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

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**Abstract**

Each year, over a thousand public schools in the US close due to declining enrollments and chronic low performance, displacing hundreds of thousands of students. Using Texas administrative data and empirical strategies that use within-student across-time and within-school across-cohort variation, I explore the impact of school closures on students' educational and labor market outcomes. The findings indicate that experiencing school closures results in disruptions in both test scores and behavior. While the drop in test scores is recovered within three years, behavioral issues persist. This study further finds decreases in post-secondary education attainment, employment, and earnings at ages 25–27. These impacts are particularly pronounced among students in secondary education, Hispanic students, and those from originally low-performing schools and economically disadvantaged families.

**JEL**: H40, I21, I28

**Keywords**: school closure, demographic shift, low-performing school, student mobility, human capital development, long-run effect

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# 1 Introduction

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

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

This paper studies the impact of school closures on both students' short- and long-run outcomes, as well as explore heterogeneity across student and school characteristics. To answer these questions, I utilize Texas longitudinal and individual-level administrative data and the difference-in-differences method. In the Texas data, I connect individuals' K-12 education records to post-secondary and labor market outcomes, and thus I observe both short-run effects on test scores and behavioral outcomes, as well as long-run effects on high school graduation, college attendance, college completion, college quality, employment, and wages. In difference-in-differences analysis, I compare within-student and across-cohort changes in outcomes following school closure to those of students from control schools that are matched based on similar student and school characteristics.

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

By analyzing within-student variation before and after school closures, I find an immediate disruption in learning. Specifically, math and reading scores drop by 0.033 and 0.034 standard deviations, respectively. Days of absence and disciplinary action increase by 0.13 days (1.8% increase relative to the pre-closure mean) and 0.36 days (15%) respectively. Although the effects on test scores dissipate within three years, the impact on the days of absence and disciplinary action persist or accumulate over time. This increase in days of disciplinary action is primarily driven by out-of-school suspensions and expulsions rather than in-school suspensions. It is particularly concerning in light of recent studies presenting the long-term negative consequences of disciplinary actions and school absences (Bacher-Hicks, Billings, and Deming 2019; Cattan et al. 2023; Liu, Lee, and Gershenson 2021; Weisburst 2019). Additionally, I find no evidence of students leaving the Texas public school system after experiencing school closure.

I use within-school across-cohorts variation to identify long-run outcomes, comparing younger cohorts who experience school closures to older cohorts who do not. I find that by age 26, experiencing school closure leads to a reduction in high school graduation rates by 1 percentage point (1.4%), the attendance rate for four-year colleges decreases by 1.2 percentage points (4.8%), and the four-year college completion rate decreases by 0.7 percentage points (4.7%). Furthermore, the closure leads to a reduction in employment rates by 0.7 percentage points (1.3%) and a decrease in yearly earnings by $793 (3.4%) at ages 25-27. These effects are robust whether I control for demographic and performance variables and use a balanced or unbalanced sample. I find no evidence of differences in test scores and attendance between younger and older cohorts before school closures.

I investigate heterogeneity across student demographics and school characteristics. I find that the negative effects are more pronounced among Hispanic students, those from economically disadvantaged families, and those in higher grades when school closes. While the drop in test scores after closure is generally recovered, students in higher grades or those moving to worse-performing schools could not recover over time. The increase in behavioral issues is concentrated among Black and Hispanic students, those from economically disadvantaged families, and those moving to better-performing schools. The increase in days of absence is concentrated on urban school closures. Similarly, long-run outcomes present more substantial negative effects among Hispanic students, those from economically disadvantaged families, those in higher grades, and closures from urban schools.

I further explore the school-level changes for displaced students. By analyzing within-student variation before and after school closures, I find an immediate drop in peer quality measured by yearly test scores. School average math and reading scores drop by 0.12 and 0.14 standard deviations, respectively. However, expected school quality, as measured by the quality of the school *before* the closures, shows the opposite pattern. Displaced students experience increases in expected school average test scores. In other words, students are supposed to have better school quality after displacement when school closures are planned, but actual peer qualities after moving are worse than those of the original schools. Further descriptive analyses suggest that this is mainly due to the rezoning of attendance areas following closures, which leads to a decline in the quality of students attending the receiving schools. Additionally, I find that school-level employment decreases by 2.3 full-time-equivalent (FTE) positions per 1000 students, with most of the decrease occurring in the number of teachers and teaching support staff.

This study contributes to three strands of literature: school closure, student mobility, and long-run effects of childhood disruptions. I advance the literature on the effects of school closures in two key directions.[1] First, I examine the long-run effects while previous studies have focused mainly on short-run outcomes, such as test scores several years after school closures (Beuchert et al. 2018; Brummet 2014; Engberg et al. 2012; Gordon et al. 2018; Larsen 2020; Özek, Hansen, and Gonzalez 2012; Steinberg and MacDonald 2019; Taghizadeh 2020; Torre and Gwynne 2009).[2] Some previous research shows the adverse effects on test scores of displaced students tend to dissipate over time, leading to the conclusion that the adverse effects do not last (e.g., Brummet 2014; De Witte and Van Klaveren 2014; Engberg et al. 2012; Özek, Hansen, and Gonzalez 2012). However, my paper presents a different narrative: certain student groups, particularly those in secondary education, struggle to recover from declining test scores, while overall enduring adverse effects on behavior are observed. Furthermore, these persistent effects translate into long-term negative effects on both higher education and labor market outcomes.

Another contribution to school closure literature is to explore heterogeneous effects. This involves examining differences across various factors, such as urban and rural areas, original school quality, school quality changes, and grades and demographics of students. Previous studies focus mainly on a single urban school district, analyzing dozens of closures (e.g., Carlson and Lavertu 2016; Engberg et al. 2012; Kirshner, Gaertner, and Pozzoboni 2010; Larsen 2020; Steinberg and MacDonald 2019).[3] In this study, I use administrative data from Texas—a large and diverse state with ample urban and rural populations—to conduct a comprehensive analysis of school closures. This allows me to document backgrounds and compare the consequences

[1] For extensive interdisciplinary review on school closure research, see Tieken and Auldridge-Reveles (2019)

[2] While a few studies investigate the long-term impacts of school closures, these studies focus on different settings than the one examined in this study. In the context of Chile, Grau, Hojman, and Mizala (2018) find that school closures led to an increase in dropout rates (1.8-2.5 pp) and a decline in student retention (3.9-4.4 pp). Using high school closure in Milwaukee public school district, Larsen (2020) show, while it is statistically insignificant, a decrease in high school graduation rates (7.5 pp) as a result of the closures.

[3] An exception is Brummet (2014) which uses Michigan public school data and highlights the importance of school quality changes for displaced students.

of closures across different school and student characteristics. The findings highlight that the adverse effects of closures are concentrated on specific groups of students and types of schools, with variations in the aspects where students are mainly affected.

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

Finally, this study contributes to the broad literature on the long-run effects of childhood intervention/disruption and school inputs. Previous studies investigate long-run effects of preschool programs such as Perry Preschool and Head Start (Garces, Thomas, and Currie 2002; Heckman, Pinto, and Savelyev 2013) and the experience of adverse shocks such as natural disasters or school shootings (Cabral et al. 2021; Sacerdote 2012), as well as teacher, peer, and school quality (Carrell, Hoekstra, and Kuka 2018; Chetty, Friedman, and Rockoff 2014; Hyman 2017). My research emphasizes once again the significance of childhood experience by showing that a policy intervention could be a negative shock in childhood. It underscores the need for careful consideration in policy-making regarding school closures, given the long-lasting adverse impacts on displaced students.

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

## 2 Background: School Closures in Texas

**Defining school closure** To identify schools that have closed down, I rely on the list of school closures from AskTED, the online Texas Education Directory (TEA 2022), which is compiled based on reports from school districts. To be considered "closed," a school has to be listed on the TEA closure list and also disappear from the ERC dataset. My analysis covers the period from 1998 to 2015 for short-run analysis and 1998 to 2003 for long-run analysis. I only consider school closures from non-charter instructional campuses and non-charter regular and independent districts.[4] 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. Figure 1 presents locations of school closures used in analyses. Closed schools are distributed all over Texas, concentrating more on populated areas.

In the years from 1998 to 2015, I have identified and documented the reasons behind 274 out of 470 school closures. My primary sources of information include local news articles and public information requests directed towards individual school districts. To the best of my knowledge, this is the first attempt to construct statewide statistics about reasons for closures.[5] It is important to note that school closure decisions often stem from a combination of factors. For instance, a decline in enrollment is frequently accompanied by budgetary constraints and the presence of aging school facilities. Furthermore, other aspects may be taken into account during the decision-making process, even if those are not reported as the main drivers of the closures.[6]

To facilitate a comprehensive understanding of the closure reasons, I categorize identified reasons into several distinct groups, including chronically low performance, financial constraints, enrollment changes, aging school infrastructure, district-level renovation including closures and rezoning, school reform, and coding changes (see Figure A.1). In this statistic, school closure

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

[5] A full list of information can be found in the supplementary material.

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

may be attributed to multiple reasons. While previous literature describing school closures emphasizes closures due to low performance (e.g., Delpier 2021; Dowdall 2011; Jack and Sludden 2013; Tieken and Auldridge-Reveles 2019), it shows that the majority of closures for non-charter public schools are primarily driven by enrollment-related factors, encompassing tight budgets, declining enrollment, aging school buildings, and restructuring district and school, accounting for about 90% of identified reasons for closures. Closures primarily associated with low performance constitute 3 percent of the cases.[7] This also challenges the conventional understanding of school closures, which often categorizes closures into a dichotomy of urban-low performance and rural-low enrollment frameworks (Tieken and Auldridge-Reveles 2019).

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

[7] I divide reasons 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 reasons for 86 out of 146 closures, 71 out of 177 closures, and 110 out of 147 closures.

[8] 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.

administrative convenience.[9][10]

# 3 Data

I use individual-level Texas administrative data sets through the University of Houston Education Research Center (UH ERC). The data sets include three sources: the Texas Education Agency (TEA), the Texas Higher Education Coordinating Board (THECB), and the Texas Workforce Commission (TWC).

TEA data includes K-12 education records in public schools starting from the academic years 1994-1995, containing information on attendance, disciplinary actions, high school graduation, and testing. The data further include student characteristics including age, sex, race/ethnicity, English second language status, special education status, and eligibility for free or reduced-price lunch. It also contains campus and district information, such as school type and charter type. Using TEA data, I construct four outcome variables at an annual level: (1) the number of days of absence; (2) the number of days in disciplinary action;[11] (3) standardized math and reading scores;[12] and (4) high school graduation by 26.

THECB data include all public and most private post-secondary education data in Texas.[13]

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

[10] To address potential concerns related to coding changes and school closures without physical closures, I take an additional estimation potentially excluding not physically closed schools in Section 5.3. Specifically, using NCES common core of data, I exclude closed schools from the analysis if a new school appears at the same address in the year immediately following the school closures. The estimation results are similar whether exclude those schools or not.

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

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

[13] 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

The data are linked to TEA data at the individual level. I construct two post-secondary education outcome variables using THECB data: (1) an indicator for ever attending a Texas four-year college by age 26; (2) an indicator for earning a bachelor's degree from a Texas post-secondary institution by age 26.[14]

TWC data includes quarterly individual data on employment, industry, and earnings for all workers covered by the Unemployment Insurance program.[15] The data is linked to TEA and THECB data at the individual level. Using TWC data, I construct the following four outcome variables at ages 25–27: (1) an indicator for being employed (measured by quarterly level); (2) average annual real earnings (measured in 2020 dollars); (3) average non-zero annual real earnings (i.e., average annual earnings given positive earnings); (4) earnings-based college quality following Chetty, Friedman, and Rockoff (2014).[16]

One limitation of the THECB and TWC data is that the data coverage is restricted to Texas. If someone goes out of Texas, I cannot observe their out-of-state educational or workforce outcomes and thus cannot distinguish whether they have moved out of state or did not attend college (in the case of education) or are non-employed (in the case of labor market outcomes). As described in Section 4.3, however, it is improbable that this will significantly bias the results.

# 4 Empirical Strategy

To estimate the causal effects of school closure on student outcomes, I use two difference-in-differences models to compare the changes in outcomes among students affected by school closures to those who are not. Specifically, I use within-student across-time variation for

and colleges (available from 2004 onward). See http://www.txhighereddata.org/Interactive/CBMStatus/ for additional information on participating institutions.

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

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

[16] Using 1982-1984 birth cohorts, I group individuals by the higher education institution they graduated by age 26. I pool individuals who were not enrolled in any college by age 26 together in a separate no college category. For each college, I construct the average earnings of the students when they are ages 29-31.

short-run analyses and within-school across-cohorts variation for long-run analyses. 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

To address concerns regarding differing trends between schools that have closed and those that have not, I choose control schools that share similar observable characteristics with the closed school at the time of closure using a nearest-neighbor matching method.

To begin, I group schools in the same year, the same school type (e.g. elementary schools are only matched with other elementary schools), and the same locale following the NCES locale category, which has 8 categories from 1998-2005 and 12 categories from 2006-2015 based on population size and proximity to populous areas.[17] Once the schools are grouped, I use nearest-neighbor matching within the group using the following school characteristics at the time of closure: the share of Black students, the share of Hispanic students, the share of students receiving free or reduced-price lunch, and the share of students with other economic disadvantages.[18] Essentially, using a scale-invariant distance metric based on observable school characteristics, I calculate the distance among schools and identify the closest schools to each closed school. In the process, I exclude schools in the same district because of concerns about spillover effects. As discussed in Section 5.3, the results are not sensitive to the alternative matching strategies.

I choose one control school for each closed school without replacement. Table A.1 displays the summary statistics after the matching process. Columns (1)-(3) show averages of school and student characteristics from closed schools, matched control schools, and all schools, respectively. It presents that schools were closed all over Texas, but certain local categories

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

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

experienced disproportionate closures. Specifically, 47% of school closures occur in cities, while these cities account for 37% of all schools. Conversely, the urban fringes experienced 14% of all school closures, but accounted for 22% of all schools. Additionally, the type of school also shows uneven distribution, with elementary schools accounting for 66% of all closures while accounting for 52% of all schools. Moreover, Hispanic and economically disadvantaged students are more likely to experience school closures. Hispanic students account for 47% of students experiencing school closures while they account for 43% of all students. Students who are eligible to receive free or reduced-price lunch account for 63% of students experiencing school closures while they account for 49% of all students. Nearest-neighbor matching results confirm that the averages of closed schools are more similar to those of control schools than to the averages of all schools.

## 4.2 Short-Run Analysis

I analyze outcome variables observed both before and after the closure: absence rate, days of disciplinary actions, and math and reading scores. The analysis begins with the sample including students enrolled in closed and control schools at the time of closure. As I discuss in Section 3, the available sample varies across outcome variables and years of closure: 3-10th grades for behavior and 5-9th grades for test scores from 470 schools. I further restrict the sample to those who are observed in the data three years before and two years after the school closure. In the main analysis, I use all available students in each outcome variable. My final short-run analysis sample includes 61,151 students for test scores and 122,911 students for behavior.[19]

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

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

where $Y_{isgt}$ is an outcome of student $i$ in relative year $t$ ($t = -1$ is the year preceding closure)

[19] As I discuss in Section 3, the disciplinary data is available from 1999, so the analysis sample is smaller than that of attendance, which is 100,797.

who was enrolled in school $s$ in match group $g$ at the time of closure. $Closure_s$ is a dummy variable taking 1 if the student $i$ is at a closed school at the time of closure. $Post_t$ is an indicator denoting observations after school closure. I include individual fixed effects, $\sigma_i$, and a full set of matched group-by-relative year fixed effects, $\kappa_{gt}$. Those account for time-invariant individual characteristics and match group specific trends respectively. $\beta$ is difference-in-differences estimator measuring the difference in the change in outcomes following a school closure between students from closed and matched control schools.

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

The regression equation takes the following form:

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

where $t \in (-3,3)$ is measured relative to the time of closure, and $\mathbf{1}_t$ is set to 1 when the relative time is $t$. Other variables are defined in the same way with equation (1). The $\rho_t$ are the difference-in-differences coefficients, which measure within-student change over time in outcomes compared to students in the matched control school. Thus, $\rho_t$ where $t \in (-3,-1)$ shows pre-trends between closed and matched control schools.

In the short-run event study format difference-in-differences analysis, I examine a balanced panel of students spanning three years before and four years after the school closure. The purpose of this approach is to remove any potential influence of composition changes that may arise from differential attrition, such as students leaving the Texas public school system after experiencing school closure to private schools or out-of-Texas.[20] To address concerns about a

[20] However, I left the third and fourth years after the school closure unbalanced when I examine the equation (1) including heterogeneity analysis since balancing those years restricts the sample to elementary students.

potential correlation between attrition from the school system and change in outcomes, I further investigate whether there is a differential pattern of attrition between closed and control schools. Additionally, I conduct a robustness check by running the same regression with an unbalanced sample.

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

## 4.3 Long-Run Analysis

I focus long-run analysis on outcomes only observed after the school closure in the TEA, THECB, or TWC data: high school graduation, four-year college attendance, bachelor's degree obtainment, school quality, employment, yearly earnings, and non-zero yearly earnings. Given that students' long-run outcomes are only observed after school closure, I cannot exploit within-student variation as it relates to changes before and after closure. Instead, I utilize variation across cohorts within school. Specifically, I compare cohorts enrolled in the school at the time of closure with cohorts who graduated within the last three years, relative to those at matched control schools.

I construct a sample of long-run analysis based on graduating cohorts using 146 closed schools between 1998 and 2003. I use six cohorts: the three highest grades experiencing school closure become three "younger cohorts", and three cohorts who potentially graduated within three years of school closure become three "older cohorts". For instance, suppose that an elementary school **A** with grades 1–5 closed at the end of the school year 2000. I first consider students in school **A** in grades 3-5 at the time of school closure (younger cohorts), and students in the same school in grades 3-5 three years before the school closure (older cohorts).[21] Thus, older cohorts would be in expected grades 6-8 at the year of school closure.I further restrict the long-run sample to the students of which test scores, absence rates, demographics (sex, race/ethnicity, English second language status, special education status) are observed. The final long-run sample experiencing school closure includes 24,221 students in 2–12 grades.[22]

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

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

where $Y_{iscg}$ is an outcome variable for student $i$ in cohort $c$ who was enrolled in school $s$ in match group $g$ at the time of the closure or three years before the closure. $Closure_s$ is a dummy variable denoting schools experiencing closure. I include school fixed effects, $\eta_s$, and cohort-by-match group fixed effects, $\lambda_{cg}$, which account for time-invariant school characteristics and flexibly match group specific cohort trends. I also control for student characteristics, $X_i$, including gender, race, English second language status, special education status, standardized math and reading scores, and standardized absence rate. To address variations in the significance of individual characteristics across schools, interaction terms between individual characteristics and school fixed effects are also controlled. $\gamma$ is the difference-in-differences estimator, measuring the difference in the change in outcomes across cohorts following a school closure between students from closed and matched control schools.

Like short-run effects, to ensure that my causal interpretation is valid, I make a standard

[21] 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, this translates to utilizing fourth grade students from 2000 to 2003. Then, fourth grade students from 2002 to 2003 represent younger cohorts, and students from 2000 to 2001 represent older cohorts. As illustrated in the Appendix Figure A.19, the outcomes using this alternative approach closely resemble those obtained from the baseline analysis.

[22] During K-12 education, on average, students in younger cohorts attended 5.54 schools (with a median of 5 schools), while students in older cohorts attended 4.65 schools (with a median of 4 schools).

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

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

where cohort $c \in (-3, 2)$ is measured relative to the time of closure, and $\mathbf{1}_c$ is set to 1 when the relative cohort is $c$. If $c \in \{0, 1, 2\}$, students are in the "younger cohort" (i.e. students in the school when closed; in the previous example of the school **A** having grade 1-5, 0, 1, 2 refers to grade 5, 4, 3 respectively), and if $c \in \{-3, -2, -1\}$, students are in the "older cohort" (i.e. students already graduated from the school when closed; in the previous example -3, -2, -1 refers to grade 8, 7, 6 respectively). $\pi_c$ is the difference-in-differences estimator, measuring differences between closed and control schools in cohort $c$ relative to the omitted cohort. The standard errors are clustered at the school-by-cohort level.[23]

In the long-run event-study format difference-in-differences analysis, I examine adjacent six cohorts around school closure assuming that these adjacent cohorts are similar except for the experience of school closure. One might still have concerns about systematically different moving-out patterns among the cohorts from closed schools *before* school closures compared to control schools.[24] To assuage the concern, I conduct a balance test across these cohorts. I use test scores and absence rates measured before the school closure as dependent variables to estimate equation (4). As depicted in Figure A.3, there are no significant differences in average test scores and absence rates across school cohorts.[25]

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

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

[25] Moreover, I estimate the same regression using short-run outcome variables one year after closure to see whether

# 5 Results

## 5.1 Short-Run Effects on Student Outcomes

Figure A.5 illustrates the raw trends of short-run analysis outcomes for closed and control schools around school closure. Sub-figures (a) and (b) show standardized math and reading scores. Prior to school closure, both closed and control schools exhibit comparable trends over the three-year period, with similar levels. The absolute raw difference remains consistently below 0.02 standard deviations. However, following school closure, a noticeable drop in the average test scores of closed schools emerges, leading to a divergence in the trends between closed and control schools. Sub-figures (c) and (d) depict days of absence and days of disciplinary action. These outcomes also demonstrate similar trends 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 are in both similar levels and trends before closures and closed schools deteriorate after experiencing school closure.

Figure 2 presents event study estimates, particularly plotting the coefficients and 95% confidence intervals of the coefficient $\rho_t$ from equation (2). First of all, there is no significant difference between closed and matched control schools before the school closures. Sub-figures (a) and (b) depict a decline of 0.04 and 0.03 standard deviations in standardized math and reading scores, respectively, following school closure. These scores subsequently recover to their initial levels within three years. In the analysis of days of absence in sub-figure (c), there is a 0.2 days increase in days of absence immediately after closure, which persists for four years post-closure.[26] School closures also result in a 0.3-day increase in the days of disciplinary actions immediately after closure, which further escalates to 0.9 days after four years of closure.

Given the significant increase in the number of days of disciplinary action following the school closure, I conduct a separate analysis for days of in-school suspensions, days of out-

I can observe changes in short-run outcomes for younger cohorts compared to older cohorts. As presented in Figure A.4, younger cohorts experience drops in test scores and an increase in days of absence while it is noisier than short-run analysis.

[26] Following Goodman (2014), each absence induced by bad weather reduces the math score by 0.05 standard deviations. Through a simple calculation, the decline in days of absence accounts for approximately one-third of the observed decrease in math scores.

of-school suspensions (including expulsions), and intensive/extensive margin of disciplinary actions. These results are presented in Figure A.6. Sub-figure (a) shows that the increase in days of in-school suspensions is at most 0.2 days and then declines back to around 0.1 days. In contrast, sub-figure (b) shows that the number of days of out-of-school suspensions and expulsions increases by 0.2 days and keeps increasing following four years up to 0.8. Sub-figure (c) and (d) examine the extensive margin—whether students had at least one day of disciplinary action—and intensive margin—analysis among students with at least one day of disciplinary action. I find an increase in both intensive and extensive margins.

Table 1 reports estimation results from equation (1), in which periods after school closure are pooled as After 1-2 Years for one to two years after closure (i.e., $t \in (0, 1)$) and After 3-4 Years for three to four years after closure (i.e., $t \in (2, 3)$). 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) present that the days of absence and days of disciplinary action increase after two years by 0.13 days and 0.36 days, which is a 2% and 15% increase relative to the pre-closure means. Days of disciplinary action further increase after 3-4 years up to 0.63 days.

**Heterogeneity analyses** I explore heterogeneous effects across the school and student characteristics. For school characteristics, I estimate equation (2) separately for sub-groups defined by the following characteristics: region, school quality, and school quality change.[27] The region is divided into urban and rural based on the NCES locale category. School quality is measured by the average math and reading test scores of each school over the four years preceding the school closure and divided into three levels: low, middle, and high school quality (SQ). School quality change is measured by the difference in school qualities between a closed school and the nearest school.[28] The distribution of difference is divided into three levels: worse, similar, and better school quality change.[29]

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

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

[29] It is divided to have an equal number of schools in each category. Then, school quality changes ranging from -0.84 to -0.032 standard deviations are classified as "worse." Changes between -0.031 and 0.18 standard deviations are categorized as "similar," while changes from 0.19 to 2.67 standard deviations are classified as "better."

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

To analyze the heterogeneous impact of school closures based on individual characteristics, I divide the sample by race/ethnicity, economic disadvantage status, and grades when the school is closed. The estimated coefficients and associated 95% confidence intervals are presented in Figure 4 separately for 1-2 years and 3-4 years. The results reveal several tendencies. Firstly, Hispanic students experience more pronounced adverse impacts on math scores and days of absence (0.5 days; 6.4%) while Black students experience a more substantial rise in days of disciplinary action (1.5 days; 33%).[30] Meanwhile, White students experience a greater drop in reading scores (0.06 standard deviations), which is not fully recovered in 4 years. These disparities across racial/ethnic groups highlight that each group is affected to varying degrees across outcomes, with Hispanic students generally experiencing the most significant overall effects. Secondly, economically disadvantaged students have more significant increases in days of absence (0.6 days; 7.3%) and days of disciplinary action (1.1 days; 38%) while not disadvantaged students experience a larger and continuous decrease in reading scores (0.05 standard deviations). Lastly, negative effects on test scores grow over time for students who were in higher grades at the time of closure, while students in lower grades appear to recover over time.

**School level changes** I explore school level changes including peer quality and the number of teachers per student after experiencing school closures. I construct peer quality measures

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

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 the equation (2). In the construction of peer quality measures, I exclude displaced students after experiencing school closures. Figure 5 illustrates the changes in peer quality. In (a) and (b) of the Figure, it shows that peers' math and reading scores decrease by 0.17 and 0.18, respectively right after closure.[31] However, the expected quality shows the opposite pattern. I construct expected quality measures using average math and reading test scores of each school over the four years *preceding* the school closure and use them as a dependent variable to estimate equation (2). As shown in (c) and (d) of the Figure, students move to schools that historically served better-performing peers. After moving, average math and reading scores increase by 0.044 and 0.042.[32]

To further understand why students do not have high-performing peers even after transitioning to originally better-performing schools, I examine the yearly school-level performance of receiving schools in Figure A.8. Test scores decline in the relative year $t = 0$, even when displaced students are excluded from the calculation of average scores. Then, I divide students into two groups for each year $t = 0$ and $t = -1$: those who were in the receiving school in the previous year and those who were not. Table A.2 presents the differences in test scores of these two groups between $t = 0$ and $t = -1$. It suggests that move-in students exhibit a more substantial difference (-0.064 to -0.078 standard deviations) in test scores than original students (-0.018 to -0.028 standard deviations) between $t = 0$ and $t = -1$. This suggests that the change in school quality in the year of closure is primarily driven by changes in student composition, potentially resulting from alterations in attendance zones along with school closures.[33]

Additionally, I analyze changes in school-level employment. Using a metric of full-time-equivalent (FTE) positions per 1000 students, I estimate the equation (2). As depicted in

[31] Based on Burke and Sass (2013), a one standard deviation increase in classroom peer quality is associated with changes in math scores of 0.0292, -0.0013, and 0.0088 for elementary, middle, and high school students, respectively, as well as 0.0271, 0.0087, and 0.0124 in reading scores. Considering the composition of my sample (45% elementary, 43% middle, and 10% high school students), the expected decrease in test scores due to changes in peer quality is calculated as follows: (0.45*0.029-0.43*0.0013+0.10*0.0088)*-0.17=-0.002 for math and (0.45*0.0271+0.43*0.0087+0.10*0.0124)*-0.18=-0.003 standard deviation for reading.

[32] In Figure A.7, I also present outcomes of days of absence and days of disciplinary action after standardization, which also present similar results.

[33] 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 more rigorous analysis of receiving schools in future research.

Figure A.9, there is a reduction of 2.3 full-time-equivalent positions in school-level employment following school closure. I categorize the employment into three groups: teachers, teaching support staff, and social support staff. While all categories experience a decrease in employment, the decline is more pronounced in teaching support staff (-1.8) and teachers (-1.4).

## 5.2 Long-Run Effects on Educational and Economic Outcomes

Figure 6 presents estimates of the effects of school closure on long-run educational outcomes by age 26 and economic outcomes at age 25-27. It includes coefficients and associated 95% confidence intervals from the estimation of equation (4), in which I estimate the event study form of the difference-in-differences model. Most of the long-run results show no indication of violating the parallel pretrend assumption, supporting the internal validity of the research design. For younger cohorts that did experience a school closure, I find overall negative effects on post-secondary education and labor market outcomes. Moreover, I observe a distinct pattern in which the negative effects are less pronounced for the highest grade students ($c = 0$) in the year of school closure, particularly from the labor market outcomes. Those would have likely moved even in the absence of school closures because they are likely in termination grade, and therefore faced less disruption than other grade students who would not have moved.

Table 2 reports estimation results from equation (3). I find that experiencing school closure decreases the likelihood of graduating from high school by 1 percentage point (1.4%), enrolling in four-year college by 1.2 percentage points (4.8%), and obtaining a bachelor's degree by 0.7 percentage points (4.7%), as well as decreases the quality of college by $256 (0.7%) by the age of 26. I further find that experiencing school closure makes students 0.7 percentage points (1.3%) less likely to be employed, leads to $793 (3.6%) lower annual earnings at ages 25-27.

**Heterogeneity analyses** I explore heterogeneous effects across the school and student characteristics for long-run outcomes. Sub-groups are defined in the same way as short-run heterogeneity analysis is. Figure A.10 displays the estimated coefficients and associated 95% confidence intervals for school characteristics. While overall negative effects exist, a few trends emerge. First, experiencing school closure in urban schools has a stronger negative impact. Second, students originally in low-performing schools experience more pronounced effects on their educational outcomes. Third, students who transition to better-performing schools tend to exhibit more pronounced negative effects on college and completion and quality while students moving to worse-performing schools also experience a significant drop in yearly earnings. This suggests that even when students move to schools with higher-performing peers, they could still encounter adverse consequences.[34]

I explore individual heterogeneity in Figure A.11 presenting the estimated coefficients and associated 95% confidence intervals. While much of the confidence intervals overlap across estimates, a few patterns are worth noting. First, students in higher grades are more negatively affected by school closure while students in grades 3-5 overall do not experience significant long-run negative effects. Second, while racial and economic status differences are not pronounced, Hispanic and economically disadvantaged students experience a larger negative effects which is more pronounced in the comparison after rescaling based on sub-group means in Figure A.12 and A.13. Corresponding well to the short-run heterogeneity analysis, the results present that the negative effects are more pronounced on students in higher grades and more vulnerable situations such as those from originally low-performing schools and racial/ethnic minorities.

**Long-run analysis attrition** As I discussed in Section 3, I do not observe post-secondary education and labor market outcomes if students leave Texas. If experiencing school closure systematically changes the attrition pattern, the interpretation of estimation is complicated. Providing the following evidence, however, I argue that differential attrition is unlikely to change meaningfully the 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

[34] It might seem counter-intuitive that students moving to better schools experience negative effects. There are multiple possible mechanisms to explain this. Firstly, days of disciplinary action increase more significantly for students transferring to better-performing schools, which might imply that adapting to better-quality schools is more difficult for students. Secondly, the decrease in ordinal rank might play an important role (Denning, Murphy, and Weinhardt 2023; Elsner, Isphording, and Zölitz 2021; Murphy and Weinhardt 2020). For example, Denning, Murphy, and Weinhardt (2023) find that 40% of peer effects can be offset by ordinal rank effects in 3rd grade students. Considering that the majority of my long-term effects stem from students in higher grades, changes in ordinal rank might have a more significant impact on educational outcomes. Lastly, even if students experience an increase in test scores in my short-run analysis, they constitute a limited sample compared to the long-run sample, as test scores are predominantly available for elementary students. The last point also addresses why long-run effects are overall negative although short-run analysis shows an increase in test scores for some groups.

and control schools. Figure A.2 (c) plots the proportion of students in a long-run analysis sample from closed and matched control schools, separately for younger and older cohorts, appearing in the data each year after school closure. In Figure A.2 (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 0.4 percentage points.[35] This finding provides reassurance that sample attrition 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% of higher education enrollment in the United States. As discussed in Section 3, the available data of higher education enrollment out-of-Texas only begins in 2008, which does not fully cover the sample. Therefore, it is not used in the baseline analysis. However, it is informative to examine whether out-of-state enrollment was affected by school closures. Using an indicator for out-of-state enrollment as the dependent variable, I estimate equation (3) and present the results in appendix Table A.3. The estimates show that the difference in enrolling in college out-of-Texas is less than 0.1 percentage points between younger cohorts from closed and control schools. 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 (Table A.5). Thirdly, using a school quality measure based on their highest education level and institution, I

[35] To see the potential impact of the attrition, I estimate Lee (2009) bounds assuming differential attrition in response to a school closure of 0.4 percentage points. The estimated bounds are presented in Panel A of Table A.4. While these Lee bounds cover a range of estimates, the bounds exclude zero for most of the outcomes.

find consistent results showing a decrease in expected wages among the sample of individuals. Lastly, I perform a bounding exercise with the non-zero earning sample, attributing all the decrease in employment rates after school closure to attrition to the labor market outside Texas (Lee 2009). The Lee bounds, presented in Table A.4, exclude zero, implying that even under the extreme assumption, the main implications remain unchanged.

## 5.3 Sensitivity Analysis

**Different matching strategies** I also examine the sensitivity of my estimates to alternative ways of choosing matched control schools to closed schools. Appendix Figure A.20 and A.21 present coefficients and associated 95% confidence intervals from estimating equations (1) and (3) respectively, using following alternative matching strategies: (1) I add more variables (share of ESL and share of special education) when measuring the distance; (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. At the end of the name of each alternative matching method, the percentages of the same matched control schools as the baseline are added. For instance, 67% of matched control schools are changed after adding more variables (share or ESL, share of special education). Reassuringly, results are robust across these alternative matching strategies while control schools change 65% on average from the baseline control schools.

I further test the robustness to the matching strategy by using the synthetic difference-in-differences method (Arkhangelsky et al. 2021). Restricting the pool to students in the same year, same school type, and same locale, I individually match students from closed schools to multiple students with weight to minimize parallel trend violation.[36] Reassuringly, estimation results in Table A.6 from the synthetic difference-in-differences are similar to baseline estimates.

[36] I randomly select 10,000 students from the donor pool if students in the donor pool are over 10,000 because of the computational burden. I examine whether the results change following random sampling by resampling 10 times. As shown in Figure A.23, the random sampling in the implementation procedure does not meaningfully affect estimated coefficients.

If anything, the synthetic difference-in-differences estimates are somewhat larger. Furthermore, Figure A.22 plots outcome trends from the implementation of synthetic difference-in-differences, mimicking the raw trend figures in Figure A.5. All outcomes show a very similar pretend. After experiencing school closure, test scores drop, and behavioral issues increase among students from closed schools. In other words, estimated coefficients obtained without any further discretion regarding the matching criteria exhibit similarities with the baseline coefficients.

**Different short-run specifications** My short-run event study analysis makes use of a balanced panel of students observed in TEA data three years before and four years after school closure. I examine robustness analysis by providing estimation results of equation (2) with different sample specifications. In appendix Figure A.14, I explore the sensitivity of my estimates to using an unbalanced sample. The unbalanced sample is relatively unstable, but overall patterns are similar to baseline results.

Appendix Figure A.15 presents estimation results using a sample excluding closed schools where new schools come in next year to the same address as closed schools to alleviate the concerns of coding changes or school reform without physical school closure. The overall trends observed closely mirror those seen in the baseline results. However, it is noteworthy that the effects appear to be more pronounced when compared to the baseline sample. For instance, test scores drop more than 0.05 standard deviations right after school closures while those are around 0.03 standard deviations in baseline estimation. This implies that certain schools identified as closed within the scope of my research may potentially be instances of alterations in school coding or not all students may be affected by the closure.

To see whether the effects of school closure vary over time, I estimate the effects after dividing school closures into three periods: 1998-2003, 2004-2009, and 2010-2015. The estimated coefficients and associated 95% confidence intervals for three periods are separately presented in Figure A.16. The overall trends across periods seem similar except for a few noticeable patterns. First, the absence rate exhibits an increase immediately following school closures, but then it follows different trajectories across periods. In the instance of early closures, the absence rate drops below its original level while in cases of middle and later closures, the elevated absence rates persist. Second, days of disciplinary action increase sharply and maintain the elevated level in early closures, but it continuously increases in the middle and later closures.

**Different long-run specifications** My long-run event study analysis relies on a balanced panel encompassing three younger cohorts at the time of school closure and three older cohorts immediately preceding the observed school closure. It also incorporates demographic and performance control variables. Estimating equation (4), I examine robustness analysis employing different sample specifications and control variables. In Appendix Figure A.17, I present estimation results using an unbalanced sample, which includes schools with a minimum of two cohorts. These results are juxtaposed with the baseline sample for reference. Overall patterns closely resemble those observed in the baseline results.

Appendix figure A.18 depicts estimation results without controlling for performance variables (test scores and behavior). General patterns observed remain largely consistent regardless of whether performance measures are controlled in the analysis. However, results obtained without the inclusion of performance measures tend to exhibit instability and weaker effects. Moreover, Appendix Table A.5 presents estimation results from equation (3) in three levels of controls: i) without demographic and performance controls, ii) with demographic controls, iii) and with demographic and performance controls. The estimation results exhibit consistency across these different specifications.

# 6 Discussion

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

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% at age 27. Similarly, a one 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 school closure is a 3.4% decrease in earnings at ages 25-27, which is equivalent to a 0.35 standard deviation decrease in class quality or a one standard deviation decrease in teacher quality for 2.5 years. Moreover, when considering disruptive events, Cabral et al. (2021) find that a school shooting in Texas high schools leads to a 13.5% reduction in earnings at ages 24-26. That is, my estimated effect of school closure is equivalent to 25% of the effect of experiencing a school shooting in high school.

I further compare my estimates to potential policy experiments. Chetty, Friedman, and Rockoff (2014) estimate that replacing teachers in the bottom 5 percent based on value-added with average teachers for one year would increase the present discounted value of earnings of the students in the classroom by $250,000. Carrell, Hoekstra, and Kuka (2018) estimate that one year exposure to a disruptive student reduces the present discounted value of lifetime earnings by $81,000 to $105,000. Under the same assumption, my estimate suggests that a classroom will experience a reduction of $443,700 in their present discounted value of lifetime earnings.[37] Thus, my estimates imply that experiencing school closure has roughly the same effect on future earnings as replacing a bottom 5% teacher with an average teacher for about 1.8 years. Or it has similar effects as having five disruptive classmates for one year.

Lastly, Cabral et al. (2021) estimate that the annual aggregate present discounted value of the cost of school shootings in the US from students who experience it is $5.8 billion. Under

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

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.[38] With approximately 250,000 students being affected by school closures annually from 2010 to 2021 (NCES 2022), the total annual cost of school closures, resulting from displaced students, amounts to about $7.6 billion. This estimation implies that the annual cost of school closures, resulting from the disruption in the accumulation of human capital among displaced students, is approximately 1.3 times the cost of school shootings in the US.[39]

# 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 nations; 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 their total (National Bureau of Statistics of China 2023), while France has closed 8,000 schools, accounting for 14% of their total (Ministry of National Education, Higher Education and Research 2023). In Brazil, rural primary schools have experienced a substantial 31% reduction in number, dropping from 88,000 to 61,000 between 2007 and 2017 (Brazil Ministry of Education 2020). In Rajasthan, India, in 2014, the government initiated the merger of 17,000 out of the over 80,000 government schools (Chowdhury 2017). Despite the pervasive utilization of school closure policy, evidence of the effect on students is limited, which calls for research quantifying the causal effects of school closure on students' short- and long-run outcomes

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

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

(Tieken and Auldridge-Reveles 2019).

Using rich administrative data from Texas, I explore the effects of school closure on displaced students' outcomes in the short-run including test scores and behavioral problems, and long-run outcomes including post-secondary education and labor market outcomes. I analyze school closures between 1998 and 2015 in Texas using the difference-in-differences method exploiting within-student and within-school across-cohort variations. I find that school closure leads to a drop in test scores and an increase in behavioral issues in the following years. While decreased test scores recover back to the original level, increased days of disciplinary action remain elevated level in the following years. I further find that school closure leaves long-run negative impacts on post-secondary education and labor market outcomes. Heterogeneity analysis reveals that the adverse effects are more pronounced among students in higher grades, Hispanic students, as well as those from originally low-performing schools and economically disadvantaged families.

The long-run negative impacts of school closures are sizable. Estimated results suggest that the size of adverse effects of school closure are big enough to offset benefits from about a 16 percent decrease in class size for two years with regard to college attendance, from a 0.35 standard deviation increase in overall class quality considering peer and teacher quality for a year when it comes to yearly earnings. My back-of-the-envelope calculations further suggest that the annual cost of school closures due to displaced students is over $7.6 billion annually in the US, without considering the potential benefits of school closures.

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

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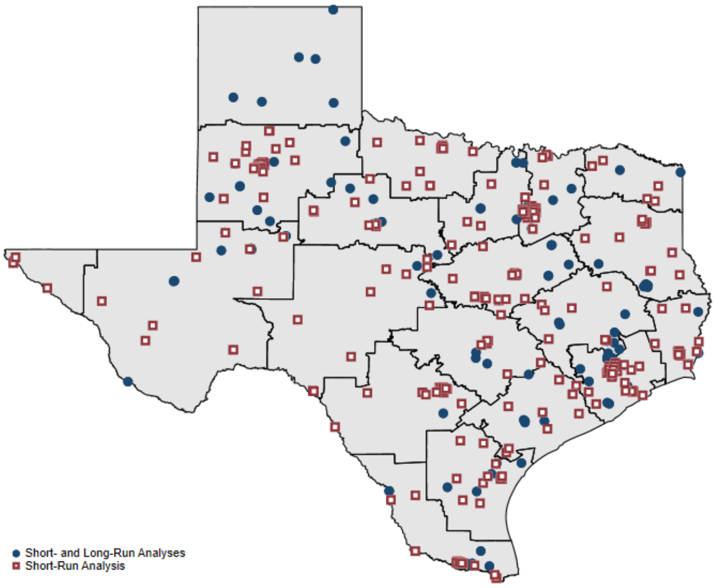
# 8 Figures and Tables

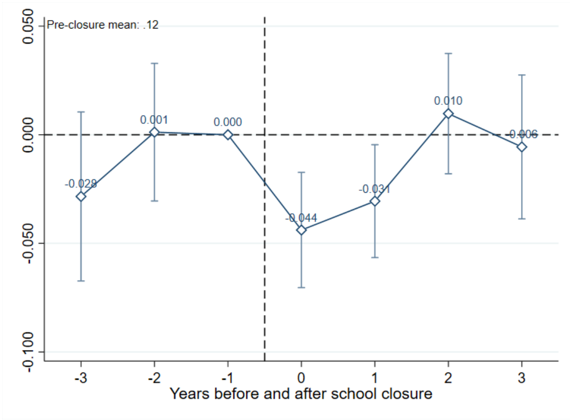
**Fig. 1.** Map of School Closures at Texas Public Schools in 1998-2015



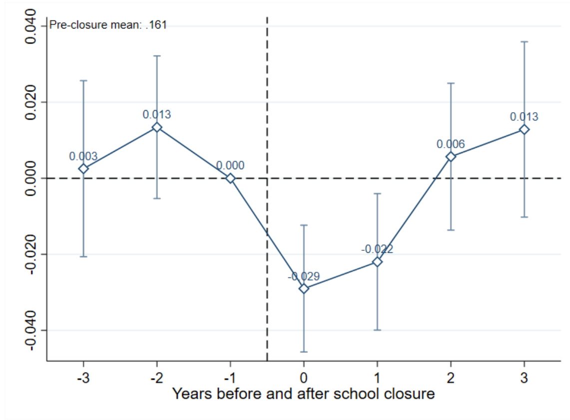

*Notes:* The figure presents the locations of 470 school closures in total: 324 school closures used in only short-run analysis and 146 school closures used in both short- and long-run analysis in Texas.

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

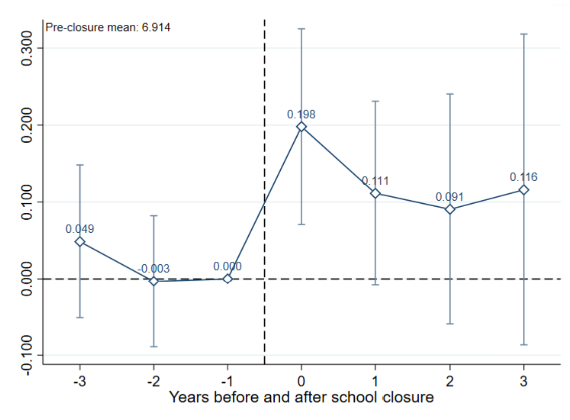
**(a)** Standardized math score



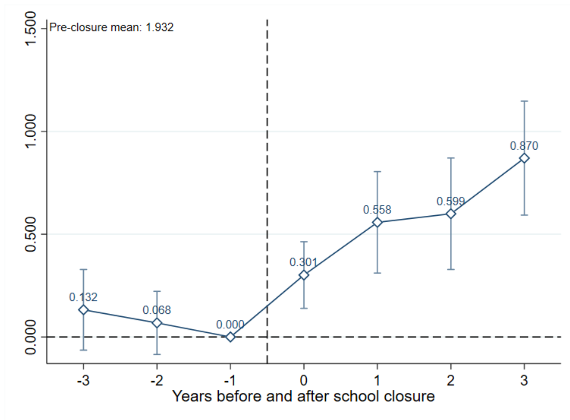

**(b)** Standardized reading score




**(c)** Days of absence

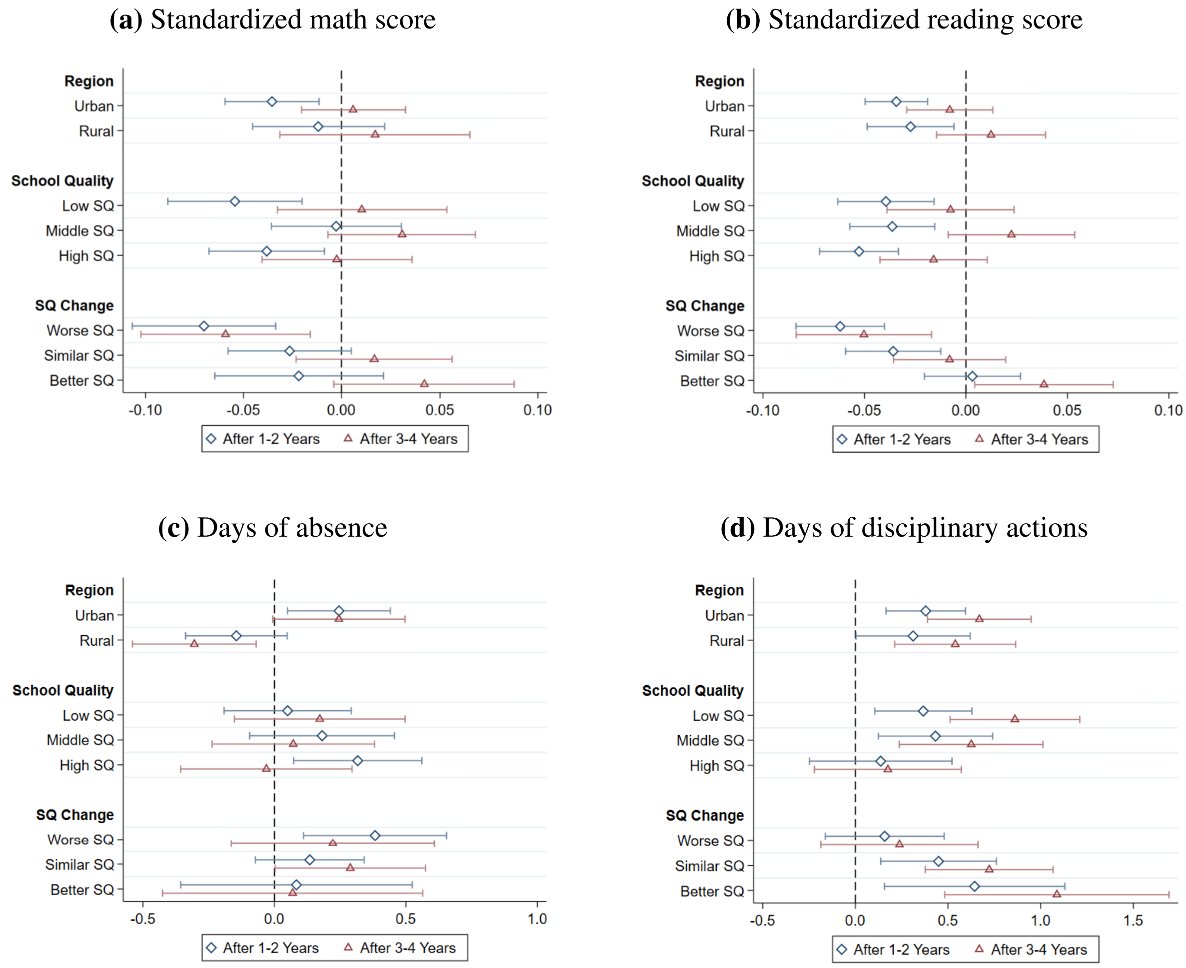



**(d)** Days of disciplinary action



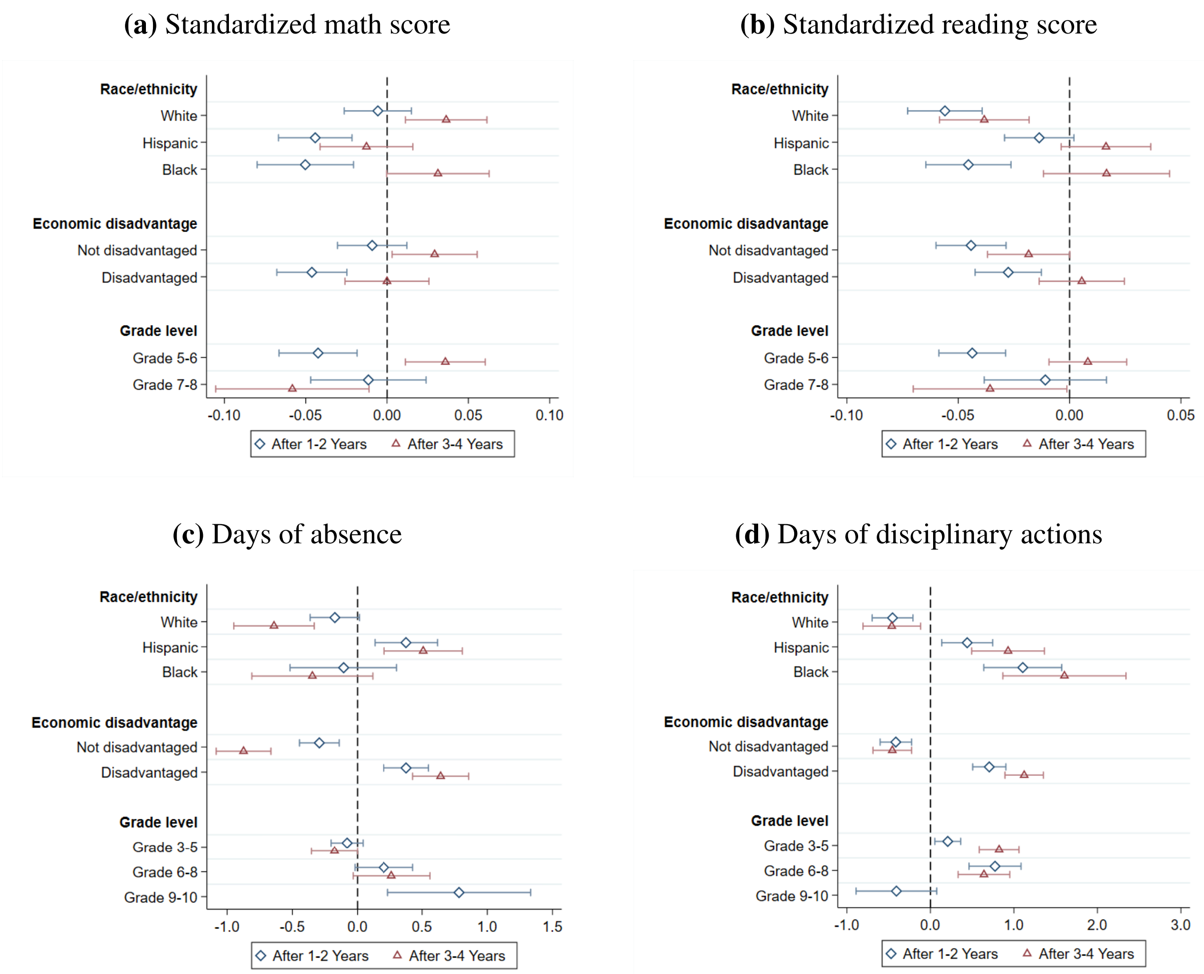

*Notes:* The figures present the coefficients, $\rho_t$, and 95% confidence intervals from equation (2).These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure 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 absence rate is computed as the number of days a student is absent divided by the total of both absent and present days. Standard errors are clustered by school.

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




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

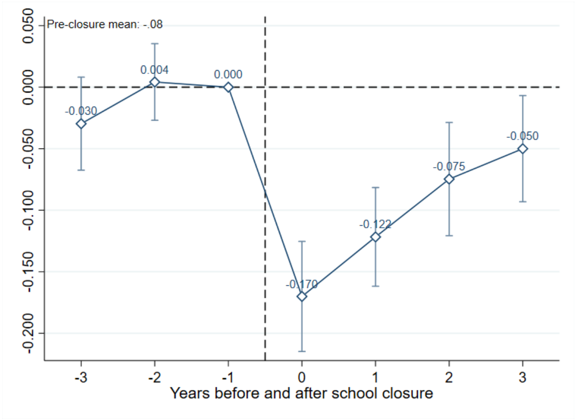
**Fig. 4.** Short-Run Effects of School Closure on Student Outcomes: Heterogeneity by Student Characteristics



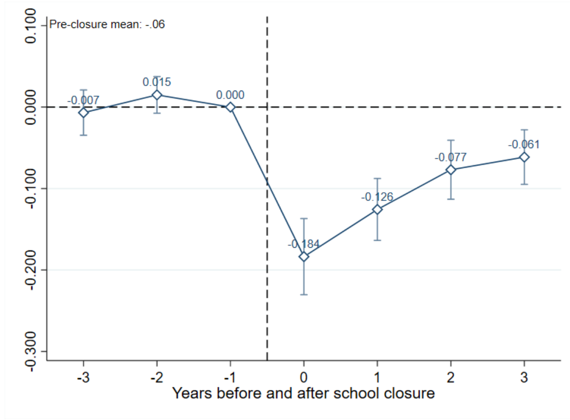

*Notes:* The figures present the coefficients, $\beta$, and 95% confidence intervals from equation (1) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote years after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

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

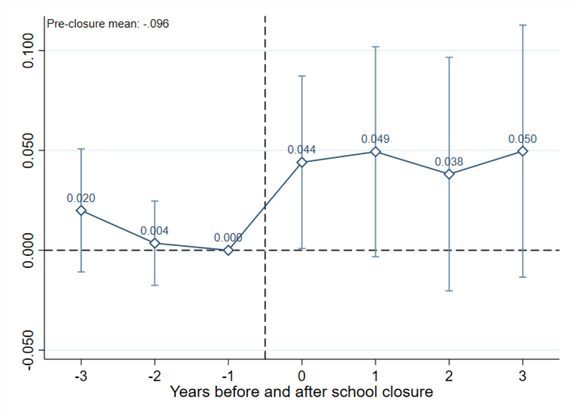
**(a)** Peer quality: standardized math score



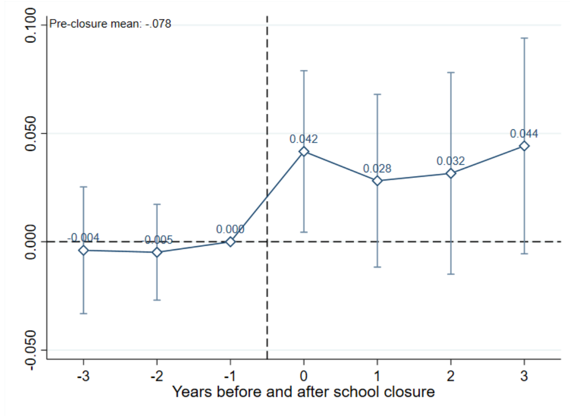

**(b)** Peer quality: standardized reading score




**(c)** Expected quality: standardized math score



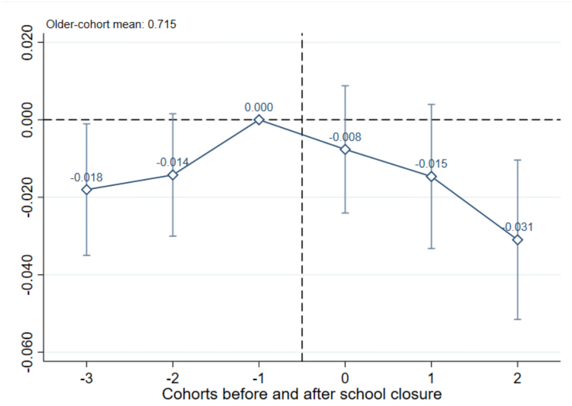

**(d)** Expected quality: standardized reading score



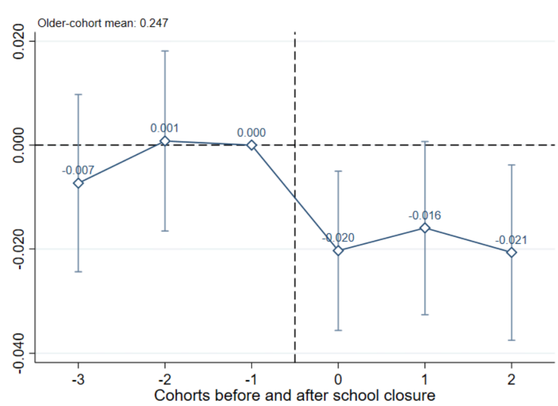

*Note:* The figures present the coefficients, $\rho_t$ , and 95% confidence intervals from equation (2), where the outcome variables are the school averages. When it comes to sub-figures (a) and (b), the outcome variables are yearly school average and the construction of average values excludes displaced students from the calculations after school closure (i.e., $t >= 0$). For sub-figures (a) and (b), the outcome variables are the school average over the four years preceding the school closure. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school.

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

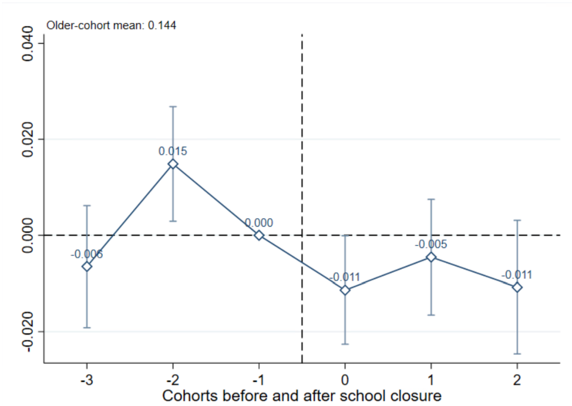
**(a)** High school graduation



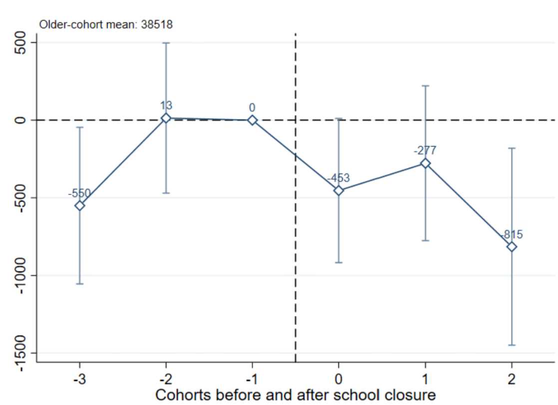

**(b)** College attendance



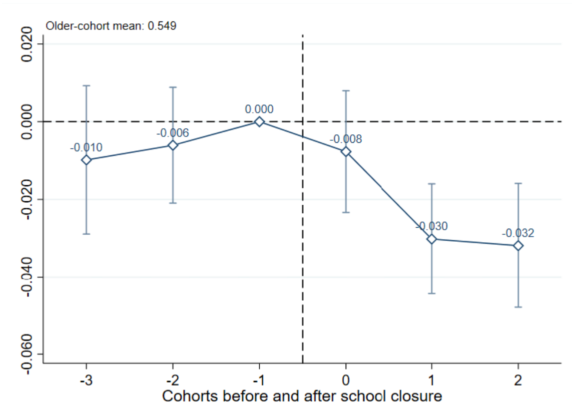

**(c)** College completion



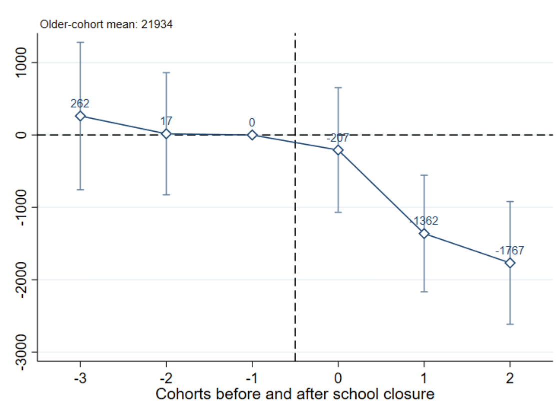

**(d)** College quality




**(e)** Employment




**(f)** Yearly earnings




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

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

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

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

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

| Panel A: Post-Secondary Outcomes | | | | |
|---|---|---|---|---|
| | (1) Graduate HS | (2) Enroll College | (3)BA Degree | (4)College Quality |
| Closed School<br>× Younger Cohorts | -0.010**<br>(0.004) | -0.012***<br>(0.004) | -0.007***<br>(0.003) | -256***<br>(114) |
| Observations | 155,660 | 155,660 | 155,660 | 155,660 |
| School FE | X | X | X | X |
| Matched group × Year FE | X | X | X | X |
| Mean of the Older Cohort | 0.713 | 0.251 | 0.148 | 38547 |

| Panel B: Labor Market Outcomes | | |
|---|---|---|
| | (1) Employment | (2) Yearly Earnings |
| Closed School<br>× Younger Cohorts | -0.007*<br>(0.004) | -793***<br>(205) |
| Observations | 155,660 | 155,660 |
| Individual FE | X | X |
| Matched group × Year FE | X | X |
| Mean of pre-closure | 0.550 | 22,138 |

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

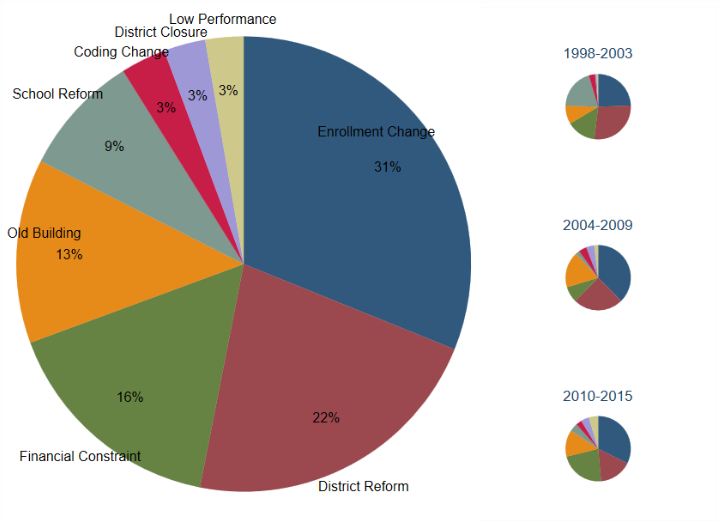
# Online Appendix

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

*Jeonghyeok Kim (2024)*

# A Appendix Figures and Tables

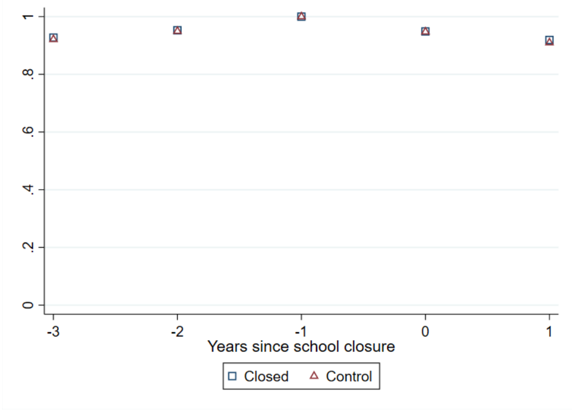
**Fig. A.1.** The Reasons for School Closures at Texas Public Schools in 2009-2015



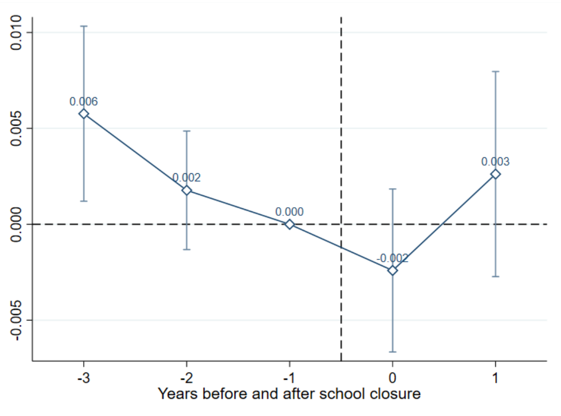

*Notes:* The figure presents the categorized reasons for 267 out of 470 school closures that occurred between 1998 and 2015. Three smaller figures depict the reasons for closures across three distinct periods: 1998-2003 (86 closures out of 146), 2004-2009 (71 closures out of 177), and 2010-2015 (110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

**Fig. A.2.** Analysis of Sample Attrition Rates of Closed and Control Schools

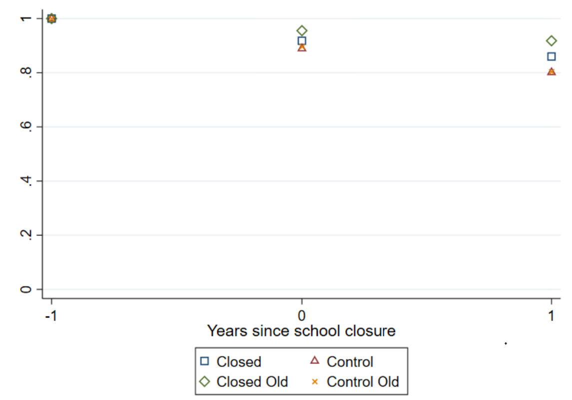
**(a)** Short-run: mean in-sample by time



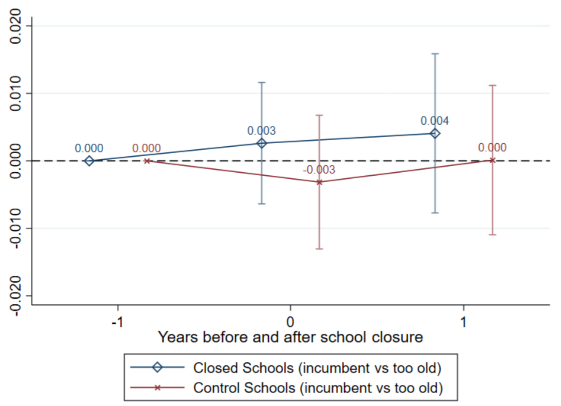

**(b)** Short-run: regression of in-sample dummy on closed-school dummy




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



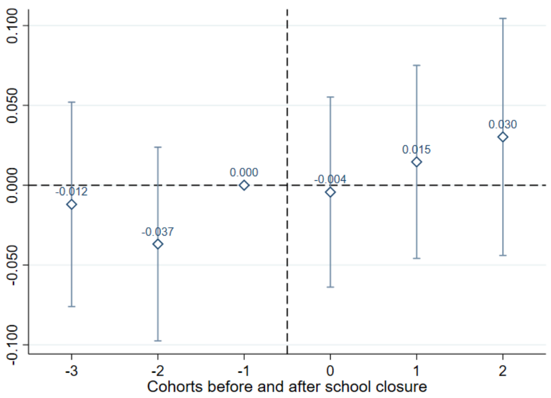

**(d)** Long-run: regression of in-sample dummy on closed-school/younger-cohort dummy



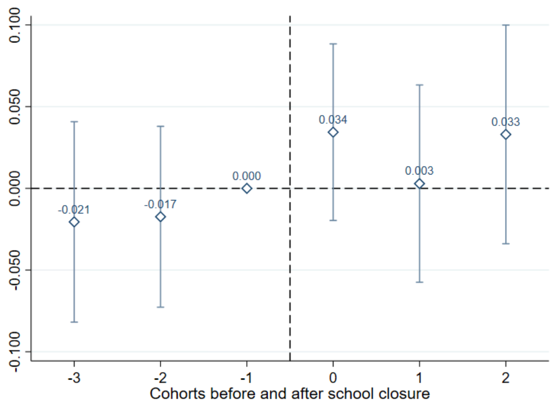

*Notes:* Sub-figures (a) and (b) consider all students in grades 5-11 enrolled in closed and matched control schools in the year preceding the closure (denoted by time 0 on the x-axis). Sub-figure (a) plots the proportion of observed students each year around school closure, separately for students in closed schools and control schools. Using this sample, sub-figure (b) presents the coefficients, $\rho_t$, 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 is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school. Sub-figure (c) and (d) consider all students in grades 3-8 enrolled in closed and matched control schools with non-missing student characteristics (race/ethnicity, sex, ESL status, special education status, test scores, absence rates) in the year preceding the closure or four years before the closure (denoted by time 0 on the x-axis). Sub-figure (c) plots the proportion of observed students in the years following time 0, separately for four groups—younger (incumbent) and older cohorts in closed schools and control schools. Using this sample, sub-figure (d) presents the coefficients, $\rho_t$, and 95% confidence intervals from equation (2), in which the dependent variable is an indicator for being observed in the data and $t \in (-1, 1)$, separately for younger and older cohorts. Other specifications are equal to sub-figure (b).

**Fig. A.3.** Long-Run Analysis Balance Test: Difference in Test Scores and Behavior Before School Closures

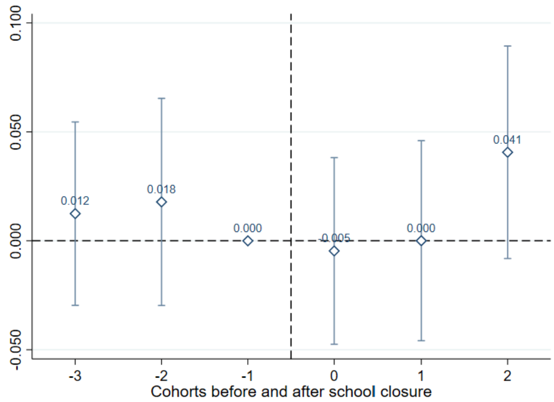
**(a)** Standardized math score




**(b)** Standardized reading score

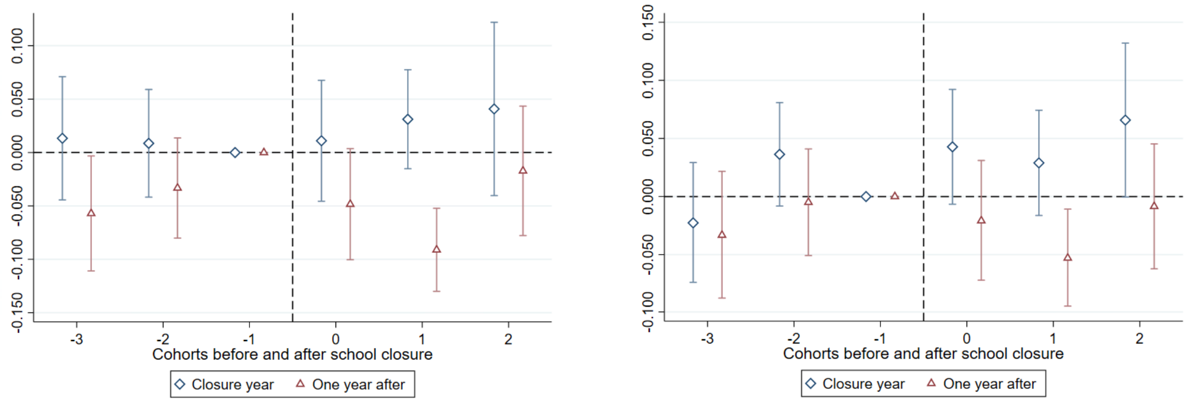



**(c)** Absence rate



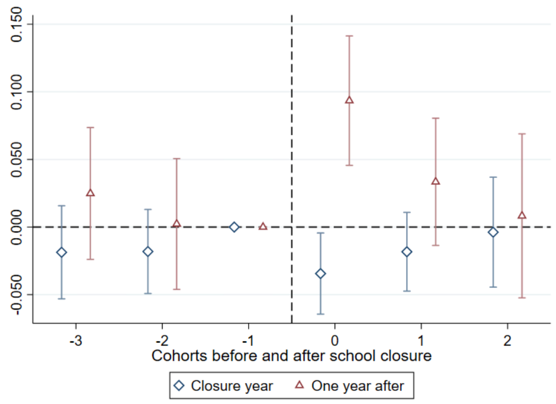

*Notes:* The figures present the coefficients, $\pi_t$, and 95% confidence intervals from equation (4), in which the dependent variables are short-run outcomes (test scores and behavior). The dependent variable is measured before school closures, specifically at $t = -1$ for younger cohorts and at $t = -4$ for older cohorts from the equation (2). 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 is the omitted category. The regression includes individual and match group-by-year fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, and special education status. Standard errors are clustered at the school-by-cohort level.

**Fig. A.4.** Long-Run Analysis Balance Test: Difference in Test Scores and Behavior Before and After School Closures




**(a)** Standardized math score

**(b)** Standardized reading score

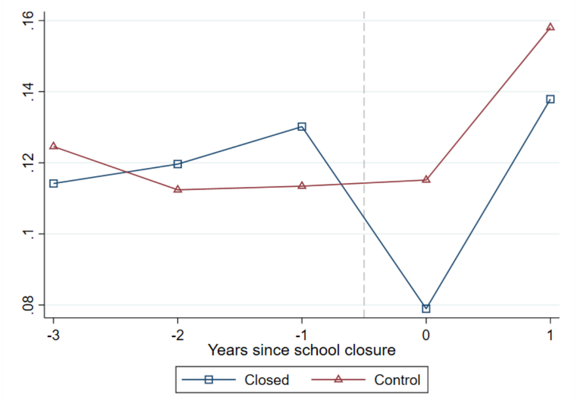



**(c)** Absence rate

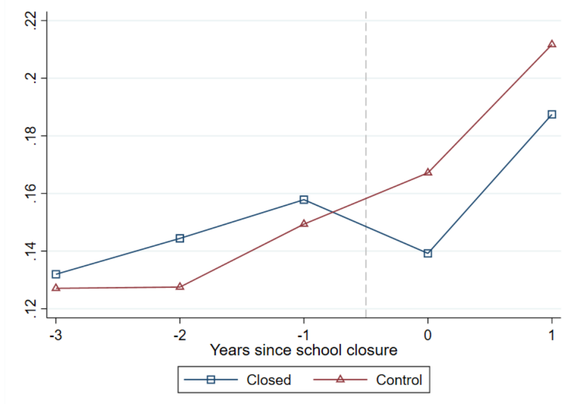
*Notes:* The figures present the coefficients, $\pi_t$, and 95% confidence intervals from equation (4), in which the dependent variables are short-run outcomes (test scores and behavior). The dependent variable is measured before school closures, specifically at $t = -1$ for younger cohorts and at $t = -4$ for older cohorts from the equation (2), and after closures, specifically at $t = 0$ for younger cohorts and at $t = -3$ for older cohorts. To be included in the analysis, individuals must be observed in both outcomes before and after closure, and they must attend schools where the highest grade is not 12. 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 is the omitted category. The regression includes individual and match group-by-year fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, and special education status. Standard errors are clustered at the school-by-cohort level.

**Fig. A.5.** Raw Trends in Short-Run Outcomes Between Closed and Control Schools

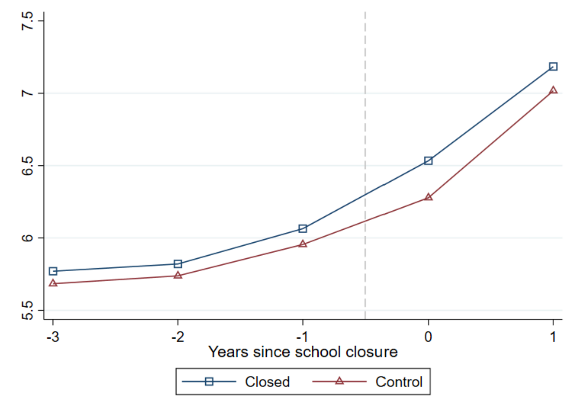
**(a)** Standardized math score



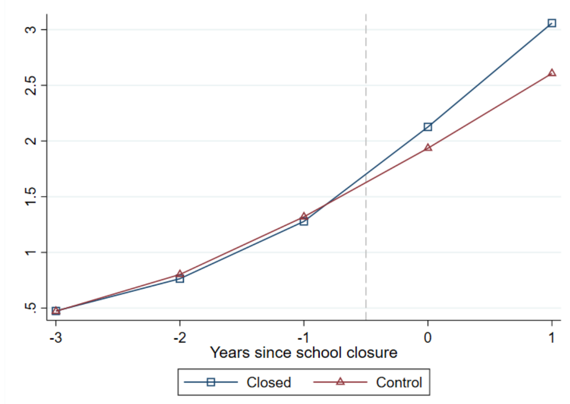

**(b)** Standardized reading score




**(c)** Days of absence



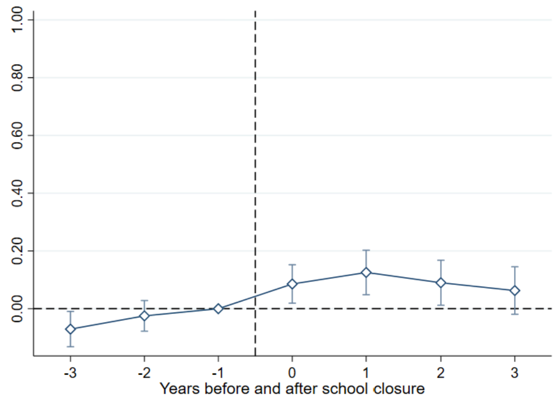

**(d)** Days of disciplinary action



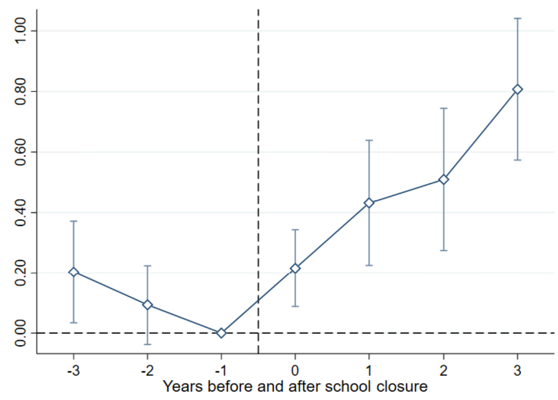

*Notes:* The figures plot raw trends over the period of three years before and two years after the school closure, separately for closed and matched control schools. I restrict the sample to students who are observed in the data over this period (i.e., the panel is balanced).

**Fig. A.6.** Short-Run Effects of School Closure on Days of Disciplinary Actions: Different Margins

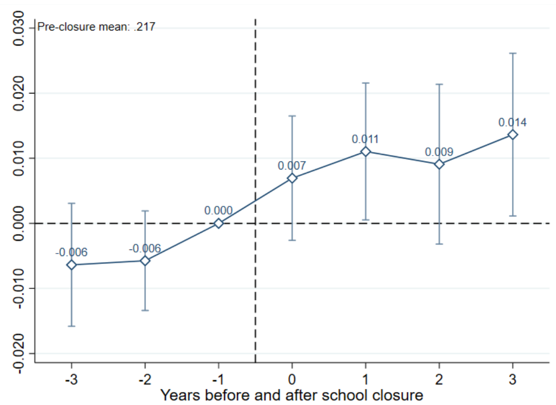
**(a)** In-school days of disciplinary action



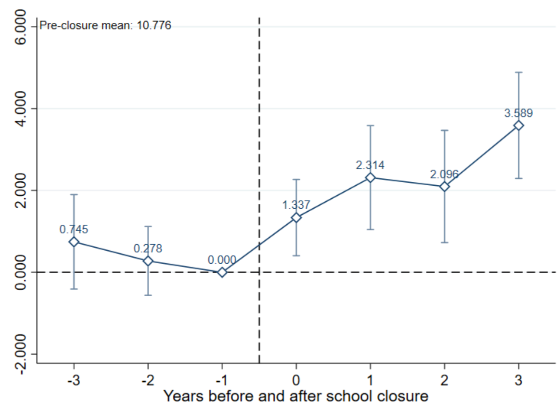

**(b)** Out-of-school days of disciplinary action




**(c)** Binary outcome variable



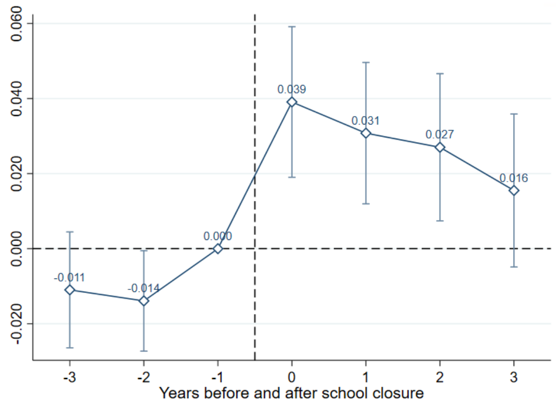

**(d)** Among at least one day



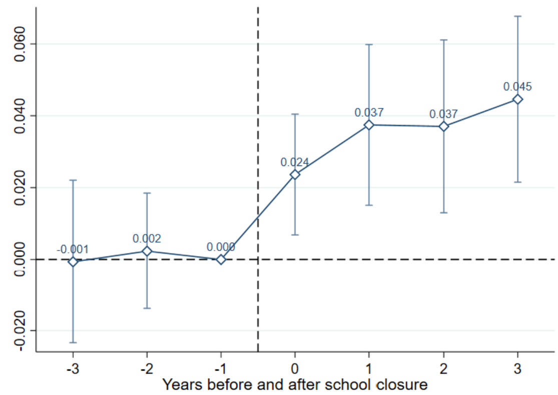

*Notes:* The figures present the coefficients, $\rho_t$, and 95% confidence intervals from equation (2) using different margins of disciplinary action—in-school suspension, out-of-school suspension including expulsion, indicator dependent variable of at least one day of disciplinary action, and analysis among students who are in disciplinary action at least one day. 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 is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school.

**Fig. A.7.** Peer and Expected School Quality Changes Before and After School Closures

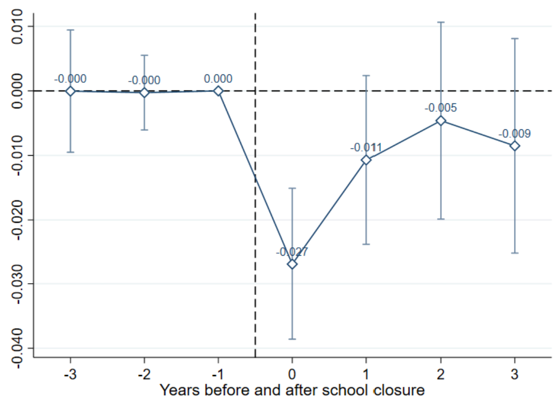
**(a)** Peer quality: standardized days of absence



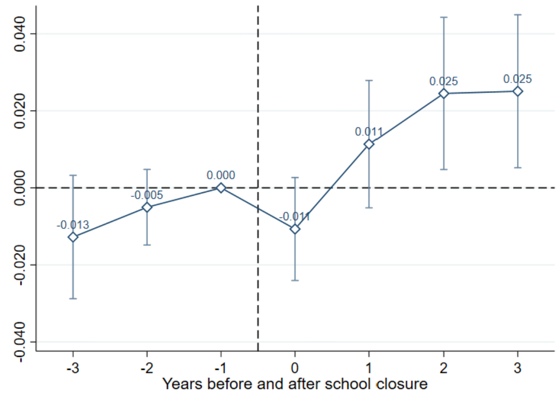

**(b)** Peer quality: Standardized days of disciplinary action




**(c)** Expected quality: standardized days of absence



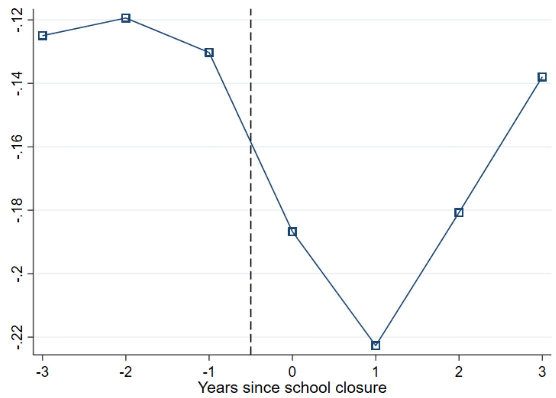

**(d)** Expected quality: standardized days of disciplinary action



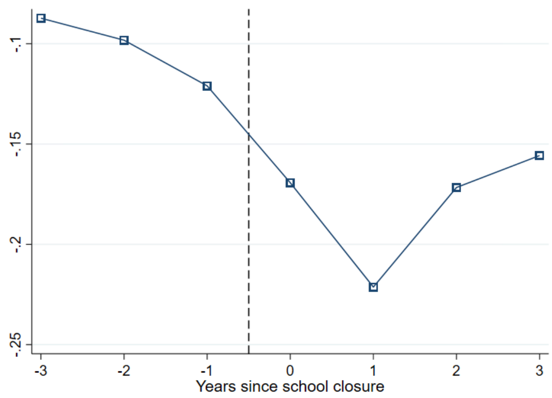

*Note:* The figures present the coefficients, $\rho_t$ , and 95% confidence intervals from equation (2), where the outcome variables are the school averages. When it comes to sub-figures (a) and (b), the outcome variables are yearly school average and the construction of average values excludes displaced students from the calculations after school closure (i.e., $t >= 0$). For sub-figures (a) and (b), the outcome variables are the school average over the four years preceding the school closure. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school.

**Fig. A.8.** Raw Trends in Test Scores of Receiving Schools around School Closures

**(a)** Standardized math score



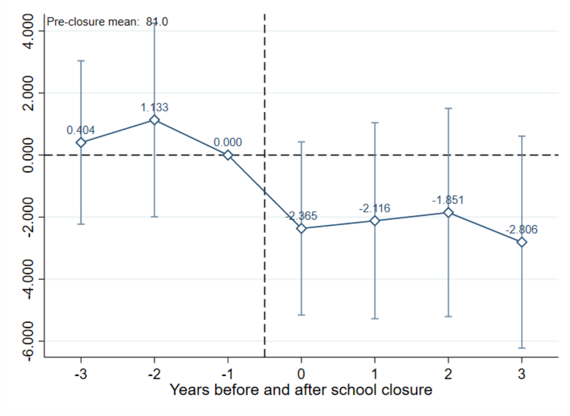

**(b)** Standardized reading score



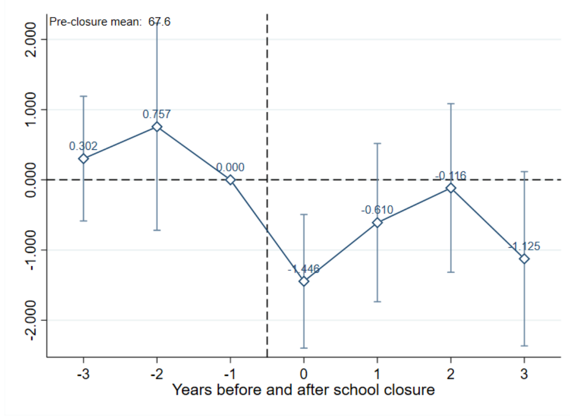

*Note:* The figures plot raw trends in average test scores for receiving schools over the period of three years before and four years after the school closure. Each dot represents the weighted average of test scores in the receiving schools where displaced students enroll in the year immediately following the closures. The average test scores are calculated without including displaced students. The weight assigned to each school is determined by the number of displaced students it accommodates. To simplify the calculations, I exclude receiving schools with fewer than 10 displaced students from the analysis.

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

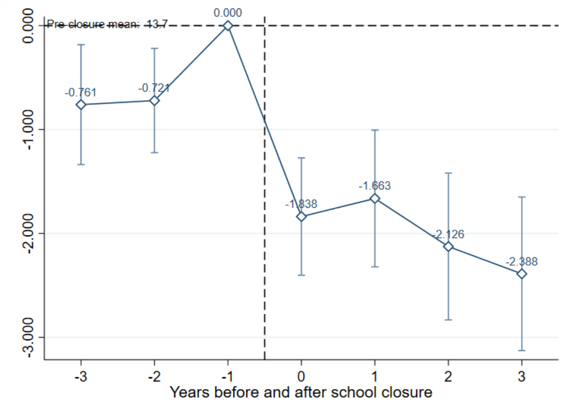
**(a)** Total school staff



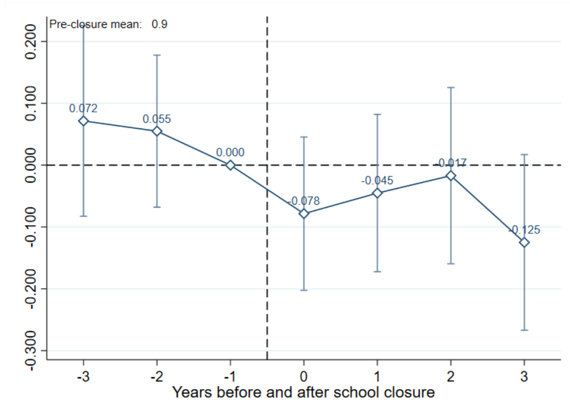

**(b)** Teachers




**(c)** Teaching support



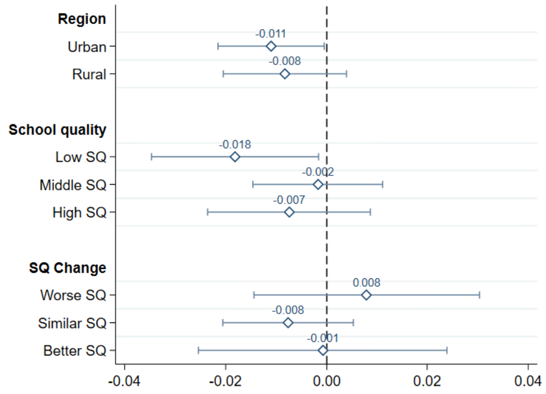

**(d)** Social support



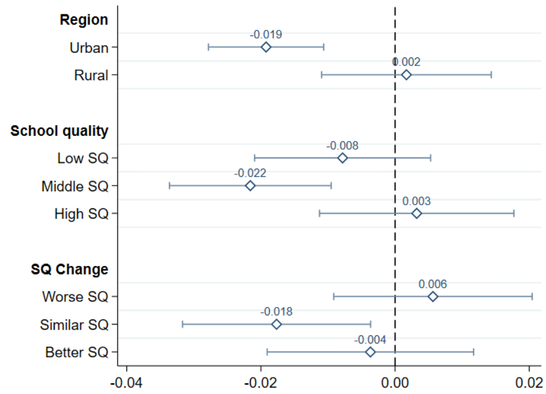

*Note:* The figures present the coefficients, $\rho_t$ , and 95% confidence intervals from equation (2), where the outcome variables are the school-level full-time-equivalent (FTE) positions per 1000 students. These coefficients represent the interactions between the indicator that denotes closed schools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

**Fig. A.10.** Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Heterogeneity by School Characteristics

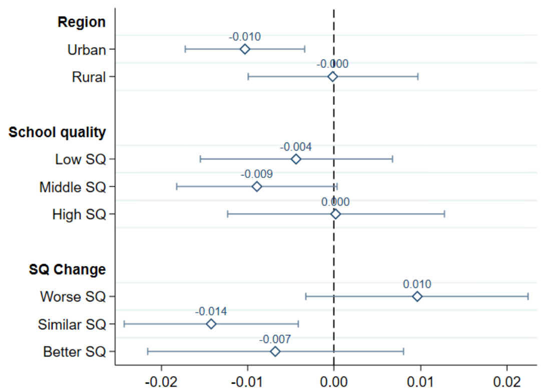
**(a)** High school graduation



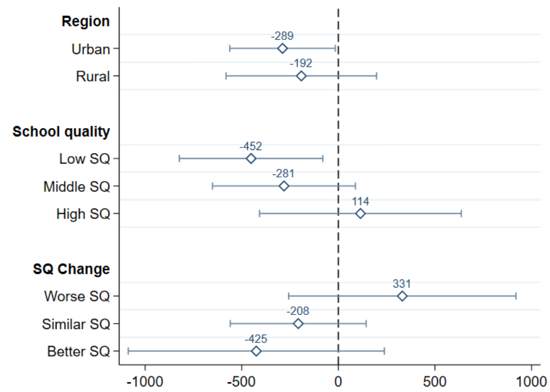

**(b)** College attendance



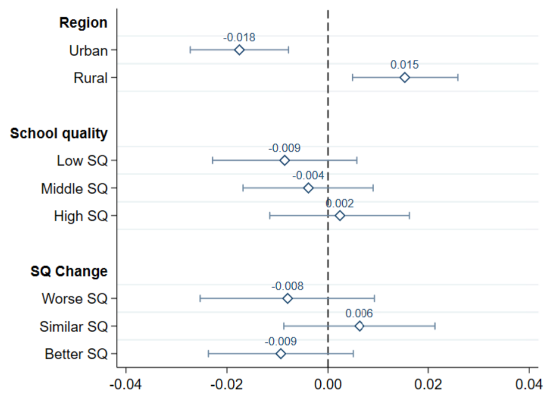

**(c)** College completion



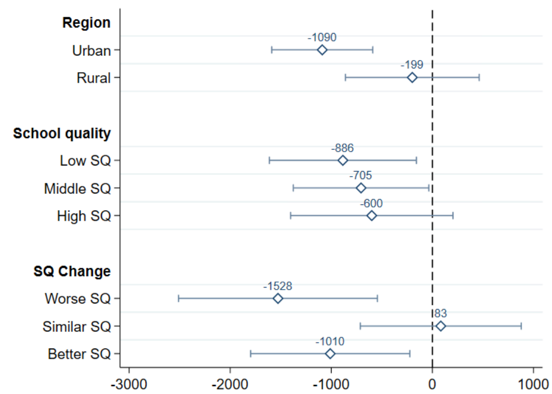

**(d)** College quality




**(e)** Employment



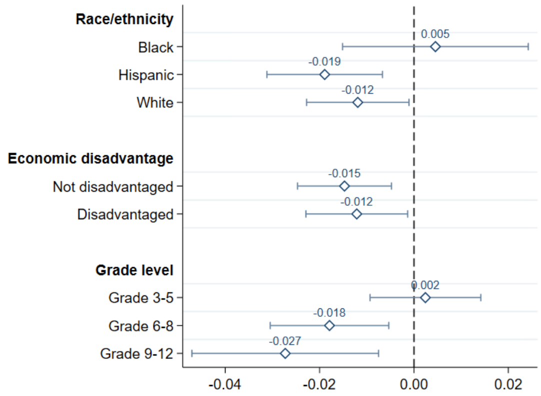

**(f)** Yearly earnings



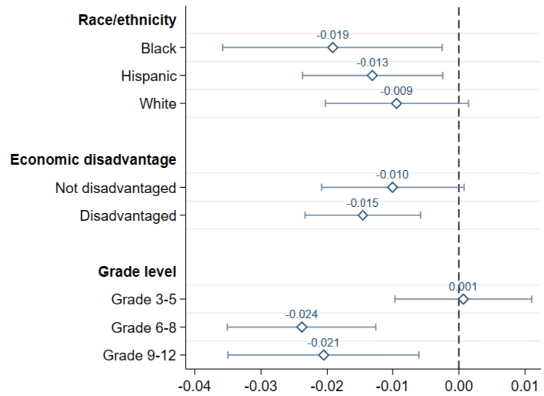

*Notes:* The figures present the coefficients, $\gamma$, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote cohorts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

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

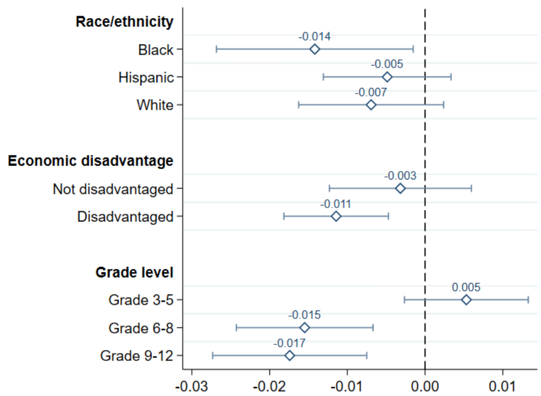
**(a)** High school graduation



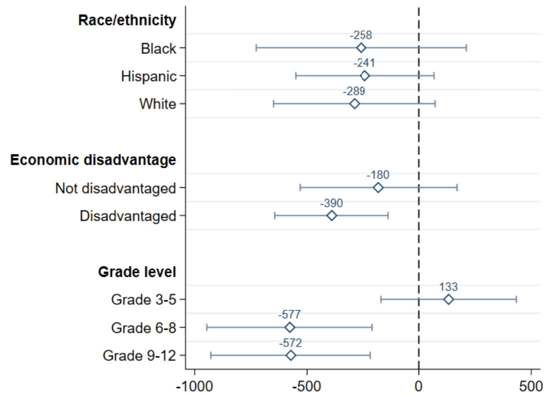

**(b)** College attendance



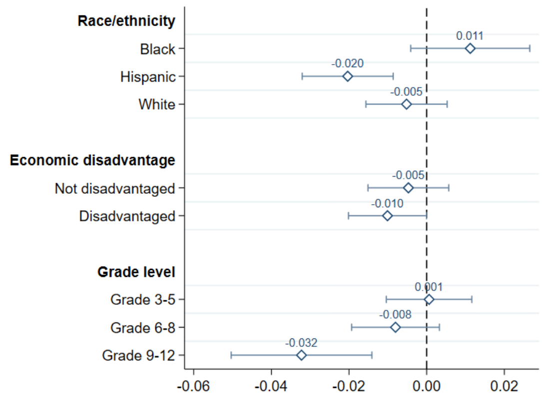

**(c)** College completion



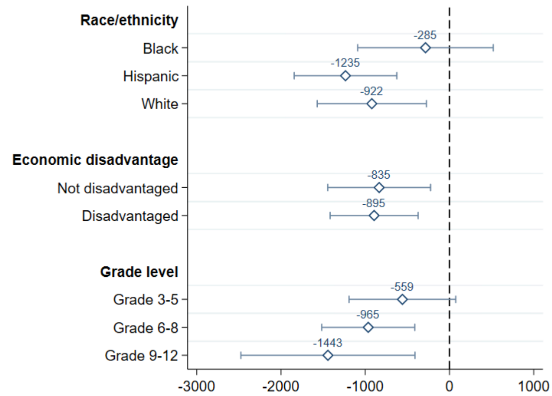

**(d)** College quality




**(e)** Employment



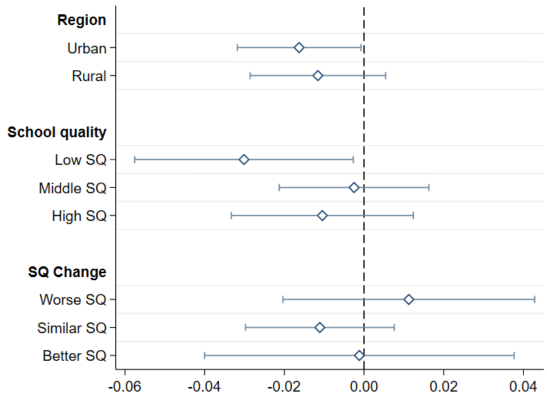

**(f)** Yearly earnings



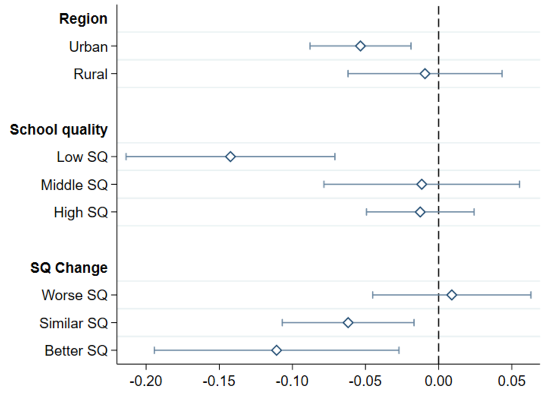

*Notes:* The figures present the coefficients, $\gamma$, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote cohorts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

**Fig. A.12.** Long-Run Effects of School Closure on Educational and Labor Market Outcomes by 26: **Rescaled** Heterogeneity by School Characteristics

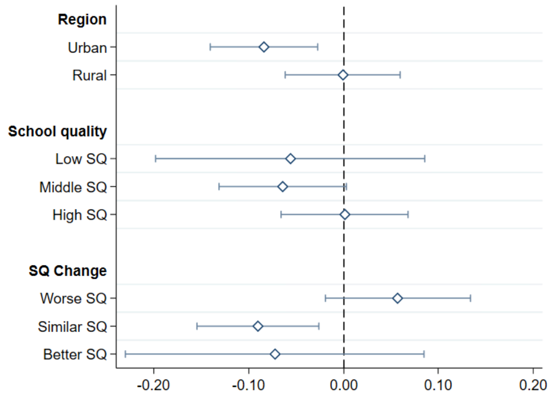
**(a)** High school graduation



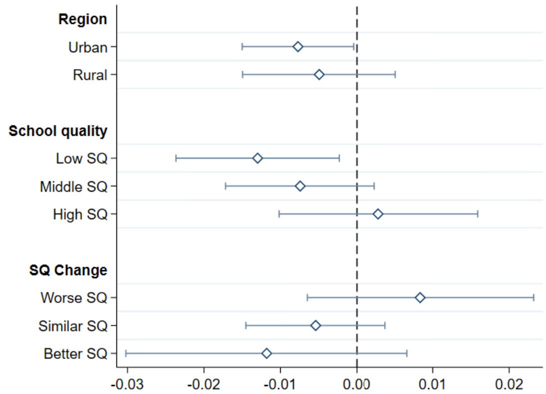

**(b)** College attendance



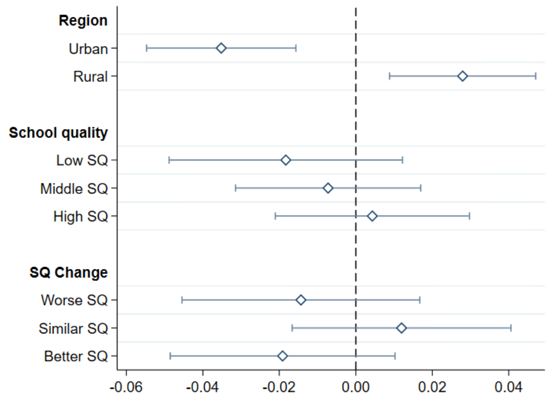

**(c)** College completion



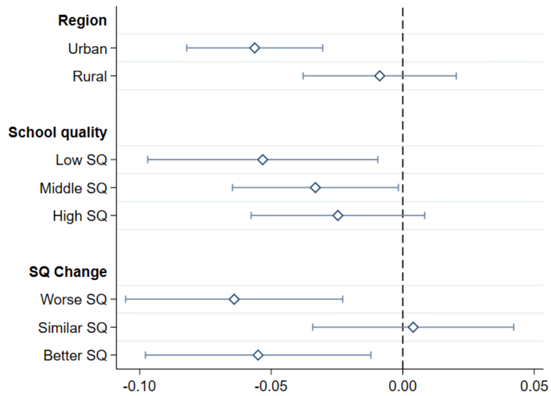

**(d)** College quality




**(e)** Employment



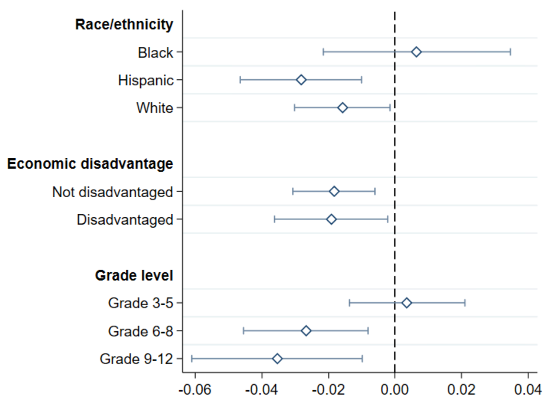

**(f)** Yearly earnings



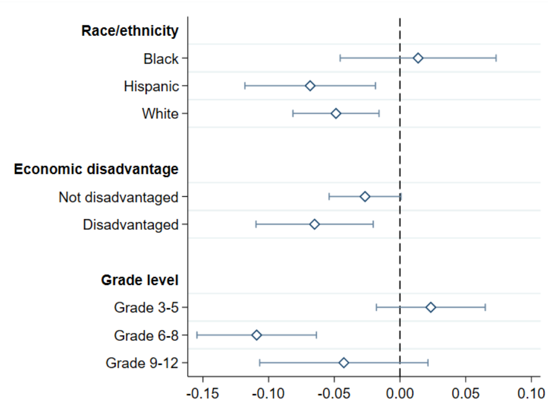

*Notes:* The figures present the coefficients, $\gamma$, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis after estimates are scaled relative to the outcome mean for each sub-group. These coefficients represent the interactions between the indicator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

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

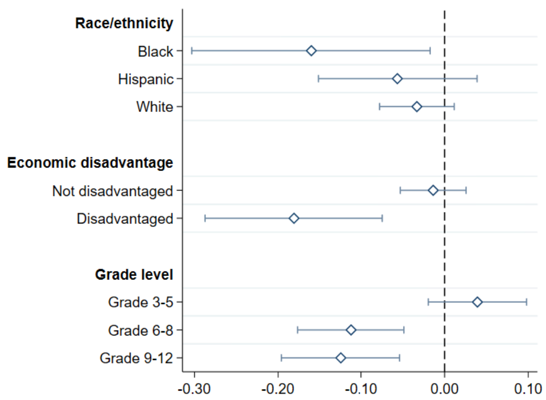
**(a)** High school graduation



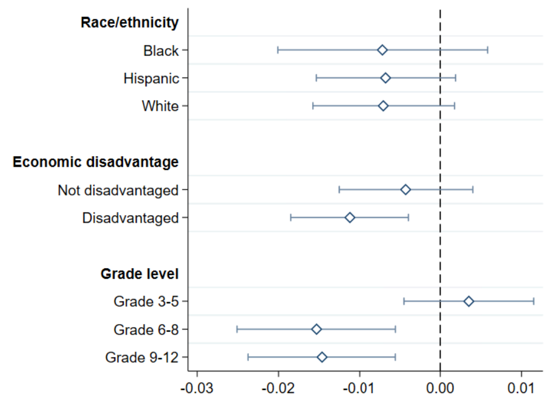

**(b)** College attendance



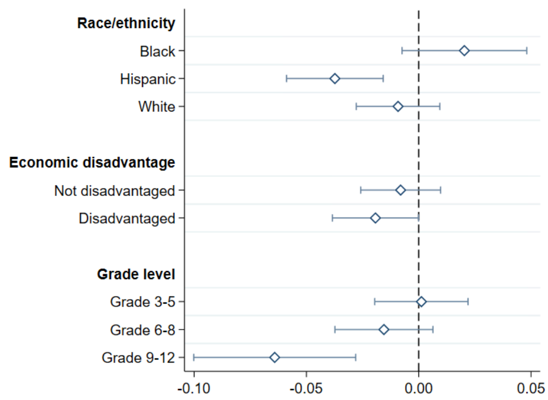

**(c)** College completion



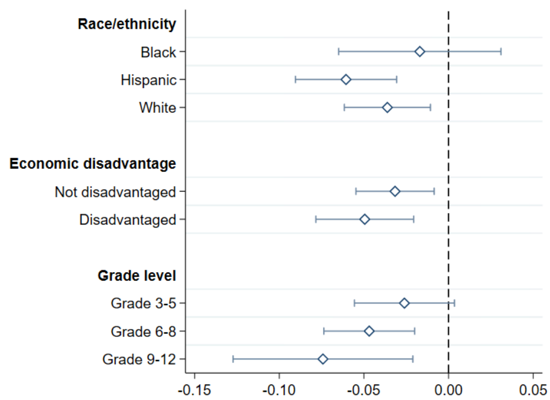

**(d)** College quality




**(e)** Employment



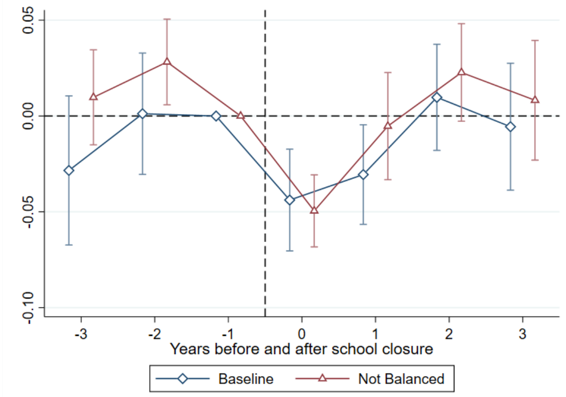

**(f)** Yearly earnings



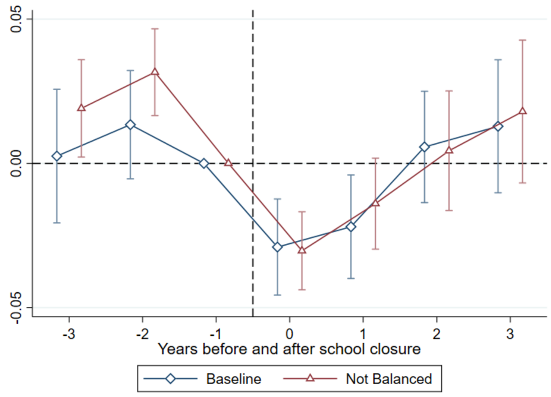

*Notes:* The figures present the coefficients, $\gamma$, and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis after estimates are scaled relative to the outcome mean for each sub-group. These coefficients represent the interactions between the indicator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

**Fig. A.14.** Short-Run Effects of School Closure on Student Outcomes: Balanced and Unbalanced Sample

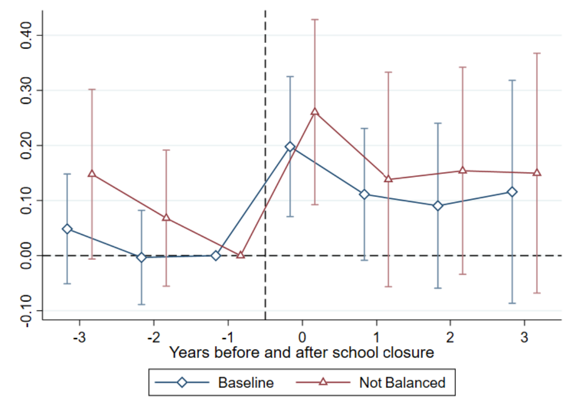
**(a)** Standardized math score



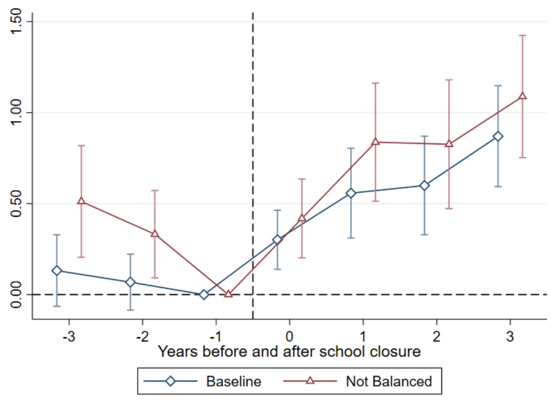

**(b)** Standardized reading score




**(c)** Days of absence



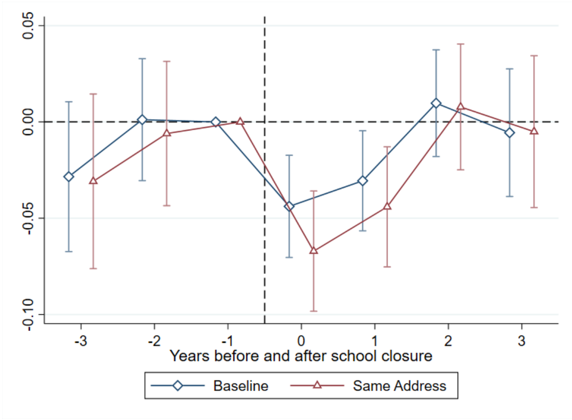

**(d)** Days of disciplinary action



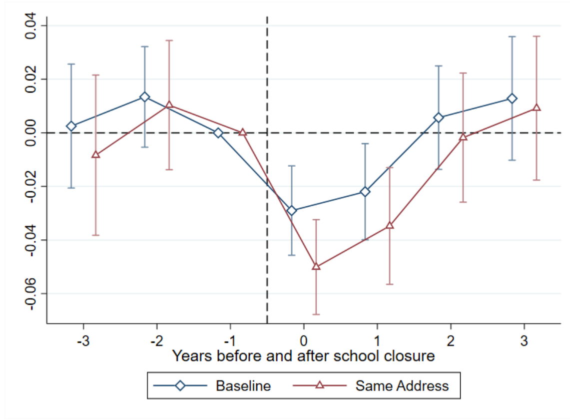

*Note:* The figures overlays the coefficients, $\rho_t$, and 95% confidence intervals from equation (2) using either baseline (balanced panel) or not balanced (unbalanced panel). These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure is the omitted category. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school.

**Fig. A.15.** Short-Run Effects of School Closure on Student Outcomes: Excluding Same Address School Opening

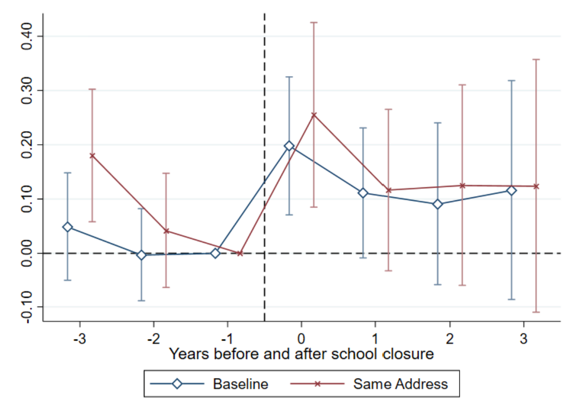
**(a)** Standardized math score



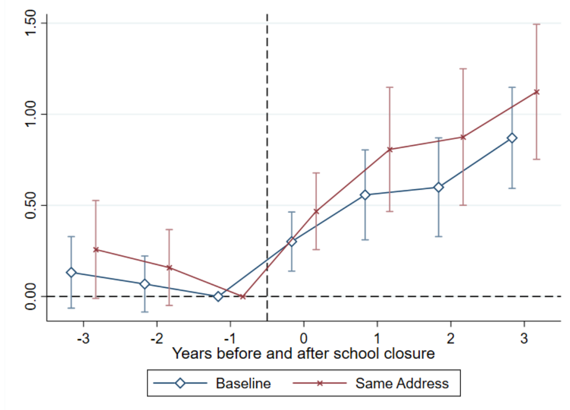

**(b)** Standardized reading score




**(c)** Days of absence



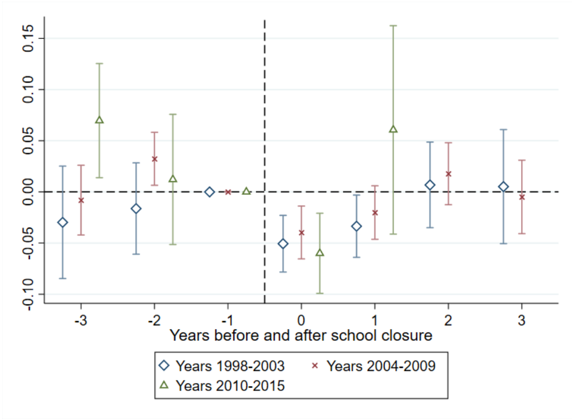

**(d)** Days of disciplinary action



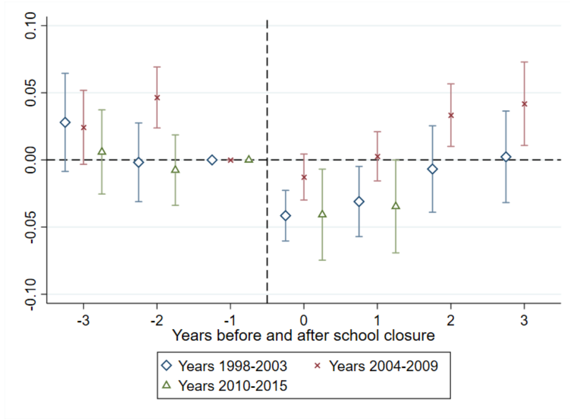

*Note:* The figures overlays the coefficients, $\rho_t$, and 95% confidence intervals from equation (2) using either baseline or same address (excluding closed schools where another school appears at the same address after the closure). These coefficients represent the interactions between the indicator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

**Fig. A.16.** Short-Run Effects of School Closure on Student Outcomes: Three Periods

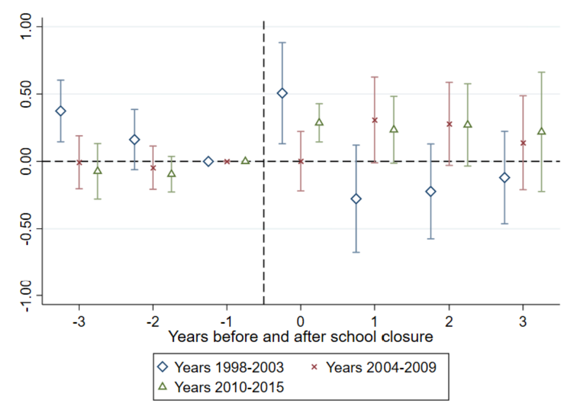
**(a)** Standardized math score



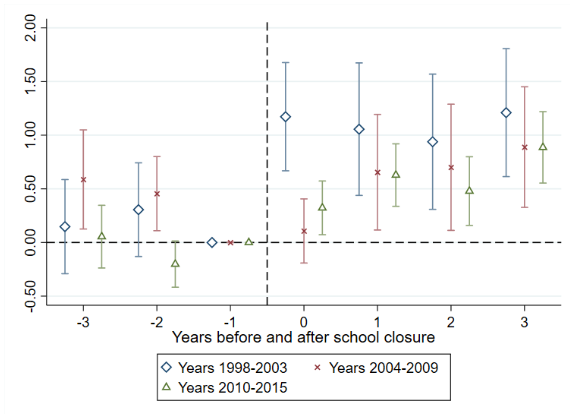

**(b)** Standardized reading score




**(c)** Days of absence



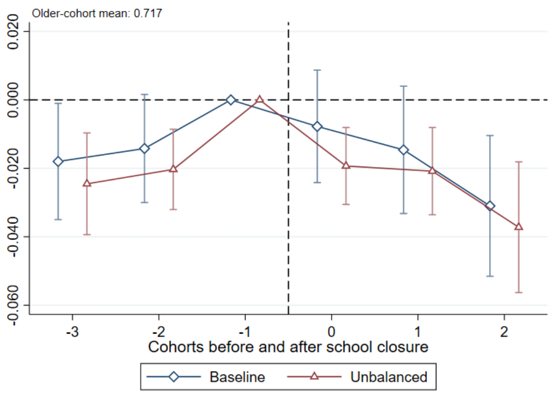

**(d)** Days of disciplinary action



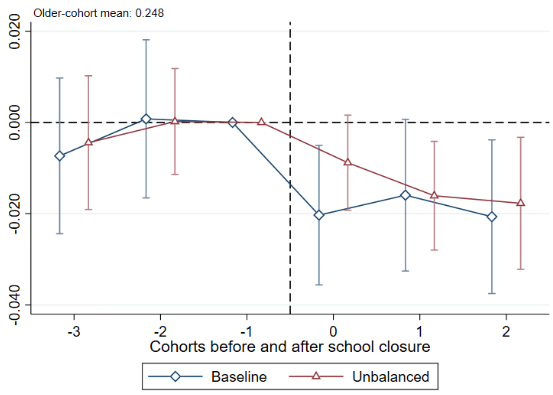

*Note:* The figures overlays the coefficients, $\rho_t$, and 95% confidence intervals from equation (2) using different periods of school closures. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a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

**Fig. A.17.** Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Balanced and Unbalanced Sample

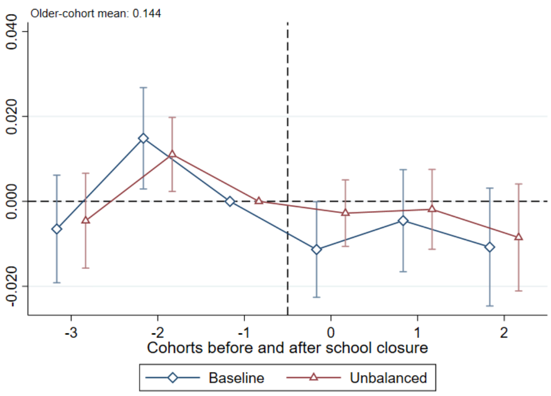
**(a)** High school graduation



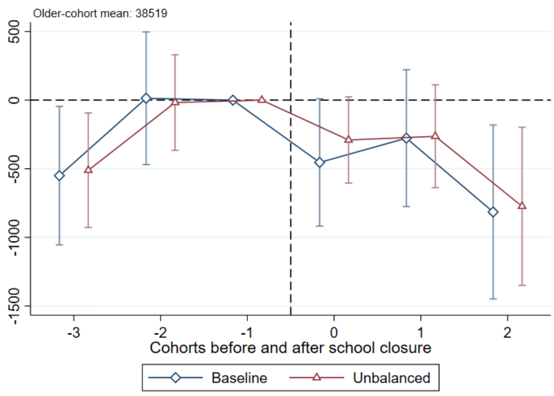

**(b)** College attendance



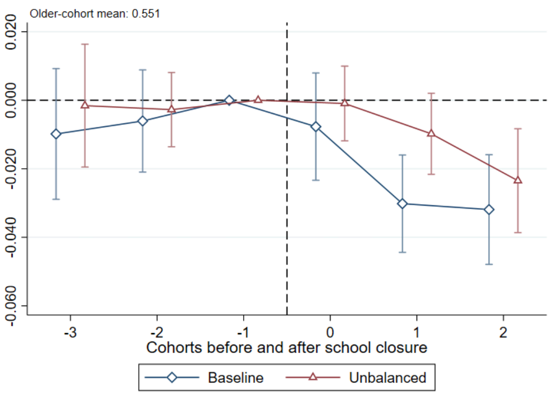

**(c)** College completion



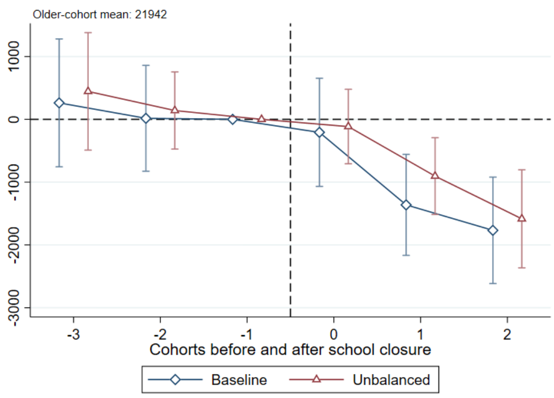

**(d)** College quality




**(e)** Employment



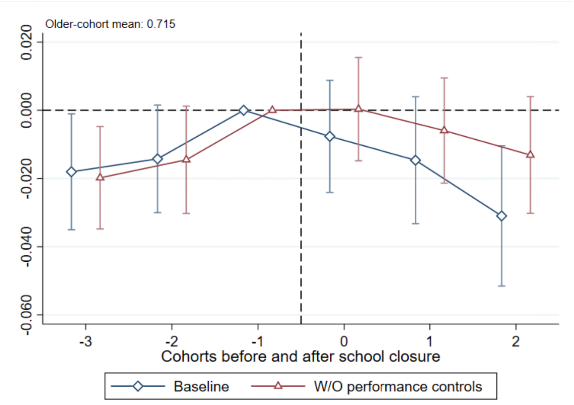

**(f)** Yearly earnings



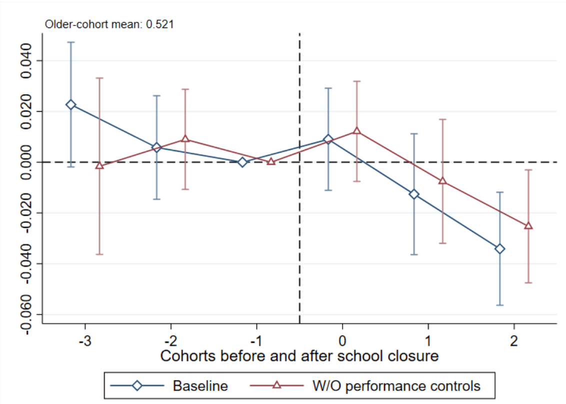

*Notes:* The figures overlay the coefficients, $\pi_t$, and 95% confidence intervals from equation (4) using either baseline (balanced) or unbalanced sample. The unbalanced sample includes closed schools having at least two grades. These coefficients represent the interactions between the indicator that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

**Fig. A.18.** Long-Run Effects of School Closure on Educational and Labor Market Outcomes: With and Without Controlling for Performance Measures

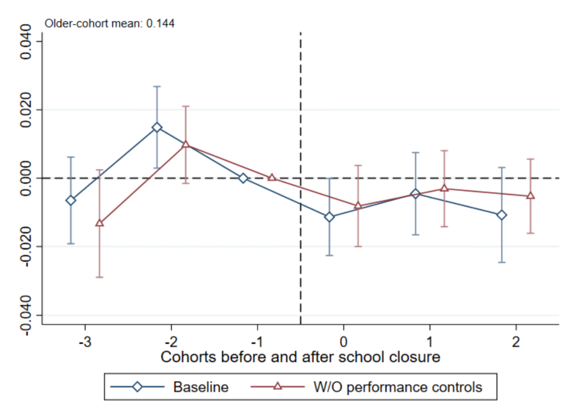
**(a)** High school graduation



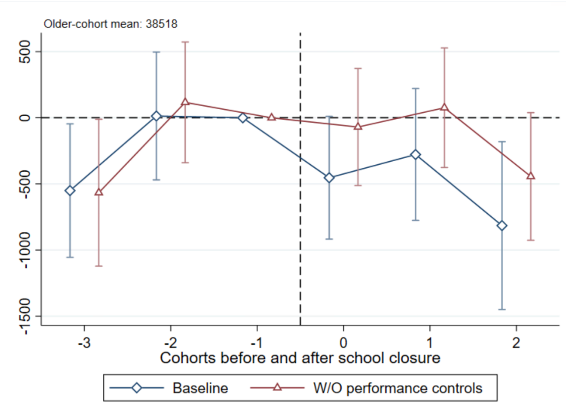

**(b)** College attendance



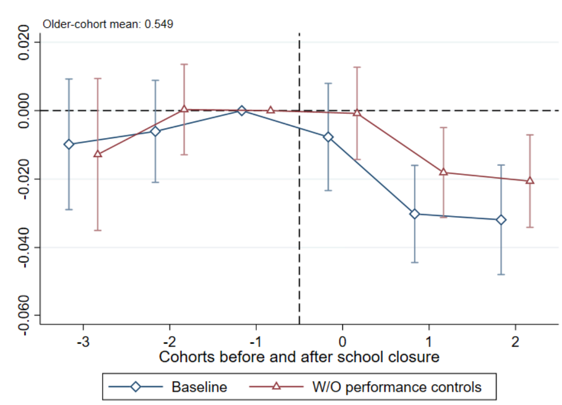

**(c)** College completion



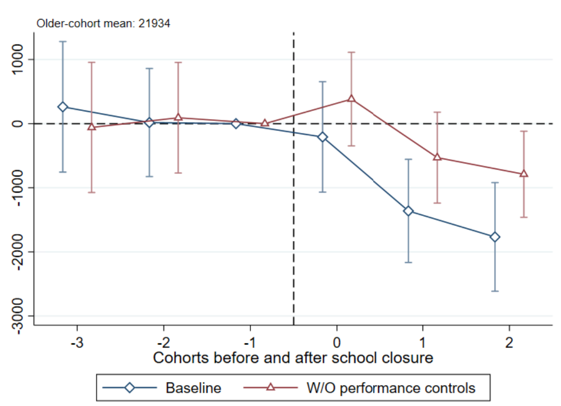

**(d)** College quality




**(e)** Employment



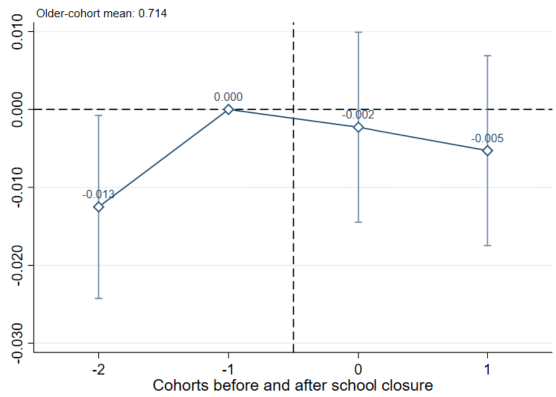

**(f)** Yearly earnings



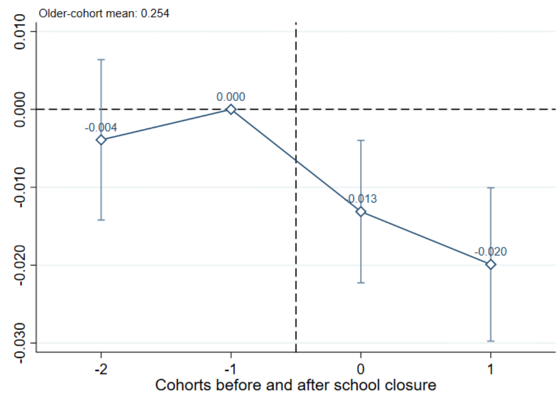

*Notes:* The figures overlay the coefficients, $\pi_t$, and 95% confidence intervals from equation (4) with and without controlling for standardized math and reading scores, and standardized absence rate. These coefficients represent the interactions between the indicator that denotes closed schools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

**Fig. A.19.** Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Alternative Way of Cohort Construction

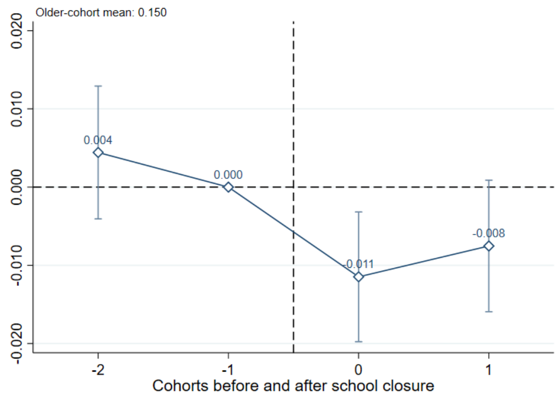
**(a)** High school graduation



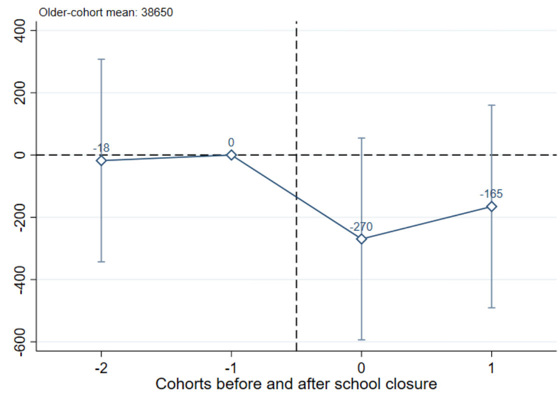

**(b)** College attendance



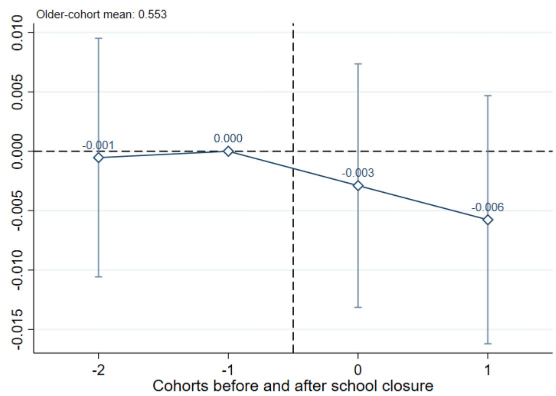

**(c)** College completion



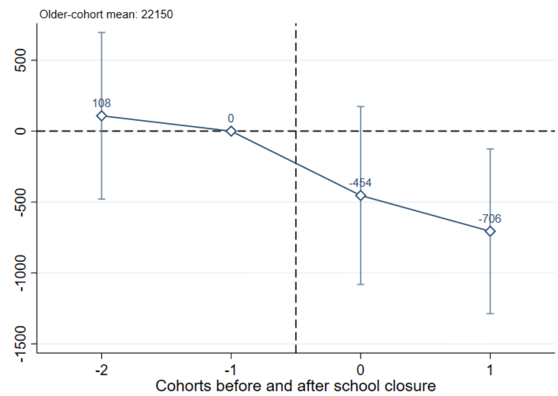

**(d)** College quality




**(e)** Employment



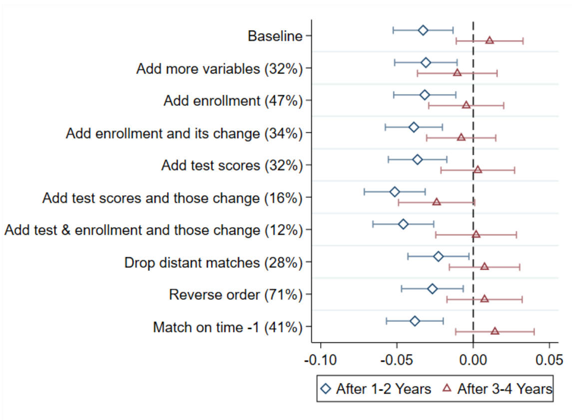

**(f)** Yearly earnings



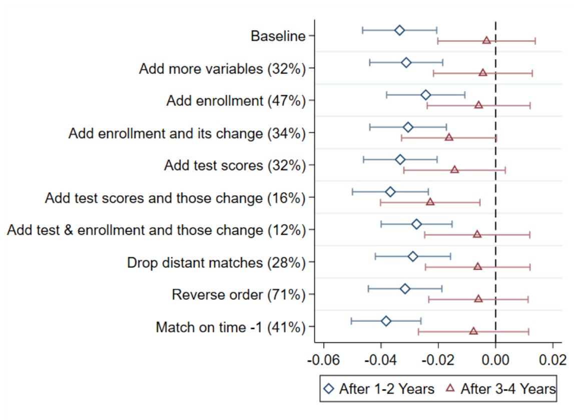

*Notes:* The figures present the coefficients, $\pi_t$, and 95% confidence intervals from equation (4). These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts already graduated within three years and in the school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

**Fig. A.20.** Short-Run Effects of School Closure on Student Outcomes: Alternative Matching Strategies

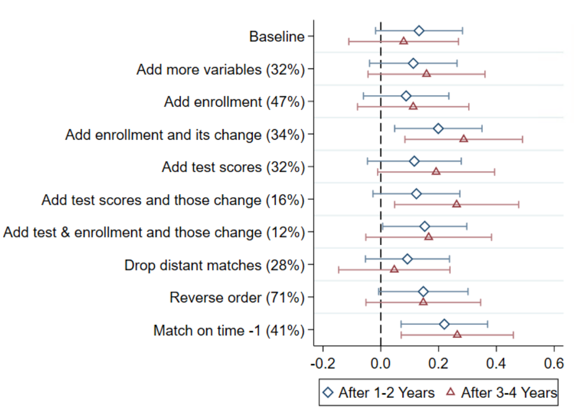
**(a)** Standardized math score



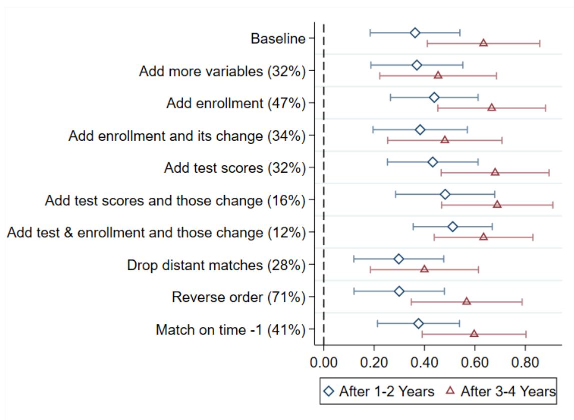

**(b)** Standardized reading score




**(c)** Days of absence



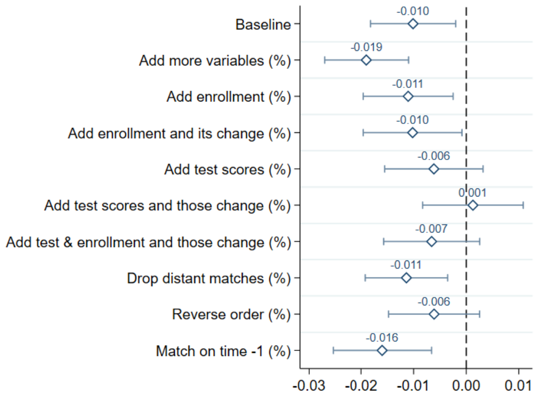

**(d)** Days of disciplinary action



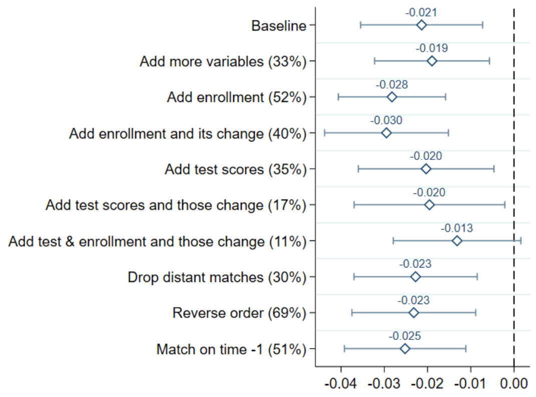

*Notes:* The figures present the coefficients, $\beta$, and 95% confidence intervals from equation (1) using control schools selected from the alternative matching strategies denoted on the y-axis. At the end of the name of each alternative matching method, the percentages of the same matched control schools as the baseline are added. The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote years after a school closure. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school. My 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.

**Fig. A.21.** Long-Run Effects of School Closure on Educational and Labor Market Outcomes: Alternative Matching Strategies

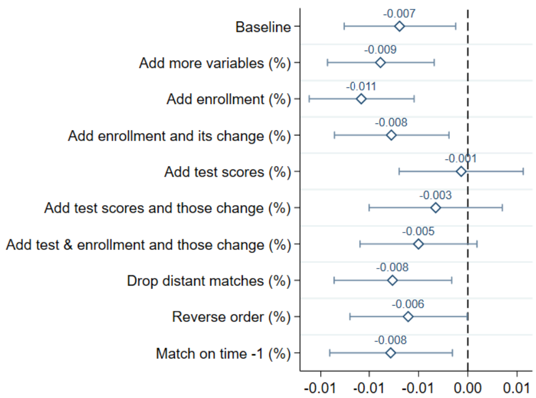
**(a)** High school graduation



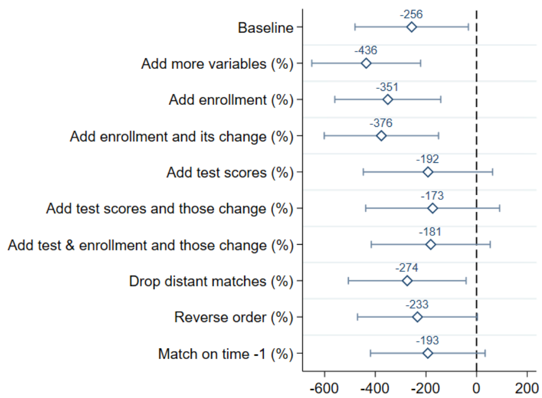

**(b)** College attendance



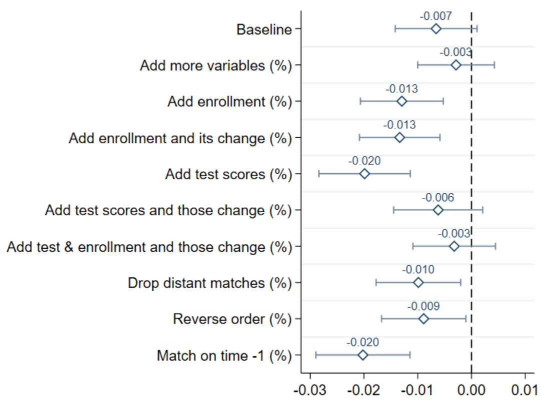

**(c)** College completion



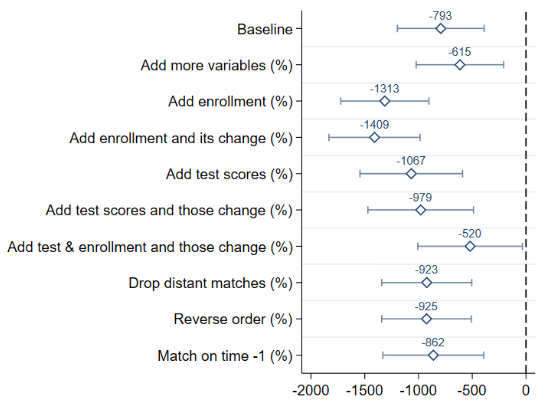

**(d)** College quality




**(e)** Employment at ages 25-27



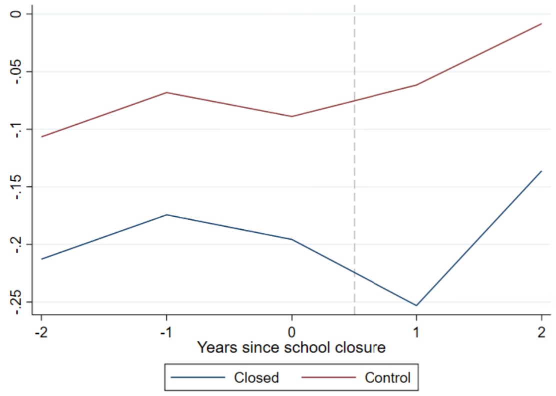

**(f)** Yearly wages at ages 25-27



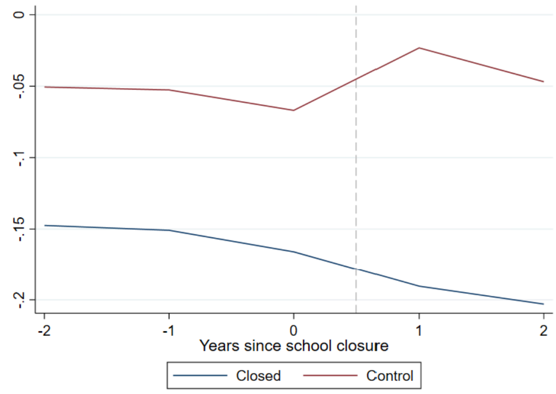

*Notes:* Each sub-figure presents the coefficients, $\gamma$, and 95% confidence intervals from equation (3) using control schools selected from the alternative matching strategies denoted on the y-axis. At the end of the name of each alternative matching method, the percentages of the same matched control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

**Fig. A.22.** Synthetic Difference-in-Differences: Outcome Trends in Short-Run Outcomes Between Closed and Control Schools

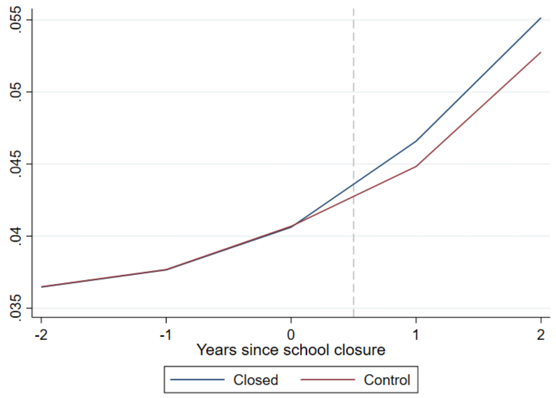
**(a)** Standardized math score



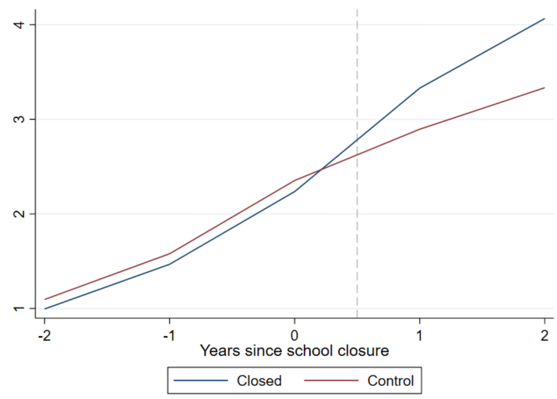

**(b)** Standardized reading score




**(c)** Absence rate



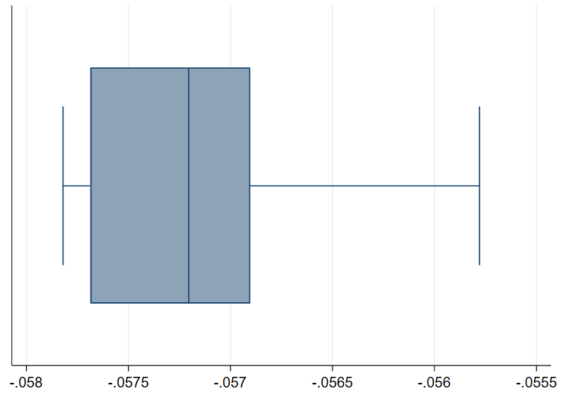

**(d)** Days of disciplinary action



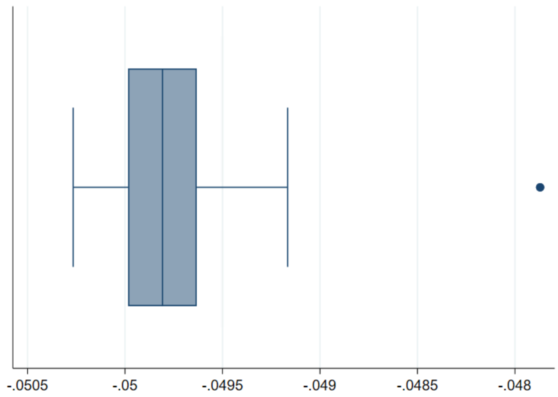

*Notes:* Each sub-figure presents outcome trends from the implementation of a synthetic difference-in-differences model following Arkhangelsky et al. (2021). In this model, a synthetic control group is constructed using a donor pool of randomly chosen 10,000 students who are enrolled in the same year,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

**Fig. A.23.** Synthetic Difference-in-Differences: Distribution of Estimated Coefficients From Different Donor Pool

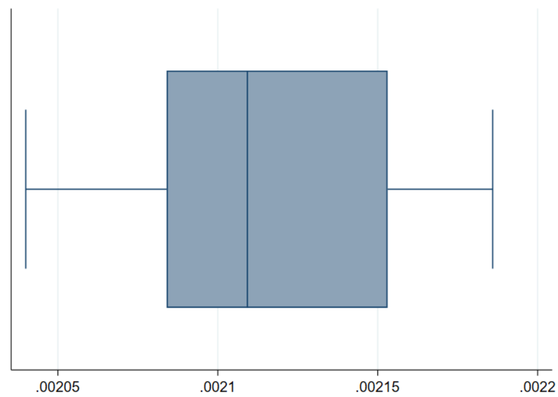
**(a)** Standardized math score



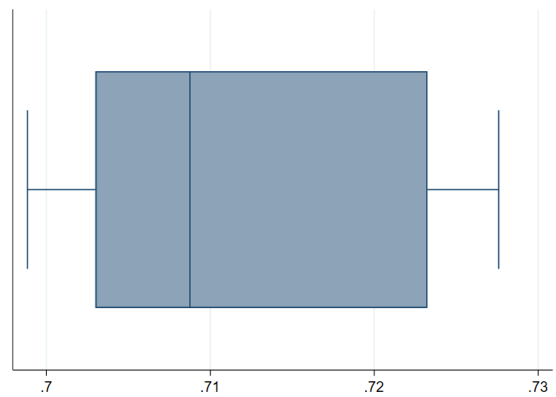

**(b)** Standardized reading score




**(c)** Absence rate




**(d)** Days of disciplinary action




*Notes:* Each sub-figure presents the distribution of the coefficients from a synthetic difference-in-differences model using a different donor pool selected at random. The distribution of these coefficients is presented using box-and-whisker plots, where the box shows the range from the 25th percentile to the 75th percentile, the line inside the box represents the median, and the whiskers outside the box show the range from the upper adjacent value to the lower adjacent value. Dots outside the whiskers represent outliers.

Table A.1: School Summary Statistics

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

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

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

| | (1) $t = -1$ | (2) $t = 0$ | (3) Difference | (4) P-Value |
|---|---|---|---|---|
| *Original Students* | | | | |
| Standardized Math Score | -0.002 | -0.030 | -0.028 | 0.000 |
| Standardized Reading Score | 0.016 | -0.002 | -0.018 | 0.002 |
| *Move-In Students* | | | | |
| Standardized Math Score | -0.228 | -0.306 | -0.078 | 0.000 |
| Standardized Reading Score | -0.226 | -0.290 | -0.064 | 0.000 |

Notes: The table presents the average test scores for 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). The original student comprises 105,258 students, and the move-in students comprise 98,721 students.

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

| | Out-of-State College Enrollment |
|---|---|
| Closed School × Younger Cohorts | -0.000 |
| | (0.002) |
| Observations | 100,287 |
| School FE | X |
| Matched group × Year FE | X |
| Mean of the Older Cohort | 0.035 |

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

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

| ***Panel A: trimming based on differential attrition out of sample*** | | | |
|---|---|---|---|
| | (1) Baseline | (2) Lee Lower Bound | (3) Lee Upper Bound |
| **High school graduation** | | | |
| Closed School | -0.010** | -0.009** | -0.13*** |
| × Younger Cohorts | (0.004) | (0.004) | (0.004) |
| **College attendance** | | | |
| Closed School | -0.012*** | -0.010** | -0.013*** |
| × Younger Cohorts | (0.004) | (0.004) | (0.004) |
| **College completion** | | | |
| Closed School | -0.007** | -0.004 | -0.007** |
| × Younger Cohorts | (0.003) | (0.003) | (0.003) |
| **College quality** | | | |
| Closed School | -256** | -6 | -297*** |
| × Younger Cohorts | (114) | (114) | (115) |
| **Employment at ages 25-27** | | | |
| Closed School | -0.007* | -0.005 | -0.007* |
| × Younger Cohorts | (0.004) | (0.004) | (0.004) |
| **Yearly wages at ages 25-27** | | | |
| Closed School | -793*** | -301 | -821*** |
| × Younger Cohorts | (205) | (202) | (205) |
| **Non-Zero Yearly wages at ages 25-27** | | | |
| Closed School | -842*** | -306 | -870*** |
| × Younger Cohorts | (243) | (239) | (244) |
| School FE | X | X | X |
| Matched group × Year FE | X | X | X |

| ***Panel B: trimming based on difference in employment rate*** | | | |
|---|---|---|---|
| | (1) Baseline | (2) Lee Lower Bound | (3) Lee Upper Bound |
| **Non-Zero Yearly wages at ages 25-27** | | | |
| Closed School | -942*** | -143 | -1,166*** |
| × Younger Cohorts | (262) | (256) | (262) |
| School FE | X | X | X |
| Matched group × Year FE | X | X | X |

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

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

| | (1) No Control | (2) Demographic Control | (3) Full Control |
|---|---|---|---|
| **High school graduation** | | | |
| Closed School | -0.006 | -0.006 | -0.010 |
| × Younger Cohorts | (0.004) | (0.004) | (0.004) |
| **College attendance** | | | |
| Closed School | -0.007* | -0.007 | -0.012*** |
| × Younger Cohorts | (0.005) | (0.004) | (0.004) |
| **College completion** | | | |
| Closed School | -0.005 | -0.003 | -0.007** |
| × Younger Cohorts | (0.003) | (0.003) | (0.003) |
| **College quality** | | | |
| Closed School | -167 | -95 | -256** |
| × Younger Cohorts | (132) | (125) | (114) |
| **Employment at ages 25-27** | | | |
| Closed School | -0.006 | -0.005 | -0.007* |
| × Younger Cohorts | (0.004) | (0.004) | (0.004) |
| **Yearly wages at ages 25-27** | | | |
| Closed School | -789*** | -632*** | -793*** |
| × Younger Cohorts | (218) | (208) | (205) |
| **Non-zero yearly wages at ages 25-27** | | | |
| Closed School | -1123*** | -714*** | -842*** |
| × Younger Cohorts | (260) | (246) | (243) |
| School FE | X | X | X |
| Matched group × Year FE | X | X | X |

Notes: Each row of the table presents the coefficients, $\gamma$, 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. The regression includes school and match group-by-cohort fixed effects. Column (1) does not include demographic and performance variables. Column (2) includes individual-level demographic control variables such as race/ethnicity, sex, ESL status, and special education status. Column (3) includes performance measures such as standardized test scores and standardized absence rate, as well as demographic variables in Column (2). Standard errors are clustered by school-by-cohort level. *** p<0.01, ** p<0.05, * p<0.10

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

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

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