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

Eric Brunner, Shangyue Jiang & Stephen Ross

Abstract

Chronic absenteeism spiked during the COVID-19 pandemic and remains one of the largest challenges facing states and school districts across the United States. To address student absenteeism and disengagement from school, the state of Connecticut launched the Learner Engagement and Attendance Program (LEAP) in April of 2021. 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 Connecticut's 15 most economically disadvantaged school districts. We use student-level administrative data from 2021–22 through 2023-2024 to examine the impact of the LEAP program on student attendance, chronic absenteeism, disciplinary incidents, and student achievement. Using event study and difference-in-differences identification strategies that exploit the staggered timing of treatment across schools and districts, we find that students who received the LEAP intervention experienced large and sustained increases in attendance and subsequent declines in chronic absenteeism and disciplinary incidents. Five months after treatment, students that participated in the LEAP program experienced approximately a 7-percentage point increase in attendance rates, a 20-percentage point decline in chronic absenteeism rates, and a 10-percentage point decline in the number of disciplinary incidents. These effects were relatively broad based, with similar impacts by gender, race and ethnicity, and free or reduced-price lunch status. Overall, our analysis suggests the LEAP program is a promising model for re-engaging students who are chronically absent and fostering significant improvements in student attendance and discipline.

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* Eric J. Brunner, School of Public Policy, University of Connecticut, 10 Prospect Street, 4th Floor, Hartford, CT 06103, eric.brunner@uconn.edu; Shangyue Jiang, Department of Economics, University of Connecticut, 341 Mansfield Road, Unit 1063, Storrs, CT 06269-1063, shangyue.jiang@uconn.edu. Stephen Ross, Department of Economics, University of Connecticut, 341 Mansfield Road, Unit 1063, Storrs, CT 06269-1063, stephen.l.ross@uconn.edu.

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 chronic absenteeism increased among students from all socio-economic and racial and ethnic groups, the increase was particularly large among students of color and those from disadvantaged backgrounds (Malkus 2022; Dewey et al. 2025; Dee 2024). Perhaps not surprisingly, the spike in chronic absenteeism has been accompanied by significant learning loss that has persisted over time with learning losses being particularly acute among students of color and those from disadvantaged backgrounds (Betthäuser et al. 2023; Brunner, et al. 2025; 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 2024-2025 school year, chronic absenteeism rates were still at 17.2 percent, a 43 percent increase from the pre-pandemic level. As documented by Brunner, Ross & Wang (2025) the sustained increase in chronic absenteeism in Connecticut has been accompanied by steep learning losses that have been particularly acute among students of color and those from disadvantaged backgrounds. 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 study 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 Connecticut's 15 most economically disadvantaged 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 2023-2024 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.

We find first that students who received the LEAP intervention experienced large and sustained increases in attendance. Five months after treatment, students that participated in the LEAP program experienced approximately a 7-percentage point increase in attendance rates and a 20-percentage point decline in chronic absenteeism rates. 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 discipline related student behavior, regardless of whether the behavior led to a specific disciplinary action. We find that the likelihood of experiencing any disciplinary incident decreased by approximately 6 percentage points in the month immediately following the first LEAP visit and continued to decline in subsequent months to approximately 10 percentage points. Similarly, the likelihood of experiencing a severe disciplinary incident fell by approximately 7 percentage points in the month after treatment and continued to decline to approximately 9 percentage points in the following months. These results are robust to the exclusion or inclusion of student level controls, and to the use of propensity score overlap weights to adjust for potential imbalance between treated and untreated students that might arise due to the exclusion of newly treated control observations in the post-treatment period. Our results are also relatively broad based, with similar impacts by gender, race and ethnicity, and free or reduced price lunch status. 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). Finally, we find little evidence that the LEAP intervention impacted student test scores for either ELA or mathematics.[1]

Our paper makes several contributions to the literature. First, and perhaps most importantly, to our knowledge 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

[1] 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.

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.[2]

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. (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" they developed to encourage daily attendance and provide 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 studied in the paper, 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.[3] 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

[2] 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.
[3] In related work, Lee, Loeb & Robinson (2025) 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.

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 score, high-school graduation rates, and adult earnings.[4]

Third, our paper is also related to the literature on the impact of attendance on student outcomes. A relatively large body of literate 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 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.[5]

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.[6] 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 color and students from disadvantaged backgrounds, were significantly more likely to have students that

[4] 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.

[5] 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.

[6] 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.

were chronically absent than students attending more affluent districts with predominantly non-Hispanic white students. To address student absenteeism and disengagement from school in predominantly urban districts with large shares of disadvantaged students and students of color, 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.[7] 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 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

[7] Attendance Works is a national organizations dedicated 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.[8] 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 disciplinary incident or a severe discipline incident in each month. Given district discretion in reporting minor incidents, severe discipline incidents provide the best indicator of behavioral effects of LEAP treatment, but for completeness we also present estimates for any disciplinary incident.

We then merged the attendance, disciplinary, 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) 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,

[8] 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.

administrative, and academic performance data for each student. We standardize student's Smarter Balanced Assessment test scores by school year and grade to have a mean of zero and standard deviation of one for each grade and each school year.

We excluded the New Haven Public School District from our analysis for two primary reasons. First, unlike other participating districts, New Haven 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).[9] 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 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). Perhaps not surprisingly, students treated earlier in the school year have math and ELA test scores that are much more comparable to students treated later in the school year than to students never treated by the LEAP program. Students treated earlier in the school year are also more similar to students treated later in the school year in terms of race and ethnicity, free or reduced-price lunch status and special education status.

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 academic performance. Given strong selection on observables into participation in LEAP and a program

[9] 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.

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

$$y_{igst} = \sum_{j=-5}^{5} \gamma_j T_{j,it} + \delta_i + \lambda_{gst} + \eta_{igst} \quad (1)$$

where y_{igst} denotes an outcome of interest for student i, in grade g, attending school s, in month t. $T_{j,it}$ represents a series of monthly lead and lag indicators for when student i was treated by LEAP, δ_i is a vector of student fixed effects, λ_{gst} is a vector of grade-by-school-by-month fixed effects, and η_{igst} is a random disturbance term. We re-center the treatment timeline such that $T_{0,it}$ corresponds to the month in which student i received their first LEAP visit. We included indicator variables for 1 to 5 months prior to a LEAP visit ($T_{-5,it} - T_{-1,it}$) and 1 to 5 months after a LEAP visit ($T_{1,it} - T_{5,it}$). The omitted category is the month of the LEAP visit conducted, $T_{0,it}$.

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 visit on our outcomes of interest in each month from $t_{-5}$to t_5. 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 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.

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 datasets that include observations from a cohort of students that were treated by LEAP in the same month and school year and all students that were treated by LEAP in a later month. 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, control observations must be dropped during the post-period as additional students are treated. We then appended (stack) these cohort specific datasets and estimate models similar to equation (1), except we replaced the student fixed

effects with student-by-cohort fixed effects and replace the grade-by-school-by-month fixed effects with grade-by-school-by-month-by-cohort fixed effects. To obtain an average treatment effect across the three school years, we further pool data from all years and extended the fixed-effect structure by replacing the student-by-cohort fixed effects with student-by-cohort-by-school-year fixed effects, and the grade-by-school-by-month-by-cohort fixed effects with grade-by-school-by-month-by-cohort-by-school-year fixed effects. Manually stacking to address bias from staggered roll-out facilitates our robustness test that uses propensity score weights to balance the data separately for each observation month by treatment timing cohort.

We complement the event-study specification with a standard difference-in-differences model to increase our precision by pooling estimates within both the pre- and post-LEAP visit period:

$$y_{igst} = \beta_0 + \beta_1(Treat_{it} \cdot Post_t) + \beta_2(Treat_{it} \cdot Trend_t) + \beta_3(Treat_{it} \cdot Trend_t \cdot Post_t) + \boldsymbol{X_i}\theta_t + \delta_i + \lambda_{gst} + \epsilon_{igst} \quad (2)$$

where $Treat_{it}$ is an indicator variable that equals one for students that were treated by the LEAP intervention, $Treat_{it} \cdot Post_t$ denotes the interaction between the treatment indicator and a post-treatment indicator that equals one for all months following the treatment month, $Treat_{it} \cdot Trend_t$ denotes the interaction between the treatment indicator and a linear time trend that counts the number of months relative to treatment timing, and $Treat_{it} \cdot Trend_t \cdot Post_t$ denotes the interaction between the treatment indicator, the linear time trend, and the post-treatment indicator. 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. Because these student characteristics are time-invariant we interact them with a linear time trend, θ_t to allow for differential trending in our outcome of interest over time by these characteristics. Finally, ϵ_{igst} 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, $Treat_{it} \cdot Post_t$, 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. Our decision to include a standard DiD "jump" variable, a linear treatment trend variable, and a linear post treatment trend in (2) stems from the fact that the impact of LEAP on our outcomes of interest is likely to evolve over time.

To examine the effect of LEAP participation on academic performance, we employ an event-study specification that compares treatment effects across the month of participation relative to the administration of standardized tests. Specifically, we estimate a model of the form

$$y_{igs\tau} = \kappa TY_{\tau} + \sum_{m=-7}^{2} \alpha_m D_{m,i\tau} + \delta_i + \lambda_{gst} + \upsilon_{igs\tau} \quad (3)$$

where $y_{igs\tau}$ denotes the test score of students i in grade g, attending school s in school year τ, TY_{τ} equals one if the observation corresponds to the student's treatment year, $D_{m,i\tau}$ is a vector of monthly lead and lag indicators for the student's participation in LEAP relative to April in the treatment year, $\upsilon_{igs\tau}$ is a random disturbance term, and all other terms are as defined in equation (1). We cluster the standard errors at the student level as specified in equation (1).

The coefficients α_m measure the differential treatment effects for students first exposed to LEAP in month m relative to those treated in April. By construction, the sample includes two observations per student—the treatment year and the preceding year—allowing for the inclusion of individual fixed effects. Equation (3) allows us to test 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., May or 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. Similar to our analysis of attendance and disciplinary outcomes, we pool data from all years and extend the fixed-effect structure by replacing the student fixed effects with student-by-school-year fixed effects, and the grade-by-school-by-month fixed effects with grade-by-school-by-month-by-school-year fixed effects.

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 and those after the April period:

$$y_{igs} = \phi_0 + \phi_1(Pre_{i\tau} \cdot TY_{\tau}) + X_i TY_{\tau} + \delta_i + \lambda_{gst} + \nu_{igs\tau} \quad (4)$$

where $Pre_{i\tau}$ is an indicator variable that equals one if student i received their first LEAP visit before April, $Pre_{i\tau} \cdot TY_{\tau}$ denotes the interaction between the pre-April indicator and the treatment year indicator, $\nu_{igs\tau}$ is a random disturbance term, and all other terms are as defined in equation (1). We interact the

control variables, $\boldsymbol{X_i}$, with the treatment year indicator to allow for differences in the outcome of interest before and after treatment across these characteristics. 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.

V. Results

We begin our analysis by estimating the impact of LEAP participation on students' attendance rates and disciplinary 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 and 1b illustrate the estimated effects of LEAP participation on student attendance rates (Figure 1a) and chronic absenteeism (Figure 1b). The horizontal axis in both Figure 1a and 1b 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 relative month of treatment (i.e., the first LEAP visit). The vertical axis in Figure 1a represents the change in attendance rates 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 1b represents the corresponding change in the likelihood of chronic absenteeism for treated students relative to the control group.

In both Figure 1a and 1b, there is no evidence of differential pre-treatment trends between students treated by LEAP and those in the control group prior to the first LEAP visit (denoted by the vertical dashed line). The estimated pre-treatment effects are close to zero and statistically insignificant, suggesting that the parallel trends assumption holds and that the control group provides a credible counterfactual for what would have occurred in the absence of LEAP treatment.

In contrast, following treatment, there is clear evidence of improvement in student attendance and reductions in chronic absenteeism. As shown in Figure 1a, attendance rates increase by approximately 5 percentage points in the month immediately following the first LEAP visit and continue to rise in subsequent months, reaching an increase of about 7 percentage points. Figure 1b shows a corresponding decline in chronic absenteeism of roughly 14 percentage points in the month after treatment, which deepens to approximately 19 percentage points over the following months. These estimates are sizable relative to the treatment sample mean attendance rate of 81% and average monthly share of students chronically absent of 54%.

We next examine whether participation in LEAP affected students' likelihood of involvement in disciplinary incidents. As before, we estimate equation (1) using the monthly disciplinary dataset and plot the estimated event-time coefficients (γ_j) along with their corresponding 95 percent confidence intervals.

Figures 1c and 1d present the estimated effects of LEAP participation on the likelihood of a student being involved in any disciplinary incident (Figure 1c) or a severe disciplinary incident (Figure 1d). The horizontal axis and vertical dashed lines in Figure 1c and Figure 1d follow the same structure as Figure 1a and Figure 1b. The vertical axis in Figure 1c represents the change in the likelihood of a student being involved in any disciplinary incident for students treated by LEAP relative to those in the control group, while the vertical axis in Figure 1d represents the corresponding change in the likelihood of a student being involved in a severe disciplinary incident.

Similar to Figures 1a and 1b., there is no evidence of differential pre-treatment trends between students treated by LEAP and those in the control group prior to the first LEAP visit, once again suggesting that the parallel trends assumption holds and that the control group provides a credible counterfactual for what would have occurred in the absence of LEAP treatment. In contrast, there is clear evidence of a decline in students' likelihood of involvement in both disciplinary and severe disciplinary incidents post treatment. As shown in Figure 1c, the likelihood of experiencing any disciplinary incident decreases by approximately 6 percentage points in the month immediately following the first LEAP visit and continues to decline in subsequent months, reaching a reduction of approximately 10 percentage points. Similarly, Figure 1d shows a decline in the likelihood of a severe disciplinary incident of roughly 7 percentage points in the month after treatment, which deepens to approximately 9 percentage points in the following months. Again, these effects are sizable relative to the treatment group means of 0.14. and 0.21 for severe disciplinary and any disciplinary incidents, respectively. As noted above, the effects for severe disciplinary incidents provide a much better indication of the behavioral effects of LEAP because very few minor disciplinary incidents are reported to the state unless reporting is mandated because it led to a suspension.

We next examine whether participation in LEAP affected students' academic performance using the event-study specification given by equation (3). Figures 2a and 2b illustrate the estimated effects of LEAP participation on ELA test scores (Figure 2a) and math test scores (Figure 2b). The horizontal axis in both Figures 2a and 2b represents the month of LEAP participation relative to April, the reference 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). The vertical dashed line indicates the reference month for testing. The vertical axis in Figure 2a 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 2b 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 larger enough that we cannot rule out modest size effects on test scores,

Difference-in-Difference Estimates

We present the difference-in-differences (DiD) estimates of the effect of LEAP participation on students' attendance rates and chronic absenteeism in Table 2. Columns 1 and 4 present baseline estimates from equation 2, while columns 2 and 5 report results from the same specification augmented with student attributes (gender, race and ethnicity, free or reduced price lunch status, ELL status, and special education status) interacted with a linear time trend to allow for differences in outcome trajectories by student attributes. Columns 3 and 6 present results based on equation 2 estimated with propensity score overlap weights to adjust for potential imbalance between treated and untreated students. Specifically, we first divided the students into cells based on the month and year of treatment, so that each cell contains both the students that were treated by the LEAP program in a given month and year and all the students not yet treated by the LEAP program by that month and year. We then further break each of these cells into smaller cells based on the month of the observation including in each cell the observation for each treated student and each control student not yet treated by the month of that observation. We then estimated each student's probability of receiving a LEAP visit separately for each treatment timing by month of observation cell, using a logistic regression model that included a student's gender, race, free lunch status, ELL status, and special education status as covariates. By estimating separate treatment models by event timing and observation month, the resulting propensity score weights assure balance between treatment and control persists, even as the control group changes in the post-treatment period as additional students are treated. Rather than inverse propensity score weights, we utilize overlap weights, defined as $\mathrm{w_i} = 1 - \hat{\mathrm{p}}_\mathrm{i}$ for treated students and $w_i = \hat{p}_i$ for untreated students, where $\hat{p}_i$ denotes the estimated propensity score (Li et al. 2018). Compared to inverse propensity score weights that yield a uniform weighted density and place much larger weight on low probability events, overlap weights yield a density over treatment probability that has substantially more mass in the regions of common support,

and so is preferable when examining whether estimate magnitudes are relatively stable to the use of propensity scores weights as a non-parametric control for observables.

Panel A of Table 2 reports estimates from the specification given by equation 2, which includes only the interaction between the treatment indicator and the post-treatment indicator. Column 1 shows that LEAP participation is associated with a 4.97 percentage point increase in student attendance rates. After incorporating student attributes in column 2, the estimated effect remains virtually unchanged. Column 3 presents results estimated using overlap propensity score weights, which yield an estimated 4.76 percentage point increase in attendance rates—very similar to the unweighted estimates.

Columns 4-6 present the corresponding estimates for chronic absenteeism. Columns 4 and 5 show that LEAP participation is associated with a 15.9 percentage point reduction in the likelihood of chronic absenteeism, both with and without controls for student attributes. Column 6 reports results based on propensity score overlap weights, indicating a 15.4 percentage point reduction in chronic absenteeism, again closely aligned with the unweighted estimates. The consistency of results across specifications and weighting approaches suggests that the estimated treatment effects are robust to the inclusion of additional controls and weighting adjustments. This stability supports the interpretation that the timing of LEAP visits was largely exogenous, allowing the difference-in-differences estimates to approximate the conditions of a randomized experiment.

Panel B of Table 2 reports estimates from the full specification in equation 2. Column 1 shows that LEAP participation is associated with an immediate 4.12 percentage point increase in student attendance rates, followed by an additional increase of approximately 0.5 percentage points in each subsequent month. After accounting for student controls in column 2, the estimated effects remain virtually unchanged. Column 3 presents results estimated using propensity score overlap weights, indicating an immediate 3.89 percentage point increase in attendance rates and a subsequent monthly increase of about 0.6 percentage points, again, very similar to the unweighted estimates.

Columns 4-6 present the corresponding estimates for chronic absenteeism. Columns 4 and 5 show that LEAP participation is associated with an immediate 13.6 percentage point reduction in the likelihood of chronic absenteeism, followed by an additional decline of roughly 1 percentage point per month, both without and with student-controls linear trends. Column 6 reports the weighted results using propensity score overlap weights, which indicate a 12.8 percentage point immediate reduction and a further 1.1 percentage point decline per month, closely aligned with the unweighted estimates. The findings for attendance rates and chronic absenteeism in Panel B are consistent with the dynamic patterns depicted in the event-study diagrams presented in Fig. 1a and Fig. 1b.

In Table 3 we present DiD estimates of the effect of LEAP participation on students' likelihood of involvement in disciplinary incidents. Columns 1 and 4 present baseline estimates from equation 2,

while columns 2 and 5 report results from the same specification augmented with student attributes interacted with a linear trend to account for individual differences in outcome trajectories. Columns 3 and 6 present results estimated with overlap propensity score weights to adjust for potential imbalance between treated and untreated students.

Similar to Table 2, Panel A reports estimates based on a DiD specification that includes only the interaction between the treatment indicator and the post-treatment indicator. Column 1 shows that LEAP participation is associated with a 5.7 percentage point reduction in students' likelihood of involvement in disciplinary incidents. After incorporating student attributes in column 2, the estimated effect remains virtually unchanged, with a 5.6 percentage point decrease. Column 3 presents results estimated using propensity score overlap weights, which yield an estimated 5.7 percentage point reduction in disciplinary incidents, again, very similar to the unweighted estimates.

Columns 4-6 present the corresponding estimates for students' likelihood of involvement in severe disciplinary incidents. Column 4 shows that LEAP participation is associated with a 6.6 percentage point reduction in the likelihood of involvement in a severe disciplinary incident while column 5 shows a nearly identical estimate of 6.5 percentage points once we add student attributes. Column 6 reports results that utilize propensity score overlap weights, indicating a 6.7 percentage point reduction in the likelihood of severe disciplinary incidents, again closely aligned with the unweighted estimates.

Panel B presents estimates based on the full specification given in equation (2) and the results are consistent with the dynamic patterns depicted in the event-study diagrams presented in Figure 1c and Figure 1d. For example, column 1 shows that LEAP participation is associated with an immediate 3.9 percentage point decrease in the likelihood of involvement in disciplinary incidents, followed by an additional decrease of approximately 1.9 percentage points in each subsequent month. After controlling for student attributes in column 2, the estimated effects remain virtually unchanged. Column 3 reports results that use propensity score overlap weights and show an immediate 3.9 percentage point decrease in the likelihood of involvement in disciplinary incidents and a subsequent monthly decrease of about 1.6 percentage points. Although not statistically significant, the magnitude is very similar to the unweighted estimates. Columns 4-6 present the corresponding estimates for severe disciplinary incidents. Columns 4 and 5 show that LEAP participation is associated with an immediate 6.2 percentage point reduction in the likelihood of severe disciplinary incidents, followed by an additional decline of roughly 0.4 percentage point per month. The results in column 6 that utilize propensity score overlap weights, once again closely aligned with the unweighted estimates.

Tables 1A, 2A, and 3A of the Appendix present DiD results that mirror the attendance and chronic absenteeism results presented in Panel B of Table 2 except we split the sample by gender (Table 1A), free or reduced-price lunch status (Table 2A) and race and ethnicity (Table 3A). We present similar

heterogeneity results for discipline in Tables 4A, 5A, and 6A. Overall, our attendance, chronic absenteeism, and discipline results are relatively broad based with similar treatment effects across gender, free or reduced-price lunch status, and race and ethnicity.

Finally, we present DiD estimates of the effect of LEAP participation on academic performance in Table 4. Columns 1 and 4 present baseline estimates from equation 4, while columns 2 and 5 report results from the same specification augmented with student attributes interacted with a linear trend. None of the estimates reported in Table 4 are statistically significant for either ELA or mathematics and the point estimates are very small in magnitude (e.g. a 0.009 standard deviation increase in ELA scores in column 1). The results are consistent with the event-study results presented in Figures 2a and 2b, which show no systematic relationship between LEAP participation and test performance. However, again with standard errors of $1/20^{th}$ of a standard deviation, we cannot rule out modest effects 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 approximately a 7-percentage point increase in attendance rates, a 20-percentage point decline in chronic absenteeism rates and a 10-percentage point decline in the number of disciplinary incidents. These effects are robust to the inclusion of student controls and propensity score overlap weighting, suggesting that they are not driven by pre-existing differences between treated and untreated students. In contrast, estimated effects on standardized ELA and mathematics test scores are small and statistically insignificant, although given our reliance on

annual test score data and the resulting larger standard errors we cannot rule out modest effects on test scores.

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Table 1
Summary Statistics

	(1)	(2)	(3)	(4)
	Treated	Untreated	Treated Early	Treated Late
Attendance Ratio	0.807	0.902	0.775	0.835
Chronic Absenteeism (0, 1)	0.539	0.324	0.592	0.491
Any Disciplinary Incidents (0, 1)	0.214	0.183	0.211	0.218
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
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. Columns 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
DiD Estimates: Attendance

	Attendance			Chronic Absenteeism		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	With Controls	With Overlap Weights	Baseline	With Controls	With Overlap Weights
Post Treatment	0.0497***	0.0498***	0.0476***	-0.159***	-0.159***	-0.154***
	(0.00260)	(0.00259)	(0.00267)	(0.00676)	(0.00675)	(0.00668)
Observations	388,305	388,305	388,074	388,305	388,305	388,074
R-squared	0.744	0.744	0.771	0.527	0.527	0.551
Panel B: Difference-in-Differences with Trends in Event Timing						
Post Treatment	0.0412***	0.0412***	0.0389***	-0.136***	-0.136***	-0.128***
	(0.00283)	(0.00283)	(0.00293)	(0.00772)	(0.00772)	(0.00747)
Trend Treatment	-0.00461***	-0.00462***	-0.00463***	0.00109	0.00113	0.00121
	(0.000727)	(0.000726)	(0.000727)	(0.00200)	(0.00199)	(0.00199)
Trend Post Treatment	0.0108***	0.0109***	0.0111***	-0.0121***	-0.0123***	-0.0134***
	(0.00125)	(0.00125)	(0.00131)	(0.00311)	(0.00311)	(0.00308)
Observations	388,305	388,305	388,074	388,305	388,305	388,074
R-squared	0.744	0.744	0.771	0.527	0.527	0.551

Notes: Table presents DiD estimates of the impact of LEAP treatment on student attendance and chronic absenteeism. Panel A present one paramter DiD estimates where Post Treatment is an indicator for treated students in the post treatment period. Panel B presents three parameter DiD estimates that allow the impact of treatment to grow gradually overtime. Trend Treatment is the interaction of a linear time trend with an indicator for treated students. Trend Post Treatment is the interaction of a linear time trend with the post treatment indicator. Columns 1 and 4 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. Columns 3 and 6 add propensity score overlap weights. All specifications include student and grade-by-school-by-month fixed effects. Standard errors clustered at the student level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3
DiD Estimates: Disciplinary Incidents

	Any Incident			Severe Incident		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	With Controls	With Overlap Weights	Baseline	With Controls	With Overlap Weights
Post Treatment	-0.0569***	-0.0560***	-0.0574***	-0.0658***	-0.0654***	-0.0670***
	(0.0204)	(0.0207)	(0.0215)	(0.0173)	(0.0174)	(0.0177)
Observations	36,713	36,713	35,759	36,713	36,713	35,759
R-squared	0.493	0.493	0.519	0.466	0.466	0.496
Panel B: Difference-in-Differences with Trends in Event Timing						
Post Treatment	-0.0390	-0.0388	-0.0392	-0.0620***	-0.0627***	-0.0597***
	(0.0255)	(0.0255)	(0.0260)	(0.0221)	(0.0220)	(0.0221)
Trend Treatment	0.00667	0.00653	0.00468	0.00545	0.00567	0.00447
	(0.00617)	(0.00616)	(0.00626)	(0.00527)	(0.00523)	(0.00527)
Trend Post Treatment	-0.0187*	-0.0181*	-0.0160	-0.00979	-0.00955	-0.0102
	(0.0107)	(0.0108)	(0.0110)	(0.00904)	(0.00901)	(0.00922)
Observations	36,713	36,713	35,759	36,713	36,713	35,759
R-squared	0.493	0.493	0.519	0.466	0.466	0.496

Notes: Table presents DiD estimates of the impact of LEAP treatment on the probability of a student being involved in any disciplinary incidents and the probability of being involved in a severe disciplinary incidents. Panel A present one paramter DiD estimates where Post Treatment is an indicator for treated students in the post treatment period. Panel B presents three parameter DiD estimates that allow the impact of treatment to grow gradually overtime. Trend Treatment is the interaction of a linear time trend with an indicator for treated students. Trend Post Treatment is the interaction of a linear time trend with the post treatment indicator. Columns 1 and 4 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. Columns 3 and 6 add propensity score overlap weights. All specifications include student and grade-by-school-by-month fixed effects. Standard errors clustered at the student level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4
DiD Estimates: Student Achievement

	ELA			Math		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	With Controls	With Overlap Weights	Baseline	With Controls	With Overlap Weights
Post Treatment	0.00888	0.000344	0.00811	-0.0303	-0.0485	-0.0399
	(0.0499)	(0.0500)	(0.0494)	(0.0487)	(0.0497)	(0.0484)
Observations	10,771	10,771	10,743	10,499	10,499	10,469
R-squared	0.926	0.926	0.926	0.928	0.929	0.928

Notes: Table presents DiD estimates of the impact of LEAP treatment on student achievement. Columns 1 and 4 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. Columns 3 and 6 add propensity score overlap weights. All specifications include student and grade-by-school-by-month 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

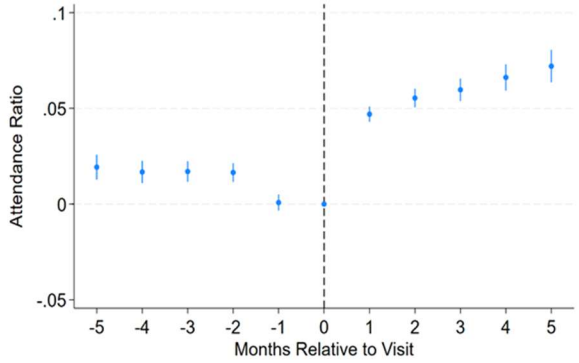


Figure 1a

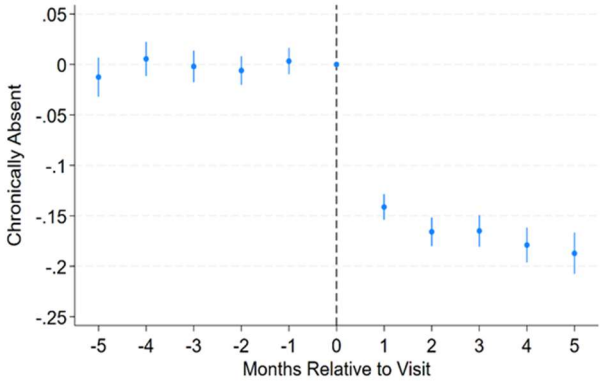


Figure 1b

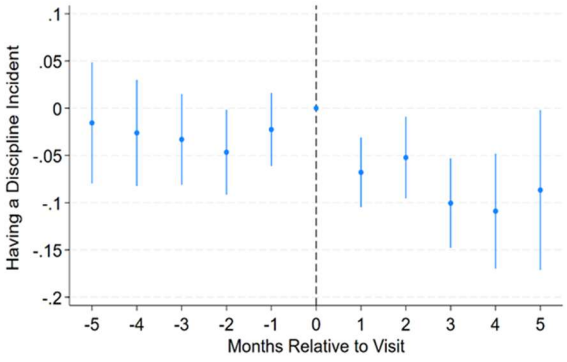


Figure 1c

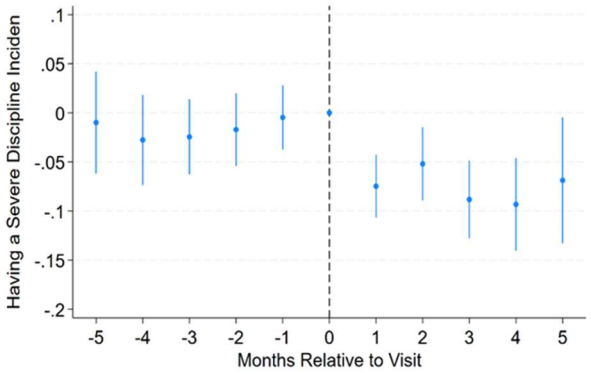


Figure 1d

Figure 2
Event Study Estimates: Student Achievement

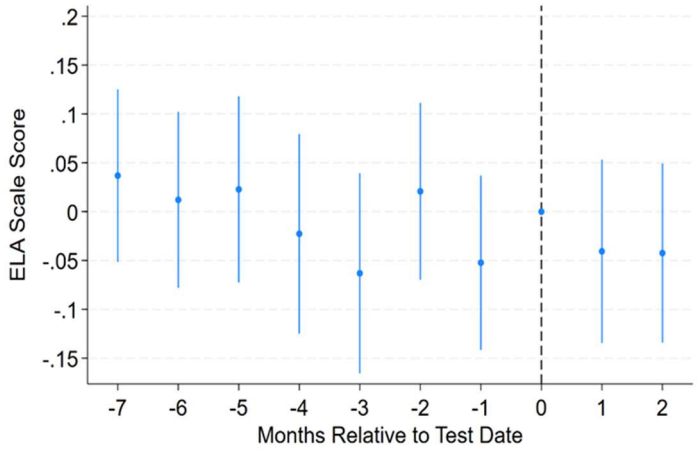


Figure 2a

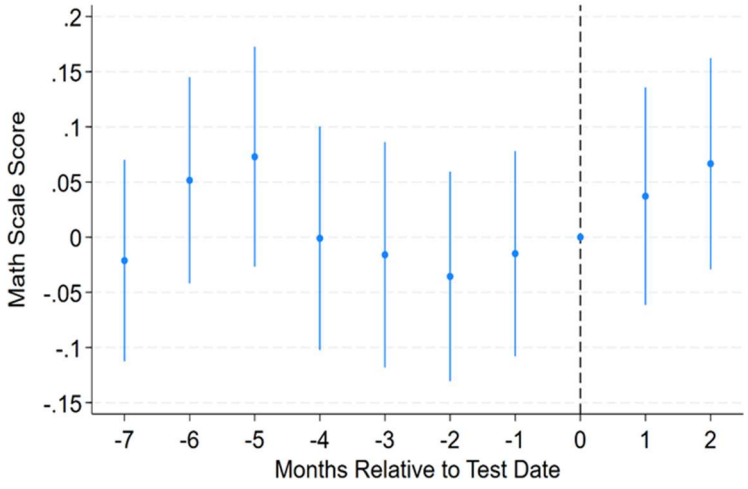


Figure 2b

Appendix

Table 1A
DiD Estimates: Attendance Heterogeneity by Free Lunch Status

	Attendance			Chronic Absenteeism		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	With Controls	With Overlap Weights	Baseline	With Controls	With Overlap Weights
	Panel A: Free or Reduced Lunch					
Post Treatment	0.0420***	0.0419***	0.0402***	-0.133***	-0.133***	-0.128***
	(0.00320)	(0.00320)	(0.00328)	(0.00842)	(0.00842)	(0.00812)
Trend Treatment	-0.00448***	-0.00447***	-0.00468***	-0.000968	-0.000949	-0.000578
	(0.000833)	(0.000833)	(0.000834)	(0.00225)	(0.00225)	(0.00225)
Trend Post Treatment	0.0104***	0.0104***	0.0106***	-0.00900***	-0.00909***	-0.00944***
	(0.00141)	(0.00141)	(0.00146)	(0.00346)	(0.00346)	(0.00341)
Observations	308,601	308,601	308,496	308,601	308,601	308,496
R-squared	0.748	0.749	0.776	0.534	0.535	0.561
	Panel B: Not Free Lunch					
Post Treatment	0.0354***	0.0356***	0.0321***	-0.143***	-0.142***	-0.124***
	(0.00771)	(0.00769)	(0.00761)	(0.0250)	(0.0250)	(0.0242)
Trend Treatment	-0.00651***	-0.00631***	-0.00592***	0.0112**	0.0114**	0.0101*
	(0.00202)	(0.00203)	(0.00202)	(0.00562)	(0.00562)	(0.00568)
Trend Post Treatment	0.0151***	0.0149***	0.0168***	-0.0325***	-0.0332***	-0.0391***
	(0.00365)	(0.00367)	(0.00378)	(0.00973)	(0.00974)	(0.00978)
Observations	61,259	61,259	61,160	61,259	61,259	61,160
R-squared	0.769	0.769	0.805	0.586	0.586	0.62

Notes: Table presents three parameter DiD estimates of the impact of LEAP treatment on student attendance and chronic absenteeism that allow the impact of treatment to grow gradually overtime. Trend Treatment is the interaction of a linear time trend with an indicator for treated students. Trend Post Treatment is the interaction of a linear time trend with the post treatment indicator. Results are presented separately by student free or reduced-price lunch status. Columns 1 and 4 present baseline estimates with no controls. Columns 2 and 4 add student level controls for gender, special education status, English language learner status, and indicators for Black, Hispanic and Asian or Pacific Islander students. Columns 3 and 6 add propensity score overlap weights. All specifications include student and grade-by-school-by-month fixed effects. Standard errors clustered at the student level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2A
DiD Estimates: Attendance Heterogeneity by Gender

	Attendance			Chronic Absenteeism		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	With Controls	With Overlap Weights	Baseline	With Controls	With Overlap Weights
			Panel A: Female			
Post Treatment	0.0404***	0.0404***	0.0387***	-0.145***	-0.145***	-0.137***
	(0.00443)	(0.00443)	(0.00447)	(0.0120)	(0.0120)	(0.0114)
Trend Treatment	-0.00657***	-0.00660***	-0.00664***	0.00594*	0.00607*	0.00681**
	(0.00110)	(0.00109)	(0.00109)	(0.00312)	(0.00311)	(0.00308)
Trend Post Treatment	0.0119***	0.0120***	0.0122***	-0.0163***	-0.0165***	-0.0182***
	(0.00190)	(0.00190)	(0.00194)	(0.00480)	(0.00480)	(0.00468)
Observations	174,065	174,065	173,862	174,065	174,065	173,862
R-squared	0.765	0.766	0.796	0.563	0.563	0.593
			Panel B: Male			
Post Treatment	0.0432***	0.0432***	0.0413***	-0.133***	-0.133***	-0.129***
	(0.00424)	(0.00424)	(0.00429)	(0.0116)	(0.0116)	(0.0111)
Trend Treatment	-0.00344***	-0.00340***	-0.00327***	-0.00312	-0.00335	-0.00238
	(0.00112)	(0.00112)	(0.00112)	(0.00294)	(0.00294)	(0.00294)
Trend Post Treatment	0.0103***	0.0103***	0.0103***	-0.00764	-0.00749	-0.00911**
	(0.00192)	(0.00192)	(0.00195)	(0.00468)	(0.00469)	(0.00459)
Observations	189,267	189,267	189,202	189,267	189,267	189,202
R-squared	0.760	0.760	0.790	0.562	0.562	0.592

Notes: Table presents three parameter DiD estimates of the impact of LEAP treatment on student attendance and chronic absenteeism that allow the impact of treatment to grow gradually overtime. Trend Treatment is the interaction of a linear time trend with an indicator for treated students. Trend Post Treatment is the interaction of a linear time trend with the post treatment indicator. Results are presented separately by gender. Columns 1 and 4 present baseline estimates with no controls. Columns 2 and 4 add student level controls for free or reduced price lunch status, special education status, English language learner status, and indicators for Black, Hispanic and Asian or Pacific Islander students. Columns 3 and 6 add propensity score overlap weights. All specifications include student and grade-by-school-by-month fixed effects. Standard errors clustered at the student level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3A
DiD Estimates: Attendance Heterogeneity by Race & Ethnicity

	Attendance			Chronic Absenteeism		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	With Controls	With Overlap Weights	Baseline	With Controls	With Overlap Weights
	Panel A: Black or Hispanic					
Post Treatment	0.0398***	0.0398***	0.0379***	-0.127***	-0.127***	-0.122***
	(0.00316)	(0.00316)	(0.00322)	(0.00845)	(0.00845)	(0.00821)
Trend Treatment	-0.00462***	-0.00463***	-0.00462***	7.83e-06	2.92e-05	4.45e-05
	(0.000813)	(0.000812)	(0.000813)	(0.00217)	(0.00217)	(0.00217)
Trend Post Treatment	0.0111***	0.0111***	0.0111***	-0.0113***	-0.0114***	-0.0117***
	(0.00140)	(0.00140)	(0.00144)	(0.00339)	(0.00340)	(0.00335)
Observations	323,580	323,580	323,580	323,580	323,580	323,580
R-squared	0.750	0.750	0.777	0.537	0.537	0.563
	Panel B: White					
Post Treatment	0.0559***	0.0556***	0.0577***	-0.185***	-0.183***	-0.181***
	(0.0138)	(0.0138)	(0.0141)	(0.0393)	(0.0394)	(0.0383)
Trend Treatment	-0.00630*	-0.00621*	-0.00695*	0.0123	0.0118	0.0150
	(0.00353)	(0.00353)	(0.00369)	(0.00973)	(0.00978)	(0.00985)
Trend Post Treatment	0.00673	0.00669	0.00789	-0.0109	-0.0111	-0.0171
	(0.00591)	(0.00592)	(0.00599)	(0.0159)	(0.0158)	(0.0154)
Observations	25,971	25,971	25,971	25,971	25,971	25,971
R-squared	0.797	0.797	0.830	0.621	0.622	0.652

Notes: Table presents three parameter DiD estimates of impact of LEAP treatment on student attendance and chronic absenteeism that allow the impact of treatment to grow gradually overtime. Trend Treatment is the interaction of a linear time trend with an indicator for treated students. Trend Post Treatment is the interaction of a linear time trend with the post treatment indicator. Results are presented separately for Black and Hispanic students (top panel) and White students (bottom panel). Columns 1 and 4 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. Columns 3 and 6 add propensity score overlap weights. All specifications include student and grade-by-school-by-month fixed effects. Standard errors clustered at the student level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4A
DiD Estimates: Disciplinary Incidents Heterogeneity by Free Lunch Status

	Any Incident			Severe Incident		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	With Controls	With Overlap Weights	Baseline	With Controls	With Overlap Weights
	Panel A: Free or Reduced Lunch					
Post Treatment	-0.0422	-0.0410	-0.0367	-0.0605**	-0.0608**	-0.0541**
	(0.0275)	(0.0274)	(0.0274)	(0.0237)	(0.0236)	(0.0234)
Trend Treatment	0.00803	0.00708	0.00404	0.00751	0.00711	0.00508
	(0.00667)	(0.00662)	(0.00668)	(0.00584)	(0.00575)	(0.00578)
Trend Post Treatment	-0.0192*	-0.0180	-0.0141	-0.0105	-0.0101	-0.00931
	(0.0115)	(0.0116)	(0.0117)	(0.00979)	(0.00978)	(0.00989)
Observations	30,161	30,161	29,442	30,161	30,161	29,442
R-squared	0.504	0.505	0.531	0.472	0.473	0.503
	Panel B: Not Free Lunch					
Post Treatment	-0.0179	-0.0320	-0.0155	-0.166	-0.175	-0.197*
	(0.115)	(0.118)	(0.117)	(0.107)	(0.109)	(0.108)
Trend Treatment	0.0140	0.00861	0.0132	0.00320	0.00312	0.0110
	(0.0253)	(0.0243)	(0.0266)	(0.0193)	(0.0204)	(0.0194)
Trend Post Treatment	-0.0418	-0.0171	-0.0469	0.0356	0.0541	0.0321
	(0.0511)	(0.0516)	(0.0517)	(0.0454)	(0.0486)	(0.0438)
Observations	2,750	2,750	2,639	2,750	2,750	2,639
R-squared	0.583	0.596	0.628	0.542	0.551	0.605

Notes: Table presents three parameter DiD estimates of the impact of LEAP treatment on student disciplinary incidents that allow the impact of treatment to grow gradually overtime. Trend Treatment is the interaction of a linear time trend with an indicator for treated students. Trend Post Treatment is the interaction of a linear time trend with the post treatment indicator. Results are presented separately by student free or reduced-price lunch status. Columns 1 and 4 present baseline estimates with no controls. Columns 2 and 4 add student level controls for gender, special education status, English language learner status, and indicators for Black, Hispanic and Asian or Pacific Islander students. Columns 3 and 6 add propensity score overlap weights. All specifications include student and grade-by-school-by-month fixed effects. Standard errors clustered at the student level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5A
DiD Estimates: Disciplinary Incidents Heterogeneity by Gender

	Any Incident			Severe Incident		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	With Controls	With Overlap Weights	Baseline	With Controls	With Overlap Weights
			Panel A: Female			
Post Treatment	-0.0721	-0.0751*	-0.0735	-0.0995***	-0.0982***	-0.0949***
	(0.0447)	(0.0449)	(0.0455)	(0.0365)	(0.0363)	(0.0364)
Trend Treatment	0.00609	0.00669	0.00808	0.00235	0.00293	0.00262
	(0.0105)	(0.0104)	(0.0106)	(0.00899)	(0.00886)	(0.00898)
Trend Post Treatment	-0.00963	-0.0104	-0.00723	0.0126	0.0105	0.0136
	(0.0184)	(0.0184)	(0.0192)	(0.0141)	(0.0140)	(0.0141)
Observations	13,454	13,454	13,101	13,454	13,454	13,101
R-squared	0.519	0.520	0.553	0.476	0.477	0.520
			Panel B: Male			
Post Treatment	-0.00899	-0.00938	-0.00334	-0.0678**	-0.0681**	-0.0615*
	(0.0391)	(0.0391)	(0.0401)	(0.0338)	(0.0339)	(0.0341)
Trend Treatment	0.00467	0.00474	1.37e-05	0.00662	0.00752	0.00540
	(0.0103)	(0.0103)	(0.0104)	(0.00875)	(0.00878)	(0.00877)
Trend Post Treatment	-0.0389**	-0.0369**	-0.0359**	-0.0238*	-0.0235*	-0.0261*
	(0.0162)	(0.0162)	(0.0165)	(0.0140)	(0.0140)	(0.0140)
Observations	17,262	17,262	16,860	17,262	17,262	16,860
R-squared	0.573	0.574	0.594	0.543	0.544	0.572

Notes: Table presents three parameter DiD estimates of the impact of LEAP treatment on student disciplinary incidents that allow the impact of treatment to grow gradually overtime. Trend Treatment is the interaction of a linear time trend with an indicator for treated students. Trend Post Treatment is the interaction of a linear time trend with the post treatment indicator. Results are presented separately by gender. Columns 1 and 4 present baseline estimates with no controls. Columns 2 and 4 add student level controls for free or reduced price lunch status, special education status, English language learner status, and indicators for Black, Hispanic and Asian or Pacific Islander students. Columns 3 and 6 add propensity score overlap weights. All specifications include student and grade-by-school-by-month fixed effects. Standard errors clustered at the student level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6A
DiD Estimates: Disciplinary Incidents Heterogeneity by Race & Ethnicity

	Any Incident			Severe Incident		
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	With Controls	With Overlap Weights	Baseline	With Controls	With Overlap Weights
			Panel A: Black or Hispanic			
Post Treatment	-0.0260	-0.0270	-0.0213	-0.0556**	-0.0571**	-0.0495**
	(0.0267)	(0.0267)	(0.0268)	(0.0230)	(0.0230)	(0.0227)
Trend Treatment	0.00343	0.00396	0.00142	0.00399	0.00467	0.00284
	(0.00661)	(0.00659)	(0.00662)	(0.00572)	(0.00568)	(0.00566)
Trend Post Treatment	-0.0233**	-0.0233**	-0.0214*	-0.0160	-0.0164*	-0.0164*
	(0.0113)	(0.0113)	(0.0116)	(0.00974)	(0.00972)	(0.00982)
Observations	32,099	32,099	31,396	32,099	32,099	31,396
R-squared	0.491	0.491	0.517	0.463	0.464	0.495
			Panel B: White			
Post Treatment	0.00884	-0.000361	-0.0306	0.154	0.149	0.115
	(0.139)	(0.145)	(0.142)	(0.105)	(0.107)	(0.113)
Trend Treatment	-0.0188	-0.00969	-0.0151	0.0113	0.0197	0.00995
	(0.0314)	(0.0270)	(0.0277)	(0.0289)	(0.0263)	(0.0258)
Trend Post Treatment	0.0258	0.0314	0.0368	-0.0275	-0.0234	-0.00904
	(0.0512)	(0.0489)	(0.0512)	(0.0522)	(0.0526)	(0.0540)
Observations	871	871	863	871	871	863
R-squared	0.650	0.662	0.698	0.693	0.700	0.716

Notes: Table presents three parameter DiD estimates of impact of LEAP treatment on student disciplinary incidents that allow the impact of treatment to grow gradually overtime. Trend Treatment is the interaction of a linear time trend with an indicator for treated students. Trend Post Treatment is the interaction of a linear time trend with the post treatment indicator. Results are presented separately for Black and Hispanic students (top panel) and White students (bottom panel). Columns 1 and 4 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. Columns 3 and 6 add propensity score overlap weights. All specifications include student and grade-by-school-by-month fixed effects. Standard errors clustered at the student level in parentheses. *** p<0.01, ** p<0.05, * p<0.1