Included To Be Excluded: Accountability Pressure and Students in Special Education*

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Abstract

School accountability systems are designed to incentivize schools to provide better education to their students. Despite many reports of positive gains in student outcomes, schools are known to engage in strategic behaviors to game the systems. This study examines how schools respond to accountability pressures for students with disabilities and how such strategic responses affect students' long-term outcomes. Using administrative data that link the educational and labor market outcomes of all students in Texas public schools between 1994 and 2019 with a difference-in-differences framework, I find that in response to the mandate of incorporating students in special education into accountability measures, schools resorted to granting more test exemptions to these students to protect their ratings. These exemptions were focused on students with lower past test scores. Furthermore, such exclusion led to adverse long-term outcomes such as less high school graduations and employment in adulthood. These results indicate that incomplete incentive designs could lead to unintended school behaviors and, consequently negatively impact students who were intended to be helped.

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1 Introduction

The expansion of school accountability systems, systems that evaluate schools based on student performance with consequential sanctions or rewards, has been one of the most important movements in U.S. education over the past few decades. Through these incentive designs, policymakers aim to encourage schools to provide the best education services. It was one of the few policies that have gathered support from both sides of the political spectrum despite some disagreement over specifics. This bipartisan agreement led to the nationwide mandate of school accountability systems through the No Child Left Behind (NCLB) Act in 2001. Ample evidence suggests that such systems indeed have had positive effects on student test scores (Hanushek and Raymond, 2005; Rockoff and Turner, 2010; Dee and Jacob, 2011; Rouse et al., 2013; Chakrabarti, 2014; Reback et al., 2014) and some long-term outcomes (Deming et al., 2016; Eren and Ozturk, 2022), following significant efforts by schools (Chiang, 2009; Craig et al., 2013).

However, not all students have benefited from these systems. Studies and media coverage have claimed that schools strategically responded to the accountability pressure, sometimes sacrificing some of their students who were less crucial to their ratings. School accountability systems, especially those centered on high-stakes tests as in the U.S., induced schools to tailor their curricula only for test preparation, allocate fewer resources to the lowest-performing students, and even exclude them from testing assessments.[1] Such actions are primarily undesirable in the sense that they often end up excluding disadvantaged students who need the most help from schools. Nevertheless, despite extensive evidence indicating that schools game the systems, no study has examined how such strategic school responses affect students in the long run.

This paper fills this gap by providing causal evidence on schools' strategic responses to

[1] See https://www.washingtonpost.com/education/2023/07/13/fixing-damage-nclb-essa/, https://www.nytimes.com/2018/05/25/learning/accountability-based-testing-is-broken.html, or https://www.msnbc.com/msnbc/bushs-texas-miracle-debunked-lone-star-st-msna18950 for major media coverage.

the school accountability pressure and their effects on students' long-term outcomes in their higher education and later-life labor markets. I use interlinked individual-level longitudinal data from multiple government agencies that cover all public schools in Texas to track down students' outcomes up to their adulthood. In the 1990s, the early-stage Texas school accountability system mainly relied on aggregate pass rates of general education students. In 1999, Texas incorporated all test scores of special education (SE) students previously not considered in the rating calculation into its accountability system. Using this policy shock, I examine how this accountability shock affected the participation and test scores of students in SE and, eventually, their long-term outcomes. This strategy directly addresses various concerns about using test scores to evaluate the effects of accountability systems.

The 1999 reform in Texas targeting SE students is considerably distinguished from other accountability variations in previous literature. It generated greater accountability pressure that schools found much more challenging to cope with. The higher education costs and the poor academic performance of SE students made them costly student groups for schools held accountable in Texas, thus creating incentives to exclude SE students from testing and education. Rather simple forms of the early-stage Texas school accountability system and the existence of exemption provisions allowed for SE students further reinforced such incentives. This policy context provides an excellent empirical setting to reveal how schools strategically exclude disadvantaged students and how such actions ultimately affect student outcomes. My study also has important implications for understanding the consequences of other state accountability programs since Texas served as a benchmark state for the national school accountability reform of NCLB, being one of the earliest adopters of a full-scale school accountability system.

For my empirical strategy, I use the school-level variations in shares of students in SE–targets of the 1999 reform–, using the difference-in-differences approach. While the reform was implemented statewide, the accountability pressure it generated was proportional to the number of SE students within each school. Schools with large numbers of SE

students expected heavier drops in aggregate test pass rates after the reform compared to those with only a few SE students. Therefore, I compare individual-level student outcomes between schools that had many SE students and schools that did not across years before and after the reform. Using comprehensive panel data, I examine the causal effects of increased accountability pressure on SE students in the short (test scores and participation) and long run (high school graduation, college, and labor market outcomes).

I find that increased accountability pressure caused schools to significantly increase test exemptions for SE students. Additional 10 percentage points of SE student shares led to a 7–10 percentage points drop in standardized test participation rates, likely to protect schools' accountability ratings. A heterogeneous effect model using students' past performance reveals that this exclusion was highly selective: Students with lower past test scores were removed first. This implies that substantial improvements in SE student performance were partly due to changes in the composition of students taking the tests. To address compositional effects, I use an individual fixed effect approach and show that there was no improvement in their test scores. I also find that these exclusions of SE students from the testing pools were more sensitive to the district-level incentives than school-level ones, providing suggestive evidence that district leadership could have been making these decisions rather than local schools.

By comparing the outcomes of different cohorts around 1999, I also show that accountability pressure negatively impacted students' long-term outcomes, contrary to the original intention of the 1999 reform. My estimates indicate that additional 10 percentage points of SE student shares at high schools lead to 0.7 percentage points lower high school graduation rates and 1.2 percentage points lower employment rates between ages 25 and 29. My results show that these negative impacts were likely due to exclusions in high schools, where low-performing SE students became less likely to take high school exit exams and more likely to drop out before reaching Grade 10.[2] Heterogeneous analyses based on students' past

[2]Before 2003, high school students in Texas had to take and pass exit-level exams in Grade 10. This exam was the only standardized test for which high schools were held accountable.

test scores show that similar to the exclusion from testing, negative impacts on long-term outcomes were largely driven by low-performing SE students.

This paper contributes to an extensive literature on schools' strategic responses to accountability pressure. Studies reveal that schools concentrate resources on the student-subject groups that matter most to their ratings (Reback, 2008; Neal and Schanzenbach, 2010) and reshape their testing pools by test exemptions (Cullen and Reback, 2006; Figlio and Getzler, 2006; Jennings and Beveridge, 2009), dropouts (Heilig and Darling-Hammond, 2008; Cilliers et al., 2021), and disciplinary actions (Figlio, 2006). Despite this large literature, past studies, particularly on strategic exclusion from testing pools, have focused on using either observational evidence or inferred accountability pressure measures.[3] These approaches might be vulnerable to measurement error from inaccurately specified models. I add to this literature by providing more concrete evidence using a direct measure of accountability pressure from a sharp policy variation directly targeted to SE students.[4] This advantage enables me to isolate the effects on SE students without concerns about spillover effects from general education students.

Furthermore, I contribute to another growing literature on the long-term effects of school accountability systems. Unlike a broad literature examining the short-term impacts of the accountability pressure, only two recent studies analyzed this important question. Deming et al. (2016) focused on the effects on long-term educational and labor market outcomes, and Eren and Ozturk (2022) studied the effects on criminal activity and self-sufficiency, both showing positive net effects on general students. I complement this literature by focusing on effects on more disadvantaged students that were not revealed by previous net effect estimates, using a clearer empirical strategy.[5] In addition, this paper stands out from previous

[3]For example, Cullen and Reback (2006) used annual changes in required pass rate thresholds of the Texas accountability system to construct marginal benefit curves of strategic exemptions, similar to Reback (2008). While they showed a positive correlation between school-level incentives and a dummy of increased exemptions, the coefficient sizes were moderate. They failed to show the same relationship between the numbers of exemptions and constructed incentives as well.

[4]Richardson (2015) used a similar identification strategy as mine, but it is based on assumptions different from mine. I briefly return to this issue in a later section.

[5]Deming et al. (2016) used an approach using inferred accountability pressure measures similar to the

studies by offering potential mechanisms to explain these long-term effects, backed by past descriptive and anecdotal evidence.

Lastly, this study has important implications for current debates on the designs of school accountability systems. Even more than two decades after NCLB and 24 years after the 1999 reform, similar problems have persistently plagued school accountability systems. They have gone through substantial overhauls by introducing more complicated, multi-layered rating calculations and alternative assessments for disabled students. However, strategic responses by schools and consequential detrimental impacts on students have still largely been overlooked. Schools exploit loopholes in rating calculations[6], and disabled students suffer from low expectations and inattention from schools (Lewis, 2008). By providing causal analyses of the unintended consequences of school accountability, I reveal that carefully designed accountability is necessary not only for better education in schools but also for better later-life outcomes of students.

2 Background

2.1 Texas Accountability System

Texas was one of the few states with a rigorous school accountability system before the well-known No Child Left Behind.[7] A basic form of standardized testing was in place in the early 1980s with exit exam requirements for high school diplomas. The Academic Excellence

aforementioned studies. Eren and Ozturk (2022) exploited classic regression discontinuity design based on the rating cutoffs. However, their empirical strategy could be problematic when identifying impacts on students' long-run outcomes because students are expected to spend four years in high schools. While they rely on regression discontinuity variation in the 9th grade, this does not capture differential treatments in their remaining 10–12th grades that will also affect their long-term outcomes.

[6]See related article in https://www.washingtonpost.com/education/2022/10/30/new-miracle-texas-school-district/

[7]Carnoy and Loeb (2002) measured the intensities of state accountability systems in 2000 with an index between 1 and 5. Texas was one of the four states with an intensity index of 5, along with New Jersey, New York, and North Carolina. Texas had both the earliest and most comprehensive system among those four. Meanwhile, most states–32 states–had much weaker systems with an intensity of 2 or below. Two states–Iowa and Nebraska–had no school accountability system before NCLB.

Indicator System (AEIS) that started in 1989, publicly reported a wide range of campus- and district-level student performance measures linked to monetary awards. This set of systems in Texas–standardized tests, school evaluations, consequent rewards, and penalties–formed one of the earliest school accountability systems in the nation and became a significant motivation for the nationwide reform of No Child Left Behind in 2001.

The early-stage Texas school accountability system in the 1990s was mainly based on several aggregate measures. Each year, schools and districts were given one of the four ratings: Exemplary, Recognized, Acceptable (Academically Acceptable), and Low-performing (Academically Unacceptable). Three measures determined such ratings: pass rates of Texas Assessment of Academic Skills (TAAS), dropout rates, and attendance rates, as illustrated in Figure 1. To progress to the next rating, each school or district had to satisfy all three criteria for all five student groups: All, Black, Hispanic, White, and Economically Disadvantaged. Failure in even one measure led to the next lower rating in principle.[8] Each student subgroup was considered only if it maintained a sufficiently large number of students.[9]

These accountability ratings were followed by significant consequences for schools, both explicitly and implicitly. Explicit incentives included monetary rewards[10] or exemptions from certain regulations and requirements for "Exemplary" campuses and districts. Schools with poor performance, typically classified as "Low-performing," had to conduct a hearing for residents and property owners. Further sanctions could follow if they did not show improvement afterward, which could even include school or district closure and consolidation.[11] Implicit incentives involved impacts on the school's reputation, as all ratings were publicly available. Multiple studies indicate that such publicly disclosed ratings could affect future

[8]Failed requirements could be waived under the "Required Improvement" rule, which was applied to schools that showed significant improvements. However, its use was limited due to several eligibility conditions.

[9]For example, evaluation was waived for a student-subject group with less than 30 students.

[10]The Texas Successful Schools Award System and the Principal Performance Incentive Program provided the awards in the 1990s and 2000s. For example, the TEA notified the distribution of funds up to $500,000 in 2003, targeting schools that exhibited significant gains in student performance.

[11]Deming et al. (2016) showed that the effect of accountability pressure in Texas was concentrated at the lowest margin, schools that could have been rated "Low Performing." The pressure to achieve higher ratings had no significant effect.

school enrollment, closure, or even local property values (Figlio and Lucas, 2004; Nunes et al., 2015; Andrabi et al., 2017).

Some lauded the Texas system for demonstrating substantial improvements in student achievement. Texas showed impressive gains in TAAS pass rates across all subjects, ranging from 8 to 20 percentage points between 1994 and 1998, after the full-scale implementation of the school accountability system. The racial gap between Black and White students narrowed from 38 to 30 percentage points, and dropout rates plunged from 2.8 to 1.6 percentage points during the same period (Haney, 2000).[12] Supporters called this drastic improvement the "Texas Miracle," the term President Bush often used during his presidential campaign in 2000. Shortly after, the Texas Miracle motivated the legislation of No Child Left Behind, where the Texas-style school accountability system was mandated nationwide.

However, plenty of evidence suggested that such improvements were not a miracle but a myth resulting from schools' strategic behaviors. Unlike steep gains in high-stakes TAAS pass rates, students showed much less improvements with increasing racial gaps in a low-stake nationwide assessment not considered by the accountability system (Haney, 2000; Klein et al., 2000). Schools were very likely to have manipulated their testing population by retaining grades, making unreported dropouts, and excluding underperforming students via SE exemptions (Haney, 2000; Fielding, 2004; Heilig and Darling-Hammond, 2008). Interviews with veteran teachers also indicate that such accomplishments were achieved through intensive "teaching to the test," rather than real gains in student learning (Ramzinski, 2019).

2.2 1999 Incorporation of Special Education Students

Special education services, mandated by the Individuals with Disabilities Education Act, provided disabled students with additional resources necessary due to their conditions in schools. Qualified Texas students receive individually tailored education following the Indi-

[12] Appendix Figure A.1 illustrates annual trends of TAAS math pass and dropout rates. Both measures show significant improvements after 1994. It should also be noted that official dropout statistics were often unrealistically low, as Haney suggested in his research.

vidual Education Program (IEP) designed by their Admission, Review, and Dismissal (ARD) committees. An ARD committee consists of a student's parents and school personnel involved with the student. It controls the entire process around SE, including initial referral, curriculum setup, giving accommodations or exemptions for testing, and managing requirements for grade promotion or graduation. Thus, schools had considerable discretion over initial referrals, education, and evaluation of SE students.

Before 1999, the Texas accountability system did not consider TAAS scores of SE students. This was based on the idea that SE students could not be evaluated appropriately as general education students taking TAAS. The Texas Education Agency (TEA) also intended to incentivize districts to actively include students with potential disabilities in the statewide assessment program.[13] On the other hand, this exacerbated the deliberate over-identification of SE students to protect school ratings by strategically placing low-performing students into SE (Nagle et al., 2006). The TEA was aware of this risk as well, with rising shares of SE students and their TAAS participation over the years.

Thus, the TEA forced all TAAS scores of SE students to be counted by the accountability system from 1999 under heavy pressure from disability advocacy groups. This expansion in the accountability subset[14] created a significant new accountability pressure on Texas schools and districts. In 1998, around 14% of all students were in SE, most of them harshly underperforming compared to general education students. Test exemption rates skyrocketed in 1999 (Figure 2), and the share of SE students started to trend downward (Figure 3). While there had been no established causal relationship between increased test exemptions and the 1999 reform (Linton, 2000), teachers and district administrators found it highly likely that the increased accountability pressure caused this sharp increase in test exemptions (Nagle et al., 2006).

Recognizing this problem, the TEA banned test exemptions due to SE status in 2001.[15]

[13] Refer to Policy Research Report 9 of TEA (1997).

[14] Appendix Figure A.2 illustrates the expansion. It shows a sharp increase in the fraction of students under accountability after the 1999 reform.

[15] In reality, a small portion of exemptions were still in place. 7.8% and 8.1% of students in SE got ARD

However, this had no real effect on schools' behaviors. A new test, the State-Developed Alternative Assessment (SDAA), developed for SE students, was introduced simultaneously as an alternative to TAAS. Because the accountability system did not include the SDAA measures,[16] the only real change to schools after 2001 was that they could put low-performing SE students into a low-stake exam, the SDAA, instead of giving full test exemptions. Therefore, the measure did not affect schools' incentives around SE students and their consequent TAAS participation, as illustrated in Figure 2.[17]

3 Data

This study uses multiple individual-level administrative datasets provided by the Texas Education Research Center (ERC). I use K-12 educational records from the TEA that cover all students in Texas public schools.[18] These records cover all aspects of educational information of each student, including enrollment, attendance, graduation, disciplinary actions, dropouts, and sociodemographics like age, gender, ethnicity, SE status, and free or reduced lunch (FRL) eligibility. The data also provide detailed institutional information on schools and districts, including their types, geographics, and budgets. In this study, I use records of SE students enrolled between 1994, the earliest enrollment year in the data, and 2002, a year before the major overhaul in testing and the accountability system in Texas.

For the short-run academic outcomes of students, I focus on high-stakes standardized exam (TAAS) participation and performance of SE students. I link the individual-level student enrollment records to the TAAS test records in the 3rd–8th grades and 10th grade, at which TAAS is administered. The TAAS subjects I use are reading and mathematics, as

test exemptions in 2001 and 2002, respectively.

[16]The SDAA was later updated and included in the accountability system, starting in 2004. However, the rigor of SDAA-based accountability was questionable because of its low passing thresholds (Lewis, 2008) determined by ARDs.

[17]This is contrary to what Richardson (2015) assumed for his core identification strategy. His study assumed that this prohibition effectively made SE students included in the accountability subset, which is not supported by this raw data trend.

[18]This includes non-traditional public institutions such as charter school districts, alternative education campuses, and juvenile detention centers. The data do not cover private institutions in Texas.

they are the two subjects tested across all grade levels. I convert the raw TAAS scores into normalized scores with a mean of zero and a standard deviation of one within each grade year to estimate the effect on test performance. I count only the first take of TAAS each year and subject for test retakes.

I do not include the earliest two years, 1994 and 1995, in my analysis because of a change in testing policy in 1996. Before 1996, schools could freely give TAAS exemptions to students without any alternative assessment to replace TAAS. The TEA reverted this rule in 1996, mandating the provision of alternative assessments when students are exempt from TAAS. Since schools and ARD committees had to develop individualized assessments matched to each student's IEP, the change increased the costs of giving test exemptions. This naturally raised overall TAAS participation rates, proportional to the number of SE students and exemptions they had received. This could theoretically threaten my identification strategy based on the same variation.[19] Therefore, I examine the seven years between 1996 and 2002 for my short-run analysis. Figure 4 illustrates this setup, where my analysis focuses on the last two periods.

I construct long-term student outcomes using three distinct datasets. First, I use the TEA's high school graduation records to observe whether each student successfully graduated from a high school. Second, the TEA student record is linked to the Texas Higher Education Coordinating Board (THECB) data, which include the students in higher education institutions within Texas. I build a measure of college completion by checking if a student has a two-year or four-year college degree after the high school graduation age. Third, I use administrative data from the Texas Workforce Commission that contain all employees subject to the unemployment insurance benefits in Texas to build outcomes on wage and employment.[20] This study uses each student's wage and employment at the ages

[19]Event study results including the first two years show identical treatment effects in 1999. Testing rate results are consistent with my prediction and show steep increases in testing rates. However, they also show that this shock is clearly different from the 1999 reform. Students were "indiscriminately" added to the testing pool, while the 1999 reform shows clear cream-skimming behaviors.

[20]Similar to TEA and THECB data, I cannot track individuals who are employed outside of Texas. Such individuals are considered unemployed in this study.

between 25 and 29.

Lastly, I use the AEIS reports, which are school reports published by the TEA. The AEIS reports contain various school- or district-level information such as ratings, student demographics, and high-stakes test performance. While most of the data are also available through the TEA data at more micro levels, I use the official accountability ratings and indicators used to determine the actual accountability ratings to supplement the primary datasets I described above. Rating information lists accountability ratings, including the "unrated" status of all schools and districts in Texas. The accountability indicators include aggregate TAAS pass rates, attendance rates, dropout rates, and the number of each student group considered by the accountability system.

Table 1 summarizes average individual characteristics along with educational and labor market outcomes of general (Columns 1 and 2) and SE students (columns 3 and 4). I show separate statistics for pre-periods (Columns 1 and 3) and post-periods (Columns 2 and 4) around the 1999 reform. Students in SE were more likely to be male and have FRL benefits but showed little differences in racial distributions and limited English proficiency status. The two groups show stark differences in educational outcomes. SE students were less likely to take TAAS exams, with far lower TAAS scores if tested. They were also less likely to graduate high schools and attend and complete colleges. These negative traits continued in future labor markets, showing lower annual earnings and employment rates. One notable change between pre- and post-periods is TAAS test rates and scores of SE students. Students in SE experienced a sharp decrease in test rates and an increase in test scores, with no changes in the outcomes of general education students.[21]

[21] General education students show small decreases in normalized scores, but they are highly likely due to spillover effects from a rise in the average scores of SE students. Note that SE students occupied significant portions of the overall student population.

4 Empirical Strategy

To estimate the effect of accountability pressure on students, this paper uses a difference-in-differences framework to compare changes in individual outcomes between schools with different accountability pressures imposed by the 1999 reform. In this section, I explain how I construct the measure of the imposed accountability pressure. Then, I describe sample construction processes and regression frameworks for both short- and long-run analyses.

4.1 Short-run Analysis

First, I estimate the causal relationship between the accountability pressure introduced by the 1999 reform and the short-run outcomes of SE students. Since there was no variation in implementation timing across schools in Texas, I use a cross-school variation in initial shares of the SE student body for this study. This strategy is based on the fact that the expected drop in aggregate pass rates after the reform was proportional to the initial number of SE students within each school. Higher fractions of SE students in schools meant more expected drops in aggregate test pass rates after the 1999 reform, which led to larger accountability pressure. This approach is similar to that of Ballis and Heath (2021), where they use pre-policy SE shares to examine the effects of the Texas SE share cap in 2005.

While the SE student shares could serve as a useful proxy for accountability pressure by the reform, they were often endogenous, subject to choices of schools. For example, incorporating scores of the SE group could have reduced a school's incentive to refer a student to SE. This is particularly likely as schools often placed low-performing students into SE before the reform.[22] Though moderate, the trend reversal of SE shares after 1999 in Figure 3 further supports this hypothesis. This is problematic not only because of the endogeneity issue but also because it could lead to different student compositions in SE after the reform, which could bias my estimates.

[22] Fielding (2004) conducted surveys on Texas educational diagnosticians, where over 78% responded that over half of initial referrals were primarily driven by poor TAAS performance. Teachers and school administrators explicitly ordered for qualification to SE services, often in "inappropriate ways."

To circumvent this issue, I construct a balanced sample based on SE designation before the reform, using only SE student shares before 1999. More specifically, my short-run analysis sample is restricted to students who had been in SE before 1999 and stayed in Texas public schools for all six years across their 3rd–8th grades, when they are required to take non-exit level TAAS. Furthermore, I use the school-level SE shares averaged between 1996 and 1998, before the 1999 reform was implemented. The distribution of the constructed measure is displayed in Figure 5. Students whose schools were not rated by the accountability system are excluded from the analysis sample. I test less restrictive specifications later in the robustness check section.

I compare changes in TAAS participation rates and scores of SE students between schools with different prior shares of SE students around 1999. This difference-in-differences framework examines whether the sharp accountability shock in 1999 incurred manipulative behaviors of excluding low-performing students from the testing pool as the literature suggests. I also examine test score changes to check whether SE students benefited from the accountability pressure like many studies have reported for general students. I focus on reading and math tests because they were the two subjects that were administered every grade under accountability.

I use this sample to run the following difference-in-differences regression analysis:

$$Y_{ist} = \alpha + \beta share_s^{pre} \times Post_t + f(X_{ist}) + \gamma_s + \tau_t + \epsilon_{ist}, \tag{1}$$

where Y_{ist} is a short-run outcome of student i at year t, $share_s^{pre}$ is a school-level, time-invariant prior share of SE students of school s at year t, and $Post_t$ is an indicator variable that turns on if $t \geq 1999$. $f(X_{ist})$ represents student-level and school-level controls such as race, age, gender, FRL status, English proficiency status, county median income, and unemployment rates. I also include a school fixed effect γ_s and a year fixed effect τ_t. Standard errors are clustered at the school level. The coefficient of interest, β, captures the difference

in changes in outcome Y_{ist} between a school with no SE students ($share = 0$) and a school with only SE students ($share = 1$) after the 1999 reform. In the rest of this paper, I divide the estimated coefficients by 10 and interpret it as a treatment effect per additional 10 percentage points of the share of SE students within a school.

The validity of Equation 1 relies on a critical assumption that without the 1999 reform, the trends of outcomes would have evolved parallel across different prior levels of SE student shares, conditional on other control variables and fixed effects. While this assumption is innately difficult to test directly, I use the following event study framework to support my previous specification:

$$Y_{ist} = \alpha + \sum_{k \neq 1998} \beta_k Share_s^{pre} \times Year_k(t) + f(X_{ist}) + \gamma_s + \tau_t + \epsilon_{ist}, \tag{2}$$

where a series of coefficients β_k captures changes in outcome relative to the reference year 1998 across different levels of prior SE student shares. All other regression components are identical to Equation 1. β_k estimated close to zero for $k < 1999$ supports my empirical strategy, suggesting that outcomes trended parallel across different SE share levels before the treatment.

Another question this study seeks to answer is whether increased test exemption due to the new accountability pressure, if it existed, was associated with "cream-skimming," selectively dropping low-performing students from their testing pool to inflate their aggregate pass rates. To answer this question, I estimate a heterogeneous effect model interacted with past test scores of each student:

$$\begin{aligned} Y_{ist} = \alpha + \beta_1 PrevScore_{it-1} \times Share_s^{pre} \times Post_t + \beta_2 PrevScore_{it-1} \times Post_t \\ + \beta_3 Share_s^{pre} \times Post_t + \theta PrevScore_{it-1} + f(X_{ist}) + \gamma_s + \tau_t + \epsilon_{ist}, \end{aligned} \tag{3}$$

In Equation 3, $PrevScore_{it-1}$ indicates average past normalized TAAS scores of student i up to year $t-1$. To address the endogeneity issue past 1999, I stop updating $PrevScore_{it-1}$

after 1999.[23] The coefficient of interest, β_1, thus, estimates the additional treatment effect of a student with a 1 standard deviation higher past score compared to a student with a lower past test score. Similar to the base model, I supplement this with a corresponding event study model:

$$\begin{aligned} Y_{ist} = \alpha + \sum_{k \neq 1998} \beta_{1k} PrevScore_{ik-1} \times Share_s^{pre} \times Year_k(t) \\ + \sum_{k \neq 1998} \beta_{2k} PrevScore_{ik-1} \times Year_k(t) + \sum_{k \neq 1998} \beta_{3k} Share_s^{pre} \times Year_k(t) \\ + \theta PrevScore_{it-1} + f(X_{ist}) + \gamma_s + \tau_t + \epsilon_{ist}, \end{aligned} \tag{4}$$

Likewise, β_{1t} shows the trend of additional treatment effects associated with past test scores better by 1 standard deviation.

4.2 Long-run Analysis

My long-run analysis focuses on the impacts of accountability pressure on outcomes such as high school and college graduation and earnings in adulthood. I cannot use the same sample as the short-run analysis here because such long-term events happen only once at maximum to each individual. Instead, I exploit differential treatment across ninth-grade cohorts around 1999. Recall that Texas students take high-stakes exams only up to Grade 10. This means that students could have been directly subject to the accountability pressure only by Grade 10. Therefore, a 9th grader in 1998 was expected to enter Grade 10 in 1999, having one year of treatment by the 1999 reform. On the other hand, a similar student in 1997 would be entering Grade 10 in 1998, not directly affected by the reform.

Therefore, I compare outcomes of ninth-grade cohorts who were in SE in their eighth grades in 1994–2002, using initial shares of SE students of high schools they were enrolled in as the main variation.[24] The analysis sample consists of 293,739 ninth-grade students in

[23] For example, an eighth-grade student in 2001 will have an average of 3rd-5th-grade TAAS scores as *PrevScore*. Students who do not have an eligible test history are excluded from the sample.

[24] Note that I allow students in the sample to acquire SE status after 1999, unlike Ballis and Heath (2021),

SE from nine years of cohorts. Like the previous section, the sample does not include high schools not rated by the accountability system.

I estimate the following regression for the long-run analysis:

$$Y_{ist} = \alpha + \beta Share_t^{pre} \times Expose_t + f(X_{ist}) + \gamma_s + \tau_t + \epsilon_{ist}, \tag{5}$$

Here, Y_{ist} is a long-run outcome of a ninth-grade student who was i in a high school s in year t. $Expose_t$ equals one if the ninth-grade cohort t was expected to spend a positive number of years under the 1999 reform by 10th grade ($t \geq 1998$). I include a school fixed effect γ_s and cohort fixed effect τ_t to control for cohort-invariant school and school-invariant cohort characteristics, respectively. $f(X_{ist})$ includes student- and school-level control variables similar to the short-run analysis. The coefficient of interest, β, estimates the effect of exposure to the 1999 reform on student long-run outcomes, compared between schools with no SE students and schools with only SE students. To examine the validity of this specification, I run the following event study regression as well, similar to the previous short-run case:

$$Y_{ist} = \alpha + \sum_{k \neq 1997} \beta_t Share_s^{pre} \times Expose_k(t) + f(X_{ist}) + \gamma_s + \tau_t + \epsilon_{ist}, \tag{6}$$

where β_t illustrates the changes in differences of long-term outcomes relative to the reference cohort that was expected to enter Grade 9 in 1997. This cohort becomes a reference cohort because the next ninth-grade cohort in 1998 was expected to be Grade 10 in 1999. Thus, the coefficients of interest β_t estimate the dynamic cumulative treatment effects from the 1999 reform on long-term outcomes, which Equation 5 does not cover. Other parts of the specification are identical.

due to the lack of pre-period cohorts. I test whether this becomes a problem later in the robustness check section. The results are all robust to the sensitivity analysis.

5 Main Results

5.1 Short-run Analysis

5.1.1 Test Score

Figures 6.(a) and (b) illustrate raw data trends of TAAS scores of Texas SE students from 1996 to 2002. Each plot represents an average outcome separately by two groups with different treatment intensities. I define the high-share group as students enrolled in the top 25% of schools in terms of initial SE student shares and the low-share group as those in the bottom 25%. The TAAS scores are all normalized to have a mean of 0 with a standard deviation of 1. Both groups' TAAS scores trended similarly before 1999 but exhibited very different responses in 1999 following the implementation of the accountability reform.[25] High-share schools showed much more significant improvements in the average test scores of SE students. This pattern was identical for both reading and math tests. Such clearly heterogeneous paths of test scores provide evidence that initial shares of SE students successfully capture variation in the treatment intensities of the 1999 reform, supporting this paper's difference-in-differences framework.[26]

Panels (a) and (b) of Figure 7 plot the regression estimates based on Equation 2. I plot the estimated coefficients and 95% confidence intervals of $Share_s^{pre} \times Year_k(t)$, which are β_ks. Event study results show a pattern analogous to the previous raw data plots of Figure 6. Both panels (a) and (b) indicate that SE students experienced drastic improvements in math and reading test scores after the 1999 reform, which began holding schools accountable for the test scores of SE students. Such short-run improvements in test scores are commonly observed regardless of the grades that SE students were in when the reform was introduced

[25] It is notable that there are considerable differences in average test scores before 1999. Appendix Table A1 describes summary statistics of both high- and low-share schools. Overall average statistics show that such differences between the two groups are not confined to SE students. This implies that students in low-share schools generally perform better than those in high-share schools.

[26] This assumes that there are no other confounding factors correlated with both SE share measures and outcome variables. Extensive investigation on the Texas accountability and education systems finds no such a confounder in this period.

(see Appendix Figure A.3). While there are some signs of significant pre-trends before 1999 that potentially violate the parallel trends assumption, their sizes are relatively small and show the same direction as the estimated treatment effects.

Columns 1 and 3 of Table 2 report estimates of the same outcome after pooling all post-treatment years for the average effects of the 1999 reform, based on Equation 1. They indicate that there were approximately 0.1 standard deviations gain in reading and math test scores per 10 percentage points of SE shares within schools. These estimates imply that schools in the top quartile in terms of the SE shares achieved around 0.057 standard deviations more gains in both test scores compared to schools in the bottom quartile. While these impacts on test scores were all large and significant, the fact that the gains were observed right after the 1999 reform with little further improvements afterward makes it difficult to conclude that they were "real" gains. Thus, I examine the potential existence of compositional effects from changes in SE test exemptions in the next subsection.

5.1.2 Test Participation

Figure 8 displays raw data trends of average test participation rates of SE students from 1996 to 2002, separately reported between high-share and low-share schools. Interestingly, trends of test rates follow a pattern exactly opposite to that of test scores. SE students became much less likely to get tested after the 1999 reform, where students in high-share schools experienced more significant decreases in test rates. Both reading and math tests showed similar declines in test participation of SE students.

Figure 9 presents event study results from Equation 2 on test participation of SE students. Estimated coefficients follow similar patterns shown in the previous raw data plots. Increased accountability pressure led to significantly lower test rates for SE students, with effect sizes getting larger over the years. These sharp decreases in test rates in 1999 were again significant regardless of students' grade cohorts within the analysis sample, though older cohorts showed larger impacts (see Appendix Figure A.4). The impacts were identical between reading and

math tests. Columns 1, 3, and 5 of Table 3 show corresponding point estimates from Equation 1. They indicate that increased accountability pressure led to 7.34 and 7.01 percentage points lower participation rates in TAAS reading and math tests, respectively, per 10 percentage points additional SE shares. These were significant exclusions from testing, which were 12%–13% drops from the pre-policy means.

Given that schools had considerable discretion over exempting SE students from testing, this drastic fall in the test rate was almost certainly due to increased test exemptions for SE students after the 1999 reform.[27] Nevertheless, it is challenging to verify whether schools intended these increases in exemptions to protect their accountability ratings because such decision-making was usually carried out implicitly or in secret. One way to indirectly test the claim is to find out which students are getting excluded first. If schools were indeed trying to inflate ratings by giving test exemptions to SE students, it would be an optimal strategy for them to exclude low-performing students and retain high-performing ones in the testing pool, making the largest ex-ante average pass rate gains with the fewest exemptions made.

Figure 10 illustrates estimates on test participation of SE students from Equation 4 and provides evidence that schools were strictly engaging in such a cream-skimming behavior. Sharp jumps in estimated coefficients in 1999 imply that SE students with higher past test scores were more likely to get tested. In other words, SE students with lower past test scores became less likely to get tested after the reform.[28] Similar to the previous outcomes, these effects were identical across test subjects and different grade cohorts in my sample (see Appendix Figure A.5). Columns 2, 4, and 6 of Table 3 report corresponding pooled estimates from Equation 3. Per 10 percentage points in the initial SE shares in schools,

[27]State-level TAAS participation statistics provided by the AEIS reports strongly support this claim (https://rptsvr1.tea.texas.gov/perfreport/aeis/99/part/state.html). Though there is no separate information for SE students, the number of ARD exemptions, which were dedicated to SE students spiked in 1999. Other channels, such as absence and Limited English Proficiency (LEP) exemptions, remained stable in the same period.

[28]Since the normalized test scores of SE students were mostly negative, most SE students were estimated to experience decreases in test rates unless they scored top 10% among tested SE students.

having 1 standard deviation lower past scores made the student 4.44 and 3.77 percentage points less likely to get tested after the reform. This suggests that schools selectively excluded students they thought were less likely to pass future tests. For example, a back-of-envelope calculation implies that a student in the bottom quartile became 36% less likely to get tested. In comparison, one in the top quartile experienced a negligible 2% drop in the test rate at an average school.

5.1.3 TAAS Score with Student-level Fixed Effects

Here, I return to the effects of accountability pressure on the test scores of SE students. Since previous results suggest that schools actively manipulated their testing pools by excluding low-performing SE students from testing, one natural question is how much of the improvements in test scores in Figure 7 were from actual gains in student achievements rather than from changes in compositions of tested students. For example, schools could still attempt to improve the education of some high-performing SE students if they were considered "promising" and, thus, kept getting tested, similar to what Neal and Schanzenbach (2010) showed. To examine this issue, I add student-level fixed effects to Equations 1 and 2 to estimate within-student treatment effects, addressing potential compositional effects.

Figure 11 presents the event study estimates on test scores from Equation 2, with student-level fixed effects added. It indicates that there was zero actual gain in both reading and math test scores, contrary to what Figure 7 suggested. Math scores even show a slight sign of deterioration in later years. Columns 2 and 4 of Table 2 report corresponding point coefficient estimates. Both estimates are statistically insignificant. These results imply that schools entirely relied on the exclusion of low-performing SE students to address the accountability pressure from the 1999 reform without efforts to improve the accomplishments of SE students as the reform initially intended.

5.1.4 Heterogeneity Analyses

Urban vs. Rural Districts One potential factor that could affect the degree of strategic responses to the accountability pressure is the competition schools face. Chakrabarti (2014) indicated that schools in more competitive environments show greater improvements in test scores. I test whether even manipulative behaviors shown in this study follow the same pattern, comparing urban and rural districts to proxy the extent of competition (Gibbons and Silva, 2008; van Maarseveen, 2021). I use district types defined by the TEA by the number of populations within districts.[29] Appendix Figure A.6 illustrates the geographical distribution of rural and urban districts by this definition, showing both narrow and broad definitions of rural districts.

Using this categorization, I examine the heterogeneous effects of the accountability pressure on test participation by district types. Appendix Figure A.7 presents the event study results from Equation 2. Though they share similar pre-policy trends, urban and rural districts exhibit significant heterogeneity after the reform. Exclusions of SE students were much more prevalent in urban districts than in rural districts. Furthermore, using a narrower definition of rural districts yielded starker heterogeneity between the two groups. This provides suggestive evidence that schools in more competitive neighborhoods responded much more actively to the accountability pressure.

School Performance Multiple studies have indicated that schools with poor previous performance that are more susceptible to sanctions from accountability systems are more sensitive to accountability pressure (Figlio and Rouse, 2006; Deming et al., 2016; Cilliers et al., 2021). I explore this heterogeneity by comparing the degree of exclusions in previously high-performing and low-performing schools. I define high- and low-performing schools by two measures: school-level average test scores and accountability ratings they previously received. To address potential endogeneity problems, I use these measures up to 1998 and

[29] See https://tea.texas.gov/reports-and-data/school-data/district-type-data-search/district-type-2020-21 for details of the district type definitions.

stop updating after that.

Appendix Figure A.8 reports the estimated heterogeneity. Consistent with the literature, previously low-performing schools were more likely to exclude SE students after the 1999 reform. Schools in the top quartile in terms of average test scores made 2–3 times stronger responses compared to the schools in the bottom quartile, and this gap widened in the later years. On the other hand, heterogeneity by previous rating was not as significant. These results imply that the threat of penalties from the accountability system was a substantial driver of school responses.

School vs. District Incentives So far, I have presumed that schools were responsible for their strategic behaviors toward SE students. This is because ARD committees consist of teachers of individual schools and parents, and thus, most decisions on the education of SE students were made at the school levels. However, district leadership could have exerted influence on its schools to inflate aggregate pass rates by excluding SE students. This is especially likely as the Texas accountability system published accountability ratings at both district and school levels, with similar rewards and sanctions at stake. Qualitative studies based on interviews with Texas teachers in the 2000s attest to the existence of such pressure from district leadership as well (Nagle et al., 2006; Ramzinski, 2019).

I investigate this possibility by examining to which level of variation–school or district–the degree of exclusion was more sensitive. This analysis exploits the fact that school-level SE shares and aggregate district-level SE shares were often very different, though they were obviously correlated with each other.[30] Therefore, a school with a relatively low SE share could have been forced to exclude SE students from testing if its district had a high overall SE share. Appendix Figure A.9 shows event study estimates from Equation 2, using both school-level baseline SE shares and district-level SE shares as the primary identifying variations. Estimated heterogeneity is evident: The degree of SE exclusion was more sensitive to district-

[30]The coefficient of correlation was $\rho = 0.67$, but more than 10% of schools had gaps between school-level and district-level SE shares larger than 5 percentage points. This was a significant difference, as the average pre-policy SE share was around 13%.

level incentives than school-level incentives, which suggests that district leadership was more responsible for the exclusion of SE students than local schools and their teachers.

5.2 Long-run Analysis

Many studies have used test scores to examine the effect of school accountability systems on student achievements. However, my short-run analysis demonstrates that schools attempted to manipulate their testing pools to inflate their aggregate test pass rates, which makes test outcomes unreliable measures of student accomplishments. Thus, in this section, I focus on the effects of accountability pressure on the long-term outcomes of SE students to explore the true impacts of accountability pressure on SE students.

5.2.1 Exclusion in High Schools

First, I examine how increased accountability pressure from the 1999 reform affected the long-run outcomes of SE students. Figure 12 presents the estimates of the effects on two high school outcomes from Equation 6: exit exam participation and whether they reached 10th grade when they were supposed to take the exit exam. Results are separately reported between high- and low-performing students based on their past test scores up to Grade 8. Panel (a) shows that the exclusion of SE students from testing happened in high school as well. Exit-level TAAS participation rates of SE students with poor past test scores decreased by 5–10 percentage points per 10 percentage points of pre-policy SE shares. Panel (b) indicates an even more extreme form of exclusion. Low-performing ninth-grade students became 2.5 percentage points more likely to drop out before 10th grade per 10 percentage points of SE shares.

These increased dropouts, which could be interpreted as exclusion from schools themselves, were due to the uniqueness of high school exit-level exams. First, unlike other TAAS exams in the 3rd–8th grades, the 10th-grade exit exam was the only high-stakes exam that fed into the accountability ratings of high schools. Second, SE students in Texas could legally

drop out from Grade 9. This meant high schools were under much heavier accountability pressure, with an additional means of exclusion other than giving test exemptions. It could still be hard to believe that schools intentionally encouraged dropouts to stop low-performing SE students from getting tested when they still could exempt them from testing. However, a very similar pattern was already observed among general education students in Texas before 1999 by Haney (2000), which was a highly controversial report. He found that many low-performing high school students "disappeared" from schools before 10th grade, even without corresponding dropout records. He suspected that this resulted from strategic actions by schools to manipulate their exit-exam testing pools, similar to what I found in this study. I also find no sign of exclusion from official dropout data (see Appendix Figure A.10).

5.2.2 Educational Outcomes

Figure 13 shows the estimated effects on educational outcomes of SE students from Equation 6. Panel a, b, and c present effects on high school graduation, college enrollment, and college completion, respectively. Table 4 reports corresponding regression estimates from Equation 5. The estimates indicate that increased accountability pressure from the 1999 reform led to a 0.73 percentage-point decrease in high school graduation or a 1.2% decrease compared to the pre-policy mean per 10 percentage points of initial high school SE shares. Effects on college enrollment and completion were statistically insignificant.

This small impact on college outcomes, compared to the effect on high school graduation, could be because only a few SE students enter and complete college education: Only 32% of ninth-grade SE students in 1998 eventually entered colleges, and less than 7% completed their programs. In addition, as my previous results have suggested, the negative impact of accountability pressure was concentrated on low-performing SE students, who were even more unlikely to attend college in the future. Figure 14 and Table 5 present the same college outcomes as Figure 13 and Table 4, separated by types of institutions. As expected from the fact that most SE students enter two-year colleges, the outcome of two-year colleges drives a

moderate decrease in college completion: a 5.9% decrease compared to the pre-policy mean. Both effects on high school graduation and college completion get larger as the number of years of exposure increases.

5.2.3 Labor Market Outcomes

Figure 15 and Table 6 present effects on labor market outcomes from age 25 to 29. Both exhibit that increased accountability pressure on SE students inflicted negative impacts on their outcomes. Ten percentage points of additional SE shares were associated with 309.4 dollars and 1.2 percentage points decreases in earning and employment in adulthood, respectively. These estimates were equivalent to 2.8% and 1.9% reductions compared to the pre-policy mean. These imply that impacts on the adulthood income were mostly in extensive margins, and log annual earnings conditional on employment do not show any significant impacts (see Appendix Figure A.11).

One common pattern of long-term impacts so far, especially on high school graduation and labor market outcomes, is that a large portion of the overall effect is observed right from the first year of exposure. This implies that students' exposure to the reform in 10th grade was a critical determinant of their long-term outcomes. Previously shown high school exclusions provide a good potential mechanism to support these results. An immediate decrease in exit-level exam participation and an increase in dropout rates of prospective 10th-grade cohorts could make them less likely to graduate high schools, with consequential adverse effects on labor market outcomes. Cumulative effects of additional exposure to the reform are also observable through downward trends of event study estimates.

5.2.4 Heterogeneity Analyses

Next, I examine how the heterogeneous treatments in short-run exclusions in schools are reflected by the long-term outcomes I have covered so far. More specifically, I focus on time-invariant variations associated with drastic differences in the degree of exclusions.

I showed that the rate of exclusion SE students faced in schools critically depended on their past performance in high-stakes tests (Figures 10 and 12). SE students who did well in the past tests were less likely to be exempted from tests and less likely to drop out than students who did not. I examine how effects on long-term outcomes differ between these two groups of SE students. I use past test scores up to Grade 8 of ninth-grade SE students.

Appendix Figure A.12 shows the results of the subsample analyses. It indicates that negative impacts on the long-term outcomes of SE students were mainly driven by low-performing students who had poor past test scores before entering high schools. While students with lower prior TAAS scores suffered from large adverse effects from increased accountability pressure on almost all outcomes, those with higher past scores showed null effects. This implies that the deterioration in long-term outcomes witnessed before is likely a consequence of the exclusions in schools.

Appendix Figure A.13 presents effects on long-term outcomes by district types. SE students in urban districts mainly drove adverse impacts, consistent with the prior results on short-run exclusions. Students in rural districts were mostly unaffected and even showed some positive estimates on college outcomes when I used broad definitions of rural districts. Estimates of rural districts under narrow definitions are highly imprecise due to the small size of the sample in the long-run analysis. Appendix Figure A.14 illustrates heterogeneous impacts on long-term outcomes by school- and district-level incentives. Similar to the previous exercise on short-run test rates, I compare results from school-level and district-level SE shares. It shows that effects on high school graduation and employment in adulthood were more significant under district-level incentives, while the same does not hold for college outcomes.

Overall, long-run heterogeneity analyses imply that adverse impacts on SE students' long-term outcomes were highly likely due to exclusions they had experienced in schools. Results indicate that student subgroups that were subject to more exclusions tend to suffer from more significant deterioration in the long run as well. Though some estimates become

largely imprecise due to smaller sample sizes, they provide strong evidence that increased accountability pressure eventually hurt SE students through strategic exclusions by schools.

5.3 Robustness Checks

In this section, I examine different empirical specifications to check the robustness of my short-run and long-run estimates. To test whether the restrictions in the short-run sample and variation drove the results, I re-estimate the short-run analyses on TAAS participation rates using a full unrestricted sample and all annual school-level shares of SE students between 1994 and 2002. For the long-run results, I address the concerns about differences in student qualities between cohorts by performing sensitivity analyses around the grades in which I pick the sample cohorts. The results are qualitatively robust to all these alternative specifications.

Columns 3 and 4 of Table 7 show estimates when I use a full sample of 3rd–8th-grade students who ever had SE status. Compared to the base results of Columns 1 and 2, the estimates are qualitatively identical, though the effect sizes tend to be smaller with the full sample. Columns 5 and 6 use all annual school-level shares of SE students, with similar results. Columns 7 and 8 use unrestricted samples and all annual shares, showing no notable differences. These results indicate that the restrictions in sample and treatment variation did not affect my short-run findings significantly.

One potential concern in my long-run analyses is that differences between sample cohorts could be driving the estimated effects. Because schools' incentives to refer students to SE status potentially became weaker after the 1999 reform, ninth-grade students in 2000 who got SE status in 1999 could be systematically different from those who got the status in 1998. This compositional effect could have driven the immediate drop in high school graduation and employment rates shown before. If schools somehow foresaw the 1999 reform in 1997, then eighth-grade students newly granted SE status in 1997 could be more likely to be genuinely disabled, with worse expected long-term outcomes compared to non-disabled students who

were strategically placed in SE. This difference would lead to a significant gap in outcomes of the 1998 and 1999 10th-grade cohorts. I test this hypothesis by changing the grades at which the sample ninth-graders were supposed to be in SE.

Figure 16 illustrates the long-run outcomes of Section 5.2 but using different cohort specifications of SE status. Blue coefficients are results based on ninth-grade cohorts who were in SE at their 7th grade, while green coefficients are those who were in SE in 9th grade. Red coefficients are the same as the base estimates of Section 5.2. The figure shows no difference across the three specifications, except for college enrollment, where all coefficients are nonetheless statistically insignificant. This shows that the aforementioned compositional effect across cohorts does not drive my long-term effect estimates.

Next, I address the potential selection problem from using variations in past test scores. While there was clear heterogeneity across students with different past score levels, a significant portion of SE students did not have any past test score histories. The heterogeneous effect model of Equations 3 and 4 only included 53% of the full short-run analysis sample, and even in the long-run sample of ninth-grade SE students, only 60% of students had previous test records.

To expand the external validity of my heterogeneity results on student abilities, I use disability types of SE students as a proxy of their academic abilities. This approach is more extensive than using past test scores because all SE students are assigned types of disabilities to receive SE benefits. Table 8 describes the fraction and test outcomes of the four most common disability types.[31] It is easily notable that there are significant variations in academic performance across disability types. SE students with speech impairments perform far better than other SE students. Those with learning disabilities are the most common but also show generally poorer outcomes.

First, I compare degrees of short-run exclusions between SE students with learning dis-

[31] Though test scores are again limited measures, they still provide good information on the overall abilities of disability groups. Appendix Table A2 presents long-term summary statistics across different disability types. They are largely consistent, except that students with emotional disturbances exhibit worse outcomes in the long run.

abilities and speech impairments, as proxies of low- and high-performing students. Panel (a) of Figure 17 shows the event study estimates of the subsample analysis between the two groups. The results are consistent with the initial conjecture. Students with learning disabilities were more likely to be relatively low-performing and experienced a much steeper decrease in test rates than those with speech impairment, who tended to be high-performing.

However, one caveat here is that the number of SE students with speech impairments declines fast in higher grades. Unlike in 3rd–8th grades, few SE students (less than 2%) in 9th grade had speech impairments. I compare the effects of accountability pressure between SE students with learning disabilities and those without learning disabilities to circumvent this issue for long-run heterogeneity. Figure 17 (b) illustrates the results and again indicates that students with learning disabilities were more likely to be excluded from testing, which reflects their poorer academic abilities. Figure 18 reports corresponding estimates of effects on long-term outcomes. Results are similar to the heterogeneity analysis using past test scores, as shown in Appendix Figure A.13. This implies that low-performing groups faced more exclusions in schools with consequential negative impacts.

6 Conclusion

School accountability systems have been one of the core elements of contemporary education in the U.S., supported by numerous studies indicating improvements in student achievements at schools. On the other hand, some have also suggested the existence of undesirable school responses, often excluding students who actually need the most resources. The lack of data and appropriate empirical settings prevented researchers from identifying how the accountability pressure affects those underperforming students in both the short and long run. Understanding the causal impacts of accountability pressure is necessary to devise better incentive designs, ensuring they properly incentivize schools to put forth their best efforts for their students.

In this paper, I exploited an accountability reform in Texas that specifically targeted disabled students in SE and incorporated their performance into the rating measures. Using extensive student-level administrative data from Texas public schools, I examined how short- and long-run student outcomes differ after the onset of the reform, across schools with varying initial shares of SE students. I found that the reform caused schools to intentionally drop students in SE from their testing pools, especially those with poor past high-stakes scores. This widespread exclusion led to negative impacts on their future long-run outcomes, such as less high school graduation and employment in adulthood. From my understanding, this is the first study to estimate both short- and long-run effects of accountability pressure on disadvantaged students using a sharp identifying policy variation.

While the empirical setting of this paper is based on a very early stage of the Texas accountability system, similar problems persist even now, 24 years past 1999. Significant reforms have been made to appropriately accommodate all sorts of students to the rating system. Alternative assessments dedicated to students with special needs began to be included in the system. The overall accountability system itself became much more complicated, with certain measures to "close the gap" for disadvantaged students. Nonetheless, tensions continue to exist between high-stakes accountability systems and schools, frequently accompanied by fierce controversy or even lawsuits.[32] Policymakers need to design both comprehensive and equitable accountability systems to provide the best incentives for their students' interests.

[32]For other media coverage I have not introduced, see https://www.texastribune.org/2023/10/27/texas-school-ratings-blocked-judge-ruling-tea/ and https://www.texasobserver.org/are-texas-miracle-graduation-rates-just-a-magic-trick/

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7 Figures and Tables

Figure 1: 1996 Texas School Accountability Manual

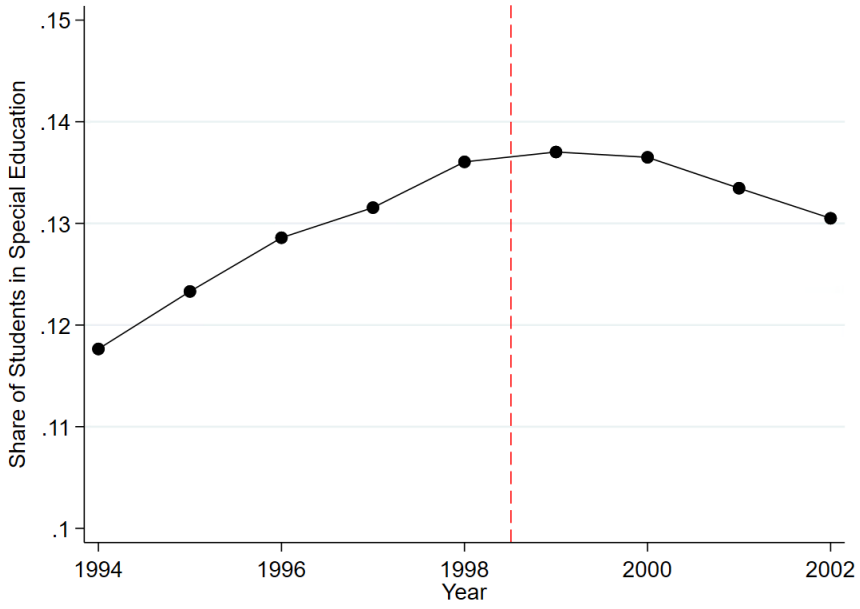
	Exemplary †	Recognized †	Academically Acceptable / Acceptable	Academically Unacceptable / Low-performing
Base Indicator Standards				
Spring '96 TAAS • Reading • Writing • Mathematics	at least 90.0% passing each subject area *(all students & each student group*)*	at least 70.0% passing each subject area *(all students & each student group*)*	at least 30.0% passing each subject area *(all students and each student group*)*	below 30.0% passing any subject area *(all students and each student group*)*
1994-95 Dropout Rate	1.0% or less *(all students and each student group*)*	3.5% or less *(all students and each student group*)*	6.0% or less *(all students and each student group*)* ‡	above 6.0% *(all students or any student group*)* ‡
1994-95 Attendance Rate	at least 94% (grades 1-12)☆	at least 94% (grades 1-12)☆	at least 94% (grades 1-12) ϕ	at least 94% (grades 1-12) ϕ

Notes: This figure shows a part of the 1996 Texas School Accountability Manual distributed to district and school personnel by the TEA. The full manuals for 2004–current year are available here: https://rptsvr1.tea.texas.gov/perfreport/account/.

Figure 2: TAAS Participation Trends, 1995–2002

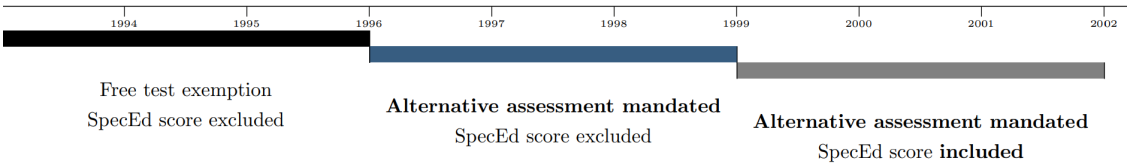
Notes: This figure illustrates trends of TAAS participation of SE students between 1995 and 2002, sourced from the Academic Excellence Indicator System (AEIS) school reports. The AEIS did not provide TAAS participation information separately for SE students in 1994. The blue line depicts shares of SE students who took TAAS each year, while the red line depicts shares of those who got exempted or took low-stakes SDAA. The sum of the two does not necessarily add up to 1 due to the existence of other minor categories, such as absence.

Figure 3: Trends of Special Education Student Shares, 1994–2002



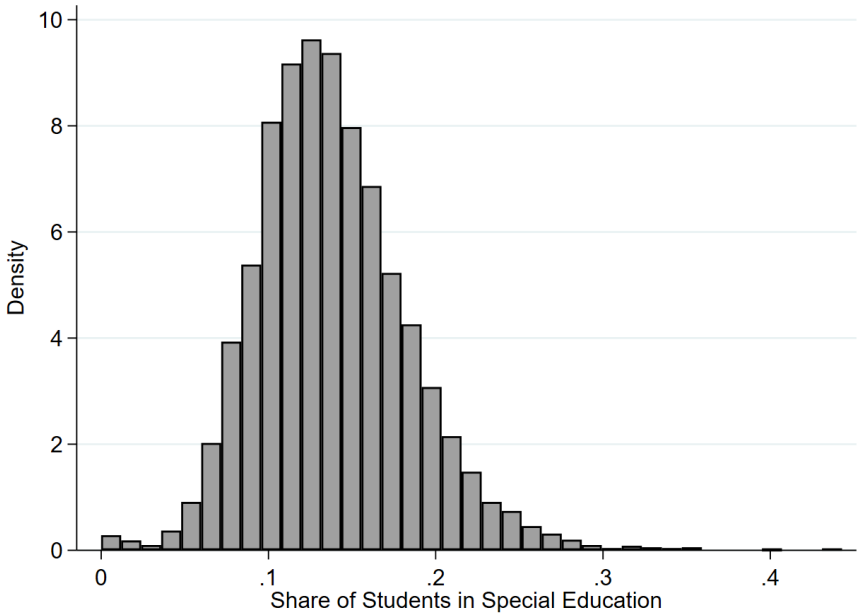
Notes: This figure presents the trend of state-level SE population shares in Texas public schools between 1994 and 2002.

Figure 4: Timeline of Special Education Assessment and Accountability in Texas



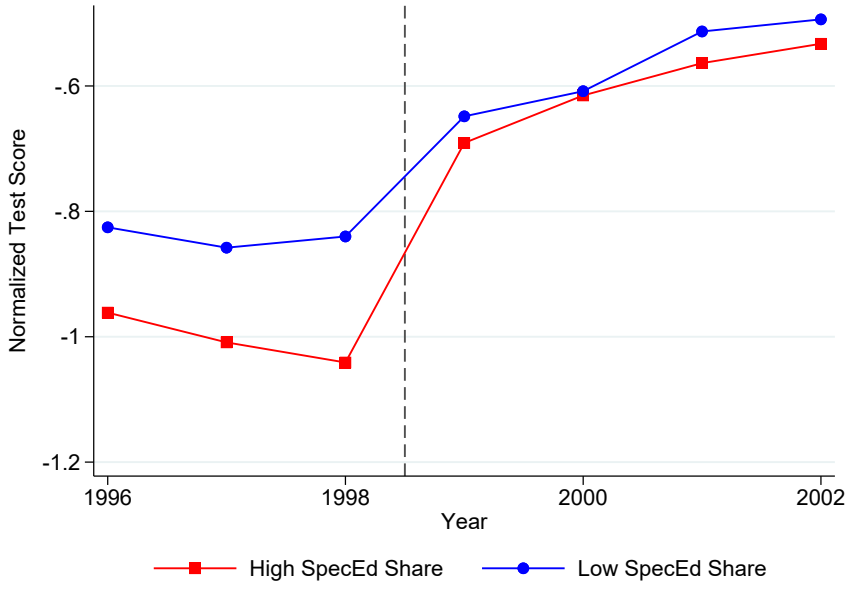
Notes: This figure depicts the two changes in Texas's SE assessment and accountability system between 1994 and 2002. This study focuses on the last two periods between 1996 and 2002, where alternative assessments for exempted students were mandated for all seven years. The accountability system was halted in 2003 for overhaul and resumed in 2004 with a new assessment (Texas Assessment of Knowledge and Skills, TAKS) and revised accountability provisions.

Figure 5: School-level Special Education Student Shares, 1998

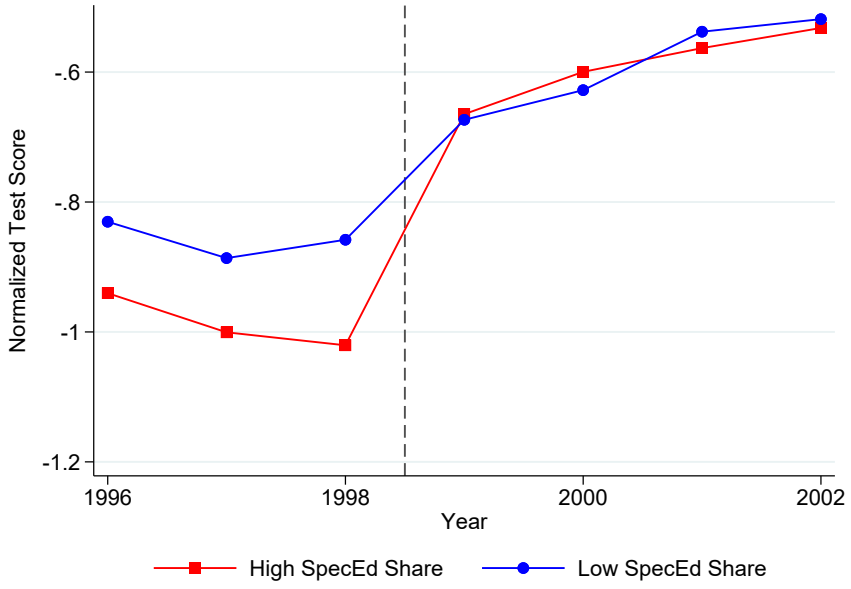


Notes: This figure shows the distribution of school-level SE student shares in 1996–1998 before the 1999 reform. The mean share was 0.138, with a standard deviation of 0.046. Shares were calculated using the student population in the accountability subset (Grade 3–8, 10). Schools not under the accountability system or with too small numbers of students (less than 30) were excluded.

Figure 6: Raw Data Plot: High Treatment Groups Showed Stronger Improvements in Average SE Test Scores



(a) Reading Score

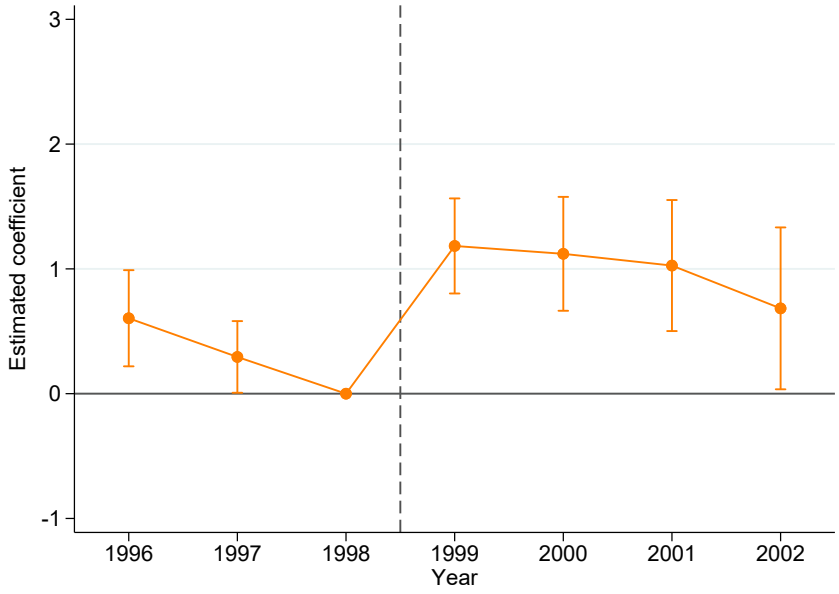


(b) Math Score

Notes: The figure illustrates raw data trends of TAAS reading and math scores of schools with high and low shares of SE students. I define "high share" schools as the top 25% schools in terms of SE shares and "low share" schools as the bottom 25%. The cutoffs of shares for the two groups were 16.5% and 10.7%, respectively. Test scores are normalized to have a mean of 0 with a standard deviation of 1 within each grade level, subject, and year.

Figure 7: Event Study: Accountability Pressure Increased Average Test Scores of SE Students

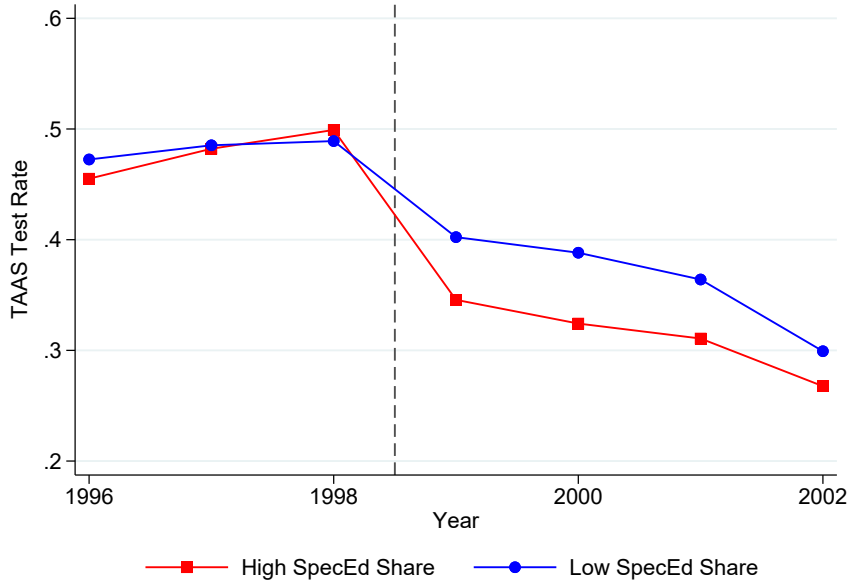
(a) Reading Score



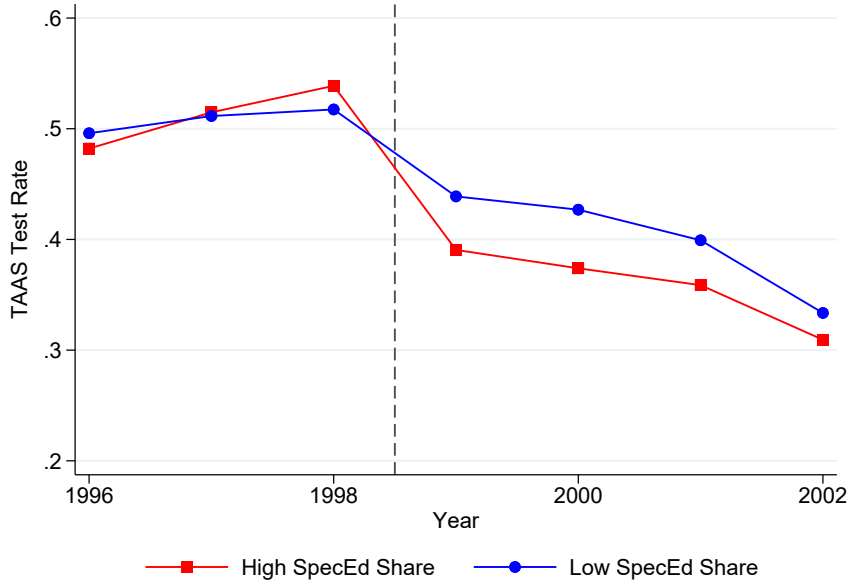
(b) Math Score

Notes: The figure plots event study estimates based on Equation 2. Panels (a) and (b) show the results using the balanced sample I described. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level. Test scores are normalized to have a mean of 0 with a standard deviation of 1 within each grade level, subject, and year.

Figure 8: Raw Data Plot: High Treatment Groups Showed Steeper Decreases in SE Test Participation



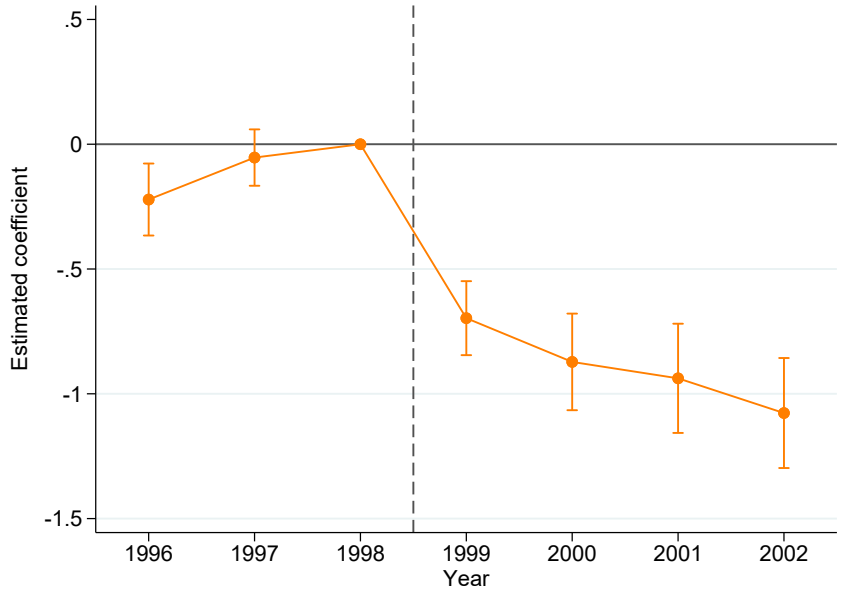
(a) Reading Test Participation



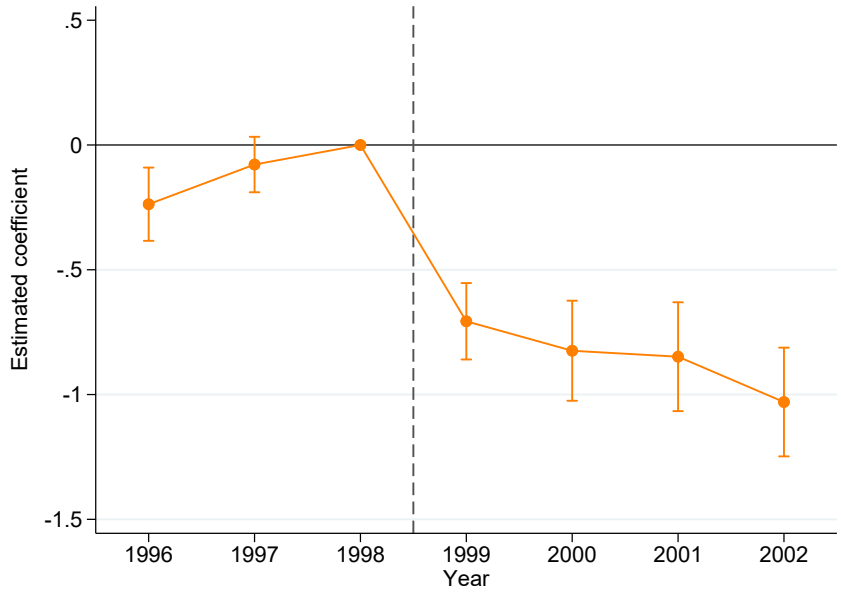
(b) Math Test Participation

Notes: The figure illustrates raw data trends of TAAS testing rates of schools with high and low shares of SE students. I define "high share" schools as the top 25% schools in terms of SE shares and "low share" schools as the bottom 25%. The cutoffs of shares for the two groups were 16.5% and 10.7%, respectively.

Figure 9: Event Study: Accountability Pressure Decreased Test Participation of SE Students



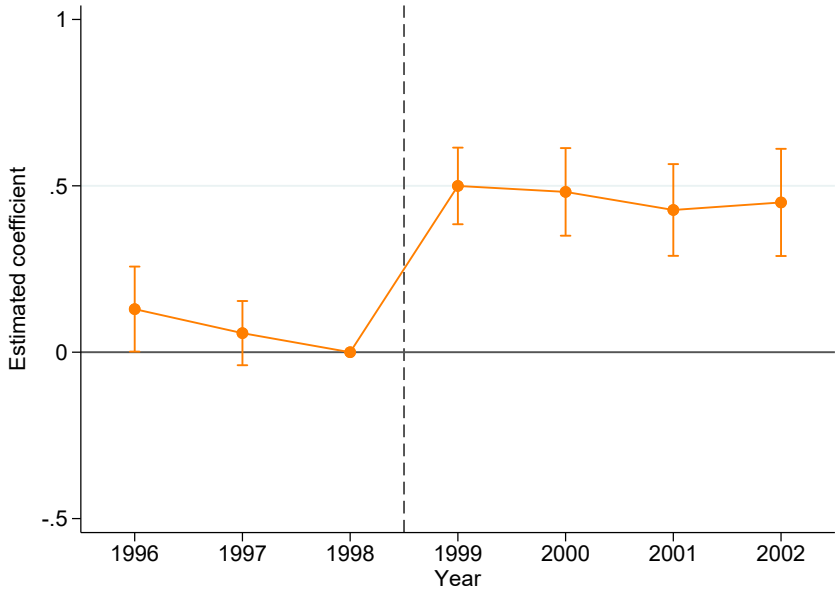
(a) Reading Test Participation



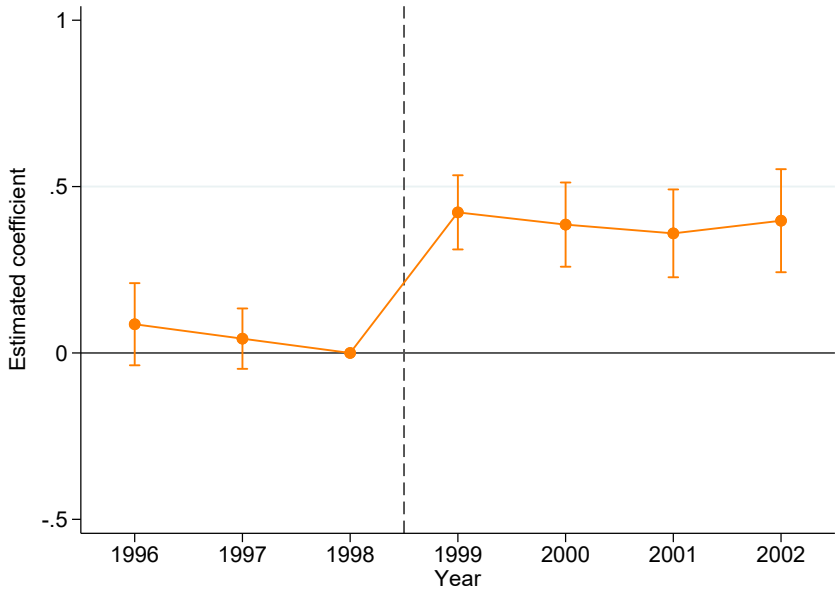
(b) Math Test Participation

Notes: The figure plots event study estimates based on Equation 2. Panels (a) and (b) show the results using the balanced sample I described. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

Figure 10: Event Study: Students with Lower Past Scores Were More Likely to Get Excluded



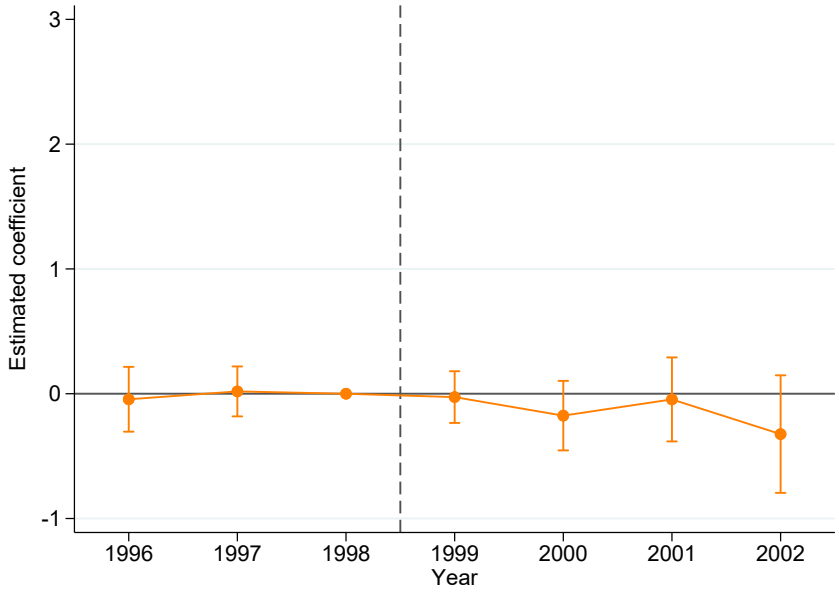
(a) Reading Test Participation



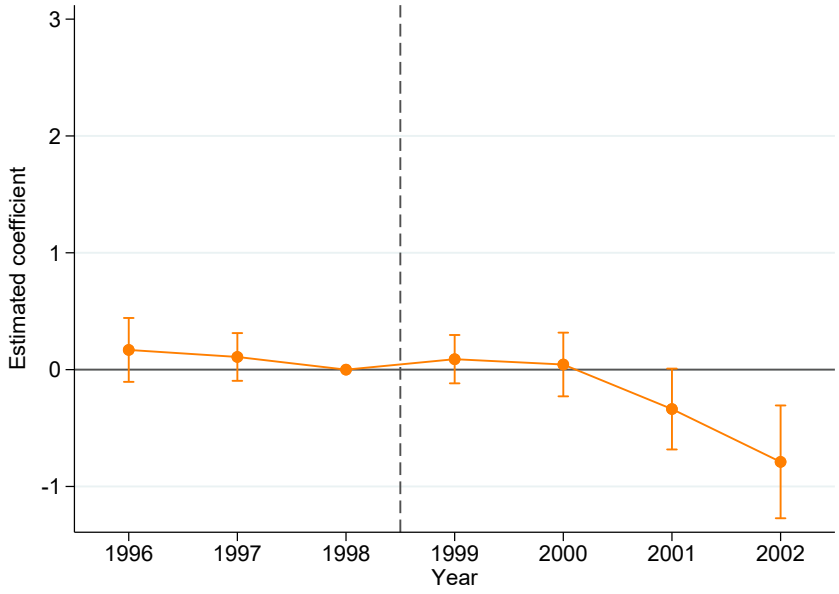
(b) Math Test Participation

Notes: The figure plots event study estimates based on Equation 4. Panels (a) and (b) show the results using the balanced sample I described. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

Figure 11: Event Study: Within-individual Comparison Shows No Actual Gain in Student Test Scores



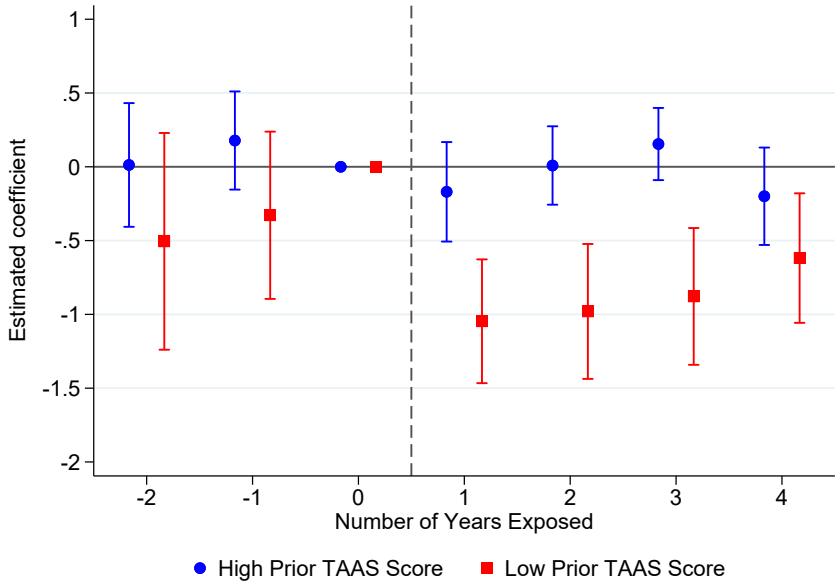
(a) Reading Score



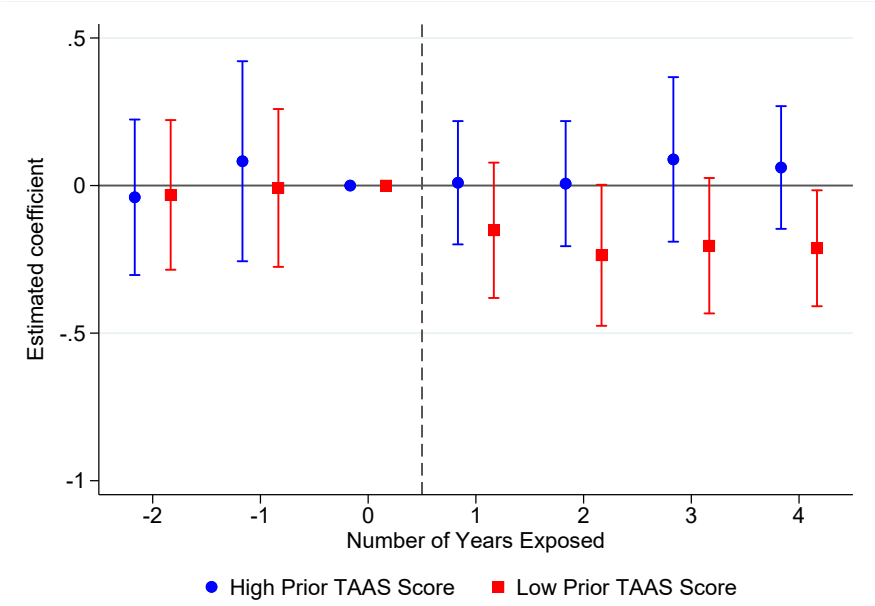
(b) Math Score

Notes: The figure plots event study estimates using a model that adds student-level fixed effects to Equation 2. Panels (a) and (b) show the results using the balanced sample I described. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level. Test scores are normalized to have a mean of 0 with a standard deviation of 1 within each grade level, subject, and year.

Figure 12: Event Study: Low-performing SE Students Faced Exclusions in High School, Including More Dropouts



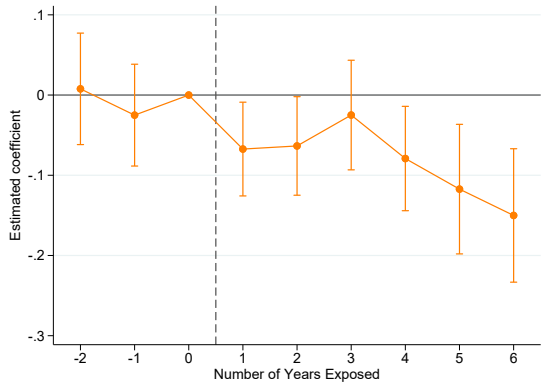
(a) Exit Exam Participation



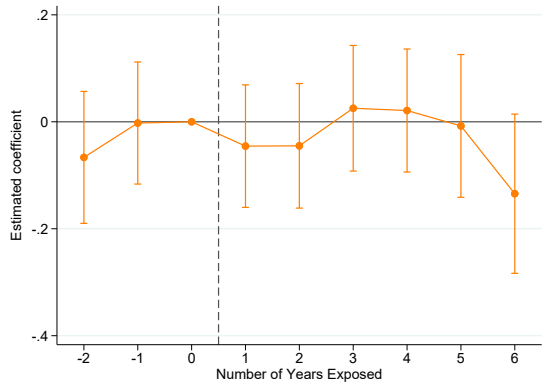
(b) Reaching 10th Grade

Notes: This figure shows long-run mechanism event study results based on Equation 6 by students' past TAAS scores. I define "high performance" groups as students in the top tertile in terms of their past TAAS scores and vice versa. I assume that a student took the exit-level TAAS if he took at least one subject of the exam. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at school levels.

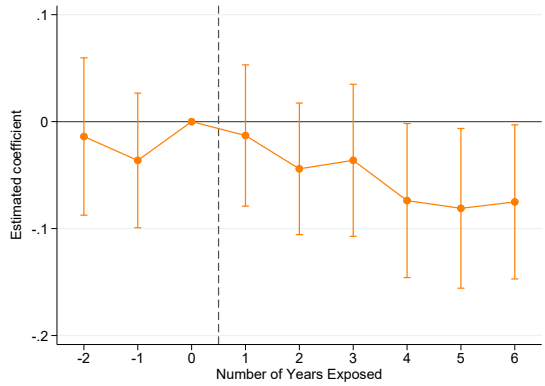
Figure 13: Event Study: The Accountability Pressure Negatively Affected Long-run Outcomes



(a) High School Graduation



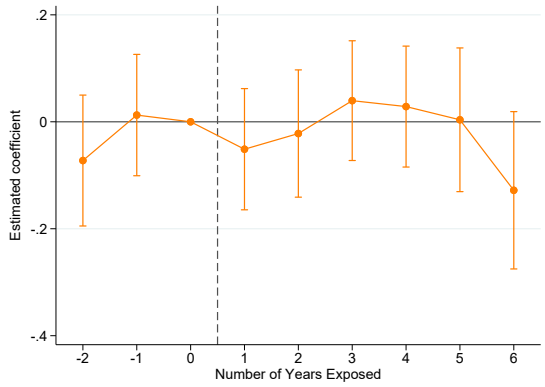
(b) College Enrollment



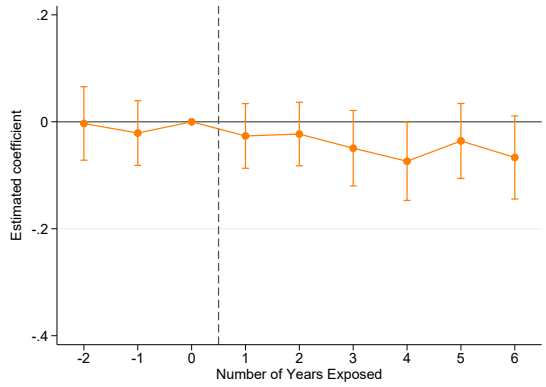
(c) College Degree

Notes: The figure plots event study estimates based on Equation 6. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

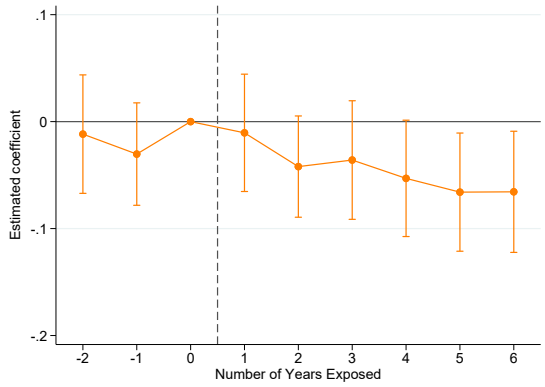
Figure 14: Event Study: College Outcomes Were Mainly Driven by Two-year Colleges



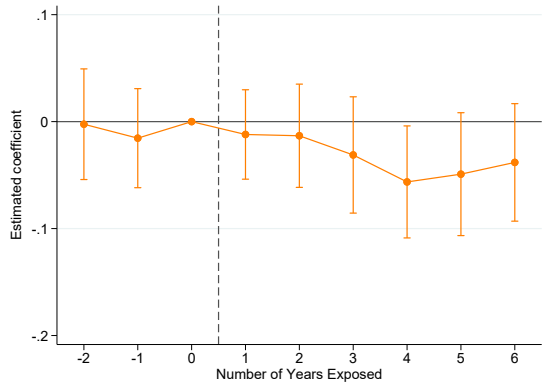
(a) College Enrollment, 2 Year



(b) College Enrollment, 4 Year



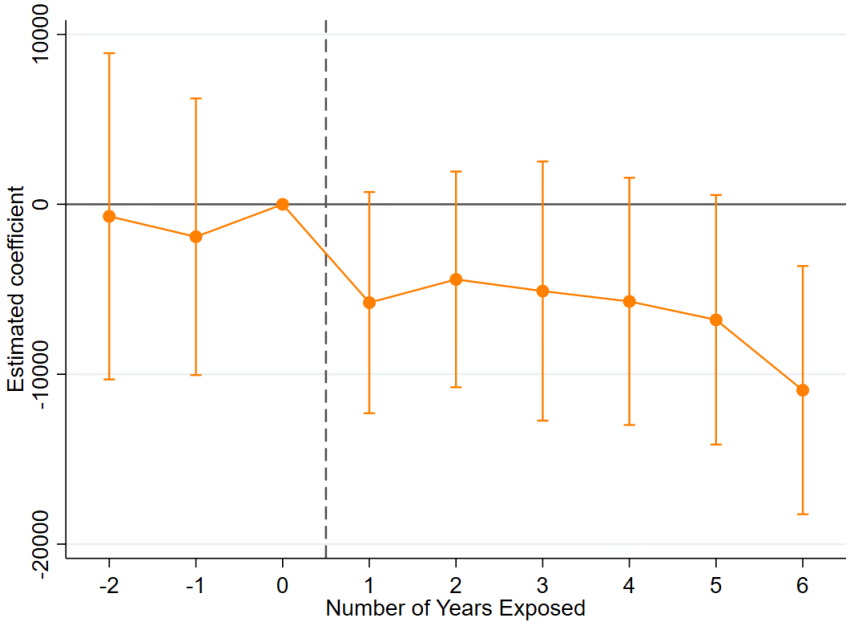
(c) College Degree, 2 Year



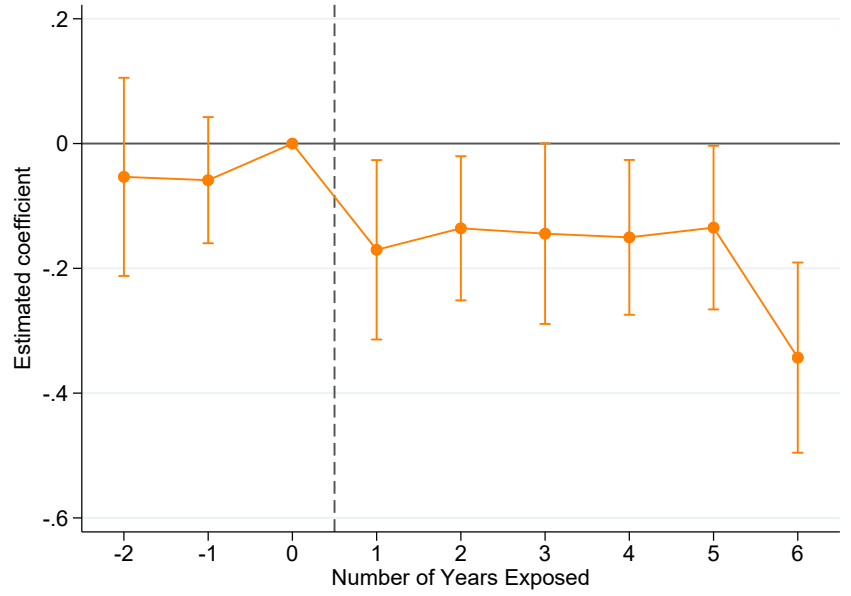
(d) College Degree, 4 Year

Notes: The figure plots event study estimates based on Equation 6. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

Figure 15: Event study: Wage Levels Decreased in Extensive Margins



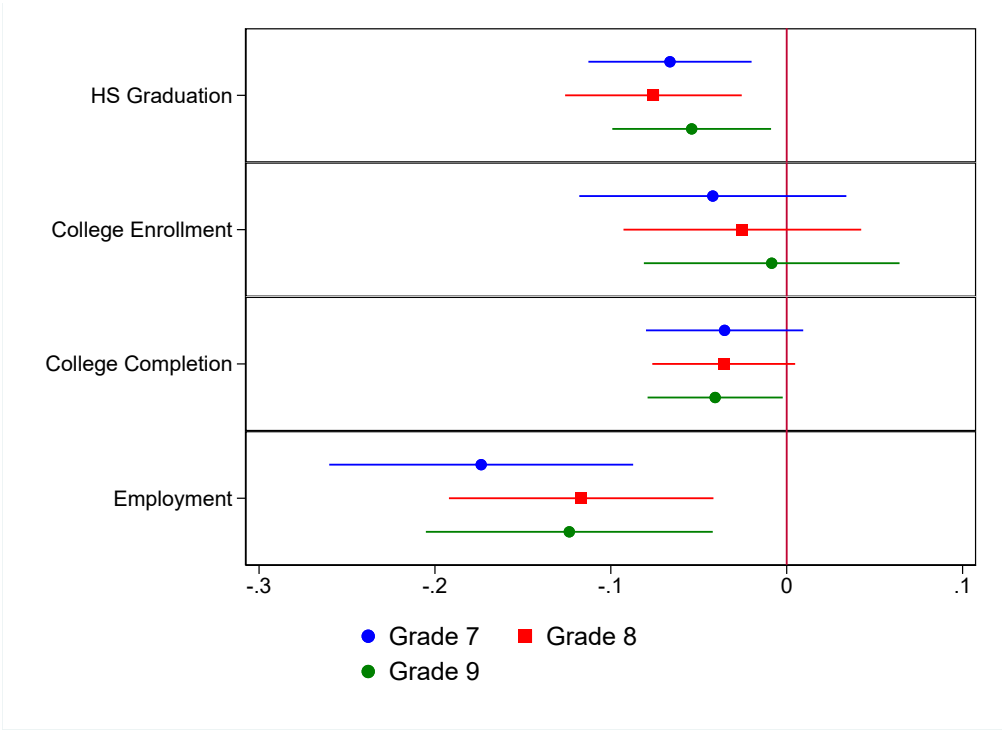
(a) Earning



(b) Employment

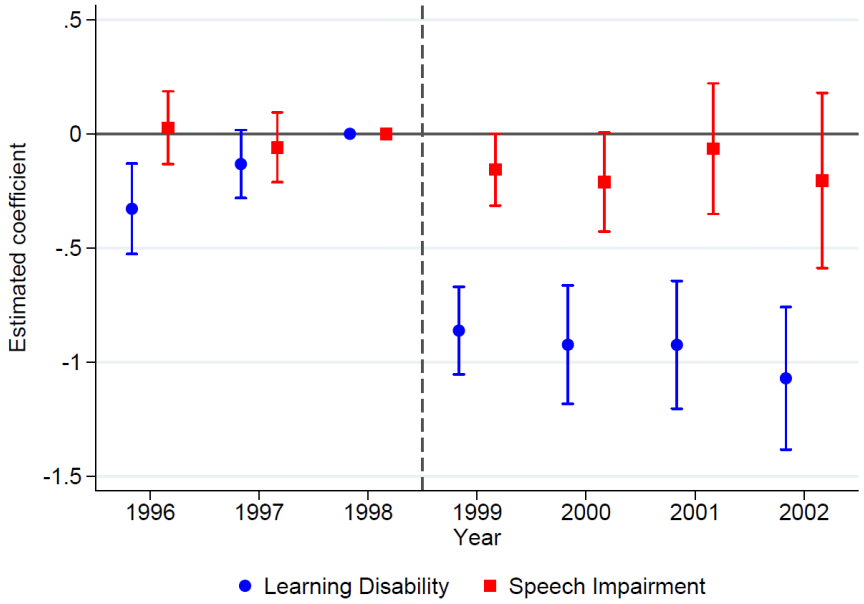
Notes: The figure plots event study estimates based on Equation 6. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level. All labor market outcomes are measured at the age between 25 and 29. Earnings include zero values.

Figure 16: Sensitivity Analysis: Timing of Special Education Status

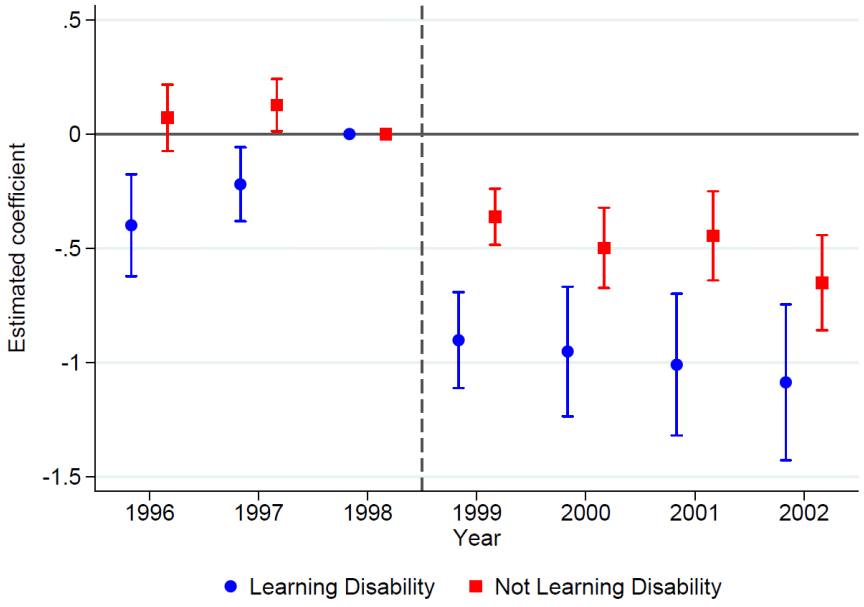


Notes: The figure depicts estimates of Equation 5 using three different sample specifications. The blue plots present regression estimates when I use ninth-grade students who were in special education in seventh grade. Similarly, the red and green plots present estimates based on students who were in special education in eighth and ninth grades, respectively. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

Figure 17: Event Study: Students With Learning Disabilities Experienced More Exclusions



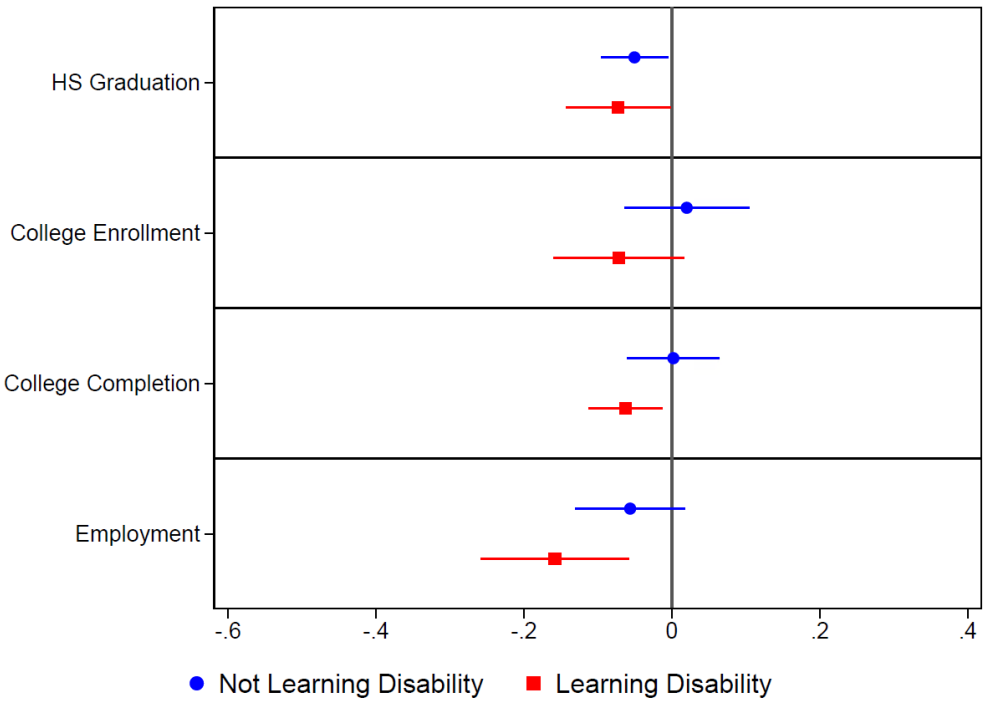
(a) Learning Disability vs. Speech Impairment



(b) Learning Disability vs. Others

Notes: This figure shows heterogeneous effect estimates on TAAS participation rates by disability types of SE students. Blue and red plots represent estimates from Equation 2. All disability types follow the categorization provided by the TEA special education data. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

Figure 18: Adverse Impacts on Long-term Outcomes Were More Severe on the Learning Disability Group



Notes: This figure shows heterogeneous effect estimates on long-term outcomes by disability types of SE students. Blue and red plots represent estimates from Equation 5. All disability types follow the categorization provided by the TEA special education data. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

Table 1: Summary Statistics – 8th Grade Cohorts Between 1996 and 2002

	General Education		Special Education	
	1996–1998	1999–2002	1996–1998	1999–2002
	(1)	(2)	(3)	(4)
Individual Characteristics				
Male	0.49	0.49	0.68	0.67
White	0.49	0.47	0.46	0.43
Black	0.13	0.13	0.18	0.19
Hispanic	0.36	0.37	0.35	0.37
Free/reduced-price lunch	0.41	0.42	0.56	0.58
Limited English proficiency	0.08	0.07	0.09	0.1
Educational Outcomes				
TAAS tested, reading	0.82	0.85	0.54	0.44
TAAS tested, math	0.82	0.85	0.53	0.42
Normalized score, reading	0.12	0.09	–1.07	–0.82
Normalized score, math	0.13	0.09	–1.12	–0.85
High school graduation	0.71	0.75	0.6	0.65
College enrollment	0.51	0.54	0.24	0.26
College enrollment, 4 year	0.25	0.26	0.05	0.05
College completion	0.22	0.24	0.06	0.06
College completion, 4 year	0.17	0.19	0.03	0.03
Labor Market Outcomes				
Annual income ($)	17,303	18,573	10,980	11,307
Employment	0.7	0.71	0.63	0.66
Rural district	0.14	0.13	0.17	0.16
Number of individuals	725,356	995,490	105,318	156,501

Notes: This table presents average individual characteristics, educational outcomes, and labor market outcomes of students in general and special education. I categorize students into general education unless they are specified as special education students in the data. Labor market outcomes are calculated between ages 25 and 29. The annual income measure includes unemployed individuals with zero earnings and is deflated using 2000 CPI.

Table 2: Short-run Effects on TAAS Scores

	Reading Score		Math Score	
	(1)	(2)	(3)	(4)
Share x Post	0.990***	–0.0631	0.951***	0.0234
	(0.176)	(0.102)	(0.181)	(0.105)
Individual FE	No	Yes	No	Yes
Observations	432,504	417,724	458,191	444,411
R-squared	0.266	0.807	0.266	0.829

Notes: This table presents estimated coefficients of Equation 1. Significance levels at 1%, 5%, and 10% are denoted by ***, **, and *, respectively. Standard errors are clustered at the school level. Test scores are normalized to have a mean of 0 with a standard deviation of 1 within each grade level, subject, and year.

Table 3: Short-run Effects on TAAS Participation Rates

	Tested		Tested, Reading		Tested, Math	
	(1)	(2)	(3)	(4)	(5)	(6)
Share × Post	–0.678***	–0.196***	–0.734***	–0.304***	–0.701***	–0.272***
	(0.0819)	(0.0640)	(0.0818)	(0.0665)	(0.0835)	(0.0643)
PrevScore × Share × Post		0.416***		0.444***		0.377***
		(0.0521)		(0.0524)		(0.0511)
Observations	819,087	435,709	819,087	435,709	819,087	435,709
R-squared	0.253	0.299	0.271	0.317	0.256	0.296

Notes: This table presents estimated coefficients of Equation 1 and 2. Significance levels at 1%, 5%, and 10% are denoted by ***, **, and *, respectively. Standard errors are clustered at the school level. For Columns 1 and 2, I assume that a student was tested if he took at least one subject of the exam.

Table 4: Long-run Effects on Educational Outcomes

	High School Graduation	College Enrollment	College Completion
	(1)	(2)	(3)
Share x Expose	-0.0732***	–0.138	–0.0367
	(0.0254)	(0.0824)	(0.0201)
Observations	355,239	355,239	355,239
R-squared	0.155	0.112	0.051

Notes: This table presents estimated coefficients of equation 5. Significance levels at 1%, 5%, and 10% are denoted by ***, **, and *, respectively. Standard errors are clustered at the school level.

Table 5: Long-run Effects on College Outcomes

	Enrollment, 2 year	Enrollment, 4 year	Completion, 2 year	Completion, 4 year
	(1)	(2)	(3)	(4)
Share x Expose	–0.131	–0.0742	–0.0354**	–0.0211
	(0.0798)	(0.0486)	(0.0145)	(0.0156)
Observations	355,239	355,239	355,239	355,239
R-squared	3103	0.078	0.022	0.054

Notes: This table presents estimated coefficients of Equation 5. Significance levels at 1%, 5%, and 10% are denoted by ***, **, and *, respectively. Standard errors are clustered at the school level.

Table 6: Long-run Effects on Labor Market Outcomes

	Earning	Employment
	(1)	(2)
Share x Expose	–3094.801*	–0.121***
	(1786.117)	(0.0370)
Observations	395,561	395,561
R-squared	0.056	0.04

Notes: This table presents estimated coefficients of Equation 5. Significance levels at 1%, 5%, and 10% are denoted by ***, **, and *, respectively. Standard errors are clustered at the school level. All outcomes are measured at ages 25–29. The annual income measure includes unemployed individuals with zero earnings and is deflated using 2000 CPI.

Table 7: Robustness Check: Alternative Sample and Treatment Intensity Specification

	Base		Unbalanced		Annual Shares		Unbalanced, Annual Shares	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share x Post	–0.678***	–0.196***	–0.495***	–0.200***	–0.670***	0.0720	–0.625***	–0.272***
	(0.0819)	(0.0640)	(0.0639)	(0.0600)	(0.0531)	(0.0531)	(0.0556)	(0.0643)
PrevScore x Share x Post		0.416***		0.225***		0.458***		0.377***
		(0.0521)		(0.0474)		(0.0442)		(0.0511)
Observations	819,087	435,709	2,288,044	824,397	823,444	441,511	2,351,265	836,447
R-squared	0.253	0.299	0.231	0.231	0.252	0.299	0.209	0.234

Notes: This table presents estimated coefficients of Equation 1 in Columns 1, 3, 5 and 3 in Columns 2, 4, 6 respectively. Significance levels at 1%, 5%, and 10% are denoted by ***, **, and *, respectively. The outcome variable is TAAS participation for all columns. Standard errors are clustered at the school level.

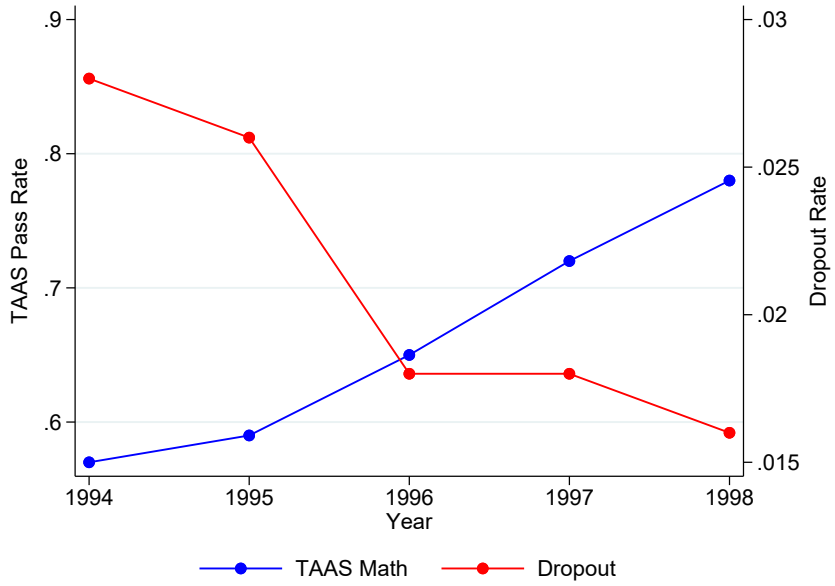
Table 8: Summary Statistics – Test Outcomes by Disability Types

	Learning Disability (1)	Speech Impairment (2)	Emotional Disturbance (3)	Mental Retardism (4)
Short-run Analysis (G3–8)				
Fraction	0.612	0.187	0.056	0.053
TAAS Participation				
Reading	0.43	0.79	0.45	0.35
Math	0.49	0.8	0.47	0.38
TAAS Score				
Reading	-1.18	-0.25	-0.67	-1.97
Math	-1.08	-0.19	-0.82	-2.06
Long-run Analysis (G9)				
Fraction	0.699	0.016	0.114	0.064
TAAS Participation				
Reading	0.55	0.77	0.51	0.05
Math	0.55	0.78	0.49	0.05
TAAS Score				
Reading	-1.45	-0.72	-0.99	-2.37
Math	-1.38	-0.69	-1.12	-2.46

Notes: This table presents summary statistics of students in special education by types of disabilities. The first panel presents statistics of students in Grades 3–8, and the second panel shows those of students in Grade 9. Disability types follow the categorization of the ERC special education records. Test scores are normalized to have a mean of 0 with a standard deviation of 1 within each grade level, subject, and year.

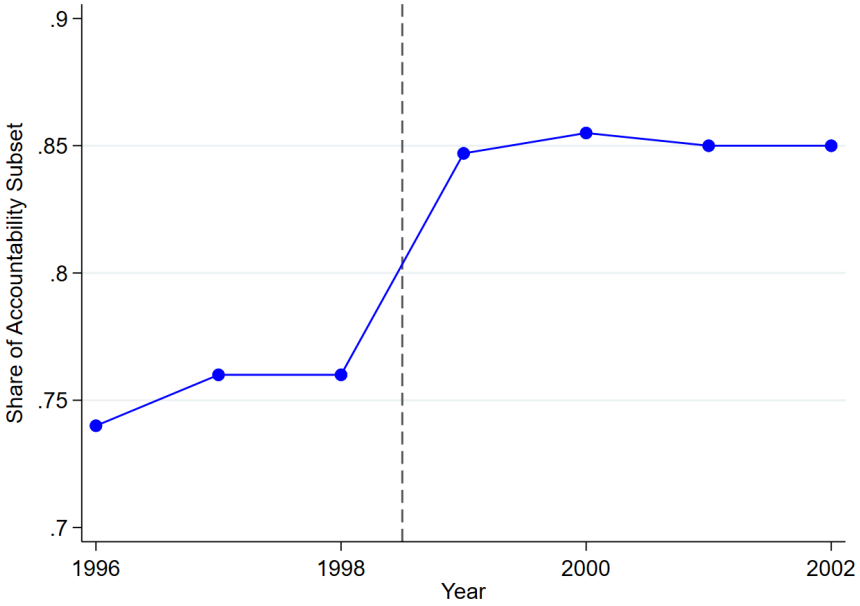
A Appendix Figures

Figure A.1: Trends Educational Outcomes After Introduction of Accountability System



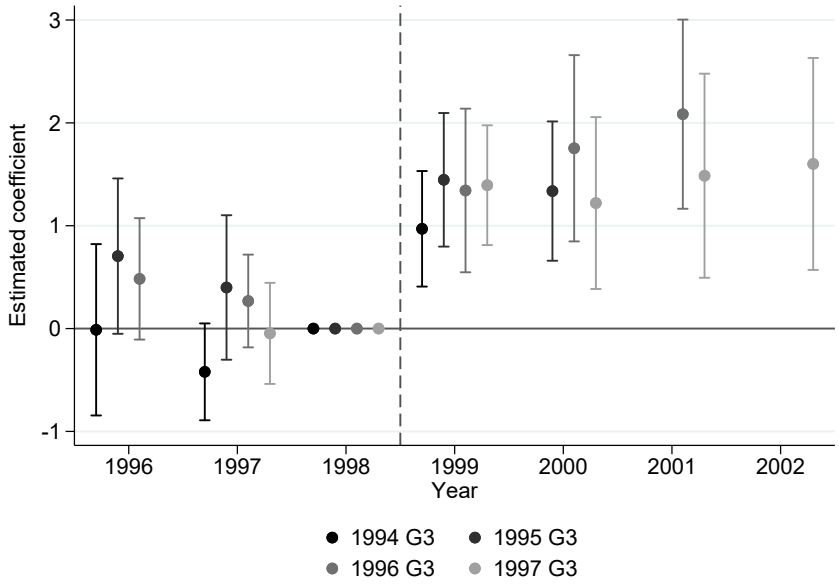
Notes: The figure plots state-level average educational outcomes of Texas after the implementation of the full-scale school accountability system in 1994. Refer to Haney (2000) for more details.

Figure A.2: Accountability Subset Expansion by 1999 Reform

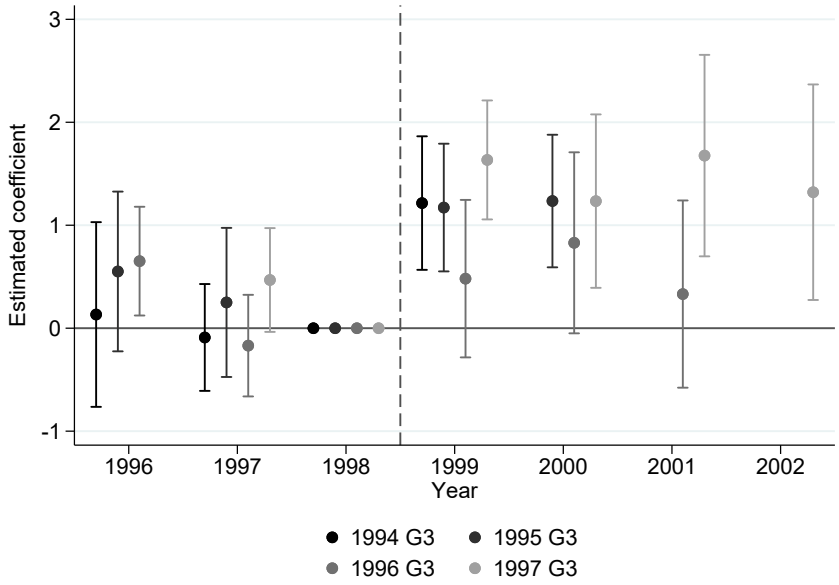


Notes: The figure plots fractions of students included in the accountability subset between 1996 and 2002. They are sourced from publicly available Texas AEIS school reports. More recent statistics could be found here: https://rptsvr1.tea.texas.gov/perfreport/aeis

Figure A.3: Event Study: Test Score by Grade Cohorts



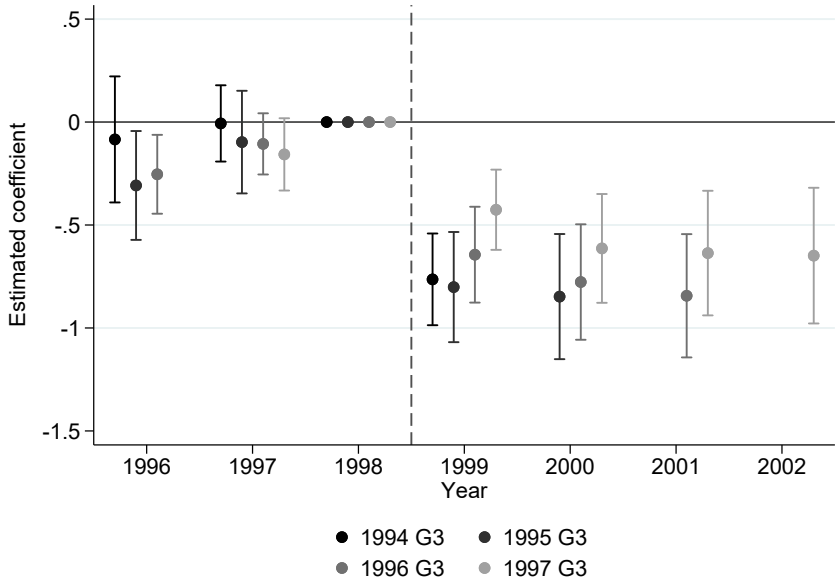
(a) Reading Score



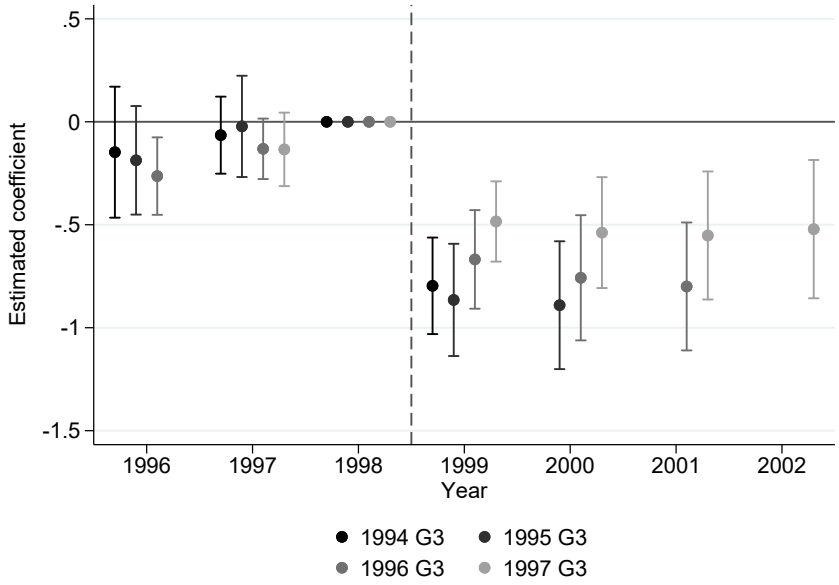
(b) Math Score

Notes: The figure plots event study estimates based on Equation 2. Panels (a) and (b) report the results separately for each cohort of the balanced sample. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level. Test scores are normalized to have a mean of 0 with a standard deviation of 1 within each grade level, subject, and year.

Figure A.4: Event Study: Test Participation by Grade Cohorts



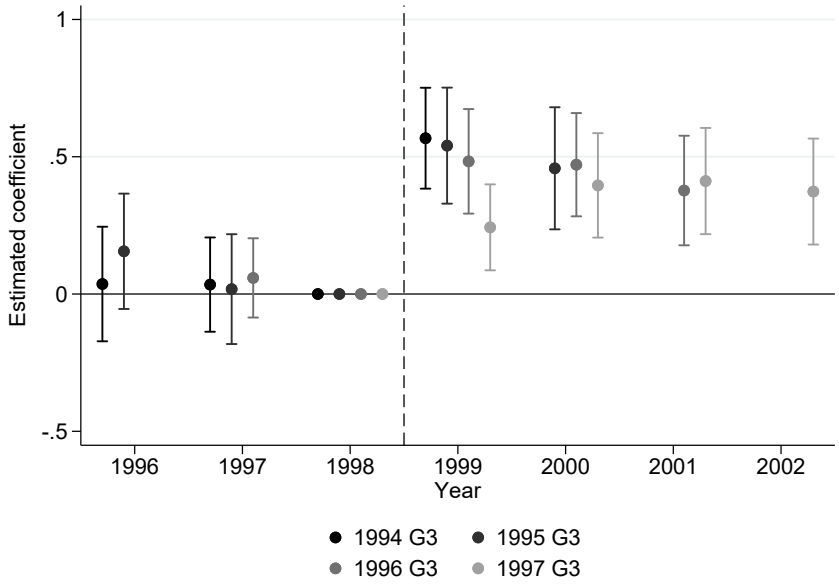
(a) Reading Test Participation



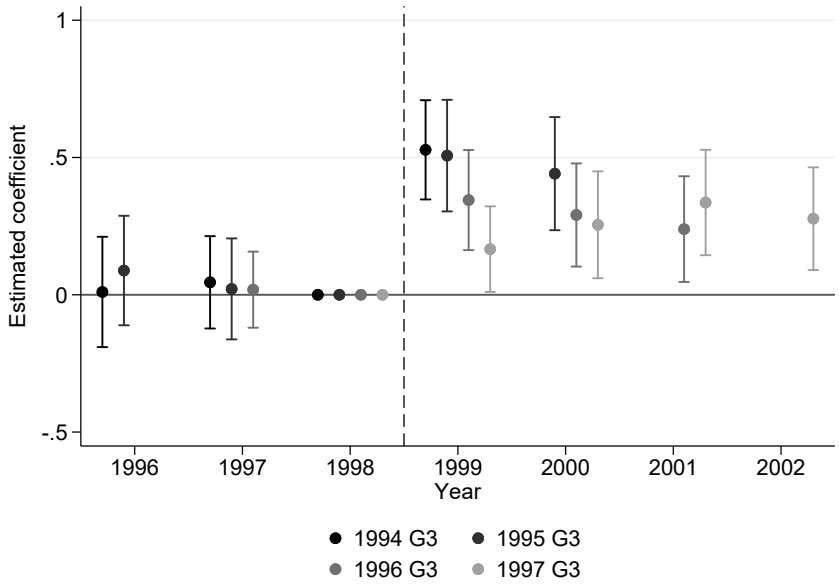
(b) Math Test Participation

Notes: The figure plots event study estimates based on Equation 2. Panels (a) and (b) report the results separately for each cohort of the balanced sample. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

Figure A.5: Event Study: Heterogeneous Effect on Test Participation by Grade Cohorts

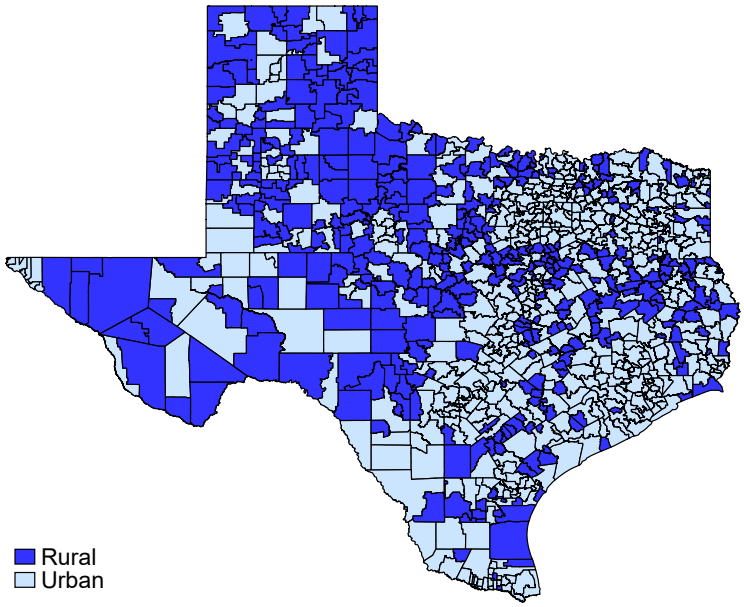


(a) Reading Test Participation

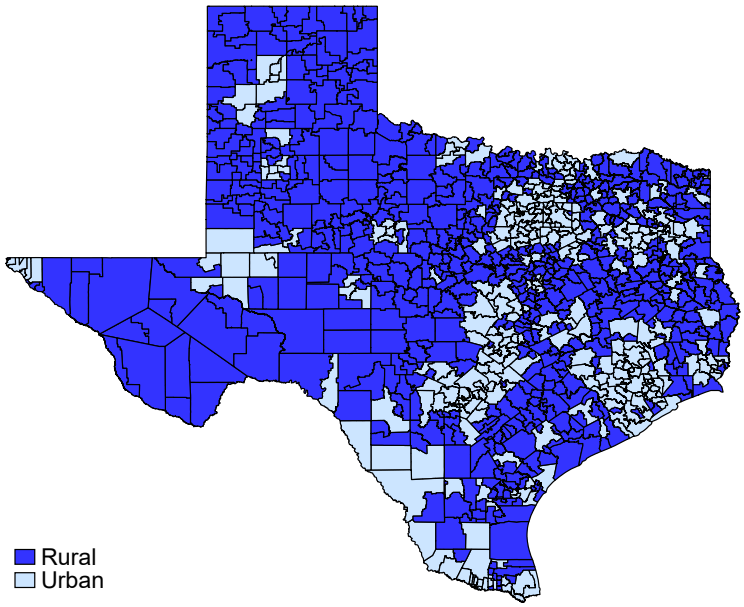


(b) Math Test Participation

Notes: The figure plots event study estimates based on Equation 4. Panels (a) and (b) show the results using the balanced sample I described. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.



(a) Narrow Definition of Rural Districts

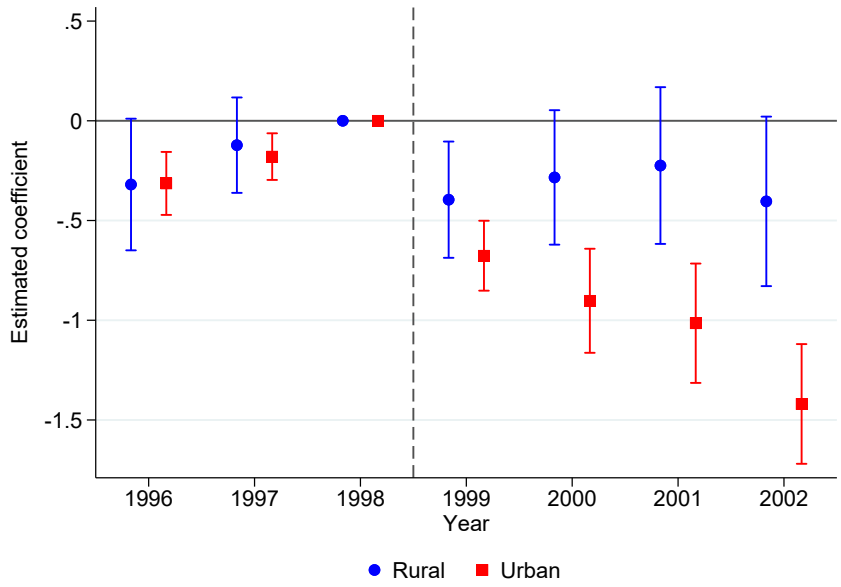


(b) Broad Definition of Rural Districts

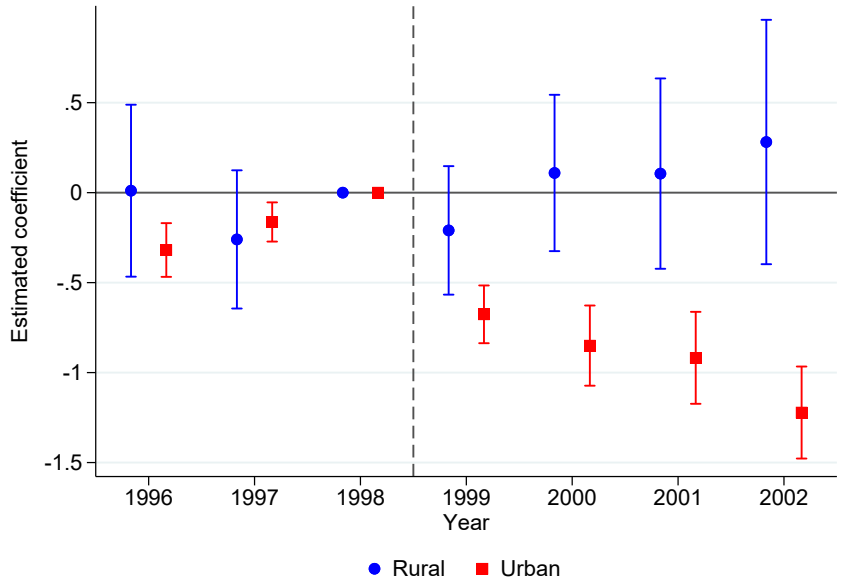
Figure A.6: Definition of rural and urban districts

Notes: The figures illustrate the geographical distribution of rural and urban districts in Texas. In panel (a), only districts tagged as "Rural" are treated as rural districts. In panel (b), districts tagged as "Rural," "Non-Metropolitan Stable," and "Non-Metropolitan Fast Growing" are treated as rural districts. I follow the 2007 definition of districts by the TEA, which is the earliest data available.

Figure A.7: Event Study: Urban Districts Show Larger Exclusion of SE Students



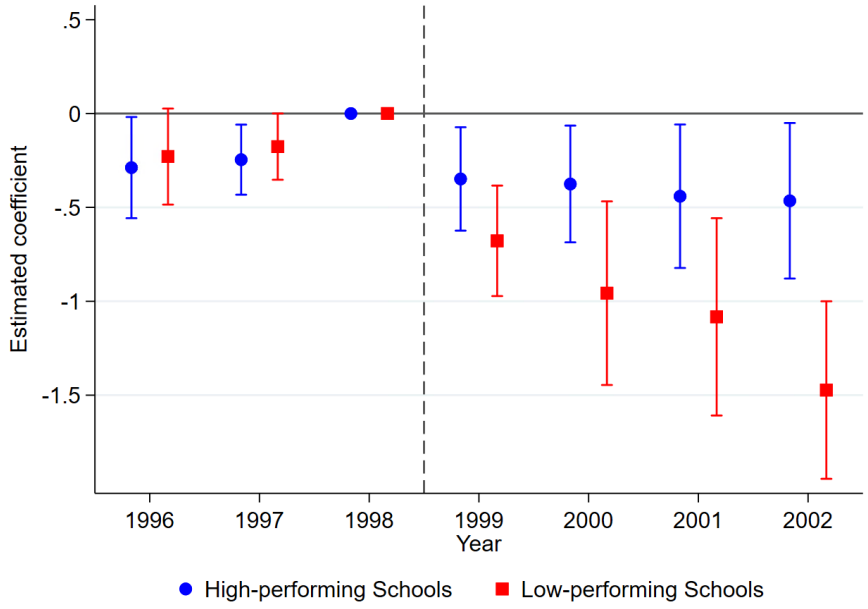
(a) Broad Definition of Rural Districts



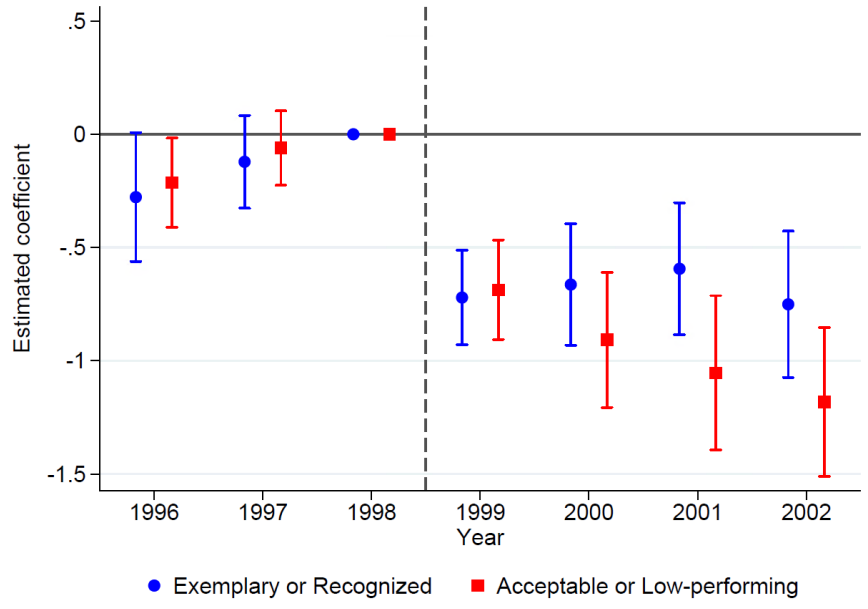
(b) Narrow Definition of Rural Districts

Notes: This figure shows heterogeneous effect estimates on TAAS participation rates by district geographic characteristics. Blue and red plots represent estimates from Equation 2 based on students in urban and rural districts, respectively. The definition of urban and rural districts follows that of Appendix Figure A.6. In panel (a), only districts tagged as "Rural" are treated as rural districts. In panel (b), districts tagged as "Rural," "Non-Metropolitan Stable," and "Non-Metropolitan Fast Growing" are treated as rural districts. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

Figure A.8: Event Study: Schools With Poorer Performance Show larger Exclusion of SE Students



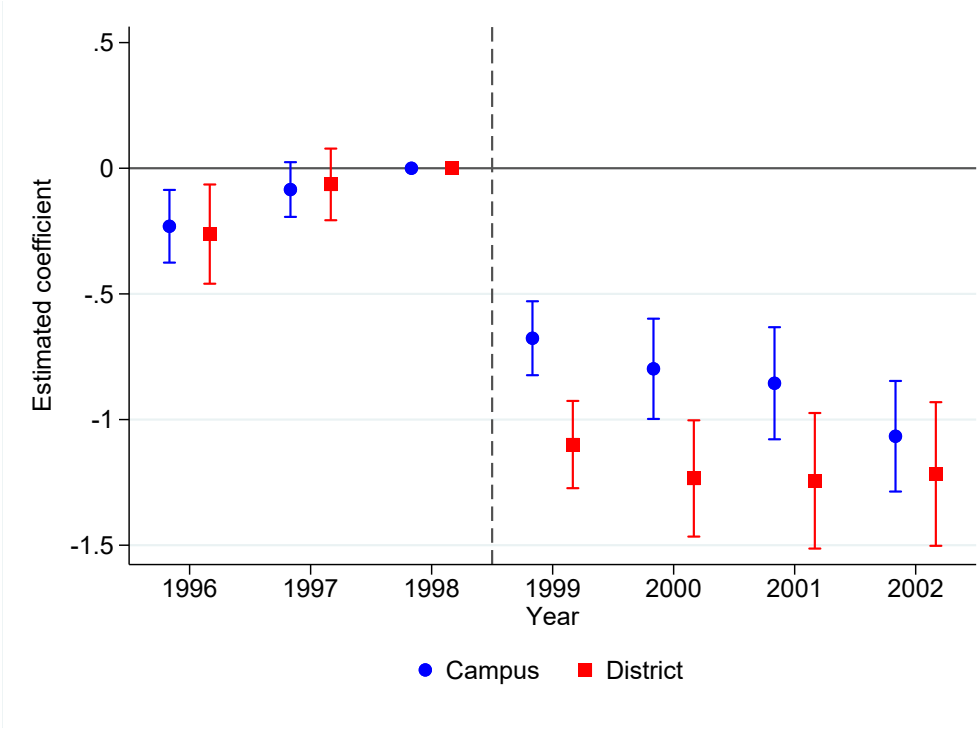
(a) By Previous Average Test Score



(b) By Previous Rating

Notes: This figure shows heterogeneous effect estimates on TAAS participation rates by measures of school performance. Blue and red plots represent estimates from Equation 2 based on students in high- and low-performing schools, respectively. In panel (a), high-performing schools are those with the top quartile of average test scores, and low-performing schools are those with the bottom quartile. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

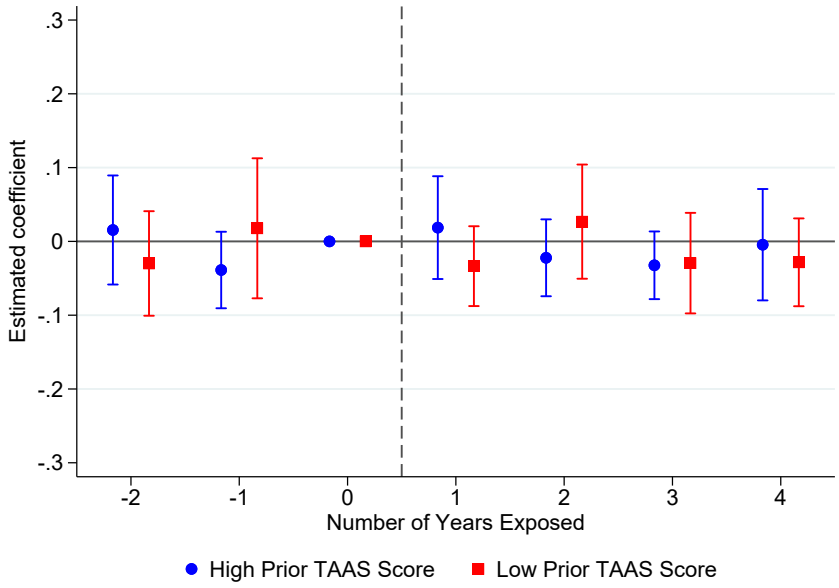
Figure A.9: Event Study: The Degree of Exclusion was More Responsive to District-level Incentives



(a) Campus- vs. District-level Variation

Notes: This figure shows heterogeneous effect estimates on TAAS participation rates by different levels of SE share variations. Blue and red plots represent estimates from Equation 2, based on school- and district-level SE share variation, respectively. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

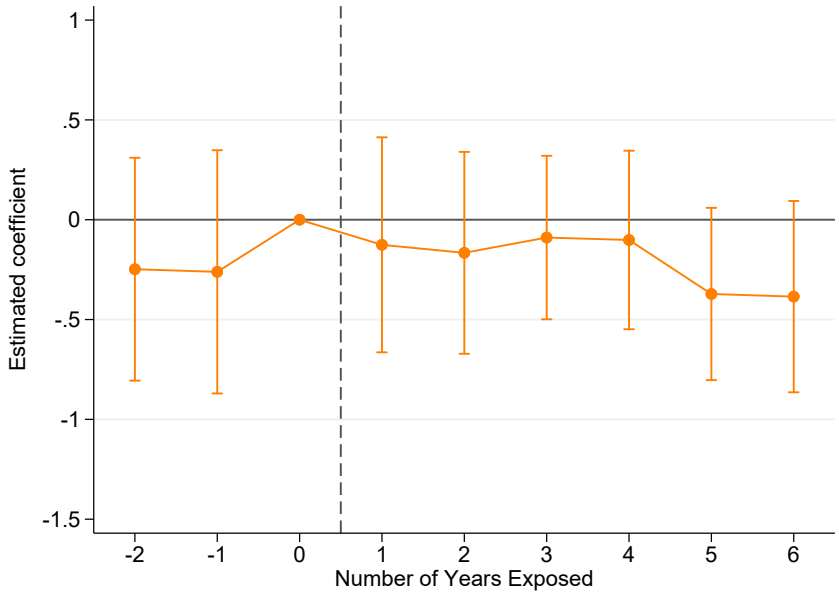
Figure A.10: Event Study: Effects on Dropouts from Official Dropout Records



(a) Dropout

Notes: This figure shows event study results on student dropouts analogous to Figure 12.(b) based on Equation 6 and the official TEA dropout data. I define "high performance" groups as students in the top tertile in terms of their past TAAS scores and vice versa. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

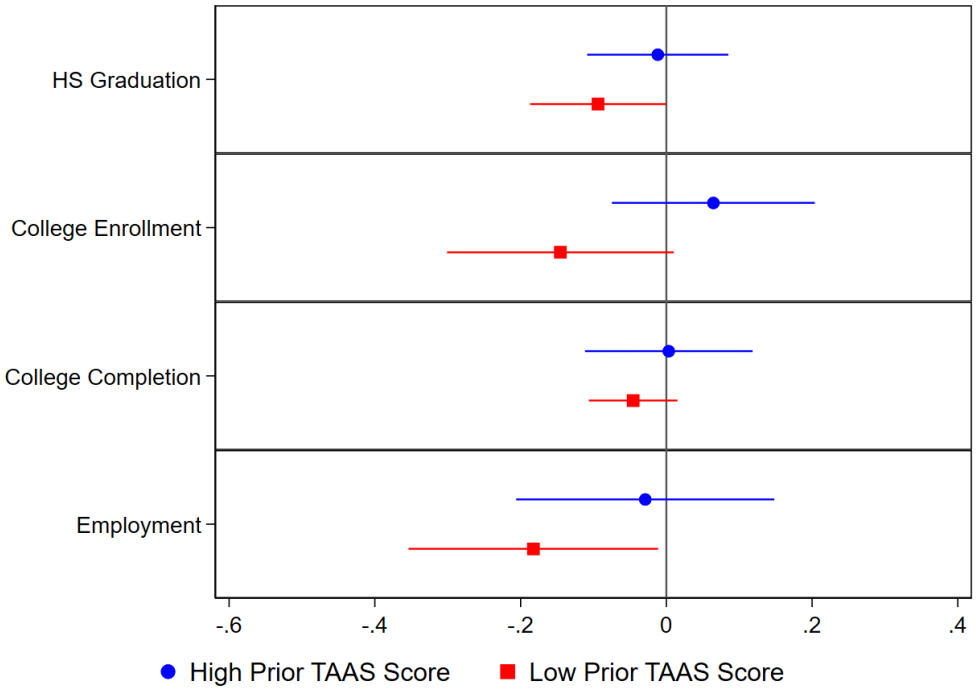
Figure A.11: Event Study: Effects on Earnings Conditional on Employment



(a) Earnings, Employed

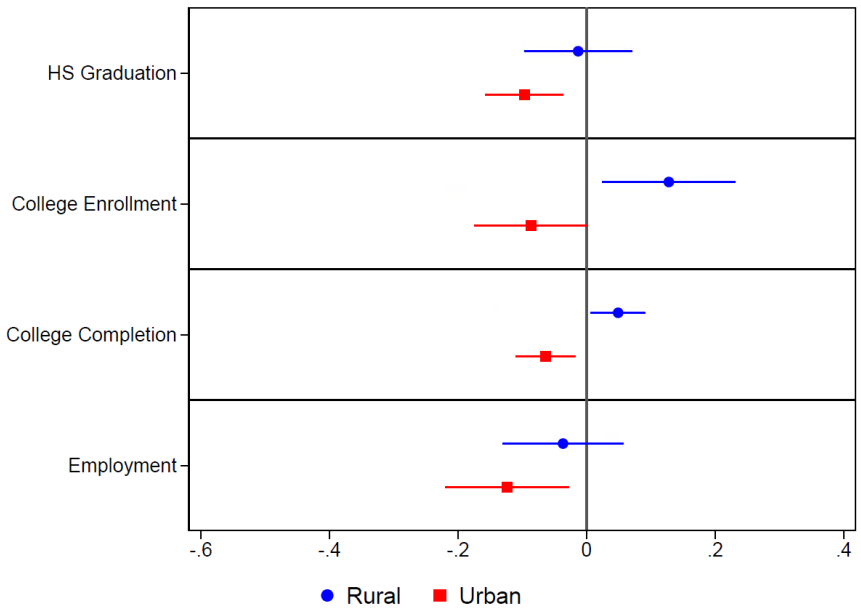
Notes: This figure shows event study results on log wage based on Equation 6. All observations with zero earnings are excluded from the analysis. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

Figure A.12: Other Outcomes Deteriorated for Low-performing Students

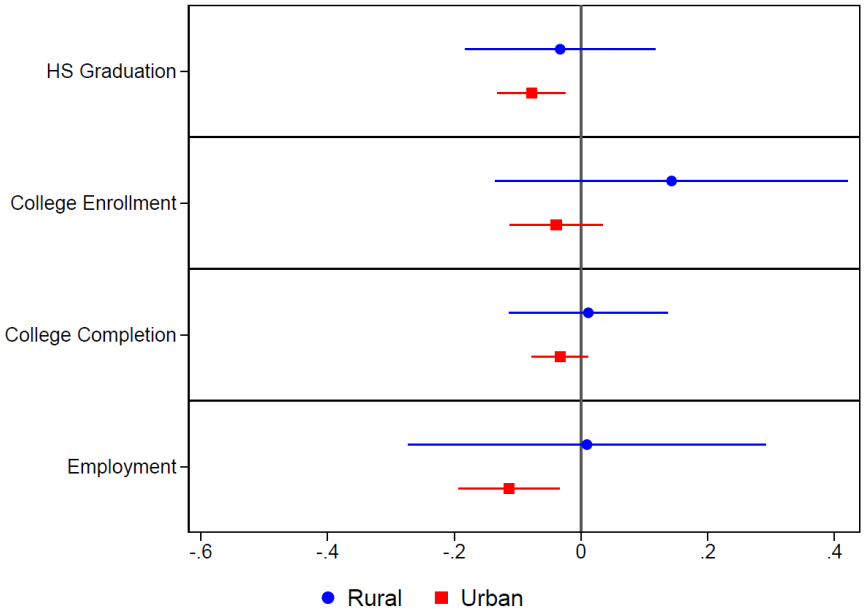


Notes: The figure reports estimated coefficients on long-run outcome based on Equation 5 by students' past TAAS scores. I define "high performance" groups as students in the top tertile in terms of their past TAAS scores and vice versa. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

Figure A.13: Urban Districts Drove Adverse Effects



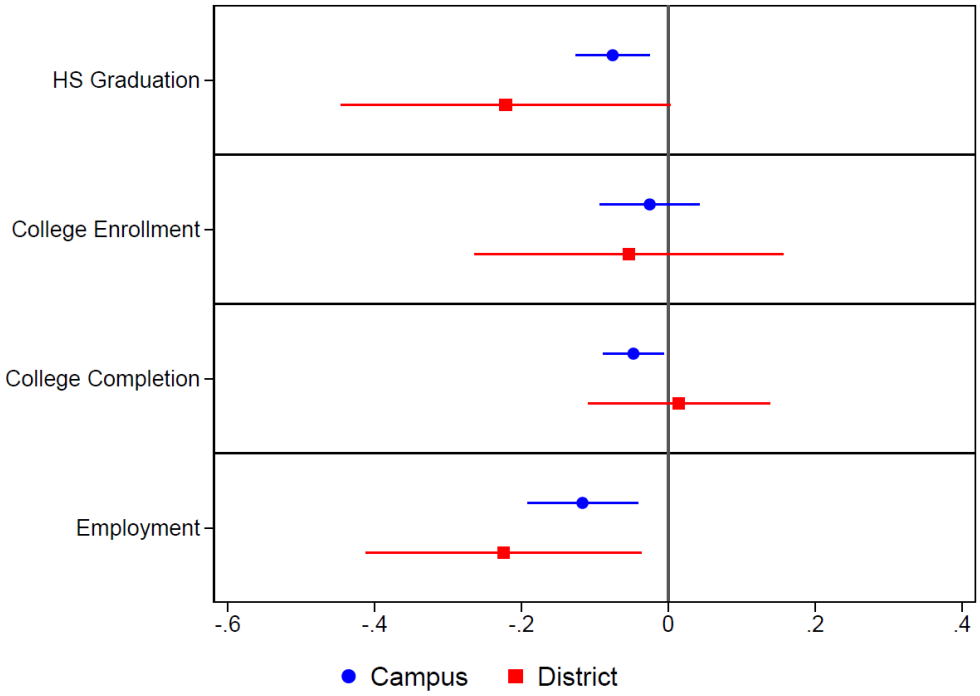
(a) Broad Definition of Rural Districts



(b) Narrow Definition of Rural Districts

Notes: The figure reports estimated coefficients on long-run outcome based on Equation 5 by district geographic characteristics. The definition of urban and rural districts follows that of Appendix Figure A.6. In panel (a), only districts tagged as "Rural" are treated as rural districts. In panel (b), districts tagged as "Rural," "Non-Metropolitan Stable," and "Non-Metropolitan Fast Growing" are treated as rural districts. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

Figure A.14: High School Graduation and Employment Were More Sensitive to District Incentives



Notes: This figure shows heterogeneous effect estimates on long-term outcomes by different levels of SE share variations. Blue and red plots represent estimates from Equation 5, based on school- and district-level SE share variation, respectively. I plot the estimated coefficients of interest with corresponding 95% confidence intervals. Standard errors are clustered at the school level.

B Appendix Tables

Table A1: Summary Statistics by SE Share Levels (Grade 8)

	High SE Share	**Low SE Share**
	(1)	(2)
Individual Characteristics		
Male	0.48	0.49
White	0.52	0.46
Black	0.13	0.12
Hispanic	0.33	0.37
Free/reduced-price lunch	0.46	0.38
Limited English proficiency	0.04	0.09
Educational Outcomes		
TAAS tested, reading	0.87	0.82
TAAS tested, math	0.87	0.82
Normalized score, reading	0.08	0.18
Normalized score, math	0.09	0.19
High school graduation	0.70	0.72
College enrollment	0.49	0.54
College enrollment, 4 year	0.22	0.29
College completion	0.19	0.25
College completion, 4 year	0.15	0.21
Labor Market Outcomes		
Annual income ($)	16,731	17,800
Employment	0.73	0.68
Number of individuals	96,168	232,334

Notes: This table presents average individual characteristics, educational outcomes, and labor market outcomes of all students in schools with high and low levels of SE shares. Test scores are normalized to have a mean of 0 with a standard deviation of 1 within each grade level, subject, and year. Labor market outcomes are calculated between ages 25 and 29. The annual income measure includes unemployed individuals with zero earnings and is deflated using 2000 CPI. Samples are limited to 8th graders between 1996 and 1998.

Table A2: Summary Statistics – Test Outcomes by Disability Types

	Learning Disability (1)	Speech Impairment (2)	Emotional Disturbance (3)	Mental Retardism (4)
Educational Outcomes				
High School Graduation	0.59	0.71	0.44	0.67
College enrollment	0.26	0.48	0.22	0.08
College enrollment, 4 year	0.05	0.21	0.04	0.01
College completion	0.05	0.19	0.04.	0.01
College completion, 4 year	0.03	0.14	0.02	0.00
Labor Market Outcomes				
Annual Income ($)	12,309	16,302	8,326	3,460
Employment	0.63	0.66	0.55	0.33
Observations	104,638	16,073	19,028	10,222

Notes: This table presents summary statistics of eighth-grade students who were ever in special education by types of disabilities. Labor market outcomes are observed in ages 25–29 for each individual. Annual incomes include zero values and are adjusted using 2000 CPI. Disability types follow the categorization of the ERC special education records.