Can Home Visits Re-Engage Chronically Absent Students? Evidence from the LEAP Program

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Abstract

Beginning in the summer of 2021, the state of Connecticut launched the Learner Engagement and Attendance Program (LEAP) where home visits by school personnel were conducted with students identified as chronically absent from Connecticut's 15 most disadvantaged school districts. Home visits were designed to connect students and their families directly with teachers to foster strong connections. We use student-level data from 2021–22 through 2023-24 to estimate event studies of the effect of LEAP on student attendance, chronic absenteeism, disciplinary incidents, and student achievement. Utilizing models that address selection into treatment timing over student observable and unobservable attributes, we find that students who received the LEAP intervention experienced approximately a 4 to 5 percentage point increase in attendance rates, a 13 to 15 percentage point decline in chronic absenteeism, and a 3 to 5 percentage point decline in the number of disciplinary incidents. Notably, initial event studies indicated increasing treatment effects over time, but after correcting for selection on unobservables treatment effects appeared constant over time. These effects were broad based, with similar impacts by gender, race and ethnicity, and free or reduced-price lunch status. These attendance and discipline results provide suggest that the LEAP intervention increased student noncognitive skills.

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I. Introduction

Since the onset of the COVID-19 pandemic in early 2020, chronic absenteeism, defined as missing 10 percent or more of school days, has surged and remains at historically high levels. Nationally, chronic absenteeism rates among K-12 students went from 15 percent in 2019 to 28 percent in 2022 and while chronic absenteeism rates have begun to fall in more recent years, even by the end of 2024 chronic absenteeism remained at over 23 percent (Malkus 2025). Moreover, while the increase in chronic absenteeism was broad based, it was particularly large among students of color and those from disadvantaged backgrounds (Malkus 2022; Dewey et al. 2025; Dee 2024; Brunner et al. 2026). Perhaps not surprisingly, the spike in chronic absenteeism has been accompanied by significant learning loss that has persisted over time again with learning losses being particularly acute among students of color and those from disadvantaged backgrounds (Betthäuser et al. 2023; Brunner, et al. 2026; Dewey et al. 2025; Goldhaber et al. 2023; Jack & Oster 2023; Strunk et al. 2025).

Similar to the rest of the nation, K-12 students in Connecticut also experienced a dramatic increase in chronic absenteeism following the onset of the Covid-19 pandemic that has persisted into the present. Prior to the pandemic in 2019, approximately 12 percent of students in Connecticut were chronically absent. That number doubled to 24 percent in 2021-22 and by the end of the 2024-25 school year, chronic absenteeism rates were still at 17.2 percent, a 43 percent increase from the pre-pandemic level. As documented by Brunner et al. (2026), the sustained increase in chronic absenteeism in Connecticut has been concentrated in schools that have a disproportionate share of students of color and those from disadvantaged backgrounds, and these same schools have experienced sustained learning losses and increases in severe discipline incidents. As a result, racial and ethnic and socio-economic disparities in student achievement have grown substantially since the onset of the pandemic with minimal recovery through 2024-25.

The purpose of this paper is to provide the first causal evidence on the efficacy of an innovative program implemented in the state of Connecticut designed to address student chronic absenteeism and disengagement from school. In April of 2021, Connecticut launched the Learner Engagement and Attendance Program (LEAP). Beginning in the summer of 2021, home visits by teachers and other school personnel were conducted with students identified as chronically absent from a targeted sample of 15 of Connecticut's most economically disadvantaged school districts. The home visits were designed to connect students and their families directly with teachers and other school personnel with the goal of fostering trust and strong connections between families and their school community.

We use student-level administrative data from 2021–22 through 2023-24 to examine the impact of the LEAP program on student attendance, chronic absenteeism, disciplinary incidents, and student achievement. In addition to including standard demographic and socio-economic characteristics of

students, the administrative data include information on the number of days a student attended school in each month, the date of the first LEAP visit, the location of the visit, and the number of follow-up visits if any. To identify the causal impact of the LEAP intervention on student outcomes, we estimate event study and difference-in-differences models that exploit the staggered timing of treatment across students and schools. Given the discretion school personnel have in selecting students for the LEAP program, along with the evolution of the program over time, we exploit the staggered timing of treatment and select a control group that consists of students treated by the LEAP program in the same academic year but in later months.

Given only 10 months in a school year, the control group is eroded significantly in positive event time as control students who were not yet treated experience treatment themselves several months after treated students' initial treatment. Under the standard staggered roll-out assumptions of no selection into treatment timing, the erosion of the control group is irrelevant. However, the potential exists for low levels of selection in timing that might create at most minimal bias in initial treatment effects, but may lead to larger bias several months after treatment. Importantly, this bias cannot be identified by examining pre-trends when utilizing a relatively standard design for not-yet-treated control units, where the control group at time zero and during the entire pre-period is composed of a constant sample of students not treated at the time of initial treatment. If the selection bias is very small at the time of initial treatment, tests for parallel trends are likely to be severely under powered even if substantial bias in treatment effect estimates exist later in event time. We utilize two approaches to address this concern. First, we utilize a non-parametric approach for selection on observables adapting Abadie (2005) and using propensity score weights to assure sample balance on observables within event time, i.e. overlap weights to concentrate weight on the common support instead of the extremes (Li et al. 2018). Second, we impose parametric assumptions to address selection on unobservables so that we can hold the control sample fixed over all event time, while partialling out the effect of students in the control sample being treated during positive event times. We know of only one other paper (Wing et al. 2024) that considers models that address the concern that treatment and/or control samples may evolve over event time, but that paper focuses more on addressing concerns that changes in treatment effects over event time may be confounded by changes in the composition of treated individuals.

While estimates are very robust to controlling for selection on observables,[1] we find evidence of bias that grows over time based on the parametric models that address selection on unobservables.

[1] Notably, the balance between treatment and control samples on observables does deteriorate as we move forward in positive event time, and the use of propensity score weights restores this balance. However, it appears that the available student observables are unrelated, or at most very weakly related, to changes in attendance patterns during the school year.

Specifically, traditional staggered roll-out event studies yield a 5 percentage point increase in attendance rates and a 14 percentage point decline in chronic absenteeism rates in the first month after treatment, but those effects exhibit a steady increase in effect size, rising to approximately a 7 percentage point increase and 18 percentage point decline, respectively, by the 5th month after treatment. After correcting for selection out of the control sample, the treatment effect estimates are relatively stable over event time with the treatment effect point estimates for attendance ranging between 5 and 6 percentage points and for chronic absenteeism ranging between 12 and 16 percentage points relative to pre-treatment attendance and chronic absenteeism rates of 79 and 58 percent, respectively. No apparent trend in the treatment effect estimates over event time persist after controlling for selection on unobservables. While these finding do raise the prospect of some level of bias in the initial treatment effect estimates at one month after treatment, the clear evidence of no pre-trends while holding the control sample fixed and the steady 2 and 4 percentage point increase in treatment effect estimates over a four month period would tend to suggest that the bias is likely no more than one-half percentage point on attendance relative to a 5 percentage point treatment effect and one percentage point relative to a 14 point treatment effect for chronic absenteeism. We also examine heterogeneity in treatment effect estimates over student attributes, but do not find any evidence of substantial heterogeneity in treatment effects by gender, race and ethnicity, or free or reduced-price lunch status.

We next examine the impact of the LEAP intervention on student disciplinary incidents. Notably, most administrative data typically focuses on administrator disciplinary decisions, like in or out of school suspensions, while our study documents school reports on incidents of severe discipline related student behavior, regardless of whether the behavior led to a specific disciplinary action. Due to the low incidence of such incidents, we restrict this analysis to a sample of individuals who had one or more discipline incidents in the academic year prior to treatment. Unlike attendance and chronic absenteeism, we do not observe a statistically significant trend in the treatment effect estimates either before or after treatment utilizing the traditional staggered roll-out estimator. However, the empirical trend while statistically insignificant has notable effects on treatment with difference-in-differences treatment effect estimates for severe discipline incidents falling from 5.7 to 3.6 (at event time one) percentage points when trends are included in the model. Our preferred model that controls for selection into treatment on unobservables eliminates any meaningful empirical trends and yields event study estimates ranging between 3 and 5 percentage points and a difference-in-differences estimate of 3.6 points, an estimate very close to the

staggered roll-out event study estimates at event time one conditional on trends and quite large relative to the sample mean 14 percent likelihood of a severe discipline incident in a month prior to treatment.[2]

Using a slightly different event study design, we also examine the effects of early LEAP treatment on standardized test scores that are conducted between April and early May and find little evidence that the LEAP program impacted student achievement.[3] Again, our results are relatively broad based with minimal evidence of heterogeneous effects over student attributes. Taken together, our attendance and discipline results provide suggestive evidence that the LEAP intervention may increase student noncognitive skills, skills that have been shown to be as valuable as cognitive skills for improving long-term outcomes (Heckman & Rubinstein 2001; Lindqvist & Vestman 2011; Kautz et al. 2014; Edin et al. 2022).

Our paper makes several contributions to the literature on student outcomes in K-12 education. First, we provide some of the first causal evidence on the impact of teacher and school personnel home visits on student outcomes. Earlier work on the impact of K-12 home visits (and family engagement programs more generally) is largely descriptive in nature. For example, Epstein & Sheldon (2002) conduct a descriptive analysis of the impact of school, family, and community partnerships designed to reduce chronic absenteeism. Based on data from 12 elementary schools, the authors find that several programs, including rewarding students for improved attendance and making home visits to chronically absent students, were effective at reducing chronic absenteeism. Cook et al. (2017) evaluate a pilot program implemented in 20 classrooms designed to improve attendance among elementary school children attending high-poverty schools by fostering communication between teachers and parents and find that the program significantly reduced absenteeism. Similarly, Sheldon (2007) evaluates a school-wide program in Ohio designed to promote school, family, and community partnerships and found that the program improved attendance, with outreach to parents being the primary mechanism for improvement.[4]

Second, we contribute to the literature that more broadly examines the causal impact of school interventions on attendance and academic achievement. Rogers & Feller (2018) and Robinson et al.

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[2] We also follow Wing et al. (2024) and both remove month of treatment subsamples that are treated late in our sample period (April-June) and then restrict event time so that the treatment population is constant over positive event time. We continue to observe constant treatment effects over event time in these models as well.

[3] We note however that we have limited power to detect the impact of LEAP on test scores given our identification strategy relies on comparing students treated early in the school year (treatment group) to students treated late in the school year (control group) and thus there may not be sufficient time between LEAP treatment and the administering of standardized exams to significantly influence academic performance.

[4] Other examples of earlier, primarily descriptive work, includes Wright et al. (2018), Ilhan, Ozfidan & Yilmaz (2019), Soule & Curtis (2021). See Pérez-Marco et al. (2025) for a review of seventy-eight programs aimed at improving school attendance and reducing chronic absenteeism. Also see Boaler & Bond (2023) for a general review of school-based programs, including family engagement programs, designed to reduce chronic absenteeism.

(2018), report results from a randomized control trial that examined interventions designed to reduce absenteeism and find that a parent-focused intervention that targeted parental beliefs about how many days their child was absent and the value of regular school attendance reduced chronic absenteeism by between 10 and 15 percent. Smythe-Leistico & Page (2018) study the impact of a "two-way, text-based parent-school communication system" that provides parents with information about their child's attendance and find that the program significantly reduced chronic absenteeism rates. Similarly, Berlinski et al. (2025) conduct a randomized control trial to evaluate a program implemented in Chile that sends attendance, grade, and classroom behavior information to parents through text messages and also find that it positively impacts attendance rates. Guryan et al. (2021) conduct a large-scale randomized control trial of a structured student mentoring program called Check & Connect implemented in Chicago Public Schools. Similar to the LEAP intervention, the Check & Connect program involved assigning a mentor to work with students and their families, personalized interventions to reengage students with their schools, and parental engagement. The authors find that the program reduced absenteeism in grades 5 to 7 by 23 percent, but had no effect on attendance in earlier grades. Similar to the results reported in this paper, the authors also found that the program had little impact of learning outcomes such as test scores and GPA.[5] Most recently, Goldman & Gracie (2025) evaluate the nationwide Communities In Schools program that connects struggling students in high-poverty schools with a "site coordinator," that oversees in-school support programs such as mentoring and tutoring and out of school services such as food and housing assistance. Leveraging the staggered roll out of the program, they find that the program improved student test scores, high-school graduation rates, and adult earnings.[6]

Third, our paper is also related to the literature on the impact of attendance on student outcomes. A relatively large body of literature documents that truancy and chronic absenteeism are negatively correlated with a host of student outcomes including student achievement (e.g. Aucejo & Romano 2016; Cattan et al. 2023; Goodman 2014; Liu, Lee & Gershenson 2021; Gottfried 2010, 2011, 2014; London, Sanchez & Castrechini 2016), educational attainment (Cattan et al. 2023; Dräger, Klein & Sosu 2024; Liu, Lee & Gershenson 2021), and adult labor market outcomes (Cattan et al. 2023; Dräger, Klein & Sosu 2024). We contribute to this literature by providing some of the only evidence on how a program designed to improve attendance rates also impacts student discipline. We note, however, that our study

[5] In related work, Lee, Loeb & Robinson (2026) examine the impact of Washington DC's High-Impact Tutoring (HIT) Initiative on student absenteeism. Using a student fixed effects identification strategy, the authors find that students were 1.2 percentage points less likely to be absent on days they were scheduled for tutoring.

[6] Lavecchia, Oreopoulos and Brown (2020) evaluate the Pathways to Education program, a program similar to the Communities in Schools program, that offers disadvantaged high school students free public transportation and postsecondary financial aid if students agree to regularly meet with an advisor, access tutoring assistance, and attend character building events.

only provides indirect evidence on the link between attendance and student outcomes because the LEAP intervention could impact outcomes other than attendance through multiple pathways.[7]

Our final contribution is more methodological and contributes to the rapidly evolving literature on difference-in-differences designs with staggered timing of treatment. Specifically, given our reliance on not yet treated students as controls, we present two new methods for addressing selection bias that might arise in positive event time as control observations are treated in the time periods following the period of treatment for the treated observations. We first address selection on observables non-parametrically following Abadie (2005), except using overlap instead of propensity score weights. The overlap weights yield near perfect balance on observables at each event time while avoiding placing large weights on extreme observations (Li et al. 2018). This allows us to verify that event studies are nearly identical when using a balanced sample over observables. Second, we impose parametric assumptions allowing us to avoid selection in the control sample by retaining control observations as they are treated, but conditioning out the effect of treatment. Using this constant control sample approach, we show that standard staggered roll-out robust event study models that rely on not yet treated control observations display a large positive trend in treatment effects for attendance and chronic absenteeism, a trend that does not persist after controlling for selection over event time based on unobservable student attributes.

The paper proceeds as follows. Section II describes the history and background of the LEAP Program. Section III describes the data we assemble to examine the impact of the LEAP program on student attendance, chronic absenteeism, student discipline, and achievement. Section IV outlines our empirical framework for isolating the causal effects of the LEAP intervention on our outcomes of interest. We present our main findings in Section V and conclude in Section VI with a discussion of the implications of our results for policy and practice.

II. Background

Following the onset of the Covid-19 pandemic, K-12 students in Connecticut, like most other states, experienced large learning losses and a dramatic increase in chronic absenteeism that has persisted into the present.[8] In January of 2021, administrative data on monthly attendance maintained by the Connecticut State Department of Education (CSDE) revealed that districts with large shares of students of

[7] For example, the LEAP intervention could impact student outcomes primarily because it increases attendance. On the other hand, LEAP might impact student outcomes primarily through its effect on student attachment and engagement which in turn impacts attendance and student discipline.

[8] The material in this section borrows heavily from "The LEAP Effect Taking A Systemic Approach to Improving Attendance & Engagement Lessons Learned from Connecticut's Learner Engagement & Attendance Program (LEAP)." A 2023 brief produced by the CSDE in partnership with Attendance Works, the Capital Regional Education Council (CREC), and EdAdvance.

color and students from disadvantaged backgrounds, were significantly more likely to have students that were chronically absent than more affluent districts with predominantly non-Hispanic white students. To address student absenteeism and disengagement from school, the CSDE partnered with Attendance Works and Parent Teacher Home Visits to develop a comprehensive program designed to combat chronic absenteeism and foster trusting relationships between students, their families, and their school community.[9] The result was the establishment of the Learner Engagement & Attendance Program (LEAP) which was formally launched in April of 2021.

As noted by the CSDE, "LEAP is a relational home visit model that serves as part of a comprehensive system of support for families whose students are struggling with consistent attendance. Its mission is to build trusting relationships with families to ultimately positively impact students' engagement and attendance." (CSDE 2023). The LEAP program is specifically designed to build and reinforce positive relationships between students, their families, and the school community rather than to enforce attendance policies or punish students for being chronically absent. Participation in the LEAP program is voluntary, both for the teachers and school personnel that conduct the home visits, and for the families of chronically absent students. Importantly, teachers and staff conducting home visits are required to maintain home visit logs to identify common barriers and needs of families. These logs, which contain information on the date of the LEAP visit, who conducted the LEAP visit, and where the visit occurred, are then submitted to the CSDE to create a central databank of all the students that participated in the LEAP program and to facilitate efforts to evaluate the efficacy of the program.

To train home visitors, build capacity for the LEAP program, and create uniform standards for conducting home visits, the CSDE created a professional development home visit hub that provides: 1) basic training for teachers and other school personnel on how to conduct a home visit; 2) district-specific LEAP training on how to integrate the LEAP model into existing family engagement programs; 3) district coordinator meetings that bring together LEAP coordinators from participating school districts to share updates and identify challenges and opportunities when implementing the LEAP program; and 4) several other resources designed to help participating school districts and home visitors implement the LEAP program with fidelity.

Leveraging federal funding from the Elementary and Secondary School Emergency Relief (ESSER) program and the American Rescue Plan Act (ARPA), the LEAP program was first implemented in the summer of 2021. Home visits by teachers and other school personnel were conducted with students

[9] Attendance Works is a national organization dedicated to helping states and school districts combat chronic absenteeism. Similarly, Parent Teacher Home Visits is a national organization dedicated to advancing student success through "relational" home visits designed to build trusting relationships between students, their families and the school community.

identified as chronically absent from a targeted sample of 15 primarily urban, high-poverty school districts in Connecticut. During 2021-22, the initial year of the LEAP program, approximately 8,700 Pre-Kindergarten through 12th grade students participated in the program across the fifteen school districts. In 2022, the Connecticut General Assembly appropriated additional funds to maintain the LEAP program across multiple years and to expand the program to serve additional school districts. As of 2024 there were 25 school districts participating in the LEAP program and the program has served approximately 46,000 students.

III. Data

Our primary source of data comes from individual student attendance and LEAP participation records maintained by the 15 school districts that participated in LEAP during the 2021–22, 2022–23, and 2023–24 school years.[10] These records include information on: 1) the date of the first LEAP visit; 2) the location of the visit; 3) information on who conducted the visit (e.g. teacher, guidance counselor); and 4) the number of follow-up visits (if any). The records also include information on: 1) the number of days each student attended school in a given month; 2) the total number of school days in that month; and 3) monthly student level data on every minor and severe disciplinary incident reported to the state by local school districts. Using this data, we construct each student's monthly attendance rate as the ratio of the number of days of school attended each month divided by the total number of school days in that month. We additionally define a student as chronically absent if their monthly attendance rate was below 90 percent. Finally, we use the student level disciplinary data to calculate whether a student had any reported severe discipline incidents in each month. The state provides criteria for determining whether a discipline incident should be classified as severe and requires reporting of any discipline incidents that meet these criteria regardless of the sanction issued by school administrators. Importantly, approximately 30% of severe discipline incidents do not result in either an in-school or out of school suspension or an expulsion. On the other hand, 98% of reported minor discipline incidents are associated with either suspension or expulsion because minor incidents only need to be reported under those circumstances.

We merged the attendance, discipline, and LEAP participation data described above with the CSDE's administrative records using each student's unique identification code. The CSDE administrative data include information on each student's: 1) gender; 2) race and ethnicity; 3) eligibility for free and/or reduced-price lunch; 4) English language learner status; 5) special education status; 6) grade level; 7)

[10] As a condition for participating in and receiving funding for the LEAP program school districts were required to maintain detailed records on who participated in the LEAP intervention among other things. These records were then transferred to the Connecticut State Department of Education (CSDE) where they were cleaned and processed. We obtained the complete files directly from the CSDE.

school code; and 8) prior academic performance, measured by the Smarter Balanced Assessment (SBA) for students in elementary and middle school or the SAT for students in high school. Using these components, we constructed a secondary dataset that combines the LEAP participation, administrative, and academic performance data for each student. We standardize Smarter Balanced Assessment test scores to have a mean of zero and standard deviation of one for each grade and each school year.

We excluded one urban school district from our analysis for two primary reasons. First, unlike other participating districts, the district contracted the initial LEAP visits to a grassroots community organization that conducted neighborhood canvassing in areas with high rates of chronic absenteeism, rather than visiting individual students directly. Second, district administrators indicated that many students were recorded as having received a LEAP visit (i.e., treated) even when the outreach worker was unable to meet with a parent or student because no one was home or no one responded at the door. Consequently, the treatment variable, the date of the first LEAP visit, likely contains substantial measurement error, which could bias the estimated treatment effects toward zero.

Table 1 presents summary statistics for our primary outcomes and control variables. Columns 1 and 2 provide summary statistics separately for students that participated in the LEAP program (column 1) and those that did not participate (column 2).[11] Students who received a LEAP visit tend to have significantly lower attendance rates, higher rates of chronic absenteeism, a greater likelihood of involvement in disciplinary incidents, and lower academic performance in the year of treatment compared with untreated students. In addition, treated students are more likely to be eligible for free or reduced-price lunch, more likely to be English language learners, and more likely to receive special education services.

As we discuss in detail in the next section, our identification strategy compares students treated early in the school year by the LEAP program to students treated later in the school year, namely in April, May or June. Consequently, in columns 3 and 4, we present summary statistics for students treated prior to April in each year (column 3) and for students treated in April, May or June of each year (column 4). Students treated earlier in the school year have math and ELA test scores, as well as severe discipline incident rates that are very similar to students treated later in the school year, while untreated students have substantially higher test scores and fewer discipline incidents. In terms of attendance and chronic absenteeism, we observe substantial differences between students treated earlier and later in the school year with later treated students appearing to be positively selected. However, these differences are approximately half the size of the difference between treated and untreated students. Differences between early and later treated students by race and ethnicity, free or reduced-price lunch status, and disability

[11] To ensure comparability between treated and untreated samples, we weight the untreated sample means by the ratio of the number of treated students to the number of untreated students in the same district and school year.

status are both small in absolute terms and also notably smaller than differences between treated and untreated students.

IV. Empirical Framework

Using the data described above, we employ a difference-in-differences (DiD) identification strategy to estimate the causal impact of LEAP on student attendance, disciplinary incidents, and standardized test scores. Given strong selection on observables into participation in LEAP and a program in the early stages of implementation where selection is likely evolving across academic years, we select control students as students who were treated by LEAP during the same academic year, but had not yet been treated at the time of each monthly observation. Specifically, student observations are only included from the academic year in which they are first treated by LEAP so that the identifying variation is based on month of treatment within the treatment year, and students are excluded from later years even if they are treated by the LEAP program a second time. We implement two distinct DiD specifications to account for differences in the temporal structure of the data: monthly data for attendance and disciplinary incidents, and annual data for academic performance.

For monthly attendance and disciplinary incidents, we begin with a non-parametric event-study specification of the following form:

$$y_{igstc} = \sum_{j=-5}^{5} \gamma_j I[j = t - m_i] T_i + \delta_i + \lambda_{gstc} + \eta_{igstc} \quad (1)$$

where y_{igstc} denotes an outcome of interest for student i, in grade g, attending school s, in month t, and academic year treated cohort c. m_i identifies the month that the student was treated so that the indicator I takes the value of 1 based on event time centered on the month of treatment, and T_i represents whether student i was treated by LEAP in a given year or is a control student. δ_i is a vector of student fixed effects, λ_{gstc} is a vector of grade-by-school-by-month-by-cohort (treatment year) fixed effects, and η_{igstc} is a random disturbance term. The omitted category is the month a LEAP visit was conducted, or when $t - m_i$ equals zero. The model combines three annual cohorts of LEAP treated students.

To address any bias in our estimated treatment effects due to the staggered timing of treatment and potential for heterogeneous treatment effects, we estimate equation (1) using a stacked DiD framework (Deshpande and Li 2019; Goodman-Bacon 2021). Specifically, we first created a set of treatment month samples that include observations from students that were either treated by LEAP in the same month and academic year (treatment) or that were treated by LEAP in a later month during that same academic year (control). When forming the control group (i.e. the students treated in later months) for each cohort, we only include observations prior to treatment, and so while the control group is fixed

for each cohort in the pre-period through the first month after treatment, control observations must be dropped during the post-period as additional students are treated. We then appended (stack) these specific subsamples for each treatment year cohort and estimate models similar to equation (1), except we replaced the student fixed effects with student-by-treatment month subsample fixed effects and replace the grade-by-school-by-month-by-cohort fixed effects with grade-by-school-by-month-by-cohort-by-treatment month subsample fixed effects. Manually stacking to address bias from staggered roll-out facilitates our later attempts to address potential bias from systematic selection into the timing of treatment.

The coefficients of primary interest in equation (1) are the γ_j's, which represent the difference-in-differences estimates of the impact of the LEAP program on our outcomes of interest in each month from $t_{-5}$to t_5 pooling the information provided by the sub-experiments created for each LEAP treatment month. The estimated coefficients on the lead treatment indicators $(\gamma_{-5}, \dots, \gamma_{-1})$ provide evidence on whether our outcomes were trending prior to the time a LEAP visit was conducted. If participation in LEAP induces exogenous changes in student outcomes, these lead treatment indicators should be small in magnitude and statistically insignificant. The lagged treatment indicators $(\gamma_1, \dots, \gamma_5)$ allow the effect of participation in LEAP on our outcome of interest to grow or fall over time and in a non-parametric way in the post treatment period. Given the treatment (participation in LEAP) occurs at the student level, in all specifications we cluster the standard errors at the student level.

While the month of treatment stack notation is suppressed for parsimony, we also complement the event-study specification with a stacked difference-in-differences model to increase precision by pooling estimates within both the pre- and post-LEAP visit period. We also estimate this model as a constant treatment effect difference-in-differences, as well as estimate an extended model that allows for both a pre-treatment linear trend and for a linear trend in the treatment effect estimates post-treatment ($Trend_t$). In the trend model, the estimate of treatment interacted with whether the month is post-treatment captures the jump in treatment for the month after treatment. Specifically,

$$y_{igstc} = \beta_0 + \beta_1(T_i \cdot Post_t) + \beta_2(T_i \cdot Trend_t) + \beta_3(T_i \cdot Trend_t \cdot Post_t) + \boldsymbol{X_i}\theta_t + \delta_i + \lambda_{gstc} + \epsilon_{igstc} \quad (2)$$

where $Post_t$ is a post-treatment indicator that equals one for all months following the treatment month, which is interacted with the treatment indicator T_i. We also included a vector of control variables, $\boldsymbol{X_i}$, which includes a student's gender, race and ethnicity, free lunch eligibility status, English learner status, and special education status, and the effect of these non-time varying attributes is allowed to vary by month. Finally, ϵ_{igstc} is a random disturbance term, and all other terms were as defined in equation (1).

The coefficients of primary interest in equation (2) are β_1, β_2 and β_3. Specifically, β_1, the coefficient on the interaction of the treatment indicator and post treatment indicator, $T_i \cdot Post_t$, captures

the post-treatment effect of LEAP participation relative to the pre-treatment period while β_2, the coefficient on the linear treatment trend variable, captures the difference in the pre-treatment trend in the outcome of interest between treated and control students. Similar to the lead treatment indicators in equation (1), the estimated coefficient on β_2 provides a test of whether the outcomes of treated students were trending differently prior to treatment relative to untreated students. Lastly, β_3, the coefficient on the interaction between the treatment indicator, the linear time trend, and a post-treatment indicator, captures the change in the trend of the outcome of interest post treatment for the treated students relative to the control group.

IV.A Selection into Treatment Timing

As noted previously, one concern with the event study and difference-in-differences models that correct for staggered roll-out bias by using not yet treated observations as controls is the potential for section into treatment timing. Specifically, observations treated later may differ from earlier treated observations in a way that influences the pattern of outcomes over time, and as the control sample shrinks as not yet treated control observations experience treatment increasing bias may appear as a trend in treatment effect estimates. To address that concern, we complement our standard staggered roll-out robust models with two approaches designed to address selection into treatment timing, first using non-parametric approaches to address selection on observables (Abadie 2005; Li et al. 2018) and second imposing parametric assumptions to address selection on unobservables.

The first approach addresses selection on observables by estimating a propensity score model of treatment timing and calculating propensity score weights. Table 2 presents the difference between the treatment and the control sample at each event time from -5 to +5 months from treatment. The student attribute means tend to trend over event time with the relative share of treated free and reduced-price lunch students, students with disabilities, and black students increasing over time and the relative share of treated English learner and Hispanic students falling. To address this selection, we estimate a propensity score model of treated (T) versus a control student separately for every event time (e), i.e. month of outcomes relative to month of treatment, as a function of a student's gender, race, free lunch status, ELL status, and disability status. Specifically, we estimate a logit model for

$$T_{ie} = \begin{cases} 1 \; if \; \psi_e X_i + \omega_{ie} \geq 0 \\ 0 \; if \; \psi_e X_i + \omega_{ie} < 0 \end{cases} \tag{3}$$

The overlap weights obtain balance by scaling observations by one minus the probability of being observed with the value T_{ie}, rather than one over the probability.

$$W_{ie} = \begin{cases} \left(1 - Pr\left[\hat{\psi}_e X_i\right]\right) \; if \; T_{ie} = 1 \\ Pr\left[\hat{\psi}_e X_i\right] \qquad if \; T_{ie} = 0 \end{cases} \tag{4}$$

Then, we simply re-estimate equation (1) and (2) using weighted regressions. After applying overlap weights, the weighted differences between the treatment and control samples by event time are all zero to the third decimal place, as compared to the raw differences documented in Table 2. Our use of overlap weights, rather than propensity score weights, is intentional because our goal is to assess the stability of estimates to re-weighting the data. By construction, propensity score weights put very large weights on observations that were unlikely to have arisen by chance, and so may have large affects on average treatment effect estimates if treatment effects are heterogeneous, while overlap weights focus the weight on the areas of common support (Li et al. 2018).

The second approach involves a parametric model that allows us to test for bias from selection on unobservables, but requires a key additional assumption. Specifically, while we allow treatment effects to vary over event time, this approach requires that average treatment effects at each event time do not change with the timing of treatment. Specifically, the proposed parametric approach avoids selection in the control group by retaining the entire control group during positive event time, while using treatment effect estimates from earlier event times to account for the fact that a portion of this control group has experienced treatment at a later event time. Recognizing that treatment effects are zero in the month of the initial LEAP visit and jump sharply in the first month after initial treatment (see the results section below), we start by estimating our event study while dropping all data for event time +2 or later.

$$y_{igstc} = \sum_{j=-5}^{1} \gamma_j I[j = t - m_i] T_i + \delta_i + \lambda_{gstc} + \eta_{igstc} \quad (5)$$

This model provides estimates for initial treatment effects $\hat{\gamma}_1$ and our best estimates for the individual fixed effects $\hat{\delta}_i$. We next estimate the treatment effect for the second period partialling out both 1) the estimated treatment effects in the first month after treatment, $\hat{\gamma}_1$ for control observations treated at one month after the treated observations and 2) the estimated fixed effects, $\hat{\delta}_i$, or

$$y_{igstc} - \hat{\delta}_i - \hat{\gamma}_1 I[t - m_i = 2] \tilde{T}_i^1 = \sum_{j=-5}^{2} \gamma_j I[j = t - m_i] T_i + \lambda_{gstc} + \eta_{igstc} \quad (6)$$

where $\tilde{T}_i^1$ takes the value of 1 if individual i is treated in event time 1 and 0 otherwise. In that case, the effects of treatment for those individuals will begin at event time 2, and so under the assumption that the month of treatment does not affect treatment effects we subtract $\hat{\gamma}_1$ from the outcomes of those students when estimating treatment effects at event time 2.

These models are estimated iteratively extending positive event time one period at a time, and the general model takes the form:

$$y_{igstc} - \hat{\delta}_i - \sum_{j'=1}^{J-1} \sum_{j''=j'}^{J-1} \hat{\gamma}_{j''-j'+1} I[t - m_i = j'' + 1] \tilde{T}_i^{j'} = \sum_{j=-5}^{J} \gamma_j I[j = t - m_i] T_i + \lambda_{gstc} + \eta_{igstc} \quad (7)$$

where $\tilde{T}_i^{j'}$ takes the value of 1 if individual i is treated in event time j' and zero otherwise. For example, for $j' = 1$, the two summations imply subtracting $\hat{\gamma}_1$ at event time 2, $\hat{\gamma}_2$ at event time 3, etc for all control students treated one month after the treatment students. For $j' = 2$, the summations imply subtracting $\hat{\gamma}_1$ at event time 3 and so on for students treated two months after the control students. We report event studies for the final event study estimates where $J = 5$, but estimates for γ_1 through γ_4 are virtually identical no matter which iteration is used to provide those estimates. The difference-in-differences models in equation (2) are estimated using the partialled-out outcome variables from the event study sample for $J = 5$ dropping the individual fixed effects as well as the calendar month linear trends. Standard errors are bootstrapped by sampling students with replacement.

Admittedly, the assumption that treatment effects at each event time do not change with the timing of treatment can be strong under a several circumstances: 1) A program is evolving over time so that the details of treatment may change based on the timing of treatment, and 2) Selection over treatment timing may imply that treatment effects differ between individuals. In our context, program evolution is likely to occur across program years, rather than during the academic year, and our models are identified using students treated in the same academic year as controls. The second issue of selection into treatment based on attributes that are associated with different treatment effect magnitudes is potentially problematic. However, presumably one would view selection over treatment timing in terms of unobservables that are associated with future outcomes as first order relative to selection over treatment timing in terms of unobservables that correlate with the magnitude of treatment effects. Therefore, while one cannot rule out the creation of bias from applying this parametric control for selection on observables, this source of bias can only exist if baseline event study estimates are already at threat of bias from selection into treatment timing, and so we would recommend applying this technique under such circumstances in order to conservatively capture treatment effect estimates.

IV.B Standardized Test Scores

Given that academic performance is only observed at one point in time each year, we examine whether exposure to LEAP well before testing (e.g., in September or October) is associated with greater improvements in test scores, and whether treatment occurring close to or after the testing month, e.g. our pre-treatment months of May and June, has limited or no impact, which is to be expected if LEAP home visits occur too late in the academic year to influence performance. We replace the not yet treated control group strategy with a within student strategy supplementing the data by including a second year of test score data from the year prior to treatment. We again employ an event-study specification that in this case compares treatment effects across the months of participation in LEAP relative to the administration of standardized tests. Specifically, we estimate a model of the form:

$$y_{igs\tau c} = \kappa I[\tau = 1] + \sum_{m=-7}^{2} \alpha_m I[m = t_i, \tau = 1] + \delta_i + \lambda_{gs\tau c} + \upsilon_{igs\tau c} \quad (8)$$

where $y_{igs\tau c}$ denotes the test score of students i in grade g, attending school s in year τ relative to the treatment year, and academic year treated cohort c. τ equals one if the observation corresponds to the student's treatment year and zero if the observation is from the year prior to treatment, t is equal to the month of treatment based on the student's initial participation in LEAP relative to April in the treatment year and so takes a value between minus seven and two, and all other terms are as defined in equation (1). As above, we cluster the standard errors at the student level.

Finally, we complement the event-study specification with a standard difference-in-differences model by pooling the monthly LEAP indicator variables before the April period ($Pre_t = 1$) and those after the April period:

$$y_{igs\tau c} = \phi_0 I[\tau = 1] + \phi_1 (I[\tau = 1] \cdot Pre_t) + \boldsymbol{X_i}\varphi_\tau + \delta_i + \lambda_{gs\tau c} + \upsilon_{igs\tau c} \quad (9)$$

All terms follow the definitions above, and we stack the data and expand the fixed effect vectors to address concerns about staggered roll-out. The coefficient of primary interest in equation (4) is ϕ_1, the coefficient on the interaction between the pre-April indicator and the treatment year indicator, and the effect of student attributes is allowed to vary between the pre-treatment and treatment years.

V. Results

V.A Attendance, Chronic Absenteeism and Severe Discipline Incidents

We begin our analysis by estimating the impact of LEAP participation on students' attendance rates, chronic absenteeism, and severe discipline incidents using the event-study specification described above. Specifically, we estimate equation (1) using our monthly dataset and plot the estimated event-time coefficients (γ_j) along with their associated 95 percent confidence intervals. Figures 1a-c illustrate the estimated effects of LEAP participation on student attendance rates (Figure 1a), chronic absenteeism (Figure 1b), and severe discipline incidents (Figure 1c). The horizontal axis in the figures delineates the months since a student was treated by a LEAP visit. For example, the label "-5" denotes that the estimate corresponds to 5 months prior to a student being treated by LEAP while the label "5" denotes that the estimate corresponds to 5 months after a student was treated by LEAP. The vertical dashed line indicates the month of treatment, i.e., the first LEAP visit. The vertical axis represents the change in student outcome for those treated by LEAP relative to those in the control group, i.e., students who had not yet received a LEAP visit. Similarly, Table 3 presents the difference-in-differences estimates with the top panel presenting the baseline estimates and the bottom panel presenting estimates conditional on differential trends prior to and after treatment.

As shown in Figure 1a, for attendance rates, there is evidence of a modest decline of just over one percentage point in attendance in the month before and the month of treatment, but for chronic absenteeism (Figure 1b) the trends between treatment and control students is flat. In the bottom panel of Table 3, this leads to a negative pre-trend estimate for attendance rates of about one half of one percentage point per month and a near zero estimate on pre-trends for chronic absenteeism. Both attendance and chronic absenteeism improve substantially in the month after treatment, with between a 4 and 5 percentage point increase in attendance rates and almost a 14 percentage point decline in chronic absenteeism. As shown in Figures 1a and 1b, these treatment effects continue to trend upwards (downwards) peaking at around a 7 percentage points increase for attendance and bottoming out at 18 percentage points decline for chronic absenteeism relative to pre-treatment mean rates of 79 and 58 percent, respectively. This trend is reflected in the bottom panel of Table 3 as statistically significant post-treatment trends of over one percentage point per month for both attendance rates and chronic absenteeism.

For severe discipline incidents, the somewhat noisier estimates in the bottom panel of Table 3 imply that both pre and post treatment trends are statistically insignificant with t-statistics less than one, but both point estimates represent just under a one-half of one percentage point decline per month in the likelihood of a severe discipline incident. Therefore, the difference-in-differences estimates based on conditioning out trends implies that treatment reduced the likelihood of a severe discipline incident in the month after treatment by 3.6 percentage points, while the point estimate from a model without trends is just under 6 percentage points, relative to a pre-treatment sample mean likelihood of a severe discipline incident of 14.1 percent. All estimates in Table 3 including the trend estimates are very stable to the inclusion of student controls interacted with calendar month.

We next address concerns about selection over treatment timing. Figure 2 presents event studies based on a weighted regression utilizing overlap weights from separate propensity score models for each event time relative to the month of treatment. Figure 3 presents the event studies that address selection over unobservables iteratively by differencing out treatment effects from preceding months. The weighted event studies in Figure 2 are nearly identical to Figure 1 with strong post-treatment trends for attendance rates and chronic absenteeism, and statistically noisy, but moderate sized pre and post treatment trends for severe discipline incidents. However, Figure 3 provides evidence of selection into treatment timing based on student unobservables. Specifically, for all three outcomes, the point estimates for the first month following treatment is nearly identical to the initial treatment effect estimates in Figure 1 (by design), and then treatment effects for later months are centered around that initial treatment effect estimate. The highly significant positive trends in treatment effects for attendance rates and chronic absenteeism do not survive our correction for selection into treatment timing over student unobservables. Following Wing et

al. (2024), we further re-estimate the event study models that correct for selection on unobservables restricting the event study to event time plus three months after treatment or earlier and dropping the month of treatment subsamples for April and May so that the treatment sample is constant during positive event time, see Appendix Figure A1. These event studies continue to support the finding that there are no trends in treatment effects over event time.

Table 4 presents the difference-in-differences estimates for these models presenting the results for attendance rates in panel A, chronic absenteeism in panel B, and severe discipline incidents in panel C. Column 1 presents the original estimates for the traditional stacked difference-in-differences model, column 2 presents the estimates for the overlap weighted models, column 3 presents the estimates for the iterative model that addresses selection on unobservables and column 4 presents the difference-in-differences estimates for the model that also holds the treatment sample constant over positive event time. Given the pre-trends for the baseline and overlap weight models, we present the difference-in-differences models with trends for columns 1 and 2 and so identify the effect one month after treatment. The selection on unobservable estimates for attendance rates and chronic absenteeism are above the baseline and overlap weighted estimates from the trend models at 5 and 15 percentage points, respectively, compared to about 4 and between 13 and 14 percentage points for the estimates in columns 1 and 2, but about one percentage point below the baseline estimates without trends from the top panel in Table 3. Our preferred selection corrected estimate for severe discipline incidents is 3.6 percentage points, nearly identical to the baseline estimate conditional on trends and below both the weighted model estimate in column 2 of 4.3 points and well below baseline estimate of 6 percentage points without the trend controls from Table 3. The attendance and chronic absenteeism treatment effect estimates in column 4 are quite similar to the main selection corrected estimates in column 3. Admittedly, the estimate for severe discipline incidents are somewhat larger at 4.8 percentage points, but the standard errors in the discipline models are substantially larger than for attendance and absenteeism, and these estimates are relatively close in magnitude to the weighted results in column 2.

Finally, we examine whether the estimated effects of LEAP on attendance, chronic absenteeism, and severe discipline incidents are heterogeneous across student attributes. To be conservative, Appendix Tables A1-A3 present treatment effects based on the iterative DiD model given by equation (7) designed to correct for selection on unobservables. We present treatment effect estimates for attendance in column 1, chronic absenteeism in column 2, and severe discipline incidents in column 3. Each table presents two panels, one for each subsample with the top panel presenting estimates for the free or reduced price lunch sample, the female sample and the sample of black or Hispanic students. The treatment effects appear to be relatively broad based with minimal evidence of heterogeneous effects over student attributes.

V.B Standardized Test Scores

Lastly, we examine whether participation in LEAP affected students' academic performance using the event-study specification given by equations (8) and (9). Figures 4a and 4b present the estimated effects of LEAP participation on ELA test scores (Figure 4a) and math test scores (Figure 4b). The horizontal axis in both Figures 4a and 4b represents the month of LEAP participation relative to April, the reference primary month for testing. For example, the label "–5" indicates that the estimate corresponds to students who received the LEAP visit five months prior to April (i.e., in November), while the label "2" denotes students treated two months after April (i.e., in June). Therefore, one should consider event times of 1 and 2 as the pre-treatment period, and negative event time as post-treatment. The vertical dashed line indicates the reference month for testing. The vertical axis in Figure 4a represents the change in standardized ELA test scores for students treated by LEAP relative to those in the control group (i.e., students who had not yet received a LEAP visit), while the vertical axis in Figure 4b represents the corresponding change in standardized mathematics test scores for treated students relative to the control group.

The results indicate no systematic relationship between the timing of LEAP participation and subsequent test performance. Estimated effects fluctuate around zero, and none are statistically significant for either ELA or mathematics. Students treated well before the testing month do not exhibit higher test scores compared with those treated closer to testing, and students treated after testing show similarly null effects. These findings suggest that, within the same academic year, earlier exposure to LEAP does not translate into measurable improvements in standardized test outcomes. However, this analysis has noticeably less power than the analyses of attendance and discipline incidents because monthly data on academic performance is not available. The resulting standard errors of the estimates are large enough that we cannot rule out modest effect sizes on test scores.

VI. Conclusion

In the aftermath of the COVID 19 pandemic states and local school districts have struggled to adequately address the sharp rise in chronic absenteeism that began in 2020 and has persisted to this day. In this paper we examine the impact of an innovative program implemented in the state of Connecticut designed to address student chronic absenteeism and disengagement from school, namely the Learner Engagement and Attendance Program (LEAP). Starting in 2021, home visits by teachers and other school personnel were conducted with students identified as chronically absent from a targeted sample of Connecticut's most economically disadvantaged districts. These visits were designed to connect students and their families directly with teachers and other school personnel with the goal of fostering trust and strong connections between families and their school community.

Using event study and difference-in-differences models that exploit the staggered timing of treatment across students and schools, we find that LEAP participation led to meaningful improvements in students' attendance and behavioral outcomes but had no detectable impact on academic performance within the same school year. Across both the event-study and difference-in-differences specifications, we find that students who received the LEAP intervention experienced large and sustained increases in attendance and subsequent declines chronic absenteeism and disciplinary incidents. Five months after treatment, students that participated in the LEAP program experienced 4 to 5 percentage point increase in attendance rates, a 13 to 15 percentage point decline in chronic absenteeism rates, and a 3 to 4 percentage point decline in the number of disciplinary incidents. These treatment effect estimates are obtained by either controlling for selection into treatment timing or including pre and post treatment trends to identify the jump in outcomes at the time of treatment. These preferred treatment effect estimates are smaller than the baseline difference-in-differences estimates, illustrating the importance of addressing selection into treatment timing when identification is obtained using not yet treated observations. This concern could be important even in cases where strong evidence is available that pre-trends are parallel, i.e. for chronic absenteeism in our application.

Finally, the impact of LEAP on attendance, chronic absenteeism, and disciplinary incidents appears to be relatively broad based with minimal evidence of heterogeneous effects over student attributes. Taken together, our attendance and discipline results provide evidence that the LEAP intervention increased student noncognitive skills, skills that have been shown to be as valuable as cognitive skills for improving long-term outcomes.

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Table 1
Summary Statistics

	(1)	(2)	(3)	(4)
	Treated	Untreated	Treated Early	Treated Late
	Student Outcomes			
Attendance Ratio	0.807	0.902	0.775	0.835
Chronic Absenteeism (0, 1)	0.539	0.324	0.592	0.491
Any Severe Disciplinary Incidents (0, 1)	0.144	0.129	0.141	0.147
ELA Scaled Score	-0.343	-0.030	-0.366	-0.323
Math Scaled Score	-0.350	-0.011	-0.383	-0.321
	Student Attributes			
Female	0.481	0.488	0.475	0.485
Free or Reduced-Price Lunch	0.799	0.617	0.805	0.808
English Language Learner	0.284	0.278	0.295	0.271
Special Education	0.212	0.151	0.213	0.216
Black	0.235	0.230	0.231	0.238
Hispanic	0.603	0.521	0.619	0.593
Asian or Pacific Islander	0.028	0.051	0.025	0.032

Notes: Table presents means of outcomes and student attributes during the year of LEAP treatment. Column 1 presents means for the full sample of treated students while column 2 presents means for the full sample of untreated students that attend the same schools as treated students. Column 3 presents means for students treated prior to April in each year while column 4 presents means for students treated in April, May or June of each year.

Table 2
Treatment versus Control Sample Attributes by Event Time

Event Time	-5	-4	-3	-2	-1	0	1	2	3	4	5
Raw Difference between Treatment and Control Samples											
Female	-0.003	-0.004	0.000	-0.002	-0.001	-0.007	-0.010	-0.010	-0.010	-0.012	-0.011
Free or Reduced-Price Lunch	0.002	0.002	0.010	0.008	0.010	0.009	0.012	0.023	0.029	0.028	0.033
English Language Learner	0.027	0.014	0.008	-0.001	-0.001	0.004	-0.010	-0.008	-0.020	-0.022	-0.018
Special Education	0.001	0.001	0.002	0.003	0.008	0.007	0.012	0.016	0.019	0.019	0.023
Black	-0.017	-0.011	0.003	0.006	0.010	0.002	0.013	0.017	0.027	0.029	0.031
Hispanic	0.033	0.020	0.009	-0.001	-0.001	0.003	-0.009	-0.013	-0.021	-0.023	-0.020
Asian or Pacific Islander	-0.008	-0.004	-0.005	0.000	-0.002	-0.001	0.000	0.001	0.002	0.002	0.001
Overlap Weights Difference between Treatment and Control Samples											
Female	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Free or Reduced-Price Lunch	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
English Language Learner	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Special Education	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Black	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Hispanic	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Asian or Pacific Islander	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Table presents difference in means of student attributes between treated and control students at each month relative to the month of treatment. For example, the column labelled -5 represents all monthly observations that occurred five months prior to the treatment month. The top panel represents the raw differences, while the bottom panel illustrates the differences after re-weighting the data. The overlap weights are based on a separate propensity score model for each event time or column in the table where the weight for treatment observations is one minus the probability that an observation is a treatment observation and the weight for control observations is the probability that an observation is a treatment observation.

Table 3
Baseline Difference-in-Differences Estimates

Panel A: Difference-in-Differences

	Attendance		Chronic Absenteeism		Severe Incident	
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	With Controls	Baseline	With Controls	Baseline	With Controls
Post Treatment	0.0497***	0.0498***	-0.159***	-0.159***	-0.0590***	-0.0567***
	(0.00260)	(0.00259)	(0.00676)	(0.00675)	(0.0148)	(0.0147)
Observations	388,305	388,305	388,305	388,305	38,110	38,110
R-squared	0.744	0.744	0.527	0.527	0.480	0.481

Panel B: Difference-in-Differences with Trends in Event Timing

Post Treatment	0.0412***	0.0412***	-0.136***	-0.136***	-0.0369*	-0.0364*
	(0.00283)	(0.00283)	(0.00772)	(0.00772)	(0.0189)	(0.0190)
Trend Treatment	-0.00461***	-0.00462***	0.00109	0.00113	-0.00444	-0.00418
	(0.000727)	(0.000726)	(0.00200)	(0.00199)	(0.00469)	(0.00470)
Trend Post Treatment	0.0108***	0.0109***	-0.0121***	-0.0123***	-0.00339	-0.00300
	(0.00125)	(0.00125)	(0.00311)	(0.00311)	(0.00722)	(0.00721)
Observations	388,305	388,305	388,305	388,305	38,110	38,110
R-squared	0.744	0.744	0.527	0.527	0.480	0.481

Notes: Table presents DiD estimates of the impact of LEAP treatment on student attendance, chronic absenteeism and severe discipline incidents. Panel A presents DiD treatment effect estimates where Post Treatment is an indicator for treated students in the post treatment period. Panel B presents three parameters: the DiD treatment effect estimate for the month after treatment, an estimated trends in the pre-treatment period, and a final parameter that allows the impact of treatment to grow gradually overtime. Trend Treatment is the interaction of a linear time trend with an indicator for treated students which captures pre-trends. Trend Post Treatment is the interaction of a linear time trend with the post treatment indicator capturing a change in outcome trends at the time of treatment. Columns 1, 3 and 5 present baseline estimates with no controls. Columns 2, 4 and 6 add student level controls for gender, free or reduced-price lunch status, special education status, English language learner status, and indicators for Black, Hispanic and Asian or Pacific Islander students that are interacted with calendar month. All specifications include student and grade-by-school-by-treatment year cohort-by-treatment month subsample fixed effects. Standard errors clustered at the student level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4
Difference-in-Differences Estimates addressing Selection on Treatment Timing

	(1) Baseline Model with Trends	(2) Overlap Weights with Trends	(3) Constant Control Sample	(3) Constant Treatment and Control Sample
Panel A: Attendance				
Post Treatment	0.0412***	0.0389***	0.0512***	0.0513***
	(0.00283)	(0.00293)	(0.00283)	(0.00400)
Observations	388,305	388,074	498,069	338,497
R-squared	0.744	0.771	NA	0.226
Panel B: Chronic Absenteeism				
Post Treatment	-0.136***	-0.128***	-0.151***	-0.168***
	(0.00772)	(0.00747)	(0.00789)	(0.00989)
Observations	388,305	388,074	498,069	338,497
R-squared	0.527	0.551	NA	NA
Panel C: Severe Discipline Incidents				
Post Treatment	-0.0369*	-0.0425**	-0.0364**	-0.0484*
	(0.0189)	(0.0190)	(0.0181)	(0.0249)
Observations	38,110	37,912	50,767	34,816
R-squared	0.480	0.512	NA	NA

Notes: Table presents DiD estimates of the impact of LEAP treatment on student attendance in Panel A, chronic absenteeism in Panel B and severe discipline incidents in Panel C. Column 1 presents baseline DID estimates conditional on trends from the bottom panel of Table 3. Column 2 presents weighted DID estimates conditional on trends where weights are based on a separate propensity score model for each event time with the weight for treatment observations being one minus the probability that an observation is a treatment observation and the weight for control observations being the probability that an observation is a treatment observation. Column 3 presents DID estimates where the control student sample is held constant over time, but the treatment effect for control observations that experience treatment later in event time is partialled out of the control student outcomes. Column 4 presents the same constant control sample model estimates, but holds the treatment sample fixed post-treatment by dropping data at event time +4 and +5 and dropping the treatment month in April and May subsamples. All models include student level controls for gender, free or reduced-price lunch status, special education status, English language learner status, and indicators for Black, Hispanic and Asian or Pacific Islander students that are interacted with calendar month. All specifications include student and grade-by-school-by-treatment year cohort-by-treatment month subsample fixed effects. Standard errors clustered at the student level for columns 1 and 2 or clustered bootstrap by student for columns 3 and 4 in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5
DiD Estimates: Student Achievement

	ELA		Math	
	(1)	(2)	(3)	(4)
	Baseline	With Controls	Baseline	With Controls
Post Treatment	0.00888	0.000344	-0.0303	-0.0485
	(0.0499)	(0.0500)	(0.0487)	(0.0497)
Observations	10,771	10,771	10,499	10,499
R-squared	0.926	0.926	0.928	0.929

Notes: Table presents DiD estimates of the impact of LEAP treatment on student achievement comparing students treated between September and March to students treated between April and June in a sample containing test scores in the year of treatment and the year before treatment for each student. Columns 1 and 3 present baseline estimates with no controls. Columns 2 and 4 add student level controls for gender, free or reduced-price lunch status, special education status, English language learner status, and indicators for Black, Hispanic and Asian or Pacific Islander students interacted with a dummy for whether the test score is from the year of treatment. All specifications include student and grade-by-school-by-prior year/year of treatment-by-treatment year cohort fixed effects. Standard errors clustered at the student level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 1
Event Study Estimates: Attendance, Chronic Absenteeism, and Discipline

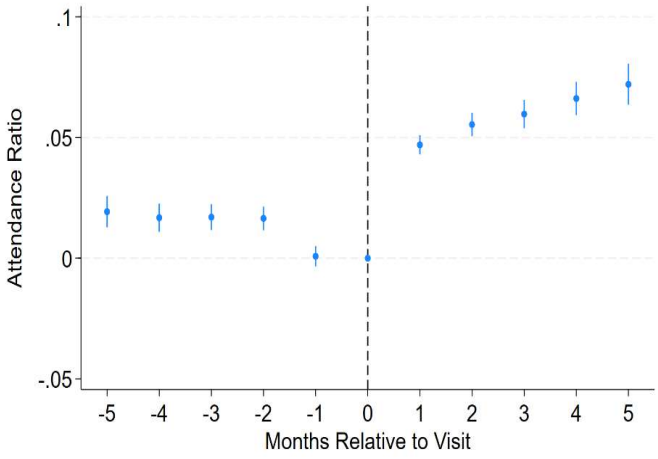


Figure 1a

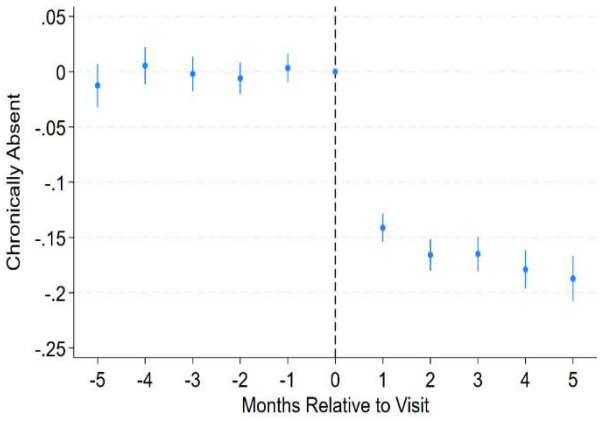


Figure 1b

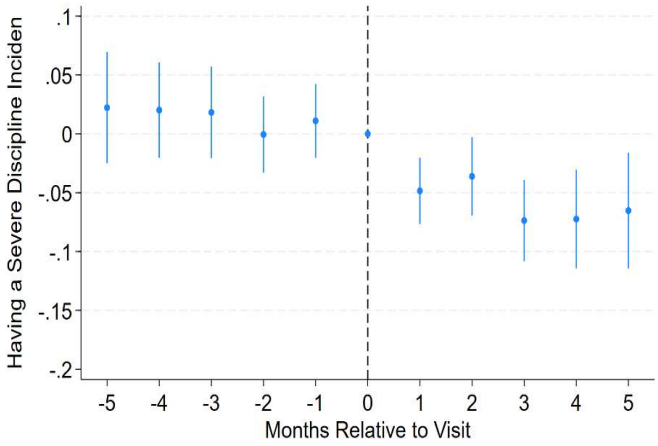


Figure 1c

Notes: Figures 1a-1c present event study estimates for attendance rates, likelihood of being chronically absent (absent 10% or more of enrolled days), and share of students with a severe discipline incident in a given month relative to the month of treatment (month zero). Event study models include controls for student and grade-by-school-by-treatment year cohort-by-treatment month subsample fixed effects. The regression sample is composed of month of treatment-by-treatment-year subsamples that contain treated students defined as treated in a specific month and year and control students defined as being treated in the same year, but treated in a later month. Control student monthly observations associated with calendar months when the control student was treated or had already been treated are omitted. The vertical bars associated with treatment effect estimates represent 95% confidence intervals based on standard errors clustered at the student level.

Figure 2
Event Study Estimates using Overlap Weights

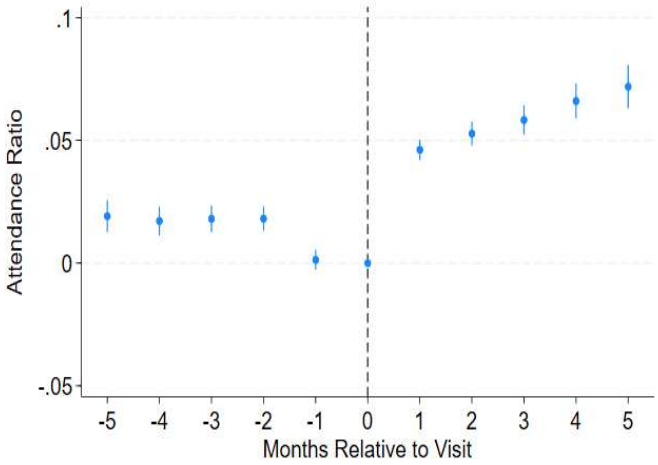


Figure 2a

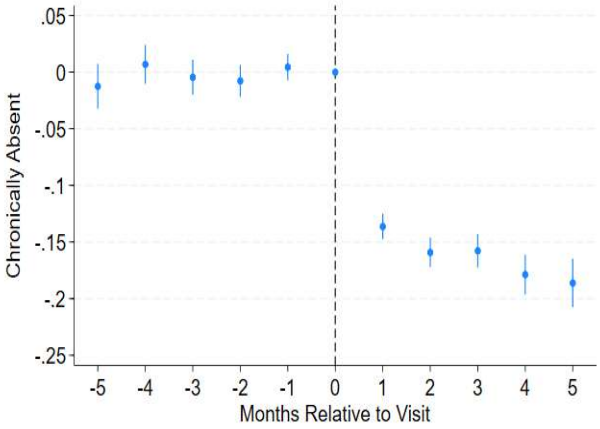


Figure 2b

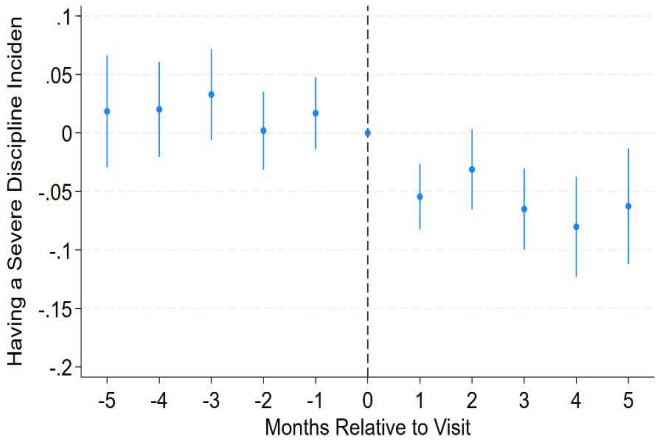


Figure 2c

Notes: Figures 2a-2c present event study estimates for attendance, chronic absenteeism, and severe discipline incidents in a given month relative to the month of treatment. Event study models include controls for student and grade-by-school-by-treatment-year cohort-by-treatment month subsample fixed effects using the month of treatment-by-treatment-year subsamples. Propensity score models are estimated separately for each event time shown on the horizontal axis where treatment status is regressed on gender, free or reduced-price lunch status, special education status, English language learner status, and indicators for Black, Hispanic and Asian or Pacific Islander students. Regression models are estimated using overlap weights where the weight for treatment observations is one minus the probability that an observation is a treatment observation and the weight for control observations is the probability that an observation is a treatment observation. The vertical bars associated with treatment effect estimates represent 95% confidence intervals based on standard errors clustered at the student level.

Figure 3
Event Study Estimates with Constant Control Sample

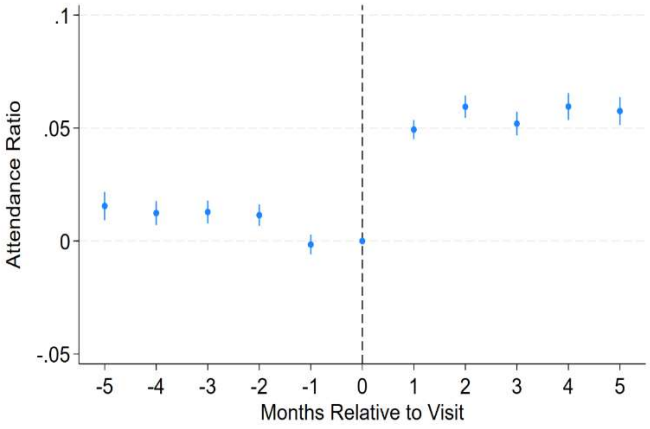


Figure 3a

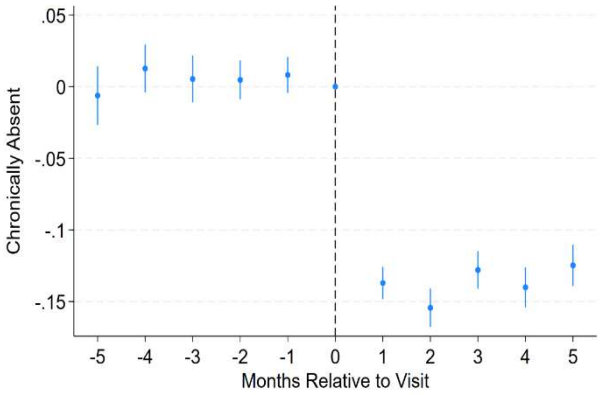


Figure 3b

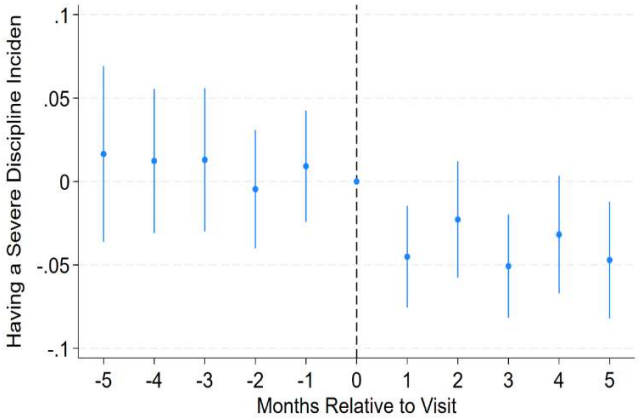


Figure 3c

Notes: Figures 3a-3c present event study estimates for attendance, chronic absenteeism, and severe discipline incidents in a given month relative to the month of treatment. Event study models include controls for student and grade-by-school-by-treatment-year cohort-by-treatment-month subsample fixed effects using the month of treatment-by-treatment year subsamples except that month observations in and after the month when a control student is treated are no longer dropped. Rather, the model is estimated iteratively starting with the first month of positive event time, and the estimated effect of treatment on currently treated control observations is partialled out of the student's outcome in each month of positive event time. The vertical bars associated with treatment effect estimates represent 95% confidence intervals based on clustered bootstrap standard errors at the student level.

Figure 4
Event Study Estimates: Student Achievement

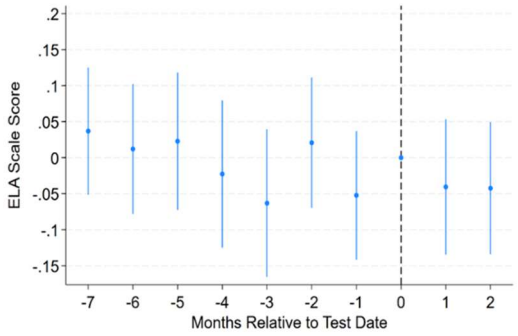


Figure 4a

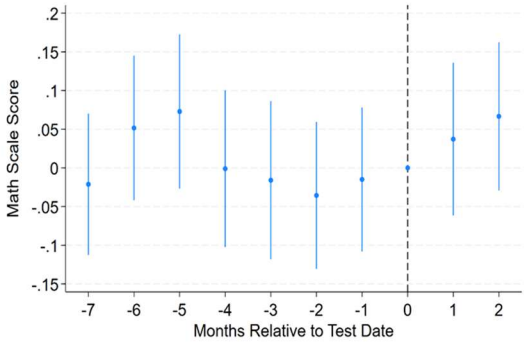


Figure 4b

Notes: Figures 4a and 4b present event study estimates for standardized ELA scaled scores and standardized math scaled scores. Event study models include controls for student and grade-by-school-by-treatment year cohort-by-whether the test was in the year of treatment or the previous year fixed effects. The regression sample is composed of treatment year subsamples that contain observations for student tests scores in the year of treatment and the year preceding treatment where students are only included in a treatment year subsample if they are treated in that year. Treatment effects are identified relative to the month of treatment as compared to the month of the test score (the Month of April labelled as time zero on the horizontal axis) and relative to tests scores in the year prior to treatment. The vertical bars associated with treatment effect estimates represent 95% confidence intervals based on standard errors clustered at the student level.

Appendix

Appendix Table A1
Treatment Effects by Free or Reduced Lunch Status using Constant Control Sample

	(1) Attendance	(2) Chronic Absenteeism	(3) Severe Discipline Incidents
	Panel A: Free or Reduced Lunch		
Post Treatment	0.0509***	-0.145***	-0.0167
	(0.00303)	(0.00840)	(0.0147)
Observations	435,794	435,794	66,507
	Panel B: Not Free Lunch		
Post Treatment	0.0507***	-0.200***	-0.135
	(0.00795)	(0.0252)	(0.0839)
Observations	100,853	100,853	7,513

Notes: Notes: Table presents three DiD estimates of the impact of LEAP treatment on student attendance, chronic absenteeism and severe discipline incidents. Event study models include controls for student and grade-by-school-by-treatment-year cohort-by-treatment-month subsample fixed effects using the month of treatment-by-treatment year subsamples except that as in Figure 3 the month observations in and after the month when a control student is treated are no longer dropped. Rather, the model is estimated iteratively starting with the first month of positive event time, and the estimated effect of treatment on currently treated control observations is partialled out of the student's outcome in each month of positive event time. Panel A presents estimates for the subsample of students who qualify for free or reduced-price lunch, and Panel B presents estimates for the subsample that does not qualify. Standard errors are bootstrapped by sampling students with replacement. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table A2
Treatment Effects by Gender using Constant Control Sample

	(1) Attendance	(2) Chronic Absenteeism	(3) Severe Discipline Incidents
		Panel A: Female	
Post Treatment	0.0520***	-0.170***	-0.0259
	(0.00482)	(0.0120)	(0.0257)
Observations	258,507	258,507	28,992
		Panel B: Male	
Post Treatment	0.0524***	-0.141***	-0.0323*
	(0.00419)	(0.0118)	(0.0173)
Observations	278,140	278,140	45,028

Notes: Notes: Table presents three DiD estimates of the impact of LEAP treatment on student attendance, chronic absenteeism and severe discipline incidents. Event study models include controls for student and grade-by-school-by-treatment-year cohort-by-treatment-month subsample fixed effects using the month of treatment-by-treatment year subsamples except that as in Figure 3 the month observations in and after the month when a control student is treated are no longer dropped. Rather, the model is estimated iteratively starting with the first month of positive event time, and the estimated effect of treatment on currently treated control observations is partialled out of the student's outcome in each month of positive event time. Panel A presents estimates for the female subsample, and Panel B presents estimates for the male subsample. Standard errors are bootstrapped by sampling students with replacement. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table A3
Treatment Effects by Race and Ethnicity using Constant Control Sample

	(1) Attendance	(2) Chronic Absenteeism	(3) Severe Discipline Incidents
		Panel A: Black or Hispanic	
Post Treatment	0.0497***	-0.141***	-0.0241*
	(0.00295)	(0.00823)	(0.0134)
Observations	454,780	454,780	67,511
		Panel B: Non-Hispanic White	
Post Treatment	0.0649***	-0.216***	-0.0271
	(0.0126)	(0.0369)	(0.0892)
Observations	52,055	52,055	3,596

Notes: Notes: Table presents three DiD estimates of the impact of LEAP treatment on student attendance, chronic absenteeism and severe discipline incidents. Event study models include controls for student and grade-by-school-by-treatment-year cohort-by-treatment-month subsample fixed effects using the month of treatment-by-treatment year subsamples except that as in Figure 3 the month observations in and after the month when a control student is treated are no longer dropped. Rather, the model is estimated iteratively starting with the first month of positive event time, and the estimated effect of treatment on currently treated control observations is partialled out of the student's outcome in each month of positive event time. Panel A presents estimates for the subsample of students who are either black or Hispanic, and Panel B presents estimates for the subsample of non-Hispanic white students. Standard errors are bootstrapped by sampling students with replacement. *** p<0.01, ** p<0.05, * p<0.1

Appendix Figure A1
Event Study Estimates with Constant Treatment and Control Sample

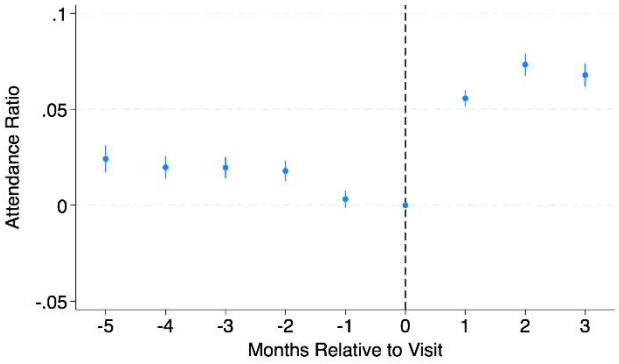


Figure A1a

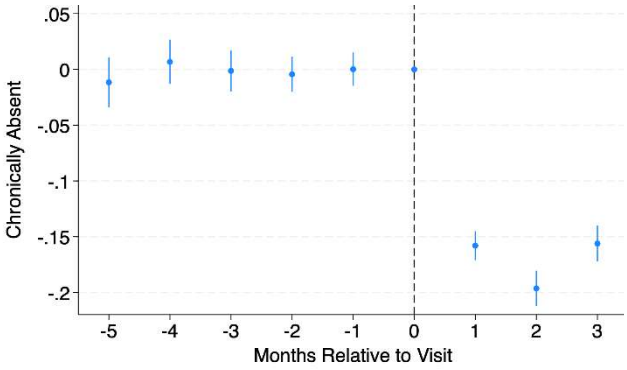


Figure A1b

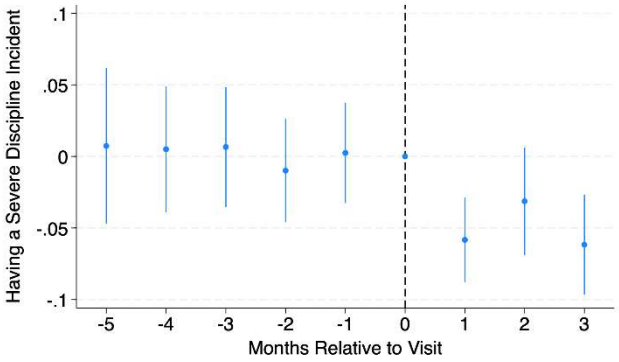


Figure A1c

Notes: Figures A1a-A1c present event study estimates for attendance, chronic absenteeism, and severe discipline incidents in a given month relative to the month of treatment. Event study models include controls for student and grade-by-school-by-treatment-year cohort-by-treatment-month subsample fixed effects using the month of treatment-by-treatment year subsamples except that as in Figure 3 the month observations in and after the month when a control student is treated are no longer dropped. Rather, the model is estimated iteratively starting with the first month of positive event time, and the estimated effect of treatment on currently treated control observations is partialled out of the student's outcome in each month of positive event time. Further, the treatment sample is held fixed during positive event time by dropping treatment month subsamples of April and May and only taking the event study out three months after treatment. The vertical bars associated with treatment effect estimates represent 95% confidence intervals based on clustered bootstrap standard errors at the student level.