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 school students and those from economically disadvantaged backgrounds.

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Keywords: school closure, demographic shift, student mobility, 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). Among Texas students entering first grade in 2001, about 4 percent experienced a school closure during their K–12 education. 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).

A growing body of research shows that school closures disrupt student outcomes, mostly focusing on test scores (Beuchert et al. 2018; Brummet 2014; Engberg et al. 2012; Larsen 2020; Steinberg and MacDonald 2019; Taghizadeh 2020b; Torre and Gwynne 2009, see Appendix Table C.1 for a brief overview of papers on the impacts of school closures). Those studies find that the negative effects on test scores tend to diminish over time, leading some to conclude that the impacts are temporary (Beuchert et al. 2018; Brummet 2014; Engberg et al. 2012; Özek, Hansen, and Gonzalez 2012; Torre and Gwynne 2009, see Appendix Figure C.1 for a forest plot). However, these patterns may reflect only short-term academic disruptions, and test scores alone may not capture the full extent of the challenges displaced students face. Drawing on a broader literature documenting the long-term consequences of early educational experiences (Chetty, Friedman, and Rockoff 2014; Cunha et al. 2006; Heckman and Mosso 2014; Jackson, Johnson, and Persico 2016), it is plausible that school closure–induced disruptions could have lasting effects. This paper examines that possibility by extending the analysis into early adulthood, focusing on outcomes such as college attainment, college quality, and, importantly, employment and earnings in the mid-20s.

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 the following criteria: the school must be listed on the official roster of closed schools on the Texas Education Agency website, must no longer be present in the Texas administrative data set, and must not be replaced by a substantially overlapping school at the same address in the following year. Using the criteria, I identify a total of 323 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, more than 90 percent of closures are broadly attributed to demographic challenges and 3 percent of closures are a consequence of persistently low performance.

By analyzing within-student variation in exposure to school closures over time between closed and control schools, I find an immediate disruption in learning. Specifically, math and reading scores drop by 0.030 and 0.034 standard deviations, respectively. Days of absence and days of disciplinary action increase by 0.05 days (0.7% increase relative to the pre-closure mean) and 0.49 days (23%) respectively. Although the effects on test scores dissipate within three years, the impacts 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 effect of school closure on the likelihood of leaving the Texas public school system.

I use across-cohort variation within-school in exposure to school closure 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 1.8 percentage points (2.7%) and the enrollment rate for any colleges decreases by 1.4 percentage points (2.8%), and college quality based on expected earnings decreases by \$191 (0.9%). Furthermore, the closure leads to a reduction in employment rates by 1.0 percentage points (1.9%) and a decrease in yearly earnings by \$700 (3.5%) at ages 25-27. Approximately one-fourth 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 \$31,000 reduction in the present discounted value of lifetime earnings per student affected by a school closure, and a total annual cost of \$7.8 billion (\$31,000×250,000) 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 students from economically disadvantaged families and those in higher grades when school closes. 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. Similarly, long-term negative outcomes are more pronounced among those from economically disadvantaged backgrounds and secondary school students. While 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 0.06 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, while school districts appear to have closed relatively lower-performing schools, the actual peer environment for displaced students worsened, potentially due to disruptions associated with the school closure process.

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; Hannum, Liu, and Wang 2021; Kirshner, Gaertner, and

Pozzoboni 2010; Larsen 2020; Steinberg and MacDonald 2019; Taghizadeh 2020a, 2020b; Torre and Gwynne 2009). Although a few studies explore the long-term impacts of school closures, they are limited to K–12 education or to college enrollment outcomes for high school students immediately after graduation (Grau, Hojman, and Mizala 2018; Larsen 2020).[1] To the best of my knowledge, this is the first paper to estimate the effects of school closures on labor market outcomes and to extend the analysis of school closure impacts into individuals' mid-20s. Investigating earnings is particularly important because it captures the broader consequences of school closures beyond education. My analysis reveals that only a portion of the observed earnings reduction can be attributed to differences in educational attainment, underscoring the need to consider labor market outcomes to fully understand the impact of school 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 (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

[1] In the context of Chile, Grau, Hojman, and Mizala (2018) find that school closures led to an increase in dropout rates by 1.8-2.5 percentage points and a decrease in student retention by 3.9-4.4 percentage points. Using *high school* closure in Milwaukee public school district, Larsen (2020) shows a decrease in high school graduation rates by 7.5 percentage points and college attendance by 5.1 percentage points right after graduation as a result of the closures.

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 (e.g., Carrell, Hoekstra, and Kuka 2018; Chetty, Friedman, and Rockoff 2014; Garces, Thomas, and Currie 2002; Heckman, Pinto, and Savelyev 2013; Hyman 2017; Sacerdote 2012). 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 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 during 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 must be listed on the Texas Education Agency's closure list, disappear from my dataset, and is not replaced by a substantially overlapping school at the same address in the following year. My analysis covers the period from 1998 to 2015 for the short-run analysis, and for the long-run analysis, it includes 1998 to 2003 for all school levels, 2004 to 2007 for middle (intermediate) and high schools, and 2008 to 2010 for high schools. I only consider school closures from non-charter instructional campuses in regular and independent districts. 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 323 school closures meeting the criteria. A list of these closed schools is provided in Appendix D with their closure years and school districts. About 18 schools closed each year from 1998 to 2015, with closures occurring fairly consistently over time, though there were some fluctuations (see Appendix Figure A.1). Figure 1 presents the locations of the 323 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 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 74 percent of those affected by closures, while they make up 58 percent of all students. Economically disadvantaged students, including those receiving free or reduced-price lunch and other forms of aid, account for 75 percent of students affected by closures, compared to 56 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 204 out of 323 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 D). 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.[2]

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.2. 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; 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 districts and schools account for about 90 percent of the identified reasons for closures. Closures primarily associated with low performance constitute 3 percent of the cases.[3] Importantly, Texas experienced an overall increase in enrollment during this period. Despite this trend, enrollment declines remained the primary driver of school closures. This suggests that similar patterns may exist in other states—for instance, Brummet (2014) finds that declining enrollment is also the primary reason for school closures in Michigan, and Harris and Martinez-Pabon (2023) identify enrollment as the strongest predictor of school closures nationwide. These parallels reinforce the generalizability of the patterns observed in Texas. Furthermore, these findings challenge the conventional understanding of school closures, which often frames them as a dichotomy between urban closures due to low performance and rural closures due to low enrollment (Tieken and Auldridge-Reveles 2019).

The category labeled "low performance" is mostly closures that are initiated by the education

[2] 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.

[3] 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 62 out of 103 closures, 56 out of 110 closures, and 86 out of 110 closures.

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.[4] 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.[5] To address potential concerns related to coding changes and school closures without physical closure, I exclude, in my baseline estimation, closures where more than 30% of displaced students are observed at the same address as the closed school after the closure.[6] As shown in Appendix Figure A.2, the number of closures classified as coding changes decreases from 13 cases (3.2%) to 1 case (0.3%) after applying the same-address restriction, implying that most coding changes are eliminated from the analysis sample after the restriction is imposed.

[4] 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.

[5] For example, 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.

[6] Estimation results using different cutoffs are presented in Appendix B.2 and B.3. While the results are consistent across different cutoffs, the negative impacts are somewhat more pronounced for certain outcomes when a stricter cutoff is applied. I also identified a potential data issue: in some cases, students appear to remain at the same address, but they have actually been moved to a different school, as confirmed by the school district. As a result, some true school closures may have been inadvertently excluded when applying the same-address restriction.

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;[7] (3) standardized math and reading scores;[8] and (4) high school graduation.[9]

THECB data includes all public and most private post-secondary education data in Texas.[10] 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 age 26.[11]

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[7] 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.

[8] 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.

[9] In the analysis of high school graduation, I exclude two closed schools due to a potential data issue. Some cohorts from these schools report a 0 percent graduation rate, while others show rates between 50 and 70 percent. I find no notable differences between these cohorts, including in average 12th-grade attendance. To address concerns about excluding these schools, I also construct a proxy for high school graduation based on 12th-grade attendance. The two measures—graduation based on TEA files and 12th-grade attendance—are highly correlated (0.83) and yield similar estimates.

[10] 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.

[11] Apart from the data provided by the Texas Higher Education Coordinating Board (THECB), I also have access

TWC data includes quarterly individual data on employment, industry, and earnings for all workers covered by the Unemployment Insurance program.[12] 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; (3) earnings-based college quality following Chetty, Friedman, and Rockoff (2014).[13] Earnings are converted to 2020 dollars using the consumer price index and are winsorized at the 99th percentile at the state level.

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 Appendix B.4.

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

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 (Appendix Section B.4).

[12] 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.

[13] 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.[14] 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.[15] 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

[14] 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.

[15] 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 74 percent and 72 percent of closed and control schools, respectively, compared to 58 percent of all schools. Economically disadvantaged students account for 74 percent and 73 percent of closed and control schools, respectively, compared to 56 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.3. 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 Appendix B.1, 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 323 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 31,557 students for test scores, 57,293 students for disciplinary action, and 69,215 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:

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

where Y_{isgt} is an outcome of student i in relative year t ($t = -1$ represents the school year preceding closure, and $t = 0$ denotes the school year immediately following 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. When I examine equation (1) including heterogeneity analysis, I left the third and fourth years after the school closure unbalanced since balancing those years restricts the sample to mostly elementary school students.

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.[16] Appendix Figure A.4 (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.4 (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 Appendix B.2, I also 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 analysis 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.

[16] Another potential concern is differential attrition before school closure, which could influence sample composition. For example, some students may move out in anticipation of a school closure. In this regard, my estimation does not fully capture all displaced students. However, this does not imply that my estimate is biased. While students who move out early may be selective, my estimation relies on within-student variation, meaning that as long as students in closed and control schools follow the same trends, differences in their average characteristics should not affect the validity of the results.

I construct a long-run analysis sample based on graduating cohorts using 130 closed schools between 1998 and 2008. 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. Thus, 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 42,447 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:

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

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. If student characteristics are not observed, I assign an additional missing category. 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). 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 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 serves grades 1 through 5, $c = 0$, $c = 1$, and $c = 2$ correspond to grades 5, 4, and 3, respectively.), and if $c \in \{-3, -2, -1\}$, students are in the "older cohort" (i.e., students who had graduated before the school closed; in the previous example, $c = -3$, $c = -2$, and $c = -1$ correspond to grades 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 level.[17]

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. 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). Appendix Figure

[17] 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 Appendix Figure B.9, I compare results using balanced and unbalanced panels, presenting consistent findings.

A.5 shows that, relative to older cohorts, younger cohorts in closed and control schools do not exhibit significant differences in demographic characteristics or performance measures prior to the experience of school closure.[18] Moreover, I do not find significant differences in either the proportion of students transferring to another school prior to closure or the average test scores of those transferring students, between closed and matched control schools (Appendix Figures A.7).

5 Estimation Results

5.1 Short-Run Effects on Student Outcomes

Figure 2 presents event study estimates, particularly plotting the coefficients and 95% confidence intervals of the coefficient ρ_t from equation (2).[19] First of all, the coefficients before the school closures are close to zero and not statistically significant, except for $t = -2$ in reading scores. 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. This represents a key advancement over previous literature, which often struggled with violations of the parallel trends assumption due to difficulties in constructing comparable comparison groups. Sub-figures (a) and (b) depict a decline in standardized math and reading scores (standardized by grade and year), 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 after closure, which persists for four years post-closure. School closures also lead to an increase in the number of disciplinary action days immediately after closure, and this effect continues to grow over the following four years. Most of the increase in disciplinary actions is due to out-of-school suspensions, and I find increases on both the extensive and intensive margins (Appendix Figure A.9).[20]

[18] Moreover, I estimate the same regression using short-run outcome variables one year after closure and calculate the difference between those two time points to see whether I can observe changes in short-run outcomes for younger cohorts after school closure compared to older cohorts. As presented in Appendix Figure A.6, younger cohorts experience drops in test scores and an increase in days of absence—patterns consistent with those observed in my baseline short-run analysis.

[19] See Appendix Figure A.8 for raw trends of short-run outcomes for closed and control schools around school closure.

[20] Appendix Figure B.8 presents the same estimation results for school closures used in the long-run analysis, alongside the baseline results. While the overall findings are consistent, some differences emerge. Compared to

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 0.05 days and 0.49 days, which is a 0.7 percent and 23 percent increase relative to the pre-closure means. Days of disciplinary action further increase after 3-4 years up to 0.78 days.[21]

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.[22] 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.[23] The distribution of difference is also divided into terciles: worse, similar, and better.[24] Importantly, the interpretation of the heterogeneity analysis is unlikely to be causal, as various factors are interrelated (e.g., urbanicity is correlated with racial composition).

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

the baseline, school closures in the long-run sample show a larger immediate drop in test scores, though these scores eventually converge to baseline levels. Additionally, while the impact on days of absence fades over time, I find a more pronounced and persistent increase in days of disciplinary action.

[21] The pre-closure mean in the table differs from that in the event study figure. This is because the table allows data from 3–4 years after the closure to be unbalanced to include more students in higher grades. In contrast, Figure 2 uses a balanced panel, which excludes test score data from later closure years due to data constraints described in Section 3. As shown in Appendix Table B.3, average student test scores are lower in the later years of school closures, which leads to the lower pre-closure mean reported in the table. For reference, estimation results using the same balanced sample as in Figure 2 are presented in Appendix Table B.1, showing consistent findings.

[22] Heterogeneity analysis regarding the reasons for closures is not conducted since the occasions are too small other than reasons related to enrollment.

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

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

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, displaced students from originally low-performing schools experience a larger increase in days of absence and disciplinary action, while those from high-performing schools experience a larger decrease in test scores. Lastly, students displaced to worse-performing schools experience a larger drop in test scores while students displaced to better-performing schools experience a larger increase in days of disciplinary action.[25]

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 while Black students experience a more substantial rise in days of disciplinary action. This aligns closely with the literature addressing racial disproportionality in exclusionary disciplines (Anderson and Ritter 2017; Barrett et al. 2021; Losen et al. 2015). Meanwhile, White students experience a greater drop in reading scores, 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. Secondly, economically disadvantaged students have more significant increases in days of absence and days of disciplinary action. 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 in peer quality 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 students moving to schools where more than 70 percent of students in

[25] In Appendix A.6, I further explore heterogeneity in outcomes based on the proportion of displaced students and the distance between closed and receiving schools. The median transfer distance is about one mile, with approximately half of the displaced students moving to the same receiving school. I overall find that students experience greater disruption when they transfer with a smaller group of peers or over a longer distance.

receiving schools are displaced students (see Appendix Figure A.12 for robustness with different cutoffs). Figure 5 (a) and (b) illustrate the changes in peer quality, showing a decrease in math and reading scores by 0.06 after closure, followed by a recovery over time.[26] However, the expected quality shows different patterns. 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). For comparison, the same sample of students used in the peer quality analysis is included. 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 0.01 to 0.09. After additional descriptive analysis, I find 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 (Brummet 2014; Imberman, Kugler, and Sacerdote 2012; Taghizadeh 2020a, see Appendix A.7 for more details).[27]

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 labor market 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. 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. However, I do not find significant impacts on four-year

[26] 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)*-0.067=-0.0009 standard deviation for math and (0.45*0.0271+0.43*0.0087+0.10*0.0124)*-0.058=-0.001 standard deviation for reading.

[27] Moreover, I explore other outcomes. In Appendix Figure A.13, I present outcomes of days of absence and days of disciplinary action after standardization, which also present consistent results. In Appendix Figure A.14, I also find a decrease in the number of staff per student following school closures.

college completion and enrollment (Appendix Figure A.16). 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 in both educational and labor market outcomes.[28] 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 if it is a part of a district reform, or any negative impacts on staff morale or turnover in the year leading up to closure. As shown in Appendix B.3, I also present robustness checks for my long-run estimation results using alternative samples and control variables.

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 1.8 percentage points (2.7%),[29] enrolling in any college by 1.4 percentage points (2.8%), and decreases the college quality by \$191 (0.9%) by the age of 26. I do not find significant effects on obtaining a bachelor's degree. I further find that experiencing school closure makes students 1.0 percentage points (1.9%) less likely to be employed and leads to \$700 (3.5%) 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). 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-fourth 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

[28] Appendix Table A.3 presents estimation results from equation (3), where I separate the younger cohorts into those in the highest grade ($c = 0$) and those not in the highest grade ($c \in \{1, 2\}$). The results show that the impacts are more pronounced among students not in the highest grade compared to the baseline estimates, while none of the estimates for the highest grade are statistically significant except for high school graduation.

[29] As noted in Section 3, I excluded two closed schools from the high school graduation analysis due to a potential data issue. To address concerns about this issue, I also conduct a robustness check using a proxy for high school graduation based on 12th-grade enrollment. Appendix Figure A.15 shows that the estimation results remain consistent regardless of whether the original graduation measure or the proxy is used.

outcomes. Appendix Figure A.17 presents heterogeneity across school characteristics. While overall negative effects are observed and many estimates are not statistically distinguishable from one another, a few patterns still emerge.[30] First, I find broadly similar effects between urban and rural school closures, except in employment outcomes, which are more negatively affected in urban areas. Second, students originally in low-performing schools generally experience more pronounced effects. 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. Moreover, I find that 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. This suggests that even when students move to schools with higher-performing peers, they could still encounter adverse consequences.

Appendix Figure A.18 presents heterogeneity across student characteristics. While much of the confidence intervals overlap across estimates, a few patterns are worth noting. First, 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. This finding aligns closely with Chetty, Hendren, and Katz (2016), which shows that adolescents face greater disruption when moving to new environments compared to younger children under age 13. Second, 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.19 and A.20. 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 or economically disadvantaged families.

[30] 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.

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 Appendix Section B.4, I discuss this issue in three layers: (i) I find no evidence of a significant difference in attrition rates immediately after school closure; (ii) I do not find increase in out-of-Texas post-secondary education attainment after school closure experience; (iii) I obtain results consistent with baseline earnings estimates when using a sample conditional on employment, as well as when estimating expected earnings based on education.

6 Discussion: Mechanism and Size of Effects

In this section, I discuss the mechanisms behind my findings and compare my estimates to previous research. First, I found a significant decline in educational attainment and earnings among displaced students. These negative impacts may arise from two main channels: changes in school quality and disruption. Since most closures are driven by low enrollment and students typically move to neighboring schools, substantial differences in school quality are unlikely. This is supported by evidence showing similar average test scores between closed and receiving schools (see footnote 26 for a simple calculation of the impact of peer quality changes). Additionally, student-teacher ratios remain relatively low at the school level (Appendix Figure A.14). The adverse effects are not fully attributable to school quality differences, but rather to the broader disruption caused by school closures. This interpretation is further supported by the finding that students who moved with a smaller peer group or over longer distances—those more likely to experience disruption—faced larger negative impacts (Appendix Figures A.10, A.11, and A.21). Lastly, Kirshner, Gaertner, and Pozzoboni (2010) highlight students' emotional experiences, noting that "students were upset to have to leave a school where they felt supported" based on surveys of displaced students (see also Jackson et al. 2020).

While the disruption from school closures entails a complex set of changes that are difficult to isolate,[31] I find a persistent increase in behavioral issues among displaced students. A simple

[31] The student mobility literature (Hanushek, Kain, and Rivkin 2004; Rockoff and Lockwood 2010; Schwartz,

regression analysis suggests that the rise in disciplinary incidents in the long-run sample is correlated to a $224 decrease in earnings and a $92 decrease in college quality, corresponding to 32% and 48% of the overall negative effects of school closures, respectively.[32] Importantly, the increase in behavioral issues may reflect more than just misbehavior; it could indicate broader adjustment difficulties that students face in their new school environments. This may suggest that students struggle to adapt, whether they express it through behavioral issues or not. It is well established that such behavioral impacts have long-term consequences (Chetty et al. 2011; Heckman, Pinto, and Savelyev 2013; Jackson 2018; Jackson et al. 2020). While average test scores tend to recover over time, students in secondary schools experience more persistent declines. Heterogeneity analysis further finds that secondary school students experience more pronounced negative long-term impacts, suggesting that disruptions in human capital accumulation might also play a significant role.[33]

Second, I compare my estimation results with previous studies on the impacts of school closures (see Appendix Section C.1 for more details). Studies in similar contexts (Brummet 2014; Engberg et al. 2012; Han et al. 2017; Kirshner, Gaertner, and Pozzoboni 2010; Larsen 2020; Özek, Hansen, and Gonzalez 2012; Steinberg and MacDonald 2019; Torre and Gwynne 2009) find overall negative effects on test scores, absenteeism, and suspensions. While test scores tend to decline following school closures, they generally recover over time, with the magnitude of the decline varying across studies. Notably, Brummet (2014) and Han et al. (2017),

Stiefel, and Cordes 2017; Schwerdt and West 2013; Xu, Hannaway, and D'Souza 2009) also documents significant declines in test scores, suggesting that changes in the overall school environment are highly disruptive. However, few studies explore the mechanisms underlying these disruptions or propose potential interventions. This represents an important area for future research.

[32] I regress earnings at ages 25–27 (or college quality) on the number of days of disciplinary actions using the long-run sample, controlling for demographic variables, school fixed effects, and their interactions. As shown in Appendix Figure B.8, other short-run outcomes converge to zero when using the long-run sample of school closures. I then multiply the estimated coefficient by the observed change in disciplinary days for this sample (0.97 additional days after 3–4 years; see Appendix Table B.2): -231 $\times$ 0.97 = –224 (32% of the total $700 earnings impact); and –95 $\times$ 0.97 = –92 (48% of the total $191 impact on college quality).

[33] Similarly, other heterogeneity analyses show similar patterns. As shown in Appendix Table A.4, students who experience a larger increase in behavioral issues—such as economically disadvantaged students, as well as those from low-quality schools—also suffer greater negative impacts on long-run outcomes. Moreover, students who move to lower-performing schools experience persistent declines in test scores and larger drops in earnings, while those who transfer to higher-performing schools exhibit greater increases in behavioral issues and larger declines in long-term educational attainment. Moreover, estimation results using different cutoffs for the proportion of displaced students observed at the same address (Appendix Figures B.7 and B.12) show that school closures under stricter cutoffs are associated with more pronounced negative impacts on disciplinary actions and several long-run outcomes, including high school graduation and earnings.

which use state-level data, find results that closely align with this paper. Additionally, Engberg et al. (2012) and Steinberg and MacDonald (2019) document relatively persistent increases in absenteeism and suspensions. Regarding long-term outcomes, Larsen (2020) finds sizable negative effects on educational attainment, and his estimate of high school graduation impacts are comparable to mine. Although many of these studies are based on a single school district and often do not satisfy the parallel trends assumption, making direct comparisons difficult, I find that the patterns in my estimation results are broadly consistent, strengthening the external validity of my findings.

Lastly, to better understand the magnitude of these effects, I compare my long-run estimates with existing research on the long-run effects of school inputs and intervention/disruption (see Appendix Section C.2 for more details). 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 3.5 percent decrease in earnings at ages 25-27, which is equivalent to a 0.36 standard deviation decrease in class quality for one year or a one standard deviation decrease in teacher quality for 2.6 years. Moreover, 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. 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 $7.8 billion. This estimation implies that the annual cost of school closures is approximately 1.3 times the cost of school shootings in the US.[34]

[34] 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.

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 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 and those from originally low-performing schools and economically disadvantaged families.

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 0.36 standard deviation increase in overall class quality for one year. My back-of-the-envelope calculations further suggest that the annual cost of school closures due to displaced students is about $7.8 billion in the US, without considering the potential benefits of school closures.

The findings of long-run negative impacts suggest that the current implementation of school closure policy does not adequately address the disruption faced by displaced students. Despite the large number of closures and the considerable backlash they have generated, there has been surprisingly little policy discussion or implementation focused on mitigating these harms. This lack of attention is especially concerning given the scale and frequency of closures. Future research should explore strategies to mitigate these adverse effects.

Potential areas include the following: First, phasing out schools gradually rather than abruptly. This approach, implemented in New York City where students could choose to move out (Bifulco and Schwegman 2020), should be tested in broader contexts without school choice models. Second, implementing strategies to reduce disruption. For instance, efforts to keep peer groups together or to introduce pre-closure interventions—such as joint extracurricular activities or shared classes between closing and receiving schools, which help students become familiar with their new environment ahead of time. Although such approaches have been adopted in at least one Vermont district (Dellinger-Pate 2025; Lazenby 2025), they have not yet been formally evaluated. Third, offering support measures such as mentorship programs, counseling, and staff training on school closure impacts. These supports are especially important in light of the observed increase in behavioral issues, which may signal broader adjustment difficulties. Supporting students and preparing staff can help alleviate these challenges and promote smoother transitions.

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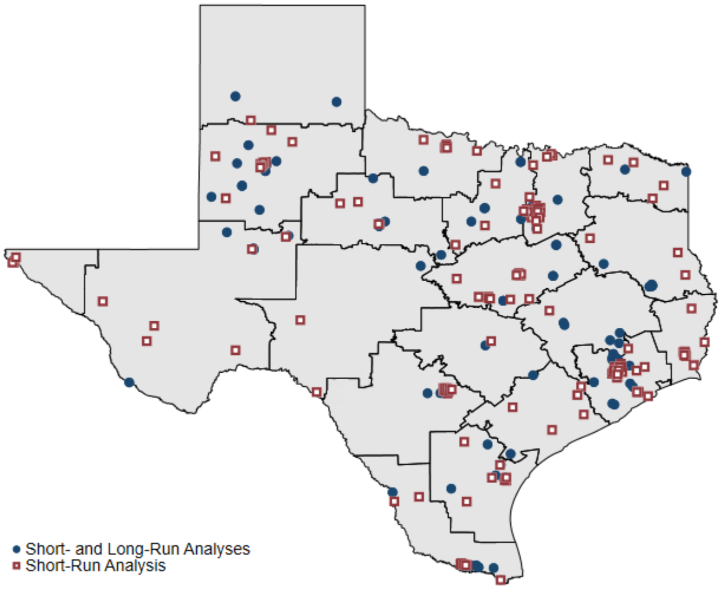
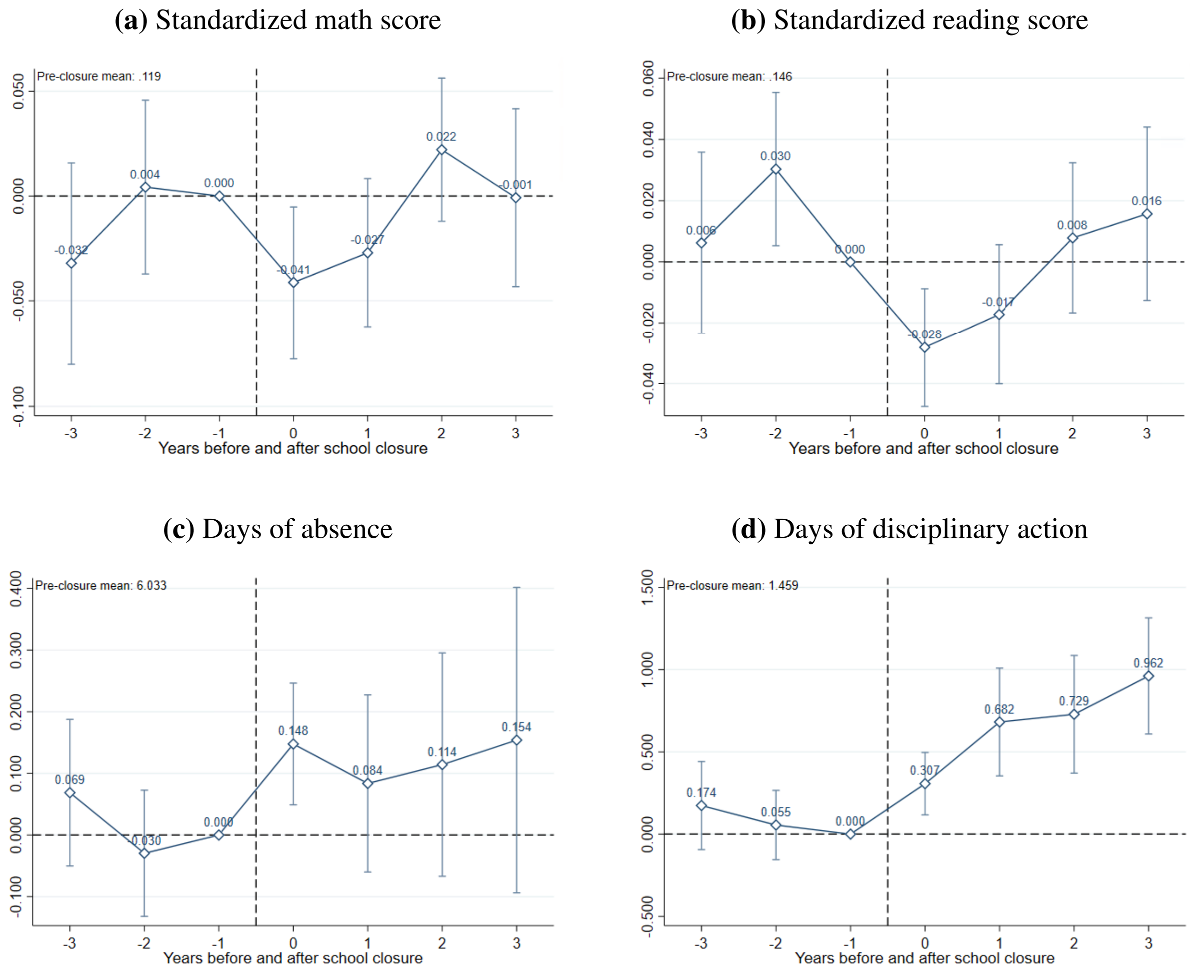
8 Figures and Tables

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



Notes: The figure presents the locations of public school closures between 1998-2015: 130 school closures used in both short- and long-run analysis and 193 school closures used in only short-run analysis. To be considered a closed school, the school must be officially listed as closed by the Texas Education Agency (TEA), be a non-charter instructional campus in a regular, independent district, have been observed during the previous period (1994–1997), and is not replaced by a substantially overlapping school at the same address in the following year. For more details on the definition of closed schools, see Section 2.

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



(a) Standardized math score

(b) Standardized reading score

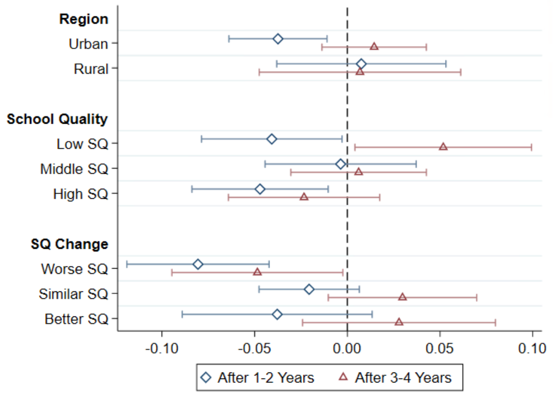
(c) Days of absence

(d) Days of disciplinary action

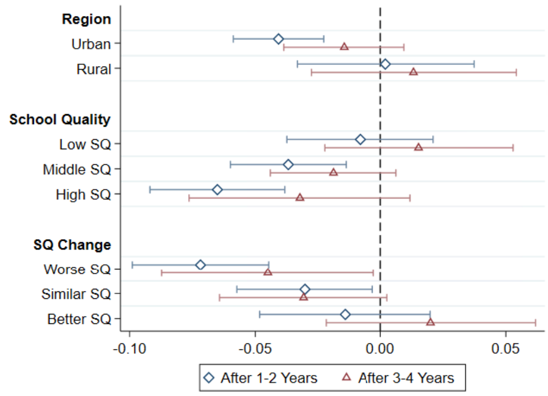
Notes: The figures present the coefficients, ρ_t, and 95% confidence intervals from equation (2).These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) 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. The analysis sample is balanced. The pre-closure mean refers to the average value of the outcome variable at time $t = -1$ for displaced students in the analysis sample. Standard errors are clustered by school at $t = -1$.

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

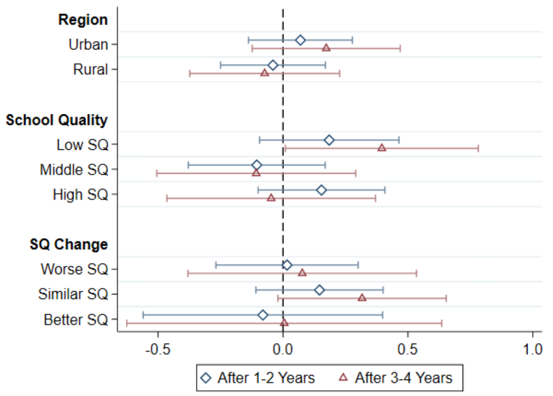
(a) Standardized math score



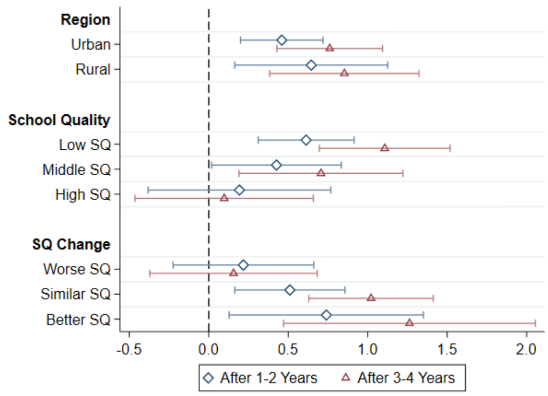
(b) Standardized reading score



(c) Days of absence



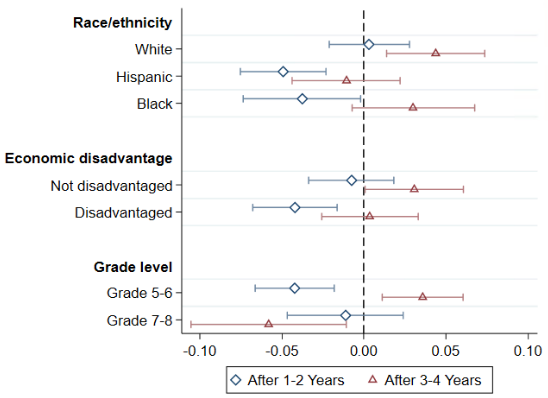
(d) Days of disciplinary actions



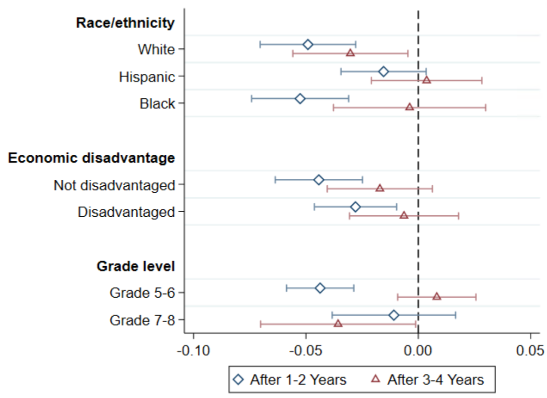
Notes: The figures present the coefficients, β, and 95% confidence intervals from equation (1) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote years after a school 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. The analysis sample is balanced, except for the third and fourth years after the school closure, which may be missing for students in higher grades. Standard errors are clustered by school at $t = -1$.

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

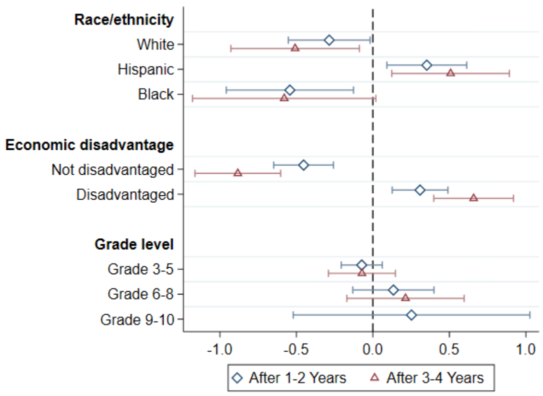
(a) Standardized math score



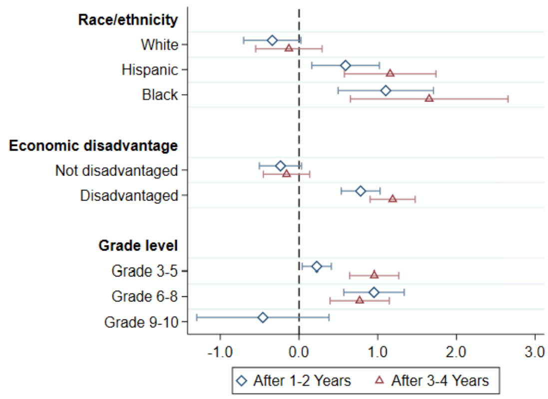
(b) Standardized reading score



(c) Days of absence



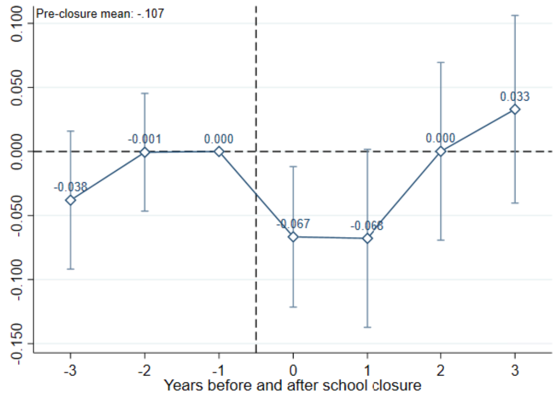
(d) Days of disciplinary actions



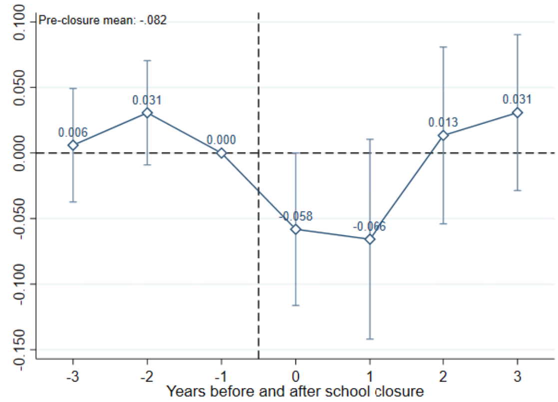
Notes: The figures present the coefficients, β, and 95% confidence intervals from equation (1) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote years after a school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

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

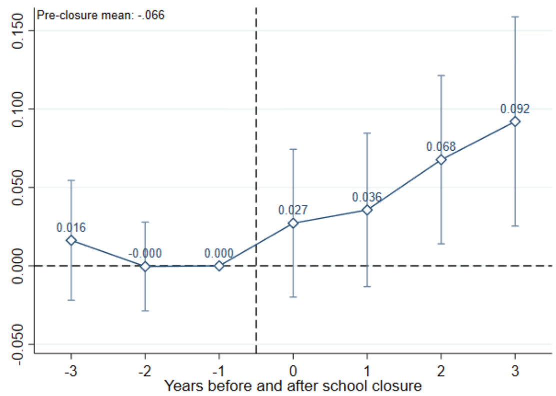
(a) Peer quality: standardized math score



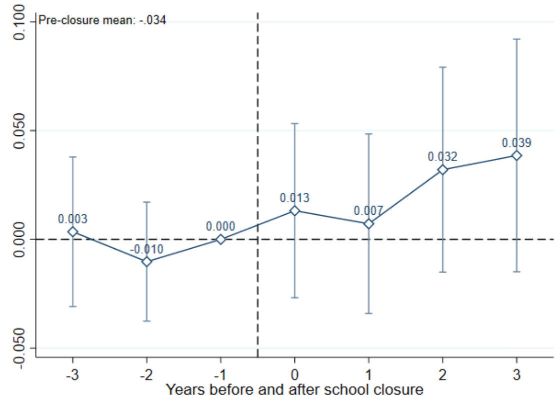
(b) Peer quality: standardized reading score



(c) Expected quality: standardized math score



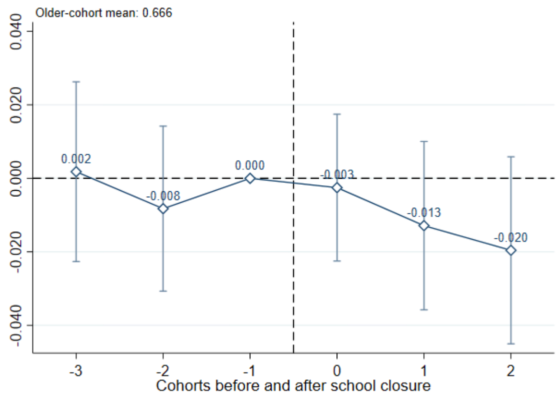
(d) Expected quality: standardized reading score



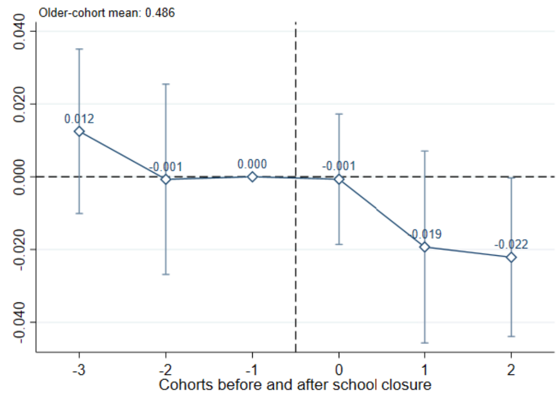
Notes: The figures present the coefficients, ρ_t, and 95% confidence intervals from equation (2), where the outcome variables are the school average test scores. When it comes to sub-figures (a) and (b), the outcome variables are yearly school average test scores and the construction of average values excludes displaced students from the calculations after school closure (i.e., $t >= 0$). For sub-figures (c) and (d), the outcome variables are the school average over the four years preceding the school closure (i.e., $t \in \{-4, \ldots, -1\}$). For all sub-figures, I exclude receiving schools if more than 70% of their students are displaced 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. The analysis sample is balanced. The pre-closure mean refers to the average value of the outcome variable at time $t = -1$ for displaced students in the analysis sample. Standard errors are clustered by school at $t = -1$.

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

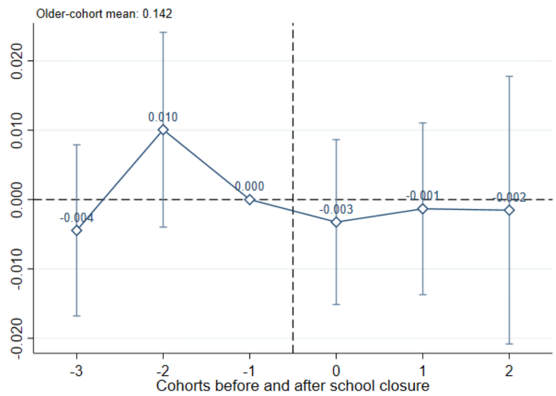
(a) High school graduation



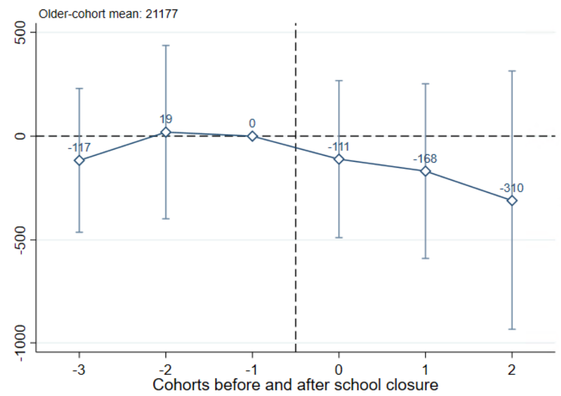
(b) Any college enrollment



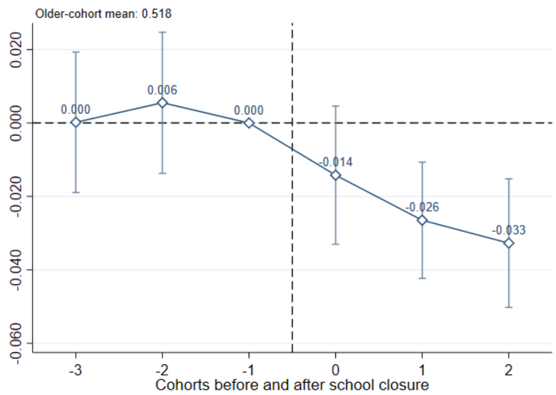
(c) Four-year college completion



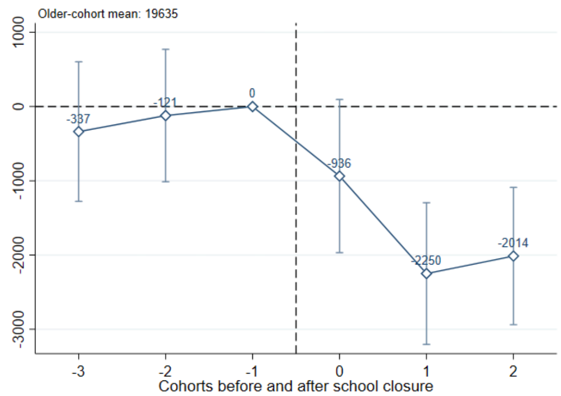
(d) College quality



(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. The analysis sample is balanced. The older-cohort mean refers to the average value of the outcome variable for students in older cohorts ($c \in \{-3, -2, -1\}$) attending closed schools in the analysis sample. Standard errors are clustered at the school 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	-0.030**	-0.034***	0.045	0.492***
	(0.012)	(0.008)	(0.086)	(0.118)
Closed School×After 3-4 Years	0.013	-0.010	0.118	0.777***
	(0.013)	(0.011)	(0.122)	(0.146)
Observations	433,726	433,726	1,145,846	957,637
Individual FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Pre-Closure Mean	-0.016	0.024	6.667	2.109

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. The analysis sample is balanced, except for the third and fourth years after the school closure, which may be missing for students in higher grades. The pre-closure mean refers to the average value of the outcome variable at time $t=-1$ for displaced students in the analysis sample. 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: Educational Outcomes

	(1) Graduate HS	(2) Enroll Any College	(3) BA Degree	(4) College Quality
Closed School × Younger Cohorts	-0.018***	-0.014***	-0.001	-191**
	(0.005)	(0.005)	(0.003)	(90)
Observations	163,336	164,497	164,497	163,336
School FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Mean of the Older Cohort	0.666	0.495	0.141	21,136

Panel B: Labor Market Outcomes

	(1) Employment	(2) Yearly Earnings
Closed School × Younger Cohorts	-0.010**	-700***
	(0.005)	(267)
Observations	164,497	164,497
School FE	X	X
Matched group × Year FE	X	X
Mean of the Older Cohort	0.524	19,739

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. The mean of the older cohort refers to the average value of the outcome variable for students in older cohorts ($c \in \{-3, -2, -1\}$) attending closed schools in the analysis sample. Note that the dependent variables for high school graduation and college quality have fewer observations due to the exclusion of two closed schools from the analysis because of a potential data issue (see Section 3 for more details). Standard errors are clustered at the school 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 (2025)

Table of Contents

A Additional Figures and Tables **1**

A.1 Closed School and Displaced Students Characteristics 1

A.2 Sample Attrition 5

A.3 Long-Run Analysis Balance Test 6

A.4 Raw Trends in Short-Run Outcomes 10

A.5 Short-Run Effects on Days of Disciplinary Actions: Different Margins 12

A.6 Short-Run Effects of School Closure: Heterogeneity 14

A.7 School Quality Changes 17

A.8 High School Graduation Proxy Across Different Definition Cutoffs 22

A.9 Long-Run Effects of School Closure on Four-Year College Enrollment 23

A.10 Long-Run Effects of School Closure: Separate Estimates for Highest Grade and Others 24

A.11 Long-Run Effects of School Closure: Heterogeneity 25

B Sensitivity Aanlysis **32**

B.1 Matching 32

B.2 Short-Run Analysis 38

B.3 Long-Run Analysis 46

B.4 Out-of-Texas Attrition 54

C Comparison with Previous Papers **59**

C.1 School Closure Impacts on Displaced Students 59

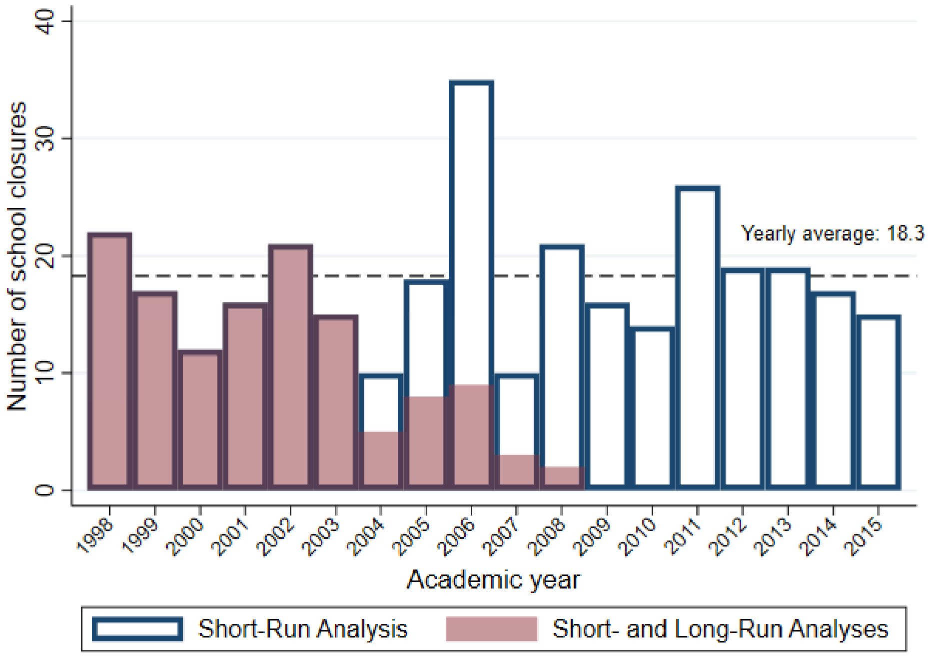
C.2 Long-run Effects of School Inputs and Intervention/Disruption 63

D Reasons for Public School Closures in Texas 1998-2015 **65**

A Additional Figures and Tables

A.1 Closed School and Displaced Students Characteristics

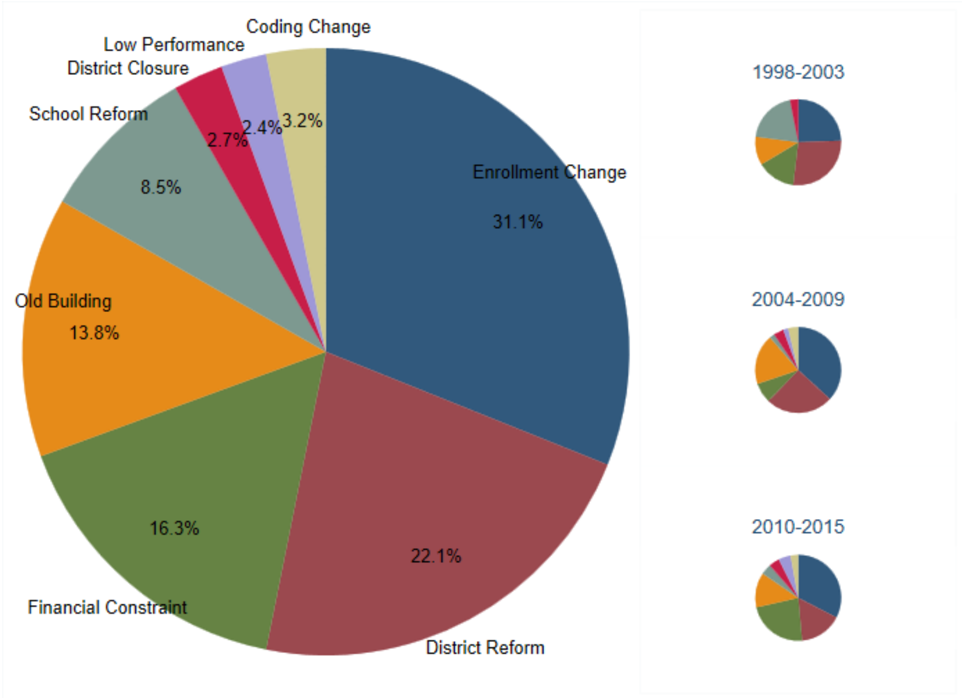
Fig. A.1. Annual Number of School Closures at Texas Public Schools in 1998–2015



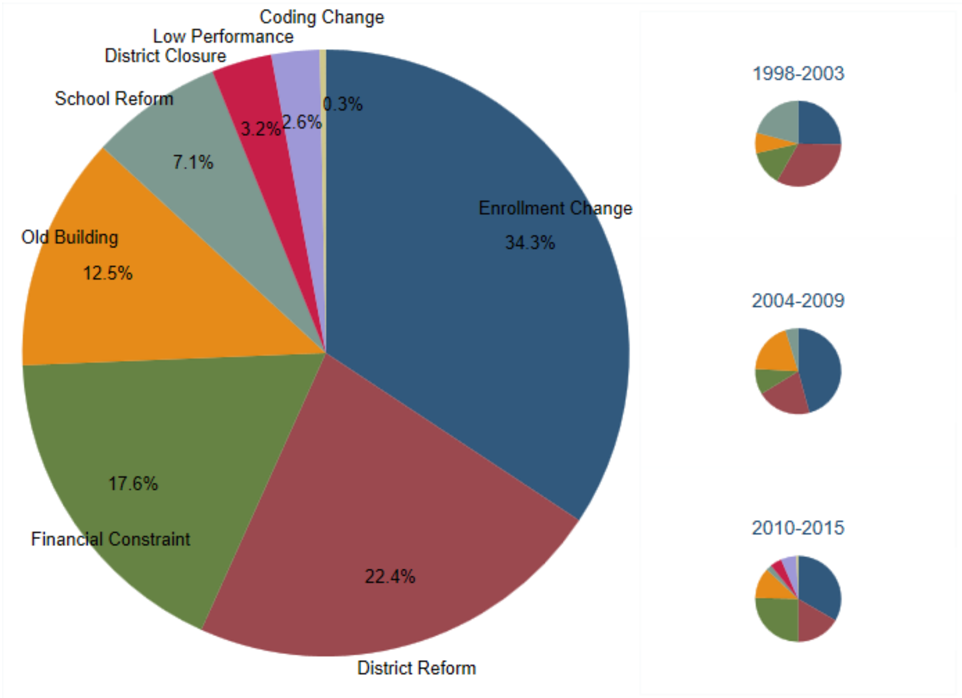
Notes: The figure presents the distribution of 323 (130) school closures that occurred between 1998 and 2015. To be considered a closed school, the school must be officially listed as closed by the Texas Education Agency (TEA), be a non-charter instructional campus in a regular, independent district, have been observed during the previous period (1994–1997), and is not replaced by a substantially overlapping school at the same address in the following year.

Fig. A.2. The Reasons for School Closures at Texas Public Schools in 1998-2015

(a) All School Closures



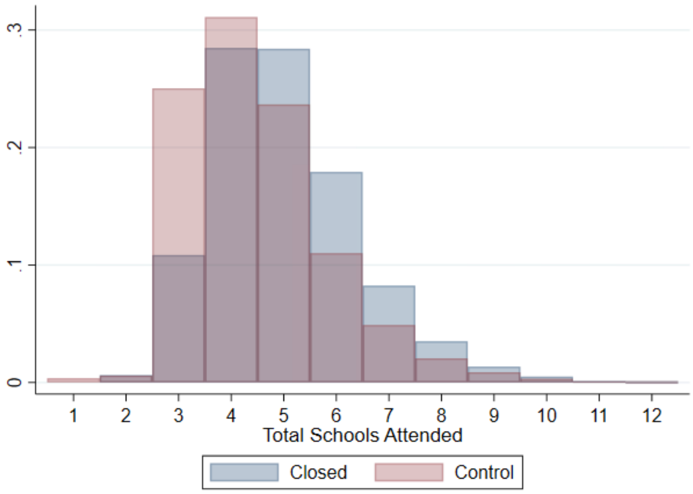
(b) School Closures Excluding Same Address Student Observations (Baseline Sample)



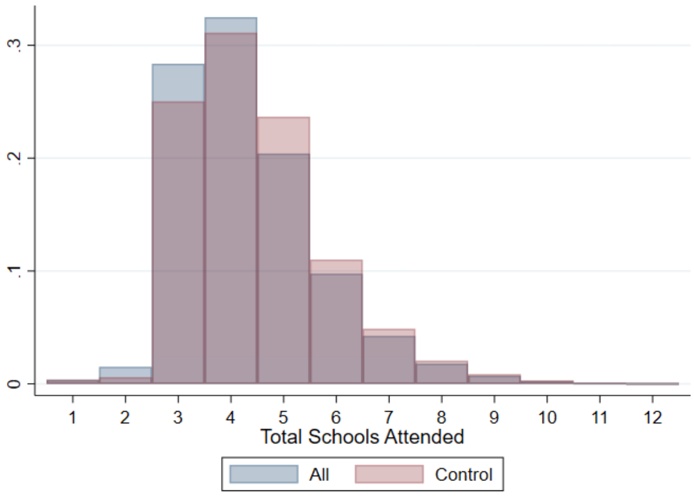
Notes: The figures display the categorized reasons for public school closures in Texas between 1998 and 2015: 274 out of 470 closures in Figure (a), and 204 out of 323 closures in Figure (b). Figure (a) includes all closures, while Figure (b) excludes closures where more than 30 percent of students were observed attending a school at the same address the following year. In both figures, to be classified as a closed school, the campus must be officially listed as closed by TEA, be a non-charter instructional campus in a regular, independent district, and have been observed during the previous period (1994–1997). Three smaller figures depict the reasons for closures across three distinct periods: for the baseline sample, 1998-2003 (62 closures out of 103), 2004-2009 (56 closures out of 110), and 2010-2015 (86 closures out of 110). 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.3. 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.

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

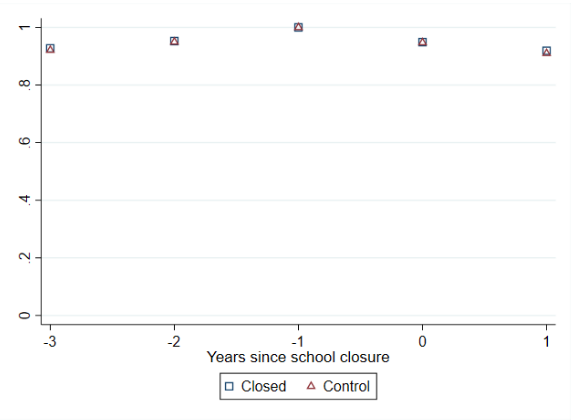
Panel A: School Closures in Short-Run Anaysis			
Matching Variables	(1) Closed Schools	(2) All Schools	(3) Control Schools
Locales			
City	0.55	0.37	0.55
Urban Fringe (Or Suburb)	0.15	0.22	0.15
Town	0.15	0.14	0.15
Rural	0.15	0.26	0.15
School Types			
Elementary	0.67	0.51	0.67
Middle	0.16	0.15	0.16
Junior High	0.09	0.05	0.09
High	0.03	0.21	0.03
Elementary/Secondary	0.05	0.08	0.05
Demographics			
Non-Hispanic Black	0.24	0.14	0.21
Hispanic	0.50	0.44	0.51
Free/reduced price lunch	0.63	0.49	0.63
Other types of disadvantages	0.12	0.07	0.10
Number of Schools	323	9,794	323
Panel B: School Closures in Long-Run Anaysis			
Matching Variables	(1) Closed Schools	(2) All Schools	(3) Control Schools
Locales			
City	0.48	0.39	0.48
Urban Fringe (Or Suburb)	0.18	0.25	0.18
Town	0.20	0.14	0.20
Rural	0.14	0.23	0.14
School Types			
Elementary	0.51	0.49	0.51
Middle	0.15	0.15	0.15
Junior High	0.15	0.05	0.15
High	0.08	0.23	0.08
Elementary/Secondary	0.11	0.09	0.11
Demographics			
Non-Hispanic Black	0.19	0.14	0.17
Hispanic	0.45	0.40	0.45
Free/reduced price lunch	0.58	0.48	0.57
Other types of disadvantages	0.06	0.05	0.05
Number of Schools	130	8,582	130

Notes: The table presents average characteristics for closed, all, and control schools for short-run sample in Panel A and long-run sample in Panel B. Following the short- and long-run sample definitions, all school-level averages are calculated over the years 1998–2015 for Panel A, and over 1998–2003 for all schools, 2004–2007 for middle and high schools, and 2008–2010 for high schools in Panel B. Locales are a simplified version. In more detail, locales follow eight categories in 1998-2005: 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. In 2006-2015, locales follow twelve categories: large city, mid-size city, small city, large suburb, mid-size suburb, small suburb, town short-distance to urban, town mid-distance to urban, town long-distance to urban, rural short-distance to urban, rural mid-distance to urban, and rural long-distance to urban.

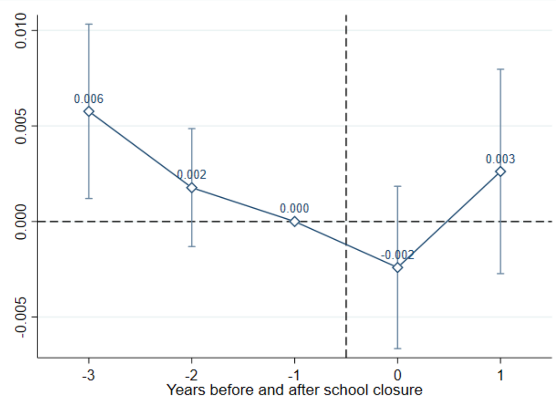
A.2 Sample Attrition

Fig. A.4. Analysis of Sample Attrition Rates of Closed and Control Schools

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

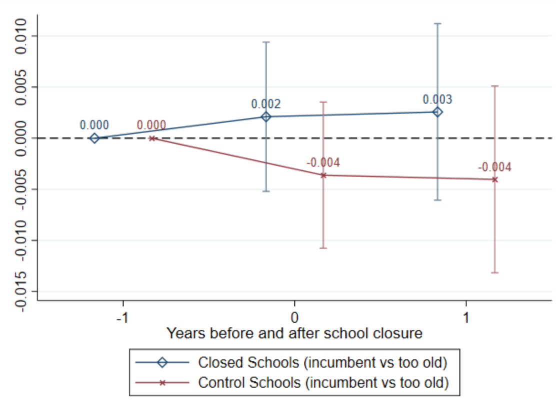


(b) Short-run: regression of in-sample dummy on closed-school dummy



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

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

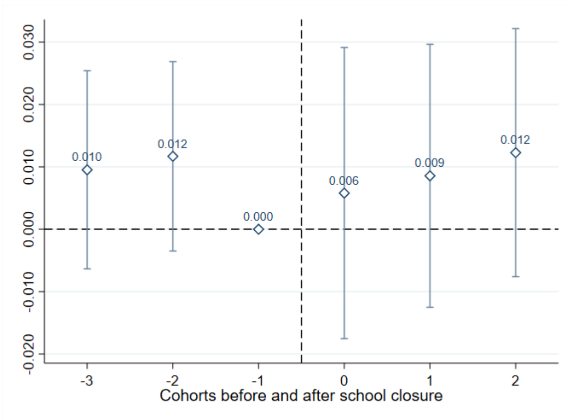


Notes: Sub-figures (a) and (b) consider all students in the short-run analysis sample enrolled in closed and matched control schools in the year preceding the closure (denoted by time -1 on the x-axis). Sub-figure (a) plots the proportion of observed students each year around school closure, separately 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 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).

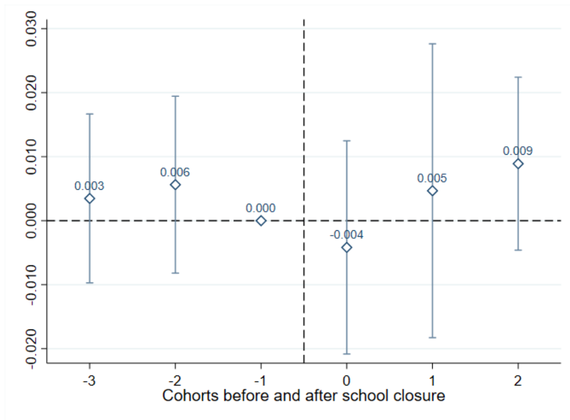
A.3 Long-Run Analysis Balance Test

Fig. A.5. Long-Run Analysis Balance Test: Difference in Student Composition, Test Scores, and Absence

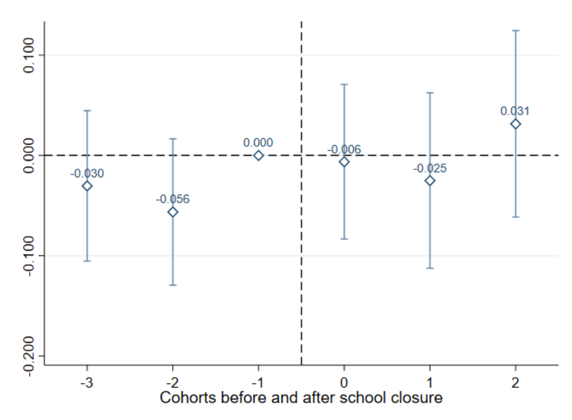
(a) Economic disadvantage status



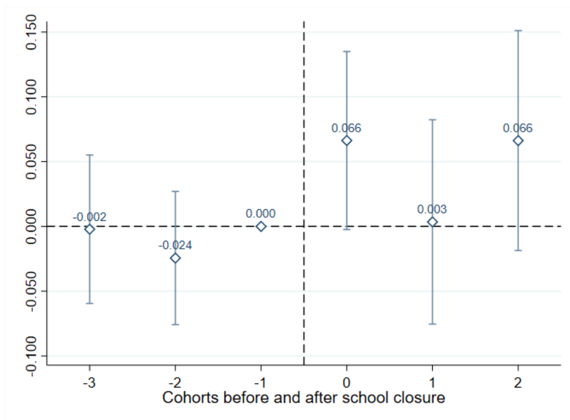
(b) Black or Hispanic students



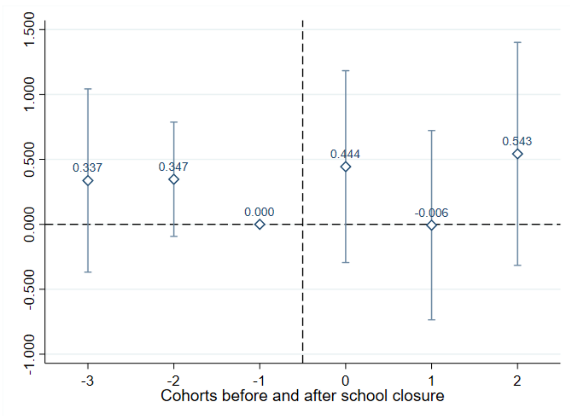
(c) Standardized math score



(d) Standardized reading score



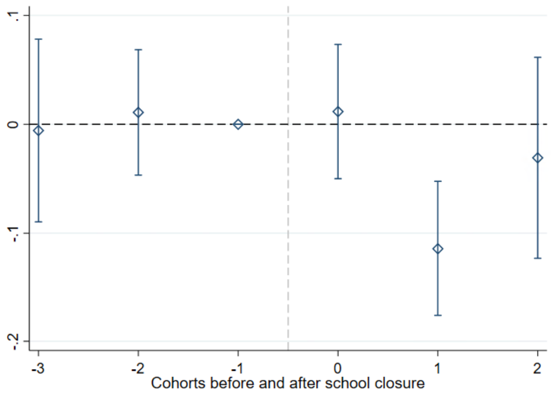
(e) Days of absence



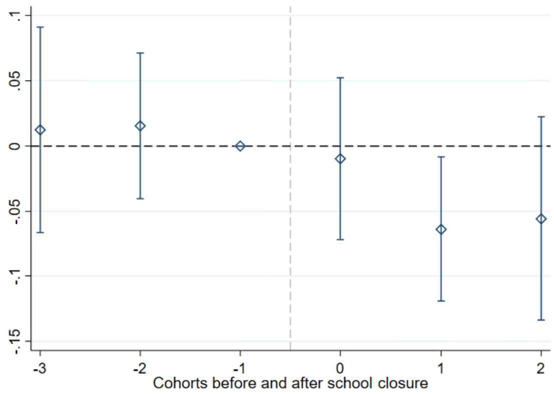
Notes: The figures present the coefficients, π_c, and 95% confidence intervals from equation (4), in which the dependent variables are student characteristics or short-run outcomes (test scores and days of absence). The short-run outcomes are measured before school closures, specifically at $t = -1$ 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.The analysis sample is balanced. Standard errors are clustered at the school level.

Fig. A.6. Long-Run Analysis Balance Test: Difference in Test Scores and Behavior Before and After School Closures

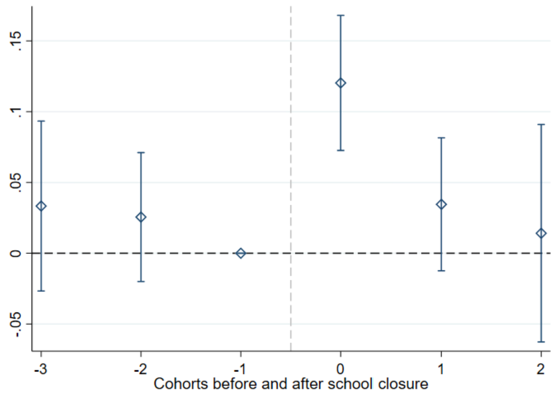
(a) Standardized math score



(b) Standardized reading score



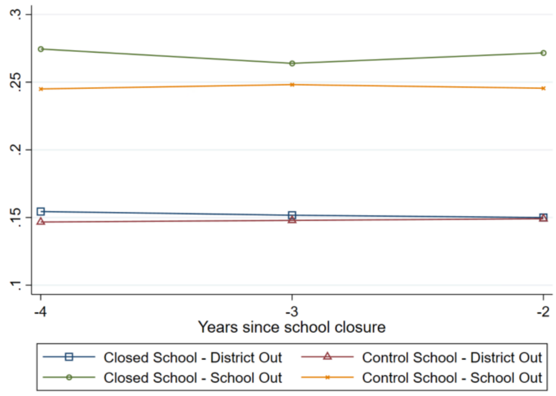
(c) Days of absence



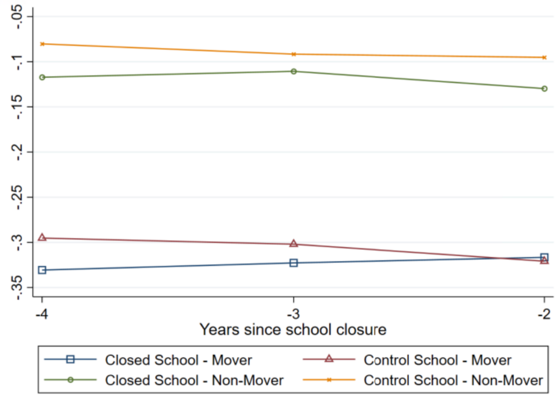
Notes: The figures present the differences in coefficients (π_t) and the corresponding 95% confidence intervals, based on equation (4), where the dependent variables are short-run outcomes—standardized test scores and days of absence. These differences capture the change in outcomes from before to after school closure in closed schools relative to control schools. 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. The displayed coefficients represent the differences in outcomes between these two time points. To be included in the analysis, individuals must be observed in both outcomes before and after 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. The analysis sample is balanced. Standard errors are clustered at the school level.

Fig. A.7. Student Move-Out Patterns in Closed and Control Schools Prior to Closure

(a) Proportion of move-out students



(b) Standardized test scores of move-out and non-move-out students



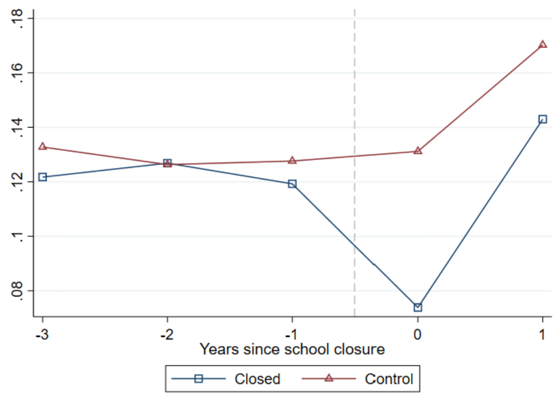
Notes: The figures illustrate student moving-out patterns in closed and matched control schools. Figure (a) shows the proportion of students who changed schools or districts in the three years leading up to a year before the school closure. Specifically, the indicator equals one if a student enrolled in a closed or control school is observed in a different school or district the following year. Figure (b) presents the average test scores of students who moved out versus those who did not, as defined in Figure (a), based on district changes.

A.4 Raw Trends in Short-Run Outcomes

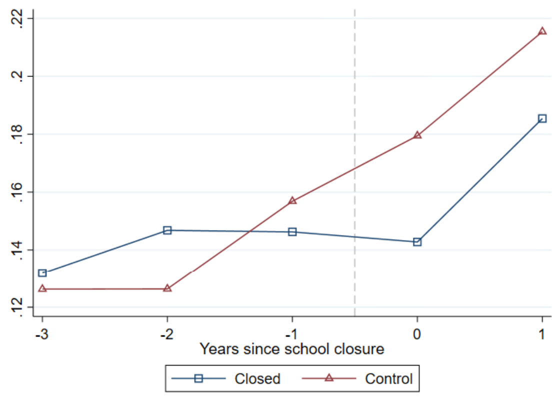
Appendix Figure A.8 illustrates the raw trends of short-run outcomes for closed and control schools around school closure. Sub-figures (a) and (b) show standardized math and reading scores. Prior to school closure, both closed and control schools exhibit comparable trends over the three-year period, 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.

Fig. A.8. Raw Trends in Short-Run Outcomes Between Closed and Control Schools

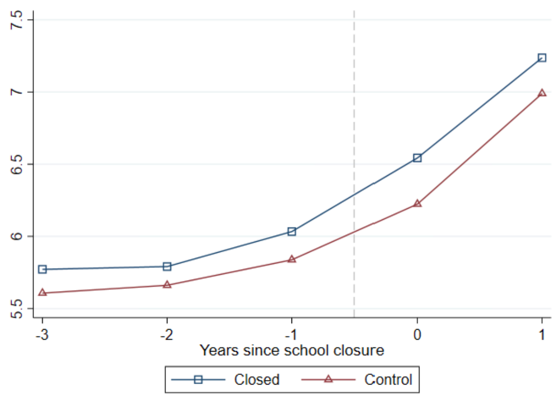
(a) Standardized math score



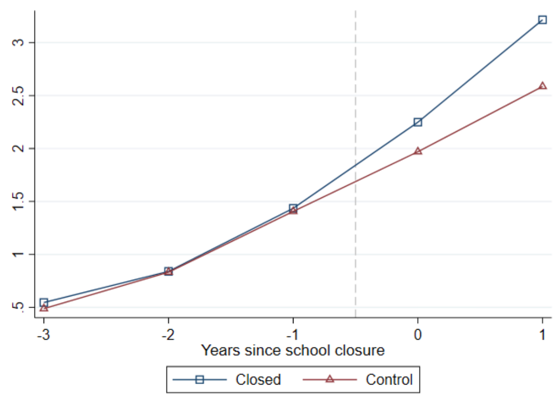
(b) Standardized reading score



(c) Days of absence



(d) Days of disciplinary action



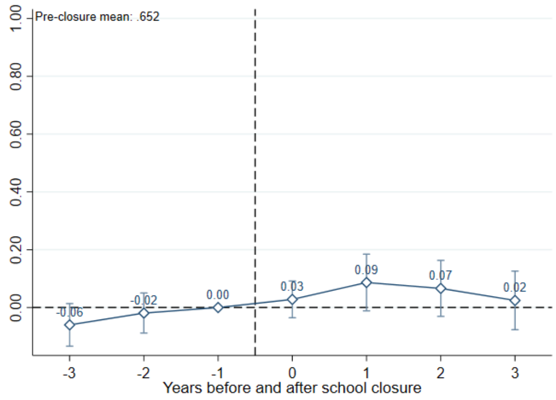
Notes: The figures plot raw trends over the period of three years before and two years after the school closure, separately for closed and matched control schools. I restrict the sample to students who are observed in the data over this period (i.e., the panel is balanced).

A.5 Short-Run Effects on Days of Disciplinary Actions: Different Margins

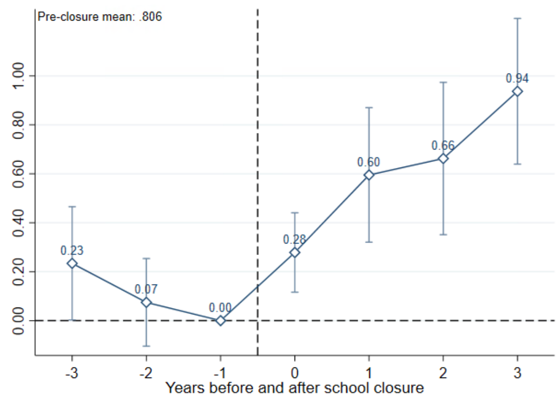
Given the significant increase in the number of days of disciplinary action following the school closure, I conduct a separate analysis for days of in-school suspensions, days of out-of-school suspensions (including expulsions), and intensive/extensive margins of disciplinary actions. These results are presented in Appendix Figure A.9. The increase in days of in-school suspensions is at most 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 0.9. 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 before closure. In addition, NCES (2018) reports that the suspension rate in Texas is comparable to the national average, which supports the generalizability of these findings to other states.

Fig. A.9. Short-Run Effects of School Closure on Days of Disciplinary Actions: Different Margins

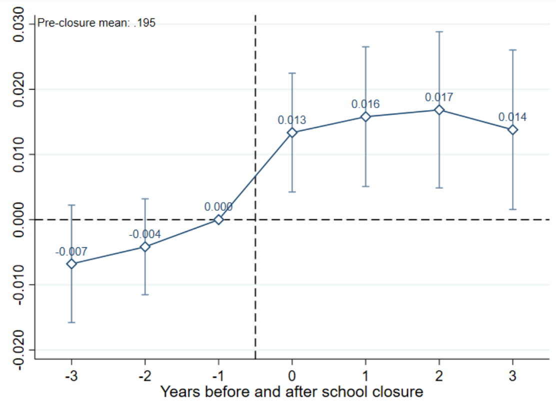
(a) In-school days of disciplinary action



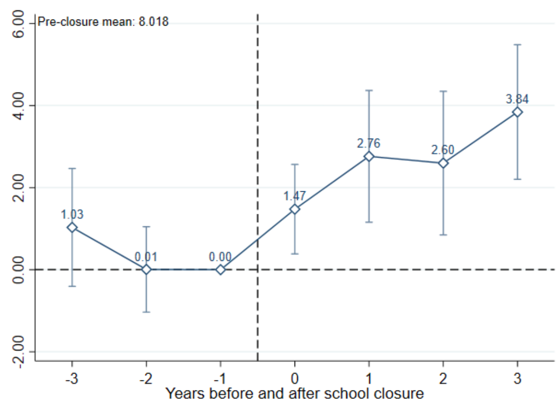
(b) Out-of-school days of disciplinary action



(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. The pre-closure mean refers to the average value of the outcome variable at time $t = -1$ for displaced students in the analysis sample. The analysis sample is balanced. Standard errors are clustered by school at $t = -1$.

A.6 Short-Run Effects of School Closure: Heterogeneity

Appendix Figure A.10 examines additional heterogeneity based on the proportion of displaced students who transfer to the same school after a closure. The median proportion of displaced students who move to the same school is 56%, with quartile cutoffs at 22%, 56%, and 76%. While most differences are not statistically significant, I find that disruptions tend to be larger for students from schools where a lower proportion of displaced students move together. In such cases, students are more likely to experience larger disruptions in math and reading scores, as well as increases in absenteeism. The number of disciplinary incidents increases the most among students from schools with a medium proportion of displaced students moving together.

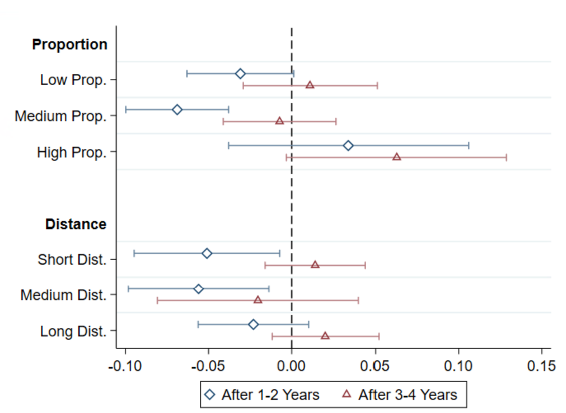
Moreover, I categorize closed schools based on their distance to receiving schools. Distance is measured as the median distance between closed and receiving schools. The median distance is 1.1 miles, with quartile cutoffs at 0.4, 1.1, and 2.0 miles. While most differences are not statistically significant, I find that test scores decline the most among students from medium-distance schools. Absenteeism increases more among students attending medium- and long-distance schools, and disciplinary incidents rise the most among those from long-distance schools.

Appendix Figure A.11 explores how the increase in days of disciplinary action varies by the racial composition of receiving schools. I categorize schools into three groups—low, medium, and high—based on the proportion of each racial group.[A.1] While most differences are not statistically significant, across all racial groups, I find larger increases in disciplinary actions when students move to schools with either low or high proportions of their own race. Notably, Black students who transfer to schools with a low proportion of Black students experience the largest increase, suggesting that adapting to new and different environments may be especially challenging for displaced students.

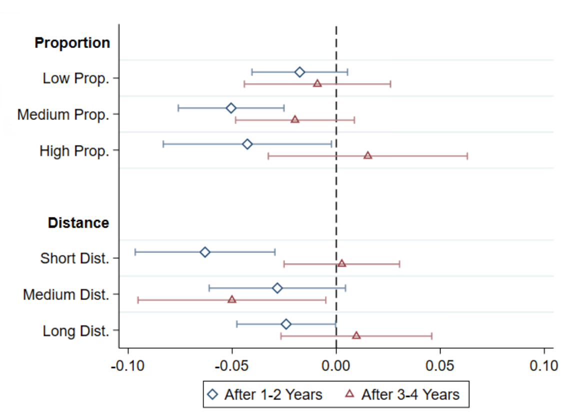
[A.1] For White students, the groups are defined as 0–17%, 17–50%, and 50–97%; for Hispanic students, as 0–27%, 27–61%, and 61–100%; and for Black students, as 0–8%, 8–25%, and 26–94%.

Fig. A.10. Short-Run Effects of School Closure on Student Outcomes: Additional Heterogeneity by School Characteristics

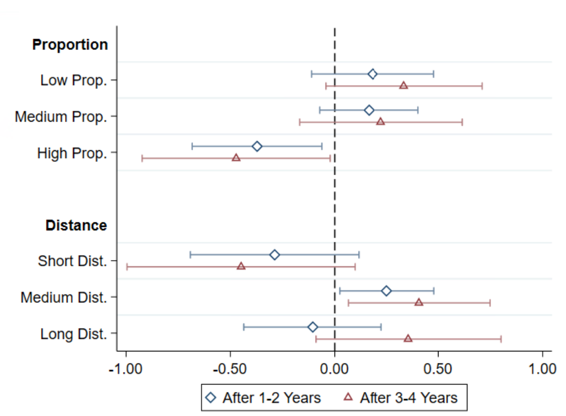
(a) Standardized math score



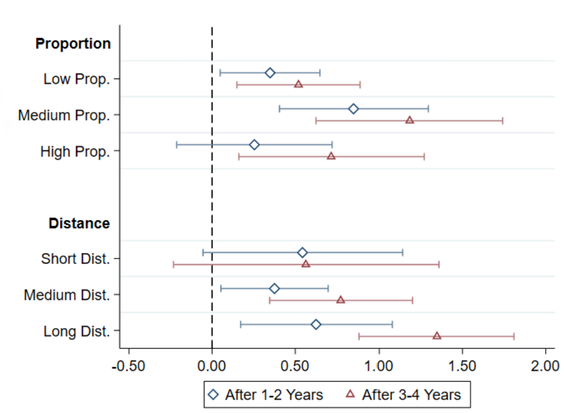
(b) Standardized reading score



(c) Days of absence

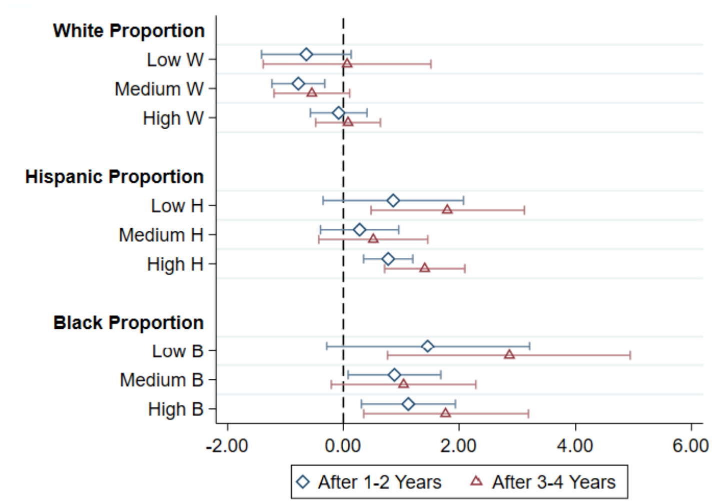


(d) Days of disciplinary actions



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 proportion is defined as the share of displaced students who enrolled in the same school immediately after the closure, relative to all displaced students. The distance is measured as the median distance between the closed schools and the schools where these displaced students enrolled. The regression includes individual and match group-by-year fixed effects. The analysis sample is balanced, except for the third and fourth years after the school closure, which may be missing for students in higher grades. Standard errors are clustered by school at $t = -1$.

Fig. A.11. Short-Run Effects of School Closure on Disciplinary Actions: Additional Heterogeneity by Racial Composition



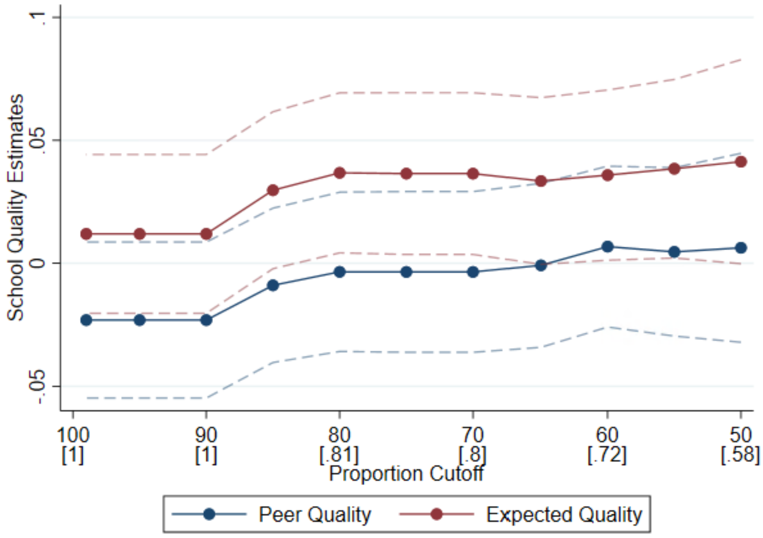
Notes: The figures present the coefficients, β, and 95% confidence intervals from equation (1) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote years after a school closure. The proportion is defined based on the share of each racial group in the receiving school. The regression includes individual and match group-by-year fixed effects. The analysis sample is balanced, except for the third and fourth years after the school closure, which may be missing for students in higher grades. Standard errors are clustered by school at $t = -1$.

A.7 School Quality Changes

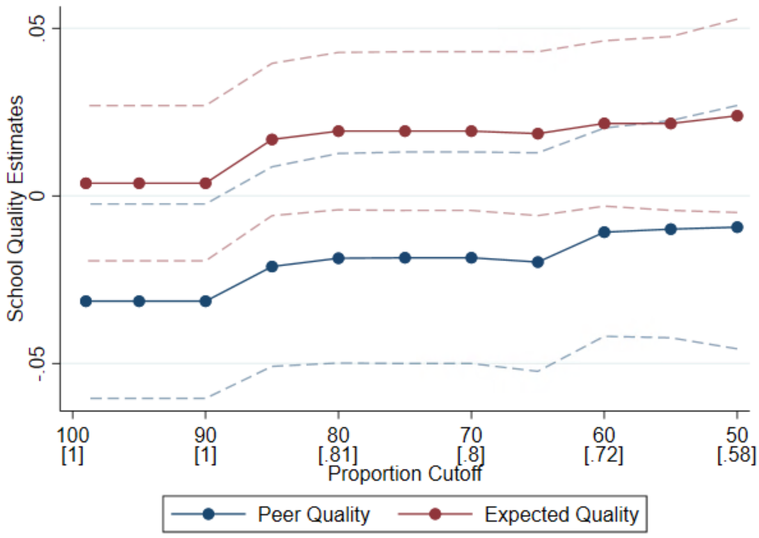
To further understand why students do not have high-performing peers even after transitioning to originally better-performing schools, I examine average test score changes of receiving schools before and after school closure (i.e., $t = 0$ and $t = -1$), dividing students into original students and move-in 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 -0.073 to -0.081 standard deviations in test scores, while original students show a decline of -0.021 to -0.038 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. However, 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.

Fig. A.12. Peer and Expected School Quality Changes Before and After School Closures: Robust to Cutoffs

(a) Standardized math score



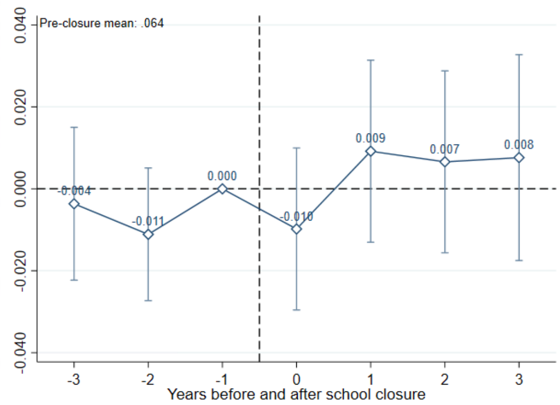
(b) Standardized reading score



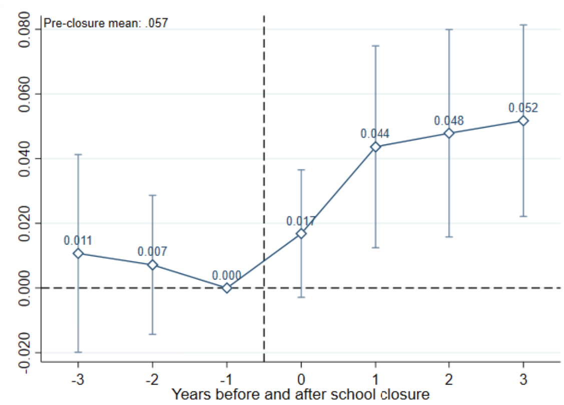
Notes: The figures present the coefficients, β, and 95% confidence intervals from equation (1), where the outcome variables are the school-level average math and reading scores. The X-axis specifies different cutoffs for sample inclusion. For peer quality, the outcome variables are the yearly school average of the outcomes, excluding displaced students from the calculation after the school closure (i.e., $t \geq 0$). For expected quality, the outcome variables are the school average over the four years preceding the school closure (i.e., $t \in \{-4, \ldots, -1\}$). The X-axis represents the sample cutoff, where students are excluded if displaced students account for more than the specified proportion of the receiving school's population. The numbers in brackets reflect the percentage of the sample included under each cutoff. For example, 80% of the sample is included under the baseline cutoff of 70%. 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 $t = -1$.

Fig. A.13. Peer and Expected School Quality Changes Before and After School Closures

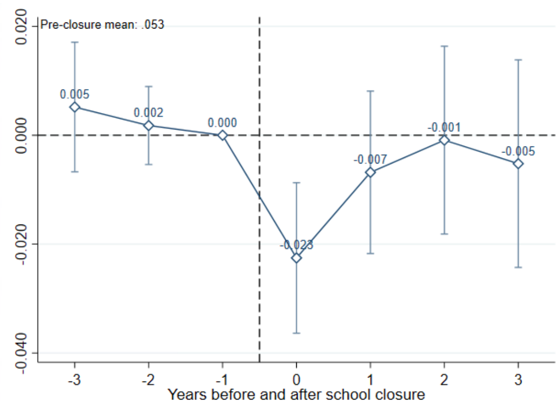
(a) Peer quality: days of absence



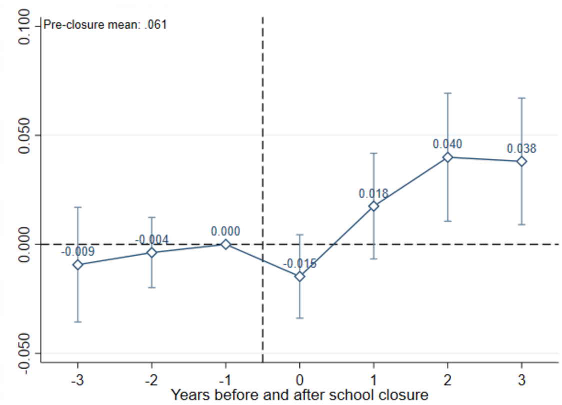
(b) Peer quality: days of disciplinary action



(c) Expected quality: days of absence



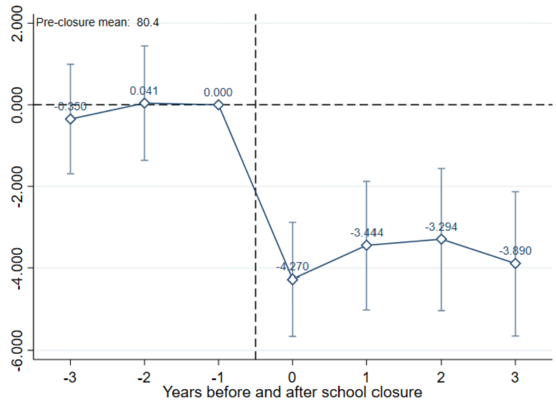
(d) Expected quality: days of disciplinary action



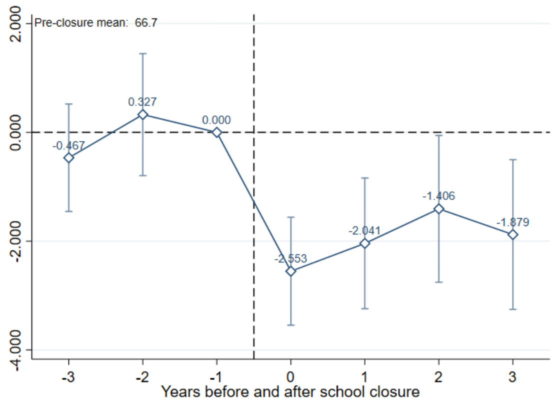
Notes: The figures present the coefficients, ρ_t, and 95% confidence intervals from equation (2), where the outcome variables are the school average days of absence and days of disciplinary action, which are standardized by year-by-grade level. When it comes to sub-figures (a) and (b), the outcome variables are yearly school average test scores and the construction of average values excludes displaced students from the calculations after school closure (i.e., $t >= 0$). For sub-figures (c) and (d), the outcome variables are the school average over the four years preceding the school closure (i.e., $t \in \{-4, \ldots, -1\}$). For all sub-figures, I exclude receiving schools if more than 70% of their students are displaced 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. The pre-closure mean refers to the average value of the outcome variable at time $t = -1$ for displaced students in the analysis sample. The analysis sample is balanced. Standard errors are clustered by school at $t = -1$.

Fig. A.14. Effects of School Closures on School-level Employment

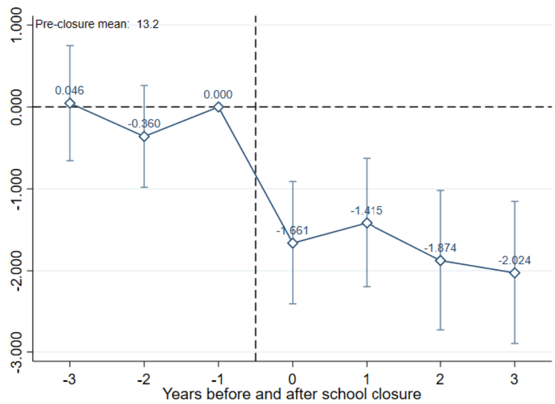
(a) Total school staff



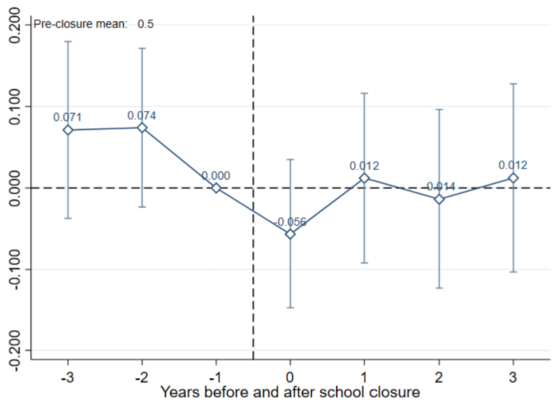
(b) Teachers



(c) Teaching support



(d) Social support



Notes: 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. The pre-closure mean refers to the average value of the outcome variable at time $t = -1$ for displaced students in the analysis sample. The analysis sample is balanced. Standard errors are clustered by school at $t = -1$.

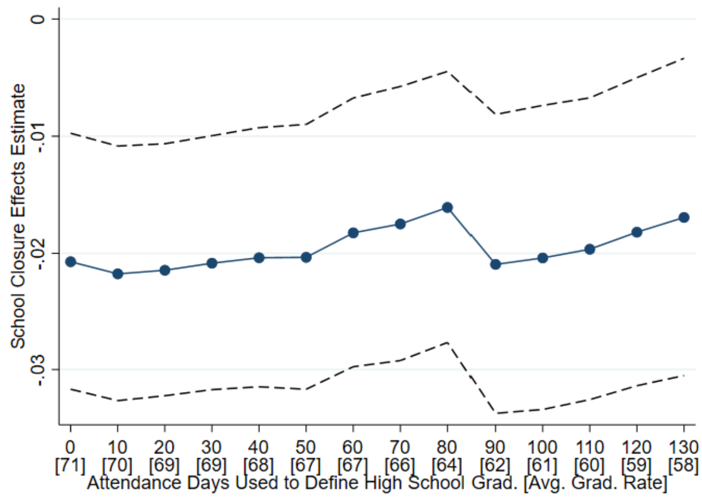
Table A.2: Receiving School Quality Change: Original and Move-In Students

	(1) $t = -1$	(2) $t = 0$	(3) Difference
Original Students			
Standardized Math Score	-0.012	-0.040	-0.028***
Standardized Reading Score	-0.000	0.021	-0.021***
Move-In Students			
Standardized Math Score	-0.218	-0.299	-0.081***
Standardized Reading Score	-0.217	-0.290	-0.073***

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

A.8 High School Graduation Proxy Across Different Definition Cutoffs

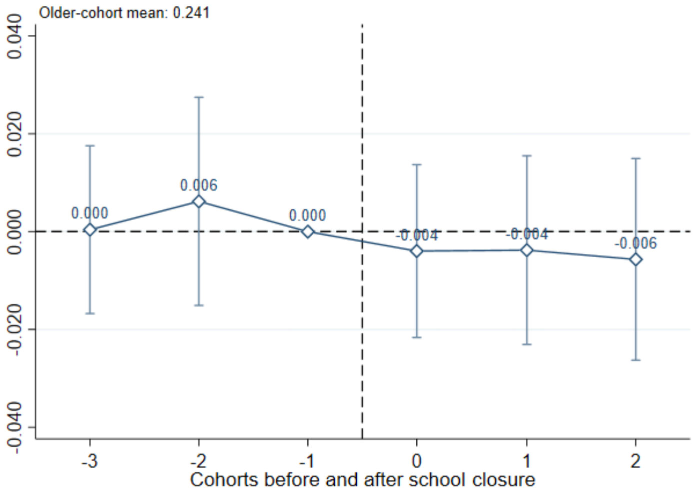
Fig. A.15. Estimates of School Closure Effects on High School Graduation Proxy Across Different Definition Cutoffs



Notes: The figure presents the coefficients, γ, and 95% confidence intervals from equation (3) with high school graduation proxy as the dependent variable, using different definition cutoffs for attending days in 12th grade. The X-axis shows different days of attendance cutoffs, ranging from 0 to 130 days, with the proportion of students attending at least each cutoff shown in brackets. 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 level.

A.9 Long-Run Effects of School Closure on Four-Year College Enrollment

Fig. A.16. Estimates of School Closure Effects on Four-Year College Enrollment



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. The older-cohort mean refers to the average value of the outcome variable for students in older cohorts ($c \in \{-3, -2, -1\}$) attending closed schools in the analysis sample. The analysis sample is balanced. Standard errors are clustered at the school level.

A.10 Long-Run Effects of School Closure: Separate Estimates for Highest Grade and Others

Table A.3: Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Separate Estimates for Highest Grade and Others

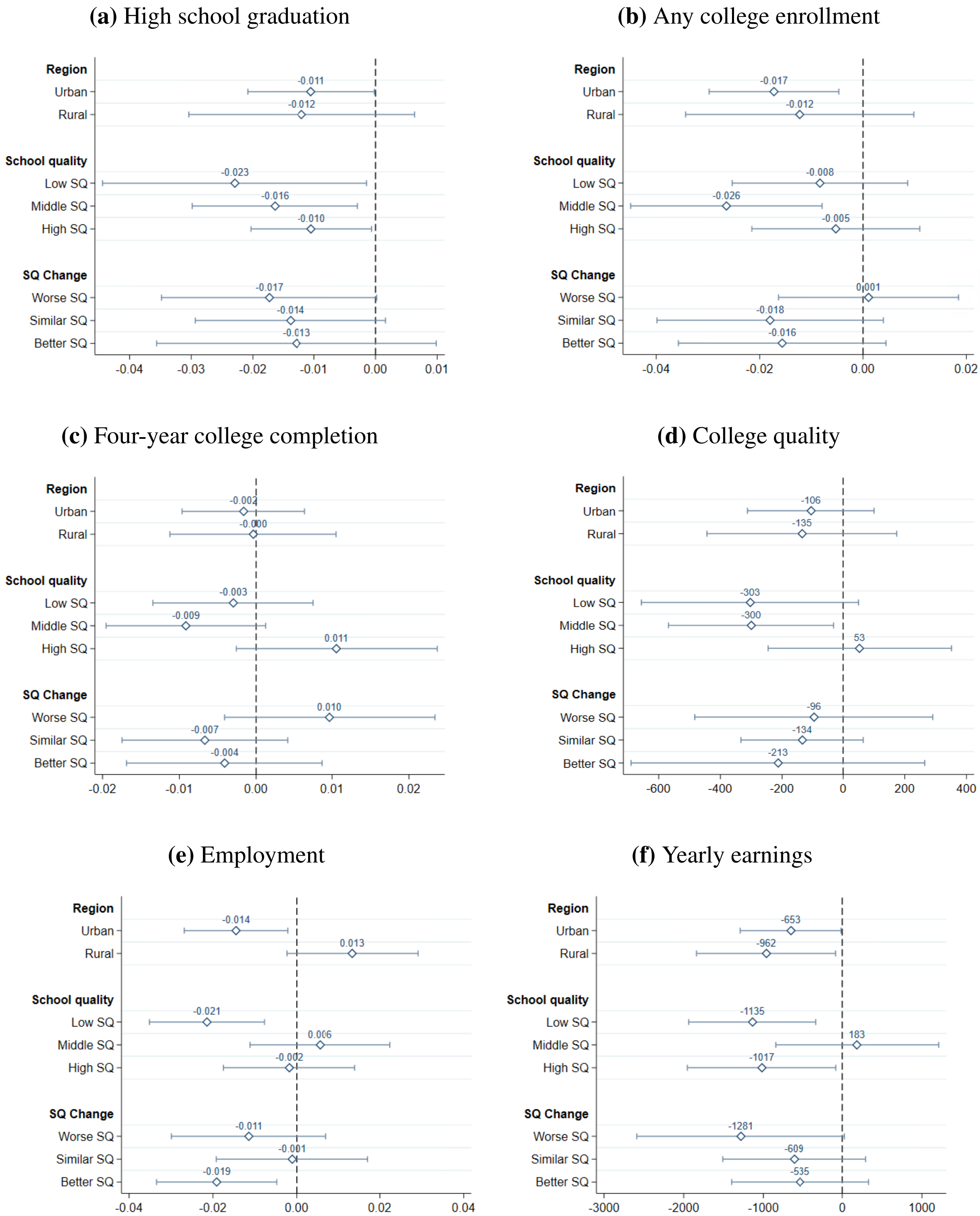
Panel A: Educational Outcomes				
	(1) Graduate HS	(2) Enroll Any College	(3) BA Degree	(4) College Quality
Closed School	-0.021***	-0.024***	-0.003	-305**
× Younger Cohorts ($c = 1, 2$)	(0.005)	(0.006)	(0.004)	(118)
Closed School	-0.015**	0.003	0.002	-66
× Younger Cohorts ($c = 0$)	(0.006)	(0.005)	(0.004)	(98)
Observations	163,336	164,497	164,497	163,336
School FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Mean of the Older Cohort	0.666	0.495	0.141	21,136

Panel B: Labor Market Outcomes		
	(1) Employment	(2) Yearly Earnings
Closed School	-0.014**	-1,110***
× Younger Cohorts ($c \in \{1, 2\}$)	(0.006)	(284)
Closed School	-0.005	-238
× Younger Cohorts ($c = 0$)	(0.005)	(314)
Observations	164,497	164,497
School FE	X	X
Matched group × Year FE	X	X
Mean of the Older Cohort	0.524	19,739

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, distinguishing between those in the highest grade ($c = 0$) and those in lower grades ($c \in \{1, 2\}$). 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. The mean of the older cohort refers to the average value of the outcome variable for students in older cohorts ($c \in \{-3, -2, -1\}$) attending closed schools in the analysis sample. Note that the dependent variables for high school graduation and college quality have fewer observations due to the exclusion of two closed schools from the analysis because of a potential data issue (see Section 3 for more details). Standard errors are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.10

A.11 Long-Run Effects of School Closure: Heterogeneity

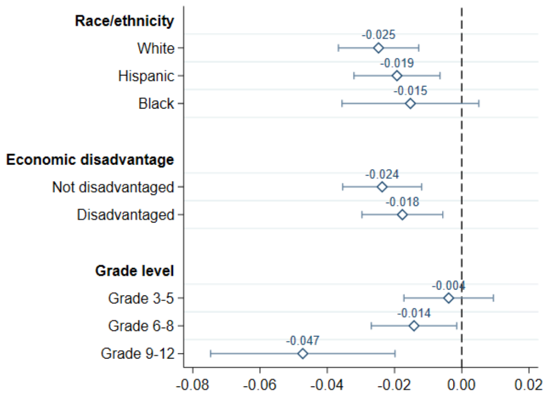
Fig. A.17. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Heterogeneity by School Characteristics



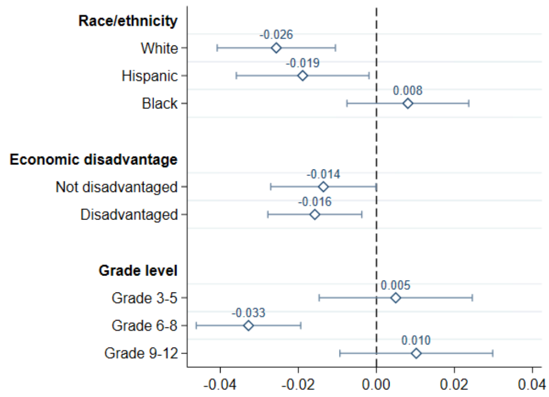
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school 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 level.

Fig. A.18. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Heterogeneity by Student Characteristics

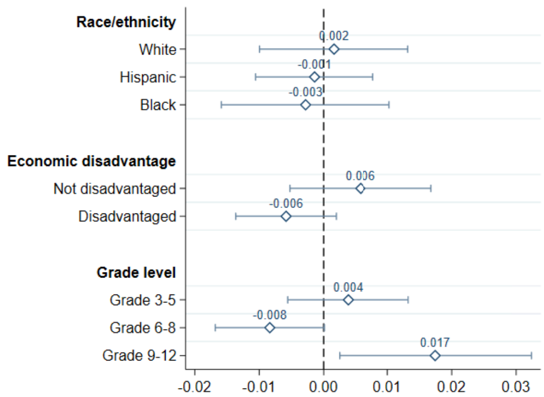
(a) High school graduation



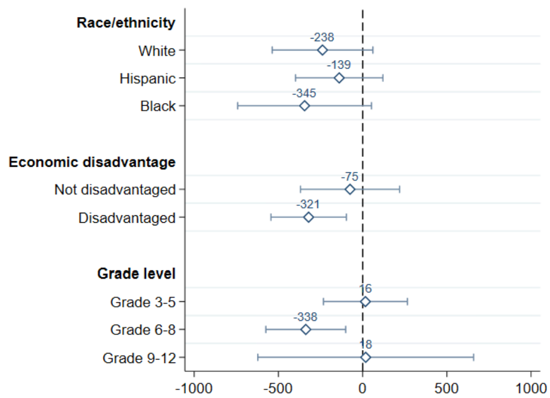
(b) Any college enrollment



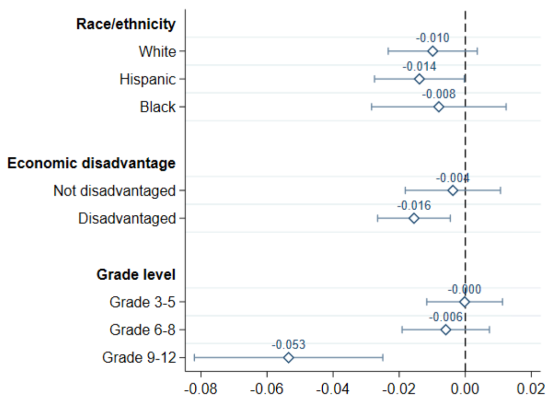
(c) Four-year college completion



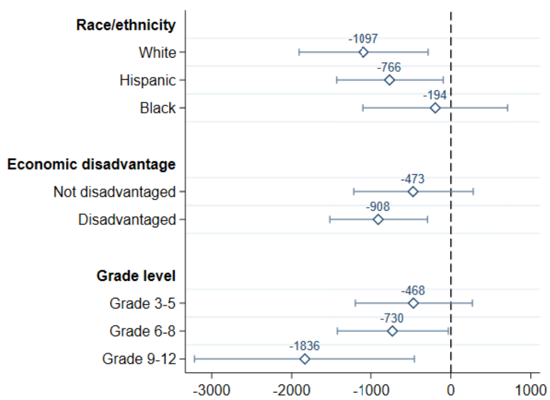
(d) College quality



(e) Employment



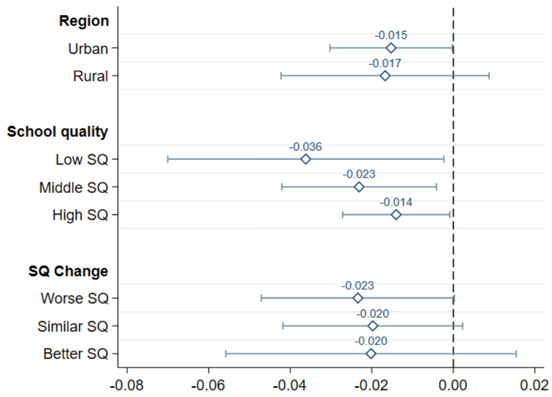
(f) Yearly earnings



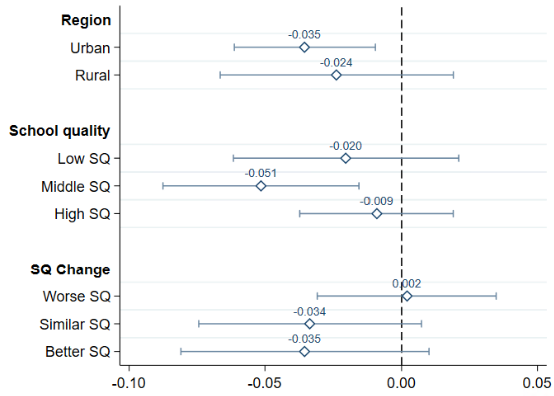
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school 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 level.

Fig. A.19. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: *Rescaled* Heterogeneity by School Characteristics

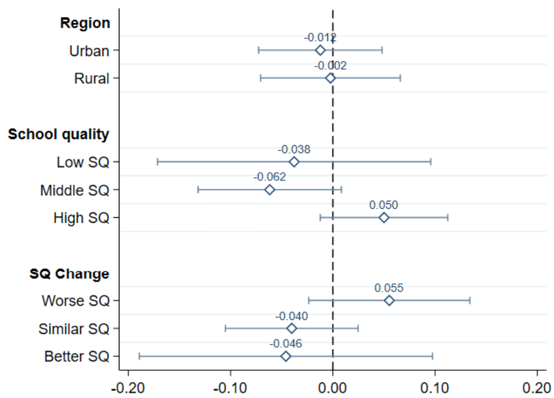
(a) High school graduation



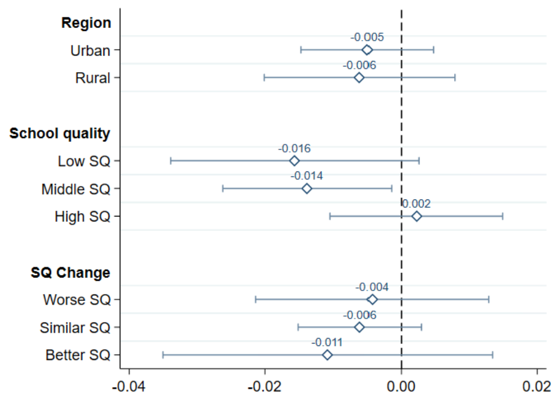
(b) Any college enrollment



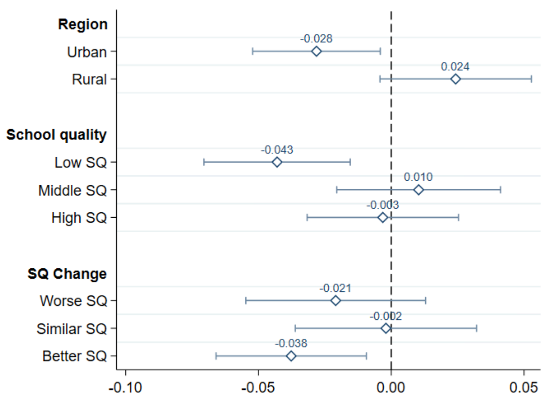
(c) Four-year college completion



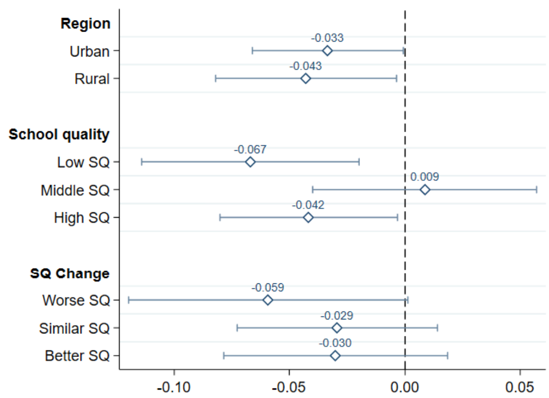
(d) College quality



(e) Employment



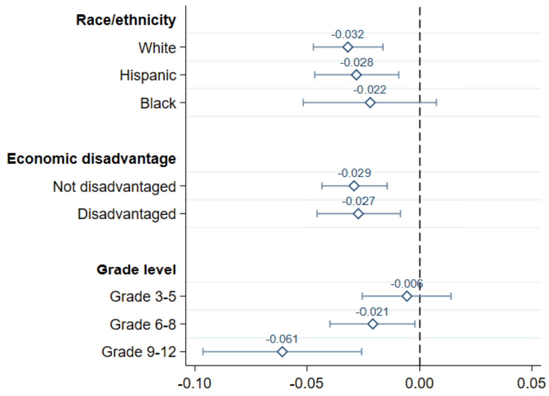
(f) Yearly earnings



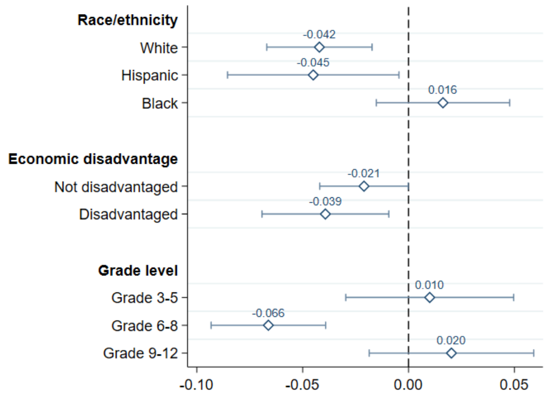
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis after estimates are scaled relative to the outcome mean for each sub-group. These coefficients represent the interactions between the indicator 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 level.

Fig. A.20. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: *Rescaled* Heterogeneity by Student Characteristics

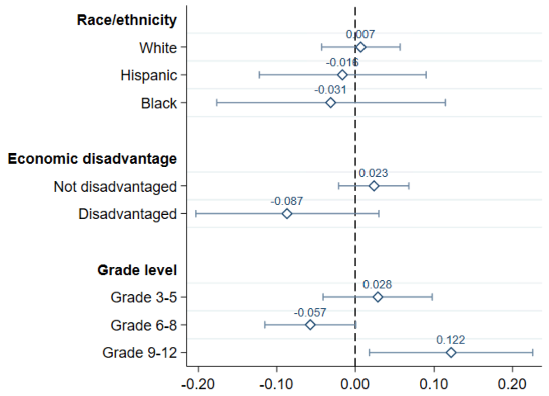
(a) High school graduation



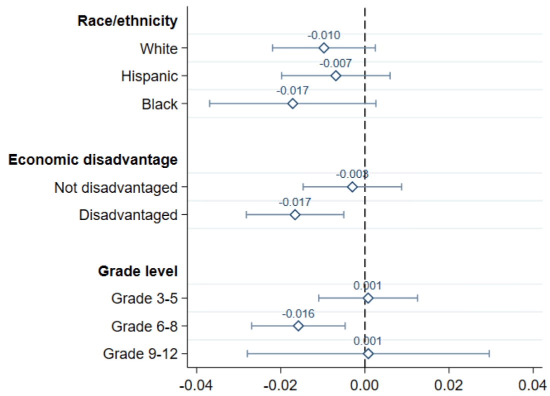
(b) Any college enrollment



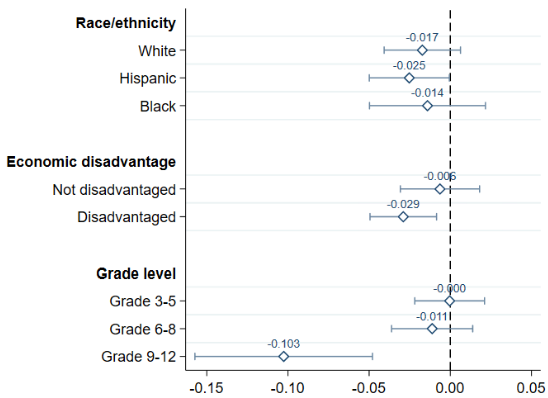
(c) Four-year college completion



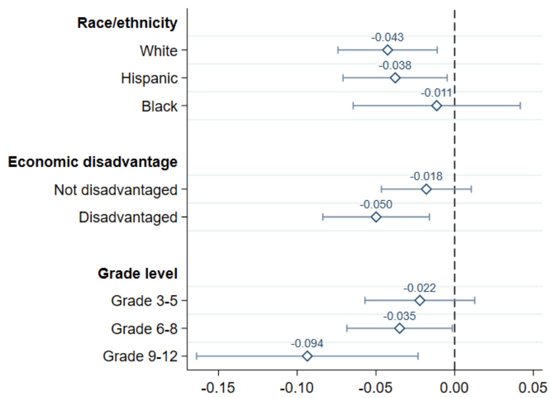
(d) College quality



(e) Employment



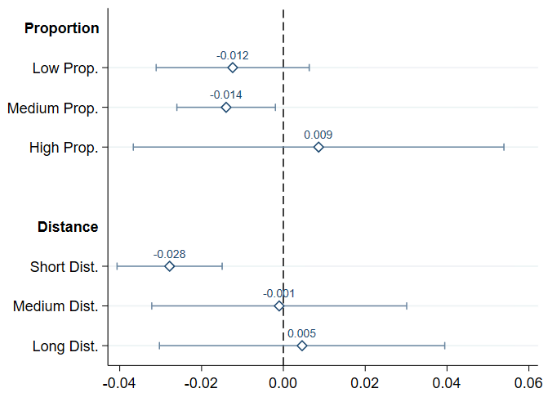
(f) Yearly earnings



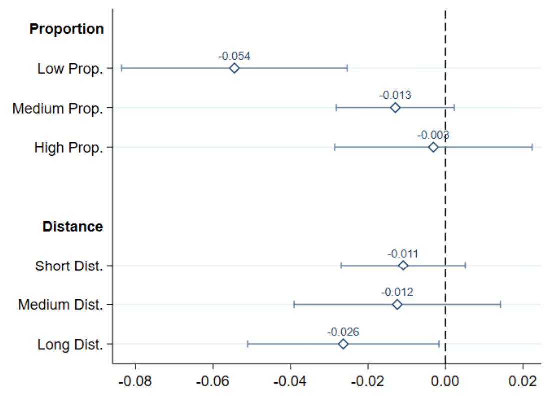
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis after estimates are scaled relative to the outcome mean for each sub-group. These coefficients represent the interactions between the indicator 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 level.

Fig. A.21. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Additional Heterogeneity by School Characteristics

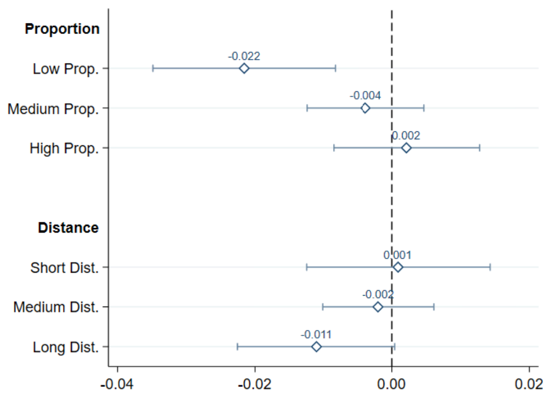
(a) High school graduation



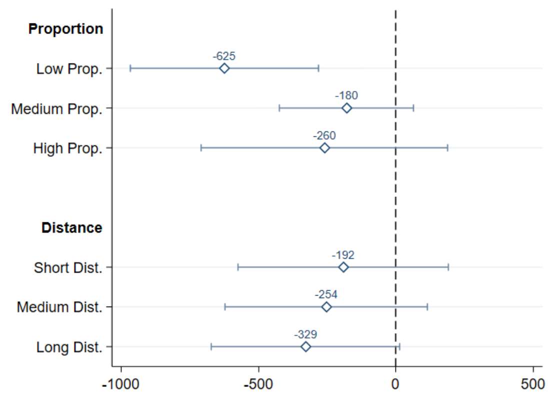
(b) Any college enrollment



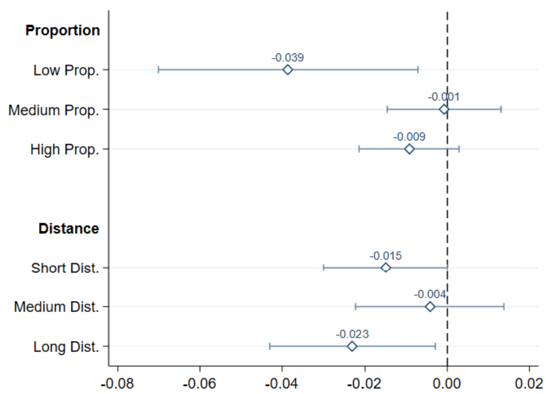
(c) Four-year college completion



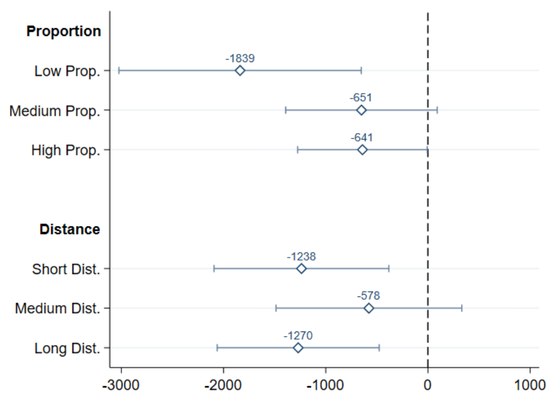
(d) College quality



(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 proportion is defined as the share of displaced students who enrolled in the same school immediately after the closure, relative to all displaced students. The distance is measured as the median distance between the closed schools and the schools where these displaced students enrolled. 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 level.

Table A.4: Short- and Long-Run Heterogeneity Comparison

	Short-Run Analysis	Long-Run Analysis
School Characteristics		
Region	Urban areas: Impacts on test scores and behavior are similar to those in rural areas	Urban areas: Impacts on educational and labor market outcomes are generally similar to those in rural areas
School Quality (SQ)	Low-quality schools: Larger negative impacts on behavior, smaller impacts on test scores compared to high-quality schools	Low-quality schools: Larger negative impacts on educational and labor market outcomes compared to high-quality schools
SQ Change	Moving to lower-quality schools: Larger negative impacts on test scores and smaller impacts on behavior compared to students who moved to better-quality schools	Moving to lower-quality schools: Smaller negative impacts on education and larger negative impacts on labor market outcomes compared to students who moved to better-quality schools
Student Characteristics		
Race	Black students: Positive impacts on math scores and absence rates; larger negative impacts on disciplinary actions Hispanic students: No effects on test scores; larger negative impacts on behavior White students: Mixed effects on test scores; positive effects on absence rates	Black students: Overall smaller impacts except for college quality Hispanic students: Larger negative impacts on both educational and labor market outcomes White students: Larger negative impacts on both educational and labor market outcomes
Economic Status	Disadvantaged students: Larger negative impacts on math scores and behavior compared to non-disadvantaged students	Disadvantaged students: Larger negative impacts on educational and labor market outcomes compared to non-disadvantaged students
Grade Level	Secondary school students: Larger negative impacts on test scores compared to elementary students	Secondary school students: Larger negative impacts on educational and labor market outcomes compared to elementary students

B Sensitivity Aanlysis

B.1 Matching

I examine the sensitivity of my estimates to alternative ways of choosing matched control schools to closed schools. Appendix Figures B.1 and B.2 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, 69 percent of matched control schools are changed after adding more variables (share or ESL, share of special education). Reassuringly, the results are generally robust across these alternative matching strategies, with most estimates falling within the 95% confidence intervals of the baseline estimates while control schools change 65 percent on average from the baseline control schools.[B.1]

I further assess the robustness of the matching strategy using the synthetic difference-in-differences method proposed by Arkhangelsky et al. (2021). In the short-run analysis, I restrict the donor pool to students in the same year, school type, and school locale. Each student from a closed school is matched to multiple control students using weights that minimize violations of the parallel trends assumption. I estimate the model separately within each donor pool and compute a weighted average based on the number of displaced students in each group. Standard errors are calculated using bootstrap resampling. As shown in Appendix Figure B.3, the results are consistent across outcomes: test scores decline and behavioral problems increase after school closure.[B.2] For the long-run analysis, I use school-by-cohort as the unit of analysis, treating each closed school and its six cohorts as a panel. The donor pool is restricted to schools from the same year, school type, and school locale. Each closed school is matched to multiple control schools using weights that minimize violations of parallel trends. I estimate the model separately for each closed school and compute a weighted average based on the number of displaced students in each case. Standard errors are calculated using bootstrap resampling. As presented in Appendix Figure B.4, the results are consistent while the impact is less pronounced: long-run outcomes decline among younger cohorts exposed to school closures. In other words,

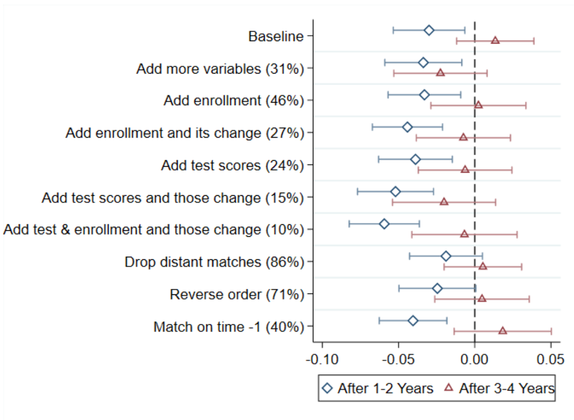
[B.1] The estimates fluctuate more in the specifications that include test scores (4, 5). For example, the estimated effects on high school graduation and college quality are close to zero in the specifications. This may be because test scores are already quite similar without being explicitly included as matching variables, as shown in the raw trends in Figure A.8. Placing greater weight on test scores in the matching process may reduce overall matching quality. For example, in the baseline matching for the long-run sample, the difference in racial minority composition between closed and control schools is 1 percentage point, whereas in specification (5), it increases to 6 percentage points.

[B.2] Notably, the synthetic difference-in-differences estimates are larger and more persistent—especially for test scores—than the baseline results. I interpret this as reflecting mean differences between treated and synthetic control students. Because the method prioritizes minimizing differences in pre-treatment trends, it can lead to larger differences in outcome levels—approximately 0.15 standard deviations in this case. For this reason, I view the baseline estimates, which rely on schools with more similar observable characteristics and performance levels, as the preferred estimates. Nonetheless, the synthetic method provides a valuable complementary benchmark.

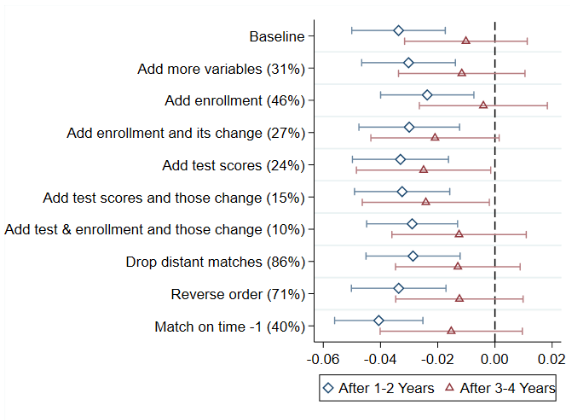
the estimated coefficients—obtained without additional discretion in matching criteria—align with the baseline results.

Fig. B.1. Short-Run Effects of School Closure on Student Outcomes: Alternative Matching Strategies

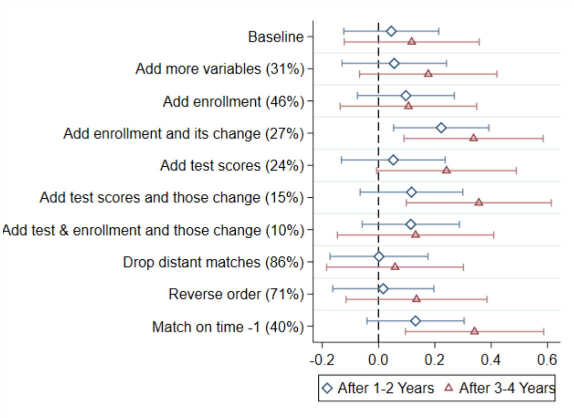
(a) Standardized math score



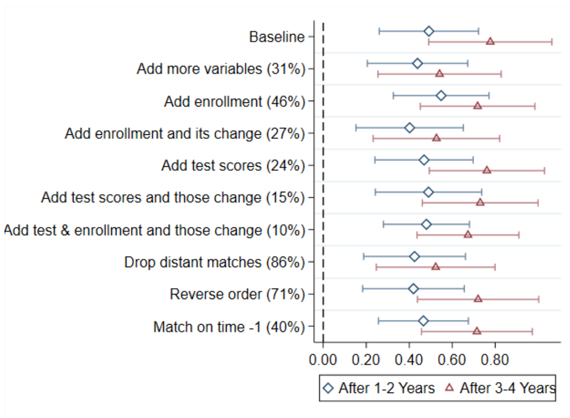
(b) Standardized reading score



(c) Days of absence



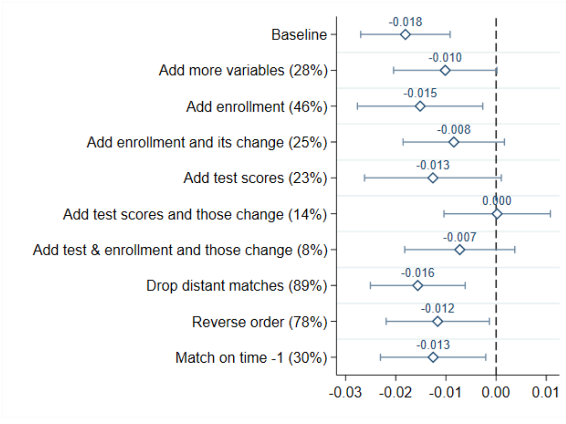
(d) Days of disciplinary action



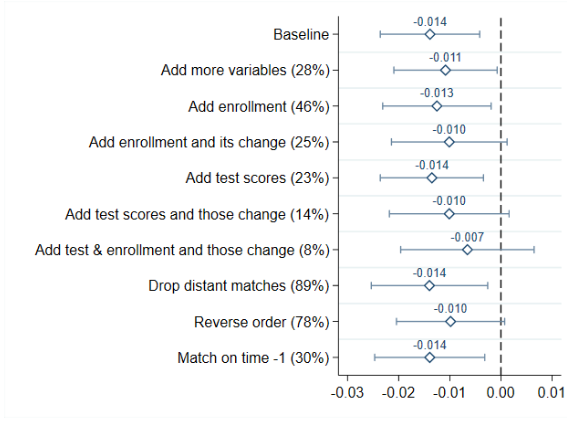
Notes: The figures present the coefficients, β, and 95% confidence intervals from equation (1) using control schools selected from the alternative matching strategies denoted on the y-axis. The baseline estimates are presented at the top of each sub-figure. The percentage in the parenthesis on the y-axis 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. The analysis sample is balanced, except for the third and fourth years after the school closure, which may be missing for students in higher grades. Standard errors are clustered by school at $t = -1$.

Fig. B.2. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Alternative Matching Strategies

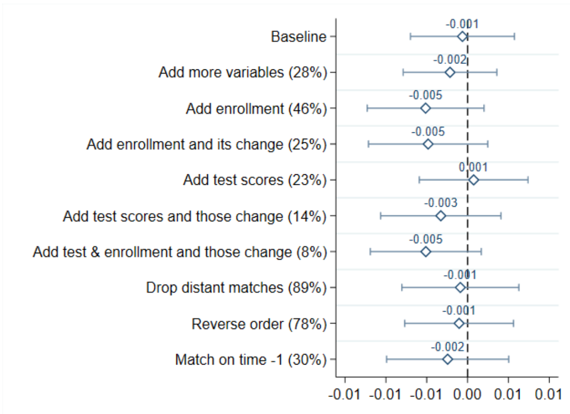
(a) High school graduation



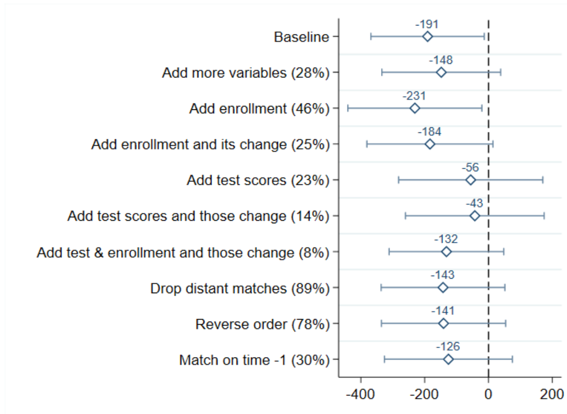
(b) Any college enrollment



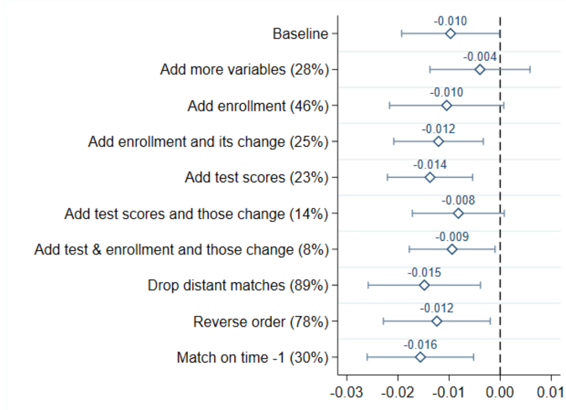
(c) Four-year college completion



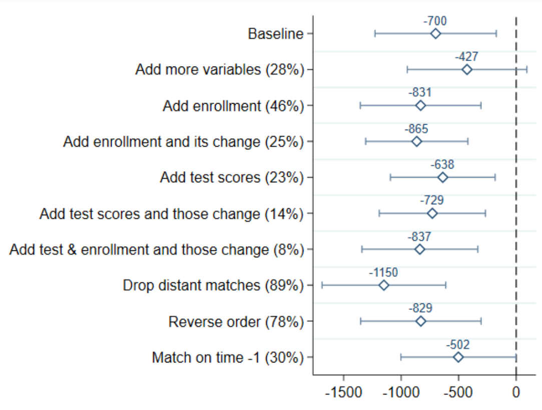
(d) College quality



(e) Employment at ages 25-27



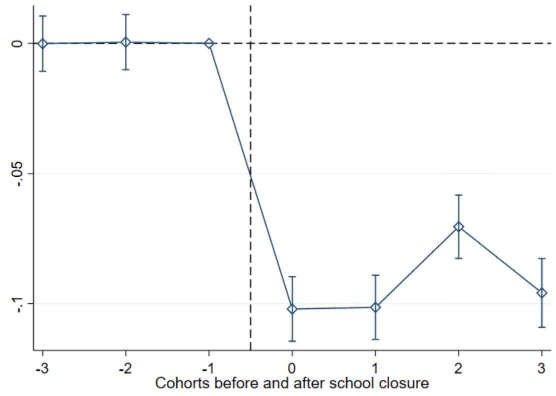
(f) Yearly wages at ages 25-27



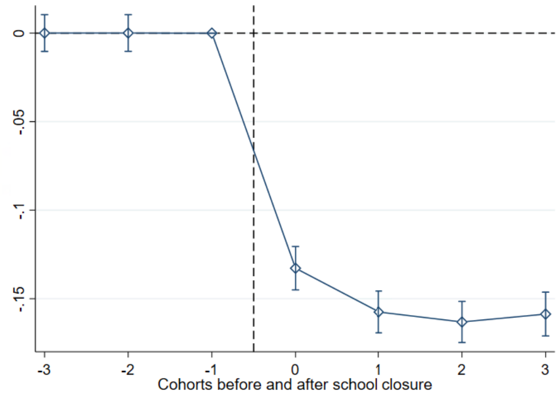
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) using control schools selected from the alternative matching strategies denoted on the y-axis. The baseline estimates are presented at the top of each sub-figure. The percentage in the parenthesis on the y-axis 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 level.

Fig. B.3. Short-Run Effects of School Closure on Student Outcomes: Synthetic Difference-in-Differences

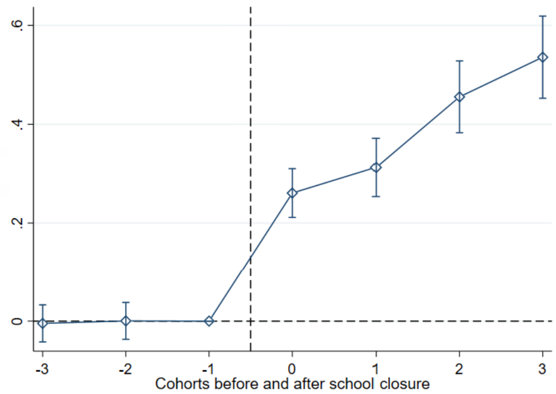
(a) Standardized math score



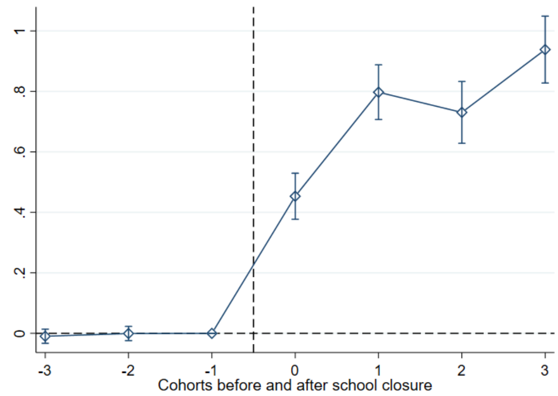
(b) Standardized reading score



(c) Days of absence



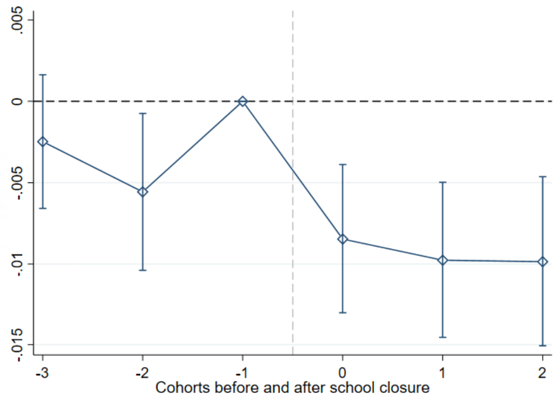
(d) Days of disciplinary action



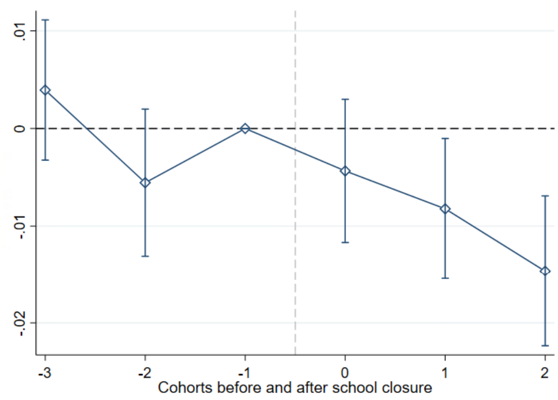
Notes: The figures present estimates from the implementation of a synthetic difference-in-differences model following Arkhangelsky et al. (2021). In this model, a synthetic control group is constructed using a donor pool of students who are enrolled in the same year, same school type (e.g. elementary schools are only matched with other elementary schools), and same school locale following the NCES locale category as the treated group.

Fig. B.4. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Synthetic Difference-in-Differences

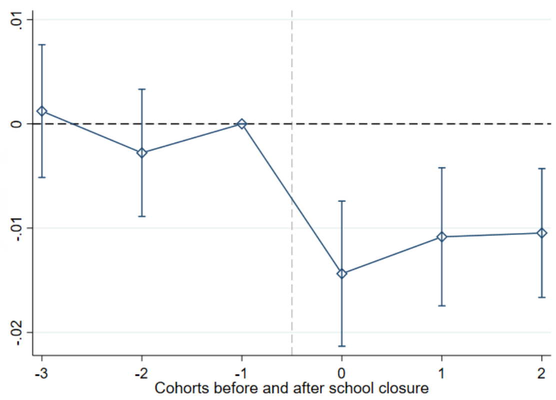
(a) High school graduation



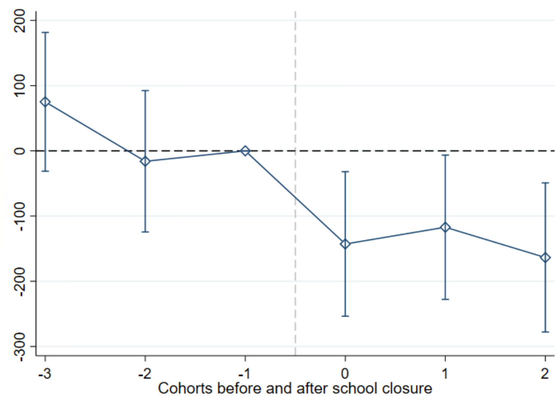
(b) Any college enrollment



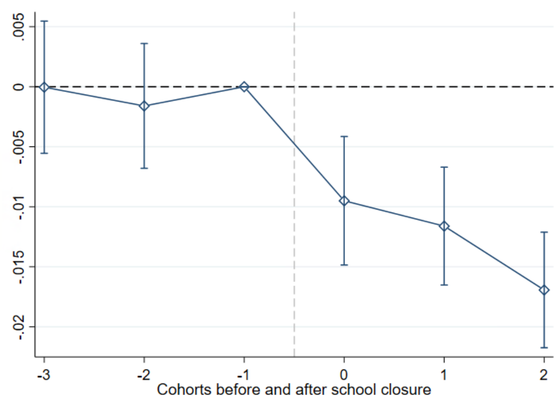
(c) Four-year college completion



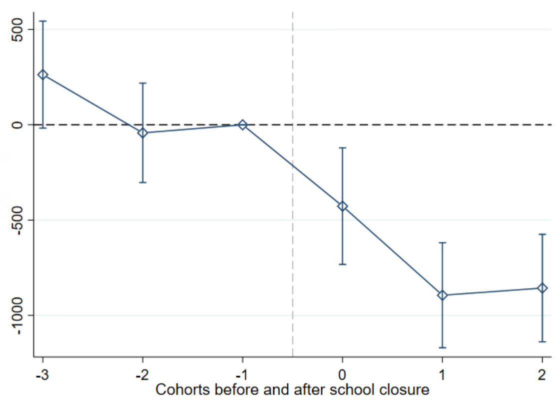
(d) College quality



(e) Employment at ages 25-27



(f) Yearly wages at ages 25-27



Notes: The figurs present estimates 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 schools that are in the same year, of the same school type (e.g. elementary schools are only matched with other elementary schools), and in the same school locale following the NCES locale category as the treated group.

B.2 Short-Run Analysis

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 of my estimates by providing estimation results of equation (2) with different sample specifications. In Appendix Figure B.5, 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 Table B.1 presents estimates from equation (1) using the same fully balanced panel as in Figure 2. The baseline estimation uses a balanced panel from $t = -3$ to $t = 2$, while data for $t = 2$ and $t = 3$ may be missing for some students, making those periods partially unbalanced. The results closely resemble the baseline estimates, although the impact on days of absence appears more pronounced.

Appendix Figure B.6 presents estimation results using different cutoffs for excluding schools based on the proportion of displaced students observed at the same address after closure. In the baseline specification (Figure 2), I exclude closed schools if more than 30% of displaced students are observed attending a school at the same address in the year following the closure. This aims to address concerns that coding changes or school reforms—rather than actual closures—may lead to an underestimation of the impacts. In this appendix figure, I test two alternative cutoffs: 10% and 90%. Under the 10% cutoff, approximately 41% of displaced students are excluded from the analysis; under the 90% cutoff, about 3% are excluded.

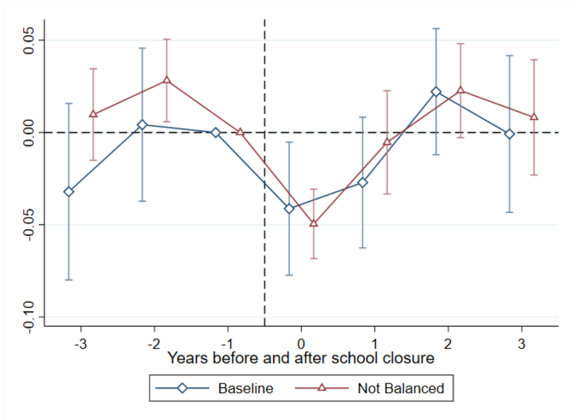
Importantly, this approach may also exclude some schools that were actually closed, as verified by school districts, simply because a share of students is reported as observed at the same address the following year. This suggests that while the exclusion strategy helps filter out non-physical closures due to coding changes or school level reforms, it may also result in the unintended removal of actual closures due to data limitations or address misreporting. Despite these variations in the exclusion threshold, however, the overall patterns remain highly consistent with the baseline results. In Appendix Figure B.7, I present the estimated coefficients (β) and 95% confidence intervals from Equation (1) across the different cutoff levels shown on the X-axis. All other specifications follow those in Table 1. While again the results are broadly similar, the negative effect on disciplinary incidents appears more pronounced when a stricter cutoff is applied.

I further compare the estimation results between the baseline and long-run sample closures in Appendix Figure B.8. Moreover, the results from equation (1) for long-run sample school closures are presented in Appendix Table B.2. Overall, the estimates are similar across the two samples. The negative impacts on test scores are more pronounced immediately after school closures but decline over time and eventually converge to zero in both samples. While the behavioral outcome estimates are noisier in the long-run sample, I find that the impact on days of absence is smaller, whereas the impact on days of disciplinary actions is larger compared to the baseline sample.

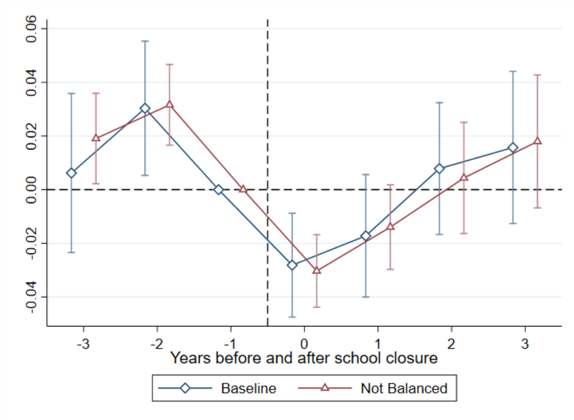
Lastly, to examine whether the effects of school closures vary over time, I estimate the effects separately for three periods: 1998–2003, 2004–2009, and 2010–2015. The results from equation (1) for each period are presented in Appendix Table B.3. While the estimates are somewhat less stable, the overall trends appear similar across periods, with a few notable differences. First, days of absence decrease following early closures but increase in the middle and later periods. Second, days of disciplinary action rise sharply and remain elevated after early and later closures, whereas they continue to increase over time following closures in the middle period.

Fig. B.5. Short-Run Effects of School Closure on Student Outcomes: Balanced and Unbalanced Sample

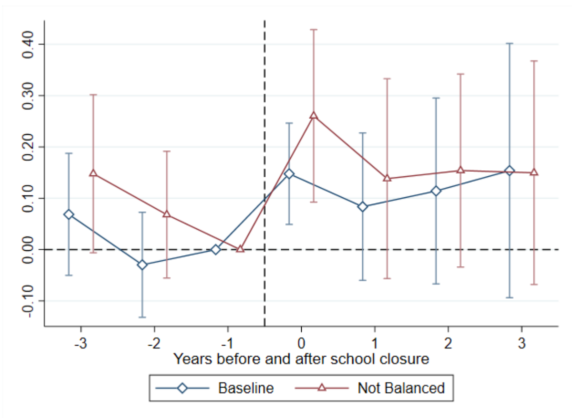
(a) Standardized math score



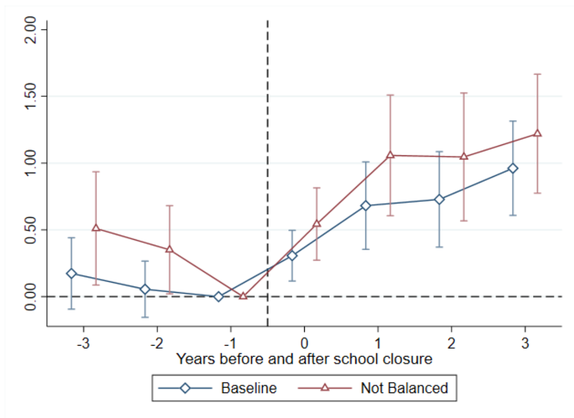
(b) Standardized reading score



(c) Days of absence



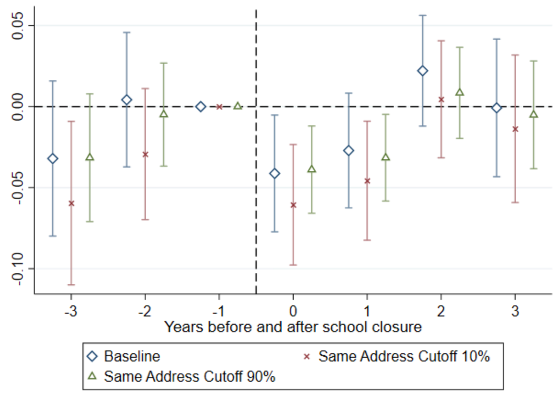
(d) Days of disciplinary action



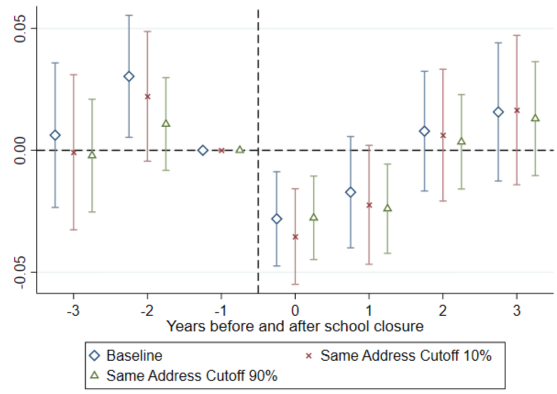
Notes: The figures overlay the coefficients, ρ_t, and 95% confidence intervals from equation (2) using either baseline (balanced panel) or unbalanced sample. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years 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. B.6. Short-Run Effects of School Closure on Student Outcomes: Excluding Same Address School Opening with Different Cutoffs (1)

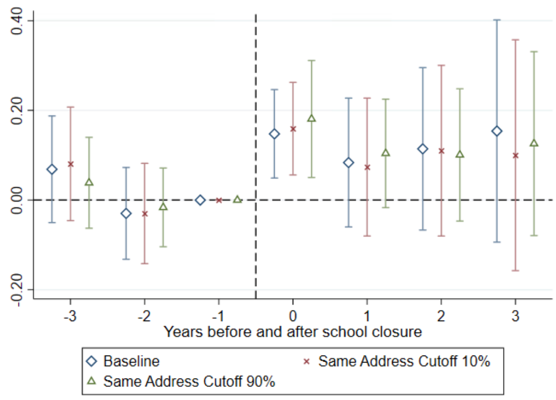
(a) Standardized math score



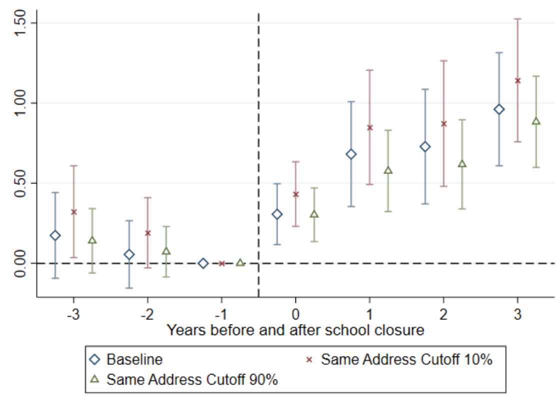
(b) Standardized reading score



(c) Days of absence



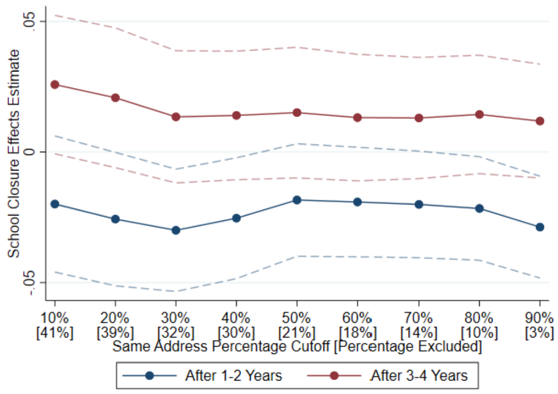
(d) Days of disciplinary action



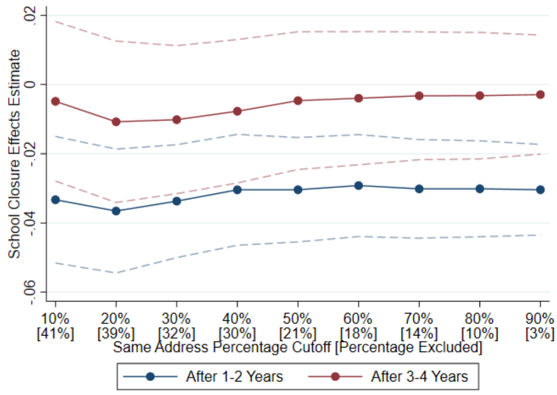
Notes: The figures overlay the coefficients, ρ_t, and 95% confidence intervals from equation (2), using either the baseline sample or alternative cutoffs based on the proportion of displaced students observed at the same address after closure. A cutoff of 10% (90%) means that schools where more than 10% (90%) of displaced students remain at the same address after closure are excluded from the analysis. 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. The analysis sample is balanced. Standard errors are clustered by school at $t = -1$.

Fig. B.7. Short-Run Effects of School Closure on Student Outcomes: Excluding Same Address School Opening with Different Cutoffs (2)

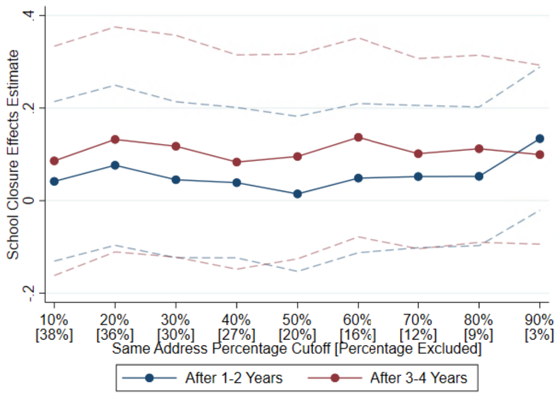
(a) Standardized math score



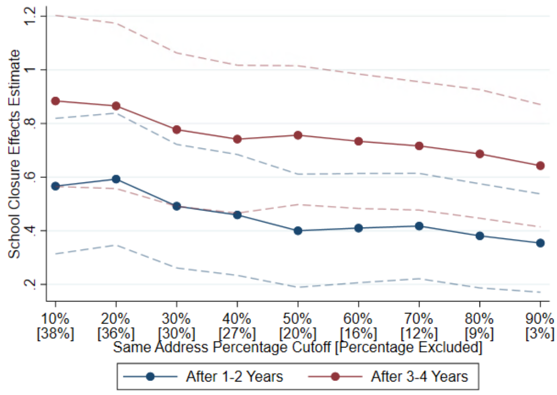
(b) Standardized reading score



(c) Days of absence



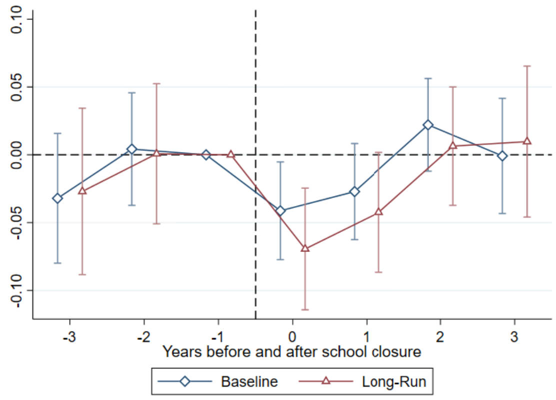
(d) Days of disciplinary action



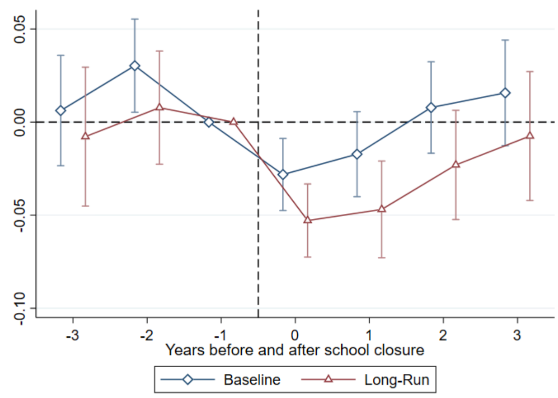
Notes: The figures overlay the coefficients, β, and 95% confidence intervals from equation (1) using a sample excluding closed schools where displaced students are observed at the same address after closure, applying different cutoffs. The x-axis shows the percentage cutoff for the proportion of displaced students, with parentheses indicating the percentage of students excluded from the analysis. 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. The analysis sample is balanced, except for the third and fourth years after the school closure, which may be missing for students in higher grades. Standard errors are clustered by school at $t = -1$.

Fig. B.8. Short-Run Effects of School Closure on Student Outcomes: Using Long-Run Sample School Closures

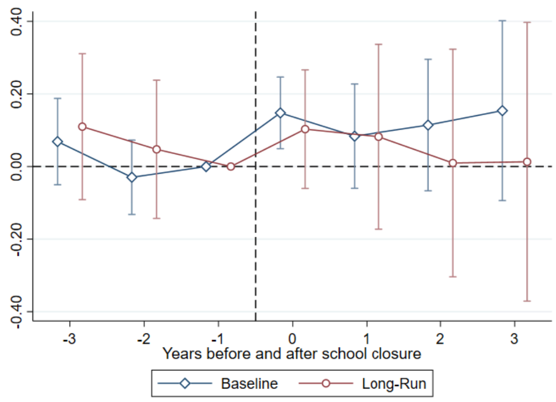
(a) Standardized math score



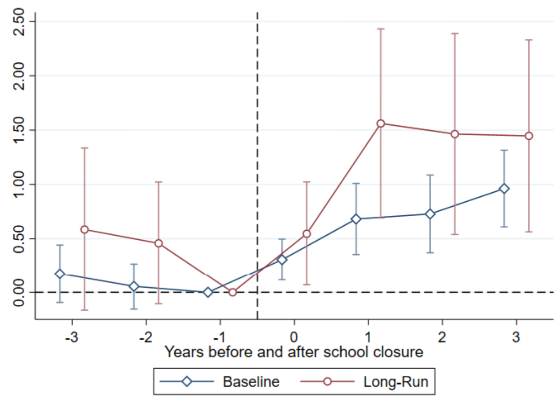
(b) Standardized reading score



(c) Days of absence



(d) Days of disciplinary action



Notes: The figures overlay the coefficients, ρ_t, and 95% confidence intervals from equation (2), using either the baseline sample or the long-run sample school closures. 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. The analysis sample is balanced. Standard errors are clustered by school at $t = -1$.

Table B.1: Short-Run Effects of School Closure on Student Outcomes: Using Fully Balanced Sample

	(1) Math	(2) Reading	(3) Days of Absence	(4) Days of Disciplinary Action
Closed School×After 1-2 Years	-0.025*	-0.035***	0.103*	0.417***
	(0.014)	(0.010)	(0.060)	(0.092)
Closed School×After 3-4 Years	0.020	-0.000	0.122	0.767***
	(0.013)	(0.011)	(0.104)	(0.131)
Observations	199,121	199,121	991,031	846,483
Individual FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Pre-Closure Mean	0.119	0.146	6.033	1.459

Notes: The table presents the coefficients, β, and standard errors from equation (1), using balanced sample. 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. The mean of pre-closure refers to the average value of the outcome variable at time $t = -1$ for displaced students in the analysis sample. The analysis sample is balanced, except for the third and fourth years after the school closure, which may be missing for students in higher grades. Standard errors are clustered by school at $t = -1$. *** p<0.01, ** p<0.05, * p<0.10

Table B.2: Short-Run Effects of School Closure on Student Outcomes: Using Long-Run Sample School Closures

	(1) Math	(2) Reading	(3) Days of Absence	(4) Days of Disciplinary Action
Closed School × After 1-2 Years	-0.050***	-0.044***	-0.113	0.615**
	(0.022)	(0.022)	(0.154)	(0.259)
Closed School × After 3-4 Years	0.008	-0.007	-0.064	0.971***
	(0.016)	(0.015)	(0.204)	(0.332)
Observations	257,470	257,470	570,843	382,633
Individual FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Mean of pre-closure	0.078	0.141	8.553	3.495

Notes: The table presents the coefficients, β, and standard errors from equation (1), using long-run sample school closures. 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. The mean of pre-closure refers to the average value of the outcome variable at time $t = -1$ for displaced students in the analysis sample. The analysis sample is balanced, except for the third and fourth years after the school closure, which may be missing for students in higher grades. Standard errors are clustered by school at $t = -1$. *** p<0.01, ** p<0.05, * p<0.10

Table B.3: Short-Run Effects of School Closure on Student Outcomes: Divided by Time Period

Panel A: School Closures in 1998-2003				
	(1) Math	(2) Reading	(3) Days of Absence	(4) Days of Disciplinary Action
Closed School×After 1-2 Years	-0.013	-0.036**	-0.280*	1.074***
	(0.015)	(0.014)	(0.155)	(0.380)
Closed School×After 3-4 Years	0.015	-0.018	-0.353*	0.971**
	(0.017)	(0.017)	(0.199)	(0.417)
Observations	119,208	119,208	385,351	197,140
Individual FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Mean of pre-closure	0.183	0.170	6.622	1.963
Panel B: School Closures in 2004-2009				
	(1) Math	(2) Reading	(3) Days of Absence	(4) Days of Disciplinary Action
Closed School×After 1-2 Years	-0.043**	-0.032**	0.098	0.126
	(0.017)	(0.012)	(0.157)	(0.167)
Closed School×After 3-4 Years	0.003	0.013	0.337	0.656**
	(0.018)	(0.015)	(0.210)	(0.261)
Observations	216,559	216,559	396,945	396,945
Individual FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Mean of pre-closure	0.027	0.092	6.911	2.314
Panel C: School Closures in 2010-2015				
	(1) Math	(2) Reading	(3) Days of Absence	(4) Days of Disciplinary Action
Closed School×After 1-2 Years	-0.017	-0.038**	0.327***	0.643***
	(0.028)	(0.017)	(0.115)	(0.145)
Closed School×After 3-4 Years			0.303	0.803***
			(0.212)	(0.147)
Observations	98,379	98,379	363,285	363,285
Individual FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Mean of pre-closure	-0.308	-0.264	6.432	1.948

Notes: The table presents the coefficients, β, and standard errors from equation (1), with the baseline sample divided into three periods. Test score impact estimates for after 3-4 years are excluded because of the data constraints for school closures in 2010-2015. 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. The mean of pre-closure refers to the average value of the outcome variable at time $t = -1$ for displaced students in the analysis sample. The analysis sample is balanced, except for the third and fourth years after the school closure, which may be missing for students in higher grades. Standard errors are clustered by school at $t = -1$. *** p<0.01, ** p<0.05, * p<0.10

B.3 Long-Run Analysis

My long-run event study analysis uses 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. To assess the robustness of the long-run estimates, I explore alternative sample specifications and sets of controls. Appendix Figure B.9 presents results based on an unbalanced sample, shown alongside the baseline results for comparison. The overall patterns closely mirror those from the baseline analysis. While the estimated effects on educational outcomes are somewhat more pronounced, the effects on labor market outcomes are comparatively less pronounced.

Appendix Figure B.10 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 B.4 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. While the results are broadly consistent across specifications, estimates that exclude performance controls are generally smaller than the baseline estimates. Particularly, the school quality and any college enrollment estimates are not statistically distinguishable from zero when performance measures are not included.

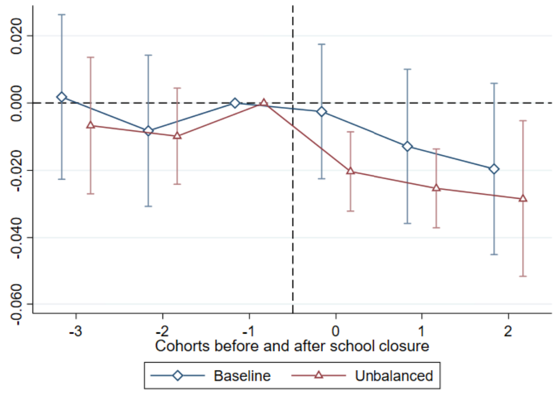
Appendix Figure B.11 presents estimation results using different cutoffs for excluding schools based on the proportion of displaced students observed at the same address after closure. In the baseline specification (Figure 6), I exclude closed schools if more than 30% of displaced students are observed attending a school at the same address in the year following the closure. This aims to address concerns that coding changes or school reforms—rather than actual closures—may lead to an underestimation of the impacts. In this appendix figure, I test two alternative cutoffs: 10% and 90%. Under the 10% cutoff, approximately 48% of displaced students are excluded from the analysis; under the 90% cutoff, about 3% are excluded.

Importantly, this approach may also exclude some schools that were actually closed, as verified by school districts, simply because a share of students is reported as observed at the same address the following year. This suggests that while the exclusion strategy helps filter out non-physical closures due to coding changes or school level reforms, it may also result in the unintended removal of actual closures due to data limitations or address misreporting. Despite these variations in the exclusion threshold, the overall patterns remain highly consistent with the baseline results. In Appendix Figure B.12, I overlay the estimated coefficients (γ) and 95% confidence intervals from Equation (3) across the different cutoff levels shown on the X-axis. All other specifications follow those in Table 2. While again the results are broadly similar, the negative effect on high school graduation and earnings appears more pronounced when a stricter cutoff is applied.

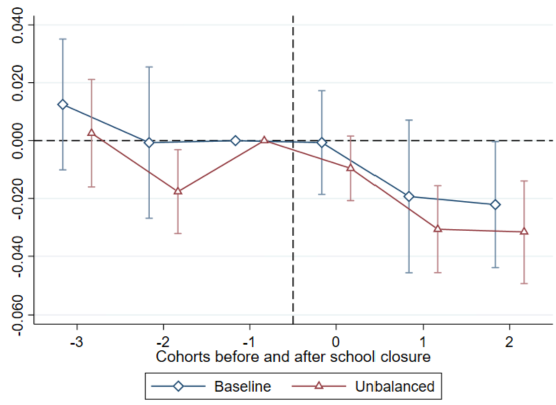
Another approach to constructing the sample involves selecting the same school grade both in the year of school closure and in preceding years. For instance, in the example of the main text, I can create a comparable sample by choosing the third highest grade from 1998 to 2003. Then, students in the 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 presented in the main text—where school **A**, serving grades 1–5, closed at the end of the 2000 school year—this corresponds to using fourth-grade students from 1997 to 2000. Then, fourth-grade students from 1997 to 1998 represent younger cohorts, and students from 1999 to 2000 represent older cohorts. As illustrated in the Appendix Figure B.13, the outcomes using this alternative approach also find similar negative impacts of school closures on displaced students.

Fig. B.9. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Balanced and Unbalanced Sample

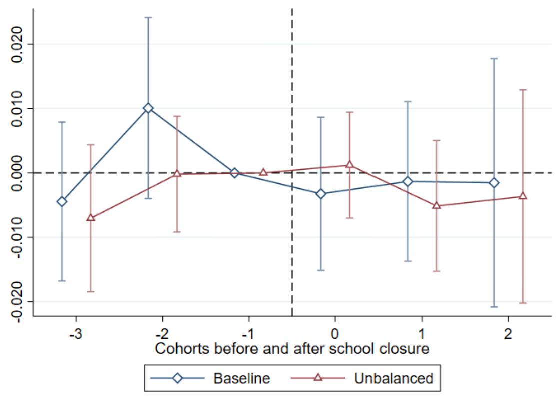
(a) High school graduation



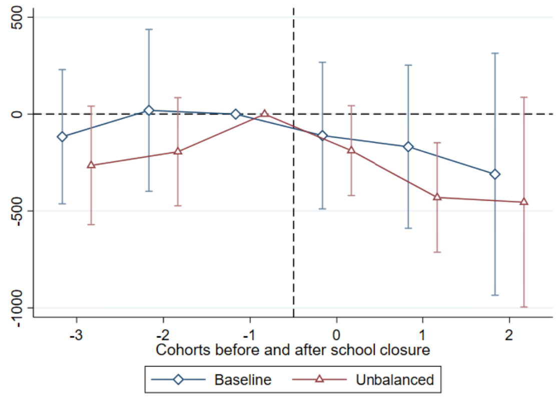
(b) Any college enrollment



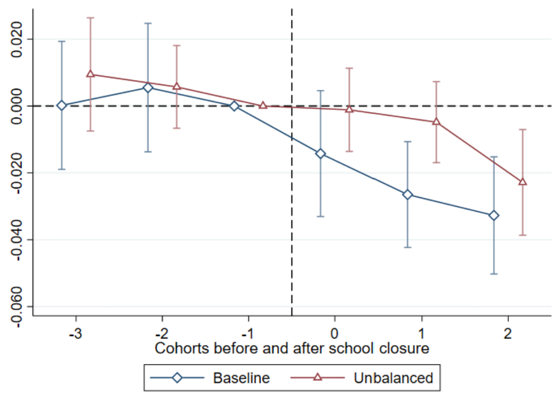
(c) Four-year college completion



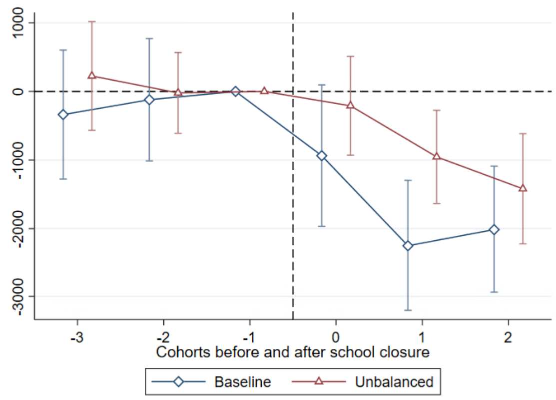
(d) College quality



(e) Employment



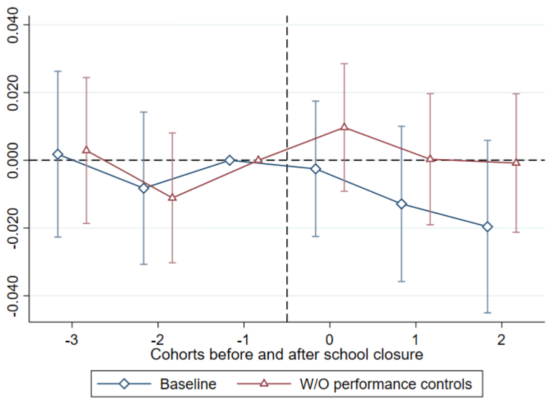
(f) Yearly earnings



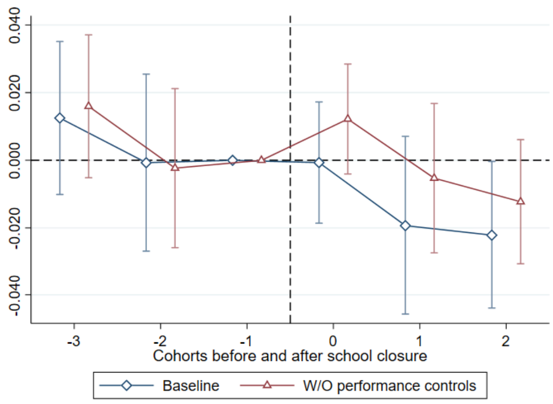
Notes: The figures overlay the coefficients, π_c, and 95% confidence intervals from equation (4), using either baseline (balanced) or unbalanced sample. The unbalanced sample includes closed schools having fewer than 3 grades. These coefficients represent the interactions between the indicator that denotes 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 level.

Fig. B.10. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: With and Without Controlling for Performance Measures

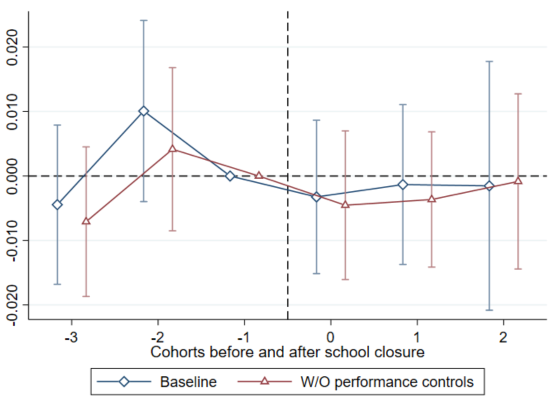
(a) High school graduation



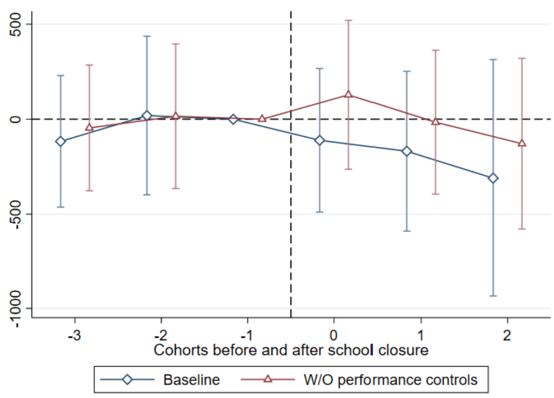
(b) Any college enrollment



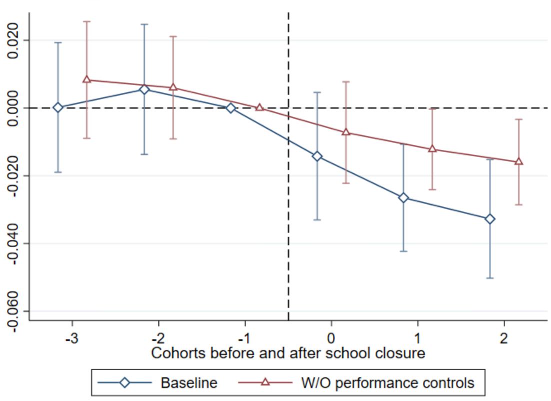
(c) Four-year college completion



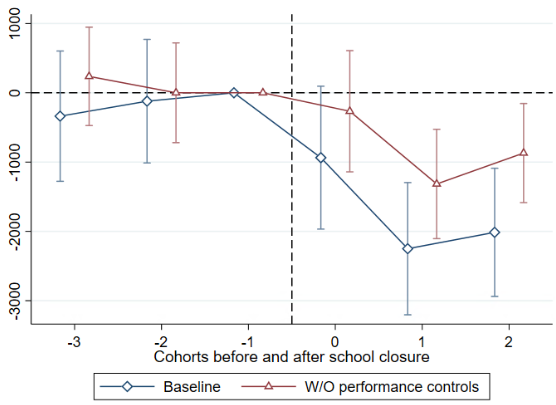
(d) College quality



(e) Employment



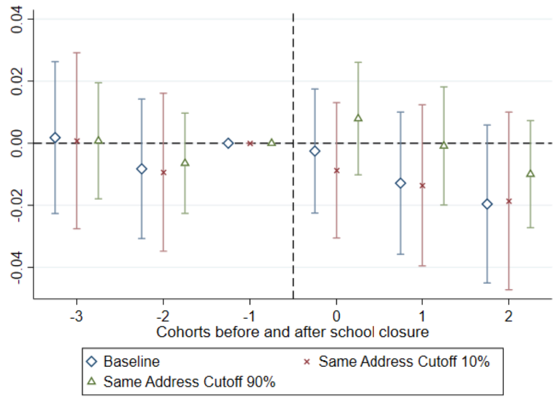
(f) Yearly earnings



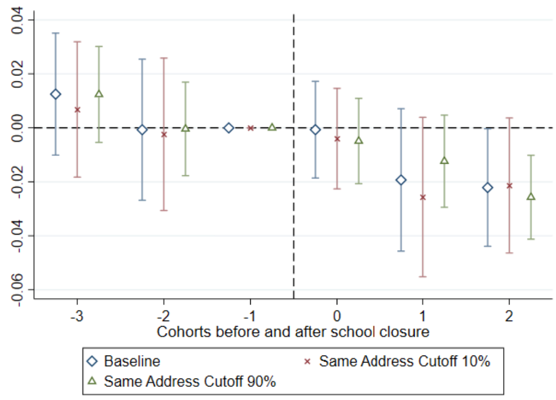
Notes: The figures overlay the coefficients, π_c, and 95% confidence intervals from equation (4) with and without controlling for standardized math and reading scores, and standardized absence rate. These coefficients represent the interactions between the indicator that denotes closed schools and the 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. The analysis sample is balanced. Standard errors are clustered at the school level.

Fig. B.11. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Excluding Same Address School Opening with Different Cutoffs (1)

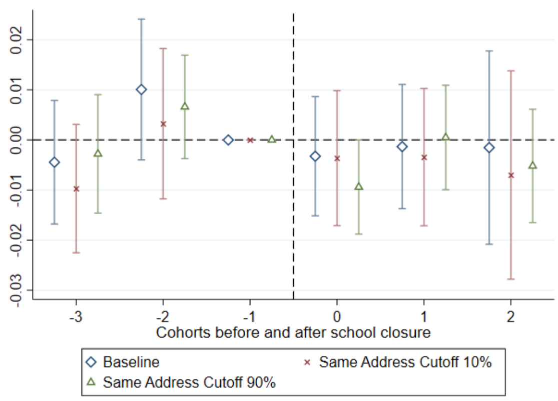
(a) High school graduation



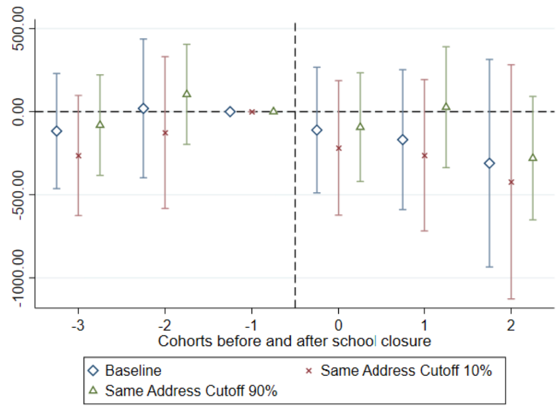
(b) Any college enrollment



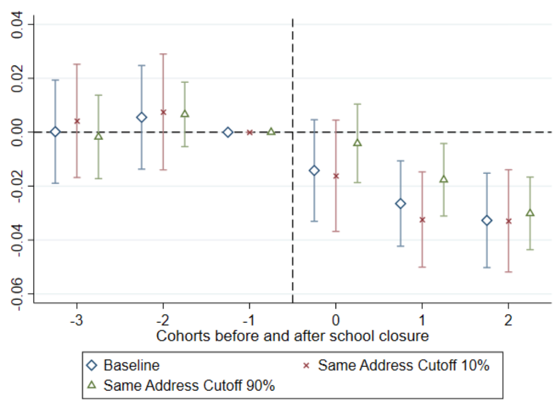
(c) Four-year college completion



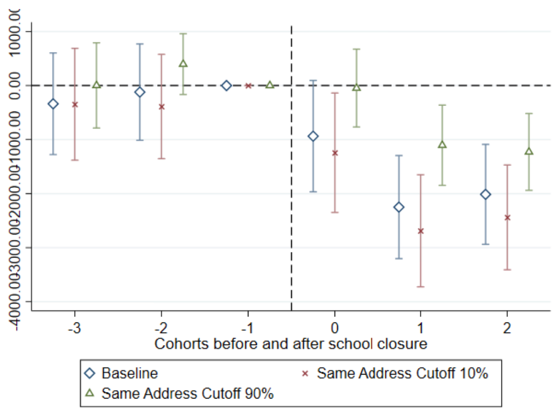
(d) College quality



(e) Employment



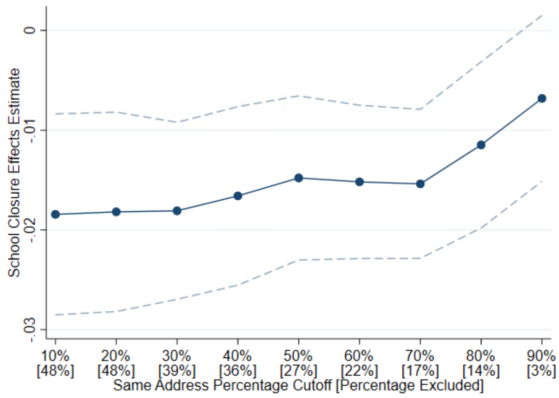
(f) Yearly earnings



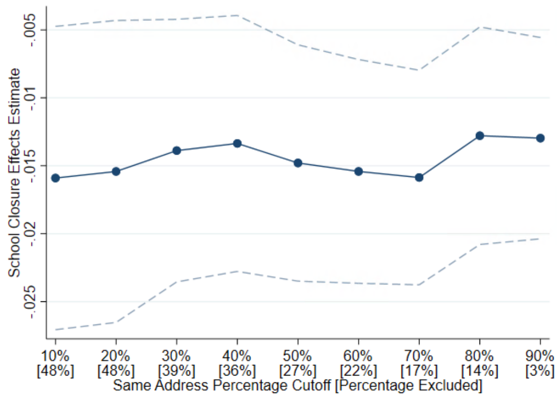
Notes: The figures overlay the coefficients, π_c, and 95% confidence intervals from equation (4), using either the baseline sample or alternative cutoffs based on the proportion of displaced students observed at the same address after closure. A cutoff of 10% (90%) means that schools where more than 10% (90%) of displaced students remain at the same address after closure are excluded from the analysis. 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. The analysis sample is balanced. Standard errors are clustered at the school level.

Fig. B.12. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Excluding Same Address School Opening with Different Cutoffs (2)

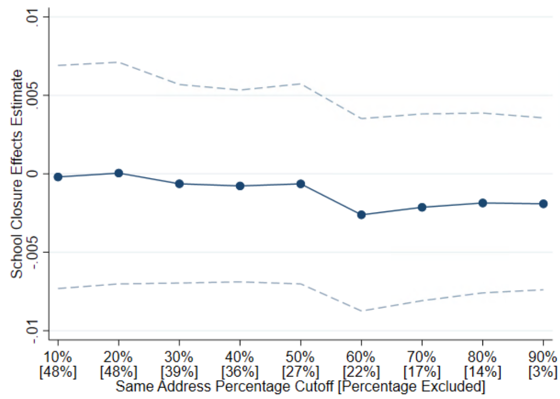
(a) High school graduation



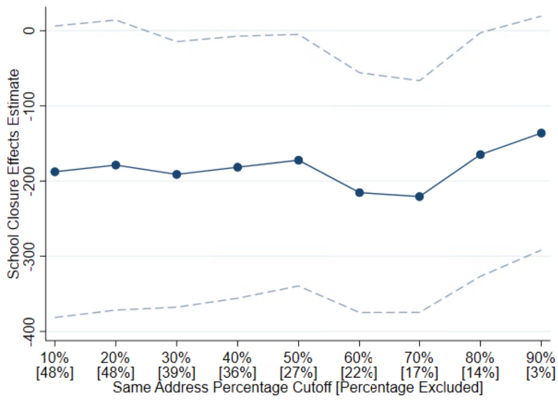
(b) Any college enrollment



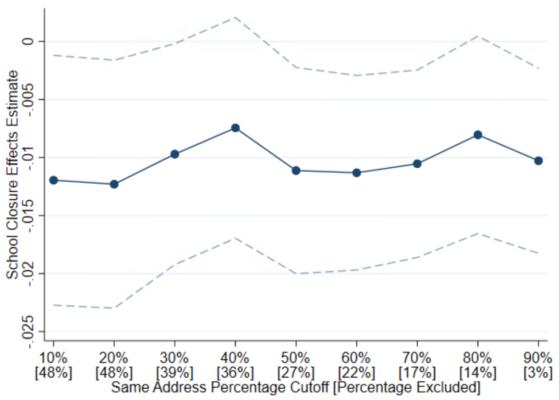
(c) Four-year college completion



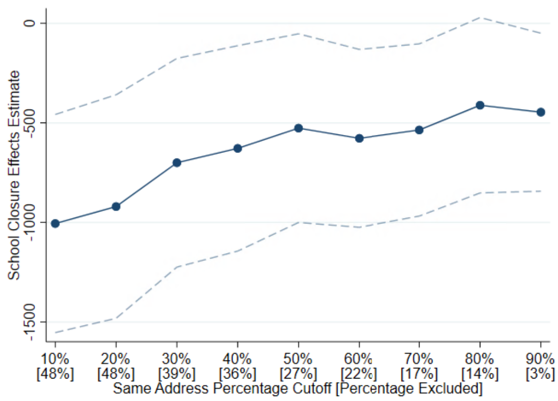
(d) College quality



(e) Employment



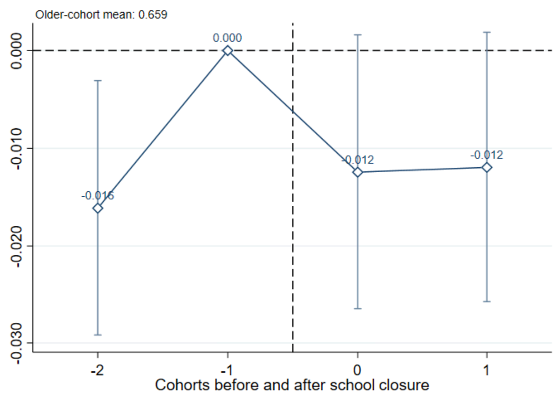
(f) Yearly earnings



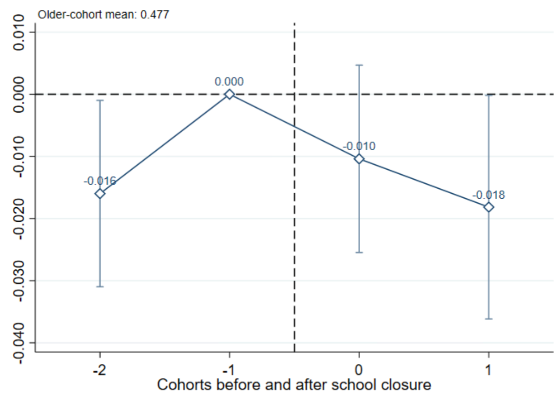
Notes: The figures overlay the coefficients, γ, and standard errors from equation (3) using a sample excluding closed schools where displaced students are observed at the same address after closure, applying different cutoffs. The x-axis shows the percentage cutoff for the proportion of displaced students, with parentheses indicating the percentage of students excluded from the analysis. 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 level.

Fig. B.13. Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Alternative Way of Cohort Construction

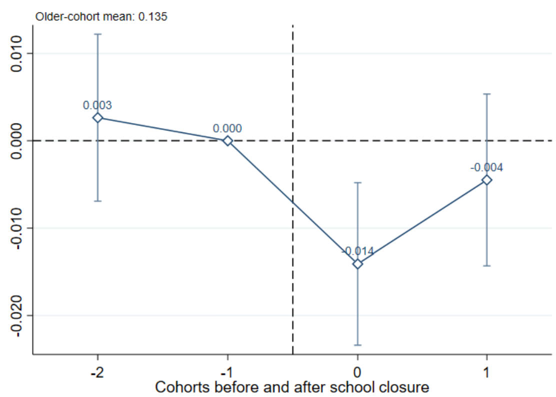
(a) High school graduation



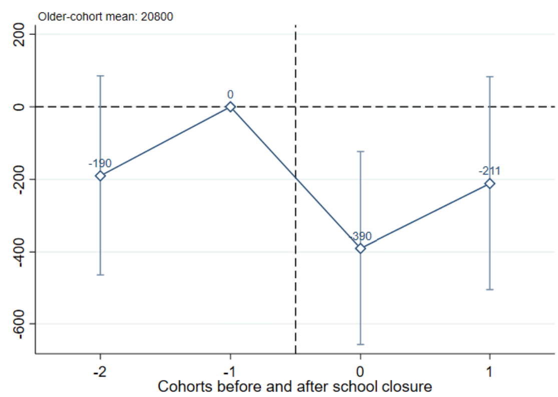
(b) Any college enrollment



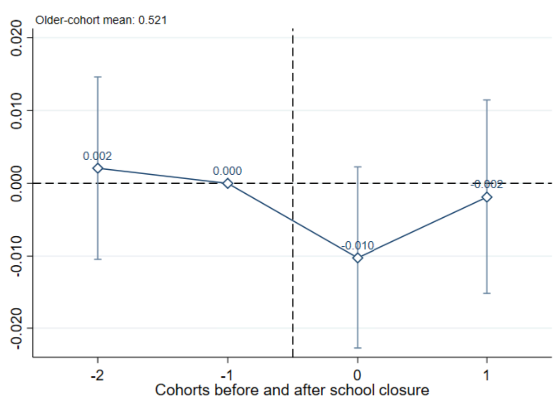
(c) Four-year college completion



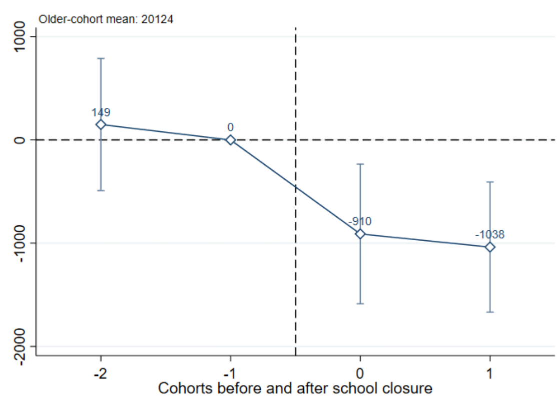
(d) College quality



(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 level.

Table B.4: 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	-0.011**	-0.011**	-0.018***
× Younger Cohorts	(0.005)	(0.005)	(0.005)
Any college enrollment			
Closed School	-0.009*	-0.008	-0.014***
× Younger Cohorts	(0.005)	(0.005)	(0.005)
Four-year college completion			
Closed School	0.003	0.004	-0.001
× Younger Cohorts	(0.003)	(0.003)	(0.003)
College quality			
Closed School	-35	0	-191**
× Younger Cohorts	(99)	(89)	(90)
Employment at ages 25-27			
Closed School	-0.011**	-0.007	-0.010**
× Younger Cohorts	(0.005)	(0.005)	(0.005)
Yearly wages at ages 25-27			
Closed School	-596**	-500*	-700***
× Younger Cohorts	(257)	(257)	(267)
Non-zero yearly wages at ages 25-27			
Closed School	-444*	-312	-458*
× Younger Cohorts	(265)	(255)	(266)
Potential full-time yearly earnings at ages 25-27			
Closed School	-676**	-590**	-705**
× Younger Cohorts	(280)	(271)	(280)
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). Potential full-time yearly earnings are calculated following Sorkin (2018). See Appendix Section B.4 for more details. Standard errors are clustered by school level. *** p<0.01, ** p<0.05, * p<0.10

B.4 Out-of-Texas Attrition

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.4 (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.4 (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 0.7 percentage points.[B.3] This finding provides reassurance that sample attrition right after closure was not a major concern, as students did not differentially 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 B.5. The estimates show that younger cohorts from closed schools are 0.2 percentage points less likely to enroll in college out-of-Texas relative to students from matched control schools, while it is not statistically significant. 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 Figure B.14 and Table B.4). Specifically, I look into two different measures of earnings following Miller (2023) and Sorkin (2018): non-zero yearly earnings and potential full-time yearly earnings. The non-zero yearly earnings sample includes only individuals with positive earnings. The potential full-time yearly earnings measure estimates full-time earnings based on the earnings structure.[B.4]

[B.3] To see the potential impact of the attrition, I estimate Lee (2009) bounds assuming differential attrition in response to a school closure of 0.7 percentage points. The estimated bounds are presented in Panel A of Appendix Table B.6. While these Lee bounds cover a range of estimates, the bounds exclude zero for all the outcomes except four-year college completion.

[B.4] Specifically, earnings in quarter t are classified into one of two mutually exclusive categories: (i) *full-quarter*, if earnings from the employer are recorded in quarters $t-1$, t, and $t+1$, or (ii) *continuous*, if earnings are recorded in either $t-1$ and t, or t and $t+1$. Here, quarterly earnings need to be at least \$3,800 following

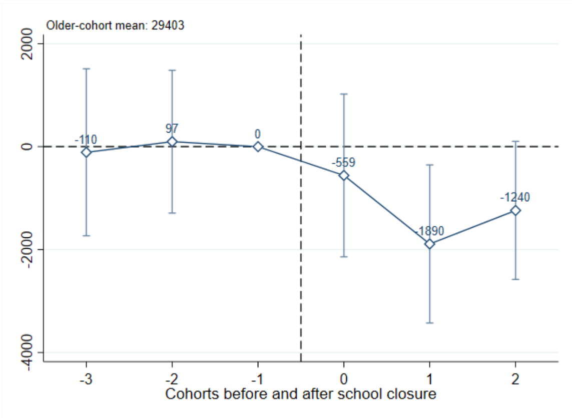
Thirdly, using a school quality measure based on their highest education level and institution, I find consistent results showing a decrease in expected earnings among the sample of individuals. Lastly, I perform a bounding exercise with the non-zero earning samples, attributing all the decrease in employment rates after school closure to attrition to the labor market outside Texas (Lee 2009). The Lee bounds, presented in Panel B of Appendix Table B.6, are mostly in the negative range.[B.5] The evidence suggests that even under the extreme assumption, the main implications remain unchanged.

Miller (2023). Earnings are annualized as follows. If the worker has any quarters with full-quarter earnings, the average of these quarters is taken and multiplied by 4 to obtain an annualized salary. If the worker does not have full-quarter earnings but has any quarters with continuous earnings, the average of these quarters is taken and multiplied by 8 to obtain an annualized salary. The justification for this procedure is that if a worker is present in only two consecutive quarters, and if employment duration is uniformly distributed, then on average, the earnings represent $\frac{1}{2}$ of a quarter's work. Conversely, if a worker is present in both adjacent quarters, the earnings reflect a full quarter's work. See Online Appendix of Sorkin (2018) for more details.

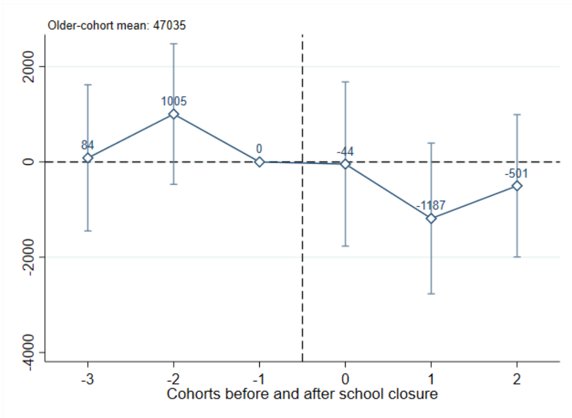
[B.5] Although the lower bound is a positive number, it is small and insignificant. Furthermore, regressing employment on standardized college quality using the same setting as in equation (3) gives an estimate of 0.1 (0.002), indicating that a one standard deviation increase in college quality is associated with a 10 percentage point increase in employment. Based on this, the lower bound is less plausible since the lower bound assumes that the highest earners from closed schools are not employed in my sample.

Fig. B.14. Long-Run Effects of School Closure on Earnings: Different Measures

(a) Non-Zero Yearly earnings



(b) Potential full-time earnings



Notes: The figures present the coefficients, π_c, and 95% confidence intervals from equation (4). Figure (a) includes only samples with positive earnings. Figure (b) includes only samples with potentially full-time earnings, following Sorkin (2018). See Appendix Section B.4 for more details. 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. The analysis sample is balanced. The older-cohort mean refers to the average value of the outcome variable for students in older cohorts ($c \in \{-3, -2, -1\}$) attending closed schools in the analysis sample. Standard errors are clustered at the school level.

Table B.5: Long-Run Effects of School Closure on Out-of-State Post-Secondary Education Enrollment

	Out-of-State College Enrollment
Closed School $\times$ Younger Cohorts	-0.002
	(0.002)
Observations	164,497
School FE	X
Matched group $\times$ Year FE	X
Mean of the Older Cohort	0.043

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. The mean of the older cohort refers to the average value of the outcome variable for students in older cohorts ($c \in \{-3, -2, -1\}$) attending closed schools in the analysis sample. Standard errors are clustered by school level. *** p<0.01, ** p<0.05, * p<0.10

Table B.6: Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Lower and Upper Bounds on the Estimated Effects

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	-0.018***	-0.016***	-0.22***
× Younger Cohorts	(0.005)	(0.005)	(0.005)
Any college enrollment			
Closed School	-0.014***	-0.011**	-0.016***
× Younger Cohorts	(0.004)	(0.004)	(0.004)
Four-year college completion			
Closed School	-0.001	0.005*	-0.001
× Younger Cohorts	(0.003)	(0.003)	(0.003)
College quality			
Closed School	-191**	-33	-251***
× Younger Cohorts	(90)	(86)	(91)
Employment at ages 25-27			
Closed School	-0.010**	-0.007	-0.011**
× Younger Cohorts	(0.005)	(0.005)	(0.005)
Yearly earnings at ages 25-27			
Closed School	-700***	-83	-749***
× Younger Cohorts	(267)	(265)	(268)
School FE	X	X	X
Matched group × Year FE	X	X	X
Panel B: trimming based on estimated impact of school closure on employment			
	(1) Baseline	(2) Lee Lower Bound	(3) Lee Upper Bound
Non-Zero Yearly earnings at ages 25-27			
Closed School	-458*	233	-685**
× Younger Cohorts	(266)	(271)	(264)
Potential full-time yearly earnings at ages 25-27			
Closed School	-705**	77	-904***
× Younger Cohorts	(280)	(270)	(276)
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 level. *** p<0.01, ** p<0.05, * p<0.10

C Comparison with Previous Papers

C.1 School Closure Impacts on Displaced Students

In this section, I provide a detailed review of previous literature, comparing their findings with my estimates (see Appendix Table C.1 for a brief overview).

Several studies in a similar context to my paper find overall negative effects of school closures on test scores, absenteeism, and suspensions (Brummet 2014; Engberg et al. 2012; Kirshner, Gaertner, and Pozzoboni 2010; Larsen 2020; Özek, Hansen, and Gonzalez 2012; Steinberg and MacDonald 2019; Torre and Gwynne 2009). Specifically, Brummet (2014), Engberg et al. (2012), Han et al. (2017), Larsen (2020), Özek, Hansen, and Gonzalez (2012), and Torre and Gwynne (2009) report declines in test scores or GPA following school closures, though these effects tend to diminish over time—consistent with my findings (see Appendix Figure C.1). The magnitude of the decline varies, with Engberg et al. (2012) estimating a decrease of more than 0.15 SD one to two years after closure, while Brummet (2014) and Han et al. (2017) find a smaller effect, under 0.05 SD.[C.1] Notably, Brummet (2014) and Han et al. (2017) are the only studies using state-level data and report findings that closely align with mine. In terms of heterogeneity, Brummet (2014) find that the impact on test scores is less disruptive, and Steinberg and MacDonald (2019) find that suspensions increase more when students move to higher-performing schools—both findings consistent with my results. Although these studies provide a useful basis for comparison with my own, the lack of evidence supporting the parallel trends assumption—or evidence contradicting it—as well as the fact that many of these studies are based on a single school district, limits the extent to which their estimates can be interpreted as causal or directly compared to mine.

Beyond test scores, Engberg et al. (2012), Larsen (2020), and Steinberg and MacDonald (2019) also document increases in absenteeism and suspensions. The magnitude of these effects varies significantly, ranging from a 13% increase in absenteeism one to two years after closure in Engberg et al. (2012) to 2% in Steinberg and MacDonald (2019).[C.2] Additionally, Engberg et al. (2012) and Steinberg and MacDonald (2019) find that relatively persistent effects

[C.1] Specifically, Torre and Gwynne (2009) find a 0.77-month decline in math learning one year after closure (10% significance), followed by a 1.41-month gain after three years (not significant). Reading gains were 0.19 and 0.21 months after one and three years, respectively, and not statistically significant. Engberg et al. (2012) find declines in test scores following school closures: in math, from -0.19 (0.05) SD one year post-closure to -0.14 (0.08) SD three years later; and in reading, from -0.20 (0.05) to -0.03 (0.05) SD over the same period. Özek, Hansen, and Gonzalez (2012) find declines in test scores following school closures: in math, from -0.132 (0.055) SD one year post-closure to -0.028 (0.58) SD two years later; and in reading, from -0.102 (0.046) to -0.017 (0.047) SD over the same period. Brummet (2014) reports similar patterns, with math scores declining from -0.074 (0.033) to -0.010 (0.016) SD, and reading scores from -0.053 (0.033) to -0.033 (0.021) SD, between one and three or more years after closure. Han et al. (2017) also find negative impact, with math scores declining from -0.01 to 0.01 SD, and reading scores from -0.02 to -0.01 SD, between one and three years after closure (significant at the 10% level for both math estimates and for reading estimate after one year). In contrast, Steinberg and MacDonald (2019) find no meaningful effects: math scores range from 0.010 (0.037) to 0.011 (0.044) SD, and reading scores from 0.019 (0.030) to -0.001 (0.039) SD over the same timeframe. Larsen (2020) finds declines in GPA from -0.157 (0.066) to -0.091 (0.071) points (on a 4.0 scale) between one and three or more years post-closure.

[C.2] Engberg et al. (2012) find an increase in absences, from 0.13 (0.05) to 0.07 (0.04) in proportion, between one and three years after closure. Similarly, Steinberg and MacDonald (2019) report increases in both suspensions and absences: suspensions rise from 0.021 (0.031) to 0.058 (0.034) days, and absences from 0.189 (0.051) to 0.089 (0.058) days over the same time frame. Larsen (2020) also finds increased behavioral disruptions, with attendance rates declining by -0.028 (0.009) after one year and recovering to 0.002 (0.014) thereafter. The number of disciplinary incidents initially increases by 0.043 (0.418) and then decreases by -0.380 (0.474) over one to three or more years following closure.

on behavioral issues. In terms of heterogeneity, Steinberg and MacDonald (2019) find that the negative behavioral impacts are larger for students who travel longer distances, which is consistent with my findings. Larsen (2020) is one of the few studies extending the analysis to longer-term educational outcomes, including high school graduation and college enrollment, while their sample is limited to high school students. They find a 7.5 percentage point decline in high school graduation rates and a 5.1 percentage point drop in college enrollment. While their estimate of the impact on high school graduation is similar to mine for 9th–12th grade students, the college enrollment estimates are significantly larger.

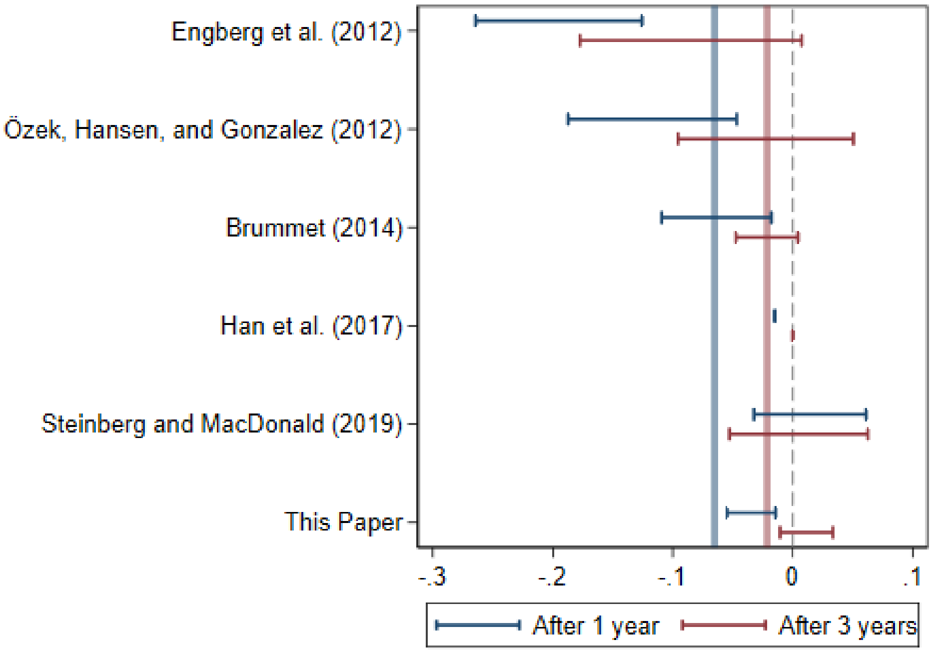
Some studies examine school closures in different contexts. Performance-based school closures have been implemented by state education agencies targeting charter schools, as well as by urban school districts—such as New York City—within the traditional public school system. Carlson and Lavertu (2016) focuses on charter school closures due to poor performance and finds that closures lead to increases in test scores. Bifulco and Schwegman (2020) evaluates middle school closures in New York City, where phase-out periods allowed students to voluntarily transfer before closures, within the context of an extensive school choice system. They find negative impacts on test scores and absenteeism for displaced students but also positive effects for the next generation of students. In a similar setting involving high school closures, Kemple (2015) finds mixed evidence on test scores and attendance, but a positive impact on high school graduation. Bross, Harris, and Liu (2023) examine the effects of performance-based school closures and subsequent reopenings and find mixed evidence.

Internationally, studies also report predominantly negative effects of school closures. Grau, Hojman, and Mizala (2018), using Chilean data, finds a significant increase in high school dropout rates by 1.8–2.5 percentage points (49–68%). Beuchert et al. (2018) documents declines in test scores in Denmark, while Taghizadeh (2020) finds no significant effects on displaced students in Sweden. Hannum, Liu, and Wang (2021) reports reduced grade completion rates in China.

Table C.1: Selected Studies Assessing the Impact of School Closures on Students

	Sample	Est. Stg.	Results
Similar Settings			
Torre and Gwynne (2009)	Chicago Public Schools	DiD	Decrease in test scores
Engberg et al. (2012)	An anonymous urban district	FE, IV	Decrease in test scores and increase in absences
Özek, Hansen, and Gonzalez (2012)	One School District in DC	FE, IV	Decrease in test scores
Brummet (2014)	Michigan	DiD	Decrease in test scores
Han et al. (2017)	Multi-State Low-Perform Schools	DiD	Decrease in test scores, but improvement when students move to higher-performing schools
Steinberg and MacDonald (2019)	School District of Philadelphia	DiD	Mixed results in test scores and increase in suspensions and absences
Larsen (2020)	Milwaukee Public School district	DiD	Decrease in GPA and increase in suspensions and absences Decrease in high school graduation and college enrollment
Not Similar Settings—Outside of US			
Grau, Hojman, and Mizala (2018)	Chile	IV	Increase in high school dropouts and grade retention
Beuchert et al. (2018)	Denmark	DiD	Decrease in test scores
Taghizadeh (2020)	Sweden	DiD	No effect on test scores
Hannum, Liu, and Wang (2021)	China	DiD	Decrease in grade completion
Not Similar Settings—Performance-Based Closures			
Kemple (2015)	New York City	DiD	Mixed evidence on test scores and attendance, and positive impact on high school graduation
Carlson and Lavertu (2016)	Ohio Charter Schools	RD	Increase in test scores
Bifulco and Schwegman (2020)	New York City	DiD	Decrease in test scores and increase in absences
Bross, Harris, and Liu (2023)	New Orleans	DiD	Increase in test scores and no clear effects on high school graduation or college entry One school shows significant decrease in high school graduation

Fig. C.1. Forest Plot: Estimates on School Closure Impacts on Test Scores



Notes: The figure presents estimates from studies examining the effects of school closures in similar settings on test scores. Each estimate reflects the 95% confidence interval of the impact after one and three years. For Özek, Hansen, and Gonzalez (2012), the estimates correspond to one and two years, and for Brummet (2014), Larsen (2020), and this paper, to one year and three or more years. Test score estimates are calculated as the average of math and reading scores $(\text{math} + \text{reading})/2$. Because Han et al. (2017) do not report standard errors, only point estimates are shown. Test score estimates from Torre and Gwynne (2009) are excluded, as their outcome is measured in months of learning, which cannot be standardized with the available information. Since Larsen (2020) uses GPA rather than test scores, the estimates are excluded. All underlying estimates are listed in footnotes C.1. The thick vertical lines represent inverse-variance weighted averages, excluding Han et al. (2017) and this paper's estimates: -0.069 (0.014) after one year and -0.023 (0.011) after three years. Specifically, the weighted average is calculated as $\hat{\beta}_{\text{avg}} = \frac{\sum w_i \hat{\beta}_i}{\sum w_i}$, where $w_i = \frac{1}{SE_i^2}$.

C.2 Long-run Effects of School Inputs and Intervention/Disruption

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 1.4 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 16 to 23 percent increase in class size for two years or a one standard deviation decrease in teacher quality for 1.7 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 3.5 percent decrease in earnings at ages 25-27, which is equivalent to a 0.36 standard deviation decrease in class quality for one year or a one standard deviation decrease in teacher quality for 2.6 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 26 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 assumptions for calculating lifetime earnings, my estimate suggests that a classroom of 25 students will experience a reduction of $456,750 in their present discounted value of lifetime earnings.[C.3] 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.[C.4] With approximately 250,000

[C.3] 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 $18,270*25=$456,750.

[C.4] 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 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 $7.8 billion. This estimation implies that the annual cost of school closures is approximately 1.3 times the cost of school shootings in the US.

reduction in the present discounted value of lifetime earnings per student is $31,110, calculated as $888,844 multiplied by the estimated effect size of 0.035.

D Reasons for Public School Closures in Texas 1998-2015

Table D.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
AUSTIN H S	PORT ARTHUR ISD	2002	✓		✓						2	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
BOGATA EL	RIVERCREST ISD	2001									0	0
BOOTH EL-NORTH	SAN BENITO CONS ISD	1999									0	0
BOWIE EL	WEATHERFORD ISD	2002	✓	✓							2	1
BOWIE SCH	MCALLEN ISD	2000									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
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
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
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
FREEMAN HEIGHTS EL	TEMPLE ISD	1998		✓							1	1
GLORIETA EL	ANDREWS ISD	1999									0	0
H O WHITEHURST EL	GROESBECK ISD	1999					✓				1	1
HAMBY EL	CLYDE CONS ISD	2003	✓								1	1
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
HUNT EL	LUBBOCK ISD	2001									0	0
J M LINDSAY EL	GAINESVILLE ISD	2000		✓							1	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
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
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
MEDINA VALLEY J H	MEDINA VALLEY ISD	2000									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
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
ROGERS EL	LAMESA ISD	1999		✓							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
SANCHEZ EL	LAREDO ISD	2001		✓							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
SKINNER EL	WEST OSO ISD	2000				✓					1	1
SOUTH EL	BROWNWOOD ISD	2002				✓					1	1
SOUTH WARD EL	BRADY ISD	1998		✓			✓				2	1
STATE SCHOOL	LUFKIN ISD	1998		✓			✓				2	1
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	WEATHERFORD ISD	2002	✓	✓							2	1
VALLEY VIEW EL	ABILENE ISD	2003									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
WESTLAWN INT	TEXARKANA ISD	2000									0	0
WHEATLEY EL	TEMPLE ISD	1998		✓							1	1
YOUTH OPPORTUNITY UNLIMITED	LAMAR CONSOLIDATED ISD	2002									0	0
Statistics			23	30	12	7	19	0	0	0	91	62

Table D.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
ALAMO EL	EL PASO ISD	2006		✓							1	1
ALLEN EL	HOUSTON ISD	2009	✓			✓					2	1
ARNETT EL	LUBBOCK ISD	2005									0	0
ATKINS J H	LUBBOCK ISD	2006									0	0
AUSTIN EL	WICHITA FALLS ISD	2008	✓	✓		✓					3	1
B F DARRELL EL SCHOOL	DALLAS ISD	2009									0	0
BELTON J H	BELTON ISD	2005									0	0
BILLY DADE EL	DALLAS ISD	2006									0	0
BINGMAN EL	BEAUMONT ISD	2009		✓							1	1
BLACKSHEAR EL	HEARNE ISD	2008									0	0
BLANCHETTE EL	BEAUMONT ISD	2009		✓							1	1
BONHAM EL	WICHITA FALLS ISD	2008	✓	✓		✓					3	1
BOWIE EL	LUBBOCK ISD	2006									0	0
BOWIE EL	MIDLAND ISD	2009									0	0
BROCK EL	HOUSTON ISD	2005	✓								1	1
BURLESON EL	EDGEWOOD ISD	2005									0	0
C W DAWSON EL	WHARTON ISD	2008									0	0
CARVAJAL EL	SAN ANTONIO ISD	2009		✓							1	1
CAVAZOS J H	LUBBOCK ISD	2006									0	0
CENTRAL MIDDLE	BROWNSVILLE ISD	2004									0	0
CHATHAM EL	HOUSTON ISD	2006	✓								1	1
CLEARWATER EL	BROWNSVILLE ISD	2004									0	0
CLINTON PARK EL	HOUSTON ISD	2005	✓								1	1
COLES EL	CORPUS CHRISTI ISD	2005	✓								1	1
COOPER MIDDLE	SAN ANTONIO ISD	2008	✓								1	1
DUNBAR J H	LUBBOCK ISD	2005									0	0
DUNCANVILLE 9TH GR SCH	DUNCANVILLE ISD	2005									0	0
EAST HOUSTON INT	NORTH FOREST ISD	2005									0	0
EASTER EL	HOUSTON ISD	2006	✓								1	1
EMMA FREY EL	EDGEWOOD ISD	2005									0	0
EVANS J H	LUBBOCK ISD	2006									0	0
FAIRCHILD EL	HOUSTON ISD	2007	✓		✓						2	1
FAIRWAY MIDDLE SCHOOL	KILLEEN ISD	2009	✓			✓					2	1
FANNIN EL	WICHITA FALLS ISD	2008	✓	✓		✓					3	1
FRANKLIN EL	PORT ARTHUR ISD	2007									0	0
H K WILLIAMS EL	EDGEWOOD ISD	2005	✓								1	1
HARDWICK EL	LUBBOCK ISD	2006									0	0
HAYNES EL	KILLEEN ISD	2006	✓								1	1
HOELSCHER EL	EDGEWOOD ISD	2005	✓								1	1
HOHL EL	HOUSTON ISD	2009	✓								1	1
HOLDEN EL	HOUSTON ISD	2004	✓								1	1
HOLLIE PARSONS EL	COPPERAS COVE ISD	2007									0	0
HUEY EL	WICHITA FALLS ISD	2008	✓	✓		✓					3	1
HUNT EL	CUERO ISD	2006									0	0
HUTCHINSON J H	LUBBOCK ISD	2006									0	0
J L WILLIAMS EL	COPPERAS COVE ISD	2007									0	0
J LESLIE PATTON INT	DALLAS ISD	2006									0	0
JACKSON EL	LUBBOCK ISD	2006									0	0
JAMES BOWIE EL	SAN ANTONIO ISD	2008	✓								1	1
JEFFERSON MIDDLE	ABILENE ISD	2004									0	0
JONES ANSON EL	HOUSTON ISD	2006	✓								1	1
JONES J WILL EL	HOUSTON ISD	2009	✓			✓					2	1
KEAHEY INT	NORTH FOREST ISD	2005									0	0
LAMAR INT	SINTON ISD	2008									0	0
LANCASTER INT	LANCASTER ISD	2006	✓	✓							2	1
LEE EL	COPPELL ISD	2008			✓						1	1
LEON R GRAHAM EL	MERCEDES ISD	2004									0	0
LIPAN H S	LIPAN ISD	2004									0	0
LUBBOCK-COOPER INT	LUBBOCK-COOPER ISD	2005									0	0
MACARTHUR EL	HOUSTON ISD	2009				✓					1	1
MACKENZIE J H	LUBBOCK ISD	2006									0	0
MAEDGEN EL	LUBBOCK ISD	2006									0	0
MARIETTA EL	MARIETTA ISD	2008							✓		1	1
MAYNARD JACKSON EL	DALLAS ISD	2006				✓					1	1
MCGAHA EL	WICHITA FALLS ISD	2008	✓	✓		✓					3	1
MCWHORTER EL	LUBBOCK ISD	2006									0	0
MEGARGEL SCHOOL	MEGARGEL ISD	2006	✓						✓		2	1
MILAM EL	HOUSTON ISD	2004	✓								1	1
MIRANDO EL	MIRANDO CITY ISD	2005							✓		1	1
MISS JEWELL EL	COPPERAS COVE ISD	2004									0	0
OAK VILLAGE MIDDLE	NORTH FOREST ISD	2009									0	0
OVERTON EL	LUBBOCK ISD	2006									0	0
PERRIN EL	SHERMAN ISD	2008	✓	✓	✓	✓					4	1
PORTER M S	AUSTIN ISD	2007	✓								1	1
PUMPHREY EL	GOOSE CREEK CISD	2009				✓					1	1
R C ANDREWS EL	FLOYDADA ISD	2008									0	0
R C FISHER CAMPUS	ATHENS ISD	2008			✓	✓					2	1
REAVES INT	CONROE ISD	2004									0	0
ROCHESTER SCHOOL	ROCHESTER COUNTY LINE ISD	2005									0	0
ROOSEVELT EL	EL PASO ISD	2006		✓							1	1
ROSEBUD INT	ROSEBUD-LOTT ISD	2007									0	0
SAN JACINTO EL	GALVESTON ISD	2006		✓							1	1
SANDERSON EL	HOUSTON ISD	2006	✓								1	1
SCHEH EL	HARLANDALE ISD	2008	✓								1	1
SLATON J H	LUBBOCK ISD	2006									0	0
SMILEY H S	NORTH FOREST ISD	2008	✓		✓						2	1
SMITH EL	LUBBOCK ISD	2006									0	0
SO SAN ANTONIO H S WEST	SOUTH SAN ANTONIO ISD	2008				✓					1	1

SPADE SCHOOL	SPADE ISD	2006	✓		✓				✓		3	1
TENAHA EL	TENAHA ISD	2007									0	0
THIGPEN EL	MCALLEN ISD	2007		✓							1	1
THOMAS JEFFERSON INT	BEEVILLE ISD	2007									0	0
TIDWELL EL	NORTH FOREST ISD	2008	✓		✓						2	1
TRAVIS EL	SAN ANGELO ISD	2004	✓								1	1
TURNER EL	HOUSTON ISD	2009	✓			✓					2	1
TURNER MIDDLE	WAXAHACHIE ISD	2008		✓		✓					2	1
TYNAN EL	SAN ANTONIO ISD	2009		✓							1	1
W J KNOX EL	SAN ANTONIO ISD	2009		✓							1	1
WAINWRIGHT EL	EL PASO ISD	2006									0	0
WASHINGTON EL	PORT ARTHUR ISD	2009									0	0
WEBSTER INT	CLEAR CREEK ISD	2005	✓								1	1
WHARTON J H	WHARTON ISD	2008									0	0
WHITESIDE EL	LUBBOCK ISD	2006									0	0
WILL ROGERS EL	HOUSTON ISD	2006	✓								1	1
WILLIAMS EL	LUBBOCK ISD	2006									0	0
WILLIE AND WANDA CROSSLAND INTERM	GRANBURY ISD	2006									0	0
WILSON S J H	LUBBOCK ISD	2006									0	0
WM B TRAVIS EL	SAN ANTONIO ISD	2008	✓								1	1
WOLFFARTH EL	LUBBOCK ISD	2006									0	0
WOODSBORO J H	WOODSBORO ISD	2007	✓		✓						2	1
Statistics			38	17	8	16	0	0	4	0	83	56

Table D.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
A M AIKIN EL	NEW CANEY ISD	✓			✓					2	1	
ALAMO EL	WICHITA FALLS ISD	✓	✓		✓					3	1	
AMBER TERRACE EL	DESOTO ISD	✓		✓						2	1	
ARLINGTON PARK COMMUNITY LEARNING	DALLAS ISD	✓								1	1	
AUSTIN MIDDLE	BEAUMONT ISD	✓	✓	✓						3	1	
BARBERS HILL MIDDLE	BARBERS HILL ISD	✓	✓							2	1	
BARWISE J H	WICHITA FALLS ISD		✓			✓				2	1	
BELT LINE INT	CEDAR HILL ISD			✓						1	1	
BONHAM EL	GRAND PRAIRIE ISD		✓							1	1	
BREWER EL	SAN ANTONIO ISD									0	0	
CARNAHAN EL	PHARR-SAN JUAN-ALAMO ISD	✓								1	1	
CASA LINDA EL	CORPUS CHRISTI ISD									0	0	
CITY PARK EL	DALLAS ISD	✓								1	1	
CLARKSVILLE MIDDLE	CLARKSVILLE ISD									0	0	
COLLEGE HEIGHTS EL	ABILENE ISD	✓		✓						2	1	
COLLINSVILLE INT	COLLINSVILLE ISD									0	0	
CORONADO ESCOBAR EL	EDGEWOOD ISD			✓	✓					2	1	
CRAWFORD EL	HOUSTON ISD	✓								1	1	
CROCKETT EL	GRAND PRAIRIE ISD		✓							1	1	
CROCKETT EL	MCALLEN ISD	✓			✓					2	1	
CROCKETT MIDDLE	PARIS ISD					✓				1	1	
D A HULCY MIDDLE	DALLAS ISD	✓								1	1	
D U BUCKNER EL	PHARR-SAN JUAN-ALAMO ISD	✓								1	1	
DECATUR INT	DECATUR ISD									0	0	
DEWEYVILLE MIDDLE	DEWEYVILLE ISD			✓						1	1	
DIRKS-ANDERSON SCH	FT DAVIS ISD						✓			1	1	
DODSON EL	HOUSTON ISD	✓								1	1	
DUBLIN J H	DUBLIN ISD				✓					1	1	
E O SMITH EL	HOUSTON ISD	✓								1	1	
EAGLE EL	CULBERSON COUNTY - ALLAMOORE ISD			✓						1	1	
EAST SIDE EL	SAN FELIPE DEL RIO CISD									0	0	
ELECTRA J H	ELECTRA ISD	✓		✓						2	1	
ESTACADO J H	PLAINVIEW ISD									0	0	
FANNIN EL	GRAND PRAIRIE ISD		✓							1	1	
FANNIN EL	ABILENE ISD	✓		✓						2	1	
FEHL EL	BEAUMONT ISD	✓	✓	✓						3	1	
FIELD EL	BEAUMONT ISD	✓	✓	✓						3	1	
FOWLER EL	KILLEEN ISD			✓						1	1	
FRANKLIN EL	PHARR-SAN JUAN-ALAMO ISD	✓								1	1	
GOLDEN RULE EL	DENISON ISD			✓	✓					2	1	
GOLIAD INT	BIG SPRING ISD									0	0	
GORDON EL	HOUSTON ISD	✓		✓						2	1	
GRIMES EL	HOUSTON ISD	✓		✓						2	1	
HAMLIN MIDDLE	HAMLIN ISD									0	0	
HART EL	HART ISD									0	0	
HIGHLANDS EL	LA MARQUE ISD							✓	✓	2	1	
HOUSTON EL	EL PASO ISD				✓					1	1	
HOUSTON EL	WICHITA FALLS ISD	✓	✓		✓					3	1	
HUTCHESON J H	ARLINGTON ISD	✓			✓					2	1	
INTER-CITY EL	LA MARQUE ISD							✓	✓	2	1	
J H ROWE INT	JASPER ISD									0	0	
JOHNSON EL	GRAND PRAIRIE ISD	✓	✓							2	1	
KENNEDY MIDDLE	GRAND PRAIRIE ISD		✓							1	1	
LA MARQUE MIDDLE	LA MARQUE ISD							✓	✓	2	1	
LAKE AIR INT	WACO ISD			✓						1	1	
LAMAR EL	CORPUS CHRISTI ISD	✓	✓		✓					3	1	
LAYNE EL	DENISON ISD			✓	✓					2	1	
LEE MIDDLE	GRAND PRAIRIE ISD		✓							1	1	
LEONEL TREVINO EL	PHARR-SAN JUAN-ALAMO ISD	✓								1	1	
LONE STAR EL	DAINGERFIELD-LONE STAR ISD			✓						1	1	
LUCAS EL	BEAUMONT ISD	✓	✓	✓						3	1	
MARFA EL	MARFA ISD									0	0	
MARTIN EL	BEAUMONT ISD	✓	✓	✓						3	1	
MARTIN EL	ROBSTOWN ISD									0	0	
MCALLISTER INT	BAY CITY ISD			✓						1	1	
MCDADE EL	HOUSTON ISD	✓		✓						2	1	
MEADOWBROOK EL	WACO ISD			✓						1	1	
MERIDITH DUNBAR EL	TEMPLE ISD				✓					1	1	
MORTON EL	MORTON ISD									0	0	
MORTON J H	MORTON ISD									0	0	
N W HARLLEE EL	DALLAS ISD	✓								1	1	
NAPPER EL	PHARR-SAN JUAN-ALAMO ISD	✓								1	1	
NELSON EL	SAN ANTONIO ISD	✓								1	1	
NORTH WACO EL	WACO ISD			✓						1	1	
OGDEN EL	BEAUMONT ISD	✓	✓	✓						3	1	
PEARCE MIDDLE	AUSTIN ISD								✓	1	1	
POINT COMFORT EL	CALHOUN COUNTY ISD			✓						1	1	
POWELL POINT EL	KENDLETON ISD								✓	1	1	
PREMONT J H	PREMONT ISD			✓					✓	2	1	
PRESCOTT EL	CORPUS CHRISTI ISD	✓	✓		✓					3	1	
PRICE EL	BEAUMONT ISD	✓	✓	✓						3	1	
RED OAK INT	RED OAK ISD				✓					1	1	
RHOADS EL	HOUSTON ISD	✓		✓						2	1	

RINGGOLD EL	GOLD BURG ISD									0	0
ROTAN J H	ROTAN ISD									0	0
RYAN MIDDLE	HOUSTON ISD	✓								1	1
SAM HOUSTON EL	GRAND PRAIRIE ISD	✓	✓							2	1
SAN AGUSTINE MIDDLE	SAN AGUSTINE ISD									0	0
SANDERSON EL	TERRELL COUNTY ISD									0	0
SCOTT EL	HOUSTON ISD	✓								1	1
SEAGRAVES EL	SEAGRAVES ISD									0	0
SEAGRAVES J H	SEAGRAVES ISD									0	0
SIMMS EL	LA MARQUE ISD							✓	✓	2	1
STAR SCHOOL	STAR ISD							✓		1	1
STEELE EL	SAN ANTONIO ISD	✓								1	1
STEVENSON EL	HOUSTON ISD	✓		✓						2	1
SUL ROSS EL	WACO ISD			✓						1	1
THREE RIVERS MIDDLE	THREE RIVERS ISD		✓							1	1
TRUMAN MIDDLE	EDGEWOOD ISD	✓		✓						2	1
UNITED D D HACHAR EL	UNITED ISD									0	0
UNIVERSITY MIDDLE	WACO ISD			✓						1	1
VAN HORN J H	CULBERSON COUNTY - ALLAMOORE ISD			✓						1	1
VIKING HILLS EL	WACO ISD			✓						1	1
VILAS EL	EL PASO ISD	✓			✓					2	1
W W WHITE EL	SAN ANTONIO ISD	✓								1	1
WEST INT	CEDAR HILL ISD									0	0
WESTLAWN EL	LA MARQUE ISD							✓	✓	2	1
WOODSON MIDDLE	HOUSTON ISD		✓			✓				2	1
WYNN SEALE ACADEMY OF FINE ARTS	CORPUS CHRISTI ISD									0	0
ZUNDELOWITZ MIDDE SCHOOL	WICHITA FALLS ISD	✓	✓		✓					3	1
Statistics		46	23	35	16	3	1	6	8	138	86

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