The Long Shadow of School Closures: Impacts on Students' Educational and Labor Market Outcomes

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Abstract

Each year, about a thousand public schools in the US close, displacing hundreds of thousands of students. I examine the impact of public-school closures on displaced students using linked schooling and labor market data from Texas. I implement difference-in-differences strategies using the within-school across-time/cohort variation in student exposure to school closure. I find that school closures decreased test scores, increased absenteeism, and led to more disciplinary actions. Furthermore, I find that school closures decreased high school completion, college attainment, employment, and earnings at ages 25-27. These impacts are larger for secondary students, Hispanic students, and those from economically disadvantaged backgrounds.

JEL: H40, I21, I28

Keywords: school closure, demographic shift, student mobility, human capital development, long-run effect, education policy, education finance

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1 Introduction

School closures are prevalent in the United States, with approximately 1,000-1,800 public schools shutting down every year and leaving 180,000-320,000 students displaced (NCES 2022). Behind these staggering figures lie two critical issues. First, the decline in the school-age population, driven by demographic shifts and outmigration, results in low enrollments and constrained funding for schools. Schools end up being consolidated to cut costs and achieve economies of scale (Dodson III and Garrett 2004; Sell and Leistritz 1997; Strange 2013). Second, school reform policies target low-performing schools for closure. Indeed, performance-based closures have been encouraged by federal policies such as the No Child Left Behind Act, the US Department of Education's Race to the Top program, and the Department's School Improvement Grants (Delpier 2021; Jack and Sludden 2013). Considering the expected decline in school enrollment and the increasing importance of school accountability in education policy, the underlying issues will persist as an ongoing concern, emphasizing the significance of implementing relevant policies to address this problem over time.

School closure policy is contentious. It often brings backlashes from parents and local communities (Griffin 2017; Mellon 2014; Rodriguez 2023). While some may argue closures are inevitable due to declining enrollment or budget constraints, district leadership also often justifies a school closure by arguing that consolidation will ultimately benefit affected students and the district as a whole. The rationale is that it will offer displaced students and future cohorts access to better-resourced schools, higher-achieving peers, and the advantages of economies of scale (Carlson and Lavertu 2016; Sunderman and Payne 2009). However, the process of moving to another school can result in significant environmental changes for displaced students (Chetty, Hendren, and Katz 2016). They may experience disruptions to their learning, new school requirements and norms, and separations from their friends. Thus, even if the policy is intended to benefit students, its actual impacts remain theoretically unclear. Additionally, historically under-served populations, such as Black, Hispanic, and economically disadvantaged students, are often disproportionately impacted by school closures (Fleisher 2013; Hurdle 2013; Tieken and Auldridge-Reveles 2019).

This paper estimates the causal effect of school closures on both students' short- and long-run outcomes and explores the heterogeneity in impacts across student and school characteristics. I utilize Texas longitudinal and individual-level administrative data and the difference-in-differences method. Connecting individuals' K-12 education records to post-secondary and labor market outcomes, I measure impacts on both short-run outcomes such as test scores and behavioral outcomes, as well as long-run outcomes such as high school graduation, college education attainment, employment, and wages. I use difference-in-differences strategies because simply taking the difference in the outcomes of displaced and non-displaced students would not generally provide the causal effect of school closure. Many observed and unobserved factors influence which school a student attends and their subsequent educational or labor market outcomes. In my difference-in-differences analysis, I compare within-school across-time/cohort changes in outcomes following school closure to those of students from control schools that are matched based on similar student and school characteristics.

I analyze school closures that occurred in Texas from 1998 to 2015, focusing on public non-charter instructional campuses in regular and independent districts. To identify schools that have been closed, I use two criteria: the school must be listed on the official roster of closed schools on the Texas Education Agency website, and it must no longer be present in the Texas administrative data set. Using these criteria, I identify a total of 470 school closures for my study. Beginning by documenting the reasons driving school closures, I find that the predominant reasons for closures are tied to demographic shifts and financial constraints. Among the closure reasons that I have been able to identify, 90 percent of closures are broadly attributed to demographic challenges and 3 percent of closures are a consequence of persistently low performance. The remaining 7 percent is divided among coding changes and district closures.

By analyzing within-student variation in exposure to school closures between closed and control schools, I find an immediate disruption in learning. Specifically, math and reading scores drop by 0.033 and 0.034 standard deviations, respectively. Days of absence and days of disciplinary action increase by 0.13 days (1.8% increase relative to the pre-closure mean) and 0.36 days (15%) respectively. Although the effects on test scores dissipate within three years, the impact on the days of absence and disciplinary action persist or accumulate over time.

This increase in days of disciplinary action is primarily driven by out-of-school suspensions and expulsions rather than in-school suspensions. It is particularly concerning in light of recent studies presenting the long-term negative consequences of disciplinary actions and school absences (Bacher-Hicks, Billings, and Deming 2019; Cattan et al. 2023; Liu, Lee, and Gershenson 2021; Weisburst 2019). Additionally, I find no evidence of students leaving the Texas public school system after experiencing school closure.

I use across-cohort variation between closed and control schools to identify the effect on long-run outcomes, comparing younger cohorts who experience school closures to older cohorts who do not. I find that by age 26, experiencing school closure leads to a reduction in high school graduation rates by 2.3 percentage points (3.2%), the enrollment rate for any colleges decreases by 1.2 percentage points (2.5%), the four-year college completion rate decreases by 0.7 percentage points (5.1%), and college quality based on expected earnings decrease by \$337 (1.1%). Furthermore, the closure leads to a reduction in employment rates by 1.0 percentage points (1.9%) and a decrease in yearly earnings by \$698 (3.4%) at ages 25-27. Approximately one-third of the drop in earnings can be explained by the expected earnings from educational attainment, suggesting that the effects of school closures extend beyond educational outcomes. My estimates imply a \$30,220 reduction in the present discounted value of lifetime earnings per student affected by a school closure, and a total annual cost of \$7.6 billion (\$30,220×250,000) across all displaced students in the US.

I investigate heterogeneity in the effect of school closures across student demographics and school characteristics. I find that the negative effects are more pronounced among Hispanic students, those from economically disadvantaged families, those in higher grades when school closes, and those from urban and originally low-performing schools. While the drop in test scores after closure is recovered on average, students in secondary schools or those moving to worse-performing schools do not recover over time. The increase in behavioral issues is concentrated among Black and Hispanic students, those from economically disadvantaged families, and those moving to better-performing schools. The increase in days of absence is concentrated on urban school closures. Similarly, long-term negative outcomes are more pronounced among Hispanic students, those from economically disadvantaged backgrounds, secondary school students, those in low-performing schools, and students from urban schools. While Black, Hispanic, and economically disadvantaged students are disproportionately affected by school closures, they also experience more significant negative effects.

I further explore the school-level changes for displaced students. By analyzing within-student variation before and after school closures, I find an immediate drop in peer quality measured by yearly test scores. School average math and reading scores drop by 0.05 standard deviations. However, expected school quality, as measured by the quality of the school *before* the closures, shows the opposite pattern. Displaced students experience increases in expected school average test scores. In other words, students are supposed to have better school quality after displacement when school closures are planned, but actual peer qualities after moving are worse than those of the original schools.

This study contributes to three strands of literature: school closure, student mobility, and long-run effects of childhood disruptions. I advance the literature on the effects of school closure in two key directions (for an extensive interdisciplinary review on school closure research, see Tieken and Auldridge-Reveles (2019)). First, I examine the long-run effects while previous studies primarily focus on short-run effects, particularly test scores (Beuchert et al. 2018; Brummet 2014; Engberg et al. 2012; Gordon et al. 2018; Larsen 2020; Özek, Hansen, and Gonzalez 2012; Steinberg and MacDonald 2019; Taghizadeh 2020; Torre and Gwynne 2009).[1] Although a few studies explore the long-term impacts of school closures, they are limited to K–12 education outcomes (Grau, Hojman, and Mizala 2018; Larsen 2020).[2] To the best of my knowledge, this is the first paper estimating school closure effects on higher education and labor market outcomes. Investigating earnings is particularly important because it captures the broader consequences of school closures beyond education. My analysis reveals that only one-third of the earnings reduction can be attributed to differences in educational attainment, underscoring the need to consider labor market outcomes.

[1] Some of them show that the adverse effects on test scores tend to dissipate over time, leading to the conclusion that the adverse effects do not last (Brummet 2014; De Witte and Van Klaveren 2014; Engberg et al. 2012; Özek, Hansen, and Gonzalez 2012).

[2] In the context of Chile, Grau, Hojman, and Mizala (2018) find that school closures led to an increase in dropout rates (1.8 -2.5 percentage points) and a decline in student retention (3.9-4.4 percentage points). Using high school closure in Milwaukee public school district, Larsen (2020) show, while it is statistically insignificant, a decrease in high school graduation rates (7.5 percentage points) as a result of the closures.

Another contribution to the school closure literature is to explore heterogeneous effects. This involves examining differences across various factors, such as urban and rural areas, original school quality, school quality changes, and grades and demographics of students. Previous studies focus mainly on a single urban school district, analyzing dozens of closures (e.g., Carlson and Lavertu 2016; Engberg et al. 2012; Kirshner, Gaertner, and Pozzoboni 2010; Larsen 2020; Steinberg and MacDonald 2019) with an exception of Brummet (2014) using Michigan data. In this study, I use data from Texas, which is a large and diverse state with numerous school closures. This allows me to compare the consequences of closures across different school and student characteristics. The findings highlight that while closures have overall negative effects, these impacts are more pronounced on specific groups of students and types of schools.

This study also contributes to the literature on student mobility by exploring its effects on various outcomes beyond test scores, without involving a concurrent residential move. Previous studies present a decline in test scores for students who change schools (Hanushek, Kain, and Rivkin 2004; Schwartz, Stiefel, and Cordes 2017; Xu, Hannaway, and D'Souza 2009). To identify the causal effect of student mobility, researchers often rely on instruments such as school grade span (Rockoff and Lockwood 2010; Schwartz, Stiefel, and Cordes 2017; Schwerdt and West 2013), as student mobility is often associated with family issues or changes in residency. This study examines the effect of school closures as a distinct situation that can initiate student mobility without concurrent changes in residential neighborhoods. By expanding the analysis beyond test scores, this study sheds light on the potential long-term consequences of student mobility on behavioral issues, post-secondary education, and labor market outcomes. My findings highlight the importance of student mobility and grade configuration as an understudied area, suggesting that it may have negative long-term consequences.

Finally, this study contributes to the broad literature on the long-run effects of childhood intervention/disruption and school inputs. Previous studies investigate long-run effects of preschool programs such as Perry Preschool and Head Start (Garces, Thomas, and Currie 2002; Heckman, Pinto, and Savelyev 2013), housing voucher program (Chetty, Friedman, and Rockoff 2014), and the experience of adverse shocks such as natural disasters or school shootings (Cabral et al. 2021; Sacerdote 2012), as well as teacher, peer, and school quality (Carrell, Hoekstra, and

Kuka 2018; Chetty, Friedman, and Rockoff 2014; Hyman 2017). My research emphasizes once again the significance of childhood experience by showing that a policy intervention could be a negative shock in childhood. It underscores the need for careful consideration in policy-making regarding school closures, given the long-lasting adverse impacts on displaced students.

The remainder of the paper is organized as follows. Section 2 provides background information on the reasons for school closures in Texas. Sections 3 and 4 describe the data and empirical strategy. Section 5 presents main results and robustness checks. Section 6 contains a discussion of the results, and Section 7 concludes.

2 Background: School Closures in Texas

The decision to close schools primarily lies within the discretion of school districts. Typically, school districts decide to close a school during a board meeting held throughout the school year. Students complete the remaining school year at the closing school and are then assigned to new schools for the following academic year based on their residential addresses.

To identify schools that have closed down, I rely on the list of school closures from AskTED, the online Texas Education Directory (TEA 2022), which is compiled based on reports from school districts. To be considered "closed" in my analysis, a school has to be listed on the Texas Education Agency closure list and also disappear from my dataset. My analysis covers the period from 1998 to 2015 for short-run analysis and 1998 to 2003 for long-run analysis. I only consider school closures from non-charter instructional campuses in regular and independent districts.[3] I further narrow down my sample by restricting school closures to those that are observed in the previous period (1994–1997) to avoid situations where a school only existed temporarily.

There are 470 school closures meeting the criteria. A list of these closed schools is provided in Appendix B with their closure years and school districts. On average, there are about 26 closures per year, evenly distributed across the years. Figure 1 presents the locations of the 470 school closures, indicating that closed schools are distributed all over Texas, with a concentration in more populated areas. Appendix Table A.1 displays the summary statistics of closed schools in

[3] There are schools reported as instructional campuses but named special education centers or disciplinary schools. In those cases, I exclude the campuses from the analysis.

column (1) and all schools in Texas in column (2). It shows that schools in cities and elementary schools experienced disproportionate closures. Moreover, students from racial minorities and economically disadvantaged backgrounds are more likely to experience school closures. Non-Hispanic Black and Hispanic students constitute 68 percent of those affected by closures, while they make up 57 percent of all students. Economically disadvantaged students, including those receiving free or reduced-price lunch and other forms of aid, account for 71 percent of students affected by closures, compared to 55 percent of all students. As discussed in the previous papers (Fleisher 2013; Hurdle 2013; Tieken and Auldridge-Reveles 2019), I also find that historically under-served populations, such as Black, Hispanic, and economically disadvantaged students, are disproportionately impacted by school closures.

School closures can occur for various reasons. To better understand the reasons driving school closures, I identify and document the reasons behind 274 out of 470 school closures. My primary sources of information include local news articles, interviews with personnel in school districts, and documents from school board meetings. To the best of my knowledge, this is the first attempt to construct statewide statistics about reasons for closures (a full list of categorized reasons can be found in Appendix B). It is important to note that school closure decisions often stem from a combination of factors. For instance, a decline in enrollment is frequently accompanied by budgetary constraints and the presence of aging school facilities. Furthermore, other aspects may be taken into account during the decision-making process, even if those are not reported as the main drivers of the closures.[4]

To facilitate an understanding of the closure reasons, I categorize identified reasons into several distinct groups, including chronically low performance, financial constraints, enrollment changes, aging school infrastructure, district-level renovation including closures and rezoning, school reform, and coding changes in Appendix Figure A.1. These categories are not mutually exclusive; a single school closure may be attributed to multiple reasons. While previous literature describing school closures emphasizes closures due to low performance (e.g., Delpier 2021;

[4] For example, consider the case of Dodson Elementary School in Houston Independent School District, which was shuttered in 2014 with students subsequently transferred to Blackshear Elementary School. The primary driver for this closure was the declining enrollment in the area. However, it is also worth noting that Dodson also performs worse on some measures of academic standards. This illustrates that while school performance may not be the primary factor for closure decisions, it can still become a point of consideration when deciding which school to close in areas experiencing depopulation.

Dowdall 2011; Jack and Sludden 2013; Tieken and Auldridge-Reveles 2019), the constructed records indicate that the majority of closures for non-charter public schools are driven by enrollment-related factors. Tight budgets, declining enrollment, aging school buildings, and restructuring district and school, account for about 90 percent of the identified reasons for closures. Closures primarily associated with low performance constitute just 3 percent of the cases.[5] This also challenges the conventional understanding of school closures, which often categorizes closures into a dichotomy of urban-low performance and rural-low enrollment frameworks (Tieken and Auldridge-Reveles 2019).

The category labeled "low performance" is mostly closures that are initiated by the education agency in response to chronic underperformance in schools. Closures falling under the "financial constraint" category often cite decreasing enrollment or statewide budget cuts as a significant factor, creating sustainability challenges for school districts. Closures categorized under "district reform" are frequently associated with shifts in youth population distribution across regions, prompting the need for school closures, construction of new schools, and rezoning attendance boundaries. "School reform" falls into a more ambiguous realm concerning school closures. In these cases, schools may not have been physically closed but instead transformed into different types of schools or undergone changes in grade levels.[6] Although schools are not physically closed, many students are displaced during the reform. The "coding changes" category refers to instances where schools are listed as closed in the records due to coding adjustments. Such adjustments can occur for specific intentions, including improving school accountability or administrative convenience.[7, 8]

[5] I divide reasons into three periods to see whether there is a change in reasons over time. In all three periods, more than 85 percent of closures are broadly related to enrollment changes. In the first (1998-2003), second (2004-2009), and last period (2010-2015), I identify reasons for 86 out of 146 closures, 71 out of 177 closures, and 110 out of 147 closures.

[6] For example, Comanche Intermediate School, which initially accommodated grades 3-6, underwent reform in 2003 and was renamed Comanche Elementary School, now serving grades PK-5. Additionally, closures are not classified as school reform if there is no overlap in grades following repurposing.

[7] For instance, an anonymous superintendent highlights the impact of school accounting policies, noting, "We consolidated to one campus identification because our class sizes are so small that statistics are skewed by only one student performing poorly. The consolidation of campuses allows for greater subgroup sizes in certain categories, thereby removing extremes in statistical calculations and variations in student performance." This suggests that school accounting practices play a role in promoting coding changes, especially in small schools within rural districts, potentially leading to more instances of coding-related closures in later periods of my analysis.

[8] To address potential concerns related to coding changes and school closures without physical closures, I estimate my baseline model potentially excluding not physically closed schools in Section 5.3. The estimation results are

3 Data

I use individual-level Texas administrative data sets, which include three sources: the Texas Education Agency (TEA), the Texas Higher Education Coordinating Board (THECB), and the Texas Workforce Commission (TWC).

TEA data includes K-12 education records in public schools starting from the academic years 1994-1995, containing information on attendance, disciplinary actions, high school graduation, and testing. The data further include student characteristics including age, sex, race/ethnicity, English second language status, special education status, and eligibility for free or reduced-price lunch. It also contains campus and district information, such as school type and charter type. Using TEA data, I construct four outcome variables: (1) the number of days of absence; (2) the number of days in disciplinary action;[9] (3) standardized math and reading scores;[10] and (4) high school graduation.[11]

THECB data includes all public and most private post-secondary education data in Texas.[12] The data are linked to TEA data at the individual level. I construct two post-secondary education outcome variables using THECB data: (1) an indicator for ever attending a Texas college by age 26; (2) an indicator for earning a bachelor's degree from a Texas post-secondary institution by

similar whether exclude those schools or not.

[9] The data about disciplinary action is only available from 1999, so the analysis sample for the days of disciplinary action is limited to students experiencing school closure after 2001.

[10] Test scores are standardized by grade and year. During the period of my analysis, different standardized tests were utilized in Texas, which were administered to different groups. The Texas Assessment of Academic Skills (TAAS) was used for 3rd–8th grade until 2002, and the Texas Assessment of Knowledge and Skills (TAKS) was used for 3rd–11th grade from 2003–2011. To ensure a minimum of a 2-year pretrend and post-outcome period, I consider students at the time of closure in the following grade configurations: grades 5–6th from schools closed in 1998–2000, grades 5–7th in 2001, grades 5–8th in 2002, grades 5–9th in 2003-2007, grades 5-6th and 8-9th in 2010, grades 5-6th and 9th in 2011, and grades 5–6th in 2012-2015. Moreover, the availability of test score data is more limited than that of attendance. The number of schools and students used in the analysis is discussed in Section 4.

[11] I construct a proxy for the high school graduation instead of using the high school graduation files provided by TEA due to data glitches. Some cohorts from several schools in my sample report a 0 percent graduation rate, while other cohorts from the same schools show 50-70 percent graduation rates. I find no significant differences between these cohorts, such as in average 12th-grade attendance. Thus, I define high school graduation as students attending 12th grade for more than 50 days. The cutoff does not have significant effects on my estimation. The two measures——high school graduation based on the TEA files and attendance in 12th grade——are highly correlated, with a correlation coefficient of 0.83.

[12] The THECB data contain all public community and technical colleges; all public universities and health-related institutions; almost all independent colleges and universities (available from 2003 onward); and career schools and colleges (available from 2004 onward). See http://www.txhighereddata.org/Interactive/CBMStatus/ for additional information on participating institutions.

age 26.[13]

TWC data includes quarterly individual data on employment, industry, and earnings for all workers covered by the Unemployment Insurance program.[14] The data is linked to TEA and THECB data at the individual level. Using TWC data, I construct the following three outcome variables at ages 25–27: (1) an indicator for being employed (measured by quarterly level); (2) average annual real earnings (measured in 2020 dollars); (3) earnings-based college quality following Chetty, Friedman, and Rockoff (2014).[15]

One limitation of the THECB and TWC data is that the data coverage is restricted to Texas. If someone goes out of Texas, I cannot observe their out-of-state educational or workforce outcomes and thus cannot distinguish whether they have moved out of state or did not attend college (in the case of education) or are non-employed (in the case of labor market outcomes). However, this is unlikely to significantly bias the results because Texas has relatively low out-migration (Foote and Stange 2022); I discuss this more in Section 4.3.

4 Empirical Strategy

To estimate the causal effects of school closure on student outcomes, I use two difference-in-differences models to compare the changes in outcomes among students affected by school closures to those who are not. Specifically, I use within-student across-time variation for short-run analyses and within-school across-cohorts variation for long-run analyses. In both strategies, I call "closed schools" the schools that are closed over the time window analyzed (see Section 2 for definition), and I call "control schools" the schools chosen through a matching procedure to control for other time/cohort effects that would have occurred in the absence of treatment

[13] Apart from the data provided by the Texas Higher Education Coordinating Board (THECB), I also have access to data from the National Student Clearinghouse (NSC) covering 98 percent of higher education enrollment in the United States since 2008. This allows me to comprehensively observe students enrolling in post-secondary institutions in and out of Texas after 2008. However, since the period covered by this data is limited relative to the analysis period, I do not use it in my main analysis. Instead, I use it to demonstrate that out-of-state attrition does not meaningfully affect the estimates (Section 5.2).

[14] Unemployment Insurance covers workers if employers pay $1,500 or more in a calendar quarter, or have at least one employee during twenty different weeks in a calendar year. Thus, TWC data does not include earnings from independent contract work, self-employment, under-the-table payments, earnings from federal jobs, and earnings outside Texas. For more details, see https://www.twc.texas.gov/tax-law-manual-chapter-3-employer-0.

[15] Using 1982-1984 birth cohorts, I group individuals by the higher education institution they graduated by age 26. I categorize individuals who have not enrolled in any college by age 26 into separate groups: high school dropouts and high school graduates. For each college and separate groups, I construct the average earnings of the students when they are ages 25-27.

to school closure. I begin by outlining the procedure for selecting control schools, and then describe the estimation strategies for the short- and long-run outcomes.

4.1 Matching Closed Schools to Control Schools

In order for the difference-in-differences estimator to provide a consistent estimate of school closure, the parallel trends assumption must hold: in absence of school closure, the change over time in outcomes would have been the same for students in the closed schools and the control schools. To mitigate concerns regarding differing trends between schools that have closed and those that have not, I choose control schools that share similar observable characteristics with the closed school at the time of closure using a nearest-neighbor matching method.

To begin, I group schools in the same year, the same school type (e.g. elementary schools are only matched with other elementary schools), and the same locale following the NCES locale category, which has 8 categories from 1998-2005 and 12 categories from 2006-2015 based on population size and proximity to populous areas.[16] Once the schools are grouped, I use nearest-neighbor matching within the group using the following school characteristics at the time of closure: the share of Black students, the share of Hispanic students, the share of students receiving free or reduced-price lunch, and the share of students with other economic disadvantages.[17] Essentially, using a scale-invariant distance metric based on observable school characteristics, I calculate the distance among schools and identify the closest schools to each closed school. In the process, I exclude schools in the same district because of concerns about spillover effects.

I choose one control school for each closed school without replacement. Appendix Table A.1 presents the summary statistics after the matching process. As expected with the nearest neighbor matching, the observable characteristics of closed schools are similar to those of the

[16] The eight categories are large city, mid-size city, urban fringe of large city, urban fringe of mid-size city, large town, small town, rural inside MSA, and rural outside MSA. The 12 categories are large city, mid-size city, small city, large suburb, mid-size suburb, small suburb, and three categories of town and rural based on the distance to urban area. In the paper, I define the city and urban fringe (or suburb) categories as urban areas, and the town and rural categories as rural areas. For more details, see https://nces.ed.gov/ccd/pubschuniv.asp

[17] Other economic disadvantages include the following: a) students from a family with an annual income at or below the official federal poverty line, b) eligible for Temporary Assistance to Needy Families (TANF) or other public assistance, c) received a Pell Grant or comparable state program of need-based financial assistance, d) eligible for programs assisted under Title II of the Job Training Partnership Act (JTPA), or e) eligible for benefits under the Food Stamp Act of 1977

matched control schools. Non-Hispanic Black and Hispanic students comprise 68 percent and 66 percent of closed and control schools, respectively, compared to 57 percent of all schools. Economically disadvantaged students account for 71 percent and 69 percent of closed and control schools, respectively, compared to 55 percent of all schools. Moreover, I present the distribution of the number of schools attended during K-12 education, separately for students in closed schools and control schools in Appendix Figure A.2. The majority of students in closed schools experience one additional move compared to both control school students and the state average, supporting the validity of the empirical design. As discussed in Section 5.3, the estimation results are not sensitive to the alternation of matching strategies.

4.2 Estimating the Short-Run Effects of School Closure

I analyze outcome variables observed both before and after the closure: days of absence, days of disciplinary action, and math and reading scores. The analysis begins with the sample including students enrolled in closed and control schools at the time of closure. As I discuss in Section 3, the available sample varies across outcome variables and years of closure: 3-10th grades for behavior and 5-9th grades for test scores from 470 schools. I further restrict the sample to those who are observed in the data three years before and two years after the school closure. In the main analysis, I use all available students in each outcome variable. My final short-run analysis sample includes 61,151 students for test scores, 100,797 students for disciplinary action, and 122,911 students for attendance.

I utilize this sample to estimate difference-in-differences models, where I compare changes in outcomes within each student following a school closure between the closed schools and their matched control schools. My difference-in-differences specification is:

$$Y_{isgt} = \beta Closure_s \times Post_t + \sigma_i + \kappa_{gt} + \eta_{isgt} \quad (1)$$

where Y_{isgt} is an outcome of student i in relative year t ($t = -1$ is the year preceding closure) who was enrolled in school s in match group g at the time of closure. $Closure_s$ is a dummy variable taking 1 if the student i is at a closed school at the time of closure. $Post_t$ is an indicator denoting observations after school closure. I include individual fixed effects, σ_i, and a full set of matched group-by-relative year fixed effects, κ_{gt}. Those account for time-invariant individual characteristics and match group specific trends respectively. β is difference-in-differences estimator measuring the difference in the change in outcomes following a school closure between students from closed and matched control schools. This stacked difference-in-differences estimator has been used as an approach to obtaining estimates of policy effects in the context of staggered adoption designs (e.g., Cengiz et al. 2019; Roth et al. 2023).

For the estimator to be causally interpreted, I must assume the standard parallel trends assumption. This means assuming that outcomes would have changed similarly for students in both closed and control schools within each match group if there had been no closure. To assess the validity of this assumption, I compare the trend before the closure between students from closed and control schools. Namely, I estimate a difference-in-differences model in an event study format. This involves comparing within-student changes before and after the school closure while controlling for secular trends by using the matched control group.

The regression equation takes the following form:

$$Y_{isgt} = \sum_{t=-3, t\neq -1}^{3} \rho_t Closure_s \times \mathbf{1}_t + \sigma_i + \kappa_{gt} + \eta_{isgt} \tag{2}$$

where $t \in \{-3, -2, \ldots, 3\}$ is measured relative to the time of closure, and $\mathbf{1}_t$ is set to 1 when the relative time is t. Other variables are defined in the same way with equation (1). The ρ_t are the difference-in-differences coefficients, which measure within-student change over time in outcomes compared to students in the matched control school, with $t = -1$ as the reference period. Thus, ρ_t where $t \in \{-3, -2, -1\}$ shows pre-trends between closed and matched control schools, and if there are no differential trends in the outcome between students from closed and control schools leading up to the time of closure, these coefficients would be zero.

In the short-run event study format difference-in-differences analysis, I examine a balanced panel of students spanning three years before and four years after the school closure. The purpose of this approach is to remove any potential influence of composition changes that may arise from differential attrition, such as students leaving the Texas public school system after experiencing school closure to private schools or out-of-Texas.

To address concerns about a potential correlation between attrition from the school system and change in outcomes, I further investigate whether there is a differential pattern of attrition between closed and control schools. Appendix Figure A.3 (a) plots the proportion of students from closed and matched control schools appearing in the data each year around school closure. The average attrition rate is 5 percent. Additionally, I use a dummy variable as a dependent variable to estimate equation (2), indicating whether each student is present in the data for a given year. As shown in Appendix Figure A.3 (b), there is no statistically significant difference in attrition rate except for $t = -3$ between closed and control schools, and any observed difference is at most 0.5 percentage points. The findings help to alleviate concerns that students who experience school closure have a systematically different trend of moving out of the Texas public school system compared to students who do not experience it. In Section 5.3, I demonstrate the robustness of short-run analysis results whether using a balanced or unbalanced panel.

4.3 Estimating the Long-Run Effects of School Closure

I focus long-run analyses on outcomes only observed after the school closure in the TEA, THECB, or TWC data: high school graduation, any college enrollment, four-year college completion, college quality based on expected earnings, employment, and yearly earnings. Given that students' long-run outcomes are only observed after school closure, I cannot exploit within-student variation as it relates to changes before and after closure. Instead, I utilize variation across cohorts within a school. Specifically, I compare cohorts enrolled in the school at the time of closure with cohorts who recently graduated, relative to those at matched control schools.

I construct a long-run analysis sample based on graduating cohorts using 146 closed schools between 1998 and 2003. I use six cohorts: the three highest grades experiencing school closure become three "younger cohorts", and three cohorts who potentially graduated within the last three years of school closure become three "older cohorts". For instance, suppose that an elementary school **A** with grades 1–5 closed at the end of the school year 2000. I consider students in school **A** in grades 3-5 at the time of school closure as younger cohorts, and students in the same school in grades 3-5 three years before the school closure as older cohorts.[18] Thus,

[18] Another approach to constructing the sample involves selecting the same school grade both in the year of school

older cohorts would be expected to be enrolled in grades 6-8 at the year of school closure. The final long-run sample experiencing school closure includes 41,753 students in 2–12 grades.

Utilizing this sample to estimate difference-in-difference models, I compare changes in outcomes across cohorts following a school closure between the closed schools and their matched control schools. My difference-in-differences specification is:

$$Y_{iscg} = \gamma Closure_s \times Post_c + \eta_s + \lambda_{cg} + \delta' X_i + \varepsilon_{iscg} \quad (3)$$

where Y_{iscg} is an outcome variable for student i in cohort c who was enrolled in school s in match group g at the time of the closure or three years before the closure. $Closure_s$ is a dummy variable denoting schools experiencing closure. $Post_c$ is an indicator denoting the younger cohorts from closed schools. I include school fixed effects, η_s, and cohort-by-match group fixed effects, λ_{cg}, which account for cohort-invariant school characteristics and flexibly match group specific cohort trends. I also control for student characteristics, X_i, including gender, race, ESL status, and special education status. Moreover, I control for performance measures, including standardized math score, reading score, and days of absence before school closure (i.e., one year prior for younger cohorts and four years prior for older cohorts).[19] To address variations in the significance of individual characteristics across schools, interaction terms between individual characteristics and school dummies are also controlled. γ is the difference-in-differences estimator, measuring the difference in the change in outcomes across cohorts following a school closure between students from closed and matched control schools.

Like short-run effects, to ensure that my causal interpretation is valid, I make the standard parallel trends assumption. Essentially, I assume that graduating cohorts enrolled in both

closure and in preceding years. For instance, in the example of the main text, I can create a comparable sample by choosing the third highest grade from 1998 to 2003. Then, students in the third highest grade from 2000 to 2003 represent younger cohorts, while those from 1998 to 2000 represent older cohorts. However, this approach cannot utilize data from school closures in 1998 due to limitations in data availability. An alternative is to utilize the second highest grade in the year of closure and for the three years prior. In the example, this translates to utilizing fourth grade students from 2000 to 2003. Then, fourth grade students from 2002 to 2003 represent younger cohorts, and students from 2000 to 2001 represent older cohorts. As illustrated in the Appendix Figure A.20, the outcomes using this alternative approach closely resemble those obtained from the baseline analysis. For instance, baseline analysis using only the two highest grades finds a drop in earnings by \$614 while another approach finds a drop by \$489.

[19] If performance measures are not observed, I assign the mean value of the school-by-cohort. If student characteristics are not observed, I assign an additional missing category.

closed and control schools within each match group would have experienced similar changes in outcomes in the absence of closure. To assess the validity of the assumption, I compare "older cohorts" between closed and control schools to see whether differential trends are observed. In other words, the outcomes of older cohorts in closed and control schools, who had left before the schools closed, should exhibit similar trajectories. To show this, I estimate a difference-in-differences model in an event study format. The formal regression equation takes the following form:

$$Y_{iscg} = \sum_{c=-3, c\neq -1}^{2} \pi_c Closure_s \times \mathbf{1}_c + \eta_s + \lambda_{cg} + \delta' X_i + \varepsilon_{iscg} \tag{4}$$

where cohort $c \in \{-3, -2, \ldots, 2\}$ is measured relative to the time of closure, and $\mathbf{1}_c$ is set to 1 when the relative cohort is c. If $c \in \{0, 1, 2\}$, students are in the "younger cohort" (i.e., students who were enrolled in the school at the time of its closure; in the previous example of school **A**, which has grades 1 through 5, $c = 0, 1, 2$ refers to grade 5, 4, and 3, respectively), and if $c \in \{-3, -2, -1\}$, students are in the "older cohort" (i.e., students already graduated from the school when closed; in the previous example $c = -3, -2, -1$ refers to grade 8, 7, and 6, respectively). π_c is the difference-in-differences estimator, measuring differences between closed and control schools in cohort c relative to the omitted cohort. The standard errors are clustered at the school-by-cohort level.[20]

In the long-run event-study format difference-in-differences analysis, I examine adjacent six cohorts in the same school around school closure assuming that these adjacent cohorts are similar except for the experience of school closure. One might still have concerns about systematically different moving-out patterns among the cohorts from closed schools *before* school closures compared to control schools.[21] To assuage the concern, I conduct a balance test across these cohorts. I use demographic characteristics including economic status and racial composition and performance measures including standardized test scores and days of absence measured before the school closure as dependent variables to estimate equation (4). As depicted in Appendix

[20] If two grades exist at the time of closure, the highest and second highest grades at the time of closure take 0 and 1 of c, and the highest and second highest grades two years before the closure take -2 and -1 of c. Thus, the regression is not balanced when $c = 2$ or $c = -3$. In the estimation of equation (4), I use a balanced panel where at least three grades exist while for equation (3) I use the entire sample. In section 5.3, I compare results using balanced and unbalanced panels, presenting consistent findings.

[21] Concerns about systematically different moving-out patterns *after* school closures are discussed in Section 5.2.

Figure A.4, there are no significant differences in demographic characteristics and performance measures across school cohorts.[22]

5 Estimation Results

5.1 Short-Run Effects on Student Outcomes

Appendix Figure A.6 illustrates the raw trends of short-run outcomes for closed and control schools around school closure. Sub-figures (a) and (b) show standardized math and reading scores, standardized by grade and year. Prior to school closure, both closed and control schools exhibit comparable trends over the three-year period, with similar levels. The absolute raw difference remains consistently below 0.02 standard deviations. However, following school closure, a noticeable drop in average test scores of closed schools emerges, leading to a divergence in the trends between closed and control schools. Sub-figures (c) and (d) depict days of absence and days of disciplinary action. These outcomes also demonstrate similar trends and levels in the three years preceding the school closure and start to deviate after experiencing school closure. The raw trends provide suggestive evidence that closed and control schools have similar levels and trends before closures and that students in closed schools deteriorate after experiencing school closure.

Figure 2 presents event study estimates, particularly plotting the coefficients and 95% confidence intervals of the coefficient ρ_t from equation (2). First of all, the coefficients before the school closures are close to zero and not statistically significant. The absence of pre-trends is supportive of the parallel trend assumption that is required to interpret the coefficients for post-closure as causal effects. Sub-figures (a) and (b) depict a decline of 0.04 and 0.03 standard deviations in standardized math and reading scores, respectively, following school closure. These scores subsequently recover to their initial levels within three years. Sub-figure (c) presents an immediate increase in days of absence by 0.2 days after closure, which persists for four years post-closure.[23] School closures also result in a 0.3-day increase in the days of disciplinary action

[22] Moreover, I estimate the same regression using short-run outcome variables one year after closure to see whether I can observe changes in short-run outcomes for younger cohorts compared to older cohorts. As presented in Appendix Figure A.5, younger cohorts experience drops in test scores and an increase in days of absence while it is noisier than short-run analysis.

[23] Goodman (2014) finds that each absence induced by bad weather reduces the math score by 0.05 standard

immediately after closure, which further escalates to 0.9 days after four years.

Given the significant increase in the number of days of disciplinary action following the school closure, I conduct a separate analysis for days of in-school suspensions, days of out-of-school suspensions (including expulsions), and intensive/extensive margins of disciplinary actions. These results are presented in Appendix Figure A.7. The increase in days of in-school suspensions is at most 0.2 days and then declines back to around 0.1 days. In contrast, the number of days of out-of-school suspensions and expulsions increases by 0.2 days and keeps increasing following four years up to 0.8. Moreover, I find an increase in both extensive margin—whether students have at least one day of disciplinary action—and intensive margin—analysis among students with at least one day of disciplinary action.

Table 1 reports estimation results from equation (1), in which periods after school closure are pooled as After 1-2 Years for $t \in (0,1)$ and After 3-4 Years for $t \in (2,3)$. As shown in columns (1) and (2), the experience of school closure decreases math and reading scores by 0.03 standard deviations following two years, but the decreased scores recover to the original level in four years. Columns (3) and (4) reveal that the days of absence and days of disciplinary action increase after two years by 0.13 days and 0.36 days, which is a 2 percent and 15 percent increase relative to the pre-closure means. Days of disciplinary action further increase after 3-4 years up to 0.63 days.

I explore heterogeneous effects across the school and student characteristics. For school characteristics, I estimate equation (2) separately for sub-groups defined by the following characteristics: region, school quality, and school quality change. The region is divided into urban and rural based on the NCES locale category. School quality is measured by the average math and reading test scores of each school over the four years preceding the school closure and divided into terciles: low, middle, and high. School quality change is measured by the difference in school qualities between a closed school and the nearest school.[24] The distribution of difference is also divided into terciles: worse, similar, and better.[25]

deviations. A back-of-the-envelope calculation shows that the reduction in absences accounts for approximately 22 percent of the observed decrease in math scores ($0.05 \times 0.198 = 0.0099$).

[24] I do not use school quality of attending school after school closure to avoid selection of students (Brummet 2014). The correlation between the closest school and the attending school after school closure is 0.45.

[25] It is divided to have an equal number of schools in each category. Then, school quality changes ranging from -0.84 to -0.032 standard deviations are classified as "worse." Changes between -0.031 and 0.18 standard

Figure 3 presents the estimated coefficients and their corresponding 95% confidence intervals separately for 1-2 years and 3-4 years after school closure. Although there is considerable overlap in the confidence intervals across the estimates, a few tendencies are noteworthy. First, the overall effect is negative, suggesting that school closures have adverse consequences on most students. Second, the negative effect is more significant for urban school closures, particularly regarding days of absence. Third, displaced students from originally low-performing schools experience a significant increase in days of disciplinary action (0.9 days; 27% increase from the subgroup mean). Lastly, students displaced to worse-performing schools experience a larger drop in test scores (0.06 standard deviations) while students displaced to better-performing schools experience a larger increase in days of disciplinary action (1.1 days; 34%).

To analyze the heterogeneous impact of school closures based on individual characteristics, I divide the sample by race/ethnicity, economic disadvantage status, and grades when the school is closed. The estimated coefficients and associated 95% confidence intervals are presented in Figure 4 separately for 1-2 years and 3-4 years. Despite the noise in the point estimates, I take them at face value, revealing several tendencies. Firstly, Hispanic students experience more pronounced adverse impacts on math scores and days of absence (0.5 days; 6.4%) while Black students experience a more substantial rise in days of disciplinary action (1.5 days; 33%).[26] Meanwhile, White students experience a greater drop in reading scores (0.06 standard deviations), which is not fully recovered in 4 years. These disparities across racial/ethnic groups highlight that each group is affected to varying degrees across outcomes, with Hispanic students generally experiencing the most significant overall effects. Secondly, economically disadvantaged students have more significant increases in days of absence (0.6 days; 7.3%) and days of disciplinary action (1.1 days; 38%) while not disadvantaged students experience a larger and continuous decrease in reading scores (0.05 standard deviations). Lastly, negative effects on test scores grow over time for students who were in higher grades at the time of closure, while students in lower grades appear to recover over time.

I explore school-level changes including peer quality and the number of teachers per student

deviations are categorized as "similar," while changes from 0.19 to 2.67 standard deviations are classified as "better."

[26] This aligns closely with the literature addressing racial disproportionality in exclusionary disciplines (Anderson and Ritter 2017; Barrett et al. 2021; Losen et al. 2015).

after experiencing school closures. I construct peer quality measures using the yearly school average of math and reading test scores around years of school closures and use them as a dependent variable to estimate equation (2). In the construction of peer quality measures, I exclude displaced students after experiencing school closures (i.e., $t \geq 0$) and receiving schools if more than 70 percent of their students are displaced students (<15% of students). Figure 5 (a) and (b) illustrate the changes in peer quality, showing a decrease in math and reading scores by 0.05 right after closure.[27] However, the expected quality shows the opposite pattern. I construct expected quality measures using average math and reading test scores of each school over the four years *preceding* the school closure (i.e., $t \in \{-4, \ldots, -1\}$) and use them as a dependent variable to estimate equation (2). As shown in (c) and (d) of the Figure, students move to schools that originally served better-performing peers on average compared to students from closed schools. After moving, expected school average math and reading scores increase by 0.02.[28]

To further understand why students do not have high-performing peers even after transitioning to originally better-performing schools, I examine average test score changes of receiving schools before and after school closure (i.e., $t = 0$ and $t = -1$), dividing students into original students and move-in students. Appendix Table A.2 presents that both groups exhibit a decline in test scores, with the move-in group showing a larger decline. Specifically, move-in students demonstrate a decline of -0.062 to -0.082 standard deviations in test scores, while original students show a decline of -0.011 to -0.020 standard deviations between students observed in $t = 0$ and $t = -1$. This suggests that the change in school quality is a combination of changes in student composition, potentially resulting from alterations in attendance zones along with school closures, and spillover effects coming from having new students.[29]

[27] Based on Burke and Sass (2013), a one standard deviation increase in classroom peer quality is associated with changes in math scores of 0.0292, -0.0013, and 0.0088 for elementary, middle, and high school students, respectively, as well as 0.0271, 0.0087, and 0.0124 in reading scores. Considering the composition of my sample (45% elementary, 43% middle, and 10% high school students), the expected decrease in test scores due to changes in peer quality is calculated as follows: (0.45*0.029-0.43*0.0013+0.10*0.0088)*-0.05=-0.0007 standard deviation for math and (0.45*0.0271+0.43*0.0087+0.10*0.0124)*-0.05=-0.0009 standard deviation for reading.

[28] In Appendix Figure A.8, I also present outcomes of days of absence and days of disciplinary action after standardization, which also present consistent results.

[29] It is important to acknowledge the limitations of comparing the same school over two years when examining the changes in school quality following closures. This approach might introduce the potential influence of other secular trends that are unrelated to school closures. Therefore, it is crucial to exercise caution in interpreting these results and recognize the need for a more rigorous analysis of receiving schools in future research.

Additionally, I analyze changes in school-level employment, categorizing staff into three groups: teachers, teaching support staff, and social support staff. Using a metric of full-time-equivalent (FTE) positions per 1000 students, I estimate the equation (2). As depicted in Appendix Figure A.9, there is a reduction of 3.3 full-time-equivalent positions in total school-level employment following school closure. While all categories experience a decrease in employment, the decline is more pronounced in teaching support staff (-1.8) and teachers (-1.5).

5.2 Long-Run Effects on Educational and Economic Outcomes

Figure 6 presents estimates of the effects of school closure on long-run educational outcomes by age 26 and economic outcomes at age 25-27. It includes coefficients and associated 95% confidence intervals from the estimation of equation (4), in which I estimate the event study form of the difference-in-differences model. Most of the long-run results show no indication of significant pre-trends, which is supportive evidence in favor of the parallel trends assumption needed to interpret the difference-in-differences estimator as the effect of school closure. For younger cohorts that did experience a school closure, I find overall negative effects on post-secondary education and labor market outcomes. Moreover, I observe a distinct pattern in which the negative effects are less pronounced for the highest grade students ($c = 0$) in the year of school closure, particularly with respect to the labor market outcomes. Those would have likely moved even in the absence of school closures because they are likely in a terminal grade, and therefore faced less disruption than other grade students who would not have moved. However, they might still experience negative effects due to the challenges of integrating into new environments, including adjusting to a new peer group, or any negative impacts on staff morale or turnover in the year leading up to closure.

Table 2 reports estimation results from equation (3), in which I pool the younger cohorts to examine the average effects of school closures on long-run outcomes. I find that experiencing school closure decreases the likelihood of graduating from high school by 2.3 percentage points (2.5%)[30], enrolling in any college by 1.2 percentage points (2.5%), and obtaining a bachelor's degree by 0.7 percentage points (5.1%), as well as decreases the college quality by \$337 (1.1%)

[30] As mentioned in Section 3, I define high school graduation as students enrolling in 12th grade for more than 50 days due to data glitches in the graduation files. Appendix Figure A.10 presents the robustness of this cutoff in attending days.

by the age of 26. I further find that experiencing school closure makes students 1.0 percentage points (1.9%) less likely to be employed and leads to $698 (3.4%) lower annual earnings at ages 25-27. These results underscore the importance of examining long-run outcomes. Some previous studies conclude that the adverse effects of school closures do not persist, showing that the negative impact on test scores tends to dissipate over time (Brummet 2014; De Witte and Van Klaveren 2014; Engberg et al. 2012; Özek, Hansen, and Gonzalez 2012). However, my findings highlight long-lasting negative effects, even if test score disruptions recover on average, aligning with the literature on childhood interventions (e.g., Chetty et al. 2011; Heckman, Pinto, and Savelyev 2013). Moreover, the decrease in expected earnings from their final educational attainment (college quality) only explains approximately one-third of the reduction in earnings, suggesting that the effects of school closures are not limited to educational attainment.

I explore heterogeneous effects across the school and student characteristics for long-run outcomes. Appendix Figure A.11 presents heterogeneity across school characteristics. While overall negative effects exist, a few trends emerge.[31] First, experiencing school closure in urban schools has a stronger negative impact. Second, students originally in low-performing schools experience more pronounced effects on their educational outcomes. Third, students who transition to better-performing schools tend to exhibit more pronounced negative effects on college quality while students moving to worse-performing schools experience a significant drop in yearly earnings. It is consistent with class rank literature known as the big-fish/little-pond effect (Denning, Murphy, and Weinhardt 2023; Marsh et al. 2008), where individuals gain confidence when they are highly ranked in their class or school, resulting in higher educational achievement.[32] This suggests that even when students move to schools with higher-performing peers, they could still encounter adverse consequences.

Appendix Figure A.12 presents heterogeneity across student characteristics. While much of the confidence intervals overlap across estimates, a few patterns are worth noting. First,

[31] I find overall negative long-run effects although short-run analyses show for some groups recovery from negative impacts and even positive outcomes. This discrepancy arises because the short-run sample is more limited compared to the long-run sample. While the majority of the long-term negative effects come from students in higher grades, short-run outcomes, particularly test scores, are primarily available for elementary students. This is due to data availability, as discussed in Section 3, and the analysis design, which requires students to be observed for three years before and two years after the school closure.

[32] Moreover, I find days of disciplinary action increase more significantly for students transferring to better-performing schools, which might imply that adapting to better-quality schools is more difficult for students.

students in higher grades are more negatively affected by school closure while students in grades 3-5 overall do not experience significant long-run negative effects. Second, while racial and economic status differences are not pronounced, Hispanic and economically disadvantaged students generally experience larger negative effects which are more pronounced in the comparison after rescaling based on sub-group means in Appendix Figures A.13 and A.14. Corresponding well to the short-run heterogeneity analysis, the results present that the negative effects are more pronounced on students in higher grades and more vulnerable situations such as those from originally low-performing schools, economically disadvantaged families, and racial/ethnic minorities.

As I discussed in Section 3, I do not observe post-secondary education and labor market outcomes if students leave Texas. If experiencing school closure systematically changes the attrition pattern, the interpretation of estimation is complicated. Providing the following evidence, however, I argue that differential attrition is unlikely to change meaningfully my estimation results. In the following paragraphs, I discuss this issue in three layers: (i) attrition right after school closure, (ii) attrition transitioning from K-12 to post-secondary education, and (iii) attrition to the labor market.

I assess the first layer by examining attrition rates after closure between students from closed and control schools. Appendix Figure A.3 (c) plots the proportion of students in a long-run analysis sample from closed and matched control schools, separately for younger and older cohorts, appearing in the data each year after school closure. In Appendix Figure A.3 (d), I plot estimated coefficients and associated 95% confidence intervals from equation (2), in which the dependent variable is an indicator for being observed in the data. I compare the attrition rates of students from closed and control schools in younger and older cohorts separately. The results show that there is no significant difference in attrition trends between students from closed and control schools. Moreover, any observed difference in attrition rate between closed and control schools is at most 0.9 percentage points.[33] This finding provides reassurance that sample attrition right after closure was not a major concern, as students did not differentially

[33] To see the potential impact of the attrition, I estimate Lee (2009) bounds assuming differential attrition in response to a school closure of 0.9 percentage points. The estimated bounds are presented in Panel A of Appendix Table A.4. While these Lee bounds cover a range of estimates, the bounds exclude zero for most of the outcomes.

leave in the imminent closure.

To address the second, I exploit National Student Clearinghouse (NSC) data, which covers 98 percent of higher education enrollment in the United States. As discussed in Section 3, the available data of higher education enrollment out-of-Texas only begins in 2008, which does not fully cover the sample. Therefore, it is not used in the baseline analysis. However, it is informative to examine whether out-of-state enrollment was affected by school closures. Using an indicator for out-of-state enrollment as the dependent variable, I estimate equation (3) and present the results in Appendix Table A.3. The estimates show that younger cohorts from closed schools are 0.3 percentage points less likely to enroll in college out-of-Texas relative to students from matched control schools, while it is not statistically significant at the 5% level. This finding alleviates concerns that the baseline estimates for post-secondary education outcomes overestimate the effects of school closures due to out-of-state enrollment.

In the final layer of analysis, I present multiple pieces of evidence to support the conclusion that attrition to the labor market outside Texas does not alter the main findings. Firstly, previous research has shown that Texas has a relatively low out-migration rate of young workers, indicating that the effects of school closures on labor market outcomes within Texas are likely to be a robust estimate (Foote and Stange 2022). Secondly, when excluding individuals with no earnings in Texas, I obtain similar effects on earnings as in the baseline analysis (Appendix Table A.5). Thirdly, using a school quality measure based on their highest education level and institution, I find consistent results showing a decrease in expected earnings among the sample of individuals. Lastly, I perform a bounding exercise with the non-zero earning sample, attributing all the decrease in employment rates after school closure to attrition to the labor market outside Texas (Lee 2009). The Lee bounds, presented in Panel B of Appendix Table A.4, are mostly in the negative range.[34] The evidence suggests that even under the extreme assumption, the main implications remain unchanged.

[34] Although the lower bound is a positive number, it is small and insignificant. Furthermore, regressing employment on standardized college quality using the same setting as in equation (3) gives an estimate of 0.1 (0.002), implying that the lower bound is less plausible.

5.3 Sensitivity Analysis

I also examine the sensitivity of my estimates to alternative ways of choosing matched control schools to closed schools. Appendix Figures A.21 and A.22 present coefficients and associated 95% confidence intervals from estimating equations (1) and (3) respectively, using following alternative matching strategies: (1) I add more variables (share of ESL and share of special education) when calculating distance metric for nearest-neighbor matching; (2, 3) I add enrollment and its changes when measuring the distance; (4, 5) I add test scores and those changes when measuring the distance; (6) I add enrollment and test scores and those changes when measuring the distance; (7) I drop distant matches, (8) I reverse order of matching since order matters in matching without replacement, and (9) I match on school characteristics of one year before the school closure. I provide a baseline estimate at the top of each sub-figure for comparison. The name of each alternative matching method is followed by the percentage of the matched control schools that are unchanged from the baseline model. For instance, 67 percent of matched control schools are changed after adding more variables (share or ESL, share of special education). Reassuringly, results are robust across these alternative matching strategies while control schools change 65 percent on average from the baseline control schools.

I further test the robustness to the matching strategy by using the synthetic difference-in-differences method (Arkhangelsky et al. 2021). Restricting the pool to students in the same year, same school type, and same locale, I individually match students from closed schools to multiple students with weight to minimize parallel trend violations.[35] Reassuringly, estimation results in Appendix Table A.6 from the synthetic difference-in-differences are similar to baseline estimates. If anything, the synthetic difference-in-differences estimates are somewhat larger. Furthermore, Appendix Figure A.23 plots outcome trends from the implementation of synthetic difference-in-differences, mimicking the raw trend in Appendix Figure A.6. All outcomes show a very similar pretend. After experiencing school closure, test scores drop, and behavioral issues increase among students from closed schools. In other words, estimated coefficients obtained without any further discretion regarding the matching criteria exhibit similarities with

[35] I randomly select 10,000 students from the donor pool if students in the donor pool are over 10,000 because of the computational burden.

the baseline coefficients.

My short-run event study analysis makes use of a balanced panel of students observed in TEA data three years before and four years after school closure. I examine robustness analysis by providing estimation results of equation (2) with different sample specifications. In Appendix Figure A.15, I explore the sensitivity of my estimates to using an unbalanced sample. The unbalanced sample is relatively unstable, but overall patterns are similar to baseline results.

Appendix Figure A.16 presents estimation results using a sample excluding closed schools where new schools come in next year at the same address as closed schools to alleviate the concerns of coding changes or school reform without physical school closure. The overall trends observed closely mirror those seen in the baseline results. However, it is noteworthy that the effects appear to be more pronounced when compared to the baseline sample. For instance, test scores drop more than 0.05 standard deviations right after school closures while those are around 0.03 standard deviations in baseline estimation. This implies that certain schools identified as closed within the scope of my research may potentially be instances of alterations in school coding or not all students may be affected by the closure.

To see whether the effects of school closure vary over time, I estimate the effects after dividing school closures into three periods: 1998-2003, 2004-2009, and 2010-2015. The estimated coefficients and associated 95% confidence intervals for three periods are separately presented in Appendix Figure A.17. The overall trends across periods seem similar except for a few noticeable patterns. First, days of absence exhibit an increase immediately following school closures, but then it follows different trajectories across periods. In the instance of early closures, days of absence drop below their original level while in cases of middle and later closures, the elevated days of absence persist. Second, days of disciplinary action increase sharply and maintain the elevated level in early closures, but it continuously increases in the middle and later closures.

My long-run event study analysis relies on a balanced panel encompassing three younger cohorts at the time of school closure and three older cohorts immediately preceding the observed school closure. It also incorporates demographic and performance control variables. Estimating equation (4), I examine robustness analysis employing different sample specifications and control variables. In Appendix Figure A.18, I present estimation results using an unbalanced sample. These results are juxtaposed with the baseline sample for reference. Overall patterns closely resemble those observed in the baseline results.

Appendix Figure A.19 depicts estimation results without controlling for performance variables (test scores and days of absence). General patterns observed remain largely consistent regardless of whether performance measures are controlled in the analysis, while results obtained without the inclusion of performance measures tend to exhibit instability and weaker effects. Moreover, Appendix Table A.5 presents estimation results from equation (3) in three levels of controls: i) without demographic and performance controls, ii) with demographic controls, iii) and with demographic and performance controls. The estimation results exhibit consistency across these different specifications.

6 Discussion

The impact of school closure on students is significant, with long-lasting consequences for their human capital accumulation and labor market performance. To better understand the magnitude of these effects, it is helpful to compare my long-run estimates with existing research on the long-run effects of school inputs and intervention/disruption. Specifically, my findings suggest that experiencing school closure reduces college enrollment by 1.2 percentage points. For instance, studies by Chetty et al. (2011) and Dynarski, Hyman, and Schanzenbach (2013) find that a 30 percent reduction in class size in Project STAR for two years led to a boost in college enrollment of 1.8 and 2.7 percentage points, respectively. Meanwhile, Chetty, Friedman, and Rockoff (2014) find that a one standard deviation increase in teacher value added in one grade increases college enrollment by 0.82 percentage points. Thus, my estimates suggest that experiencing school closure is equivalent to a 13 to 20 percent increase in class size for two years or a one standard deviation decrease in teacher quality for 1.5 years in terms of its impact on college enrollment.

Regarding labor market outcomes, Chetty et al. (2011) find that a one standard deviation increase in class quality within schools, which incorporates peer quality, teacher quality, and random class-level shock, increases earnings by 9.6 percent at age 27. Similarly, a one standard deviation improvement in teacher value-added for one year is associated with a 1.34 percent increase in earnings at age 28 (Chetty, Friedman, and Rockoff 2014). In comparison, my estimated effect of school closure is a 3.4 percent decrease in earnings at ages 25-27, which is equivalent to a 0.35 standard deviation decrease in class quality for one year or a one standard deviation decrease in teacher quality for 2.5 years. Moreover, when considering disruptive events, Cabral et al. (2021) find that a school shooting in Texas high schools leads to a 13.5 percent reduction in earnings at ages 24-26. That is, my estimated effect of school closure is equivalent to 25 percent of the effect of experiencing a school shooting in high school.

I further compare my estimates to potential policy experiments. Chetty, Friedman, and Rockoff (2014) estimate that replacing teachers in the bottom 5 percent based on value-added with average teachers for one year would increase the present discounted value of earnings of the students in the classroom by $250,000. Carrell, Hoekstra, and Kuka (2018) estimate that one year exposure to a disruptive student reduces the present discounted value of lifetime earnings by $81,000 to $105,000. Under the same assumption, my estimate suggests that a classroom of 25 students will experience a reduction of $443,700 in their present discounted value of lifetime earnings.[36] Thus, my estimates imply that experiencing school closure has roughly the same effect on future earnings as replacing a bottom 5 percent teacher with an average teacher for about 1.8 years. Or it has similar effects as having one more disruptive classmate for five year.

Lastly, Cabral et al. (2021) estimate that the annual aggregate present discounted value of the cost of school shootings in the US from students who experience it is $5.8 billion. Under the same setup, I estimate the annual aggregate present discounted value of the cost of school closures based on the effects on annual earnings at ages 25-27.[37] With approximately 250,000 students being affected by school closures annually from 2010 to 2021 (NCES 2022), the total annual cost of school closures, resulting from displaced students, amounts to about $7.6 billion.

[36] I assume that the percentage impact of school closure on earnings at age 25-27 is constant over the life cycle. I also assume that there are no general equilibrium effects and that, to facilitate comparison, the present discounted value of earnings from children at age 12 are $522,000 from Chetty, Friedman, and Rockoff (2014). This estimate follows Krueger (1999), assuming that earnings are discounted at a 3 percent real annual rate. The effects on one classroom will be $17,748*25=$443,700.

[37] Assuming a persistent average effect of exposure through age 64 and a 3 percent real discount rate on earnings, the earnings stream from ages 15-64 in the March CPS is discounted back to age 15. For comparison purposes, I use the calculated present discounted value of lifetime earnings, which is $888,844. Based on this, the estimated reduction in the present discounted value of lifetime earnings per student is $30,220, calculated as $888,844 multiplied by the estimated effect size of 0.034.

This estimation implies that the annual cost of school closures is approximately 1.3 times the cost of school shootings in the US.[38]

7 Conclusion

According to OECD (2018), school closures are becoming an inevitable consequence of declining populations. This issue of diminishing school-age populations is no longer confined to East Asian and European countries; it is a global phenomenon, extending across North and Latin Americas, as well as South Asia (Hannum, Kim, and Wang 2022). Notably, over the last two decades, China has shuttered approximately 40,000 primary schools, constituting 70 percent of their total (National Bureau of Statistics of China 2023), while France has closed 8,000 schools, accounting for 14 percent of their total (Ministry of National Education, Higher Education and Research 2023). In Brazil, rural primary schools have experienced a 31 percent reduction, dropping from 88,000 to 61,000 between 2007 and 2017 (Brazil Ministry of Education 2020). In Rajasthan, India, in 2014, the government initiated the merger of 17,000 out of the over 80,000 government schools (Chowdhury 2017). Despite the pervasive global utilization of school closure policy, evidence of the effect on students is limited, which calls for research quantifying the causal effects of school closure on students' short- and long-run outcomes (Tieken and Auldridge-Reveles 2019).

Using rich administrative data from Texas, I explore the effects of school closure on displaced students' outcomes in the short-run including test scores and behavioral problems, and long-run outcomes including post-secondary education and labor market outcomes. I analyze school closures between 1998 and 2015 in Texas using difference-in-differences empirical strategies and find that school closures negatively impact displaced students both immediately following school closure and over a decade later when they are young adults. I find that school closure leads to a drop in test scores and an increase in behavioral issues in the following years. I further

[38] It is important to note that the calculated costs are not net costs. I have chosen not to calculate potential benefits in my analysis. My focus is to highlight the hidden costs associated with school closures that have been overlooked, rather than to compare costs with benefits to evaluate the economic gains of the policy. School closures have the potential to bring financial benefits to school districts through economies of scale. The benefits might lead to better outcomes for students who are in school districts but do not experience school closures including future cohorts (Bifulco and Schwegman 2020). However, it is challenging to estimate the benefits of school closures without access to school-level budget information and feeder pattern of schools, which are not accessible in my data.

find that school closure leaves negative impacts on post-secondary education and labor market outcomes. Heterogeneity analysis reveals that the adverse effects are more pronounced among students in higher grades, Hispanic students, as well as those from originally low-performing schools and economically disadvantaged families. Furthermore, displaced students do not have better-performing peers after experiencing school closure.

The long-run negative impacts of school closures are sizable. Estimated results suggest that the adverse effects are large enough to offset the benefits equivalent to a 16 percent decrease in class size over two years for college enrollment, or a 0.35 standard deviation increase in overall class quality for one year in terms of yearly earnings. My back-of-the-envelope calculations further suggest that the annual cost of school closures due to displaced students is about $7.6 billion in the US, without considering the potential benefits of school closures.

The findings of long-run negative impacts and concentration on certain groups of students suggest that the current implementation of school closure policy is not sufficient to address the disruption for displaced students adequately. Future research is necessary to explore ways to mitigate the adverse effects such as phasing schools out rather than abruptly closing them.

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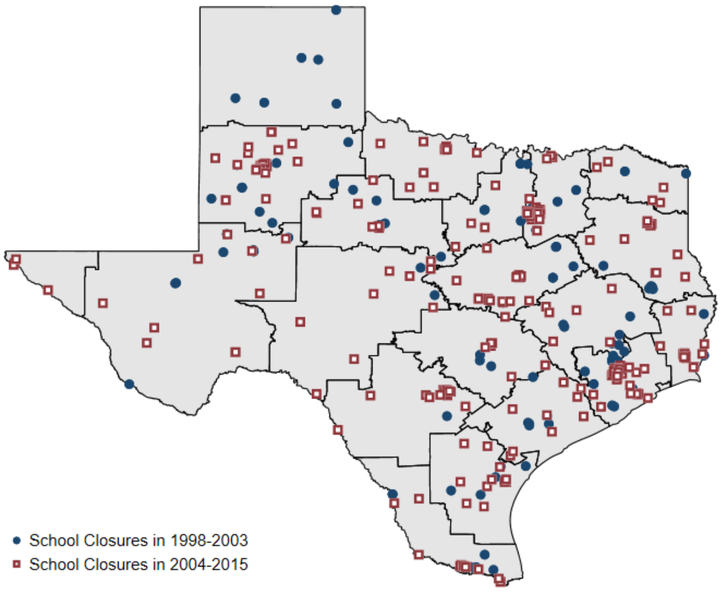
8 Figures and Tables

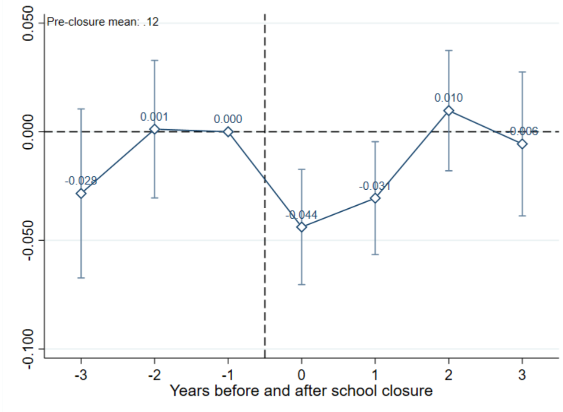
Fig. 1. Map of School Closures at Texas Public Schools in 1998-2015



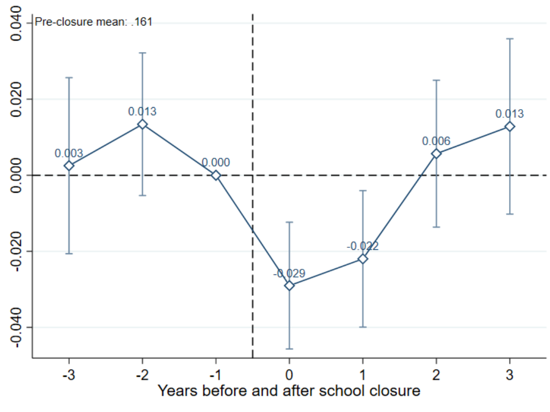
Notes: The figure presents the locations of 470 public school closures: 146 school closures in 1998-2003 used in both short- and long-run analysis in Texas and 324 school closures between 2004-2015 used in only short-run analysis. To be considered a closed school, the school must be officially listed on TEA as a closed school, be a non-charter instructional campus in a regular and independent district, and have been observed during the previous period (1994–1997).

Fig. 2. Short-Run Effects of School Closure on Student Outcomes

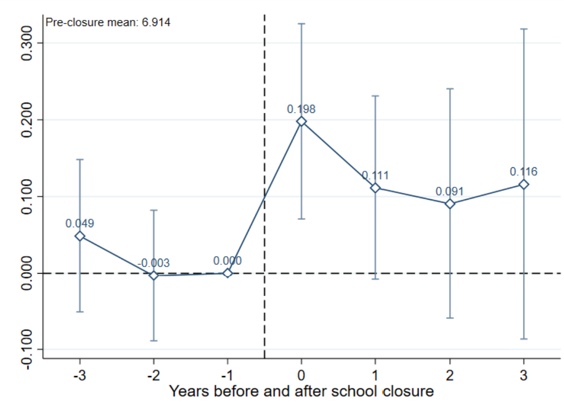
(a) Standardized math score



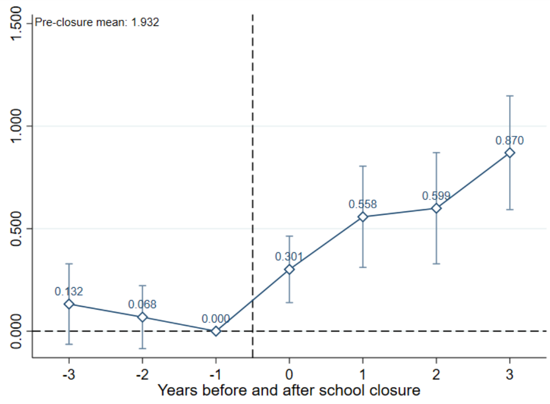
(b) Standardized reading score



(c) Days of absence



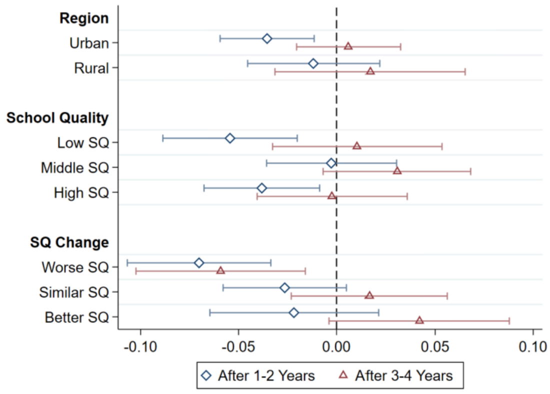
(d) Days of disciplinary action



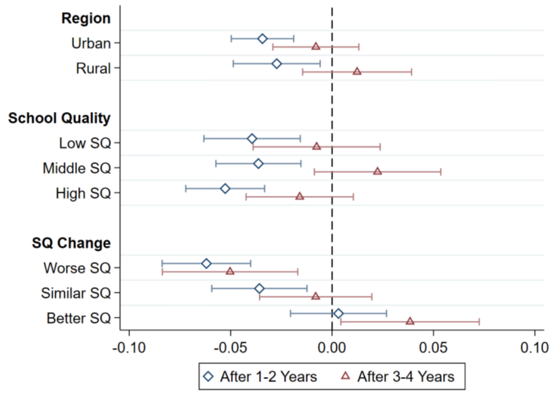
Notes: The figures present the coefficients, ρ_t, and 95% confidence intervals from equation (2).These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) is the omitted category. The regression includes individual and match group-by-year fixed effects. Math and reading scores are standardized by year-by-grade level. Standard errors are clustered by school at $t = -1$.

Fig. 3. Short-Run Effects of School Closure on Student Outcomes: Heterogeneity by School Characteristics

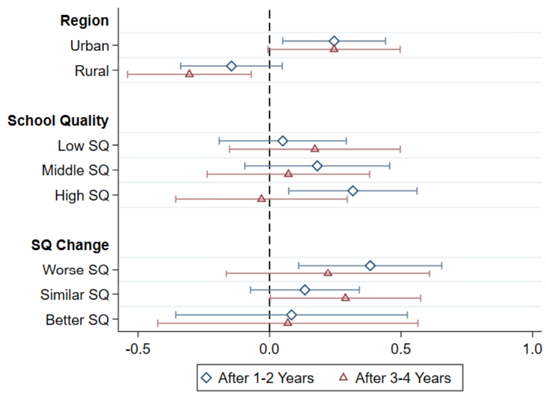
(a) Standardized math score



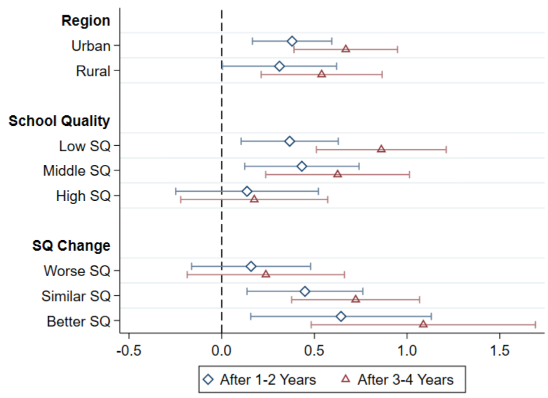
(b) Standardized reading score



(c) Days of absence



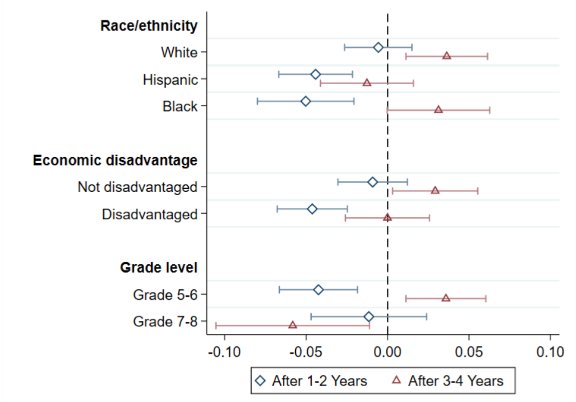
(d) Days of disciplinary actions



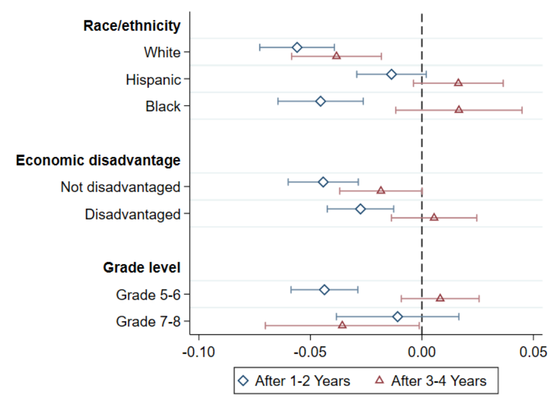
Notes: The figures present the coefficients, β, and 95% confidence intervals from equation (1) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote years after a school closure. The region is defined based on the NCES locale categories, with urban areas including cities and urban fringes, and rural areas including towns and rural areas. School quality is measured by the average test scores of the students in closed schools before the closure. The difference between the average test scores of students from the closed school and the nearest school of the same school type is used to measure school quality change (SQ Change). The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$.

Fig. 4. Short-Run Effects of School Closure on Student Outcomes: Heterogeneity by Student Characteristics

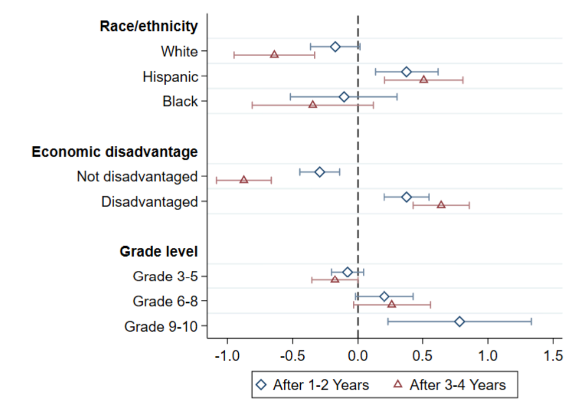
(a) Standardized math score



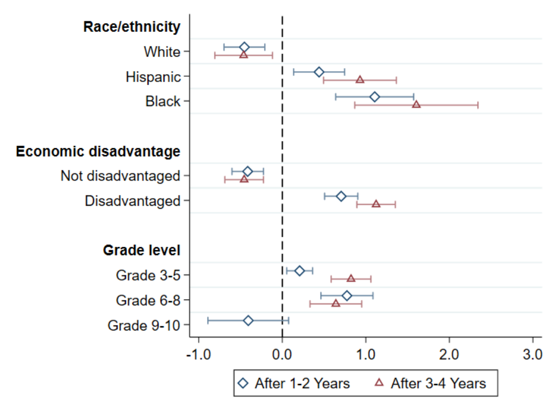
(b) Standardized reading score



(c) Days of absence



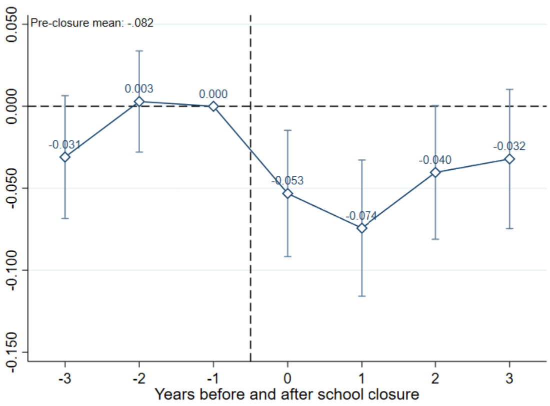
(d) Days of disciplinary actions



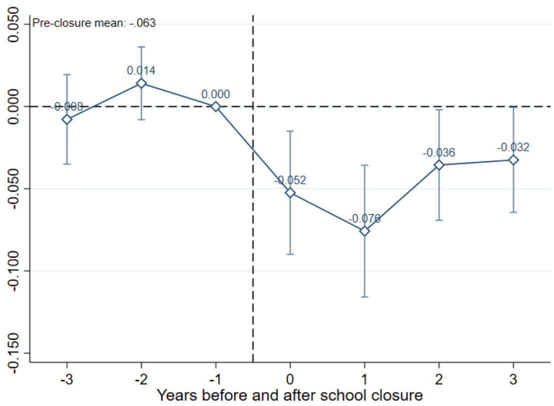
Notes: The figures present the coefficients, β, and 95% confidence intervals from equation (1) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote years after a school closure. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$.

Fig. 5. Peer and Expected School Quality Changes Before and After School Closures

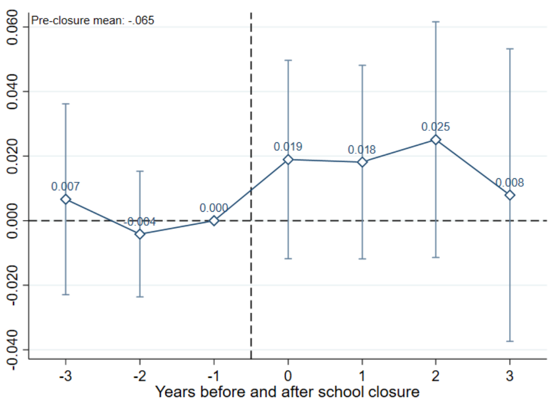
(a) Peer quality: standardized math score



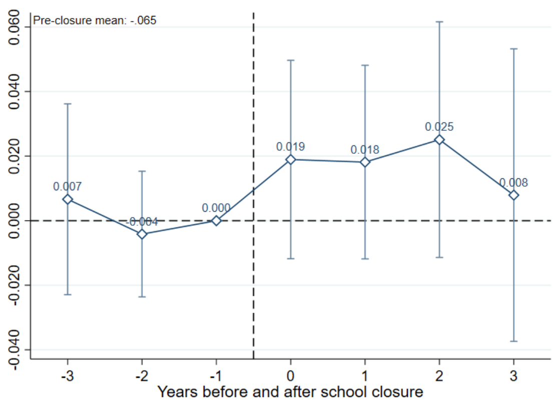
(b) Peer quality: standardized reading score



(c) Expected quality: standardized math score



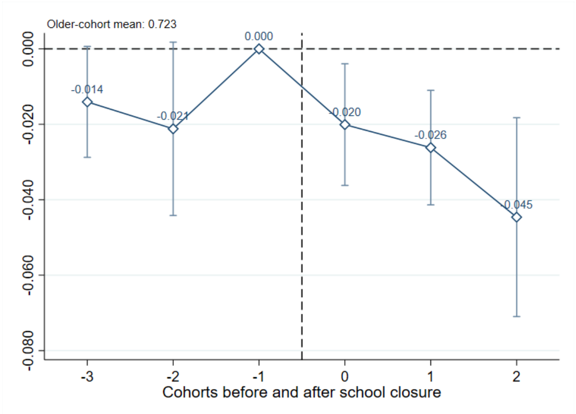
(d) Expected quality: standardized reading score



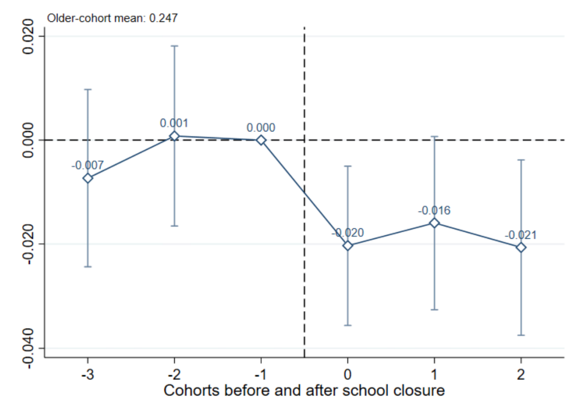
Note: The figures present the coefficients, ρ_t, and 95% confidence intervals from equation (2), where the outcome variables are the school average test scores. When it comes to sub-figures (a) and (b), the outcome variables are yearly school average test scores and the construction of average values excludes displaced students from the calculations after school closure (i.e., $t >= 0$). Moreover, I exclude receiving schools if more than 70% of their students are displaced students. For sub-figures (c) and (d), the outcome variables are the school average over the four years preceding the school closure (i.e., $t \in \{-4, \ldots, -1\}$). These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$.

Fig. 6. Long-Run Effects of School Closure on Educational and Labor Market Outcomes

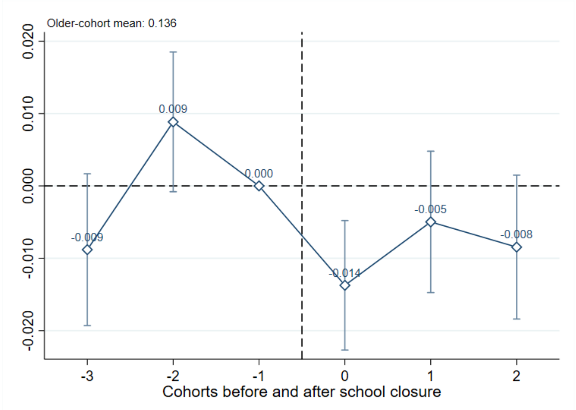
(a) High school graduation



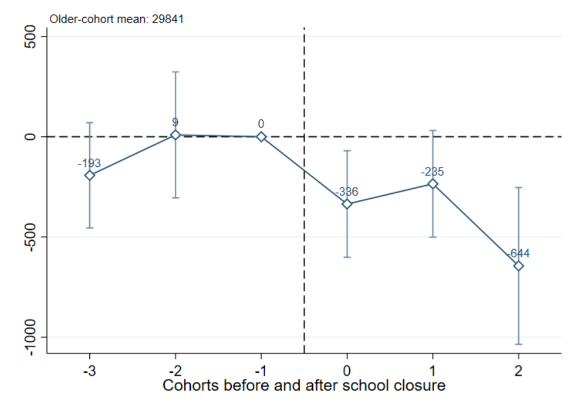
(b) Any college enrollment



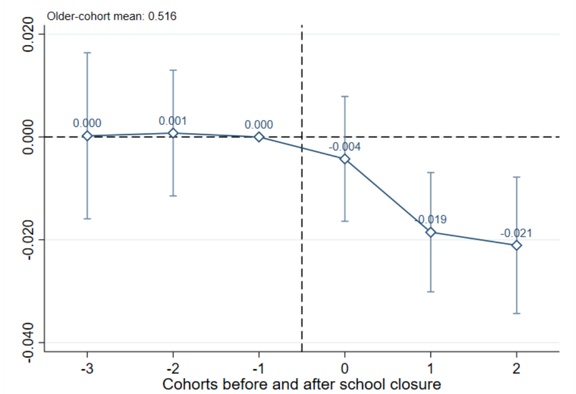
(c) Four-year college completion



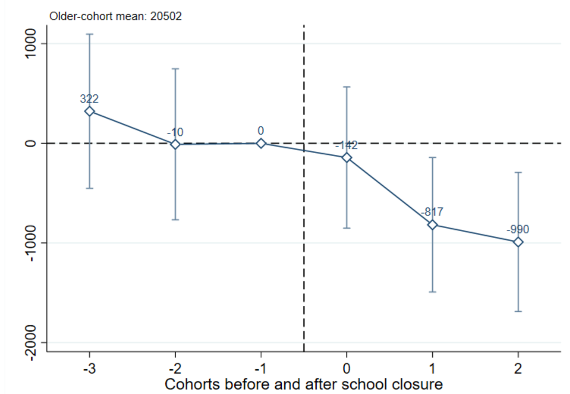
(d) College quality



(e) Employment



(f) Yearly earnings



Notes: The figures present the coefficients, π_c, and 95% confidence intervals from equation (4). These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts already graduated within three years and in the school at the time of closure. The cohort that graduated one year before the closure ($c = -1$) is the omitted category. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standardized test scores and standardized absence rate are measured before the school closure. Standard errors are clustered at the school-by-cohort level.

Table 1: Short-Run Effects of School Closure on Student Outcomes

	(1) Math	(2) Reading	(3) Days of Absence	(4) Days of Disciplinary Action
Closed School×After 1-2 Years	-0.033***	-0.034***	0.132*	0.362***
	(0.010)	(0.007)	(0.077)	(0.091)
Closed School×After 3-4 Years	0.011	-0.003	0.079	0.634***
	(0.011)	(0.009)	(0.097)	(0.114)
Observations	646,238	646,839	1,646,428	1,378,575
Individual FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Mean of pre-closure	0.022	0.057	7.535	2.423

Notes: The table presents the coefficients, β, and standard errors from equation (1). The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote years after school closure. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$. *** p<0.01, ** p<0.05, * p<0.10

Table 2: Long-Run Effects of School Closure on Educational and Labor Market Outcomes

Panel A: Post-Secondary Outcomes				
	(1) Graduate HS	(2) Enroll College	(3)BA Degree	(4)College Quality
Closed School × Younger Cohorts	-0.023*** (0.005)	-0.012*** (0.004)	-0.007** (0.003)	-337*** (82)
Observations	187,817	187,817	187,817	187,817
School FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Mean of the Older Cohort	0.712	0.487	0.138	29809

Panel B: Labor Market Outcomes		
	(1) Employment	(2) Yearly Earnings
Closed School × Younger Cohorts	-0.010*** (0.003)	-698*** (181)
Observations	187,817	187,817
Individual FE	X	X
Matched group × Year FE	X	X
Mean of pre-closure	0.519	20,763

Notes: The table presents the coefficients, γ, and standard errors from equation (3). The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standardized test scores and standardized absence rate are measured before the school closure. Standard errors are clustered at the school-by-cohort level. *** p<0.01, ** p<0.05, * p<0.10

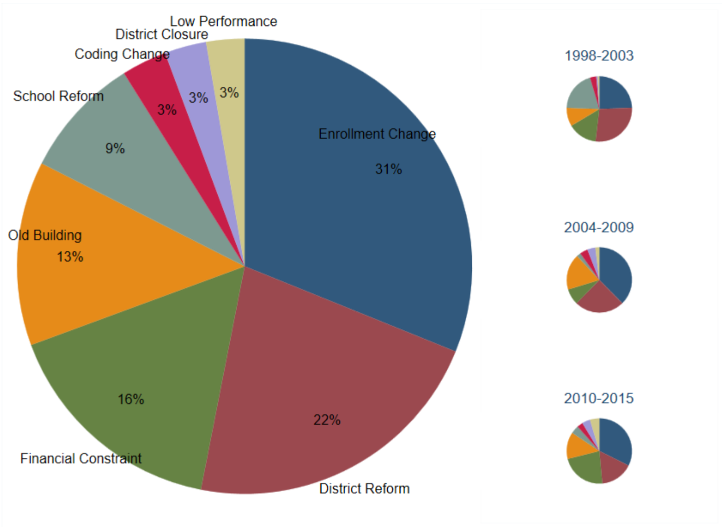
Online Appendix

The Long Shadow of School Closures: Impacts on Students' Educational and Labor Market Outcomes

Jeonghyeok Kim (2024)

A Appendix Figures and Tables

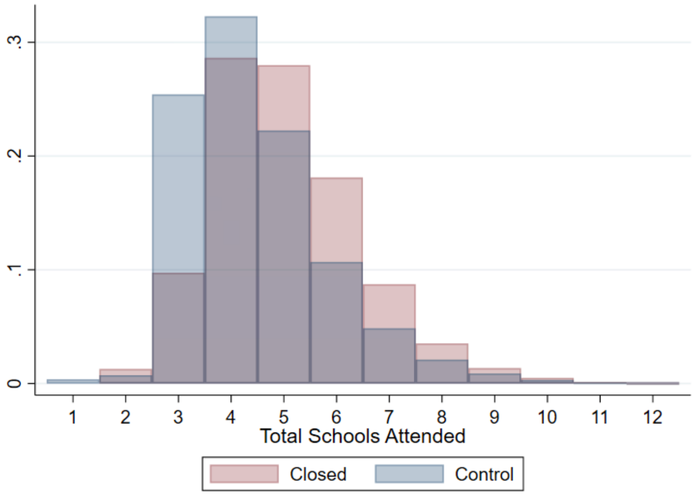
Fig. A.1. The Reasons for School Closures at Texas Public Schools in 2009-2015



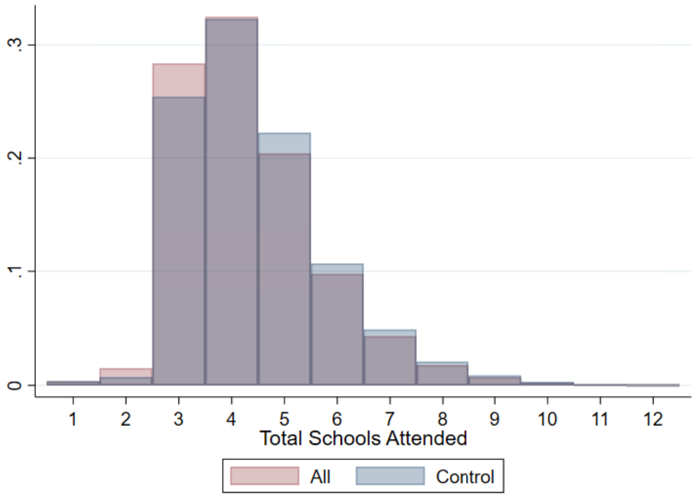
Notes: The figure presents the categorized reasons for 267 out of 470 public school closures that occurred between 1998 and 2015. Three smaller figures depict the reasons for closures across three distinct periods: 1998-2003 (86 closures out of 146), 2004-2009 (71 closures out of 177), and 2010-2015 (110 closures out of 147). As school closures can be attributed to multiple factors, each closure may have multiple reasons. Therefore, the percentages in the figure represent the proportion of each type of reason relative to all reasons reported.

Fig. A.2. The Number of Schools Attended

(a) Closed and Control Schools



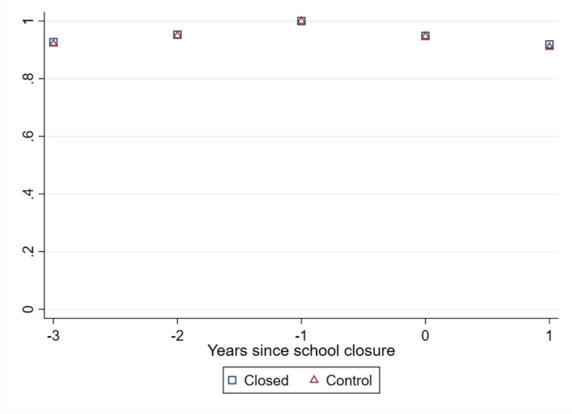
(b) State Average and Control Schools



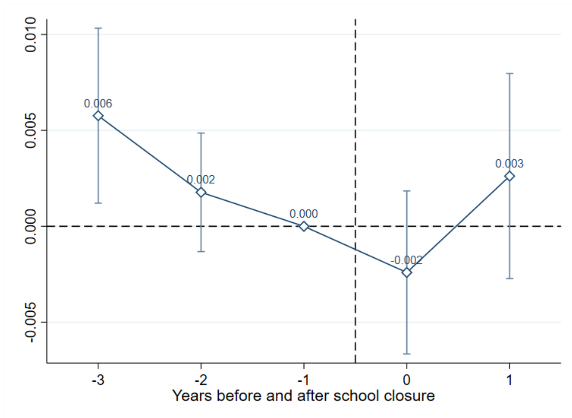
Notes: The figure presents the number of schools attended by students. In sub-figure (a), I compare students in my analysis sample enrolled in closed and control schools at the time of closure. In sub-figure (b), I compare the state average with students in my control group. The state average is calculated based on those who are observed throughout all years of K-12 education.

Fig. A.3. Analysis of Sample Attrition Rates of Closed and Control Schools

(a) Short-run: mean in-sample by time

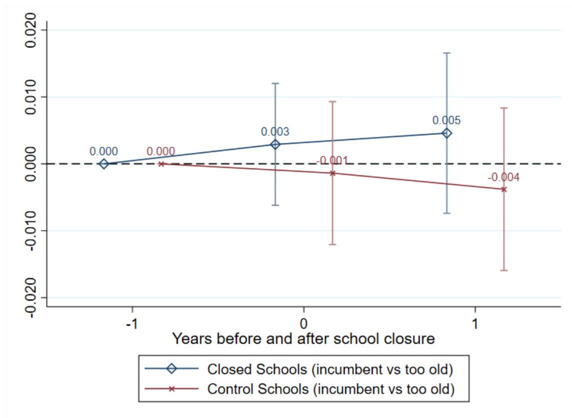


(b) Short-run: regression of in-sample dummy on closed-school dummy



(c) Long-run: mean in-sample by time

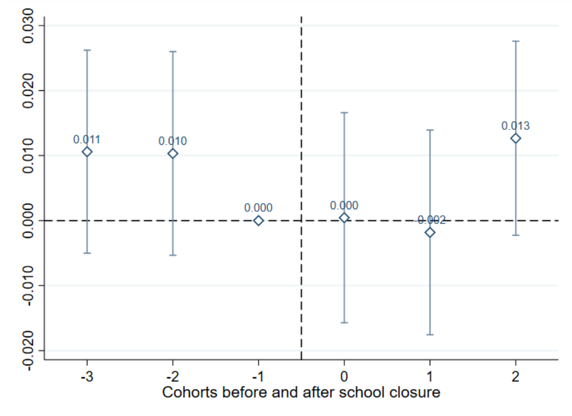
(d) Long-run: regression of in-sample dummy on closed-school/younger-cohort dummy



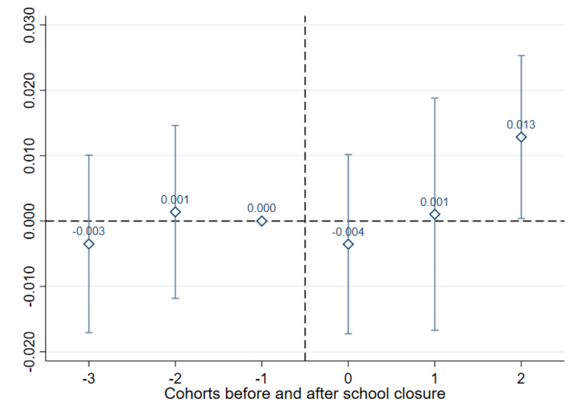
Notes: Sub-figures (a) and (b) consider all students in the short-run analysis sample enrolled in closed and matched control schools in the year preceding the closure (denoted by time -1 on the x-axis). Sub-figure (a) plots the proportion of observed students each year around school closure, separately for students in closed schools and control schools. Using this sample, sub-figure (b) presents the coefficients, ρ_t, and 95% confidence intervals from equation (2), in which the dependent variable is an indicator for being observed in the data. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at year $t = -1$. Sub-figure (c) and (d) consider all students in the long-run analysis sample enrolled in closed and matched control schools in the year preceding the closure or four years before the closure (denoted by time -1 on the x-axis). Sub-figure (c) plots the proportion of observed students in the years following time -1, separately for four groups—younger (incumbent) and older cohorts in closed schools and control schools. Using this sample, sub-figure (d) presents the coefficients, π_c, and 95% confidence intervals from equation (2), in which the dependent variable is an indicator for being observed in the data and $c \in \{-1, 0, 1\}$, separately for closed and control schools. Other specifications are equal to sub-figure (b).

Fig. A.4. Long-Run Analysis Balance Test: Difference in Student Composition, Test Scores, and Behavior

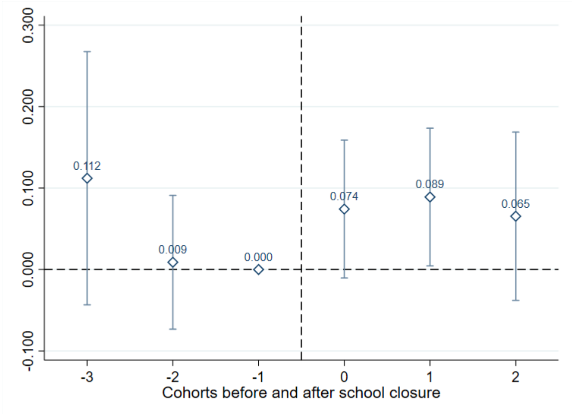
(a) Economic disadvantage status



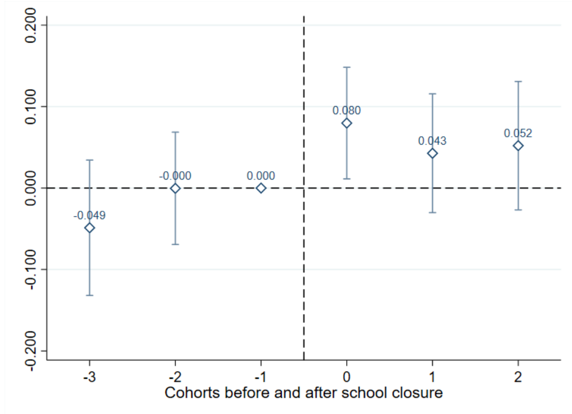
(b) Black or Hispanic students



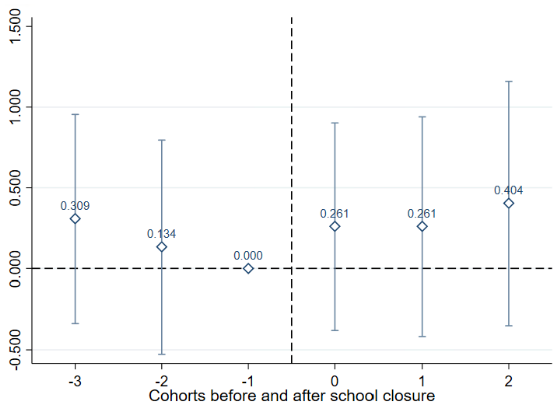
(c) Standardized math score



(d) Standardized reading score



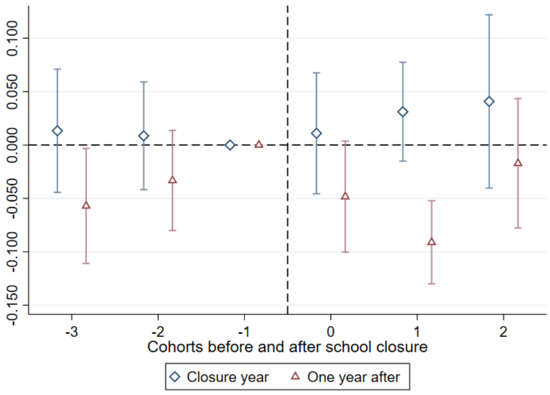
(e) Days of absence



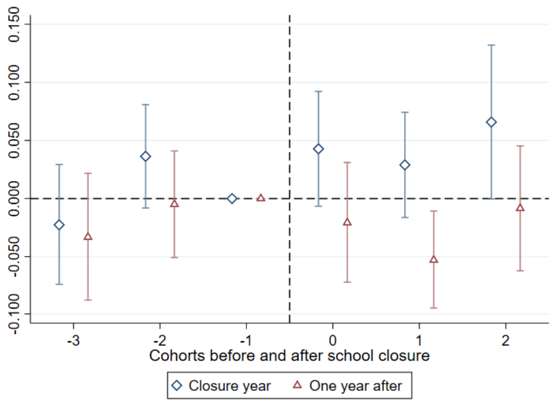
Notes: The figures present the coefficients, π_c, and 95% confidence intervals from equation (4), in which the dependent variables are student characteristics (economic disadvantage status and Black/Hispanic students) or short-run outcomes (test scores and behavior). The short-run outcomes are measured before school closures, specifically at $t = -1$ for younger cohorts and at $t = -4$ for older cohorts from the equation (2). These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts already graduated within three years and in the school at the time of closure. The cohort that graduated one year before the closure ($c = -1$) is the omitted category. The regression includes school and match group-by-cohort fixed effects. Standard errors are clustered at the school-by-cohort level.

Fig. A.5. Long-Run Analysis Balance Test: Difference in Test Scores and Behavior Before and After School Closures

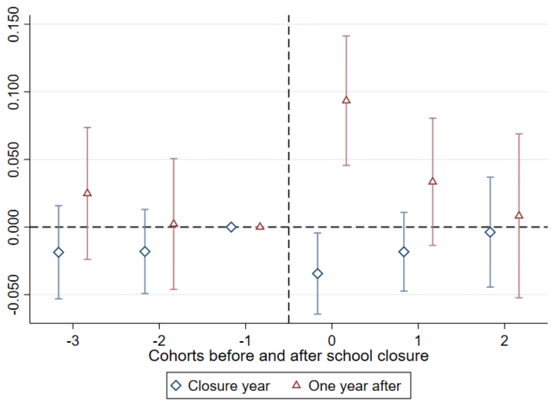
(a) Standardized math score



(b) Standardized reading score



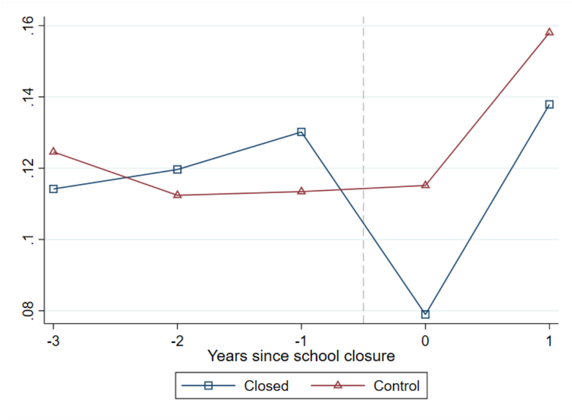
(c) Absence rate



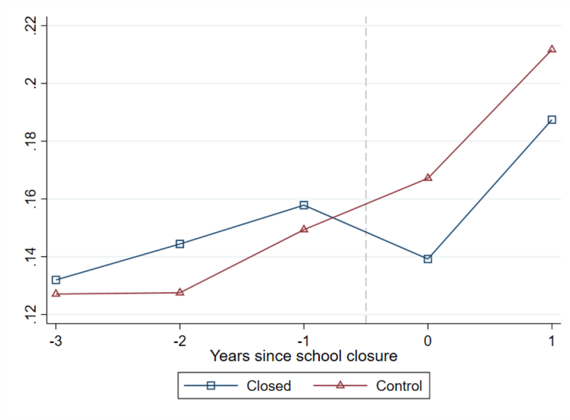
Notes: The figures present the coefficients, π_t, and 95% confidence intervals from equation (4), in which the dependent variables are short-run outcomes (test scores and behavior). The dependent variable is measured before school closures, specifically at $t = -1$ for younger cohorts and at $t = -4$ for older cohorts from the equation (2), and after closures, specifically at $t = 0$ for younger cohorts and at $t = -3$ for older cohorts. To be included in the analysis, individuals must be observed in both outcomes before and after closure. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts already graduated within three years and in the school at the time of closure. The cohort that graduated one year before the closure ($c = -1$) is the omitted category. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, and special education status. Standard errors are clustered at the school-by-cohort level.

Fig. A.6. Raw Trends in Short-Run Outcomes Between Closed and Control Schools

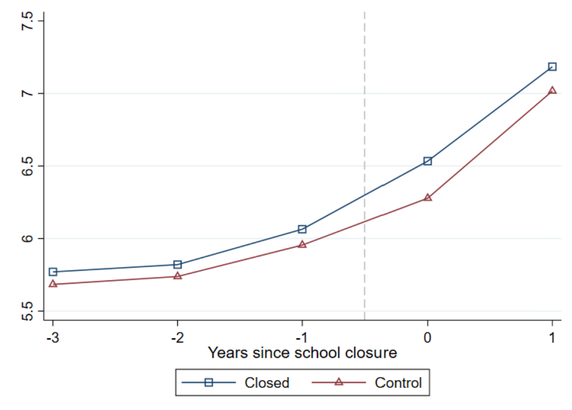
(a) Standardized math score



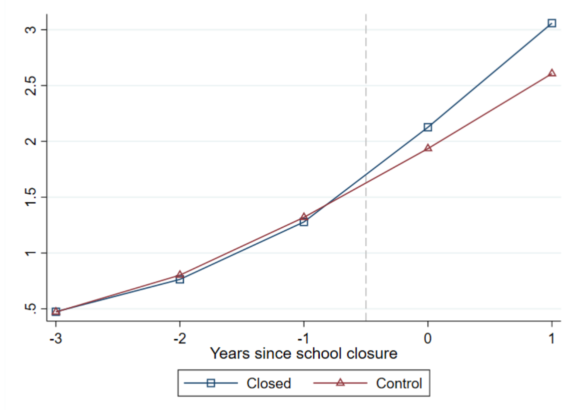
(b) Standardized reading score



(c) Days of absence



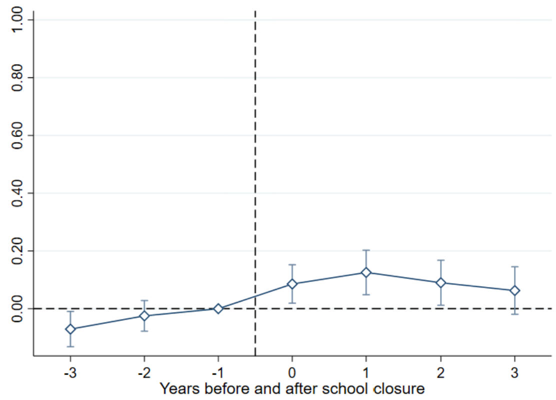
(d) Days of disciplinary action



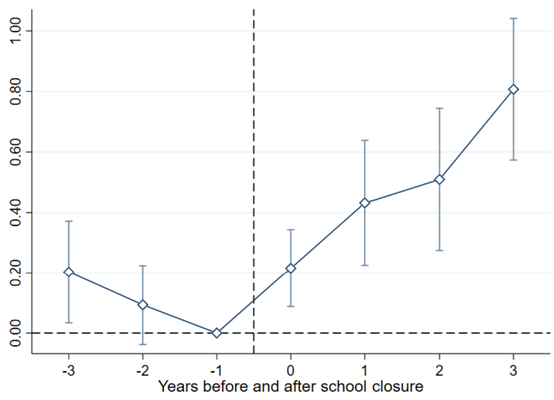
Notes: The figures plot raw trends over the period of three years before and two years after the school closure, separately for closed and matched control schools. I restrict the sample to students who are observed in the data over this period (i.e., the panel is balanced).

Fig. A.7. Short-Run Effects of School Closure on Days of Disciplinary Actions: Different Margins

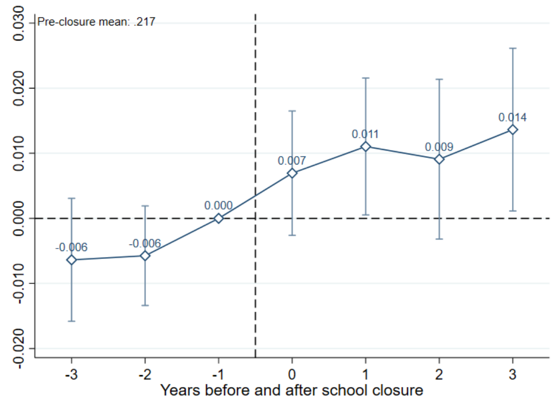
(a) In-school days of disciplinary action



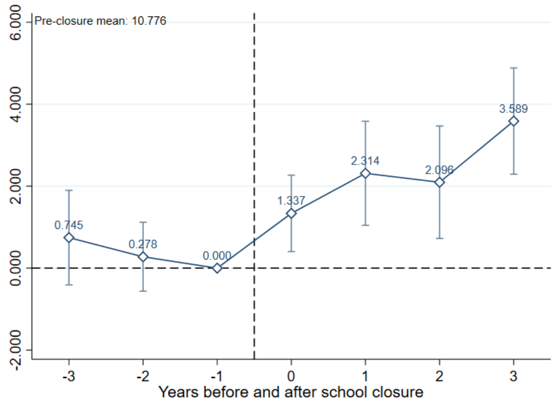
(b) Out-of-school days of disciplinary action



(c) Binary outcome variable



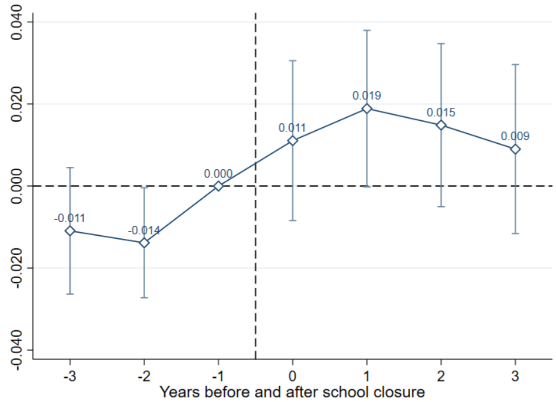
(d) Among at least one day



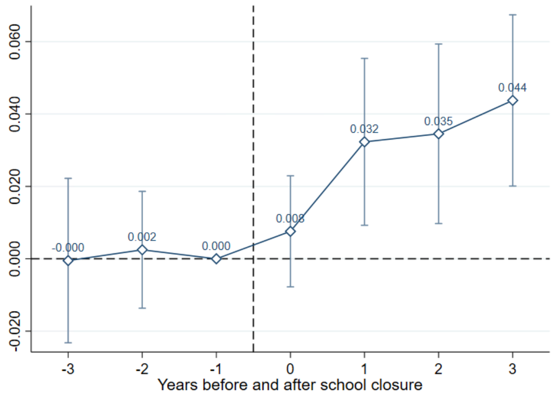
Notes: The figures present the coefficients, ρ_t, and 95% confidence intervals from equation (2), using different margins of disciplinary action as the dependent variable: in-school suspension, out-of-school suspension (including expulsion), an indicator variable that equals 1 if a student has at least one day of disciplinary action, and a sample restricted to students with at least one day of disciplinary action. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$.

Fig. A.8. Peer and Expected School Quality Changes Before and After School Closures

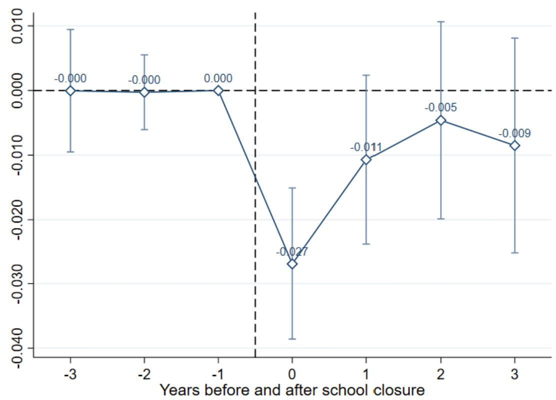
(a) Peer quality: days of absence



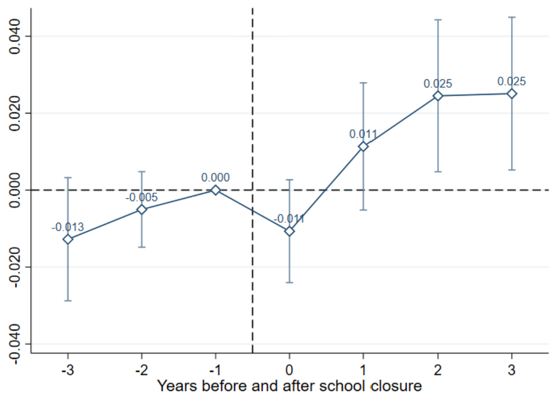
(b) Peer quality: days of disciplinary action



(c) Expected quality: days of absence



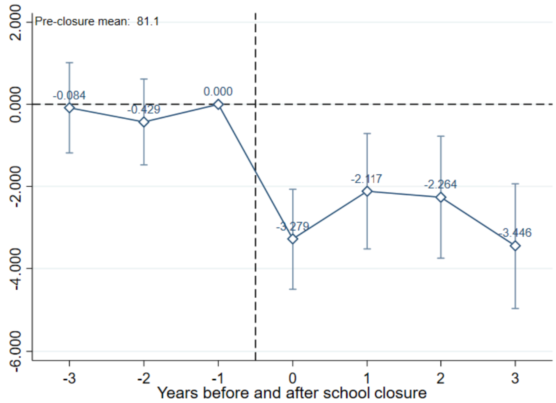
(d) Expected quality: days of disciplinary action



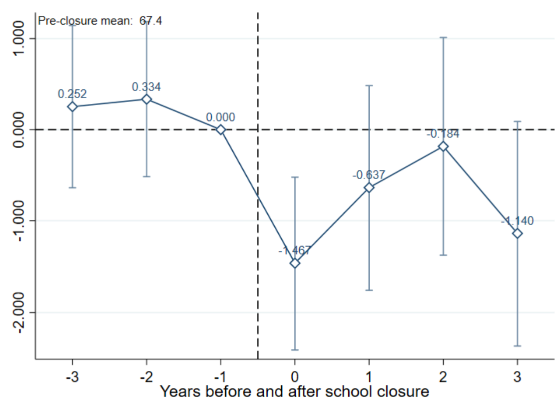
Note: The figures present the coefficients, ρ_t, and 95% confidence intervals from equation (2), where the outcome variables are the school average days of absence and days of disciplinary action, which are standardized by year-by-grade level. When it comes to sub-figures (a) and (b), the outcome variables are the yearly school average of the outcomes. The construction of average values excludes displaced students from the calculations after school closure (i.e., $t >= 0$). Moreover, I exclude receiving schools if more than 70% of their students are displaced students. For sub-figures (c) and (d), the outcome variables are the school average over the four years preceding the school closure (i.e., $t \in \{-4, \ldots, -1\}$). These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$.

Fig. A.9. Effects of School Closures on School-level Employment

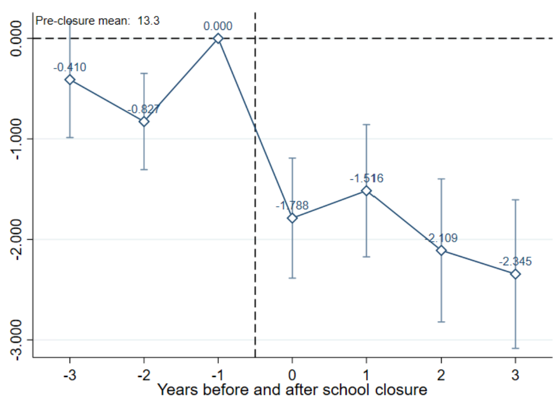
(a) Total school staff



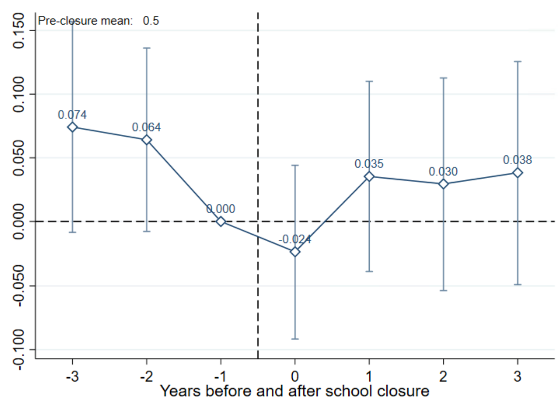
(b) Teachers



(c) Teaching support

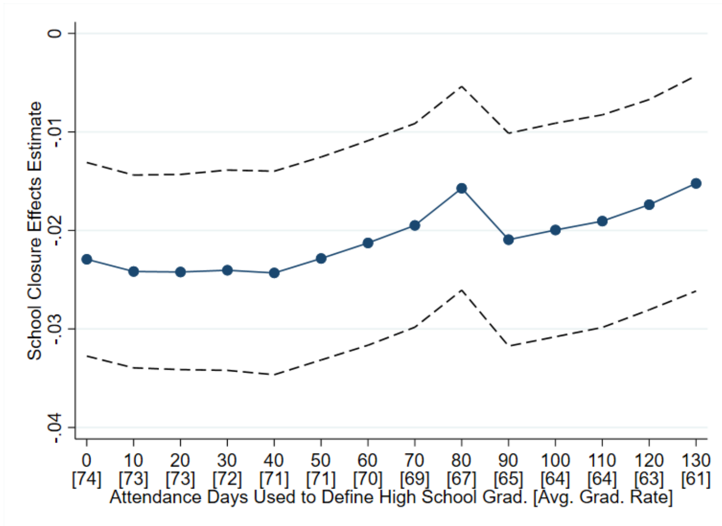


(d) Social support



Note: The figures present the coefficients, ρ_t, and 95% confidence intervals from equation (2), where the outcome variables are the school-level full-time-equivalent (FTE) positions per 1000 students. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$.

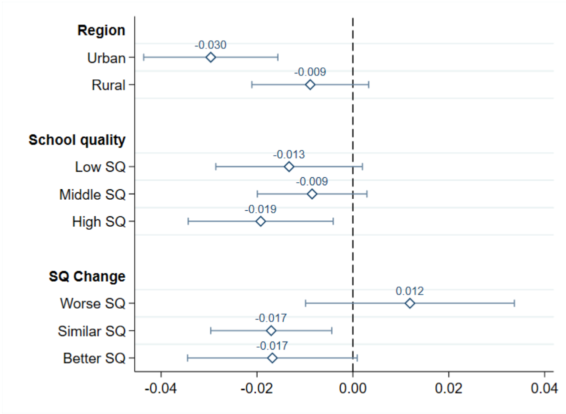
Fig. A.10. Estimates of School Closure Effects on High School Graduation Across Different Definition Cutoffs



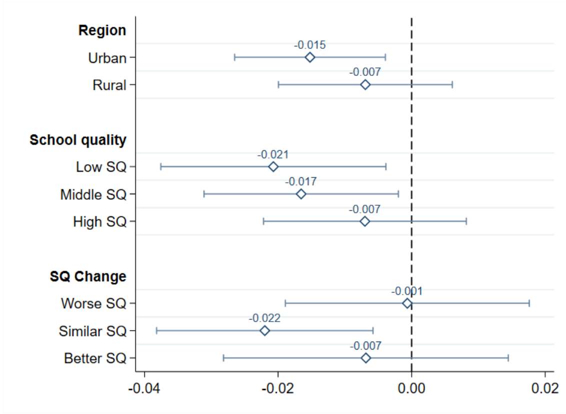
Notes: The figure presents the coefficients, γ, and 95% confidence intervals from equation (3) with high school graduation as the dependent variable, using different definition cutoffs for attending days in 12th grade. The baseline results define high school graduation as attending 12th grade for at least 50 days. The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. The X-axis shows different attendance cutoffs, ranging from 0 to 130 days, with the proportion of students graduating at each cutoff indicated in brackets. Standard errors are clustered at the school-by-cohort level.

Fig. A.11. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Heterogeneity by School Characteristics

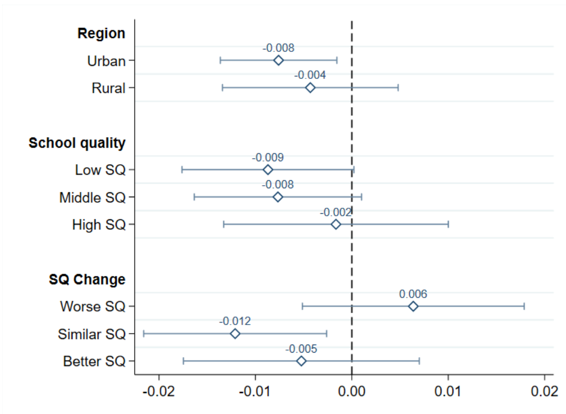
(a) High school graduation



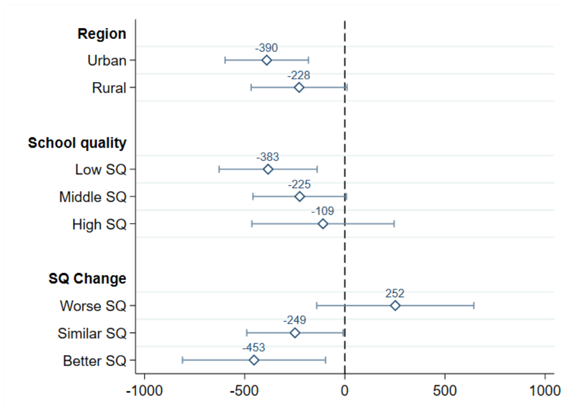
(b) Any college enrollment



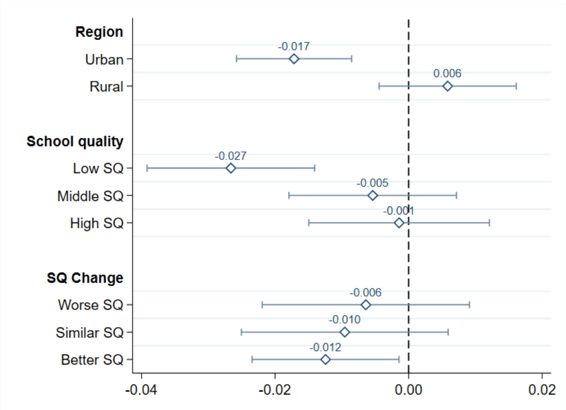
(c) Four-year college completion



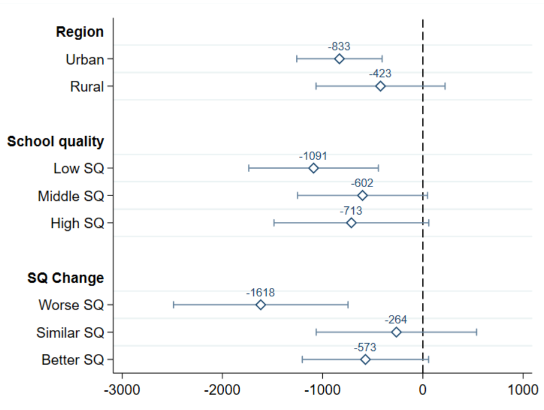
(d) College quality



(e) Employment



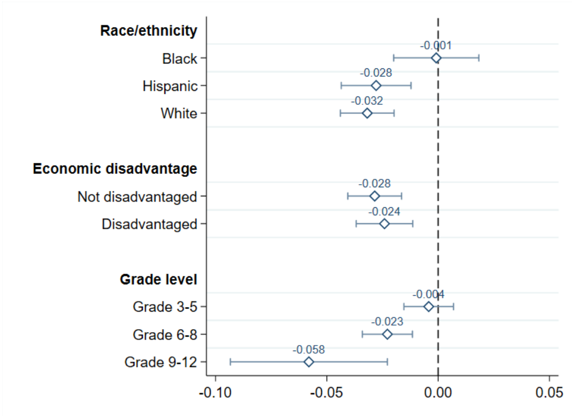
(f) Yearly earnings



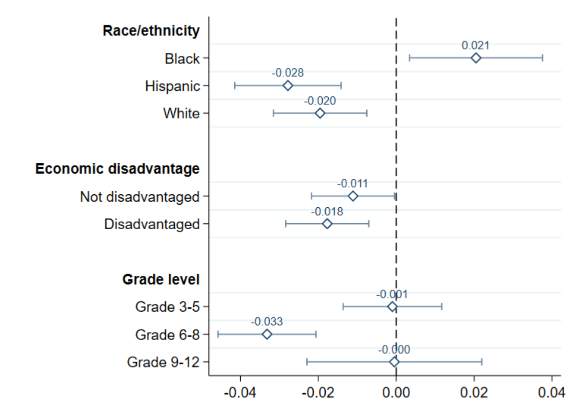
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure. The region is defined based on the NCES locale categories, with urban areas including cities and urban fringes, and rural areas including towns and rural areas. School quality is measured by the average test scores of the students in a closed school before the closure. The difference between the average test scores of students from the closed school and the nearest school of the same school type is used to measure school quality change (SQ Change). The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standard errors are clustered at the school-by-cohort level.

Fig. A.12. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Heterogeneity by Student Characteristics

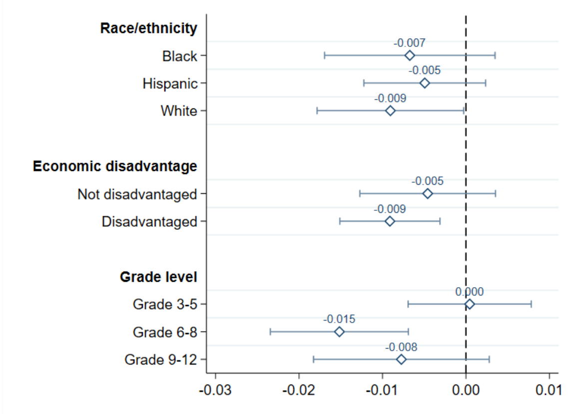
(a) High school graduation



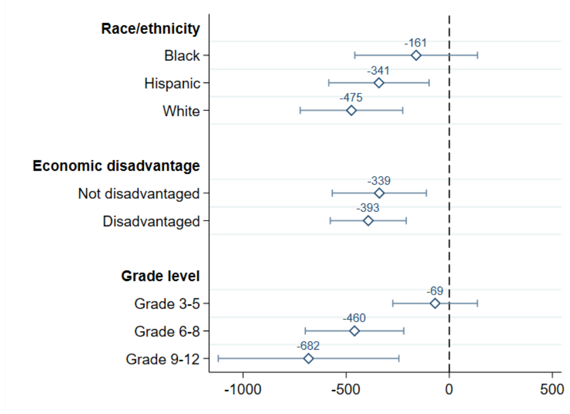
(b) Any college enrollment



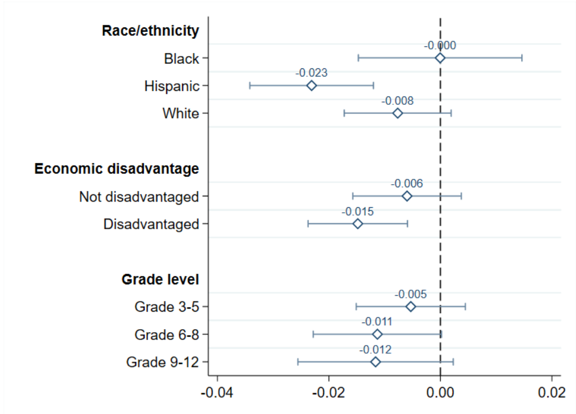
(c) Four-year college completion



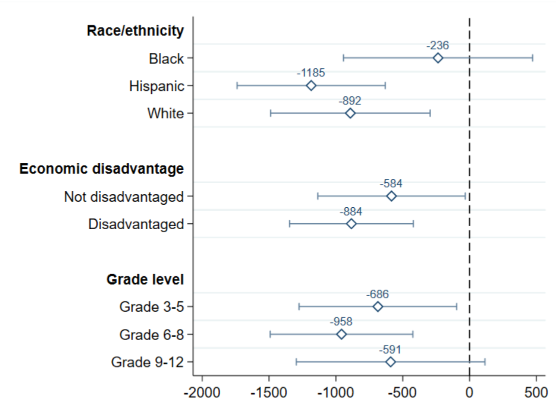
(d) College quality



(e) Employment



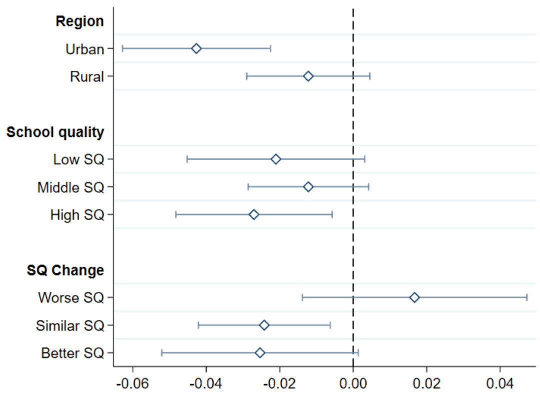
(f) Yearly earnings



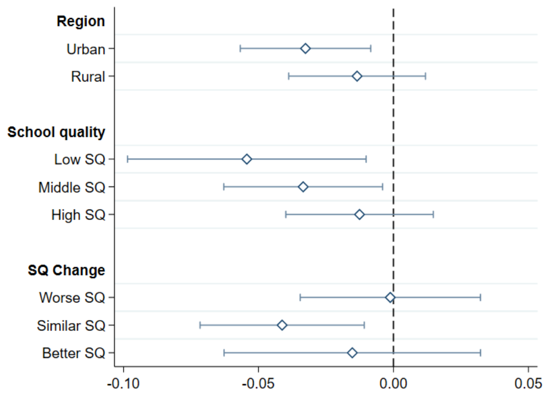
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure. The regression includes school and match group-by-cohort fixed effects, as well as individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standard errors are clustered at the school-by-cohort level.

Fig. A.13. Long-Run Effects of School Closure on Educational and Labor Market Outcomes by 26: *Rescaled* Heterogeneity by School Characteristics

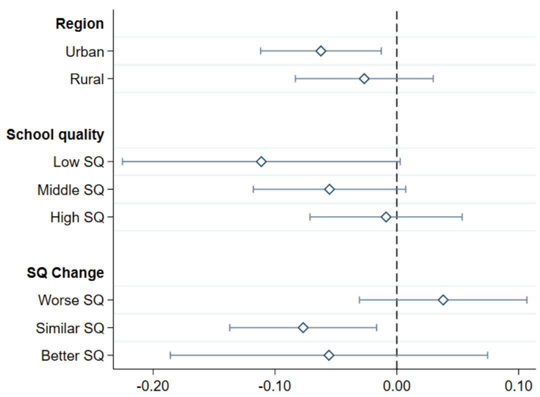
(a) High school graduation



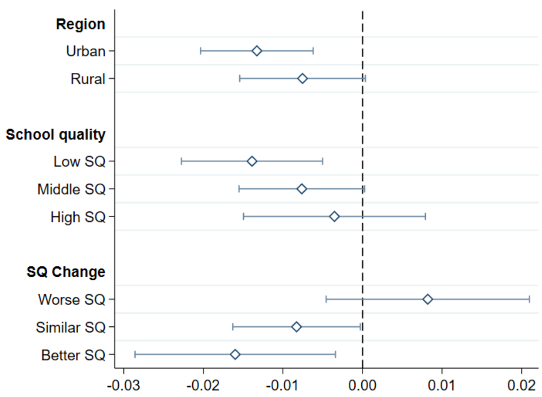
(b) Any college enrollment



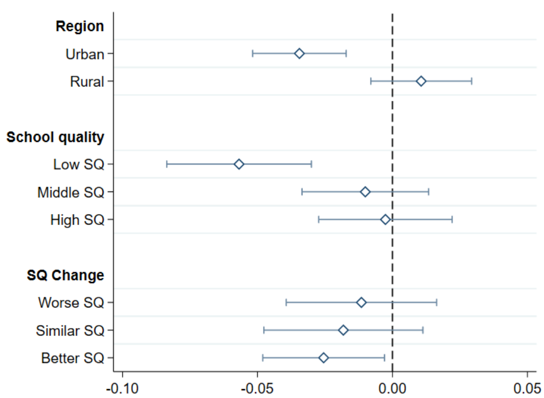
(c) Four-year college completion



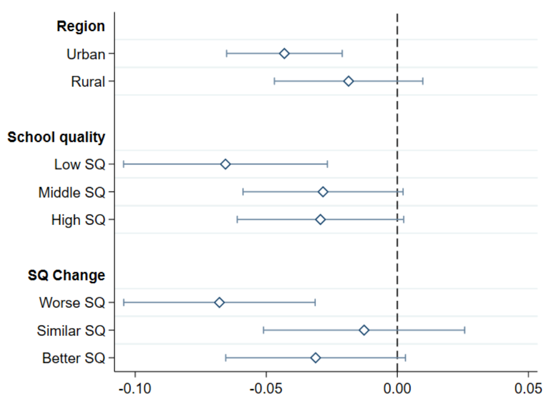
(d) College quality



(e) Employment



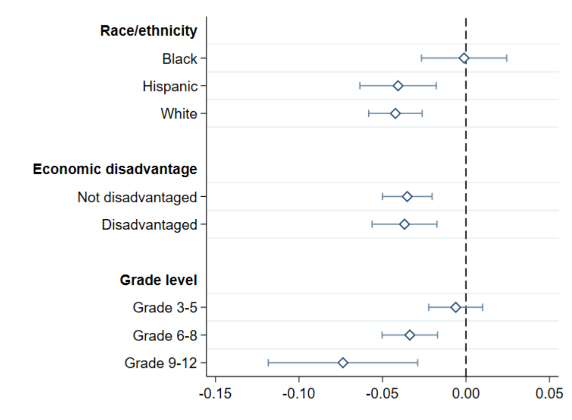
(f) Yearly earnings



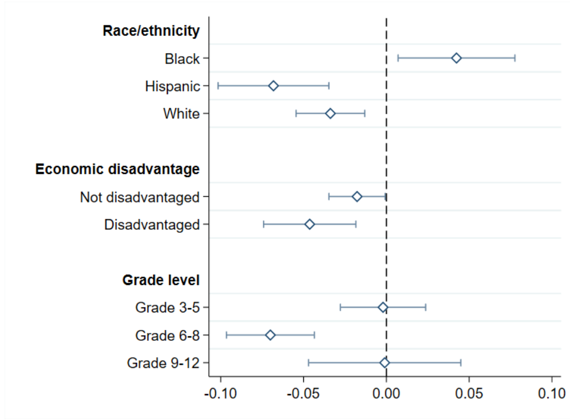
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis after estimates are scaled relative to the outcome mean for each sub-group. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure. The region is defined based on the NCES locale categories, with urban areas including cities and urban fringes, and rural areas including towns and rural areas. School quality is measured by the average test scores of the students in a closed school before the closure. The difference between the average test scores of students from the closed school and the nearest school of the same school type is used to measure school quality change (SQ Change). The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standard errors are clustered at the school-by-cohort level.

Fig. A.14. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: *Rescaled* Heterogeneity by Student Characteristics

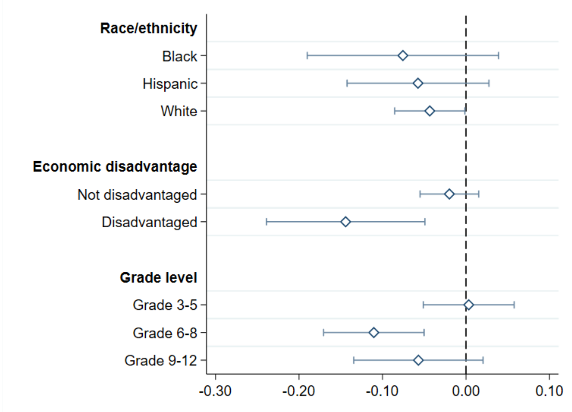
(a) High school graduation



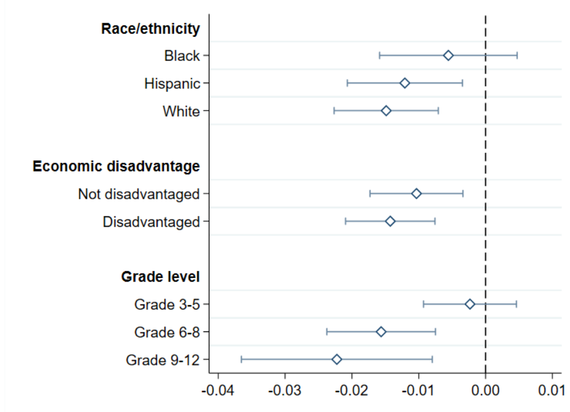
(b) Any college enrollment



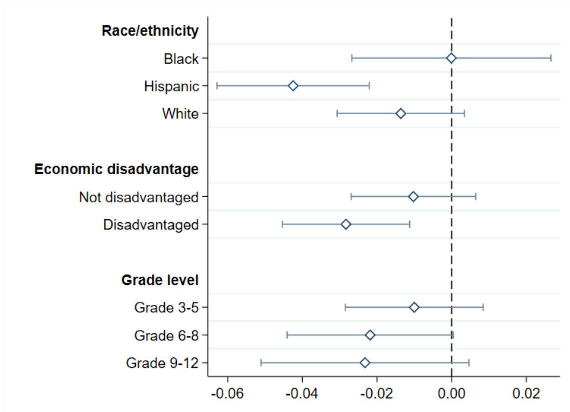
(c) Four-year college completion



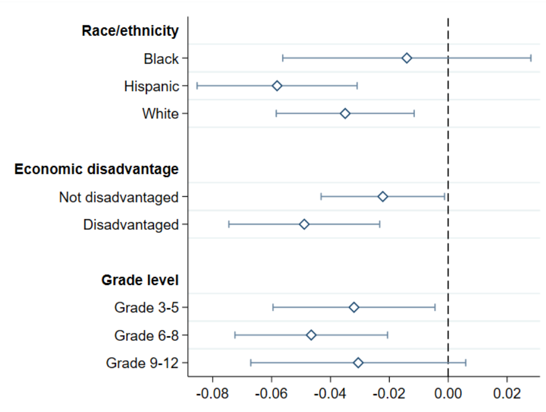
(d) College quality



(e) Employment



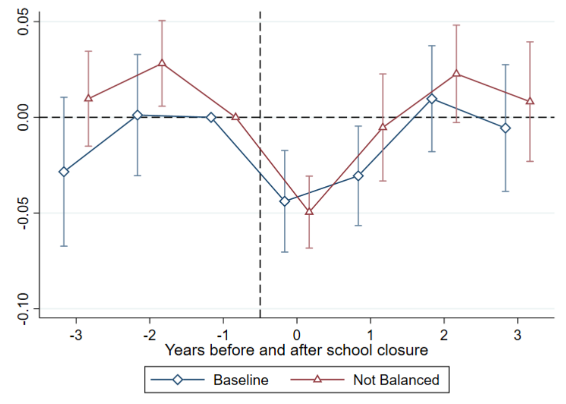
(f) Yearly earnings



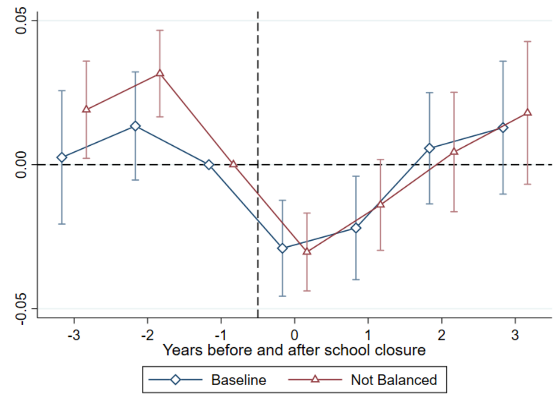
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis after estimates are scaled relative to the outcome mean for each sub-group. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure. The regression includes school and match group-by-cohort fixed effects, as well as individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standard errors are clustered at the school-by-cohort level.

Fig. A.15. Short-Run Effects of School Closure on Student Outcomes: Balanced and Unbalanced Sample

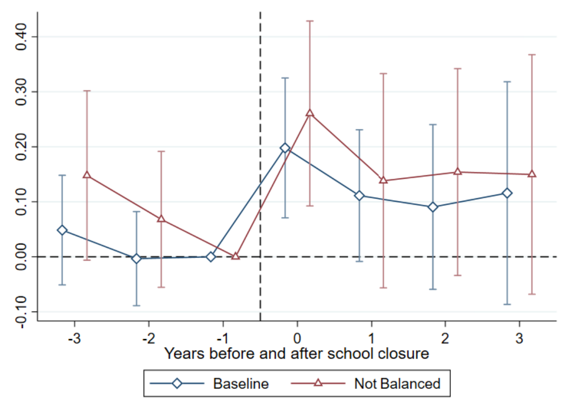
(a) Standardized math score



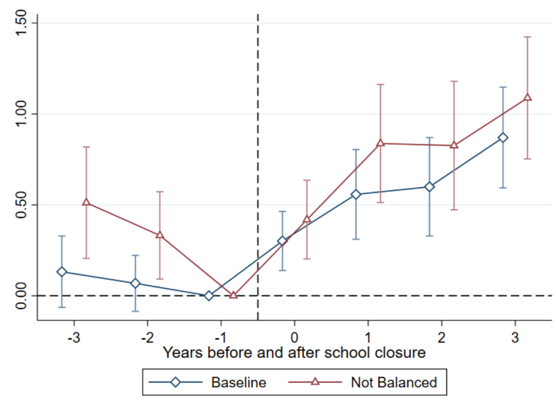
(b) Standardized reading score



(c) Days of absence

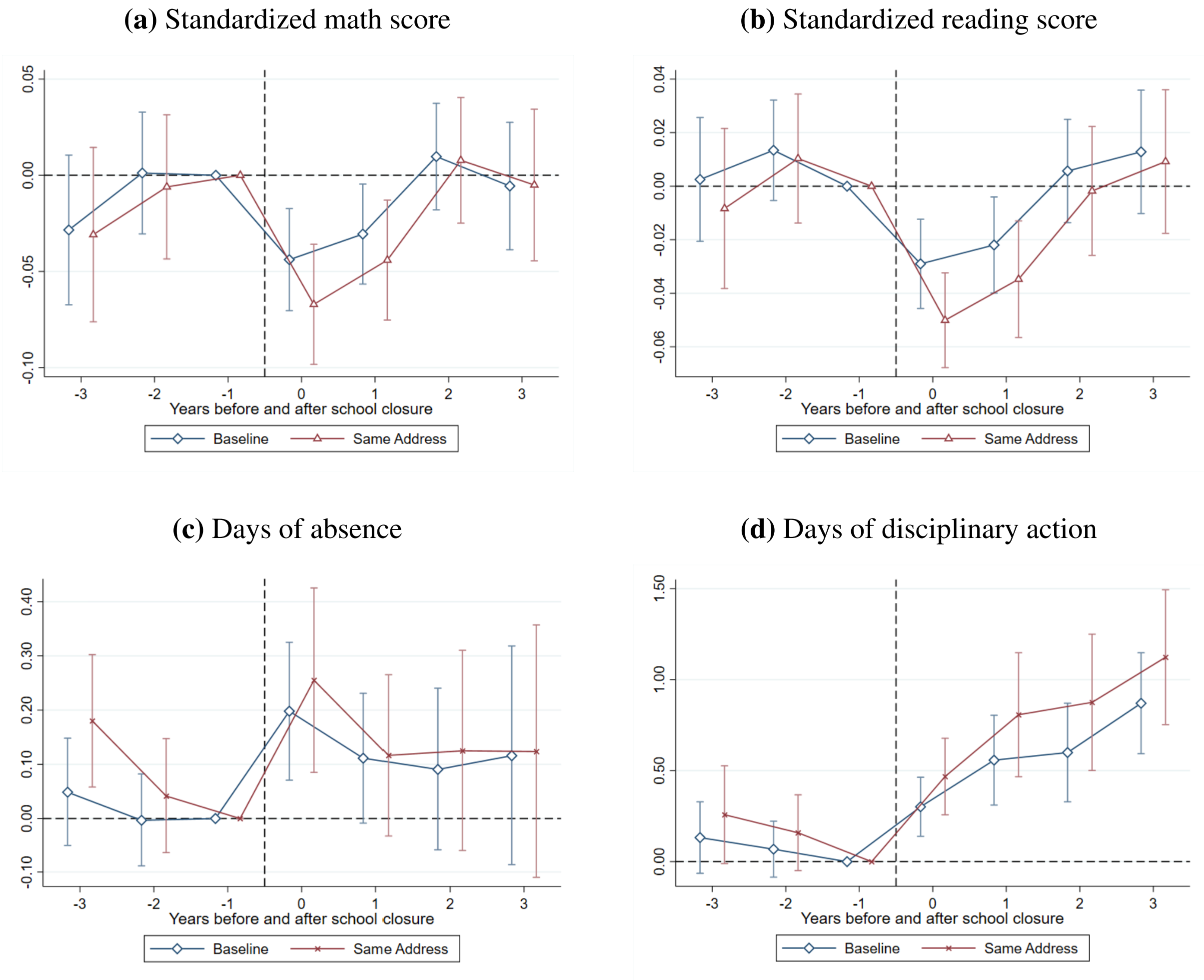


(d) Days of disciplinary action



Note: The figures overlays the coefficients, ρ_t, and 95% confidence intervals from equation (2) using either baseline (balanced panel) or unbalanced sample. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$.

Fig. A.16. Short-Run Effects of School Closure on Student Outcomes: Excluding Same Address School Opening



(a) Standardized math score

(b) Standardized reading score

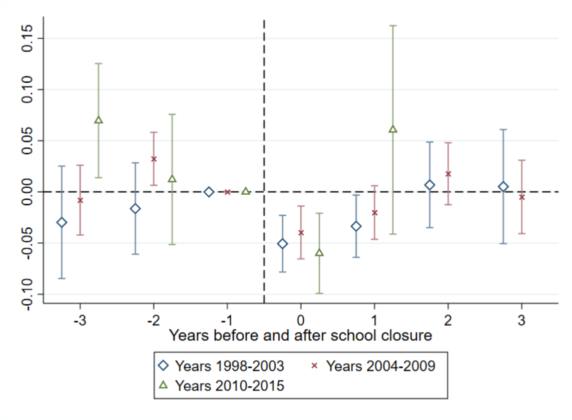
(c) Days of absence

(d) Days of disciplinary action

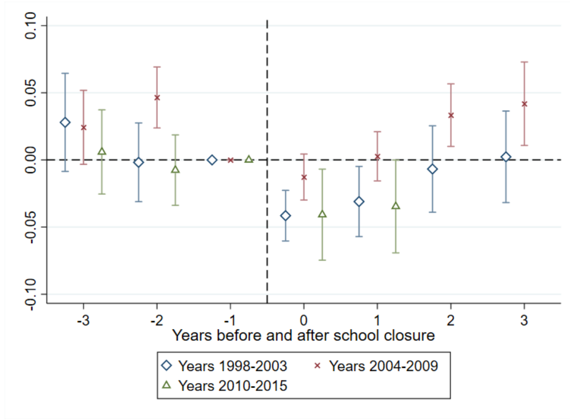
Note: The figures overlays the coefficients, ρ_t, and 95% confidence intervals from equation (2) using either baseline sample or sample excluding closed schools where another school appears at the same address after the closure. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$.

Fig. A.17. Short-Run Effects of School Closure on Student Outcomes: Three Periods

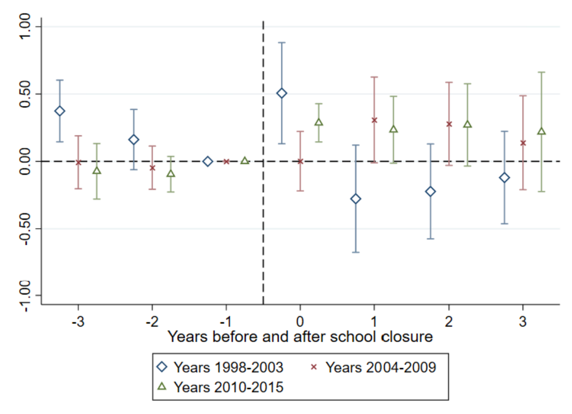
(a) Standardized math score



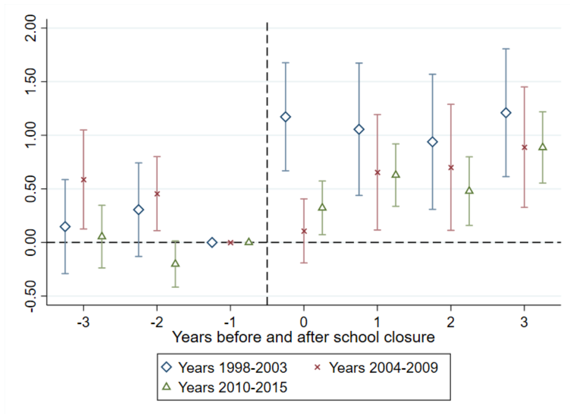
(b) Standardized reading score



(c) Days of absence



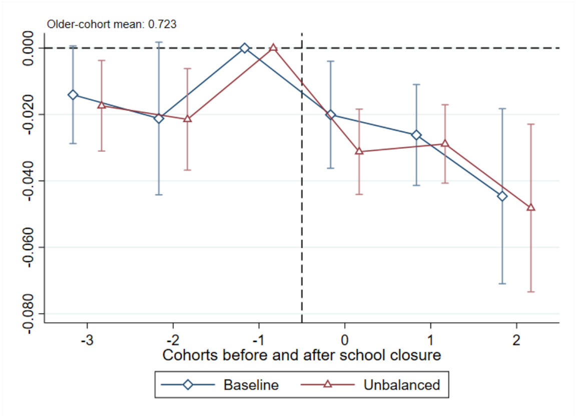
(d) Days of disciplinary action



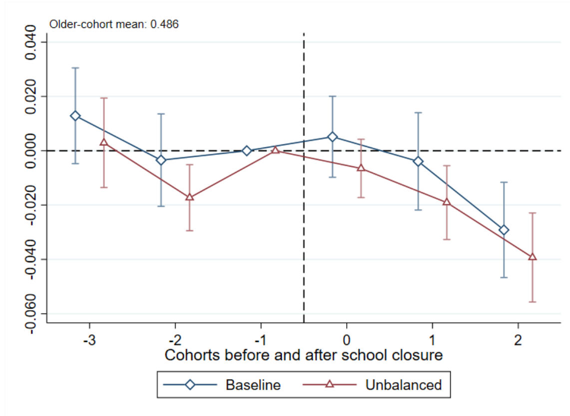
Note: The figures overlays the coefficients, ρ_t, and 95% confidence intervals from equation (2) using different periods of school closures: 1998-2003, 2004-2009, and 2010-2015. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$.

Fig. A.18. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Balanced and Unbalanced Sample

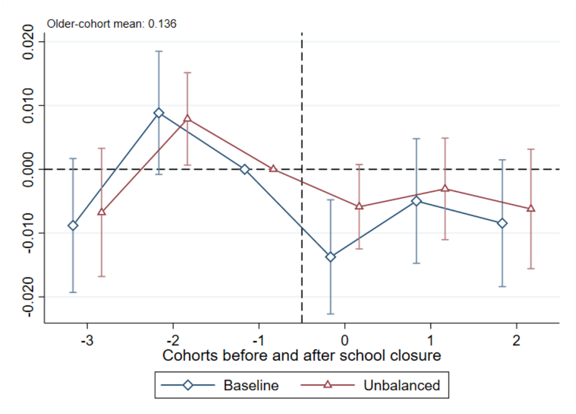
(a) High school graduation



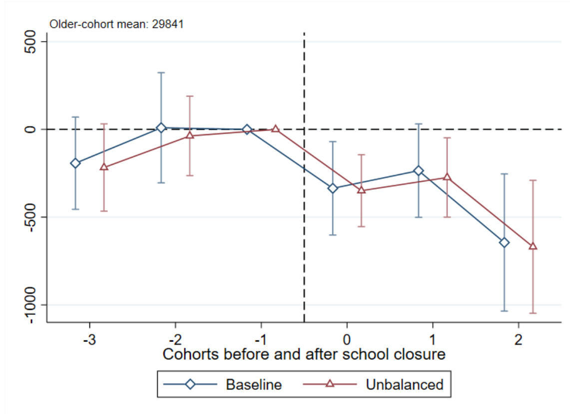
(b) Any college enrollment



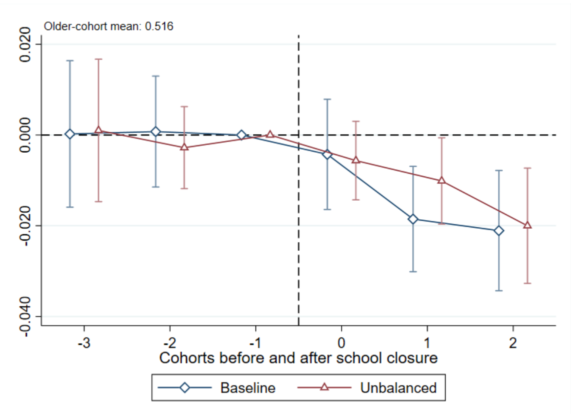
(c) Four-year college completion



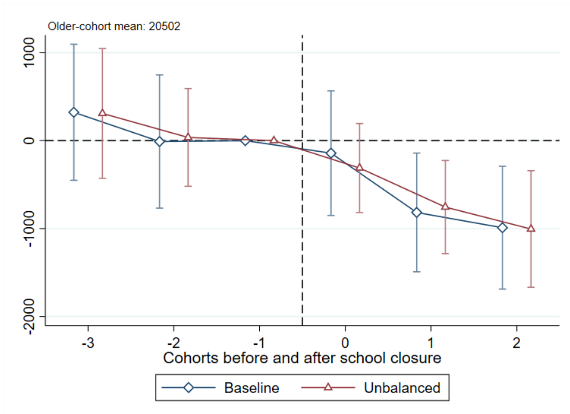
(d) College quality



(e) Employment



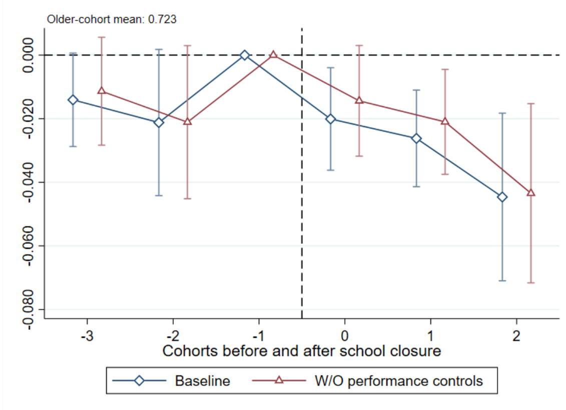
(f) Yearly earnings



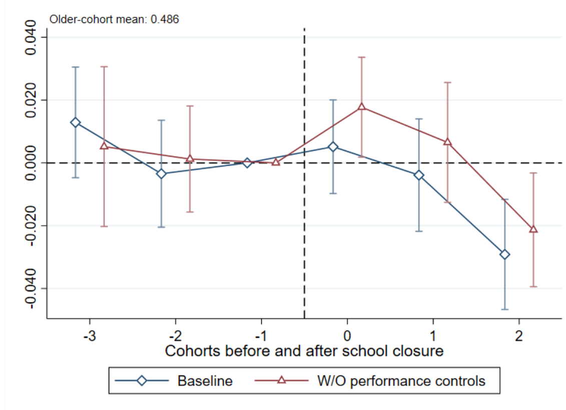
Notes: The figures overlay the coefficients, π_c, and 95% confidence intervals from equation (4) using either baseline (balanced) or unbalanced sample. The unbalanced sample includes closed schools having fewer than 3 grades. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts already graduated within three years and in the school at the time of closure. The cohort that graduated one year before the closure ($c = -1$) is the omitted category. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standard errors are clustered at the school-by-cohort level.

Fig. A.19. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: With and Without Controlling for Performance Measures

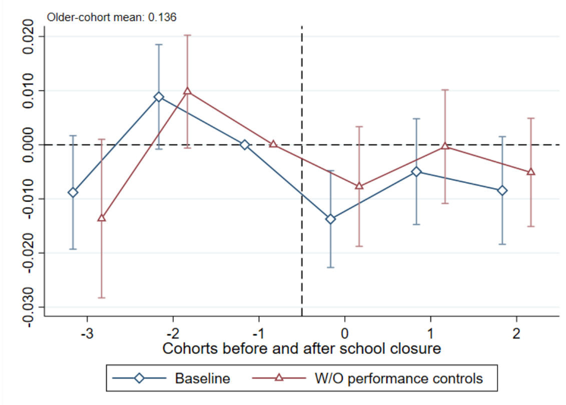
(a) High school graduation



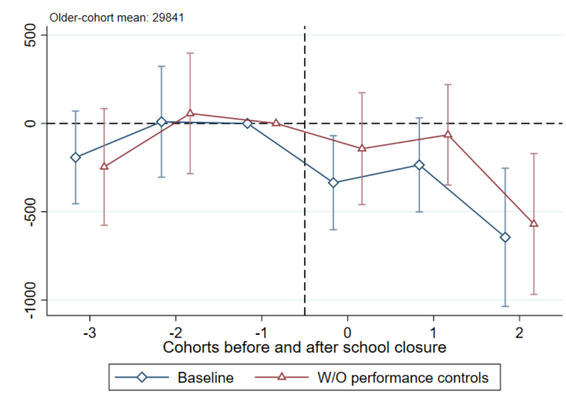
(b) Any college enrollment



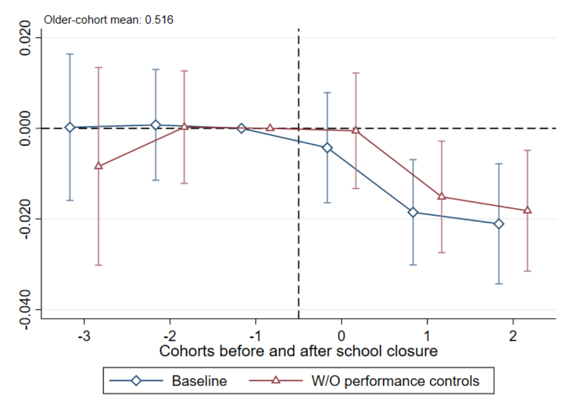
(c) Four-year college completion



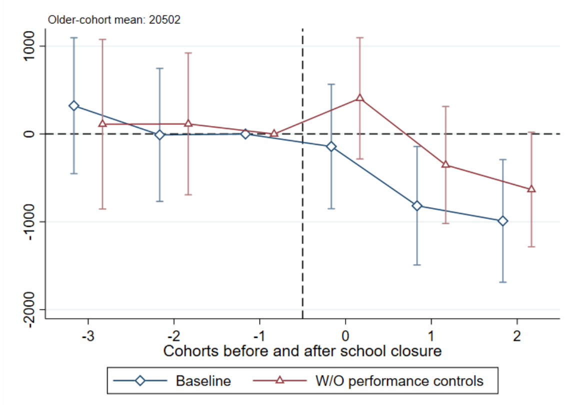
(d) College quality



(e) Employment



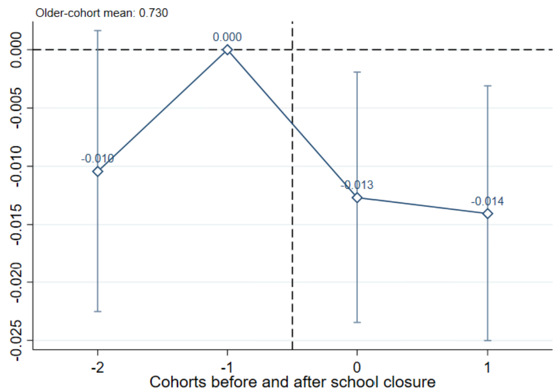
(f) Yearly earnings



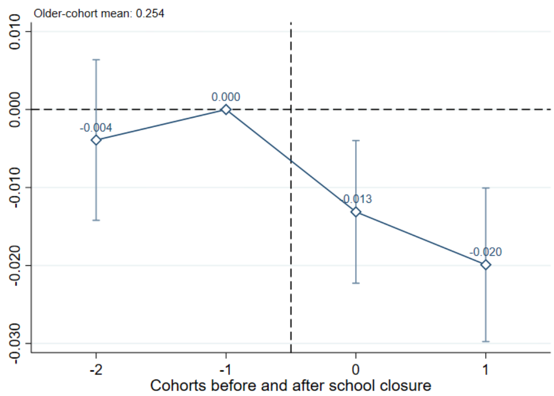
Notes: The figures overlay the coefficients, π_c, and 95% confidence intervals from equation (4) with and without controlling for standardized math and reading scores, and standardized absence rate. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts already graduated within three years and in the school at the time of closure. The cohort that graduated one year before the closure is the omitted category. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standard errors are clustered at the school-by-cohort level.

Fig. A.20. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Alternative Way of Cohort Construction

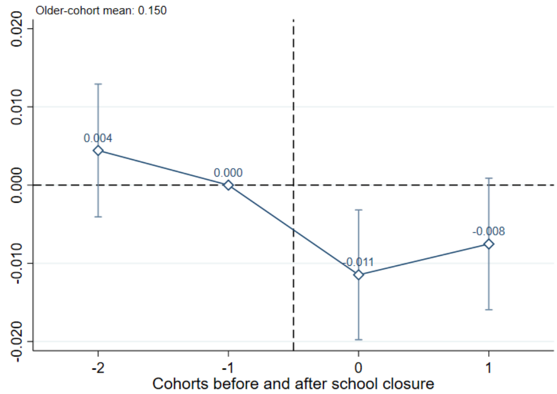
(a) High school graduation



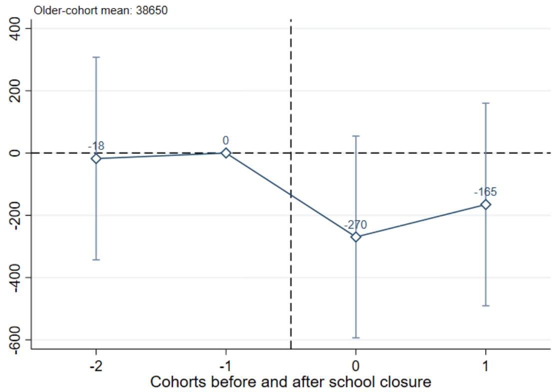
(b) Any college enrollment



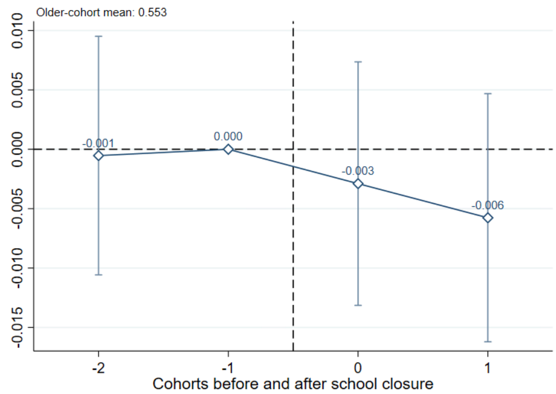
(c) Four-year college completion



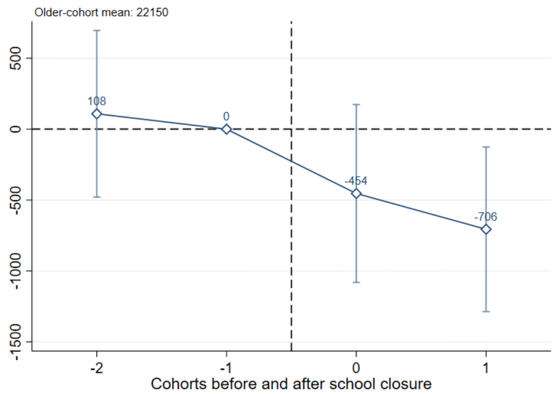
(d) College quality



(e) Employment



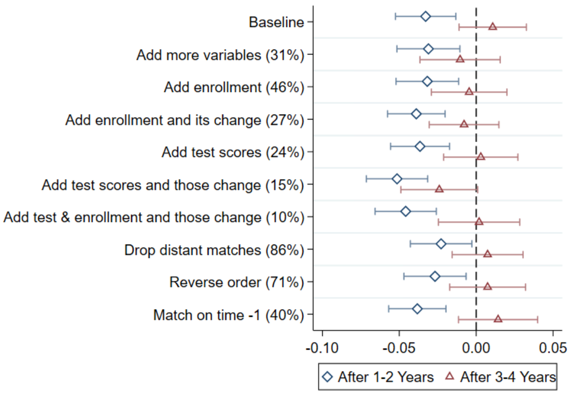
(f) Yearly earnings



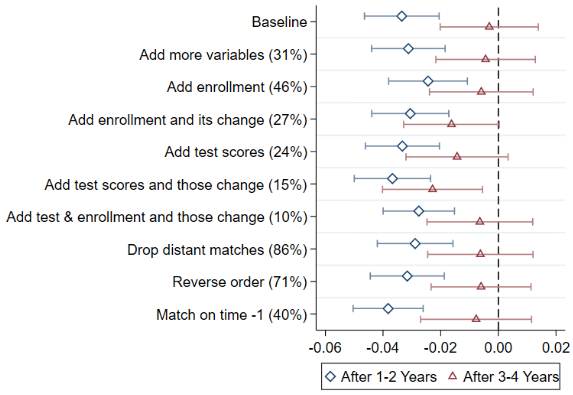
Notes: The figures present the coefficients, π_c, and 95% confidence intervals from equation (4), with alternative way of sample construction: instead of going three years back to create older cohorts, I choose the second highest grade in the year of closure and for the three years prior. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts already graduated within three years and in the school at the time of closure. The cohort that graduated one year before the closure ($c = -1$) is the omitted category. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standard errors are clustered at the school-by-cohort level.

Fig. A.21. Short-Run Effects of School Closure on Student Outcomes: Alternative Matching Strategies

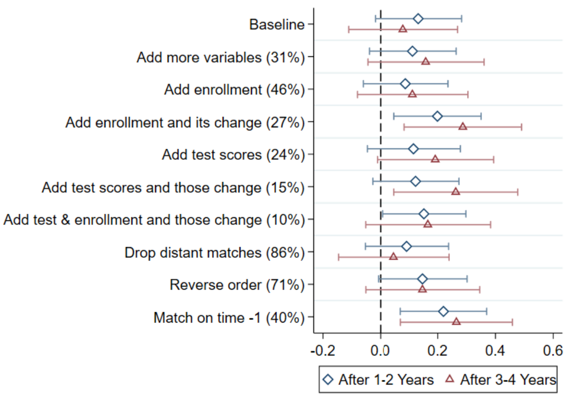
(a) Standardized math score



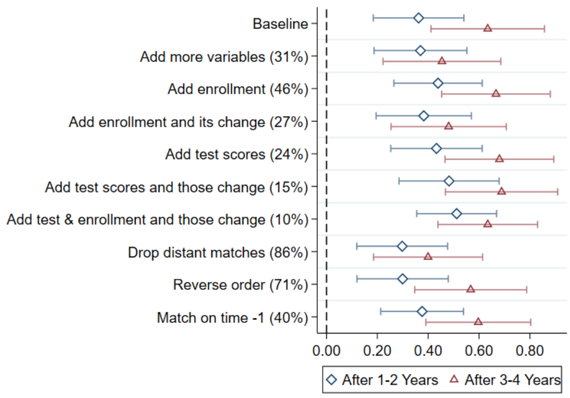
(b) Standardized reading score



(c) Days of absence



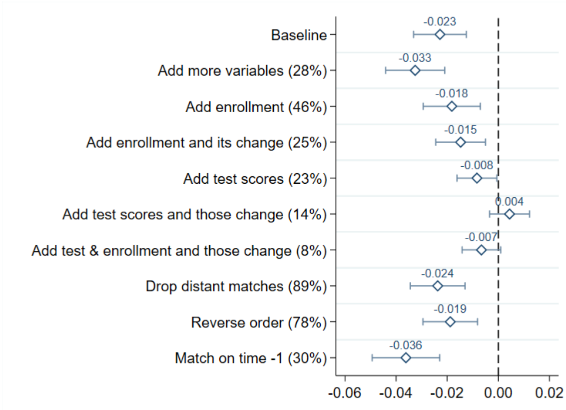
(d) Days of disciplinary action



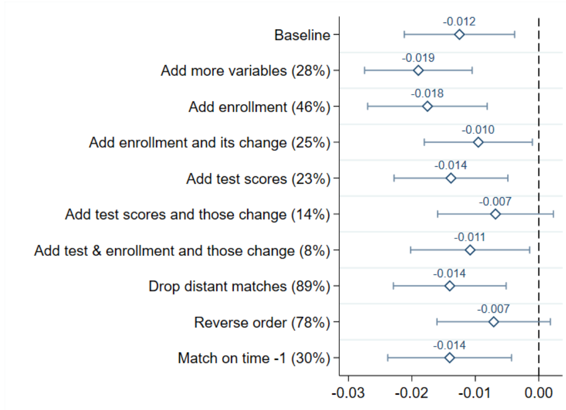
Notes: The figures present the coefficients, β, and 95% confidence intervals from equation (1) using control schools selected from the alternative matching strategies denoted on the y-axis. The baseline estimates are presented at the top of each sub-figure. The percentage in the parenthesis on the y-axis denotes the proportion of the same matched control schools as those of the baseline. The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote years after a school closure. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$.

Fig. A.22. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Alternative Matching Strategies

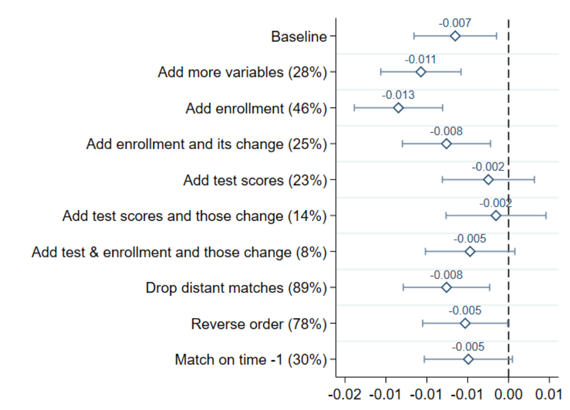
(a) High school graduation



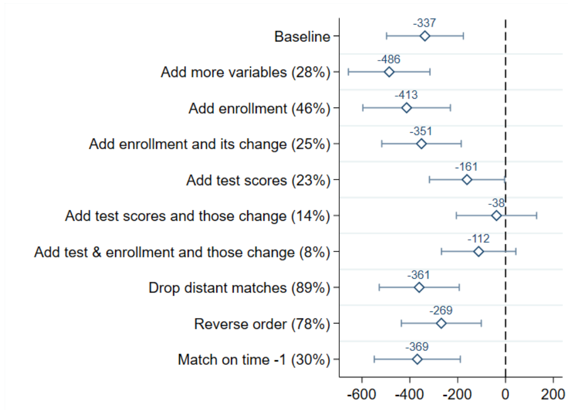
(b) Any college enrollment



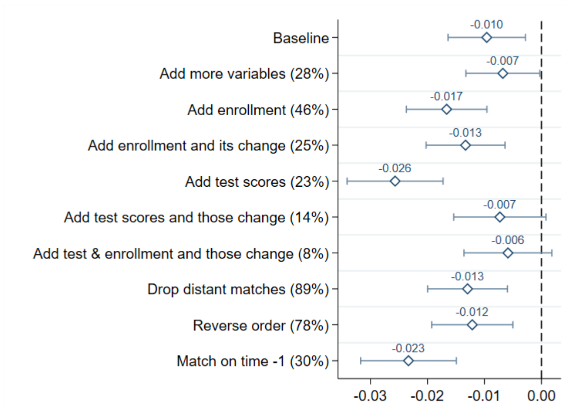
(c) Four-year college completion



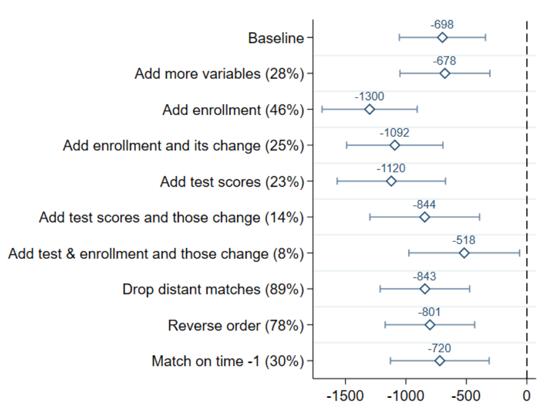
(d) College quality



(e) Employment at ages 25-27



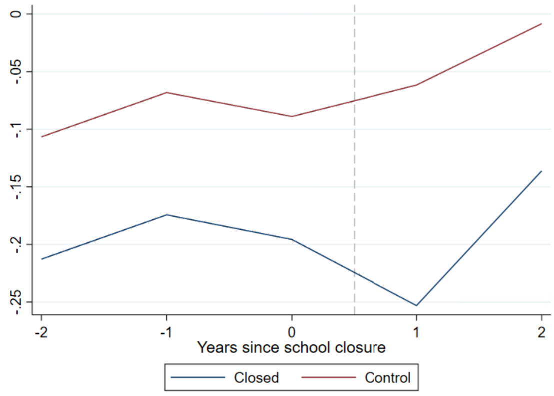
(f) Yearly wages at ages 25-27



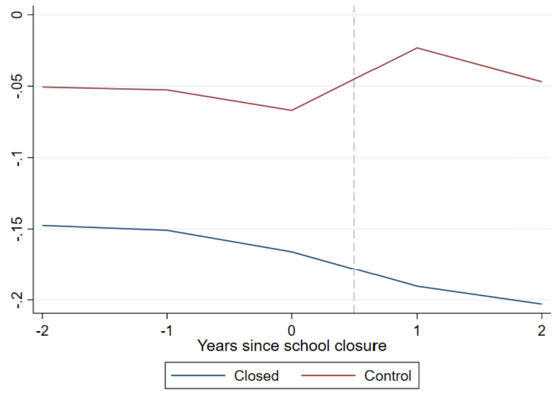
Notes: Each sub-figure presents the coefficients, γ, and 95% confidence intervals from equation (3) using control schools selected from the alternative matching strategies denoted on the y-axis. The baseline estimates are presented at the top of each sub-figure. The percentage in the parenthesis on the y-axis denotes the proportion of the same matched control schools as those of the baseline. The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standard errors are clustered at the school-by-cohort level.

Fig. A.23. Synthetic Difference-in-Differences: Outcome Trends in Short-Run Outcomes Between Closed and Control Schools

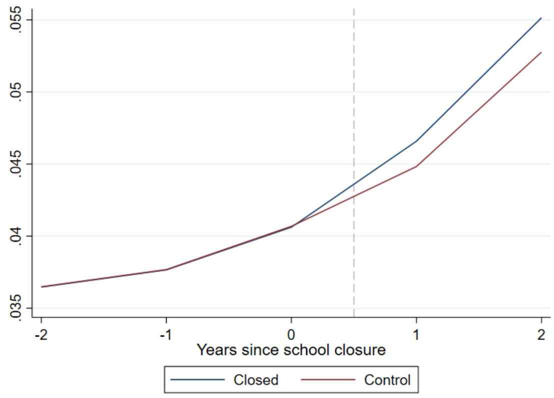
(a) Standardized math score



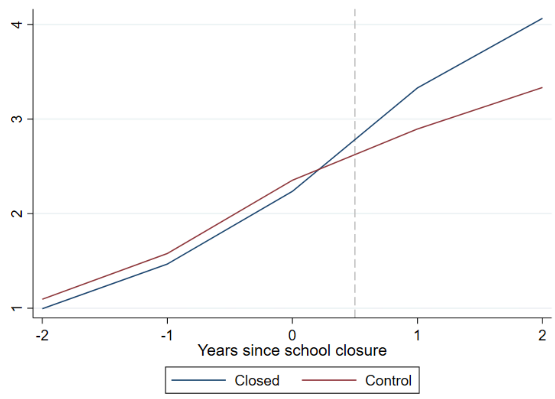
(b) Standardized reading score



(c) Absence rate



(d) Days of disciplinary action



Notes: Each sub-figure presents outcome trends from the implementation of a synthetic difference-in-differences model following Arkhangelsky et al. (2021). In this model, a synthetic control group is constructed using a donor pool of randomly chosen 10,000 students who are enrolled in the same year, same school type (e.g. elementary schools are only matched with other elementary schools), and same school locale following the NCES locale category as the treated group.

Table A.1: Average School Characteristics Across Closed, All, and Control Schools

Matching Variables	(1) Closed Schools	(2) All Schools	(3) Control Schools
Locales			
City	0.47	0.37	0.47
Urban Fringe (Or Suburb)	0.14	0.22	0.14
Town	0.16	0.14	0.16
Rural	0.24	0.26	0.24
School Types			
Elementary	0.66	0.52	0.66
Middle	0.18	0.15	0.18
Junior High	0.08	0.05	0.08
High	0.05	0.21	0.05
Elementary/Secondary	0.04	0.08	0.04
Demographics			
Non-Hispanic Black	0.21	0.14	0.18
Hispanic	0.47	0.43	0.48
Free/reduced price lunch	0.63	0.49	0.62
Other types of disadvantages	0.08	0.06	0.07
Number of Schools	470	9,288	470

Notes: The table presents average characteristics for closed, all, and control schools. For all schools, averages are calculated over the years 1998-2015. Locales are a simplified version. In more detail, locales follow eight categories in 1998-2005: large city (0.15; the proportion of closed schools), mid-size city (0.25), urban fringe of large city (0.13), urban fringe of mid-size city (0.05), large town (0.05), small town (0.15), rural inside MSA (0.00), and rural outside MSA (0.23). In 2006-2015, locales follow twelve categories: large city (0.22), mid-size city (0.22), small city (0.08), large suburb (0.07), mid-size suburb (0.02), small suburb (0.02), town short-distance to urban (0.02), town mid-distance to urban (0.05), town long-distance to urban (0.05), rural short-distance to urban (0.05), rural mid-distance to urban (0.08), and rural long-distance to urban (0.12).

Table A.2: Receiving School Quality Change: Original and Move-In Students

	(1) $t = -1$	(2) $t = 0$	(3) Difference	(4) P-Value
Original Students				
Standardized Math Score	-0.002	-0.023	-0.020	0.001
Standardized Reading Score	0.012	0.001	-0.011	0.074
Move-In Students				
Standardized Math Score	-0.221	-0.303	-0.082	0.000
Standardized Reading Score	-0.224	-0.287	-0.062	0.000

Notes: The table presents the average test scores of students in receiving schools in two distinct time points: the year right after school closures ($t = 0$) and the year immediately preceding the closures ($t = -1$). These scores are presented separately for two groups of students: those who have been enrolled in the school for at least two years (original) and those who are new arrivals in the year (move-in). For example, students observed at time point -1 in column (1) are classified as original students if they are observed in both the $t = -2$ and $t = -1$ periods at the same receiving school. The original student comprises 50,502 ($t = -1$) and 48,105 ($t = 0$) students, and the move-in students comprise 49,573 ($t = -1$) and 44,832 ($t = 0$) students.

Table A.3: Out-of-State Post-Secondary Education Enrollment After 2008

	Out-of-State College Enrollment
Closed School $\times$ Younger Cohorts	-0.003*
	(0.001)
Observations	187,817
School FE	X
Matched group $\times$ Year FE	X
Mean of the Older Cohort	0.034

Notes: The table presents the coefficient, γ, and standard errors from equation (3). The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standard errors are clustered by school-by-cohort level. *** p<0.01, ** p<0.05, * p<0.10

Table A.4: Lower and Upper Bounds on the Estimated Effect Sizes

Panel A: trimming based on differential attrition out of sample			
	(1) Baseline	(2) Lee Lower Bound	(3) Lee Upper Bound
High school graduation			
Closed School	-0.023***	-0.021***	-0.27***
× Younger Cohorts	(0.005)	(0.005)	(0.005)
Any college enrollment			
Closed School	-0.012***	-0.008*	-0.014***
× Younger Cohorts	(0.004)	(0.004)	(0.004)
Four-year college completion			
Closed School	-0.007**	-0.001	-0.007***
× Younger Cohorts	(0.003)	(0.003)	(0.003)
College quality			
Closed School	-337***	-44	-394***
× Younger Cohorts	(82)	(83)	(82)
Employment at ages 25-27			
Closed School	-0.010***	-0.006*	-0.010***
× Younger Cohorts	(0.003)	(0.003)	(0.0034)
Yearly wages at ages 25-27			
Closed School	-698***	173	-753***
× Younger Cohorts	(181)	(181)	(181)
School FE	X	X	X
Matched group × Year FE	X	X	X
Panel B: trimming based on difference in employment rate			
	(1) Baseline	(2) Lee Lower Bound	(3) Lee Upper Bound
Non-Zero Yearly wages at ages 25-27			
Closed School	-750***	272	-1,055***
× Younger Cohorts	(219)	(215)	(219)
School FE	X	X	X
Matched group × Year FE	X	X	X

Notes: The table presents the coefficients, γ, and standard errors from equation (3) with baseline sample and two trimmed samples, constructed following the Lee (2009) bounds procedure. The difference in the out-of-sample attrition rates and the decrease in employment rates after experiencing a school closure are used for calculating trimming size for Panels A and B, respectively. In the control sample, observations are trimmed by the amount at the bottom or top of the outcome distribution. The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure. The regression includes school and match group-by-cohort fixed effects, as well as individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standard errors are clustered by school-by-cohort level. *** p<0.01, ** p<0.05, * p<0.10

Table A.5: Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Different Controls

	(1) No Control	(2) Demographic Control	(3) Full Control
High school graduation			
Closed School	-0.021***	-0.021***	-0.023***
× Younger Cohorts	(0.006)	(0.006)	(0.005)
Any college enrollment			
Closed School	-0.003	-0.004	-0.012***
× Younger Cohorts	(0.005)	(0.005)	(0.004)
Four-year college completion			
Closed School	-0.002	-0.001	-0.007**
× Younger Cohorts	(0.003)	(0.003)	(0.003)
College quality			
Closed School	-227**	-206**	-337***
× Younger Cohorts	(95)	(89)	(82)
Employment at ages 25-27			
Closed School	-0.005	-0.005	-0.010***
× Younger Cohorts	(0.004)	(0.004)	(0.003)
Yearly wages at ages 25-27			
Closed School	-416**	-350*	-698***
× Younger Cohorts	(203)	(192)	(181)
Non-zero yearly wages at ages 25-27			
Closed School	-860***	-510**	-750***
× Younger Cohorts	(231)	(216)	(219)
School FE	X	X	X
Matched group × Year FE	X	X	X

Notes: Each row of the table presents the coefficients, γ, and standard errors from equation (3) with the denoted dependent variable. The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure. In all columns, the regression includes school and match group-by-cohort fixed effects. Column (1) does not include demographic and performance variables. Column (2) includes individual-level demographic control variables such as race/ethnicity, sex, ESL status, and special education status. Column (3) includes performance measures such as standardized test scores and standardized absence rate, as well as demographic variables in Column (2). Standard errors are clustered by school-by-cohort level. *** p<0.01, ** p<0.05, * p<0.10

Table A.6: Short-Run Effects of School Closure on Student Outcomes: Synthetic DID

	(1) Math	(2) Reading	(3) Absence Rate	(4) Days of Disciplinary Action
Closed School×Post	-0.053***	-0.063***	0.002***	0.691***
	(0.005)	(0.005)	(0.000)	(0.035)

Notes: The table presents the coefficients, β, and standard errors from equation (1), using synthetic difference-in-differences method from Arkhangelsky et al. (2021). The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote years after a school closure. *** p<0.01, ** p<0.05, * p<0.10

B Reasons for Public School Closures in Texas 1998-2015

Table B.1: School Closures in 1998-2003

Campus	District	Year	Enroll.	District Reform	Financial Constraint	Old Building	School Reform	Coding Change	District Closure	Low Perform	Total	Info
ALDERSON J H	LUBBOCK ISD	2001									0	0
ALTA VISTA EL	ABILENE ISD	2003									0	0
ANDERSON EL	LUFKIN ISD	1998		✓			✓				2	1
ANGLETON MIDDLE-EAST	ANGLETON ISD	2002		✓			✓				2	1
ANGLETON MIDDLE-WEST	ANGLETON ISD	2002		✓			✓				2	1
ANNAVILLE EL	CALALLEN ISD	2003		✓							1	1
ANTONIO OLIVARES EL	SOUTH SAN ANTONIO ISD	2002									0	0
ASHERTON EL	ASHERTON ISD	1998			✓						1	1
ASHERTON SCHOOL	ASHERTON ISD	1999			✓						1	1
ASPERMONT J H	ASPERMONT ISD	2002									0	0
AUSTIN H S	PORT ARTHUR ISD	2002	✓		✓						2	1
BAILEY INGLISH EL	BONHAM ISD	2002				✓					1	1
BAMMEL MIDDLE	SPRING ISD	2003	✓	✓							2	1
BARSTOW EL	PECOS-BARSTOW-TOYAH ISD	1998	✓		✓	✓					3	1
BELT LINE EL	DESOTO ISD	2003	✓		✓						2	1
BENAVIDES PRI	BENAVIDES ISD	2002									0	0
BENJAMIN F CLARK EL	SPRING ISD	2003	✓	✓							2	1
BLEAKWOOD EL	NEWTON ISD	1999			✓	✓					2	1
BOGATA EL	RIVERCREST ISD	2001									0	0
BOOKER J H	BOOKER ISD	1999						✓			1	1
BOOTH EL-NORTH	SAN BENITO CONS ISD	1999									0	0
BOWIE EL	WEATHERFORD ISD	2002	✓	✓							2	1
BOWIE SCH	MCALLEN ISD	2000									0	0
BRANDON EL	LUFKIN ISD	1998		✓			✓				2	1
BRAZORIA EL	COLUMBIA-BRAZORIA ISD	2002									0	0
BRAZORIA INT	COLUMBIA-BRAZORIA ISD	2002									0	0
BROOKHOLLOW EL	LUFKIN ISD	1998		✓			✓				2	1
BROWNFIELD INT	BROWNFIELD ISD	2002									0	0
BRYAN H S AT LAMAR	BRYAN ISD	1999									0	0
BURNET BAYLAND H S	HOUSTON ISD	1998									0	0
CAMPBELLTON EL	PLEASANTON ISD	1999	✓								1	1
CANDELARIA EL	PRESIDIO ISD	1998			✓						1	1
CENTRAL EL	BELTON ISD	1999									0	0
COMANCHE INT	COMANCHE ISD	2003		✓			✓				2	1
COSTON EL	LUFKIN ISD	1998		✓			✓				2	1
CREIGHTON INT	CONROE ISD	2001					✓				1	1
CROSSLEY EL	CORPUS CHRISTI ISD	2001	✓								1	1
D ODEM ELEMENTARY	SINTON ISD	2003									0	0
DAHLSTROM INT	HAYS CONS ISD	2000					✓				1	1
DAVID BARKLEY EL	SAN ANTONIO ISD	2002	✓								1	1
DAVID G BURNET EL	SAN ANTONIO ISD	1999	✓								1	1
DAYTOP CAMPUS	PALESTINE ISD	1999			✓						1	1
DE ZAVALA ELEMENTARY	PECOS-BARSTOW-TOYAH ISD	2003	✓		✓	✓					3	1
DE ZAVALA MIDDLE-7TH GR	PECOS-BARSTOW-TOYAH ISD	2000	✓		✓	✓					3	1
DENVER CITY INT	DENVER CITY ISD	2003									0	0
DICKSON EL	TEMPLE ISD	1998		✓							1	1
DOBIE INT	SCHERTZ-CIBOLO-U CITY ISD	1998	✓	✓			✓				3	1
ENGE-WASHINGTON INT	GROESBECK ISD	1999					✓				1	1
ERMA NASH ELEMENTARY	MANSFIELD ISD	2003									0	0
FAIRFIELD INT	FAIRFIELD ISD	2001									0	0
FREEMAN HEIGHTS EL	TEMPLE ISD	1998		✓							1	1
GLORIETA EL	ANDREWS ISD	1999									0	0
GOLDSMITH EL	ECTOR COUNTY ISD	1999	✓		✓						2	1
GOODWIN EL	PADUCAH ISD	2002	✓								1	1
H O WHITEHURST EL	GROESBECK ISD	1999					✓				1	1
H OCHOA EL	LAREDO ISD	2001									0	0
HAMBY EL	CLYDE CONS ISD	2003	✓								1	1
HAPPY MIDDLE	HAPPY ISD	2000									0	0
HERMAN E UTLEY MIDDLE SCHOOL	ROCKWALL ISD	1999									0	0
HOMEBOUND	IRVING ISD	1999									0	0
HOUSER INT	CONROE ISD	2001					✓				1	1
HOUSTON EL	CORSICANA ISD	2000									0	0
HOUSTON EL	GREENVILLE ISD	2002	✓			✓					2	1
HUNT EL	LUBBOCK ISD	2001									0	0
J M LINDSAY EL	GAINESVILLE ISD	2000		✓							1	1
JAYTON EL	JAYTON-GIRARD ISD	1999									0	0
JEFFERSON H S	PORT ARTHUR ISD	2002	✓		✓						2	1
JOHN E BARBER EL	DICKINSON ISD	2001	✓				✓				2	1
JONES EL	ABILENE ISD	2001									0	0
KENNEDY EL	MERCEDES ISD	2002									0	0
KONDIKE EL	KLONDIKE ISD	2002			✓						1	1
KYLE INT	HAYS CONS ISD	2000					✓				1	1
LAKEVIEW SCHOOL	LAKEVIEW ISD	2000	✓								1	1
LAMAR EL	GRAND PRAIRIE ISD	1999	✓								1	1
LAMAR EL	HOUSTON ISD	2002				✓					1	1
LAMAR MIDDLE	MCALLEN ISD	2000									0	0
LANIER EL	TEMPLE ISD	1998		✓							1	1
LEE ACADEMY	CORSICANA ISD	2001									0	0
LEE EL	HOUSTON ISD	2002				✓					1	1

LINCOLN H S	PORT ARTHUR ISD	2002	✓		✓						2	1
LOCKHART J H	LOCKHART ISD	2000	✓	✓							2	1
LUFKIN DUNBAR INT	LUFKIN ISD	1998		✓			✓				2	1
LUFKIN H S	LUFKIN ISD	1998		✓			✓				2	1
LUFKIN WEST J H	LUFKIN ISD	1998		✓			✓				2	1
MARLBORO EL	KILLEEN ISD	2003	✓	✓	✓						3	1
MARLIN MIDDLE	MARLIN ISD	1998					✓				1	1
MARY HOGE ACAD	WESLACO ISD	2000									0	0
MCCARDELL ACAD	HOUSTON ISD	2000									0	0
MCMURRAY EL	GAINESVILLE ISD	2000		✓							1	1
MCROBERTS EL	KINGSVILLE ISD	2002									0	0
MEDINA VALLEY J H	MEDINA VALLEY ISD	2000									0	0
MEGARGEL EL	MEGARGEL ISD	2000	✓		✓						2	1
NEW CANEY INT	NEW CANEY ISD	2001									0	0
NORTHWEST MIDDLE	NORTHWEST ISD	1998		✓							1	1
NORTHWOOD MIDDLE	NORTH FOREST ISD	2001	✓								1	1
OAKWOOD INT	COLLEGE STATION ISD	1999					✓				1	1
ONALASKA MIDDLE	ONALASKA ISD	2003		✓			✓				2	1
PEASE EL	MIDLAND ISD	2001									0	0
PEASE EL	PORT ARTHUR ISD	2002	✓		✓						2	1
POSEY EL	LUBBOCK ISD	2001									0	0
REDFORD EL	MARFA ISD	2002									0	0
REYNOLDS EL	STAMFORD ISD	2003									0	0
ROCKPORT INT	ARANSAS COUNTY ISD	2000		✓							1	1
ROGERS EL	LAMESA ISD	1999		✓							1	1
ROYAL INT	ROYAL ISD	2002					✓				1	1
RUNNELS J H	BIG SPRING ISD	1999									0	0
SALYERS EL	SPRING ISD	2003	✓	✓							2	1
SAM HOUSTON EL	SAN ANGELO ISD	1998		✓							1	1
SAM HOUSTON PRI	EDNA ISD	2000				✓					1	1
SAN SABA INT	SAN SABA ISD	1999									0	0
SANCHEZ EL	LAREDO ISD	2001		✓							1	1
SANDS EL	SANDS CISD	2002						✓			1	1
SCHULENBURG J H	SCHULENBURG ISD	2002									0	0
SHADOWBRIAR MIDDLE	HOUSTON ISD	2002									0	0
SHAW EL	CORPUS CHRISTI ISD	2003				✓					1	1
SHELDON EL	SHELDON ISD	2003	✓								1	1
SHIRLEY EL	HEREFORD ISD	2001	✓		✓						2	1
SKELLYTOWN EL	WHITE DEER ISD	2002									0	0
SKINNER EL	WEST OSO ISD	2000				✓					1	1
SLACK EL	LUFKIN ISD	1998		✓			✓				2	1
SOUTH EL	BROWNWOOD ISD	2002				✓					1	1
SOUTH WARD EL	BRADY ISD	1998		✓			✓				2	1
SPRINGLAKE-EARTH JH	SPRINGLAKE-EARTH ISD	2001						✓			1	1
STANLY EL	VICTORIA ISD	2001									0	0
STATE SCHOOL	LUFKIN ISD	1998		✓			✓				2	1
STROMAN H S	VICTORIA ISD	2000									0	0
STUBBS EL	LUBBOCK ISD	2001									0	0
T C WILEMON EL	WAXAHACHIE ISD	1999		✓		✓					2	1
THREE WAY SCHOOL	THREE WAY ISD	2002									0	0
TOMBALL EL	TOMBALL ISD	1998									0	0
TRAVIS EL	GRAND PRAIRIE ISD	1999	✓								1	1
TRAVIS EL	LYFORD CISD	2000									0	0
TRAVIS EL	WEATHERFORD ISD	2002	✓	✓							2	1
VALLEY VIEW EL	ABILENE ISD	2003									0	0
VANDERBILT EL	INDUSTRIAL ISD	2003				✓					1	1
VICTORIA H S	VICTORIA ISD	2000									0	0
W A TODD MIDDLE	DONNA ISD	2000									0	0
WALLIS EL	BRAZOS ISD	1998									0	0
WASHINGTON EL	MIDLAND ISD	2001									0	0
WASHINGTON INT	CONROE ISD	2000					✓				1	1
WEST ORANGE-STARK INT	WEST ORANGE-COVE CONS ISD	2003									0	0
WESTLAWN INT	TEXARKANA ISD	2000									0	0
WHEATLEY EL	TEMPLE ISD	1998		✓							1	1
WOODBINE INTERMEDIATE	CALLISBURG ISD	2003			✓						1	1
YOUTH OPPORTUNITY UNLIMITED	LAMAR CONSOLIDATED ISD	2002									0	0
ZAPATA MIDDLE	ZAPATA COUNTY ISD	2002		✓				✓			2	1
Statistics			32	36	19	14	26	4	0	0	131	88

Table B.2: School Closures in 2004-2009

Campus	District	Year	Enroll.	District Reform	Financial Constraint	Old Building	School Reform	Coding Change	District Closure	Low Perform	Total	Info
ALDERSON J H	LUBBOCK ISD	2001									0	0
ALTA VISTA EL	ABILENE ISD	2003									0	0
ANDERSON EL	LUFKIN ISD	1998		✓			✓				2	1
ANGLETON MIDDLE-EAST	ANGLETON ISD	2002		✓			✓				2	1
ANGLETON MIDDLE-WEST	ANGLETON ISD	2002		✓			✓				2	1
ANNAVILLE EL	CALALLEN ISD	2003		✓							1	1
ANTONIO OLIVARES EL	SOUTH SAN ANTONIO ISD	2002									0	0
ASHERTON EL	ASHERTON ISD	1998			✓						1	1
ASHERTON SCHOOL	ASHERTON ISD	1999			✓						1	1
ASPERMONT J H	ASPERMONT ISD	2002									0	0
AUSTIN H S	PORT ARTHUR ISD	2002	✓		✓						2	1
BAILEY INGLISH EL	BONHAM ISD	2002				✓					1	1
BAMMEL MIDDLE	SPRING ISD	2003	✓	✓							2	1
BARSTOW EL	PECOS-BARSTOW-TOYAH ISD	1998	✓		✓	✓					3	1
BELT LINE EL	DESOTO ISD	2003	✓		✓						2	1
BENAVIDES PRI	BENAVIDES ISD	2002									0	0
BENJAMIN F CLARK EL	SPRING ISD	2003	✓	✓							2	1
BLEAKWOOD EL	NEWTON ISD	1999			✓	✓					2	1
BOGATA EL	RIVERCREST ISD	2001									0	0
BOOKER J H	BOOKER ISD	1999						✓			1	1
BOOTH EL-NORTH	SAN BENITO CONS ISD	1999									0	0
BOWIE EL	WEATHERFORD ISD	2002	✓	✓							2	1
BOWIE SCH	MCALLEN ISD	2000									0	0
BRANDON EL	LUFKIN ISD	1998		✓			✓				2	1
BRAZORIA EL	COLUMBIA-BRAZORIA ISD	2002									0	0
BRAZORIA INT	COLUMBIA-BRAZORIA ISD	2002									0	0
BROOKHOLLOW EL	LUFKIN ISD	1998		✓			✓				2	1
BROWNFIELD INT	BROWNFIELD ISD	2002									0	0
BRYAN H S AT LAMAR	BRYAN ISD	1999									0	0
BURNET BAYLAND H S	HOUSTON ISD	1998									0	0
CAMPBELLTON EL	PLEASANTON ISD	1999	✓								1	1
CANDELARIA EL	PRESIDIO ISD	1998			✓						1	1
CENTRAL EL	BELTON ISD	1999									0	0
COMANCHE INT	COMANCHE ISD	2003		✓			✓				2	1
COSTON EL	LUFKIN ISD	1998		✓			✓				2	1
CREIGHTON INT	CONROE ISD	2001					✓				1	1
CROSSLEY EL	CORPUS CHRISTI ISD	2001	✓								1	1
D ODEM ELEMENTARY	SINTON ISD	2003									0	0
DAHLSTROM INT	HAYS CONS ISD	2000					✓				1	1
DAVID BARKLEY EL	SAN ANTONIO ISD	2002	✓								1	1
DAVID G BURNET EL	SAN ANTONIO ISD	1999	✓								1	1
DAYTOP CAMPUS	PALESTINE ISD	1999			✓						1	1
DE ZAVALA ELEMENTARY	PECOS-BARSTOW-TOYAH ISD	2003	✓		✓	✓					3	1
DE ZAVALA MIDDLE-7TH GR	PECOS-BARSTOW-TOYAH ISD	2000	✓		✓	✓					3	1
DENVER CITY INT	DENVER CITY ISD	2003									0	0
DICKSON EL	TEMPLE ISD	1998		✓							1	1
DOBIE INT	SCHERTZ-CIBOLO-U CITY ISD	1998	✓	✓			✓				3	1
ENGE-WASHINGTON INT	GROESBECK ISD	1999					✓				1	1
ERMA NASH ELEMENTARY	MANSFIELD ISD	2003									0	0
FAIRFIELD INT	FAIRFIELD ISD	2001									0	0
FREEMAN HEIGHTS EL	TEMPLE ISD	1998		✓							1	1
GLORIETA EL	ANDREWS ISD	1999									0	0
GOLDSMITH EL	ECTOR COUNTY ISD	1999	✓		✓						2	1
GOODWIN EL	PADUCAH ISD	2002	✓								1	1
H O WHITEHURST EL	GROESBECK ISD	1999					✓				1	1
H OCHOA EL	LAREDO ISD	2001									0	0
HAMBY EL	CLYDE CONS ISD	2003	✓								1	1
HAPPY MIDDLE	HAPPY ISD	2000									0	0
HERMAN E UTLEY MIDDLE SCHOOL	ROCKWALL ISD	1999									0	0
HOMEBOUND	IRVING ISD	1999									0	0
HOUSER INT	CONROE ISD	2001					✓				1	1
HOUSTON EL	CORSICANA ISD	2000									0	0
HOUSTON EL	GREENVILLE ISD	2002	✓			✓					2	1
HUNT EL	LUBBOCK ISD	2001									0	0
J M LINDSAY EL	GAINESVILLE ISD	2000		✓							1	1
JAYTON EL	JAYTON-GIRARD ISD	1999									0	0
JEFFERSON H S	PORT ARTHUR ISD	2002	✓		✓						2	1
JOHN E BARBER EL	DICKINSON ISD	2001	✓				✓				2	1
JONES EL	ABILENE ISD	2001									0	0
KENNEDY EL	MERCEDES ISD	2002									0	0
KONDIKE EL	KLONDIKE ISD	2002			✓						1	1
KYLE INT	HAYS CONS ISD	2000					✓				1	1
LAKEVIEW SCHOOL	LAKEVIEW ISD	2000	✓								1	1
LAMAR EL	GRAND PRAIRIE ISD	1999	✓								1	1
LAMAR EL	HOUSTON ISD	2002				✓					1	1
LAMAR MIDDLE	MCALLEN ISD	2000									0	0
LANIER EL	TEMPLE ISD	1998		✓							1	1
LEE ACADEMY	CORSICANA ISD	2001									0	0
LEE EL	HOUSTON ISD	2002				✓					1	1
LINCOLN H S	PORT ARTHUR ISD	2002	✓		✓						2	1
LOCKHART J H	LOCKHART ISD	2000	✓	✓							2	1
LUFKIN DUNBAR INT	LUFKIN ISD	1998		✓			✓				2	1
LUFKIN H S	LUFKIN ISD	1998		✓			✓				2	1

LUFKIN WEST J H	LUFKIN ISD	1998		✓			✓				2	1
MARLBORO EL	KILLEEN ISD	2003	✓	✓	✓						3	1
MARLIN MIDDLE	MARLIN ISD	1998					✓				1	1
MARY HOGE ACAD	WESLACO ISD	2000									0	0
MCCARDELL ACAD	HOUSTON ISD	2000									0	0
MCMURRAY EL	GAINESVILLE ISD	2000		✓							1	1
MCROBERTS EL	KINGSVILLE ISD	2002									0	0
MEDINA VALLEY J H	MEDINA VALLEY ISD	2000									0	0
MEGARGEL EL	MEGARGEL ISD	2000	✓		✓						2	1
NEW CANEY INT	NEW CANEY ISD	2001									0	0
NORTHWEST MIDDLE	NORTHWEST ISD	1998		✓							1	1
NORTHWOOD MIDDLE	NORTH FOREST ISD	2001	✓								1	1
OAKWOOD INT	COLLEGE STATION ISD	1999					✓				1	1
ONALASKA MIDDLE	ONALASKA ISD	2003		✓			✓				2	1
PEASE EL	MIDLAND ISD	2001									0	0
PEASE EL	PORT ARTHUR ISD	2002	✓		✓						2	1
POSEY EL	LUBBOCK ISD	2001									0	0
REDFORD EL	MARFA ISD	2002									0	0
REYNOLDS EL	STAMFORD ISD	2003									0	0
ROCKPORT INT	ARANSAS COUNTY ISD	2000		✓							1	1
ROGERS EL	LAMESA ISD	1999		✓							1	1
ROYAL INT	ROYAL ISD	2002					✓				1	1
RUNNELS J H	BIG SPRING ISD	1999									0	0
SALYERS EL	SPRING ISD	2003	✓	✓							2	1
SAM HOUSTON EL	SAN ANGELO ISD	1998		✓							1	1
SAM HOUSTON PRI	EDNA ISD	2000				✓					1	1
SAN SABA INT	SAN SABA ISD	1999									0	0
SANCHEZ EL	LAREDO ISD	2001		✓							1	1
SANDS EL	SANDS CISD	2002						✓			1	1
SCHULENBURG J H	SCHULENBURG ISD	2002									0	0
SHADOWBRIAR MIDDLE	HOUSTON ISD	2002									0	0
SHAW EL	CORPUS CHRISTI ISD	2003				✓					1	1
SHELDON EL	SHELDON ISD	2003	✓								1	1
SHIRLEY EL	HEREFORD ISD	2001	✓		✓						2	1
SKELLYTOWN EL	WHITE DEER ISD	2002									0	0
SKINNER EL	WEST OSO ISD	2000				✓					1	1
SLACK EL	LUFKIN ISD	1998		✓			✓				2	1
SOUTH EL	BROWNWOOD ISD	2002				✓					1	1
SOUTH WARD EL	BRADY ISD	1998		✓			✓				2	1
SPRINGLAKE-EARTH JH	SPRINGLAKE-EARTH ISD	2001						✓			1	1
STANLY EL	VICTORIA ISD	2001									0	0
STATE SCHOOL	LUFKIN ISD	1998		✓			✓				2	1
STROMAN H S	VICTORIA ISD	2000									0	0
STUBBS EL	LUBBOCK ISD	2001									0	0
T C WILEMON EL	WAXAHACHIE ISD	1999		✓		✓					2	1
THREE WAY SCHOOL	THREE WAY ISD	2002									0	0
TOMBALL EL	TOMBALL ISD	1998									0	0
TRAVIS EL	GRAND PRAIRIE ISD	1999	✓								1	1
TRAVIS EL	LYFORD CISD	2000									0	0
TRAVIS EL	WEATHERFORD ISD	2002	✓	✓							2	1
VALLEY VIEW EL	ABILENE ISD	2003									0	0
VANDERBILT EL	INDUSTRIAL ISD	2003				✓					1	1
VICTORIA H S	VICTORIA ISD	2000									0	0
W A TODD MIDDLE	DONNA ISD	2000									0	0
WALLIS EL	BRAZOS ISD	1998									0	0
WASHINGTON EL	MIDLAND ISD	2001									0	0
WASHINGTON INT	CONROE ISD	2000					✓				1	1
WEST ORANGE-STARK INT	WEST ORANGE-COVE CONS ISD	2003									0	0
WESTLAWN INT	TEXARKANA ISD	2000									0	0
WHEATLEY EL	TEMPLE ISD	1998		✓							1	1
WOODBINE INTERMEDIATE	CALLISBURG ISD	2003			✓						1	1
YOUTH OPPORTUNITY UNLIMITED	LAMAR CONSOLIDATED ISD	2002									0	0
ZAPATA MIDDLE	ZAPATA COUNTY ISD	2002		✓				✓			2	1
Statistics			32	36	19	14	26	4	0	0	131	88

Table B.3: School Closures in 2010-2015

Campus	District	Year	Enroll.	District Reform	Financial Constraint	Old Building	School Reform	Coding Change	District Closure	Low Perform	Total	Info
ALDERSON J H	LUBBOCK ISD	2001									0	0
ALTA VISTA EL	ABILENE ISD	2003									0	0
ANDERSON EL	LUFKIN ISD	1998		✓			✓				2	1
ANGLETON MIDDLE-EAST	ANGLETON ISD	2002		✓			✓				2	1
ANGLETON MIDDLE-WEST	ANGLETON ISD	2002		✓			✓				2	1
ANNAVILLE EL	CALALLEN ISD	2003		✓							1	1
ANTONIO OLIVARES EL	SOUTH SAN ANTONIO ISD	2002									0	0
ASHERTON EL	ASHERTON ISD	1998			✓						1	1
ASHERTON SCHOOL	ASHERTON ISD	1999			✓						1	1
ASPERMONT J H	ASPERMONT ISD	2002									0	0
AUSTIN H S	PORT ARTHUR ISD	2002	✓		✓						2	1
BAILEY INGLISH EL	BONHAM ISD	2002				✓					1	1
BAMMEL MIDDLE	SPRING ISD	2003	✓	✓							2	1
BARSTOW EL	PECOS-BARSTOW-TOYAH ISD	1998	✓		✓	✓					3	1
BELT LINE EL	DESOTO ISD	2003	✓		✓						2	1
BENAVIDES PRI	BENAVIDES ISD	2002									0	0
BENJAMIN F CLARK EL	SPRING ISD	2003	✓	✓							2	1
BLEAKWOOD EL	NEWTON ISD	1999			✓	✓					2	1
BOGATA EL	RIVERCREST ISD	2001									0	0
BOOKER J H	BOOKER ISD	1999						✓			1	1
BOOTH EL-NORTH	SAN BENITO CONS ISD	1999									0	0
BOWIE EL	WEATHERFORD ISD	2002	✓	✓							2	1
BOWIE SCH	MCALLEN ISD	2000									0	0
BRANDON EL	LUFKIN ISD	1998		✓			✓				2	1
BRAZORIA EL	COLUMBIA-BRAZORIA ISD	2002									0	0
BRAZORIA INT	COLUMBIA-BRAZORIA ISD	2002									0	0
BROOKHOLLOW EL	LUFKIN ISD	1998		✓			✓				2	1
BROWNFIELD INT	BROWNFIELD ISD	2002									0	0
BRYAN H S AT LAMAR	BRYAN ISD	1999									0	0
BURNET BAYLAND H S	HOUSTON ISD	1998									0	0
CAMPBELLTON EL	PLEASANTON ISD	1999	✓								1	1
CANDELARIA EL	PRESIDIO ISD	1998			✓						1	1
CENTRAL EL	BELTON ISD	1999									0	0
COMANCHE INT	COMANCHE ISD	2003		✓			✓				2	1
COSTON EL	LUFKIN ISD	1998		✓			✓				2	1
CREIGHTON INT	CONROE ISD	2001					✓				1	1
CROSSLEY EL	CORPUS CHRISTI ISD	2001	✓								1	1
D ODEM ELEMENTARY	SINTON ISD	2003									0	0
DAHLSTROM INT	HAYS CONS ISD	2000					✓				1	1
DAVID BARKLEY EL	SAN ANTONIO ISD	2002	✓								1	1
DAVID G BURNET EL	SAN ANTONIO ISD	1999	✓								1	1
DAYTOP CAMPUS	PALESTINE ISD	1999			✓						1	1
DE ZAVALA ELEMENTARY	PECOS-BARSTOW-TOYAH ISD	2003	✓		✓	✓					3	1
DE ZAVALA MIDDLE-7TH GR	PECOS-BARSTOW-TOYAH ISD	2000	✓		✓	✓					3	1
DENVER CITY INT	DENVER CITY ISD	2003									0	0
DICKSON EL	TEMPLE ISD	1998		✓							1	1
DOBIE INT	SCHERTZ-CIBOLO-U CITY ISD	1998	✓	✓			✓				3	1
ENGE-WASHINGTON INT	GROESBECK ISD	1999					✓				1	1
ERMA NASH ELEMENTARY	MANSFIELD ISD	2003									0	0
FAIRFIELD INT	FAIRFIELD ISD	2001									0	0
FREEMAN HEIGHTS EL	TEMPLE ISD	1998		✓							1	1
GLORIETA EL	ANDREWS ISD	1999									0	0
GOLDSMITH EL	ECTOR COUNTY ISD	1999	✓		✓						2	1
GOODWIN EL	PADUCAH ISD	2002	✓								1	1
H O WHITEHURST EL	GROESBECK ISD	1999					✓				1	1
H OCHOA EL	LAREDO ISD	2001									0	0
HAMBY EL	CLYDE CONS ISD	2003	✓								1	1
HAPPY MIDDLE	HAPPY ISD	2000									0	0
HERMAN E UTLEY MIDDLE SCHOOL	ROCKWALL ISD	1999									0	0
HOMEBOUND	IRVING ISD	1999									0	0
HOUSER INT	CONROE ISD	2001					✓				1	1
HOUSTON EL	CORSICANA ISD	2000									0	0
HOUSTON EL	GREENVILLE ISD	2002	✓			✓					2	1
HUNT EL	LUBBOCK ISD	2001									0	0
J M LINDSAY EL	GAINESVILLE ISD	2000		✓							1	1
JAYTON EL	JAYTON-GIRARD ISD	1999									0	0
JEFFERSON H S	PORT ARTHUR ISD	2002	✓		✓						2	1
JOHN E BARBER EL	DICKINSON ISD	2001	✓				✓				2	1
JONES EL	ABILENE ISD	2001									0	0
KENNEDY EL	MERCEDES ISD	2002									0	0
KONDIKE EL	KLONDIKE ISD	2002			✓						1	1
KYLE INT	HAYS CONS ISD	2000					✓				1	1
LAKEVIEW SCHOOL	LAKEVIEW ISD	2000	✓								1	1
LAMAR EL	GRAND PRAIRIE ISD	1999	✓								1	1
LAMAR EL	HOUSTON ISD	2002				✓					1	1
LAMAR MIDDLE	MCALLEN ISD	2000									0	0
LANIER EL	TEMPLE ISD	1998		✓							1	1
LEE ACADEMY	CORSICANA ISD	2001									0	0
LEE EL	HOUSTON ISD	2002				✓					1	1
LINCOLN H S	PORT ARTHUR ISD	2002	✓		✓						2	1
LOCKHART J H	LOCKHART ISD	2000	✓	✓							2	1
LUFKIN DUNBAR INT	LUFKIN ISD	1998		✓			✓				2	1
LUFKIN H S	LUFKIN ISD	1998		✓			✓				2	1

LUFKIN WEST J H	LUFKIN ISD	1998		✓			✓				2	1
MARLBORO EL	KILLEEN ISD	2003	✓	✓	✓						3	1
MARLIN MIDDLE	MARLIN ISD	1998					✓				1	1
MARY HOGE ACAD	WESLACO ISD	2000									0	0
MCCARDELL ACAD	HOUSTON ISD	2000									0	0
MCMURRAY EL	GAINESVILLE ISD	2000		✓							1	1
MCROBERTS EL	KINGSVILLE ISD	2002									0	0
MEDINA VALLEY J H	MEDINA VALLEY ISD	2000									0	0
MEGARGEL EL	MEGARGEL ISD	2000	✓		✓						2	1
NEW CANEY INT	NEW CANEY ISD	2001									0	0
NORTHWEST MIDDLE	NORTHWEST ISD	1998		✓							1	1
NORTHWOOD MIDDLE	NORTH FOREST ISD	2001	✓								1	1
OAKWOOD INT	COLLEGE STATION ISD	1999					✓				1	1
ONALASKA MIDDLE	ONALASKA ISD	2003		✓			✓				2	1
PEASE EL	MIDLAND ISD	2001									0	0
PEASE EL	PORT ARTHUR ISD	2002	✓		✓						2	1
POSEY EL	LUBBOCK ISD	2001									0	0
REDFORD EL	MARFA ISD	2002									0	0
REYNOLDS EL	STAMFORD ISD	2003									0	0
ROCKPORT INT	ARANSAS COUNTY ISD	2000		✓							1	1
ROGERS EL	LAMESA ISD	1999		✓							1	1
ROYAL INT	ROYAL ISD	2002					✓				1	1
RUNNELS J H	BIG SPRING ISD	1999									0	0
SALYERS EL	SPRING ISD	2003	✓	✓							2	1
SAM HOUSTON EL	SAN ANGELO ISD	1998		✓							1	1
SAM HOUSTON PRI	EDNA ISD	2000				✓					1	1
SAN SABA INT	SAN SABA ISD	1999									0	0
SANCHEZ EL	LAREDO ISD	2001		✓							1	1
SANDS EL	SANDS CISD	2002						✓			1	1
SCHULENBURG J H	SCHULENBURG ISD	2002									0	0
SHADOWBRIAR MIDDLE	HOUSTON ISD	2002									0	0
SHAW EL	CORPUS CHRISTI ISD	2003				✓					1	1
SHELDON EL	SHELDON ISD	2003	✓								1	1
SHIRLEY EL	HEREFORD ISD	2001	✓		✓						2	1
SKELLYTOWN EL	WHITE DEER ISD	2002									0	0
SKINNER EL	WEST OSO ISD	2000				✓					1	1
SLACK EL	LUFKIN ISD	1998		✓			✓				2	1
SOUTH EL	BROWNWOOD ISD	2002				✓					1	1
SOUTH WARD EL	BRADY ISD	1998		✓			✓				2	1
SPRINGLAKE-EARTH JH	SPRINGLAKE-EARTH ISD	2001						✓			1	1
STANLY EL	VICTORIA ISD	2001									0	0
STATE SCHOOL	LUFKIN ISD	1998		✓			✓				2	1
STROMAN H S	VICTORIA ISD	2000									0	0
STUBBS EL	LUBBOCK ISD	2001									0	0
T C WILEMON EL	WAXAHACHIE ISD	1999		✓		✓					2	1
THREE WAY SCHOOL	THREE WAY ISD	2002									0	0
TOMBALL EL	TOMBALL ISD	1998									0	0
TRAVIS EL	GRAND PRAIRIE ISD	1999	✓								1	1
TRAVIS EL	LYFORD CISD	2000									0	0
TRAVIS EL	WEATHERFORD ISD	2002	✓	✓							2	1
VALLEY VIEW EL	ABILENE ISD	2003									0	0
VANDERBILT EL	INDUSTRIAL ISD	2003				✓					1	1
VICTORIA H S	VICTORIA ISD	2000									0	0
W A TODD MIDDLE	DONNA ISD	2000									0	0
WALLIS EL	BRAZOS ISD	1998									0	0
WASHINGTON EL	MIDLAND ISD	2001									0	0
WASHINGTON INT	CONROE ISD	2000					✓				1	1
WEST ORANGE-STARK INT	WEST ORANGE-COVE CONS ISD	2003									0	0
WESTLAWN INT	TEXARKANA ISD	2000									0	0
WHEATLEY EL	TEMPLE ISD	1998		✓							1	1
WOODBINE INTERMEDIATE	CALLISBURG ISD	2003			✓						1	1
YOUTH OPPORTUNITY UNLIMITED	LAMAR CONSOLIDATED ISD	2002									0	0
ZAPATA MIDDLE	ZAPATA COUNTY ISD	2002		✓				✓			2	1
Statistics			32	36	19	14	26	4	0	0	131	88