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

Jeonghyeok Kim*

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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. Using within-school across-time/cohort variation in exposure to school closures, I implement difference-in-differences strategies to identify causal effects. I find that school closures decrease test scores, increase absenteeism, and lead to more disciplinary actions. Furthermore, school closures decrease 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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*Jeonghyeok Kim: Department of Economics, University of Houston, Houston, Texas, USA (jkim124@uh.edu).
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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 displacing 180,000–320,000 students (NCES 2022). Among Texas students entering first grade in 2001, about 4% experienced a closure during their K–12 education. Despite their prevalence, school closure policy is contentious and often provokes backlash from parents and local communities (Griffin 2017; Mellon 2014; Rodriguez 2023). While some argue that closures are inevitable due to declining enrollment or budget constraints, district leaders often justify them by arguing that consolidation will ultimately benefit affected students and the district as a whole. The rationale is that displaced students and future cohorts will gain access to better-resourced schools, higher-achieving peers, and the advantages of economies of scale (Carlson and Lavertu 2016; Sunderman and Payne 2009). However, transferring to another school can result in significant environmental changes for displaced students (Chetty, Hendren, and Katz 2016), such as disruptions to learning, new school requirements and norms, and separations from friends. Thus, even when the policy is intended to benefit students, its actual impacts remain theoretically unclear.

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). In Appendix Figure C.1, I present a forest plot summarizing these findings, showing that school closures lead to an average decline in test scores of approximately 0.07 standard deviations, followed by a recovery to around 0.02 standard deviations after three years. Based on this recovery, some studies 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). 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 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 use 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 estimate the impact of school closures on both short-run outcomes (test scores and behavioral outcomes) and long-run outcomes (high school graduation, college attainment, employment, and earnings). A simple comparison of displaced and non-displaced students would not yield causal estimates, as both observed and unobserved factors influence which school students attend and their subsequent educational or labor market outcomes. To address this, I use two difference-in-differences strategies: one exploiting within-student variation over time for the short-run outcomes and another exploiting within-school variation across cohorts for the long-run outcomes. In both cases, I compare changes in outcomes for students in affected schools to those in matched control schools with 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 dataset, and must not be replaced by a substantially overlapping school at the same address in the following year. Using these criteria, I identify 323 school closures for my study. The predominant reasons for closures are tied to demographic shifts and financial constraints: more than 90% are broadly attributed to demographic challenges, and 3% are due to persistently low performance. Given that enrollment decline—particularly in areas experiencing population loss—remains an ongoing concern, the pressures that drove closures are unlikely to subside. Instead, similar demographic and fiscal challenges will continue to generate new waves of school closures, suggesting that the issue is not temporary but a persistent feature of the educational landscape (Hannum, Kim, and Wang 2022).

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 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, those on 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).

I use within-school across-cohort variation in exposure to school closures between closed and control schools to identify the effect on long-run outcomes, comparing younger cohorts who experience closures to older cohorts who do not. I find that by age 26, experiencing a school closure reduces high school graduation rates by 1.8 percentage points (2.7%), lowers college enrollment by 1.4 percentage points (2.8%), and decreases college quality based on expected earnings by $191 (0.9%). Furthermore, it reduces employment rates by 1.0 percentage points (1.9%) and lowers 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. Back-of-the-envelope calculations imply a $31,000 reduction in the present discounted value of lifetime earnings per student affected by a closure, corresponding to a total annual cost of approximately $7.8 billion ($31,000 $\times$ 250,000) across all displaced students in the US.

I investigate heterogeneity in the effect of school closures across student demographics and school characteristics. While the drop in test scores after closure recovers on average, students in secondary schools or those transferring 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 transferring 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 (Fleisher 2013; Hurdle 2013; Tieken and Auldridge-Reveles 2019), they also experience more significant negative effects.

I further explore the school-level changes for displaced students. Analyzing within-student variation before and after school closures, I find an immediate drop in peer quality measured by yearly test scores, with school average math and reading scores falling by 0.06 standard deviations. In contrast, expected school quality, as measured by the quality of the school *before* closure, shows the opposite pattern: displaced students experience increases in expected average test scores. In other words, while school districts appear to close relatively lower-performing schools, the actual peer environment for displaced students worsens, potentially due to disruptions associated with the closure process.

This study contributes to three strands of literature: school closures, student mobility, and the long-run effects of childhood disruptions. I advance the literature on the effects of school closures 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 extend the analysis of their 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

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

involves examining differences across various factors, such as urban and rural areas, original school quality, school quality changes, and student grades and demographics. 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 the exception of Brummet (2014), who uses Michigan data. In my 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 effects are more pronounced for specific groups of students and types of schools.

Second, this study 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 show a decline in test scores for students who change schools (Hanushek, Kain, and Rivkin 2004; Schwartz, Stiefel, and Cordes 2017; Xu, Hannaway, and D'Souza 2009). To identify the causal effect of student mobility, researchers often rely on instruments such as school grade span (Rockoff and Lockwood 2010; Schwartz, Stiefel, and Cordes 2017; Schwerdt and West 2013), as student mobility is often associated with family issues or changes in residency. In contrast, 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, it sheds light on the potential long-term consequences of student mobility on behavioral issues, post-secondary education, and labor market outcomes. The 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). Its findings once again emphasize the significance of childhood experience by showing that a policy intervention could be a negative shock in childhood. It also 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 the estimation results. 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, I use 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 TEA's closure list, disappear from my dataset, and not be 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 1998–2003 (all school levels), 2004–2007 (middle and high schools), and 2008–2010 (high schools) for the long-run analysis. 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. 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 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), showing 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. Black and Hispanic students constitute 74% of those affected by closures, while they make up 58% of all students. Economically disadvantaged students, including those receiving free or reduced-price lunch and other forms of aid, account for 75% of students affected by closures, compared to 56% of all students. As discussed in 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 these drivers, I identify and document the reasons behind 204 of the 323 closures. My primary sources include local news articles, interviews with school district personnel, and documents from school board meetings. To the best of my knowledge, this is the first attempt to construct statewide statistics on reasons for closures (see Appendix D for a full list of categorized reasons with closed schools). It is important to note that closure decisions often stem from a combination of factors. For instance, a decline in enrollment is frequently accompanied by budgetary constraints and aging school facilities. Other aspects may also be considered during the decision-making process, even if they are not reported as the main drivers.[2]

To facilitate an understanding of the reasons for closures, in Appendix Figure A.2 I categorize identified reasons into several distinct groups: chronically low performance, financial constraints, enrollment changes, aging school infrastructure, district-level renovation (including closures and rezoning), school reform, and coding changes. These categories are not mutually exclusive; a single school closure may be attributed to multiple reasons. While previous literature emphasizes closures due to low performance (e.g., Delpier 2021; Dowdall 2011; Jack and Sludden 2013; Tieken and Auldridge-Reveles 2019), my 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% of the identified reasons for closures, whereas closures primarily associated with low

[2] For example, consider the case of Dodson Elementary School in the 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, Dodson also performed worse on some measures of academic standards. This example 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.

performance constitute only 3%.[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 closures nationwide. These parallels reinforce the generalizability of the patterns observed in Texas and 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).

Among the categories in Appendix Figure A.2, *low performance* refers mostly to closures initiated by the education agency in response to chronic underperformance in schools. Closures in the *financial constraint* category often cite decreasing enrollment or statewide budget cuts as significant factors, creating sustainability challenges for school districts. Closures categorized as *district reform* are frequently associated with shifts in youth population distribution across regions, prompting the need for school closures, the construction of new schools, and rezoning attendance boundaries. *School reform* is a more ambiguous category. 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 the 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 as improving school accountability or administrative convenience.[5]

[3] I divide the causes into three periods to see whether there is a change in reasons over time. In all three periods, more than 85% of closures are broadly related to enrollment changes. In the first (1998–2003), second (2004–2009), and last period (2010–2015), I identify causes for 62 out of 103 closures, 56 out of 110 closures, and 86 out of 110 closures.

[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 sub-group 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.

To address potential concerns that coding changes or non-physical school closures might drive the results, I exclude in the baseline estimation any closures where more than 30% of displaced students are observed at the same address as the closed school after the closure. 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 this same-address restriction, implying that most coding changes are eliminated from the analysis sample. Appendices B.2 and B.3 present results using alternative cutoffs. The findings remain consistent across different thresholds, while the estimated negative impacts tend to be slightly larger for certain outcomes when a stricter cutoff is used.

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).

The TEA data include K–12 education records in public schools starting in the 1994–1995 academic year, with information on attendance, disciplinary actions, high school graduation, and testing. They also include student characteristics (e.g., age, sex, race/ethnicity, English as a second language status, special education status, and eligibility for free or reduced-price lunch) and campus and district information (e.g., school type and charter type). Using these data, I construct four outcome variables: (1) the number of days of absence, (2) the number of disciplinary action days,[6] (3) standardized math and reading scores,[7] and (4) high school graduation.[8]

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

[7] Test scores are standardized by grade and year. During the period of my analysis, different standardized tests were used in Texas, which were administered to different groups. The Texas Assessment of Academic Skills (TAAS) was used for grades 3–8 until 2002, the Texas Assessment of Knowledge and Skills (TAKS) for grades 3–11 from 2003 to 2011, and the State of Texas Assessments of Academic Readiness (STAAR) for grades 3–8 beginning in 2012. High school students in 2011 continued to take the TAKS test. To ensure at least a three-year pre-closure and a two-year post-closure observation window, I consider students at the time of closure in the following grade configurations: grades 5–6 from schools closed in 1998–2000, grades 5–7 in 2001, grades 5–8 in 2002, grades 5–9 in 2003–2009, grades 5–6 and 8–9 in 2010, grades 5–6 and 9 in 2011, and grades 5–6 in 2012–2015.

[8] 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% graduation rate, while others show rates between 50% and 70%. I find

The THECB data include all public and most private post-secondary education data in Texas[9] and are linked to the TEA data at the individual level. I construct two post-secondary education outcome variables using these data: (1) an indicator for ever attending a Texas college by age 26, and (2) an indicator for earning a bachelor's degree from a Texas post-secondary institution by age 26.[10]

The TWC data include quarterly individual-level information on employment, industry, and earnings for all workers covered by the Unemployment Insurance program[11] and are linked to the TEA and THECB data at the individual level. Using the TWC data, I construct three outcome variables for ages 25–27: (1) the share of quarters employed, (2) average annual real earnings, and (3) earnings-based college quality following Chetty, Friedman, and Rockoff (2014).[12] 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 leaves Texas, I cannot observe their out-of-state educational or workforce outcomes and thus cannot distinguish whether they have moved out of state or did not attend college (in the case of education) or are non-employed (in the case of labor market outcomes). However, this is unlikely to significantly bias the results because Texas has relatively low out-migration (Foote and Stange 2022). I discuss this more in Section 5.2 and Appendix B.4.

no notable differences between these cohorts, including in average grade 12 attendance. To address concerns about excluding these schools, I also construct a proxy for high school graduation based on grade 12 attendance. The two measures—graduation based on TEA files and grade 12 attendance—are highly correlated (0.83) and yield similar estimates.

9 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.

10 Apart from the data provided by the THECB, I also have access to data from the National Student Clearinghouse (NSC), which cover 98% of higher education enrollment in the United States since 2008. These data allow me to comprehensively observe students enrolling in post-secondary institutions both in and out of Texas after 2008. However, since the coverage period is limited relative to the analysis period, I do not use them in the main analysis. Instead, I use them to demonstrate that out-of-state attrition does not meaningfully affect the estimates (see Appendix B.4).

11 Unemployment Insurance covers workers if employers pay $1,500 or more in a calendar quarter or have at least one employee during 20 different weeks in a calendar year. Thus, the TWC data do not include earnings from independent contract work, self-employment, under-the-table payments, earnings from federal jobs, and earnings outside of Texas. For more details, see https://www.twc.texas.gov/tax-law-manual-chapter-3-employer-0.

12 Using 1982–1984 birth cohorts, I group individuals by the higher education institution they graduated from 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.

4 Empirical Strategy

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

For the difference-in-differences estimator to provide a consistent estimate of the average treatment effect on the treated for students enrolled in schools that close, the parallel trends assumption must hold: in the absence of closures, outcomes would have evolved similarly for students in closing schools and in control schools (over time in the short-run analysis; across cohorts in the long-run analysis). 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 closed schools 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 to 2005 and 12 categories from 2006 to 2015, based on population size and proximity to populous areas.[13] 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

[13] The 8 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 an urban area. 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.

students receiving free or reduced-price lunch, and the share of students with other economic disadvantages.[14] 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 selecting matched control schools, I exclude those in the same district due to concerns about spillover effects as well as schools that were closed during the analysis period.

I choose one control school for each closed school without replacement. Appendix Table A.1 presents summary statistics after the matching process. As expected with nearest-neighbor matching, the observable characteristics of closed schools are similar to those of matched control schools. Black and Hispanic students comprise 74% and 72% of closed and control schools, respectively, compared to 58% of all schools. Economically disadvantaged students account for 74% and 73% of closed and control schools, respectively, compared to 56% of all schools. Moreover, Appendix Figure A.3 presents the distribution of the number of schools attended during K–12 education, separately for students in closed schools and control schools. The majority of students in closed schools experience one additional transfer compared to both control school students and the state average, supporting the validity of the empirical design. As discussed in Appendix B.1, the estimates are not sensitive to the alternation of matching strategies.

4.2 Estimating the Short-Run Effects of School Closures

I analyze outcome variables observed both before and after closures: 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 discussed in Section 3, the available sample varies across outcome variables and years of closures. I further restrict the sample to students who are observed in the data for at least three years before and two years after school closures. The coverage spans grades 3–10 for behavior and grades 5–9 for test scores from 323 schools. I use all available students in each outcome variable. The final short-run analysis sample includes 31,557 students for test scores, 57,293 students for disciplinary action,

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

and 69,215 students for attendance.

I use 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 it) who was enrolled in school s in match group g at the time of closure. $Closure_s$ is a dummy variable equal to 1 if the student i is at a closed school at the time of closure, and $Post_t$ is an indicator denoting observations after closure. I include individual fixed effects, σ_i, and a full set of matched group-by-relative year fixed effects, κ_{gt}. These account for time-invariant individual characteristics and match-group-specific trends, respectively. β is a 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).

To causally interpret the estimator, 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 closures. To assess the validity of this assumption, I compare the trend before the closures 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 school closures while controlling for secular trends by using the matched control group.

The regression equation takes the following form:

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

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 as in equation (1). β_t are 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, β_t where $t \in \{-3, -2\}$ show pre-trends between closed and matched control schools; 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 difference-in-differences analysis, I examine a balanced panel of students spanning three years before and four years after school closures. In other words, I apply two different balancing conventions for equations (1) and (2) in the short-run analysis. This is due to a trade-off inherent in balancing over a longer period: extending the balanced panel helps mitigate potential biases from compositional changes, such as students leaving the Texas public school system for private schools or moving out of state after closures. However, such balancing limits the sample primarily to younger-grade students who can be observed throughout the entire period. To address this, I use a fully balanced panel for the event study figures based on equation (2) to capture the full dynamics of the closure effects. For the estimates from equation (1), I allow the panel to become unbalanced after three or four years post-closure to retain a more comprehensive set of displaced students. Appendix Figure B.5 and Table B.1 present estimates under both balancing conventions and show that the results are qualitatively and quantitatively consistent across specifications.

To address concerns about potential attrition bias, I examine whether students from closed schools are more likely to leave the Texas public school system than those from control schools. Appendix Figure A.4 shows that similar proportions of students in closed and control schools remain in the data before and after school closures. Specifically, I estimate equation (2) using, as the dependent variable, a binary indicator for whether a student appears in the data in a given year, and find that attrition rates are statistically similar across all years except $t = -3$, with differences never exceeding 0.5 percentage points. This result alleviates concerns about systematic differences in attrition between the two groups. Another potential concern is differential attrition before school closures, which could affect the sample composition. For example, some students may leave in anticipation of the closure. In this case, my estimation may not capture all displaced students. However, this does not necessarily imply bias. Although early leavers may be selective, the analysis relies on within-student variation. As long as students in closed and control schools would have followed similar trends in the absence of closures, differences in their average characteristics should not threaten the validity of the results.

4.3 Estimating the Long-Run Effects of School Closures

The long-run analysis focuses on outcomes only observed after school closures 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 closures, I cannot exploit within-student variation as it relates to changes before and after closures. Instead, I use variation across cohorts within a school. Specifically, I compare cohorts enrolled in the school at the time of closure with cohorts who recently graduated relative to those at matched control schools.

I construct a long-run analysis sample based on graduating cohorts using 130 closed schools between 1998 and 2008. I use six cohorts: the three highest grades experiencing a school closure become three "younger cohorts," and three cohorts who potentially graduated within the last three years of a school closure become three "older cohorts." For instance, suppose that elementary school **A** with grades 1–5 closed at the end of the 2000 school year. I consider students in school **A** in grades 3–5 at the time of closure as younger cohorts and those in the same school in grades 3–5 three years before the closure as older cohorts. Thus, older cohorts would be expected to be enrolled in grades 6–8 in the year of closure. The final long-run sample experiencing school closures includes 42,447 students in grades 2–12.

Using this sample to estimate difference-in-differences models, I compare changes in outcomes across cohorts following a school closure between the closed schools and their matched control schools. The 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, English as a second language status, and special education status. If student characteristics are not observed, I assign an additional missing category. Moreover, I control for pre-closure performance measures, including standardized math scores, reading scores, and days of absence before school closures (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 for. γ is the difference-in-differences estimator, measuring the difference in the change in outcomes across cohorts following a closure between students from closed and matched control schools.

As with the short-run effects, to ensure that my causal interpretation is valid, I make the standard parallel trends assumption. Essentially, I assume that within each match group and conditional on observed covariates, graduating cohorts from closed and control schools would have experienced similar changes in outcomes in the absence of closures. To assess the validity of this 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:

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

where cohort $c \in \{-3, -2, \ldots, 2\}$ is measured relative to the time of closure, and $\mathbf{1}_c$ is equal to 1 when the relative cohort is c. If $c \in \{0, 1, 2\}$, students are in the younger cohorts (enrolled at the time of closure; in the previous example of school **A** with grades 1–5, $c = 0$, $c = 1$, and $c = 2$ correspond to grades 5, 4, and 3, respectively), whereas if $c \in \{-3, -2, -1\}$, students are in the older cohorts (graduated before the school closed; in the example, $c = -3$, $c = -2$, and $c = -1$

correspond to grades 8, 7, and 6, respectively). γ_c are the difference-in-differences coefficients, measuring differences between closed and control schools in cohort c relative to the omitted cohort.[15] The standard errors are clustered at the school level. As in the short-run analysis, I use a balanced panel of schools with at least three grade levels to estimate equation (4), while equation (3) is estimated using the full sample. Appendix Figure B.9 and Appendix Table B.4 compare the results using balanced and unbalanced panels and show consistent findings across different balancing conventions.

In the long-run event-study format difference-in-differences analysis, I examine six adjacent cohorts in the same school around school closures with extensive controls for student characteristics, 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, which could create differences across cohorts even in the absence of closure. To assuage this concern, I conduct a balance test across these cohorts. Specifically, I use demographic characteristics (including economic status and racial composition) and performance measures (including standardized test scores and days of absence) measured before school closures as dependent variables to estimate equation (4). Appendix Figure 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.[16] Moreover, between closed and matched control schools, I find no significant differences in either the proportion of students transferring to another school prior to closure or the average test scores of those transferring students (Appendix Figure A.7).

[15] If two grades exist at the time of closure, the highest and second highest grades at the time of closure are set to 0 and 1 of c, and the highest and second highest grades two years before the closure are set to -2 and -1 of c. Thus, the regression is not balanced when $c = 2$ or $c = -3$.

[16] 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 examine changes in short-run outcomes for younger cohorts after school closures compared to older cohorts. As shown 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 the baseline short-run analysis.

5 Estimation Results

5.1 Short-Run Effects on Student Outcomes

Figure 2 presents event study estimates from equation (2), plotting the coefficients and 95% confidence intervals for β_t (see Appendix Figure A.8 for raw trends of short-run outcomes). 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 one advancement over previous literature, which often struggled with violations of the parallel trends assumption due to difficulties in constructing comparable comparison groups. The figure also depicts a decline in standardized math and reading scores (standardized by grade and year) following school closure. These scores subsequently recover to their initial levels within three years. Moreover, the figure presents an immediate increase in days of absence after closures, which persists for four years. The number of disciplinary action days also immediately increases after closures, with this effect continuing 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).

Table 1 reports estimates from equation (1), in which periods after school closures 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) show that the days of absence and disciplinary action increase after two years by 0.05 days and 0.49 days, which is a 0.7% and 23% increase relative to the pre-closure means. Days of disciplinary action further increase after three to four years up to 0.78 days.[17]

I next explore heterogeneous effects across school and student characteristics. For school

[17] The pre-closure mean reported in the table differs from that in the event-study figure because they are based on different sample definitions, as explained in Section 4.2. For example, in the case of test scores, Figure 2 relies on a balanced panel, which excludes data from later closure years due to the 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 explains why the pre-closure mean in the table is lower than that in the figure.

characteristics, I estimate equation (2) separately for sub-groups defined by characteristics such as region, school quality, and school quality change. The region is divided into urban and rural categories 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. Following Brummet (2014), I do not use the quality of the school that students actually attend after closure to avoid bias due to selective school choice. The correlation between the nearest school and the actual post-closure school attended is 0.45. Based on the distribution of these differences, I divide school quality changes into terciles: worse, similar, and better.[18] 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 one to two years and three to four years after school closure. Although there is considerable overlap in the confidence intervals across the estimates, a few patterns are noteworthy. First, the overall effect is negative, suggesting that school closures negatively affect 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. Last, students displaced to worse-performing schools experience a larger drop in test scores, while those displaced to better-performing schools experience a larger increase in days of disciplinary action. I further examine heterogeneity based on the proportion of displaced students and the distance between closed and receiving schools in Appendix Figures A.10 and A.11. The median transfer distance is approximately one mile, and about half of displaced students transfer to the same receiving school. Overall, I find that students experience greater disruption when they transfer with fewer peers or over longer distances.

To analyze the heterogeneous impact of school closures based on individual characteristics, I

[18] 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 those between 0.19 and 2.67 standard deviations are classified as better.

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 one to two years and three to four years. Despite the noise in the point estimates, I take them at face value and note several patterns. First, 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). White students, by contrast, experience a greater drop in reading scores, which is not fully recovered in four years. These disparities across racial/ethnic groups highlight that each group is affected to varying degrees across outcomes. Second, economically disadvantaged students have more significant increases in days of absence and disciplinary action. Last, negative effects on test scores grow over time for students who were in higher grades at the time of closure, while those in lower grades appear to recover over time.

I next 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 constructing peer quality measures, I exclude displaced students after experiencing school closures (i.e., $t \geq 0$) and students who transfer to schools where more than 70% of students are displaced (the results are robust to alternative cutoffs; see Appendix Figure A.12).

Panels (a) and (b) of Figure 5 illustrate the changes in peer quality, showing a decrease of 0.06 in math and reading scores after closures, followed by a recovery over time.[19] However, the expected peer quality shows a different pattern. I construct expected quality measures using average math and reading test scores of each school over the four years *preceding* the school closure (i.e., $t \in \{-4, \ldots, -1\}$) and use them as dependent variables to estimate equation (2).

[19] 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: $(0.45 \times 0.029 - 0.43 \times 0.0013 + 0.10 \times 0.0088) \times -0.067 = -0.0009$ standard deviations for math and $(0.45 \times 0.0271 + 0.43 \times 0.0087 + 0.10 \times 0.0124) \times -0.058 = -0.001$ standard deviations for reading.

For comparison, the same student sample used in the peer-quality analysis is included. As shown in panels (c) and (d), students transfer to schools that originally served better-performing peers on average compared to students from closed schools. After transferring, the 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). Moreover, I explore other outcomes and find consistent results: an increase in days of absence and disciplinary action among peers (Appendix Figure A.13) and a decrease in the number of staff per student following school closures (Appendix Figure A.14).

5.2 Long-Run Effects on Educational and Economic Outcomes

Figure 6 presents estimates of the effects of school closures on long-run educational outcomes by age 26 and labor market outcomes at age 25–27. It includes the 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 who experience a school closure, I find overall negative effects on high school graduation, post-secondary education, and labor market outcomes. However, I find no significant impacts on four-year college completion and enrollment (see Appendix Figure A.15 for four-year college enrollment results).

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 closures in both educational and labor market outcomes (see Appendix Table A.3, which separates younger cohorts into those in the highest grade and those in lower grades, showing mostly insignificant effects for the former). This pattern likely reflects that students in terminal grades would have graduated regardless of the closures and therefore experienced less disruption than their younger peers. However, they may still experience negative effects due to the challenges of integrating into new environments if this occurs as part of a district reform, or due to negative impacts on staff morale or turnover in the year leading up to closures. Robustness checks with alternative samples and control variables, presented in Appendix B.3, confirm that the main findings are consistent.

Table 2 reports estimates 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 a school closure decreases the likelihood of graduating from high school by 1.8 percentage points (2.7%),[20] lowers enrollment in any college by 1.4 percentage points (2.8%), and decreases college quality by $191 (0.9%) by the age of 26. I find no significant effects on obtaining a bachelor's degree. Additionally, I find that experiencing a 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 next explore heterogeneous effects across school and student characteristics for long-run 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.[21] First, I find broadly similar effects between urban and rural school closures, except that employment is more negatively affected in urban areas.

[20] As noted in Section 3, I exclude two closed schools from the high school graduation analysis due to a potential data issue. To address concerns about this issue, I conduct a robustness check using a proxy for high school graduation based on grade 12 enrollment. Appendix Figure A.16 shows that the estimates remain consistent regardless of whether the original graduation measure or the proxy is used.

[21] I find overall negative long-run effects, though short-run analyses show recovery from negative impacts and even positive outcomes for some groups. This discrepancy arises because the short-run sample is more limited than 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 (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.

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 those transferring to worse-performing schools experience a significant drop in yearly earnings. This is consistent with the 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 who transfer to better-performing schools, which may imply that adapting to better-quality schools is more difficult for some students. This suggests that even when students transfer to schools with higher-performing peers, some may 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 closures, while those in grades 3–5 overall do not experience significant long-run negative effects. This finding aligns closely with that of Chetty, Hendren, and Katz (2016), who show 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 become more pronounced in the comparison after rescaling based on sub-group means in Appendix Figures A.19 and A.20. Consistent with the short-run heterogeneity analysis, the results indicate that the negative effects are more pronounced for students in higher grades and those in more vulnerable situations, such as students from originally low-performing schools or economically disadvantaged families.

As 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 the estimation is complicated. However, based on the following evidence, I argue that differential attrition is unlikely to meaningfully change the estimation results. In Appendix B.4, I discuss this issue in three layers: (i) I find no evidence of a significant difference in attrition rates immediately after school closures; (ii) I do not find an increase in out-of-Texas post-secondary education attainment after school closures; and (iii) I obtain results consistent with the 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 the findings and compare the estimates to previous research. First, I find 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 transfer to neighboring schools, substantial differences in school quality are unlikely. This is consistent with evidence showing similar average test scores between closed and receiving schools (see footnote 19 for a simple calculation of the impact of peer quality changes, which shows a negligible effect size). Additionally, student-teacher ratios remain relatively low at the school level (Appendix Figure A.14). The adverse effects are therefore not fully attributable to school quality differences but instead reflect 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—face larger negative impacts (Appendix Figures A.10, A.11, and A.21). Last, 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 Jackson et al. 2020, who emphasize the importance of emotional experiences in school for students' development).

While the disruption from school closures entails a complex set of changes that are difficult to isolate,[22] I find a persistent increase in behavioral issues among displaced students. A simple regression analysis suggests that the rise in disciplinary incidents in the long-run sample is correlated with a \$224 decrease in earnings and a \$92 decrease in college quality, corresponding

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

to 32% and 48% of the overall negative effects of school closures, respectively.[23] 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, even if they express this through behavioral issues. 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 may also play a significant role.[24]

Second, I compare my estimation results with previous studies on the impacts of school closures on students (see Appendix C.1 for more details). Studies in similar contexts find overall negative effects on test scores, absenteeism, and suspensions (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). 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), who 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 is

[23] I regress earnings at ages 25–27 (or college quality) on the number of days of disciplinary action 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 three to four 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).

[24] 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 and those from low-quality schools—also suffer greater negative impacts on long-run outcomes. Moreover, students who transfer 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. Finally, estimates 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.

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, which strengthens the external validity of my findings.

Last, 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 C.2 for more details). Chetty et al. (2011) find that a 1 standard deviation increase in class quality within schools, which incorporates peer quality, teacher quality, and random class-level shocks, increases earnings by 9.6% at age 27. Similarly, a 1 standard deviation improvement in teacher value-added for one year is associated with a 1.34% increase in earnings at age 28 (Chetty, Friedman, and Rockoff 2014). In comparison, my estimated effect of a school closure is a 3.5% 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 1 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 them 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 affected by school closures annually from 2010 to 2021 (NCES 2022), the total annual cost of closures, resulting from displaced students, amounts to about $7.8 billion. This implies that the annual cost of school closures is approximately 1.3 times the cost of school shootings in the US.

It is important to note that the estimated costs presented in this paper are not net costs, as I do not attempt to quantify the potential benefits of school closures. Rather than conducting a full cost-benefit analysis, my focus is on highlighting the often-overlooked costs borne by students directly affected by closures. While such closures may lead to financial savings or more efficient resource allocation, these benefits are more likely to accrue to students in unaffected schools or to future cohorts (Bifulco and Schwegman 2020). However, it is challenging to estimate these benefits without access to school-level budget information and feeder patterns, which are not available in my data. Given these data limitations, my analysis focuses on documenting the adverse consequences for students who directly experience school closures rather than weighing them against potential system-wide gains.

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% of its total (National Bureau of Statistics of China 2023), while France has closed 8,000 schools, accounting for 14% of its total (Ministry of National Education, Higher Education and Research 2023). In Brazil, rural primary schools have experienced a 31% reduction, dropping from 88,000 to 61,000 between 2007 and 2017 (Brazil Ministry of Education 2020). In Rajasthan, India, the government initiated the merger of 17,000 of its 80,000 government schools in 2014 (Chowdhury 2017). Despite the pervasive global use of school closure policies, evidence on their effects on students is limited, underscoring the need for research that quantifies the causal impact of school closure on both short- and long-run student outcomes (Tieken and Auldridge-Reveles 2019).

Using rich administrative data from Texas, I explore the effects of school closures on displaced students' short-run outcomes, including test scores and behavioral problems, and long-run outcomes, including post-secondary education and labor market outcomes. I analyze closures between 1998 and 2015 using difference-in-differences empirical strategies and find that school closures negatively affect displaced students both immediately and over a decade later, when they are young adults. Test scores decline and behavioral issues rise in the years following closure, and post-secondary education and labor market outcomes also worsen. Heterogeneity analysis shows that these 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 are sizable: the estimates 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. Back-of-the-envelope calculations further suggest that the annual cost of closures due to displaced students is about $7.8 billion in the US, without considering the potential benefits of the closures. These long-run impacts suggest that current school closure policies do not adequately address the disruptions experienced by displaced students. Despite the high 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 and policy efforts should explore strategies to mitigate these adverse effects.

Potential strategies include the following. First, schools could be phased out gradually rather than abruptly. This approach, implemented in New York City, where students could choose to transfer (Bifulco and Schwegman 2020), should be tested in broader contexts without school choice models. Second, strategies to reduce disruption could be implemented. For instance, districts could make efforts to keep peer groups together or 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, districts could offer 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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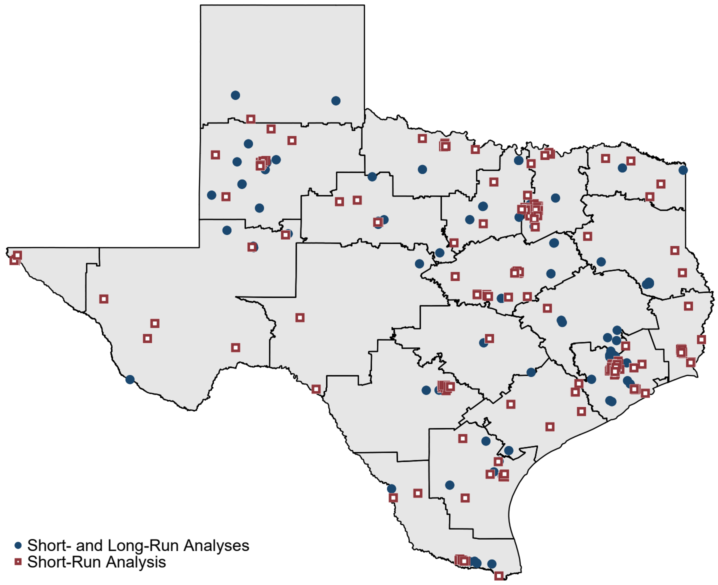
8 Figures and Tables

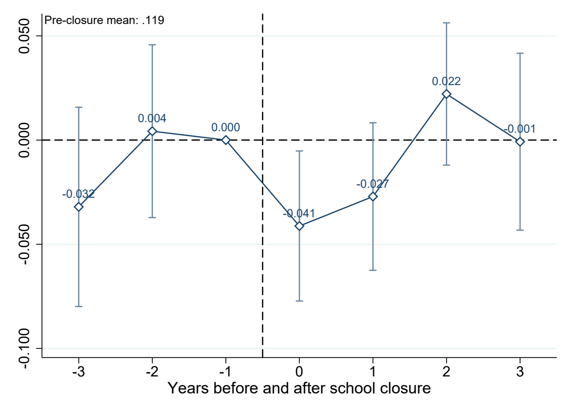
Fig. 1. Map of School Closures at Texas Public Schools in 1998–2015



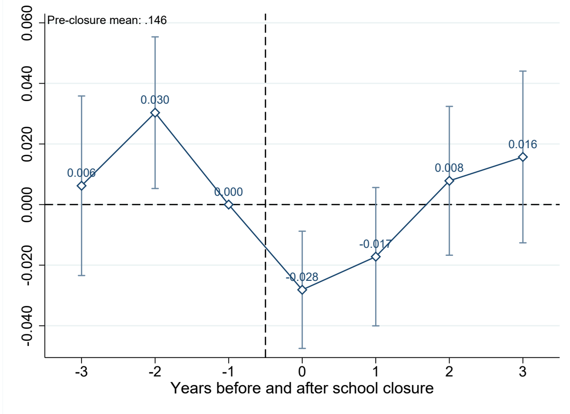
Notes: The figure presents the locations of public school closures between 1998–2015: 130 school closures used in both short- and long-run analysis and 193 school closures used in only short-run analysis. To be considered a closed school, the school must be officially listed as closed by the Texas Education Agency (TEA), be a non-charter instructional campus in a regular, independent district, have been observed during the previous period (1994–1997), and 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 Closures on Student Outcomes

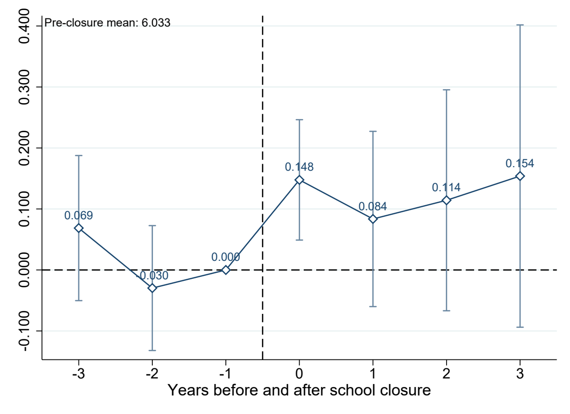
(a) Standardized math score



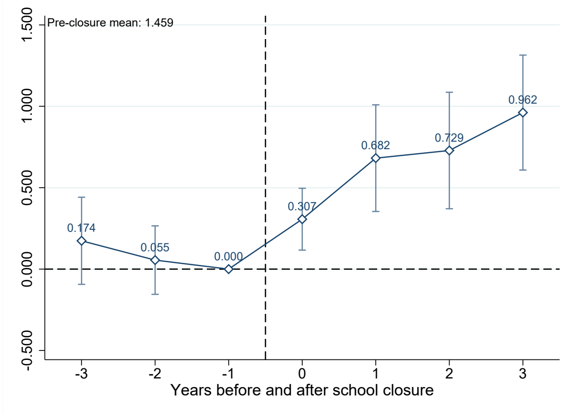
(b) Standardized reading score



(c) Days of absence



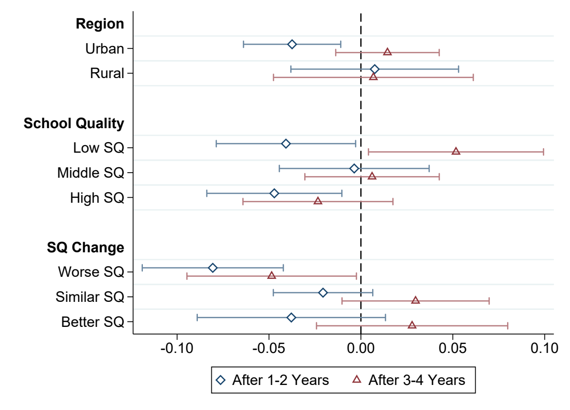
(d) Days of disciplinary action



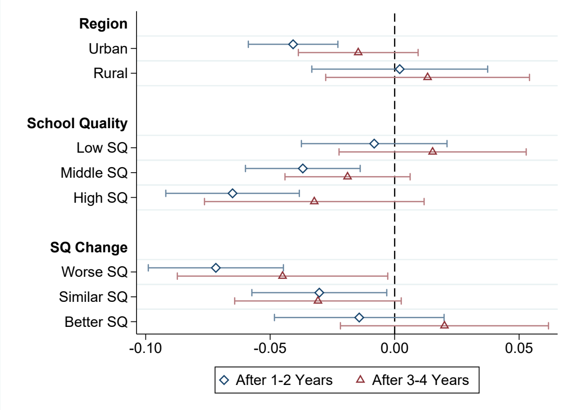
Notes: The figures present the coefficients, β_t, and 95% confidence intervals from equation (2).These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) 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 Closures on Student Outcomes: Heterogeneity by School Characteristics

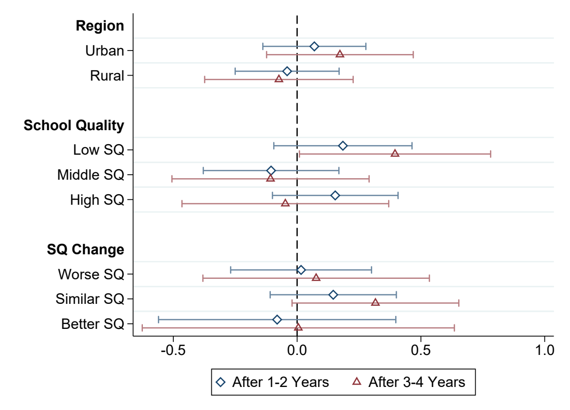
(a) Standardized math score



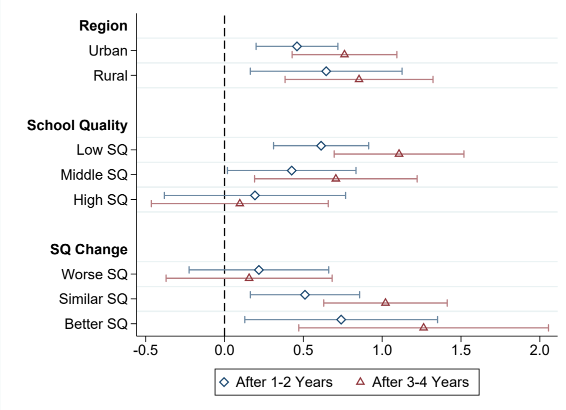
(b) Standardized reading score



(c) Days of absence

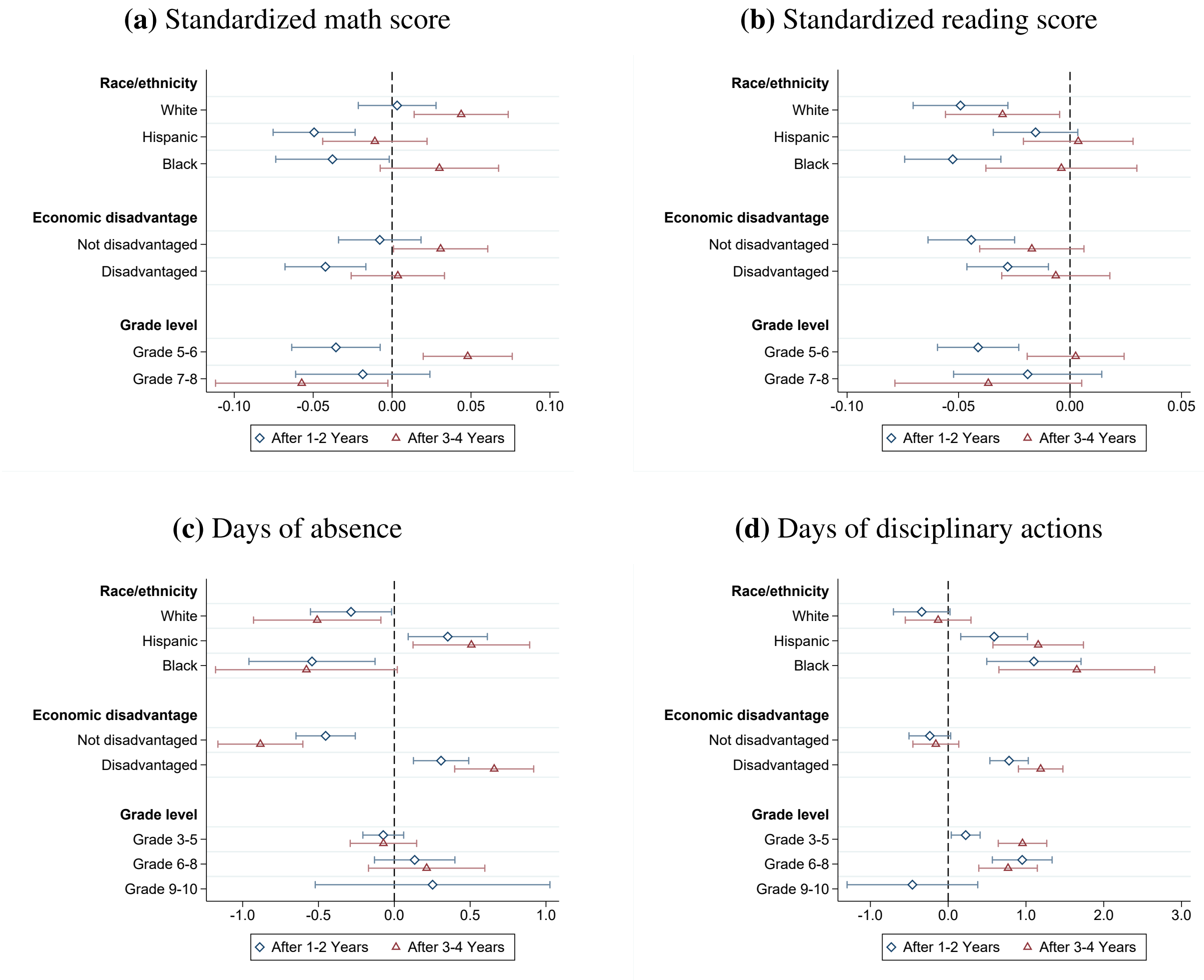


(d) Days of disciplinary actions



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 $t = -1$.

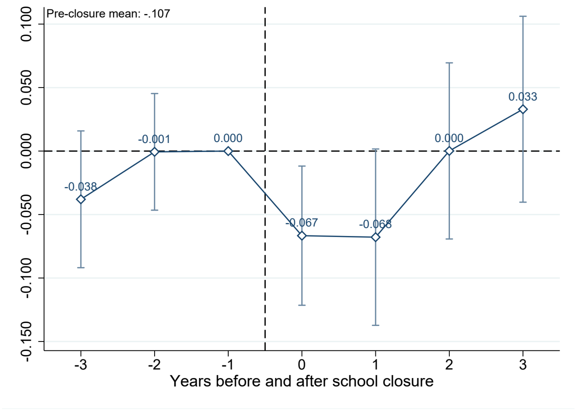
Fig. 4. Short-Run Effects of School Closures on Student Outcomes: Heterogeneity by Student Characteristics



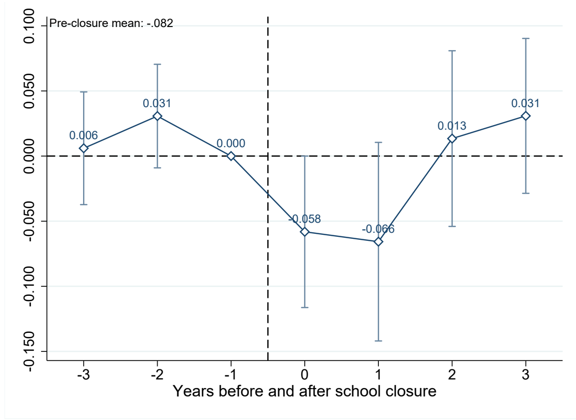
Notes: The figures present the coefficients, β, and 95% confidence intervals from equation (1) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote years after a school 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

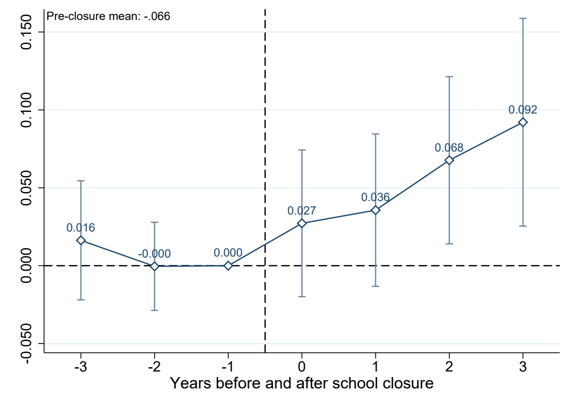
(a) Peer quality: standardized math score



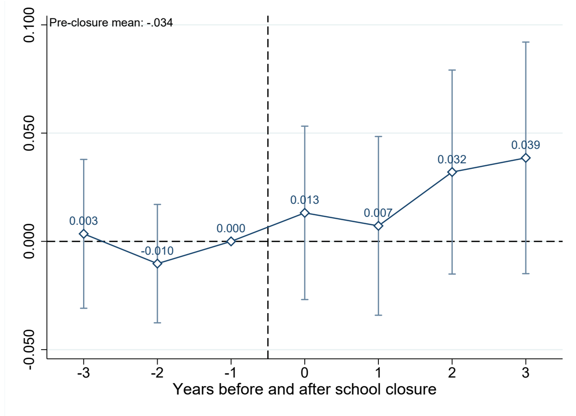
(b) Peer quality: standardized reading score



(c) Expected quality: standardized math score

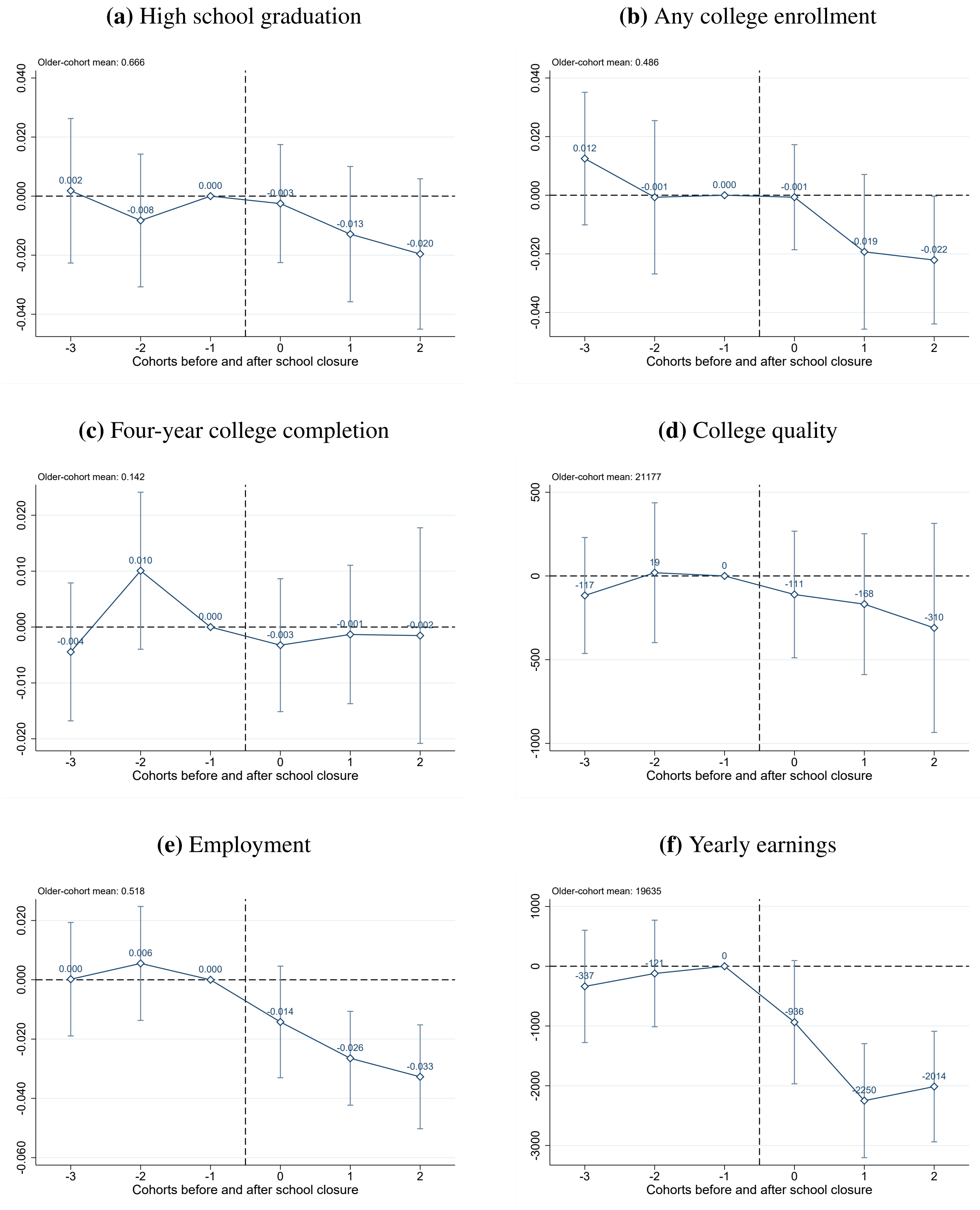


(d) Expected quality: standardized reading score



Notes: The figures present the coefficients, β_t, and 95% confidence intervals from equation (2), where the outcome variables are the school average test scores. When it comes to panels (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 panels (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 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 Closures on Educational and Labor Market Outcomes



Notes: The figures present the coefficients, γ_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—those who graduated within three years prior to the closure and those enrolled at the time of the 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 Closures 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 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. 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 Closures on Educational and Labor Market Outcomes

Panel A: Educational Outcomes				
	(1) Graduate HS	(2) Enroll Any College	(3) UG Degree	(4) College Quality
Closed School	-0.018***	-0.014***	-0.001	-191**
× Younger Cohorts	(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	-0.010**	-700***
× Younger Cohorts	(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. "Graduate HS" and "UG Degree" refer to high school graduation and four-year college completion, respectively. 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)

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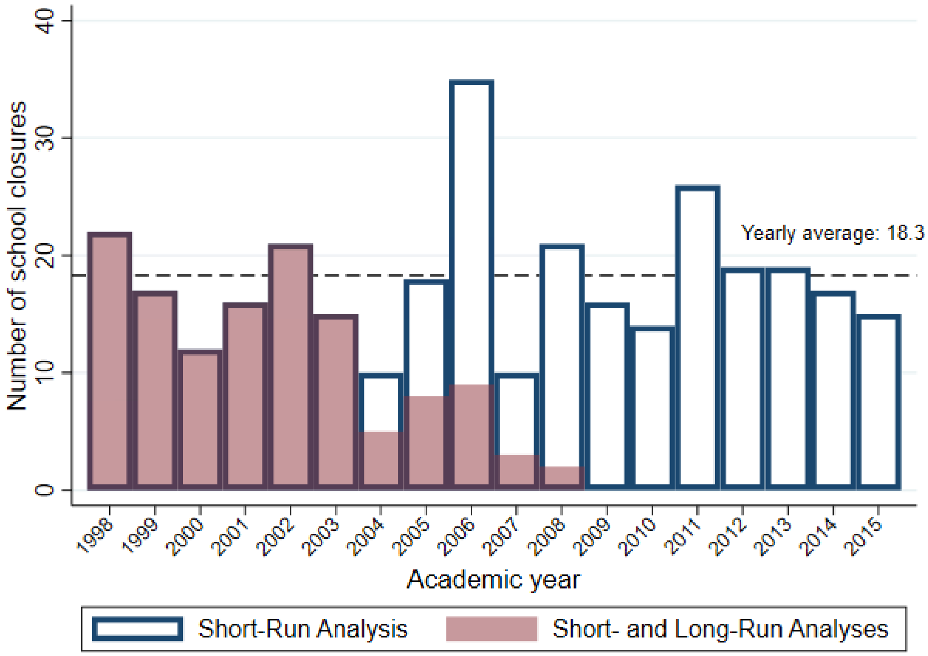
A Additional Figures and Tables

A.1 Characteristics of Closed Schools and Displaced Students

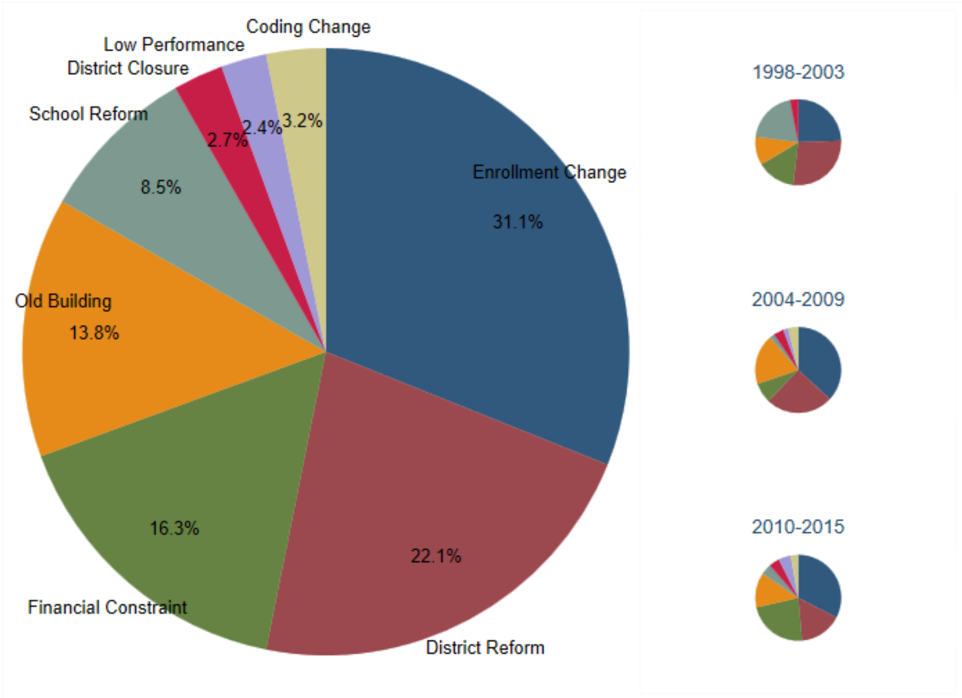
Fig. A.1. Annual Number of School Closures at Texas Public Schools in 1998–2015



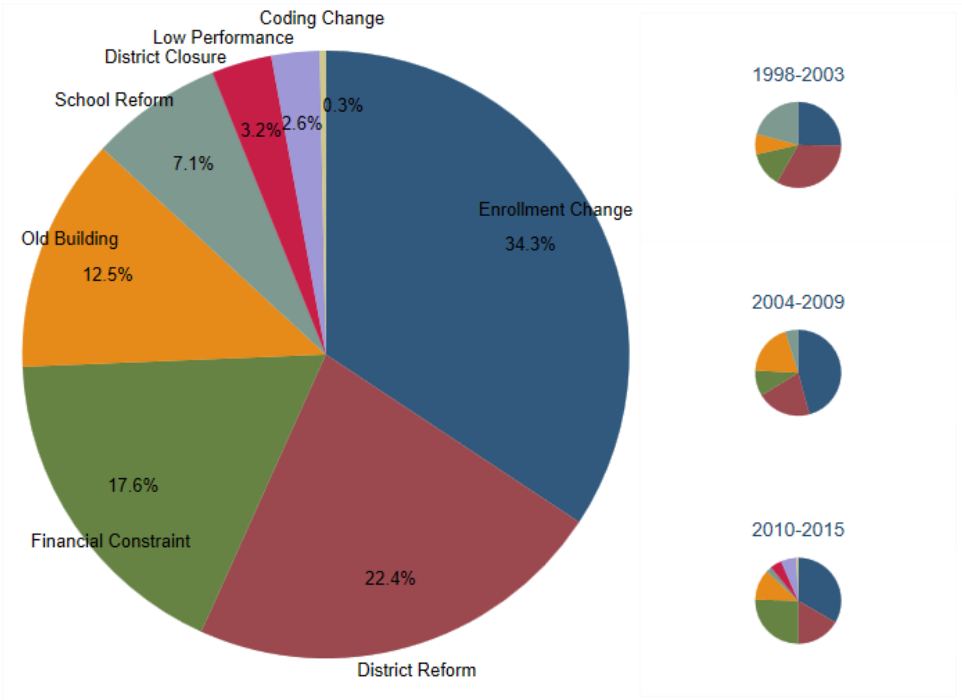
Notes: The figure presents the distribution of 323 (130) school closures that occurred between 1998 and 2015. To be considered a closed school, the school must be officially listed as closed by the Texas Education Agency (TEA), be a non-charter instructional campus in a regular, independent district,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



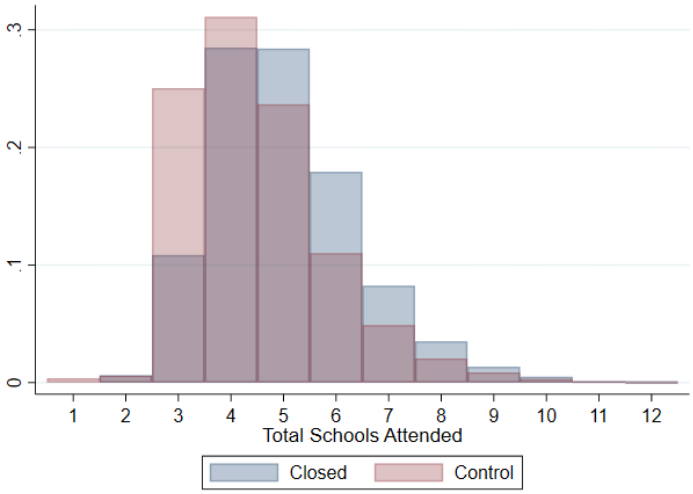
(b) Baseline Sample: Excluding Closures with >30% of Students Remaining at the Same Address



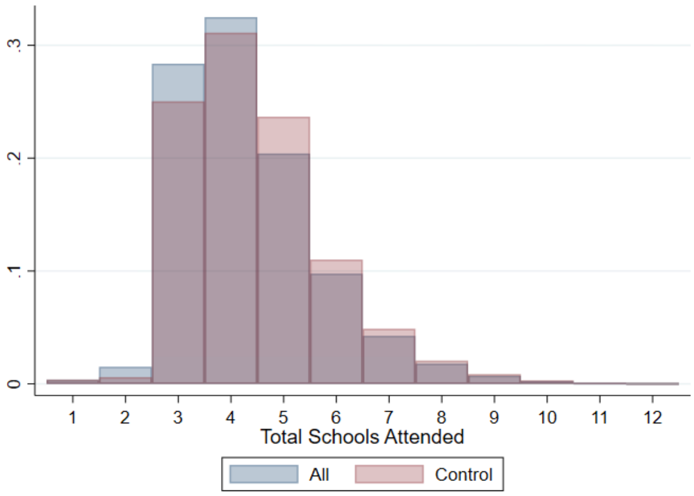
Notes: The figures display the categorized reasons for public school closures in Texas between 1998 and 2015: 274 out of 470 closures in panel (a), and 204 out of 323 closures in panel (b). Panel (a) includes all closures, while panel (b) excludes closures where more than 30 percent of students were 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 panel (a), I compare students in my analysis sample enrolled in closed and control schools at the time of closure. In panel (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

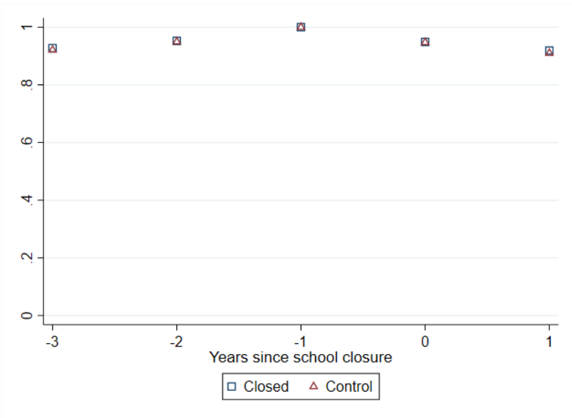
Panel B: School Closures in Long-Run Anaysis			
Matching Variables	(1) Closed Schools	(2) All Schools	(3) Control Schools
Locales			
City	0.48	0.39	0.48
Urban Fringe (Or Suburb)	0.18	0.25	0.18
Town	0.20	0.14	0.20
Rural	0.14	0.23	0.14
School Types			
Elementary	0.51	0.49	0.51
Middle	0.15	0.15	0.15
Junior 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 the short-run sample in Panel A and the 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.

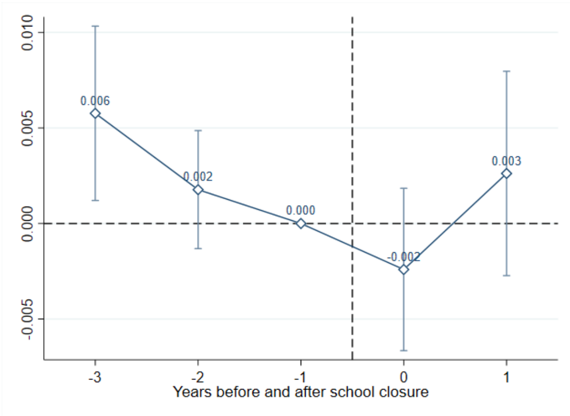
A.2 Sample Attrition

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

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

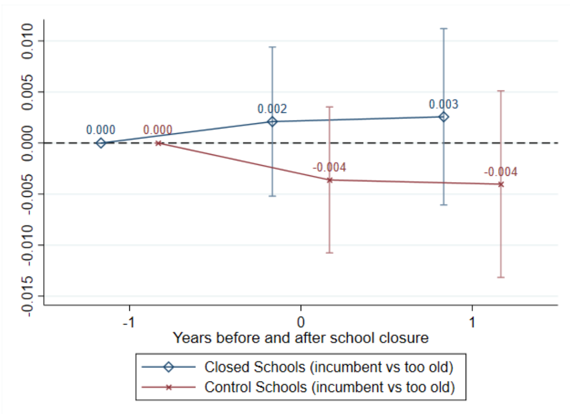


(b) Short-run: regression of in-sample dummy on closed-school dummy



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

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

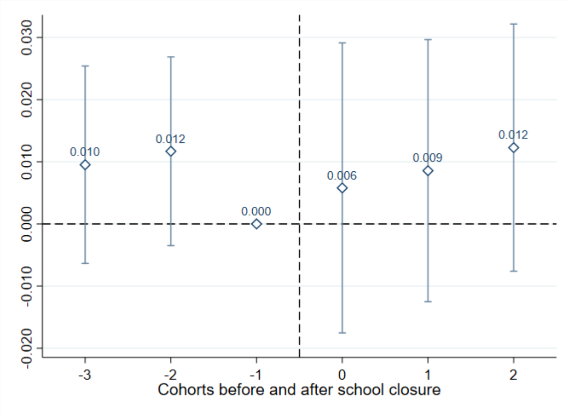


Notes: Panels (a) and (b) consider all students in the short-run analysis sample enrolled in closed and matched control schools in the year preceding the closure (denoted by time -1 on the x-axis). Panel (a) plots the proportion of observed students each year around school closure, separately for students in closed schools and control schools. Using this sample, panel (b) presents the coefficients, β_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 $t = -1$. Panels (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). Panel (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, panel (d) presents the coefficients, γ_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 panel (b).

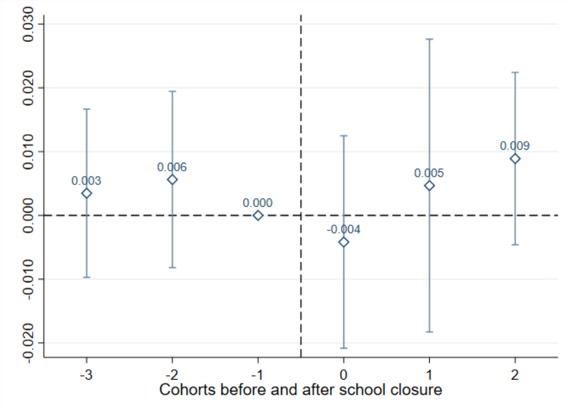
A.3 Long-Run Analysis Balance Across School Cohorts

Fig. A.5. Long-Run Analysis Balance Test: Composition and Performance

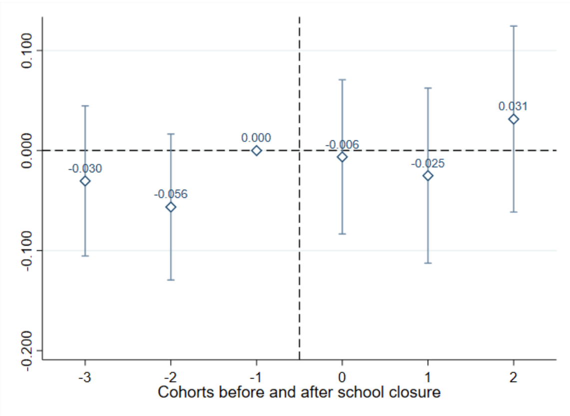
(a) Economic disadvantage status



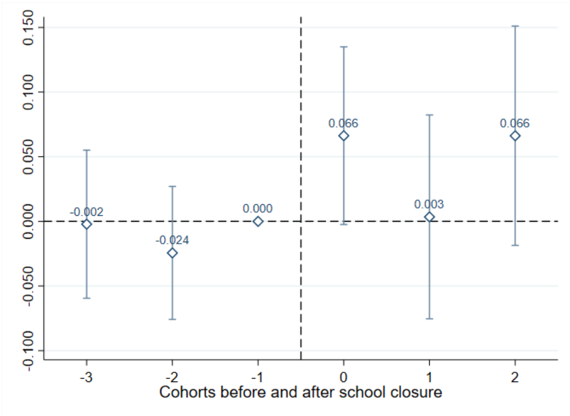
(b) Black or Hispanic students



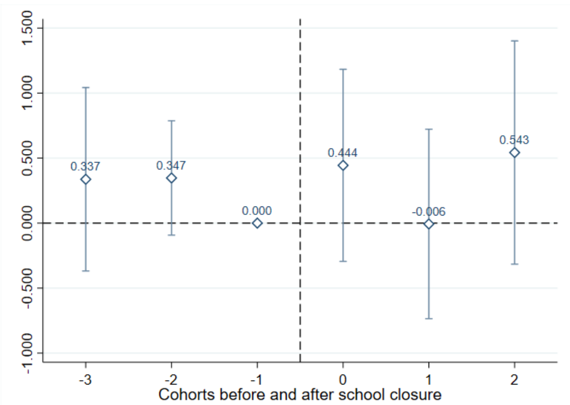
(c) Standardized math score



(d) Standardized reading score



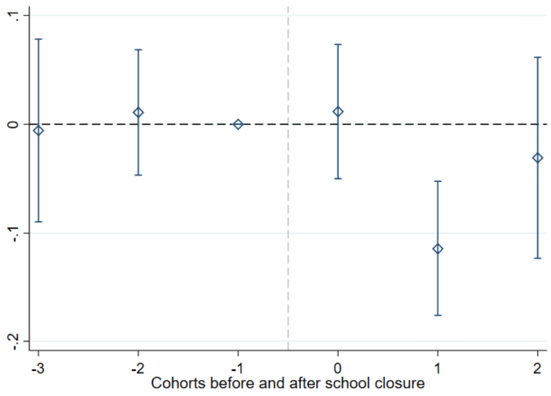
(e) Days of absence



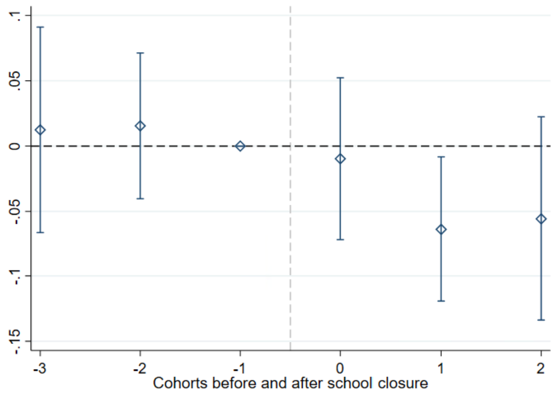
Notes: The figures present the coefficients, γ_c, and 95% confidence intervals from equation (4), in which the dependent variables are student characteristics or short-run outcomes (test scores and days of absence). The short-run outcomes are measured before school closures, specifically at $t = -1$ 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—those who graduated within three years prior to the closure and those enrolled at the time of the 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

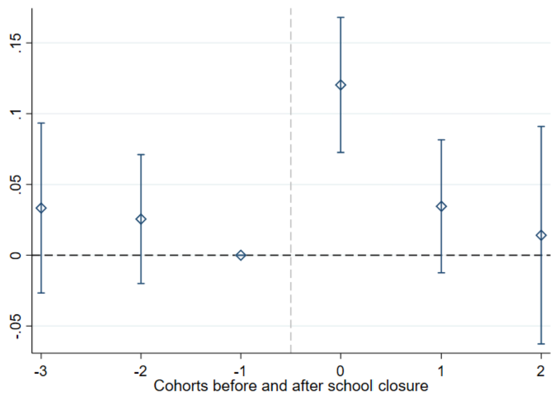
(a) Standardized math score



(b) Standardized reading score



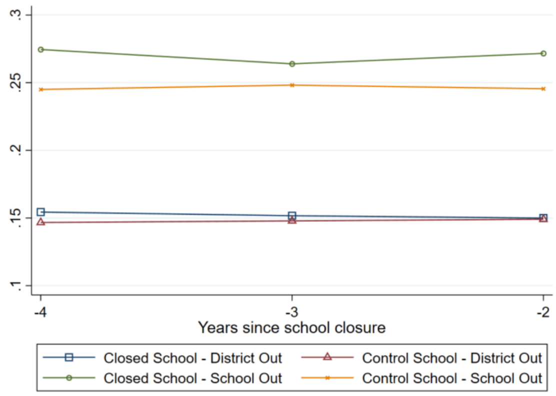
(c) Days of absence



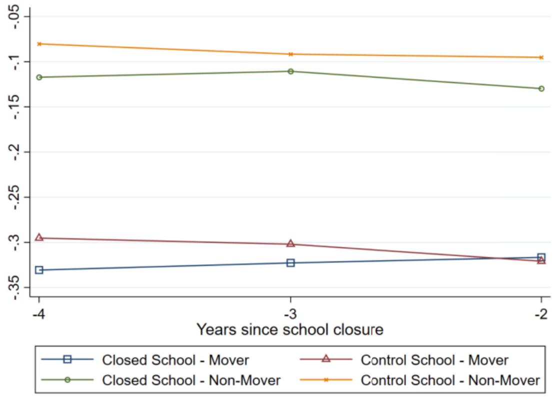
Notes: The figures present the differences in coefficients (γ_c) and the corresponding 95% confidence intervals, based on equation (4), where the dependent variables are short-run outcomes—standardized test scores and days of absence. These differences capture the change in outcomes from before to after 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



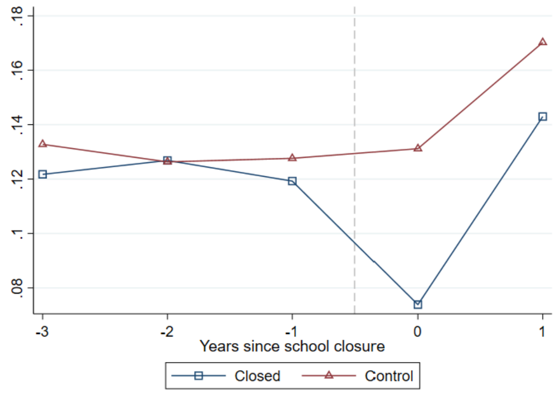
Notes: The figures illustrate student moving-out patterns in closed and matched control schools. Panel (a) shows the proportion of students who changed schools or districts in the three years leading up to a year before the school closure. Specifically, the indicator equals one if a student enrolled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

A.4 Raw Trends in Short-Run Outcomes

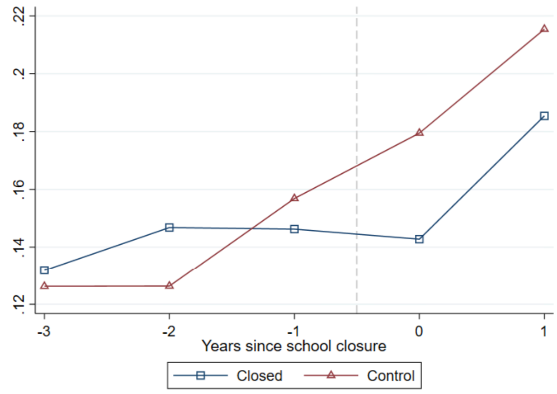
Appendix Figure A.8 illustrates the raw trends of short-run outcomes for closed and control schools around school closure. Panels (a) and (b) show standardized math and reading scores. Prior to school closure, both closed and control schools exhibit comparable trends over the three-year period, with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

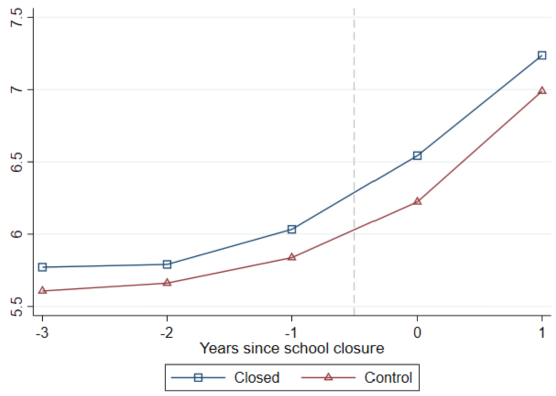
(a) Standardized math score



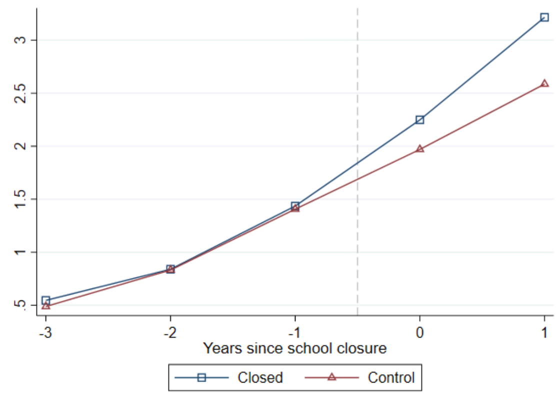
(b) Standardized reading score



(c) Days of absence



(d) Days of disciplinary action



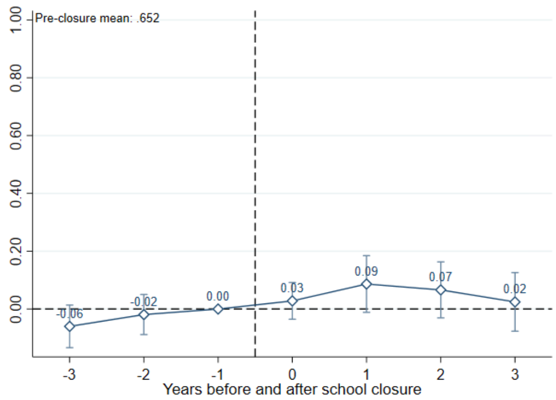
Notes: The figures plot raw trends over the period of three years before and two years after the school closure, separately for closed and matched control schools. I restrict the sample to students who are observed in the data over this period (i.e., the panel is balanced).

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

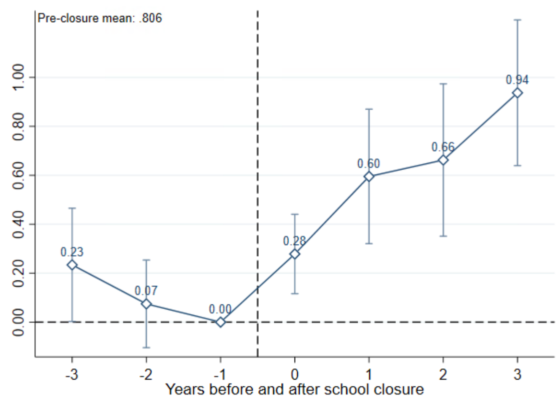
Given the significant increase in the number of days of disciplinary action following the school closure, I conduct separate analyses for days of in-school suspensions, days of out-of-school suspensions (including expulsions), and intensive/extensive margins of disciplinary actions. These results are 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 over the following four years up to 0.9. Moreover, I find an increase in both the extensive margin—whether students have at least one day of disciplinary action—and the 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 Closures on Days of Disciplinary Actions: Different Margins

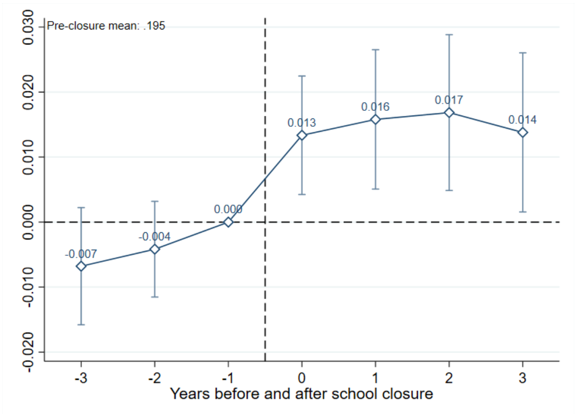
(a) In-school days of disciplinary action



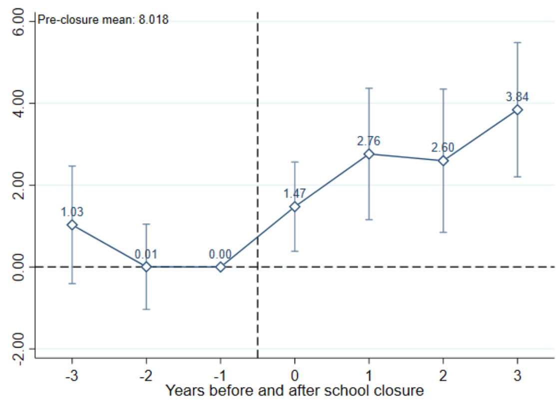
(b) Out-of-school days of disciplinary action



(c) Binary outcome variable



(d) Among at least one day



Notes: The figures present the coefficients, β_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 Closures: Additional 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.

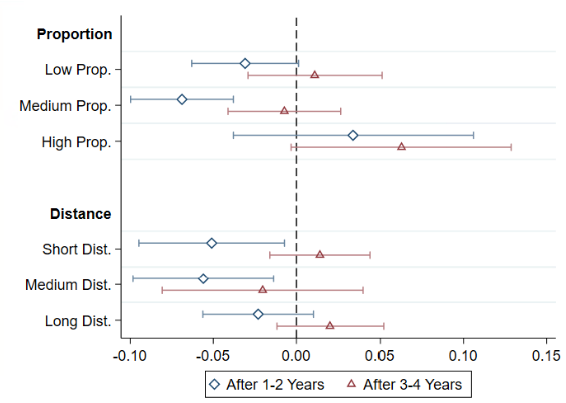
Moreover, I categorize closed schools based on their distance to receiving schools. Distance is measured as the median distance between closed and receiving schools. The median distance is 1.1 miles, with quartile cutoffs at 0.4, 1.1, and 2.0 miles. While most differences are not statistically significant, 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.

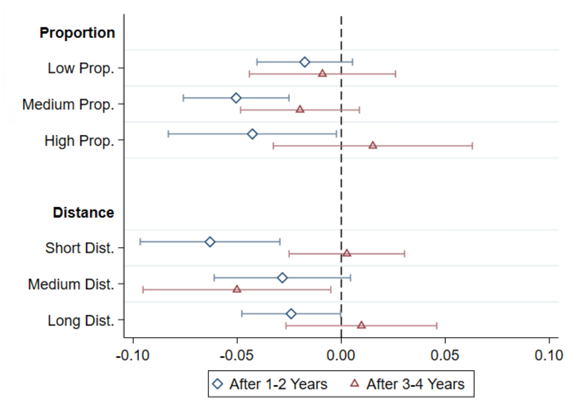
[A.1] For White students, the groups are defined as 0–17%, 17–50%, and 50–97%; for Hispanic students, as 0–27%, 27–61%, and 61–100%; and for Black students, as 0–8%, 8–25%, and 26–94%.

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

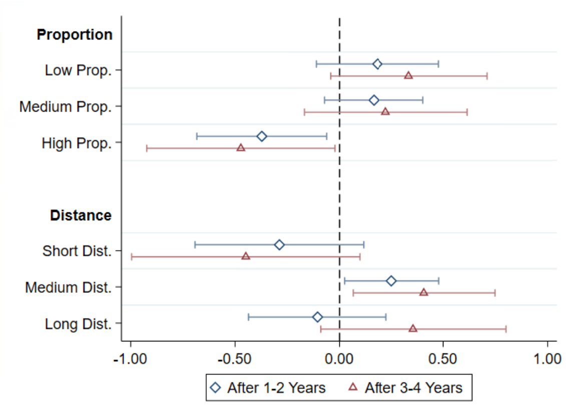
(a) Standardized math score



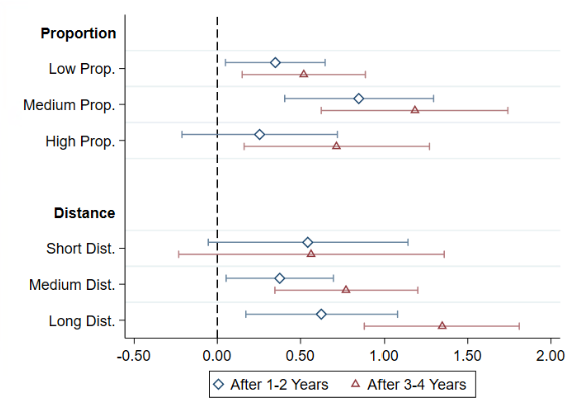
(b) Standardized reading score



(c) Days of absence

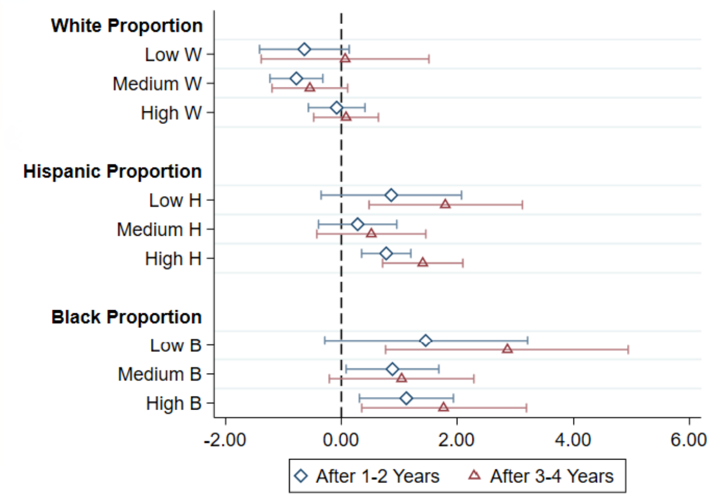


(d) Days of disciplinary actions



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



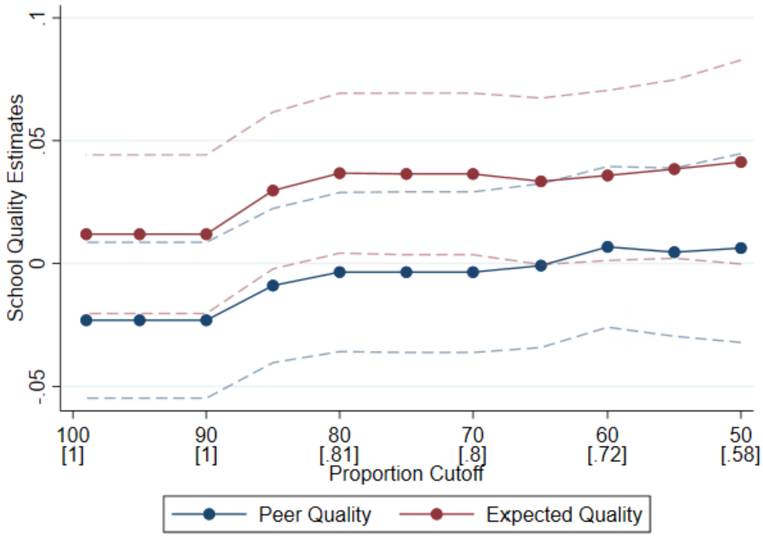
Notes: The figures present the coefficients, β, and 95% confidence intervals from equation (1) for students belonging to the sub-group denoted on the y-axis. For instance, "Low W" refers to White students who moved to a school with a low proportion of White students. These coefficients represent the 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

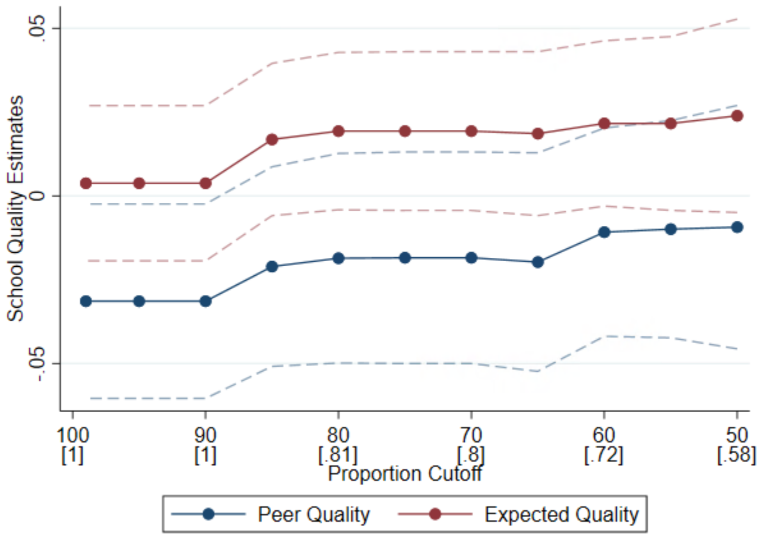
To further understand why students do not have high-performing peers even after transitioning to originally better-performing schools, I examine average test score changes of receiving schools before and after school closure (i.e., $t = 0$ and $t = -1$), dividing students into original students and move-in 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



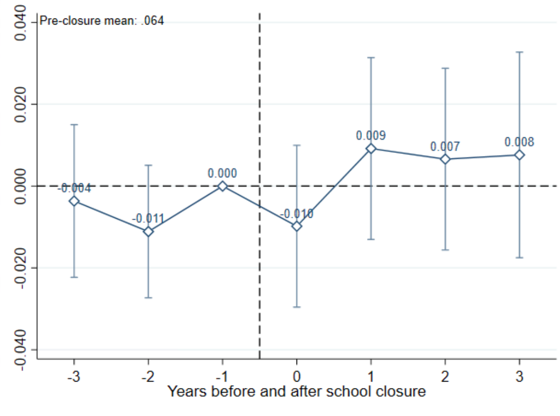
(b) Standardized reading score



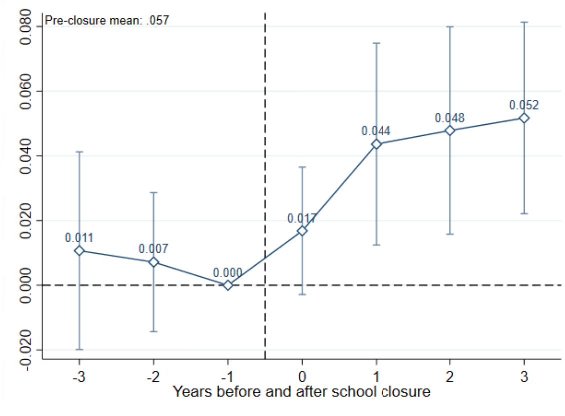
Notes: The figures present the coefficients, β, and 95% confidence intervals from equation (1), where the outcome variables are the school-level average math and reading scores. The x-axis specifies different cutoffs for sample inclusion. For peer quality, the outcome variables are the yearly school 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

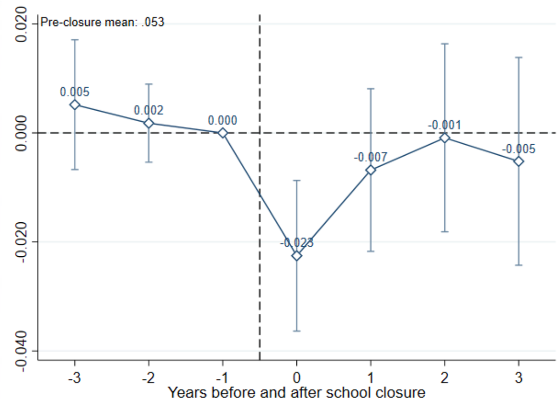
(a) Peer quality: days of absence



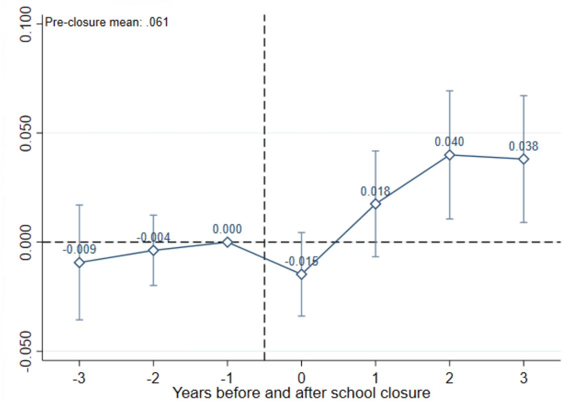
(b) Peer quality: days of disciplinary action



(c) Expected quality: days of absence



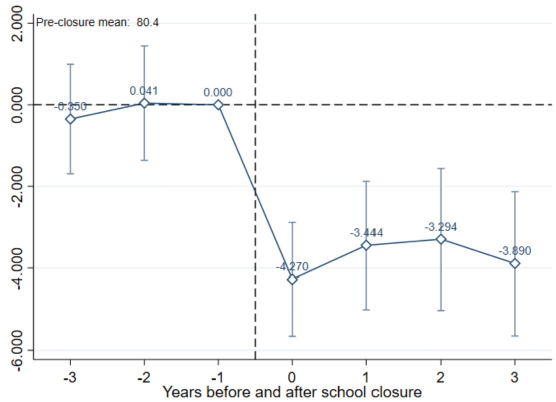
(d) Expected quality: days of disciplinary action



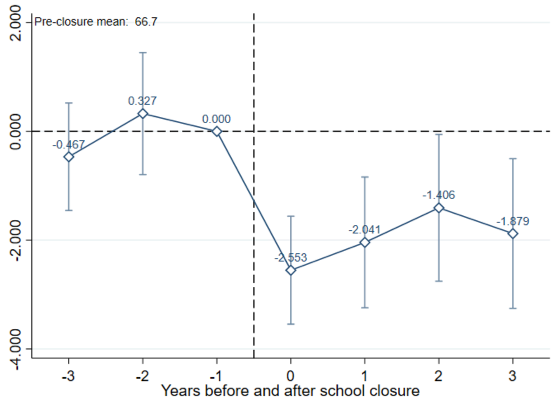
Notes: The figures present the coefficients, β_t, and 95% confidence intervals from equation (2), where the outcome variables are the school average days of absence and days of disciplinary action, which are standardized by year-by-grade level. When it comes to panels (a) and (b), the outcome variables 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 panels (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 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

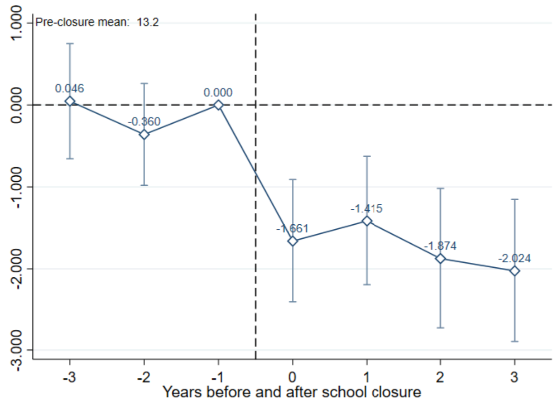
(a) Total school staff



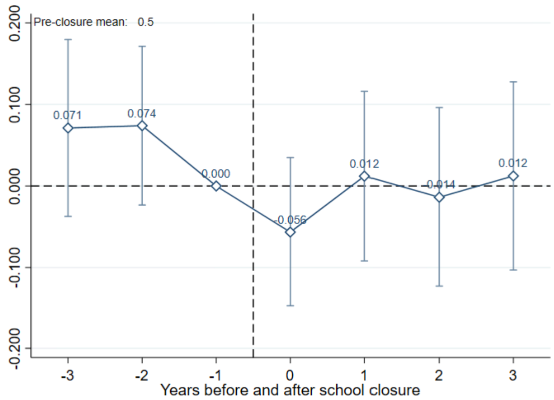
(b) Teachers



(c) Teaching support



(d) Social support



Notes: The figures present the coefficients, β_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 $t = -1$.

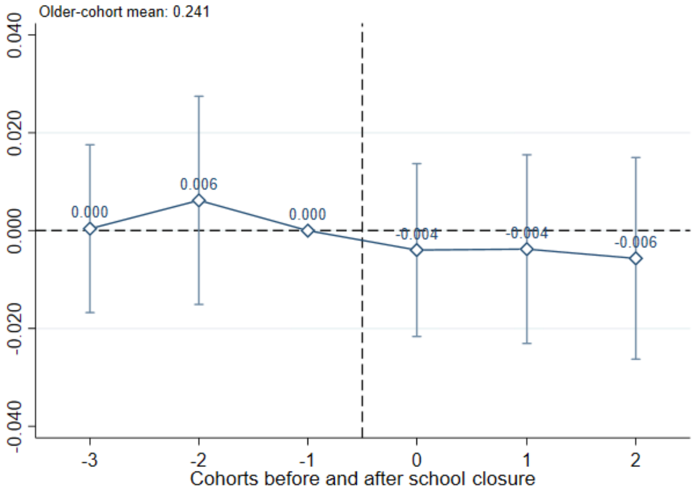
Table A.2: Receiving School Quality Change: Original and Move-In Students

	(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 Long-Run Effects of School Closures on Four-Year College Enrollment

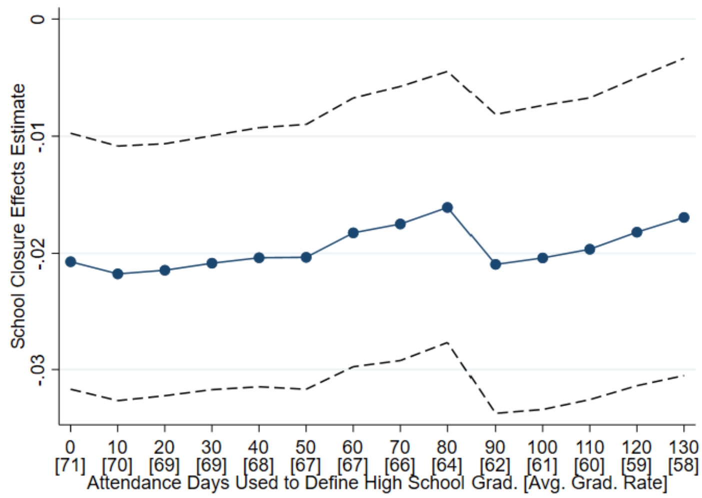
Fig. A.15. Estimates of School Closure Effects on Four-Year College Enrollment



Notes: The figure presents the coefficients, γ_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—those who graduated within three years prior to the closure and those enrolled at the time of the 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.9 High School Graduation Proxy Across Different Definition Cutoffs

Fig. A.16. 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.10 Long-Run Effects of School Closures: Separate Estimates for Highest Grade and Others

Table A.3: Long-Run Effects of School Closures 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) UG 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

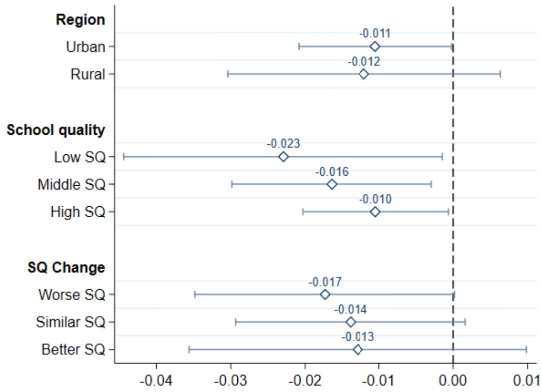
Panel B: Labor Market Outcomes		
	(1) Employment	(2) Yearly Earnings
Closed School	-0.014**	-1,110***
× Younger Cohorts (c=1,2)	(0.006)	(284)
Closed School	-0.005	-238
× Younger Cohorts ($c = 0$)	(0.005)	(314)
Observations	164,497	164,497
School FE	X	X
Matched group × Year FE	X	X
Mean of the Older Cohort	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. "Graduate HS" and "UG Degree" refer to high school graduation and four-year college completion, respectively. 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

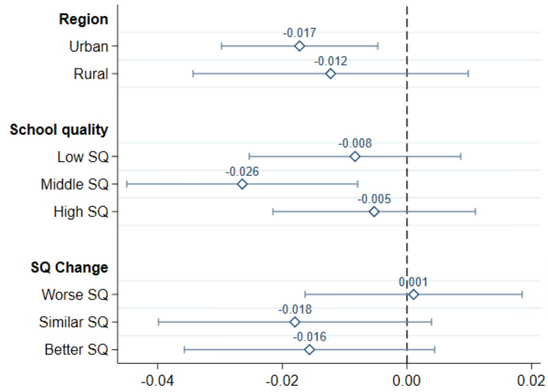
A.11 Long-Run Effects of School Closures: Heterogeneity

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

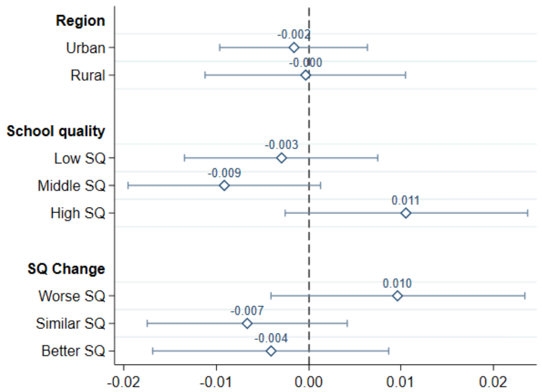
(a) High school graduation



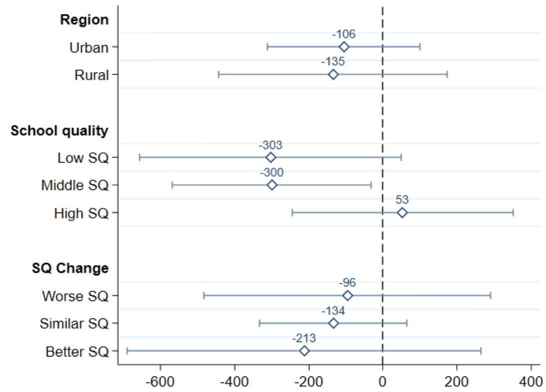
(b) Any college enrollment



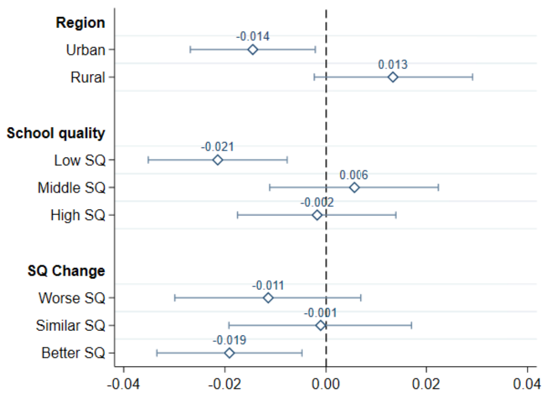
(c) Four-year college completion



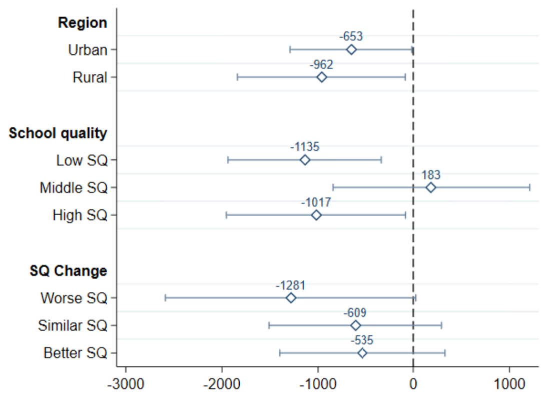
(d) College quality



(e) Employment



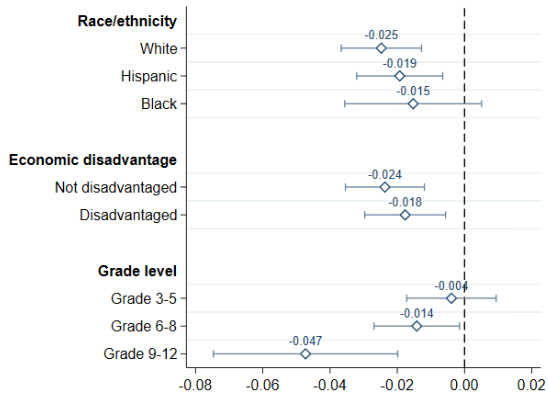
(f) Yearly earnings



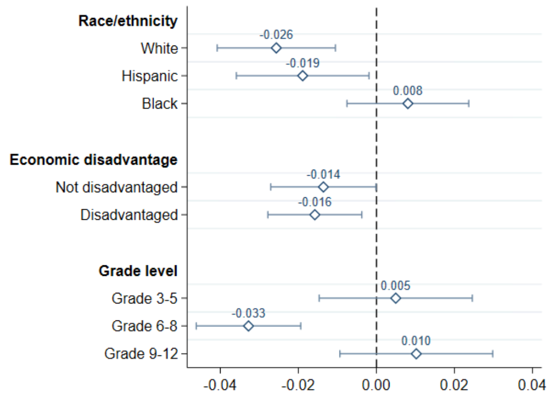
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school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

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

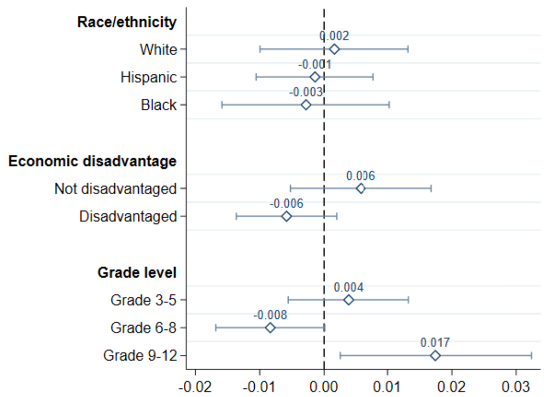
(a) High school graduation



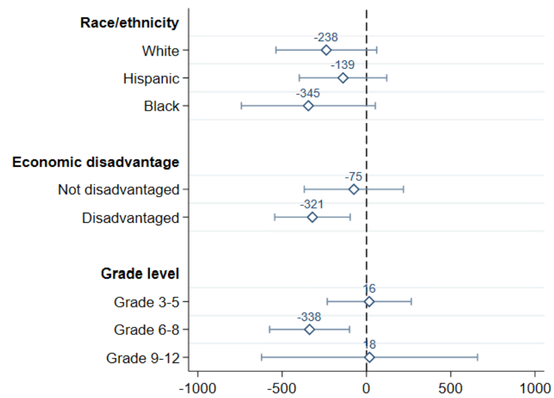
(b) Any college enrollment



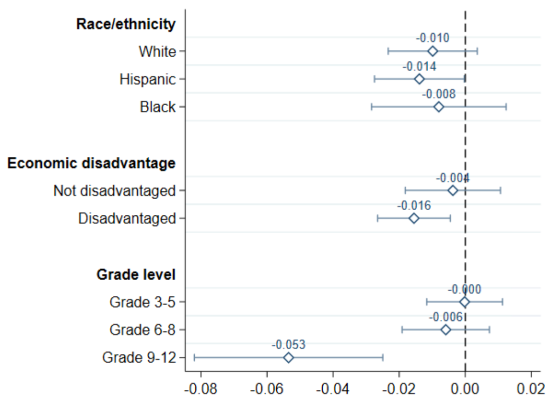
(c) Four-year college completion



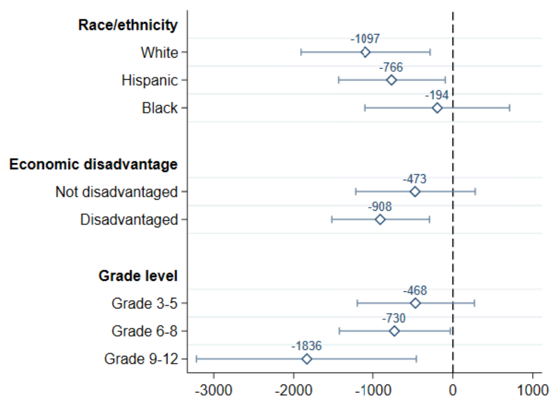
(d) College quality



(e) Employment



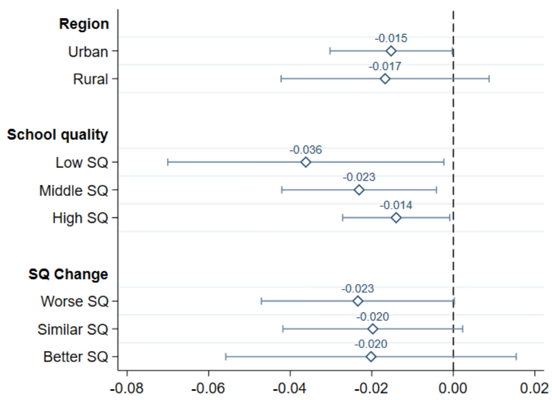
(f) Yearly earnings



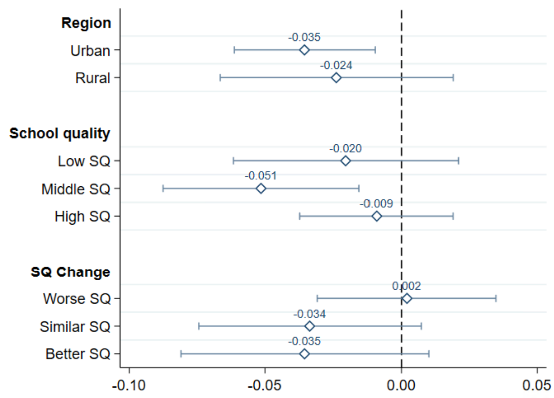
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school 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 Closures on Educational and Labor Market Outcomes: *Rescaled* Heterogeneity by School Characteristics

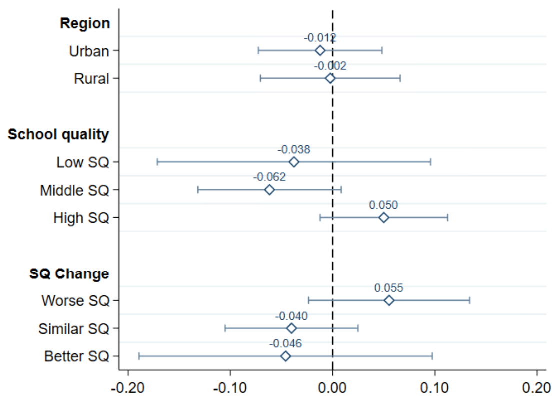
(a) High school graduation



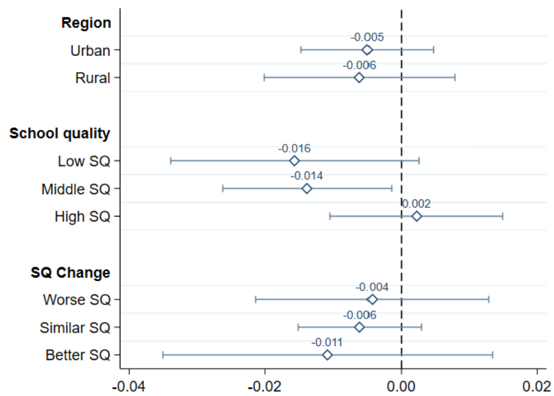
(b) Any college enrollment



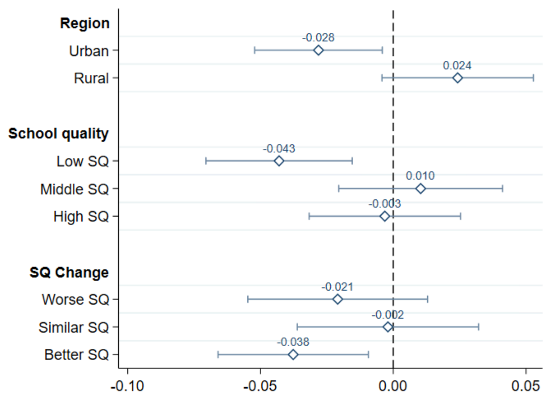
(c) Four-year college completion



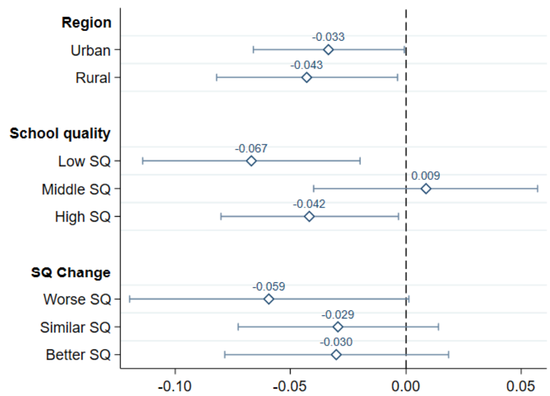
(d) College quality



(e) Employment



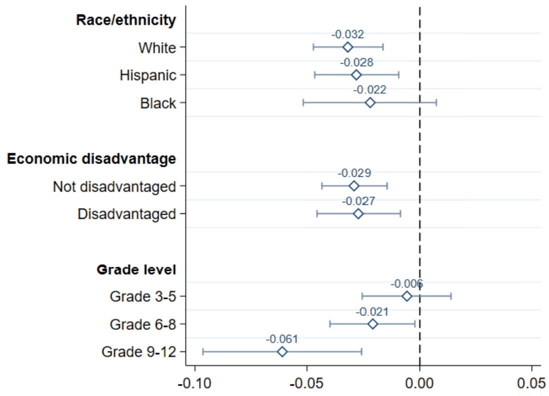
(f) Yearly earnings



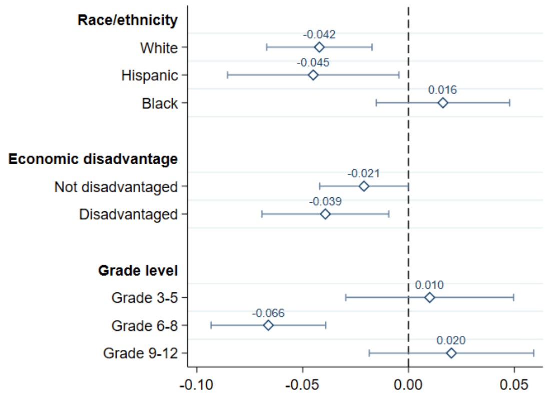
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis after estimates are scaled relative to the outcome mean for each sub-group. These coefficients represent the interactions between the indicator 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 Closures on Educational and Labor Market Outcomes: *Rescaled* Heterogeneity by Student Characteristics

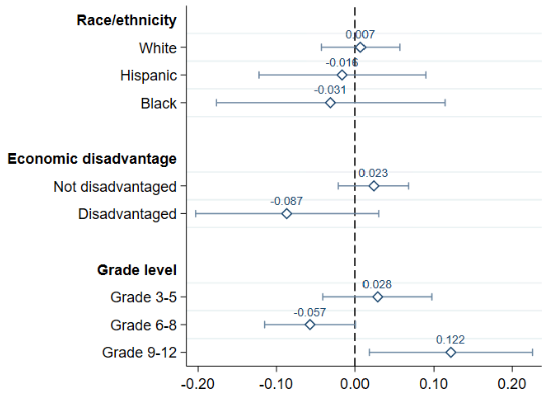
(a) High school graduation



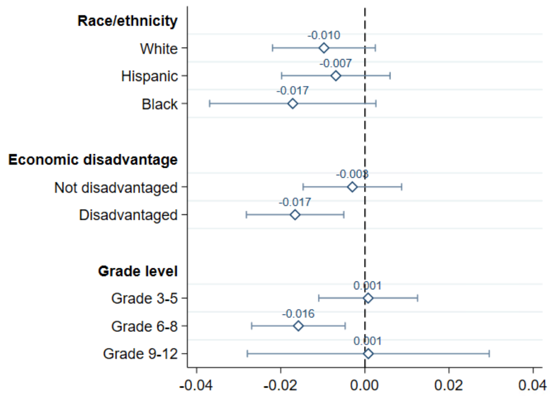
(b) Any college enrollment



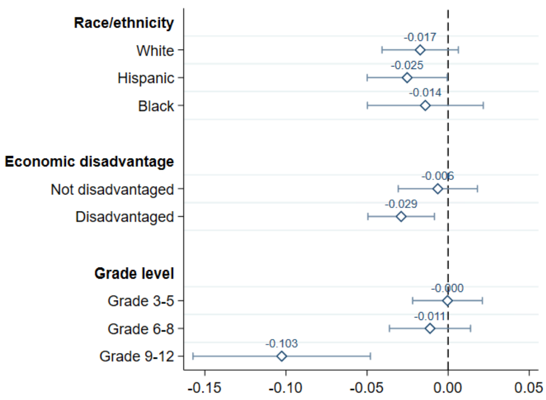
(c) Four-year college completion



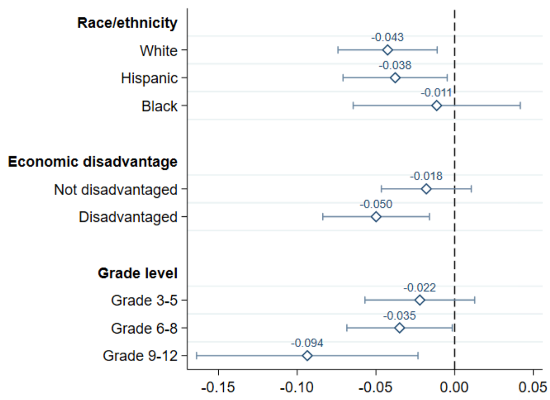
(d) College quality



(e) Employment



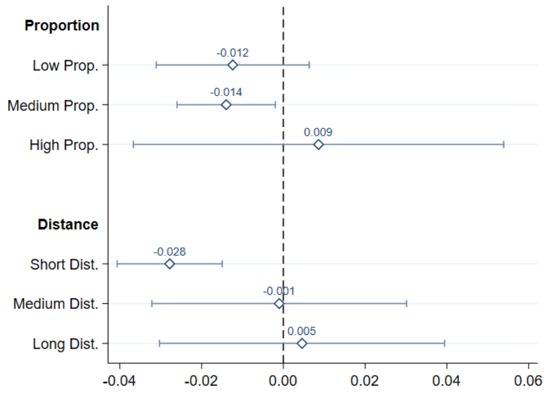
(f) Yearly earnings



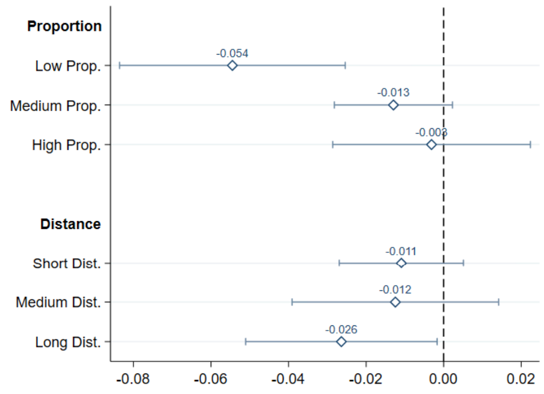
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis after estimates are scaled relative to the outcome mean for each sub-group. These coefficients represent the interactions between the indicator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

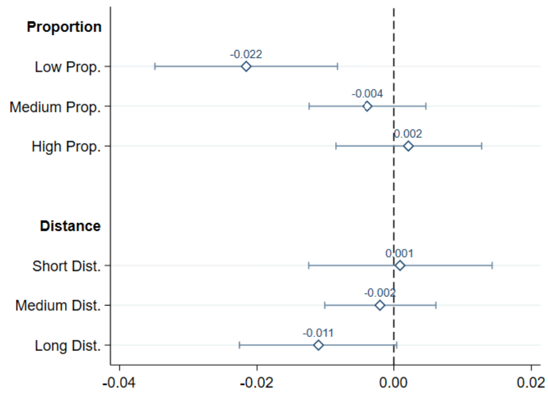
(a) High school graduation



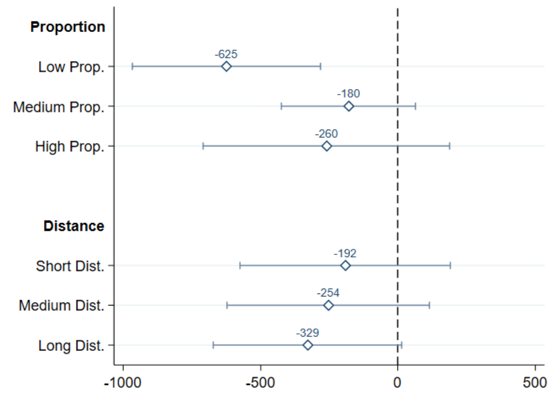
(b) Any college enrollment



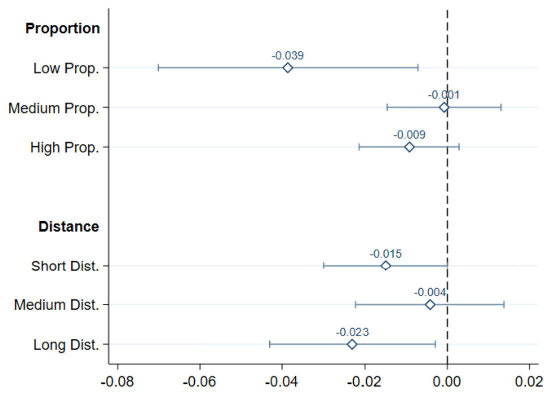
(c) Four-year college completion



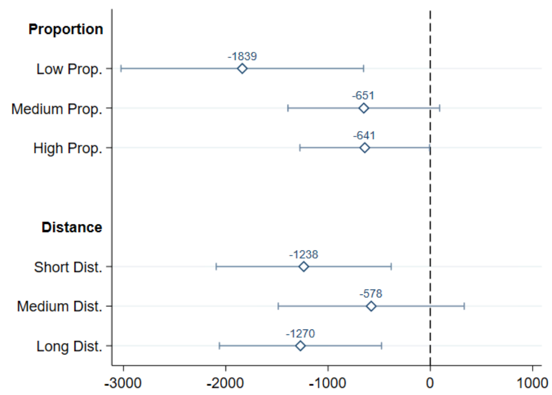
(d) College quality



(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 students from high-quality schools	Low-quality schools: Larger negative impacts on educational and labor market outcomes compared to students from 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 Analyses

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 English as a second language status and share of special education status) 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 of English as a second language, 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:

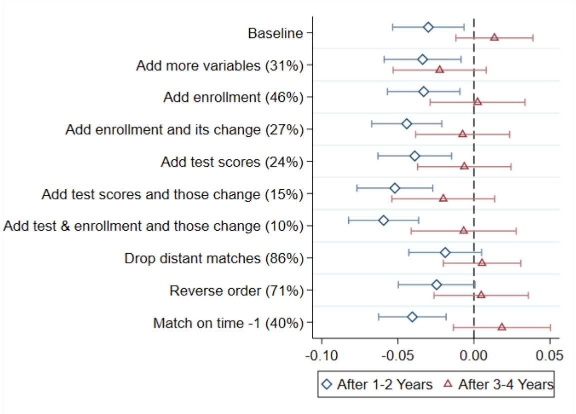
[B.1] The estimates fluctuate more in the specifications that include test scores (4, 5). For example, the estimated effects on high school graduation and college quality are close to zero in the specifications. This may be because test scores are already quite similar without being explicitly included 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.

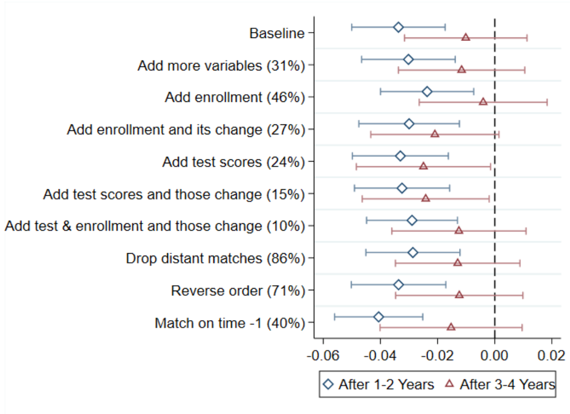
long-run outcomes decline among younger cohorts exposed to school closures. In other words, the estimated coefficients—obtained without additional discretion in matching criteria—align with the baseline results.

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

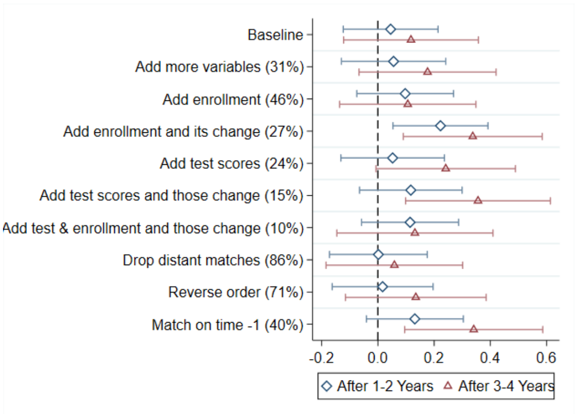
(a) Standardized math score



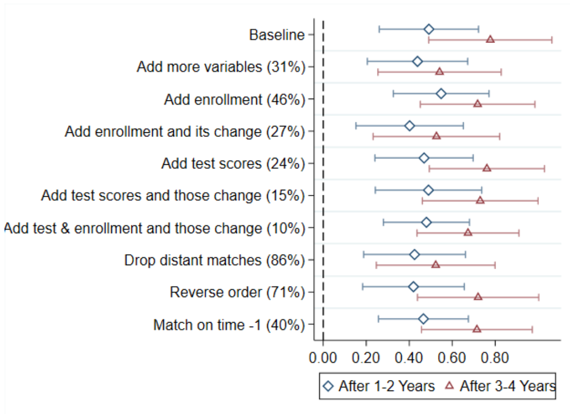
(b) Standardized reading score



(c) Days of absence



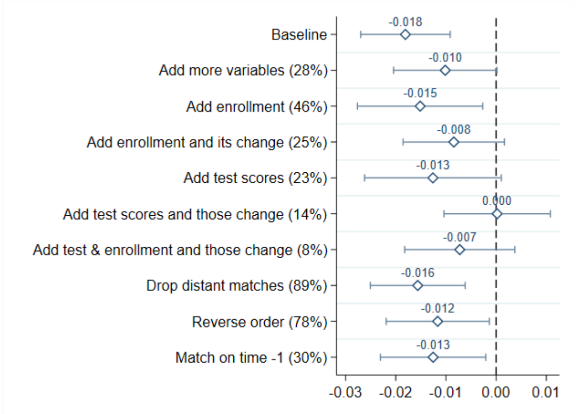
(d) Days of disciplinary action



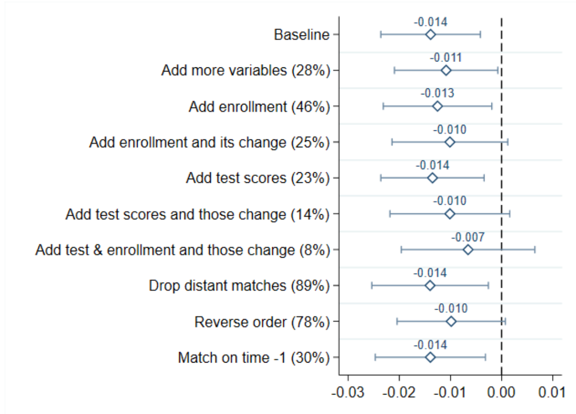
Notes: The figures present the coefficients, β, and 95% confidence intervals from equation (1) using control schools selected from the alternative matching strategies denoted on the y-axis. The baseline estimates are presented at the top of each sub-figure. The percentage in the parenthesis on the y-axis 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 Closures on Educational and Labor Market Outcomes: Alternative Matching Strategies

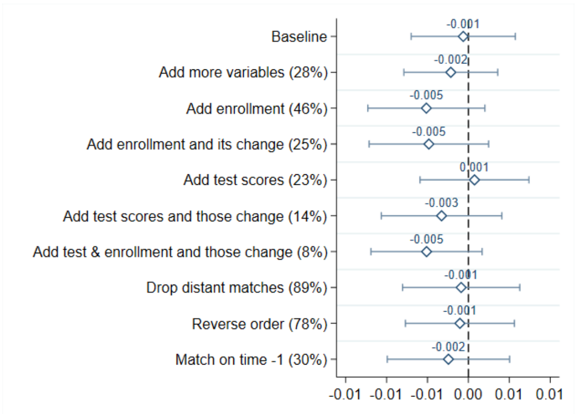
(a) High school graduation



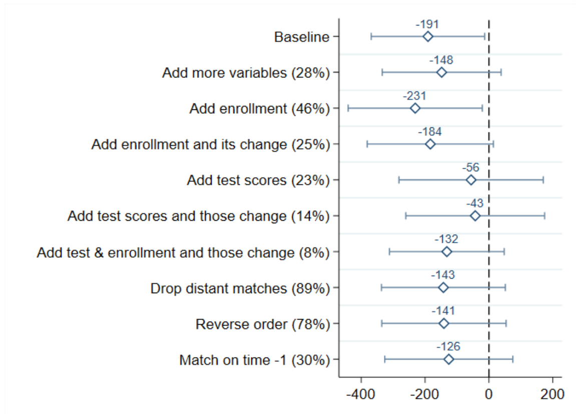
(b) Any college enrollment



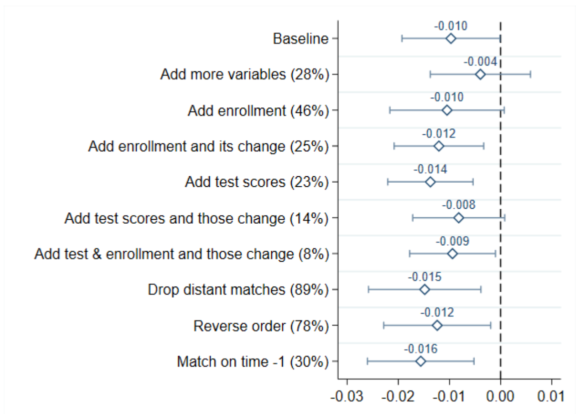
(c) Four-year college completion



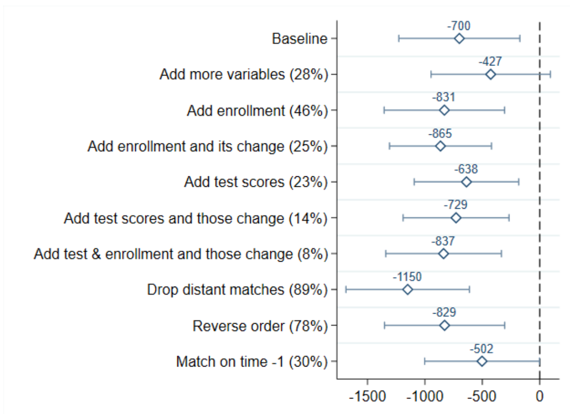
(d) College quality



(e) Employment at ages 25-27



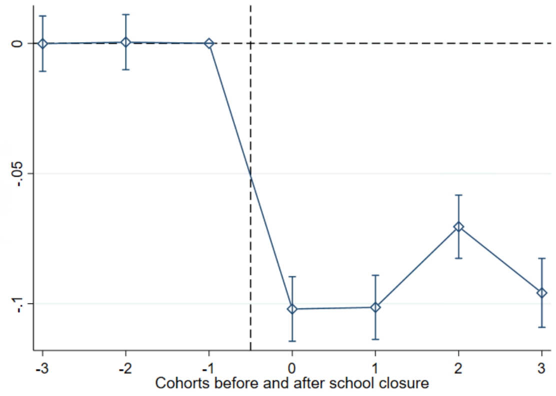
(f) Yearly wages at ages 25-27



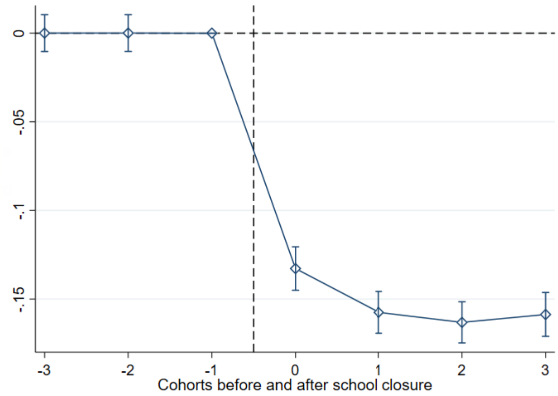
Notes: The figures present the coefficients, γ, and 95% confidence intervals from equation (3) using control schools selected from the alternative matching strategies denoted on the y-axis. The baseline estimates are presented at the top of each sub-figure. The percentage in parentheses on the y-axis denotes the proportion of the same matched control schools as those of the baseline. The coefficient represents the interaction between the indicator that denotes closed schools and the indicator that denotes 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 Closures on Student Outcomes: Synthetic Difference-in-Differences

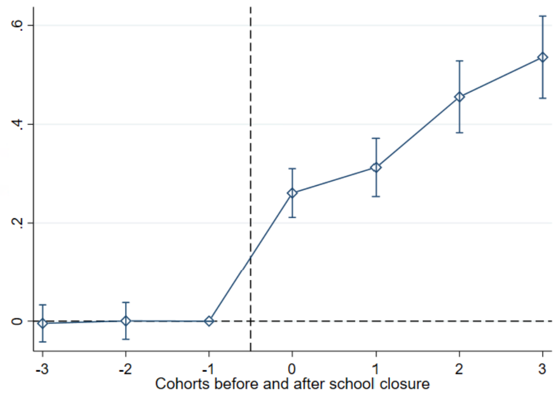
(a) Standardized math score



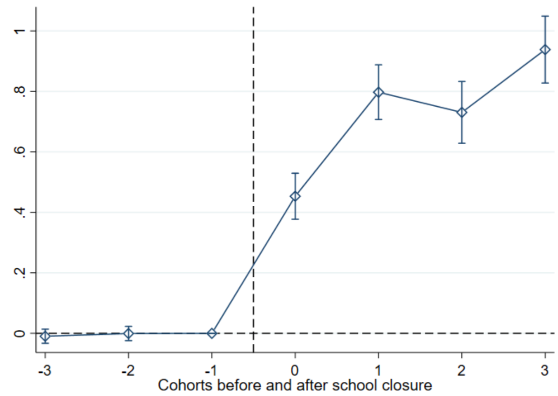
(b) Standardized reading score



(c) Days of absence



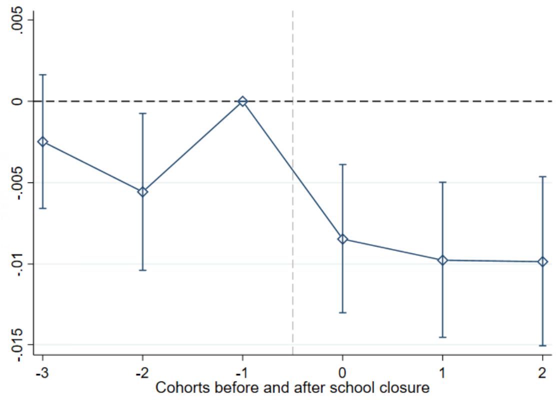
(d) Days of disciplinary action



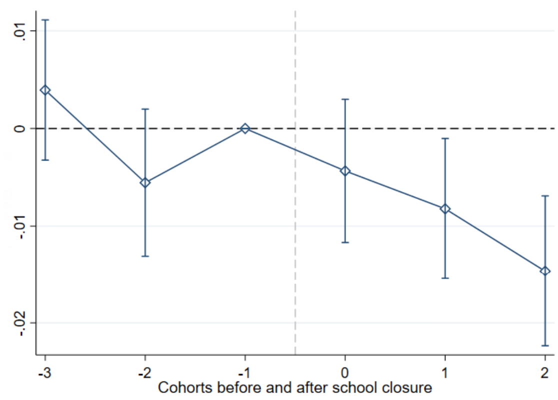
Notes: The figures present estimates from the implementation of a synthetic difference-in-differences model following Arkhangelsky et al. (2021). In this model, a synthetic control group is constructed using a donor pool of students who are enrolled in the same year, same school type (e.g., elementary 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 Closures on Educational and Labor Market Outcomes: Synthetic Difference-in-Differences

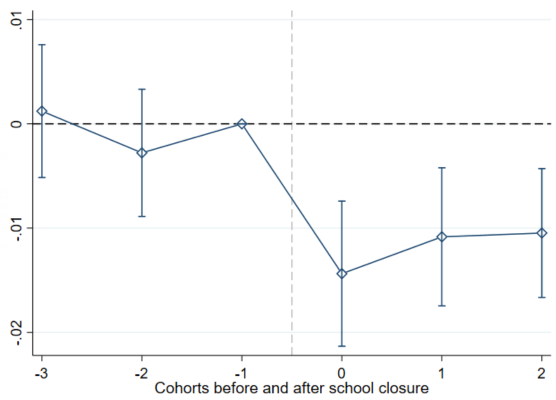
(a) High school graduation



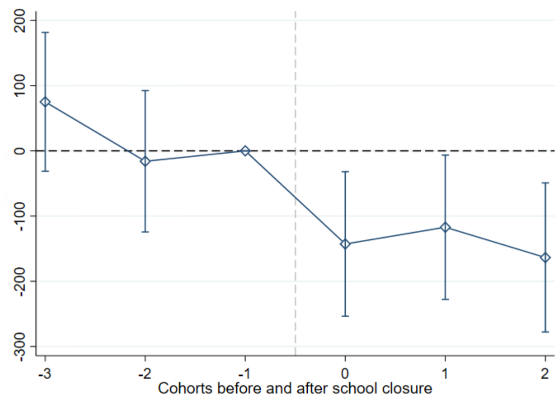
(b) Any college enrollment



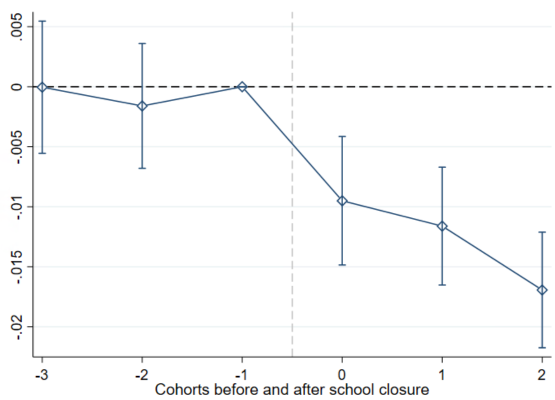
(c) Four-year college completion



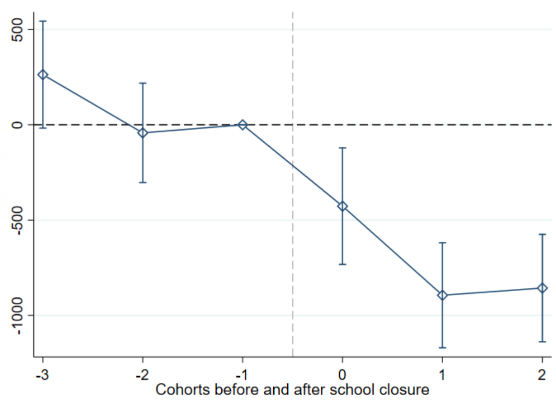
(d) College quality



(e) Employment at ages 25-27



(f) Yearly wages at ages 25-27



Notes: The 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 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

I estimate short-run event study models on a balanced panel that follows students from three years before to four years after a school closure. To assess robustness, I re-estimate equation (2) under alternative sample constructions. Appendix Figure B.5 contrasts three specifications: (i) the baseline sample balanced over $t = -3$ to $t = 3$; (ii) an unbalanced sample; and (iii) a sample balanced over $t = -3$ to $t = 1$.[B.3] While estimates from the unbalanced sample are somewhat less stable, the overall patterns closely track the baseline. My primary estimates from equation (1) use the sample balanced over $t = -3$ to $t = 1$. As an additional check, Appendix Table B.1 reports equation (1) using the $t = -3$ to $t = 3$ balanced sample and the unbalanced sample; these results are highly consistent with the baseline.

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.

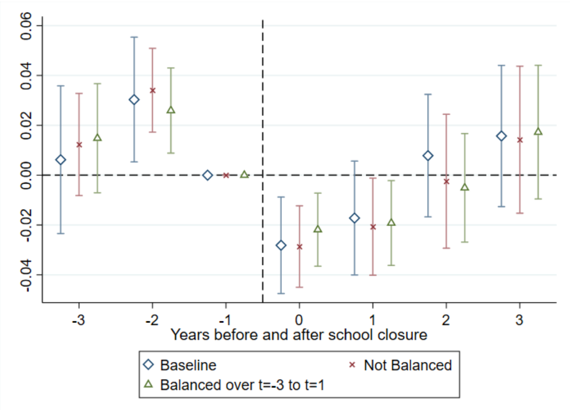
I further compare the estimation results between the baseline and long-run sample closures in Appendix Figure B.8. Moreover, the results from equation (1) for long-run sample school closures are presented in Appendix Table B.2. Overall, the estimates are similar across the two samples. The negative impacts 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.

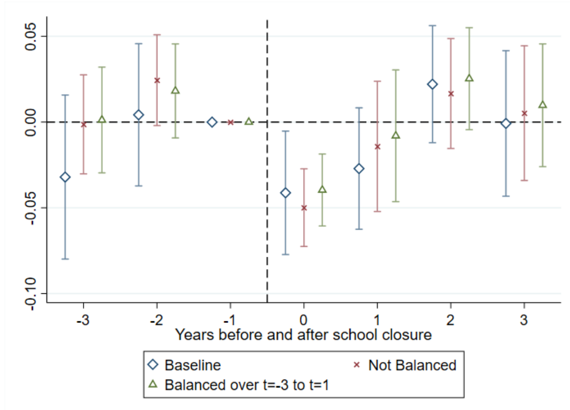
[B.3] Sample sizes by outcome and balancing rule: test scores—(i) 12,846; (ii) 40,548; (iii) 31,557; attendance—(i) 58,065; (ii) 69,215; (iii) 81,320; disciplinary actions—(i) 49,300; (ii) 66,434; (iii) 57,293.

Fig. B.5. Short-Run Effects of School Closures on Student Outcomes: Under Different Balancing Conventions

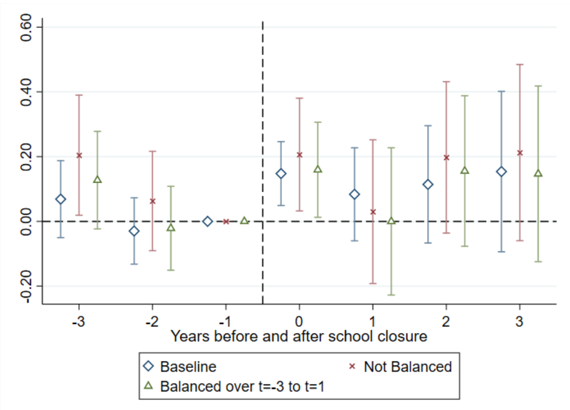
(a) Standardized math score



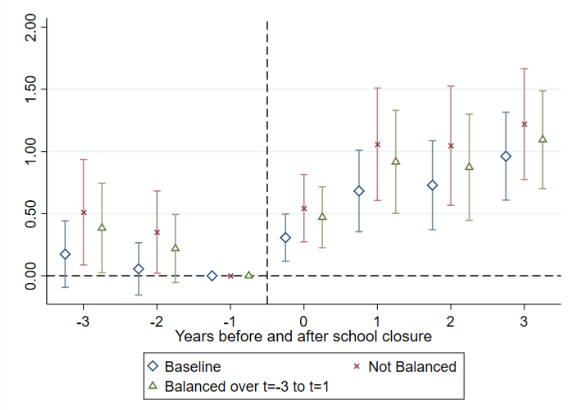
(b) Standardized reading score



(c) Days of absence



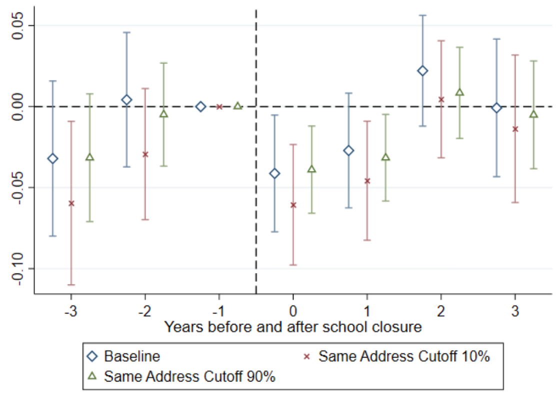
(d) Days of disciplinary action



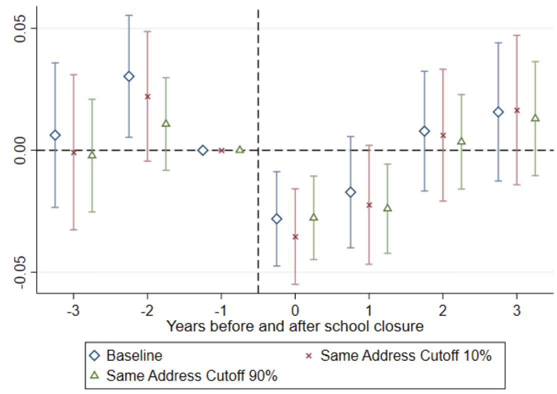
Notes: The figures overlay the coefficients, β_t, and 95% confidence intervals from equation (2), using three different sample constructions: (i) the baseline sample balanced over $t = -3$ to $t = 3$, (ii) an unbalanced sample, and (iii) a sample balanced over $t = -3$ to $t = 1$. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$.

Fig. B.6. Short-Run Effects of School Closures on Student Outcomes: Excluding Same Address School Opening with Different Cutoffs (1)

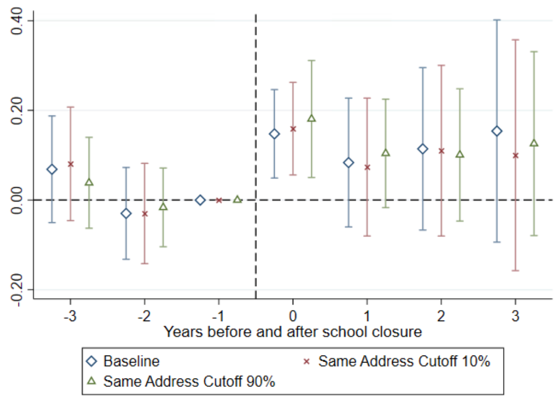
(a) Standardized math score



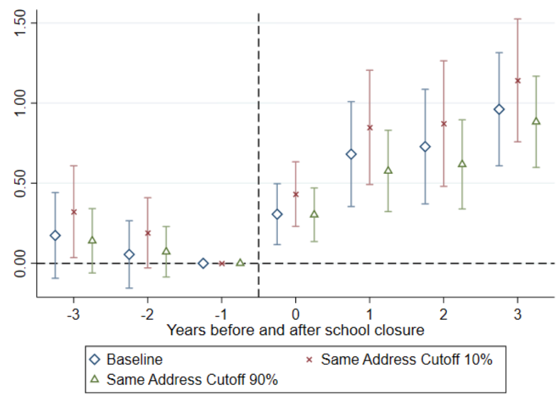
(b) Standardized reading score



(c) Days of absence



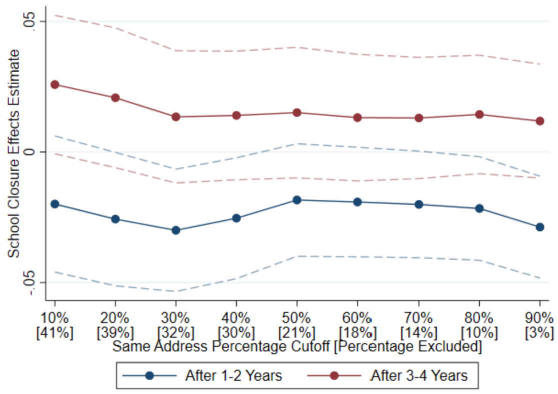
(d) Days of disciplinary action



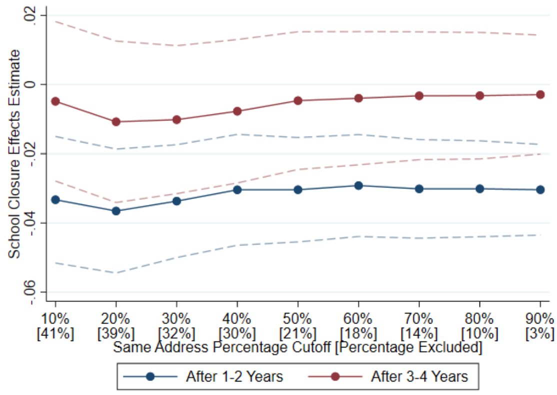
Notes: The figures overlay the coefficients, β_t, and 95% confidence intervals from equation (2), using either the baseline sample or alternative cutoffs based on the proportion of displaced students observed at the same address after closure. A cutoff of 10% (90%) means that schools where more than 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 Closures on Student Outcomes: Excluding Same Address School Opening with Different Cutoffs (2)

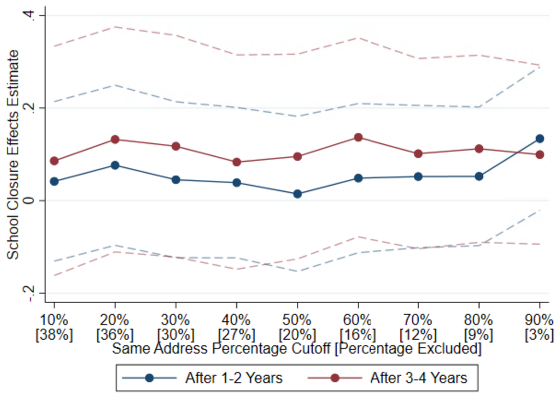
(a) Standardized math score



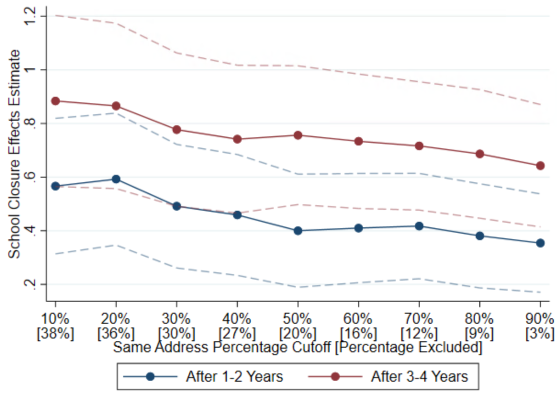
(b) Standardized reading score



(c) Days of absence



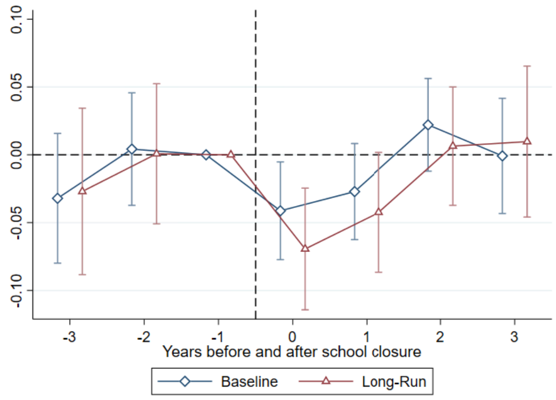
(d) Days of disciplinary action



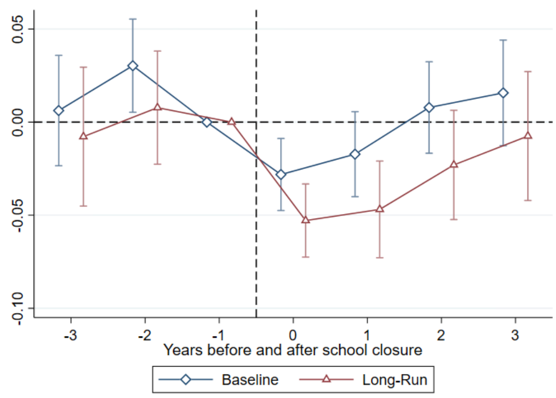
Notes: The figures overlay the coefficients, β, and 95% confidence intervals from equation (1) using a sample excluding closed schools where displaced students are observed at the same address after closure, applying different cutoffs. The x-axis shows the percentage cutoff for the proportion of displaced 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 Closures on Student Outcomes: Using Long-Run Sample School Closures

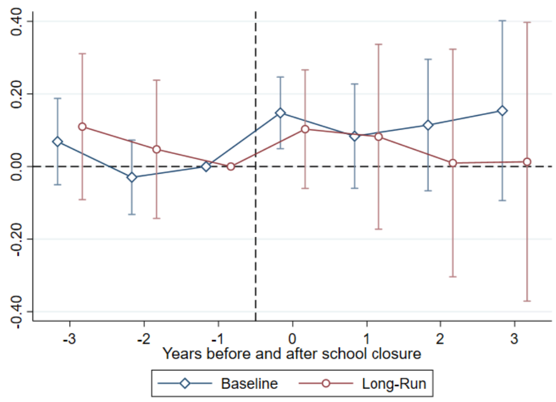
(a) Standardized math score



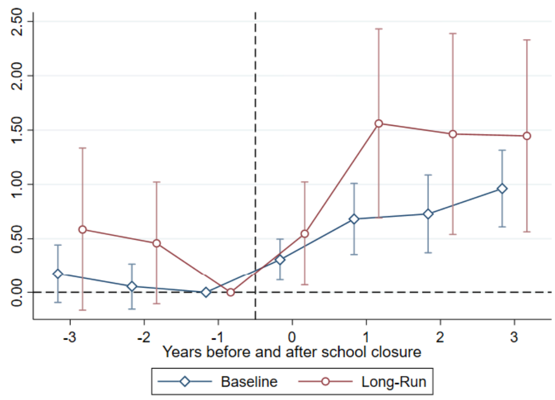
(b) Standardized reading score



(c) Days of absence



(d) Days of disciplinary action



Notes: The figures overlay the coefficients, β_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 Closures on Student Outcomes: Under Different Balancing Conventions

Panel A: Balanced over all periods ($t = -3$ **to** $t = 3$)				
	(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

Panel B: Not Balanced				
	(1) Math	(2) Reading	(3) Days of Absence	(4) Days of Disciplinary Action
Closed School×After 1–2 Years	-0.040***	-0.039***	0.035	0.516***
	(0.013)	(0.009)	(0.088)	(0.115)
Closed School×After 3–4 Years	0.005	-0.011	0.121	0.848***
	(0.015)	(0.014)	(0.120)	(0.151)
Observations	518,469	519,202	1,271,967	1,056,866
Individual FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Pre-Closure Mean	-0.152	-0.124	7.107	2.389

Notes: The table presents the coefficients, β, and standard errors from equation (1), using two sample constructions: Panel A presents results from the sample balanced over all periods ($t = -3$ to $t = 3$), while Panel B uses an unbalanced 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. 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 Closures 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 Closures on Student Outcomes: Divided by Time Period

	(1) Math	(2) Reading	(3) Days of Absence	(4) Days of Disciplinary Action
Panel A: School Closures in 1998–2003				
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 detailed 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. In addition, the estimation based on equation (3) includes all school closures, regardless of grade span. Appendix Table B.4 restricts the sample to closures with at least three cohorts, consistent with the baseline event study design. The findings remain consistent across specifications.

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.5 presents estimation results from equation (3) in three levels of controls: i) without demographic and performance controls, ii) with demographic controls, and iii) 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.

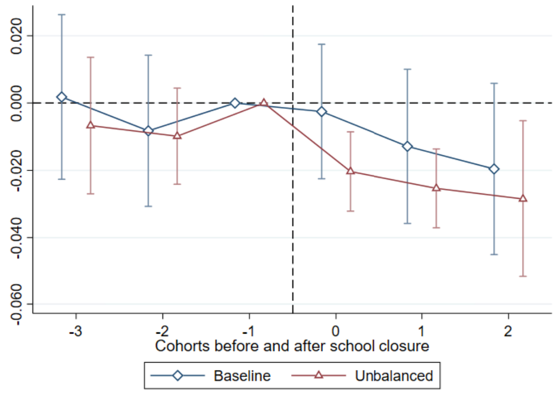
Appendix Figure B.11 presents estimation results using different cutoffs for excluding schools based on the proportion of displaced students observed at the same address after closure. In the baseline specification (Figure 6), I exclude closed schools if more than 30% of displaced students are observed 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.

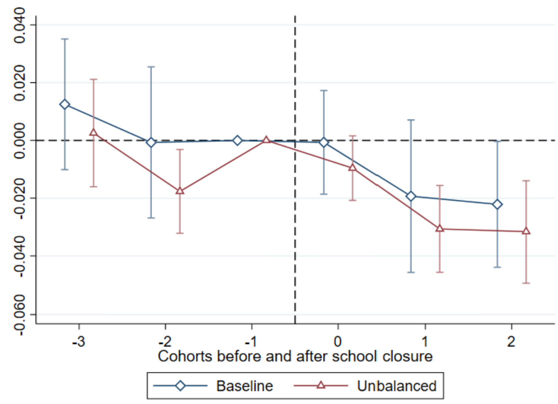
Another approach to constructing the sample involves selecting the same school grade both in the year of school closure and in preceding years. For instance, in the example of the main text, I can create a comparable sample by choosing the third-highest grade from 1998 to 2003. Then, students in the 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 Closures on Educational and Labor Market Outcomes: Balanced and Unbalanced Samples

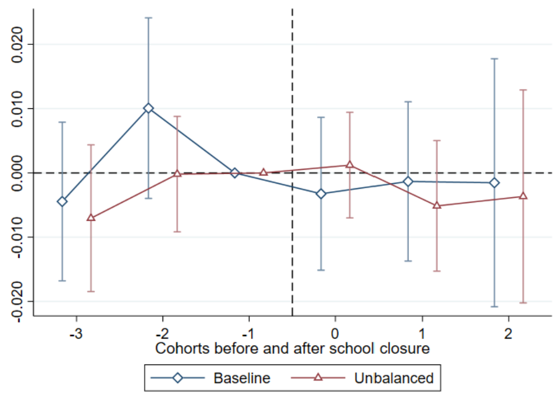
(a) High school graduation



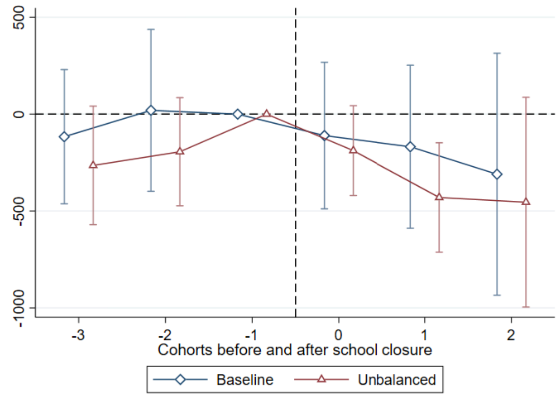
(b) Any college enrollment



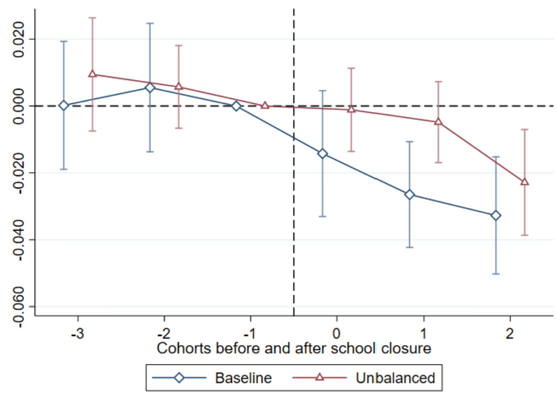
(c) Four-year college completion



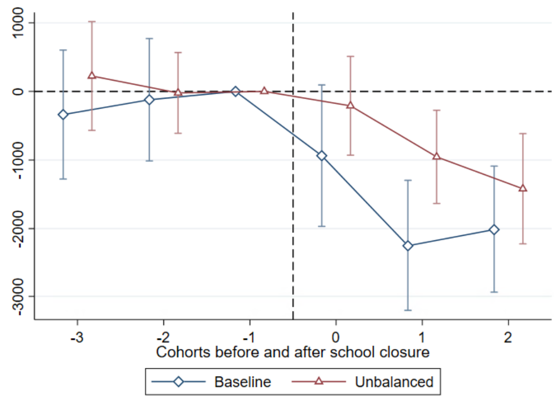
(d) College quality



(e) Employment



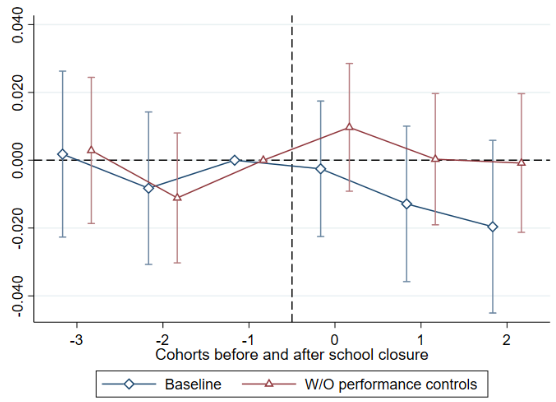
(f) Yearly earnings



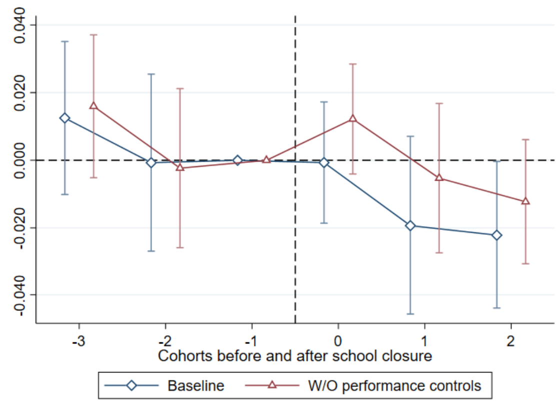
Notes: The figures overlay the coefficients, γ_c, and 95% confidence intervals from equation (4), using either baseline (balanced) or unbalanced sample. The unbalanced sample includes closed schools having fewer than 3 grades. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts—those who graduated within three years prior to the closure and those enrolled at the time of the 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 Closures on Educational and Labor Market Outcomes: With and Without Controlling for Performance Measures

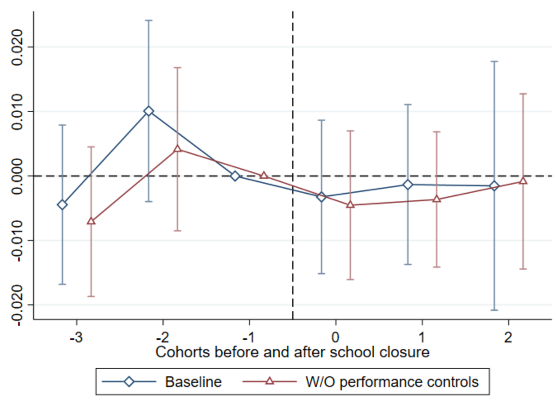
(a) High school graduation



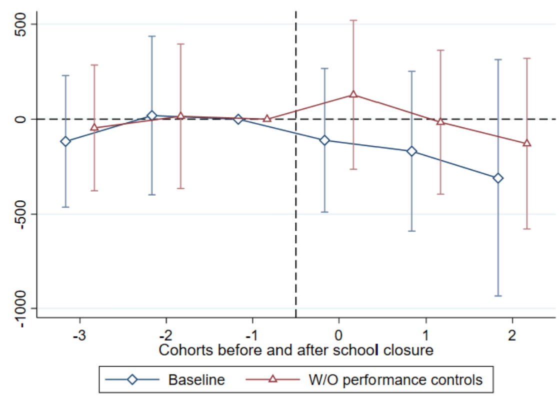
(b) Any college enrollment



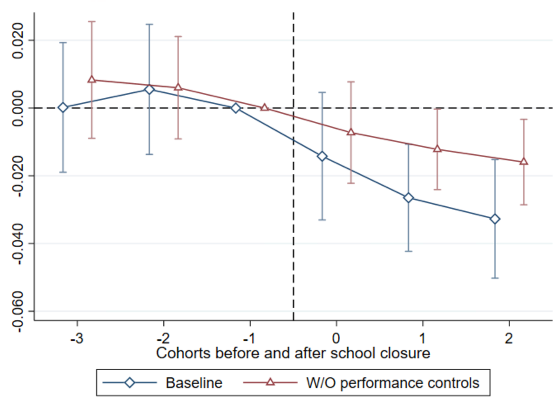
(c) Four-year college completion



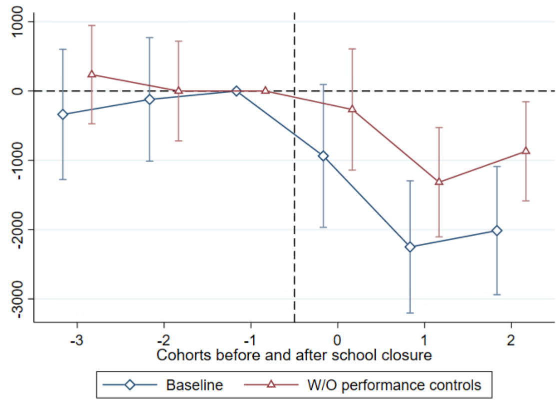
(d) College quality



(e) Employment



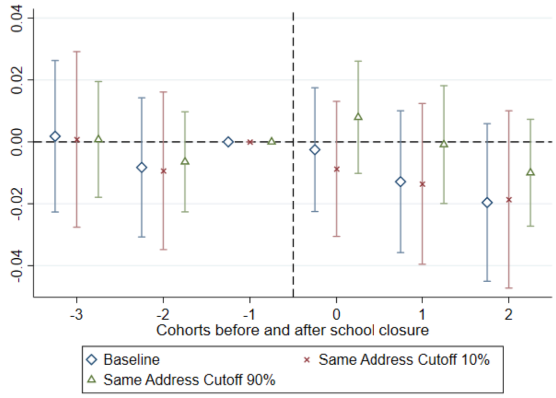
(f) Yearly earnings



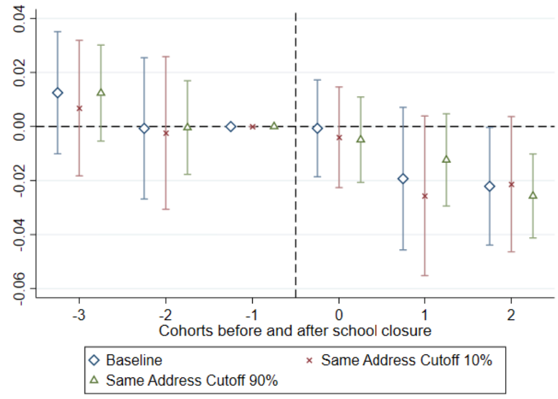
Notes: The figures overlay the coefficients, γ_c, and 95% confidence intervals from equation (4) with and without controlling for standardized math and reading scores, and standardized absence rate. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts—those who graduated within three years prior to the closure and those enrolled at the time of the 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 Closures on Educational and Labor Market Outcomes: Excluding Same Address School Openings with Different Cutoffs (1)

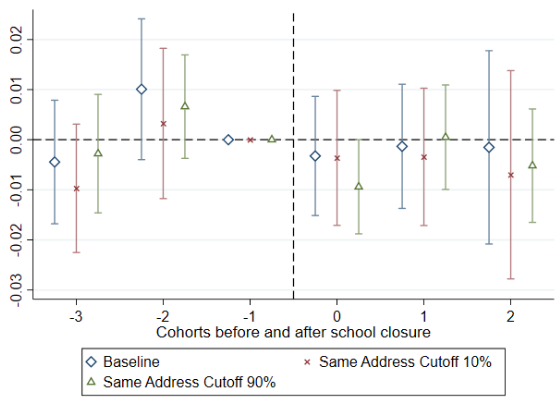
(a) High school graduation



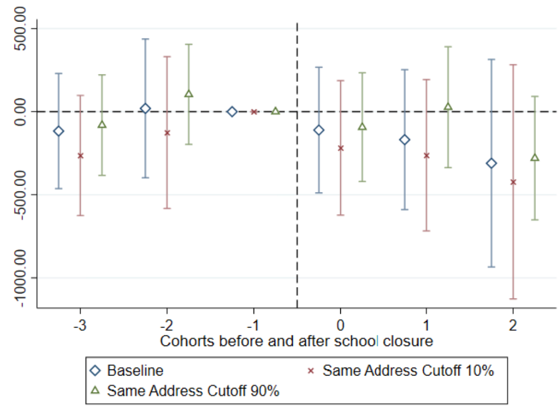
(b) Any college enrollment



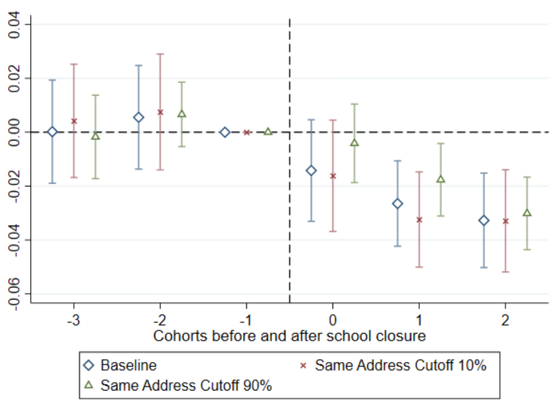
(c) Four-year college completion



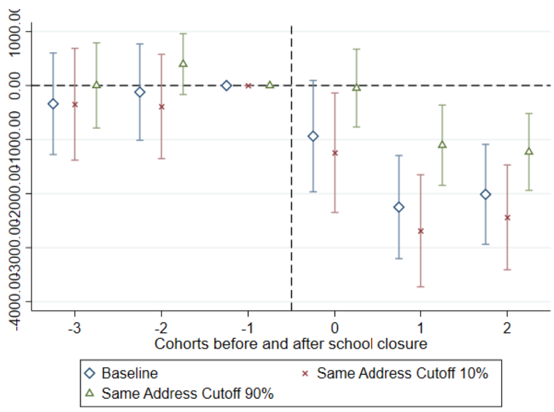
(d) College quality



(e) Employment



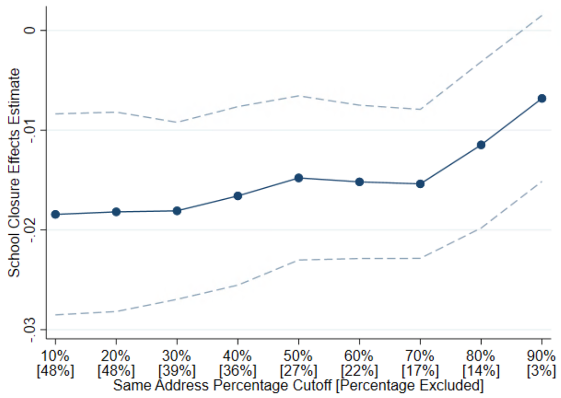
(f) Yearly earnings



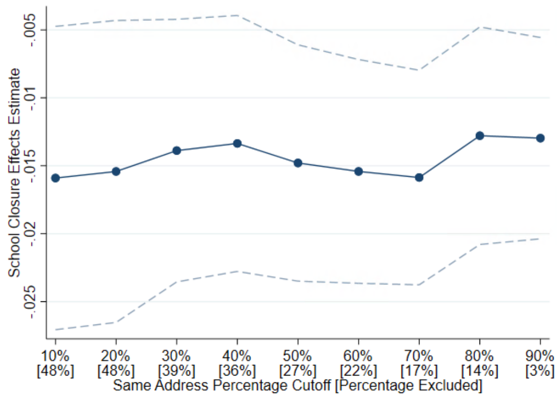
Notes: The figures overlay the coefficients, γ_c, and 95% confidence intervals from equation (4), using either the baseline sample or alternative cutoffs based on the proportion of displaced students observed at the same address after closure. A cutoff of 10% (90%) means that schools where more than 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—those who graduated within three years prior to the closure and those enrolled at the time of the 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 Closures on Educational and Labor Market Outcomes: Excluding Same Address School Openings with Different Cutoffs (2)

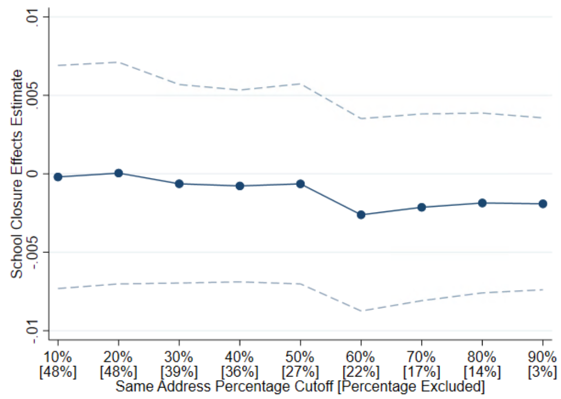
(a) High school graduation



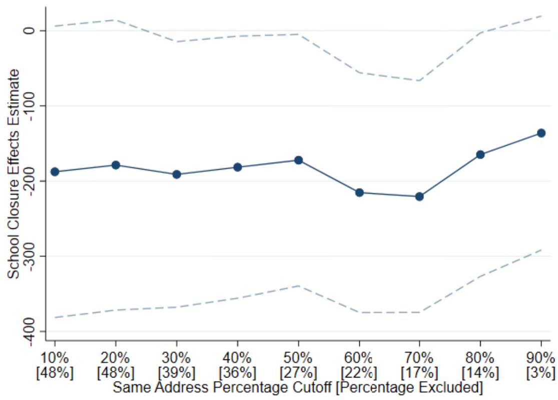
(b) Any college enrollment



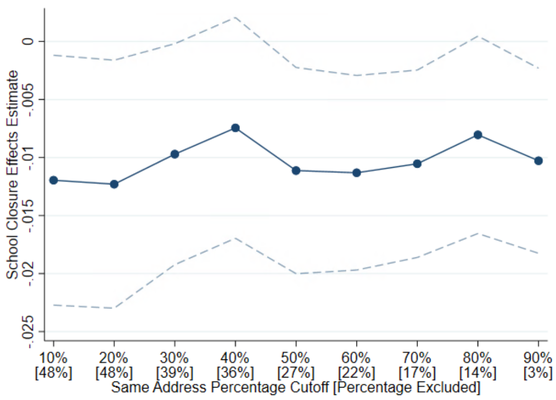
(c) Four-year college completion



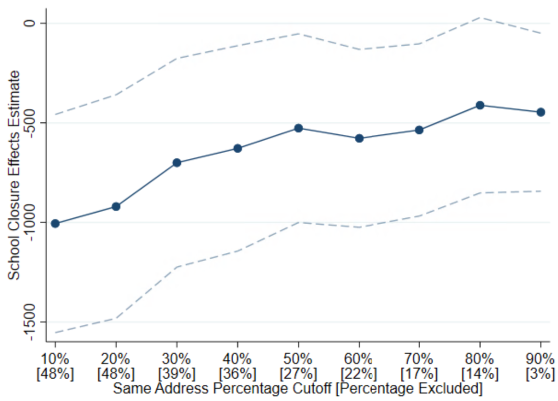
(d) College quality



(e) Employment



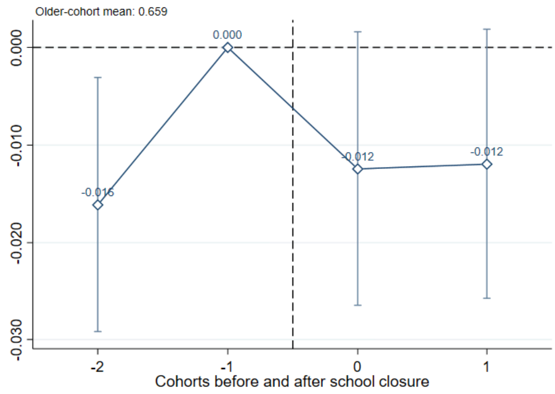
(f) Yearly earnings



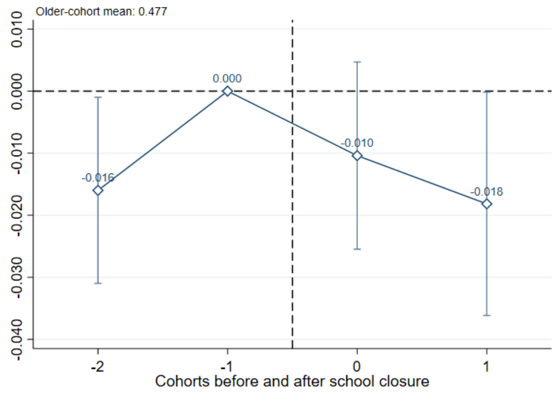
Notes: The figures overlay the coefficients, γ, and standard errors from equation (3) using a sample excluding closed schools where displaced students are observed at the same address after closure, applying different cutoffs. The x-axis shows the percentage cutoff for the proportion of displaced students, 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—those who graduated within three years prior to the closure and those enrolled at the time of the 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 Closures on Educational and Labor Market Outcomes: Alternative Way of Cohort Construction

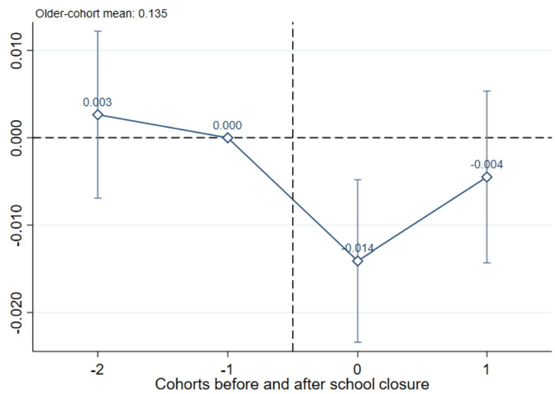
(a) High school graduation



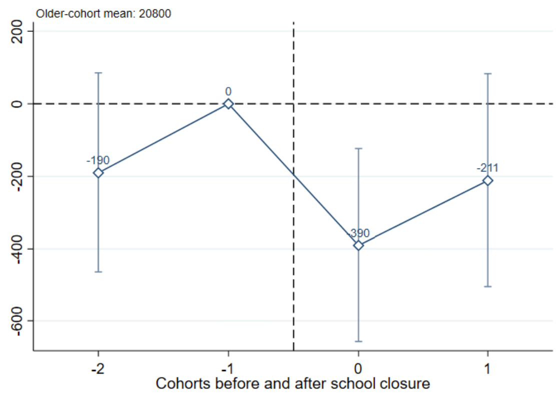
(b) Any college enrollment



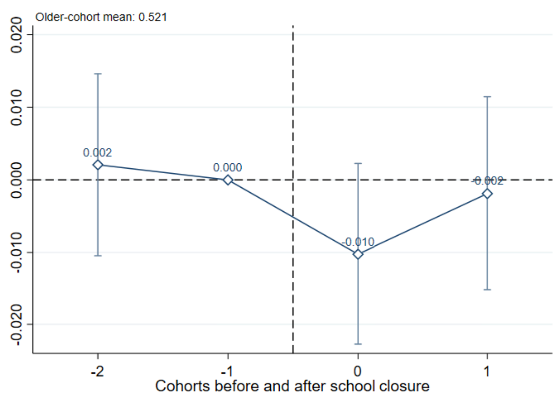
(c) Four-year college completion



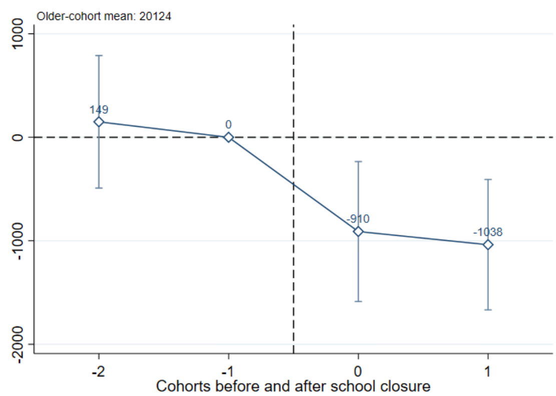
(d) College quality



(e) Employment



(f) Yearly earnings



Notes: The figures present the coefficients, γ_c, and 95% confidence intervals from equation (4), with an 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—those who graduated within three years prior to the closure and those enrolled at the time of the 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 Closures on Educational and Labor Market Outcomes: Using Balanced Sample

Panel A: Educational Outcomes				
	(1) Graduate HS	(2) Enroll Any College	(3) UG Degree	(4) College Quality
Closed School	-0.009	-0.018**	-0.004	-154
× Younger Cohorts	(0.007)	(0.008)	(0.004)	(131)
Observations	105,061	106,222	106,222	105,061
School FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Mean of the Older Cohort	0.666	0.486	0.142	21,177

Panel B: Labor Market Outcomes		
	(1) Employment	(2) Yearly Earnings
Closed School	-0.026***	-1,553***
× Younger Cohorts	(0.007)	(331)
Observations	106,222	106,222
School FE	X	X
Matched group × Year FE	X	X
Mean of the Older Cohort	0.518	19,635

Notes: The table presents the coefficients, γ, and standard errors from equation (3), using a balanced sample (i.e., schools with at least three grades). 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. "Graduate HS" and "UG Degree" refer to high school graduation and four-year college completion, respectively. 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

Table B.5: Long-Run Effects of School Closures on Educational and Labor Market Outcomes: Alternative Sets of 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 at the 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 the 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) shows the share of students in the long-run analysis sample—disaggregated by younger and older cohorts—who appear in the data each year after school closure, and the trends are similar across the groups. 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.4] 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 on 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.6. The estimates show that younger cohorts from closed schools are 0.2 percentage points less likely to enroll in college out-of-state relative to students from matched control schools, although 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.5). 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.5]

[B.4] 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.7. While these Lee bounds cover a range of estimates, the bounds exclude zero for all the outcomes except four-year college completion.

[B.5] 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

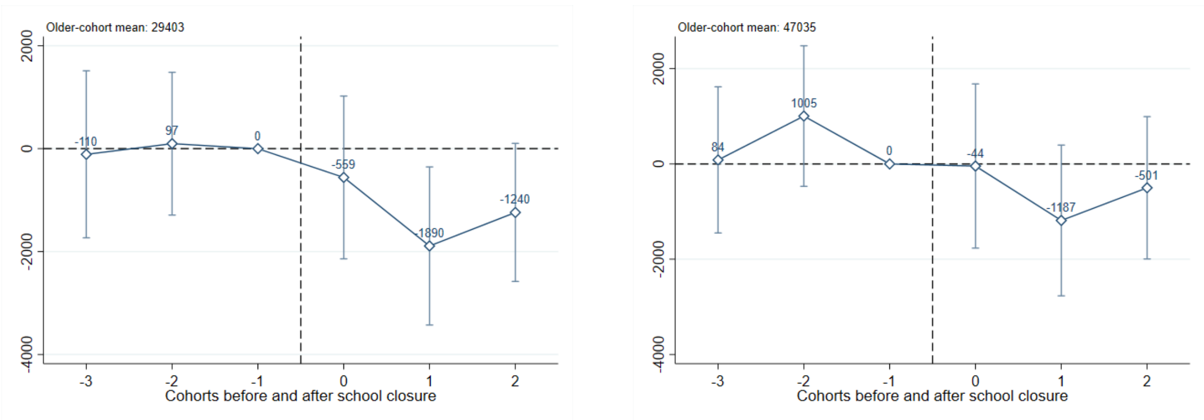
Thirdly, using a school quality measure based on their highest education level and institution, I find consistent results showing a decrease in expected earnings among the sample of individuals. Lastly, I perform a bounding exercise with the non-zero earning samples, attributing all the decrease in employment 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.7, are mostly in the negative range.[B.6] 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.6] 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 Closures on Earnings: Different Measures

(a) Non-Zero Yearly earnings **(b)** Potential full-time earnings



Notes: The figures present the coefficients, γ_c, and 95% confidence intervals from equation (4). Panel (a) includes samples with positive earnings. Panel (b) includes 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—those who graduated within three years prior to the closure and those enrolled at the time of the 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.6: Long-Run Effects of School Closures on Out-of-State Post-Secondary Education Enrollment

	Out-of-State College Enrollment
Closed School × Younger Cohorts	-0.002
	(0.002)
Observations	164,497
School FE	X
Matched group × 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 interaction between the indicator that denotes closed schools and the indicator that denotes 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. Standard errors are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.10

Table B.7: Long-Run Effects of School Closures 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 the 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 at the 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 when students move to higher-performing schools, which is 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 on behavioral issues. In terms of heterogeneity, Steinberg and MacDonald (2019) find that the

[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 were 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 a 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.

negative behavioral impacts are larger for students who move to better-performing schools and travel longer distances—both of them are 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. The paper finds 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.

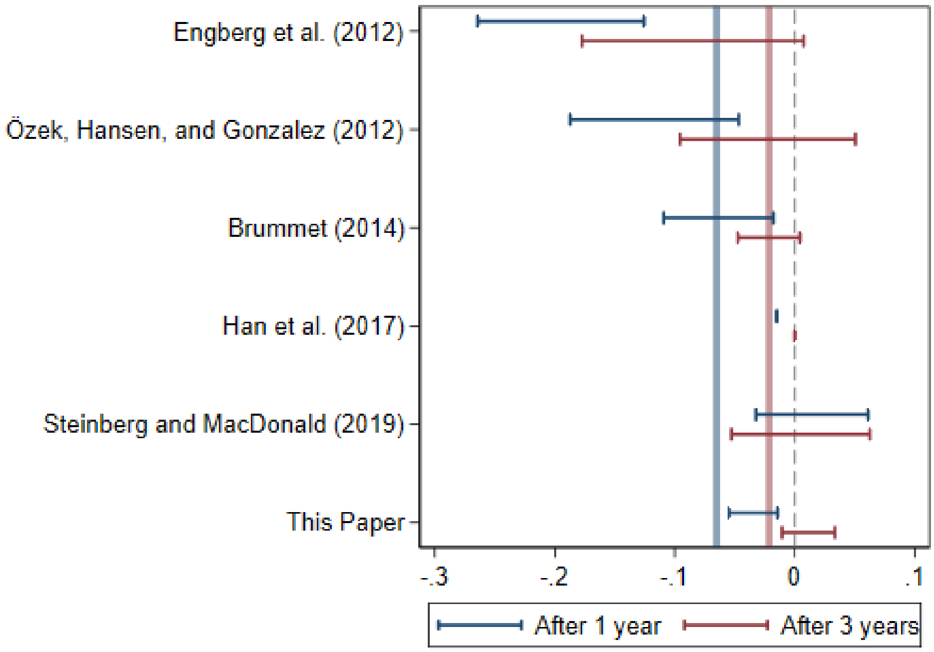
Some studies examine school closures in different contexts. Performance-based school closures have been implemented by state education agencies targeting charter schools, as well as by urban school districts—such as New York City—within the traditional public school system. Carlson and Lavertu (2016) 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 absences, 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 footnote 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 of 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 years.

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 is $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	2015	✓			✓					2	1
ALAMO EL	WICHITA FALLS ISD	2014	✓	✓		✓					3	1
AMBER TERRACE EL	DESOTO ISD	2015	✓		✓						2	1
ARLINGTON PARK COMMUNITY LEARNING	DALLAS ISD	2012	✓								1	1
AUSTIN MIDDLE	BEAUMONT ISD	2014	✓	✓	✓						3	1
BARBERS HILL MIDDLE	BARBERS HILL ISD	2014	✓	✓							2	1
BARWISE J H	WICHITA FALLS ISD	2014		✓			✓				2	1
BELT LINE INT	CEDAR HILL ISD	2011			✓						1	1
BONHAM EL	GRAND PRAIRIE ISD	2010		✓							1	1
BREWER EL	SAN ANTONIO ISD	2015									0	0
CARNAHAN EL	PHARR-SAN JUAN-ALAMO ISD	2012	✓								1	1
CASA LINDA EL	CORPUS CHRISTI ISD	2011									0	0
CITY PARK EL	DALLAS ISD	2012	✓								1	1
CLARKSVILLE MIDDLE	CLARKSVILLE ISD	2015									0	0
COLLEGE HEIGHTS EL	ABILENE ISD	2012	✓		✓						2	1
COLLINSVILLE INT	COLLINSVILLE ISD	2015									0	0
CORONADO ESCOBAR EL	EDGEWOOD ISD	2013			✓	✓					2	1
CRAWFORD EL	HOUSTON ISD	2011	✓								1	1
CROCKETT EL	GRAND PRAIRIE ISD	2010		✓							1	1
CROCKETT EL	MCALLEN ISD	2011	✓			✓					2	1
CROCKETT MIDDLE	PARIS ISD	2010					✓				1	1
D A HULCY MIDDLE	DALLAS ISD	2012	✓								1	1
D U BUCKNER EL	PHARR-SAN JUAN-ALAMO ISD	2014	✓								1	1
DECATUR INT	DECATUR ISD	2010									0	0
DEWEYVILLE MIDDLE	DEWEYVILLE ISD	2011			✓						1	1
DIRKS-ANDERSON SCH	FT DAVIS ISD	2013						✓			1	1
DODSON EL	HOUSTON ISD	2014	✓								1	1
DUBLIN J H	DUBLIN ISD	2013				✓					1	1
E O SMITH EL	HOUSTON ISD	2011	✓								1	1
EAGLE EL	CULBERSON COUNTY - ALLAMOORE ISD	2015			✓						1	1
EAST SIDE EL	SAN FELIPE DEL RIO CISD	2011									0	0
ELECTRA J H	ELECTRA ISD	2013	✓		✓						2	1
ESTACADO J H	PLAINVIEW ISD	2013									0	0
FANNIN EL	GRAND PRAIRIE ISD	2012		✓							1	1
FANNIN EL	ABILENE ISD	2010	✓		✓						2	1
FEHL EL	BEAUMONT ISD	2011	✓	✓	✓						3	1
FIELD EL	BEAUMONT ISD	2011	✓	✓	✓						3	1
FOWLER EL	KILLEEN ISD	2014			✓						1	1
FRANKLIN EL	PHARR-SAN JUAN-ALAMO ISD	2011	✓								1	1
GOLDEN RULE EL	DENISON ISD	2014			✓	✓					2	1
GOLIAD INT	BIG SPRING ISD	2012									0	0
GORDON EL	HOUSTON ISD	2012	✓		✓						2	1
GRIMES EL	HOUSTON ISD	2011	✓		✓						2	1
HAMLIN MIDDLE	HAMLIN ISD	2014									0	0
HART EL	HART ISD	2015									0	0
HIGHLANDS EL	LA MARQUE ISD	2013							✓	✓	2	1
HOUSTON EL	EL PASO ISD	2010				✓					1	1
HOUSTON EL	WICHITA FALLS ISD	2014	✓	✓		✓					3	1
HUTCHESON J H	ARLINGTON ISD	2015	✓			✓					2	1
INTER-CITY EL	LA MARQUE ISD	2013							✓	✓	2	1
J H ROWE INT	JASPER ISD	2014									0	0
JOHNSON EL	GRAND PRAIRIE ISD	2013	✓	✓							2	1
KENNEDY MIDDLE	GRAND PRAIRIE ISD	2012		✓							1	1
LA MARQUE MIDDLE	LA MARQUE ISD	2013							✓	✓	2	1
LAKE AIR INT	WACO ISD	2012			✓						1	1
LAMAR EL	CORPUS CHRISTI ISD	2010	✓	✓		✓					3	1
LAYNE EL	DENISON ISD	2012			✓	✓					2	1
LEE MIDDLE	GRAND PRAIRIE ISD	2010		✓							1	1
LEONEL TREVINO EL	PHARR-SAN JUAN-ALAMO ISD	2010	✓								1	1
LONE STAR EL	DAINGERFIELD-LONE STAR ISD	2010			✓						1	1
LUCAS EL	BEAUMONT ISD	2010	✓	✓	✓						3	1
MARFA EL	MARFA ISD	2013									0	0
MARTIN EL	BEAUMONT ISD	2010	✓	✓	✓						3	1
MARTIN EL	ROBSTOWN ISD	2012									0	0
MCALLISTER INT	BAY CITY ISD	2011			✓						1	1
MCDADE EL	HOUSTON ISD	2011	✓		✓						2	1
MEADOWBROOK EL	WACO ISD	2012			✓						1	1
MERIDITH DUNBAR EL	TEMPLE ISD	2013				✓					1	1
MORTON EL	MORTON ISD	2014									0	0
MORTON J H	MORTON ISD	2014									0	0
N W HARLLEE EL	DALLAS ISD	2012	✓								1	1
NAPPER EL	PHARR-SAN JUAN-ALAMO ISD	2014	✓								1	1
NELSON EL	SAN ANTONIO ISD	2015	✓								1	1
NORTH WACO EL	WACO ISD	2012			✓						1	1
OGDEN EL	BEAUMONT ISD	2011	✓	✓	✓						3	1
PEARCE MIDDLE	AUSTIN ISD	2014								✓	1	1
POINT COMFORT EL	CALHOUN COUNTY ISD	2011			✓						1	1
POWELL POINT EL	KENDLETON ISD	2010								✓	1	1
PREMONT J H	PREMONT ISD	2011			✓					✓	2	1
PRESCOTT EL	CORPUS CHRISTI ISD	2012	✓	✓		✓					3	1
PRICE EL	BEAUMONT ISD	2011	✓	✓	✓						3	1
RED OAK INT	RED OAK ISD	2015				✓					1	1
RHOADS EL	HOUSTON ISD	2011	✓		✓						2	1

RINGGOLD EL	GOLD BURG ISD	2011									0	0
ROTAN J H	ROTAN ISD	2015									0	0
RYAN MIDDLE	HOUSTON ISD	2013	✓								1	1
SAM HOUSTON EL	GRAND PRAIRIE ISD	2010	✓	✓							2	1
SAN AGUSTINE MIDDLE	SAN AGUSTINE ISD	2013									0	0
SANDERSON EL	TERRELL COUNTY ISD	2013									0	0
SCOTT EL	HOUSTON ISD	2011	✓								1	1
SEAGRAVES EL	SEAGRAVES ISD	2013									0	0
SEAGRAVES J H	SEAGRAVES ISD	2013									0	0
SIMMS EL	LA MARQUE ISD	2013							✓	✓	2	1
STAR SCHOOL	STAR ISD	2014							✓		1	1
STEELE EL	SAN ANTONIO ISD	2015	✓								1	1
STEVENSON EL	HOUSTON ISD	2011	✓		✓						2	1
SUL ROSS EL	WACO ISD	2012			✓						1	1
THREE RIVERS MIDDLE	THREE RIVERS ISD	2013		✓							1	1
TRUMAN MIDDLE	EDGEWOOD ISD	2011	✓		✓						2	1
UNITED D D HACHAR EL	UNITED ISD	2011									0	0
UNIVERSITY MIDDLE	WACO ISD	2012			✓						1	1
VAN HORN J H	CULBERSON COUNTY - ALLAMOORE ISD	2013			✓						1	1
VIKING HILLS EL	WACO ISD	2012			✓						1	1
VILAS EL	EL PASO ISD	2015	✓			✓					2	1
W W WHITE EL	SAN ANTONIO ISD	2015	✓								1	1
WEST INT	CEDAR HILL ISD	2015									0	0
WESTLAWN EL	LA MARQUE ISD	2013							✓	✓	2	1
WOODSON MIDDLE	HOUSTON ISD	2011		✓			✓				2	1
WYNN SEALE ACADEMY OF FINE ARTS	CORPUS CHRISTI ISD	2011									0	0
ZUNDELOWITZ MIDDE SCHOOL	WICHITA FALLS ISD	2014	✓	✓		✓					3	1
Statistics		46	23	35	16	3	1	6	8	138	86	

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