}$ year post-grade 9 | 27 | The 12$^{th}$ year post-grade 9 | 26 |
| Ever Dropout | 2004 to 2018 | | 2004 to 2018 | | 2004 to 2018 | |
| Ever HG Grad. | 2009 to 2022 | | 2009 to 2022 | | 2009 to 2022 | |
| On-Time HG Grad. | By 2013 | | By 2014 | | By 2015 | |
| On-Time College Enroll. | By 2013 | | By 2014 | | By 2015 | |
| Ever College Enroll. | 2010 to 2021 | | 2010 to 2021 | | 2010 to 2021 | |

### A.1.3 Outcome Definition and Attrition

**Earnings outcomes**: This study employs two types of measures for earnings outcomes. The primary category defines earnings by age, including annual earnings at each age from 23 to 26, as

---

[9]Only a tiny fraction of students in the TEA files lack an ID2.

well as the average earnings across these years. Additionally, this category assesses the presence of positive earnings at each age from 23 to 26 and the average over these years. The second category measures annual earnings from the $8^{th}$ to the $11^{th}$ year post-ninth grade and calculates average earnings across this period. This category also includes indicators for positive earnings each year and overall during this period.

**College enrollment outcomes**: College enrollment outcomes are classified into two categories: "ever enrolling" in a college or university and "on-time" college enrollment. The ever enrolling category tracks any college enrollment from 2010 through 2021, while on-time enrollment specifically refers to enrolling in college by the ninth year post-grade three (coded as one if enrolled on time and zero otherwise). These outcomes are further differentiated by the selectivity and type of institution, including community colleges in Texas, public universities in Texas, and colleges outside Texas. Additionally, I define variables to track college enrollment status annually for each age from 18 to 26, providing a detailed view of the impact of third-grade retention on college enrollment.

**College graduation outcomes**: The NSC files within the Texas ERC lack detailed information on graduation status and dates. Therefore, this study focuses solely on college graduations within Texas. Graduation outcomes are categorized similarly to college enrollment outcomes, differentiated as "ever graduated" and "on-time graduation" from either a community college or a public university in Texas. Ever graduated tracks students who graduated at any time between 2010 and 2021. On-time graduation from a community college is defined as graduating by age 20, while for a public university, it is described as graduating by age 22. Additionally, on-time graduation from any college is also expected by the age of 22.

**High School Graduation**: High school graduation is defined as obtaining a high school diploma from a Texas public school between 2009 and 2022, excluding those who earn only a General Educational Development (GED) certificate. The term high school graduation attrition refers to students who leave the Texas public school system before graduation, presumably to attend schools elsewhere. I define high school graduation attrition as one if a student exits the

Texas public school system before graduation and zero otherwise. This coding provides an upper-bound estimate of attrition, recognizing that transferring students may not necessarily complete high school elsewhere.

**Dropout**: Texas public schools follow the U.S. Department of Education's National Center for Education Statistics (NCES) definition of dropout, effective from the 2005-06 school year. By this definition, a dropout is a student who is enrolled in a Texas public school in grades 7-12, does not return to public school the following fall, is not expelled, and does not graduate, does not receive a GED, does not continue school outside the Texas public school system, does not begin college, or does not die (Secondary School Completion and Dropouts, 2008-09). In this study, dropout refers to individuals who have ever dropped out of a Texas public school, as defined by the NCES from 2004 through 2018.

**Test scores**: I conduct same-grade and same-year comparisons when analyzing the effects on reading and math scores. First, I estimate the impact of retention on test scores when both retained and promoted students reach the same grade, spanning from grades four to eight. Second, I compare the test scores by year from the first to the fifth year following the third grade. Reading and math scores are standardized by subject-cohort-grade/year, with mean zero and standard deviation one.

**Behavioral Outcomes**: The TEA disciplinary reasons and actions files identify incidents of violence and crime, which are detailed in Tables A5 and A6, respectively. I define the outcome variables for violence and crime as the total number of incidents recorded yearly from one to nine years following grade three. Furthermore, I calculate the cumulative total and annual average number of incidents over the entire nine-year period for these outcome variables. Similarly, I define the school absence variables as the number of days a student is absent from school each year, from one to nine years following grade three. Additionally, I calculate the cumulative total and the annual average of days absent over the nine years.

Table A3: Definition and Attrition of Educational and Behavioral Outcomes

| Outcomes | Definition | Attrition |
|---|---|---|
| **Panel A: Educational Outcomes** | | |
| Same-year test scores | First-attempt reading or math scores from the first through the fifth year post-grade three | Have test scores outside public schools |
| Same-grade test scores | First-attempt reading/math scores from grades four to eight | Have test scores outside of public schools |
| High school graduation | Graduated from a public high school from 2009 through 2022, excluding GED recipient | Graduated outside of the Texas public school |
| Any college enrollment | Enrolled in any college or university nationwide in the U.S. between 2010 and 2021 | N/A |
| Community college enrollment | Enrolled in a community college in Texas between 2010 and 2021 | N/A |
| Public University enrollment | Enrolled in a public university in Texas between 2010 and 2021 | N/A |
| College enrollment outside Texas | Enrolled in any college outside Texas between 2011 and 2019 | N/A |
| Any college graduation in Texas | Graduated from any college in Texas between 2010 and 2021 | N/A |
| Community college graduation | Graduated from a community college in Texas between 2010 and 2021 | N/A |
| Public University graduation | Graduated from a Public University in Texas between 2010 and 2021 | N/A |
| **Panel B: Behavioral Outcomes** | | |
| School absence | Total/average/annual days absent from school | Days absent outside of the Texas public schools |
| Crime | Total/average/annual criminal activities defined in Table A6 | Crime committed outside of Texas public schools |
| Violent behaviors | Total/average/annual violent behaviors defined in Table A5 | Violence committed outside Texas public schools |

Notes: This table summarizes the definitions and attrition of educational and behavioral outcomes.

51

Table A4: Definition and Attrition of Labor Market Outcomes

| Outcomes | Definition | Attrition |
|---|---|---|
| **Panel A: earnings by age** | | |
| Wages 26 | Annual wages at age 26 | Have wages at age 26 from self-employment and employment outside of Texas |
| Wages 25 | Annual wages at age 25 | Have wages at age 25 from self-employment and employment outside of Texas |
| Wages 24 | Annual wages at age 24 | Have wages at age 24 from self-employment and employment outside of Texas |
| Wages 23 | Annual wages at age 23 | Have wages at age 23 from self-employment and employment outside of Texas |
| Av. Wages 23-25 | Average wages btw ages 23 and 25 | Have earnings from self-employment and employment outside of Texas |
| Av. Wages 23-26 | Average earnings btw ages 23 and 26 | Have earnings from self-employment and employment outside of Texas |
| Positive wages2325 | Have positive earnings btw ages 23 & 25 | Have positive earnings from self-employment and employment outside of Texas |
| Positive wages2326 | Have positive earnings btw ages 23 & 26 | Have positive earnings from self-employment and employment outside of Texas |
| **Panel B: earnings from the $8^{th}$ to the $11^{th}$ years post-grade nine** | | |
| Earnings grade 9+$8^{th}$ | Annual earnings in the $8^{th}$ year post-grade nine | Have earnings from self-employment and employment outside of Texas |
| Earnings grade 9+$9^{th}$ | Annual earnings in the $9^{th}$ year post-grade nine | Have earnings from self-employment and employment outside of Texas |
| Earnings grade 9+$10^{th}$ | Annual earnings in the $10^{th}$ year post-grade nine | Have earnings from self-employment and employment outside of Texas |
| Earnings grade 9+$11^{th}$ | Annual earnings in the $11^{th}$ year post-grade nine | Have earnings from self-employment and employment outside of Texas |
| Av.Earnings grade 9+$(8-11)^{th}$ | Average earnings btw the $8^{th}$ to $11^{th}$ years post-grade nine | Have earnings from self-employment and employment outside of Texas |

Notes: This table summarizes the definition and attrition of earnings outcome variables.

52

## Table A5: Definition of Violent Behavior

| CODE | TRANSLATION |
|------|-------------|
| 2 | CONDUCT PUNISHABLE AS A FELONY 37.006(A)(2)(A) |
| 4 | POSSESSED, SOLD, USED OR WAS UNDER THE INFLUENCE OF MARIHUANA OR OTHER CONTROLLED SUBSTANCE |
| 5 | POSSESSED, SOLD, USED OR WAS UNDER THE INFLUENCE OF AN ALCOHOLIC BEVERAGE |
| 6 | ABUSE OF A VOLATILE CHEMICAL |
| 7 | PUBLIC LEWDNESS OR INDECENT EXPOSURE |
| 8 | RETALIATION AGAINST SCHOOL EMPLOYEE |
| 9 | TITLE 5 FELONY - OFF CAMPUS AND NOT AT SCHOOL SPONSORED ACTIVITY |
| 10 | NON TITLE 5 FELONY CONDUCT-NOT ON CAMPUS OR AT SCHOOL SPONSORED ACTIVITY |
| 11 | BROUGHT FIREARM TO SCHOOL - TEC37.007(e) OR UNLAWFUL CARRY OF A HANDGUN |
| 12 | UNLAWFUL CARRY OF AN ILLEGAL KNIFE UNDER PENAL CODE 46.02-TEC 37.007(a)(1) |
| 13 | UNLAWFUL CARRYING OF A CLUB UNDER PENAL CODE 46.02 - TEC 37.007(a)(1) |
| 14 | CONDUCT CONTAINING THE ELEMENTS OF AN OFFENSE RELATING TO PROHIBITED WEAPONS |
| 16 | ARSON |
| 17 | MURDER, CAPITAL MURDER, CRIMINAL ATTEMPT TO COMMIT MURDER OR CAPITAL MURDER |
| 18 | INDECENCY WITH A CHILD |
| 19 | AGGRAVATED KIDNAPPING |
| 22 | CRIMINAL MISCHIEF |
| 26 | TERRORISTIC THREAT - TEC SECTION 37.006(A)(1) OR 37.007(B) |
| 27 | ASSAULT UNDER PENAL CODE SECT. 22.01(A)(1) AGAINST A DISTRICT EMPLOYEE/VOLUNTEER |
| 28 | ASSAULT UNDER PENAL CODE SECT 22.01(A)(1) OTHER THAN SCHOOL DISTRICT EMPLOYEE/VOLUNTEER |
| 29 | AGGRAVATED ASSAULT (PC SECT 22.02) AGAINST SCHOOL DISTRICT EMPLOYEE/VOLUNTEER |
| 30 | AGGRAVATED ASSAULT (PC SECT 22.02) AGAINST SOMEONE OTHER THAN DISTRICT EMPLOYEE |
| 31 | SEXUAL OR AGGRAVATED ASSAULT AGAINST A SCHOOL DISTRICT EMPLOYEE/VOLUNTEER |
| 32 | SEXUAL OR AGGRAVATED SEXUAL ASSAULT AGAINST SOMEONE O/T DISTRICT EMPLOYEE/VOLUNTEER |
| 34 | SCHOOL-RELATED GANG VIOLENCE |
| 35 | FALSE ALARM/FALSE REPORT - TEC SECTION 37.006(A)(1) AND 37.007(B) |
| 36 | FELONY CONTROLLED SUBSTANCE VIOLATION - TEC SECTION 37.007(A)(3) |
| 37 | FELONY ALCOHOL VIOLATION - TEC SECTION 37.007(A)(3) |
| 41 | FIGHTING/MUTUAL COMBAT |
| 46 | AGGRAVATED ROBBERY - TEC 37.007(a)(2)(F), TEC 37.006 (c)-(d) |
| 47 | MANSLAUGHTER |
| 48 | CRIMINALLY NEGLIGENT HOMICIDE |
| 49 | ENGAGES IN DEADLY CONDUCT |
| 53 | ENGAGED IN CONDUCT THAT OFFENSES ARE SPEC IN TEC OFF-CAMPUS BUT W/IN 300FT |

Notes: This table lists incidents categorized as violence.

## Table A6: Definition of Criminal Behavior

| CODE | TRANSLATION |
|---|---|
| 1 | PERMANENT REMOVAL BY TEACHER FROM CLASS |
| 2 | CONDUCT PUNISHABLE AS A FELONY 37.006(A)(2)(A) |
| 4 | POSSESSED, SOLD, USED OR WAS UNDER THE INFLUENCE OF MARIHUANA OR OTHER CONTROLLED SUBSTANCE |
| 5 | POSSESSED, SOLD, USED OR WAS UNDER THE INFLUENCE OF AN ALCOHOLIC BEVERAGE |
| 6 | ABUSE OF A VOLATILE CHEMICAL |
| 7 | PUBLIC LEWDNESS OR INDECENT EXPOSURE |
| 8 | RETALIATION AGAINST SCHOOL EMPLOYEE |
| 9 | TITLE 5 FELONY - OFF CAMPUS AND NOT AT SCHOOL SPONSORED ACTIVITY |
| 10 | NON TITLE 5 FELONY CONDUCT-NOT ON CAMPUS OR AT SCHOOL SPONSORED ACTIVITY |
| 11 | BROUGHT FIREARM TO SCHOOL - TEC37.007(e) OR UNLAWFUL CARRY OF A HANDGUN |
| 12 | UNLAWFUL CARRY OF AN ILLEGAL KNIFE UNDER PENAL CODE 46.02-TEC 37.007(a)(1) |
| 13 | UNLAWFUL CARRYING OF A CLUB UNDER PENAL CODE 46.02 - TEC 37.007(a)(1) |
| 14 | CONDUCT CONTAINING THE ELEMENTS OF AN OFFENSE RELATING TO PROHIBITED WEAPONS |
| 16 | ARSON |
| 17 | MURDER, CAPITAL MURDER, CRIMINAL ATTEMPT TO COMMIT MURDER OR CAPITAL MURDER |
| 18 | INDECENCY WITH A CHILD |
| 19 | AGGRAVATED KIDNAPPING |
| 22 | CRIMINAL MISCHIEF |
| 26 | TERRORISTIC THREAT - TEC SECTION 37.006(A)(1) OR 37.007(B) |
| 27 | ASSAULT UNDER PENAL CODE SECT. 22.01(A)(1) AGAINST A DISTRICT EMPLOYEE/VOLUNTEER |
| 28 | ASSAULT UNDER PENAL CODE SECT 22.01(A)(1) OTHER THAN SCHOOL DISTRICT EMPLOYEE/VOLUNTEER |
| 29 | AGGRAVATED ASSAULT (PC SECT 22.02) AGAINST SCHOOL DISTRICT EMPLOYEE/VOLUNTEER |
| 30 | AGGRAVATED ASSAULT (PC SECT 22.02) AGAINST SOMEONE OTHER THAN DISTRICT EMPLOYEE |
| 31 | SEXUAL OR AGGRAVATED ASSAULT AGAINST A SCHOOL DISTRICT EMPLOYEE/VOLUNTEER |
| 32 | SEXUAL OR AGGRAVATED SEXUAL ASSAULT AGAINST SOMEONE O/T DISTRICT EMPLOYEE/VOLUNTEER |
| 35 | FALSE ALARM/FALSE REPORT - TEC SECTION 37.006(A)(1) AND 37.007(B) |
| 36 | FELONY CONTROLLED SUBSTANCE VIOLATION - TEC SECTION 37.007(A)(3) |
| 37 | FELONY ALCOHOL VIOLATION - TEC SECTION 37.007(A)(3) |
| 46 | AGGRAVATED ROBBERY - TEC 37.007(a)(2)(F), TEC 37.006 (c)-(d) |
| 47 | MANSLAUGHTER |
| 48 | CRIMINALLY NEGLIGENT HOMICIDE |
| 49 | ENGAGES IN DEADLY CONDUCT |
| 50/52 | USED, EXHIBITED OR POSSESSED A NON-ILLEGAL KNIFE PER STUDENT CODE CONDUCT |
| 51 | USED, EXHIBITED, OR POSSESSED A FIREARM OFF-CAMPUS BUT W/IN 300FT OF SCHOOL |
| 53 | ENGAGED IN CONDUCT THAT OFFENSES ARE SPEC IN TEC OFF-CAMPUS BUT W/IN 300FT |
| 54 | ENGAGE IN CONDUCT PUNISH AS FELONY OFF-CAMPUS BUT W/IN 300FT |

Notes: This table lists the incidents that are categorized as crime.

## A.2  Tables and Figures of Main Results

### Figure A1: Effects of Grade Retention on Raw Scores by Years



(a) Reading Scores                    (b) Math Scores
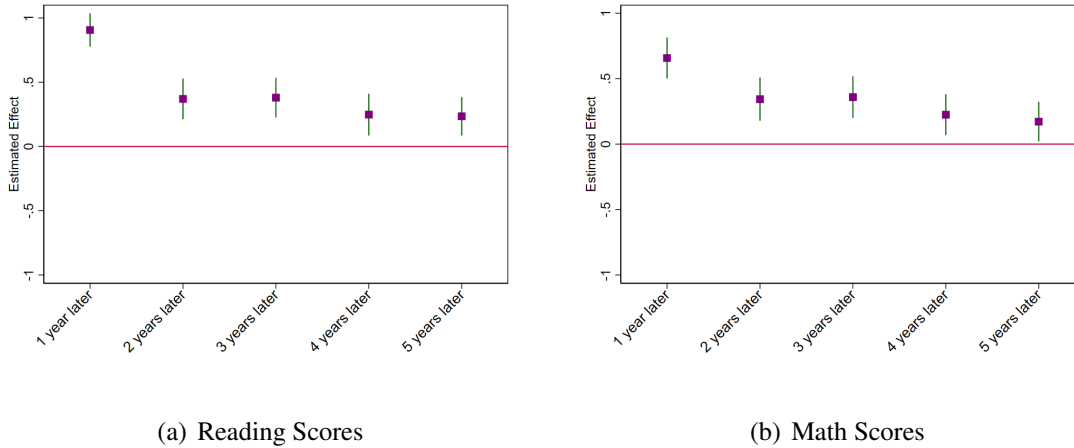
Notes: These figures report estimates of the impacts of third-grade retention on reading and math raw scores from one to five years following third grade. Raw scores indicate the number of questions answered correctly and are standardized within each subject-year-cohort to have mean zero and standard deviation one.

### Figure A2: Effects of Grade Retention on Raw Scores by Grades
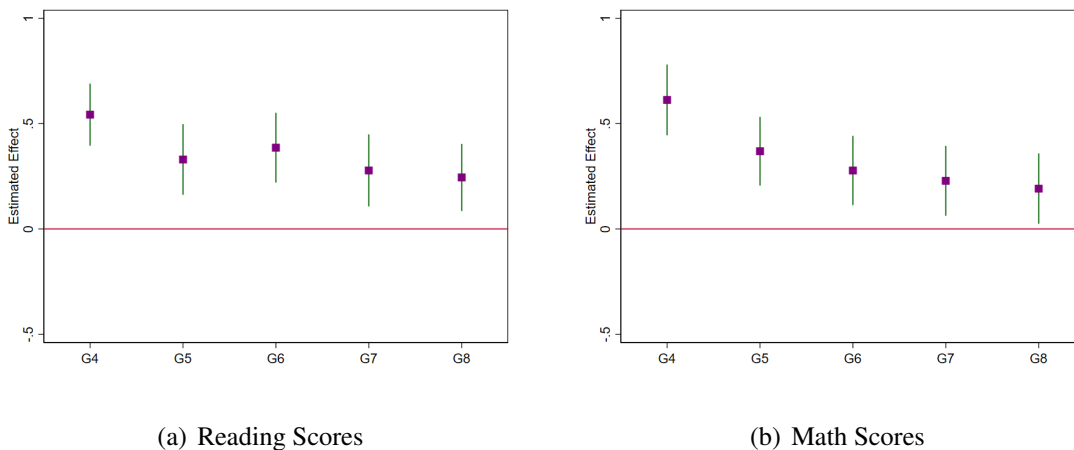


(a) Reading Scores                    (b) Math Scores

Notes: These figures display estimates of the effects of third-grade retention on reading and math raw scores from the fourth through the eighth grades. Raw scores indicate the number of questions answered correctly and are standardized within each subject-grade-cohort to have mean zero and standard deviation one.

## Figure A3: Effects of Retention on Timing of Reaching Each Grade
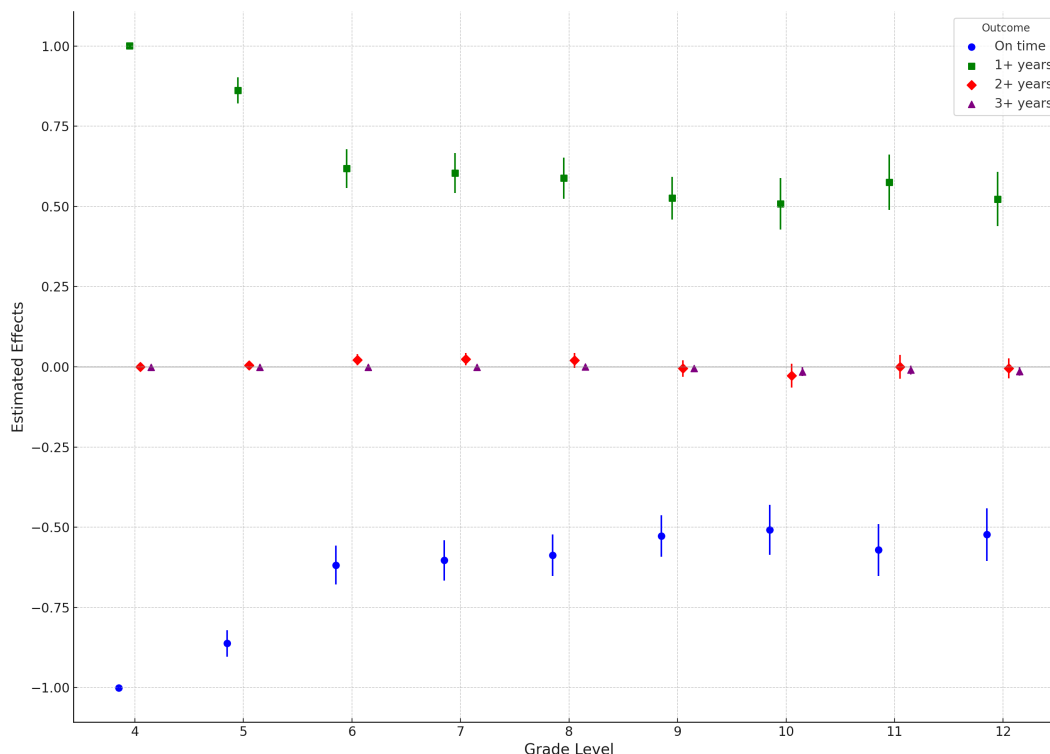


Notes: This figure reports estimates of the impacts of third-grade retention on the likelihood of reaching each grade from four to twelve on time, 1+, 2+, 3+, and 4+ more years relative to the expected time.

## Table A7: Summary Statistics by Test Performance and Retention Status

|  | Below Cutoff | | | | Above Cutoff | | | |
|---|---|---|---|---|---|---|---|---|
|  | Retained | Promoted | Diff. | t-stat | Promoted | Retained | Diff. | t-stat |
| Age | 8.15 | 8.44 | -0.29 | -32.5 | 8.25 | 8.15 | 0.10 | 4.8 |
| Eligible for Free Meals | 0.65 | 0.65 | 0.01 | 0.6 | 0.61 | 0.63 | -0.01 | -0.5 |
| Male | 0.55 | 0.57 | -0.02 | -2.0 | 0.52 | 0.53 | -0.01 | -0.4 |
| Limited English Proficiency | 0.38 | 0.33 | 0.05 | 5.9 | 0.35 | 0.31 | 0.04 | 2.0 |
| Bilingual Program | 0.20 | 0.15 | 0.04 | 6.3 | 0.17 | 0.14 | 0.03 | 1.7 |
| Migrant | 0.04 | 0.04 | 0.01 | 2.3 | 0.04 | 0.03 | 0.01 | 0.8 |
| Special Education | 0.05 | 0.09 | -0.04 | -8.1 | 0.06 | 0.05 | 0.01 | 1.4 |
| Hispanic | 0.63 | 0.57 | 0.06 | 6.8 | 0.60 | 0.60 | -0.00 | -0.0 |
| Black | 0.26 | 0.28 | -0.02 | -2.1 | 0.24 | 0.27 | -0.03 | -1.6 |
| White | 0.09 | 0.14 | -0.05 | -8.2 | 0.14 | 0.11 | 0.03 | 2.1 |
| Third Grade Reading Score | -4.34 | -3.94 | -0.39 | -9.5 | 3.44 | 2.87 | 0.57 | 5.5 |
| High school graduation | 0.58 | 0.58 | -0.01 | -0.7 | 0.65 | 0.57 | 0.08 | 3.8 |
| Any College Enrollment | 0.34 | 0.34 | -0.00 | -0.2 | 0.41 | 0.37 | 0.04 | 1.8 |
| Average Earnings btw Ages 23 to 25 | 14466 | 14916 | -450 | -1.4 | 16076 | 14722 | 1354 | 1.8 |

Notes: This table presents summary statistics for retained students versus those promoted, further categorized by whether their reading scores fell eight points below or above the cutoff.

Table A8: Effect of Failing the Reading Test on Earnings between Ages 23 and 26

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Annual Earnings at Each Age from 23 to 26 | | | | Average Earnings | |
| | Age23 | Age24 | Age25 | Age26 | Ages23-25 | Ages23-26 |
| **Below cutoff** | -1,682 | -1,243 | -1,399 | -1,338 | -1,493 | -1,508 |
| | (665) | (695) | (764) | (795) | (666) | (675) |
| Above cutoff mean | 14,864 | 15,948 | 17,221 | 18,557 | 16,012 | 16,645 |
| Effect size | -11.32% | -7.79% | -8.12% | -7.21% | -9.32% | -9.06% |
| Observations | 14509 | 14509 | 14507 | 14468 | 14509 | 14509 |

Notes: This table displays reduced form estimates of the effects of failing the third-attempt reading test on annual earnings at each age from 23 to 26 and the average earnings between 23 and 25 and 23 and 26. These estimates are obtained by replacing "Retention" on left-hand side of equation (1) with earnings outcomes. The "Above cutoff mean" indicates the average earnings outcome for students whose reading scores are zero to eight points above the cutoff.

Table A9: Effect of Grade Retention on Earnings Outcomes by Cohort

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Annual Earnings at Each Age from 23 to 26 | | | | Average Earnings | |
| | Age23 | Age24 | Age25 | Age26 | Ages23-25 | Ages23-26 |
| Cohorts 2003-05 | -4,324 | -2,921 | -3,278 | -3,477 | -3,516 | -3,518 |
| | (1,535) | (1,583) | (1,753) | (1,843) | (1,468) | (1,493) |
| Observations | 19,784 | 19,784 | 19,782 | 19,731 | 19,784 | 19,784 |
| Cohorts 2003-04 | -5,691 | -3,192 | -4,388 | -5,882 | -4,502 | -4,931 |
| | (2,153) | (2,140) | (2,265) | (2,475) | (2,049) | (2,096) |
| Observations | 9,792 | 9,792 | 9,791 | 9,789 | 9,792 | 9,792 |
| Cohort 2003 | -6,352 | -4,305 | -2,663 | -2,166 | -4,583 | -4,184 |
| | (3,566) | (3,645) | (3,710) | (4,292) | (3,402) | (3,498) |
| Observations | 3,786 | 4,594 | 3,785 | 3,783 | 3,786 | 3,786 |
| Cohort 2004 | -5,088 | -1,603 | -4,754 | -8,325 | -3,861 | -5,015 |
| | (2,364) | (2,382) | (2,635) | (2,638) | (2,276) | (2,301) |
| Observations | 5,958 | 5,958 | 6,619 | 6,619 | 5,958 | 5,958 |
| Cohort 2005 | -3,201 | -3,819 | -2,283 | -173 | -3,147 | -2,514 |
| | (2,922) | (3,062) | (3,416) | (3,687) | (2,849) | (2,886) |
| Observations | 7,502 | 8,470 | 8,469 | 8,420 | 8,470 | 8,470 |

Notes: This table displays fuzzy RDD estimates of the effects of third-grade retention on annual earnings at age 23 to 26 and earnings averaging across these periods by cohort. The sample includes the 2003 to 2005 cohorts of first-time third-grade students who took the third-attempt reading test.

Table A10: Effect of Third-Grade Retention on First Earnings Outcomes and Work Experience

|  | (1) | (2) | (3) |
|---|---|---|---|
|  | Age of First Wages | First Positive Wages | Total Work Experience btw Ages 21 to 26 |
| Retention | 0.085 | -1,649 | -0.359 |
|  | (0.143) | (685) | (0.213) |
| Above cutoff mean | 18.56 | 7,174 | 4.01 |
| Effect size | 0.46% | -23.0% | -8.96% |
| Observations | 14925 | 19764 | 19909 |

Notes: This table displays fuzzy RDD estimates of the effects of third-grade retention on the age of first earnings, first earnings, and total work experience between ages 21 and 26.

Table A11: Effect of Retention on Earnings from the $8^{th}$ to $11^{th}$ Year after Grade Nine

|  | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
|  | Annual Earnings from the $8^{th}$ to the $11^{th}$ Year Post-Grade Nine | | | | Average Earnings |
|  | Grade 9+$8^{th}$ | Grade 9+$9^{th}$ | Grade 9+$10^{th}$ | Grade 9+$11^{th}$ | Grade 9+$8^{th}$ to $11^{th}$ |
| Retention | -2,687 | -2,649 | -1,917 | -2,518 | -2,425 |
|  | (1,484) | (1,616) | (1,711) | (1,791) | (1,514) |
| Above cutoff mean | 13,742 | 14,805 | 16,066 | 17,035 | 15,412 |
| Effect size | -20% | -18% | -12% | -15% | -16% |
| Observations | 19909 | 19909 | 19909 | 22070 | 19909 |

Notes: This table displays fuzzy RDD estimates of the effects of third-grade retention on earnings from the $8^{th}$ to $11^{th}$ years following grade nine and the average earnings during this period. The bandwidth for each outcome is determined by minimizing the mean squared error, following the approach outlined in Calonico et al. (2017). Variations in bandwidth across outcomes account for differences in observations.

Table A12: Effect of Retention on Earnings Attrition and Imputed Earnings

|  | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
|  |  |  | Imputed Earnings | | |
|  | Attrition | Av.Earnings btw ages 23-26 | mean | mean-one sd | mean+one sd |
| Retention | 0.01 | -3,518 | -3,488 | -3,475 | -3,501 |
|  | (0.01) | (1,493) | (1,530) | (1,517) | (1,561) |
| Above cutoff mean | 0.021 | 16,645 | 17071 | 16,704 | 17,438 |
| Effect size | 48% | -21% | -20% | -21% | -20% |
| Observations | 19909 | 19784 | 19784 | 19784 | 19784 |

Notes: This table displays fuzzy RDD estimates of the effects of third-grade retention on earnings attrition and imputed earnings outcomes. Earnings attrition is defined as attending schools or colleges outside Texas and missing wage records in the TWC data between ages 23 and 26. In columns (3) to (5), zero wage records for students ever attending educational institutions outside of Texas are replaced with three scenarios: the average positive earnings of their peers who are initially promoted and have the same reading scores, this average minus one standard deviation, and this average plus one standard deviation.

Table A13: Effect of Third-Grade Retention on Retention Probability after Third Grade

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | | | | | Retention in Each Grade Post-Grade Three | | | | |
| | Grade 4 | Grade 5 | Grade 6 | Grade 7 | Grade 8 | Grade 9 | Grade 10 | Grade 11 | Grade 12 |
| Third-grade Retention | -0.124 | -0.206 | -0.012 | -0.019 | -0.042 | -0.064 | 0.040 | -0.009 | -0.015 |
| | (0.022) | (0.025) | (0.012) | (0.012) | (0.013) | (0.030) | (0.021) | (0.015) | (0.013) |
| Above cutoff mean | 0.075 | 0.125 | 0.020 | 0.024 | 0.029 | 0.140 | 0.050 | 0.030 | 0.028 |
| Effect size | -165.33% | -164.8% | -60% | -79.17% | -144.83% | -45.71% | 80% | -30% | -53.57% |
| Observations | 37201 | 37201 | 37201 | 37201 | 37201 | 37201 | 37201 | 37201 | 37201 |

Notes: This table provides fuzzy RDD estimates of the impacts of third-grade retention on the likelihood of repeating each subsequent grade after third grade.

Table A14: Effect of Grade Retention on the Likelihood of Enrolling in a College at Each Age

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Age 18 | Age 19 | Age 20 | Age 21 | Age 22 | Age 23 | Age 24 | Age 25 | Age 26 |
| **College Enrollment** | -0.041 | -0.006 | 0.028 | -0.028 | 0.011 | -0.007 | 0.012 | 0.011 | 0.000 |
| | (0.031) | (0.034) | (0.031) | (0.027) | (0.025) | (0.020) | (0.019) | (0.013) | (0.007) |
| Observations | 22070 | 19909 | 19909 | 19909 | 19909 | 22070 | 19909 | 19909 | 19909 |

Notes: This table presents estimates of the impacts of third-grade retention on the status of college enrollment at each age from 18 to 26, analyzed using a fuzzy RDD approach. Data on college outcomes are sourced from the Texas Higher Education Coordinating Board (THECB) and the National Student Clearinghouse (NSC). College enrollment is defined as enrolling in any college in Texas or other states from 2010 through 2021.

Table A15: Effect of Retention on Grade Progression to Subsequent Grade

|  | (1) On time attendance | (2) 1+ years later | (3) 2+ years later | (4) 3+ years later | (5) Do not attend this grade in public schools |
|---|---|---|---|---|---|
| Grade 4 | -1.001 | 1.001 | -0.001 | -0.001 | 0.065 |
|  | (0.003) | (0.003) | (0.002) | (0.001) | (0.016) |
| Observations | 19308 | 19308 | 19308 | 19308 | 19909 |
| Grade 5 | -0.862 | 0.862 | 0.005 | -0.001 | 0.036 |
|  | (0.021) | (0.021) | (0.006) | (0.001) | (0.019) |
| Observations | 20936 | 18882 | 20936 | 16522 | 22070 |
| Grade 6 | -0.618 | 0.618 | 0.022 | -0.001 | 0.032 |
|  | (0.031) | (0.031) | (0.009) | (0.001) | (0.021) |
| Observations | 18495 | 18495 | 20496 | 18495 | 22070 |
| Grade 7 | -0.603 | 0.604 | 0.024 | -0.001 | 0.014 |
|  | (0.032) | (0.032) | (0.010) | (0.001) | (0.022) |
| Observations | 18328 | 18328 | 20323 | 18328 | 22070 |
| Grade 8 | -0.587 | 0.588 | 0.020 | 0.000 | 0.018 |
|  | (0.033) | (0.033) | (0.012) | (0.002) | (0.025) |
| Observations | 18076 | 18076 | 20029 | 18076 | 19909 |
| Grade 9 | -0.527 | 0.526 | -0.005 | -0.005 | 0.014 |
|  | (0.033) | (0.034) | (0.013) | (0.003) | (0.024) |
| Observations | 20065 | 18104 | 20065 | 20065 | 22070 |
| Grade 10 | -0.508 | 0.508 | -0.027 | -0.015 | 0.030 |
|  | (0.040) | (0.041) | (0.019) | (0.007) | (0.036) |
| Observations | 15278 | 15278 | 16898 | 16898 | 22070 |
| Grade 11 | -0.571 | 0.575 | -0.000 | -0.010 | 0.131 |
|  | (0.041) | (0.044) | (0.019) | (0.007) | (0.042) |
| Observations | 13680 | 11965 | 13680 | 13680 | 19909 |
| Grade 12 | -0.523 | 0.523 | -0.005 | -0.014 | 0.112 |
|  | (0.042) | (0.043) | (0.016) | (0.007) | (0.045) |
| Observations | 13223 | 13223 | 13223 | 13223 | 17404 |

Notes: This table presents fuzzy RDD estimates of the impacts of third-grade retention on the timing of progression through subsequent grades. "On-time attendance" indicates advancing to fourth grade immediately after third grade. "1+ years" denotes entering fourth grade (and subsequent grades) one or more years later than the standard schedule. Similarly, "2+ years" and "3+ years" indicate delays of two and three more years, respectively, relative to the expected schedule.

### Table A16: Attrition Rates Among Students Above and Below the Cutoff

| | (1)<br>Above Cutoff Mean | (2)<br>Below Cutoff Mean | (3)<br>Difference | (4)<br>t-statistic |
|---|---|---|---|---|
| Attend private or home school | 0.099 | 0.114 | -0.015 | -3.7 |
| Attend out-of-state school | 0.059 | 0.061 | -0.002 | -0.7 |
| Attend out-of-state college | 0.012 | 0.008 | 0.004 | 3.0 |
| **Attrition Rate of High School Graduation** | | | | |
| Leave and attend school elsewhere before grade 9 | 0.016 | 0.018 | -0.002 | -1.3 |
| Leave and attend school elsewhere before grade 10 | 0.051 | 0.066 | -0.014 | -4.7 |
| Leave and attend school elsewhere before grade 11 | 0.083 | 0.105 | -0.021 | -5.7 |
| Leave and attend school elsewhere before grade 12 | 0.112 | 0.133 | -0.021 | -4.9 |
| Ever leave and attend school elsewhere | 0.131 | 0.151 | -0.020 | -4.4 |
| **Attrition Rate of Earnings Outcome** | | | | |
| Attend school/college outside Texas with zero earnings at age 23 | 0.028 | 0.031 | -0.004 | -1.6 |
| Attend school/college outside Texas with zero earnings at age 24 | 0.028 | 0.031 | -0.003 | -1.3 |
| Attend school/college outside Texas with zero earnings at age 25 | 0.029 | 0.031 | -0.002 | -0.8 |
| Attend school/college outside Texas with zero earnings at age 26 | 0.029 | 0.031 | -0.002 | -0.9 |
| Attend school/college outside Texas with zero earnings btw ages 23-25 | 0.022 | 0.025 | -0.003 | -1.3 |
| Attend school/college outside Texas with zero earnings btw ages 23-26 | 0.021 | 0.024 | -0.002 | -1.2 |

Notes: This table compares attrition rates for students whose third-grade reading scores fall within eight points above or below the promotion cutoff. High school graduation attrition is defined as leaving the public school system without graduating and enrolling elsewhere, including private, home, or out-of-state schools. Attrition for earnings outcomes refers to attending an educational institution outside Texas and having zero wages in the Texas TWC data during the corresponding age period.

### Table A17: Effect of Retention on High School Graduation Attrition

| | (1)<br>Exit before Grade 9 | (2)<br>Exit before Grade 10 | (3)<br>Exit before Grade 11 | (4)<br>Exit before Grade 12 | (5)<br>Ever exit |
|---|---|---|---|---|---|
| | | | High School Graduation Attrition | | |
| Retention | -0.010 | -0.004 | 0.031 | 0.027 | 0.030 |
| | (0.010) | (0.020) | (0.027) | (0.031) | (0.032) |
| Above cutoff mean | 0.016 | 0.051 | 0.083 | 0.112 | 0.131 |
| Effect size | -62.50% | -7.84% | 37.35% | 24.11% | 22.90% |
| Observations | 19909 | 19909 | 19909 | 17404 | 19909 |

Notes: This table shows fuzzy RDD estimates of the effects of retention on high school graduation attrition outcomes. In this context, attrition refers to students who leave for other schools and do not obtain a high school graduation diploma from the Texas public school system. This includes scenarios where students transferred to schools outside of Texas, switched to private or homeschooling, enrolled in the Texas University high school diploma program, or completed high school through alternative pathways, such as enrolling in college without a high school diploma.

## Table A18: Effect of Retention on Attending School Outside Texas Public School System

|  | (1)<br>Attending Out-of-State School | (2)<br>Attending Private or Home School |
|---|---|---|
| Retention | 0.042 | -0.003 |
|  | (0.024) | (0.025) |
| Above cutoff mean | 0.059 | 0.099 |
| Effect size | 71% | -3% |
| Observations | 17404 | 22070 |

Notes: This table presents estimates of the effects of third-grade retention on leaving Texas public school without graduation to attend private or home schools and out-of-state schools using the fuzzy RDD method. Out-of-State School indicates a student who left the Texas public school and attended a school outside Texas between 2004 and 2018. Private or home school is defined as attending a private or home school between 2004 and 2018.

## Table A19: Effect of Grade Retention on Earnings Outcomes using Math Cutoff

|  | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
|  | Annual Earnings at Each Age from 23 to 26 | | | | Average Earnings | |
|  | Age23 | Age24 | Age25 | Age26 | Ages23-25 | Ages23-26 |
| RDD Estimates | -439 | -583 | -435 | -110 | -470 | -363 |
|  | (732) | (781) | (867) | (883) | (740) | (755) |
| Observations | 12,496 | 12,496 | 12,495 | 15,731 | 12,496 | 12,496 |
| Fuzzy RDD Estimates | -7,966 | -11,479 | -11,316 | -7,367 | -12,472 | -11,060 |
|  | (14,762) | (15,471) | (15,827) | (16,226) | (13,878) | (13,966) |
| Observations | 12,496 | 12,496 | 15,758 | 15,731 | 15,759 | 15,759 |

Notes: The top panel of this table shows reduced form estimates of the effect of failing the minimum standard cutoff of the first math test in third grade on earnings outcomes. The bottom panel of this table displays fuzzy RDD estimates of the effects of third-grade retention due to failing the math test on earnings at each age from 23 through 26 and the average earnings between ages 23 to 25 and ages 23 to 26.

Table A20: Effect of Grade Retention on Earnings by Methods

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Annual Earnings at Each Age from 23 to 26 | | | | Average Earnings | |
| | Age23 | Age24 | Age25 | Age26 | Ages23-25 | Ages23-26 |
| Local linear regression | -4,324 | -2,921 | -3,278 | -3,477 | -3,516 | -3,518 |
| | (1,536) | (1,583) | (1,752) | (1,842) | (1,468) | (1,492) |
| Bias-corrected | -4,737 | -3,750 | -4,244 | -4,352 | -4,353 | -4,414 |
| | (1,536) | (1,583) | (1,752) | (1,842) | (1,468) | (1,492) |
| Robust | -4,737 | -3,750 | -4,244 | -4,352 | -4,353 | -4,414 |
| | (2,076) | (2,109) | (2,386) | (2,473) | (1,942) | (1,974) |
| Observations | 36,970 | 36,970 | 36,966 | 36,874 | 36,970 | 36,970 |

Notes: This table presents estimates of the effects of grade retention on earnings outcomes using rdrobust methods introduced in (Calonico et al., 2017). Three estimation procedures are employed: (i) local linear estimates with heteroskedasticity-robust standard errors, (ii) bias-corrected estimates with heteroskedasticity-robust standard errors, and (iii) bias-corrected estimates with robust standard errors.