Every Day Counts: Absenteeism and the Returns to Education in High Poverty Schools*

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Abstract

Why do students in high-poverty schools perform worse academically and in the labor market than their peers in low-poverty schools? We show that a key difference is how regularly students attend school. To estimate the effect of improving school-wide attendance, we construct a leave-out instrument based on neighboring schools, using transitory shocks to absences caused by factors like respiratory illnesses. We establish the validity of the instrument by showing that only shocks that occur before outcomes are measured have an impact on those outcomes. In high-poverty schools, we find that reducing absences from the 75th to the 25th percentile of the distribution would increase high school graduation rates by 2.3 percentage points and raise average earnings at age 25 by $3,600. Our results suggest that absenteeism may account for up to one-third of the achievement gap between high- and low-poverty schools. In the second part of our analysis, we further demonstrate that policies focused on improving attendance in high-poverty schools can reduce these outcome disparities. Nationwide adoption of "Communities in Schools," the country's largest student support program, could reduce the achievement gap by 20%, largely through its impact on attendance.

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I Introduction

Students in high-poverty schools tend to perform worse academically, are less likely to graduate or attend college, and ultimately earn less in the labor market compared to their peers in low-poverty schools. These disparities persist even among children from households with similar parental income levels, and children from low-income families are significantly more likely to attend high-poverty schools.[1] As a result, improving outcomes in these schools could meaningfully impact overall levels of economic mobility in the U.S.

To date, researchers and policymakers have primarily focused on factors such as teacher quality, class sizes, and school funding to explain the differences in outcomes between students in low- and high-poverty schools (e.g. Chetty et al., 2011; Chetty, Friedman and Rockoff, 2014; Jackson, Johnson and Persico, 2016; Hyman, 2017). However, a key distinction between high- and low-poverty schools is how effectively students are able to make use of available resources. Students in high-poverty schools are twice as likely as their low-poverty peers to be absent for more than 10% of the school year (Malkus, 2024). To what extent can these differences in attendance rates explain disparities in outcomes, and is it feasible to close these gaps through policy interventions?

In the first half of the paper, we estimate the causal effect of student absences on academic performance, graduation rates, and labor market outcomes. Our research design isolates variation in absences caused by transitory, exogenous factors, such as the spread of respiratory illnesses. Absences during elementary and middle school have significant negative effects on both short-term outcomes (test scores) and long-term outcomes (high school graduation, college attendance, and earnings). Notably, absences appear to harm not only the students who are absent but also their peers, possibly by disrupting classroom teaching and coordination. Our estimates suggest that absenteeism may account for up to one-third of the achievement gap between low- and high-poverty schools.

In the second part of the paper, we evaluate whether policy interventions aimed at reducing absenteeism can mitigate these negative effects. We focus on Communities in Schools (CIS), a student support program designed to reduce absenteeism in high-poverty schools. Leveraging the staggered rollout of CIS programs across schools, we find that in a cohort of 100 students, CIS reduces absences by approximately 50 student-days per year (equivalent to 0.5 days per student) and increases test scores by an average of 0.05 standard deviations. Related work by Goldman, Gracie and Porter (2023) shows that CIS increases labor market earnings by approximately $1,500 by age 27. Based on our estimates from the first part of the paper, this earnings increase is largely attributable to CIS's impact on reducing absences, with 45% of the effect on test scores explained by

[1] Assigning Census tracts to school catchment zones as in Chetty et al. (2018), low-income students in high-poverty (top quintile) catchment zones earn 12% less in adulthood than similarly low-income students in low-poverty (bottom quintile) catchment zones. Low-income students are concentrated in the high-poverty catchment zones.

lower absenteeism. Our findings suggest that nationwide implementation of CIS in high-poverty schools could reduce the achievement gap between high- and low-poverty schools by 20%. These results underscore the potential for absenteeism-focused interventions to improve educational outcomes in disadvantaged schools, even without increasing traditional school resources.

We conduct our analysis using administrative data from the Texas Education Research Center, which links educational records to college attendance and labor market outcomes. The data cover the universe of public school students in Texas between 1994 and 2022, though our analysis focuses on approximately 7.5 million students born between 1990 and 2009. We measure a variety of outcomes including standardized test scores, high school graduation, college attendance (both in Texas and out-of-state), and earnings for workers in Texas unemployment insurance records. A key feature of the data for this paper is the detailed tracking of student absences, measured in six-week periods throughout the school year.

On any given day, students in high-poverty schools are approximately 15% more likely to be absent than their peers in low-poverty schools. We show that in low-poverty schools, which we define as schools in the bottom quintile of the across-school distribution of the share of years students are eligible for free and reduced price lunch, the typical student misses approximately 45 days of school between first and ninth grade.[2] In high-poverty schools (top quintile), students miss more than 6 additional days on average — a difference of roughly 1 standard deviation in the school-cohort absence distribution.[3] In a typical cohort size of 100 students, this amounts to more than 600 additional absences spread out across students, days, and grades, which could potentially disrupt coordination between students and teachers, also impacting kids who themselves are not absent. We show that graduation rates in the high-poverty schools are approximately 6 percentage points lower, and earnings at age 25 are $6,500 lower (22%), compared to those in low-poverty schools. To what extent can differences in attendance rates explain these differences in outcomes?

To answer this question, we focus on the total effect of absences—including any spillovers onto students who attend. When students miss school, they not only miss out on instructional time, but they fall out of sync with 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.

Consistent with the presence of spillovers, we show that cohort-level absences are predictive of outcomes, even conditional on a student's own absence rate. Students in cohorts where

[2]We define poverty rates based on the average number of years that students in a given school are eligible for free and reduced price lunch, which has been shown to be a good predictor of socio-economic status above and beyond an indicator for ever being eligible (Michelmore and Dynarski, 2017).

[3]The gap is not driven by a small number of students missing at very high rates in high-poverty schools. The gap is more than 5 days at the median.

the average student misses 3 days per year score approximately 0.4 standard deviations higher on standardized math tests, compared to cohorts where the average student misses 8 days. This relationship could be driven purely by the fact that students who themselves miss many days of school have lower test scores, but we show this is not the case. In fact, roughly 80% of the relationship between test scores and cohort-level absences remains after accounting for individual attendance.[4] These patterns, though correlational, motivate our focus on changes in group-level absence rates. To estimate the causal effect of absences, we develop an empirical design that isolates exogenous, year-to-year variation in absences in a particular neighborhood.

To build intuition, we begin with a concrete example: the spread of the flu. We show that in places and years with higher flu activity, absences increase, and test scores decline. While average flu rates across areas over time may not be random, there are many exogenous factors that 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). Using data collected from a Texas hospital network, we measure 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. These absences lower test scores by approximately 1% of a standard deviation, though the estimate is somewhat noisy. In order to gain the additional precision needed to estimate impacts on long-run outcomes, we generalize this strategy to develop an instrument that captures similar types of local shocks to attendance.

Our instrument captures spikes in absence rates in certain schools and years, above and beyond the typical absence rates for those students and schools. To construct it, we remove the school-by-cohort and school-by-grade components of absences, which ensures that the variation we use is not influenced 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 due to more lenient teacher attendance policies). After removing these effects, the remaining variation reflects year-specific changes in absences that affect all students in a given school. For instance, if 2002 had an unusually high spread of the flu in Austin, we would observe absences above and beyond the baseline rates expected for particular cohorts and grades.

Rather than use variation in one's own school, our 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 out-

[4]We show that half of the relationship persists even for students who themselves have perfect attendance in a given grade. This evidence is consistent with earlier work, which also finds 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).

comes, and isolates the plausibly exogenous variation in absences that is shared across schools in the same district in a given year. Intuitively, our first stage examines how absences in a focal school and year change when students in nearby schools happen to attend at lower rates in that same year. This type of variation is consistent with absences generated by the spread of respiratory illnesses in a particular area in a particular year. We show directly that the instrument is correlated with our measure of local flu activity in years where the data are available.

We show that absences are higher in the focal school when students in nearby schools are absent at higher rates. Our instrument has a strong first stage: a 1 standard deviation increase in the instrument results in an increase of 0.15 absences per student on average in the focal school. Given the typical school-cohort in our sample has 100 students, this translates to an additional 15 student-day absences in the grade. We then turn to measuring the effect of this increase in cohort-level absence rates on contemporaneous test scores.

Our instrumental variable (IV) estimates show that higher absence rates in nearby schools lead to lower test scores in the focal school. We find that in a typical school-cohort, an increase of 100 total absences (one day per student on average) lowers test scores by 0.038 standard deviations.[5]. The IV effects are fairly precise; we can rule out that the effect is smaller than a 0.026 standard deviation reduction in test scores.

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. We may have been concerned that when the value of the instrument is high, there are other observable characteristics of students and teachers that are also high—for example the fraction of free and reduced-price eligible students or the fraction of inexperienced teachers—and this imbalance, not absences, causes lower outcomes. We show balance on a broad range of student and teacher characteristics. Alternatively, we might be worried that our effects are driven by other types of local shocks. For example, if absences spiked because of a negative economic shock to the area, that could impact outcomes for reasons other than school attendance. To address this concern, we show that our instrument is not correlated with changes in local unemployment rates.

Having established that our instrument is unrelated to certain time-varying characteristics of schools and communities, we turn to evaluating selection on unobservables using outcome-based placebo tests in the style of Danieli et al. (2024) to tighten the bounds on possible local shocks that would bias our treatment effects. We construct versions of our instrument within-year using six-week intervals, and compare the reduced form effect for the instrument in periods before and

[5]We are not the first paper to measure the effects of absences on test scores. Our approach relates most closely to 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)

after students take standardized exams in the spring. If our results had been driven by a local, unobservable shock—such as a spike in crime—and that spike lasted for at least 6 weeks, it would generate a correlation between test scores in April and the instrument in May, even though those absences occur after students have taken the exam. In practice, however, we find that only the periods leading up to, but not occurring after standardized testing show a significant reduced-form effect on test scores. This test rules out bias from any type of unobserved shock that both impacts absences and outcomes and persists across six-week periods. The type of violation we cannot rule out would involve a shock that directly affects both absences and outcomes but dissipates within six weeks.

Respiratory illness shocks are high-frequency, transitory shocks so an alternative concern is that our 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., 2022). We focus on exposure to shocks during adolescence, a period with substantially less neural and physical development (Currie and Almond, 2011). Even so, we directly evaluate the extent to which the relationship between the instrument and test scores is driven by absences, as opposed to other effects of respiratory illnesses, by estimating the first stage and reduced form in 10 separate samples based on prior absence rates. We find that the reduced form effect is smaller in settings with smaller first stages—i.e. places where the instrument induces fewer absences. This is consistent with the treatment effect running through absences as opposed to the direct effect of respiratory illnesses.

Having validated our empirical strategy in the short-run, we turn to measuring the effects of absenteeism on outcomes measured later in life. To do so, we develop an aggregate measure of exposure to absenteeism shocks by summing our instrument over grades 1 through 9. Intuitively, students with high draws of the instrument were exposed to several bad years of illness during these grades, resulting in elevated cohort-level absence rates during their time in school. Conversely, students with low draws of the instrument faced fewer years of widespread illness and thus attended school more regularly. Our first stage shows that the aggregated instrument does in fact generate meaningful variation in cohort-level 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.

We find that absenteeism has substantial long-term effects on students' outcomes. Our estimates imply that an additional 100 student-days of absences between grades 1 and 9 in a school-cohort of 100 students (an additional one day per student) lowers high school graduation rates by

0.26 percentage points. Moreover, students from these cohorts are 1 percentage point less likely to attend at least one year of a four-year college and earn approximately $400 less on average at age 25.

Our results suggest that a significant portion of the gap in outcomes between students in high- and low-poverty schools is driven by disparities in attendance. Specifically, our counterfactual analysis shows that if we were to equalize absence rates across these schools and apply our IV estimates, we could close approximately one-fifth of the gap in high school graduation rates and one-third of the earnings gap at age 25. These findings indicate that efforts to improve attendance in high-poverty schools are well-targeted and could have lasting positive effects on students, both in their academic careers and later in life.

Given the promising implied impacts of reducing absenteeism, the second part of our analysis explores the feasibility of addressing these achievement gaps through targeted policy interventions. We focus on Communities in Schools (CIS), the nation's largest student-support program, which serves over 2 million students in 3,000 schools, predominantly in high-poverty areas. Nearly half of these programs are located in Texas. A primary goal of CIS is to reduce absenteeism. To do so, the program places "navigators" – often trained social workers—into high poverty schools to help connect students to resources, enabling them to attend school and be ready to learn when present. These resources help address common barriers to consistent attendance such as transportation challenges, food insecurity, lack of clean clothes, and housing instability.

We use the roll-out of CIS across elementary schools in Texas to measure the program's impact on absences and outcomes. We implement a staggered-adoption design, comparing outcomes in elementary schools that received CIS programs in different years under the identifying assumption that the exact timing of when a CIS program begins is quasi-random. In support of the design, we show that other time-varying characteristics of schools do not change around the time that the program begins.

After CIS is introduced in a school, average attendance improves by 0.46 days per student annually, and fifth grade test scores increase by 0.05 standard deviations, with 45% of the test score improvement explained by reduced absenteeism. While CIS is expected to influence outcomes through various channels—such as addressing non-academic barriers like food insecurity or housing instability—its impact on absenteeism alone accounts for a substantial portion of the improvement. Our estimates suggest that reducing absenteeism to this extent could close about 10% of the achievement gap between high- and low-poverty schools. The total effect of providing all high-poverty schools with access to CIS would be even larger.

In related work, Goldman, Gracie and Porter (2023) estimate the effect of the nationwide roll-out of CIS on earnings, using administrative IRS and Census records. In that paper, we find that four years of CIS increases earnings at age 27 by $1,500, closing roughly 20% of the earnings

gap between students in high- and low-poverty schools. These findings underscore the potential of programs like CIS to address long-run disparities in economic outcomes by targeting absenteeism and other barriers to success.

This paper relates to a expansive literature on the returns to education. Much of the prior work focuses on improving the quality and availability of resources in schools, whether through smaller class sizes, school finance reform, higher quality teachers and administrators, or improved infrastructure (e.g. Cellini, Ferreira and Rothstein, 2010; Chetty et al., 2011; Chetty, Friedman and Rockoff, 2014; Neilson and Zimmerman, 2014; Jackson, Johnson and Persico, 2016; Hyman, 2017; Jackson, 2018; Mulhern, 2023). Our findings highlight a different mechanism: increasing the effectiveness of existing resources by improving student attendance. In doing so, we show that even without changes to traditional school inputs, boosting attendance can improve labor market outcomes for students.

Our paper is also related to the literature on peer effects in schools (e.g. Hoxby, 2000; Chetty et al., 2011), as well as research on the externalities caused by disruptions. In our setting, 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). Our 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.

We also contribute new evidence on the efficacy of student support programs that aim to improve outcomes for low-income students. Prior work on CIS as well as evidence from comprehensive support programs for students in public housing has shown persistent effects on labor market outcomes (Goldman, Gracie and Porter, 2023; Lavecchia, Oreopoulos and Brown, 2020; Oreopoulos, Brown and Lavecchia, 2017). Our results suggest that one channel through which these programs can impact long-run outcomes is through their ability to improve attendance. Our results imply that in some settings, absences can serve as a useful surrogate measure (Athey et al., 2019). Interventions that successfully improve attendance in the short-run can have meaningful impacts on long-run outcomes.

Finally, our results speak to the potential long-run effects of the large increase in absence rates following the COVID pandemic. During the 2022-23 school year, more than 1 in 4 of students nationwide missed more than 10% of school days, compared to approximately 1 in 8 students before the pandemic (Malkus, 2024). Our results suggest that without intervention, these absences will substantially alter these students' labor market trajectories. Programs like CIS can be an

effective tool to mitigate the potential harmful effects of increased absenteeism in the aftermath of the pandemic.

The rest of the paper proceeds as follows. Section II describes the data; Section III presents descriptive evidence on 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. In Section V, we estimate the effect of a program designed to improve attendance in high poverty schools on both absences and outcomes. Section VI concludes.

II Data

We use administrative data on all students who attend public schools in Texas, the country's second largest state, maintained by the Education Research Center at UT Austin. These data combine records from many different sources – the Texas Education Agency, the Texas Higher Education Coordinating Board, the National Student Clearinghouse, and the Texas Workforce Commission. These data allow us to trace a students' outcomes from kindergarten into their young adulthood. We then supplement these data with measures of local unemployment rates from the Bureau of Labor Statistics and the incidence of the flu from a Texas hospital network.

We start with data from the Texas Education Agency (TEA) on primary and secondary school enrollment. In order to measure exposure to absences through a student's school-life, we limit to students who appear in the data from each grade between kindergarten and ninth grade. We focus our analysis on the 7.5 million students who were born between 1990 and 2009. The TEA data contain invariant characteristics about students – their birth cohort, gender, and race/ethnicity – as well as traits that are year specific – the school they enroll in, their grade, and free and reduced price lunch (FRPL) eligibility. Absences are measured as the total days a student was absent from school over six, six-week intervals throughout the year.

We supplement these student data with in-school outcomes, also maintained by TEA. We measure elementary school standardized test math scores (taken in grades 3-5) between 1997-2019.[6] We also measure graduation for each student, defined as an indicator for ever graduating from high school.

We measure post-secondary outcomes using data provided by both the Texas Higher Education Coordinating Board (THECB) and the National Student Clearinghouse. The THECB data capture post-secondary enrollment in Texas colleges, both public and private. We supplement these data with data from the National Student Clearinghouse, which we have for students who graduate

[6]The type of standardized tests that students take changes multiple times throughout our sample period. Students took TAAS exams between 1991 and 2002, TAKS exams between 2003-2013, and STAAR exams starting in 2014. Because our empirical analysis will rely on within-year comparisons, these changes are not a concern.

from Texas high schools and which cover approximately 90% of college enrollment nationwide (Dynarski, Hemelt and Hyman, 2015). For our analysis, we focus on whether students ever attend a four-year university between ages 18-22.

We use data from the Texas Workforce Commission, an unemployment insurance database, to measure earnings. The data contain quarterly earnings, which we annualize by imputing zeros in missing quarters. Unemployment insurance records cover workers who are not self-employed, independent contractors, in the military, or working in the informal sector (Stevens, 2007). Because we cannot distinguish between people who are not in the data because they are not working (in a job covered by the database) versus because they do not work in Texas, we restrict our earnings analysis to people who ever have positive earnings between ages 21 and 27 and impute zeros in years where a given person has no earnings record. We find that 90% of our sample ever appear in the earnings records. This aligns with Sprung-Keyser et al. (2022) who find that among the people who were living in Texas at age 16, 88% were still in Texas at age 26.

In addition to these administrative records, we collect measures of flu case-loads from a Texas hospital network, measured in 2018 and 2019. These data are at the ZIP code level and record the number of flu cases in each quarter, by patient home ZIP code. Appendix Figure A.1 shows the coverage of these data. We match these data to schools using the school's ZIP code. We use these data in Section IV to motivate our empirical strategy.

We also use monthly data on local unemployment rates from the Bureau of Labor Statistics. These data contain cover all cities and towns with at least 25,000 residents, which covers more than 85% of Texas residents (Green, 2023). We collect these data from 2000 to 2019 and assign them to schools based on the city where the school is located. We use these data in a test of our identification strategy in Section IV.

Throughout the paper, we focus on the difference between outcomes in low- and high-poverty schools, which we define based on students' eligibility for free and reduced price lunch. For each student we calculate the fraction of years between grades 1 and 9 that the student was FRPL-eligible and take the average across students in an elementary school-cohort (Michelmore and Dynarski, 2017). We split the school-cohorts into quintiles based on the FRPL-eligibility rate. Students in schools in the bottom quintile (low-poverty) are FRPL eligible on average for 12% of years between grades 1 and 9, relative to 77% in the top quintile (high-poverty).

II.A Summary Statistics

Table I reports summary statistics for our sample. Our analysis focuses on 7.5 million unique students born between 1990 and 2009. The sample of students for whom we can measure the effect of absences on test scores contains students who were in grades 3-5 at some point between 1997

and 2019. We have more than 80,000 school-by-birth-cohort-by-grade cells for this analysis. Our analysis of the effects of absences on long-run outcomes is based on students born between 1990 and 2000. These students are 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 school-cohort (in an elementary school) is comprised of 99 students. Our focus throughout is on changes in the cohort-level absence rate across these 99 students.

Panel B describes the characteristics of teachers the elementary and middle schools that we focus on. 21% of teachers have an advanced degree (either a masters of a PhD). Approximately 16% of teachers are male and 35% are non-white. The average teacher in these schools has been teaching for 11 total years and has been at their current school for nearly 8 years. The average salary is $51k in 2015 dollars. There is evidence that teachers can directly effect student absences, so we return to these characteristics in Section IV as part of a test of our identification strategy (e.g. Jackson, 2018; Tran and Gershenson, 2021).

Our focus in this paper is on student absenteeism, which we report separately in Panel C. In grades 3-5, the average student misses about 5 days per year (SD = 1.2 days per student). This is the measure of absences we use for our test score analysis. When we turn to long-run effects, we focus on the total number of absences per student between grades 1 and 9. On average, students miss around 50 days of school over these years (SD = 7.7).

Finally, in Panel D we report statistics on the later-in-life outcomes that we focus our analysis on. The average high school graduation rate is 88%. In our sample, approximately 30% of students attend a four-year college at some point between ages 18-22. This includes both in-state and out-of-state colleges. By age 25, the typical student in our sample earns just over $26k.

III Descriptive Facts on Absenteeism and Outcomes

We begin by showing that absences are substantially more common in high-poverty schools. Figure I Panel *A* shows the average absences rate, between grades 1 and 9, for school-cohorts in the bottom and top quintile of the poverty distribution. In low-poverty schools, the typical student misses approximately 46 days of school between grades 1 and 9. In high poverty schools, the typical student misses 6.25 additional days over this period, equal to approximately 1 SD in the cross school-cohort distribution. The typical school-cohort in our sample contains 100 students, meaning that in high-poverty schools there are an additional 625 total absences, spread out across students and years. These gaps are not driven by a few students in high-poverty schools missing at very high rates. The attendance gap between the median student in low- and high-poverty schools

is 5.7 days.

Students in high-poverty schools experience higher rates of absenteeism and, later in life, are less likely to graduate from high school and tend to have lower earnings. Figure I Panel *B* shows that only 84% of students in high-poverty schools go on to graduate from high school, relative to 92% of students in the low-poverty schools (SD = 6%). Similarly, students in high-poverty schools earn approximately $23,000 at age 25, relative to nearly $30,000 in low-poverty schools, as shown in Figure I Panel *C* (SD = $6,600). Whether or not these outcome gaps would shrink if students in high-poverty schools attended at higher rates depends on the effect of attendance on outcomes.

We start by showing that there is a strong correlation between cohort-level absences and outcomes measured in school and beyond. First, Figure II Panel *A* shows the relationship between standardized math scores in grades 3-5 and school-cohort level absences in that grade. A one day increase in days absent per student is associated with test scores that are 0.086 standard deviations lower. Absences are predictive not only of contemporaneous test scores, but also later-in-life outcomes. We also see a strong relationship between high school graduation rates and cohort-level absence rates, here measured as the average number of total absences between grades 1 and 9 (Figure III). A 9 day increase in the average total absence rate (one day per grade per student on average) is associated with graduation rates that are 2.43 percentage points lower. Both relationships combine two forces: the individual relationship between absences and outcomes – i.e. students who themselves miss many days of school score lower on exams and graduate at lower rates, as shown in Appendix Figure A.2 – and the group-level relationship between cohort-level absences and average outcomes.

The relationship between outcomes and absences is largely driven by cohort-level absences, not purely individual absences. We demonstrate this using test scores. Figure II Panel *B* shows the same relationship between average test scores and school-cohort level absences rates, but restricts to students who miss no days of school that year (approximately 15% of students in grades 3-5). There is still a clear negative relationship between average test scores for these students and cohort-level absences; more than 50% of the unconditional relationship persists.[7] In Appendix Figure A.3, we show a similar figure, but rather than restrict to students with perfect attendance, we control for quartiles of students' own absence rate. Again, there is a strong relationship between cohort-level absences and student outcomes.

The strong correlation between outcomes and cohort-level absences, even after controlling for individual absences, is consistent with either selection or a causal effect of cohort-level absences on outcomes. When students are absent, they miss not only the material covered that day but also fall

[7] A similar relationship between cohort-level absences and outcomes has been documented in other work, but we focus specifically on students who are never absent to hone in on the possible spillovers (e.g. Monk and Ibrahim, 1984; Gottfried, 2011; Goodman, 2014; Gottfried, Kirksey and Ozuna, 2020; Gottfried and Ansari, 2022).

out of sync with their classmates. Goodman (2014) shows that this mismatch explains a significant portion of the negative impact of absences on test scores. Missing key lessons makes it difficult for students to catch up, and their absence may also affect their peers if teachers alter their instruction to help students who have fallen behind. Similar disruptions, such as exposure to students who act out in class, have been shown to negatively affect classmates' performance both in the short-term and later in life (Carrell and Hoekstra, 2010; Gottfried, 2013; Kinsler, 2013; Carrell, Hoekstra and Kuka, 2018). There is reason to believe that cohort-level absences can have significant effects on outcomes beyond the individual's own attendance.

At the same time, many differences between school cohorts with high and low absenteeism rates could explain the descriptive relationships we document, even if cohort-level absences do not have a causal effect on outcomes. Students in high-absence cohorts may have lower potential outcomes for unobserved reasons, such as unstable family circumstances, or may be exposed to different types of teachers. Jackson (2018) shows that some teachers have positive causal effects on both attendance and test scores, suggesting that teacher quality could play a role. Additionally, high-absence periods may coincide with other forms of instability. Growing evidence shows that student performance declines during local economic shocks (e.g. Page, Stevens and Lindo, 2009; Stevens and Schaller, 2011; Jackson, Wigger and Xiong, 2021). If absenteeism rises during these periods, this could generate the patterns we observe. In the next section, we develop an identification strategy to address these selection concerns and estimate the causal effect of changes in cohort-level absences on short- and long-term outcomes

IV Causal Effect of Absenteeism on Outcomes

Estimating the causal effect of cohort-level absences on short- and long-term outcomes requires exogenous variation in group-level absence rates 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. Because we are interested in the total effect of absences, including spillover effects across students, we focus on exogenous variation in total, cohort-level absence rates.

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

We develop an empirical strategy that isolates plausibly exogenous variation in total, cohort-level absences, such as variation driven by the spread of respiratory illnesses. These types of shocks impact school-wide attendance rates in a particular year. To build intuition for the design, we start with one concrete example—the spread of the flu—before generalizing the approach.

We exploit random variation in the severity of flu outbreaks across years within the same local area. Unlike structural factors, such as vaccine coverage or healthcare access, which might lead some places to have consistently high or low flu rates, the flu's intensity fluctuates unpredictably 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). These fluctuations create exogenous variation in school-wide attendance that is not driven by systematic differences across locations. For instance, while one district may experience a particularly severe flu season in a given year, the following year the district might experience a more mild season, even though underlying health infrastructure remains unchanged.[8] This random variation in flu severity provides exogenous variation in cohort-level absences.

We start by estimating the effect of the exogenous variation in the spread of the flu on absences. For example, suppose 2018 was a year with higher spread of the flu in Austin compared to 2019 (the two years that we have flu data). We would expect that the students who were in 3rd grade at Austin elementary school in 2018 would have higher absence rates than their peers who were in third grade in the next year in 2019, due to differences in flu rates. We formalize this intuition and show that absences are higher when flu cases are higher by estimating the following first stage:

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

We predict absences A_{sgy}, in school s, grade g, and year y using the the log number of flu cases in the ZIP code of school s, $zip(s)$, in year y. We measure absences for students in third through fifth grade, observing cohort c in school s in two different grades. We include school-by-grade fixed effects, ϕ_{sg}, which compares within school-grade across years (e.g. example comparing 3rd graders in Austin Elementary School in 2018—the high flu year—and 2019—the low flu year), and state-wide year-by-grade fixed effects γ_{yg}, which accounts for state-wide differences in flu levels in these two years (e.g to isolate changes across the years specific to Austin). The coefficient of interest is $\beta^{FS,flu}$, which captures the effect of higher flu cases on cohort-level absences.

[8] One concern is that high exposure in one year causes low exposure in the subsequent year, due to cross-immunity from past infections. However, this is also subject to the same exogenous factors including how close of an antigenic match the strains are in subsequent years (Krauland et al., 2022).

Absences are higher when the flu is spreading at higher rates. Figure IV Panel *A* shows a binned scatter plot of this relationship. To construct the figure, we residualize A_{sgy} and $\text{LogFlu}_{zip(s)y}$ on the remaining controls in equation 1 and construct 20 equally sized ventiles of the residuals. We plot the mean of the residuals within each bin as well as the line of best fit to visualize the relationship. We also report the coefficient from the linear model in equation 1. We show that moving from the 5th to 95th percentile 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 these absences negatively effect test scores? We turn to estimating the reduced form:

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

for standardized math scores, Y_{sgy}. Figure IV 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.[9] To gain the level of precision needed to estimate effects on long-run outcomes, we generalize this approach to capture similar types of local shocks to attendance.

IV.B Effects of Local Shocks to Attendance on Outcomes

Our goal in this paper is to estimate the effect of exogenous changes in cohort-level absence rates on long-run outcomes. Given we only have data on the spread of the flu in two recent years, we cannot use this variation to estimate long-run effects; third graders exposed to a bad year of the flu in 2018 are not yet adults. Instead, we generalize the intuition behind isolating exogenous variation in absences caused by the spread of the flu to capture similar spikes in absenteeism in local areas. In addition to allowing us to measure effects on long-run outcomes, we also generalize beyond one specific type of absence and develop an identification strategy that picks up the type of variation caused by the spread of respiratory illnesses more broadly.

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

B.1 Effects on Short-Run Outcomes

We develop an empirical strategy to to isolate variation in cohort-level absence rates arising from transitory, exogenous factors such as the spread of respiratory illnesses. To build intuition, we present a stylized example in 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, student 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 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. To separate between these possibilities and others, we make use of the panel structure of our data.

Adding data on a second cohort rules out the possibility that the spike is generated by year-invariant teacher effects. In the series marked by squares, we show absence rates for the 1993 birth cohort. If the spike were driven by the specific traits of third grade teachers at John Harvard Elementary, then we would see a corresponding spike for the 1993 cohort in 2001, when these students are in third grade. Instead, despite the fact that the absence rates for this cohort are somewhat lower on average, we see a similar spike also in 2000. This is consistent with the type of shock that is specific to the local area around John Harvard Elementary in the year 2000 and affects all students in the school, such as an illness shock.[10]

We parameterize the intuition behind this stylized example to identify local spikes in absenteeism in the data. We start with a panel of absences from 1995 to 2019, covering 57 million student-years. We start by removing the school-by-cohort and school-by-grade components of absence rates. The school-by-cohort component accounts for the fact that certain students may have consistently high or low absence rates (in the stylized example, the 1992 cohort tended to be absent at higher rates than the 1993 cohort). The school-by-grade component accounts for the possible role of year-invariant teacher effects (as we hypothesized with third grade teachers in the example). We also remove the state-wide, year-specific component of absences to remove any variation shared across all schools in a given year (e.g. driven by a recession). Concretely, we estimate the following model:

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

where we regress absences, A_{iy}, for student i, born in birth cohort c who attends school $s(i,y)$ in

[10] In this example, there was a shock in a single year, 2000. In Appendix A, we show a related example with a shock in 5 adjacent years. We show that persistent shocks across years does not lead to bias in our estimates.

year y on school-by-cohort (δ), school-by-grade (γ), and year (ϕ) fixed effects.

The residuals from this model allow us to isolate local, year-specific spikes in absenteeism. We start by aggregating to the school-year level by summing over students i in school s in year y,

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

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_{sy} due to the school-by-cohort fixed effects. Similarly, if absences are high in school s 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.

Depending on what generates these "blips," using $z_{s,y}$ directly may lead to biased estimates of the effect of absences on outcomes. Suppose we decompose local shocks into two components - one that is district-by-year specific and shared across all schools s in a given district d and the remaining component which is school-by-year specific. In Appendix A, we illustrate that when the school-by-year specific component of the shocks is also correlated with a school-by-year shock in outcomes, using $z_{s,y}$ as an instrument for absences in school s will lead to biased estimates of the true causal effect. As an example of one type of school-by-year shock, Ang (2021) shows that acts of police violence in a community lead to both a rise in absences and a decrease in student outcomes and that these effects are hyper local (even within school); only students in a very small radius around the incident are absent at higher rates. This type of variation would enter in the school-specific, as opposed to the shared-district component, and using it would overstate the effect of absences on outcomes by misattributing some of the direct effect of exposure to police violence to the absence channel.

To address this potential bias, we use a leave-out measure of the local shocks, isolating the component of the variation that is shared across schools in a given district in a year. Concretely, we construct a weighted average of the "blips," using other schools in the same district as follows:

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

where $N_{d(s)}$ and N_s are the total number of students in district $d(s)$ and school s, respectively.[11] By using data on the other schools in district $d(s)$ other than s, we remove the component of the variation in absences that is school-by-year specific and may be correlated with shocks to outcomes, as in the police violence example. Appendix A demonstrates in a simple simulation how this method corrects for the bias that would have been present had we simply used z_{sy}. The remaining variation is the year-specific spikes in absenteeism, shared across schools in the same district.

This approach does not rely on the assumption that local shocks to attendance last only one year. As detailed in Appendix B, even if a cohort experiences persistent shocks throughout elementary school (e.g. high flu spread in all five years), we can still detect the shock by using cohorts at the start and end of the shock period (see Appendix Figure A.13). This requires a sufficiently long panel, which in our case spans 25 years. In this scenario, estimates of the local shock's magnitude will be attenuated, as the shock is collinear with the school-by-cohort fixed effect for the cohort that is always exposed, but, this does not introduce bias.[12]

The resulting local, year-specific shocks to absences are consistent with absences induced by the spread of respiratory illnesses and in Appendix Figure A.5 Panel *A* we show that the instrument is directly correlated with the flu. In periods where flu cases are higher, our instrument is also higher.[13] The R^2 of this relationship, however, is not 1. Instead, about 20% of the variation in our instrument is explained by this measure of the spread of the flu.[14] However, there are many other types of upper respiratory track infections that behave similarly to the flu and for which the spread across areas would plausibly impact all schools in a given district, but that we do not measure directly.

In order for our instrument to identify the true causal effect of absences on outcomes, it must be the case that the variation in $\hat{z}_{sy}$ is generated by quasi-random shocks that only impact outcomes through effects on 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. We conduct several

[11] In our current procedure, we do not account for the distance between schools when weighting the $z'_{sy}s$. There are many ways that one could weight across schools in addition to distance. For example, Kuchler, Russel and Stroebel (2022) show that for Covid, social connections were more predictive of spread than distance. Not accounting for these other factors will simply weaken the first stage relationship between $\hat{z}_{sy}$ and absences in school s in year y, but will not bias our IV estimates.

[12] What matters for the IV estimates is not that the magnitude of the local shocks is unbiased, but rather that we are detect some of this exogenous variation in absence rates. We show in simulations in Appendix B that this is the case and that the corresponding IV estimates are unbiased even in the presence of correlated shocks.

[13] In Panel *B* of the same figure, we show that this relationship is unique to the contemporaneous measurement of the instrument and of flu cases. Flu cases in one period have essentially no relationship with the value of our instrument in the prior period.

[14] In addition to the correlation, we also show in Appendix Figure A.6 that the spatial autocorrelation of the z_{sy}'s across nearby schools is very similar to that of the flu. Note we use z_{sy}, as opposed to $\hat{z}_{sy}$, because much of the variation in $\hat{z}_{sy}$ is shared across nearby schools because it is aggregated over all schools in the district.

tests to rule out selection on observables and unobservables after presenting the main results.[15]

We estimate the effect of shocks to cohort-level absence rates on contemporaneous test scores using the following first stage and reduced form specifications:

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

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

We instrument for the absence rate, A_{scg}, in school s, birth cohort c, and grade g using our leave-out measure of local shocks for school s in the corresponding year y. Because we have test scores in grades 3-5, we fully interact our controls with grade; this produces results analogous to averaging over grade-specific regressions. We include cohort-by-grade fixed effects, δ_{cg}, and school-by-grade fixed effects, γ_{sg}. For test scores, our panel spans more than 20 years, so in our baseline specifications we allow for a school-grade specific trend in birth-cohort, $\Gamma_{sc} \cdot c$ allowing teacher effects to evolve over time as personnel changes.[16] The reduced form specification is analogous, replacing absences with standardized test scores as the dependent variable.

We show in Figure V Panel *A* that the first stage is strong and absences in the focal school are closely related to our leave-out measure of local shocks to attendance. When there are "blips" in absences in nearby schools, students in the focal school are absent at higher rates. The instrument generates substantial variation; moving from the 5th to 95th percentile of the instrument, after controlling for the set of fixed effects described above, generates an additional 0.5 absences per student per year on average. In a cohort of 100 students, the instrument generates 50 additional absences, spread across students and days In our sample, the increase is equal to nearly 0.5 standard deviations in the distribution of school-cohort absences for elementary students.

Panel *B* presents the reduced form, which shows a clear negative relationship between test scores and spikes in absences in nearby schools. Students who are in grade g when absences in other schools are high, score lower on standardized math tests in that grade, relative to their peers in the same school, in other birth cohorts. In addition to the reduced form coefficient, we report the IV coefficient in Panel *B*. For a 1 day per student increase in absences on average (100 total absences in a cohort of 100), test scores fall by 3.8% of a standard deviation. We have substantially more precision with this design relative to using the flu as an instrument directly; we can rule out effects smaller than a 2.6% of a standard deviation decrease in test scores.

We show that our results are robust to alternative sets of controls. Our baseline estimates suggest that raising absences rates by one student per day on average lowers standardized test scores by approximately 4% of a standard deviation. In Table II we show that this result is not

[15] One potential concern is that the flu and other respiratory illnesses have a direct effect on outcomes. We also return to a more formal discussion of this after showing the main results.

[16] We show in Table II, that our results are not sensitive to this inclusion.

sensitive to the inclusion of the school-specific linear trend in cohort and that removing this changes the treatment effect only slightly. We also show that we can include school-by-cohort fixed effects, in line with the within-kid design that prior work has used. In this specification, we ask for a given cohort whether their third grade scores are relatively lower than their fourth grade scores if, during third grade, students in nearby schools missed school for exogenous reasons at higher rates than in fourth grade. Here we find similar, if somewhat smaller treatment effects, than in our across-cohort specification. Nonetheless, we show that test scores are lower when absences induced by the instrument are higher.

How can we be sure that outcomes in the focal school are not low for reasons other than increased absences? We perform several tests to assess the plausibility of the identifying assumption that the variation in $\hat{z}_{sy}$ is generated by quasi-random shocks that only impact outcomes through effects on absences. We might have been concerned that when the value of the instrument is high, schools have higher rates of FRPL-eligible students or inexperienced teachers, which could both raise absence rates and lower outcomes. In Figure VI Panel *A*, we show this is not the case; the instrument is balanced on a comprehensive set of observable, time-varying student and teacher characteristics.[17] Similarly, we could have been concerned that variation in the instrument is driven by other, observable local shocks like increases in unemployment rates that both increase absences and have been shown to negatively impact students' outcomes (e.g. Page, Stevens and Lindo, 2009; Stevens and Schaller, 2011; Jackson, Wigger and Xiong, 2021). In Figure VI Panel *B*, we test for this relationship directly and show there is no relationship between the instrument and local unemployment rates.[18] We are able to rule out local unemployment shocks directly, but there could be other neighborhood shocks, e.g. a rise in crime, that we do not directly observe.

We develop outcome-based placebo tests in the style of Danieli et al. (2024) to tighten the bounds on possible unobserved shocks that pose a threat to identification. Suppose that the true model were:

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

where outcomes, Y_{scg}, in school s for birth cohort g in grade g, were determined by the true causal effect of absences, β, fixed school-by-cohort, ν_{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}

[17] We show that the instrument is uncorrelated with the presence of non-white teachers, a trait that has been linked to lower absence rates for students of color (Holt and Gershenson, 2019; Tran and Gershenson, 2021; Gershenson et al., 2022)

[18] In Appendix Figure A.7 we show that there is no relationship, even around the Great Recession (2008-2012), when there were substantial changes in local unemployment rates.

would lead to biased estimates of the true effect, β. We start with a case in which 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 $y+1$ is also at least partially driven by the increase in crime— i.e. $\text{Cov}(\hat{z}_{s,y(c,g)+1}, \varepsilon_{scg}) \neq 0$. If the effect of the instrument runs purely through absences in the current year, we should not expect test scores this year to be correlated with absences in nearby schools next year.

Figure VII Panel *A* shows there is no correlation between test scores in year y and the value of the instrument in year $y+1$. The figure shows a binned scatter plot analogous to the reduced form, but instead of measuring the instrument contemporaneously, $\hat{z}_{sy}$, we measure it in the subsequent year, $\hat{z}_{s,y+1}$.[19] If our results had been driven by an unobserved shock that persisted for at least a year, then we would have expected relationship like in Figure V*B*, but instead the relationship is totally flat. We can reject that our reduced form effects were driven by unobserved shocks such as an increase in crime that last for at least one year.

We might still be concerned, however, about local shocks that are more transitory and do not persist for a full year. For example, the spikes in crime could last only for a few months and have gone undetected in the prior test, but still have biased our IV estimates. We use a related placebo test to tighten the bounds further and rule out bias driven unobserved shocks that persist across six-week periods. Using the same logic, if our reduced form effects were driven an increase in crime that lasted for at least six weeks, that would generate a correlation between test scores today and the instrument in periods after the students take the exam in the spring because crime rates continue to be high even after the exam. That correlation would similarly indicate a possible exclusion violation, as absences after the exam could not have impacted test scores in earlier periods.

We do not find evidence that our results are driven by unobserved shocks that last for at least six weeks. We construct versions of the instrument, $\hat{z}_{spy}$, for each of our six absence periods period ($p \in [1,6]$), using an analogous procedure as described above in equation 3. We then regress test scores in year y on the series of within-year instruments and plot the reduced form coefficients in Figure VII Panel *B*. In periods prior to the exam, to the left of the dashed line, the reduced form coefficients are negative and stable. Students take the exams in the spring as indicated by the line. In periods 5 and 6, after the exams are taken and to the right of the dashed line, the pattern is very different. The coefficients are much smaller in magnitude and we cannot reject the null that they are zero. Using the same logic as we used for shocks that persist across years, this test rules out local shocks that directly impact both absences and outcomes and persist for at least six weeks.

Given the results from the placebo tests, the remaining concern is that there are unobserved shocks that directly impact both absences and outcomes for students, and yet dissipate within six

[19] In the Danieli et al. (2024) framework, $\hat{z}_{s,y+1}$ is a "negative control instrument".

weeks.[20] We showed that at least part of the variation in the instrument is explained by variation in spread of the flu. The flu and other respiratory illnesses have this transitory nature and so if these illnesses also directly impact student's outcomes, even when they are not absent that could violate our identifying assumption. While there is evidence that exposure to the flu can be harmful for child development and has long-term consequences on labor market outcomes, that literature emphasizes exposure in utero or during infancy (Almond, 2006; Currie and Schwandt, 2013; Ward and Phipps, 2014; Schwandt, 2018; Daysal et al., 2022). Importantly, we focus on exposure during adolescence, which is after the major period of neural and physical development during early childhood (Currie and Almond, 2011). Nonetheless, we directly evaluate the extent to which the relationship between the instrument and test scores is driven by absences, as opposed to other effects of respiratory illnesses, by estimating the first stage and reduced form in 10 separate samples based on prior absence rates.

If the effect of our instrument does in fact run through absenteeism, then we expect to see large reduced form effects in settings where the first stage is also larger. Instead, if the results were driven by other effects of respiratory illnesses, the reduced form could be large regardless of whether students happen to be absent or not, because what matters is the spread of the illness, not the subsequent absences. We test this in Appendix Figure A.8. We split school-cohorts into 10 bins based on the prior year's absence rate and estimate the first stage and reduced form separately in each bin.[21] We plot these coefficients as well as the line of best fit. There is variation in the first stage across the bins and on average in the cases where the first stage is stronger, the reduced form is also larger in magnitude. The pattern is broadly consistent with the effect of the instrument running through absenteeism.

Having extensively validated our empirical design in the short-run, we turn to asking whether these absences matter later in life or whether they only matter for the test and dissipate? In order for differences in absenteeism to contribute to gaps in graduation and earnings between low- and high-poverty schools, absences need to have a causal effect on these later-life outcomes. 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 this deficit

[20] Note that because we use a leave-out measure for the instrument, capturing variation in attendance that is shared across nearby schools, it must be the case that these transitory shocks are not so local that they would only impact one school in the district, as in Ang (2021). If $\hat{z}_{sy}$ were high because there was a hyper-local shock for a school nearby to school s, this would not create bias. In this case, there would be no first stage, because that shock would not be related to absences in school s.

[21] One way to think about this is as using last year's absence rates as an instrument for the first stage, as opposed to endogenously cutting on places where the observed first stage is higher versus lower. In that case, we might worry that there is a unobservable difference driving the first stage, which is also correlated with heterogeneity in the LATE.

may compound in 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, we adapt our 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 we generate a measure of exposure, aggregating over our single-year instrument between first and ninth grade to capture exposure to local illness shocks over a student's time in school. We end our exposure measure in ninth grade, prior to when students can endogenously choose to leave school or drop out. Concretely, for each student i we construct

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

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.[22] We then link students back to where they begin elementary school, in kindergarten, and construct the average exposure for students in that school-cohort:

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

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 constructed, we turn to estimating first stage and reduced form models, similar to the ones we used to estimate test score effects. Concretely, we estimate

$$A_{sc} = \beta^{FS}\hat{Z}_{sc} + \delta_c^{FS} + \gamma_s^{FS} + \varepsilon_{sc}^{FS} \tag{6}$$

$$Y_{sc} = \beta^{RF}\hat{Z}_{sc} + \delta_c^{RF} + \gamma_s^{RF} + \varepsilon_{sc}^{RF}. \tag{7}$$

[22] We allow for students to move across schools between grades 1 and 9, rather than fixing them to the path of schools implied by where they first enroll in elementary school. In many cases, the inferring the exact expected path of schools is not straightforward, so we do not take that approach.

Note that in these specifications, there is no g subscript, because we have aggregated over grades. We now include cohort, δ_c, and school, γ_s, fixed effects, where $s(i)$ indexes the school where student i attended kindergarten.

We start by showing that exposure to absences in nearby schools between grades 1 and 9 does predict higher absences for students in the focal school. The first stage relationship is shown in Figure VIII Panel *A*. As in the single year case, overall exposure to local absence shocks substantially raises absences, now average total absences between grades 1 and 9, in the focal school. Moving from the 5th to 95th percentile of the instrument distribution increases absences by approximately 1.5 days per student, or 150 total days in a cohort of 100 students. Recall that the gap in average absences in low- and high-poverty schools in Figure I was approximately 6 days per student; 1.5 days is substantial. How does the increase in absences impact outcomes?

Increases in cohort-level absences between grades 1 and 9 lower high school graduation rates. Panel *B* of Figure VIII 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. We also show the IV coefficient, which is scaled for an increase of one day absent per student (total over grades 1-9). We find that an increase in absence rates of 1 day per student on average over this periods 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 our instrument, where college attendance measures whether students ever attend a four-year college between the age of 18 and 22. We define an indicator equal to one for any student who ever enrolls in such a school, using data from THECB for in-state enrollment, and from the National Student Clearinghouse for schools outside of Texas. As with graduation, we find that exposure to local absence shocks during elementary and secondary school lowers the probability that students attend 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. The reduced form relationship between earnings and our instrument is shown in Panel *D*. 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 $78 to a decrease of $734.[23]

[23]To further assess the statistical significance, we conduct a permutation test, as shown in Appendix Figure A.10.

The earnings effects are not driven by attrition from the unemployment insurance records. Because we only have earnings data for people who work in Texas, one concern is that absences causes attrition from the data, e.g. by causing people to move out of the state. We show this is not the case in Appendix Figure A.9 Panel *A*; we regress the fraction of the cohort ever appearing in the UI records on the instrument and cannot reject that the slope is 0. 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.

We also show that the decrease in earnings is not driven by delayed entry to four-year colleges. We found that students were less likely to attend a four-year college between ages 18 and 22, but it could have been the case that absences cause students to delay entrance, but they do eventually matriculate. Appendix Figure A.9 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 treatment effects we estimate on long-run outcomes are not sensitive to the inclusion of time-varying controls for school and community characteristics. We construct aggregated measures of the covariates used in Figure VI over grades 1-9 (e.g. the average fraction of male teachers that students were exposed to between grades 1 and 9 or the average local unemployment rate in the city over those years) and include those as controls in our IV estimates. Table A.1 shows that our IV estimates are essentially unchanged when we add controls for these characteristics.

How much of the outcome gaps between students in low- and high-poverty schools can be explained by the causal effect of higher absence rates in high-poverty schools? In Figure X, we show how high school graduation rates and average earnings would increase in high-poverty schools if attendance rates in high-poverty schools were equal to those in low-poverty schools. The counterfactual is an extrapolation beyond the observable variation in our instrument, which lowered absences by about 1.5 days per student compared to 6 days per student required to equalize absence rates. Nonetheless, it provides one way to quantify the overall role of absences in explaining disparities in outcomes between low- and high-poverty schools. We find that differences in absenteeism explain a substantial portion of the gap; lowering absence rates in high-poverty schools to equal the absence rates in low-poverty schools would increase graduation rates by 1.6 percentage points and increase average earnings at 25 by $2,500, based on our IV estimates. Equalizing attendance rates would close approximately one-fifth and one-third of the high school graduation and earnings gaps, respectively.

Specifically, we randomly reshuffle the $\hat{Z}_{sc}$ instruments across schools and cohorts and recompute the IV earnings effects 10,000 times. This non-parametric approach constructs a distribution of placebo treatment effects under the null hypothesis of no causal relationship, allowing us to evaluate whether our observed IV estimate stands out against this distribution. We find that the distribution of placebo effects is centered near zero, and the true IV estimate is in the tail, consistent with the p-value from our baseline regression.

We find that even fairly small changes in cohort-level absence rates have substantial effects on later-in-life outcomes. Importantly, these effects incorporate possible spillover effects of one student's absences onto other students. As such, we should not compare the magnitudes to alternative policies like lengthening the school year by one day, which does not incorporate these spillover effects.[24]. We cannot directly estimate what fraction of our effects are caused by individual- versus group-level effects. However, as an additional piece of evidence in support of spillovers, we compare our IV effects to the OLS relationship between individual-level absences and own-outcomes, which we might think of as an upper bound for the true causal effect. Figure IX shows these comparisons for test scores and high school graduation. In both cases, we find IV estimates that are somewhat smaller than group-level OLS estimates, which we showed in Section III, and somewhat larger than the individual OLS estimates.

Our results show that students who experience higher levels of attendance cohort-wide for exogenous reasons, such as mild years of illness spread, do substantially better in the long-run. This suggests that if high-poverty schools could systematically improve their attendance rates, students would benefit both in the short run, through improved in-school performance, and in the labor market as adults. How can schools improve attendance? Would these efforts yield improvements in outcomes consistent with our estimates? In the next section, we evaluate the effects of a program that aims to reduce absenteeism in high-poverty schools on attendance, academic performance, and later-life outcomes.

V Effect of an Absence Intervention in High Poverty Schools

Our results in the previous section suggest that students in high-poverty schools fail to take advantage of existing resources due to high levels of absenteeism and differences in absence rates contribute substantially to the observed outcome gaps between students in high- and low-poverty schools. Do interventions that specifically target absenteeism close these gaps?

We focus on one model, called "Communities in Schools" (CIS). CIS is the country's largest implementation of an "integrated student support" program; a program that coordinates resources between schools and communities to address the specific needs of each student (Moore et al., 2017). We leverage the roll-out of CIS across schools in Texas to estimate the effect of program-access on attendance and test scores in elementary schools. We use our IV estimates to decompose the total effect on test scores into the portion due to changes in absenteeism and the portion due to other effects of CIS. We can then project the attendance effects onto high school graduation and adult earnings using the long-run IV estimates from Section IV to construct counterfactual

[24]There is mixed evidence on the effect of changes in the length of the school year and instructional time on both short- and long-run outcomes (e.g. Pischke, 2007; Goodman, 2014; Aucejo and Romano, 2016)

outcomes similar to those in Figure X in a world where all high-poverty elementary schools have access to CIS.

V.A Background on Communities in Schools

Communities in Schools is the largest and one of the longest running integrated student support programs in the country. It dates back to the late 1970's and today serves students in more than 3,000 schools across the country. In the 2021-22 school year, CIS worked with approximately 2 million students in 25 states and the District of Columbia (Communities in Schools, 2023). Importantly, CIS has a particularly strong presence in Texas; approximately half of all schools with CIS programs are in Texas. Unlike in other states, CIS is written into the state education charter and receives funding directly from the Texas Education Agency.[25]

In Texas, there are 27 local CIS affiliate organizations, each of which sits under the state and national umbrella organizations. These local affiliates serve regions of the state – e.g. CIS of Houston or CIS of Central Texas, which serves Austin. Within these regions, each CIS affiliate operates in specific schools. For example, in the 2022-23 school year, CIS of Houston provided services in 155 schools across 6 local school districts, covering approximately 15% of all schools in these districts. CIS specifically targets schools in high-poverty neighborhoods.[26] All students in CIS-affiliated schools are eligible to receive CIS services.

The key element of the CIS model is the "site-coordinator," who serves as a navigator and connects students to resources that they need to succeed. There is a growing body of evidence in other settings that providing people with personalized services through a navigator can be highly impactful (e.g. Bergman et al., 2024; Evans et al., 2020, 2023; van der Steeg, van Elk and Webbink, 2015; Weiss et al., 2019). Part of the site coordinator's role is to connect with students as a caring adult and a mentor. Another part is to help broker connections to resources provided by the school, the government, or the local community. Correspondingly, a large number of CIS site coordinators are formally trained social workers (Communities in Schools, 2023).

CIS takes an "any means necessary" approach to improving attendance and student outcomes more broadly. There are countless reasons that students in these schools may not attend regularly, many of which revolve around challenges students face outside of the classroom. Students may lack transportation options, face mental or physical health challenges, lack access to clean clothes, or face food insecurity. Students can be referred to the site coordinator, whose job is to connect students with the resources they need to address the specific challenges they face. Many of these

[25] In 2022, the state provided about $33 million to CIS programs in Texas. In addition, CIS received about $4 million of Texas TANF funds (Agency, 2024).

[26] Figure 1 in Goldman, Gracie and Porter (2023) shows the correlates of neighborhoods with CIS programs in states across the country.

challenges not only prevent students from attending school on a regular basis, but they also affect students' ability to engage and be ready to learn even when they do attend. Connecting students to resources can have impacts on both channels. We estimate the total effect of CIS on outcomes and then decompose the effects into two parts— the portion explained by attendance and the portion that runs through other channels.

V.B Causal Effect of CIS on Absences and Outcomes

We leverage the timing in when schools get CIS programs to estimate the impact of CIS on attendance and academic performance. Because CIS programs are not randomly assigned to schools, we cannot simply compare outcomes in schools that have a CIS program to schools that never have a CIS program. Instead, we take a staggered adoption approach, comparing outcomes in schools that got a CIS program somewhat earlier to those that got a program somewhat later. The key identifying assumption for this approach is that the exact timing of when a school gets a CIS program, is quasi-random.

For each school with an active CIS program, we have the year the program began, which allows us to determine which students in each school have been treated. We limit our analysis to elementary schools, where CIS programs emphasize attendance improvements specifically and focus on outcomes for cohorts of 5th graders (Somers and Haider, 2017).[27] Concretely, if a program begins in an elementary school in the 1999-2000 school year, the 5th graders in the spring of 2000 are the first cohort treated by CIS. The 5th graders in prior year, the spring of 1999, had already progressed to middle school by the time the program began and therefore were not treated by CIS. We define the "treatment cohort" as the oldest birth cohort to be treated in the year a CIS program beings (5th graders in 2000—the 1990 birth cohort—in the example).

For each treatment year, we define a control group from the schools that receive a CIS program in later years. For each treatment cohort t, the control group is defined as all schools with a treatment cohort more than five years later, $t' > t+5$. The construction of the control group allows us to measure treatment effects for up to five cohorts of 5th graders following the introduction of CIS in the treated schools.

We follow the approach in Sun and Abraham (2021), stacking the data for various treatment cohorts and the associated control schools. We assign the sets of treatment and control schools a group-treatment-cohort, which serves as an ID, and is equal to the treatment-cohort for the treated schools in that group.[28] We run an event study with a five cohort pre- and a five cohort post-

[27] In high schools, for example the focus is less on attendance more on getting students to successfully graduate.

[28] For example, students in schools that got a CIS program in 2008 can serve as controls for programs that first got a program in 2000 and 2001. A student in a school that got the program in 2008 will separate rows serving as a control for the 2000 and 2001 schools and have a group-treatment-cohort identifier equal to 2000 and 2001, respectively.

period. To account for the fact that we have stacked the data and the same schools can serve as controls for multiple treatment years, we fully interact our specification with indicators for the group-treatment-cohort.[29] Concretely, we estimate:

$$A_{i,\bar{y}} = \sum_{\substack{t\in[-5,5]\\ t\neq -1}} \beta_t \mathbf{1}\{y(i) - \bar{y} = t\} \cdot T_{s(i)} + \delta_{y(i),\bar{y},r(i),p(i)} + \phi_{s(i),\bar{y},r(i),p(i)} + \varepsilon_{i,\bar{y}}, \tag{8}$$

where $A_i,\bar{y}$ is the number of days in 5th grade that student i in school $s(i)$ is absent and $\bar{y}$ identifies the group-treatment-cohort. One potential concern is that families select into schools that have CIS programs. While we don't believe this is particularly likely, we assign students to schools invariantly based on where they enroll in third grade. This does not entirely alleviate concerns about school selection in the post-period, but separates the choice somewhat from when we measure outcomes in 5th grade. $y(i)$ is the year that student i is in 5th grade and $\bar{y}$ is the group-treatment-cohort in that row (i.e. the first year that 5th graders were eligible for CIS in treated schools for that group-treatment-cohort). We restrict the treated schools to those with a CIS program beginning between 1999 and 2009. $T_{s(i)}$ is an indicator equal to one if the student i is in the treatment group for this group-treatment-cohort and equal to zero if in the control group. In addition to group-treatment-cohort interactions, the controls are fully interacted with student race/ethnicity, $r(i)$, and student eligibility for FRPL, $p(i)$. The result is equivalent to estimating the event study separately by ethnicity-income group and aggregating. These interactions account for the possibility that the composition of students in CIS schools is differentially changing over time. The fixed effects in δ capture average outcomes for untreated students in each group-treatment-cohort-by-race-by-income cell. The β_t coefficients capture the path of estimated treatment effects in the five year pre- and post-periods.

The key identifying assumption for this design the standard parallel trends assumption: that other than for the introduction of the CIS program, the absence rates in treated and comparison schools would have maintained the same pre-period difference in outcomes. Time-invariant differences between treatment and control schools do not pose a threat to identification. Instead, it must be the case that nothing other than the introduction of CIS changes in the treated schools relative to the control schools. For example, suppose schools who get CIS programs also get new principals at the same time who make many changes including bringing in CIS but also hiring better teachers. That would confound our estimates of the effect of CIS with the effect of better teachers.

We believe the exact timing of when a school gets a CIS program to be idiosyncratic. From discussions with CIS administrators, we do not believe that individual schools have substantial control over when a program begins, as there are many external constraints in terms of funding

[29] Schools can enter the data in multiple group-treatment-cohorts, but the data are unique within a group-treatment-cohort.

and the sequencing across schools, etc. Among characteristics of students and teachers that we can observe, we do not find evidence of differential changes in treated schools when CIS programs begin. We provide one example of this in Appendix Figure A.11. We estimate a version of Equation 8 where the dependent variable is the fraction of teachers in a school in a given year who are new to the school. We find no effect of CIS on this outcome or the other covariates included in Figure VI Panel *A*. The identifying assumption is that other, unobservable characteristics that are correlated with students' potential outcomes also do not change in treatment schools relative to control schools when CIS begins.

We start by estimating the effect of CIS on absences in 5th grade. Figure XI Panel *A* shows the β_t coefficients from Equation 8. Prior to when CIS enters the treated schools, absence rates evolve similarly in the treated and control schools; we do not find evidence of differential pre-trends. After the program begins, absence rates begin to decline in the treated schools. By the time the program has been in the treated schools for several years, students in the treated schools are attending approximately 0.5 additional days of 5th grade on average.

In addition to the β_t coefficients, we also provide a difference in differences coefficient from a more parsimonious specification:

$$\begin{aligned} Y_{i,\bar{y}} = {} & \beta_{post,0-2}\mathbf{1}\{0 \leq y(i) - \bar{y} \leq 2\} \cdot T_{s(i)} + \beta_{post,3-5}\mathbf{1}\{3 \leq y(i) - \bar{y} \leq 5\} \cdot T_{l(i)} \\ & + \tilde{\delta}_{y(i),\bar{y},r(i),p(i)} + \tilde{\phi}_{s(i),\bar{y},r(i),p(i)} + \varepsilon_{i,\bar{y}}, \end{aligned} \tag{9}$$

where we estimate two post-period coefficients – $\beta_{post,0-2}$ and $\beta_{post,3-5}$. $\beta_{post,3-5}$, the coefficient shown on Panel *A*, is the average treatment effect in years 3-5 relative to the entire pre-period. The fixed effects included are analogous to those in Equation 8. We show that CIS improves attendance in 5th grade by 0.46 days per student several years after the program begins. In a cohort of 100 students, CIS reduces absences by 46 student-days per year. The program is effective in improving attendance in high poverty schools. We turn next to asking whether the intervention ultimately improves outcomes for students.

We estimate the treatment effect of CIS on fifth grade test scores directly using Equation 9. We show in Table A.3 that CIS improves fifth grade test scores substantially. On average, between three and five years after a CIS program begins, test scores have increased by 0.05 standard deviations in the treated schools relative to the control schools. When students have access to resources that address their basic needs, outcomes in those schools improve.

We show that a substantial portion of the treatment effect of CIS on outcomes can be explained by the improvements in attendance that the program generates. In Figure XI Panel *B*, the bar on the left shows the DID treatment effect of CIS on fifth grade test scores. We compare the total effect to the component that is explained by the decrease in absences, shown on the right. To compute

this measure, we scale the CIS treatment effect on absences by the IV estimate from Section IV.[30] 45% of the overall effect of CIS on outcomes can be explained by the effects on attendance.

Nearly half of the effect of CIS on test scores runs through absences, but it is not surprising that there are also other channels through which the program impacts outcomes. In recent years, we have additional data on which students were referred to site coordinators and why. In 2014, among students referred to a site coordinator for attendance challenges, one of the most common categories of services students received is related to "self-esteem enhancement".[31] Improving a student's self-esteem may encourage them to attend more regularly, but of course can also have a direct effect on their experience in school even on days that they otherwise would have attended. Similarly, several thousand students with attendance problems were sent home with food in their backpacks. Reducing hunger can similarly impact students through multiple channels. The CIS model attempts to alleviate the very real challenges that students in high-poverty schools face that prevent them for attending school consistently. This is in contrast to an alternative approach to attendance, which focuses on nudges and which we might not expect to have direct effects on outcomes (e.g. Rogers and Feller, 2018; Smythe-Leistico and Page, 2018, 2019; Lasky-Fink et al., 2021). Nonetheless our results speak to just how important the attendance channel is for the total effect of CIS that we document.

If a policy intervention could increase attendance by the same amount that CIS does, how much would that policy improve outcomes in high-poverty schools? We start by estimating treatment effects on overall attendance in elementary school using the difference in differences specification in Equation 9, where the dependent variable is total days absent between 1st and 5th grade. We find that four years of exposure to CIS improves attendance by 1.9 days per student on average (see Appendix Table A.3).[32] Using the estimated reduction in absenteeism, we predict impacts on high school graduation and earnings and return to the outcome gaps between low- and high-poverty schools as we did in Figure X using the IV estimates on long-run outcomes from Section IV. We compute counterfactual outcomes for a world in which all high-poverty schools get an absence reduction equal in size to that caused by 4 years of access to CIS.

Figure XII shows these counterfactuals. Our IV estimates imply that reducing absences in high-poverty schools by the amount that CIS programs reduce absences would increase high school graduation rates in high-poverty schools by 0.50 percentage points. This which would close nearly 7% of the gap in graduation rates between students in high- and low-poverty schools. Similarly,

[30] Our baseline estimates in Section IV were average effects for scores in grade 3-5. For this exercise we use 5th grade specific estimates from Appendix Table A.2.

[31] This is consistent with Gottfried (2014) who shows that chronic absenteeism is correlated with lower levels of socioemotional outcomes for students.

[32] We scale to four years of exposure which is the average number of years that students in the cohorts 3-5 years after the program begins have access to CIS.

our estimates suggest that an absence reduction of this size would increase average earnings at age 25 by $760, closing approximately 11% of the gap in young adult earnings in this setting.

Our estimates show that policy interventions that target absenteeism can make a real progress towards improving disparities in outcomes between low- and high-poverty schools. For context, the increase of $760 in our sample from four years of CIS is approximately equal to a 2.9% increase in annual earnings. Having a one standard deviation higher quality teacher for two years raises annual earnings by approximately the same amount (Chetty, Friedman and Rockoff, 2014). CIS targets absenteeism, but does not change traditional school inputs, like teacher quality. Yet, it has impacts that are on par with those types of investments.

The counterfactuals considered only attendance improvements as large as those caused by CIS, but four years of exposure to the actual program has larger total effects on students' long-run outcomes. In related work, Goldman, Gracie and Porter (2023) estimate the effect of CIS on adult earnings nationwide, using administrative data from the US Census and the IRS. In that paper, we find that four years of CIS increases adult earnings at age 25 by approximately $1,350 on average. Just as with test scores, approximately half of this effect can be explained by the effect of CIS on attendance. We show this estimate in XII Panel *C*. If we were to use this total earnings effect in the counterfactual, we estimate that providing high-poverty schools with four years of CIS would close approximately 1/5 of the gap in adult earnings between students in high- and low-poverty schools.

CIS is effective at improving attendance and student outcomes and approximately half of the effects on outcomes can be explained by the effects on attendance. Consistent with our estimates in Section IV, we show that when high poverty schools actually improve the rate at which students attend, outcomes improve in turn. This doesn't mean that all interventions that improve attendance will necessarily have the same effects. However, our estimates suggest that integrated student support programs that target absenteeism may be one effective method for improving outcomes in the long-run for students in high-poverty schools.

VI Conclusion

There are substantial gaps in outcomes between students who attend high and low-poverty schools. In addition to having students from higher income families, low-poverty schools tend to have higher quality teachers, better infrastructure, more access to advanced academic programs, and greater parental involvement, all of which may contribute to better outcomes (e.g. Hanushek, 1992; Reardon, 2018). They also have substantially better attendance rates. Our results show that differences in attendance rates can explain up to one third of the differences in outcomes between students in high- and low-poverty schools. We also show that policy interventions in high-poverty

schools that target absenteeism can make substantial progress in closing these gaps, even holding fixed other types of school resources.

We show that shocks to cohort-level absence rates, even prior to high school, have negative effects that persist into adulthood. Importantly, these effects not only account for the direct effect of absences on students who miss school, but also any negative disruption costs of absences on students who attend. This has important implications for students today, who have been exposed to record-high levels of absenteeism in the aftermath of the pandemic (Malkus, 2024). Our results, though estimated prior to the pandemic, provide context for these increases. In 2022, students in Texas elementary schools missed approximately 4 additional days per student on average, compared to pre-pandemic rates. Our estimates imply that this increase alone would lower high school graduation rates and four-year college attendance rates by 1.2 and 4.5 percentage points, respectively. However, there has been substantial effort nationwide to get these students back on track and our paper suggests that these efforts, if successful, can have lasting effects on student outcomes (e.g. Bakuli and Mauriello, 2023; Feinberg, 2023; of Education, 2023; Cineas, 2024; Jordan, 2024; MacGillis, 2024).

One program that has ramped up efforts to get students back in school is the very one we focus on, Communities in Schools. In our data, we can see that in Texas that in 2022, twice as many students were referred to CIS site coordinators for attendance challenges relative to in 2019. Evidence suggests that students today are missing for a variety of non-school related reasons – e.g. no access to transportation, caring for siblings, mental health challenges (Mervosh and Paris, 2024). We show that when CIS programs begin in high-poverty schools and students get connected to the resources they need, attendance rates and outcomes improve. And in the long-run, adult earnings increase by approximately 6% annually (Goldman, Gracie and Porter, 2023).

Our results highlight the broader importance of student engagement with school. Regular attendance enables students to fully benefit from existing school resources and may enhance the returns to investing in higher-quality teachers or other improvements in high-poverty schools. Moreover, consistent attendance can strengthen peer relationships, which are predictors of both in-school success and long-term economic opportunity (e.g. Lavy and Sand, 2012; Chetty et al., 2022; Lleras-Muney et al., 2022; Alan, Carlana and Leone, 2024). Efforts to improve attendance could therefore complement interventions aimed at improving other dimensions of schools. These complementarities are especially relevant given current challenges with absenteeism and may have significant implications for the rates of economic mobility that students today will experience.

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TABLE I: Summary Statistics

	Mean	Standard Deviation
A. Student Characteristics		
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
B. Teacher Characteristics		
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
C. Absence Rates		
Days Absent per Student (single grade 3-5)	5.1	1.2
Days Absent per Student Grades 1-9	49.5	7.7
D. Long-Run Outcomes		
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 our 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 our baseline definition of absence rates. Our 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. Our long-run analysis uses the total number of absences between grades 1 and 9. In Panel D, we provide statistics on the long-run outcomes of interest. See Section II for additional details on their construction. Finally, we present 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	IV	IV	IV
	(1)	(2)	(3)	(4)
Absences in Grade	0.145	-0.038	-0.050	-0.023
	(0.003)	(0.006)	(0.006)	(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 our 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 V Panel *A*. In Column (2), we present 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 V 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. In Columns (3) and (4), we 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, we cluster standard errors at the school-by-cohort level.

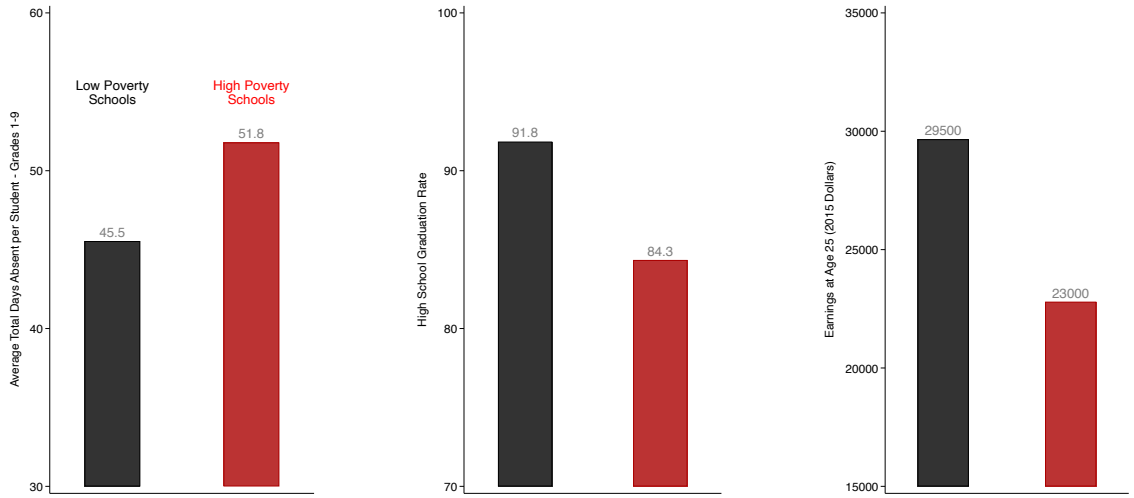
TABLE III: Effect of Cohort-Level Absences on Long-Run Outcomes

		IV Estimates		
	First Stage	HS Graduation	4-Yr. College Enroll.	Earnings Age 25
	(1)	(2)	(3)	(4)
Total Absences Grade 1-9	0.416	-0.261	-0.978	-401.060
	(0.038)	(0.060)	(0.181)	(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 our 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 VIII Panel *A*. In column (2)-(4), we 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 bewteen ages 18 and 22, and earnings at age 25. These relationships are depicted graphically in Panels *B*-*D* of Figure VIII. 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. In each specification, we include school (the school where students attended kindergarten) and birth cohort fixed effects and cluster standard errors at the school-by-cohort level.

FIGURE I: Absences and Outcomes in Low- and High-Poverty Schools

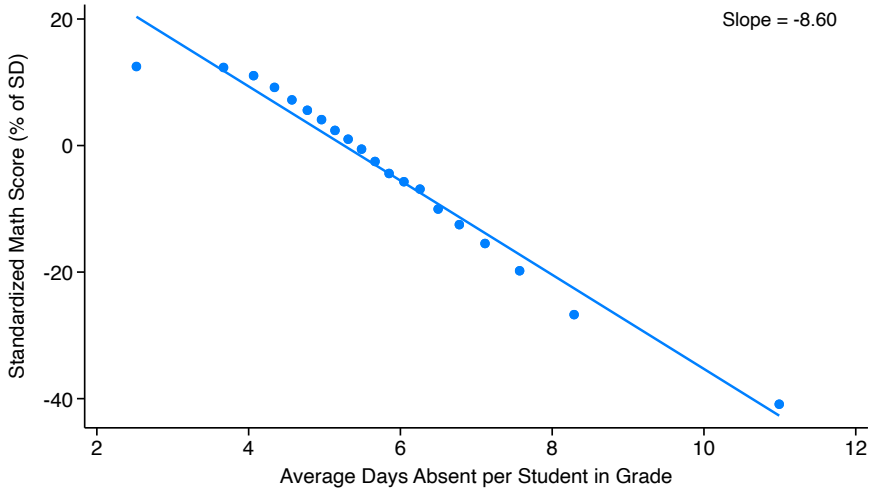
A: Absences Grade 1-9 *B*: High School Graduation *C*: Earnings at Age 25



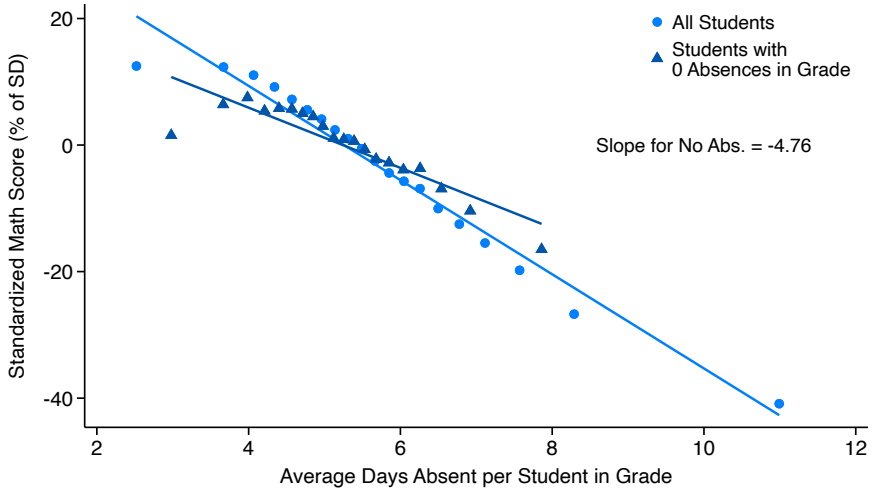
Notes: This figure summarizes differences between low- and high-poverty schools. We construct poverty rates for each school-by-birth cohort cell by calculating the average number of years students are eligible for free and reduced price lunch between grades 1 and 9, assigning students to the school they attended for kindergarten. Low-poverty schools (in black) are the bottom 20% of school-cohorts by this measure and high-poverty schools (in red) are the top 20% of school-cohorts by this measure. In Panel *A*, we plot the average number of days absent per student between grades 1 and 9 in low- and high-poverty schools. Panel *B* shows the average high school graduation rates and Panel *C* shows the average labor market earnings at age 25 for both types of schools. See Section II for additional details on how these variables are defined.

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

A: All Students

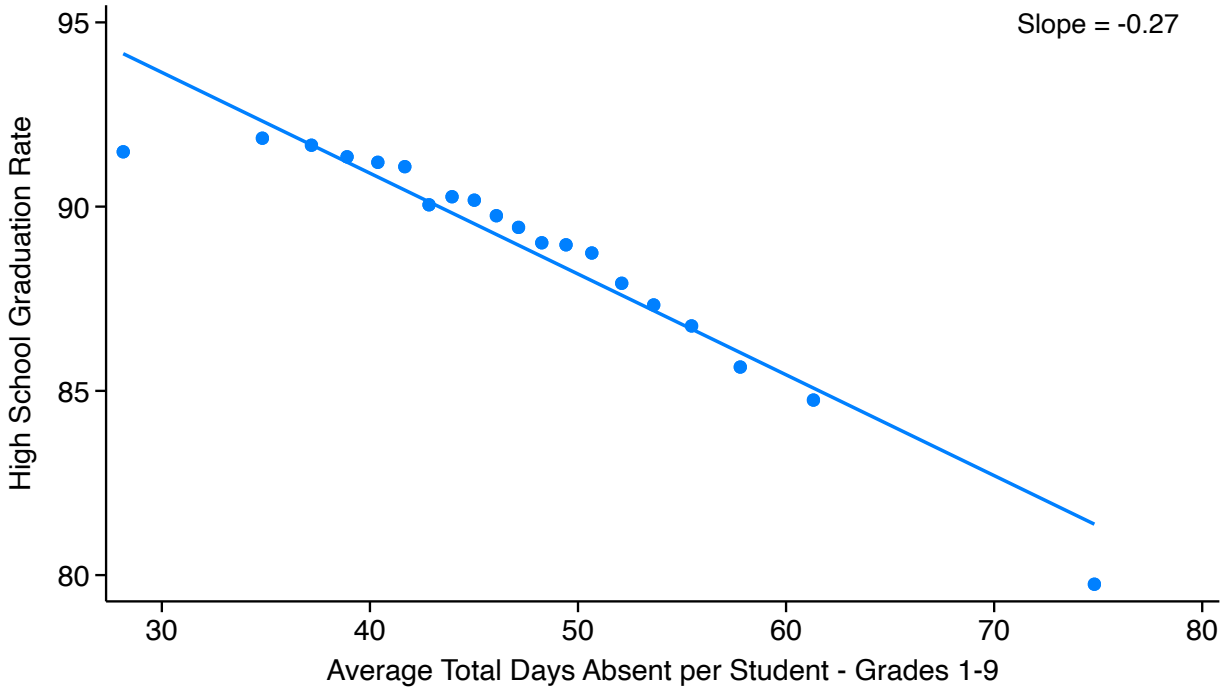


B: Students with No Absences



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*, we residualize test scores and absence rates on the controls and construct 20 equally sized ventiles of the residualized absence rates. We add the means back to the residuals for ease of interpretation The figure plots the mean residualized test scores in each ventile against the mean absence rate in the ventile. We also include the line of best fit. 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. In Panel *B*, we repeat the exercise, restricting to students who miss 0 days of school in a given grade. In Figure A.3, we repeat a similar exercise where we control for quartiles of own-student absence rates in the background, rather than restrict to students who are never absent.

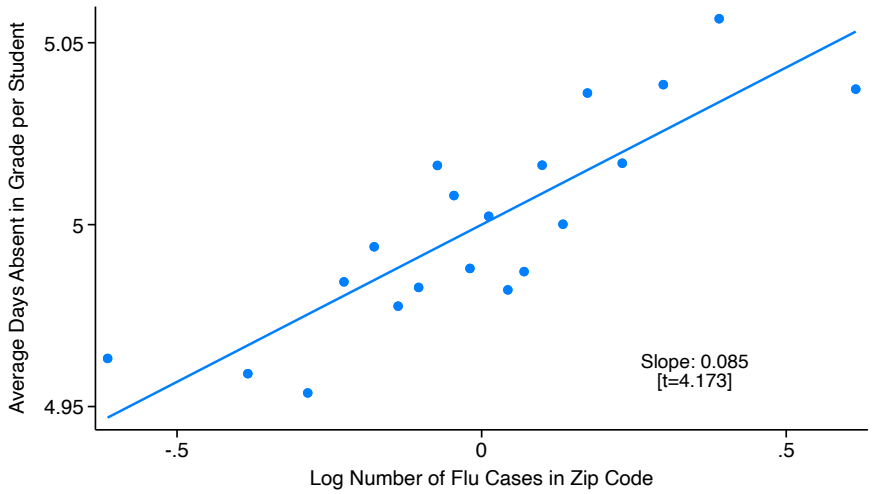
FIGURE III: OLS Relationship Between High School Graduation Rates and School-Cohort Absences



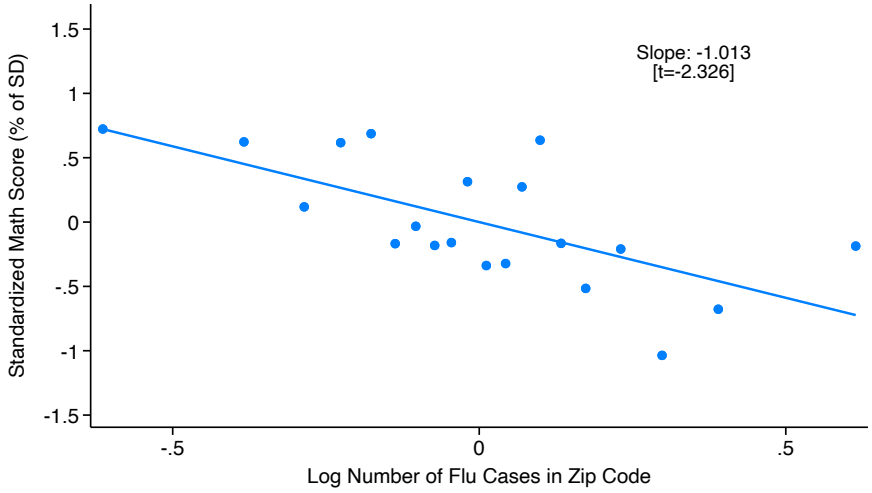
Notes: This figure presents a binned scatter plot of the relationship bewteen high school graduation rates and school-cohort level absence rates. Absence rates are the average number of days missed per student between grades 1 and 9 and students are assigned to the school where they attend kindergarten. 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 II for additional details on the construction procedure.

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

A: First Stage



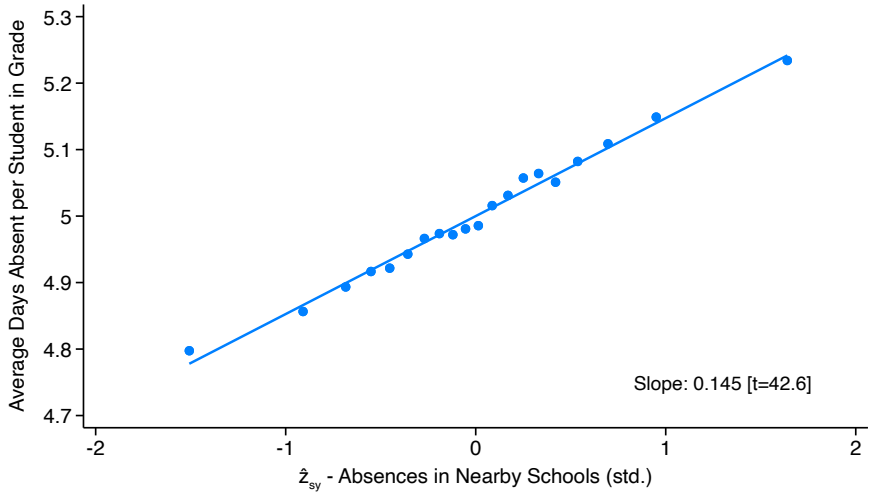
B: Reduced Form



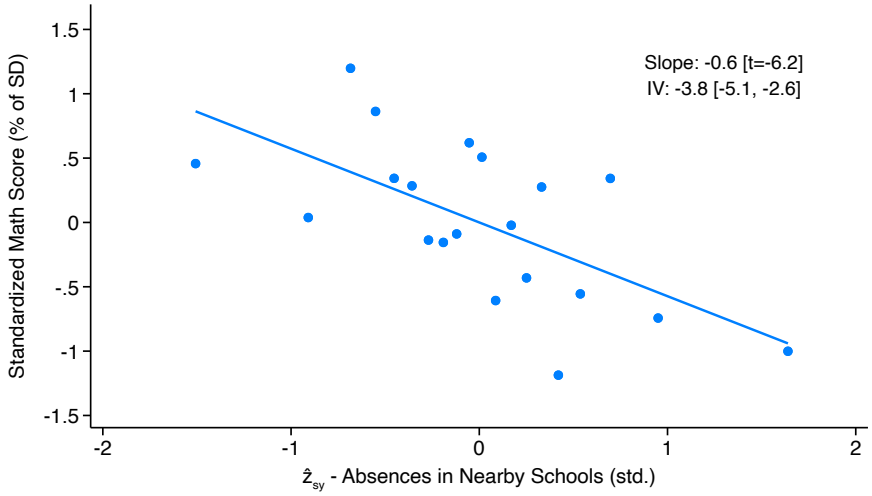
Notes: This figure presents binned scatter plots of the first stage and reduced form relationships between the spread of the flu and absences and test scores, respectively. The binned scatter plots are constructed as in Figure II; see figure notes for details. Panel *A* is estimated using Equation 1 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. We restrict to schools in ZIP codes with 5 or more flu cases in our data. Standard errors are clustered at the school-by-cohort level.

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

A: First Stage



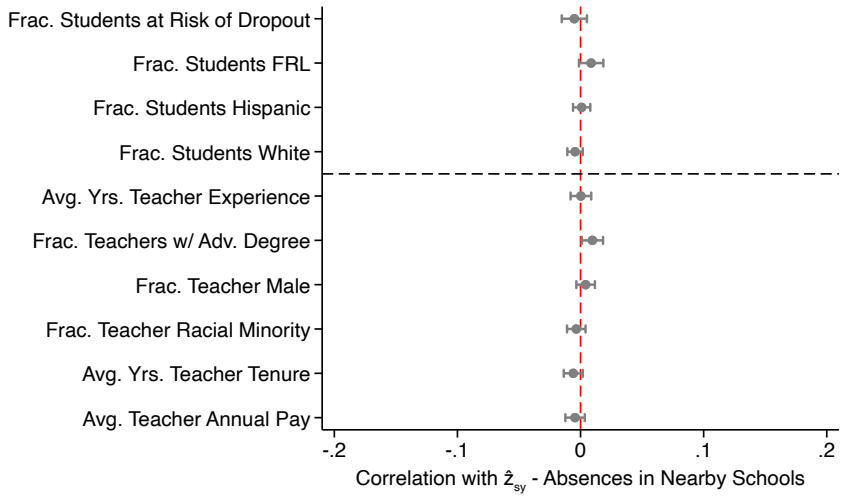
B: Reduced Form



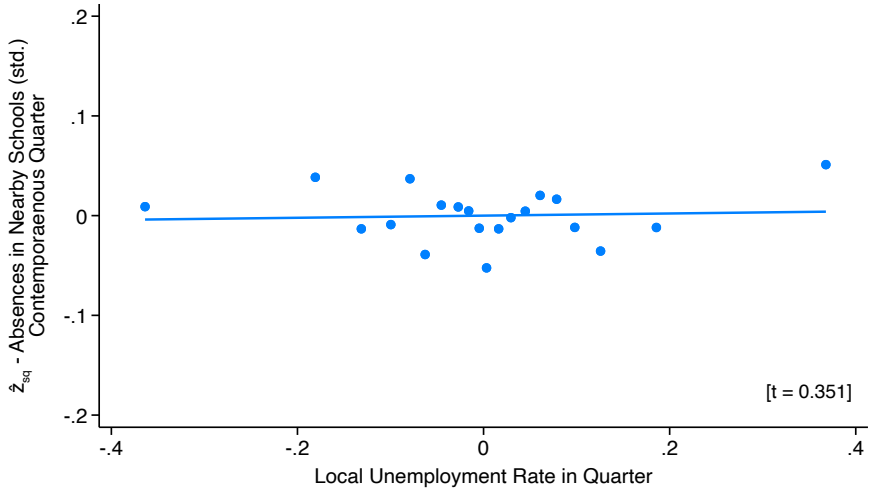
Notes: This figure presents binned scatter plots of the first stage and reduced form relationships between local shocks to attendance in nearby schools and absences and test scores in the focal school, respectively. Standardized tests are taken in grades 3-5 between 1997 and 2019. In both panels, the instrument, $\hat{z}_{sy}$, is shown in standard deviation units. The binned scatter plots are constructed as in Figure II; 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 VI: Balance Tests

A: Student/Teacher Characteristics



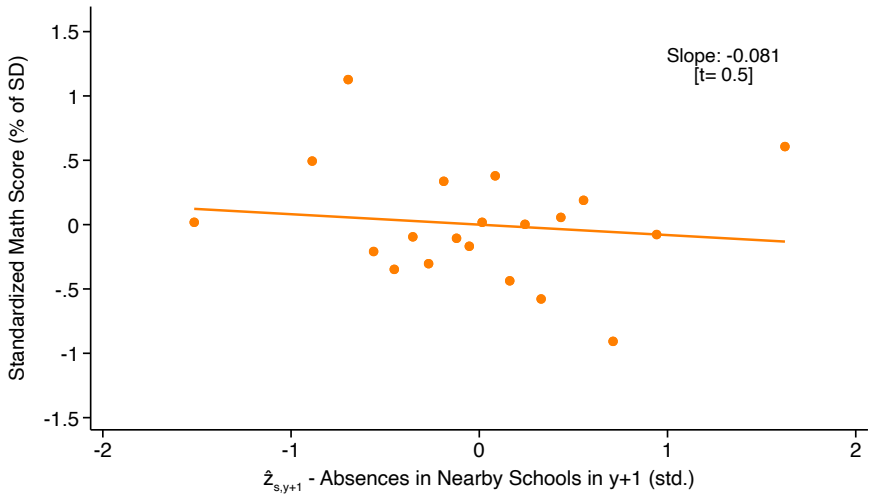
B: Local Unemployment Rates



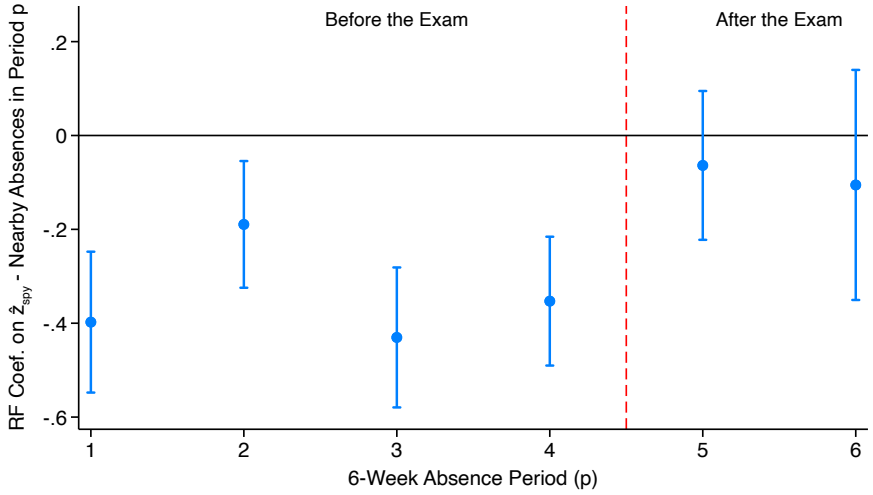
Notes: This figure presents balance tests for the single-year instrument, $\hat{z}_{sy}$, which captures shocks to attendance in nearby schools, showing correlations with characteristics of students, teachers, and local economic shocks. In Panel *A*, we present a series of correlation coefficients and 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. We control 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 II; 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 VII: Outcome-Based Placebo Tests

A: Across Year



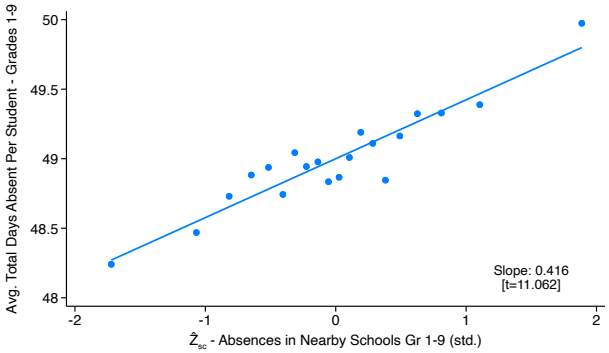
B: Within Year



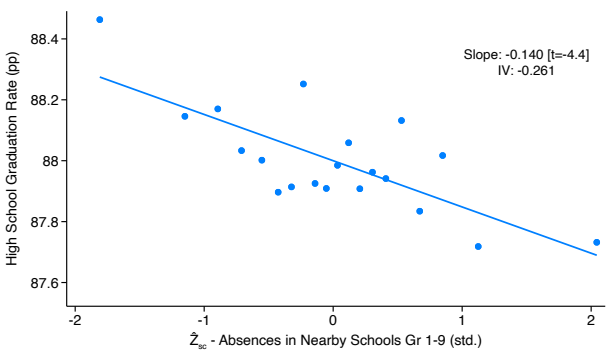
Notes: This figure presents two outcome-based placebo tests in support of our identification assumptions, described in Section IV. In Panel *A* we present a binned scatter plot analogous to the reduced form in Figure V Panel *B*, but rather than measure the instrument contemporaneously ($\hat{z}_{sy}$), we measure it 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 V for additional details on the construction of the binned scatter plot. In Panel *B*, we present a similar placebo exercise, but within instead of across years. We construct analogous instruments, $\hat{z}_{spy}$, 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, we run a reduced form regression similar to Equation 5, but including all six within-year instruments. We plot the coefficients on each of those instruments here. 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 VIII: Long-Run Effects: First Stage and Reduced Form

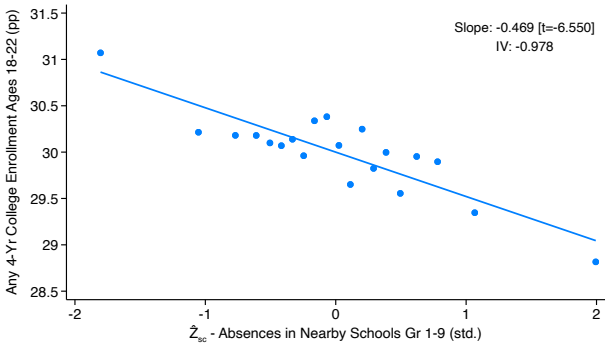
A: First Stage



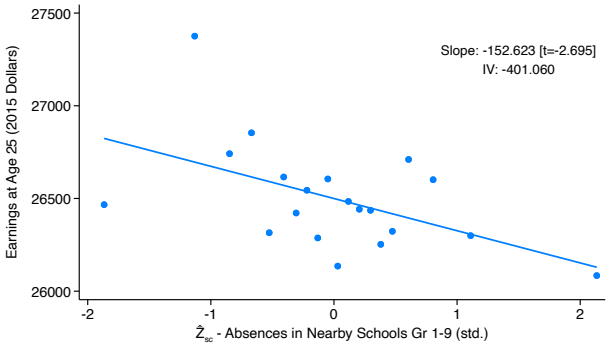
B: Reduced Form: HS Graduation



C: Reduced Form: 4-Year College (18-22)



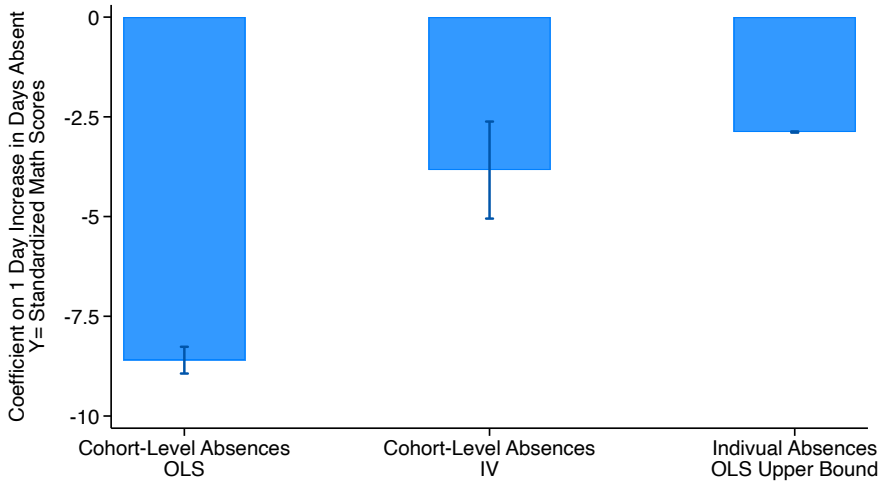
D: Reduced Form: Earnings at Age 25



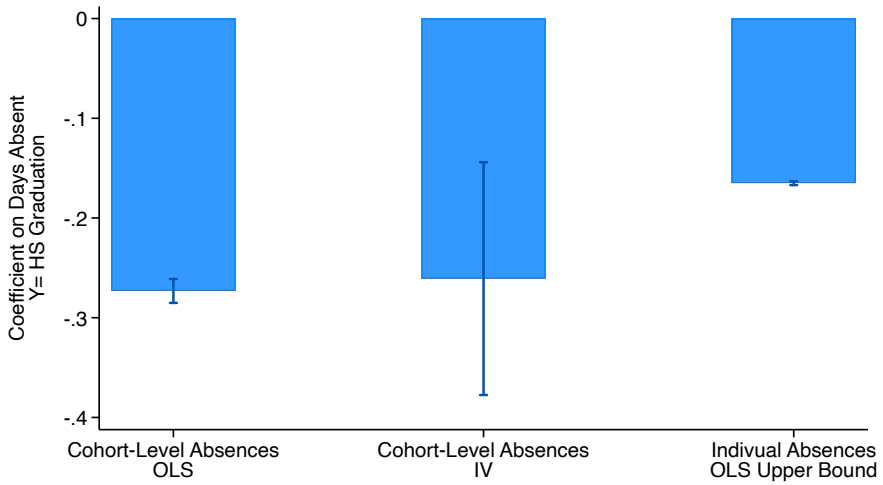
Notes: This figure presents first stage (Panel *A*) and reduced form (Panels *B*-*D*) relationships between absences in nearby schools and long-run outcomes in the focal school. Unlike Figure V, where absences are measured in a single grade, here the absence rate is the total number of days absent per student between grades 1 and 9. Each plot is constructed using the same basic procedure as in Figure IV; see notes for details. In each panel, the regressions contain controls for school (the school where a student attended kingergarten) 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. In each reduced form plot, we report 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 IX: Comparison of Cohort-Level versus Individual-Level Effects of Absences

A: Test Scores



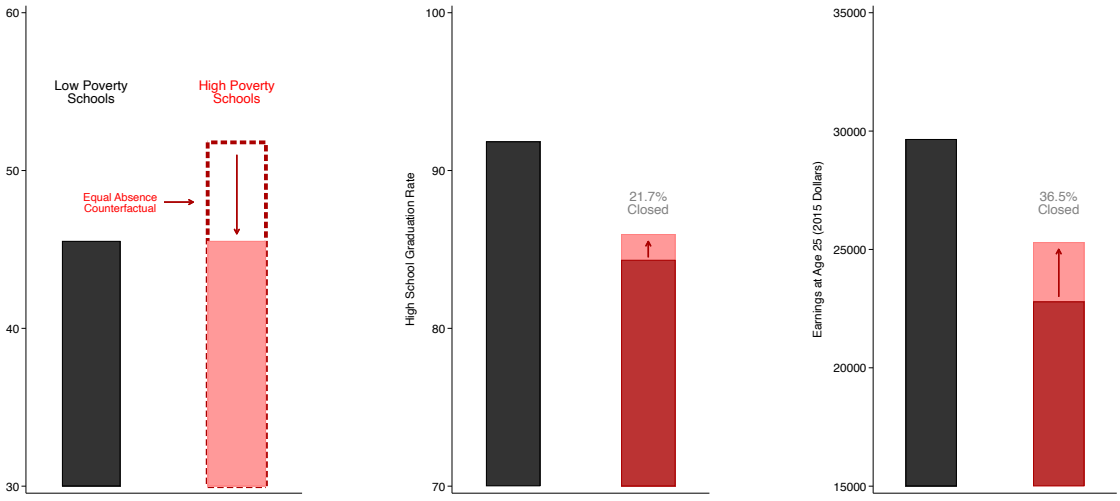
B: High School Graduation



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 II and III, respectively. The second bar shows our baseline IV estimates, shown in Figures V and VIII, respectively. The third bar shows the individual level OLS relationship between absences and the outcome, which we think of as 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 X: Equal Absence Counterfactual Outcomes in Low- and High-Poverty Schools

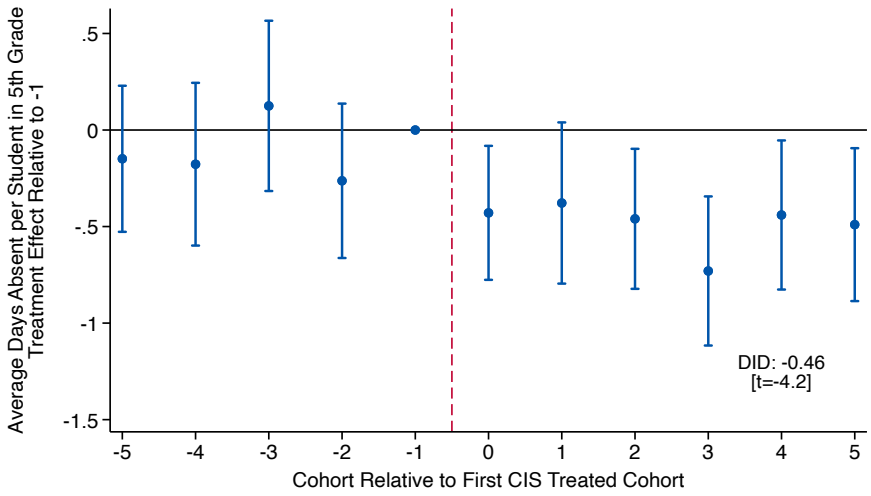
A: Absences Grade 1-9 *B*: High School Graduation *C*: Earnings at Age 25



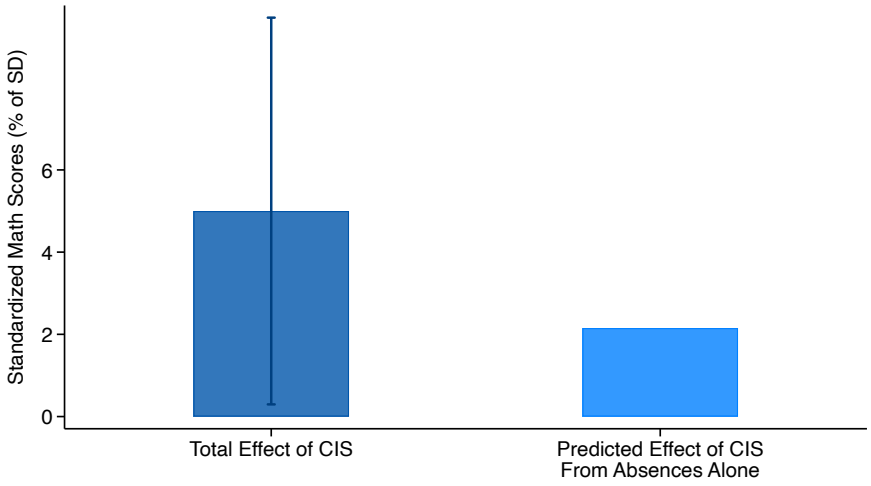
Notes: This figure presents counterfactual outcomes for high school graduation rates and earnings at age 25 in high-poverty schools from an exercise where absence rates in high-poverty schools are lowered to match those in low-poverty schools. The starting point for the figure is Figure I; see notes for additional details on the definitions of the groups and outcomes. The true absence rate in high-poverty schools is shown in by the dashed line in Panel *A*, and under the counterfactual the absence rate would be given by the light colored bar (equal to the black bar). In Panels *B* and *C*, the dark red bars are the same as in Figure I, and under the counterfactual are raised by the amount indicated in the lighter color. To construct the counterfactual we multiply the change in absence rates in the high-poverty schools (-6.25 days per student) with our IV estimates shown in Table III and add that to high-poverty school outcomes in Panels *B* and *C*. The increase from the counterfactual is shown in light red. We also report what fraction of the initial gap in Figure I is closed under the counterfactual.

FIGURE XI: Effect of CIS on Absences and Outcomes

A: Absences in 5th Grade



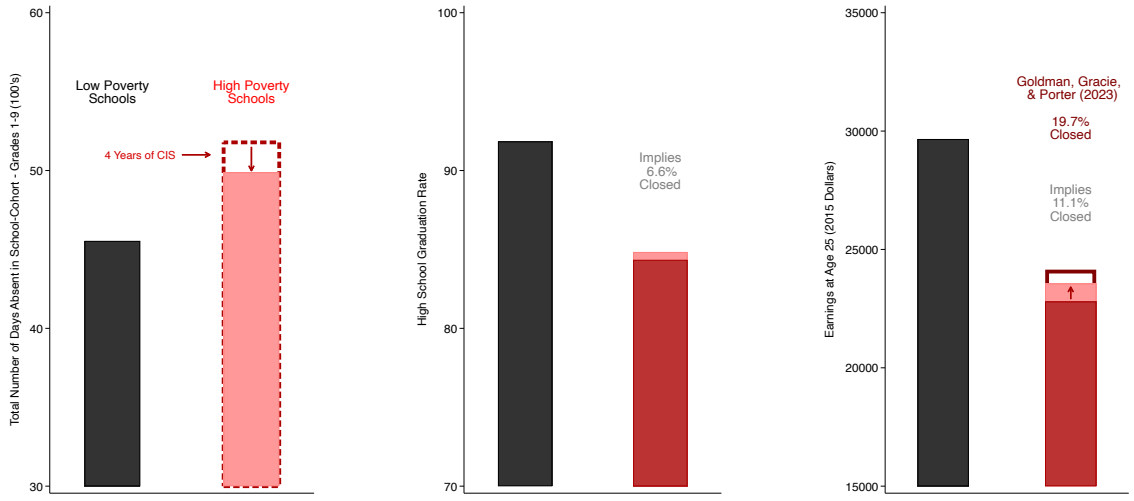
B: Standardized Math Scores in 5th Grade



Notes: This figure shows the effect of CIS on absences (Panel *A*) and test scores (Panel *B*). Panel *A* presents event study coefficients estimated using Equation 8, where the dependent variable is the number of days absent per student in 5th grade. The specification is a stacked design comparing schools that got a CIS program in earlier years to those that got CIS programs in later years. See Section V for additional details. In addition to the event study coefficients, we also report a difference in differences coefficient from a more parsimonious specification shown in Equation 9. Panel *B* also reports the coefficient from the difference in differences specification, where the dependent variable is standardized math scores in fifth grade (left side of the plot). In addition to that coefficient, we also report a predicted increase in test scores, coming only from the decrease in absenteeism shown in Panel *A*. To construct the prediction, we take the difference in differences coefficient on absenteeism and scale it by the IV effect on test scores that we estimate in Section IV. We use a 5th grade-specific IV estimate for the prediction, which we report in Table A.2. In both panels, the results are estimated using CIS programs that began between 1999 and 2009 and standard errors are clustered at the school-by-cohort level.

FIGURE XII: 4 Years of CIS Counterfactual Outcomes in Low- and High-Poverty Schools

A: Absences Grade 1-9 *B*: High School Graduation *C*: Earnings at Age 25



Notes: This figure presents a series of counterfactuals, where high-poverty schools are given access to four years of a CIS program. To construct the counterfactual, we first estimate a difference in differences specification using Equation 9, where the dependent variable is the total number of days absent in elementary school. The difference in differences specification estimates the treatment effect of four years of CIS, relative to later treated schools. In Panel *A*, we represent this treatment effect as the difference between the dashed bar (the true absence rate from Figure I) and the light colored bar. Then, we construct the counterfactual high school graduation rates and labor market earnings using the IV effects from Table III using the same procedure as in Figure X; see notes for additional details. We also report the fraction of the gap closed. These counterfactuals are constructed using only the effect of CIS on absences and do not incorporate any direct effects of CIS on outcomes, which may exist. In Panel *C*, we also provide a counterfactual that incorporates these direct effects. Goldman, Gracie and Porter (2023) estimate the direct effect of CIS on labor market outcomes, using the nation-wide roll out of CIS and a similar research design. We also provide that counterfactual (equal to the sum of the light bar and the hollow bar) as well as the fraction of the gap between low- and high-poverty schools that the total effect would close.

Supplementary Appendix

A School-by-year specific shocks and bias

As described in Section IV, our instrument does not use "blips" in attendance in the focal school (z_{sy}) directly, and instead uses a leave-out measure that takes other schools from the same district ($\hat{z}_{sy}$). This removes any bias coming from a shock in the focal school that impacts both absences and outcomes. In this section, we use a stylized simulation to build 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, we abstract away from state-wide year shocks.

Without loss of generality, we can split up the idiosyncratic noise into three components—one that is shared across districts in the year and one that is specific to the school in the year (but shared across grades), and the remaining idiosyncratic noise. We can write this as

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

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

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

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.

We construct measures of "blips" in the focal school by regressing absences on school-by-cohort and school-by-grade fixed effects and calling the residuals z_{sy}. In this simple model, $z_{sy} = \varepsilon_{scg} = \tilde{\rho}_{d(s),y(c,g)} + \tilde{\pi}_{s,y(c,g)} + \tilde{\varepsilon}_{scg}$.

z_{sy} is not a valid instrument for A in this model; the exclusion restriction is violated because z_{sy} is a function of $\tilde{\pi}$ and $\tilde{\pi}$ directly affects outcomes. Instead, we show that in this model $\hat{z}_{sy}$, the leave-out measure is unbiased.

[1] Note, the shock need not enter directly, we could have allowed for a component that is simply correlated with $\tilde{\pi}$. We opt for this for simplicity.

As in Section IV, define the leave-out measure as

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

the average of the z_{sy} measures for other schools, s', in the same district d, where N_d is the number of schools in the district. By construction, $\tilde{\pi}$ (the school-by-year shock) and $\tilde{\rho}$ (the district-by-year shock) are orthogonal. Therefore, $\hat{z}_{sy}$ is uncorrelated with $\tilde{\pi}_{sy}$ and $\hat{z}_{sy}$ is a valid instrument.

We show that the leave-out instrument is unbiased in a simple simulation. We start with a world containing 50 school districts, 5 schools per district. Each school has 3 grades and we have 10 years of data. We draw values for each of the terms in equation A1 from independent, normal distributions to generate A_{scg}. For simplicity, all terms except $\tilde{\pi}$ are drawn from $N(0, 0.5)$. We vary the standard deviation $\tilde{\pi}$ is drawn from to show below how the bias varies. We then construct Y_{scg} using equation A2. For simplicity, we choose values for the parameters in the outcome equation ($\beta = -4, \alpha = -0.5$). Then, we construct z_{sy} and $\hat{z}_{sy}$ as described above. We use each as instruments for A_{scg} and compare the coefficients.

Appendix Figure A.12 shows that as the variance in the school-by-year shock increases, so does the bias. This is because as the variance increases, the average share of the variance in ε_{scg} explained by $\tilde{\pi}_{sy}$ compared to $\tilde{\rho}_{d(s),y(c,g)}$ and $\tilde{\varepsilon}_{scg}$ increases (shown in Panel *B*. When this share is high, the exclusion violation becomes more pronounced. Unlike with z_{sy}, estimates using $\hat{z}_{sy}$ are unbiased. They become somewhat more noisy as the variance of $\tilde{\pi}_{sy}$ increases, but remain centered around the true causal effect.

Note that in this simple model only the school-by-year shock had a direct effect on outcomes. If the district specific shock also had an effect, the leave-out measure would also be biased for the same reasons.

B Correlated Illness Shocks Across Years

In Section IV, we motivated our empirical strategy with a hypothetical example of absences in a single elementary school (shown in Appendix Figure A.4). In this example, we thought there may have been a local illness shock in the year 2000 as absences spike for both cohorts and then return more or less to baseline levels in other years. In this case, the shock hits in year 2000 and has dissipated by 2001. We might be worried about shocks that are correlated across years and/or persist beyond a single year. In this appendix, we show that this does not bias our IV estimates.

As an extreme example, we show another hypothetical absence pattern in Panel *A* of Appendix Figure A.13. In this case, we see that the 1992 cohort has high absences in every year of elementary

school. If we consider only this one cohort, we cannot discern whether it is because this set of students has certain traits that lead them to be absent at high rates on average, or if there were 5 years in a row of high illness spread, for example. As in our previous example, we can make progress on discerning between these possibilities by considering other cohorts.

When we look at other cohorts, we see that they also have spikes in absences in 1998 and 2003, relative to their baseline rates. This is suggestive of there being a some kind of local shock in this area between 1998 and 2003. Unlike in the prior example, this shock persists across several years.

Because we are interested in isolating the exogenous component of absences, rather than constructing an unbiaesd measure of illness spread, these correlated shocks do not pose a threat to our identification strategy. Intuitively, when there are more correlated shocks, our measure of these "blips" in absences will be attenuated relative to the true value; in the example, the shocks is essentially collinear with the school-by-cohort fixed effect that we would estimate for the 1992 birth-cohort. We can show this by adapting the simulation in Appendix A.

We take the simulation we developed and allow for the district-by-year shock to be equal to the shock in the prior year with some probability. Concretely, with some probability, p, we replace $\bar{\rho}_{d(s),y(c,g)}$ with $\bar{\rho}_{d(s),y(c,g)-1}$. This generates persistence in the shocks. We then compute the slope between the residuals we estimate, ε_{scg}, and the true values of $\bar{\rho}_{d(s),y(c,g)}$. This slope captures a measure of how attenuated our measure of local illness shocks is relative to the truth. We repeat this exercise for values of p between 0 and 0.2.

Appendix Figure A.13 Panel *B* shows that as the probability of persistence increases, the slope attenuates somewhat. Note, that even when there is no persistence, the slope is not 1. Our goal is not to construct unbiased estimates of the respiratory illness spread, but instead to isolate the component of absences that are explained by these exogenous shocks. As long as we have successfully done that, the first stage in the IV accounts for the attenuation.

We show in Appendix Figure A.13 Panel *C* that the IV estimates remain unbiased as the persistence increases. Under the same assumptions as we described in Appendix A, i.e. that the district-by-year shock does not directly impact outcomes, the fact that shocks persist does not pose a threat to identification.

If shocks persist, however, we need a long panel to be able to identify the local absence shocks. Even in this example, where a shock persisted for 5 years, we rely on 14 years of data to construct our estimates. If we only had data in 5 years (between 1997-2004), we would have suffered from a version of short-panel bias (Nickell, 1981). We would not have been able to identify this period as a high absence period because it would have been fully collinear with the school-by-cohort components that we estimate. We estimate the blips using data from 1995 to 2019. This long-panel allows us to capture local shocks, even when they persist.

TABLE A.1: Effect of Cohort-Level Absences on Test Scores: Additional Controls

		IV Estimates					
	First Stage	HS Grad	HS Grad	4-Yr. Coll.	4-Yr. Coll.	Earnings Age 25	Earnings Age 25
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total Absences Grade 1-9	0.416	-0.261	-0.223	-0.978	-0.890	-401.060	-378.732
	(0.038)	(0.060)	(0.057)	(0.181)	(0.168)	(169.995)	(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 where we include controls for the covariates shown in Figure VI. Columns (1), (2), (4), (6) are identical to those in Table III; see notes for additional details. In Columns (3), (5), and (7), we 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, we include controls for the average experience level of teachers that students experience 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.

TABLE A.2: Effect of Cohort-Level Absences on Test Scores

	Baseline Estimates		5th Grade Only	
	First Stage	IV	First Stage	IV
	(1)	(2)	(3)	(4)
Absences in Grade	0.145	-0.038	0.135	-0.043
	(0.003)	(0.006)	(0.006)	(0.012)
Observations (School-Cohort-Grade Cells)	82345	82345	25365	25365
Allow for Trend in Cohort	Yes	Yes	Yes	Yes

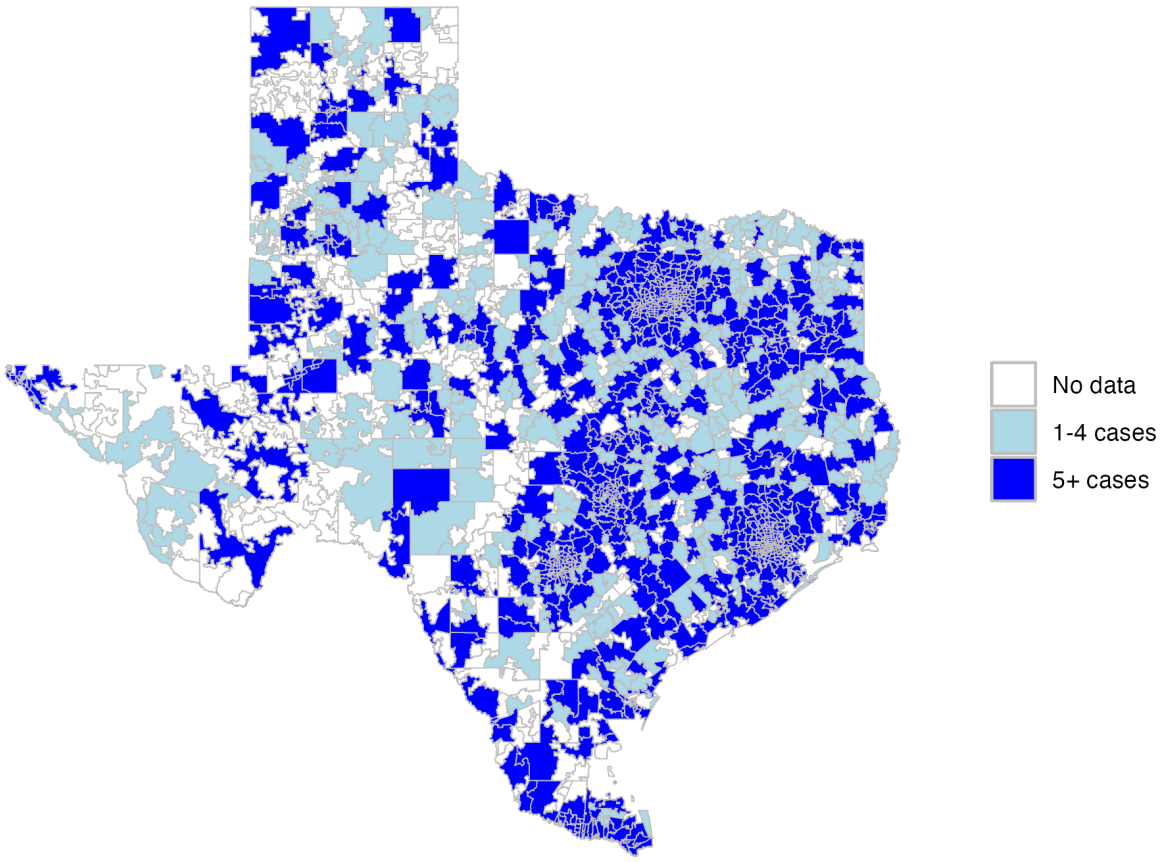
Notes: This table presents statistics analogous to those shown in Table II. Columns (1) and (2) are the same as those shown there. See table notes to Table II for additional details. In Columns (3) and (4) we run the same specification, restricting the sample to 5th grade only. We use the 5th grade IV estimate to construct the prediction in Figure XI Panel *B*. Standard errors are clustered at the school-by-cohort level.

TABLE A.3: CIS Difference in Differences Results

	Days Absent Grade 5 (1)	Total Days Absent Grades 1-5 (2)	5th Grade Test Scores (3)
Effect of CIS	-0.460	-1.900	0.050
	(0.110)	(0.460)	(0.024)
Observations (Number of Students)	383804	383804	383804
Include Race X Income Interactions	Yes	Yes	Yes

Notes: This table presents difference in differences effects of CIS on absences and test scores. The regressions are estimated using Equation 9.

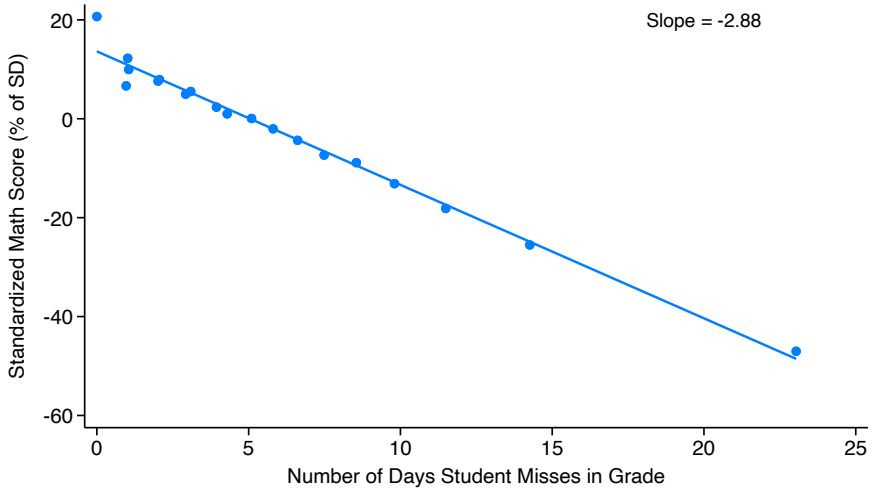
FIGURE A.1: Coverage of Flu Data: 2018-2019



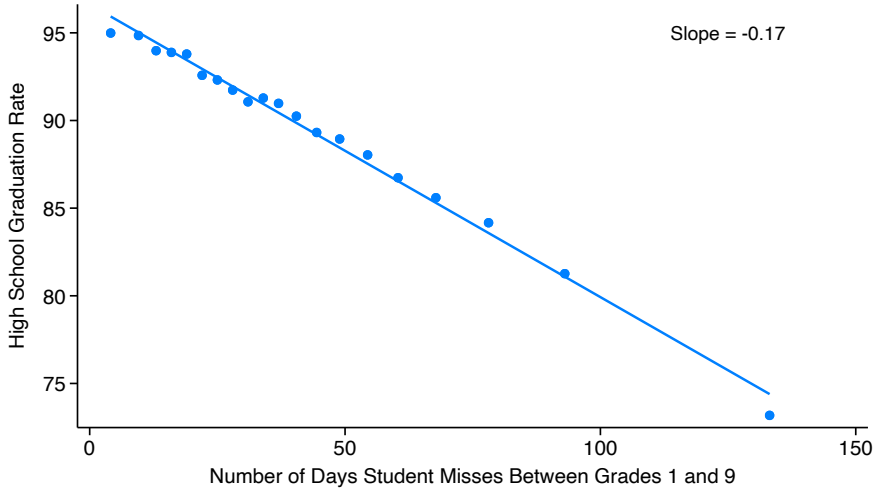
Notes: This map shows the coverage of our flu data at the ZIP code level. We have data in 2018 and 2019, collected from a Texas hospital network. See Section II for additional details. The map classifies ZIP codes into three groups: those with no data, those with only between 1 and 4 reported cases over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

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

A: Standardized Math Scores

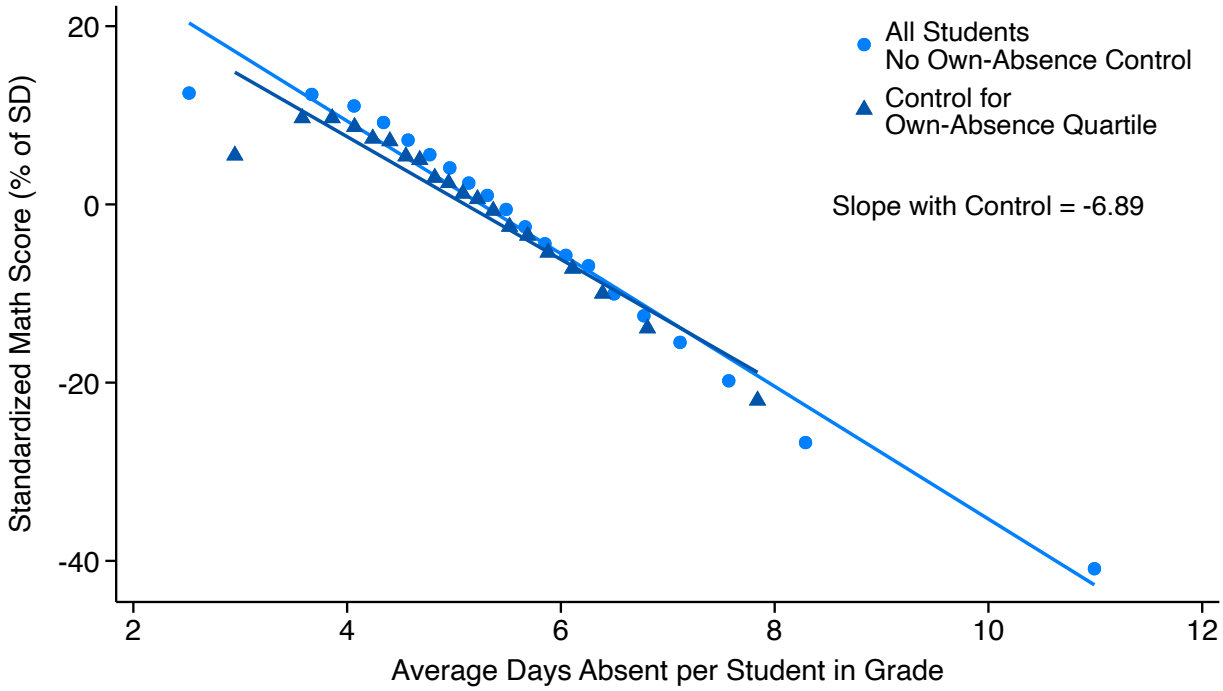


B: High School Graduation



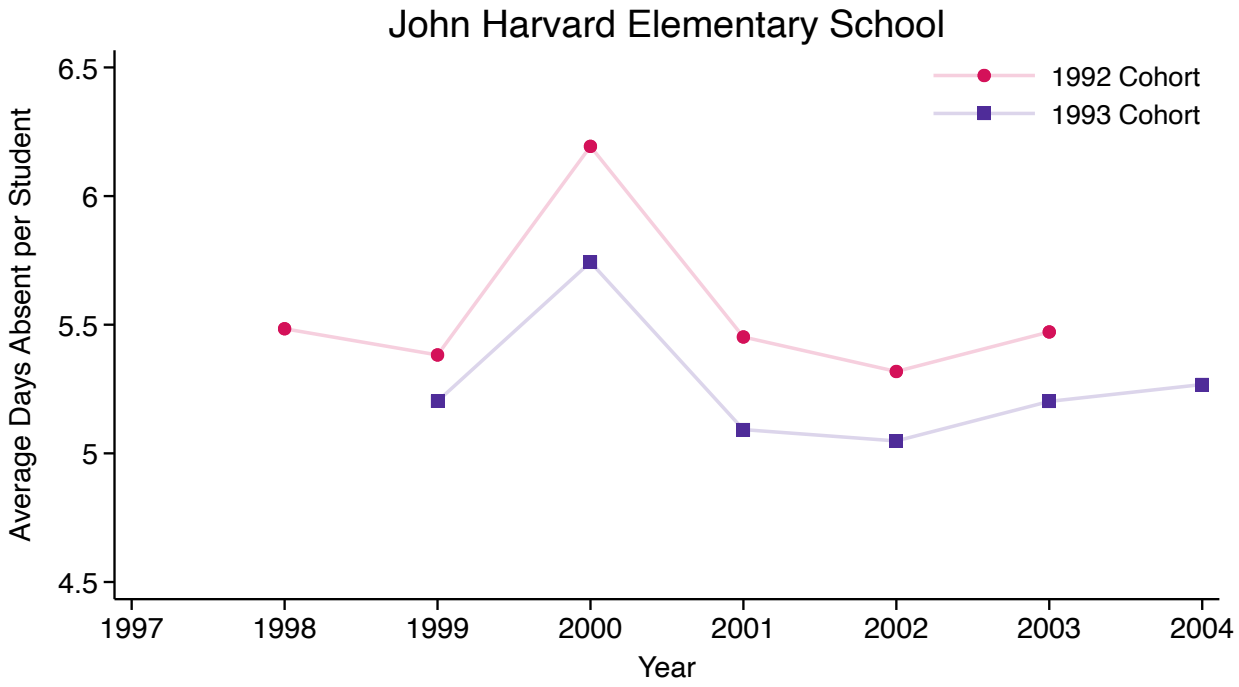
Notes: This figure presents the a binned scatter plot of the relationship between outcomes and a student's own absence rate. Panel *A* shows the relationship between standardized math test scores and a student's own absence rate in that grade. Test scores and absences are measured in grades 3-5 and years 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 II; see notes for additional details. We include 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 II 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 II. The series with triangles contains the own-absence quartile fixed effects. We include controls for birth-cohort and grade fixed effects.

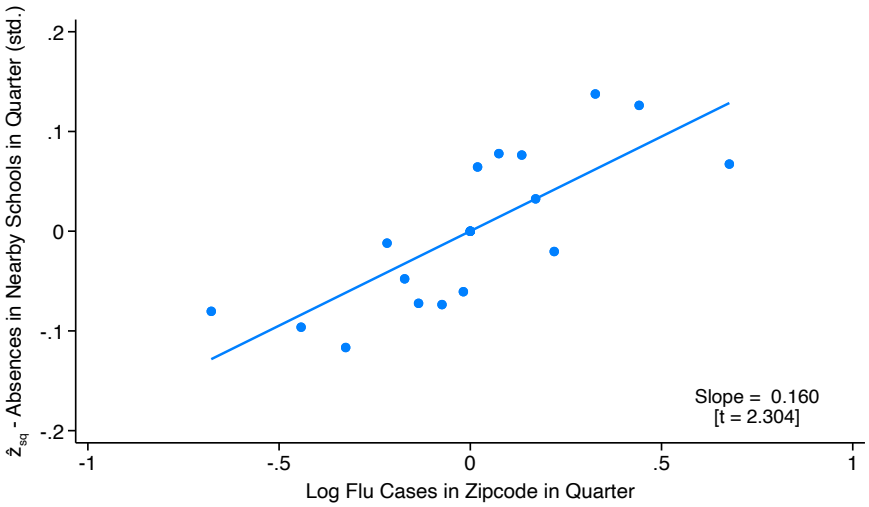
FIGURE A.4: Example of Local Shocks to Attendance



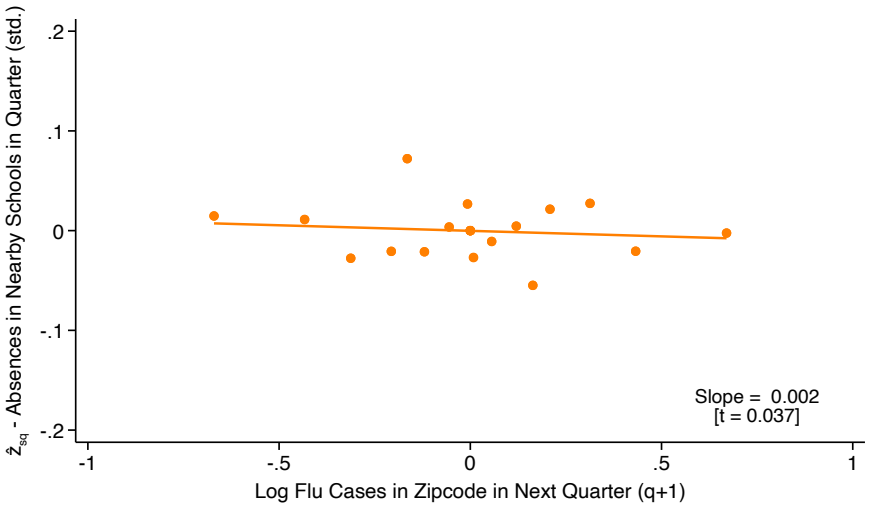
Notes: This figure presents a stylized example of absence rates in each year for two birth cohorts in a hypothetical elementary school. The series denoted by circles illustrates the 1992 birth cohort, who attends grades 1-5 between 1998 and 2003. The series denoted by squares illustrates the 1993 birth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

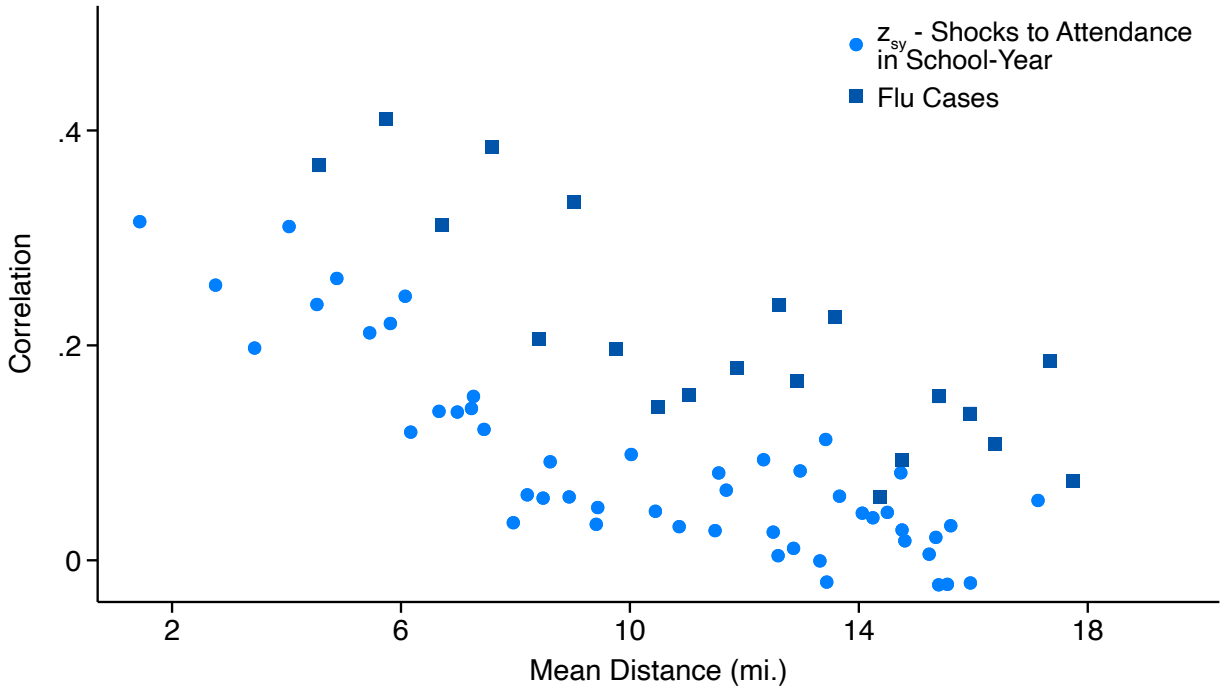


B: Flu Cases Next Quarter



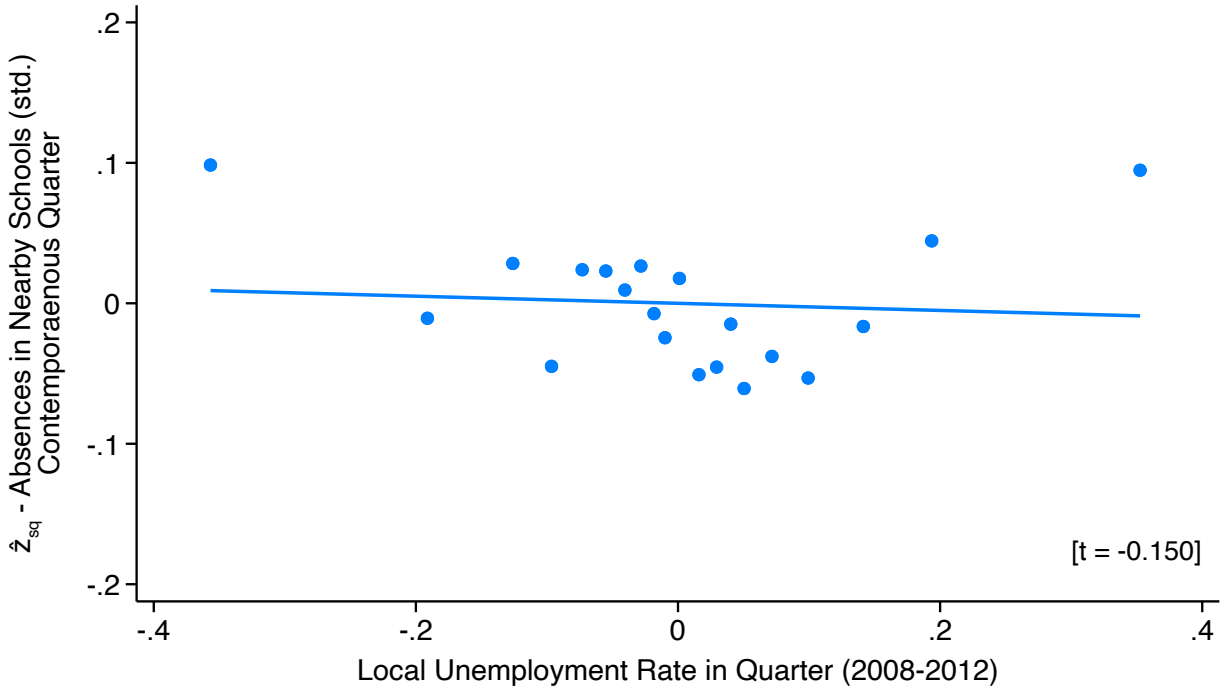
Notes: This figure presents binned scatter plots of the relationship between spikes in absence rates in nearby schools in a given year and local flu rates in those areas and years. The binned scatter plots are constructed using the procedure described in Figure II; see notes for details. Unlike our baseline instrument described in Section IV, which is measured at the school-by-year level, here we construct an analog at the school-by-quarter level, making use of absence rates measured every six weeks. In Panel *A*, we present 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 *A*, we measure the log number of cases in the subsequent quarter ($q+1$). We show that 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



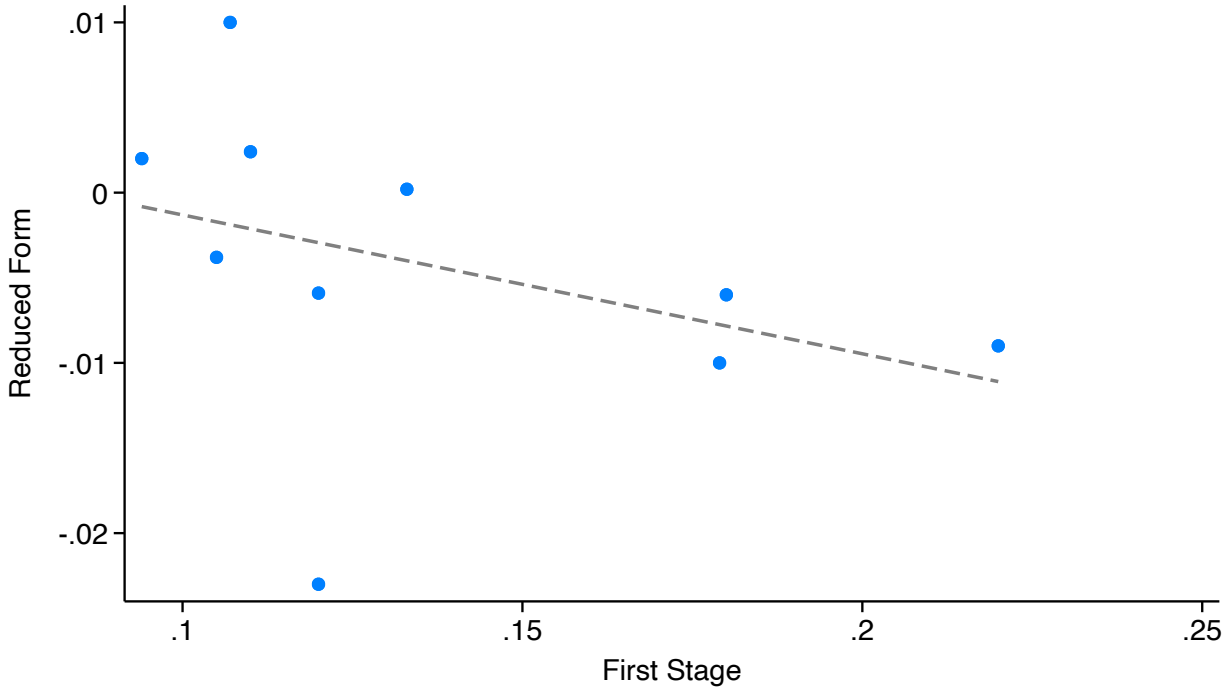
Notes: This figure presents the spatial autocorrelation between flu cases in nearby ZIP codes and spikes in absenteeism in nearby schools. To construct the series for flu cases, we construct an adjacency matrix for ZIP codes using the centroid of each zip code. For each ZIP code, we identify the 20 nearest neighbor ZIP codes. We calculate the correlation between the log number of flu cases in the focal ZIP and each neighbor. We plot 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, we correlate our measure z_{sy} in a focal school with the $z_{s'y}$ in neighboring schools. Like with ZIP codes, we construct an adjacency matrix for schools using the latitude and longitude of each school and compute the 50 nearest neighbor schools for each focal school. We calculate the correlations and plot them using the same procedure as with ZIP codes. Note that here we use z_{sy} for school s as opposed to $\hat{z}_{sy}$ (our baseline instrument which is a leave-out measure of 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 VI Panel *B*, but limits the years to 2008-2012, to isolate the years around the Great Recession. See the notes to Figure VI for additional details.

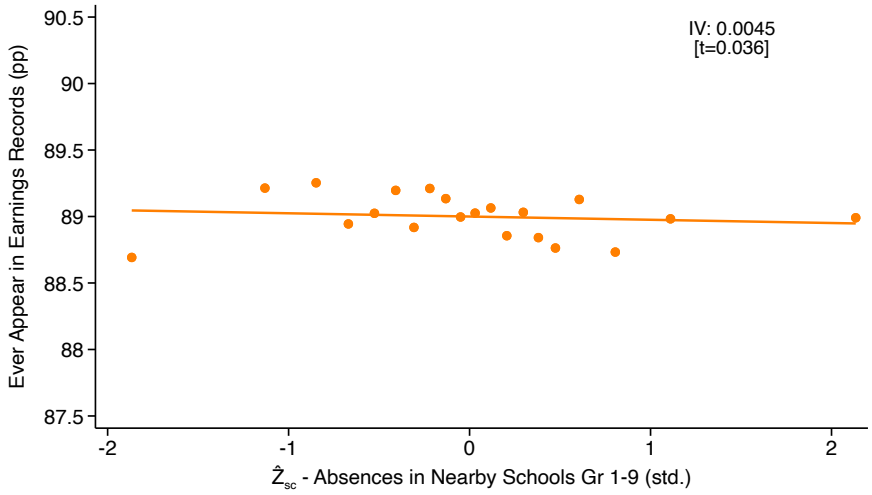
FIGURE A.8: Relationship Between First Stage and Reduced Form Effects of Local Shocks to Attendance on Test Scores



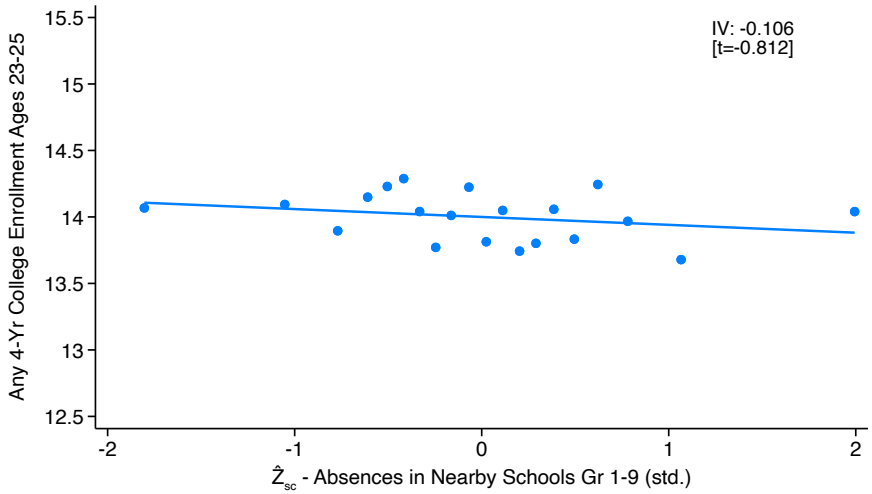
Notes: This figure plots the relationship between the reduced form and the first stage in 10 separate sub-samples. First, we split the sample used to estimate Figure V into deciles based on the observed absence rate in the school in the prior year. Using the specifications in Equations 4 and 5, we estimate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

FIGURE A.9: Additional Reduced Form Effects on Long-Run Outcomes

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

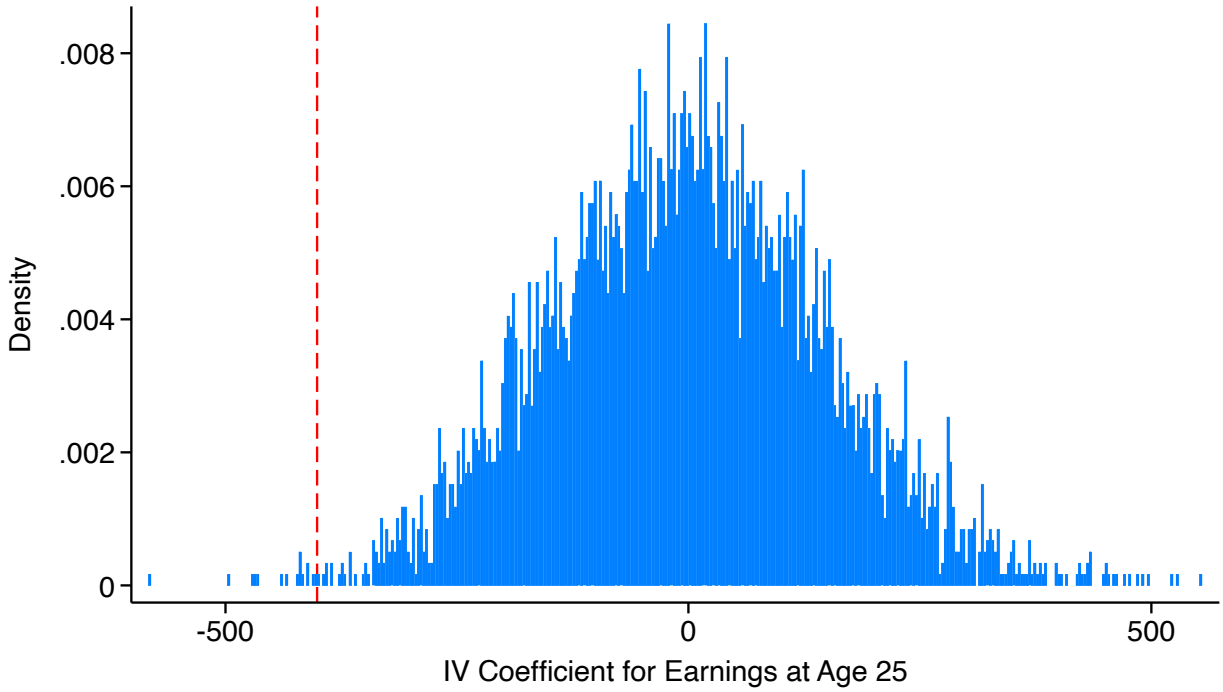


B: Four Year College Enrollment Between Ages 23-25



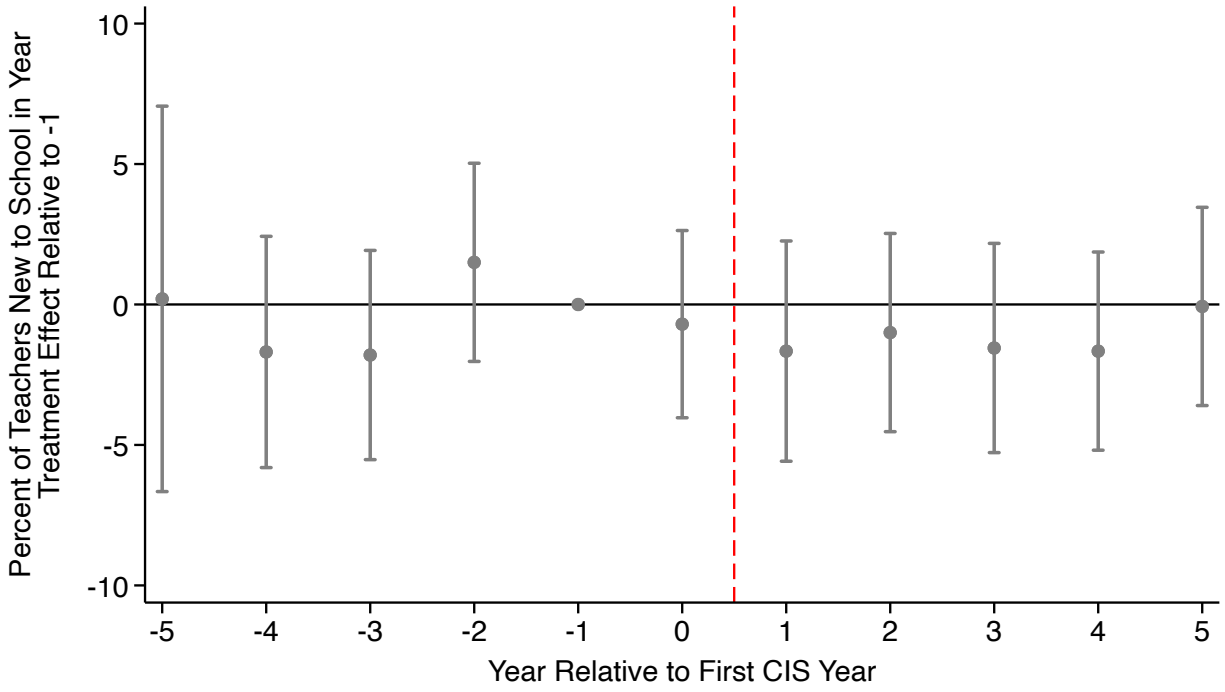
Notes: This figure presents two binned scatter plots related to the effects on wages that we present in Figure VIII Panel *D*. In Panel *A*, we test whether our instrument, $\hat{Z}_{sc}$, has any effect on the probability that students ever appear in the unemployment insurance database we use to measure earnings. We present the reduced form relationship as well as the IV coefficient. We find 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. In Panel *B*, we plot an analog of Figure VIII Panel *C*, but we measure four-year college attendance between ages 22 and 25. We find no relationship between absences in nearby schools on 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.10: 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 VIII Panel *D* and Table III is marked by the dashed red line. To construct the histogram we randomly reshuffle the $\hat{Z}_{sc}$ instrument across schools and cohorts and re-estimate the IV. We repeat the process 10,000 times and plot the distribution of treatment effects.

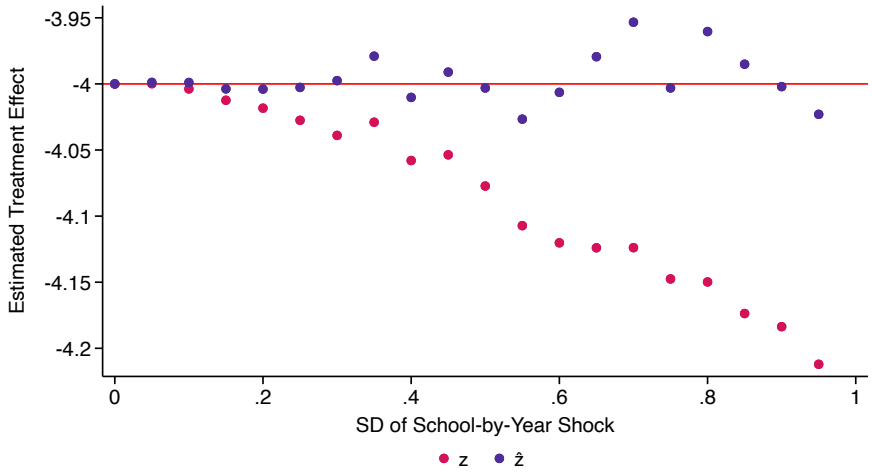
FIGURE A.11: CIS Balance Test: Fraction of New Teachers at School



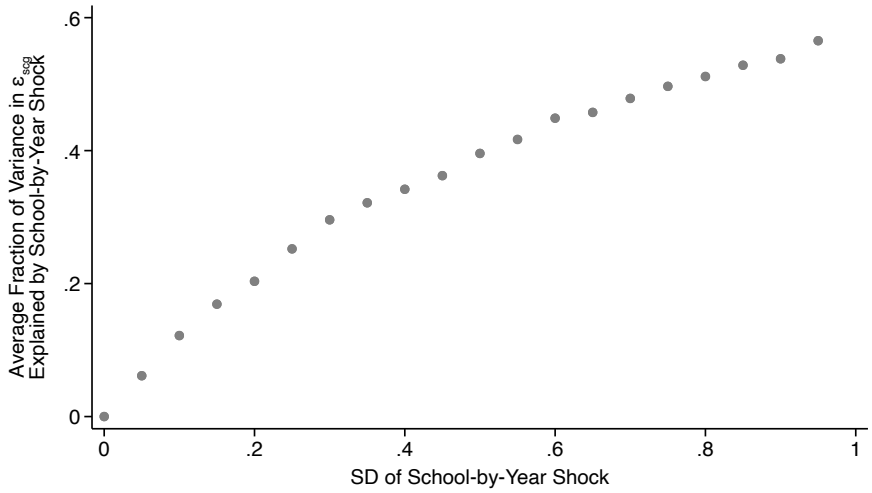
Notes: This figure presents event study coefficients estimated from Equation 8, where the dependent variable is the percent of teachers who are new to a given school in a given year, y. The specification is a stacked design comparing schools that got a CIS program in earlier years to those that got CIS programs in later years. We use CIS programs that begin between 1999 and 2009. Standard errors are clustered at the school-by-cohort level.

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

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



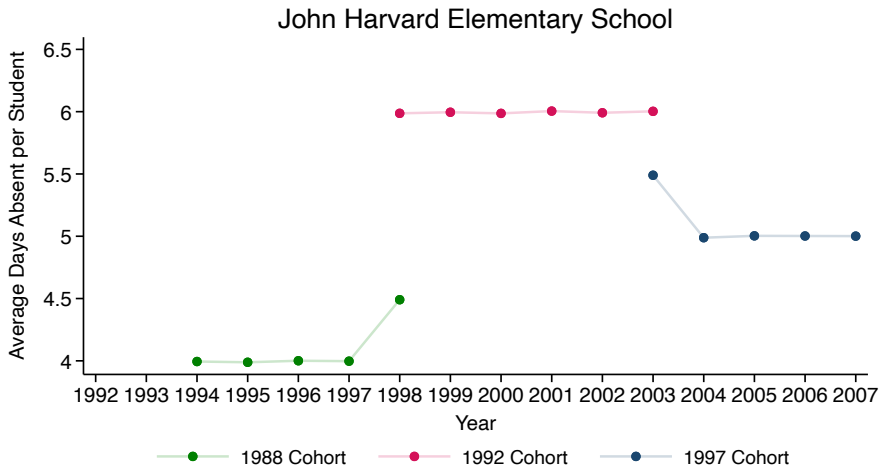
B: Share of Variance Explained by School-by-Year Shock



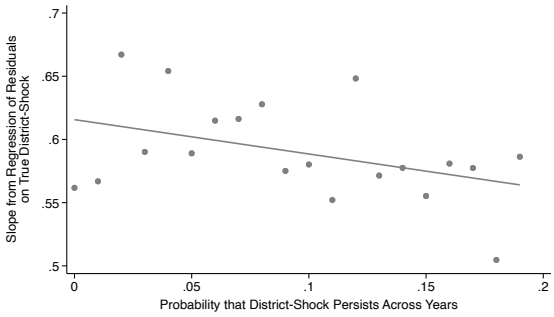
Notes: This figure shows the results of a simple simulation described in Appendix A. The simulation is designed to highlight the bias that school-by-year shocks to absences that are correlated with shocks to outcomes would bias our IV estimates and that using a leave-out measure of the instrument (using other schools in the same district) corrects for that bias. We show in Panel *A* that as the variance in the school-by-year shocks increases. This is because, as Panel *B* highlights, that as the variance in the school-by-year shocks increases, the fraction of the variation in absences explained by the shocks also rises. See Appendix A for additional details on the simulation.

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

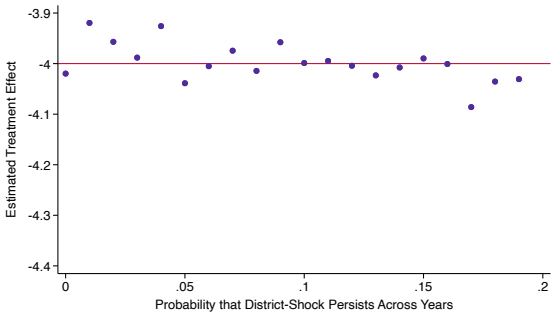
A: Example of Persistent Local Shock to Attendance



B: Attenuated Measure of Shocks



C: Unbiased IV Estimates



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