

Missing Together: The Long-Run Costs of Student Absenteeism*

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Abstract

Since schools reopened after COVID-19, students have missed substantially more school each year than before the pandemic. In Texas, the increase occurred throughout the distribution of prior attendance and appears even among cohorts that entered school after pandemic-era closures. Peer absences predict student outcomes conditional on a student's own attendance. To estimate the causal effect of changes in school-cohort-level absences prior to the pandemic, I build a leave-out attendance instrument from local attendance shocks across neighboring schools. I establish the validity of the instrument by showing that only shocks that occur before outcomes are measured have an impact on those outcomes. Aggregating the shocks across grades 1 through 9 yields large effects of school-cohort attendance on long-run outcomes. Moving from the 75th to the 25th percentile of the school-by-cohort absence distribution would increase high school graduation by 2.6 percentage points and average earnings at age 25 by \$4,000. Classroom heterogeneity suggests that peer externalities amplify group-level effects beyond the direct effect of a student's own absences.

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I Introduction

Time in school is one of the primary inputs into children’s human capital, and a great deal of education policy is an effort to add more of it. Districts extend the school day and the school year, fund summer programming, and pay for tutoring, all to buy instructional time at the margin (e.g., Patall, Cooper and Allen, 2010; Nickow, Oreopoulos and Quan, 2024). Since 2020, American schools have moved sharply in the opposite direction, though not by design. After schools reopened following the Covid-19 pandemic, absence rates rose sharply nationwide and have since receded only modestly (Dee, 2024; Malkus, 2025). In Texas, one in ten students was chronically absent before the pandemic, compared with one in six in 2025. If current attendance trajectories continue, a child entering kindergarten in Texas after the pandemic is on track to miss roughly 23 more days of school by sixth grade than a child who entered a decade earlier—equivalent to nearly five additional weeks of lost instruction.

Unlike changes to the school calendar, which affect all students uniformly, absences create disruptions that cause students to fall out of sync with their peers. When students miss school, they not only miss out on instructional time, but they also fall behind their peers who attend (Lazear, 2001; Goodman, 2014). These disruptions may cause teachers to slow down or change their teaching in an effort to catch these students up (Carrell and Hoekstra, 2010). Even a student who attends consistently may be impacted by the changes teachers make, generating spillover effects across the cohort.

How might this persistent, cohort-wide change in attendance affect students’ outcomes? I begin by documenting the change in absenteeism following the pandemic. As has been widely reported, absences increased substantially and have remained elevated since schools reopened. Tracking students over time shows that the increase occurred throughout the distribution of prior attendance, including among students who previously attended consistently. The increase is also not merely evidence of scarring among cohorts disrupted by school closures: even students who entered school in 2023 or later have higher absence rates than comparable pre-pandemic students. Finally, peer absences predict student outcomes conditional on a student’s own attendance, which motivates a focus on changes in school-cohort-level absence rates.

The second part of the paper estimates the effects of school-cohort absences on student outcomes. Using pre-2019 data, a leave-out attendance instrument isolates variation in school-cohort absences generated by transitory local attendance shocks, such as the spread of respiratory illnesses. Higher absences lower test scores and long-run outcomes. Classroom heterogeneity provides suggestive evidence of peer externalities: the leave-out instrument changes students’ own attendance similarly regardless of how absence-prone their classmates are, but test-score responses are larger where classmates have a higher propensity to be absent.

The analysis uses administrative data from the Texas Education Research Center, which links educational records to college attendance and labor market outcomes. The records extend through 2025. The causal analysis, however, draws on data from 2019 and earlier, for students born between 1990 and 2009 — restricting to earlier cohorts for the long-run analysis. The data cover standardized test scores, high school graduation, college attendance, and earnings, as well as student absences in six-week reporting periods throughout the school year.

When schools reopened in the 2021–22 school year, absences were substantially higher than before the pandemic. In Texas, the average absence rate had remained near 4.8% of the school year for the prior twenty-five years, jumped to 7.8% in 2022, and remained 30% above baseline in 2025. The increase occurred throughout the distribution: even the 25th percentile of absences was 67% higher in 2025 than in 2019. Prior to the pandemic, students were about as likely to be chronically absent, missing more than 10% of the school year, as they were to have perfect attendance (9.6% versus 10.6%). By 2025, students were 2.7 times as likely to be chronically absent as to have perfect attendance, with the decline in perfect attendance accounting for 45% of the change in this ratio.

These cross-sectional comparisons suggest that the increase was not confined to students who were already frequently absent. To examine the pattern within students, consider two cohorts: students born in 2006, who attended first and fifth grade in the springs of 2013 and 2017, and students born in 2012, who attended those grades in the springs of 2019 and 2023. The first cohort was not exposed to the pandemic between grades 1 and 5, whereas the second was. In both cohorts, first-grade absences strongly predict fifth-grade absences. For the pandemic-exposed cohort, however, fifth-grade absences are higher at every point of the prior-attendance distribution. Among students with perfect attendance in first grade, the exposed cohort missed an additional 2 days in fifth grade, a 61% increase. Among students who were chronically absent in first grade, the exposed cohort missed an additional 4.5 days, a 39% increase. The pandemic therefore appears to have shifted the entire distribution of absences upward, including among students who had previously attended consistently.

These within-student patterns establish a broad shift among cohorts that experienced the pandemic, but they do not say whether elevated absence rates are entirely a legacy of that exposure. If the increase reflects scarring among students who experienced school closures, it should be specific to the exposed cohorts and should fade as they age out of school. Absences show little sign of being unique to those cohorts. Even students who entered school in 2023 or later—and therefore did not themselves experience pandemic-era school closures—have absence rates approximately 2 percentage points higher than their pre-pandemic peers. At the same time, public-school enrollment and student composition changed after the pandemic (Dee, 2023; Francis and Goodman, 2025), so the comparison could mix scarring with compositional change. The

difference persists after controlling flexibly for student composition and time-invariant differences across campuses. Netting out the common drift in attendance between 2023 and 2025 leaves essentially no difference between newly entering and partially exposed cohorts. Elevated absences therefore appear to characterize the post-pandemic school environment more broadly and may not recede simply as exposed cohorts age out.

Do group-level absences predict student outcomes beyond a student's own attendance? Before the pandemic, students in cohorts averaging three absences a year scored 0.4 standard deviations higher on math tests than students in cohorts averaging eight. This relationship could be driven entirely by high absence students having lower test scores, but it is not. Roughly 80% of it remains after accounting for a student's own attendance rate.¹ The correlation motivates a focus on school-cohort absences, but it does not identify their effects. I therefore turn to pre-pandemic local attendance shocks.

To build intuition, start with a concrete example: the spread of the flu. In places and years with higher flu activity, absences rise and test scores fall. While average flu rates across areas over time may not be random, many exogenous factors determine the severity of the flu in a given place in a particular year, including weather fluctuations and viral mutations, which are plausibly exogenous to students' potential outcomes (e.g., Tamerius et al., 2013; Webster et al., 2014; Perofsky et al., 2024). Data collected from a Texas hospital network give quarterly flu caseloads over two years at the ZIP code level. High levels of flu cases generate an additional 10 absences per school-cohort per year. Those absences lower test scores by approximately 1% of a standard deviation (SD), though the estimate is somewhat noisy, with a standard error (S.E.) of 0.44 pp. Measuring the effect on long-run outcomes requires more precision. The next step therefore replaces measured flu cases with a leave-out instrument built from absences in other schools, to capture the same kind of transitory local shock.

The instrument captures spikes in absence rates in particular schools and years, above and beyond the typical absence rates for those students and schools. Construction begins by removing the school-by-cohort and school-by-grade components of absences, which ensures that the remaining variation is not driven by specific cohorts (e.g., the class of 2005 at Austin Elementary School consistently having high absences) or particular grades (e.g., third graders at Austin Elementary School having higher absences under more lenient teacher attendance policies). What is left reflects year-specific changes in absences that affect all students in a given school. If 2002 brought an unusually severe flu season to Austin, absences that year would run above the baseline rates expected for particular cohorts and grades.

¹Half of the relationship persists even for students who themselves have perfect attendance in a given grade, consistent with earlier work finding a relationship between peer absences and outcomes conditional on own absences (e.g. Monk and Ibrahim, 1984; Gottfried, 2011; Goodman, 2014; Gottfried, Kirksey and Ozuna, 2020; Gottfried and Ansari, 2022).

Rather than use variation in the focal school itself, the instrument is a leave-out measure, aggregating over other schools in the same school district. Using variation from nearby schools removes any potential bias introduced by school-by-year specific shocks that impact both absences and outcomes, and isolates the plausibly exogenous variation in absences that is shared across schools in the same district in a given year. The first stage therefore asks how absences in a focal school and year change when students in nearby schools happen to attend at lower rates in that same year. That variation is consistent with absences generated by the spread of respiratory illnesses in a particular area in a particular year. The instrument correlates with measured local flu activity in the years where the data are available.

Absences are higher in the focal school when students in nearby schools are absent at higher rates. The first stage is strong: a 1 standard deviation increase in the instrument raises absences by 0.15 days per student in the focal school (S.E. = 0.003). In a typical school-cohort of 100 students, that is 15 additional student-days missed in the grade. How does the increase in school-cohort absence rates affect contemporaneous test scores?

Higher school-cohort absences lower math scores. One additional day absent per student lowers scores by 0.038 standard deviations (S.E. = 0.006).² The 95% confidence interval ranges from -0.050 to -0.026 standard deviations.

The key identification concern is that when absences in nearby schools are high, outcomes in the focal school are low for reasons other than absences in that school. One concern is imbalance: when the instrument is high, other observable characteristics of students and teachers may be high too—the fraction of free and reduced-price eligible students, say, or the fraction of inexperienced teachers—and the imbalance, not absences, causes lower outcomes. The instrument turns out to be balanced on a broad range of student and teacher characteristics. A second concern is that the instrument is correlated with a different local shock. If absences spiked because of a negative economic shock to the area, that shock could move outcomes for reasons other than school attendance. In practice, the instrument is not correlated with changes in local unemployment rates.

Balance on time-varying observable characteristics of schools and communities still leaves open the possibility of selection on unobservables. Outcome-based placebo tests in the style of Danieli et al. (2024) tighten the bounds on the local shocks that would bias the treatment effects. A within-year version of the instrument, built separately for each six-week attendance period, allows a comparison of reduced-form effects before and after students take standardized exams in the spring. Suppose a local, unobservable shock—a spike in crime, say—drove the results and lasted

²Other work has measured the effects of absences on test scores. The closest is Goodman (2014), who uses local weather shocks as an instrument for absences. Other work has typically relied on observational data with classroom and/or student fixed effects or relied on random assignment to teachers of different types, such as non-white teachers (e.g. Gottfried, 2009, 2011; Gershenson, Jacknowitz and Brannegan, 2017; Gershenson, McBean and Tran, 2018; Liu, Lee and Gershenson, 2021; Tran and Gershenson, 2021)

at least six weeks. It would correlate test scores in April with the instrument in May, even though those absences occur after students have taken the exam. Instead, only the periods leading up to the exam show a significant reduced-form effect on test scores. The test rules out any unobserved shock that moves both absences and outcomes and persists across six-week periods. What remains is a shock that moves both but dissipates within six weeks.

Respiratory illness shocks are high-frequency, transitory shocks so an alternative concern is that the IV estimates are biased by the direct effect of illness on students' outcomes. There is a growing body of evidence that in utero exposure to the flu is harmful for development and that these effects persist into adult outcomes (e.g. Almond, 2006; Currie and Schwandt, 2013; Ward and Phipps, 2014; Schwandt, 2018). Even after birth, exposure to illnesses during infancy has adverse effects on long-run outcomes (Daysal et al., 2026). The shocks here fall during adolescence, a period with substantially less neural and physical development (Currie and Almond, 2011). Even so, estimating the first stage and reduced form in 10 separate samples based on prior absence rates speaks to how much of the relationship between the instrument and test scores runs through absences. The reduced-form effect is smaller where the first stage is smaller—where the instrument induces fewer absences. The pattern is consistent with the treatment effect running through absences rather than the direct effect of respiratory illnesses.

With the strategy validated in the short run, the next part of the paper measures the effects of absenteeism on outcomes later in life. Summing the instrument over grades 1 through 9 gives an aggregate measure of exposure to absence shocks. Students with high draws met several bad years of illness during those grades, and their cohorts carried elevated absence rates through school. Students with low draws met fewer such years and attended more regularly. The first stage shows that the aggregated instrument generates meaningful variation in school-cohort absence rates. School-cohorts in the 95th percentile of the instrument distribution have approximately 150 additional total absences between first and ninth grade, compared to school-cohorts at the 5th percentile. This is equivalent to roughly one-quarter of the difference in absence rates between low- and high-poverty schools.

Absenteeism has substantial long-term effects on students' outcomes. Moving from the 75th to the 25th percentile of the school-by-cohort absence distribution (a decrease of 10.0 days absent per student between grades 1 and 9) would increase high school graduation rates by 2.6 percentage points (S.E. = 0.6 pp) and raise average earnings at age 25 by \$4,000 (S.E. = 1,700).

The treatment effects combine the direct effects of student absences on test scores with possible spillover effects from peer absences. If there were no negative externalities from peer absences, then conditional on a student's own response to the instrument, the reduced-form effect should be the same regardless of the first stage for her peers. It is not. Middle school math classroom assignments between 2012 and 2019 identify a student's class as high- or low-absence,

based on the lagged average absences of her peers in the prior year, controlling for school-by-grade and year-by-grade fixed effects. Conditional on a student's own lagged absences, the first stage in response to the respiratory illness instrument is the same whether she sits in a high- or a low-absence class (p-value on the difference between a 1 SD higher absence classroom = 0.4). The first stage for her peers, however, is higher in a high-absence class. The same 1 SD increase in the respiratory shock leads to a 15.3% higher response from her peers (p-value on the difference = 0.008). Correspondingly, the reduced form for the focal student is larger in high-absence classes (p-value on the difference = 0.097). The pattern, though suggestive, is consistent with negative externalities from peer absences, which amplify the effect of a given illness shock beyond the absences of any one student. Small individual changes in attendance can therefore generate larger group-level effects.

The large long-run effects of changes in school-cohort absences have important implications for students in school today, whose absences remain elevated following the pandemic. The post-pandemic increase lies outside the support of the respiratory illness instrument, so these calculations should be interpreted as extrapolations rather than forecasts. Applied mechanically to the increase in absences in 2022 alone, the estimates imply a 1.6 pp decrease in high school graduation rates. Extrapolating current absence-rate trends relative to baseline, students who began school after the pandemic in Texas will miss an additional 23 days of school by sixth grade, which would imply a 6 pp decrease. Schools have also undertaken substantial efforts to mitigate the effects of the pandemic—including tutoring, summer learning, and other academic interventions—supported by nearly \$190 billion in federal relief (Carbonari et al., 2022; Dewey et al., 2024). Evidence suggests that this spending contributed to academic recovery, although many district interventions proved difficult to implement at their intended scale and intensity (Carbonari et al., 2022; Dewey et al., 2024). Therefore, one should not expect the extrapolated effects to materialize one-for-one. Instead, the estimates underscore the importance of sustaining recovery efforts, particularly efforts to restore regular attendance.

This paper contributes to a large literature on the consequences of student absenteeism. Studies using student fixed effects, value-added models, and quasi-experimental variation consistently find that individual absences reduce contemporaneous academic achievement (e.g. Gottfried, 2010; Goodman, 2014; Aucejo and Romano, 2016; Gershenson, Jacknowitz and Brannegan, 2017; Liu, Lee and Gershenson, 2021). Evidence on longer-run consequences is more limited and generally relies on stronger identifying assumptions. Liu, Lee and Gershenson (2021) find that secondary-school absences predict lower high school graduation and college enrollment after controlling for prior achievement and a rich set of student, school, and neighborhood characteristics. Similarly, Dräger, Klein and Sosu (2024) document conditional associations between childhood absences and lower educational attainment and labor-force participation in

Britain. The closest quasi-experimental evidence comes from Cattani et al. (2023), who use historical Swedish data and sibling fixed effects to show that elementary-school absences are associated with lower earnings throughout adulthood. This paper extends that literature by using transitory local shocks to school-cohort attendance to estimate effects on short-run achievement and long-run educational and labor-market outcomes in a contemporary U.S. setting.

This paper also relates to an emerging literature on post-pandemic absenteeism. Dee (2024) and Fuller et al. (2025) document the large and broad-based rise in absences after schools reopened. Swiderski, Fuller and Bastian (2025) and Yaow et al. (2026) compare the relationship between absences and student outcomes before and after the pandemic, finding that absences remain consequential even though their estimated marginal effects were modestly smaller in the immediate post-pandemic years. This paper complements that work by showing that elevated absences extend across the distribution of prior attendance and to cohorts that entered school after pandemic-era closures, and by using pre-pandemic quasi-experimental variation to quantify the long-run consequences of a persistent increase in cohort-wide absenteeism.

This paper is also related to the literature on peer effects in schools (e.g. Hoxby, 2000; Chetty et al., 2011; Mallah, 2026), as well as research on the externalities caused by disruptions. Here spillovers occur because students in high-absence cohorts perform worse even if they themselves are not frequently absent. Goodman (2014) demonstrates the importance of keeping students aligned with their peers for academic success. Similarly, studies have shown that when individual students act out in class, the disruptions they cause can have negative effects on their classmates, both immediately and in the long-run (Carrell and Hoekstra, 2010; Gottfried, 2011, 2013; Carrell, Hoekstra and Kuka, 2018). The results suggest that absenteeism represents another significant form of disruption—one that can affect students’ learning even when they themselves are consistently present and can impact students even after they leave school.

The rest of the paper proceeds as follows. Section II describes the data; Section III presents descriptive evidence on the increase in absences following the pandemic and the relationship between group-level absences and outcomes; and Section IV estimates the causal effect of absenteeism on academic performance, graduation rates, college attendance, and labor market outcomes. Section V explores the effect of spillovers across students and Section VI concludes.

II Data

The analysis uses administrative records for all students who attend Texas public schools. The Education Research Center at UT Austin links records from the Texas Education Agency, the Texas Higher Education Coordinating Board, the National Student Clearinghouse, and the Texas Workforce Commission. Together, the records follow students from kindergarten into young

adulthood. Two outside sources round out the data: local unemployment rates from the Bureau of Labor Statistics and flu incidence from a Texas hospital network.

The sample is restricted to students observed in every grade from kindergarten through ninth grade, so that absences can be measured across a whole school career. The resulting sample contains 7.5 million students born between 1990 and 2009. TEA records time-invariant characteristics—birth cohort, gender, and race/ethnicity—and annual characteristics—school, grade, and free or reduced-price lunch (FRPL) eligibility. Annual absences are defined as the total days missed across the year’s six six-week reporting periods.

TEA also provides standardized math scores in grades 3–5 from 1997 through 2019.³ High school graduation is defined as an indicator for ever graduating.

THECB records enrollment at public and private colleges in Texas. For Texas high school graduates, National Student Clearinghouse records add approximately 90% of college enrollment nationwide (Dynarski, Hemelt and Hyman, 2015). The college attendance outcome measures whether a student ever attends a four-year university between ages 18 and 22.

Texas Workforce Commission unemployment-insurance records provide quarterly earnings. The records exclude self-employed workers, independent contractors, military personnel, and workers in the informal sector (Stevens, 2007). A missing record can therefore indicate either no covered employment or work outside Texas. The earnings sample is restricted to people with positive earnings at least once between ages 21 and 27. Within that sample, earnings are set to zero for missing quarters and summed over the year. Ninety percent of the analysis sample meets this criterion. That coverage is close to the 88% of people living in Texas at age 16 who remain there at age 26, as documented by Sprung-Keyser et al. (2022).

Data from a Texas hospital network supply flu caseloads for 2018 and 2019, recorded quarterly by patient home ZIP code. Appendix Figure A.1 shows the coverage. The counts are matched to schools using the school’s ZIP code. Section IV uses them to motivate the empirical strategy.

Monthly local unemployment rates come from the Bureau of Labor Statistics. They cover all cities and towns with at least 25,000 residents, which accounts for more than 85% of Texas residents (Green, 2023). The series runs from 2000 to 2019 and is matched to schools by the city where the school is located, based on its street address. Section IV uses these data in a test of the identification strategy.

³Students took TAAS exams between 1991 and 2002, TAKS exams between 2003 and 2013, and STAAR exams beginning in 2014. Because the empirical analysis compares students within years, changes in the test do not threaten comparability.

II.A Summary Statistics

Table I reports summary statistics. The sample holds 7.5 million unique students born between 1990 and 2009. The test-score sample covers students who were in grades 3–5 at some point between 1997 and 2019, or more than 80,000 school-by-birth-cohort-by-grade cells. The long-run sample covers students born between 1990 and 2000, distributed over nearly 30,000 elementary-school-by-birth-cohort cells.

Panel A describes the characteristics of students in the sample. Approximately 50% of students are FRPL eligible in any given year. About 50% of students are Hispanic and 34% are non-Hispanic white. The typical elementary school-cohort has 99 students. The object of interest throughout is the change in the school-cohort absence rate across those 99 students.

Panel B describes teachers in the elementary and middle schools in the sample. Twenty-one percent hold an advanced degree (a master’s or doctorate), 16% are male, and 35% are non-white. Teachers average 11 years of experience, including nearly eight years at their current school, and earn \$51,000 in 2015 dollars. Because teachers can affect student absences, Section IV uses these characteristics to probe the validity of the leave-out attendance instrument (e.g. Jackson, 2018; Tran and Gershenson, 2021).

Panel C reports statistics on student absenteeism, the focus of this paper. In grades 3–5, the average student misses about 5 days per year ($SD = 1.2$ days per student). This is the measure used in the test-score analysis. The long-run analysis instead uses total absences per student between grades 1 and 9. On average, students miss around 50 days of school over those years ($SD = 7.7$).

Finally, Panel D reports statistics on later-in-life outcomes. The average high school graduation rate is 88%. About 30% of students attend a four-year college at some point between ages 18 and 22, in-state or out-of-state. By age 25, the typical student earns just over \$26,000.

III Descriptive Facts on Absenteeism

Sharp post-pandemic increases in chronic absenteeism and absences more broadly have been widely documented across the country (Dee, 2024; Malkus, 2025; Fuller et al., 2025; Kirksey, 2025; Swiderski, Fuller and Bastian, 2026). Texas’s long administrative panel places the rise in historical context. Panel A of Figure I plots the mean and the 25th and 75th percentiles of the share of the school year that students missed from 1995 to 2025. From 1995 through 2019, the average absence rate remained near 4.8%. It jumped to 7.8% when schools reopened in 2022, and by 2025 had fallen to 6.1%, still approximately 30% above its pre-COVID-19 level. The increase was not confined to the average student: the 25th percentile rose from 1.2% to 2.5% and the 75th from 5.8% to 9.8% between 2019 and 2022, before falling back to 2.0% and 7.7% by 2025. Panel

B traces the same pattern at the tails: the chronically absent share rose from roughly 10% before the pandemic to 24.2% in 2022 before declining to 16.4% by 2025, while the share with perfect attendance fell from a similar 10% to 4.8% and has recovered only to 6.0%. The partial recovery since 2022 has not restored either tail to its pre-COVID-19 level.

The aggregate trends in Figure **I** are based on repeated cross-sections and therefore do not show whether attendance changed after the pandemic relative to before for a given student. To examine this directly, Figure **II** compares the relationship between first- and fifth-grade absences for two birth cohorts.⁴ Students born in 2006 attended first and fifth grade in 2013 and 2017, respectively, and therefore had completed fifth grade prior to the pandemic. Students born in 2012 attended first grade in 2019 and fifth grade in 2023, experiencing pandemic-era school closures between the two grades. In both cohorts, first-grade attendance strongly predicts fifth-grade attendance. For the pandemic-exposed cohort, however, fifth-grade absences are higher throughout the distribution of first-grade attendance. Among students with perfect attendance in first grade, the exposed cohort missed approximately two additional days in fifth grade, a 61% increase relative to the earlier cohort. Among students who were chronically absent in first grade, the difference was approximately 4.5 days, a 39% increase. Thus, the post-pandemic increase extends even to students who previously attended school consistently, although the largest absolute increases occurred among students who already had high absence rates.

One possible explanation for the elevated absences among the later cohort in Figure **II** is scarring among students who experienced pandemic-era school closures. The interruption of established attendance routines or weakening of students' attachment to school may have continued to affect these cohorts after in-person instruction resumed. Consistent with this possibility, previous work finds that subsequent increases in chronic absenteeism were larger in places that spent more time in remote instruction, although this evidence is correlational (Dee, 2024; Evans, Muchnick and Rosenlund, 2024). Panel **A** of Figure **III** provides a simple first test of this explanation by comparing first- and eighth-grade absence rates over time. All eighth graders observed through 2025 were old enough to have been enrolled in K–12 schools during the pandemic. In contrast, the most recent cohorts of first graders entered kindergarten after pandemic-era K–12 closures, although they may have experienced closures of preschools or child-care centers. If scarring from exposure to K–12 closures were the primary explanation for elevated absences, recent eighth graders should remain above their pre-pandemic baseline while first graders returned toward theirs. Instead, the two grades have had remarkably similar absence rates in every year since 2022. By 2025, first and eighth graders missed 5.7% and 5.8% of the

⁴Kirksey (2025) uses longitudinal data, including from Texas, to track students' movements across grade-specific absence quintiles before and after the pandemic. The exercise here instead compares the first-to-fifth-grade attendance gradient across cohorts and conditions directly on students' first-grade absence rates.

school year, respectively.

Panel *B* examines this pattern more systematically by separating students into three groups. The first consists of cohorts observed entirely before the pandemic (1991-2001 birth cohorts). The second consists of post-pandemic cohorts that had not yet entered K–12 schools when closures occurred (2017-2020 birth cohorts). The third consists of post-pandemic cohorts that were already enrolled and were therefore partially exposed to pandemic-era closures, though the figure plots only their post-pandemic absence rates (2011-2016 birth cohorts). Because these cohorts are observed at different stages of their schooling, grades 2 and 3 are currently the only grades in which all three groups can be compared. In second grade, never-exposed cohorts have absence rates between 5.2% and 5.5%, compared with approximately 5.8% among partially exposed cohorts and 3.3% before the pandemic. The same pattern appears in third grade: absence rates are approximately 4.8% among never-exposed cohorts, between 5.2% and 5.4% among partially exposed cohorts, and 3.1% among pre-pandemic cohorts. Thus, although never-exposed students have slightly lower absence rates than partially exposed students, they remain much closer to the partially exposed cohorts than to the pre-pandemic benchmark. These raw comparisons suggest that most of the post-pandemic increase is shared by cohorts that were and were not directly exposed to K–12 school closures.

Students who never experienced K–12 closures share nearly 80% of the post-COVID-19 increase in absences among partially exposed students. Panel *C* formalizes the comparison in grades 2 and 3, where all three groups overlap. Each student’s absence rate is regressed on a post-COVID-19 indicator and its interaction with an indicator for a never-exposed cohort. The panel reports the increase for never-exposed cohorts as a share of the increase for partially exposed cohorts; with grade fixed effects, the ratio is 0.79. One concern is that public-school enrollment and student composition changed following the pandemic (Dee, 2023; Francis and Goodman, 2025). If the students entering public schools after the pandemic have characteristics associated with higher baseline absence rates, the raw comparison could confound a shared post-pandemic shift with compositional change. The specification therefore adds fixed effects progressively, comparing students in the same grade and demographic group, at the same campus, and with the same measures of economic disadvantage. These controls account for changes along observed dimensions of student composition and for fixed differences across campuses, but cannot rule out changes in unobserved student characteristics or in campus environments over time. The estimated shared fraction remains between 0.79 and 0.80 across all four specifications. The final specification also conditions on free- or reduced-price lunch and at-risk status, which may themselves have been affected by the pandemic and can therefore be viewed as potentially endogenous controls; including them nonetheless has virtually no effect on the estimate.

The 0.79 ratio likely understates the shared component because never-exposed cohorts appear

later, after absence rates had begun to fall. Their smaller increase therefore mixes any effect of exposure to closures with a calendar-time decline in absences (i.e., drift). Panel *D* gauges that decline with three placebo comparisons. The placebos preserve the calendar years of the main comparison but move to higher grades and earlier birth cohorts, so both groups experienced COVID-19 school closures. Any difference between the placebo groups cannot reflect whether students experienced K–12 closures. If cohort-specific scarring explained the 20% gap in the main comparison, the placebo ratios should be close to one. Instead, they range from 0.81 to 0.84, close to the main estimate of 0.79. The calendar-time decline can therefore explain the remaining gap. Benchmarked against that decline, elevated absences do not appear concentrated among cohorts exposed to K–12 closures. They point instead to a broader change in schools after COVID-19 closures.⁵

Taken together, the evidence shows that the rise in absenteeism after COVID-19 was broad and persistent: absences rose throughout the distribution of prior attendance, within students, and nearly as much among cohorts that entered K–12 after school closures ended. Elevated absences therefore appear to reflect a broader change in schools after COVID-19 closures, not only lingering disruption among directly exposed students. Because absence rates rose across entire cohorts, the consequences may run through both a student’s own absences and her classmates’ absences. Next, pre-COVID-19 data show how individual- and school-cohort-level absences relate to outcomes in school and later in life.

There is a strong correlation between school-cohort absences and outcomes measured in school and beyond. In Panel *A* of Figure *IV*, one additional day absent per student in a school cohort is associated with math scores that are 0.086 standard deviations lower. Figure *V* measures school-cohort absences as average total absences per student from grades 1 through 9. Nine additional days—one per grade—are associated with a 2.43-percentage-point decline in high school graduation. Both correlations mix two associations. Students who miss more school score lower and graduate at lower rates (Appendix Figure *A.2*), and students in higher-absence cohorts also have lower average outcomes.

The negative correlation between test scores and group-level absences persists even among students with perfect attendance. Panel *B* of Figure *IV* restricts the sample to students who miss no days of school that year (approximately 15% of students in grades 3–5). The slope remains negative, and more than 50% of the unconditional relationship persists.⁶ Appendix Figure

⁵The scarring results are consistent with Malkus (2026), who uses aggregate grade-level chronic absenteeism rates across states to show that absenteeism remains elevated among students who entered school after COVID-19 closures. Student-level data allow for the analysis of continuous absence rates; comparisons within grades, demographic groups, and campuses; and a benchmark against the subsequent decline in absences.

⁶Other work documents similar associations between cohort-level absences and outcomes; the restriction to students with perfect attendance probes possible spillovers (e.g. Monk and Ibrahim, 1984; Gottfried, 2011; Goodman, 2014; Gottfried, Kirksey and Ozuna, 2020; Gottfried and Ansari, 2022).

A.3 reaches the same conclusion after controlling for quartiles of students' own absence rates: school-cohort absences remain negatively correlated with test scores.

The correlation could reflect either a causal effect of group-level absences or selection. A student who misses class loses that day's material and falls out of sync with classmates. Goodman (2014) shows that this mismatch explains a significant share of the effect of absences on test scores. The disruption may reach classmates if teachers slow or repeat instruction for students who have fallen behind. Related classroom disruptions, including exposure to students who act out, lower classmates' performance in the short and long run (Carrell and Hoekstra, 2010; Gottfried, 2013; Kinsler, 2013; Carrell, Hoekstra and Kuka, 2018). These mechanisms could make group-level absences matter beyond a student's own attendance.

Selection offers a competing explanation. Students in high-absence cohorts may face unstable family circumstances, lower-quality teaching, or local economic shocks that independently lower outcomes. Jackson (2018) shows that some teachers improve both attendance and test scores. Student performance also falls during local economic shocks (e.g. Page, Stevens and Lindo, 2009; Stevens and Schaller, 2011; Jackson, Wigger and Xiong, 2021); if absences rise at the same time, the shocks could generate the observed patterns. The next section first uses two years of flu data as a direct test, then builds the leave-out attendance instrument from pre-COVID-19 local attendance shocks to estimate causal effects on short- and long-term outcomes.

IV Causal Effect of Absenteeism on Outcomes

Estimating the causal effect of school-cohort absences on short- and long-run outcomes requires exogenous variation in cohort-level absences across cohorts within a school. Prior research on the causal effects of absenteeism has largely focused on test scores, which are observed multiple times for the same student, allowing for within-student comparisons (e.g. Goodman, 2014; Liu, Lee and Gershenson, 2021). For example, Liu, Lee and Gershenson (2021) compare within-student performance across subjects, arguing that, conditional on total absences, it is exogenous which subject a student misses. They find that students who miss relatively more math classes perform worse on math exams than on English exams, and vice versa. This design, as they note, cannot be used to measure the effect on high-school graduation, which has no within-student variation. Estimating long-run causal effects requires exogenous variation in total absences. The object of interest is the total effect of absences, spillovers included, so the design needs exogenous variation in total, group-level absence rates.

IV.A The Effect of the Flu on Absences and Outcomes

The empirical strategy isolates plausibly exogenous variation in school-cohort absences generated by transitory local attendance shocks. One observed example—the spread of the flu—builds intuition before the approach is generalized.

The design isolates year-to-year variation in the severity of flu outbreaks within the same local area. Unlike persistent factors—such as vaccine coverage or healthcare access—that can make some places systematically more prone to the flu, its intensity fluctuates from year to year due to factors like viral mutations and environmental conditions (e.g. Tamerius et al., 2013; Webster et al., 2014; Perofsky et al., 2024). A district with a severe flu season one year may see a milder season the next, even though its underlying health infrastructure is unchanged. Year-to-year changes in flu severity are plausibly exogenous to other determinants of student outcomes, once school-by-grade and year-by-grade fixed effects absorb differences across schools, grades, and years.⁷ This year-to-year variation in flu severity provides plausibly exogenous variation in group-level absences.

Start with the effect of flu spread on absences. Suppose 2018 was a worse flu year in Austin than 2019, the two years with flu data. Third graders at Austin Elementary School in 2018 should then have higher absence rates than the third graders who followed them in 2019. The following first stage formalizes the comparison and shows that absences are higher when flu cases are higher:

$$A_{sgy} = \beta^{FS,flu} \text{LogFlu}_{zip(s)y} + \phi_{sg}^{FS,flu} + \gamma_{yg}^{FS,flu} + \epsilon_{sgy}^{FS,flu}. \quad (1)$$

The first stage predicts absences A_{sgy} in school s , grade g , and year y from the log number of flu cases in the ZIP code of school s , $zip(s)$, in year y . Absences are measured for students in grades 3–5. Each cohort c in school s therefore appears in two different grades. The specification includes school-by-grade fixed effects, ϕ_{sg} , which compare within a school-grade across years (e.g. third graders at Austin Elementary School in 2018—the high flu year—and 2019—the low flu year), and statewide year-by-grade fixed effects, γ_{yg} , which absorb statewide differences in flu levels between the two years, isolating the change specific to Austin. The coefficient of interest, $\beta^{FS,flu}$, captures the effect of higher flu cases on school-cohort absences.

Absences are higher when the flu is spreading at higher rates. Figure VI Panel A shows a binned scatter plot of this relationship. The figure residualizes A_{sgy} and $\text{LogFlu}_{zip(s)y}$ on the remaining controls in equation 1, groups the residuals into 20 equally sized bins, and plots the mean of the residuals within each bin and the line of best fit to visualize the relationship. The figure also reports the coefficient from the linear model. Moving from the 5th to 95th percentile

⁷Cross-immunity could make exposure in one year predict lower exposure in the next. The relationship also depends on factors such as the antigenic match between strains in successive years (Krauland et al., 2022).

of the residualized $\text{LogFlu}_{zip(s)}$ distribution generates an additional 0.1 days absent per student (on average students miss 5 days per year). In a cohort of 100 students, this is equivalent to 10 additional absences, or approximately 10% of a standard deviation in the school-cohort-grade absence distribution.

Do the absences lower test scores? The reduced form:

$$Y_{sgy} = \beta^{RF,flu} \text{LogFlu}_{zip(s)y} + \phi_{sg}^{RF,flu} + \gamma_{yg}^{RF,flu} + \epsilon_{sgy}^{RF,flu}. \quad (2)$$

for standardized math scores, Y_{sgy} . Figure VI Panel B shows the analogous binned scatter plot and the coefficient from the linear model in equation 2. There is a clear negative relationship between the spread of the flu and test scores; the cohort exposed to higher flu cases in grade g performs worse on standardized exams. Moving from the 5th to 95th percentile of the residualized $\text{LogFlu}_{zip(s)}$ distribution lowers average test scores by approximately 1% of a standard deviation. The corresponding IV estimates imply that an additional 1 day missed per student on average lowers test scores by approximately 10% of a standard deviation, but the estimate is noisy; the 95% confidence interval ranges from an effect of -0.22 SDs to -0.01 SDs. Nonetheless, this exercise does show that absences and subsequently test scores respond to local variation in the spread of the flu.⁸ To gain the level of precision needed to estimate effects on long-run outcomes, I generalize this approach to capture similar types of local shocks to attendance.

IV.B Effects of Local Shocks to Attendance on Outcomes

Because the flu data cover only two recent years, they cannot identify long-run effects: third graders exposed to a severe flu season in 2018 are not yet adults. The leave-out attendance instrument captures local attendance shocks shared across schools. This broader design is not limited to measured flu outbreaks and can be applied to older cohorts with adult outcomes.

B.1 Effects on Short-Run Outcomes

The empirical strategy isolates variation in school-cohort absence rates arising from transitory, exogenous factors such as the spread of respiratory illnesses. A stylized example builds the intuition (Appendix Figure A.4). Absence rates for two cohorts in this hypothetical school, John Harvard Elementary, are shown. In the 1992 birth cohort, the series marked by circles, students miss approximately 5.5 days per grade on average. However, in 2000, when the students are in 3rd grade, absences are substantially higher. What might be generating this spike? With data from only

⁸Aucejo and Romano (2016) also use the flu as an instrument for absences, and find suggestive negative effects, but the results are not statistically significant.

one cohort it is difficult to say. For example, it could be the case that the third grade teachers at John Harvard Elementary are not effective at promoting attendance as in Jackson (2018). Alternatively, the spread of respiratory illnesses could have been particularly high in this neighborhood in 2000. Distinguishing between these possibilities requires making use of the panel structure of the data.

Adding a second cohort rules out the possibility that the spike is generated by year-invariant teacher effects. The series marked by squares gives absence rates for the 1993 birth cohort. If the spike were driven by the specific traits of third grade teachers at John Harvard Elementary, the 1993 cohort would spike in 2001, when it reached third grade. Instead, although its absence rates run somewhat lower on average, it spikes in 2000 as well. The pattern points to a shock specific to the local area around John Harvard Elementary in 2000 that affects all students in the school, such as an illness shock.⁹

Parameterizing the intuition behind the stylized example identifies local spikes in absenteeism in the data. The construction begins with a panel of absences from 1995 to 2019, covering 57 million student-years, and removes the school-by-cohort and school-by-grade components of absence rates. The school-by-cohort component accounts for students who are consistently absent at high or low rates (in the stylized example, the 1992 cohort against the 1993 cohort). The school-by-grade component accounts for year-invariant teacher effects (the third grade teachers of the example). A statewide, year-specific component is also removed, absorbing variation shared across all schools in a given year, such as a recession. Concretely, the model is:

$$A_{iy} = \delta_{s(i,y),c(i)} + \gamma_{s(i),g(i,y)} + \phi_y + \varepsilon_{iy} \quad (3)$$

which regresses absences, A_{iy} , for student i , born in birth cohort c and attending school $s(i,y)$ in year y , on school-by-cohort (δ), school-by-grade (γ), and year (ϕ) fixed effects.

The residuals isolate local, year-specific spikes in absenteeism. Summing over students i in school s in year y aggregates them to the school-year level,

$$z_{sy} = \sum_{\substack{i \text{ such that} \\ s(i,y)=s}} \varepsilon_{iy},$$

to capture the portion of the variation in absence rates at the school-by-year level that is unexplained by the school-by-cohort, school-by-grade, and year components.

What generates variation in $z_{s,y}$? If absences are high in school s in year y due solely to the composition of cohorts in the school who all have high latent absence rates, this will not be reflected in $z_{s,y}$ due to the school-by-cohort fixed effects. Similarly, if absences are high in school s

⁹The example has a shock in a single year, 2000. Appendix A shows a related example with a shock in five adjacent years and shows that persistence across years does not bias the estimates.

in year y because a cohort c with typically high absence rates is in third grade, which has teachers with lenient attendance policies, this will not be reflected in z_{sy} due to the school-by-grade fixed effects. Instead, z_{sy} isolates “blips” in absence rates in a given school and year above and beyond these factors, as motivated in Appendix Figure A.4.

A school-year “blip” is not necessarily a valid instrument. A school-by-year-specific event could both raise absences and lower outcomes directly. For example, Ang (2021) shows that nearby police violence raises absences and lowers achievement within a narrow geographic radius. Using the focal school’s z_{sy} would then misattribute part of the event’s direct effect to absences.

To remove school-by-year-specific confounders, the instrument takes an enrollment-weighted average of the blips in every other school in the district:

$$\hat{z}_{sy} = \frac{1}{N_{d(s)} - N_s} \sum_{\substack{s' \text{ such that} \\ d(s)=d(s') \text{ and } s' \neq s}} N_{s'} z_{s'y},$$

where $N_{d(s)}$ and N_s are enrollment in district $d(s)$ and school s , respectively.¹⁰ The leave-out attendance instrument excludes the focal school’s own shock and retains district-year variation shared across schools. It therefore protects against focal-school confounds such as the police-violence example. It does not by itself establish that district-wide shocks affect outcomes only through absences. Appendix A shows how the leave-out construction corrects bias under the simulated structure.

This approach does not rely on the assumption that local shocks to attendance last only one year. As detailed in Appendix B, even when a cohort experiences persistent shocks throughout elementary school (high flu spread in all five years, say), cohorts at the start and end of the shock period still reveal it (see Appendix Figure A.13). Detection requires a sufficiently long panel; this one spans 25 years. Estimates of the local shock’s magnitude will be attenuated, since the shock is collinear with the school-by-cohort fixed effect for the cohort that is always exposed. But that attenuation does not create bias.¹¹

The leave-out attendance instrument is positively related to contemporaneous flu incidence: when flu cases are higher, the instrument is also higher (Appendix Figure A.5, Panel A).¹² If the instrument captured only reported flu cases, measured without error, this relationship would

¹⁰The weights do not account for distance or social connections, which can predict disease spread better than distance (e.g. Kuchler, Russel and Stroebel, 2022). Omitting such information may weaken the first stage. It does not bias the IV estimate if the resulting leave-out variation affects outcomes only through absences in the focal school.

¹¹What matters for the IV estimates is not an unbiased magnitude of the local shocks, but detection of some of the exogenous variation in absence rates. Simulations in Appendix B show that the corresponding IV estimates are unbiased even in the presence of correlated shocks.

¹²Panel B of the same figure shows the relationship is unique to contemporaneous flu and instrument values. Flu cases in one period have essentially no relationship with the instrument in the prior period.

have an R^2 of 1. Instead, flu incidence explains only about 20% of the instrument’s variation¹³ — consistent with unmeasured respiratory illnesses and with imperfect measurement of flu itself. Therefore, the instrument should be interpreted as capturing broader local attendance shocks, not the flu alone.

For the instrument to identify the true causal effect of absences on outcomes, the variation in $\hat{z}_{sy}$ must come from quasi-random shocks that affect outcomes only through absences. It cannot be that when absences in nearby schools are high, outcomes in the focal school are low for reasons other than absences in the focal school. Several tests of selection on observables and unobservables follow the main results.¹⁴

The first stage and reduced form for the effect of absence shocks on contemporaneous test scores are:

$$A_{scg} = \beta^{FS} \hat{z}_{s,y(c,g)} + \delta_{cg}^{FS} + \gamma_{sg}^{FS} + \Gamma_{sg}^{FS} \cdot c + \epsilon_{scg}^{FS} \quad (4)$$

$$Y_{scg} = \beta^{RF} \hat{z}_{s,y(c,g)} + \delta_{cg}^{RF} + \gamma_{sg}^{RF} + \Gamma_{sg}^{RF} \cdot c + \epsilon_{scg}^{RF}. \quad (5)$$

The leave-out measure of local shocks for school s in the corresponding year y instruments for the absence rate, A_{scg} , in school s , birth cohort c , and grade g . Test scores cover grades 3-5, so the controls interact fully with grade, which produces results analogous to averaging over grade-specific regressions. Cohort-by-grade fixed effects, δ_{cg} , and school-by-grade fixed effects, γ_{sg} , enter throughout. The test-score panel spans more than 20 years, long enough for personnel to turn over, so the baseline specification also allows a school-grade specific trend in birth cohort, $\Gamma_{sg} \cdot c$, letting teacher effects evolve over time.¹⁵ The reduced form is analogous, replacing absences with standardized test scores as the dependent variable.

Figure VII Panel A shows a strong first stage. Moving from the 5th to the 95th percentile of the residualized leave-out attendance instrument increases absences in the focal school by 0.5 days per student per year. In a cohort of 100 students, this change equals 50 student-days, or nearly 0.5 standard deviations of the elementary-school-cohort absence distribution.

Panel B shows the corresponding negative reduced form: within a school and grade, cohorts score lower when the leave-out attendance instrument is higher. Scaling the reduced form by the first stage, one additional day absent per student lowers math scores by 0.038 standard deviations (S.E. = 0.006). The 95% confidence interval ranges from -0.050 to -0.026 standard deviations, giving the main design substantially more precision than the direct two-year flu exercise.

¹³Appendix Figure A.6 also shows that spatial autocorrelation in z_{sy} across nearby schools resembles that of the flu. The figure uses z_{sy} rather than $\hat{z}_{sy}$ because aggregating over other schools in the district mechanically creates spatial correlation in $\hat{z}_{sy}$.

¹⁴Respiratory illness may also affect outcomes directly. A more formal discussion of that possibility follows the main results as well.

¹⁵Table II shows that the estimates are not sensitive to the inclusion of the trend.

The estimates survive alternative sets of controls. The baseline says that one additional day absent per student on average lowers standardized test scores by approximately 4% of a standard deviation. Table II shows that the result is not sensitive to the inclusion of the school-specific linear trend in cohort, and that removing it changes the treatment effect only slightly. The table also adds school-by-cohort fixed effects, in line with the within-student design that prior work has used. That specification asks whether a given cohort scored relatively lower in third grade than in fourth grade if, during third grade, students in nearby schools missed school for exogenous reasons at higher rates than in fourth grade. The treatment effects there are similar, if somewhat smaller, than in the across-cohort specification. Either way, test scores are lower when absences induced by the instrument are higher.

Identification requires the leave-out attendance instrument to affect focal-school outcomes only through focal-school absences. The first test asks whether it covaries with observed student, teacher, or community characteristics that could affect both attendance and outcomes. Figure VIII Panel A shows no systematic relationship with the observed time-varying student and teacher characteristics, including free or reduced-price lunch eligibility and teacher experience.¹⁶ Panel B likewise shows no detectable relationship with local unemployment, one observable local shock that may affect both attendance and student outcomes (e.g. Page, Stevens and Lindo, 2009; Stevens and Schaller, 2011; Jackson, Wigger and Xiong, 2021).¹⁷ These balance tests address measured confounders. They cannot exclude any number of unobserved neighborhood shocks, such as changes in crime.

Outcome-based placebo tests in the style of Danieli et al. (2024) tighten the bounds on the unobserved shocks that threaten identification. Suppose that the true model were:

$$Y_{scg} = \beta A_{scg} + v_{sc} + \eta_{sg} + \varepsilon_{scg},$$

where outcomes, Y_{scg} , in school s for birth cohort c in grade g , were determined by the true causal effect of absences, β , fixed school-by-cohort, v_{sc} , and school-by-grade components, η_{sg} , and idiosyncratic noise, ε_{scg} . The identification concern is that the instrument is correlated with this idiosyncratic component – i.e. $\text{Cov}(\hat{z}_{sy(c,g)}, \varepsilon_{scg}) \neq 0$. Using $\hat{z}_{sy(c,g)}$ as an instrument for A_{scg} would lead to biased estimates of the true effect, β . Start with the case where the instrument is correlated with an unobserved local shock that persists across years (e.g. crime rates rise in a place this year and stay high next year also— i.e., low value of ε_{scg}). The persistence generates a correlation between test scores in year y and the value of the instrument in year $y + 1$ as the instrument in

¹⁶The share of non-white teachers, a characteristic linked to lower absence rates for students of color, shows no detectable relationship either (Holt and Gershenson, 2019; Tran and Gershenson, 2021; Gershenson et al., 2022).

¹⁷Appendix Figure A.7 shows the same pattern around the Great Recession (2008–2012), when local unemployment changed sharply.

$y + 1$ is also at least partially driven by the increase in crime— i.e. $\text{Cov}(\hat{z}_{s,y(c,g)+1}, \epsilon_{scg}) \neq 0$. If the instrument runs purely through absences in the current year, this year’s test scores should not correlate with absences in nearby schools next year.

Figure IX Panel A replaces the contemporaneous instrument, $\hat{z}_{sy}$, with its value in the next year, $\hat{z}_{s,y+1}$.¹⁸ Test scores in year y have no detectable relationship with the next year’s instrument. The result is inconsistent with an omitted local shock that drives the reduced form and persists for at least a year, but it says less about shorter-lived shocks.

Shorter shocks—a spike in local crime that fades within a few months—call for a within-year instrument, $\hat{z}_{spy}$, built separately for each of six attendance periods ($p \in [1, 6]$) by the procedure in equation 3. If a local confound like this persisted across six-week periods, its direct effect on spring test scores could remain correlated with the instrument after students took the exam. Post-exam absences themselves cannot affect an earlier score, so such a correlation would signal a possible exclusion violation.

Figure IX Panel B plots the six reduced-form coefficients. The pre-exam coefficients are negative and stable; the two post-exam coefficients are smaller, and neither differs statistically from zero. This test helps to rule out local shocks that directly impact both absences and outcomes and persist for at least six weeks.

After the placebo tests, the remaining concern is an unobserved shock that directly moves both absences and outcomes for students and yet dissipates within six weeks.¹⁹ Flu explains part of the variation in the instrument, and respiratory illness has exactly this transitory character. If such illnesses also reached students’ outcomes directly, even when students are not absent, the identifying assumption would fail. The evidence that flu is harmful for child development and has long-term consequences for labor market outcomes emphasizes exposure in utero or during infancy (Almond, 2006; Currie and Schwandt, 2013; Ward and Phipps, 2014; Schwandt, 2018; Daysal et al., 2026). The shocks here fall during adolescence, after the major period of neural and physical development in early childhood (Currie and Almond, 2011). Even so, estimating the first stage and reduced form in 10 separate samples based on prior absence rates speaks to how much of the relationship between the instrument and test scores runs through absences.

If the effect of the instrument does in fact run through absenteeism, the reduced form should be larger where the first stage is larger. If instead other effects of respiratory illnesses drove the results, the reduced form could be large regardless of whether students happen to be absent, because what matters is the spread of the illness, not the absences that follow. Appendix Figure A.8 splits school-cohorts into 10 bins based on the prior year’s absence rate, estimates the first

¹⁸In the Danieli et al. (2024) framework, $\hat{z}_{s,y+1}$ is a “negative control instrument”.

¹⁹Because the instrument is a leave-out measure, capturing variation in attendance shared across nearby schools, such a shock cannot be so local that it reaches only one school in the district, as in Ang (2021). A hyper-local shock at a school near school s would raise $\hat{z}_{sy}$ without raising absences in s , so there would be no first stage and no bias.

stage and reduced form separately in each, and plots the coefficients with the line of best fit.²⁰ There is variation in the first stage across the bins, and on average, where the first stage is stronger the reduced form is larger in magnitude. The pattern is broadly consistent with the effect of the instrument running through absenteeism.

A related concern is that the instrument captures teacher absences in addition to student absences. Respiratory illness may spread to teachers as well, and a class taught by a substitute may learn less than one taught by its regular teacher. Two considerations weigh against that explanation. First, the mechanism is different in kind. Goodman (2014) finds that individual absences lower achievement while school closures do not, because a closure moves an entire school at once and teachers adjust accordingly. It is easy to imagine that a substitute teacher works the same way: the whole room is affected, so no student falls out of step with her classmates. Second, the ERC records do not measure teacher absences directly, but they do report district spending on substitute teachers. On average, districts whose students miss more school spend more on substitutes. The leave-out attendance shock, however, is not correlated with that spending, which suggests limited scope for the instrument to move teacher attendance (see Appendix Figure A.9). Taken together, teacher absences do not seem a likely explanation for the results.

With the design validated in the short run, the question is whether absences matter later in life or whether they only matter for test scores and then dissipate. It is possible that the test score effects are driven by changes in test-specific human capital, where absences create disruptions that prevent students from learning how to answer test questions, but this form of human capital is not meaningful for later-in-life outcomes. On the other hand, higher absenteeism could genuinely hinder the development of meaningful human capital, and the deficit may compound over time. For instance, high absenteeism could lead teachers to reduce their focus on cultivating non-cognitive skills, which have been shown to be critical for students' future success (e.g. Deming, 2009; Chetty et al., 2011; Chetty, Friedman and Rockoff, 2014; Jackson, 2018). To test for long-run effects, I adapt the empirical strategy to measure exposure to absence shocks between grades 1 and 9.

B.2 Effects on Long-Run Outcomes

To estimate the effect of absences on long-run outcomes, the single-year instrument is aggregated between first and ninth grade to capture exposure to local illness shocks over a student's time in school. The measure stops in ninth grade, before students can choose to leave school or drop out.

²⁰The bins amount to using last year's absence rates as an instrument for the first stage, rather than endogenously cutting on places where the observed first stage is higher versus lower. Cutting endogenously would risk an unobservable difference that drives the first stage and also correlates with heterogeneity in the LATE.

Concretely, for each student i ,

$$\hat{Z}_i = \sum_{g=1}^9 \hat{z}_{s(i,g)y(i,g)},$$

which is the sum of the corresponding single-year instruments for the correct set of schools and years that student i attends for grades 1 through 9.²¹ Students are linked back to where they began elementary school in kindergarten, and exposure is averaged within that school-cohort:

$$\hat{Z}_{sc} = \frac{1}{N_{sc}} \sum_{i \text{ such that } s(i)=s \text{ and } c(i)=c} \hat{Z}_i.$$

Concretely, $\hat{Z}_{sc}$ is high if cohort c in school s happened to be in grades 1-9 in a time and place where other students in nearby schools missed school at high rates for exogenous reasons. By chance, some students will have been exposed to higher rates of local illness compared to other students, even conditional on attending the same school, based on how much spread there was in the particular set of years during which they were in school.

With the exposure instrument in hand, the first stage and reduced form mirror the test-score models:

$$A_{sc} = \beta^{FS} \hat{Z}_{sc} + \delta_c^{FS} + \gamma_s^{FS} + \epsilon_{sc}^{FS} \quad (6)$$

$$Y_{sc} = \beta^{RF} \hat{Z}_{sc} + \delta_c^{RF} + \gamma_s^{RF} + \epsilon_{sc}^{RF}. \quad (7)$$

There is no g subscript here, because grades are aggregated. Cohort, δ_c , and school, γ_s , fixed effects replace their grade-specific counterparts, with $s(i)$ the school where student i attended kindergarten.

Figure X Panel A shows that cumulative exposure to local attendance shocks raises absences in the focal school between grades 1 and 9. Moving from the 5th to the 95th percentile of the instrument increases absences by approximately 1.5 days per student, or 150 student-days in a cohort of 100. The remaining panels show how this increase affects long-run outcomes.

Increases in school-cohort absences between grades 1 and 9 lower high school graduation rates. Panel B of Figure X shows a binned scatter plot of the reduced form relationship between high school graduation rates and exogenous variation in absences in nearby schools. There is a clear, negative relationship; within a given school, cohorts exposed to higher local absence shocks graduate at lower rates. The panel also reports the IV coefficient, scaled to one additional

²¹The measure lets students move across schools between grades 1 and 9, rather than fixing them to the path implied by where they first enroll.

day absent per student over grades 1-9. One such day lowers high school graduation rates by 0.26 percentage points. This is equivalent to approximately 0.04 standard deviations in the school-cohort distribution of high school graduation rates.

Absences impact students after leaving high school and lead to lower levels of enrollment in four-year colleges. Panel [C](#) shows a binned scatter plot of the reduced form relationship between college attendance and the instrument. The indicator equals one for any student who ever enrolls in a four-year college between ages 18 and 22, using THECB data for in-state enrollment and the National Student Clearinghouse for schools outside Texas. As with graduation, exposure to local absence shocks during elementary and secondary school lowers the probability of attending a four-year college. The IV estimate implies that an increase in absence rates by one day per student on average, lowers the probability that students attend a four-year school by nearly 1 percentage point, relative to a baseline rate of 30%.

In addition to reduced educational attainment, exposure to higher levels of local absence shocks lowers earnings at age 25. Panel [D](#) shows the reduced form relationship between earnings and the instrument. The IV estimate implies that an increase of 1 absence per student on average lowers mean earnings in the cohort by approximately \$400. However, the effects are somewhat noisy; the 95% confidence interval spans from a decrease of \$68 to a decrease of \$734.^{[22](#)}

The earnings effects are not driven by attrition from the unemployment insurance records. The earnings data cover only people who work in Texas, so absences might cause attrition—by pushing people out of the state, for instance. In practice, the data do not support this concern. Appendix Figure [A.10](#) Panel [A](#) regresses the fraction of the cohort ever appearing in the UI records on the instrument, and the slope is indistinguishable from zero. The probability of being in the data is the same, but average earnings at age 25 are lower for cohorts exposed to higher rates of absences in nearby schools when they were in school.

Delayed college entry does not explain the earnings decline either. Students exposed to more absence shocks are less likely to attend a four-year college between ages 18 and 22. One possibility is that they simply enroll later. Appendix Figure [A.10](#) Panel [B](#) shows that this is not the case. There is no effect on four-year college attendance between ages 23 and 25. The fraction of students who ever attend a four-year college (up to age 25) decreases as a result of increased absences.

The long-run treatment effects are not sensitive to time-varying controls for school and community characteristics. Aggregating the covariates from Figure [VIII](#) over grades 1-9—the average fraction of male teachers a student met between grades 1 and 9, say, or the average local unemployment rate in the city over those years—gives a set of controls. Table [A.1](#) shows that the

²²A permutation test gives a further read on statistical significance (Appendix Figure [A.11](#)). Reshuffling the $\hat{Z}_{sc}$ instruments at random across schools and cohorts and recomputing the IV earnings effect 10,000 times builds a nonparametric distribution of placebo effects under the null of no causal relationship. The placebo distribution centers near zero and the true IV estimate falls in its tail, consistent with the p-value from the baseline regression.

IV estimates are essentially unchanged with them.

Consider a cohort of 100 students whose cumulative absences between grades 1–9 fall by 10 days per student, a move from the 75th to the 25th percentile across school cohorts. The change amounts to slightly more than one additional day of attendance per student per grade. Mechanically scaling the IV estimates implies a 2.6 pp increase in high school graduation (S.E. = 0.6 pp), or approximately three additional graduates, and \$4,000 more in average earnings at age 25 (S.E. = \$1,700). This calculation is an extrapolation beyond the variation generated by the instrument, which raised absences by about 1.5 days per student. However, it provides one way to quantify the importance of absences for long-run outcomes. Importantly, these school-cohort estimates combine the direct effects on students who miss school with any externalities their absences impose on classmates who attend, which the next section explores.

V Evidence of Peer Externalities

Even small changes in school-cohort absences affect outcomes into adulthood. The group-level estimand combines the direct effect on students who miss school with externalities imposed on classmates who attend; it cannot separate the two channels. Figure XI offers an initial calibration by comparing the IV estimates with the individual- and school-cohort-level OLS relationships from Section III. For both test scores and high school graduation, the school-cohort IV estimate falls below the school-cohort OLS estimate but above the individual-level OLS estimate. The ordering is consistent with negative peer externalities, but it is not evidence of them: the estimates use different sources of variation and face different forms of selection.

This section tests for peer spillovers using a comparison of classrooms where the leave-out attendance instrument produces similar changes in a focal student's own attendance but different changes in her classmates' attendance. If her test score responds more when her classmates' attendance changes more, the pattern suggests a peer externality. To make these comparisons, I use middle school math class assignments from 2012 through 2019. Middle schools provide more classes per birth cohort than elementary schools. For each student-year, class rosters identify a specific math class, teacher, and period.²³ Three measures follow for each student-year: her prior-year absences, her classmates' mean prior-year absences excluding her own, and her classmates' mean prior-year standardized math scores.

Classmates' pre-existing absence propensity is their mean lagged absences, residualized on their mean lagged test scores and on school-by-grade and year-by-grade fixed effects. The measure therefore compares classrooms within a school grade after removing differences associated with prior peer achievement and statewide grade-by-year trends. Figure II documents why lagged

²³I identify math classes using the course names – e.g. “7th Grade Math”, “Algebra 1”, etc.

absences are informative: students who miss school frequently in one year tend to do so again the next. The same local attendance shock should therefore produce a larger peer-attendance response in classrooms with higher lagged peer absence propensity.

The first test asks whether peer absence propensity predicts the focal student's own response to the leave-out attendance instrument. If it did, differences in test-score responses could reflect differences in her own attendance rather than her peers'. I regress the focal student's attendance on her lagged absence rate, the instrument, residualized lagged peer absences, and the interaction between the instrument and the peer measure, with school-by-grade and year-by-grade fixed effects. The first pair of bars in Figure XII compares a classroom with average peer absence propensity with one a standard deviation above the mean. The focal student's attendance response is nearly identical in the two classrooms ($p = 0.427$).

Next, the outcome is replaced by the contemporaneous attendance of the focal student's classmates, excluding her own attendance. In classrooms where lagged peer absence propensity is one standard deviation above the mean, the peer-attendance response to the leave-out attendance instrument is 15.3% larger ($p = 0.008$). The instrument therefore changes peer attendance more in these classrooms even though the focal student's own attendance response is the same.

If only the focal student's own absences affected her test score, similar own-attendance first stages should produce similar reduced-form test-score responses across the two classroom types. Instead, the focal student's test-score response is larger in the classroom with higher peer absence propensity ($p = 0.097$). Classroom composition is not randomly assigned, so the exercise is not a definitive test and cannot separate direct effects or decompose the share of the effect that comes from spillovers. It does, however, yield the pattern predicted by negative peer externalities, with similar own-attendance responses but different peer-attendance and test-score responses across the two classroom types. The pattern also cautions against equating the estimates with the effect of extending the school year. The group-level estimand captures not only the effect of additional days in school, but also reduced disruption from absent classmates.

VI Conclusion

School attendance fell sharply once schools reopened after the COVID-19 pandemic, and it has not recovered. In Texas, absence rates remain roughly 30% above their pre-pandemic level. Absences rose across the distribution of prior attendance and nearly as much among students who entered school after pandemic-era closures as among students who experienced them. The pattern is broad and unlikely to disappear as exposed cohorts age out of school.

Efforts to reverse this increase are particularly important because group-level absences have lasting effects. A leave-out attendance instrument built from pre-pandemic local attendance shocks

shows that higher school-cohort absences lower test scores, high school graduation, four-year college attendance, and earnings at age 25. Ten fewer days absent per student between grades 1–9—a move from the 75th to the 25th percentile of the school-cohort distribution—would raise high school graduation by 2.6 percentage points and average earnings at age 25 by about \$4,000. These findings suggest that reduced attendance is not merely a symptom of post-pandemic disruption, but may itself contribute to persistently weaker student outcomes. Schools’ investments in tutoring, summer learning, and other recovery efforts are therefore warranted, and restoring regular attendance should remain part of those efforts (Carbonari et al., 2022; Dewey et al., 2024).

In 2022, Texas students missed 6.1 more days than the pre-pandemic average; mechanically applying the IV estimate to that gap gives a 1.6 percentage point reduction in high school graduation (Figure XIII, Panel A). Under current attendance trajectories, students who entered kindergarten after the pandemic will accumulate roughly 23 additional absences by sixth grade, corresponding to a 6 percentage point reduction at the same rate (Panel B). These calculations are extrapolations rather than forecasts: the post-pandemic increase lies outside the support of the instrument, and ongoing recovery efforts have been deployed specifically to offset these types of long-run effects. Nevertheless, the estimates show why restoring regular attendance belongs alongside other recovery efforts. Because absences impose externalities on the whole cohort, reducing them even among students who are not chronically absent can benefit their classmates too. Recovery efforts should therefore extend beyond students who cross the chronic-absence threshold. Restoring attendance is an investment in the shared learning environment of the entire cohort.

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TABLE I: Summary Statistics

	Mean	Standard Deviation
<i>A. Student Characteristics</i>		
Fraction of Students with Dropout Risk	0.45	0.22
Fraction of Students FRPL eligible	0.50	0.29
Fraction of Students Hispanic	0.49	0.31
Fraction of Students White	0.34	0.28
Number of Students in Cohort	99	35
<i>B. Teacher Characteristics</i>		
Fraction Teachers w/ Adv. Degrees	0.21	0.10
Fraction Teachers Male	0.16	0.12
Fraction Teachers Racial Minority	0.35	0.29
Years of Teacher Experience	10.97	2.60
Years Teacher Tenure	7.65	2.55
Teacher Annual Pay (2015 Dollars)	50900	4101
<i>C. Absence Rates</i>		
Days Absent per Student (single grade 3-5)	5.1	1.2
Days Absent per Student Grades 1-9	49.5	7.7
<i>D. Long-Run Outcomes</i>		
Graduation Rate	0.88	0.06
4-Year College Attendance 18-22	0.30	0.11
Earnings at Age 25 (2015 Dollars)	26,400	6,600
Number of Unique Students	7,500,000	
Number of School-Cohort Cells	29,419	
Number of School-Cohort-Grade Cells	83,250	

Notes: This table presents a set of summary statistics for the sample. Panel A presents statistics on student characteristics, measured annually from 1995 to 2019. Panel B presents statistics on teacher characteristics, measured annually from 1995 to 2019. Panel C presents the baseline definition of absence rates. The short-run analysis uses single-year absence rates measured between grades 3 and 5 in the years 1997-2019, the years and grades that students take standardized math exams. The long-run analysis uses the total number of absences between grades 1 and 9. Panel D provides statistics on the long-run outcomes of interest. See Section II for additional details on their construction. Finally, the table presents the total number of unique students in the analysis sample (rounded to the nearest 500,000), the number of school-cohort cells (the unit of analysis in the long-run analysis), and the number of school-cohort-grade cells, where grade is between 3 and 5 (the unit of analysis in the short-run analysis).

TABLE II: Effect of Cohort-Level Absences on Test Scores

	Baseline Estimates		Alternative Controls	
	First Stage (1)	IV (2)	IV (3)	IV (4)
Absences in Grade	0.145 (0.003)	-0.038 (0.006)	-0.050 (0.006)	-0.023 (0.007)
Observations (School-Cohort-Grade Cells)	82345	82345	82345	72714
Allow for Trend in Cohort	Yes	Yes	No	No
School-Cohort FEs	No	No	No	Yes

Notes: This table presents a series of coefficients on the effect of absences in a given year on standardized test scores in that same year. In each case, the instrument is the leave-out measure $\hat{z}_{sy}$, which captures blips in attendance in nearby schools (same district), in year y . Column (1) shows the first stage, detailed in Equation 4, which is scaled to show the effect of a one standard deviation increase in the instrument on the average days absent per student in the focal school. For a graphical representation of this relationship, see Figure VII Panel A. Column (2) presents the IV scaling where the dependent variable is the average standardized test score (in standard deviation units) and the endogenous variable is the average days absent per student in the grade. The coefficient can be interpreted as the effect of a 1 day per student increase in absences. For a graphical representation of the underlying reduced form relationship, see Figure VII Panel B. Columns (1) and (2) contain school-by-grade and cohort-by-grade fixed effects as well allow for school-by-grade specific linear trend in birth cohort. Section IV contains a more detailed discussion of the controls. Columns (3) and (4) provide estimates using an alternate set of controls. Column (3) removes the school-by-grade specific trend in cohort. Column (4) removes the trend and adds a school-by-cohort fixed effect, essentially comparing the effect of the instrument across grades while holding fixed the set of students. In all columns, standard errors are clustered at the school-by-cohort level.

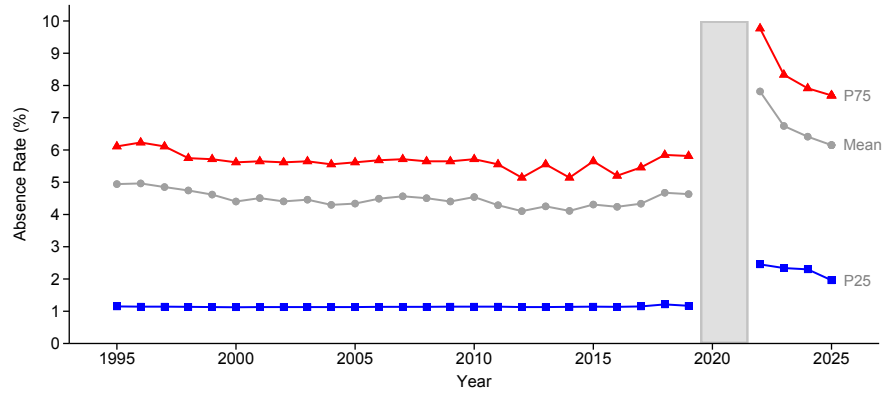
TABLE III: Effect of Cohort-Level Absences on Long-Run Outcomes

	First Stage (1)	IV Estimates		
		HS Graduation (2)	4-Yr. College Enroll. (3)	Earnings Age 25 (4)
Total Absences Grade 1-9	0.416 (0.038)	-0.261 (0.060)	-0.978 (0.181)	-401.060 (169.995)
Observations (School-Cohort Cells)	29419	29419	12407	12396
School and Cohort FE's	Yes	Yes	Yes	Yes

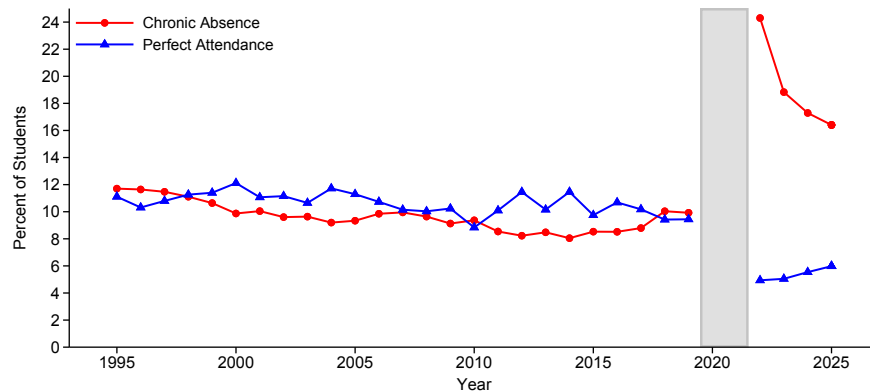
Notes: This table presents a series of coefficients on the effect of absences between grades 1 and 9 on later-in-life outcomes. In each case, the instrument is the leave-out measure, $\hat{Z}_{sc}$, which captures blips in attendance over those same years in nearby schools (same district). Column (1) shows the first stage, estimated using Equation 6, which is scaled to show the effect of a 1 standard deviation increase in the instrument on average total days absent per student in the focal school, between grades 1 and 9. For a graphical representation of this relationship, see Figure X Panel A. Columns (2)-(4) present the IV effect of a 1 day per student increase in the average total days absent between grades 1 and 9 on high school graduation, enrollment in a 4-year college between ages 18 and 22, and earnings at age 25. These relationships are depicted graphically in Panels B-D of Figure X. Columns (1) and (2) are estimated using students born between 1990 and 2000, whereas Columns (3) and (4) are estimated using students born between 1990 and 1994. Each specification includes school (the school where students attended kindergarten) and birth cohort fixed effects, with standard errors clustered at the school-by-cohort level.

FIGURE I: Change in Attendance After the Pandemic

A: Distribution of Student Absences by Year

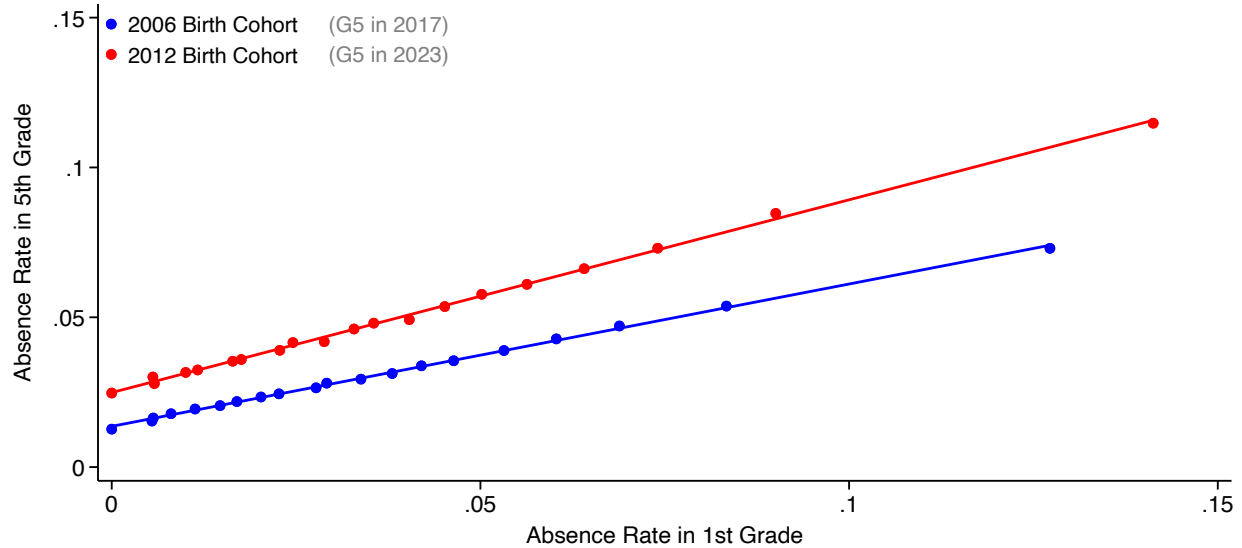


B: Tails of the Absence Distribution by Year



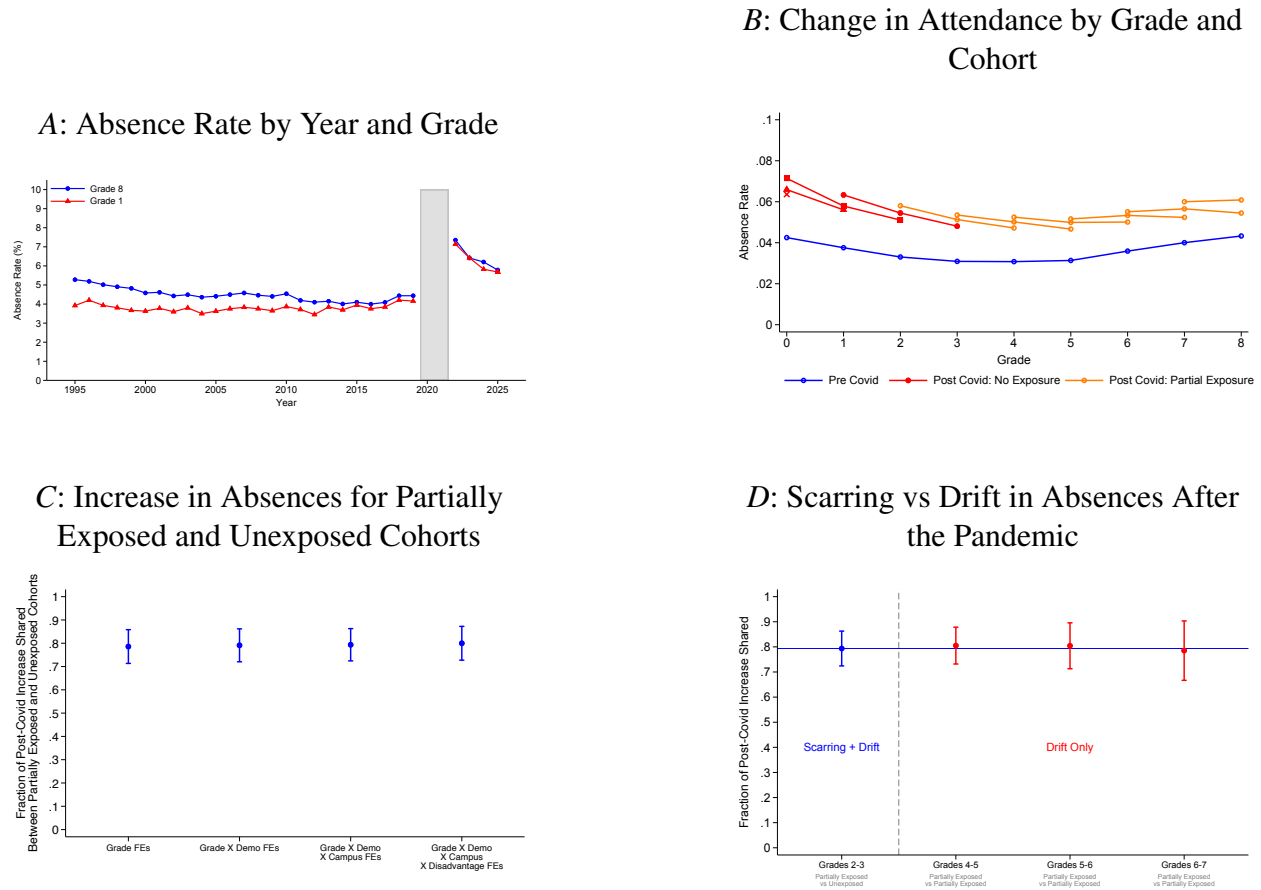
Notes: This figure documents the evolution of student absence rates in Texas public schools between 1995 and 2025. Absences are measured as the share of the school year that a student misses, constructed from the days absent recorded over the six, six-week reporting intervals in each school year. Panel *A* plots the mean of the student-level absence rate in each year, in grey circles, together with the 25th percentile, in blue squares, and the 75th percentile, in red triangles. Panel *B* plots the tails of the same distribution: the share of students who are chronically absent, defined as missing more than 10% of the school year, in red circles, and the share of students with perfect attendance, defined as missing no days of school, in blue triangles. The shaded region in both panels covers the 2020 and 2021 school years, which are omitted throughout because pandemic-era school closures and the shift to remote instruction make recorded attendance in those years incomparable to attendance in the surrounding years. Years are labeled by the spring of the school year, so that 2022 denotes the 2021–22 school year, the first year in which schools had fully reopened. The sample includes all students enrolled in K-12 in each year.

**FIGURE II: Within Student Correlation in Absences in Grade 1 and 5
For Cohorts Exposed or Not Exposed to the Pandemic**



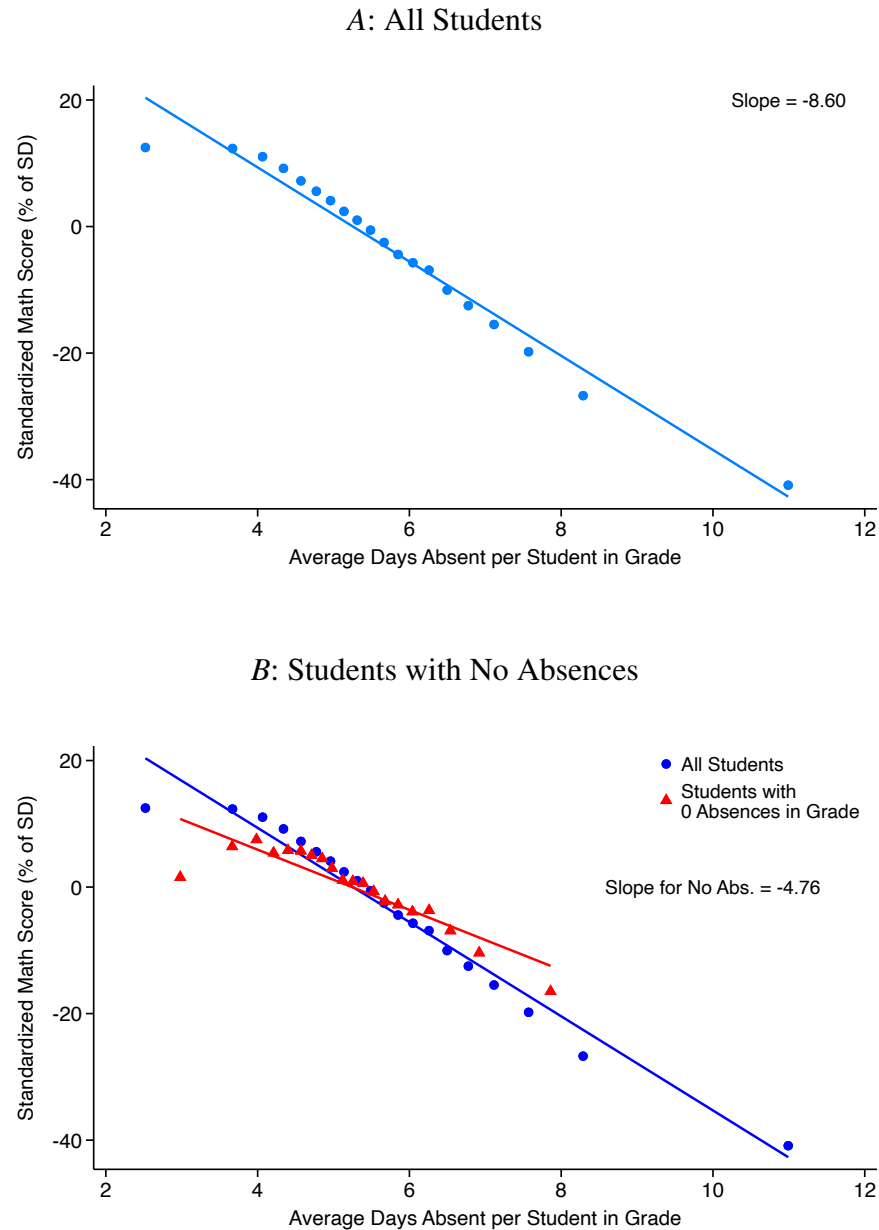
Notes: This figure compares the within-student persistence of absences across a birth cohort that was exposed to the pandemic between first and fifth grade and one that was not. It plots a binned scatter of a student's absence rate in fifth grade against her own absence rate in first grade, separately by birth cohort. The series in blue is the 2006 birth cohort, who attended first grade in the spring of 2013 and fifth grade in the spring of 2017, and was therefore not exposed to the pandemic in any of the intervening grades. The series in red is the 2012 birth cohort, who attended first grade in the spring of 2019 and fifth grade in the spring of 2023, and so was enrolled during the pandemic-era school closures between these two grades. Absence rates are measured as the share of the school year missed in the relevant grade, as in Figure I. To construct the figure, students within each cohort are grouped into 20 equally sized bins of their own first grade absence rate, and the mean fifth grade absence rate within each bin is plotted against the mean first grade absence rate in that bin. The figure also plots the line of best fit from a regression on the binned points.

FIGURE III: Increase in Absences for Cohorts Entering School After the Pandemic



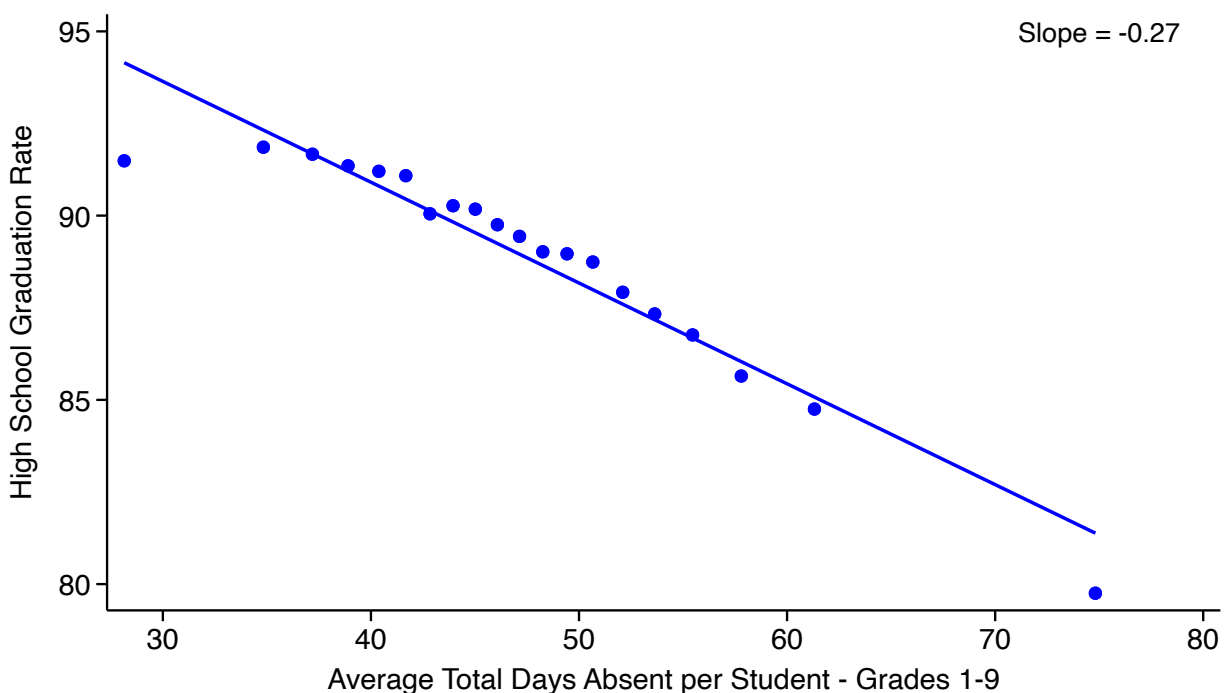
Notes: This figure asks whether the post-pandemic increase in absences reflects scarring among cohorts enrolled during school closures, or a broader shift in absenteeism that also affects cohorts entering school afterwards. Panel *A* plots the mean absence rate by year for first graders, in red triangles, and eighth graders, in blue circles; the shaded region covers the omitted 2020 and 2021 school years, as in Figure *I*. Panel *B* plots mean absence rates by grade for three groups of birth cohorts: cohorts observed entirely before the pandemic, in blue (1991-2001 birth cohorts); post-pandemic cohorts who had not yet entered school when schools closed, in solid red (2017-2020 birth cohorts where the different shapes indicate different cohorts); and post-pandemic cohorts who were enrolled during the closures, in orange (2011-2016 birth cohorts) where each connected series represents a different cohort). In each case, data are excluded from 2020, 2021 and 2022. Panels *C* and *D* report the share of the increase common to both post-pandemic groups. The student-level absence rate is regressed on a post-pandemic indicator, its interaction with a never-exposed indicator, and the fixed effects listed on the horizontal axis, and the figure plots the ratio of the never-exposed to the partially exposed elevation with the 95% confidence interval, estimated using the delta method. Demographic fixed effects include sex by race/ethnicity fixed effects and disadvantage fixed effects include interaction with FRPL status and an indicator for being at risk of dropping out. The specifications are estimated on grades 2 and 3 only, the only grades in which both post-pandemic groups appear. Panel *D* benchmarks the ratio against common drift after 2022: the blue point is the Panel *C* estimate using grade-by-demographic group-by campus fixed effects; the red points are placebos that shift the grade range up and the cohorts back, holding the calendar years compared fixed while neither group crosses the exposure threshold. In the grades 4-5 comparison the post-period cohorts are 2015 and 2016, for grades 5-6 they are 2014 and 2015, and for grades 6-7 they are 2013 and 2014. Standard errors are clustered at the birth-cohort level.

FIGURE IV: OLS Relationship Between Test Scores and School-Cohort Absences



Notes: This figure presents binned scatter plots for the relationship between a student's test scores and the average absence rate of her peers in the same school-cohort. Test scores are measured in grades 3-5 between 1997 and 2019 and absences are measured contemporaneously in the same year. Both panels contain controls for birth-cohort and grade fixed effects. To construct the figure in Panel *A*, test scores and absence rates are residualized on the controls and grouped into 20 equally sized bins of the residualized absence rates following the procedure described in Figure *I*. The slope reported on the panel is the slope from a regression of test scores on cohort-level absence rates in the microdata, controlling for year and grade fixed effects. Panel *B* repeats the exercise, restricting to students who miss 0 days of school in a given grade. Figure *A.3* repeats a similar exercise, controlling for quartiles of own-student absence rates in the background rather than restricting to students who are never absent.

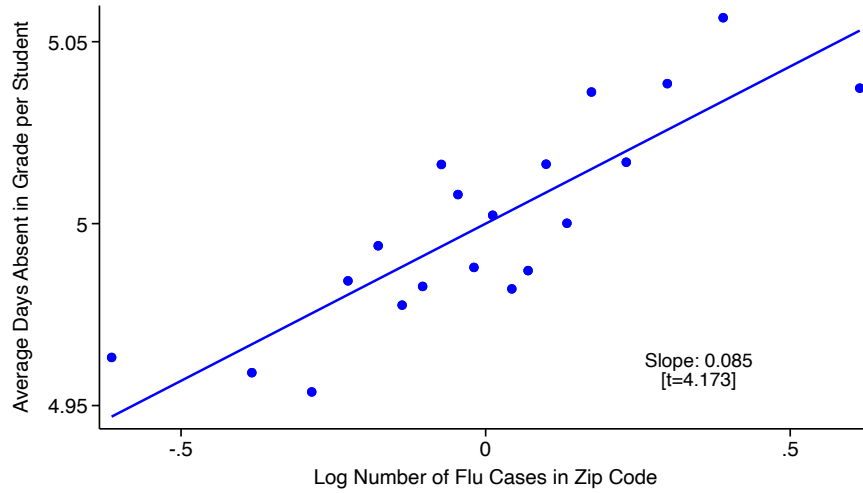
FIGURE V: OLS Relationship Between High School Graduation Rates and School-Cohort Absences



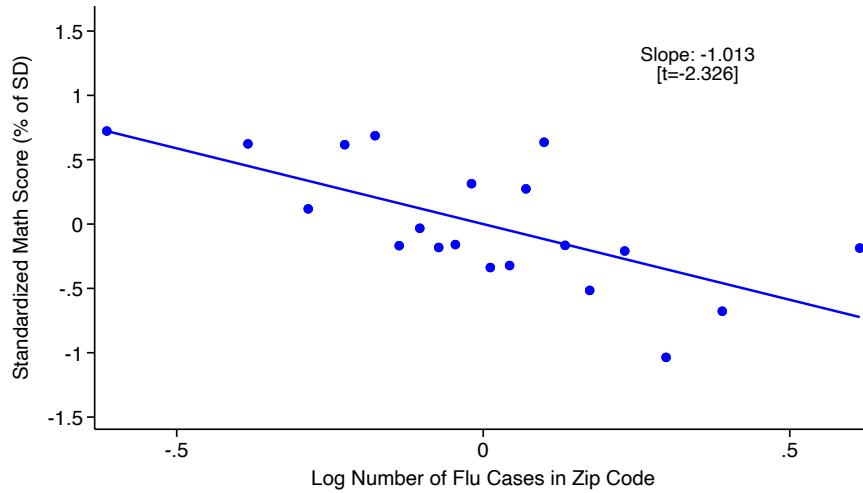
Notes: This figure presents a binned scatter plot of the relationship between high school graduation rates and school-cohort level absence rates. Absence rates are the average number of days missed per student between grades 1 and 9 and students are assigned to the school where they attend kindergarten. The figure contains controls for birth-cohort fixed effects. The figure is estimated using students in the 1990-2000 birth cohorts. See the notes to Figure IV for additional details on the construction procedure.

FIGURE VI: Effect of the Flu on Absences and Test Scores

A: First Stage



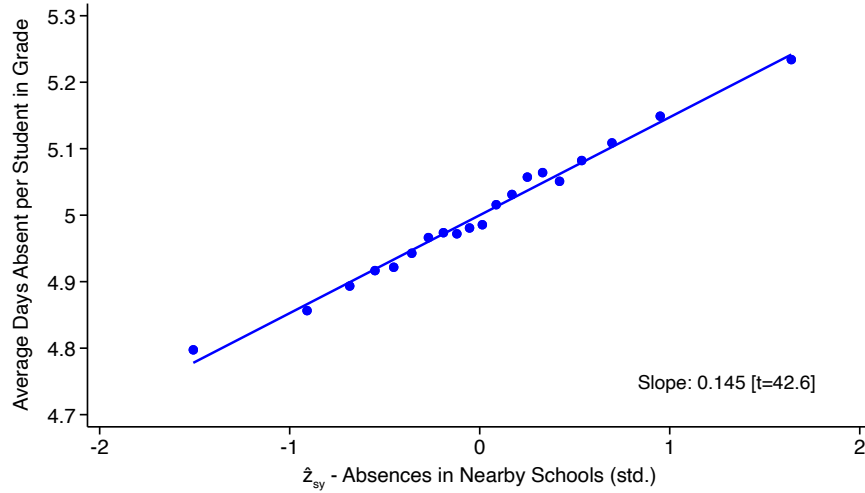
B: Reduced Form



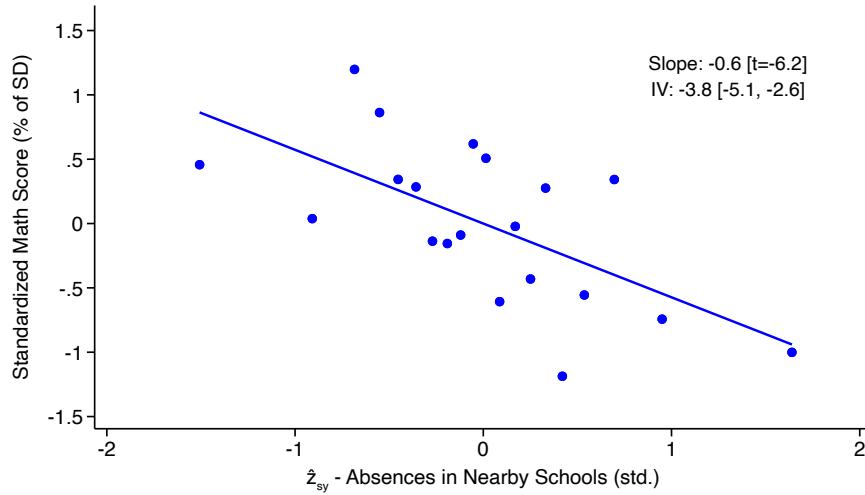
Notes: This figure presents binned scatter plots of the first stage and reduced form relationships between the spread of the flu and absences and test scores, respectively. The binned scatter plots are constructed as in Figure IV; see figure notes for details. Panel A is estimated using Equation 1 and contains school-by-grade and year-by-grade fixed effects. The coefficient from Equation 1 is shown. Panel B is estimated analogously using Equation 2. Both panels use data from 2018 and 2019. The sample is restricted to schools in ZIP codes with 5 or more flu cases in the data. Standard errors are clustered at the school-by-cohort level.

FIGURE VII: First Stage and Reduced Form Effects on Test Scores

A: First Stage



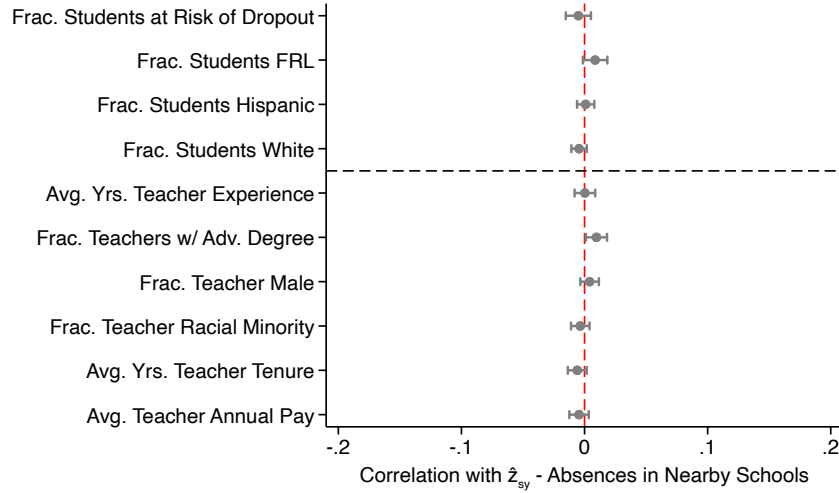
B: Reduced Form



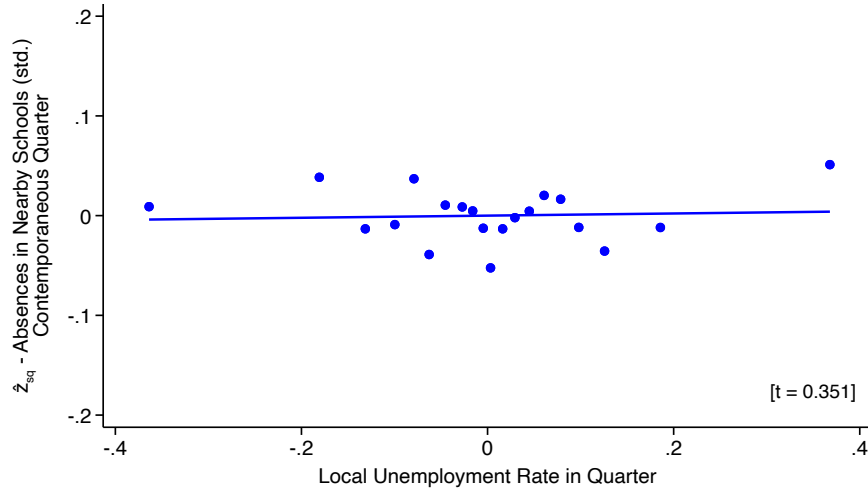
Notes: This figure presents binned scatter plots of the first stage and reduced form relationships between local shocks to attendance in nearby schools and absences and test scores in the focal school, respectively. Standardized tests are taken in grades 3-5 between 1997 and 2019. In both panels, the instrument, $\hat{z}_{sy}$, is shown in standard deviation units. The binned scatter plots are constructed as in Figure IV; see figure notes for details. Panel A is estimated using Equation 4 and the coefficient from that specification is shown. Panel B is estimated using Equation 5. The reduced form slope is shown as is the IV rescaling, using the first stage from Panel A, as well as the 95% confidence interval for the IV coefficient. Both panels contain controls for school-by-grade and cohort-by-grade fixed effects and allow for a school-by-grade specific linear trend in cohort. Standard errors are clustered at the school-by-cohort level. For analogous IV estimates with different controls, see Table II.

FIGURE VIII: Balance Tests

A: Student/Teacher Characteristics



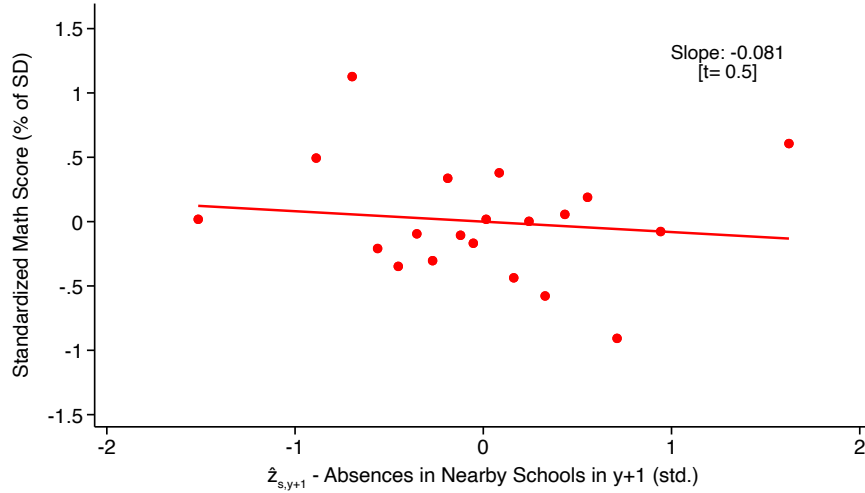
B: Local Unemployment Rates



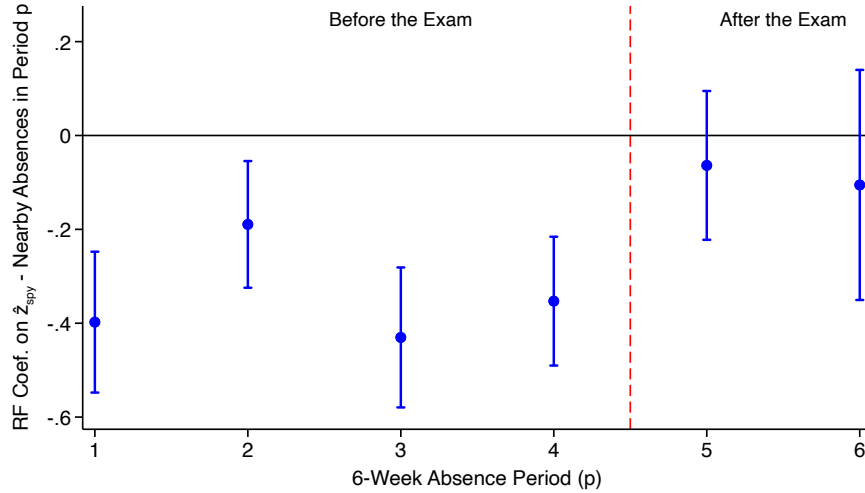
Notes: This figure presents balance tests for the single-year instrument, $\hat{z}_{sy}$, which captures shocks to attendance in nearby schools, showing correlations with characteristics of students, teachers, and local economic shocks. Panel *A* presents a series of correlation coefficients and the corresponding 95% confidence intervals between the instrument and time-varying characteristics of students and teachers. Correlations control for school and state-wide year fixed effects and are estimated using years 1995-2019. Panel *B* presents a binned scatter plot of the relationship between local unemployment rates at the city level, between 2000 and 2019 and a version of the instrument measured in each quarter, $\hat{z}_{sq}$, on the vertical axis. The specification controls for campus-by-year and year-by-quarter fixed effects. The quarterly instrument is constructed using an analogous procedure as described in Section IV, using measures of absences at six-week intervals. The binned scatter plot is constructed as in Figure IV; see figure notes for details. A similar plot estimated only using years 2008-2012, around the Great Recession is shown in Appendix Figure A.7. In both panels standard errors are clustered at the school level.

FIGURE IX: Outcome-Based Placebo Tests

A: Across Year

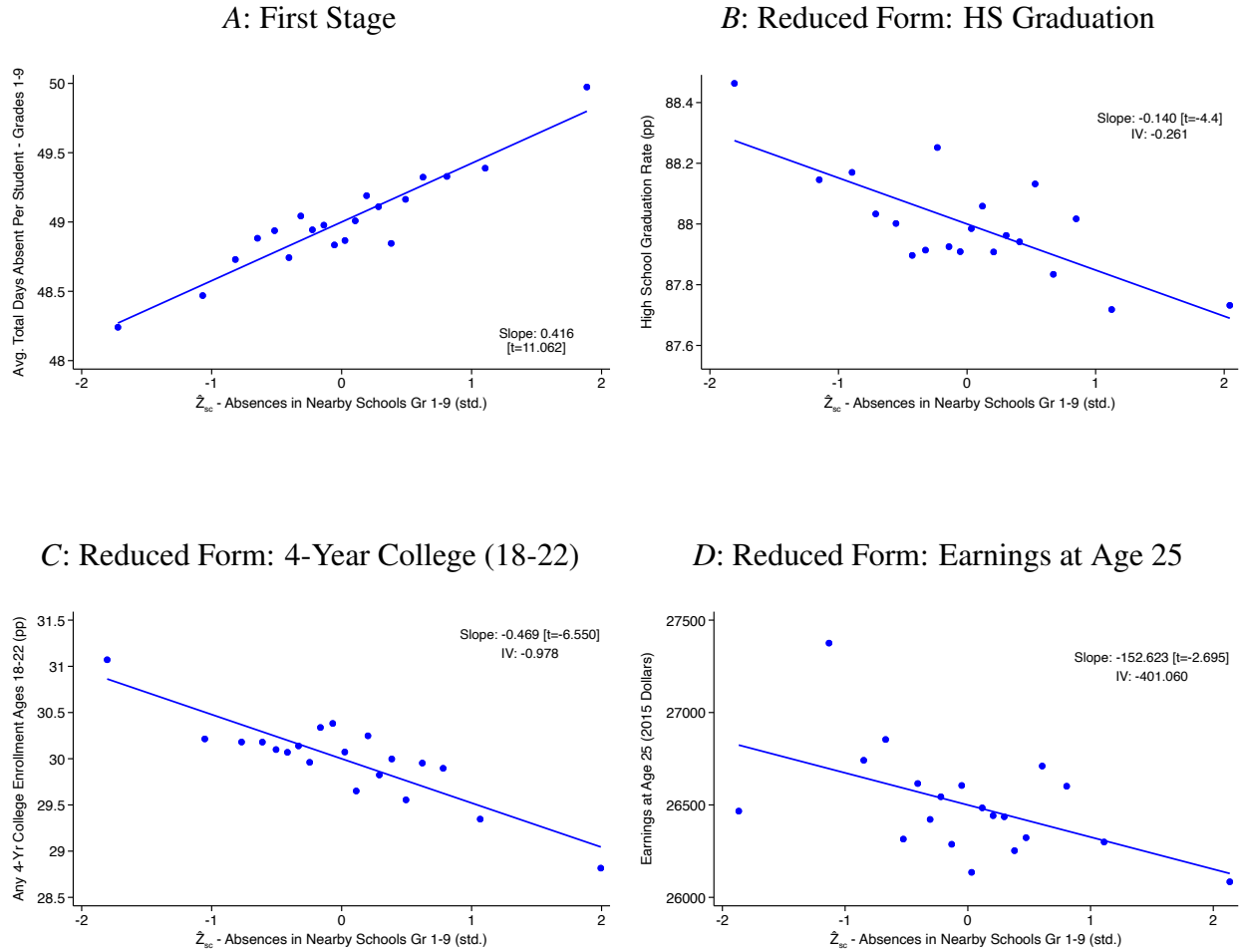


B: Within Year



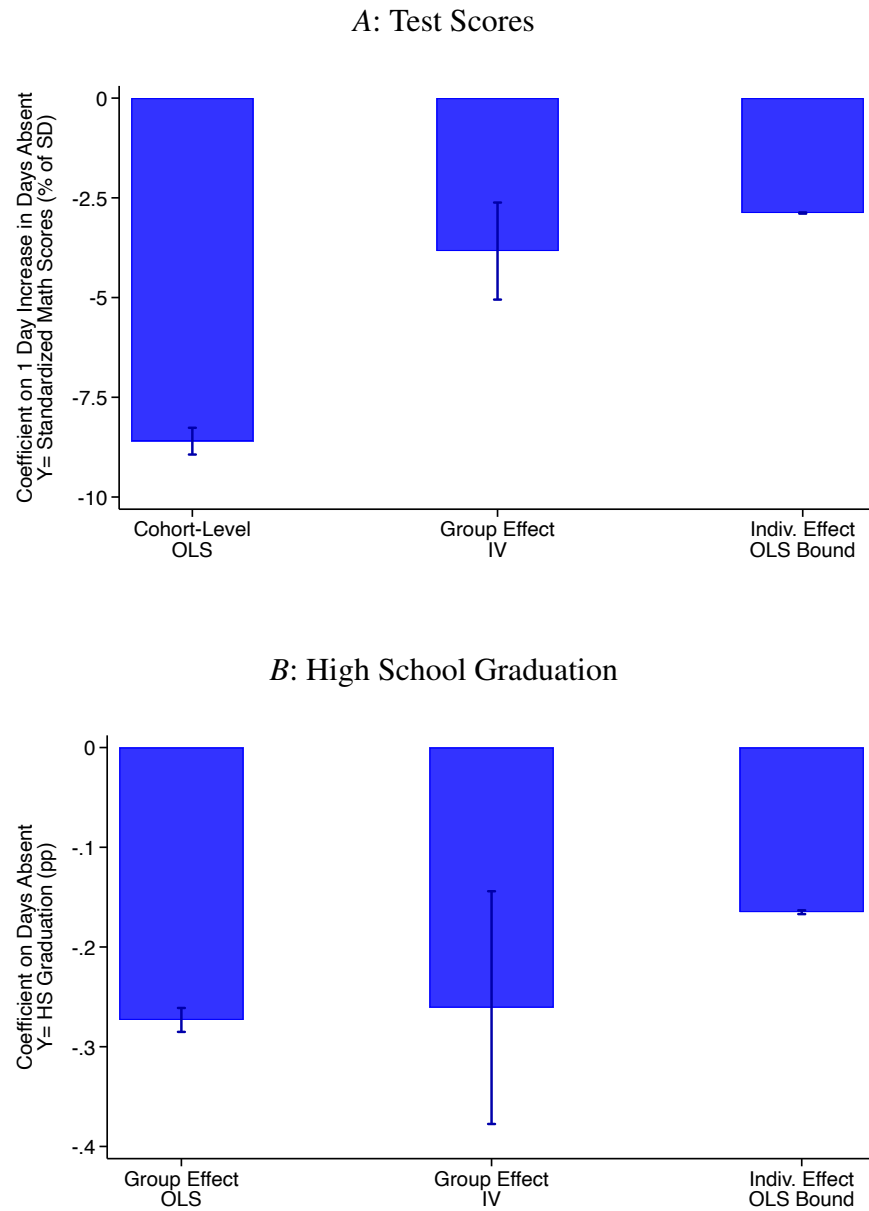
Notes: This figure presents two outcome-based placebo tests in support of the identification assumptions, described in Section IV. Panel A presents a binned scatter plot analogous to the reduced form in Figure VII Panel B, but rather than measuring the instrument contemporaneously ($\hat{z}_{sy}$), it is measured in the subsequent year ($\hat{z}_{s,y+1}$). This is a “negative control instrument” test in the style of Danieli et al. (2024). See notes to Figure VII for additional details on the construction of the binned scatter plot. Panel B presents a similar placebo exercise, but within instead of across years. Analogous instruments, $\hat{z}_{spy}$, are constructed, where p indexes the 6-week absence periods within a year from 1 to 6 using the same procedure described in Section IV. To construct the figure, a reduced form regression similar to Equation 5 is run, including all six within-year instruments. The figure plots the coefficients on each of those instruments. The dashed line in red indicates approximately the time of year when students take the exams. Periods 1-4 fall before the exam, whereas periods 5 and 6 fall during and after the exam period. In both panels, standard errors are clustered at the campus-by-cohort level.

FIGURE X: Long-Run Effects: First Stage and Reduced Form



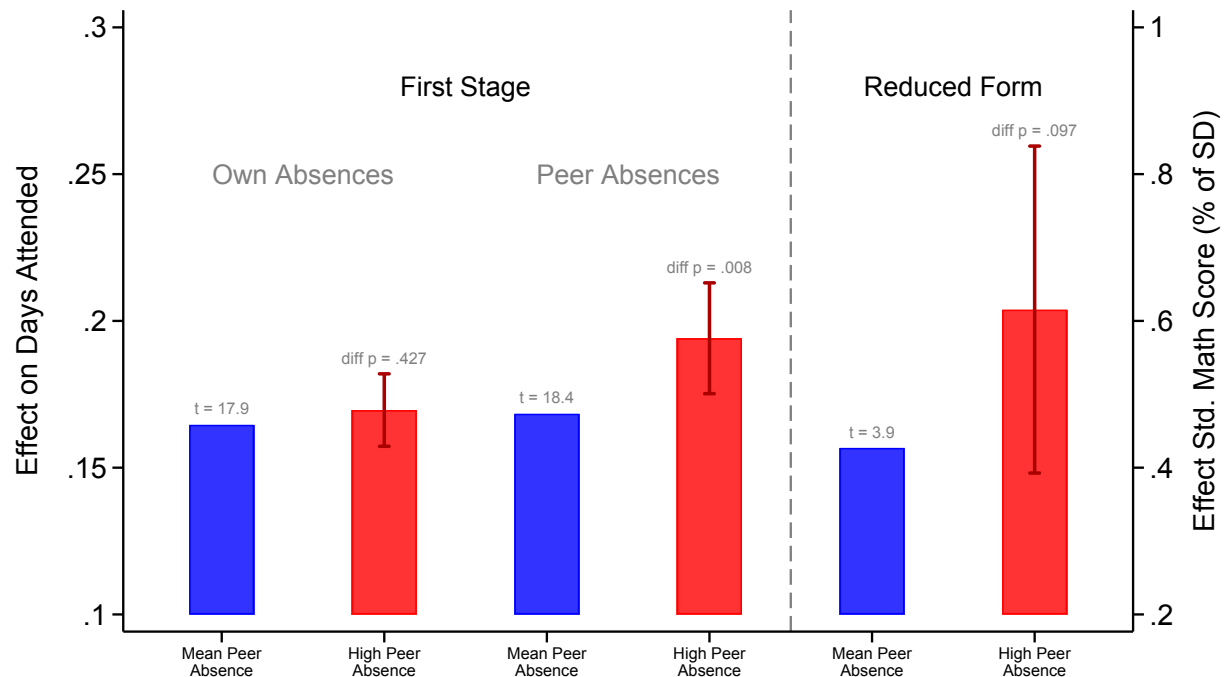
Notes: This figure presents first stage (Panel A) and reduced form (Panels B-D) relationships between absences in nearby schools and long-run outcomes in the focal school. Unlike Figure VII, where absences are measured in a single grade, here the absence rate is the total number of days absent per student between grades 1 and 9. Each plot is constructed using the same basic procedure as in Figure VI; see notes for details. In each panel, the regressions contain controls for school (the school where a student attended kindergarten) and birth cohort fixed effects. Panels A and B are estimated using the 1990-2000 birth cohorts. Panels C and D are estimated using the 1990-1994 birth cohorts. Each reduced form plot reports the slope from Equation 7 as well as the corresponding IV re-scaling. The IV coefficient is the causal effect of a one day per student increase in total absences between grades 1 and 9. In all panels, standard errors are clustered at the school-by-cohort level.

FIGURE XI: Comparison of Cohort-Level versus Individual-Level Effects of Absences



Notes: This figure presents a comparison between the effects of changes in cohort-level versus individual absences. Panel **A** presents statistics for test scores and Panel **B** presents statistics for high school graduation. In each case, the first bar shows the cohort-level OLS relationship, shown in Figures **IV** and **V**, respectively. The second bar shows the baseline IV estimates, shown in Figures **VII** and **X**, respectively. The third bar shows the individual level OLS relationship between absences and the outcome, which is a likely upper bound for the effect of an own student's absences on outcomes. In Panel **A**, absences are measured in a single year and in Panel **B** absences are total days missed between grades 1 and 9.

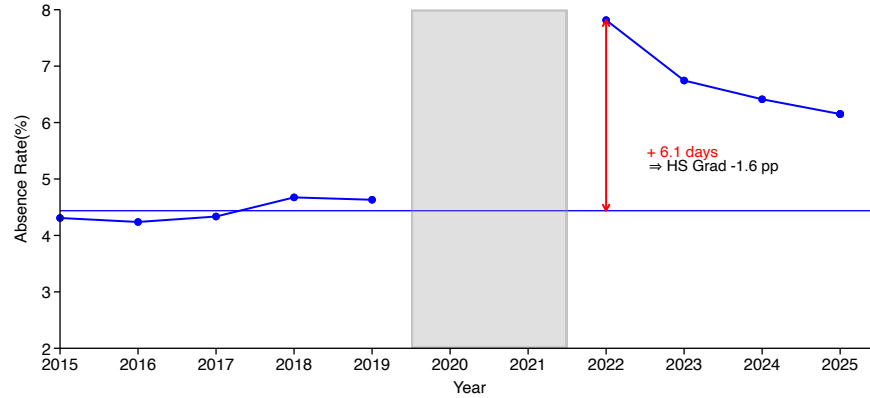
FIGURE XII: Effect of Absence Shocks in Lower vs. Higher Peer Absence Classrooms



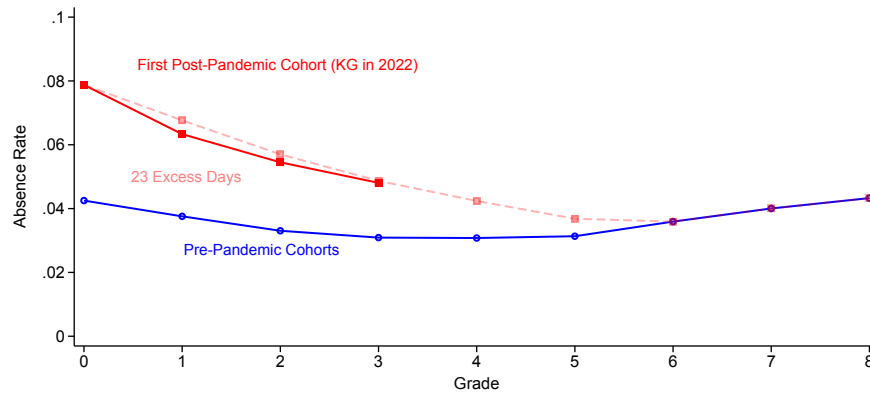
Notes: This figure tests for spillovers from peer absences by asking whether the effect of the respiratory illness instrument on a student's own test scores depends on how absence-prone her classmates are, holding fixed her own absence response to the same instrument. The three pairs of bars report, from left to right, the first stage effect of the instrument on the focal student's own absences, the first stage effect on her classmates' absences, and the reduced form effect on her own standardized math score, read against the right axis. Within each pair, the blue bar is the effect evaluated at the mean of the classroom absence measure and the red bar the effect one standard deviation above the mean, with a 95% confidence interval; the p-value above each red bar is for the difference between the two, and the t-statistic above each blue bar is for the level effect. The sample uses middle school math classroom assignments (grades 6-8) between 2012 and 2019, assigning each student to their math classroom. See Section V for details. The classroom absence measure is the residual from a regression of the leave-out mean of her classmates' absences in the prior year on their lagged test scores and school-by-grade and year-by-grade fixed effects, standardized to unit variance. Each regression controls for the focal student's own lagged absences and her classmates' lagged test scores, absorbs school-by-grade and year-by-grade fixed effects, and scales the instrument in standard deviation units. For positive values and visual simplicity, the figure reports the effects of a 1 SD decrease in the respiratory illness on days attended and standardized math scores. Standard errors are clustered at the school-by-cohort level.

FIGURE XIII: Post-Pandemic Attendance Patterns and Implications for Long-Run Outcomes

A: Absence Rate in 2022 Relative to Baseline



B: Projected Absences for Post-Pandemic Cohorts



Notes: This figure translates the post-pandemic increase in absences into implied effects on high school graduation, using the IV estimates from Table III. Panel A plots the mean absence rate by year between 2015 and 2025. The dashed line is the pre-pandemic baseline, the average absence rate over 2015–2019; the shaded region covers the omitted 2020 and 2021 school years, as in Figure I. The red arrow marks the gap between the 2022 absence rate and baseline, translated to days using a standard 180 day school year. Panel B projects the absences of the first fully post-pandemic cohort, which entered kindergarten in 2022, over grades K–8. The series in blue is the mean absence rate by grade for pre-pandemic cohorts, as in Figure III Panel B. The solid red series is that cohort’s observed absence rate in the grades observed through 2025. The dashed series is the projection, constructed by differencing the two series in each observed year, regressing that difference on year, extrapolating the fitted line forward, and setting the difference to zero wherever the fitted value would fall below zero. The projected rate in each grade is the pre-pandemic rate plus that difference, so the projection inherits the shape of the pre-pandemic grade profile while the gap converges to zero.

Supplementary Appendix

A School-by-year specific shocks and bias

As described in Section IV, the instrument does not use “blips” in attendance in the focal school (z_{sy}) directly; it uses a leave-out measure over other schools in the same district ($\hat{z}_{sy}$). The leave-out construction removes any bias from a shock in the focal school that moves both absences and outcomes. A stylized simulation builds the intuition.

Consider a simple model of absences

$$A_{scg} = \tilde{\delta}_{sc} + \tilde{\gamma}_{sg} + \varepsilon_{scg},$$

where absences, A , in school s , birth cohort c , and grade g are a function of a time-invariant school-by-cohort component, $\tilde{\delta}$, a time-invariant school-by-grade component, $\tilde{\gamma}$, and idiosyncratic noise. For simplicity, the simulation abstracts from statewide year shocks.

The residual decomposes into three terms: a district-by-year shock shared by schools in the same district, $\tilde{\rho}_{d(s),y(c,g)}$; a school-by-year shock shared across grades in the same school, $\tilde{\pi}_{s,y(c,g)}$; and remaining school-by-cohort-by-grade noise, $\tilde{\varepsilon}_{scg}$:

$$A_{scg} = \tilde{\delta}_{sc} + \tilde{\gamma}_{sg} + \tilde{\rho}_{d(s),y(c,g)} + \tilde{\pi}_{s,y(c,g)} + \tilde{\varepsilon}_{scg}. \quad (\text{A1})$$

Now, suppose that the true model of test scores is simply

$$Y_{scg} = \beta A_{scg} + \alpha(\tilde{\pi}_{s,y(c,g)}) + \varepsilon_{scg}. \quad (\text{A2})$$

Test scores are a function of the true causal effect of absences, β , as well as a direct effect, α , of the school-by-year specific shock ($\tilde{\pi}$).¹ In this scenario, using “blips” in absences in the focal school will lead to biased estimates of β .

Regressing absences on school-by-cohort and school-by-grade fixed effects gives the focal-school “blips,” z_{sy} . In this simple model, $z_{sy} = \varepsilon_{scg} = \tilde{\rho}_{d(s),y(c,g)} + \tilde{\pi}_{s,y(c,g)} + \tilde{\varepsilon}_{scg}$.

The focal-school residual, z_{sy} , violates the exclusion restriction in this model because it contains $\tilde{\pi}_{sy}$, which directly affects outcomes. To address this issue, the leave-out instrument

¹The shock need not enter directly; a component merely correlated with $\tilde{\pi}$ would do. However, direct entry is simpler.

replaces it:

$$\hat{z}_{sy} = \frac{1}{N_d - 1} \sum_{\substack{s' \neq s \\ d(s') = d(s)}} z_{s'y}.$$

Under the simulation's assumptions that $\tilde{\rho}$ and $\tilde{\pi}$ are independent and that $\tilde{\rho}$ has no direct effect on outcomes, $\hat{z}_{sy}$ is uncorrelated with the focal school's $\tilde{\pi}_{sy}$ and satisfies the exclusion restriction. The simulation has 50 districts with five schools each, three grades, and ten years. Components of Equation A1 are drawn independently from normal distributions, all terms except $\tilde{\pi}$ from $N(0, 0.5)$, with the standard deviation of $\tilde{\pi}$ varying. With $\beta = -4$ and $\alpha = -0.5$, both z_{sy} and $\hat{z}_{sy}$ instrument for A_{scg} . Appendix Figure A.12 shows the result: as the variance of $\tilde{\pi}_{sy}$ rises, estimates using z_{sy} grow more biased, while estimates using $\hat{z}_{sy}$ remain centered on β , though less precise. The conclusion is conditional. If district-by-year shocks directly affect outcomes—or correlate with shocks that do—the leave-out attendance instrument can also be biased.

B Correlated Illness Shocks Across Years

The preceding simulation lets district-by-year shocks change freely across years. What happens when local attendance shocks persist? In the example in Panel A of Appendix Figure A.13, the 1992 cohort has high absences throughout elementary school, while other cohorts also spike in 1998 and 2003. The cross-cohort pattern points to a persistent local shock rather than a fixed trait of the 1992 cohort. Persistence makes the shock partly collinear with school-by-cohort fixed effects and therefore weakens the measured local attendance shock. Under the maintained assumption that district-by-year shocks do not directly affect outcomes, the first stage can absorb this attenuation; persistence alone need not bias the IV estimate.

Persistence enters by setting $\tilde{\rho}_{d(s),y(c,g)} = \tilde{\rho}_{d(s),y(c,g)-1}$ with probability p , varied from 0 to 0.2. Regressing the residualized attendance measure on the true $\tilde{\rho}_{d(s),y(c,g)}$ gives a slope that measures attenuation. Panel B of Appendix Figure A.13 shows that the slope falls modestly as persistence rises. It is below one even when $p = 0$ because the residualized attendance measure also contains school-by-year and idiosyncratic noise. Panel C shows that the IV estimates remain centered on β as persistence rises, conditional on the same independence and exclusion assumptions used in Appendix A.

Persistence can still make local attendance shocks difficult to distinguish from school-by-cohort effects in a short panel. In the example, a five-year shock can be separated from the cohort component only because the panel also contains years before and after it. A panel spanning only the shock would absorb the episode into the school-by-cohort effect, producing a

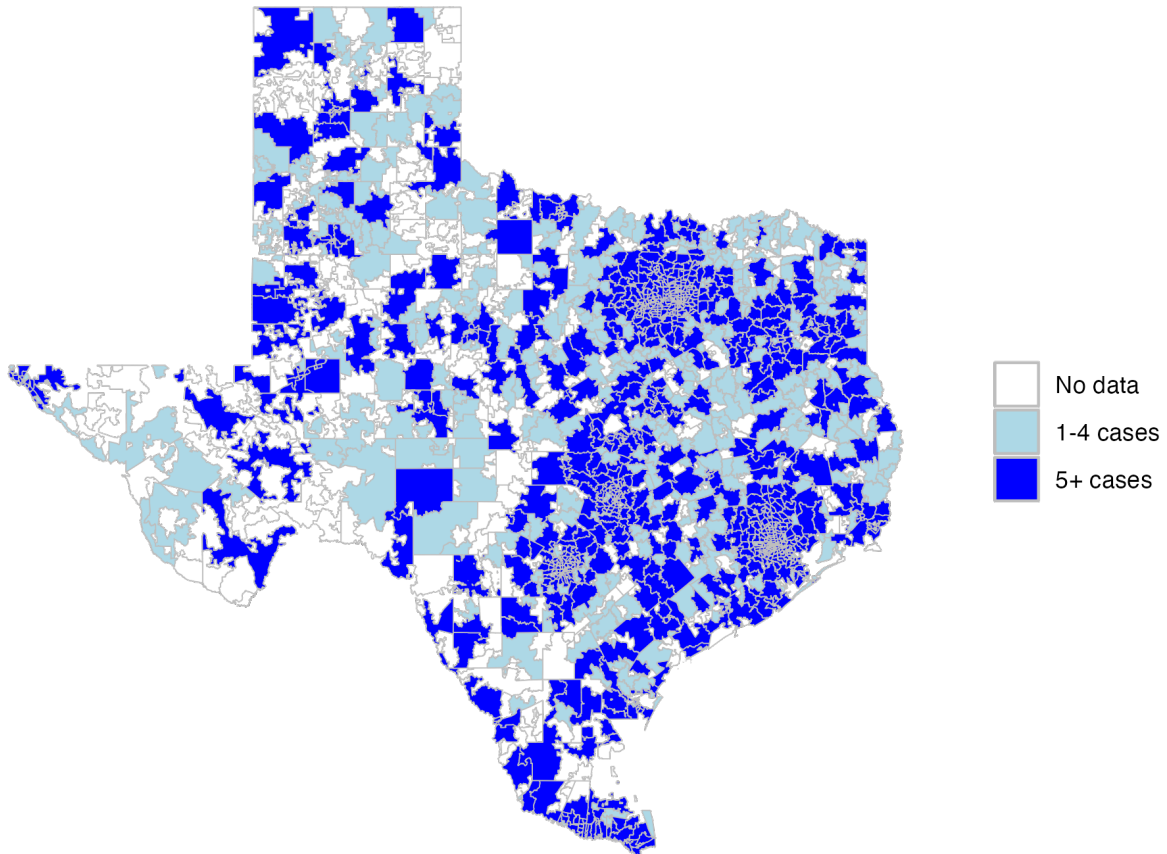
version of short-panel bias (Nickell, 1981). The 1995–2019 panel provides a long baseline on both sides of many shocks and therefore mitigates this problem, subject to the exclusion restriction.

TABLE A.1: Effect of Cohort-Level Absences on Long-Run Outcomes:
Additional Controls

	First Stage (1)	IV Estimates					
		HS Grad (2)	HS Grad (3)	4-Yr. Coll. (4)	4-Yr. Coll. (5)	Earnings Age 25 (6)	Earnings Age 25 (7)
Total Absences Grade 1-9	0.416 (0.038)	-0.261 (0.060)	-0.223 (0.057)	-0.978 (0.181)	-0.890 (0.168)	-401.060 (169.995)	-378.732 (174.591)
Observations (School-Cohort Cells)	29419	29419	29285	12407	12372	12396	12361
School and Cohort FE's	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariate Controls	No	No	Yes	No	Yes	No	Yes

Notes: This table is analogous to Table III, but also includes specifications with controls for the covariates shown in Figure VIII. Columns (1), (2), (4), (6) are identical to those in Table III; see notes for additional details. Columns (3), (5), and (7) re-estimate the IV, including controls for the average student/teacher covariates between grades 1 and 9 as well as the average local unemployment rate over that period. For example, the controls include the average experience level of teachers that students encounter between grades 1 and 9, assigning a value to each school-year and taking the average over the years corresponding to grades 1 and 9. All columns contain school and birth cohort fixed effects. Standard errors are clustered at the school-by-cohort level.

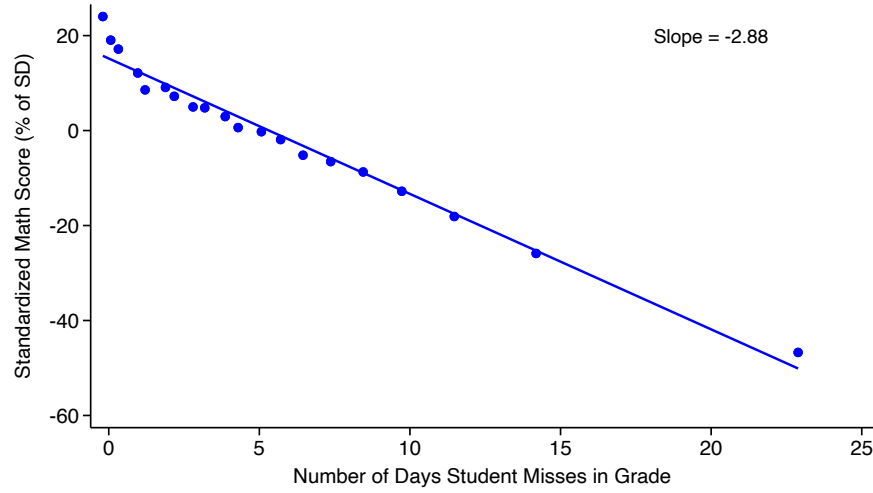
FIGURE A.1: Coverage of Flu Data: 2018-2019



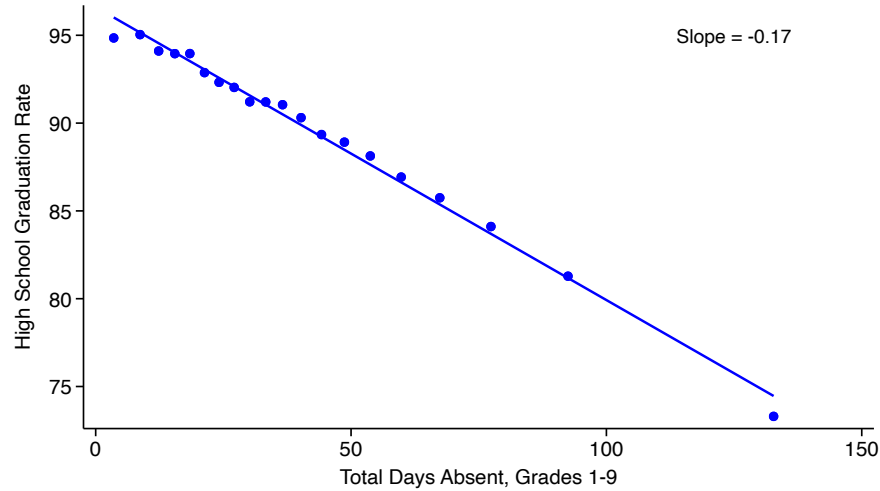
Notes: This map shows the coverage of the flu data at the ZIP code level. The data cover 2018 and 2019, collected from a Texas hospital network. See Section II for additional details. The map classifies ZIP codes into three groups: those with no data, those with only between 1 and 4 reported cases over the sample period, and those with 5+ cases. Figure VI estimates the effect of the flu on absences and outcomes, restricting to schools in ZIP codes with 5+ cases in the data.

FIGURE A.2: Student-Level OLS Relationship Between Outcomes and Own-Absences

A: Standardized Math Scores

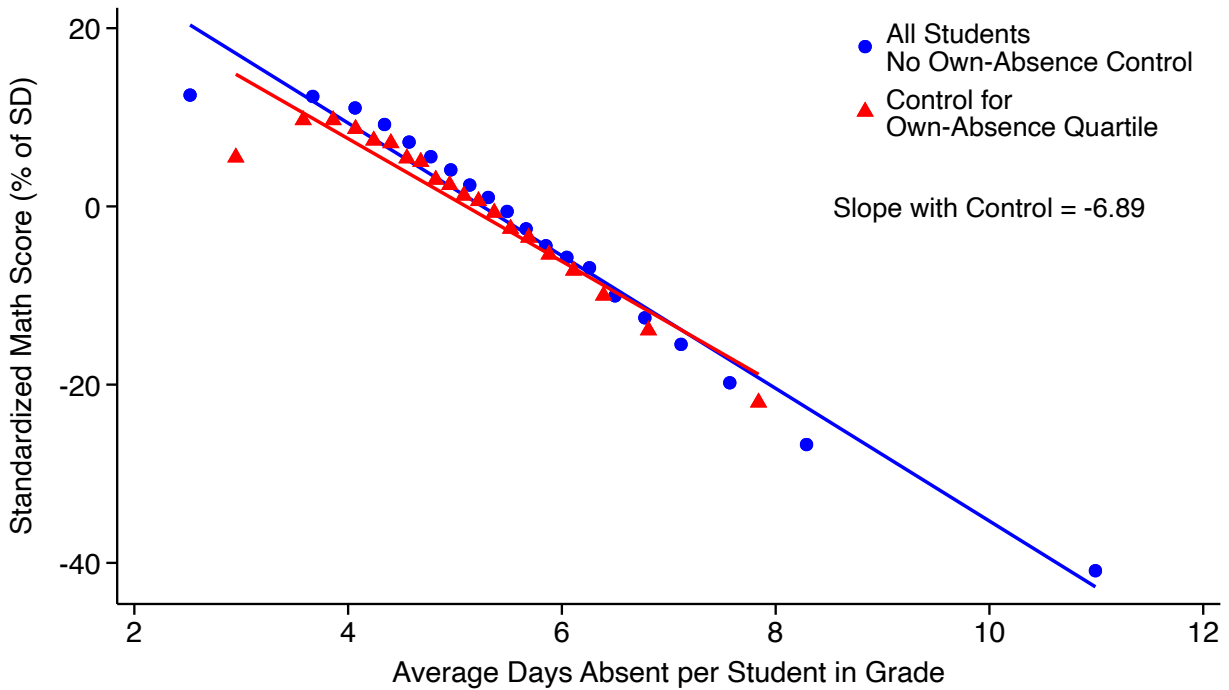


B: High School Graduation



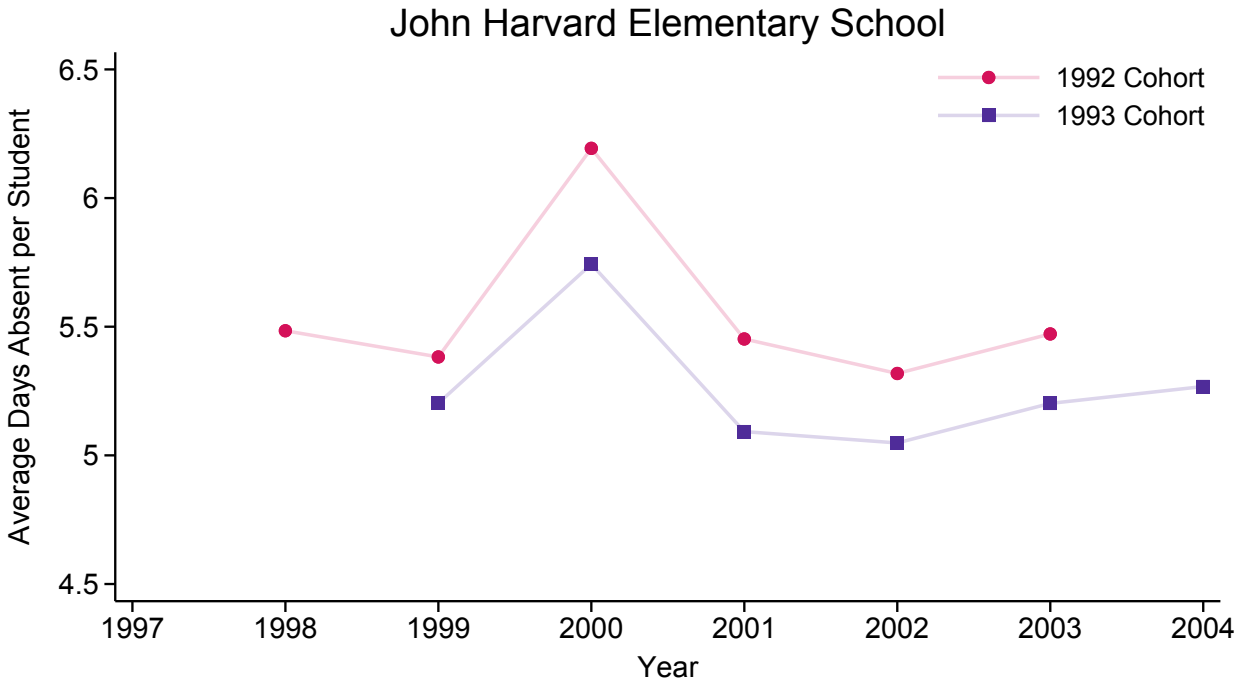
Notes: This figure presents a binned scatter plot of the relationship between outcomes and a student's own absence rate. Panel **A** shows the relationship between standardized math test scores and a student's own absence rate in that grade. Test scores and absences are measured in grades 3-5 and years 1997-2019. Panel **B** shows the relationship between high school graduation rates and a student's total number of absences between grades 1-9. The series is estimated using students in the 1990-2000 birth cohorts. The binned scatter plots are constructed using the procedure described in Figure IV; see notes for additional details. The specification includes controls for birth cohort (both panels) and grade fixed effects (Panel **A**).

FIGURE A.3: OLS Relationship Between Test Scores and School-Cohort Absences with Own-Absence Control



Notes: This figure is an analogous binned scatter plot to Figure IV Panel B, but instead of restricting to students who are never absent, it controls for quartiles of students' own-absence rates. The series marked by circles is the same as the series with circles in Figure IV. The series with triangles contains the own-absence quartile fixed effects. The specification includes controls for birth-cohort and grade fixed effects.

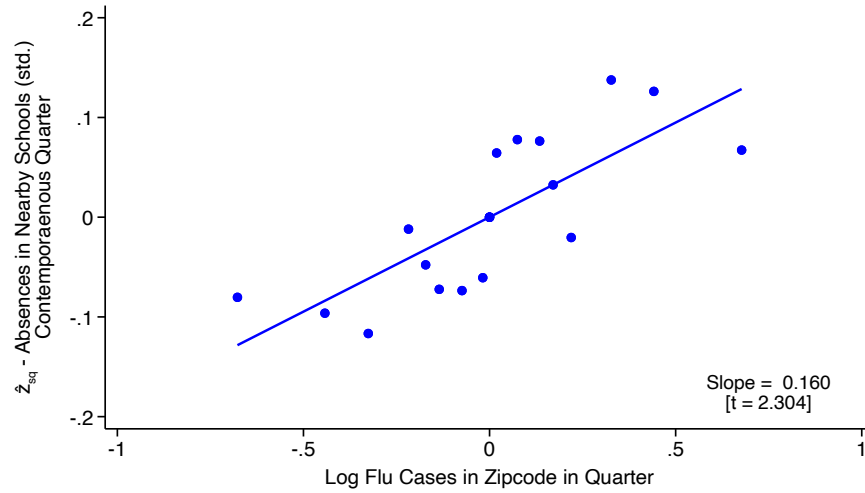
FIGURE A.4: Example of Local Shocks to Attendance



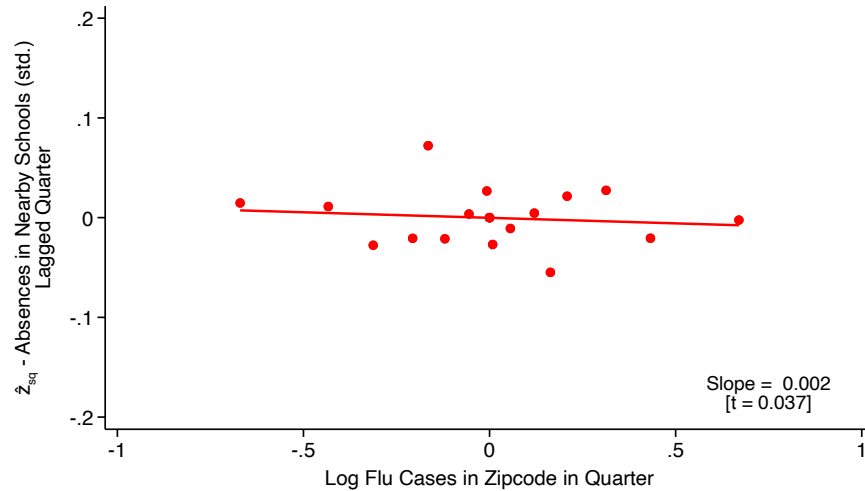
Notes: This figure presents a stylized example of absence rates in each year for two birth cohorts in a hypothetical elementary school. The series denoted by circles illustrates the 1992 birth cohort, who attends grades 1-5 between 1998 and 2003. The series denoted by squares illustrates the 1993 birth cohort, who attends grades 1-5 between 1999 and 2004. In both cohorts there is a spike up in absences in the year 2000. Section IV discusses possible causes for this spike and how the identification strategy isolates similar, year-specific blips in absences.

FIGURE A.5: Relationship Between Instrument $\hat{z}_{sy}$ and Local Flu Cases

A: Contemporaneous Flu Cases

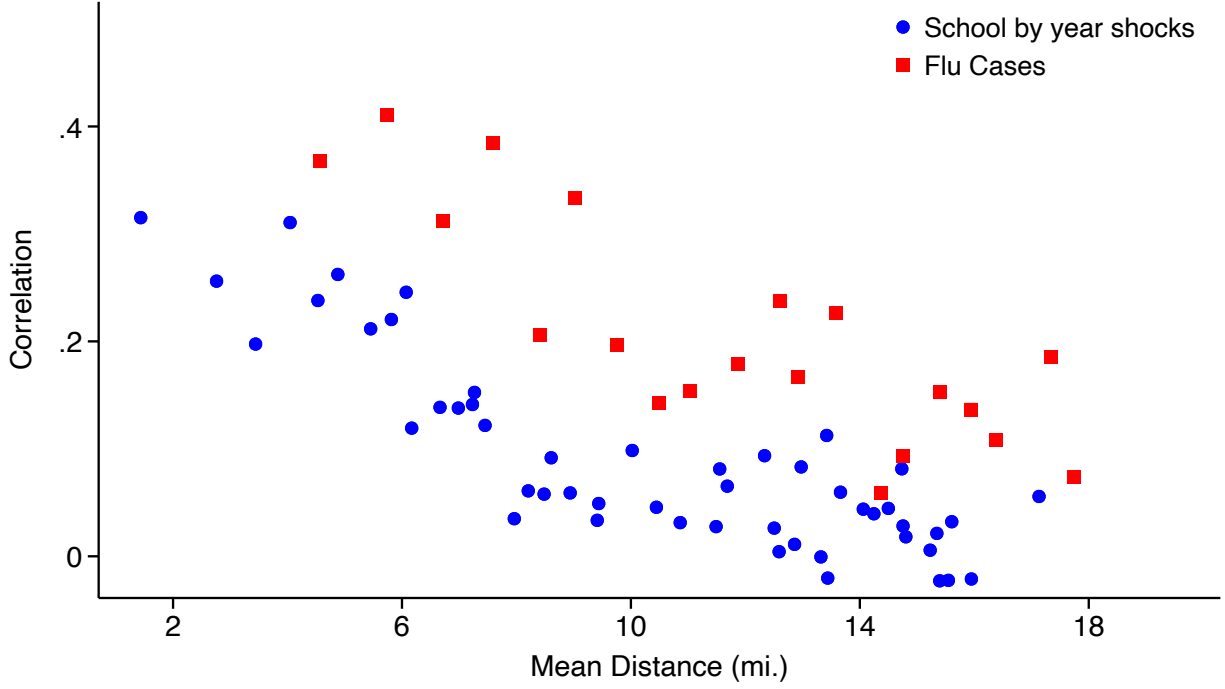


B: Flu Cases Next Quarter



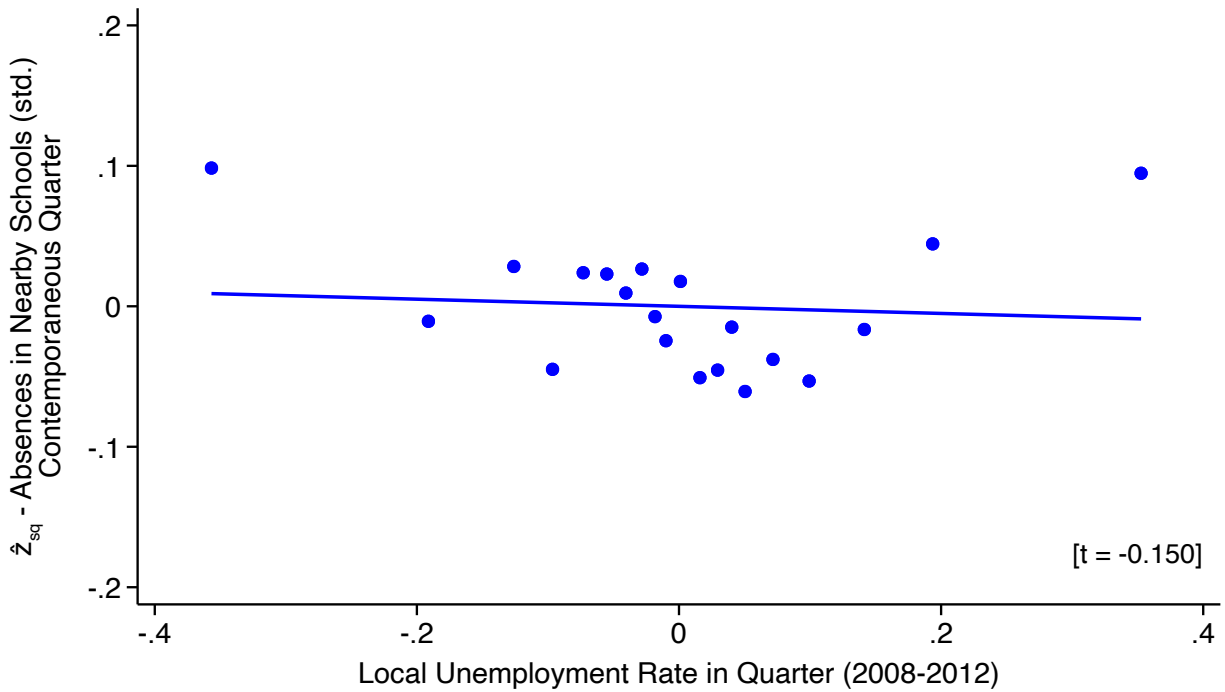
Notes: This figure presents binned scatter plots of the relationship between spikes in absence rates in nearby schools in a given year and local flu rates in those areas and years. The binned scatter plots are constructed using the procedure described in Figure IV; see notes for details. Unlike the baseline instrument described in Section IV, which is measured at the school-by-year level, the analog here is constructed at the school-by-quarter level, using absence rates measured every six weeks. In Panel A, the figure presents the relationship between $\hat{z}_{sq}$ (in standard deviation units) on the vertical axis, and the log number of cases in the ZIP code of the school in the same quarter-by-year. In Panel B, the log number of cases is measured in the subsequent quarter ($q + 1$). The instrument is correlated with flu cases in the contemporaneous quarter, but not in the subsequent quarter. Both panels are estimated using data from 2018 and 2019 and standard errors are clustered at the ZIP code level.

FIGURE A.6: Spatial Autocorrelation of
Flu Cases and Local Shocks to Attendance



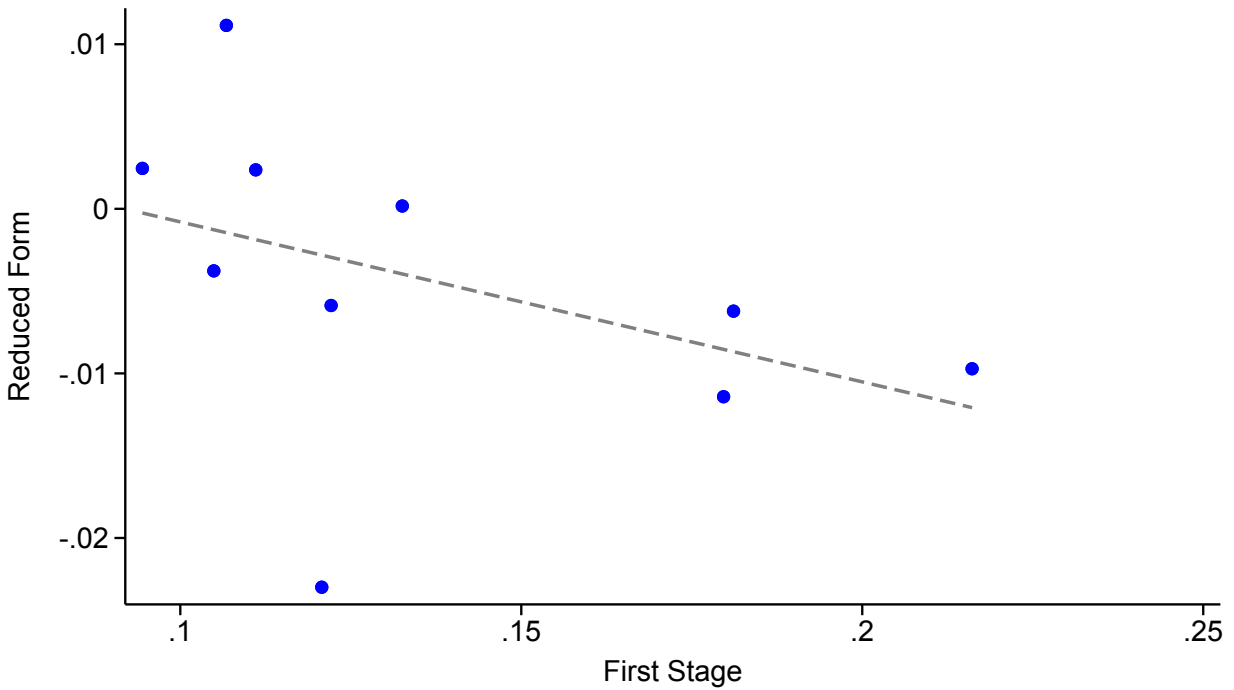
Notes: This figure presents the spatial autocorrelation between flu cases in nearby ZIP codes and spikes in absenteeism in nearby schools. To construct the series for flu cases, an adjacency matrix for ZIP codes is built using the centroid of each ZIP code. For each ZIP code, the 20 nearest neighbor ZIP codes are identified. The correlation between the log number of flu cases in the focal ZIP and each neighbor is calculated. The figure plots the correlations for those 20 nearest neighbors on the vertical axis against the mean distance between each neighbor pair on the horizontal axis. For the spikes in absences in nearby schools, the measure z_{sy} in a focal school is correlated with $z_{s'y}$ in neighboring schools. As with ZIP codes, an adjacency matrix for schools is built using the latitude and longitude of each school, and the 50 nearest neighbor schools are computed for each focal school. The correlations are calculated and plotted using the same procedure as with ZIP codes. Here z_{sy} is used for school s rather than $\hat{z}_{sy}$ (the baseline instrument, a leave-out measure over other schools in the same district) because $\hat{z}_{sy}$ and $\hat{z}_{s'y}$ are mechanically correlated for adjacent schools.

FIGURE A.7: Relationship Between Local Shocks to Attendance
and Local Unemployment Rates: 2008-2012



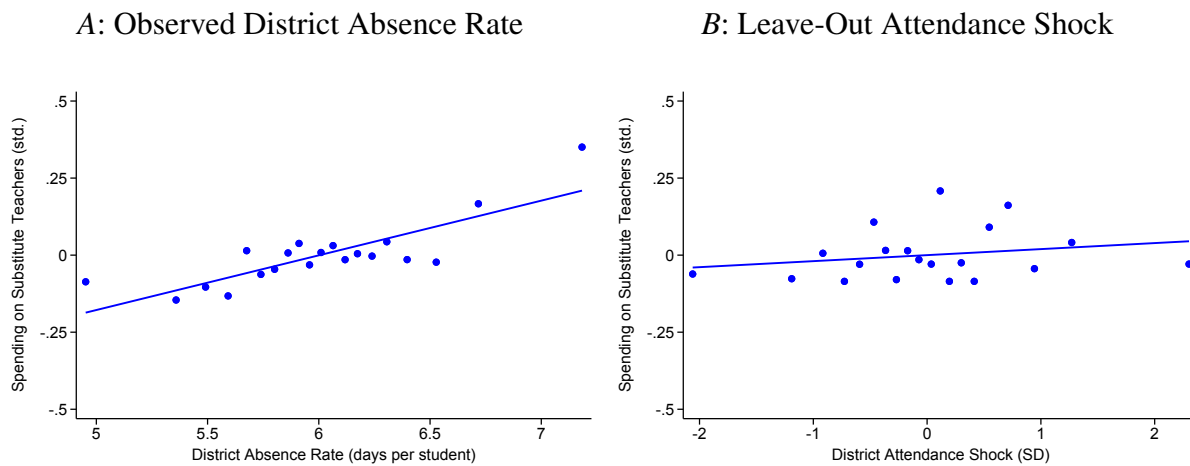
Notes: This figure is an analog to Figure VIII Panel B, but limits the years to 2008-2012, to isolate the years around the Great Recession. See the notes to Figure VIII for additional details.

FIGURE A.8: Relationship Between First Stage and Reduced Form Effects of Local Shocks to Attendance on Test Scores



Notes: This figure plots the relationship between the reduced form and the first stage in 10 separate sub-samples. First, the sample used to estimate Figure VII is split into deciles based on the observed absence rate in the school in the prior year. Using the specifications in Equations 4 and 5, the first stage and reduced form are estimated separately in each sample and plotted. The figure also includes the line of best fit. On average, in samples where the first stage is larger, the reduced form is also larger in magnitude. See notes for Figure VII for additional details on the sample and estimation.

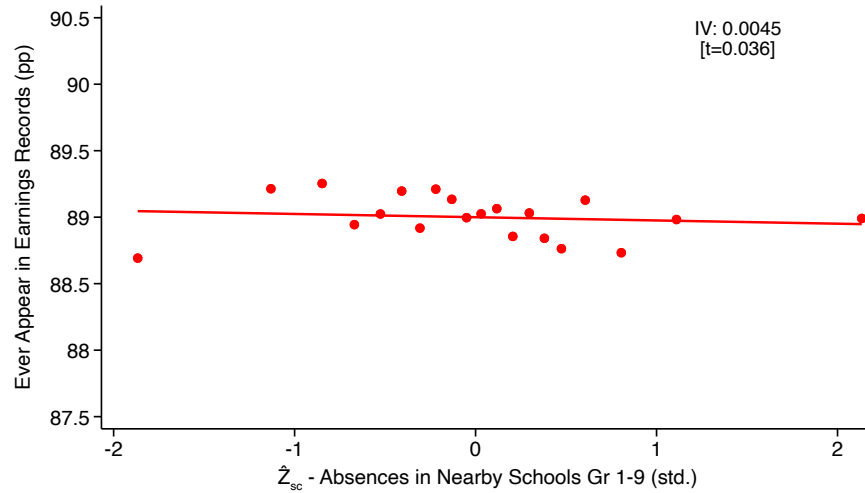
FIGURE A.9: Spending on Substitute Teachers, Absence Rates, and the Attendance Shock



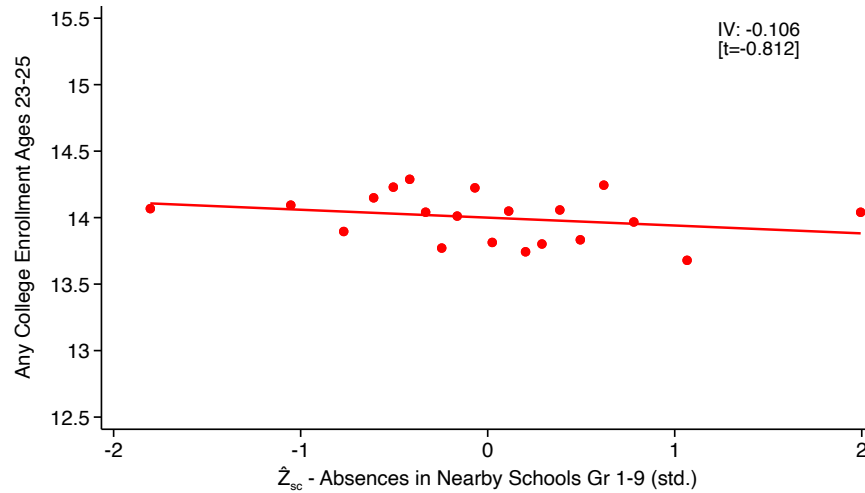
Notes: This figure relates district spending on substitute teachers to student absence rates and the leave-out absence instrument. Panel *A* plots spending against the district's observed elementary absence rate, in days absent per student. Panel *B* plots the same spending measure against the leave-out attendance shock, averaged over schools within a district, in standard deviations. The sample covers 1995 through 2019. Spending is expressed in standard deviations across districts, and both axes are residualized on district and year fixed effects before binning. Each panel sorts district-years into 20 equally sized bins of the horizontal-axis variable and plots the bin mean together with the line of best fit.

FIGURE A.10: Additional Reduced Form Effects on Long-Run Outcomes

A: Ever Appearing in UI Wage Records Between Ages 21-27

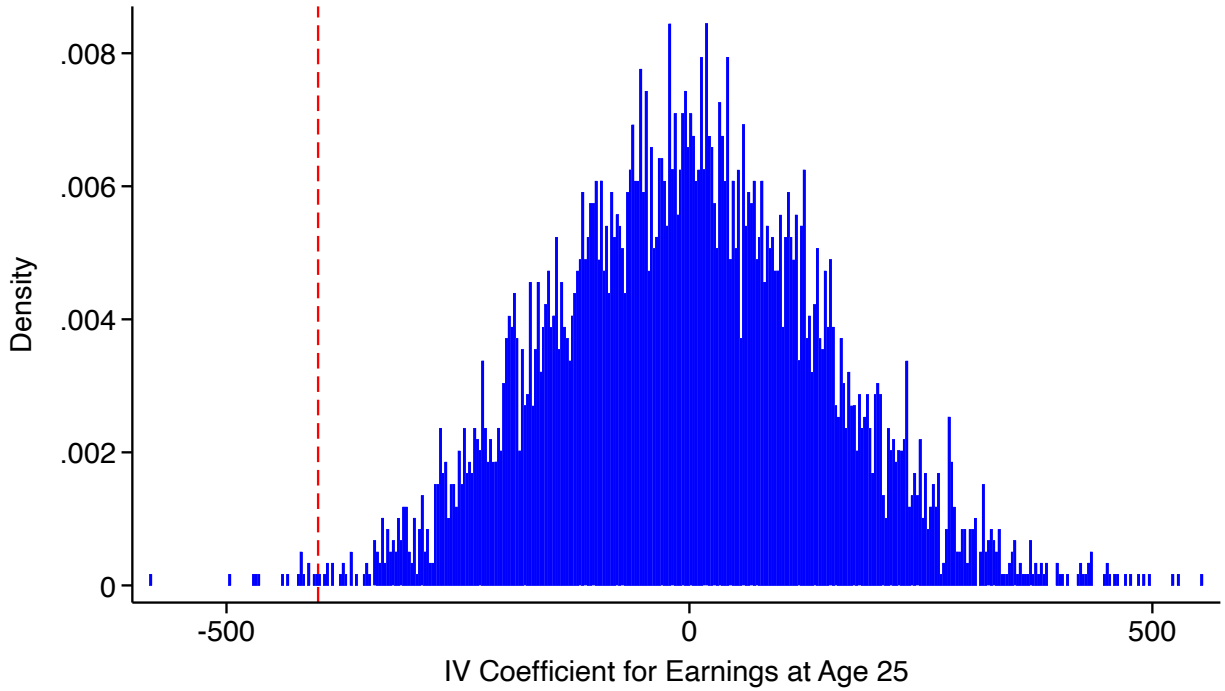


B: Four Year College Enrollment Between Ages 23-25



Notes: This figure presents two binned scatter plots related to the effects on wages presented in Figure X Panel D. Panel A tests whether the instrument, $\hat{Z}_{sc}$, has any effect on the probability that students ever appear in the unemployment insurance database used to measure earnings. The panel presents the reduced form relationship as well as the IV coefficient. There is no evidence that absences in nearby schools impact the probability that students in the focal school ever appear in the earnings records between ages 21 and 27. Panel B plots an analog of Figure X Panel C, but measures four-year college attendance between ages 22 and 25. Absences in nearby schools show no relationship with the probability that a person attends a four-year college at these later ages. In both panels, standard errors are clustered at the school-by-cohort level.

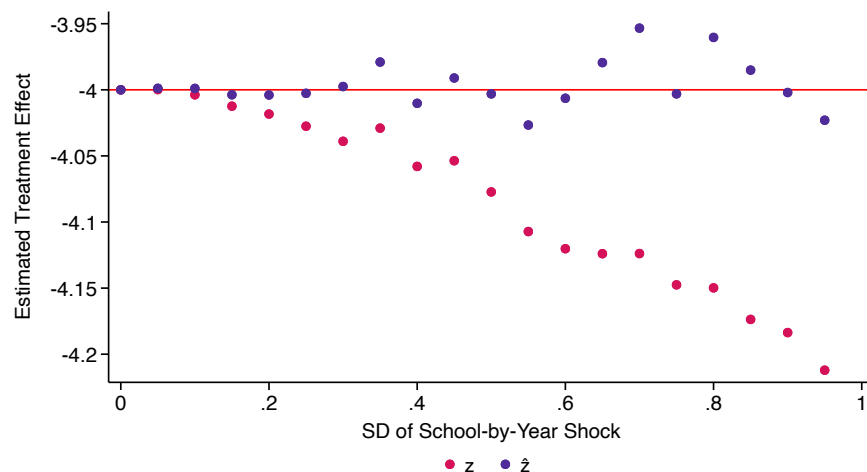
FIGURE A.11: Permutation Test for IV Effect on Earnings at Age 25



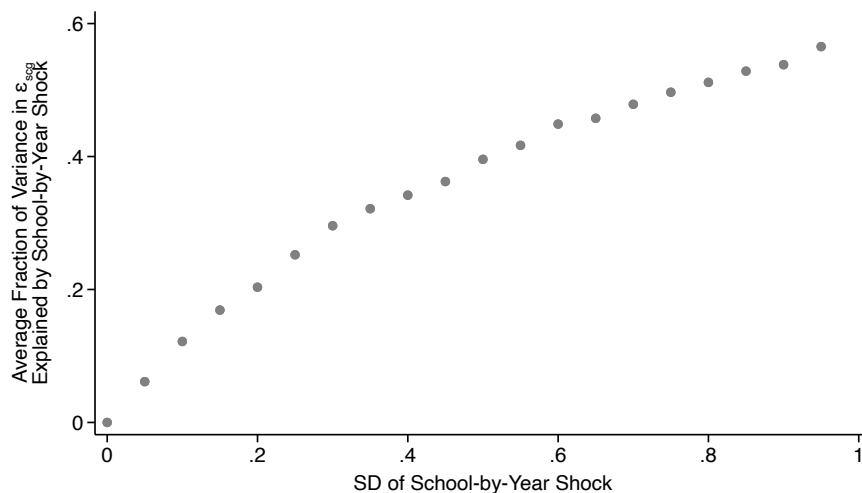
Notes: This figure presents a histogram of IV estimates from a permutation test on the effect of absences on earnings at age 25. The baseline IV estimate shown in Figure X Panel D and Table III is marked by the dashed red line. To construct the histogram, the $\hat{Z}_{sc}$ instrument is reshuffled at random across schools and cohorts and the IV is re-estimated. The process repeats 10,000 times, and the figure plots the distribution of treatment effects.

FIGURE A.12: Simulation Results: Leave-Out Bias Correction

A: IV Estimates Using Own-School vs Leave-Out Instrument



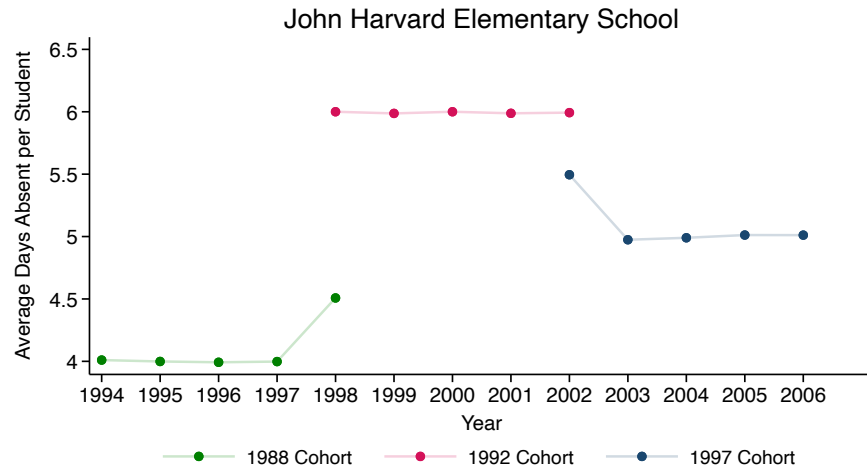
B: Share of Variance Explained by School-by-Year Shock



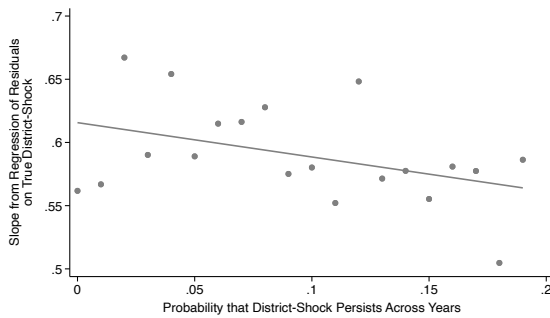
Notes: This figure shows the results of a simple simulation described in Appendix A. The simulation is designed to highlight the bias caused by school-by-year shocks to absences that are correlated with shocks to outcomes and to show that using a leave-out measure of the instrument (using other schools in the same district) corrects for that bias. Panel A plots IV estimates using the own-school instrument and the leave-out instrument as the variance of the school-by-year shock rises: estimates using the own-school instrument grow increasingly biased, while estimates using the leave-out instrument remain centered on the true effect, though less precisely estimated. Panel B shows that as the variance of the school-by-year shock rises, it explains a larger share of the variation in absences, so the own-school instrument increasingly captures the shock rather than transitory attendance variation. See Appendix A for additional details on the simulation.

FIGURE A.13: Simulation Results: Persistent Shocks to Attendance

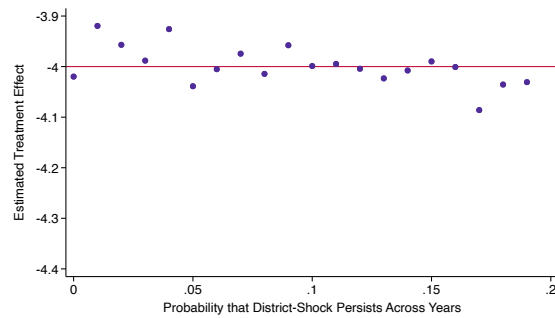
A: Example of Persistent Local Shock to Attendance



B: Attenuated Measure of Shocks



C: Unbiased IV Estimates



Notes: This figure provides intuition on how the estimation strategy can handle local illness shocks that are correlated across years. The figure gives an example of what a shock that persists over 5 years might look like in Panel A. Appendix B discusses the intuition behind this example. Panel B shows that as the probability of correlated shocks increases, the measure of illness shocks is attenuated. But, as shown in Panel C, this does not introduce bias into the IV estimates, under the same assumptions as described in Appendix A.