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Keywords: Student absences, achievement gaps, education production function
JEL Classification: I2

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1 Introduction

There is an emerging consensus that student attendance is both a critical input and an intermediate outcome of the education production function. The U.S. Department of Education recently called chronic absenteeism, defined as missing at least 10% of school days, "a hidden educational crisis."[1] Accordingly, education policy-makers are increasingly incorporating student attendance into accountability measures used to gauge schools' and teachers' performance, most notably via the *Every Student Succeeds Act* (ESSA), which has renewed interest in interventions to reduce student absenteeism (Bauer et al., 2018). A growing body of evidence shows that student absences are highly malleable to the influence of teachers, school-based interventions, students' household circumstances, and daily shocks to the cost of attending school (Currie et al., 2009; Gershenson, 2016; Goodman, 2014; Gottfried and Hutt, 2019; Liu and Loeb, forthcoming; Tran and Gershenson, 2018).

Heightened policy and research interest in student absenteeism is prefaced on the well-documented correlation between student absences and educational outcomes representing a causal relationship. While it makes intuitive sense that absences harm student achievement, causal identification of the relationship remains a persistent challenge (Jacob and Lovett, 2017) because unobserved time-varying, student-level shocks, such as illness or lack of sleep, can affect both students' attendance and their academic performance. In the current study, we overcome the threat posed by such shocks by using a decade's worth of rich administrative data from a large urban school district in California that include the date and class period of each absence. We focus our analysis on secondary schools, as students in secondary school have far more absences (and agency over those absences) than students in elementary school.

Our identification strategy exploits within-student, between-subject differences in absences in a given school year to remove the threat posed by unobserved student-year shocks. We examine the relationship between absences and student achievement using two strategies, both of which rely on two assumptions that we demonstrate are likely to hold: first, that in a given school year, within-student differences in absences in math and English language arts (ELA) are conditionally random; and second, that any spillover effects of absences in one subject on academic mastery in another are relatively trivial. The first empirical approach we employ is a proxy plug-in solution (Wooldridge, 2010), similar to twins-based identification strategies (Ashenfelter and Krueger, 1994), in which we use absences in math to control for the time-varying, subject-invariant factors that cause absences in order to identify the impact of ELA absences on ELA achievement, and vice versa. The second approach is similar:

[1] U.S. Department of Education. Chronic absenteeism in the nation's schools. From: https://www2.ed.gov/datastory/chronicabsenteeism.html

We stack the data by subject and then estimate student-year fixed-effects models.

We also exploit information on the timing of absences, since both the frequency and the impact of absences may vary over time. Figure 1 plots average daily class absences and shows that students' absences are not uniformly distributed throughout the school year. The frequency of absences sharply increases over the fall semester, remains relatively constant at the beginning of the spring semester, and then increases again at the end of the school year. Trends in absences across the school year also vary by students' racial/ethnic background: Both the white-black and white-Hispanic attendance gaps increase significantly over the course of the school year, with the white-black gap more than tripling and the white-Hispanic gap more than doubling, as shown in Figure 2. This type of nuance is missed when the focus (or available data) is solely on students' total annual absences. Thus, a secondary contribution of the current study is its demonstration that student absences are a dynamic phenomenon and should be treated as such in research and policy endeavors.

[Figures 1 and 2 about here]

We find that absences in the spring semester are far more detrimental than those in the fall semester. This too is a novel contribution and a departure from previous research, which has generally used administrative data that do not indicate when absences occurred and focused on early-grade students as opposed to older students (Gershenson et al., 2017; Gottfried, 2009). An exception is a study by Gottfried and Kirksey (2017), who found that spring but not fall absences are negatively associated with end-of-year elementary school test scores in a small urban district in California. We develop this idea in a causal framework that allows the effects of absences to vary by week of the school year. This level of granularity allows us to show that absences that occur after the annual window for state standardized testing do not appear to affect test scores, reaffirming the causal interpretation of the baseline estimates, as in Herrmann and Rockoff's (2012) analysis of teacher absences. We also consider variation in the length of absences to test whether the spacing of absences matters over and above the total number of absences.

Finally, we provide relatively novel evidence on the causal effects of high school student absences on long-term educational outcomes. These results cross-validate the main results for test scores and show that absences not only matter for contemporaneous performance on state tests and in specific courses but also have consequences for high school completion and college enrollment. We apply selection-on-unobservables bounding methods to our baseline value-added models to show that an implausibly large degree of sorting on unobservables is needed to explain away the estimated effects of absences on high school completion and college enrollment (Altonji et al., 2005; Oster, 2019). Given that almost all of the existing evidence on absences' harms focuses on contemporaneous achievement effects, credible estimates of the long-term impacts of absences on the outcomes of ultimate policy interest, such as educational attainment, significantly improve our understanding of the consequences of absenteeism and better inform policy-making around this issue.

Our results show that on average, missing 10 math classes in the spring semester reduces math scores on state standardized tests by 7% of a standard deviation, which is roughly equivalent to the effect of replacing an average teacher with one from the 20th percentile of the effectiveness distribution. Similarly, having 10 absences in the spring semester reduces course grades by 19% of a standard deviation. In contrast, absences in the fall semester appear to matter very little, if at all. The effects of absences are approximately linear and similar in magnitude across student and school subgroups. Moreover, absences in 9th and 10th grade have long-lasting impacts on educational attainment. Having 10 absences in 9th grade reduces the probability of on-time high school graduation by 8% and the probability of immediate college enrollment by 7%. These results suggest that middle and high school absences are harmful for both contemporaneous academic achievement and long-run educational attainment.

This paper advances the literature on student absenteeism in several ways. First, it provides the cleanest identification of the causal effects of student absences on educational achievement to date. Prior studies have largely relied on student fixed effects or value-added models to control for time-invariant student traits or time-varying observable characteristics, respectively; these studies have generally failed to address the concern that time-varying, unobservable shocks jointly influence both achievement and absences (Aucejo and Romano, 2016; Gershenson et al., 2017; Gottfried, 2009, 2011). A notable exception is a study by Goodman (2014), which used moderate levels of snowfall as an instrument for absences, though the exclusion restriction in this case is debatable and the resulting local average treatment effects lack external validity. Other papers have used experimental evaluations of interventions designed to improve student attendance by sending information to parents through personalized text messages (Rogers and Feller, 2018; Bergman and Chan, 2019). These randomized controlled trials provided a unique opportunity to indirectly identify the impact of absences on achievement, though there too the results are estimates of local average treatment effects that do not necessarily generalize to the average harm of a student absence.

Second, using rich class-by-day-level data, we provide novel evidence at the secondary school level that enhances our understanding of student absenteeism in several ways. Existing evidence comes almost exclusively from primary school settings and considers absenteeism using full-day absences.[2] This is understandable, as the organization of primary schools around self-contained classrooms makes both data collection and empirical analyses relatively straightforward.[3] However, in secondary school settings, part-day and class-specific absences become more prevalent as students change classrooms and teachers several times during the day for different subjects (Whitney and Liu, 2017). Absences in later grades are fundamentally different from those in earlier grades, as older students have more agency over their school attendance habits; thus, it is not obvious that evidence on the impacts of student absences in elementary school applies to middle and high school settings. The dearth of credible evidence on how student absences affect academic performance in middle and high school is troubling, as how, when, and where absences affect secondary students' performance has important implications for the design and targeting of interventions, the consequences of absence-based accountability policies, and the role of student absences in contributing to demographic gaps in educational outcomes. In addition, middle and high school students are at a critical developmental stage as they transition into college and young adulthood, so understanding the causes and consequences of absences during this time is essential for efforts to reduce high school dropout rates and increase college readiness and enrollment.

The current study helps fill this gap in the literature by providing comprehensive, arguably causal evidence of the distributional impacts of middle and high school student absences on state standardized test scores, school grades, and educational attainment. The paper proceeds as follows. Section 2 describes the administrative dataset used in our analyses. Section 3 introduces our basic value-added model and the intuition behind our identification strategy. Section 4 presents the main results on absences' impacts on academic achievement. Section 5 examines the long-run effects of absences on high school graduation and college enrollment. Section 6 concludes the paper by discussing the main takeaways from the study.

[2] Interventions designed to reduce student absences are similarly disproportionately focused on the early grades (Bauer et al., 2018). Notable exceptions are described by Rogers and Feller (2018), who found that a light-touch information intervention reduced chronic absenteeism rates by about 10% in both primary and secondary grades, and Bergman and Chan (2019), who found that text messages to the parents of middle and high school students about their grades and attendance increased student attendance by 12%. A recent paper used student fixed-effects models to show that high school students' full-day absences harm achievement in a small urban district in California (Kirksey, 2019). Student absences likely matter in post-secondary settings as well (Arulampalam et al., 2012), though this is beyond the scope of the current paper.

[3] Primary school absences are typically full-day affairs, and part-day absences would pull students from the same self-contained classroom regardless of when in the day they occur.

2 Data

2.1 Administrative Data

Our main analyses use administrative data from a large urban school district in California from the school years 2002-2003 through 2012-2013. The dataset is unique in that it contains student attendance records for each class on each day, along with the reason for any absence and whether or not it was formally excused. The attendance data contain a unique identifier for each class, allowing us to match each class to its corresponding teacher and identify other students enrolled in the class. The dataset also provides information on student and teacher demographics and students' academic performance, including scores on state-mandated tests and course-level grades. Prior studies using the same data to examine attendance gaps by socioeconomic status (Whitney and Liu, 2017) and teachers' impacts on student attendance (Liu and Loeb, forthcoming) have included extensive analyses to validate the accuracy of the attendance data.

These data are ideal for the current study for several reasons. First, they provide rich class-level attendance data that include the class period, day, and course; data with this level of detail are rarely available to researchers. Nearly all existing attendance studies use full-day absences to measure total absences. Since part-day absences account for more than half of total absences in secondary school (Whitney and Liu, 2017), disregarding part-day absences may result in considerable measurement error, which may bias estimates of the impact of absences on student achievement, especially when part-day absences are nonrandomly distributed across the student population. In addition, such nuanced data not only allow us to compute the total class absences a student has for a specific class but also provide flexibility to code absences based on what day, week, or month they occurred, a feature key to our identification strategy. Lastly, the studied district has a large and diverse student body, providing the power and variation necessary to conduct the analysis.

We combined several databases to construct the analytic sample. First, we match the attendance data to student course-taking data and identify the corresponding class, subject area, and end-of-course grades. We focus our analysis on math and English language arts (ELA), as these two subjects are consistently tested across all grade levels in state-mandated exams during the study's timeframe. We only look at classes with instructional content included in end-of-year state standardized tests; this excludes courses such as remedial math or reading, tutoring, study hall, and courses for English language learners. About 20% of all courses in our dataset are considered not directly related to state standardized tests, so attendance records from these courses are excluded from our analyses. We use end-of-course grades as an outcome variable in addition to standardized test scores. Second, we link student attendance data to a rich set of demographic variables, including race/ethnicity, gender, English language learner status, special education status, disability status, gifted status, and state standardized test scores. Students in grades 2 through 11 were required to take state standardized tests during the years of our study.

The attendance data show whether absences occurred before, during, or after the annual state standardized testing window. From the school years 2002-2003 to 2010-2011, the California Education Code required all standardized tests to be administered within a 21-day window, where the median day is that on which 85% of yearly instruction is complete, plus or minus 10 instructional days. Beginning in the 2011-2012 school year, requirements were revised to a 25-day window, or 12 instructional days around the day on which 85% of yearly instruction is complete. For more recent school years (i.e., since 2008-2009), we observe the exact date on which students began testing. A trivial portion of students start their tests after the required date, which may be due to make-up exams for students who were absent on the test date. We also cannot verify whether all tests were administered before the last day of the testing window, as each school can choose its own test dates and can choose how it spreads out the exam dates. Thus, while we can clearly identify absences before the state tests, there is some ambiguity as to whether absences late in the year occurred during or after the testing window. We discuss in greater detail how we incorporate this information into our identification strategy in Section 3.

The final analytic sample consists of student-year-classroom observations with nonmissing values for the covariates of interest: demographic indicators, test scores, and attendance records. We exclude from the analysis students who missed more than 50% of the school year, as these students likely have extreme circumstances such as medical issues that hinder their ability to attend school normally. Lastly, we exclude students who enrolled in multiple math or ELA classes in the same semester, as this subgroup of students makes up less than 5% of the overall sample and may not be comparable to the more typical student sample. These students tend to be special education students, students with severe disabilities that require individualized plans for learning, or students who are otherwise on an alternative pathway to high school completion.

2.2 Student Data

Table 1 shows the demographics of the analytic sample. The district is racially diverse: About 44% of the students are Asian, slightly over one-fourth are Hispanic, and about 14% are black. Given this diversity, it is not surprising that a quarter of the students observed are ever flagged as English language learners. The average (standardized by grade and year) test scores on both math and ELA in our sample are slightly larger than zero, which is an artifact of the exclusion criteria described above. Panel B of Table 1 provides descriptive statistics at the classroom-year level, which are similar to those at the student and student-year level.

[Table 1 here]

2.3 Absence Data

During the timeframe of the current study, teachers used paper scantron sheets to mark students as absent, tardy, or present in each class period. For an absent student, a clerk in the school office would mark the student as excused if they received a phone call from a parent or guardian providing reasons for the absence; otherwise, the absence was identified as unexcused. We have no reason to believe there would be any systematic bias or error in this traditional way of marking attendance (Whitney and Liu, 2017). These data allow us to classify each school day as having a full-day absence when a student misses every single class and a part-day absence when a student misses some but not all classes.

Figure 3 displays the distributions of total absences in math classes by middle school students and high school students separately. The distributions are positively skewed. Both levels of secondary school have a fair amount of students with zero absences, with middle schools having a higher share of students with perfect attendance than high schools. This contrast suggests that high school students tend to be less engaged than younger students, or at least that they have more complicated lives that lead to missing particular classes. The distribution of absences from ELA classes is very similar (see Figure A1).

Table 2 provides descriptive statistics by subject and semester. On average, students in a math class have 4.1 class absences during the fall semester. ELA classes have almost identical statistics, suggesting that there are few differences between the subjects' attendance patterns. There is a clear increase in the average number of absences in both subjects in the spring semester. Indeed, daily absences increase fairly monotonically throughout the school year, as shown in Figure 1. This dynamism presents both challenges and opportunities for modeling the impact of absences on achievement, as we discuss in Section 3.

Table 2 also shows statistics based on the standardized testing windows defined in the state education code. Students accrue approximately three absences prior to the testing window. Students also have about the same number of absences during the testing window, which typically lasts 21 to 25 school days, as they do during the period after the testing window. Within-student standard deviations of absences are relatively large compared with the overall standard deviation, indicating that more than half the variation in absences occurs within, as opposed to between, students.

[Table 2 here]

[Figure 3 here]

To demonstrate the basic correlation between absences and student achievement, we report test scores separately for chronically and non-chronically absent students (with chronic absence defined as missing at least 18 full school days in a year) in Table 3. About 3.7% of student-year observations are classified as chronically absent. As expected, chronically absent students are academically weaker than their peers: The gap in math test scores between the two groups is 0.84 standard deviations. The gap in ELA test scores is slightly smaller yet still sizable at 0.73 standard deviations (see Table A1). Racial and socioeconomic achievement gaps are present as well among both chronically and non-chronically absent students. Interestingly, non-chronically absent black students fare worse than chronically absent white students, and the Asian-white achievement gap (in favor of Asians) changes direction when looking at chronically absent students.

[Table 3 here]

2.4 Long-Term Measures

We augment the dataset described thus far by incorporating measures of long-run outcomes, such as on-time high school graduation and college enrollment. We observe whether a student graduated from the district "on time" (four years after initial enrollment in high school). The district does not track graduation information for students who do not officially graduate or drop out. Based on the district's suggestion, we code students who should have graduated from high school prior to 2015 but whose graduation status is missing as not having graduated on time. We also observe students' post-secondary enrollment data, which the district obtained from the National Student Clearinghouse, a nonprofit organization that provides degree and enrollment verification for more than 3,300 colleges and 93% of students nationwide. Such data is available through the end of 2016, covering slightly more than 55% of students in our sample.[4] We examine these two long-term outcomes to evaluate whether absences in secondary school have effects above and beyond their immediate impact on student achievement.

[4] There is no college enrollment information for students who do not officially graduate or drop out from the district. To be consistent with how we code graduation, we code these students as not enrolling in college.

3 Identification Strategy

This section describes our novel approach to isolating the causal effects of middle and high school student absences on academic achievement. Section 3.1 introduces a lagged-score value-added model of the education production function. This baseline model is similar to those used in analyses of elementary school absences (Aucejo and Romano, 2016; Gershenson et al., 2017). It also serves as a point of departure for our preferred method, described in Section 3.2, which leverages the availability of subject-specific absences. As described in Section 3.2, our preferred approach exploits between-subject differences in absences within a single student-year to remove the threat to validity posed by unobserved student-year shocks. Finally, Section 3.3 provides some supporting evidence for the identifying assumptions.

3.1 Value-Added Model

Following Gershenson et al. (2017), we consider student attendance to be a current input in the education production function. We specify the education production function as a typical value-added model, in which lagged achievement measures on the right hand side proxy for the unobserved histories of educational and familial inputs received by the child (Todd and Wolpin, 2003). Mounting evidence suggests that simple lagged-score models control for the most common forms of nonrandom classroom assignments (Chetty et al., 2014; Guarino et al., 2015; Kane and Staiger, 2008). In the case of school-provided inputs, such as teacher quality or class size, such sorting is the primary threat to identification, as a student's performance in the previous year is readily available to both parents and school administrators, who might act on this information when making classroom assignments.

However, when estimating the harm attributable to student absences, several new threats (sources of endogeneity) emerge. Because attendance is an intermediate output of the education production function as well as an input, it might be affected by the same shocks that affect test scores and other educational outcomes. The concern is that idiosyncratic (time-varying) shocks jointly affect current achievement and attendance but are not adequately controlled for by lagged test scores or student or school fixed effects. For example, there may be year-specific student-, classroom-, school-, or neighborhood-level shocks that jointly affect both attendance and achievement.

Aggregate shocks at the classroom, school, or neighborhood level are relatively easy to adjust for: In each case, there is within-group variation in both absences and outcomes. Accordingly, we follow Gershenson et al. (2017) in conditioning on classroom fixed effects, which addresses the concern that classroom-level shocks (e.g., an especially effective teacher or disruptive student) influence both absences and achievement. Controlling for classroom fixed effects has extra benefits in the secondary school setting because it also helps account for scheduling differences (e.g., regular schedules vs. block schedules) between class meetings and academic tracks. Although the district we study does not have formal tracks, students select into different math classes and tests. Classroom fixed effects allow us to compare students who are in the same academic "tracks." Classroom fixed effects also make school-year fixed effects redundant, since classrooms are nested within schools and thus also control for year-specific school-level shocks (e.g., a school-wide policy change). Similarly, we leverage rich residential data to condition on census-tract-by-year fixed effects, which reduces concerns that an external, community-level shock, such as a police shooting, affects both absences and achievement (Gershenson and Hayes, 2017). The tract-by-year fixed effects are not redundant because they refer to students' home addresses, which do not perfectly determine school attendance, as students from different tracts can be present in the same classroom.[5]

All of this leads to a fairly standard value-added model, which we take as our baseline model:

$$y_{ijt} = \beta X_{ijt} + f(A_{ijt}) + u_{ijt}. \tag{1}$$

Here, i, j, and t index students, subjects, and academic years, respectively; y is a standardized end-of-year test score; X is a time-varying vector of observed student characteristics, such as lagged test scores, lagged absences, English learner status, and full sets of classroom and census-tract-by-year indicators; f is a general function of subject-specific absences (A); and u is an idiosyncratic error. As discussed above, the classroom fixed effects subsume the year, school, and teacher fixed effects as well as observed classroom characteristics, such as class size and sociodemographic composition, that are typically included in a value-added model, as these are specific course sections taught by a given teacher in a given year. The baseline model specifies f as a linear function of total classroom-specific absences in each semester. We separate absences in the fall and spring semesters because absences that occur later in the year (closer to the time of the state standardized tests) might matter more for student achievement. We start with the linear form because Gershenson et al. (2017) found the effects of annual primary school absences to be approximately linear, though we subsequently test for nonlinearities by specifying f as a nonparametric step function and as a parametric polynomial function. We estimate this model separately for math and ELA and for middle and high school students, as absences might be more detrimental in math, and the reasons for absences might differ by school type.

[5]Students in this district receive secondary school assignments based on a choice process and may commute across the city to a school outside of their tract if desired.

3.2 Exploiting Subject-Specific Absence Data

The lagged scores and classroom and tract-by-year fixed effects included in X eliminate many of the threats to validity articulated above, but concerns about idiosyncratic student-level shocks to both A and y remain. It is useful to formalize this threat by decomposing the error term u_{ijt} into a possibly endogenous student-year "educational shock" (θ_{it}) and a random error (ϵ_{ijt}). One way forward, then, is to use absences in another subject (A_{-j}) to proxy for θ_{it}.[6] Specifically, let $\theta_{it} = \alpha + \delta A_{it,-j} + \nu_{it}$. Plugging this into Equation 1 yields the following:

$$y_{ijt} = \alpha + \beta X_{ijt} + f(A_{ijt}) + \delta A_{it,-j} + \nu_{it} + \epsilon_{ijt}. \tag{2}$$

Importantly, we have removed the troublesome year-specific shock from the model. We can now look at Equation 2 and clearly state the two identifying assumptions for this proxy plug-in solution (Wooldridge, 2010). The first identifying assumption hinges on the redundancy of the proxy A_{-j} from Equation 1: Conditional on math absences, X, and the year-specific student shock, ELA absences do not affect math achievement. This assumption is plausible, as the primary mechanism through which absences harm achievement is via lost instructional time. It is certainly more reasonable than the assumption that there are no idiosyncratic, student-specific shocks that jointly affect attendance and achievement. The second assumption regards the presence of v_{it} in Equation 2: A_{-j} needs to be a good proxy in the sense that conditional on A_{-j}, A_j is no longer correlated with the idiosyncratic shock (v_{it} in Equation 2). This too is plausible in the current setting, given the strong correlation between subject-specific absences in a given year and the underlying process through which an idiosyncratic shock would affect attendance.

This proxy plug-in solution is quite similar to another strategy for eliminating potentially endogenous idiosyncratic student shocks: stacking the data across subjects so that there are two observations per student-year (math and ELA) and estimating a model with student-by-year fixed effects. Formally, we estimate models of the form

$$y_{ijt} = \alpha + \beta A_{ijt} + \gamma_j + \theta_{it} + e_{ijt}, \tag{3}$$

where γ and θ are subject and student-year fixed effects, respectively. We can allow for subject-specific effects by augmenting Equation 3 to include the interaction $A \times \gamma$. The student-year fixed effects θ control for student-year-specific shocks that jointly affect achieve-

[6] This idea is similar in spirit to comparing identical twins with different schooling levels to identify the returns to schooling (Ashenfelter and Krueger, 1994).

ment and absences *the same way in both subjects*; they also make any student-year or school-year controls redundant. Like the proxy plug-in procedure described above, this stacked regression controls for the sorts of time-varying student-specific shocks that prior studies on student absences have failed to fully account for.

3.3 Identifying Assumptions

Both strategies described in Section 3.2 exploit the fact that there is considerable within-student-year variation in subject-specific absences: While highly correlated (with a .85 correlation coefficient), they are not perfectly correlated, as two-thirds of student-year observations have a different number of math and ELA absences. This raises the question of why student absences would vary between subjects in a given year, as both the proxy plug-in and stacked student-year fixed-effects strategies assume that such differences are conditionally random (e.g., because of the timing of a dentist appointment or leaving school early due to illness) and not the result of a year-specific preference for one subject over the other.[7] Specifically, the concern is that time-varying subject-specific preferences are present in the idiosyncratic error terms in Equations 2 and 3, which might bias the estimates.

We address this concern in two ways, which both show that within-student differences between subjects in absences are conditionally random. First, in Figure 4, we plot both math and ELA absences against lagged math achievement. The two plots are nearly identical, suggesting that math and ELA absences differ for reasons unrelated to math ability or preferences (which are loosely captured by lagged math performance). If differences between math and ELA absences were driven by student preferences, we would expect the ELA plot in Figure 4 to be a relatively flat, horizontal line. The fact that both math and ELA absences have nearly the same negative, approximately linear relationship with lagged math scores suggests that *absences in general* are sticky within students and are associated with negative shocks and poor academic performance. The analogous relationship between math and ELA absences and lagged ELA scores is presented in Appendix Figure A2 and shows the same pattern: Both math and ELA absences have nearly identical relationships with lagged ELA scores.

Second, we regress the differences between math and ELA, science, social studies, foreign language, physical education, and "other subject" absences on student, grade, year, and school fixed effects and then plot the estimated coefficients by grade in Figure 5. A nega-

[7]Time-invariant student preferences for a subject can be accounted for by conditioning on student fixed effects or by first-differencing the lagged-score model, which we consider when reporting the main results in Section 4.1.

tive coefficient represents a math preference (fewer math absences). The figure shows two patterns in the data that suggest that time-varying subject preferences are unlikely to pose a threat to identification. First, between-subject differences tend to be small in magnitude and statistically indistinguishable from zero. Second, these between-subject differences are relatively stable over time. We also directly present the coefficients by grade in Appendix Table A3. Most of the coefficients are insignificant other than when comparing math with science, suggesting that students' preferences do not change systematically as they progress.

Finally, we probe the plausibility of the redundancy assumption made by the proxy plug-in solution, which is that non-math (e.g., ELA) absences do not directly affect math performance. In terms of the education production function, this rules out cross-subject spillover effects of absences. At first blush, this assumption seems questionable, as more ELA absences might create more make-up work that takes away time from students' math preparation.

To understand the degree to which this potential nonredundancy is a practical concern, we estimate Equation 2 several times, each time using a different non-math subject's absences as the proxy. We also estimate one version of this equation by controlling for total absences across all of the off-subjects. The idea is that if spillover effects exist, they likely vary across courses if for no other reason than that courses vary in how much make-up work is required following an absence. Subjects such as physical education should have relatively little make-up work and thus should have smaller spillover effects from absences than more reading-intensive subjects, such as social studies. Thus, if the estimated harm of math absences is robust to which non-math subject or subjects' absences are used as proxies, it is likely that any such spillover effects are trivial.[8] This is exactly what we see in Appendix Table A4, which reinforces the validity of the proxy plug-in identification strategy.

4 Student Absences and Academic Achievement

This section presents evidence on the impact of middle and high school student absences on academic achievement. Section 4.1 reports the baseline estimates of Equations 1-3, which show an arguably causal, sizable effect of absences on standardized test scores. Section 4.3

[8] This test is similar in spirit to those of Hausman (1978) in that we compare estimates of Equation 2 using different proxies, and if the proxies are valid, we should get similar estimates. Intuitively, this is analogous to an overidentification test of the two-stage least squares estimator that repeatedly implements the two-stage least squares estimator using different excluded instruments. Bollinger and Minier (2015) gave a formal treatment of the model with an unobserved variable and multiple observed proxies for the unobserved variable and showed that using the full set of available proxies minimizes the inconsistency in the estimated coefficients on observed variables.

examines how the timing of student absences, both during the school year and in relation to other absences, moderates their effects. Finally, Section 4.4 tests for potential nonlinearity and heterogeneity in the relationship between student absences and academic achievement.

4.1 Main Results

Table 4 reports estimates of the math-score value-added models specified in Equations 1 and 2 and discussed in Section 3. Panel A presents estimates for six slightly different model specifications using the sample of middle school students (grades 6-8). Column 1 presents estimates from a relatively standard, parsimonious value-added model that conditions on linear and quadratic lagged test scores in both math and ELA; lagged GPA; school fixed effects; and some basic student, school, and classroom characteristics. The effect of fall-semester absences is negative and marginally significant but small in magnitude, suggesting that having 10 absences lowers test scores by about 1% of a standard deviation. Absences in the spring semester have a larger, more precisely estimated negative effect of about 6% of a standard deviation per 10 absences. This effect size is similar to that estimated using similar methods for 4th- and 5th-graders in North Carolina (Gershenson et al., 2017).

Columns 2-6 enrich the value-added model in Column 1 in various ways. Column 2 replaces the school fixed effects with classroom fixed effects (which are necessarily year-specific) but does not change the basic results. Column 3 augments the classroom fixed effects model to also include census-tract-by-year fixed effects, which again does not appreciably change the point estimates. The robustness of the point estimates in Columns 1-3 suggests that the estimates in Column 1 are not biased by unobserved year-specific school, classroom, or neighborhood shocks.

[Table 4 here]

Columns 4 and 5 add student fixed effects to the model. This is potentially important, as in the case of absences, time-invariant unobserved student heterogeneity may be a greater threat to validity than classroom sorting. For example, a student's baseline level of school engagement may affect both attendance and achievement. Column 4 replaces the lagged test scores with student fixed effects and provides ordinary least squares estimates, while Column 5 removes the student fixed effects by first-differencing the lagged-score model and then applying the two-stage least squares procedure suggested by Anderson and Hsiao (Anderson and Hsiao, 1982). Once again, the results are robust, suggesting that relatively stable unobserved student and family characteristics are not biasing the simple lagged-score estimates reported in Column 1. This robustness to adjusting for student fixed effects is consistent with results for 4th- and 5th-graders in North Carolina (Gershenson et al., 2017).

Finally, Column 6 reports estimates of the proxy model described by Equation 2, which includes lagged achievement as well as tract-by-year and classroom fixed effects. This strategy addresses concerns of time-varying student-level shocks, such as an illness or death in the family, that affect multiple dimensions of educational performance. The estimate reported in Column 6 controls for ELA absences, though Appendix Table A4 shows that this result is robust to using absences in other classes, including physical education. This is important because as discussed in Section 3.3, it shows that the proxy strategy's redundancy assumption is likely to hold.

In sum, Panel A of Table 4 documents several results regarding the impact of middle school students' math class absences on math performance. First, all point estimates are remarkably robust across a variety of models that account for the potential sources of endogeneity: nonrandom classroom assignments; unobserved school, classroom, or neighborhood shocks; unobserved student and family background characteristics; and time-varying student-level shocks. This suggests that a relatively simple lagged-score value-added model does a reasonably good job of identifying the causal effects of student absences. We lean on this result in Section 5 when we estimate the effects of absences on longer-run educational outcomes.

Second, middle school students' absences in the fall semester have little to no effect on end-of-year standardized test scores in math. Depending on the model specification, the effect of having 10 fall-semester absences ranges from 1 to 3% of a standard deviation and is statistically indistinguishable from zero in the preferred proxy-variable model of Column 6.

Third, the effect of having 10 spring-semester absences is significantly larger, ranging from 6 to 9% of a standard deviation. The point estimate in the preferred proxy-variable model of Column 6 is 7% of a standard deviation and is strongly statistically significant. This effect is economically significant, as it is roughly equivalent to replacing an average teacher with one from the 20th percentile of the effectiveness distribution (Herrmann and Rockoff, 2012). This also suggests that pooling annual absences into a scalar count of total absences yields a misleading result, as the timing of absences during the school year matters. Naively pooling fall and spring absences into a single annual count of absences yields an estimated effect of 4% of a standard deviation. This weighted average of the fall and spring impacts uncovered in Table 4 is misleading, as it overstates the importance of fall absences and understates the importance of spring absences. This is a fairly novel result in the literature on student absences, as previous research has generally used administrative data that do not specify when during the year absences occurred (Gershenson et al., 2017; Gottfried, 2009). An exception is a study by Gottfried and Kirksey (2017), who similarly found that spring, but not fall, absences are associated with end-of-year elementary school test scores in a small urban district in California.

Finally, the middle school math results are not unique to middle school or to math. Panel B of Table 4 and Appendix Table A2 show nearly identical results in high school math and in ELA courses, respectively. Because of this similarity, we combine middle and high school students in later analyses. To our knowledge, this is the first credible evidence that absences harm achievement in both middle and high schools, and this finding suggests that absences are a serious concern at every level of K-12 schooling. Moreover, the harm attributable to absences is fairly similar across all levels of schooling: Comparable estimates from North Carolina showed similar effects of absences in grades 4 and 5.

4.2 Stacked Regressions

As discussed in Section 3, another approach closely related to the proxy plug-in solution is to stack student math and ELA observations and directly identify the effects of absences from within-student-year, between-subject variation. As shown in Equation 3, using student-by-year fixed effects, we control for time-varying student-specific shocks and directly attribute the difference between students' math and ELA performance to their subject-specific absences. Estimates are reported in Table 5.

[Table 5 here]

For comparison purposes, in the first two columns, we start with a model controlling for student fixed effects and year fixed effects separately and then interact the absence measures with a subject indicator. This model only accounts for student time-invariant variation and yearly shocks common to all students. The results are very similar to those in Table 4. For example, as Column 2 shows, having 10 spring absences reduces student achievement by 9.5% of a standard deviation, a slightly bigger estimate than the coefficient from our preferred model in Table 4 (7.0%). We then replace the student and year fixed effects with student-year fixed effects to nonparametrically control for time-varying student-specific shocks. The coefficients shrink a bit but are still closely aligned with those in Table 4: Having 10 spring absences reduces students' math test scores by about 6% of a standard deviation.[9]

[9]These estimates are also robust to the replacement of subject fixed effects with teacher fixed effects.

4.3 Timing of Absences

In Section 4.1, we show that absences during the spring semester have a fairly large, arguably causal effect on student achievement, while fall-semester absences have essentially no effect. We now examine whether more granular differences in the timing of student absences matter for student achievement. This is a useful exercise for two reasons. First, we now include absences that occurred *after* the state standardized testing window and that thus should not affect test scores. As in Herrmann and Rockoff's (2012) analysis of the impact of teacher absences, this is a useful placebo test of the identifying assumption that estimates are not biased by idiosyncratic shocks to achievement and attendance. Second, weekly differences in the impact of student absences can shed light on the mechanisms through which absences harm student achievement and how the average classroom spends its time leading up to end-of-year exams.

With daily class-level absences, we are able to examine how absences during different weeks of the school year affect test scores. We distinguish between spring-semester absences that occur in three time periods: before, during, and after the state standardized testing window. As discussed in Section 2, we can only impute testing windows, as the exact timing of testing is unknown and may vary across students within a school. We thus identify the start date of a testing window based on the state education code and then code all absences after that date nonparametrically using weekly measures. Because estimates using weekly windows are quite noisy, we group weeks 1 to 14 together as the pre-testing window to improve the precision of our estimates; thereafter, we use biweekly bins to approximate during- and post-testing-window absences. A typical testing window starts in week 15 or 16 in the spring semester and lasts till week 19 or 20.

Results of this exercise are shown in Figure 6 (See Table A5 for corresponding estimates for ELA.) Estimated coefficients for absences in weeks 1-14 and 15-16 are both around - 0.05 and are statistically significant though slightly smaller than the baseline estimate that restricts the effects of all spring-semester absences to be the same. The coefficient estimates for absences in weeks 17-18 and 19-20 are almost twice as large, suggesting that absences during the testing window are more harmful than those that occur earlier in the spring semester. Finally, we see an insignificant and close-to-zero coefficient estimate for week 21-22 absences, which is reassuring because those absences almost certainly occurred after the test was taken. These results further confirm that lost instructional time induced by absences harms student performance and that the baseline estimate is not biased by selection (Herrmann and Rockoff, 2012).

The findings in Figure 6 are generally consistent with findings by Herrmann and Rockoff

(2012) indicating that teacher absences immediately preceding testing windows are more harmful than teacher absences earlier in the school year. There are several possible reasons for this pattern. One possibility is that teachers spend more classroom time preparing students for the test in the class periods leading up to the test, either in terms of content or test-taking strategies. This is likely at least part of the story because our data are from a period when schools faced strong accountability pressure under the No Child Left Behind Act. Another possibility is that compared with earlier in the semester, students have less time to catch up following an absence that occurs close to the test. We further investigate this possibility in Section 5, where we use course grades as an alternative measure of achievement.

[Figure 6 here]

Another important dimension of the timing of absences is their timing relative to other absences. We now consider the duration of absence spells — for instance, are four consecutive absences any more or less harmful than four absences that occur in four different months? There are several reasons to expect that the marginal effect of an absence varies with the length of the absence spell it is part of, though the direction of the correlation is theoretically ambiguous. For example, teachers and schools might make better plans to help students catch up when they miss several classes in a row as opposed to one class. There might also be fixed costs to catching up following an absence spell, such that the average catch-up cost is lower for longer spells. Alternatively, longer spells might be more harmful if too much content is missed.

Appendix Figure A3 presents a histogram of the distribution of absence spell lengths in the analytic sample.[10] The modal absence is a one-off event (i.e., part of a one-day spell). Most absences are part of one- or two-day spells. On average, a student has 1.7 absences that only last for one day and 0.7 absence spells that last for two days.

To test whether the effect of absences varies by spell length, we modify f in Equation 1 to include six separate counts of absences by spell length, grouping spells of 1, 2-3, 4-5, 6-10, 11-30, and 31 or more consecutive absences. The coefficient estimates are reported in Figure 7 and indicate the marginal effect of one absence that is part of a spell of a given length. The effect of a stand-alone absence is bigger than the marginal effect of an absence that is part of a longer absence spell; this finding is consistent with the notion of fixed costs to catching up following any absences and/or economies of scale in making up missed work. Still, the effects of absences are fairly similar in magnitude, especially when

[10] Since we are using class absences, here the spells are computed at the class period level. Thus, a two-day spell means a student misses two math classes contiguously, although the student might attend other classes during these two days.

comparing across multi-way spells of different lengths. It should also be noted that because longer spells are relatively rare, as shown in Appendix Figure A3, estimates for absences associated with longer spells are less precisely estimated. In sum, all absences are harmful, while the marginal cost of absences that are part of a longer absence spell is smaller.

[Figure 7 here]

4.4 Nonlinearity and Heterogeneity

The main results presented in Section 4.1 assume that the effects of absences are linear — that is, that there are neither diminishing nor increasing costs to student absences, nor is there a discrete effect of crossing the threshold for being considered chronically absent, which in this district is defined as missing at least 10% of total school days (accruing about 18 absences). We make this simplifying assumption in the baseline model because Gershenson et al. (2017) and Kirksey (2019) found the effects of student absences to be approximately linear. Here, we use quadratic and nonparametric specifications of f to verify that the same is true in middle and high school settings.

Figure 8 plots fitted regression lines from the linear, quadratic, and nonparametric models, which approximately overlap with each other, though the nonparametric estimates are fairly noisy for higher absence counts, as there are relatively few students in those bins.[11] The similarity between the three models suggests that the effect of absences is approximately linear and that there is no nonlinearity in achievement at the threshold of chronic absenteeism (though chronically absent students certainly perform significantly worse than students with just one or two absences).

[Figure 8 here]

The models estimated to this point also assume that the harm of absences is the same across all student and school types. We now relax this assumption and test whether the harm attributable to absences varies by school or student characteristics. Columns 1-6 of Table 6 show estimates from the baseline model given by Equation 2 separately by student demographic characteristics. The effects are similar for boys and girls and for students from different racial and ethnic backgrounds, though the harm for black students is larger than for other groups, and the effect for white students is imprecisely estimated.

[11] We interact the linear and quadratic absence counts with an indicator for having at least one absence to better align with the nonparametric plot.

Columns 7-9 of Table 6 show estimates of the baseline model for different school poverty levels. We do not see any systematic differences between more- and less-advantaged schools; absences seem similarly harmful across school types. We further interrogate this idea by estimating the baseline model separately for each of the 45 schools in our sample. Kernel density estimates of the distribution of estimated coefficients are plotted in Figure 9. More than 75% of the estimates are negative, and most are within one standard deviation of the baseline point estimate. This suggests that absences are broadly harmful in all schools. Harm does vary somewhat across schools, but it does not vary systematically by student demographic or socioeconomic background.

[Table 6 here]
[Figure 9 here]

5 Other Outcomes

The results presented in Section 4 provide compelling evidence that middle and high school absences harm student learning, as measured by standardized end-of-year tests. But test scores are just one measure of achievement, and they are primarily interesting because they proxy for longer-term outcomes of policy interest, such as high school completion and college entry. The correlation between high school absences and poor long-run outcomes, such as dropout, drug use, and criminal activity, is well documented (Rumberger and Rotermund, 2012; Hawkins et al., 1998; Henry and Huizinga, 2007; Loeber and Farrington, 2000; Rocque et al., 2017). However, this research is largely correlational.

In this section, we first use end-of-course grades as an alternative contemporaneous measure of educational achievement and engagement. Although grades are a more subjective measure of student academic performance than test scores are, research has consistently found high school grades to be more predictive than test scores of long-term student success (Easton et al., 2017).[12] This might be partly due to grades capturing some measure of noncognitive or socioemotional skills in addition to academic skills. We then directly examine whether the impact of absences persists for long-run measures of educational attainment of ultimate interest, such as high school graduation and college enrollment.

[12]Grades in the district are explicitly based on academic performance and are not allowed to be based on nonacademic factors such as absences.

5.1 Course Grades

To study absences' impact on course grades, we simply replace test scores with course grades in the baseline model (Equation 2). These estimates are presented in Table 7. Grades are standardized by course, school, and year to have a mean of 0 and a standard deviation of 1, so we can compare the coefficients across schools, years, and courses. Similar to the main results for test scores, the timing of absences matters for course grades: Spring absences are much more influential than fall absences. For example, having 10 absences in math classes in the spring reduces math grades by 20% of a standard deviation.

In Column 2 of Table 7, we mimic the analysis reported in Figure 6 and allow the effect of absences on course grades to vary across weeks of the spring semester. The coefficients tend to increase over time, indicating that absences later in the year have bigger impacts on course grades than earlier absences. This seems to rule out a "teaching-to-the-test" explanation of the large effects of absences during the testing window on test scores. Instead, the mechanism is likely that students simply have less time to catch up on lessons missed later in the school year. Overall, the results in Table 7 provide additional evidence that middle and high school student absences harm achievement, that this relationship is causal, that absences later in the year are more harmful than those earlier in the year, and that a likely mechanism is simply lost instructional time.

5.2 Long-Term Outcomes

To this point, we have identified arguably causal impacts of middle and high school student absences on contemporaneous measures of educational achievement. However, this finding is interesting primarily because real-time measures of achievement, such as end-of-course exams and course grades, proxy for longer-run educational and socioeconomic success. We now provide direct evidence that absences have causal effects on such outcomes by using the identification strategy laid out in Equation 1 and used in Columns 1-3 of Table 4. We focus on the effects of 9th-grade math absences using the basic lagged-score model with classroom and census-tract fixed effects because the redundancy assumption made by the proxy plug-in strategy defined in Equation 2 likely fails by definition, as absences in any subject could affect student-specific outcomes, such as high school completion, and student fixed effects estimators are not identified because there is no within-student variation in such long-run outcomes.[13]

The lagged-score model likely produces good approximations of the causal effects of

[13]The effect of absences in grade 10 are quite similar and are provided in Appendix Table A6.

absences for two reasons. First, as shown in Table 4, the estimated impact of absences on math scores is robust to augmenting the basic lagged-score model to account for either time-invariant or time-varying student unobservables (compare across Columns 3, 5, and 6). This suggests that the lagged-score model is not too biased by unobserved student heterogeneity or student-year shocks. Second, we estimate bounds for the causal effects that rely on different assumptions about the amount of selection on unobservables into student absences (Altonji et al., 2005; Oster, 2019). Even when assuming a strong degree of selection on unobservables (i.e., that they are just as important as the observables in explaining the treatment), we still find a nonzero, statistically significant negative effect of absences on the long-run outcome in question. Put differently, there would have to be an implausibly large amount of selection on unobservables (i.e., two to three times as much) as there is on observables to fully explain away the estimated impacts of absences on long-run outcomes. Given that the observables include lagged achievement and school and neighborhood fixed effects, this is extremely unlikely.

One difficulty of coding class absences when using long-run outcomes is that students have different numbers of class meetings based on which classes they take. For this reason, we do not aggregate absences across all subjects, as they are not comparable between students. One solution is to use full-day absences, which we know misses a large portion of actual absences secondary school students have. However, we continue to use the subject-specific absence measure, which is likely representative of how often students are absent, given that class absences in different subjects are highly correlated with each other: The correlation between math absences and ELA absences is around .85. We would therefore expect using absences from different subjects to yield similar results, which is exactly what we find.

Column 1 of Table 8 shows the results of the long-run model for an on-time high school graduation. Having 10 absences in 9th-grade math classes reduces a student's likelihood of graduating from high school on time by about 6 percentage points. This is a sizable effect, given a base graduation rate of 74%. The lower bound of the causal effect, assuming an equal amount of selection on unobservables and observables, is a qualitatively similar and statistically and economically significant effect of 4 percentage points. Put differently, there would need to be more than three times as much sorting on unobservables as there is on observables to attribute the estimated effect entirely to selection bias. Columns 2 and 3 show that these absolute effects are smaller for college enrollment but remain statistically significant and in percentage terms are similarly sized effects, given the lower base rate of college enrollment. Once again, the Oster bounds suggest that there is a causal effect on college enrollment, though sorting seems to play a bigger role here than in the high school graduation analysis.

In Columns 4 and 5 of Table 8, we further investigate the college enrollment results by distinguishing between enrollments in two- and four-year institutions. We would expect absences to primarily disrupt the long-run attainment of students on the margin of pursuing post-secondary schooling. Because many students on the margin of pursuing college opt (at least initially) to enroll in two-year institutions, we would expect the causal effect of absences to disproportionately influence enrollments in two-year institutions. Indeed, this is exactly what we see, as the point estimate in Column 5 is twice as large as that in Column 4 and is not driven by selection on unobservables: The Oster bound is nearly identical to the point estimate. The effect of absences on four-year college enrollments is not only smaller but also likely due to selection on unobservables, as even a modest (60%) amount of selection on unobservables is enough to explain away the point estimate of -0.02.

Panel B of Table 8 shows that these basic findings are replicated when we run the same set of regressions for ELA classroom absences. Again, this is to be expected because math absences and ELA absences are highly correlated. Also, consistent with the heterogeneity analyses conducted in Table 6, we find no evidence of heterogeneity in the long-run effects of absences. We also replicate the results using 10th-grade absences, as shown in Appendix Table A6, and find results that are qualitatively similar to those based on 9th-grade absences. In sum, the results of Table 8 reaffirm a causal interpretation of the main finding that absences harm achievement and provide arguably novel evidence that 9th- and 10th-grade absences have a causal effect on subsequent educational attainment, which motivates the potential value of absence-related interventions in high school.

6 Conclusion

This study uses a novel identification strategy to estimate the immediate and longer-run impacts of middle and high school student absences. The empirical approach, which uses absences in one subject to proxy for unobserved student-year specific shocks that might affect attendance in another subject, is facilitated by an unusually detailed administrative dataset from a large urban school district providing the date and class period of course-specific student absences. Consistent with evidence from the elementary school sector, we find approximately linear effects of student absences on test scores that are not biased by time-varying student shocks and are relatively constant across student sociodemographic groups. We find that absences in the spring semester are significantly more harmful than those in the fall semester, particularly absences in the weeks leading up to end-of-year testing. Having 10 spring-semester absences reduces test scores by about 7% of a standard deviation in middle and high school in both math and ELA. These are economically significant effects on par with the impacts of other popular interventions. More importantly, we use selection-on-observables techniques to show that student absences in the 9th and 10th grades have long-lasting impacts on the probability of educational attainment, at least for those students who are on the margin of obtaining credentials: Having 10 absences in a core subject reduces the probability of high school graduation by 0.06 (8%) and of enrolling in college by 0.05 (7%). The college enrollment result is almost entirely driven by the impact on enrollments in two-year post-secondary institutions. To our knowledge, this is the first credible evidence of the harm attributable to middle and high school student absences.

Leveraging data on the timing of end-of-course exams, we provide an additional check on the causal interpretation of the main results by showing that post-exam absences do not appear to affect exam scores. We then combine this information with data on course grades to probe the possible mechanisms driving the results. Because absences later in the year are more detrimental to course grades and exam scores, and in the case of course grades even after the testing window has closed, we can rule out a pure teaching-to-the-test explanation. Instead, it appears that late-year absences are simply harder to make up, either because there is less time to do so or because students are busier and encountering more difficult material later in the year. Future work should consider the design and impact of interventions that aim to remediate the harmful effects of absences in addition to reducing absences, as it is unlikely that all absences are discretionary.

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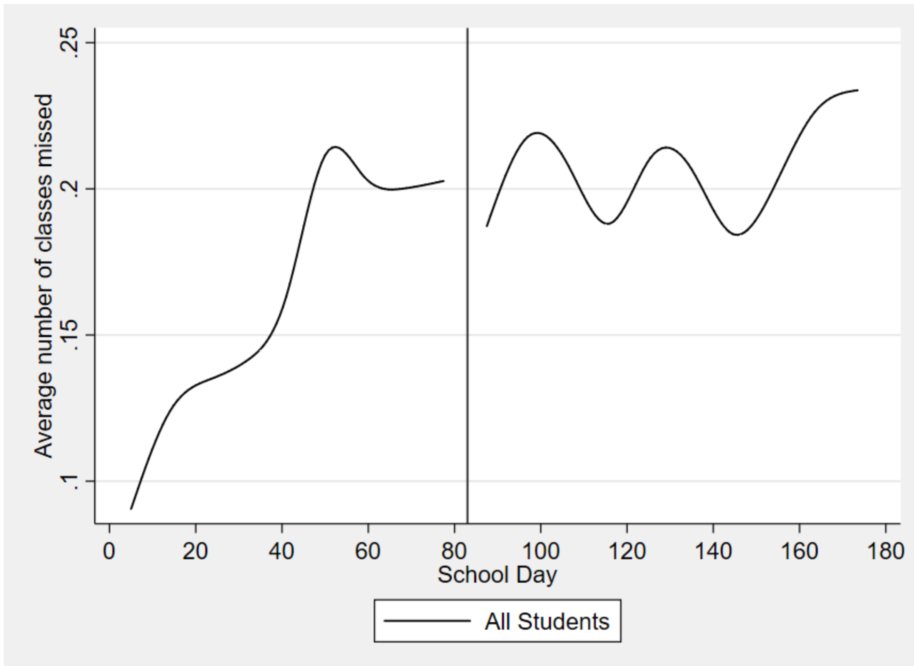
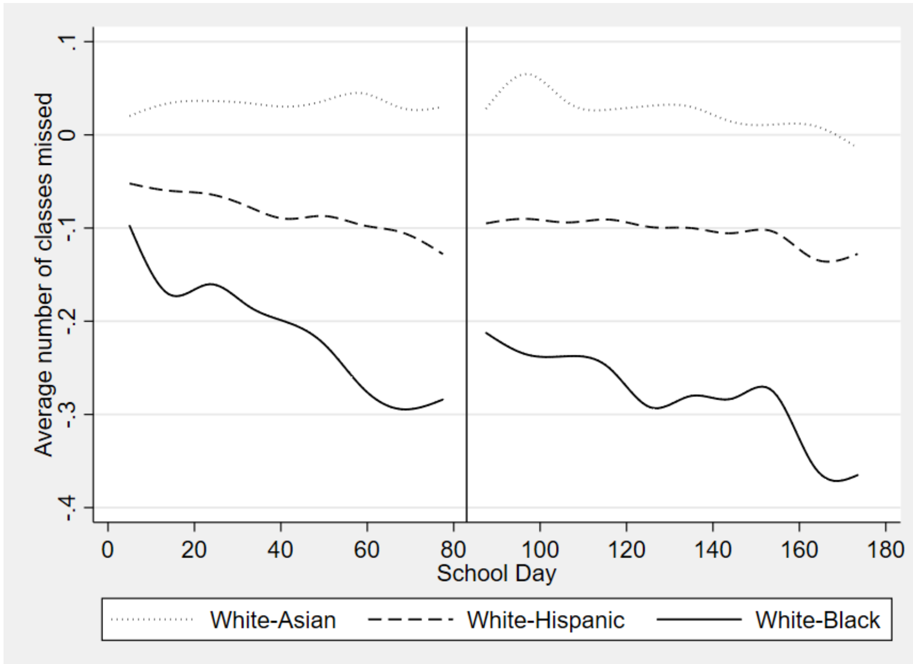
Figures and Tables

Figure 1: Average Number of Missed Classes in the District



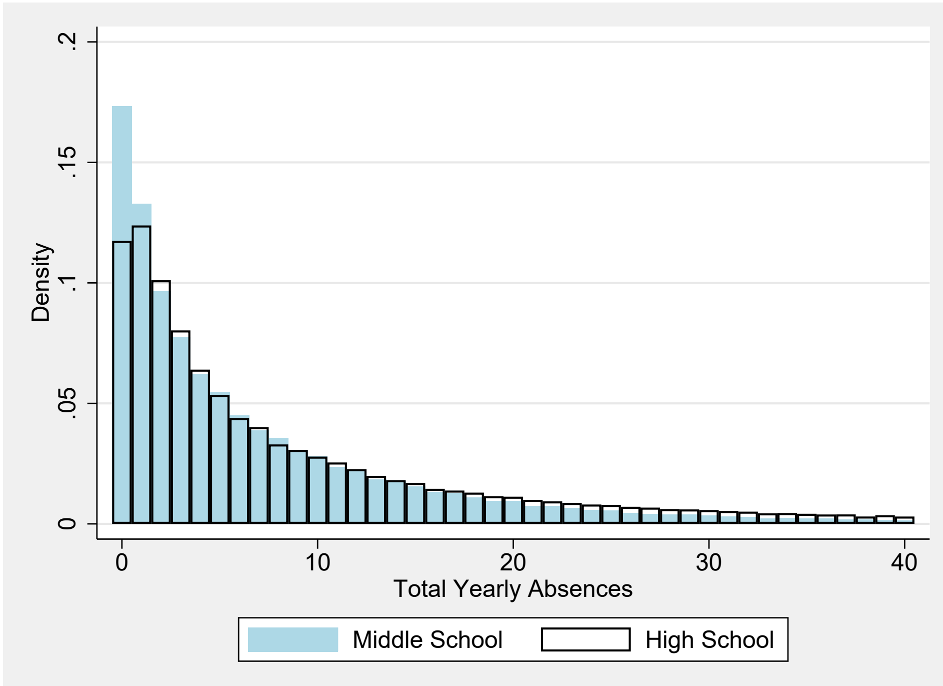
Note: Observations are the mean number of classes missed by day across all students in the district in a sample year (2012-2013). Patterns are similar when using other school years. A vertical line denotes the cutoff between the fall and spring semesters. The figure uses a smoothed line for the purpose of simplifying the visualization of moving averages across 180 school days.

Figure 2: Differences by Race/Ethnicity in Average Number of Missed Classes in the District



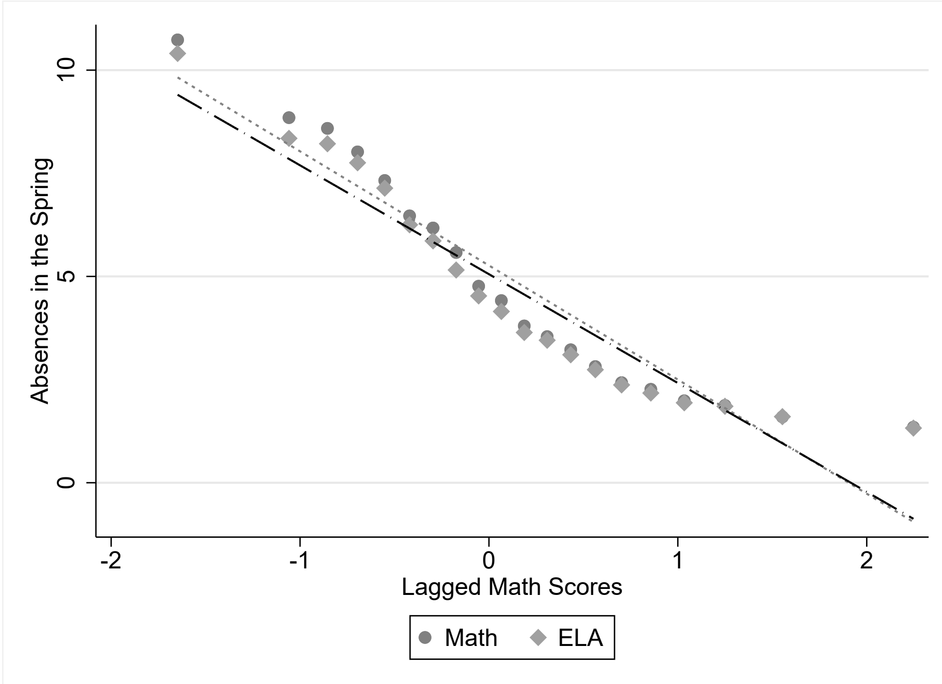
Note: Observations are differences in the mean number of classes missed by day among white and nonwhite student groups in the district in a sample year (2012-2013). Patterns are similar when using other school years. A vertical line denotes the cutoff between the fall and spring semesters. The figure uses smoothed lines for the purpose of simplifying the visualization of moving averages across 180 school days.

Figure 3: Distribution of Absences in Math Classes



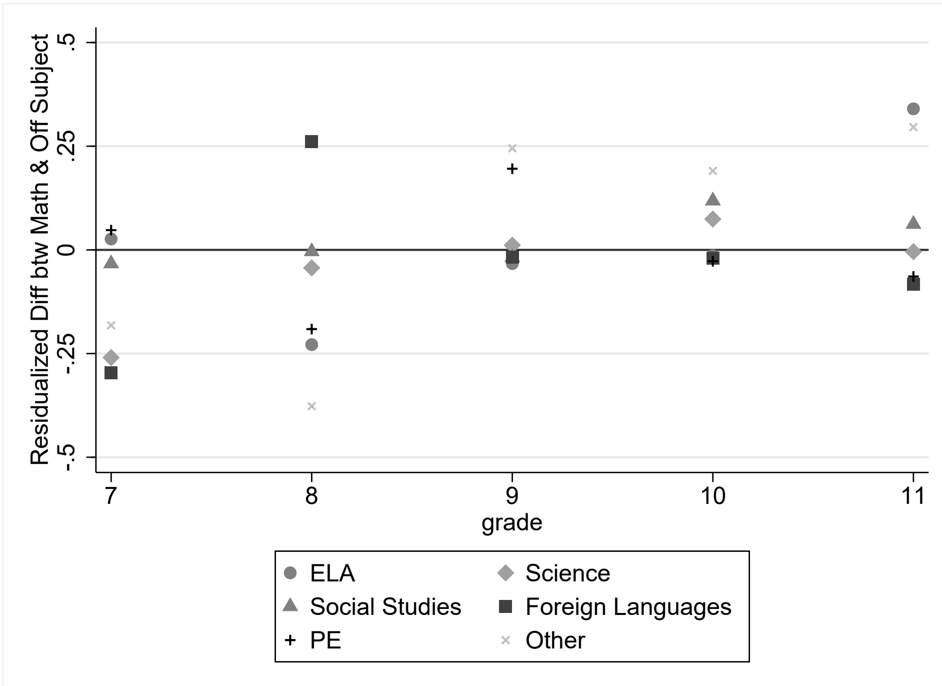
Note: Observations are counts of student absences in math classes that occur in the entire school year from 2002-2003 to 2012-2013. Data are trimmed at 40 absences to show the bulk of variation.

Figure 4: Binned Scatter Plot of Absences vs. Lagged Math Scores



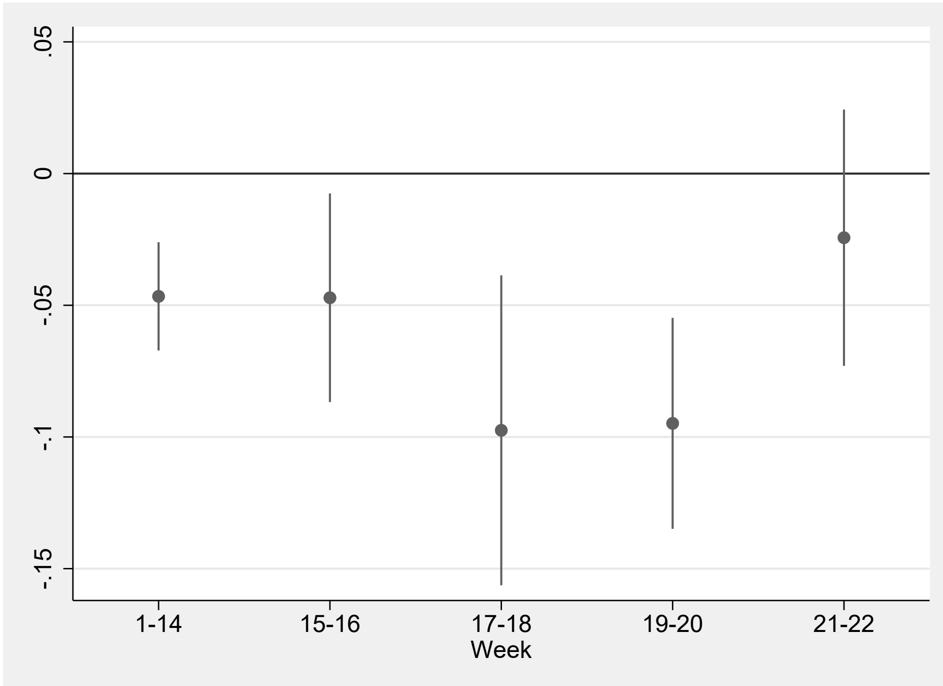
Note: Observations are counts of student absences in math classes that occur in the spring semester from 2002-2003 to 2012-2013.

Figure 5: Changes Over Time in Math Preferences Compared with an Alternative Subject



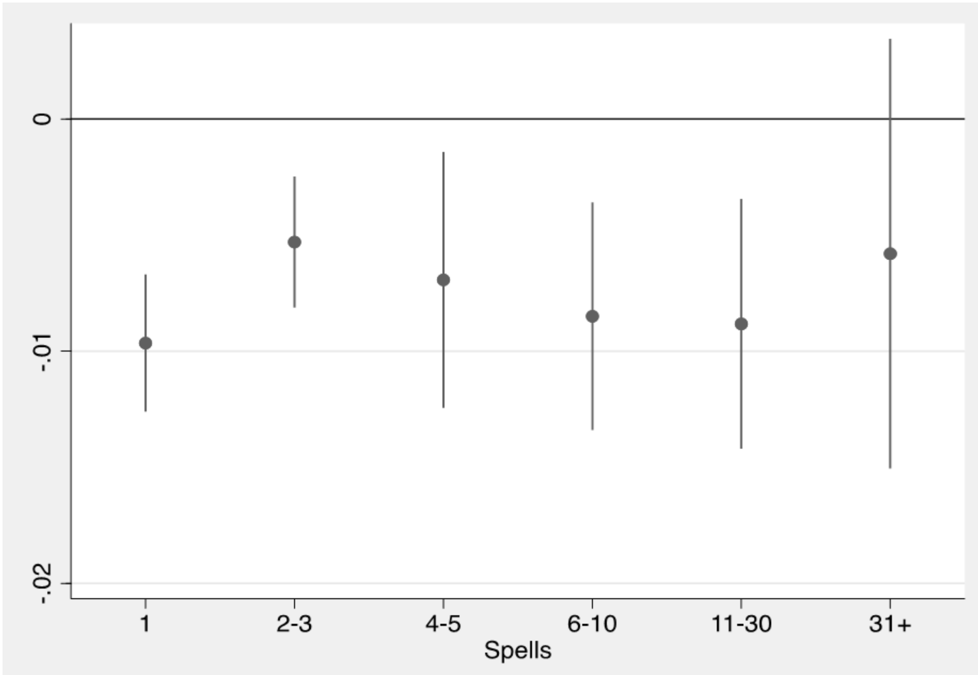
Note: Data are from residualized differences between math absences compared with those in an alternative subject after conditioning on student fixed effects, school-year fixed effects, and school fixed effects. When a student takes multiple class periods for an alternative subject, we take the average number of absences in that subject.

Figure 6: The Effects of Absences on Math Test Scores Using Weekly Measures



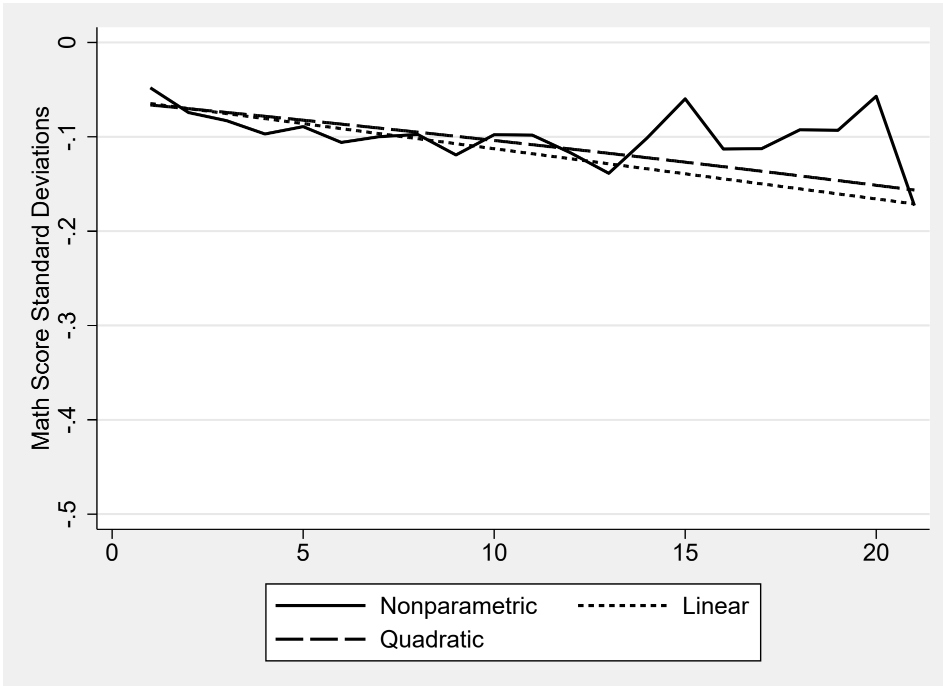
Note: The model used here controls for math absences in fall semesters and ELA absences in both spring and fall semesters. ELA absences are coded as weekly measures the same way as math in spring semesters. The graph only shows coefficients for weekly measures in spring semesters for math classes. All coefficients are scaled by a factor of 10 to ease interpretation. Bars show confidence intervals at the 95% level.

Figure 7: The Effects of Absences on Math Test Scores by Absence Spell Length



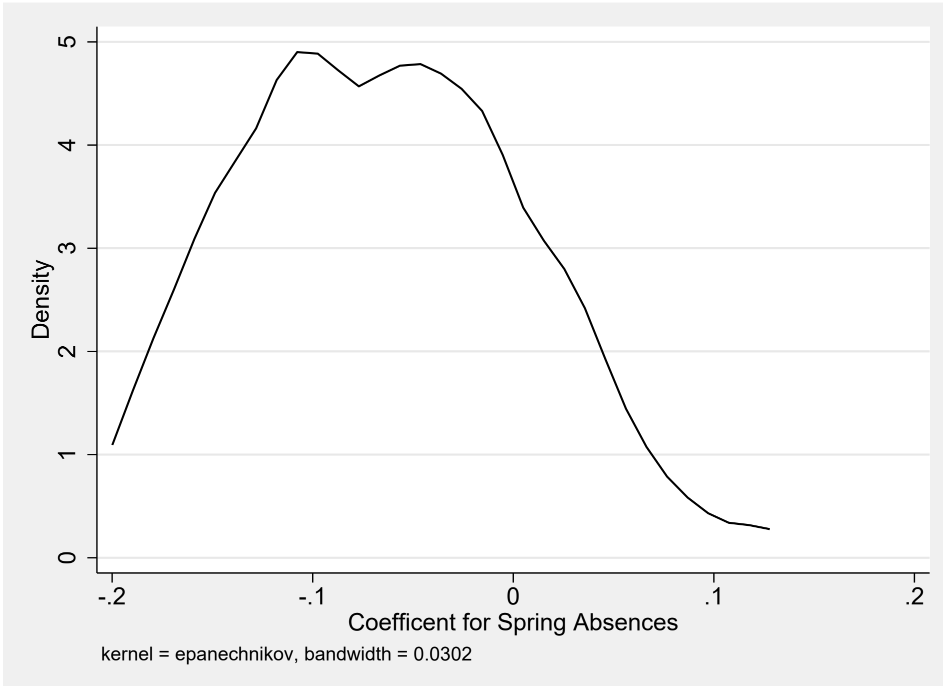
Note: Coefficients show the impact of one absence within a certain absence spell length on math test scores. An absence spell length of 4, for example, indicates a student who misses math for four consecutive days. Bars show confidence intervals at the 95% level.

Figure 8: Linear, Quadratic, and Nonparametric Estimates of the Impact of Absences on Achievement



Note: The graph plots linear, quadratic, and nonparametric models separately. In the nonparametric model, absences beyond 20 are binned as one group.

Figure 9: Kernel Density Plot by School



Note: The figure shows coefficients on spring absences with achievement as the outcome for 45 different middle and high schools in our sample.

Table 1: Sample Means

	(1)	(2)
	Mean	SD
A. Student Characteristics		
Female	0.484	
White	0.083	
Black	0.111	
Hispanic	0.210	
Asian	0.506	
Special Education	0.104	
Gifted	0.282	
Limited English Proficiency	0.206	
Disabled	0.035	
Math Score	0.048	(0.988)
English Score	0.031	(0.988)
GPA	2.765	(1.025)
Student-by-Year Observations	247306	
B. Classroom Characteristics		
Percentage White	0.088	(0.098)
Percentage Black	0.109	(0.143)
Percentage Hispanic	0.199	(0.207)
Percentage Asian	0.512	(0.252)
Percentage English Learner	0.200	(0.038)
Percentage Disabled	0.040	(0.055)
Average CST Math Score	0.067	(0.699)
Average CST ELA Score	0.075	(0.725)
Student-Year-Course Observations	445,646	
Number of Unique Students	73,257	
Number of Unique Courses	24,822	
Number of Schools	45	

Note: Data include 7th- to 11th-graders from the school years 2002-2003 to 2012-2013. Panel A summarizes characteristics at the student level, and Panel B summarizes characteristics at the classroom-year level.

Table 2: Absences by Subject Type and Semester

	(1)	(2)	(3)	(4)
	Math		ELA	
	Mean	SD [Within-Student SD]	Mean	SD [Within-Student SD]
A. Fall Semester				
Total Absences	4.1	(6.9) [3.9]	4.1	(6.9) [4.0]
B. Spring Semester				
Total Absences	6.1	(9.9) [4.1]	6.2	(10.0) [4.1]
Absences Before Testing Window	3.3	(5.7) [3.3]	3.3	(5.7) [3.2]
Absences During Testing Window	1.5	(2.9) [1.8]	1.6	(3.0) [1.8]
Absences After Testing Window	1.3	(2.3) [1.4]	1.3	(2.3) [1.4]
Observations	202,902		200,574	
Unique Students	70,624		27,645	

Note: Observations are at the student-semester-subject level and include the full analytic sample with nonmissing values. Data include 7th- to 11th-graders from the school years 2002-2003 to 2012-2013 at the student-classroom level. The testing window is based on the California Education Code and does not necessarily reflect the actual test dates, which might include make-up periods at the discretion of each school.

Table 3: Average Math Achievement by Chronic Absenteeism Status

	(1)	(2)
	Non-Chronically Absent Mean (SD)	Chronically Absent Mean (SD)
All Students	0.108	-0.728
	(0.962)	(0.926)
White	0.334	-0.273
	(0.96)	(1.086)
Black	-0.67	-0.999
	(0.73)	(0.825)
Asian	0.432	-0.398
	(0.89)	(0.955)
Hispanic	-0.495	-0.805
	(0.734)	(0.809)
Other Race	0.067	-0.696
	(0.946)	(0.949)
SES Bottom Quartile	-0.18	-0.897
	(0.914)	(0.863)

Note: Chronic absenteeism is defined as having 18 or more missed school days in a year. The table shows student-year observations with nonmissing values for the full analytic sample. Socioeconomic status is determined by the median income status of the census tract in which each student resides. All sample means between chronically absent and non-chronically absent student groups yield statistically significant differences ($p < .001$) via two-sample t-tests.

Table 4: Main Results: The Impact of Absences on Math Scores

	(1)	(2)	(3)	(4)	(5)	(6)
A. Middle School: Math Score						
Total Math Absences, Fall	-0.016*	-0.020**	-0.018*	-0.025*	-0.007	0.001
	(0.007)	(0.007)	(0.008)	(0.010)	(0.010)	(0.020)
Total Math Absences, Spring	-0.063**	-0.068**	-0.068**	-0.087**	-0.077**	-0.070**
	(0.006)	(0.006)	(0.008)	(0.008)	(0.009)	(0.010)
Total ELA Absences, Fall						-0.021
						(0.021)
Total ELA Absences, Spring						-0.001
						(0.014)
p-value (Fall = Spring)	0.000	0.000	0.000	0.000	0.000	0.015
R^2	0.748	0.767	0.791	0.904	0.332	0.793
Observations	43,777	43,777	43,777	81,702	16,138	40,953
B. High School: Math Score						
Total Absences, Fall	-0.008	-0.009	-0.006	-0.034*	-0.031**	-0.006
	(0.006)	(0.009)	(0.009)	(0.014)	(0.011)	(0.010)
Total Absences, Spring	-0.058**	-0.088**	-0.089**	-0.090**	-0.085**	-0.068**
	(0.005)	(0.007)	(0.007)	(0.009)	(0.009)	(0.006)
Total ELA Absences, Fall						0.007
						(0.007)
Total ELA Absences, Spring						-0.030*
						(0.011)
p-value (Fall = Spring)	0.000	0.000	0.000	0.005	0.000	0.000
R^2	0.728	0.706	0.730	0.881	0.357	0.738
Observations	64,360	64,360	64,360	82,596	41,276	57,366
School FE	Y					
Class FE		Y	Y	Y	Y	Y
Neighborhood-Year FE			Y			Y
Student FE				Y		
Anderson-Hsiao Estimator					Y	
ELA Absences						Y

Notes: Each column of each panel uses a separate model. All coefficients are multiplied by 10 to ease interpretation. Total absences in the spring semester only includes absences before and during the testing window defined by the California Education Code. Column 1 controls for student-, classroom- and school-level covariates, as well as school year and grade fixed effects. Student-level controls include both linear and quadratic lagged math and ELA test scores, lagged total absence rate, lagged total suspension days, race, gender, English language learner status, disability status, and special education status. Classroom- and school-level controls use the same set of control variables as the student level. All models cluster standard errors at the school level. Columns 2, 3, 5, and 6 control for only student-level covariates. ELA absences include absences in both the fall and spring semesters. After dropping singletons because of different fixed effects, the actual sample sizes drop slightly for Models 2 and 3. For Column 2, the observations are 43,769 for middle schoool and 64,322 for high school. For Column 3, the observations are 64,322 for middle school and 64,001 for high school.
+ .10 * .05 ** .01.

Table 5: Analysis of Main Results Using Stacked Data

	(1)	(2)	(3)	(4)
Total Absences, Fall	-0.019**	0.003	0.022+	0.029*
	(0.005)	(0.009)	(0.013)	(0.011)
Total Absences, Fall X Math		-0.044*	-0.043*	-0.057**
		(0.019)	(0.020)	(0.017)
Total Absences, Spring	-0.083**	-0.065**	-0.025**	-0.022**
	(0.005)	(0.007)	(0.008)	(0.006)
Total Absences, Spring X Math		-0.030*	-0.032*	-0.031**
		(0.012)	(0.013)	(0.011)
p-value (Fall = Spring)	0.000	0.000	0.000	0.000
Student FE	X	X		
Year FE	X	X		
StudentYear FE			X	X
Teacher FE				X
School Controls	X	X		
Observations	293,433	293,433	288,100	288,100

Note: Statistics are from stacked data that append math and ELA datasets together. The sample includes student-by-year-level observations from the school years 2002-2003 to 2012-2013 for grades 7 to 11.
+ .10 * .05 ** .01.

Table 6: Heterogeneity by Student- and School-Level Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Male	Female	White	Black	Hispanic	Asian	Sch Poverty Bottom Q	Sch Poverty Middle Q	Sch Poverty Top Q
Total Absences, Fall	-0.011	-0.005	-0.067+	0.017	-0.003	-0.016	-0.003	-0.017	-0.006
	(0.013)	(0.011)	(0.039)	(0.022)	(0.013)	(0.012)	(0.010)	(0.011)	(0.013)
Total Absences, Spring	-0.059**	-0.063**	-0.070	-0.087**	-0.044**	-0.058**	-0.065**	-0.062**	-0.069**
	(0.009)	(0.005)	(0.049)	(0.028)	(0.010)	(0.011)	(0.009)	(0.006)	(0.021)
Observations	48,791	48,682	5,349	5,902	15,362	53,198	58,564	27,137	11,541

Note: All coefficients are multiplied by 10 to ease interpretation. Total absences in the spring semester only includes absences before and during the testing window defined by the California Education Code. All models control for classroom and neighborhood-by-year fixed effects, student characteristics, and total absences in both the fall and spring semesters in the alternative subject. Prior achievement is measured using lagged test scores in the corresponding subject for math and ELA. Prior attendance is measured using absences of all subjects in the prior year.

+ .10 * .05 ** .01

Table 7: Main Results Using Course-level Grades as Outcomes

	Math		ELA	
	(1)	(2)	(3)	(4)
Total Math Absences, Fall	-0.032**	-0.029*	0.005	0.003
	(0.011)	(0.011)	(0.010)	(0.011)
Total Math Absences, Spring	-0.194**		-0.013	
	(0.015)		(0.011)	
Total ELA Absences, Fall	-0.001	-0.001	-0.035**	-0.028**
	(0.012)	(0.012)	(0.011)	(0.010)
Total ELA Absences, Spring	0.013		-0.201**	
	(0.010)		(0.015)	
Math Spring Absences				
Weeks 1-14		-0.132**		0.011
		(0.017)		(0.015)
Weeks 15-16		-0.117*		-0.012
		(0.046)		(0.036)
Weeks 17-18		-0.220**		-0.060
		(0.043)		(0.044)
Weeks 19-20		-0.261**		-0.064
		(0.049)		(0.039)
Weeks 21-22		-0.319**		0.016
		(0.060)		(0.036)
ELA Spring Absences				
Weeks 1-14		-0.001		-0.153**
		(0.015)		(0.014)
Weeks 15-16		-0.033		-0.119**
		(0.039)		(0.037)
Weeks 17-18		0.055		-0.183**
		(0.041)		(0.056)
Weeks 19-20		0.060		-0.232**
		(0.036)		(0.035)
Weeks 21-22		0.034		-0.359**
		(0.030)		(0.034)
Observations	98,706	98,706	99,098	99,098
R^2	0.502	0.503	0.557	0.558

Note: Outcomes are standardized end-of-course grades at the year-school-grade-course level. All coefficients are multiplied by 10 to ease interpretation. Total absences in the spring semester only includes absences before and during the testing window defined by the California Education Code. Biweekly measures are constructed using school days in the spring semester. All models control for classroom and neighborhood-by-year fixed effects, student characteristics, total absences in the fall semester in both math and ELA, and biweekly absences in the alternative subject.
+ .10 * .05 ** .01.

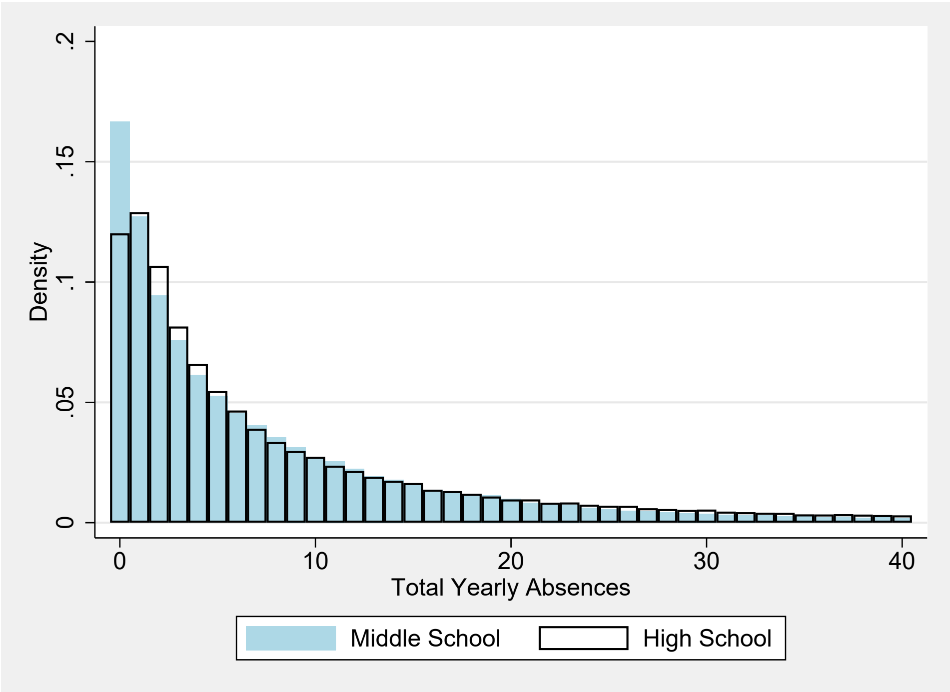
Table 8: Long-Term Outcomes for 9th-Grade Students

	(1)	(2)	(3)	(4)	(5)
	High School Graduation	Immediate College Enrollment	Ever Enrolled in College	Ever Enrolled in 4-Year	Ever Enrolled in 2-Year
Math Classrooms					
Total Math Absences	-0.059**	-0.040**	-0.048**	-0.020**	-0.044**
	(0.002)	(0.005)	(0.004)	(0.004)	(0.004)
Oster Bound	[-0.042]	[-0.013]	[-0.025]	[0.012]	[-0.040]
Delta for $\beta = 0$	3.375	1.466	2.122	0.624	12.788
Outcome Averages	0.741	0.596	0.658	0.487	0.436
Observations	20,256	20,137	20,137	20,137	20,137
ELA Classrooms					
Total ELA Absences	-0.066**	-0.050**	-0.058**	-0.026**	-0.054**
	(0.003)	(0.003)	(0.004)	(0.005)	(0.004)
Oster Bound	[-0.050]	[-0.025]	[-0.038]	[0.006]	[-0.053]
Delta for $\beta = 0$	4.215	1.979	2.899	0.815	49.776
Outcome Averages	0.742	0.597	0.662	0.484	0.442
Observations	21,092	20,952	20,952	20,952	20,952

Note: Each coefficient comes from a separate regression. All models control for student-level covariates and classroom and neighborhood-year fixed effects. All absence measures include those in both the spring and fall semesters in the same school year. Coefficients, standard errors, and Oster bounds are scaled by a factor of 10 to ease interpretation. Oster bounds are computed based on the assumption that the maximum R-squared is 1.3 times as big as the R-squared from the full model. Standard errors are clustered at the school level.
+ .10 * .05 ** .01

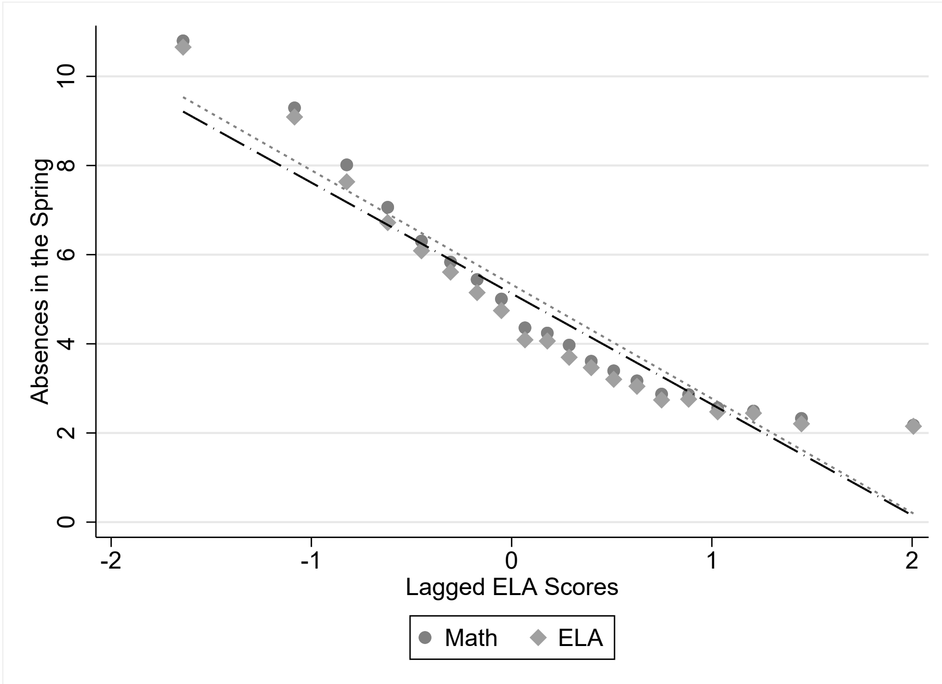
Appendix

Figure A1: Distribution of Absences in ELA Classes



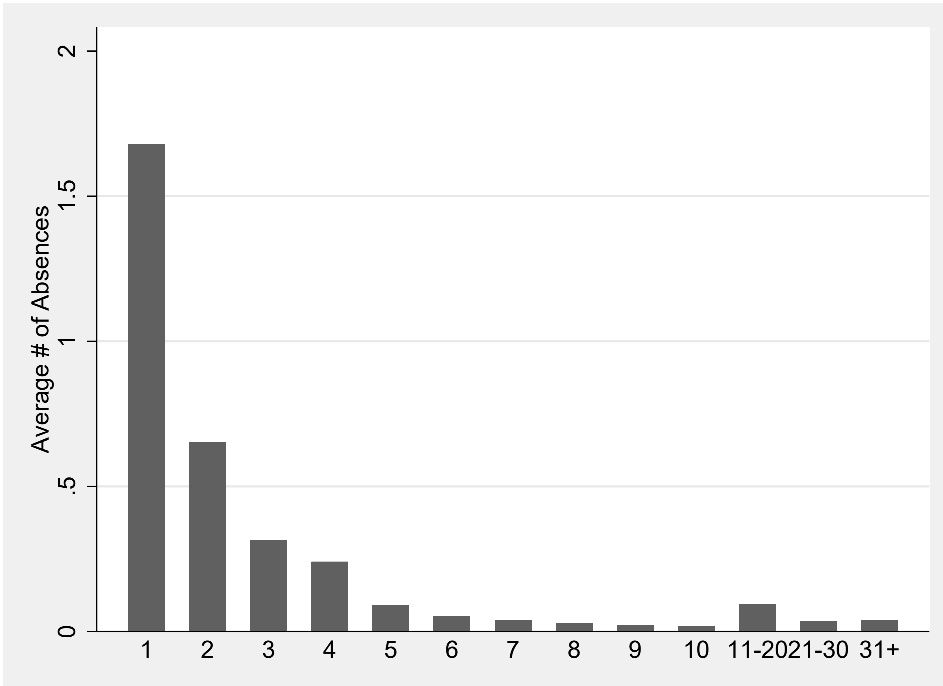
Note: Observations are counts of student absences in ELA classes that occur in the entire school year from 2002-2003 to 2012-2013. Data are trimmed at 40 absences to show the bulk of variation.

Figure A2: Binned Scatter Plot of Absences vs. Lagged ELA Scores



Note: Observations are counts of student absences in ELA classes in the spring semester only from the school years 2002-2003 to 2012-2013.

Figure A3: Distribution of Absence Spell Lengths



Note: Each observation is the average number of absences per student corresponding to each absence spell. For example, an average of 0.6 absences occur within an absence spell that lasts two consecutive days.

Table A1: Average ELA Achievement by Chronic Absenteeism Status

	Non-Chronically Absent Mean (SD)	Chronically Absent Mean (SD)
All Students	0.102	-0.625
	(0.946)	(1.07)
White	0.695	0.041
	(0.954)	(1.197)
Black	-0.553	-0.93
	(0.844)	(0.972)
Asian	0.286	-0.356
	(0.863)	(1.063)
Hispanic	-0.37	-0.73
	(0.849)	(1.001)
Other Race	0.147	-0.555
	(0.943)	(1)
SES Bottom Quartile	-0.209	-0.841
	(0.892)	(0.987)

Note: Chronic absenteeism is defined as having 18 or more missed school days in a year. The table shows student-year observations with nonmissing values for the full analytic sample. Socioeconomic status is determined by the median income status of the census tract in which each student resides. All sample means between chronically absent and non-chronically absent student groups yield statistically significant differences ($p < .001$) via two-sample t-tests.

Table A2: Impact of Absences on ELA Scores

	(1)	(2)	(3)	(4)	(5)	(6)
A. Middle School: ELA Score						
Total ELA Absences, Fall	0.009	0.007	0.008	-0.012	-0.012	0.010
	(0.007)	(0.009)	(0.008)	(0.008)	(0.009)	(0.013)
Total ELA Absences, Spring	-0.064**	-0.062**	-0.062**	-0.074**	-0.057**	-0.020
	(0.007)	(0.007)	(0.007)	(0.014)	(0.015)	(0.021)
Total Math Absences, Fall						0.002
						(0.014)
Total Math Absences, Spring						-0.049*
						(0.022)
p-value (Fall = Spring)	0.000	0.000	0.000	0.003	0.019	0.255
R^2	0.754	0.777	0.802	0.909	0.302	0.792
Observations	44,468	44,468	44,468	80,445	16,247	40,851
B. High School: ELA Score						
Total ELA Absences, Fall	-0.007	-0.011	-0.009	-0.021*	-0.012+	0.007
	(0.008)	(0.009)	(0.009)	(0.009)	(0.006)	(0.009)
Total ELA Absences, Spring	-0.079**	-0.086**	-0.085**	-0.089**	-0.075**	-0.072**
	(0.009)	(0.008)	(0.007)	(0.013)	(0.007)	(0.008)
Total Math Absences, Fall						-0.015+
						(0.008)
Total Math Absences, Spring						-0.014+
						(0.007)
p-value (Fall = Spring)	0.000	0.000	0.000	0.001	0.000	0.000
R^2	0.704	0.732	0.753	0.888	0.168	0.760
Observations	69,641	69,641	69,641	87,206	48,870	57,399
School FE	Y					
Class FE		Y	Y	Y	Y	Y
Neighborhood-Year FE			Y			Y
Student FE				Y		
Anderson-Hsiao Estimator					Y	
The Other Subject's Absences						Y

Notes: Each column of each panel comes from a separate model. All coefficients are multiplied by 10 to ease interpretation. Total absences in the spring semester only includes absences before and during the testing window defined by the California Education Code. Column 1 controls for student-, classroom-, and school-level covariates, as well as school year and grade fixed effects. Student-level controls include both linear and quadratic lagged math and ELA test scores, lagged total absence rate, lagged total suspension days, race, gender, English language learner status, disability status, and special education status. Classroom- and school-level controls use the same set of control variables as the student level. All models cluster standard errors at the school level. Columns 2, 3, 5, and 6 control for only student-level covariates. ELA absences include absences in both the fall and spring semesters. After dropping singletons because of different fixed effects, the actual sample sizes drop slightly for Models 2 and 3. For Column 2, the number of observations is 69,640 for high school. For Column 3, the number of observations is 43,952 for middle school and 69,359 for high school.
+ .10 * .05 ** .01.

Table A3: Math Preferences Table

	(1)	(2)	(3)	(4)	(5)	(6)
	ELA	Science	Social Studies	Foreign Languages	PE	Other
Grade 8	0.413	1.398**	1.162**	-1.285	1.263**	0.162
	(0.377)	(0.363)	(0.382)	(1.365)	(0.471)	(0.455)
Grade 9	1.413	2.460*	0.967	-3.104	1.613	-0.222
	(1.069)	(1.255)	(1.200)	(3.285)	(1.374)	(1.261)
Grade 10	1.926	3.385*	2.043	-4.733	2.435	-0.273
	(1.358)	(1.497)	(1.467)	(4.497)	(1.729)	(1.626)
Grade 11	2.596	4.378*	3.143+	-6.493	3.164	-0.401
	(1.680)	(1.779)	(1.777)	(5.772)	(2.141)	(2.020)
Observations	148,175	162,840	136,535	68,685	138,370	129,712

Notes: All models regress the difference between math absences and an alternative subject's absences on grade and control for school year and school fixed effects.
+ .10 * .05 ** .01.

Table A4: Robustness Check Using Off-Subject Absences

	(1)	(2)	(3)	(4)	(5)	(6)
			Control for Absences in...			
	Science	Social Studies	Foreign Langs	PE	Other	All
Total Math Absences, Fall	-0.008	-0.013+	-0.010	-0.011	-0.011	-0.010
	(0.009)	(0.007)	(0.012)	(0.011)	(0.009)	(0.011)
Total Math Absences, Spring	-0.060**	-0.059**	-0.072**	-0.064**	-0.065**	-0.055**
	(0.005)	(0.009)	(0.010)	(0.007)	(0.006)	(0.007)
Total Off-Subject Absences, Fall	-0.003	-0.003	-0.002	0.003	-0.004	-0.000
	(0.006)	(0.006)	(0.012)	(0.006)	(0.007)	(0.002)
Total Off-Subject Absences, Spring	-0.025**	-0.024**	-0.020**	-0.018**	-0.022**	-0.008**
	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)	(0.002)
R^2	0.745	0.755	0.754	0.763	0.754	0.743
Observations	105,887	88,567	44,731	87,892	78,279	109,824

Note: Each column is from a separate regression. The table replicates the results from Column 6 in the main table by using one off-subject at a time. The last column uses total absences from all subjects other than math.
+ .10 * .05 ** .01.

Table A5: Absences in Biweekly Measures and ELA/Math Test Scores

	(1)	(2)
	Math	ELA
Math Absences		
Weeks 1-14	-0.047**	0.038
	(0.010)	(0.045)
Weeks 15-16	-0.047*	-0.055
	(0.020)	(0.055)
Weeks 17-18	-0.097**	-0.030
	(0.029)	(0.057)
Weeks 19-20	-0.095**	0.003
	(0.020)	(0.030)
Weeks 21-22	-0.024	-0.020
	(0.024)	(0.028)
ELA Absences		
Weeks 1-14	-0.001	-0.020+
	(0.039)	(0.011)
Weeks 15-16	-0.050	-0.061+
	(0.043)	(0.035)
Weeks 17-18	-0.070+	-0.142**
	(0.041)	(0.033)
Weeks 19-20	0.022	-0.064*
	(0.027)	(0.030)
Weeks 21-22	0.008	-0.041+
	(0.021)	(0.023)
Observations	99,973	98,066
R^2	0.751	0.766

Note: Each column reports coefficients from a separate regression. Biweekly measures are constructed using school days in the spring semester. All coefficients are multiplied by 10 to ease interpretation. State tests typically end in weeks 21-22. All models control for classroom and neighborhood-by-year fixed effects, student characteristics, total absences in the fall semester in both math and ELA, and biweekly absences in the other subject.
+ .10 * .05 ** .01.

Table A6: Long-Term Outcomes for 10th-Grade Students

	(1)	(2)	(3)	(4)	(5)
	High School Graduation	Immediate College Enrollment	Ever Enrolled in College	Ever Enrolled in 4-Year	Ever Enrolled in 2-Year
Math Classrooms					
Total Math Absences	-0.049**	-0.039**	-0.045**	-0.021**	-0.039**
	(0.005)	(0.003)	(0.004)	(0.005)	(0.002)
Oster Bound	[-0.026]	[0.000]	[-0.015]	[0.025]	[-0.037]
Delta for $\beta = 0$	2.085	1.000	1.511	0.451	20.721
Outcome Averages	0.504	0.493	0.481	0.580	0.311
Observations	26,290	26,105	26,105	26,105	26,105
ELA Classrooms					
Total ELA Absences	-0.057**	-0.044**	-0.052**	-0.020**	-0.047**
	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)
Oster Bound	[-0.030]	[-0.002]	[-0.019]	[0.032]	[-0.046]
Delta for $\beta = 0$	2.108	1.054	1.582	0.386	183.708
Outcome Averages	0.471	0.458	0.442	0.553	0.281
Observations	25,855	25,710	25,710	25,710	25,710

Note: Each coefficient comes from a separate regression. All models control for student-level covariates and classroom and neighborhood-year fixed effects. All absence measures include those in both the spring and fall semesters in the same school year. Coefficients, standard errors, and Oster bounds are scaled by a factor of 10 to ease interpretation. Oster bounds are computed based on the assumption that the maximum R-squared is 1.3 times as big as the R-squared from the full model. Standard errors are clustered at the school level.
+ .10 * .05 ** .01