

Supplemental Appendix

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

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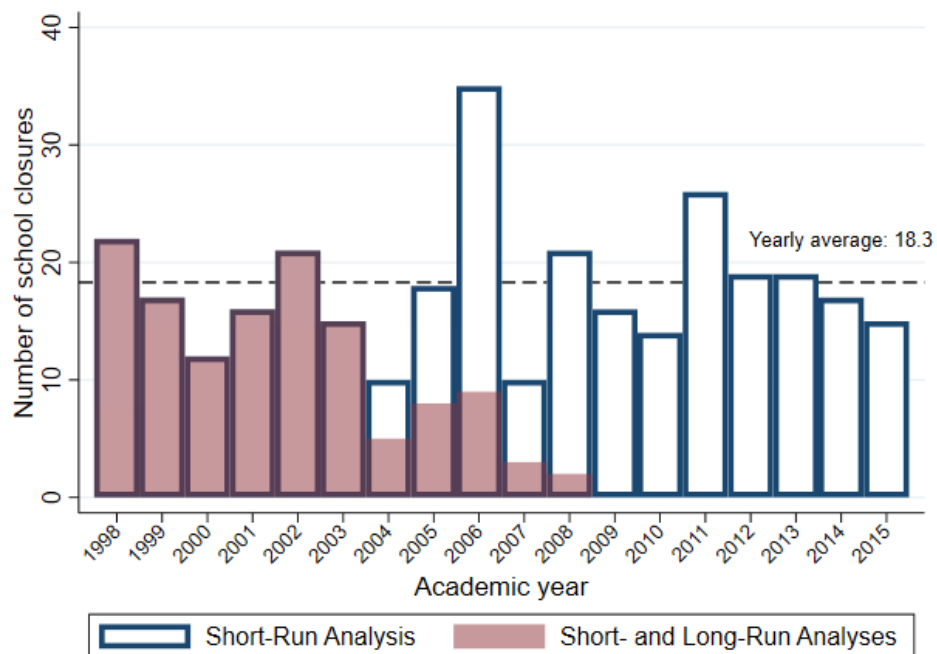
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A Additional Figures and Tables

A.1 Characteristics of Closed Schools and Displaced Students

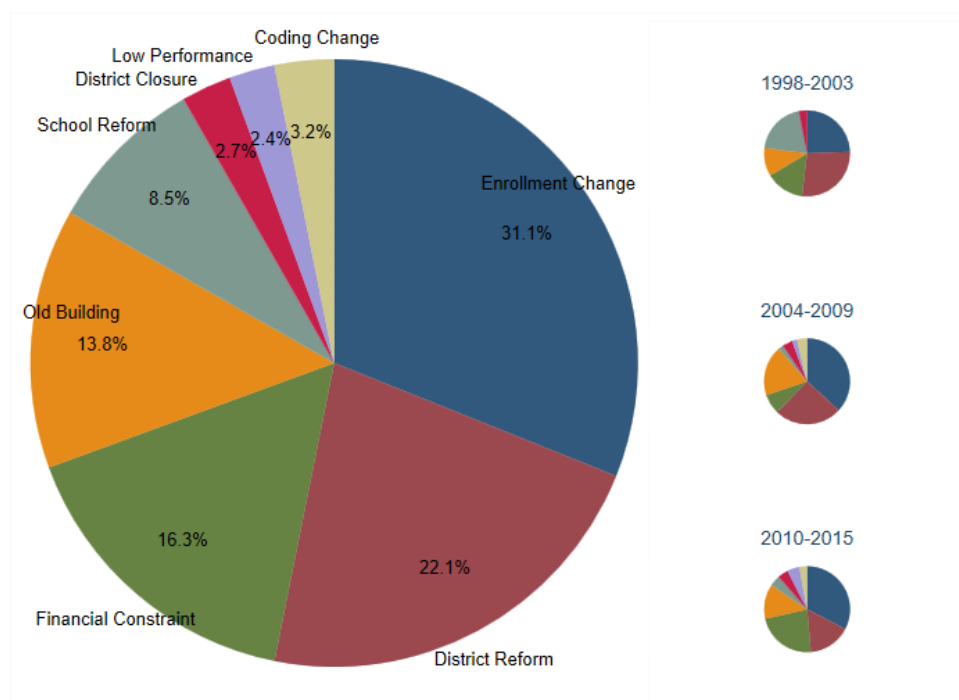
Fig. A.1. Annual Number of School Closures at Texas Public Schools in 1998–2015



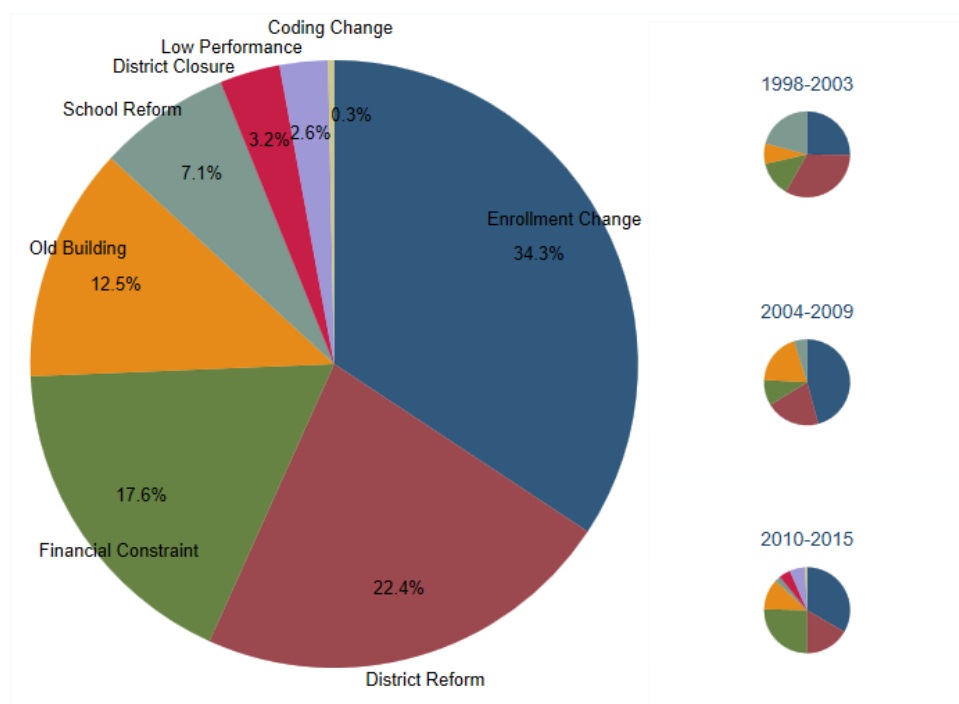
Notes: The figure presents the distribution of 323 (130) school closures that occurred between 1998 and 2015. To be considered a closed school, the school must be officially listed as closed by the Texas Education Agency (TEA), be a non-charter instructional campus in a regular, independent district,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

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

(a) All School Closures



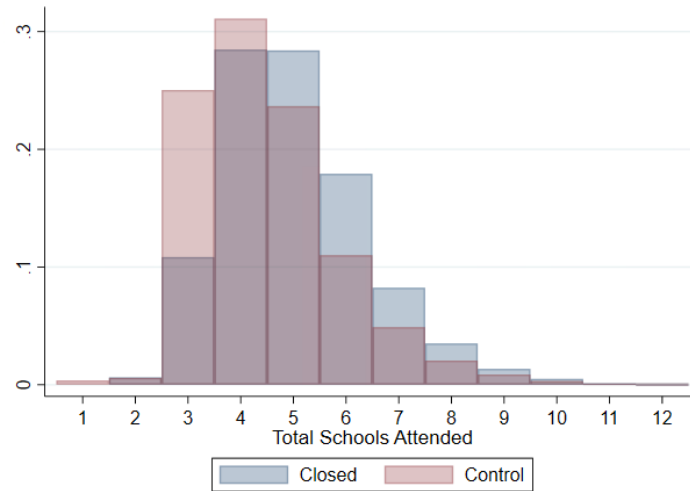
(b) Baseline Sample: Excluding Closures with >30% of Students Remaining at the Same Address



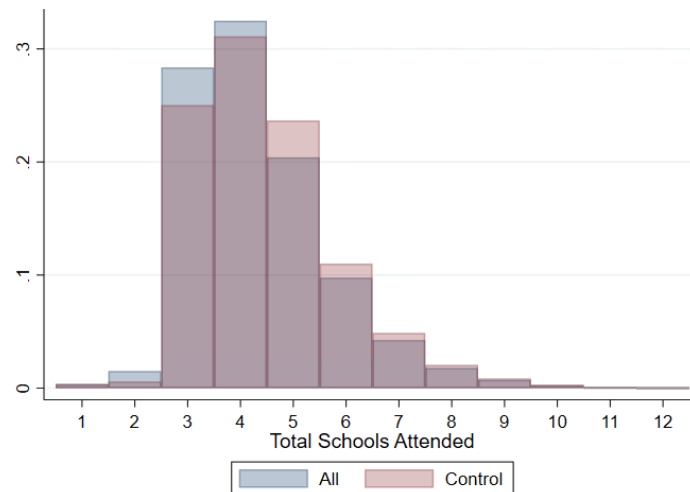
Notes: The figures display the categorized reasons for public school closures in Texas between 1998 and 2015: 274 out of 470 closures in panel (a), and 204 out of 323 closures in panel (b). Panel (a) includes all closures, while panel (b) excludes closures where more than 30 percent of students were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

Fig. A.3. The Number of Schools Attended

(a) Closed and Control Schools



(b) State Average and Control Schools



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

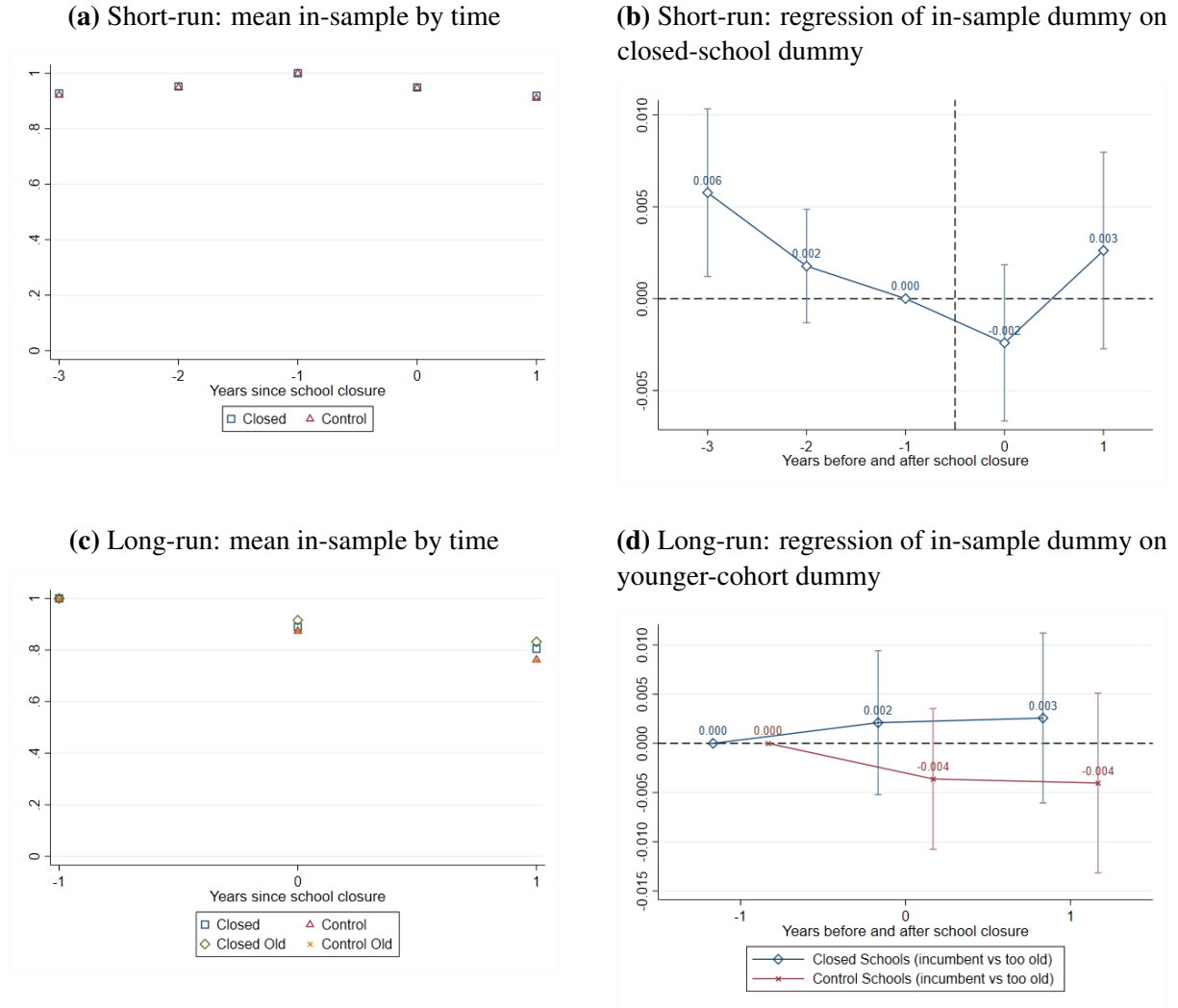
Table A.1: Average School Characteristics Across Closed, All, and Control Schools

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

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

A.2 Sample Attrition

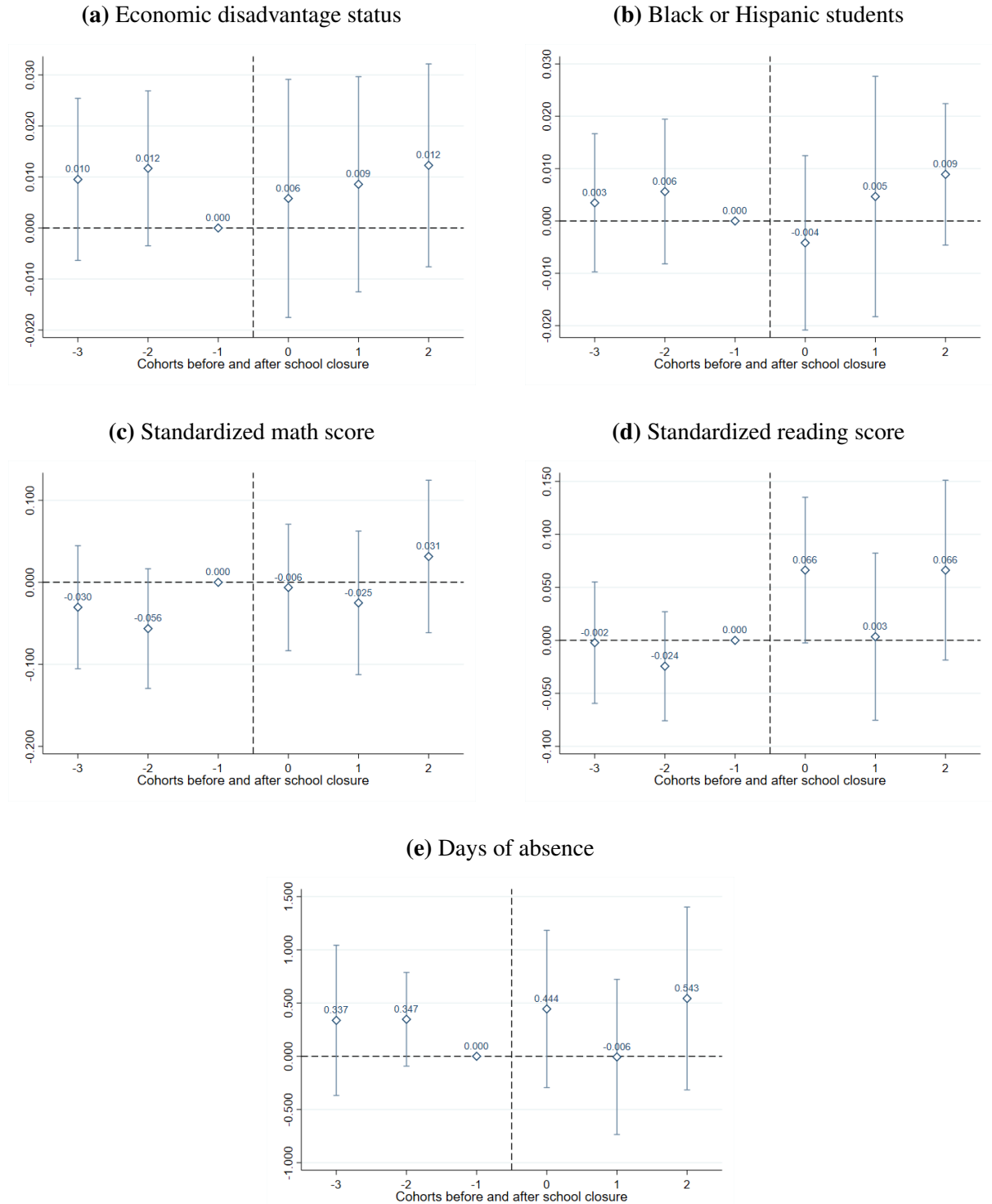
Fig. A.4. Analysis of Sample Attrition Rates of Closed and Control Schools



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

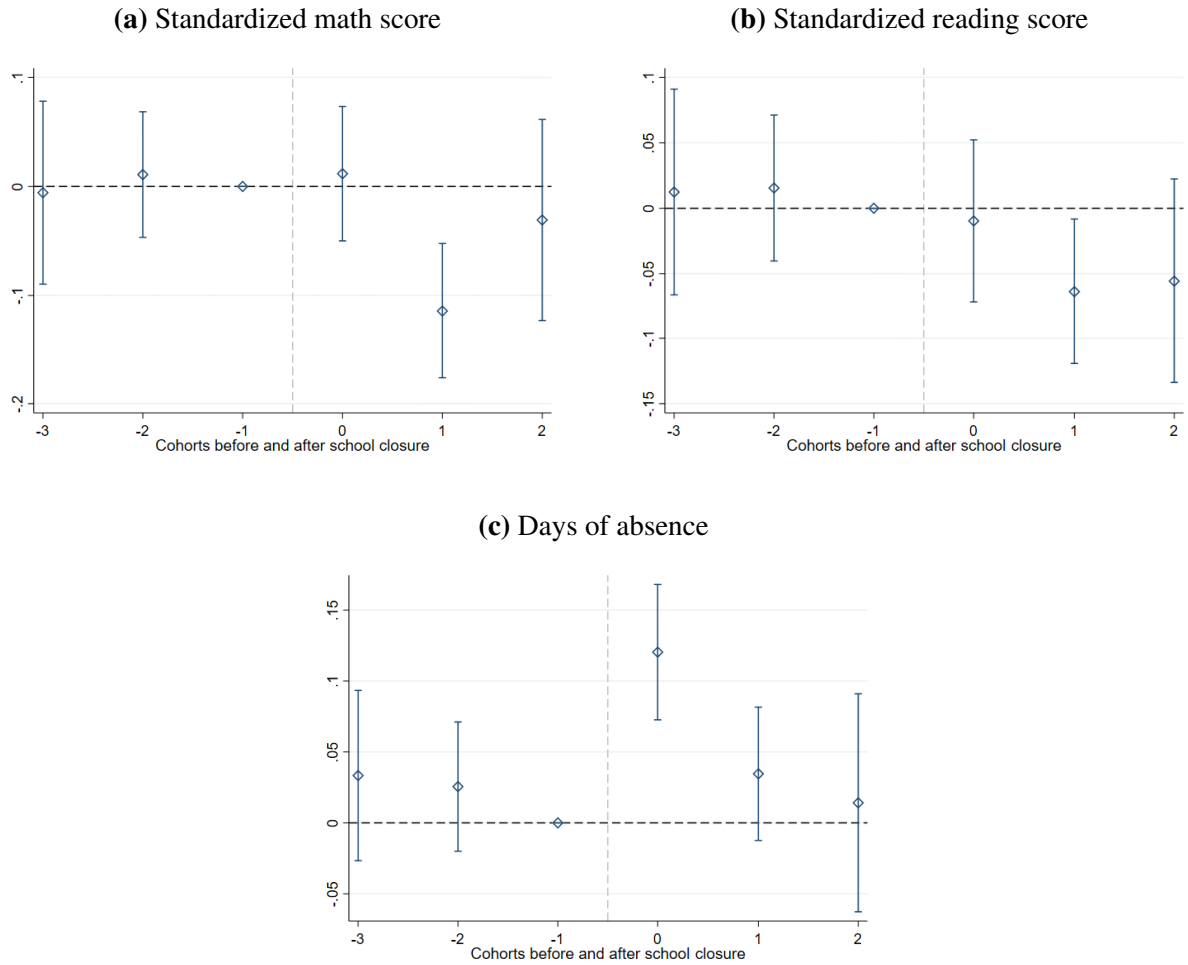
A.3 Long-Run Analysis Balance Across School Cohorts

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



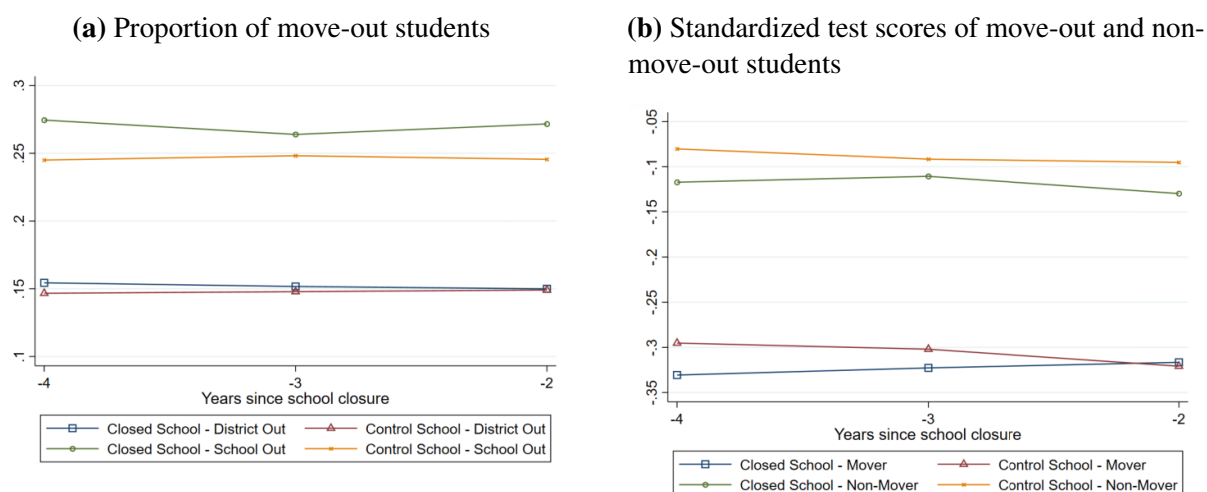
Notes: The figures present the coefficients, γ_c , and 95% confidence intervals from equation (4), in which the dependent variables are student characteristics or short-run outcomes (test scores and days of absence). The short-run outcomes are measured before school closures, specifically at $t = -1$ for younger cohorts and at $t = -4$ for older cohorts from the equation (2). These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts—those who graduated within three years prior to the closure and those enrolled at the time of the closure. The cohort that graduated one year before the closure ($c = -1$) is the omitted category. The regression includes school and match group-by-cohort fixed effects. The analysis sample is balanced. Standard errors are clustered at the school level.

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



Notes: The figures present the differences in coefficients (γ_c) and the corresponding 95% confidence intervals, based on equation (4), where the dependent variables are short-run outcomes—standardized test scores and days of absence. These differences capture the change in outcomes from before to after school closure in closed schools relative to control schools. The dependent variable is measured before school closures, specifically at $t = -1$ for younger cohorts and at $t = -4$ for older cohorts from the equation (2), and after closures, specifically at $t = 0$ for younger cohorts and at $t = -3$ for older cohorts. The displayed coefficients represent the differences in outcomes between these two time points. To be included in the analysis, individuals must be observed in both outcomes before and after closure. The cohort that graduated one year before the closure ($c = -1$) is the omitted category. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, and special education status. The analysis sample is balanced. Standard errors are clustered at the school level.

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

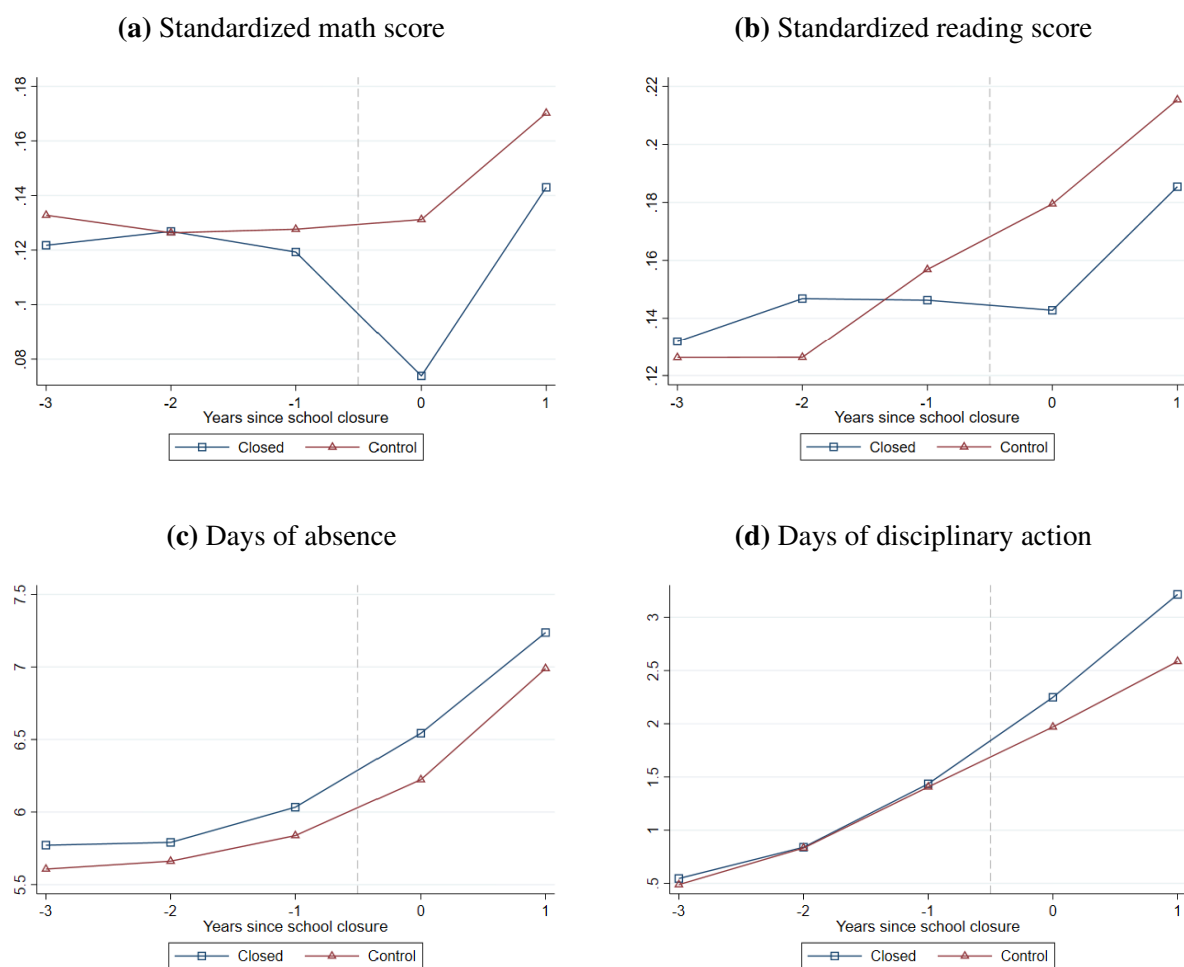


Notes: The figures illustrate student moving-out patterns in closed and matched control schools. Panel (a) shows the proportion of students who changed schools or districts in the three years leading up to a year before the school closure. Specifically, the indicator equals one if a student enrolled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

A.4 Raw Trends in Short-Run Outcomes

Appendix Figure [A.8](#) illustrates the raw trends of short-run outcomes for closed and control schools around school closure. Panels (a) and (b) show standardized math and reading scores. Prior to school closure, both closed and control schools exhibit comparable trends over the three-year period, with similar levels. The absolute raw difference remains consistently below 0.02 standard deviations. However, following school closure, a noticeable drop in average test scores of closed schools emerges, leading to a divergence in the trends between closed and control schools. Panels (c) and (d) depict days of absence and days of disciplinary action. These outcomes also demonstrate similar trends and levels in the three years preceding the school closure and start to deviate after experiencing school closure. The raw trends provide suggestive evidence that closed and control schools have similar levels and trends before closures and that students in closed schools deteriorate after experiencing school closure.

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

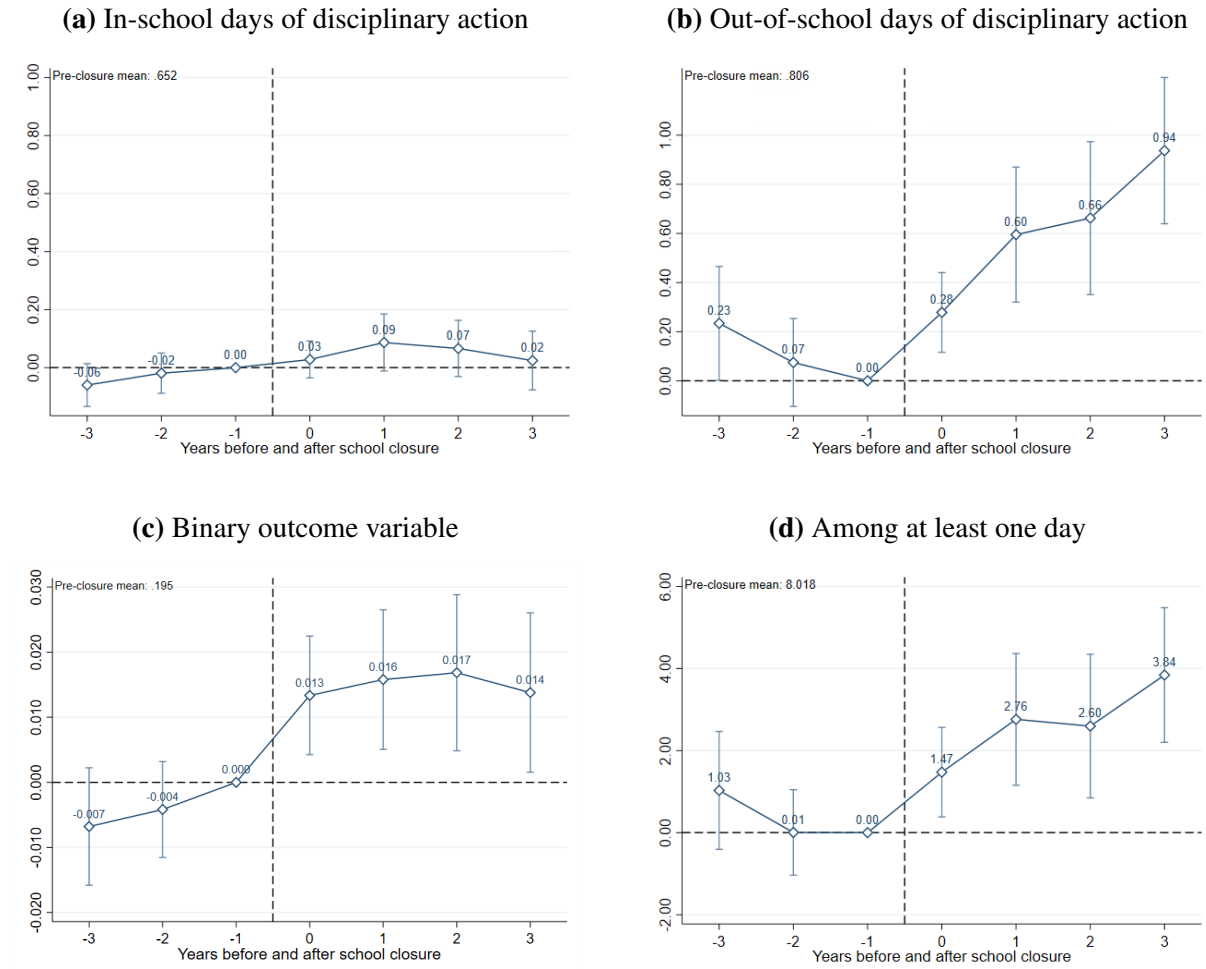


Notes: The figures plot raw trends over the period of three years before and two years after the school closure, separately for closed and matched control schools. I restrict the sample to students who are observed in the data over this period (i.e., the panel is balanced).

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

Given the significant increase in the number of days of disciplinary action following the school closure, I conduct separate analyses for days of in-school suspensions, days of out-of-school suspensions (including expulsions), and intensive/extensive margins of disciplinary actions. These results are presented in Appendix Figure [A.9](#). The increase in days of in-school suspensions is at most 0.1 days. In contrast, the number of days of out-of-school suspensions and expulsions increases by 0.2 days and keeps increasing over the following four years up to 0.9. Moreover, I find an increase in both the extensive margin—whether students have at least one day of disciplinary action—and the intensive margin—analysis among students with at least one day of disciplinary action before closure. In addition, NCES ([2018](#)) reports that the suspension rate in Texas is comparable to the national average, which supports the generalizability of these findings to other states.

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



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

A.6 Short-Run Effects of School Closures: Additional Heterogeneity

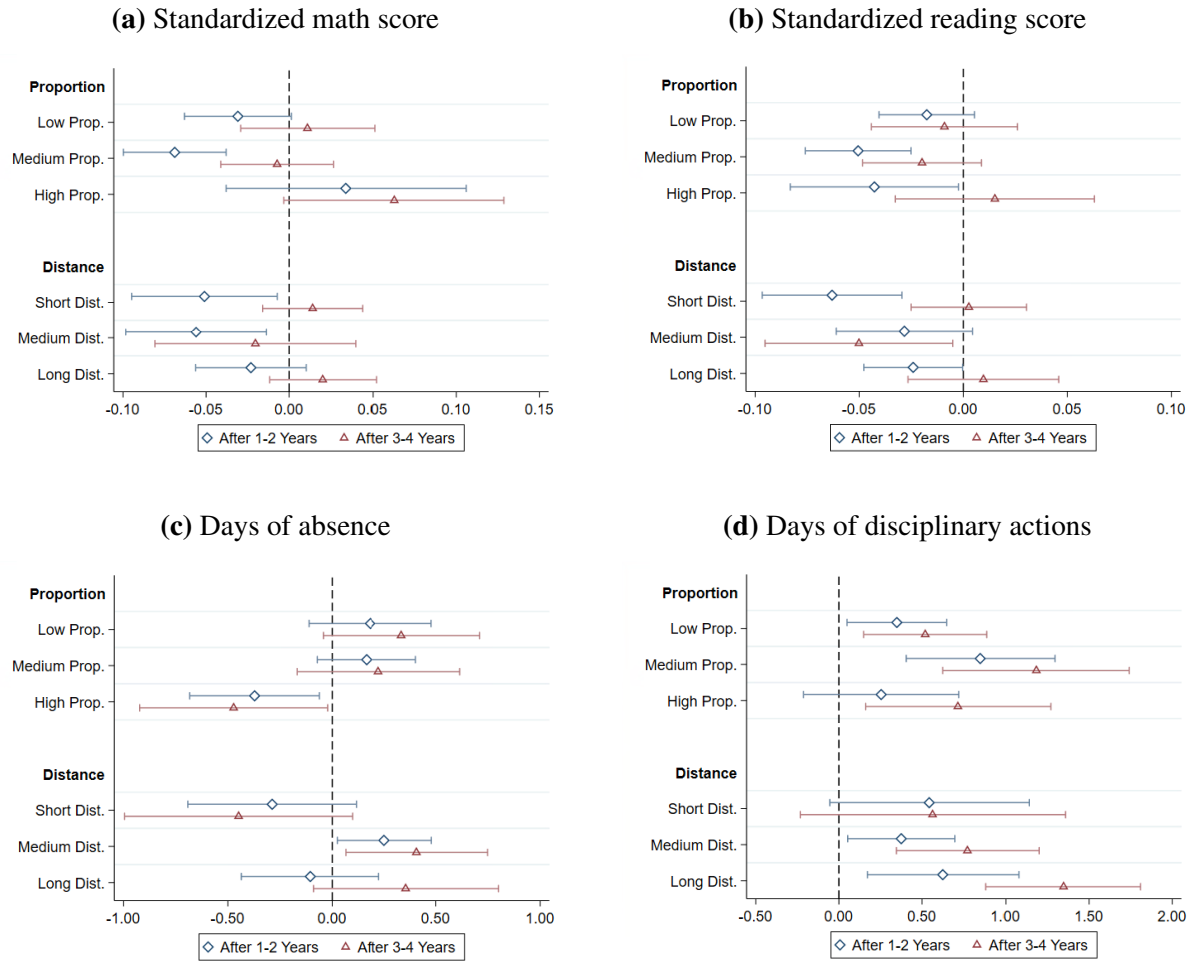
Appendix Figure A.10 examines additional heterogeneity based on the proportion of displaced students who transfer to the same school after a closure. The median proportion of displaced students who move to the same school is 56%, with quartile cutoffs at 22%, 56%, and 76%. While most differences are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

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

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

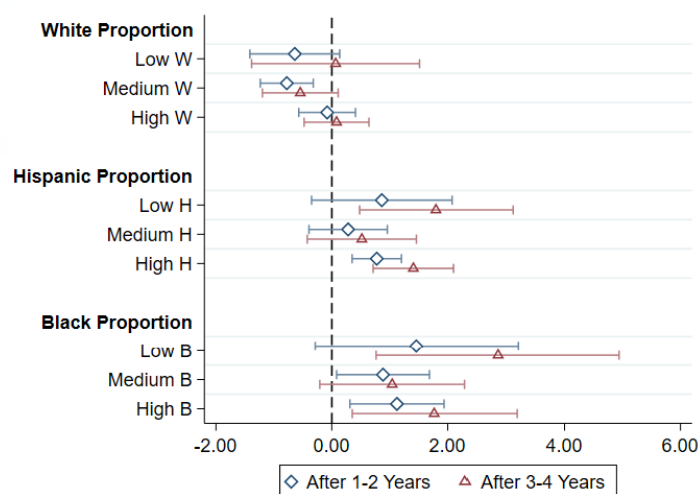
^{A.1} For White students, the groups are defined as 0–17%, 17–50%, and 50–97%; for Hispanic students, as 0–27%, 27–61%, and 61–100%; and for Black students, as 0–8%, 8–25%, and 26–94%.

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



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

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

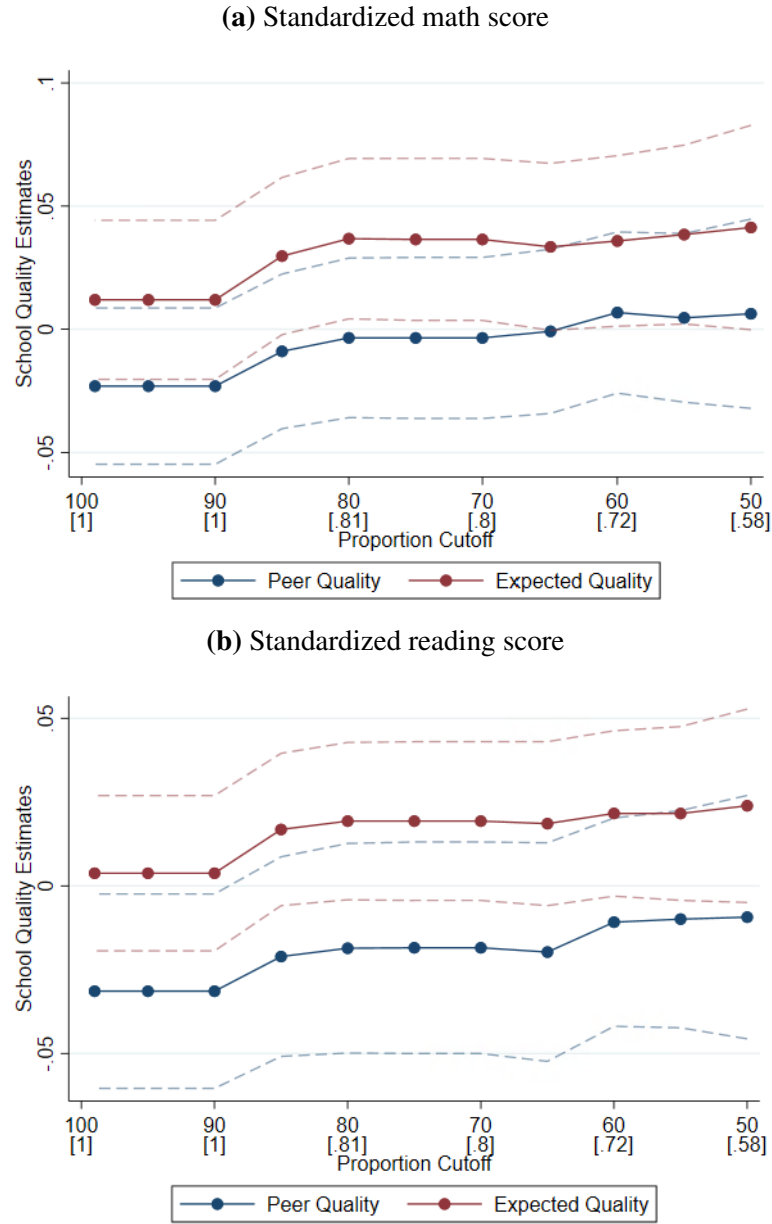


Notes: The figures present the coefficients, β , and 95% confidence intervals from equation (1) for students belonging to the sub-group denoted on the y-axis. For instance, "Low W" refers to White students who moved to a school with a low proportion of White students. These coefficients represent the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

A.7 School Quality Changes

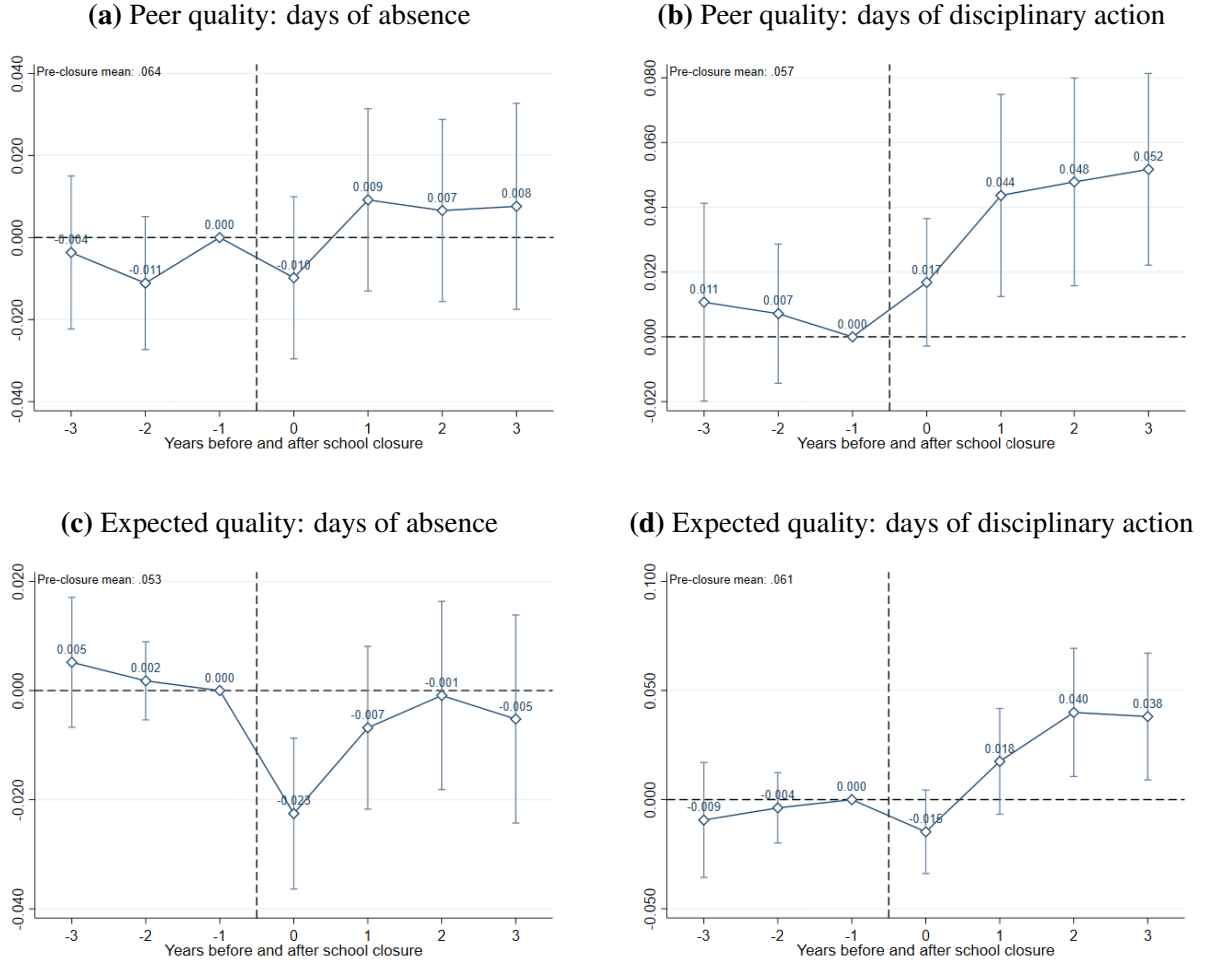
To further understand why students do not have high-performing peers even after transitioning to originally better-performing schools, I examine average test score changes of receiving schools before and after school closure (i.e., $t = 0$ and $t = -1$), dividing students into original students and move-in students. Appendix Table A.2 presents that both groups exhibit a decline in test scores, with the move-in group showing a larger decline. Specifically, move-in students demonstrate a decline of -0.073 to -0.081 standard deviations in test scores, while original students show a decline of -0.021 to -0.038 standard deviations between students observed in $t = 0$ and $t = -1$. This suggests that the change in school quality is a combination of changes in student composition, potentially resulting from alterations in attendance zones along with school closures, and spillover effects coming from having new students. However, it is important to acknowledge the limitations of comparing the same school over two years when examining the changes in school quality following closures. This approach might introduce the potential influence of other secular trends that are unrelated to school closures. Therefore, it is crucial to exercise caution in interpreting these results and recognize the need for a more rigorous analysis of receiving schools in future research.

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



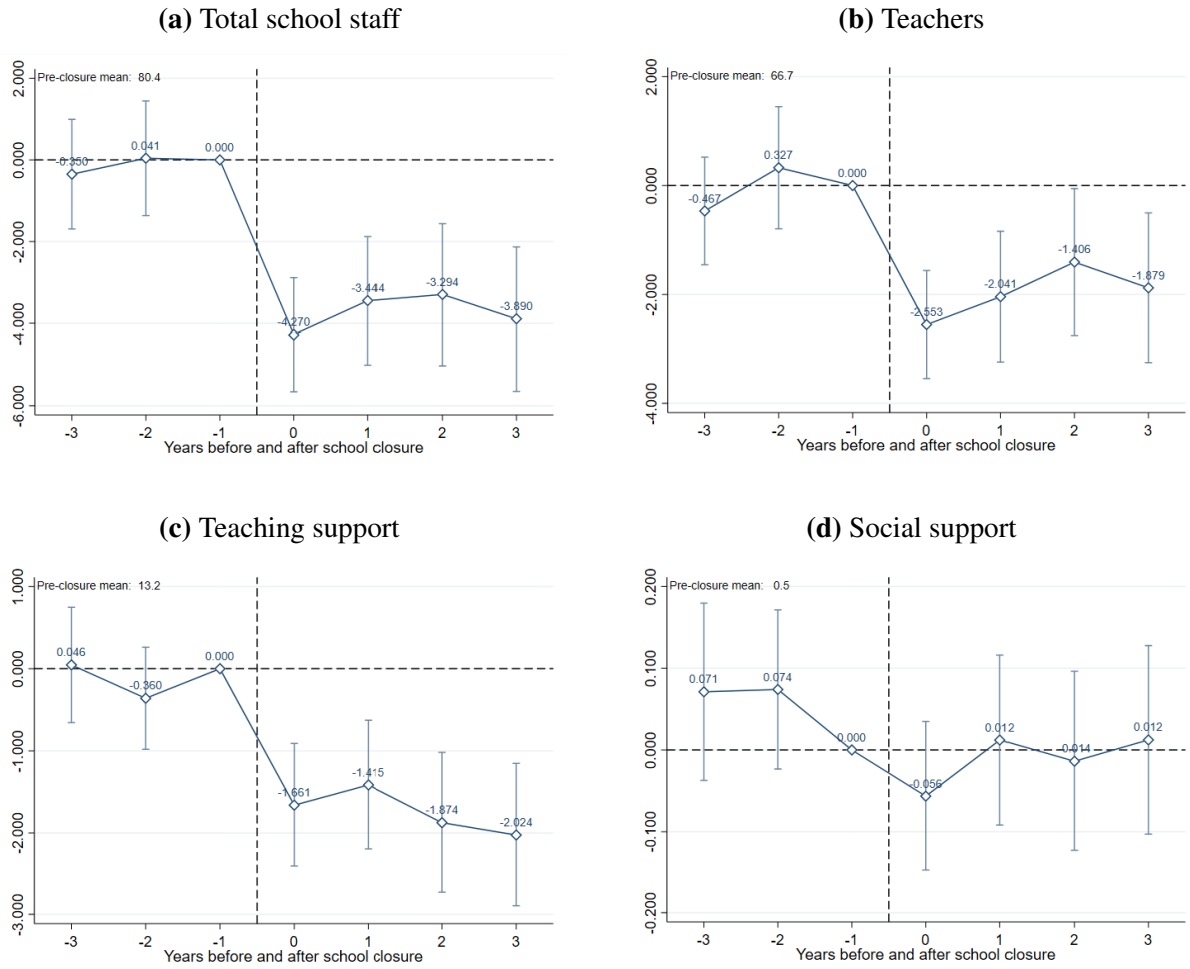
Notes: The figures present the coefficients, β , and 95% confidence intervals from equation (1), where the outcome variables are the school-level average math and reading scores. The x-axis specifies different cutoffs for sample inclusion. For peer quality, the outcome variables are the yearly school average of the outcomes, excluding displaced students from the calculation after the school closure (i.e., $t \geq 0$). For expected quality, the outcome variables are the school average over the four years preceding the school closure (i.e., $t \in \{-4, \dots, -1\}$). The x-axis represents the sample cutoff, where students are excluded if displaced students account for more than the specified proportion of the receiving school's population. The numbers in brackets reflect the percentage of the sample included under each cutoff. For example, 80% of the sample is included under the baseline cutoff of 70%. The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote years after school closure. The regression includes individual and match group-by-year fixed effects. Standard errors are clustered by school at $t = -1$.

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



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

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



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

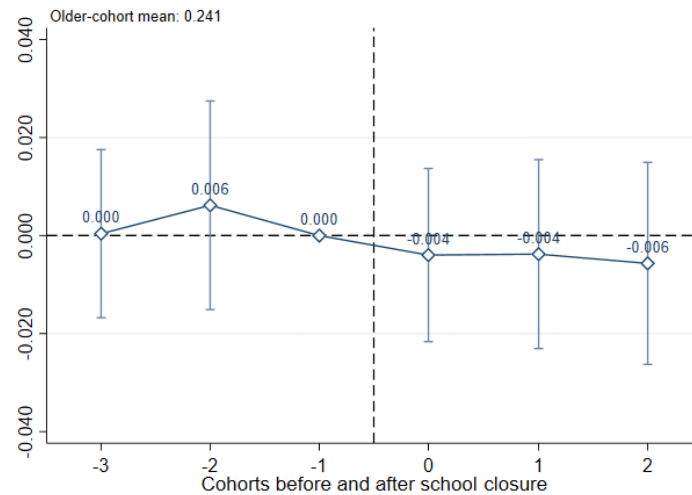
Table A.2: Receiving School Quality Change: Original and Move-In Students

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

Notes: The table presents the average test scores of students in receiving schools in two distinct time points: the year right after school closures ($t = 0$) and the year immediately preceding the closures ($t = -1$). These scores are presented separately for two groups of students: those who have been enrolled in the school for at least two years (original) and those who are new arrivals in the year (move-in). For example, students observed at time point -1 in column (1) are classified as original students if they are observed in both the $t = -2$ and $t = -1$ periods at the same receiving school. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

A.8 Long-Run Effects of School Closures on Four-Year College Enrollment

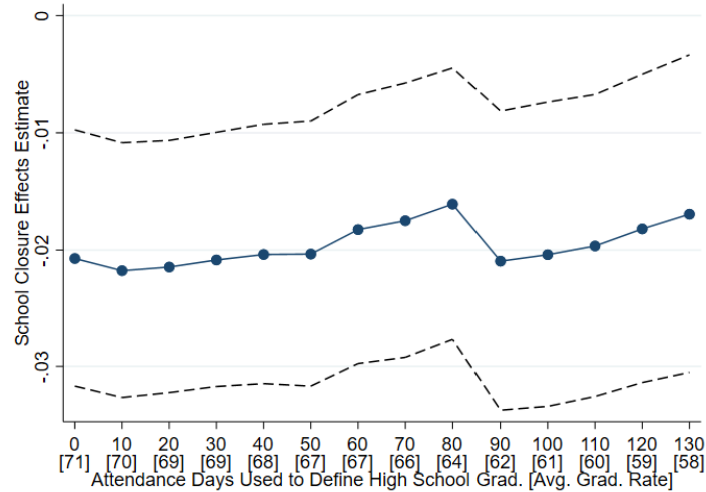
Fig. A.15. Estimates of School Closure Effects on Four-Year College Enrollment



Notes: The figure presents the coefficients, γ_c , and 95% confidence intervals from equation (4). These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts—those who graduated within three years prior to the closure and those enrolled at the time of the closure. The cohort that graduated one year before the closure ($c = -1$) is the omitted category. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standardized test scores and standardized absence rate are measured before the school closure. The older-cohort mean refers to the average value of the outcome variable for students in older cohorts ($c \in \{-3, -2, -1\}$) attending closed schools in the analysis sample. The analysis sample is balanced. Standard errors are clustered at the school level.

A.9 High School Graduation Proxy Across Different Definition Cutoffs

Fig. A.16. Estimates of School Closure Effects on High School Graduation Proxy Across Different Definition Cutoffs



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

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

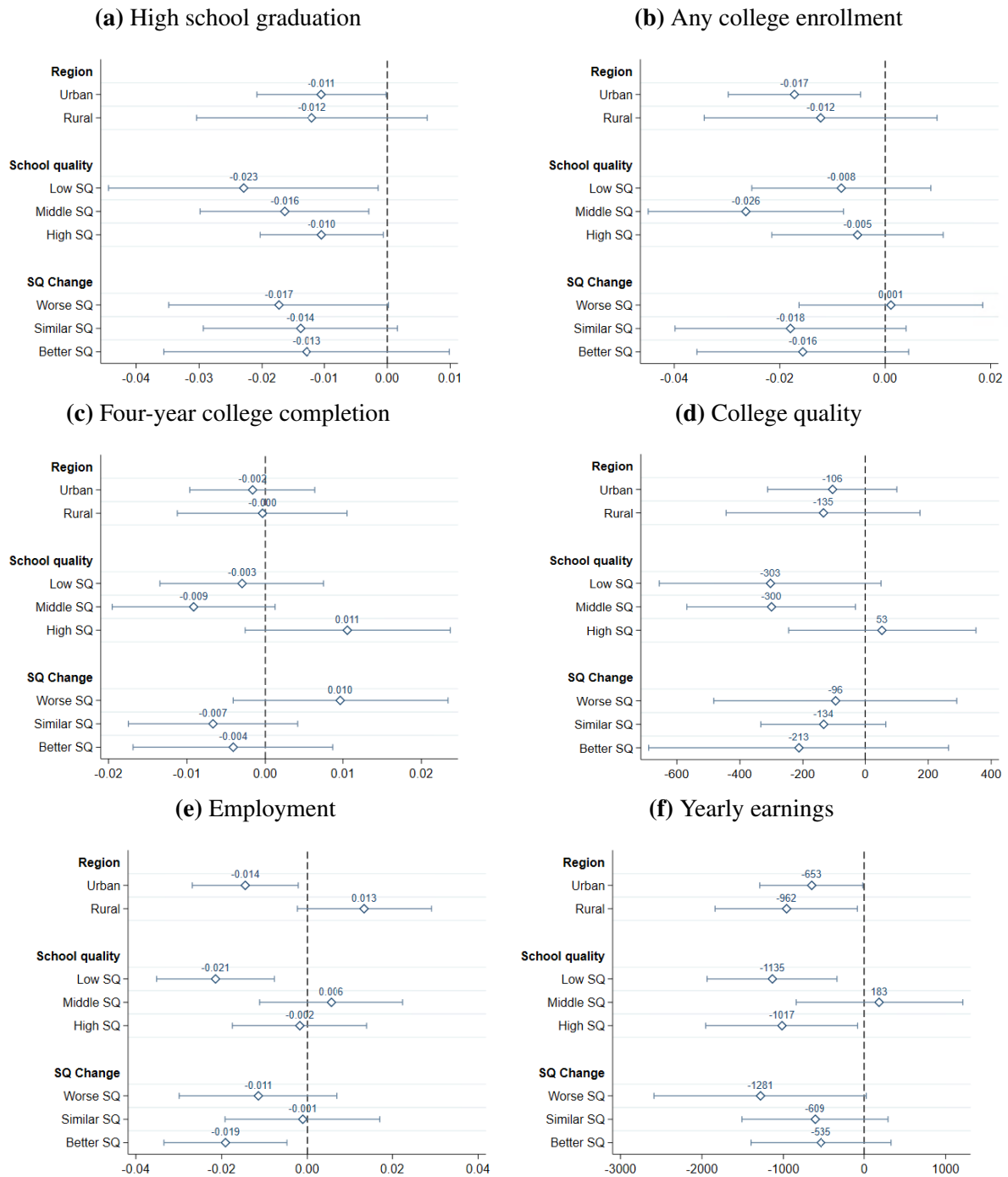
Table A.3: Long-Run Effects of School Closures on Educational and Labor Market Outcomes: Separate Estimates for Highest Grade and Others

Panel A: Educational Outcomes				
	(1) Graduate HS	(2) Enroll Any College	(3) UG Degree	(4) College Quality
Closed School	-0.021***	-0.024***	-0.003	-305**
× Younger Cohorts ($c = 1, 2$)	(0.005)	(0.006)	(0.004)	(118)
Closed School	-0.015**	0.003	0.002	-66
× Younger Cohorts ($c = 0$)	(0.006)	(0.005)	(0.004)	(98)
Observations	163,336	164,497	164,497	163,336
School FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Mean of the Older Cohort	0.666	0.495	0.141	21,136
Panel B: Labor Market Outcomes				
	(1) Employment	(2) Yearly Earnings		
Closed School	-0.014**	-1,110***		
× Younger Cohorts ($c=1,2$)	(0.006)	(284)		
Closed School	-0.005	-238		
× Younger Cohorts ($c = 0$)	(0.005)	(314)		
Observations	164,497	164,497		
School FE	X	X		
Matched group × Year FE	X	X		
Mean of the Older Cohort	0.524	19,739		

Notes: The table presents the coefficients, γ , and standard errors from equation (3). The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure, distinguishing between those in the highest grade ($c = 0$) and those in lower grades ($c \in \{1, 2\}$). The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standardized test scores and standardized absence rate are measured before the school closure. The mean of the older cohort refers to the average value of the outcome variable for students in older cohorts ($c \in \{-3, -2, -1\}$) attending closed schools in the analysis sample. “Graduate HS” and “UG Degree” refer to high school graduation and four-year college completion, respectively. Note that the dependent variables for high school graduation and college quality have fewer observations due to the exclusion of two closed schools from the analysis because of a potential data issue (see Section 3 for more details). Standard errors are clustered at the school level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

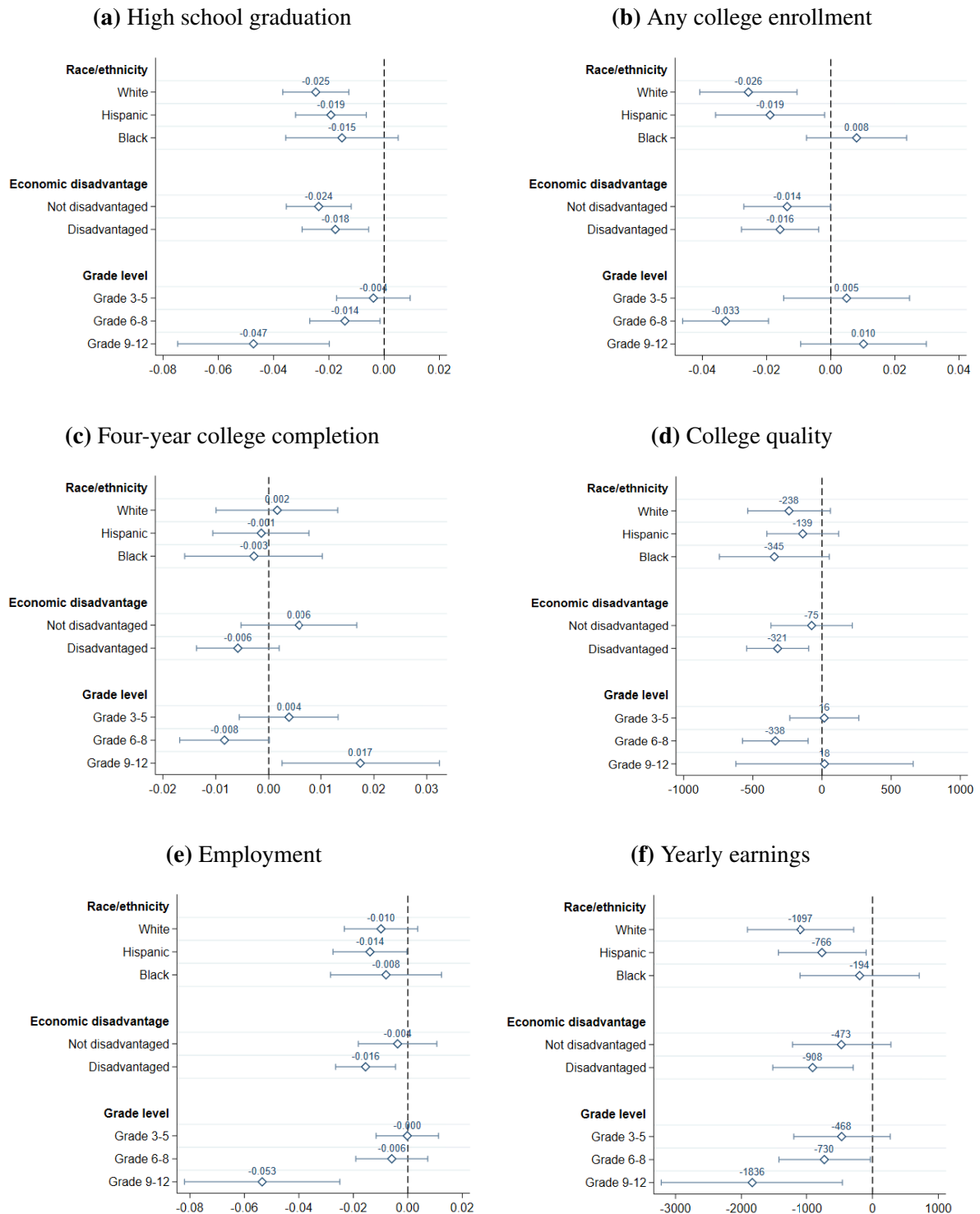
A.11 Long-Run Effects of School Closures: Heterogeneity

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



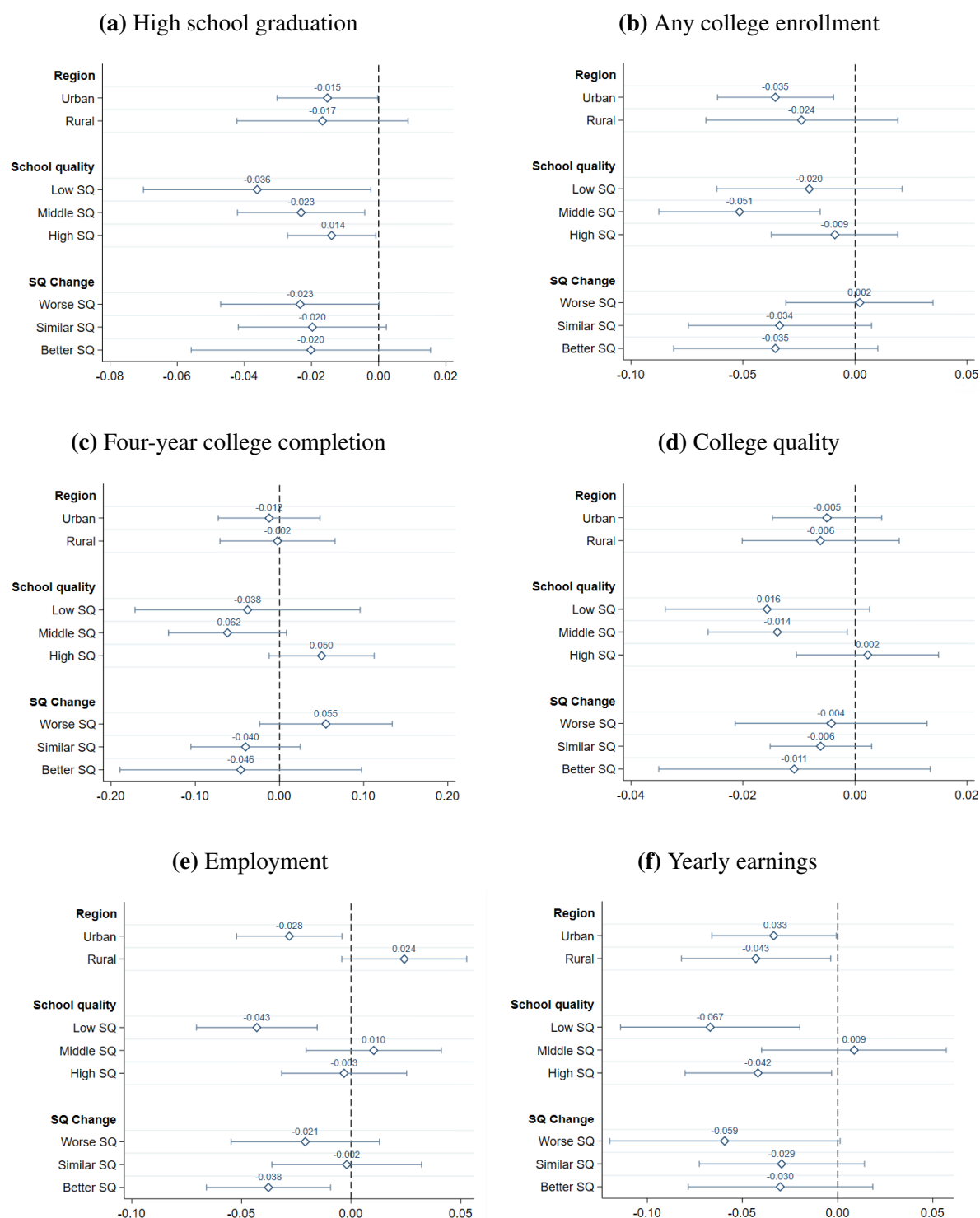
Notes: The figures present the coefficients, γ , and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the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

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



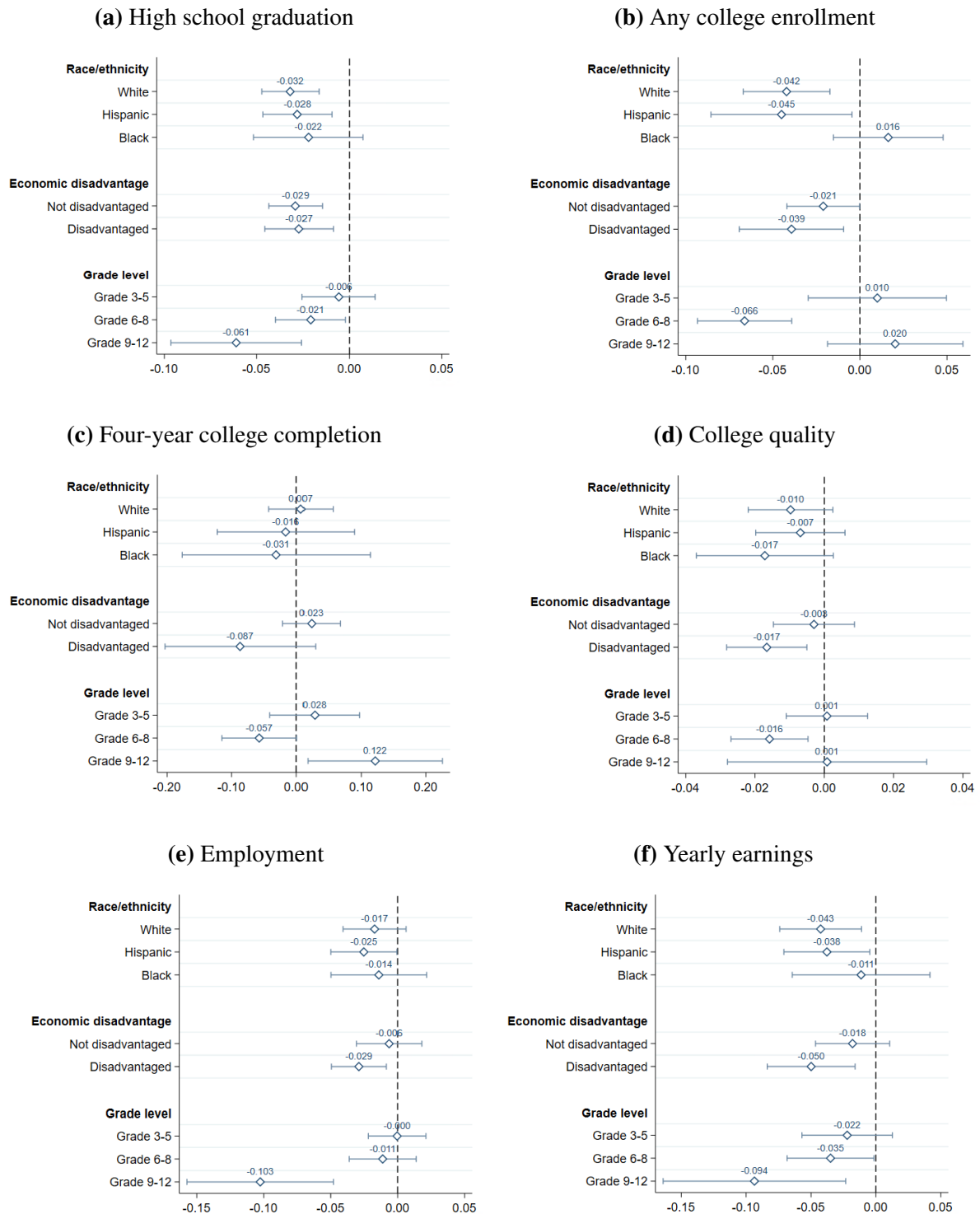
Notes: The figures present the coefficients, γ , and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the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

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



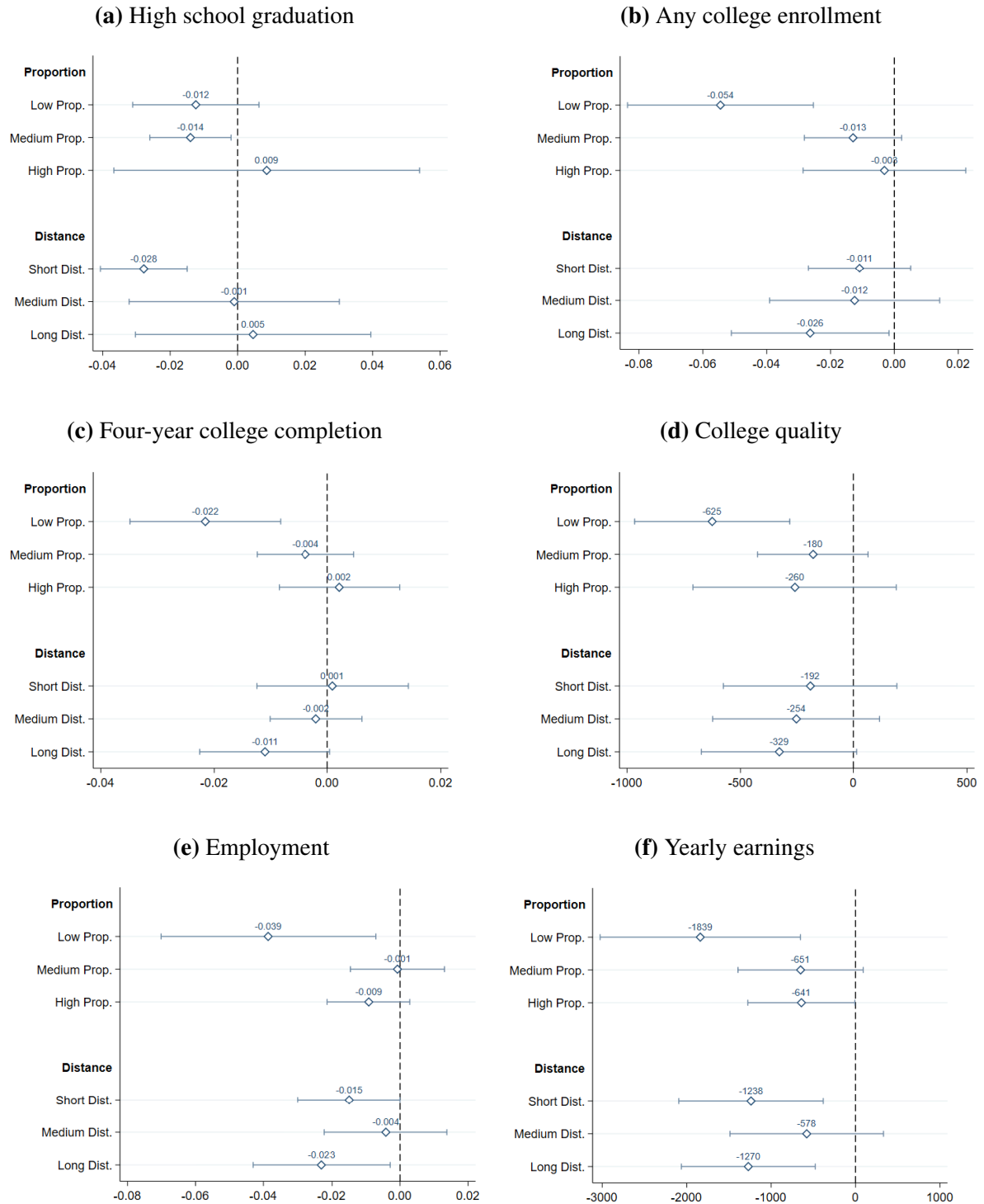
Notes: The figures present the coefficients, γ , and 95% confidence intervals from equation (3) for students belonging to the sub-group denoted on the y-axis after estimates are scaled relative to the outcome mean for each sub-group. These coefficients represent the interactions between the indicator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

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



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

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



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

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

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

B Sensitivity Analyses

B.1 Matching

I examine the sensitivity of my estimates to alternative ways of choosing matched control schools to closed schools. Appendix Figures B.1 and B.2 present coefficients and associated 95% confidence intervals from estimating equations (1) and (3) respectively, using following alternative matching strategies: (1) I add more variables (share of English as a second language status and share of special education status) when calculating distance metric for nearest-neighbor matching; (2, 3) I add enrollment and its changes when measuring the distance; (4, 5) I add test scores and those changes when measuring the distance; (6) I add enrollment and test scores and those changes when measuring the distance; (7) I drop distant matches, (8) I reverse order of matching since order matters in matching without replacement, and (9) I match on school characteristics of one year before the school closure. I provide a baseline estimate at the top of each sub-figure for comparison. The name of each alternative matching method is followed by the percentage of the matched control schools that are unchanged from the baseline model. For instance, 69 percent of matched control schools are changed after adding more variables (share of English as a second language, share of special education). Reassuringly, the results are generally robust across these alternative matching strategies, with most estimates falling within the 95% confidence intervals of the baseline estimates while control schools change 65 percent on average from the baseline control schools.^{B.1}

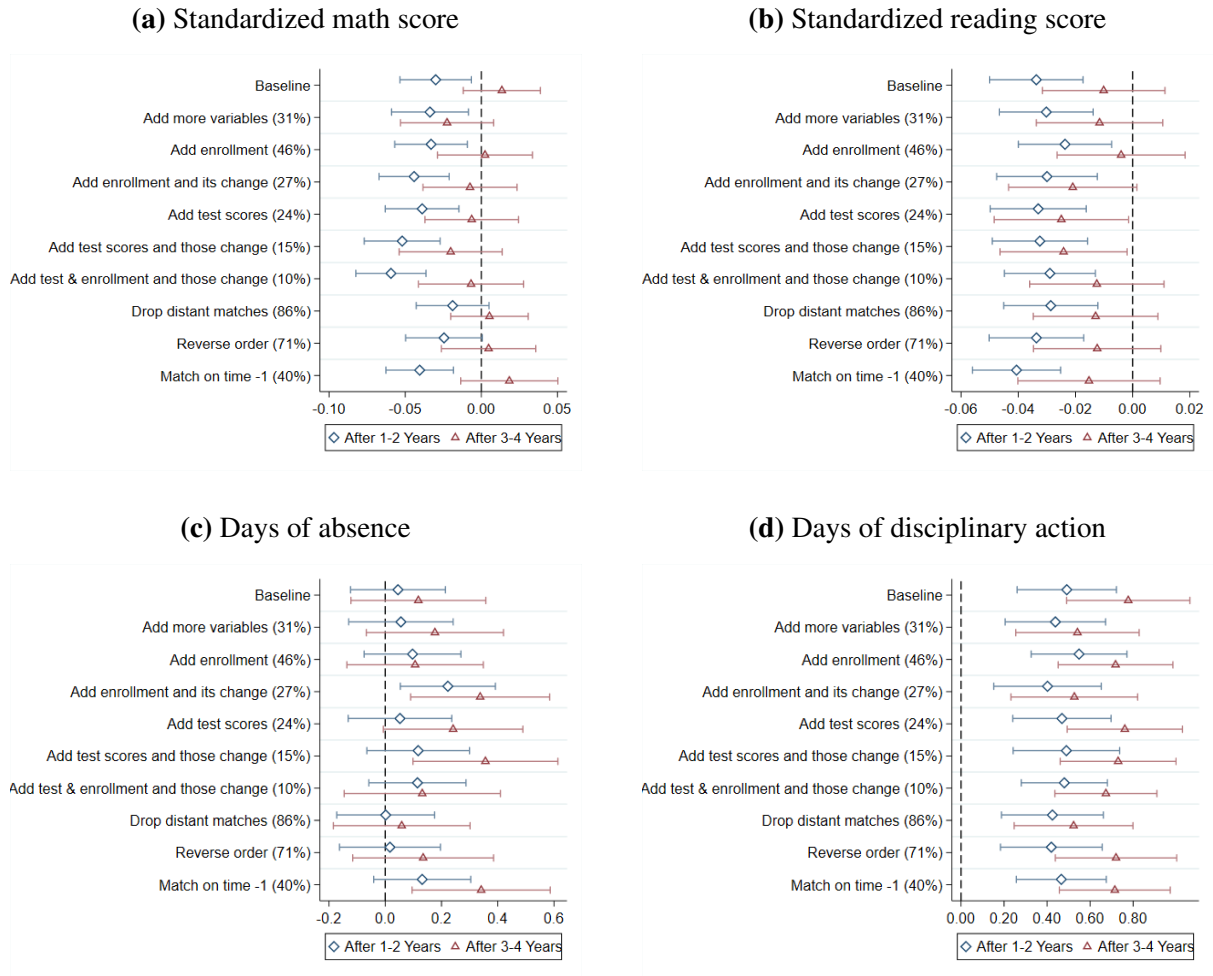
I further assess the robustness of the matching strategy using the synthetic difference-in-differences method proposed by Arkhangelsky et al. (2021). In the short-run analysis, I restrict the donor pool to students in the same year, school type, and school locale. Each student from a closed school is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

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

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

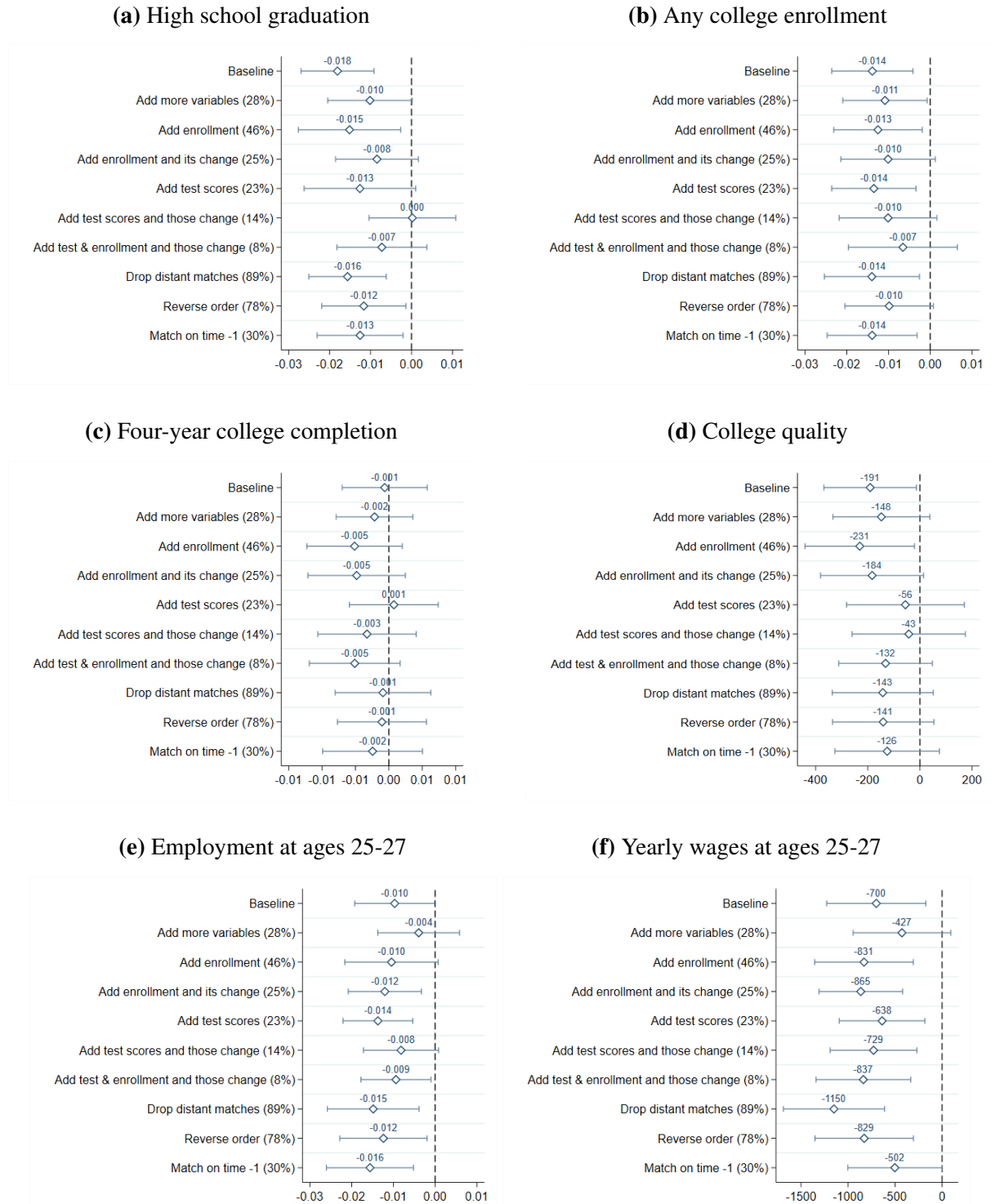
long-run outcomes decline among younger cohorts exposed to school closures. In other words, the estimated coefficients—obtained without additional discretion in matching criteria—align with the baseline results.

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



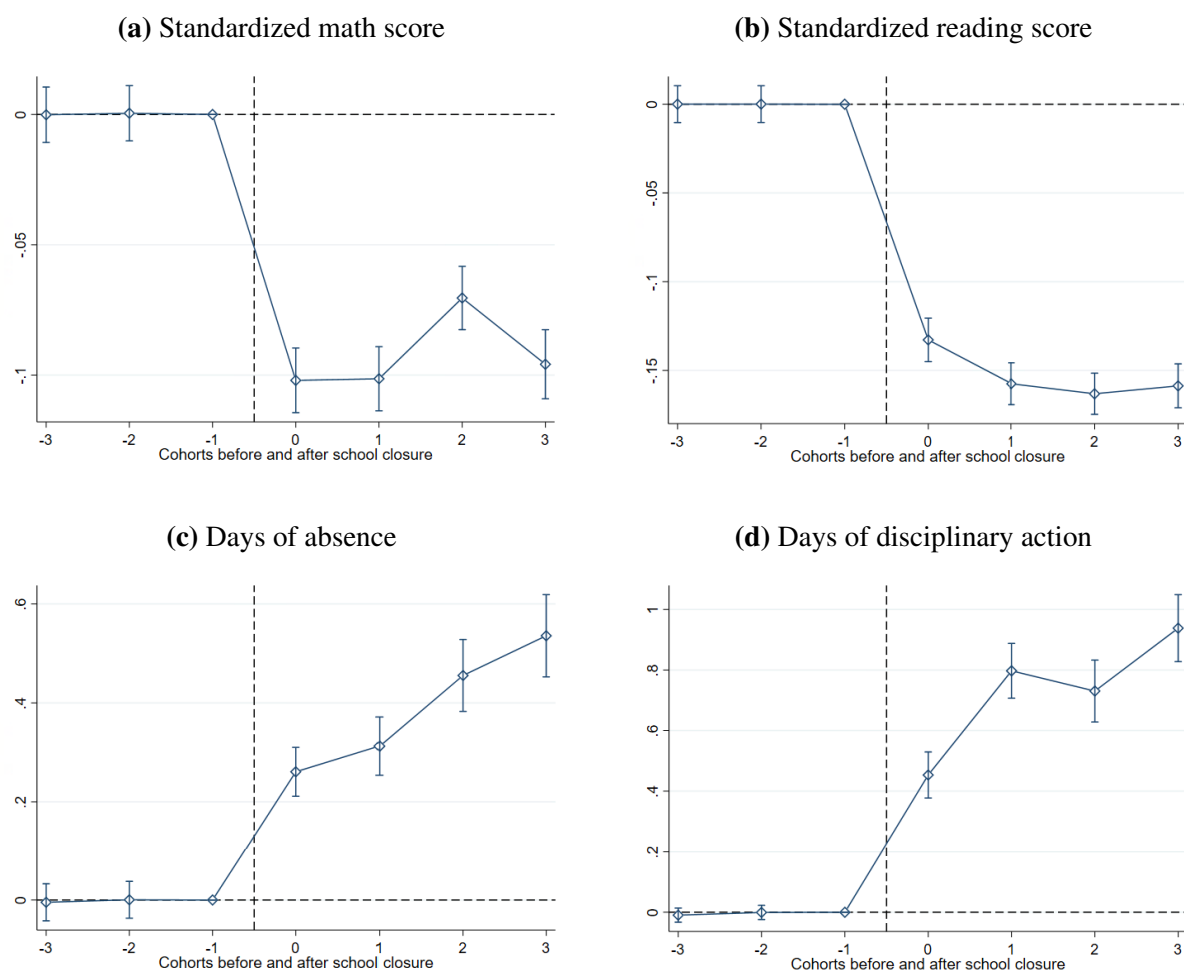
Notes: The figures present the coefficients, β , and 95% confidence intervals from equation (1) using control schools selected from the alternative matching strategies denoted on the y-axis. The baseline estimates are presented at the top of each sub-figure. The percentage in the parenthesis on the y-axis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

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



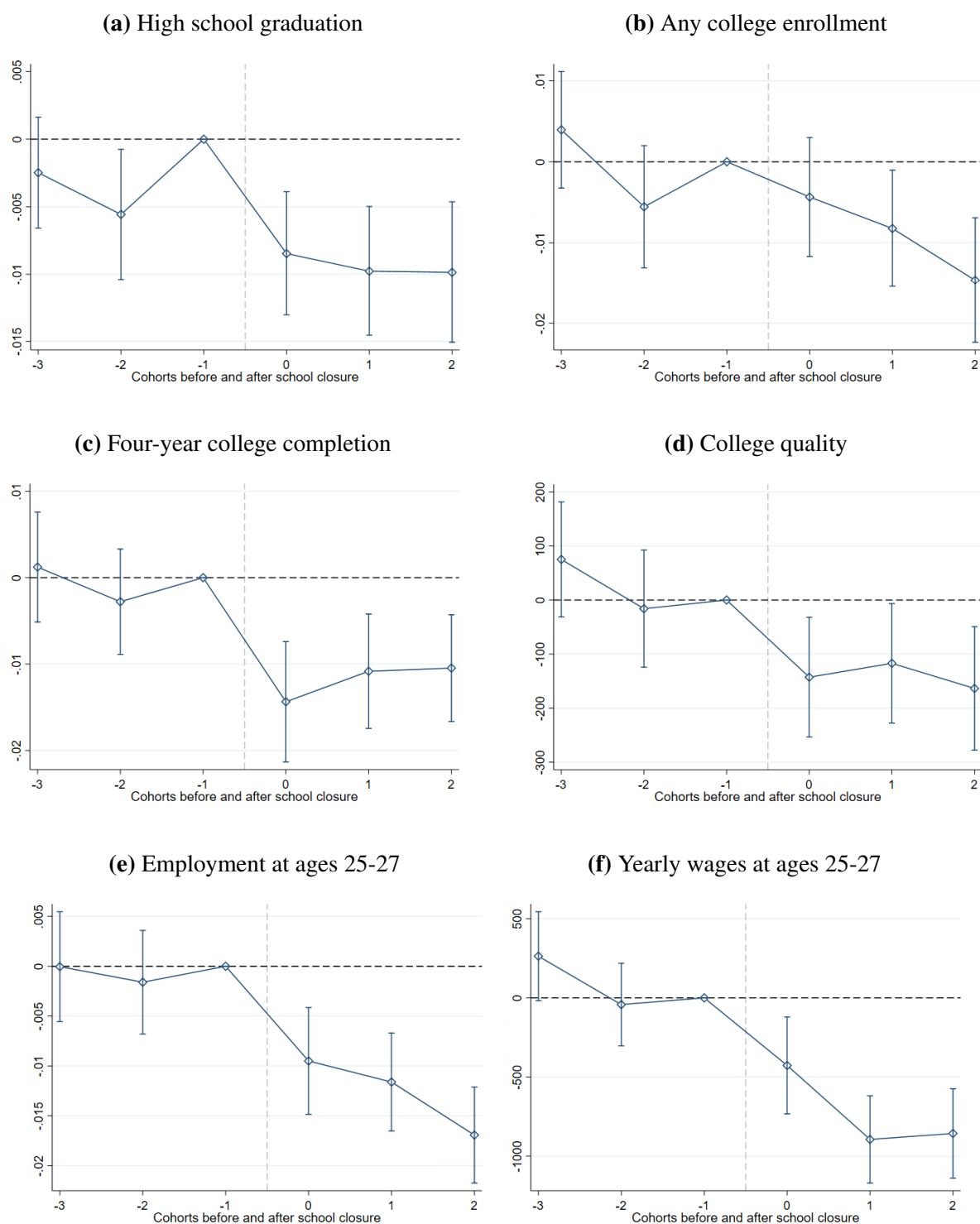
Notes: The figures present the coefficients, γ , and 95% confidence intervals from equation (3) using control schools selected from the alternative matching strategies denoted on the y-axis. The baseline estimates are presented at the top of each sub-figure. The percentage in parentheses on the y-axis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

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



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

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



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

B.2 Short-Run Analysis

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

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

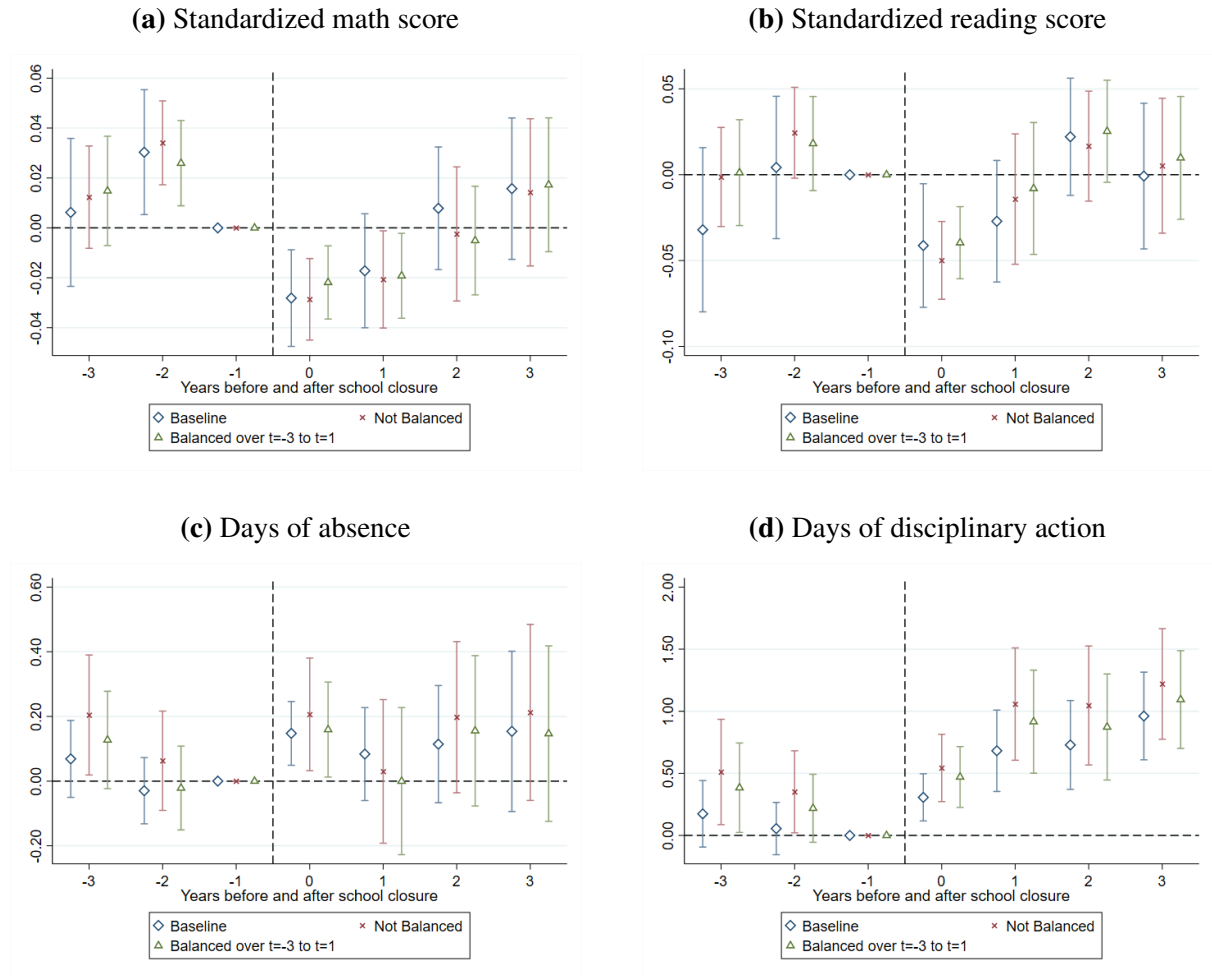
Importantly, this approach may also exclude some schools that were actually closed, as verified by school districts, simply because a share of students is reported as observed at the same address the following year. This suggests that while the exclusion strategy helps filter out non-physical closures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

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

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

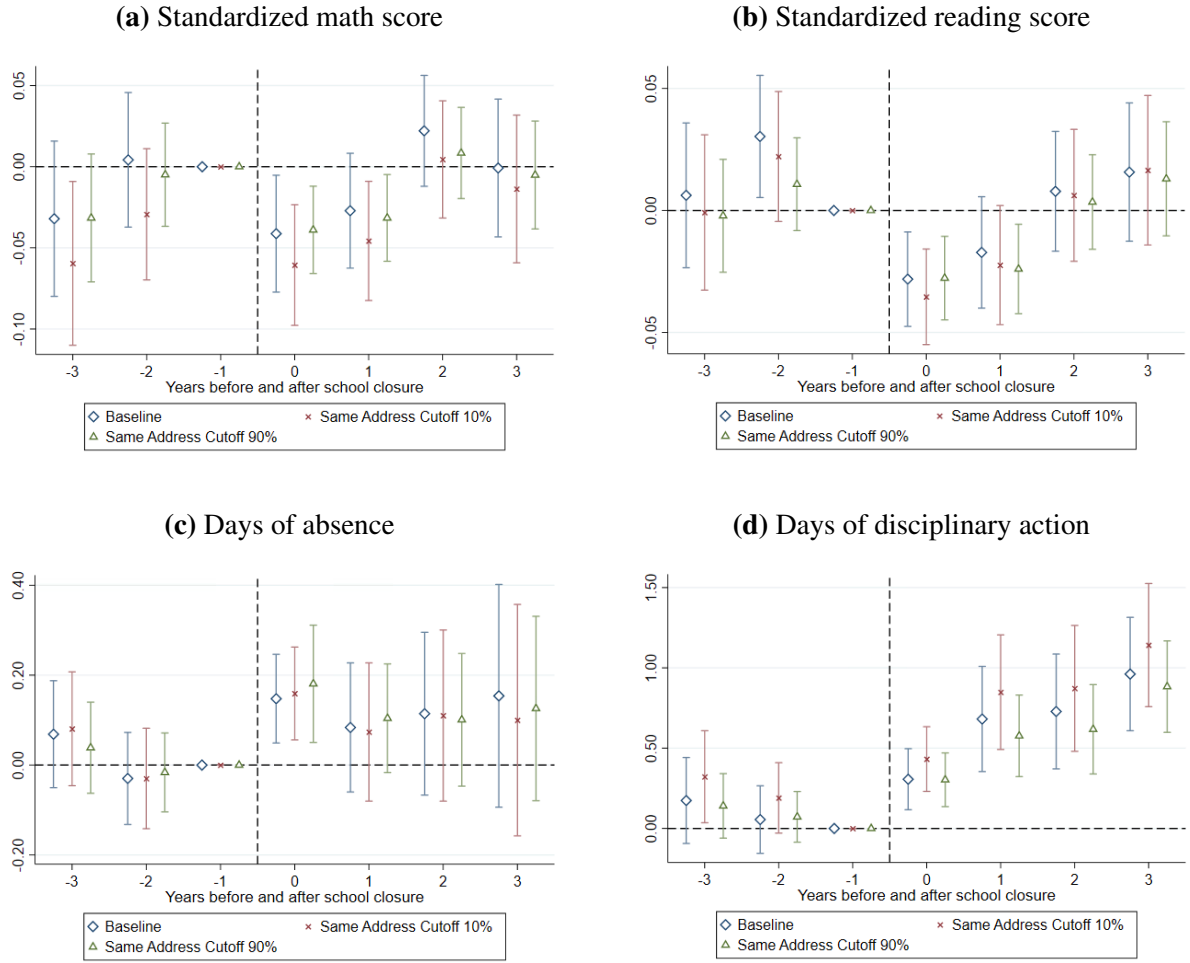
^{B.3} Sample sizes by outcome and balancing rule: test scores—(i) 12,846; (ii) 40,548; (iii) 31,557; attendance—(i) 58,065; (ii) 69,215; (iii) 81,320; disciplinary actions—(i) 49,300; (ii) 66,434; (iii) 57,293.

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



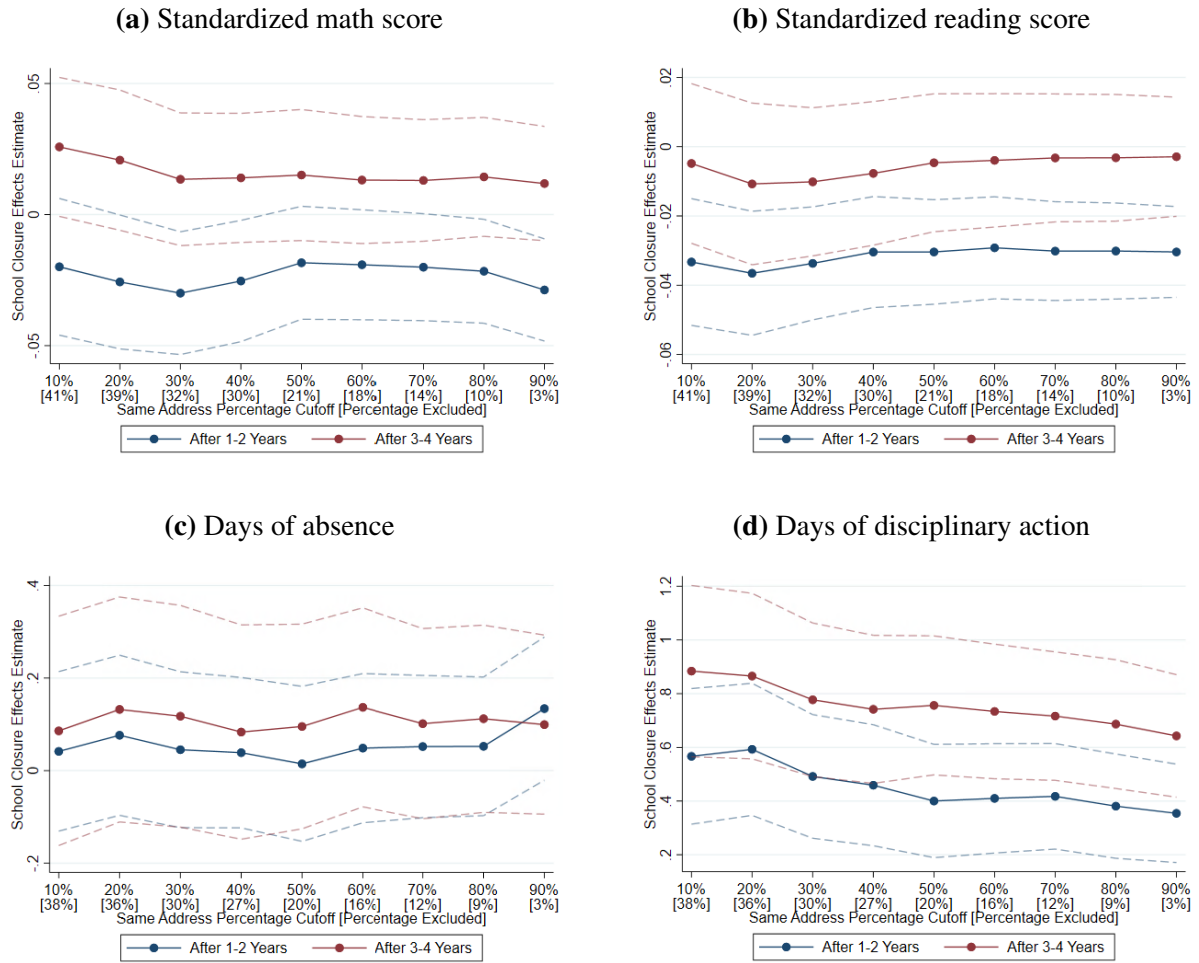
Notes: The figures overlay the coefficients, β_t , and 95% confidence intervals from equation (2), using three different sample constructions: (i) the baseline sample balanced over $t = -3$ to $t = 3$, (ii) an unbalanced sample, and (iii) a sample balanced over $t = -3$ to $t = 1$. These coefficients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

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



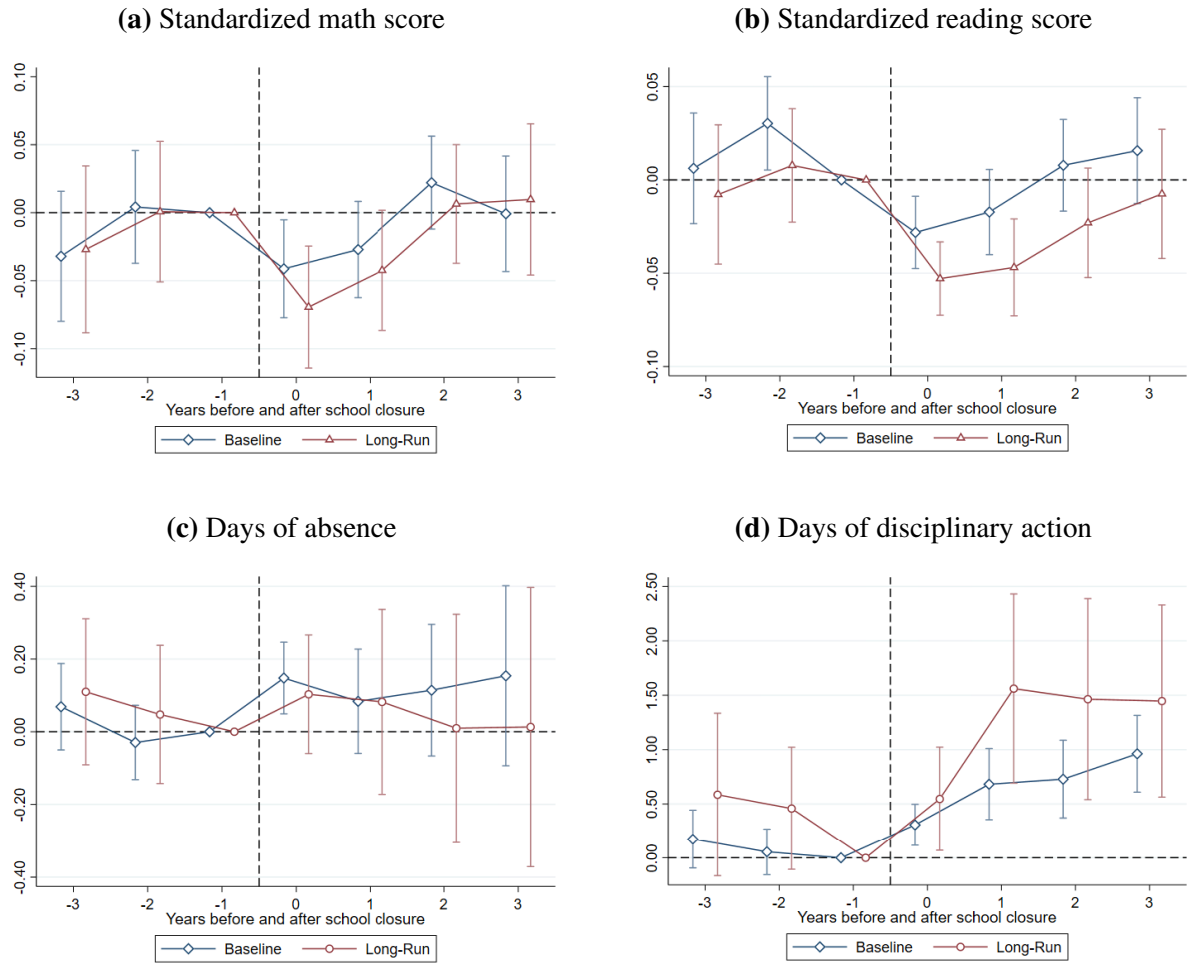
Notes: The figures overlay the coefficients, β_t , and 95% confidence intervals from equation (2), using either the baseline sample or alternative cutoffs based on the proportion of displaced students observed at the same address after closure. A cutoff of 10% (90%) means that schools where more than 10% (90%) of displaced students remain at the same address after closure are excluded from the analysis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) is the omitted category. The regression includes individual and match group-by-year fixed effects. The analysis sample is balanced. Standard errors are clustered by school at $t = -1$.

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



Notes: The figures overlay the coefficients, β , and 95% confidence intervals from equation (1) using a sample excluding closed schools where displaced students are observed at the same address after closure, applying different cutoffs. The x-axis shows the percentage cutoff for the proportion of displaced students, with parentheses indicating the percentage of students excluded from the analysis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the years around a school closure. The academic year before the closure ($t = -1$) is the omitted category. The regression includes individual and match group-by-year fixed effects. The analysis sample is balanced, except for the third and fourth years after the school closure, which may be missing for students in higher grades. Standard errors are clustered by school at $t = -1$.

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



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

Table B.1: Short-Run Effects of School Closures on Student Outcomes: Under Different Balancing Conventions

Panel A: Balanced over all periods ($t = -3$ to $t = 3$)				
	(1) Math	(2) Reading	(3) Days of Absence	(4) Days of Disciplinary Action
Closed School $\times$ After 1–2 Years	-0.025* (0.014)	-0.035*** (0.010)	0.103* (0.060)	0.417*** (0.092)
Closed School $\times$ After 3–4 Years	0.020 (0.013)	-0.000 (0.011)	0.122 (0.104)	0.767*** (0.131)
Observations	199,121	199,121	991,031	846,483
Individual FE	X	X	X	X
Matched group $\times$ Year FE	X	X	X	X
Pre-Closure Mean	0.119	0.146	6.033	1.459
Panel B: Not Balanced				
	(1) Math	(2) Reading	(3) Days of Absence	(4) Days of Disciplinary Action
Closed School $\times$ After 1–2 Years	-0.040*** (0.013)	-0.039*** (0.009)	0.035 (0.088)	0.516*** (0.115)
Closed School $\times$ After 3–4 Years	0.005 (0.015)	-0.011 (0.014)	0.121 (0.120)	0.848*** (0.151)
Observations	518,469	519,202	1,271,967	1,056,866
Individual FE	X	X	X	X
Matched group $\times$ Year FE	X	X	X	X
Pre-Closure Mean	-0.152	-0.124	7.107	2.389

Notes: The table presents the coefficients, β , and standard errors from equation (1), using two sample constructions: Panel A presents results from the sample balanced over all periods ($t = -3$ to $t = 3$), while Panel B uses an unbalanced sample. The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote years after school closure. The regression includes individual and match group-by-year fixed effects. The mean of pre-closure refers to the average value of the outcome variable at time $t = -1$ for displaced students in the analysis sample. Standard errors are clustered by school at $t = -1$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

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

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

Notes: The table presents the coefficients, β , and standard errors from equation (1), using long-run sample school closures. The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote years after school closure. The regression includes individual and match group-by-year fixed effects. The mean of pre-closure refers to the average value of the outcome variable at time $t = -1$ for displaced students in the analysis sample. The analysis sample is balanced, except for the third and fourth years after the school closure, which may be missing for students in higher grades. Standard errors are clustered by school at $t = -1$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

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

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

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

B.3 Long-Run Analysis

My long-run event study analysis uses a balanced panel encompassing three younger cohorts at the time of school closure and three older cohorts immediately preceding the observed school closure. It also incorporates detailed demographic and performance control variables. To assess the robustness of the long-run estimates, I explore alternative sample specifications and sets of controls. Appendix Figure B.9 presents results based on an unbalanced sample, shown alongside the baseline results for comparison. The overall patterns closely mirror those from the baseline analysis. While the estimated effects on educational outcomes are somewhat more pronounced, the effects on labor market outcomes are comparatively less pronounced. In addition, the estimation based on equation (3) includes all school closures, regardless of grade span. Appendix Table B.4 restricts the sample to closures with at least three cohorts, consistent with the baseline event study design. The findings remain consistent across specifications.

Appendix Figure B.10 depicts estimation results without controlling for performance variables (test scores and days of absence). General patterns observed remain largely consistent regardless of whether performance measures are controlled in the analysis, while results obtained without the inclusion of performance measures tend to exhibit instability and weaker effects. Moreover, Appendix Table B.5 presents estimation results from equation (3) in three levels of controls: i) without demographic and performance controls, ii) with demographic controls, and iii) with demographic and performance controls. While the results are broadly consistent across specifications, estimates that exclude performance controls are generally smaller than the baseline estimates. Particularly, the school quality and any college enrollment estimates are not statistically distinguishable from zero when performance measures are not included.

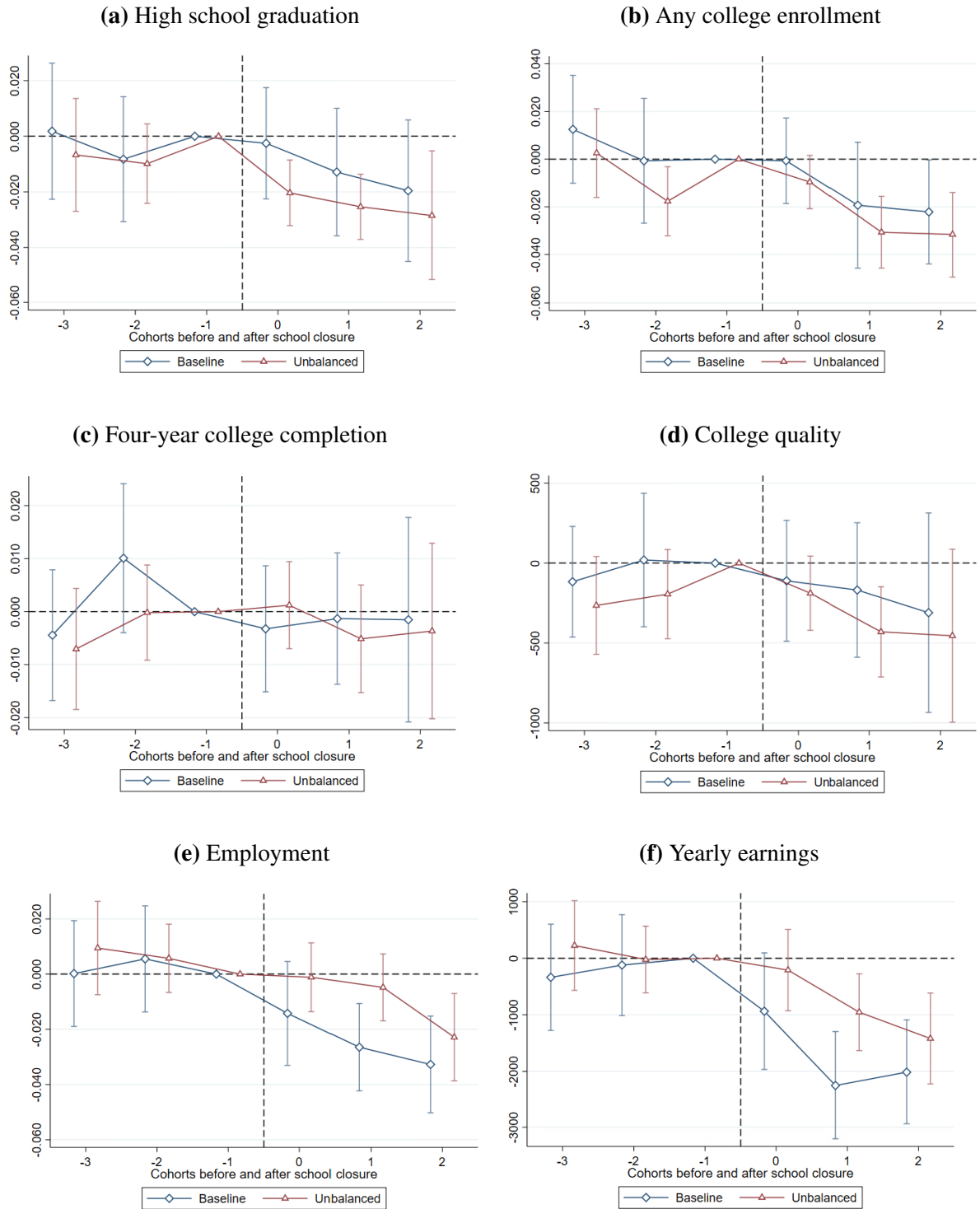
Appendix Figure B.11 presents estimation results using different cutoffs for excluding schools based on the proportion of displaced students observed at the same address after closure. In the baseline specification (Figure 6), I exclude closed schools if more than 30% of displaced students are observed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

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

Another approach to constructing the sample involves selecting the same school grade both in the year of school closure and in preceding years. For instance, in the example of the main text, I can create a comparable sample by choosing the third-highest grade from 1998 to 2003. Then, students in the third highest grade from 2000 to 2003 represent younger cohorts, while those from 1998 to 2000 represent older cohorts. However, this approach cannot utilize data from school closures in 1998 due to limitations in data availability. An alternative is to utilize the second-highest grade in the year of closure and for the three years prior. In the example presented

in the main text—where school **A**, serving grades 1–5, closed at the end of the 2000 school year—this corresponds to using fourth-grade students from 1997 to 2000. Then, fourth-grade students from 1997 to 1998 represent younger cohorts, and students from 1999 to 2000 represent older cohorts. As illustrated in the Appendix Figure [B.13](#), the outcomes using this alternative approach also find similar negative impacts of school closures on displaced students.

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



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

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

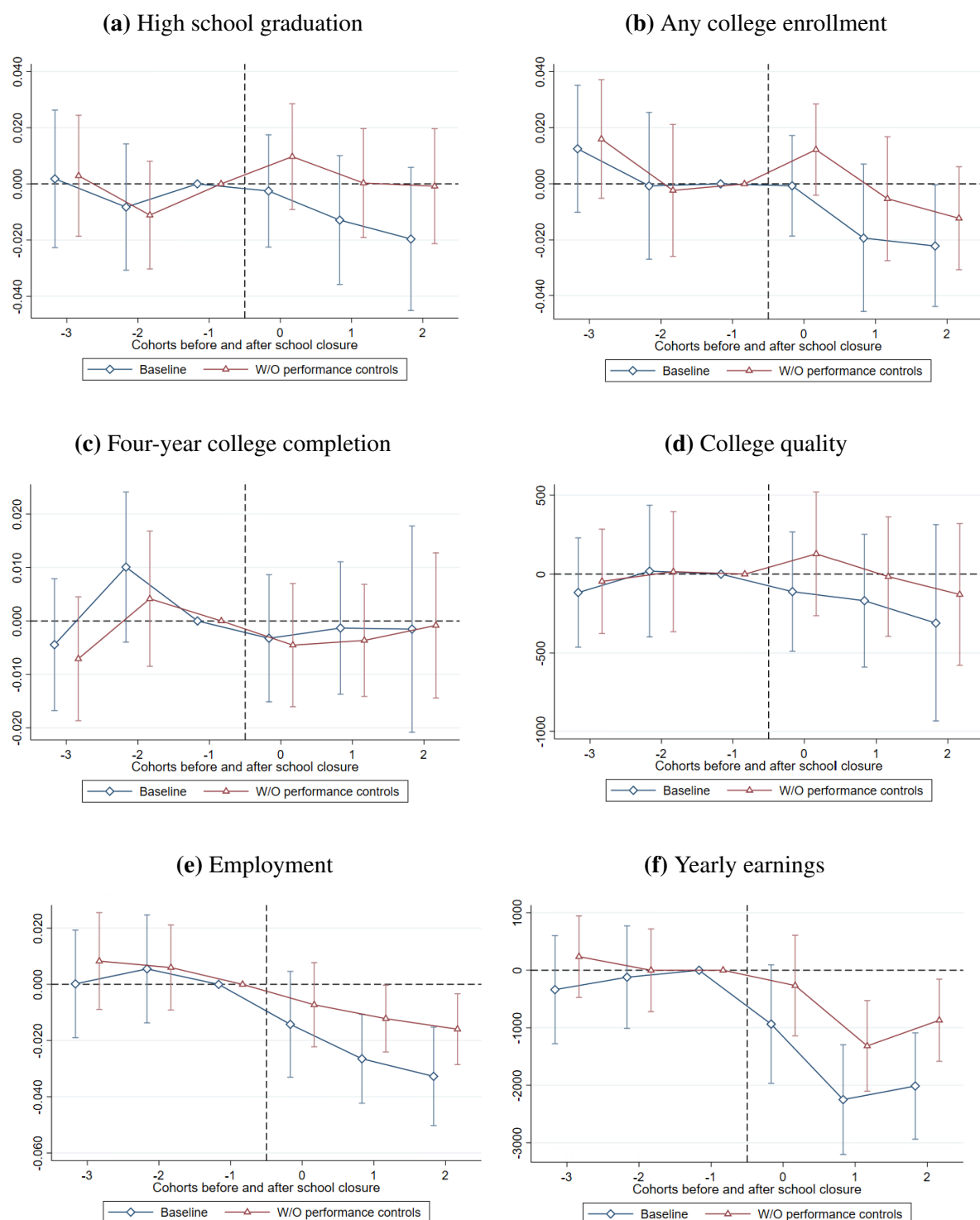
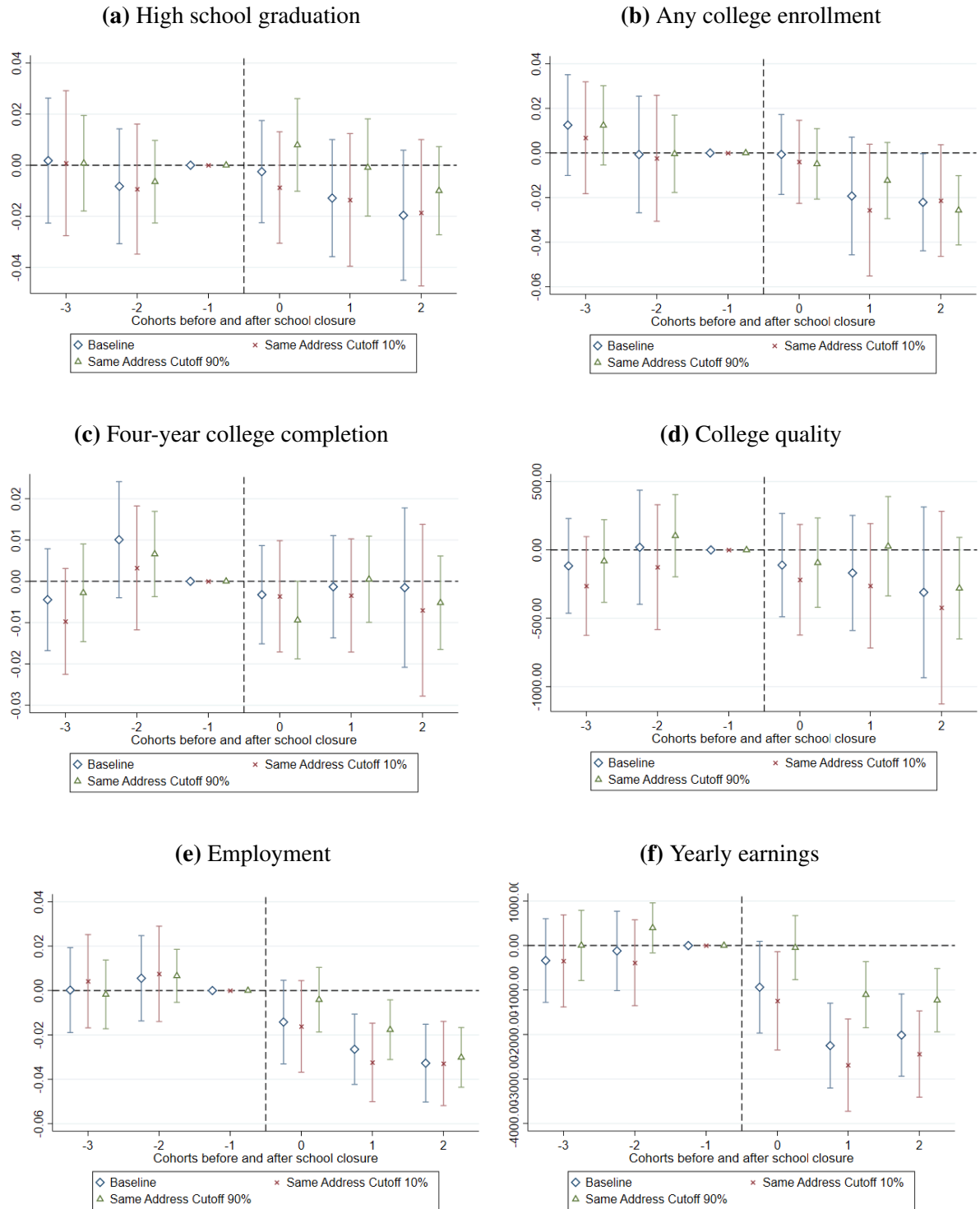
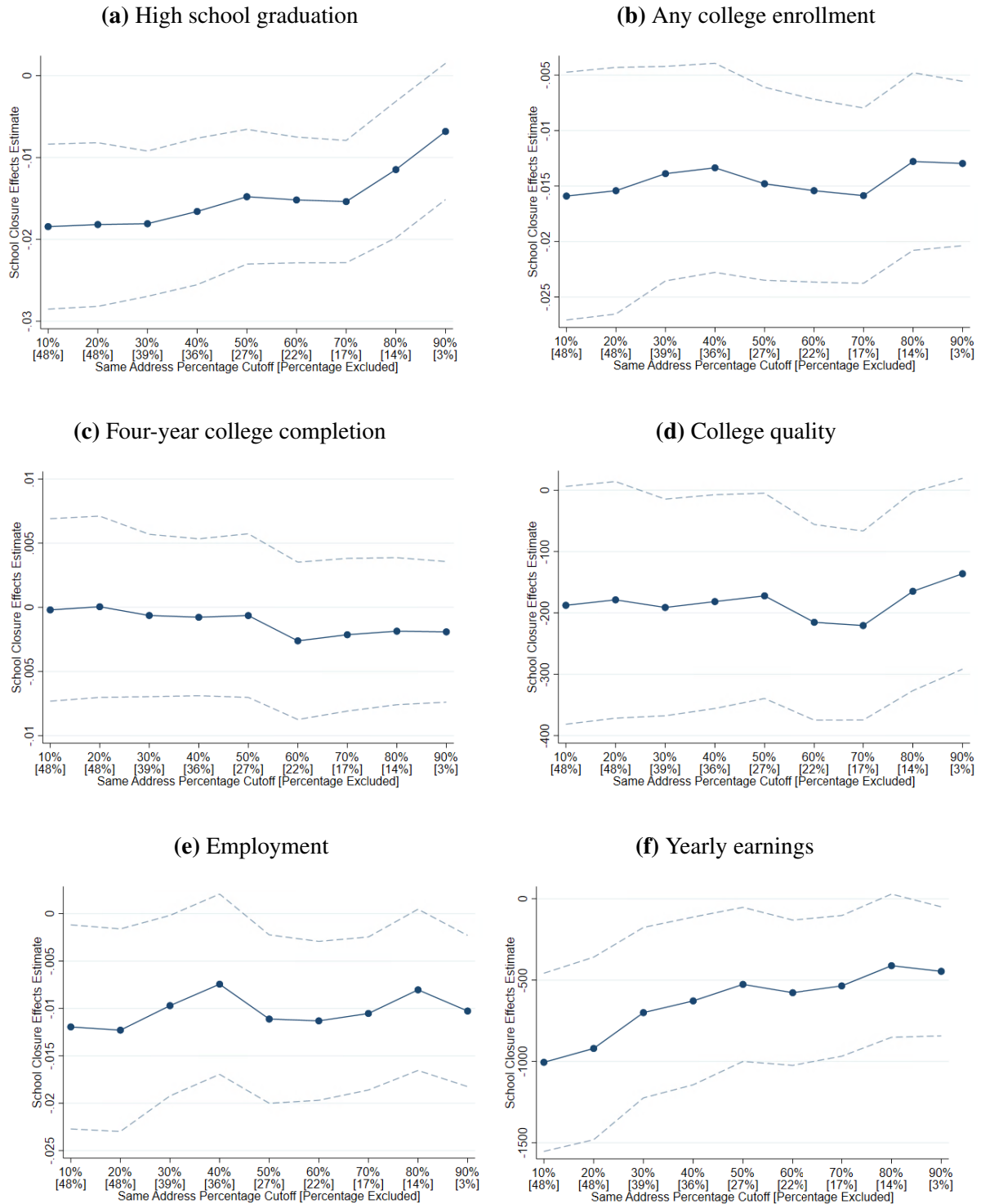


Fig. B.11. Long-Run Effects of School Closures on Educational and Labor Market Outcomes: Excluding Same Address School Openings with Different Cutoffs (1)



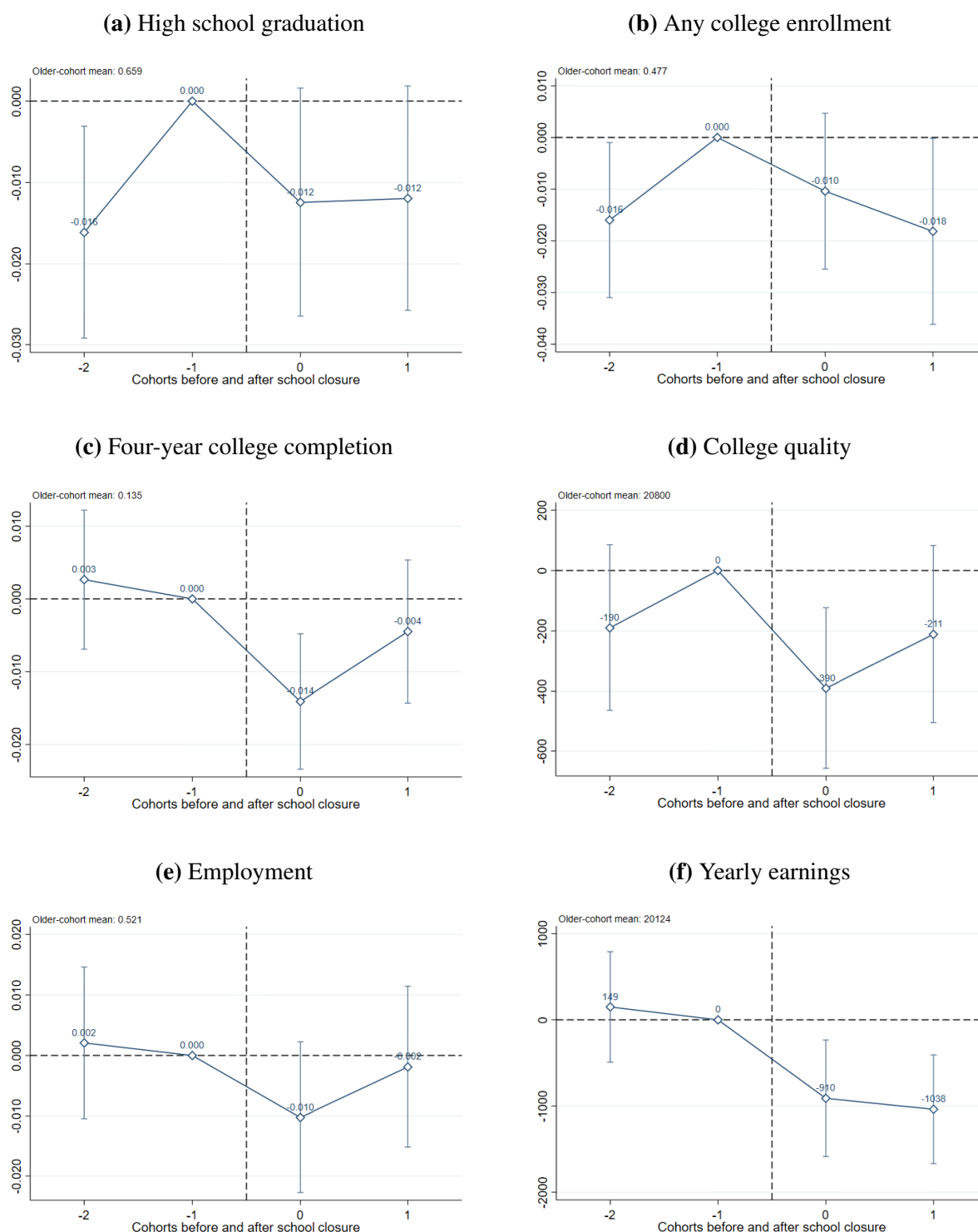
Notes: The figures overlay the coefficients, γ_c , and 95% confidence intervals from equation (4), using either the baseline sample or alternative cutoffs based on the proportion of displaced students observed at the same address after closure. A cutoff of 10% (90%) means that schools where more than 10% (90%) of displaced students remain at the same address after closure are excluded from the analysis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts—those who graduated within three years prior to the closure and those enrolled at the time of the closure. The cohort that graduated one year before the closure is the omitted category. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. The analysis sample is balanced. Standard errors are clustered at the school level.

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



Notes: The figures overlay the coefficients, γ , and standard errors from equation (3) using a sample excluding closed schools where displaced students are observed at the same address after closure, applying different cutoffs. The x-axis shows the percentage cutoff for the proportion of displaced students, with parentheses indicating the percentage of students excluded from the analysis. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts—those who graduated within three years prior to the closure and those enrolled at the time of the closure. The cohort that graduated one year before the closure is the omitted category. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standard errors are clustered at the school level.

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



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

Table B.4: Long-Run Effects of School Closures on Educational and Labor Market Outcomes: Using Balanced Sample

Panel A: Educational Outcomes				
	(1) Graduate HS	(2) Enroll Any College	(3) UG Degree	(4) College Quality
Closed School × Younger Cohorts	-0.009 (0.007)	-0.018** (0.008)	-0.004 (0.004)	-154 (131)
Observations	105,061	106,222	106,222	105,061
School FE	X	X	X	X
Matched group × Year FE	X	X	X	X
Mean of the Older Cohort	0.666	0.486	0.142	21,177
Panel B: Labor Market Outcomes				
	(1) Employment	(2) Yearly Earnings		
Closed School × Younger Cohorts	-0.026*** (0.007)	-1,553*** (331)		
Observations	106,222	106,222		
School FE	X	X		
Matched group × Year FE	X	X		
Mean of the Older Cohort	0.518	19,635		

Notes: The table presents the coefficients, γ , and standard errors from equation (3), using a balanced sample (i.e., schools with at least three grades). The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standardized test scores and standardized absence rate are measured before the school closure. The mean of the older cohort refers to the average value of the outcome variable for students in older cohorts ($c \in \{-3, -2, -1\}$) attending closed schools in the analysis sample. “Graduate HS” and “UG Degree” refer to high school graduation and four-year college completion, respectively. Note that the dependent variables for high school graduation and college quality have fewer observations due to the exclusion of two closed schools from the analysis because of a potential data issue (see Section 3 for more details). Standard errors are clustered at the school level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table B.5: Long-Run Effects of School Closures on Educational and Labor Market Outcomes: Alternative Sets of Controls

	(1) No Control	(2) Demographic Control	(3) Full Control
High school graduation			
Closed School × Younger Cohorts	-0.011** (0.005)	-0.011** (0.005)	-0.018*** (0.005)
Any college enrollment			
Closed School × Younger Cohorts	-0.009* (0.005)	-0.008 (0.005)	-0.014*** (0.005)
Four-year college completion			
Closed School × Younger Cohorts	0.003 (0.003)	0.004 (0.003)	-0.001 (0.003)
College quality			
Closed School × Younger Cohorts	-35 (99)	0 (89)	-191** (90)
Employment at ages 25-27			
Closed School × Younger Cohorts	-0.011** (0.005)	-0.007 (0.005)	-0.010** (0.005)
Yearly wages at ages 25-27			
Closed School × Younger Cohorts	-596** (257)	-500* (257)	-700*** (267)
Non-zero yearly wages at ages 25-27			
Closed School × Younger Cohorts	-444* (265)	-312 (255)	-458* (266)
Potential full-time yearly earnings at ages 25-27			
Closed School × Younger Cohorts	-676** (280)	-590** (271)	-705** (280)
School FE	X	X	X
Matched group × Year FE	X	X	X

Notes: Each row of the table presents the coefficients, γ , and standard errors from equation (3) with the denoted dependent variable. The coefficient represents the interactions between the indicator that denotes closed schools and the indicators that denote cohorts in the school at the time of closure. In all columns, the regression includes school and match group-by-cohort fixed effects. Column (1) does not include demographic and performance variables. Column (2) includes individual-level demographic control variables such as race/ethnicity, sex, ESL status, and special education status. Column (3) includes performance measures such as standardized test scores and standardized absence rate, as well as demographic variables in Column (2). Potential full-time yearly earnings are calculated following Sorkin (2018). See Appendix Section B.4 for more details. Standard errors are clustered at the school level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

B.4 Out-of-Texas Attrition

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

I assess the first layer by examining attrition rates after closure between students from closed and control schools. Appendix Figure A.4(c) shows the share of students in the long-run analysis sample—disaggregated by younger and older cohorts—who appear in the data each year after school closure, and the trends are similar across the groups. In Appendix Figure A.4 (d), I plot estimated coefficients and associated 95% confidence intervals from equation (2), in which the dependent variable is an indicator for being observed in the data. I compare the attrition rates of students from closed and control schools in younger and older cohorts separately. The results show that there is no significant difference in attrition trends between students from closed and control schools. Moreover, any observed difference in attrition rate between closed and control schools is at most 0.7 percentage points.^{B.4} This finding provides reassurance that sample attrition right after closure was not a major concern, as students did not differentially leave in the imminent closure.

To address the second, I exploit National Student Clearinghouse (NSC) data, which covers 98 percent of higher education enrollment in the United States. As discussed in Section 3, the available data on higher education enrollment out of Texas only begins in 2008, which does not fully cover the sample. Therefore, it is not used in the baseline analysis. However, it is informative to examine whether out-of-state enrollment was affected by school closures. Using an indicator for out-of-state enrollment as the dependent variable, I estimate equation (3) and present the results in Appendix Table B.6. The estimates show that younger cohorts from closed schools are 0.2 percentage points less likely to enroll in college out-of-state relative to students from matched control schools, although it is not statistically significant. This finding alleviates concerns that the baseline estimates for post-secondary education outcomes overestimate the effects of school closures due to out-of-state enrollment.

In the final layer of analysis, I present multiple pieces of evidence to support the conclusion that attrition to the labor market outside Texas does not alter the main findings. Firstly, previous research has shown that Texas has a relatively low out-migration rate of young workers, indicating that the effects of school closures on labor market outcomes within Texas are likely to be a robust estimate (Foote and Stange 2022). Secondly, when excluding individuals with no earnings in Texas, I obtain similar effects on earnings as in the baseline analysis (Appendix Figure B.14 and Table B.5). Specifically, I look into two different measures of earnings following Miller (2023) and Sorkin (2018): non-zero yearly earnings and potential full-time yearly earnings. The non-zero yearly earnings sample includes only individuals with positive earnings. The potential full-time yearly earnings measure estimates full-time earnings based on the earnings structure.^{B.5}

^{B.4} To see the potential impact of the attrition, I estimate Lee (2009) bounds assuming differential attrition in response to a school closure of 0.7 percentage points. The estimated bounds are presented in Panel A of Appendix Table B.7. While these Lee bounds cover a range of estimates, the bounds exclude zero for all the outcomes except four-year college completion.

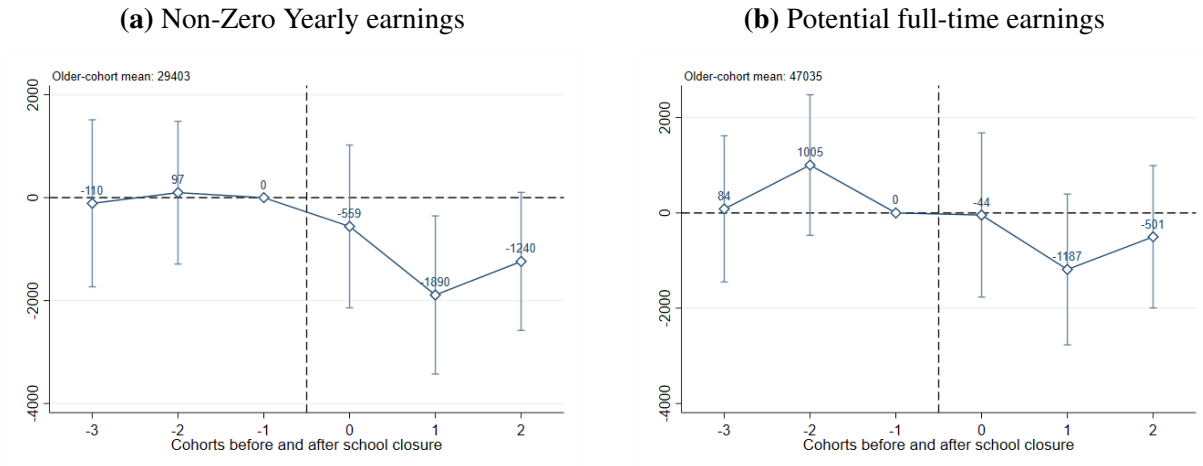
^{B.5} Specifically, earnings in quarter t are classified into one of two mutually exclusive categories: (i) *full-quarter*, if earnings from the employer are recorded in quarters $t - 1$, t , and $t + 1$, or (ii) *continuous*, if earnings are recorded in either $t - 1$ and t , or t and $t + 1$. Here, quarterly earnings need to be at least \$3,800 following

Thirdly, using a school quality measure based on their highest education level and institution, I find consistent results showing a decrease in expected earnings among the sample of individuals. Lastly, I perform a bounding exercise with the non-zero earning samples, attributing all the decrease in employment rates after school closure to attrition to the labor market outside Texas (Lee 2009). The Lee bounds, presented in Panel B of Appendix Table B.7, are mostly in the negative range.^{B.6} The evidence suggests that even under the extreme assumption, the main implications remain unchanged.

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

^{B.6} Although the lower bound is a positive number, it is small and insignificant. Furthermore, regressing employment on standardized college quality using the same setting as in equation (3) gives an estimate of 0.1 (0.002), indicating that a one standard deviation increase in college quality is associated with a 10 percentage point increase in employment. Based on this, the lower bound is less plausible since the lower bound assumes that the highest earners from closed schools are not employed in my sample.

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



Notes: The figures present the coefficients, γ_c , and 95% confidence intervals from equation (4). Panel (a) includes samples with positive earnings. Panel (b) includes samples with potentially full-time earnings, following Sorkin (2018). See Appendix Section B.4 for more details. These coefficients represent the interactions between the indicator that denotes closed schools and the indicators that denote each of the cohorts—those who graduated within three years prior to the closure and those enrolled at the time of the closure. The cohort that graduated one year before the closure ($c = -1$) is the omitted category. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standardized test scores and standardized absence rate are measured before the school closure. The analysis sample is balanced. The older-cohort mean refers to the average value of the outcome variable for students in older cohorts ($c \in \{-3, -2, -1\}$) attending closed schools in the analysis sample. Standard errors are clustered at the school level.

Table B.6: Long-Run Effects of School Closures on Out-of-State Post-Secondary Education Enrollment

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

Notes: The table presents the coefficient, γ , and standard errors from equation (3). The coefficient represents the interaction between the indicator that denotes closed schools and the indicator that denotes cohorts in the school at the time of closure. The regression includes school and match group-by-cohort fixed effects, as well as school-specific individual-level controls for race/ethnicity, sex, ESL status, special education status, standardized test scores, and standardized absence rate. Standardized test scores and standardized absence rate are measured before the school closure. The mean of the older cohort refers to the average value of the outcome variable for students in older cohorts ($c \in \{-3, -2, -1\}$) attending closed schools in the analysis sample. Standard errors are clustered at the school level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table B.7: Long-Run Effects of School Closures on Educational and Labor Market Outcomes: Lower and Upper Bounds on the Estimated Effects

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

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

C Comparison with Previous Papers

C.1 School Closure Impacts on Displaced Students

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

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

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

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

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

negative behavioral impacts are larger for students who move to better-performing schools and travel longer distances—both of them are consistent with my findings. Larsen (2020) is one of the few studies extending the analysis to longer-term educational outcomes, including high school graduation and college enrollment, while their sample is limited to high school students. The paper finds a 7.5 percentage point decline in high school graduation rates and a 5.1 percentage point drop in college enrollment. While their estimate of the impact on high school graduation is similar to mine for 9th–12th grade students, the college enrollment estimates are significantly larger.

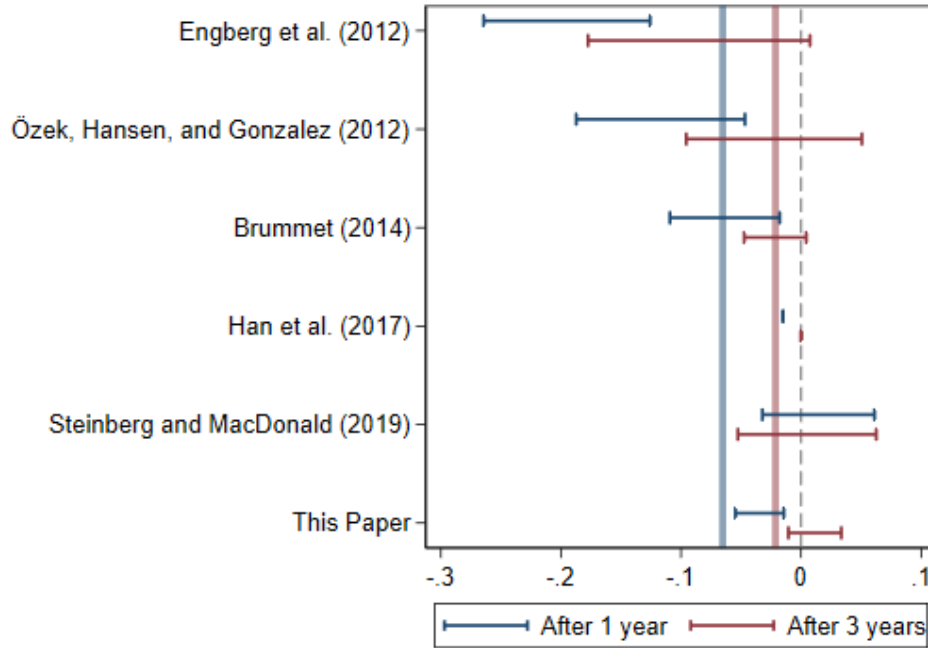
Some studies examine school closures in different contexts. Performance-based school closures have been implemented by state education agencies targeting charter schools, as well as by urban school districts—such as New York City—within the traditional public school system. Carlson and Lavertu (2016)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

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

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

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

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



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

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

The impact of school closure on students is significant, with long-lasting consequences for their human capital accumulation and labor market performance. To better understand the magnitude of these effects, it is helpful to compare my long-run estimates with existing research on the long-run effects of school inputs and intervention/disruption. Specifically, my findings suggest that experiencing school closure reduces college enrollment by 1.4 percentage points. For instance, studies by Chetty et al. (2011) and Dynarski, Hyman, and Schanzenbach (2013) find that a 30 percent reduction in class size in Project STAR for two years led to a boost in college enrollment of 1.8 and 2.7 percentage points, respectively. Meanwhile, Chetty, Friedman, and Rockoff (2014) find that a one standard deviation increase in teacher value added in one grade increases college enrollment by 0.82 percentage points. Thus, my estimates suggest that experiencing school closure is equivalent to a 16 to 23 percent increase in class size for two years or a one standard deviation decrease in teacher quality for 1.7 years in terms of its impact on college enrollment.

Regarding labor market outcomes, Chetty et al. (2011) find that a one standard deviation increase in class quality within schools, which incorporates peer quality, teacher quality, and random class-level shock, increases earnings by 9.6 percent at age 27. Similarly, a one standard deviation improvement in teacher value-added for one year is associated with a 1.34 percent increase in earnings at age 28 (Chetty, Friedman, and Rockoff 2014). In comparison, my estimated effect of school closure is a 3.5 percent decrease in earnings at ages 25-27, which is equivalent to a 0.36 standard deviation decrease in class quality for one year or a one standard deviation decrease in teacher quality for 2.6 years. Moreover, when considering disruptive events, Cabral et al. (2021) find that a school shooting in Texas high schools leads to a 13.5 percent reduction in earnings at ages 24-26. That is, my estimated effect of school closure is equivalent to 26 percent of the effect of experiencing a school shooting in high school.

I further compare my estimates to potential policy experiments. Chetty, Friedman, and Rockoff (2014) estimate that replacing teachers in the bottom 5 percent based on value-added with average teachers for one year would increase the present discounted value of earnings of the students in the classroom by \$250,000. Carrell, Hoekstra, and Kuka (2018) estimate that one year of exposure to a disruptive student reduces the present discounted value of lifetime earnings by \$81,000 to \$105,000. Under the same assumptions for calculating lifetime earnings, my estimate suggests that a classroom of 25 students will experience a reduction of \$456,750 in their present discounted value of lifetime earnings.^{C.3} Thus, my estimates imply that experiencing school closure has roughly the same effect on future earnings as replacing a bottom 5 percent teacher with an average teacher for about 1.8 years. Or it has similar effects as having one more disruptive classmate for five years.

Lastly, Cabral et al. (2021) estimate that the annual aggregate present discounted value of the cost of school shootings in the US from students who experience it is \$5.8 billion. Under the same setup, I estimate the annual aggregate present discounted value of the cost of school closures based on the effects on annual earnings at ages 25-27.^{C.4} With approximately 250,000

^{C.3} I assume that the percentage impact of school closure on earnings at age 25-27 is constant over the life cycle. I also assume that there are no general equilibrium effects and that, to facilitate comparison, the present discounted value of earnings from children at age 12 is \$522,000 from Chetty, Friedman, and Rockoff (2014). This estimate follows Krueger (1999), assuming that earnings are discounted at a 3 percent real annual rate. The effects on one classroom will be $\$18,270 \times 25 = \$456,750$.

^{C.4} Assuming a persistent average effect of exposure through age 64 and a 3 percent real discount rate on earnings, the earnings stream from ages 15-64 in the March CPS is discounted back to age 15. For comparison purposes, I use the calculated present discounted value of lifetime earnings, which is \$888,844. Based on this, the estimated

students being affected by school closures annually from 2010 to 2021 (NCES [2022](#)), the total annual cost of school closures, resulting from displaced students, amounts to about \$7.8 billion. This estimation implies that the annual cost of school closures is approximately 1.3 times the cost of school shootings in the US.

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

D Reasons for Public School Closures in Texas 1998-2015

Table D.1: School Closures in 1998-2003

Campus	District	Year	Enroll.	District Reform	Financial Constraint	Old Building	School Reform	Coding Change	District Closure	Low Perform	Total	Info
ALDERSON J H	LUBBOCK ISD	2001									0	0
ALTA VISTA EL	ABILENE ISD	2003									0	0
ANDERSON EL	LUFKIN ISD	1998		✓			✓				2	1
ANGLETON MIDDLE-EAST	ANGLETON ISD	2002		✓			✓				2	1
ANGLETON MIDDLE-WEST	ANGLETON ISD	2002		✓			✓				2	1
ANNAVILLE EL	CALLEN ISD	2003		✓							1	1
ANTONIO OLIVARES EL	SOUTH SAN ANTONIO ISD	2002									0	0
ASHERTON EL	ASHERTON ISD	1998			✓						1	1
ASHERTON SCHOOL	ASHERTON ISD	1999			✓						1	1
AUSTIN H S	PORT ARTHUR ISD	2002	✓		✓						2	1
BAMMEL MIDDLE	SPRING ISD	2003	✓	✓							2	1
BARSTOW EL	PECOS-BARSTOW-TOYAH ISD	1998	✓		✓	✓					3	1
BELT LINE EL	DESOTO ISD	2003	✓		✓						2	1
BENAVIDES PRI	BENAVIDES ISD	2002									0	0
BENJAMIN F CLARK EL	SPRING ISD	2003	✓	✓							2	1
BOGATA EL	RIVERCREST ISD	2001									0	0
BOOTH EL-NORTH	SAN BENITO CONS ISD	1999									0	0
BOWIE EL	WEATHERFORD ISD	2002	✓	✓							2	1
BOWIE SCH	MCALLEN ISD	2000									0	0
BROOKHOLLOW EL	LUFKIN ISD	1998		✓			✓				2	1
BROWNFIELD INT	BROWNFIELD ISD	2002									0	0
BRYAN H S AT LAMAR	BRYAN ISD	1999									0	0
BURNET BAYLAND H S	HOUSTON ISD	1998									0	0
CANDELARIA EL	PRESIDIO ISD	1998			✓						1	1
CENTRAL EL	BELTON ISD	1999									0	0
COMANCHE INT	COMANCHE ISD	2003		✓			✓				2	1
COSTON EL	LUFKIN ISD	1998		✓			✓				2	1
CREIGHTON INT	CONROE ISD	2001					✓				1	1
CROSSLEY EL	CORPUS CHRISTI ISD	2001	✓								1	1
D ODEM ELEMEN-TARY	SINTON ISD	2003									0	0
DAVID BARKLEY EL	SAN ANTONIO ISD	2002	✓								1	1
DAVID G BURNET EL	SAN ANTONIO ISD	1999	✓								1	1
DAYTOP CAMPUS	PALESTINE ISD	1999			✓						1	1
DENVER CITY INT	DENVER CITY ISD	2003									0	0
DICKSON EL	TEMPLE ISD	1998		✓							1	1
DOBIE INT	SCHERTZ-CIBOLO-U CITY ISD	1998	✓	✓			✓				3	1
ENGE-WASHINGTON INT	GROESBECK ISD	1999					✓				1	1
ERMA NASH ELEMEN-TARY	MANSFIELD ISD	2003									0	0
FREEMAN HEIGHTS EL	TEMPLE ISD	1998		✓							1	1
GLORIETA EL	ANDREWS ISD	1999									0	0
H O WHITEHURST EL	GROESBECK ISD	1999					✓				1	1
HAMBY EL	CLYDE CONS ISD	2003	✓								1	1
HERMAN E UTLEY MIDDLE SCHOOL	ROCKWALL ISD	1999									0	0
HOMEBOUND	IRVING ISD	1999									0	0
HOUSER INT	CONROE ISD	2001					✓				1	1
HOUSTON EL	CORSICANA ISD	2000									0	0
HUNT EL	LUBBOCK ISD	2001									0	0
J M LINDSAY EL	GAINESVILLE ISD	2000		✓							1	1
JOHN E BARBER EL	DICKINSON ISD	2001	✓				✓				2	1
JONES EL	ABILENE ISD	2001									0	0
KENNEDY EL	MERCEDES ISD	2002									0	0
KONDIKE EL	KLONDIKE ISD	2002			✓						1	1
LAKEVIEW SCHOOL	LAKEVIEW ISD	2000	✓								1	1
LAMAR EL	GRAND PRAIRIE ISD	1999	✓								1	1
LAMAR EL	HOUSTON ISD	2002				✓					1	1
LAMAR MIDDLE	MCALLEN ISD	2000									0	0
LANIER EL	TEMPLE ISD	1998		✓							1	1
LEE ACADEMY	CORSICANA ISD	2001									0	0
LEE EL	HOUSTON ISD	2002				✓					1	1
LINCOLN H S	PORT ARTHUR ISD	2002	✓		✓						2	1
LUFKIN DUNBAR INT	LUFKIN ISD	1998		✓			✓				2	1
LUFKIN H S	LUFKIN ISD	1998		✓			✓				2	1
LUFKIN WEST J H	LUFKIN ISD	1998		✓			✓				2	1
MARLBORO EL	KILLEEN ISD	2003	✓	✓	✓						3	1
MARLIN MIDDLE	MARLIN ISD	1998					✓				1	1
MARY HOGE ACAD	WESLACO ISD	2000									0	0
MCCARDELL ACAD	HOUSTON ISD	2000									0	0
MCMURRAY EL	GAINESVILLE ISD	2000		✓							1	1
MEDINA VALLEY J H	MEDINA VALLEY ISD	2000									0	0
NORTHWEST MIDDLE	NORTHWEST ISD	1998		✓							1	1
NORTHWOOD MIDDLE	NORTH FOREST ISD	2001	✓								1	1
OAKWOOD INT	COLLEGE STATION ISD	1999					✓				1	1
PEASE EL	MIDLAND ISD	2001									0	0
PEASE EL	PORT ARTHUR ISD	2002	✓		✓						2	1
POSEY EL	LUBBOCK ISD	2001									0	0
REDFORD EL	MARFA ISD	2002									0	0
ROGERS EL	LAMESA ISD	1999		✓							1	1
RUNNELS J H	BIG SPRING ISD	1999									0	0

SALYERS EL	SPRING ISD	2003	✓	✓								2	1
SAM HOUSTON EL	SAN ANGELO ISD	1998		✓								1	1
SANCHEZ EL	LAREDO ISD	2001		✓								1	1
SCHULENBURG J H	SCHULENBURG ISD	2002										0	0
SHADOWBRIAR MID-DLE	HOUSTON ISD	2002										0	0
SHAW EL	CORPUS CHRISTI ISD	2003					✓					1	1
SHELDON EL	SHELDON ISD	2003	✓									1	1
SHIRLEY EL	HEREFORD ISD	2001	✓		✓							2	1
SKINNER EL	WEST OSO ISD	2000					✓					1	1
SOUTH EL	BROWNWOOD ISD	2002					✓					1	1
SOUTH WARD EL	BRADY ISD	1998		✓					✓			2	1
STATE SCHOOL	LUFKIN ISD	1998		✓					✓			2	1
STUBBS EL	LUBBOCK ISD	2001										0	0
T C WILEMON EL	WAXAHACHIE ISD	1999		✓			✓					2	1
THREE WAY SCHOOL	THREE WAY ISD	2002										0	0
TOMBALL EL	TOMBALL ISD	1998										0	0
TRAVIS EL	GRAND PRAIRIE ISD	1999	✓									1	1
TRAVIS EL	WEATHERFORD ISD	2002	✓	✓								2	1
VALLEY VIEW EL	ABILENE ISD	2003										0	0
W A TODD MIDDLE	DONNA ISD	2000										0	0
WALLIS EL	BRAZOS ISD	1998										0	0
WASHINGTON EL	MIDLAND ISD	2001										0	0
WESTLAWN INT	TEXARKANA ISD	2000										0	0
WHEATLEY EL	TEMPLE ISD	1998		✓								1	1
YOUTH OPPORTUNITY UNLIMITED	LAMAR CONSOLIDATED ISD	2002										0	0
Statistics			23	30	12	7	19	0	0	0		91	62

Table D.2: School Closures in 2004-2009

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

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

Table D.3: School Closures in 2010-2015

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

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

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