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Supporting Student Engagement During Remote Learning: Three Randomized Controlled Trials in Chicago Public Schools

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Abstract

This paper presents the results of three field experiments testing interventions designed to increase engagement and improve learning during remote schooling. Since the COVID-19 pandemic, the use of remote learning when schooling is interrupted has become more common, prompting educators to ask: How can we better engage students during remote instruction? This is especially salient because much of what we know about student engagement is based on in-person schooling, not virtual instruction. In the first experiment, the authors find that personalized phone calls increased families' likelihood of registering for a virtual summer schooling program in Chicago Public Schools, the pre-specified primary outcome. In the second experiment, the authors find sending weekly text messages had no effect on students' summer days absent and usage of Khan Academy, the primary outcomes; in analyses of secondary outcomes, they find that the weekly text messages increased students' likelihood of passing their summer math course. In the third experiment, the authors find adding an instructional aide to supplement classroom teachers had no effect on the primary outcomes of summer days absent and usage of Khan Academy; in analyses of secondary outcomes, we find beneficial impacts in the following school year on students' math grades and passing rates.

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Any opinion, findings, and conclusions or recommendations expressed in this material are those of the authors and do not necessarily reflect the views of the National Science Foundation. All errors are their own.

1 Introduction

During the COVID-19 pandemic, students around the world experienced unprecedented disruptions in schooling. Abrupt changes in instructional mode and repeated school closures had profound impacts on student learning, prompting a discussion of the immediate and long-term impacts of remote instruction on educational outcomes. Concerns about the efficacy of remote schooling increased as districts across the country reported low test scores, poor achievement growth, decreased attendance, and decreased student participation (Barry et al., 2022; Goldhaber et al., 2022; Kogan & Lavertu, 2021; Jack et al., 2023; Pan et al., 2021). This switch to remote instruction exacerbated preexisting inequities: the largest declines in test scores were shown to be in districts that provided the lowest amount of in-person learning opportunities, which also tended to be districts with higher proportions of Black and Hispanic students (Jack et al., 2023).

In the wake of the pandemic, technology has advanced to enable greater opportunities for remote instruction. Schools are now more equipped to provide instruction remotely and are frequently using it as an option in situations where they did not previously. For example, students used to have school completely canceled due to inclement weather on snow days, but now schools are able to continue instruction via e-learning days. Of the states that regularly get snow, over 75 percent had policies during the 2022-23 school year that allowed remote learning as an option instead of snow days (Sorber, 2022). In the state of Illinois, the setting of this study, state law allows districts to switch to e-learning days, allowing these days not to count as a full cancellation day (Vinicky, 2024). Beyond snow days, many states have passed legislation that requires all students to take a course online in order to graduate (Etherington, 2017).

Given this increasing prevalence of remote instruction, it is important to understand how to promote attendance and learning participation for students attending school virtually. While there is a large body of work on how to increase attendance for students attending school in person, little has been done to address attendance in virtual schooling. This gap in knowledge was made apparent by the difficulty in promoting engagement with virtual schooling during the pandemic (Barry et al., 2022; Jack et al., 2023). The implications go beyond the COVID-19 pandemic, as disparities in engagement with virtual schooling during COVID may persist and contribute to intensified inequality (Malkus, 2024).

Blended instruction involving both virtual and in-person components is more common now than prior to the pandemic; however, there may come a time when schooling is forced to temporarily move fully online again in the future. If and when this happens, it will likely not be under ideal circumstances. Students increasingly experience unplanned disruptions in schooling related to climate change, school-based violence, natural disasters, political and social unrest, and health events. Exposure to violence, bullying, hate speech, and school shootings are consistently correlated with heightened absenteeism and truancy among middle and high school students (Cabral et al., 2020; Cano, 2020; Copperman et al., 2022; Grinshteyn & Yang, 2017; National Center for Education Statistics, 2019). Climate change and resultant natural disasters also increasingly cause unplanned school disruptions. In California alone, nearly 21,100 school closures related to wildfires have been documented from 2002-2019, accounting for roughly two thirds of the state's school closures over that period (Cano, 2020). Research in Cambodia, Tajikistan, and Bosnia and Herzegovina have further documented the devastating effects social and political unrest have on both access to education and long-term outcomes (Islam et al., 2016; Kreso, 2008; O'Brien, 2020). Testing solutions under challenging conditions and constraints, as this study was able to do, is key to understanding how they would actually play out in the future.

This paper presents evidence on the effectiveness of three interventions, implemented at scale during a time of crisis in the summer of 2020, to try to bolster remote engagement and learning. On March 16, 2020, as instances of COVID-19 continued to grow, Chicago Public Schools (CPS) announced that all district schools would be closed through March 30, 2020 (Chicago Public Schools, 2021). Two weeks later, on April 13, 2020, CPS decided that students would learn remotely for the remainder of the school year. Students did not return to in-person schooling until the middle of the following school year. According to the education tracker created by Opportunity Insights, Cook County (the county in which Chicago is situated) saw a decrease of 16.3% in total students' participation in online math coursework from January 2020 to May 3, 2020 (the last recorded date of the tracker for school year 2019-2020) (Chetty et al., 2023).

Motivated by this drop in school participation, our research team ran three randomized controlled trials in partnership with CPS to evaluate the effectiveness of interventions designed to re-engage students who struggled the most during remote schooling in the spring of 2020. During the summer of 2020, CPS held a virtual program called Summer Learning designed for 1st through 8th grade students who either never attended class during the remote learning period in the 2019-2020 school year, or who received a grade of "incomplete" in math or reading. We evaluated the effectiveness of several interventions to re-engage students in three separate experiments: 1) calling eligible families to help students register for Summer Learning, 2) sending weekly text message reminders to families to encourage their registered students to attend class, or 3) providing more intensive instructional support to improve learning and engagement. In this third intervention, we assigned an assistant teacher to a randomized set of virtual classrooms to assist with contacting families and providing in-class support to the lead teachers.

In Experiment 1, we randomized the 14,567 students who were eligible for Summer Learning but had not yet registered to either a personalized outreach treatment group or a control group. In the treatment group, outreach workers were assigned to help contact, encourage, and support parents in registering their child for the program. We find that calling families to register for Summer Learning led to a 13.3% increase in registration for the program, the primary outcome of interest for this experiment.

In Experiment 2, we randomized the 7,608 students who had registered for Summer Learning and consented to receive text messages into either a text message reminder treatment group or a control group. In the treatment group, CPS sent weekly text message reminders to students' parents to encourage their child to log on to the Summer Learning program. On the primary outcome of attendance, we do not find that sending text messages to encourage students' attendance during the program had a statistically significant effect on the number of days absent using our main specification; however, all other models we estimate do detect significant decreases in days absent, ranging from 0.24 days to 0.58 days depending on model specification. We find no detectable effect on the other primary outcome of students' usage of Khan Academy. On the secondary outcomes, we find text messages caused a 2.4% increase in students' likelihood of passing the summer math course.

In Experiment 3, we randomly assigned the 362 lead math teachers in the Summer Learning program to either an instructional support treatment group or a control group. Treatment teachers were provided a teacher's aide to assist them within their virtual classroom. We do not find any impact on the primary outcomes for this experiment, which were summer attendance and Khan Academy usage. However, on the secondary outcomes, we find that providing additional instructional support for teachers led to a 4 percentage point increase in students' likelihood of passing math during the following school year (a 5.1% increase from the control mean of 78.2%), and increased math grades during the following school year by 5.1%.

The rest of the paper is organized as follows. Section 2 provides motivation and background on relevant literature. Section 3 describes the Summer Learning program. Section 4 outlines the research design, data sources, and outcomes of interest. Section 5 presents results for each experiment. Section 6 investigates heterogeneous treatment effects by grade to explore whether effects were stronger for older or younger children, motivated by the fact that the Summer Learning program structure differed for older and younger students. Section 7 explores treatment effects across the distribution of absent days. Section 8 discusses the implications of the findings in relation to student engagement with remote learning in the future.

2 Background and Prior Literature

Chronic absenteeism

Chronic absenteeism is a persistent challenge in education that has gained renewed urgency in recent years. In practice, chronic absenteeism is defined as a student missing at least 10% of the days in a school year (Allensworth & Evans, 2016). Chronic absenteeism is most frequent in high poverty areas, with the youngest and oldest students in a school district having the highest rates (Balfanz & Byrnes, 2012). Having higher numbers of absences is linked to a higher likelihood of having lower academic performance, failing classes, and dropping out of school (Allensworth & Easton, 2007; Balfanz & Byrnes, 2012). In particular, the strong connection between frequent absences and dropping out of school matters greatly due to the well-established link between high school drop-out and negative long-term effects, including worse employment outcomes (Binder & Bound, 2019), greater likelihood of involvement with the criminal justice system (Lochner & Moretti, 2004), and poor health (Cutler & Lleras-Muney, 2006).

The effects of the COVID-19 pandemic on rates of chronic absenteeism are stark and persistent. In the 2021-2022 school year, over 14 million students in the U.S. were chronically absent (U.S. Department of Education, 2024). This number is nearly double the number of chronically absent students prior to the pandemic: roughly 8 million students were chronically absent from the 2018-2019 school year (U.S. Department of Education, 2024). These increases in chronic absenteeism were more pronounced in districts that were in less-resourced areas that already faced higher rates of chronic absenteeism before the pandemic, implying that the pandemic exacerbated existing disparities in absenteeism (Malkus, 2024).

Recent analysis suggests that increases in chronic absenteeism are not driven by decreased school enrollment, rates of COVID-19 cases, school masking policies, or children's mental health declines (Dee, 2024), highlighting a need to better understand the underlying causes. Absenteeism has also been linked to academic setback, with suggestive evidence that increased absenteeism explains between 16-27% of the declines in math test scores and 36-45% of the declines in reading test scores observed post-pandemic (Council of Economic Advisors, 2023). While research has uncovered successful interventions that target chronic absenteeism prior to the pandemic, little work has studied whether and how interventions that took place *during* the pandemic may have served to alleviate some of these emergent and persistent issues of chronic absenteeism, and how the interventions could be applied in the future to promote engagement in both virtual and in-person schooling.

Interventions to improve in-person attendance

While this study focuses on improving attendance and engagement with *remote* schooling, it is useful to understand what the literature has shown on how to successfully increase students' in-person attendance. These interventions can be categorized into three broad types: contacting parents, mentoring programs, and systematic school-based strategies.

In the first type of intervention, parents are contacted either by mail or by phone to encourage their child to attend class. Mailing attendance records information to the parents of frequently absent students has been shown to significantly decrease absences (Robinson et al., 2018; Rogers & Feller, 2018). Heppen et al. (2020) found that texting parents successfully reduced absences. Personalized messages from school staff sent directly to parents were particularly effective for students with a prior history of high absences, but even basic automated text messaging increased students' attendance. However, this study found no effect from text messaging on academic achievement (Heppen et al., 2020). Contacting parents to increase attendance has been effective for students across grade levels, with Robinson et al. (2018) and Heppen et al. (2020) finding positive effects on attendance for students in grades K-5, and Rogers & Feller (2018) for students in grades K-12.

The second well-studied type of intervention to improve attendance is mentoring. Mentoring programs dedicate time to creating connections between mentor figures and students to provide these children with a caring relationship with an adult. One example of a well-studied mentoring program is Big Brothers Big Sisters, which creates one-on-one mentoring relationships between adults and children. Big Brothers Big Sisters has been shown consistently to significantly improve both academic achievement and attendance for students in grades 4-9 (Bayer et al., 2015; Grossman et al., 2012; Herrera et al., 2007; Schwartz et al., 2011). Similarly, a randomized controlled trial of the Check & Connect mentoring program found a decrease in absences for students in older grades (5th-7th grade), with no detectable effects found for students in younger grades (1st-4th grade) (Guryan et al., 2020).

Other interventions are delivered at the whole-school level, but still have been found to have positive effects on students' attendance. Kirksey & Gottfried (2021) found that providing free breakfast to all students at school is linked to improved attendance for elementary, middle, and high school students (Kirksey & Gottfried, 2021). A more targeted school-based approach is using early warning indicator and intervention systems, which identify high school students as not on-track to graduate through indicators like chronic absenteeism and academic performance. Once identified, those high school students are assigned to relevant interventions chosen by each school to get "back on track," such as setting meetings between parents and teachers or developing strategies among staff members to provide more assistance to those students. These early warning indicator and intervention systems have been found to have positive effects on attendance, with mixed findings on academic outcomes (Davis et al., 2018; Faria et al., 2017; Sepanik et al., 2021).

Virtual schooling

In 2006, Michigan was the first state to require all students to take at least one course online in order to graduate high school. Since then, Florida, Virginia, Arkansas, and Alabama have followed suit, and other states and school districts across the country have implemented legislation that encourages students to take online courses (Etherington, 2017). As a result, ensuring regular attendance among students enrolled in virtual school was already a pressing policy issue before the pandemic. A report that examined state legislative behavior related to virtual schools found that at least 45 bills were enacted in 25 states from 2017-2019, with policies often focused on attendance (Ujifusa, 2019). For example, Indiana passed legislation in 2019 that requires students to complete each virtual school's annual onboarding process alongside their parent before being able to enroll to promote family engagement (Ujifusa, 2019).

While a good deal of research studies how to improve attendance at in-person schools, this study is one of the first to investigate how to reduce absences and improve learning engagement for students attending school virtually. For example, the previously mentioned mailing intervention implemented in Rogers & Feller (2018) explicitly excluded online schools from their sample. One study of an intervention that did seek to improve online attendance tested a commitment program during the pandemic, in which university students in the Netherlands pledged to attend class, but the study found no effect of the intervention on online attendance (Weijers et al., 2022). During the summer of 2021, Connecticut implemented a home visit intervention for students identified as chronically absent, and researchers found that it significantly increased attendance rates for students, with stronger effects seen for students in grades 6-12 as compared to grades pre-K-5 (Stemler et al., 2022). There is not much work done on the link between online attendance and academic performance, but one pandemic-era on online college courses in China found that students' class attendance had a positive impact on academic outcomes: a 1 SD increase in attendance was associated with a 0.08 SD increase in a student's academic performance (Ha et al., 2024).

Ensuring student attendance during virtual schooling is an important issue, as the setting presents unique challenges to sustaining engagement and interest. The absence of structure and the physical presence of teachers and peers makes it more difficult to attend and focus during class. Virtual schooling also comes with more distractions within a student's immediate environment, such as from their family members or external noise from their surroundings. Additionally, measuring "attendance" for virtual classrooms is not as clear as physically being able to see a student in a classroom. It can be defined in different ways: is it recorded by a platform's internal system (such as Zoom or Google Classroom) when a student merely opens up a link to attend class? Is it measuring whether a student stayed for the entire duration of the class period? Does it consider whether a student's camera was on, or if they spoke during class? All of these questions demonstrate the complicated nature of promoting virtual attendance as compared to in-person attendance.

Interventions to improve learning engagement

It is important to consider whether and how students are engaged in learning beyond merely attending school. It is insufficient to simply measure how often students are physically present— it is essential to capture whether students are bettering themselves academically through their improved attendance and presence in class. The literature on teacher's assistants (TAs) suggests that their presence in classrooms can have positive effects on students' learning engagement, boosting test scores, particularly in reading (Andersen et al., 2020; Hemelt et al., 2021). Teaching assistants with and without teaching degrees have both been shown to have positive effects on test scores, either through reducing the student-teacher ratio or assisting with classroom behavioral issues (Andersen et al., 2020).

In our study, we randomly assigned teacher's aides to virtual classrooms to conduct several tasks, which are similar to those of a traditional TA: tracking students' attendance, connecting with parents and students (particularly those who were not showing up to class), helping students with technology issues, assisting the lead teacher in identifying which students to prioritize, and providing 2-on-1 sessions with students assisting them in using online math learning platforms. The teacher's aides employed in our study were in the process of completing their degrees in teaching and were tasked with both classroom assistance tasks and instructional labor. Notably, this took place within entirely remote classrooms rather than in-person ones, an area of study that has not received much attention. It is not immediately clear whether utilizing similar strategies in a virtual context would have similar benefits, as TAs in virtual classrooms are limited in their ability to provide physical help or interact with students through a computer screen. We are able to show that employing teacher's aides within a virtual classroom can improve students' academic performance.

3 Program Description

CPS's Summer Learning program was a four week long program with four hours of instruction per day. It ran from July 20 to August 14, 2020. The program provided an opportunity for first through eighth grade students who either did not attend class or received an incomplete during the remote spring quarter of 2020 to improve their fourth quarter grade from Incomplete to Pass. Students were required to take both math and reading even if they only received an Incomplete in one subject (but their summer grade in that subject would not count against the student's already-existing Pass score). Guidance from CPS to teachers stated that students who earned a D or higher and had no more than 3 unexcused absences during the program would have their grade historically changed from Incomplete to Pass. Students were to be counted as present if they participated for more than half of each summer school day's synchronous components. CPS emphasized that participation during Summer Learning should count towards students' grades, and was supposed to be worth roughly 20-33% of each student's overall grade. As shown in Appendix Table 13, students who were eligible for Summer Learning had lower math and reading standardized test scores in the quarter before the pandemic, were almost all eligible for free or reduced price lunch, and had a higher percentage of Black students than CPS students in the same grades who were not eligible.

Teachers from schools across the district were involved in supporting this program. For grades 1-5, as in the school year structure of CPS, teachers were trained in instructional methods for both math and reading. Students in these younger grades would spend their entire summer school day with the same teacher, who would instruct them in both math and reading. For grades 6-8, teachers specialized in either math or reading instruction. Students in these older grades would spend half of their summer school day with their math teacher, and half of their day with a different reading teacher. The program used Google Suite as its learning management system; Khan Academy, ST Math, and Amplify as its educational technology tools; and Khan Academy, MARS Tasks, Amplify, and Lurie Children's Hospital SEL Units as curricular resources.

4 Experimental Design and Research Questions

In this study, we partnered with CPS to understand how to best support students' engagement and learning during remote schooling. We implemented three large-scale randomized controlled trials during Chicago's virtual Summer Learning program. Two of these interventions were phone-based nudges to encourage registration and participation, and one was a more intensive intervention to provide additional instructional support within the virtual classrooms. The interventions assisted with different stages of program implementation: 1) signing up for the program; 2) encouraging attendance during the program; and 3) receiving additional attention and quality instruction while attending class. The goal of these interventions was to provide support for students who struggled to engage with remote learning in the 2019-2020 school year, and to encourage their school readiness for the following school year.

Design of Experiment 1: Phone calls to encourage sign-up

The first experiment took place prior to the start of the program, but after CPS had made an initial outreach effort to eligible students and families. This initial outreach effort involved sending an official Summer Learning Invitation from the district's Central Office via United States Postal Service or email, providing eligible families with information on how to access the online registration form and ways to contact their home schools to assist them in registering.[1] Before our intervention, 6,006 students out of 21,439 total eligible students had already registered for Summer Learning.

In a more personalized intervention, we sought to encourage registration from the students who had not yet registered even after CPS's initial outreach effort. CPS decided to extend its original registration deadline due to the low initial enrollment rate, and it was during this extended period that we conducted Experiment 1. We randomized the 14,567 eligible students who had not already registered by CPS's first deadline into a personalized outreach treatment group (8,000 students) and a control group (6,567 students). We provided 20 outreach workers (all of whom were employed CPS workers at the time) with a list of 400 treatment students each along with the students' phone numbers.[2] These outreach workers were available for this intervention at a relatively low cost to CPS, as they were specialists already employed by the Family

[1] A student's home school is the school they attended the year prior.

[2] In practice, this number varied from 399 to 442 students per list. This number varied due to implementation issues. In one example, an outreach worker attempted 30 calls, then communicated that they were not available to make any more calls. In this case, we re-assigned their students to other outreach workers' lists. The window for making calls ended up being half the length of what was initially planned, so we intended this re-assignment to increase the likelihood that those students received calls. The experiment was originally supposed to run for two weeks, and we assigned 400 students to each outreach worker to call based on that timeline. After one work week, CPS closed registration to give more time to schools to schedule students into the program. The shorter timeline prevented outreach workers from attempting to call all students assigned to them.

and Community Engagement (FACE) program in CPS, and their work traditionally involves interacting with parents and guardians to become more involved with their school community.

The outreach workers made phone calls during the week of July 6-10. Their goal was to inform parents about the program and their child's eligibility, as well as to encourage parents to register their child and provide assistance in registering during the phone call. The contents of the outreach workers' call scripts are presented in Section 15 of the Appendix. Given the shortened timeline, outreach workers were not able to contact all students in their assigned lists. Outreach workers attempted to contact 38% of students assigned to treatment and successfully contacted 24% of the students assigned to treatment (see Figure 2 for details). We define a successful contact as the outreach reaching a parent or guardian or leaving a voicemail.

The research question for Experiment 1 was: *What is the impact of this outreach campaign on Summer Learning registration?* Accordingly, the primary outcome for this experiment is registration for the Summer Learning program. To evaluate the impact of the outreach campaign on registration, we run the following Intent-to-Treat (ITT) model:

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \gamma_g + \sigma_s + \epsilon_i \tag{1}$$

where Y_i is an indicator for whether student i registered in Summer Learning, T_i is a treatment indicator which takes the value of 1 if the student is randomly assigned to the outreach treatment, X_i is a set of individual-level baseline (School Year 2019-2020, or SY20)[3] characteristics (which specifically include age, learning disability, race, gender, English as a Second Language (ESL) status, Students in Temporary Living Situations indicator (STLS), previous year GPA, previous year attendance, whether the student received an incomplete score in Math, Reading, or both, previous year standardized math and reading scores that we standardized relative to the full CPS sample within grade, eligibility for free or reduced priced lunches), γ_g is grade fixed effect, σ_s is assigned school site (summer teachers and students were assigned to virtual home school sites that tended to be the school they worked at or attended during the school year) fixed effect, and ϵ_i is a random error term.

The Intent-to-Treat (ITT) measures the effect of being assigned to the outreach treatment group. As outreach workers did not get the chance to contact all students to whom they were assigned, the ITT does not measure the effect of receiving the phone call from an outreach worker. As noted in the pre-analysis plan, the ITT is the primary outcome of investigation, but we also estimate Treatment-on-the-Treated (TOT) effects. To do this, we measure the effect of the phone calls using random assignment of T as an instrument for contact. Because treatment was randomly assigned, this method calculates the effect of the treatment

[3]For brevity, we refer to each school year by the year of the spring semester; for example, SY2019–2020 is referred to as SY20.

on the treated, or the effect of the intervention for the group of students who were successfully contacted by outreach workers. For the TOT analysis we define treatment as the outreach successfully reaching a parent or leaving a message at their phone number. The first-stage equation is:

$$D_i = \alpha_0 + \alpha_1 T_i + \alpha_2 X_i + \gamma_g + \sigma_s + \xi_i \tag{2}$$

where D_i is an indicator for a successful contact having been made, ξ_i is a random error term, and all other variables are as previously defined. The second-stage equation for TOT is as follows:

$$Y_i = \tau_0 + \tau_1 \hat{D}_i + \tau_2 X_i + \gamma_g + \sigma_s + \theta_i \tag{3}$$

where τ_1 measures the effect of receiving treatment (i.e., being successfully contacted) on program registration, θ_i is a random error term, and all other variables are as previously defined.

Design of Experiment 2: Text reminders to encourage attendance

The second experiment took place during the program. As part of the Summer Learning registration form, parents and families were asked to select whether they consented to receive text messages during the program. Students whose parents registered them for Summer Learning *and* consented to receive text messages from CPS (7,608 students) were randomized into either the text message reminder treatment (3,804 students) or the control group (3,804 students). For more information on the randomization, please see Figure 3.

Weekly text messages (5 in total) were sent to parents of students who were randomized into treatment reminding and encouraging their students to log on. The contents of these messages are presented in Section 15 of the Appendix. Because we cannot determine whether or not a text message was successfully received by a parent, we cannot estimate treatment-on-the-treated effects here.

The research question for Experiment 2 was: *What is the impact of text message reminders on: 1) attendance and Khan usage during the summer (primary outcomes), and 2) academic performance during the summer and the following school year (secondary outcomes)?* To answer this question, we estimate the following ITT model:

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \gamma_g + \lambda_c + \epsilon_i \tag{4}$$

where Y_i is the outcome of student i who was assigned to classroom c in Summer Learning, T_i is a treatment indicator which takes the value of 1 if the student is randomly assigned to the text messaging treatment, X_i is a set of individual-level and classroom level baseline (SY20) characteristics (which specifically include age, learning disability, race, gender, English as a Second Language (ESL) status, Students in Temporary Living Situations indicator (STLS), previous year GPA, previous year attendance, whether the student received an incomplete score in Math, Reading, or both, previous year standardized math and reading scores that we standardized relative to the full CPS sample within grade, eligibility for free or reduced priced lunches, and indicators for ESL or Spanish Language classrooms), γ_g is grade fixed effect, λ_c is classroom (math teacher and reading teacher) fixed effects, and ϵ_i is a random error term.[4]

Design of Experiment 3: Teacher's Aides

The third experiment also took place during the program. We randomized the 362 lead teachers set to work in the Summer Learning program into treatment (94 teachers) and control (268 teachers). This randomization rate was driven by the number of teacher's aides that were available. We included only those lead teachers who were either teaching both math and reading (grades 1-5, where all teachers taught both subjects) or just math (grades 6-8, where there were separate math and reading teachers). We created randomization blocks by grade band (1-3, 4-5, and 6-8) and language (English or Spanish-speaking classrooms). We randomized classrooms proportionally by allocating teacher's aides based on how many of each type of classroom in each grade level there were in total for the Summer Learning program. We randomized only teacher's aides with Spanish-speaking abilities to Spanish-speaking classrooms. We assigned each teacher's aide to assist two classrooms. Each 1-5th grade lead teacher was only assigned to one classroom since they would teach the same group of students both math and reading; each 6-8th grade lead teacher was assigned to teach only one subject (either math or reading) to two classrooms, so a math teacher would teach two different groups of students.[5] For more detail on the randomization, see Figure 4.

Treatment teachers were provided a teacher's aide, who was a CPS employee through their teacher residency program. The resident teacher program is a multi-year engagement where resident teachers start as teaching or classroom assistants, and work their way up to full-time lead teaching. The teacher residency program is specifically for individuals seeking to start a new career path in education, or for people who are already employed by CPS and want to become full-time teachers (Chicago Public Schools: Teach Chicago, 2024). It is meant for those who do not have an Illinois state-issued teaching license, and provides a pathway

[4]Classroom fixed effects refer to separate fixed effects for a student's math teacher and reading teacher. As previously noted, in grades 1-5, students were assigned to just one teacher who taught both subjects, but in grades 6-8, students were assigned to separate math and reading teachers. We assign randomized students who do not appear in the post-program data containing teacher information the same classroom ID so that they do not drop from these regressions. This happens to about 9 percent of the randomized sample, and CPS did not have insight into why this is the case.

[5]In the randomization, there were 461 Summer Learning classrooms included. This consisted of 94 treatment teachers and 268 control teachers, for a total of 362 teachers included in randomization. Of the 94 treatment teachers, 68 are in grades 1-5, and 26 are in grades 6-8. This is 120 total treatment classrooms (68 + 26*2). Of the 268 control teachers, 195 are in grades 1-5, and 73 are in grades 6-8. This is 341 total control classrooms (195 + 73*2).

into becoming a teacher in CPS. All residents are committed to teaching in CPS for two years after they complete their residency.

The responsibilities of the teacher's aides were threefold: 1) to connect with families to increase engagement and attendance; 2) to provide mentorship to students through developing positive relationships; and 3) to provide in-class 2-on-1 math support using Khan Academy to improve the use and efficacy of online math learning platforms. Due to the classroom structures, the teacher's aides interacted with students in grades 1-5 every *other* day, and every day with students in grades 6-8.

These teacher's aides were instructed to do the following tasks on a **weekly basis**: hold a 30-minute call with each lead teacher to identify students to prioritize supporting, conduct wellness check-ins with parents and students, and record in a tracker for our study whether each of the following took place: the lead teacher sent a daily attendance report, the teacher's aide had a 30 minute call with teachers, and the teacher's aide attended a class. On a **daily basis**, teacher's aides were instructed to: reach out to students who were not in attendance (if they were having a technology or device issue, they were to connect them with tech resources in CPS; if they were having other difficulties, they were to motivate the child to attend) and conduct daily reporting tasks (log which classrooms they attended and for how long; log the students they contacted for attendance reasons; log the students they contacted for weekly check-up calls).

The research question for experiment 3 was: *What is the impact of additional instructional support on 1) attendance and Khan usage during the summer (primary outcomes), and 2) academic performance during the summer and the following school year (secondary outcomes)?* To answer this question, we estimate the following ITT model:

$$Y_{i,c} = \beta_0 + \beta_1 T_{i,c} + \beta_2 X_{i,c} + \gamma_g + \rho_b + \epsilon_i \tag{5}$$

where $Y_{i,c}$ is the outcome of student i who was assigned to classroom c in Summer Learning, $T_{i,c}$ is a treatment indicator which takes the value of 1 if the student was in a class that was randomly assigned to the teacher's aide treatment, $X_{i,c}$ is a set of student-level baseline (SY20) characteristics (which specifically include age, learning disability, race, gender, English as a Second Language (ESL) status, Students in Temporary Living Situations indicator (STLS), previous year GPA, previous year attendance, whether the student received an incomplete score in math, reading, or both, previous year standardized math and reading scores that we standardized relative to the full CPS sample within grade, and eligibility for free or reduced priced lunches), γ_g is grade fixed effect, ρ_b is randomization block (based on grade band and language spoken in the classroom, English or Spanish) fixed effect, and $\epsilon_{i,c}$ is a random error term. Standard errors are robust and clustered at the level of randomization (lead teacher).

4.1 Data sources and assumptions

We use confidential, longitudinal, administrative student-level data from CPS to conduct the analysis.[6] This includes students' demographics, attendance, standardized test scores, and grades during the relevant school years (SY20 and SY21) and the Summer Learning program. We also have student-level data on their usage of online learning platforms Khan Academy, ST Math, and Google Classroom.

Because Experiment 3 (Teacher's Aides) included randomization at the classroom-level, we make several assumptions due to the structure of the available data. Rosters of students and their classrooms for the summer were unavailable. Instead, we rely on end-of-program data which contains students and their teacher assignment(s) throughout the summer program. Because students could and did transfer between classrooms, students could be listed with multiple teachers or with teachers whose classes they never attended (or that they attended for a very short period). It is not possible for us to determine which teacher a student had for the longest amount of time. We make several assumptions to proceed with the analysis of Experiment 3.

When merging teacher names between the original randomization list and CPS's post-program data, we are able to match 333 out of the 362 teachers originally randomized. These teachers were assigned to 7,592 students. Twenty-nine teachers in the randomization do not appear in this roster – 6 missing treatment teachers, and 23 missing control teachers. This is because these teachers were initially assigned to teach during the summer but ultimately did not teach, as CPS over-hired for the program.

In matching students to teachers, we make several assumptions. There are 7,382 students who are listed with only one teacher. There are 81 students assigned to both a treatment and a control teacher (4 treatment and 4 control teachers). We keep these students' assignments to the treatment teacher. Two control teachers have all of their students also listed with a treatment teacher. As a result, when we mark these students as only being in the treatment teacher's classroom, two control teachers are excluded from the analysis, reducing the total number of matched teachers (from the original randomization and post-program data) to 331 – meaning 91.4% of randomized teachers are included in the final analysis. No students match to more than 1 treatment teacher. For the 129 students matched to 2 control teachers, we randomly assign them to one of the two control teachers.

4.2 Outcome variables and their availability

In our design phase, we specified the primary and secondary outcomes for each experiment based on our theory of change of what outcomes we thought each intervention might influence. Each experiment's primary outcomes are related to what they intended to target: for Experiment 1, this was registration; for Experiments 2 and 3, this was summer program engagement as measured by during-program attendance and

[6] Under our data sharing agreement with CPS, we are not able to publicize these data outside of our research team.

engagement with Khan Academy. In Experiments 2 and 3, the secondary outcomes included other measures of engagement and learning during the summer program and the following school year. More details on these are described in the following section.

Primary outcomes

Registration for Summer Learning: In Experiment 1, the primary outcome as defined in the pre-analysis plan is whether students registered for the Summer Learning program. This variable is obtained from CPS's post-program data, which we define as equal to 1 for those students marked as one of the following: registered and earned at least 1 passing grade; registered and enrolled but not scheduled; and registered but earned no passing grades. It is equal to 0 for those students marked in CPS's data as "did not register."[7]

Attendance during Summer Learning: In Experiments 2 and 3, the first primary outcome is attendance. The provided metric for attendance in CPS's program data is the recorded number of days absent. The definitions of being "present" or "absent" were consistently laid out by the district for educators ahead of the program. The measure of attendance we use is one collected and recorded by the summer school teachers. A student is marked as "present" if they participated for more than half of each summer school day's synchronous components. This implies some degree of participation and learning took place for a student to be marked as "attending" virtually.

Khan Academy platform usage: In Experiments 2 and 3, the second primary outcome is students' engagement with Khan Academy, which we defined as using Khan's provided metric of "Total Learning Minutes" that the student accumulated in the platform. This is the total number of minutes that a student spent actively engaged with content during the summer program. Only those students in grades 3-8 used Khan Academy during Summer Learning.

Secondary outcomes

Other measures of engagement with Khan Academy: We also investigated effects on other measures of Khan usage, including general time spent on the platform, the number of problems attempted on Khan, and percent of course content that students mastered.

Students' Use of Other Computer-Assisted Learning (CAL) Platforms: During Summer Learning, CPS also used ST Math (grades 1–2), Amplify (for reading), and Google Classroom (all grades). We focus the analysis on Khan Academy for the following reasons:

[7] There are 52 students (19 control; 33 treatment) in the randomization who do not show up in CPS's post-program data, and thus we do not observe their registration outcome. To test whether the results are robust to inclusion of these 52 students, we run the models with and without these students in the sample (see Table 6). The models that include these students assume that they did not register. These 52 students are included in the Experiment 1 balance table, in Table 2.

- ST Math was used by a very small share of students in the sample—only 6% of both treatment and control students (see Table 1), as it was only used for grades 1 and 2.

- Amplify focuses exclusively on reading, while this intervention targeted math instruction and skills. As such, we do not report findings on Amplify usage.

- Google Classroom data were flagged by CPS as having an unclear interpretation. While we present these results for transparency, they should be interpreted with caution due to data limitations. According to CPS, the engagement metric may not distinguish between meaningful participation (e.g., attending a full meeting) or minimal activity (e.g., quickly opening and closing Google Classroom).

SY21 academic engagement: We measured academic engagement using two variables: 1) a binary variable indicating whether a student was enrolled on the 20th day of the school year and 2) their attendance as measured by days present in SY21. We were unable to measure CAL engagement during the following school year as SY21 Khan Academy and Google Classroom data were both unavailable to us.

SY21 academic performance: We investigated academic performance using several variables. Students' overall GPA in SY21 is calculated by taking students' final letter grades and converting them into numeric format (4 = A, 3 = B, 2 = C, 1 = D, and 0 = F) and taking a simple average to get their SY21 GPA. Their SY21 math and reading grades are similarly converted into numeric format when used as an outcome. A student is marked as passing math or reading in SY21 if they received either an A, B, C, or D letter grade in that subject course, and marked as not passing if they received an F or an incomplete. CPS did not administer standardized tests to elementary school students in SY21, so we do not have test scores as an available outcome.

Outcome availability

Table 1 displays the availability of the primary and secondary outcomes, as defined for each experiment in our pre-analysis plan, to examine differential availability by randomization assignment in any of the experiments' samples.

Attrition occurs in Experiment 1 when a student was present in the list of eligible students, but their registration status for Summer Learning is not available (this was very rare, as we have registration data for over 99% of eligible students). In Experiment 2, attrition occurs when a student signed up for Summer Learning, but ultimately did not end up appearing in the summer program data or the following school year's data (i.e., they did not end up actually participating in the program or potentially left the district). In Experiment 3, students who were present in teachers' virtual classrooms do not have all summer program outcomes recorded, and are not all present in CPS's following-school-year data because they may have left the district. We run t-tests to test whether differences in outcome availability are statistically significant and find no significant differences (at conventional levels: $p < 0.001$) in primary or secondary outcome availability for treatment versus control students in any of the experiments.

4.3 Balance

Tables 2, 3, and 4 present baseline characteristics for students randomized into the treatment and control groups for each experiment. We regress each covariate on a treatment dummy and report the p-value on the difference in means between treatment and control. We then regress treatment on all covariates and test the joint significance of all of their coefficients; F-statistics are reported at the bottom of each table. In the t-tests and F-test for Experiment 3, we also include randomization block fixed effects and cluster standard errors at the teacher level. While a few individual variables show statistically significant differences between treatment and control groups based on t-tests, none of the joint F-tests are significant, indicating that overall, the sample is well balanced.

5 Results

5.1 Take-up rates for all experiments

Table 5 displays the randomized samples of students or teachers in each experiment, as well as the rates at which participants took-up their assigned treatments. In Experiment 1, take-up is defined as the number of treatment students successfully contacted by outreach workers (N = 1,898) out of all students assigned to treatment. No students from the control group were contacted by outreach workers. In Experiment 2, we have no way of determining whether a family received or opened a text message. For this reason, we cannot determine which students actually received treatment, and thus cannot report a take-up rate.

In Experiment 3, we randomized at the classroom-level, with 362 teachers included in the randomization (268 control and 94 treatment teachers). Due to CPS's over-hiring for Summer Learning, some lead teachers in the randomization ultimately did not participate in the program, so we re-assigned their teacher's aides to other lead teachers. Before the start of the program, we learned that three lead teachers would not actually be teaching, and one teacher initially assigned to control became a treatment teacher. The Intent-to-Treat analysis reflects this teacher as a control teacher, and the Treatment-on-Treated analysis reflects this teacher as a treatment teacher. The take-up rate displays take-up among treatment teachers only.

5.2 Experiment 1 Results: Phone calls to encourage sign-up

Due to the shortened timeline, outreach workers were not able to contact all students on their lists. The Intent-to-Treat results in Table 6 use the original randomization assignments of students. The ITT effect from the prespecified main model on registration for Summer Learning is a 2.2 percentage point increase from the control mean registration rate of 16.5%, or a 13.3% increase. Depending on model specification, the range of estimates is a 1.9 to 2.7 percentage point increase. This intervention, which incurred only minimal costs[8], led to 385 more students registering for the Summer Learning program in the assigned-to-treatment group as compared to the assigned-to-control group.[9]

To estimate the Treatment-on-Treated effects, we use random assignment as an instrumental variable for being successfully contacted by an outreach worker. The Treatment-on-Treated results in Table 6 use the 1,898 students who were contacted successfully. The TOT effect from the prespecified main model is a 9.5 percentage point increase from the control complier mean registration rate of 16%, or an increase of 59%. Depending on model specification, the range of estimates is a 7.8 to 10.9 percentage point increase.[10]

5.3 Experiment 2 Results: Text reminders to encourage attendance

Each week, we sent text messages to students and families who had registered for Summer Learning reminding their students to attend the program. The scripts of these text messages are in Section 15 of the Appendix. However, we cannot confirm whether a student or family received or opened these text messages and thus was actually treated. For this reason, we can only measure the ITT effects.

The primary outcomes for this experiment were Khan Total Learning Minutes and days absent during the program. We do not detect any statistically significant effects ($p < 0.1$) on students' Khan Academy usage. This could mean that there is truly no effect, or it could mean that we were not adequately powered to detect an effect, as we have Khan data for less than half of the students in this experiment because it was not used in 1st and 2nd grade classrooms.

While we do not detect a statistically significant effect on students' days absent using the main specification, Model I (p = 0.127), all other models detect significant reductions in days absent. Within the context of this program only being 4 weeks long (or 20 total days), this decrease in absences (which ranges from 0.24 to 0.58 days, based on model specification) is important in magnitude. There are some students with recorded absences greater than the length of the program, and we top-code those absences at the maximum length of the program (20 days). This is 1.43% of students in the sample. To check the sensitivity of the results to this top-coding, we run two alternative analyses. In one, we do not top-code absences; results are shown in Appendix Table 16. In the other, we do not top-code absences and we run a median regression instead of ordinary least squares, which is less sensitive to outliers in the data; results are shown in Appendix Table

[8]CPS had already hired a set of employees through the Family and Community Engagement (FACE) program for general district assistance work during the summer. These FACE workers served as the outreach workers in Experiment 1.

[9]1,469 of 8,000 treatment students registered, or 18%; 1,084 of 6,567 control students registered, or 16.5%.

[10]As a robustness check, we also run the models restricting the treatment sample to only those students assigned to treatment whom the outreach workers *likely attempted* to contact in Appendix Table 14. The ITT and TOT results from this restriction look very similar in significance and sign to the ITT results presented here.

17. Both of these analyses show consistent, statistically significant, and negative effects on days absent.

The secondary outcomes for this experiment include various measures of academic performance during the program and the following school year. We see statistically significant and positive effects of text messages on students' likelihood of passing summer math, with an increase of 1.7 percentage points, or a 2.4% increase from the control mean of 69.8%. The effect on passing summer reading is of a similar size (a 1.4 percentage point increase, or a 2% increase from the control mean of 70.7%), but this finding is more sensitive to model specification. We do not detect consistently significant effects on summer math or reading grade point averages (GPAs). This intervention was extremely low-cost, as CPS already had a system in place for sending mass text messages to families, and resulted in higher average rates of passing math and reading in the treatment group as compared to the control group. CPS informed us that Summer Learning teachers were instructed to place emphasis on students' attendance in calculating their Summer Learning grades, so the findings on days absent and passing math are likely, at least partially, linked.[11]

We do not detect any statistically significant effects on summer CAL platform usage or academic outcomes during the following school year (including enrollment, attendance, GPA, or passing math and reading). These results are displayed in Appendix Table 15.

5.4 Experiment 3 Results: Teacher's Aides

During Summer Learning, we randomly assigned teacher's aides, employed through CPS's teacher residency program, to provide support in virtual classrooms. This support included reaching out to families when students were absent and holding small group sessions on Khan Academy. Teacher's aides were each assigned to 2 math classrooms. Teacher's aides were explicitly trained to provide small group sessions for math, not reading, which provides useful context in interpreting the positive findings related to math.[12]

The primary outcomes for this experiment were total Khan Academy learning minutes and days absent during the program. We do not detect statistically significant effects on either outcome ($p > 0.1$). There are some students with recorded absences greater than the length of the program, and we top-code those absences at the maximum length of the program (20 days). This is 1.84% of students in the sample.

The secondary outcomes included academic performance and CAL engagement during the Summer Learning program, as well as academic outcomes in the following school year (SY21). We find that providing additional instructional support to teachers had no statistically significant effect on students' summer grades, likelihood of passing, or use of CAL platforms such as ST Math or Google Classroom ($p < 0.1$).

However, we find that this additional instructional support significantly increased the likelihood of stu-

[11] In the data, we see that these are highly correlated, with a correlation of 0.77 between days absent and passing summer math, and a correlation of 0.78 between days absent and passing summer reading.

[12] See Table 19 for summer and SY21 reading outcomes. We do not detect any statistically significant effects on these.

dents' passing math in the following school year by 4 percentage points – a 5.1% increase from the control mean of 78.2%. We also find that it increased math grades during the following school year by 5.1%. We do not detect an impact on any other SY21 outcomes, including enrollment, attendance, or reading grades or likelihood of passing in reading.[13]

5.5 Experiments 2 and 3: Synergistic effects

Because the same population of students was randomized across multiple interventions within the Summer Learning program, we also examine whether receiving both text message reminders and personalized instructional support had a synergistic effect on student outcomes. To do this, we focus on the subpopulation of students who were included in both experiments – a total of 6,517 students. Within this group, 3,261 students were assigned to the treatment group in Experiment 2, and 3,256 to the control group. For Experiment 3, 1,636 students were assigned to treatment and 4,881 to control. There were 822 students assigned to treatment in both experiments. We estimate potential synergistic effects using the following equation:

$$Y_{i,c} = \pi_0 + \pi_1 T_i^{msg} + \pi_2 T_c^{RT} + \pi_3 T_i^{msg} * T_c^{RT} + \pi_4 X_{i,c} + \gamma_g + \upsilon_{i,c} \tag{6}$$

where $Y_{i,c}$ is the outcome of student i who was assigned to classroom c in Summer Learning, T_i^{msg} is a treatment indicator which takes the value of 1 if the student is randomly assigned to the text messaging treatment, T_c^{RT} is a treatment indicator which takes the value of 1 if the student is randomly assigned to a classroom with the teacher's aide treatment, $X_{i,c}$ is a set of individual-level and classroom level baseline (SY20) characteristics, γ_g is grade fixed effect, ρ_b is randomization block (based on grade band and language spoken in the classroom, English or Spanish) fixed effect, and $\upsilon_{i,c}$ is a random error term. Standard errors are robust and clustered at the teacher-level.

We are interested in π_3, the coefficient on the interaction term between the Experiment 2 treatment assignment and the Experiment 3 treatment assignment, as this estimates the combined effect of being randomized into the treatment group for both interventions. Results on π_3 are shown in Table 9. We do not detect any statistically significant effects that are consistent across models ($p < 0.1$) on either the primary outcomes (Khan Total Learning Minutes and summer days absent) or on the secondary outcomes (academic outcomes and CAL engagement during Summer Learning or the following school year) ($p < 0.1$).

Because these two experiments took place simultaneously, it is possible that contamination bias was present, and students' likelihood of being in the data for either experiment was influenced by their treatment status in the *other* experiment. For example, if receiving text messages made a student more likely to come

[13] We present additional models in the Appendix, including imputing ITT control means by randomization block for missing baseline academic variables and free/reduced lunch indicators in Appendix Table 18, as well as TOT results in Appendix Table 20. Both of these are consistent with the main ITT findings shown here.

to class, we would be more likely to observe treatment students in the Experiment 3 sample. To test this, we focus on the sample of students in the analysis sample for each experiment[14], which was 7467 students for Experiment 2 and 6471 for Experiment 3. Using the Experiment 2 analysis sample, we create an indicator for whether they appear in Experiment 3, then run a regression on their Experiment 2 treatment status and this indicator; the t-statistic is insignificant ($t = 0.08$, $p = 0.94$), indicating no evidence of contamination bias. To check the reverse, we use the Experiment 3 analysis sample, create an indicator for whether they appear in Experiment 2, then run a regression on their Experiment 3 treatment status and this indicator; the t-statistic is also insignificant ($t = -0.49$, $p = 0.62$), also indicating no evidence of contamination bias.

6 Heterogeneity of treatment effects

To understand which students benefited most from the outreach worker intervention, we disaggregate the analysis by grade band—grades 1–5 and grades 6–8—reflecting CPS's classroom structure groupings. These grade bands align with how CPS organizes instruction, as outlined in Section 11.1 of the Appendix. In this analysis shown in Table 10, we find that in Experiment 1, outreach workers' calls to families had a significant positive effect on Summer Learning registration across both grade bands, with larger and more robust effects among students in the older grades.

We also explored potential heterogeneous treatment effects of Experiment 2's text messages by grade band. As shown in Table 11, the positive impact of text messages on summer math passing rates and the reduction in days absent are primarily driven by students in the older grade band, while no significant effects are observed among students in the younger grades.

Finally, we explore heterogeneous treatment effects of additional instructional support for teachers by grade band. This is particularly salient for this experiment, as the structure of classrooms for grades 1-5 was different from the structure for grades 6-8. Each 1-5th grade lead teacher was only assigned to one classroom and they taught the same group of students both math and reading. Each 6-8th grade lead teacher was assigned to teach only one subject (either math or reading) to two classrooms, so a math teacher would teach two different groups of 6-8th grade students, and 6-8th grade students would have 2 different teachers.

The results in Table 12 show that the positive, significant effects from additional instructional support are driven by students in grades 6-8, while the intervention has little impact on students in grades 1-5.[15] This follows logically when considering the structure of the teacher's aide intervention: students in grades 6-8 received more support from teacher's aides than students in grades 1-5. As shown in Figure 11.1 of the

[14]Because SL program variables are differentially available, we define being in the analysis sample as having days absent recorded (non-missing), as this is the most-frequently-available SL program variable and also indicates the students that showed up for some amount of the program.

[15]The outcomes displayed in Table 12 differ from those shown in Table 11 because we focus on only the significant findings for each experiment to investigate whether they are driven by specific subgroups.

Appendix, teacher's aides for grades 1-5 joined each of their assigned lead teacher's math class every *other* day, while teacher's aides for grades 6-8 joined their assigned lead teacher's math class every day.

7 Treatment effects across the distribution of absent days

The attendance results reported thus far are estimates of the average treatment effects on days absent. It is possible that these average effects could mask heterogeneous treatment effects for students with different baseline attendance patterns. An average reduction in absences could be consistent with a small reduction in absences for all students, or a large reduction in absences for a small subset of students. In this section, we explore whether the interventions had different effects on different parts of the distribution of absences.

To estimate effects on the distribution of absences, we estimate versions of the Model I regression from Experiment 2 and Experiment 3's main Intent-to-Treat analyses, but replacing the absences dependent variable with a series of dummy variables that indicate whether absences were greater than each possible value of days absent.[16] We define these dummies as equal to 0 if the student has fewer than the specified number of absent days, and equal to 1 if the student has greater than or equal to the specified number of absent days (for example, in the regression that estimates whether there was a treatment effect on having more than 5 days of absences, the dependent variable equals 0 if the student has fewer than 5 absent days and equals 1 if the student has 5 or more absent days). We run the Model I regression with dummy indicators for each threshold of absences, from 1 to 20. The resulting series of estimated treatment effects are estimates of the change in the cumulative distribution function caused by assignment to the treatment group.

In Figure 1, we plot each experiment's cumulative density function for the control group, and subtract the treatment effect estimate at each number of days absent present in the data.[17] The declines in absent days in Experiment 2 seem to be concentrated between around 7 days through 20 days. The marginal, but inconsistently significant, increases in absent days in Experiment 3 occur between 4 and 20 absent days.

8 Discussion

Chronic absenteeism continues to be an issue of paramount importance in education, particularly as post-pandemic chronic absenteeism has worsened and remains persistent. As remote learning has become more prevalent, school districts need to understand how to better promote attendance for students both in-person and virtually. In particular, as natural disasters continue to become more frequent and worsen in scope

[16] We do not conduct this analysis for Experiment 1, as there is an insurmountable issue related to selection. We can only observe days absent during Summer Learning for students who sign up for the program. Because Experiment 1 was explicitly designed to encourage program registration for those assigned to treatment, we have differential outcome availability for Summer Learning absent days for treatment and control students.

[17] While the program only lasted 20 days, 1.43% of the Experiment 2 analysis sample and 1.84% of the Experiment 3 analysis sample have more than 20 absent days recorded in the summer data. We top-code these students' absent days at 20 days.

and cause schools to physically close, children are increasingly facing the need to be able to learn remotely. Research shows that natural disasters can have negative impacts on children's development because it leads to interrupted learning and less time spent in the classroom (Lai & La Greca, 2020). The alternative option available to schools in these moments is virtual schooling, which can be used to ensure that children continue to grow, both educationally and developmentally.

However, little research exists on how to improve students' attendance or engagement when learning remotely. Virtual schooling requires unique strategies as compared to in-person schooling in order to capture students' attention and promote learning. In this paper, we find it is difficult, though not impossible, to improve students' participation in virtual learning. Personalized outreach from outreach workers in Experiment 1 yielded positive effects on program registration, though the effects of texting and assignment of teacher's aides showed no statistically detectable effects on attendance. Texting and teacher's aides may have improved student learning, however, given increases in summer and fall math grades.

While we do not find evidence of synergistic complementarities between the two separate types of interventions, it may be the case that we are too under-powered to be able to detect an effect, as only a small portion of the sample of students was assigned to treatment in both Experiments 2 and 3. We also found inconsistent results from Experiment 3, in that we do *not* find significant effects on summer math grades or passing rates, but we find positive effects in the following school year on students' math grades and likelihood of passing math. One potential suggestion as to why this is the case could be if simply all students in Summer Learning passed their summer math course. However, it is not the case that there is not enough variation in summer outcomes to detect changes, as many students in both the treatment and control group do not pass summer math. Another proposed explanation is that due to the discretionary grading abilities of the Summer Learning program's teachers, summer grades may be measured with a high degree of noise, and school year grades (having more consistency across teachers and schools) are capturing true learning. We are not able to confidently assert that this was the case, but we see it as the most likely explanation.

Taken together, the bundle of findings we show in this paper suggests that we must do more work in understanding when and how to improve student participation in virtual learning environments, but that the combination of light-touch nudges and more intensive instructional support could be one set of interventions to explore further. This has crucial importance when considering issues of equity during remote schooling. The students in this study were some of the least engaged students during the spring of 2020 and therefore in the most need of assistance. We see that these interventions were able to boost their academic performance, suggesting that simple nudges and learning support may be used in a virtual schooling environment to assist those students with the greatest need.

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9 Tables

Table 1: Availability of Primary and Secondary Outcomes, by Experiment

	Mean Control	Mean Treatment	Coefficient	P-Value
Experiment 1 Primary Outcome				
SL Registration Status	0.997	0.996	0.001	0.215
Experiment 2 Primary Outcomes				
Khan Total Learning Minutes	0.442	0.433	0.009	0.419
Days Absent	0.982	0.981	0.001	0.671
Experiment 2 Secondary Outcomes				
Passed Summer Math	0.909	0.907	0.001	0.843
Passed Summer Reading	0.909	0.911	-0.002	0.749
Summer Math GPA	0.909	0.907	0.001	0.843
Summer Reading GPA	0.909	0.911	-0.002	0.749
ST Math Total Days Used	0.059	0.058	0.001	0.922
Number of Days in Google Data	0.940	0.938	0.002	0.738
Activity in Google Meet, Classroom, Docs	0.940	0.938	0.002	0.738
GPA SY21	0.919	0.920	-0.002	0.800
Passed Math in SY21	0.899	0.902	-0.003	0.674
Passed Reading in SY21	0.911	0.914	-0.003	0.626
Math GPA in SY21	0.899	0.902	-0.003	0.674
Reading GPA in SY21	0.911	0.914	-0.003	0.626
Enrolled on Day 20 of SY21	1.000	1.000	0.000	.
Days Present in SY21	0.978	0.977	0.001	0.697
Experiment 3 Primary Outcomes				
Khan Total Learning Minutes	0.933	0.940	-0.008	0.732
Days Absent	0.987	0.969	0.018	0.390
Experiment 3 Secondary Outcomes				
Passed Summer Math	0.983	0.940	0.043	0.104
Passed Summer Reading	0.972	0.955	0.017	0.501
Summer Math GPA	0.983	0.940	0.043	0.104
Summer Reading GPA	0.972	0.955	0.017	0.501
ST Math Total Days Used	0.100	0.093	0.007	0.742
Number of Days in Google Data	1.000	1.000	0.000	.
Activity in Google Meet, Classroom, Docs	1.000	1.000	0.000	.
GPA SY21	0.929	0.917	0.012	0.332
Passed Math in SY21	0.908	0.891	0.016	0.195
Passed Reading in SY21	0.920	0.912	0.007	0.568
Math GPA in SY21	0.908	0.891	0.016	0.195
Reading GPA in SY21	0.920	0.912	0.007	0.568
Enrolled on Day 20 of SY21	1.000	1.000	0.000	.
Days Present in SY21	0.979	0.982	-0.003	0.431

Notes: In this table, we regress each outcome's availability on a binary variable for treatment status and report the coefficient on the treatment dummy and its p-value. For Experiment 3, we include randomization block fixed effects and cluster at the teacher level.

Table 2: Experiment 1 Baseline Balance

Variable	N	(1) Control Mean/(SE)	N	(2) Treatment Mean/(SE)	(1)-(2) Pairwise t-test N	P-value
Black	6567	0.536 (0.006)	8000	0.528 (0.006)	14567	0.310
Latinx	6567	0.414 (0.006)	8000	0.423 (0.006)	14567	0.259
Age in Sept 2020	6567	9.799 (0.027)	8000	9.782 (0.024)	14567	0.645
Free/Reduced Price Lunch	5282	0.986 (0.002)	6424	0.986 (0.001)	11706	0.887
Male	6567	0.552 (0.006)	8000	0.553 (0.006)	14567	0.900
English Language Learners	6567	0.204 (0.005)	8000	0.203 (0.004)	14567	0.959
Students in Temporary Living Situations	6567	0.065 (0.003)	8000	0.073 (0.003)	14567	0.074+
Learning disability	6567	0.087 (0.003)	8000	0.088 (0.003)	14567	0.807
Days attended SY20	6567	144.441 (0.204)	8000	144.322 (0.184)	14567	0.664
Member days SY20	6567	154.455 (0.174)	8000	154.542 (0.155)	14567	0.711
GPA SY19	6097	3.149 (0.009)	7453	3.147 (0.008)	13550	0.863
Course Failures SY20	6567	0.038 (0.003)	8000	0.036 (0.002)	14567	0.492
Stdzd math wint SY20	5339	-0.534 (0.012)	6457	-0.564 (0.011)	11796	0.061+
Stdzd read wint SY20	5064	-0.466 (0.013)	6096	-0.481 (0.012)	11160	0.409
Free/Reduced Price Lunch Missing	6567	0.196 (0.005)	8000	0.197 (0.004)	14567	0.841
Days attended, SY20 Missing	6567	0.000 (0.000)	8000	0.000 (0.000)	N/A	N/A
Member days, SY20 Missing	6567	0.000 (0.000)	8000	0.000 (0.000)	N/A	N/A
GPA, SY20 Missing	6567	0.072 (0.003)	8000	0.068 (0.003)	14567	0.452
Course Failures, SY20 Missing	6567	0.000 (0.000)	8000	0.000 (0.000)	N/A	N/A
Stdized math score, winter SY20 Missing	6567	0.187 (0.005)	8000	0.193 (0.004)	14567	0.368
Stdized read score, winter SY20 Missing	6567	0.229 (0.005)	8000	0.238 (0.005)	14567	0.195
Grade 1	6567	0.097 (0.004)	8000	0.097 (0.003)	14567	0.959
Grade 2	6567	0.098 (0.004)	8000	0.100 (0.003)	14567	0.639
Grade 3	6567	0.123 (0.004)	8000	0.121 (0.004)	14567	0.701
Grade 4	6567	0.128 (0.004)	8000	0.129 (0.004)	14567	0.793
Grade 5	6567	0.147 (0.004)	8000	0.151 (0.004)	14567	0.527
Grade 6	6567	0.155 (0.004)	8000	0.160 (0.004)	14567	0.405
Grade 7	6567	0.163 (0.005)	8000	0.152 (0.004)	14567	0.091+
Grade 8	6567	0.089 (0.004)	8000	0.089 (0.003)	14567	1.000
F-test of joint significance (F-stat)						0.94
F-test, p-value						0.539
F-test, number of observations						8267

Significance: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors are robust.

Table 3: Experiment 2 Baseline Balance

Variable	N	(1) Control Mean/(SE)	N	(2) Treatment Mean/(SE)	(1)-(2) Pairwise t-test N	P-value
Black	3804	0.464 (0.008)	3804	0.459 (0.008)	7608	0.679
Latinx	3804	0.478 (0.008)	3804	0.478 (0.008)	7608	1.000
Age in Sept 2020	3804	10.100 (0.034)	3804	10.071 (0.033)	7608	0.545
Free/Reduced Price Lunch	3063	0.977 (0.003)	3090	0.982 (0.002)	6153	0.224
Male	3804	0.535 (0.008)	3804	0.542 (0.008)	7608	0.565
English Language Learners	3804	0.227 (0.007)	3804	0.229 (0.007)	7608	0.848
Students in Temporary Living Situations	3804	0.057 (0.004)	3804	0.055 (0.004)	7608	0.689
Learning disability	3804	0.057 (0.004)	3804	0.069 (0.004)	7608	0.030*
Days attended SY20	3804	146.684 (0.223)	3804	146.861 (0.231)	7608	0.580
Member days SY20	3804	155.782 (0.172)	3804	155.786 (0.180)	7608	0.987
GPA SY19	3563	3.119 (0.011)	3563	3.142 (0.011)	7126	0.134
Course Failures SY20	3798	0.048 (0.004)	3790	0.044 (0.004)	7588	0.439
Stdzd math wint SY20	3244	-0.459 (0.015)	3224	-0.451 (0.015)	6468	0.702
Stdzd read wint SY20	3118	-0.344 (0.016)	3110	-0.356 (0.016)	6228	0.602
Free/Reduced Price Lunch Missing	3804	0.195 (0.006)	3804	0.188 (0.006)	7608	0.431
Days attended, SY20 Missing	3804	0.000 (0.000)	3804	0.000 (0.000)	N/A	N/A
Member days, SY20 Missing	3804	0.000 (0.000)	3804	0.000 (0.000)	N/A	N/A
GPA, SY20 Missing	3804	0.063 (0.004)	3804	0.063 (0.004)	7608	1.000
Course Failures, SY20 Missing	3804	0.002 (0.001)	3804	0.004 (0.001)	7608	0.073+
Stdized math score, winter SY20 Missing	3804	0.147 (0.006)	3804	0.152 (0.006)	7608	0.521
Stdized read score, winter SY20 Missing	3804	0.180 (0.006)	3804	0.182 (0.006)	7608	0.812
Grade 1	3804	0.074 (0.004)	3804	0.075 (0.004)	7608	0.862
Grade 2	3804	0.080 (0.004)	3804	0.080 (0.004)	7608	0.966
Grade 3	3804	0.098 (0.005)	3804	0.099 (0.005)	7608	0.847
Grade 4	3804	0.109 (0.005)	3804	0.115 (0.005)	7608	0.345
Grade 5	3804	0.162 (0.006)	3804	0.162 (0.006)	7608	0.975
Grade 6	3804	0.195 (0.006)	3804	0.196 (0.006)	7608	0.862
Grade 7	3804	0.211 (0.007)	3804	0.200 (0.006)	7608	0.223
Grade 8	3804	0.072 (0.004)	3804	0.073 (0.004)	7608	0.929
F-test of joint significance (F-stat)						.92
F-test, p-value						0.566
F-test, number of observations						4640

Significance: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors are robust.

Table 4: Experiment 3 Baseline Balance

Variable	(1) Control N/Cluster	Mean/(SE)	(2) Treatment N/Cluster	Mean/(SE)	(1)-(2) Pairwise t-test N/Cluster	P-value
Black	5651	0.426	1941	0.500	7592	0.152
	246	(0.029)	85	(0.050)	331	
Latinx	5651	0.507	1941	0.452	7592	0.311
	246	(0.028)	85	(0.050)	331	
Age in Sept 2020	5651	10.106	1941	10.223	7592	0.196
	246	(0.136)	85	(0.230)	331	
Free/Reduced Price Lunch	4548	0.980	1585	0.977	6133	0.632
	246	(0.002)	85	(0.005)	331	
Male	5651	0.531	1941	0.557	7592	0.031*
	246	(0.007)	85	(0.011)	331	
English Language Learners	5651	0.232	1941	0.214	7592	0.489
	246	(0.029)	85	(0.050)	331	
Students in Temporary Living Situations	5651	0.053	1941	0.055	7592	0.850
	246	(0.004)	85	(0.006)	331	
Learning disability	5651	0.066	1941	0.055	7592	0.119
	246	(0.004)	85	(0.006)	331	
Days attended SY20	5651	146.892	1939	146.644	7590	0.631
	246	(0.243)	85	(0.425)	331	
Member days SY20	5651	155.675	1939	155.773	7590	0.695
	246	(0.165)	85	(0.270)	331	
GPA SY20	5319	3.143	1823	3.113	7142	0.264
	246	(0.019)	85	(0.027)	331	
Course Failures SY20	5641	0.049	1935	0.044	7576	0.621
	246	(0.005)	85	(0.007)	331	
Stdzd math wint SY20	4726	-0.428	1696	-0.468	6422	0.139
	243	(0.023)	85	(0.035)	328	
Stdzd read wint SY20	4585	-0.339	1624	-0.346	6209	0.493
	237	(0.028)	83	(0.040)	320	
Free/Reduced Price Lunch Missing	5651	0.195	1941	0.183	7592	0.351
	246	(0.006)	85	(0.010)	331	
Days attended SY20 Missing	5651	0.000	1941	0.001	7592	0.153
	246	(0.000)	85	(0.001)	331	
Member days SY20 Missing	5651	0.000	1941	0.001	7592	0.153
	246	(0.000)	85	(0.001)	331	
GPA SY20 Missing	5651	0.059	1941	0.061	7592	0.912
	246	(0.005)	85	(0.009)	331	
Course Failures SY20 Missing	5651	0.002	1941	0.003	7592	0.403
	246	(0.001)	85	(0.001)	331	
Stdzd math wint SY20 Missing	5651	0.164	1941	0.126	7592	0.098+
	246	(0.014)	85	(0.019)	331	
Stdzd read wint SY20 Missing	5651	0.189	1941	0.163	7592	0.314
	246	(0.016)	85	(0.022)	331	
Grade 1	5651	0.067	1941	0.060	7592	0.732
	246	(0.014)	85	(0.021)	331	
Grade 2	5651	0.077	1941	0.066	7592	0.639
	246	(0.015)	85	(0.023)	331	
Grade 3	5651	0.097	1941	0.103	7592	0.850
	246	(0.016)	85	(0.035)	331	
Grade 4	5651	0.111	1941	0.138	7592	0.454
	246	(0.019)	85	(0.036)	331	
Grade 5	5651	0.168	1941	0.138	7592	0.339
	246	(0.023)	85	(0.038)	331	
Grade 6	5651	0.210	1941	0.210	7592	0.955
	246	(0.032)	85	(0.058)	331	
Grade 7	5651	0.216	1941	0.171	7592	0.403
	246	(0.031)	85	(0.049)	331	
Grade 8	5651	0.054	1941	0.115	7592	0.105
	246	(0.015)	85	(0.036)	331	
F-test of joint significance (F-stat)						1.35
F-test, p-value						0.117
F-test, number of observations						4632
F-test, number of clusters						313

Significance: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors are robust and clustered at the teacher-level.

Table 5: Summer Learning Experiments, Take-up Rates

	Unit	Assigned Control	Assigned Treatment	Study Total	Take-up Rate
Experiment 1	Student	6,567	8,000	14,567	23.7%
Experiment 2	Student	3,804	3,804	7,608	-
Experiment 3	Teacher	268	94	331	90.4%

Table 6: Effect of Outreach Phone Calls on Summer Learning Registration

	I	II	III	IV	V	VI
Intent-to-Treat						
Registration for SL	0.022***	0.027**	0.023***	0.019**	0.022**	0.019**
	(0.006)	(0.008)	(0.006)	(0.006)	(0.008)	(0.006)
Control Mean	0.165	0.165	0.165	0.165	0.165	0.165
Observations	14567	8267	14515	14567	8267	14515
Treatment-on-Treated						
Registration for SL	0.095***	0.109**	0.095***	0.078**	0.091**	0.079**
	(0.026)	(0.034)	(0.026)	(0.027)	(0.035)	(0.027)
Control Complier Mean	0.160	0.160	0.160	0.160	0.160	0.160
Observations	14567	8267	14515	14567	8267	14515
Controls, missing covariates imputed with zeros	X		X			
Controls, missing covariates dropped		X				
Students missing from CPS data included	X	X		X	X	
Full sample	X			X		
Restricted sample		X	X		X	X

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The preferred model is the one that we specified in our pre-analysis plan, model I. Controls include age, race, gender; indicators for free/reduced lunch, disability status, English Language Learner status, and students in temporary living situations; grade dummies, school site dummies, and baseline academic information from school year 2020, including GPA, course failures, attendance days, member days, and standardized math and reading scores from winter of SY2020 (winter is the most recently available baseline test score for all students, as CPS did not administer tests in the spring of 2020). We are never missing treatment status or baseline demographic information. When marked in the table, zeros are imputed for missing baseline academic variables from SY20 (listed above) and free/reduced lunch indicators, and flags for missingness are included. 43% of students (6,300) in this sample are missing baseline academic variables from SY20. The full sample includes all students in the randomization. The restricted sample drops students with missing baseline academic information or those who we do not observe in CPS's summer data. There are 52 students (19 control; 33 treatment) in the randomization who do not show up in CPS's summer data (we do not observe registration outcome for). To test whether the results are robust to inclusion of these 52 students, we run the model with and without these students in the sample. The models that include these students assume they did not register. The regressions where these students are kept in and marked as "not registered" are labeled as "Students missing from CPS data included." When not marked, these 52 students are dropped from the regressions. Standard errors are robust and in parentheses.

Table 7: Effect of Text Messaging on Attendance and Summer School Pass Rates (ITT)

	I	II	III	IV	V	VI
Primary Outcomes						
Days Absent	-0.241	-0.277+	-0.302+	-0.394+	-0.450*	-0.583**
	(0.158)	(0.165)	(0.178)	(0.204)	(0.209)	(0.221)
Control Mean	7.563	7.563	7.563	7.563	7.563	7.563
Observations	7467	7467	7467	4584	4584	4584
Khan Total Learning Minutes	-2.390	0.550	0.045	0.352	1.841	1.375
	(7.807)	(9.051)	(9.269)	(9.944)	(11.350)	(11.636)
Control Mean	267.335	267.335	267.335	267.335	267.335	267.335
Observations	3331	3331	3331	2237	2237	2237
Secondary Outcomes						
Passed Summer Math (0 = incomplete)	0.017+	0.016	0.018	0.035**	0.030*	0.035*
	(0.010)	(0.011)	(0.011)	(0.013)	(0.013)	(0.014)
Control Mean	0.698	0.698	0.698	0.698	0.698	0.698
Observations	6907	6907	6907	4272	4272	4272
Passed Summer Reading (0 = incomplete)	0.014	0.015	0.017	0.027*	0.024+	0.028*
	(0.010)	(0.010)	(0.011)	(0.013)	(0.013)	(0.014)
Control Mean	0.707	0.707	0.707	0.707	0.707	0.707
Observations	6924	6924	6924	4273	4273	4273
Summer Math GPA (4.0 = A)	0.046	0.038	0.048	0.093*	0.067	0.093*
	(0.033)	(0.035)	(0.037)	(0.042)	(0.044)	(0.046)
Control Mean	1.902	1.902	1.902	1.902	1.902	1.902
Observations	6907	6907	6907	4272	4272	4272
Summer Reading GPA (4.0 = A)	0.011	0.004	0.015	0.066	0.040	0.063
	(0.033)	(0.034)	(0.036)	(0.042)	(0.043)	(0.045)
Control Mean	1.914	1.914	1.914	1.914	1.914	1.914
Observations	6924	6924	6924	4273	4273	4273
Controls, missing covariates imputed with zeros	X	X				
Controls, missing covariates dropped				X	X	
Teacher fixed effects included	X			X		
Full sample	X	X	X			
Restricted sample				X	X	X

Standard errors in parentheses

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Controls include age, race, gender; indicators for free/reduced lunch, disability status, English Language Learner status, and students in temporary living situations; grade dummies, and baseline academic information from school year 2020, including GPA, course failures, attendance days, member days, and standardized math and reading scores from winter of SY2020 (winter is the most recently available baseline test score for all students, as CPS did not administer tests in the spring of 2020). The list of controls always includes grade fixed effects, and where flagged, also includes teacher fixed effects. Teacher fixed effects refer to separate fixed effects for a student's math teacher and reading teacher. In grades 1-5, students were assigned to just one teacher who taught both subjects, but in grades 6-8, students were assigned to separate math and reading teachers. In the handful of cases where students have more than one math or more than one reading teacher, we randomly choose one teacher to assign them to. For students in the randomization who do not appear in CPS's summer grades file where we pull teacher information from, we assign them all the same classroom ID so that they do not drop from these regressions. This is about 9 percent of the randomized sample. The outcomes of passed summer math and reading are equal to 1 when a student was marked as 'passed,' and equal to 0 when a student was marked as 'incomplete'; students marked as N/A or not eligible are marked as missing for these variables. Numeric summer math and reading grades are based on the following scale: 4 = A, 3 = B, 2 = C, 1 = D, 0 = F, and 0 = incomplete. Minutes spent "learning" on Khan Academy is marked as 0 for students in 3-8 grades who do not show up in the Khan data. When marked in the table, zeros are imputed for missing baseline academic variables from SY20 (listed above) and free/reduced lunch indicators, and flags for missingness are included. 39% of students (2,968) in this sample are missing baseline academic variables from SY20. The full sample includes all students in the randomization. The restricted sample drops students with missing baseline academic information. Standard errors are robust and in parentheses.

Table 8: Effect of Teacher's Aides on Student Outcomes (ITT)

	I	II	III	IV
Primary Outcomes				
Khan Total Learning Minutes	-10.140	-3.640	-11.420	-7.930
	(16.689)	(18.475)	(19.101)	(21.916)
Control Mean	136.086	136.086	136.086	136.086
Observations	7097	4569	7097	4569
Days Absent	0.478	0.543	0.794+	0.935*
	(0.338)	(0.377)	(0.444)	(0.474)
Control Mean	7.021	7.021	7.021	7.021
Observations	7461	4547	7461	4547
Secondary Outcomes, Summer 2020				
Passed Summer Math (0 = incomplete)	0.011	0.004	-0.003	-0.016
	(0.023)	(0.025)	(0.024)	(0.026)
Control Mean	0.690	0.690	0.690	0.690
Observations	7380	4498	7380	4498
Summer Math GPA (4.0 = A)	0.028	-0.031	-0.016	-0.078
	(0.099)	(0.101)	(0.103)	(0.105)
Control Mean	1.874	1.874	1.874	1.874
Observations	7380	4498	7380	4498
ST Math Total Days Used	-0.481	-1.551	-0.239	-0.203
	(0.456)	(0.963)	(0.897)	(1.347)
Control Mean	2.378	2.378	2.378	2.378
Observations	744	216	744	216
Number of Days in Google Data	-0.080	-0.095	-0.087	-0.094
	(0.249)	(0.274)	(0.287)	(0.321)
Control Mean	10.963	10.963	10.963	10.963
Observations	7592	4632	7592	4632
Activity in Google Meet, Classroom, Docs	-0.217	-0.187	-0.359	-0.375
	(0.218)	(0.239)	(0.282)	(0.306)
Control Mean	5.956	5.956	5.956	5.956
Observations	7592	4632	7592	4632
Secondary Outcomes, SY21				
GPA, SY21	-0.001	0.017	-0.026	-0.007
	(0.031)	(0.034)	(0.041)	(0.043)
Control Mean	2.029	2.029	2.029	2.029
Observations	7030	4409	7030	4409
Passed Math in SY21	0.040**	0.046**	0.039**	0.048**
	(0.013)	(0.016)	(0.014)	(0.016)
Control Mean	0.782	0.782	0.782	0.782
Observations	6859	4292	6859	4292
Math GPA in SY21 (4.0 = A)	0.082*	0.094*	0.067+	0.085+
	(0.037)	(0.047)	(0.039)	(0.047)
Control Mean	1.602	1.602	1.602	1.602
Observations	6859	4292	6859	4292
Enrolled on Day 20 of SY21	0.007+	0.001	0.004	0.001
	(0.004)	(0.002)	(0.005)	(0.002)
Control Mean	0.966	0.966	0.966	0.966
Observations	7592	4632	7592	4632
Days Present in SY21	-0.117	-0.535	-2.782+	-3.523+
	(0.998)	(1.307)	(1.551)	(1.824)
Control Mean	149.817	149.817	149.817	149.817
Observations	7442	4629	7442	4629
Controls, missing covariates imputed with zeros	X			
Controls, missing covariates dropped		X		
Full randomization sample	X		X	
Restricted sample		X		X

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Intent-to-Treat is defined as the students assigned to the set of lead teachers we initially randomized to teacher's aides (0 = student assigned to lead teacher without a teacher's aide; 1 = student assigned to lead teacher with a teacher's aide). There are 246 control teachers and 85 treatment teachers. Controls include age, race, gender; indicators for free/reduced lunch, disability status, English Language Learner status, and students in temporary living situations; grade dummies, and baseline academic information from school year 2020, including GPA, course failures, attendance days, member days, standardized math score from winter of SY2020, and standardized reading score from winter of SY2020 (winter is the most recently available baseline test score for all students, as CPS did not administer tests in the spring of 2020). When including controls, we include fixed effects for grade and randomization block (based on grade band and language spoken in the classroom, English or Spanish). The outcomes of passed summer math and reading are equal to 1 when a student was marked as 'passed,' and equal to 0 when a student was marked as 'incomplete'; students marked as N/A or not eligible are marked as missing for these variables. Numeric summer math and reading grades are based on the following scale: 4 = A, 3 = B, 2 = C, 1 = D, 0 = F, and 0 = incomplete. Minutes spent "learning" on Khan Academy is marked as 0 for students in 3-8 grades who do not show up in the Khan data. When marked in the table, zeros are imputed for missing baseline academic variables and free/reduced lunch indicators, and flags for missingness are also included. 39% of students (2,960) in this sample are missing baseline academic variables from SY20. The full sample includes all students in the randomization. The restricted sample drops students with missing baseline academic information. Standard errors are robust and clustered at the teacher-level.

Table 9: Synergistic Effects of Experiments 2 and 3

	I	II	III	IV
Primary Outcomes				
Khan Total Learning Minutes	-2.0871	-5.5300	1.5176	-1.1924
	(11.9117)	(15.1488)	(11.6694)	(14.9926)
Control Mean	141.696	141.696	141.696	141.696
Observations	6098	3955	6098	3955
Days Absent	-0.6428	-0.6092	-0.7315+	-0.8130+
	(0.4094)	(0.4777)	(0.4305)	(0.4568)
Control Mean	6.859	6.859	6.859	6.859
Observations	6494	4002	6494	4002
Secondary Outcomes, Summer 2020				
Passed Summer Math (0 = incomplete)	0.0136	0.0027	0.0256	0.0192
	(0.0275)	(0.0304)	(0.0279)	(0.0295)
Control Mean	0.713	0.713	0.713	0.713
Observations	6374	3918	6374	3918
Passed Summer Reading (0 = incomplete)	0.0204	0.0278	0.0307	0.0417
	(0.0253)	(0.0280)	(0.0259)	(0.0275)
Control Mean	0.729	0.729	0.729	0.729
Observations	6375	3914	6375	3914
Summer Math GPA (4.0 = A)	0.0792	0.0062	0.1265	0.0547
	(0.0815)	(0.0959)	(0.0846)	(0.0918)
Control Mean	1.944	1.944	1.944	1.944
Observations	6374	3918	6374	3918
Summer Reading GPA (4.0 = A)	0.0146	0.0164	0.0605	0.0651
	(0.0831)	(0.0926)	(0.0871)	(0.0907)
Control Mean	1.960	1.960	1.960	1.960
Observations	6375	3914	6375	3914
Secondary Outcomes, SY21				
GPA SY21	-0.0280	0.0516	-0.0054	0.0665
	(0.0490)	(0.0607)	(0.0533)	(0.0635)
Control Mean	2.013	2.013	2.013	2.013
Observations	6041	3814	6041	3814
Passed Math in SY21	0.0346	0.0509+	0.0346	0.0513+
	(0.0250)	(0.0287)	(0.0258)	(0.0286)
Control Mean	0.779	0.779	0.779	0.779
Observations	5906	3723	5906	3723
Passed Reading in SY21	0.0332	0.0451	0.0373	0.0527+
	(0.0250)	(0.0284)	(0.0260)	(0.0298)
Control Mean	0.781	0.781	0.781	0.781
Observations	5989	3797	5989	3797
Math GPA in SY21 (4.0 = A)	0.0245	0.0586	0.0396	0.0656
	(0.0682)	(0.0796)	(0.0686)	(0.0800)
Control Mean	1.580	1.580	1.580	1.580
Observations	5906	3723	5906	3723
Reading GPA in SY21 (4.0 = A)	-0.0055	0.0576	0.0146	0.0754
	(0.0651)	(0.0797)	(0.0703)	(0.0864)
Control Mean	1.586	1.586	1.586	1.586
Observations	5989	3797	5989	3797
Enrolled on Day 20 of SY21	-0.0054	0.0024	-0.0074	0.0031
	(0.0100)	(0.0042)	(0.0104)	(0.0040)
Control Mean	0.968	0.968	0.968	0.968
Observations	6517	4009	6517	4009
Controls, missing covariates imputed with zeros	X			
Controls, missing covariates dropped		X		
Full randomization sample	X		X	
Restricted sample		X		X

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Results displayed here are for the coefficient α_3, the interaction term between the Experiment 2 (Texting) treatment assignment and the Experiment 3 (Teacher's Aides) Intent-to-Treat assignment. Controls include age, race, gender; indicators for free/reduced lunch, disability status, English Language Learner status, and students in temporary living situations; grade dummies, and baseline academic information from school year 2020, including GPA, course failures, attendance days, member days, standardized math score from winter of SY2020, and standardized reading score from winter of SY2020 (winter is the most recently available baseline test score for all students, as CPS did not administer tests in the spring of 2020). When including controls, we include fixed effects for grade and randomization block (based on grade band and language spoken in the classroom, English or Spanish). The outcomes of passed summer math and reading are equal to 1 when a student was marked as 'passed,' and equal to 0 when a student was marked as 'incomplete'; students marked as N/A or not eligible are marked as missing for these variables. Numeric summer math and reading grades are based on the following scale: 4 = A, 3 = B, 2 = C, 1 = D, 0 = F, and 0 = incomplete. Minutes spent "learning" on Khan Academy is marked as 0 for students in 3-8 grades who do not show up in the Khan data. When marked in the table, zeros are imputed for missing baseline academic variables and free/reduced lunch indicators, and flags for missingness are also included. 38% of students (2,508) in this sample are missing baseline academic variables from SY20.

Table 10: Effect of Outreach Phone Calls on Summer Learning Registration, by Grade Band (ITT)

	I	II	III	IV	V	VI
Grades 1-5						
Registration for SL	0.013+	0.009	0.013+	0.010	0.005	0.010
	(0.008)	(0.011)	(0.008)	(0.008)	(0.011)	(0.008)
Control Mean	0.158	0.158	0.158	0.158	0.158	0.158
Observations	8686	4628	8666	8686	4628	8666
Grades 6-8						
Registration for SL	0.037***	0.049***	0.037***	0.032**	0.044***	0.033**
	(0.010)	(0.013)	(0.010)	(0.010)	(0.013)	(0.010)
Control Mean	0.175	0.175	0.175	0.175	0.175	0.175
Observations	5881	3639	5849	5881	3639	5849
Controls, missing covariates imputed with zeros	X		X			
Controls, missing covariates dropped		X				
Students missing from CPS data included	X	X		X	X	
Full sample	X			X		
Restricted sample		X	X		X	X

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Controls include age, race, gender; indicators for free/reduced lunch, disability status, English Language Learner status, and students in temporary living situations; grade dummies, school site dummies, and baseline academic information from school year 2020, including GPA, course failures, attendance days, member days, and standardized math and reading scores from winter of SY2020 (winter is the most recently available baseline test score for all students, as CPS did not administer tests in the spring of 2020). We are never missing treatment status or baseline demographic information. When marked in the table, zeros are imputed for missing baseline academic variables from SY20 (listed above) and free/reduced lunch indicators, and flags for missingness are included. 43% of students (6,300) in this sample are missing baseline academic variables from SY20. The full sample includes all students in the randomization. The restricted sample drops students with missing baseline academic information or those who we do not observe in CPS's summer data. There are 52 students (19 control; 33 treatment) in the randomization who do not show up in CPS's summer data (we do not observe registration outcome for). To test whether the results are robust to inclusion of these 52 students, we run the model with and without these students in the sample. The models that include these students assume they did not register. The regressions where these students are kept in and marked as "not registered" are labeled as "Students missing from CPS data included." When not marked, these 52 students are dropped from the regressions. Standard errors are robust and in parentheses.

Table 11: Effect of Text Messaging on Attendance and Summer School Performance, By Grade Band (ITT)

	I	II	III	IV	V	VI
Grades 1-5						
Passed Summer Math, Grades 1-5	0.008	0.008	0.016	0.021	0.012	0.025
	(0.015)	(0.015)	(0.016)	(0.020)	(0.020)	(0.020)
Control Mean	0.684	0.684	0.684	0.684	0.684	0.684
Observations	3501	3501	3501	2038	2038	2038
Passed Summer Reading, Grades 1-5	0.009	0.010	0.014	0.014	0.008	0.017
	(0.015)	(0.015)	(0.015)	(0.020)	(0.019)	(0.020)
Control Mean	0.692	0.692	0.692	0.692	0.692	0.692
Observations	3539	3539	3539	2049	2049	2049
Summer Math GPA (4.0 = A), Grades 1-5	0.027	0.011	0.038	0.075	0.020	0.072
	(0.049)	(0.051)	(0.053)	(0.065)	(0.065)	(0.068)
Control Mean	1.931	1.931	1.931	1.931	1.931	1.931
Observations	3501	3501	3501	2038	2038	2038
Summer Reading GPA (4.0 = A), Grades 1-5	0.013	-0.011	0.004	0.044	-0.004	0.032
	(0.048)	(0.050)	(0.052)	(0.064)	(0.064)	(0.066)
Control Mean	1.954	1.954	1.954	1.954	1.954	1.954
Observations	3539	3539	3539	2049	2049	2049
Days Absent, Grades 1-5	-0.132	-0.126	-0.199	-0.203	-0.069	-0.359
	(0.232)	(0.237)	(0.254)	(0.311)	(0.308)	(0.322)
Control Mean	8.373	8.373	8.373	8.373	8.373	8.373
Observations	3939	3939	3939	2256	2256	2256
Grades 6-8						
Passed Summer Math, Grades 6-8	0.024+	0.021	0.020	0.047**	0.047*	0.044*
	(0.014)	(0.015)	(0.015)	(0.018)	(0.018)	(0.019)
Control Mean	0.712	0.712	0.712	0.712	0.712	0.712
Observations	3406	3406	3406	2234	2234	2234
Passed Summer Reading, Grades 6-8	0.017	0.020	0.021	0.037*	0.040*	0.038*
	(0.014)	(0.015)	(0.015)	(0.018)	(0.018)	(0.019)
Control Mean	0.723	0.723	0.723	0.723	0.723	0.723
Observations	3385	3385	3385	2224	2224	2224
Summer Math GPA (4.0 = A), Grades 6-8	0.063	0.059	0.057	0.117*	0.117*	0.109+
	(0.044)	(0.047)	(0.051)	(0.055)	(0.059)	(0.063)
Control Mean	1.872	1.872	1.872	1.872	1.872	1.872
Observations	3406	3406	3406	2234	2234	2234
Summer Reading GPA (4.0 = A), Grades 6-8	0.007	0.017	0.025	0.086	0.087	0.088
	(0.044)	(0.047)	(0.050)	(0.055)	(0.058)	(0.062)
Control Mean	1.872	1.872	1.872	1.872	1.872	1.872
Observations	3385	3385	3385	2224	2224	2224
Days Absent, Grades 6-8	-0.352+	-0.434+	-0.455+	-0.585*	-0.840**	-0.825**
	(0.214)	(0.229)	(0.245)	(0.268)	(0.284)	(0.302)
Control Mean	6.676	6.676	6.676	6.676	6.676	6.676
Observations	3528	3528	3528	2328	2328	2328
Controls, missing covariates imputed with zeros	X	X				
Controls, missing covariates dropped				X	X	
Teacher fixed effects included	X			X		
Full sample	X	X	X			
Restricted sample				X	X	X

Standard errors in parentheses
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Controls include age, race, gender; indicators for free/reduced lunch, disability status, English Language Learner status, and students in temporary living situations; grade dummies, and baseline academic information from school year 2020, including GPA, course failures, attendance days, member days, standardized math score from winter of SY2020, and standardized reading score from winter of SY2020 (winter is the most recently available baseline test score for all students, as CPS did not administer tests in the spring of 2020). Controls always includes grade fixed effects, and where flagged, also includes teacher fixed effects. Teacher fixed effects refer to separate fixed effects for a student's math teacher and reading teacher. In grades 1-5, students were mainly assigned to just one teacher who taught both subjects, but in grades 6-8, students were assigned to separate math and reading teachers. In the handful of cases where students have more than one math or more than one reading teacher, we randomly choose one teacher to assign them to. For students in the randomization who do not appear in CPS's summer grades file where we pull teacher information from, we assign them all the same classroom ID so that they do not drop from these regressions. This is about 9 percent of the randomized sample. The outcomes of passed summer math and reading are equal to 1 when a student was marked as 'passed,' and equal to 0 when a student was marked as 'incomplete'; students marked as N/A or not eligible are marked as missing for these variables. Numeric summer math and reading grades are based on the following scale: 4 = A, 3 = B, 2 = C, 1 = D, 0 = F, and 0 = incomplete. Minutes spent "learning" on Khan Academy is marked as 0 for students in 3-8 grades who do not show up in the Khan data. When marked in the table, zeros are imputed for missing baseline academic variables from SY20 (listed above) and free/reduced lunch indicators, and flags for missingness are included. 39% of students (2,968) in this sample are missing baseline academic variables from SY20. The full sample includes all students in the randomization. The restricted sample drops students with missing baseline academic information.

Table 12: Effect of Teacher's Aides on Student Math Outcomes in Summer and Fall, by Grade Band (ITT)

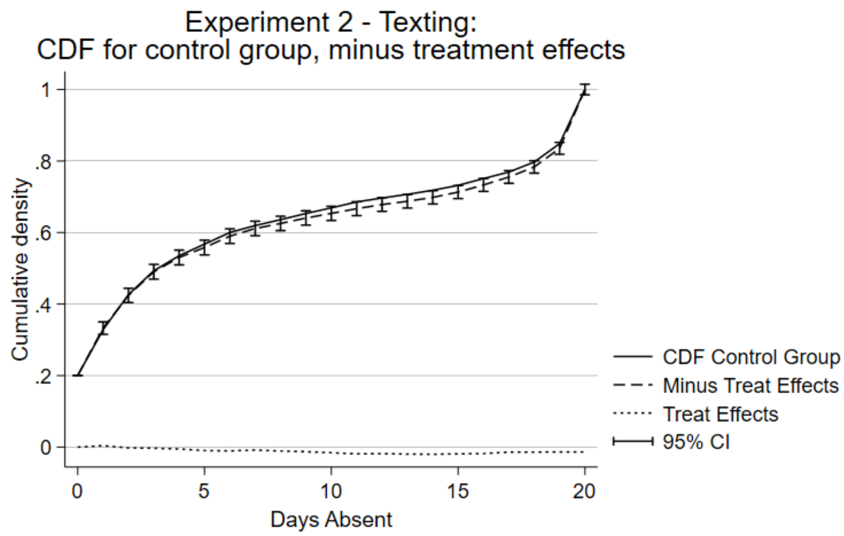
	I	II	III	IV
Grades 1-5				
Days Absent, Grades 1-5	0.221	0.453	0.581	0.839
	(0.431)	(0.456)	(0.529)	(0.521)
Control Mean	7.939	7.939	7.939	7.939
Observations	3820	2217	3820	2217
Passed Summer Math, Grades 1-5	0.036	0.012	0.022	-0.008
	(0.031)	(0.032)	(0.033)	(0.033)
Control Mean	0.660	0.660	0.660	0.660
Observations	3730	2169	3730	2169
Summer Math GPA (4 = A), Grades 1-5	0.119	0.014	0.084	-0.024
	(0.115)	(0.121)	(0.124)	(0.128)
Control Mean	1.849	1.849	1.849	1.849
Observations	3730	2169	3730	2169
Passed Math in SY21, Grades 1-5	0.027	0.012	0.024	0.012
	(0.019)	(0.023)	(0.019)	(0.023)
Control Mean	0.817	0.817	0.817	0.817
Observations	3622	2176	3622	2176
Math GPA in SY21 (4.0 = A), Grades 1-5	0.041	0.033	0.018	0.012
	(0.050)	(0.068)	(0.053)	(0.071)
Control Mean	1.688	1.688	1.688	1.688
Observations	3622	2176	3622	2176
Grades 6-8				
Days Absent, Grades 6-8	0.842+	0.722	1.095	1.071
	(0.507)	(0.575)	(0.680)	(0.759)
Control Mean	6.033	6.033	6.033	6.033
Observations	3641	2330	3641	2330
Passed Summer Math, Grades 6-8	-0.022	-0.010	-0.030	-0.024
	(0.035)	(0.038)	(0.036)	(0.040)
Control Mean	0.722	0.722	0.722	0.722
Observations	3650	2329	3650	2329
Summer Math GPA (4.0 = A), Grades 6-8	-0.085	-0.088	-0.110	-0.117
	(0.157)	(0.157)	(0.161)	(0.160)
Control Mean	1.901	1.901	1.901	1.901
Observations	3650	2329	3650	2329
Passed Math in SY21, Grades 6-8	0.056**	0.083***	0.057**	0.085***
	(0.020)	(0.021)	(0.020)	(0.021)
Control Mean	0.743	0.743	0.743	0.743
Observations	3237	2116	3237	2116
Math GPA in SY21 (4.0 = A), Grades 6-8	0.113*	0.142*	0.122*	0.162**
	(0.055)	(0.063)	(0.055)	(0.062)
Control Mean	1.505	1.505	1.505	1.505
Observations	3237	2116	3237	2116
Controls, missing covariates imputed with zeros	X			
Controls, missing covariates dropped		X		
Full randomization sample	X		X	
Restricted sample		X		X

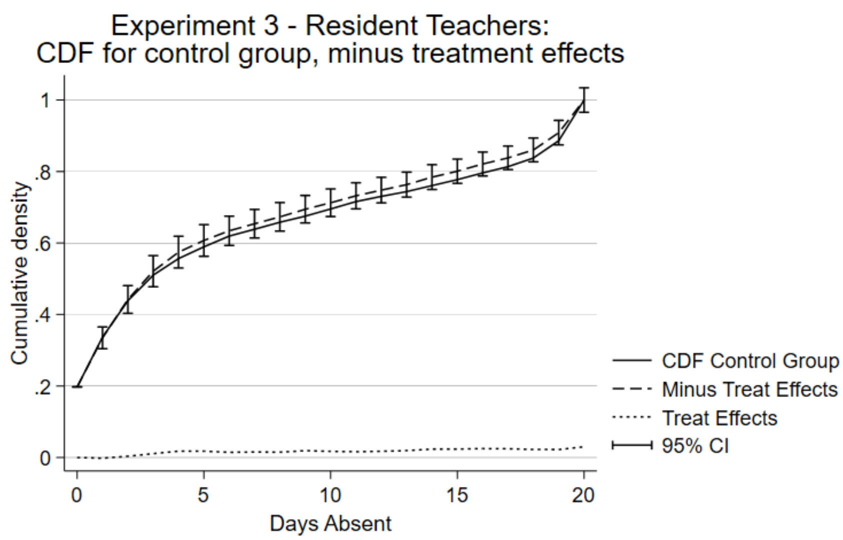
Standard errors in parentheses
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Intent-to-Treat is defined as the students assigned to the set of lead teachers we initially randomized to teacher's aides (0 = student assigned to lead teacher without a teacher's aide; 1 = student assigned to lead teacher with a teacher's aide). There are 246 control teachers and 85 treatment teachers. Controls include age, race, gender; indicators for free/reduced lunch, disability status, English Language Learner status, and students in temporary living situations; grade dummies, and baseline academic information from school year 2020, including GPA, course failures, attendance days, member days, standardized math score from winter of SY2020, and standardized reading score from winter of SY2020 (winter is the most recently available baseline test score for all students, as CPS did not administer tests in the spring of 2020). When including controls, we include fixed effects for grade and randomization block (based on grade band and language spoken in the classroom, English or Spanish). The outcomes of passed summer math and reading are equal to 1 when a student was marked as 'passed,' and equal to 0 when a student was marked as 'incomplete'; students marked as N/A or not eligible are marked as missing for these variables. Numeric summer math and reading grades are based on the following scale: 4 = A, 3 = B, 2 = C, 1 = D, 0 = F, and 0 = incomplete. Minutes spent "learning" on Khan Academy is marked as 0 for students in 3-8 grades who do not show up in the Khan data. When marked in the table, zeros are imputed for missing baseline academic variables and free/reduced lunch indicators, and flags for missingness are also included. Standard errors are robust and clustered at the teacher-level.

10 Figures

Figure 1: Experiments 2 and 3: Treatment effects across absences distribution





11 Appendix A: Program Details

11.1 Summer Learning Teacher's Aides' Schedule (Experiment 3)

	Grades 1-5	Teacher's Aide
8:30 AM - 10:30 AM	Literacy whole class meet	Takes attendance for both lead teachers; outreach to parents of absent students
10:30 AM - 12:30 PM	Math whole class meet	Joins ONE teacher's Math class **(alternate days)**
Afternoon		Additional support for parents and students
	Grades 6-8	Teacher's Aide
8:30 - 10:30 AM	Math class 1 whole class meet	Attends lead teacher's 1st Math class **every day**
10:30 AM - 12:30 PM	Math class 2 whole class meet	Attends lead teacher's 2nd math class **every day**
Afternoon		Additional support for parents and students

12 Appendix B: Sample

12.1 Sample

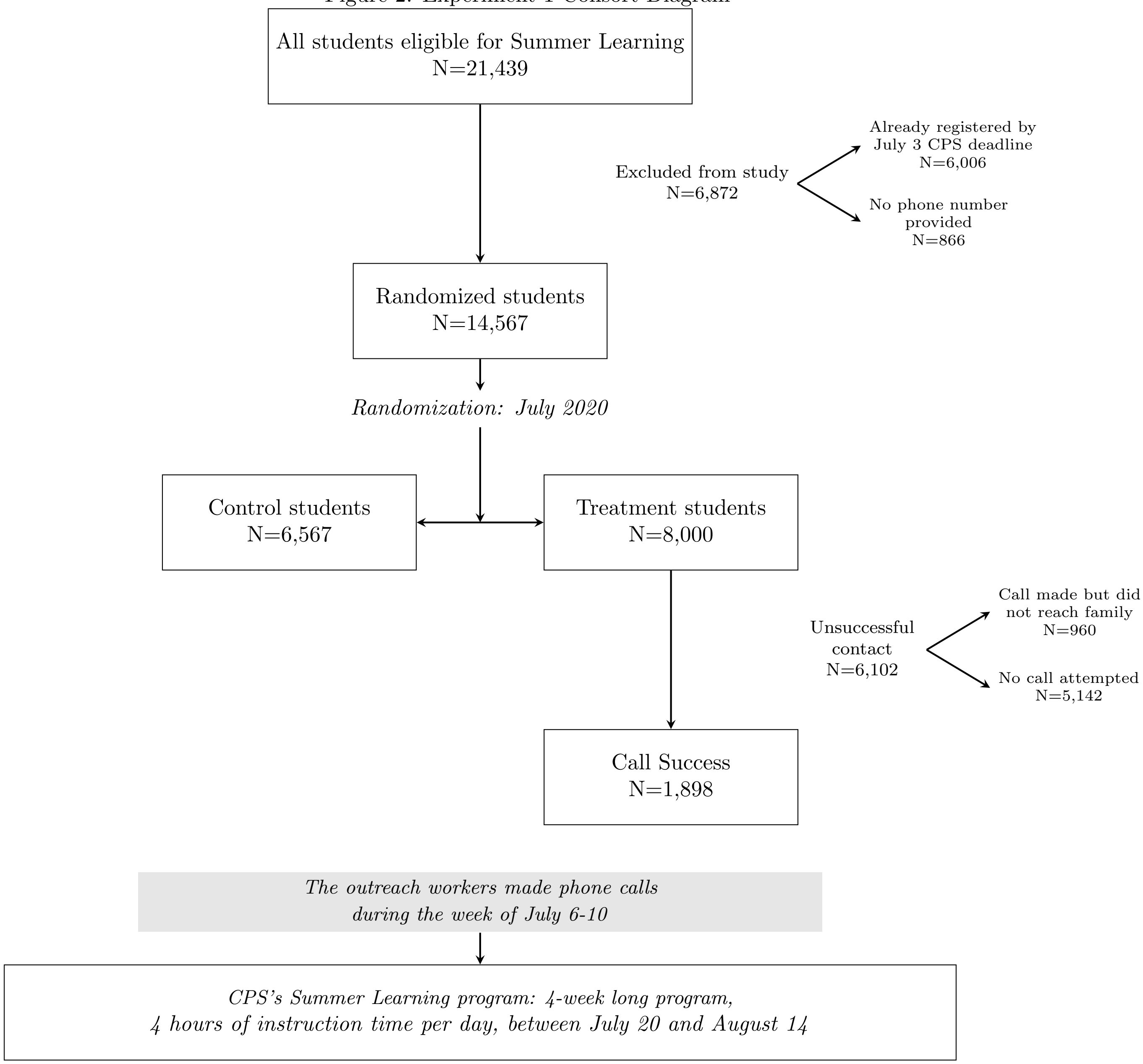
The following table compares baseline, end-of-year, SY20 information on average demographics of CPS students who were eligible for Summer Learning and those who were not.

Table 13: Summer Learning Eligible Students compared to CPS Overall

Variable Mean	Eligible for SL	CPS 1-8th students ineligible
Black students	0.51	0.34
Latinx students	0.44	0.46
Students eligible for free/reduced lunch	0.96	0.78
Male students	0.55	0.50
Students with a learning disability	0.08	0.07
Students in Temporary Living Situations	0.07	0.03
English Language Learners	0.21	0.22
GPA in SY20	3.14	3.41
Num. course failures/incompletes in 4th qtr SY20	5.19	0.95
Days attended SY20	144.96	149.25
Member days SY20	154.71	155.20
Stdzd math score, winter SY20	-0.52	0.06
Stdzd read score, winter SY20	-0.43	0.05

13 Appendix C: Consort Diagrams

Figure 2: Experiment 1 Consort Diagram



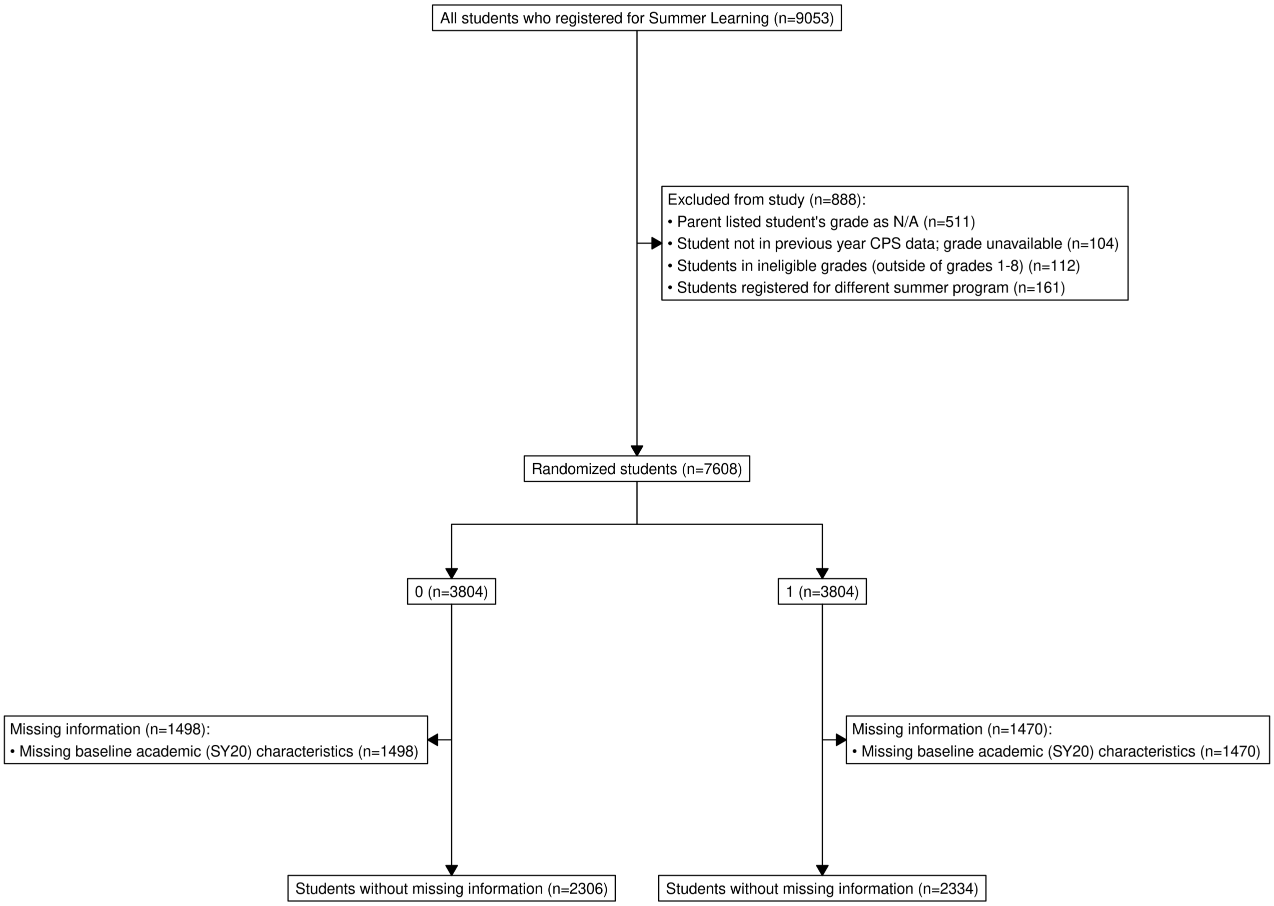


Figure 3: Experiment 2 Consort Diagram

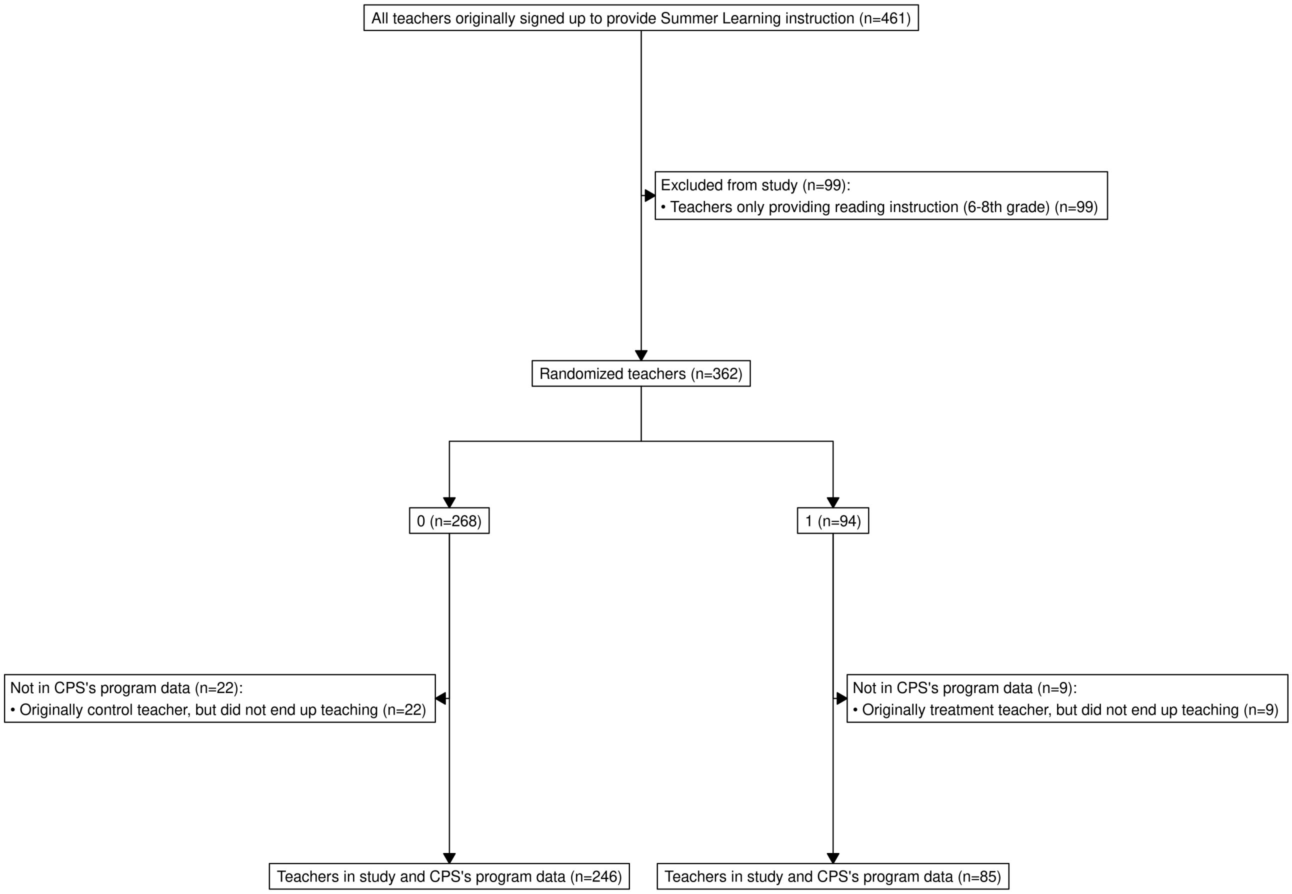


Figure 4: Experiment 3 Consort Diagram

14 Appendix D: Robustness checks

14.1 Experiment 1: Alternative definition of sample (ITT, TOT)

In the main analysis for Experiment 1, we present the Intent-to-Treat and Treatment-on-Treated effects of outreach workers calling families on their students' registration for the Summer Learning program. These models were pre-specified and include all students who were randomized into this experiment.

Ideally, all of the 8,000 students who were randomized onto outreach workers' lists would have received a call. In reality, outreach workers did not have enough time to contact all the students they were assigned to because CPS shortened the registration window unexpectedly from 2 weeks to 1 week. On average, an outreach worker attempted to contact only 38% of students in their list. This is 2,858 students.

The data suggests that the majority of outreach workers started making calls with the students on the top of their list and worked their way as far down the list as they were able. The lists of students assigned to outreach workers were randomly ordered. Therefore, the students higher on the list than the last student an outreach worker attempted to contact should be similar, on average, to the students below that student on the list. The random ordering on the list therefore offers an alternative experimental research design we can use to test for robustness of the results.

While not described in our pre-analysis plan, as a robustness check we test some alternative definitions for the ITT and TOT based on each outreach worker's "stopping point" student on their lists. In this alternative analysis, we define the treatment group as all the students on the top of an outreach worker's list down to the last student the outreach worker attempted to call. We present two ITT results in the following table: one in which every student *below* the last student called on each list is dropped from the sample, and one in which every student below the last student called on each list is moved into the control group. The latter estimate assumes that being assigned to treatment but having no contact from an outreach worker would have any effect on students' likelihood of registration. We also present a TOT where we define all students randomly assigned to treatment as the treatment group, and define a call attempt (the endogenous regressor in the TOT) to be all of those students up and until the last student called on each list.

The ITT and TOT results from each of these approaches look similar in significance and sign to one another. Because they more accurately capture attempted treatment than merely being placed on a list, they are larger than the ITT results in the main body of the paper, and more similar to the main TOT results.

Table 14: Effect of Outreach Phone Calls on Summer Learning Registration, Alternative Definitions

	I	II	III	IV	V	VI
Intent-to-Treat, Add Never-Called to Control	0.064***	0.064***	0.064***	0.060***	0.060***	0.060***
	(0.008)	(0.010)	(0.008)	(0.008)	(0.011)	(0.008)
Control Mean	0.161	0.161	0.161	0.161	0.161	0.161
Observations	14567	8267	14515	14567	8267	14515
Intent-to-Treat, Dropping Never-Called	0.061***	0.063***	0.062***	0.056***	0.058***	0.057***
	(0.008)	(0.011)	(0.008)	(0.008)	(0.011)	(0.009)
Control Mean	0.165	0.165	0.165	0.165	0.165	0.165
Observations	9939	5663	9907	9939	5663	9907
Treatment-on-Treated, Alternate Definition	0.053***	0.063**	0.054***	0.044**	0.052**	0.045**
	(0.015)	(0.020)	(0.015)	(0.015)	(0.020)	(0.015)
Control Complier Mean	0.168	0.168	0.168	0.168	0.168	0.168
Observations	14567	8267	14515	14567	8267	14515
Controls	X	X	X			
Zero Imputation	X		X			
Missingness Flags Included	X		X			
CPS Missing Included	X	X		X	X	
Full sample	X					
Drop students missing baseline info		X			X	
Drop students not in CPS registration data			X			X

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

14.2 Experiment 2 Secondary Outcomes (ITT Only)

In the following table, we show additional Intent-to-Treat effects for Experiment 2's weekly randomized text messages on the pre-specified secondary outcomes. The secondary outcomes we specified were students' engagement with other CAL platforms during the summer program, as well as students' academic outcomes in the following school year. The texting nudge did not have an effect on usage of other CAL platforms besides increasing students' usage of Google platforms. The Google activity variable provides an alternative measure of CAL participation by indicating the number of days that a student demonstrated some degree of activity in Google platforms. However, a weakness of this measure is that it does not distinguish between students who just log in and students who devote time to working in Google, such as contributing in a Google Document or joining a classroom meeting. We do not have more detailed data on what students did once they were logged in. This increase is also not consistent with there being no effect of text messages on the number of days students appear in Google data. Finally, we find that text messages had no impact on academic outcomes in SY21.

Table 15: Effect of Text Messaging on Other CAL Summer Outcomes and SY21 Outcomes (ITT)

	I	II	III	IV	V	VI
CAL Engagement Outcomes, Summer 2020						
ST Math Total Days Used	-0.669	-0.942*	-0.726	-0.034	-0.536	-0.509
	(0.449)	(0.462)	(0.474)	(0.722)	(0.734)	(0.711)
Control Mean	7.371	7.371	7.371	7.371	7.371	7.371
Observations	446	446	446	190	190	190
Number of Days in Google Data	-0.031	-0.036	-0.036	-0.013	-0.017	-0.014
	(0.022)	(0.022)	(0.022)	(0.025)	(0.025)	(0.025)
Control Mean	12.039	12.039	12.039	12.039	12.039	12.039
Observations	7143	7143	7143	4512	4512	4512
Activity in Google Meet, Classroom, Docs	0.189**	0.202**	0.195*	0.221*	0.207*	0.244*
	(0.071)	(0.078)	(0.085)	(0.091)	(0.098)	(0.104)
Control Mean	6.142	6.142	6.142	6.142	6.142	6.142
Observations	7143	7143	7143	4512	4512	4512
SY21 Outcomes						
GPA, SY21	0.010	0.018	0.025	0.033	0.033	0.042
	(0.021)	(0.020)	(0.022)	(0.026)	(0.025)	(0.027)
Control Mean	2.017	2.017	2.017	2.017	2.017	2.017
Observations	6996	6996	6996	4407	4407	4407
Passed Math in SY21	0.002	0.004	0.007	-0.002	-0.004	0.001
	(0.010)	(0.010)	(0.010)	(0.013)	(0.012)	(0.012)
Control Mean	0.789	0.789	0.789	0.789	0.789	0.789
Observations	6851	6851	6851	4307	4307	4307
Passed Reading in SY21	-0.000	0.002	0.005	-0.005	-0.006	-0.002
	(0.010)	(0.010)	(0.010)	(0.013)	(0.012)	(0.012)
Control Mean	0.784	0.784	0.784	0.784	0.784	0.784

	(1)	(2)	(3)	(4)	(5)	(6)
Observations	6942	6942	6942	4389	4389	4389
Math GPA in SY21 (4.0 = A)	0.018	0.023	0.034	0.045	0.040	0.058+
	(0.027)	(0.027)	(0.028)	(0.035)	(0.033)	(0.035)
Control Mean	1.608	1.608	1.608	1.608	1.608	1.608
Observations	6851	6851	6851	4307	4307	4307
Reading GPA in SY21 (4.0 = A)	-0.000	0.003	0.014	0.019	0.010	0.027
	(0.027)	(0.026)	(0.028)	(0.034)	(0.033)	(0.034)
Control Mean	1.601	1.601	1.601	1.601	1.601	1.601
Observations	6942	6942	6942	4389	4389	4389
Controls, missing covariates imputed with zeros	X	X				
Controls, missing covariates dropped				X	X	
Teacher fixed effects included	X			X		
Full sample	X	X	X			
Restricted sample				X	X	X

Standard errors in parentheses

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

14.3 Experiment 2 Effects on Days Absent (ITT), No Top-Coding

In this table, we show the Intent-to-Treat effect of text messaging on days absent for each of the models, *without* top-coding absent days at the maximum program length of 20 days.

Table 16: Effect of Text Messaging on Days Absent (ITT), No Top-Coding

	I	II	III	IV	V	VI
Days Absent	-0.287+	-0.355*	-0.381*	-0.467*	-0.581**	-0.721**
	(0.165)	(0.175)	(0.187)	(0.216)	(0.225)	(0.236)
Control Mean	7.759	7.759	7.759	7.759	7.759	7.759
Observations	7467	7467	7467	4584	4584	4584
Controls, missing covariates imputed with zeros	X	X				
Controls, missing covariates dropped				X	X	
Teacher fixed effects included	X			X		
Full sample	X	X	X			
Restricted sample				X	X	X

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

14.4 Experiment 2 Median Regressions on Days Absent (ITT)

In order to test whether the results are driven by outliers, and considering the cases of reported absent days that are longer than the length of the Summer Learning program, we also run median regressions for the Intent-to-Treat effect of Experiment 2 on students' days absent. When we choose to top-code days absent at 20 days, that changes the conditional average, and therefore the OLS estimates. However, this does not impact the conditional median. The following table displays the results from running median regressions on days absent, without top-coding, for Experiment 2. We see consistent decreases in the median days absent with varying degrees of significance across the main models.

Table 17: Effect of Text Messaging on Days Absent, Median Regression

	I	II	III	IV	V	VI
Days Absent	-0.250+	-0.194	-0.205	-0.429*	-0.296+	-0.385*
	(0.140)	(0.135)	(0.137)	(0.176)	(0.163)	(0.159)
Constant	0.401	0.057	4.000***	0.716	4.071	3.385***
	(2.471)	(2.053)	(0.137)	(4.618)	(3.521)	(0.162)
Observations	7467	7467	7467	4584	4584	4584
Controls, missing covariates imputed with zeros	X	X				
Controls, missing covariates dropped				X	X	
Teacher fixed effects included	X			X		
Full sample	X	X	X			
Restricted sample				X	X	X

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

14.5 Experiment 3 Primary Outcomes, Imputing Means for Missing Covariates (ITT Only)

In the following table, we run a robustness check where ITT control means (by randomization block) are imputed for missing baseline academic variables and free/reduced lunch indicators from SY20, and missingness flags are included. We include the full standard list of controls here as well. These results match the findings from the other models in the main section of the paper, with significant and positive effects of teacher's aides on SY21 math outcomes.

Table 18: Effect of Teacher's Aides (ITT), Imputing Means for Missing Baseline Covariates

	I
Summer 2020	
Khan Total Learning Minutes	-10.048
	(16.684)
Control Mean	136.086
Observations	7097
Days Absent	0.467
	(0.338)
Control Mean	7.021
Observations	7461
Passed Summer Math (0 = incomplete)	0.012
	(0.023)
Control Mean	0.690
Observations	7380
Summer Math GPA (4.0 = A)	0.032
	(0.098)
Control Mean	1.874
Observations	7380
SY21	
Passed Math in SY21 (0 = incomplete)	0.041**
	(0.013)
Control Mean	0.782
Observations	6859
Math GPA in SY21 (4.0 = A)	0.084*
	(0.037)
Control Mean	1.602
Observations	6859

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

14.6 Experiment 3 Secondary Outcomes (ITT Only)

Due to the math-focused nature of Experiment 3, we focus on the math outcomes in the main text. But for broader context, here, we provide the findings on reading outcomes. As shown in the following table, it does not look like the teacher's aide intervention had much of an effect on any reading-related outcomes during the summer or the following school year.

Table 19: Effect of Teacher's Aides on Reading Outcomes, Summer and SY21 (ITT)

	I	II	III	IV
Summer 2020				
Passed Summer Reading (0 = incomplete)	-0.002	-0.004	-0.014	-0.020
	(0.023)	(0.026)	(0.026)	(0.028)
Control Mean	0.708	0.708	0.708	0.708
Observations	7347	4460	7347	4460
Summer Reading GPA (4.0 = A)	0.055	0.037	0.018	-0.004
	(0.085)	(0.092)	(0.090)	(0.097)
Control Mean	1.884	1.884	1.884	1.884
Observations	7347	4460	7347	4460
SY21				
Passed Reading in SY21 (0 = incomplete)	0.018	0.017	0.017	0.018
	(0.016)	(0.016)	(0.016)	(0.018)
Control Mean	0.779	0.779	0.779	0.779
Observations	6967	4388	6967	4388
Reading GPA in SY21 (4.0 = A)	0.046	0.052	0.034	0.046
	(0.043)	(0.048)	(0.047)	(0.053)
Control Mean	1.593	1.593	1.593	1.593
Observations	6967	4388	6967	4388
Controls, missing covariates imputed with zeros	X			
Controls, missing covariates dropped		X		
Full randomization sample	X		X	
Restricted sample		X		X

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

14.7 Experiment 3 TOT Primary Outcomes

In our pre-analysis plan, we outlined several alternative definitions of TOT in order to attempt to isolate, as best we could, whether there were specific components of the teacher's aide intervention that were most helping students. However, while the assignment of teacher's aides to classrooms was random, the specific behaviors and tasks they worked on were not random, but up to each teacher's aide's discretion. We provided guidelines and instructions on what they should do, but in the qualitative surveys we collected, we could observe that not all teacher's aides were doing the same things in their classrooms.

Using these qualitative survey responses, we constructed different treatment intensity definitions, and focused on relevant outcomes for each definition. We created several TOT versions of interest, defining treatment and relevant outcomes as following:

- **For all outcomes:** Teacher's aides providing any form of support to the teacher they were assigned to, the students in the classroom, or their families. We use randomization into treatments as an instrument (IV) for receiving any forms of support from teacher's aides to estimate the impact of teacher's aides' support. Note this treatment is defined at the classroom- (and not at the student-) level.
- **For attendance as outcome:** Teacher's aides reaching out to any families of students who were absent; We use randomization into treatments as an instrument (IV) for teacher's aides providing attendance support to estimate the impact of teacher's aides reaching out to families of absent children on student attendance. Note this treatment is defined at the classroom- (and not at the student-) level.
- **For outcomes measuring engagement with Khan platform:** Teacher's aides providing engagement support with Khan academy by holding small-group sessions with students; We use randomization into treatments as an instrument (IV) for teacher's aides providing small-group Khan support to estimate the impact of Khan support on engagement with Khan. Note this treatment is defined at the classroom- (and not at the student-) level.

14.7.1 Experiment 3 TOT, based on actual treatment assignment

As noted in the take-up section (see Table 5), one teacher initially assigned to control became a treatment teacher. These results estimate the TOT effects using an IV approach where we use randomization as an instrument for taking up treatment (where treatment is defined as receiving a teacher's aide). These results, with significant and positive effects on SY21 math outcomes, look the same as the ITT results in Table 8.

Table 20: Effect of Teacher's Aides on Student Outcomes (TOT)

	I	II	III	IV
Primary Outcomes				
Khan Total Learning Minutes	-10.235	-3.683	-11.523	-8.014
	(16.769)	(18.604)	(19.236)	(22.103)
Control Mean	135.238	135.238	135.238	135.238
Observations	7097	4569	7097	4569
Days Absent	0.482	0.549	0.800+	0.945*
	(0.340)	(0.380)	(0.447)	(0.479)
Control Mean	7.352	7.352	7.352	7.352
Observations	7461	4547	7461	4547
Secondary Outcomes, Summer 2020				
Passed Summer Math (0 = incomplete)	0.011	0.004	-0.003	-0.016
	(0.024)	(0.025)	(0.025)	(0.027)
Control Mean	0.675	0.675	0.675	0.675
Observations	7380	4498	7380	4498
Summer Math GPA (4.0 = A)	0.029	-0.031	-0.016	-0.079
	(0.099)	(0.102)	(0.104)	(0.106)
Control Mean	1.832	1.832	1.832	1.832
Observations	7380	4498	7380	4498
Secondary Outcomes, SY21				
Passed Math in SY21	0.041**	0.046**	0.039**	0.048**
	(0.013)	(0.016)	(0.014)	(0.016)
Control Mean	0.783	0.783	0.783	0.783
Observations	6859	4292	6859	4292
Math GPA in SY21 (4.0 = A)	0.083*	0.095*	0.067+	0.086+
	(0.037)	(0.048)	(0.039)	(0.048)
Control Mean	1.592	1.592	1.592	1.592
Observations	6859	4292	6859	4292
Controls, missing covariates imputed with zeros	X			
Controls, missing covariates dropped		X		
Full randomization sample	X		X	
Restricted sample		X		X

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

14.7.2 Experiment 3 TOT, based on indication of teacher's aides providing any form of support

There are 80 teachers whose teacher's aide indicated in the qualitative surveys that they were providing any form of support. These results estimate the TOT effects using an IV approach where we use randomization as an instrument for taking up treatment (where treatment is defined as having a teacher's aide who indicated that they provided any form of support). These results, with significant and positive effects on SY21 math outcomes, look the same as the ITT results in Table 8.

Table 21: Effect of Teacher's Aides on Student Outcomes (TOT), Based on Any Evidence of TA Support

	I	II	III	IV
Primary Outcomes				
Khan Total Learning Minutes	-11.008	-3.934	-12.467	-8.611
	(18.097)	(19.896)	(20.851)	(23.782)
Control Mean	137.555	137.555	137.555	137.555
Observations	7097	4569	7097	4569
Days Absent	0.524	0.587	0.873+	1.017+
	(0.374)	(0.411)	(0.498)	(0.527)
Control Mean	7.076	7.076	7.076	7.076
Observations	7461	4547	7461	4547
Secondary Outcomes, Summer 2020				
Passed Summer Math (0 = incomplete)	0.012	0.005	-0.003	-0.017
	(0.026)	(0.027)	(0.027)	(0.029)
Control Mean	0.680	0.680	0.680	0.680
Observations	7380	4498	7380	4498
Summer Math GPA (4.0 = A)	0.031	-0.033	-0.018	-0.085
	(0.108)	(0.109)	(0.114)	(0.115)
Control Mean	1.868	1.868	1.868	1.868
Observations	7380	4498	7380	4498
Secondary Outcomes, SY21				
Passed Math in SY21	0.044**	0.049**	0.043**	0.052**
	(0.014)	(0.017)	(0.015)	(0.017)
Control Mean	0.784	0.784	0.784	0.784
Observations	6859	4292	6859	4292
Math GPA in SY21 (4.0 = A)	0.090*	0.101*	0.073+	0.092+
	(0.040)	(0.051)	(0.042)	(0.051)
Control Mean	1.587	1.587	1.587	1.587
Observations	6859	4292	6859	4292
Controls, missing covariates imputed with zeros	X			
Controls, missing covariates dropped		X		
Full randomization sample	X		X	
Restricted sample		X		X

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

14.7.3 Experiment 3 TOT, based on indication of outreach to parents and families

There are 48 teachers whose teacher's aide indicated in the qualitative surveys that they were reaching out to parents and families. These results estimate the TOT effects using an IV approach where we use randomization as an instrument for taking up treatment (where treatment is defined as having a teacher's aide who indicated that they reached out to parents and families). We were interested in investigating whether having a teacher's aide who made phone calls to families of absent students had a negative impact on days absent; however, we see here that there are still no effects on days absent.

Table 22: Effect of Teacher's Aides on Days Absent (TOT), Based on TAs Reaching out to Families

	I	II	III	IV
Days Absent	0.829	0.936	1.412+	1.669+
	(0.593)	(0.660)	(0.821)	(0.894)
Control Mean	6.881	6.881	6.881	6.881
Observations	7461	4547	7461	4547
Controls, missing covariates imputed with zeros	X			
Controls, missing covariates dropped		X		
Full randomization sample	X		X	
Restricted sample		X		X

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

14.7.4 Experiment 3 TOT, based on indication of conducting small-group sessions

There are 35 teachers whose teacher's aide indicated in the surveys that they were conducting small-group (2-on-1) sessions on Khan Academy. These results estimate the TOT effects using an IV approach where we use randomization as an instrument for taking up treatment (where treatment is defined as having a teacher's aide who indicated they were holding small group sessions with students). We were interested in whether having a teacher's aide who held these Khan Academy-related sessions had a positive impact on students' Khan use. However, we see here that there are still no effects on Khan Total Learning Minutes.

Table 23: Effect of Teacher's Aides on Khan Use (TOT), Based on TAs Conducting 2-on-1 Sessions

	I	II	III	IV
Khan Total Learning Minutes	-22.505	-8.090	-25.519	-17.771
	(37.413)	(41.060)	(43.724)	(49.925)
Control Mean	175.342	175.342	175.342	175.342
Observations	7097	4569	7097	4569
Controls, missing covariates imputed with zeros	X			
Controls, missing covariates dropped		X		
Full randomization sample	X		X	
Restricted sample		X		X

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

15 Appendix E: Scripts for Interventions

15.1 Experiment 1: Outreach worker recruitment scripts

Summer Learning Recruitment First Contact Script

Hi, am I speaking with [parent name]?

My name is [staff name], I am calling from Chicago Public Schools to encourage you to register your child, [student name], for the virtual Summer Learning Program. Based on your child's fourth quarter grades in reading or math, your child qualifies and is recommended to participate in this remote program. Are you familiar with Summer Learning?

A **If not, introduce program:** Summer Learning is a free daily online program, where your child will receive whole group and small group instruction in reading, math, and social emotional learning. Your child will also have the opportunity to improve their fourth quarter grade from Incomplete to Pass. The program runs from July 20th to August 14th and is scheduled Monday through Friday, 8:30 a.m. until 12:30 p.m.. The purpose is to help students re-engage with the material so that they can be prepared to return to the next grade level in the fall.

B **Continue or if yes, skip to:** The program offers an opportunity for students who, for any reason, might have struggled to engage with virtual learning this spring. Because the learning will be remote, as in the spring, we would like to understand any barriers that families encountered previously. Did you or your child have any particular barriers in accessing or engaging with the online learning during the spring?

[Record responses in tracker, including no barriers.]

C **Continue or if no barriers, skip to:** I can walk you through the registration process now or if you would like to think about it I can give you the registration link. Are you interested in registering your child for Summer Learning?

(a) **If hard no, ask:** Could I ask why you are not interested in the program?

[Record response in tracker.]

[End call.] Thank you for your time. If you change your mind, you can still register for the program by following the link in our previous email notification or by calling your home school.

(b) **If unsure, or soft no, ask:** Do you have any questions about the program that I could answer for you now?

[Refer to the Q&A as needed. Ask about registration again.]

(c) **If yes or would like to think about it, ask:** Would like help registering for the program now or later? Or would you like me to send you the registration link?

[Refer to the site and support registration or email link, as needed.]

Do you have access to your child's CPS email address and password?

[If no, provide contact information to retrieve it.]

Communication about the program will be primarily directed at this email address, so it will be important that you check it often.

We look forward to having [student name] in the Summer Learning Program. Before I go, could I ask you what made you interested in joining the program?

[Record response in tracker.]

[End call.] Thank you for your time. You will get a confirmation email between July 13 and July 17, as well as an invitation to join the Google Classroom Homeroom. The confirmation email will be sent to you, as well as your child's CPS email address. If you have any questions before the program begins, please contact [contact information here].

Summer Learning Registration FAQs

I don't have a computer. How do I get one?

Fill out the registration form and your home school will reach out to you.

I don't have internet access. How do I get it for free?

Fill out the registration form and the IT department will reach out to you.

I don't have access to my child's email address and password. How can I recover access?

Please reach out to [contact information here].

How is this going to be different from the school year?

The program includes daily interaction with teachers and peers. There are structured lessons that are easier to understand. There is more clear communication between teachers, students, and parents.

How do I log on during the program?

You will get a confirmation email between July 13 and July 17, as well as an invitation to join the Google Classroom Homeroom. The confirmation email will be sent to the parent who completes the form, as well as your child's CPS email address.

How much is the cost of the program?

There is no cost to you for participation!

Contact List:

- School directory
- Parent Hotline: [phone number here]
- FACE (Outreach Worker) Line: [phone number here]
- Registration Link: cps.edu/SummerLearningRegistration

Summer Learning Recruitment First Contact Script (Spanish)

Hola, hablo con [nombre de padres]?

Mi nombre es [nombre de empleado], llamo de Chicago Public Schools para motivarle a que registre a [nombre de estudiante], para el programa de aprendizaje virtual, Summer Learning. Según las calificaciones del cuarto plazo de lectura o matemáticas, su estudiante califica y se recomienda que participe en este programa remoto. Usted tiene familiaridad con el programa de Summer Learning?

A **If not, introduce program:** Summer Learning es un programa diario y gratis, donde el estudiante va a recibir instrucción de lectura, matemáticas, y aprendizaje social y emocional, en grupos completos y pequeños. Los estudiantes también tienen la oportunidad de ascender sus calificaciones del cuarto plazo de Incompleto a Aprobado. El programa dura del 20 de julio hasta el 14 de agosto y está programado de lunes a viernes, de las 8:30 de la mañana hasta las 12:30 de la tarde. El propósito es ayudar a los estudiantes volver a interactuar con el material para que se puedan preparar a regresar al próximo grado en el otoño.

B **Continue or if yes, skip to:** El programa ofrece una oportunidad para estudiantes que, por cualquier razón, pudieron tener dificultades con el aprendizaje virtual esta primavera. Ya que el aprendizaje será remoto, como en la primavera, quisiéramos entender cualquier obstáculos que se le presentaron anteriormente. Usted o el estudiante tuvo algún obstáculo en particular para acceder o interactuar con el aprendizaje virtual durante la primavera?

[Record responses in tracker, including no barriers.]

C **Continue or if no barriers, skip to:** Yo puedo guiarle a través del proceso de registro ahora o si quiere pensarlo le puedo dar el enlace del registro. Le interesa registrar al estudiante para Summer Learning?

(a) **If hard no, ask:** Puedo preguntarle por qué no le interesa el programa?

[Record response in tracker.]

[End call.] Gracias por su tiempo. Si cambia de opinión, todavía puede registrarse siguiendo el enlace en nuestro correo electrónico anterior o llamando a su escuela de origen.

(b) **If unsure, or soft no, ask:** Tiene alguna pregunta sobre el programa que le pueda contestar ahora?

[Refer to the Q&A as needed. Ask about registration again.]

(c) **If yes or would like to think about it, ask:** Quiere ayuda registrándose ahora o más tarde? O quiere que le mande el enlace del registro?

[**Refer to the site and support registration or email link, as needed.**]

Tiene acceso al correo electrónico y la contraseña del estudiante?

[**If no, provide contact information to retrieve it.**]

Comunicaciones del programa serán dirigidas a ese correo electrónico, entonces será importante que lo revise a menudo.

Esperamos tener a [nombre de estudiante] en el programa de Summer Learning. Antes de irme, le puedo preguntar que le intereso para unirse al programa?

[**Record response in tracker.**]

[**End call.**] Gracias por su tiempo. Va a recibir confirmación por correo electrónico entre el July 13 and July 17, así como una invitación para unirse al Google Classroom Homeroom. El correo de confirmación se le mandará a usted, así como al correo del estudiante. Si tiene preguntas antes de que empiece el programa, por favor [contact information here].

Summer Learning Registration FAQs (Spanish)

No tengo computadora, cómo consigo una?

Complete el formulario de registración y su escuela de origen se comunicará con usted.

No tengo acceso de internet, cómo lo obtengo gratis?

Complete el formulario de registración y su escuela de origen se comunicará con usted.

No tengo acceso al correo electrónico y la contraseña. Cómo lo puedo recuperar?

Por favor [contact information here].

Cómo será esto diferente del año escolar?

El programa incluye interacción diaria con los maestros y compañeros. Hay lecciones estructuradas que son más fáciles de entender. Hay comunicación clara entre los maestros, los estudiantes, y los papás.

Cómo inicio sesión durante el programa?

Va a recibir confirmación por correo electrónico entre el July 13 and July 17, así como una invitación para unirse a Google Classroom Homeroom. El correo electrónico de confirmación se enviará al padre que completa el formulario, así como a el correo del estudiante.

Cuánto cuesta el programa?

Es gratis para participar!

Lista de Contactos:

- School directory
- Parent Hotline: [phone number here]
- FACE (Outreach Worker) Line: [phone number here]
- Registration Link: cps.edu/SummerLearningRegistration

15.2 Experiment 2: Content of text messages

Type of text	Text language	Date sent
Intro Summer Learning text	CPS' Summer Learning begins tomorrow. From 7/20-8/14, your child should log on to Google Classroom by 8:30 a.m. M-F. Para el programa Summer Learning durante el 20 de julio al 14 de agosto, su hijo deberá ingresar a Google Classroom antes de 8:30 a.m. de lunes a viernes.	Sunday 7/19 between 4-5 PM
Class reminder text	Reminder: Summer Learning begins today. Your child can log on to Google Classroom with their CPS email by 8:30 a.m. Recordatorio: El programa Summer Learning empieza hoy. Su hijo deberá ingresar a Google Classroom antes de las 8:30 a.m. usando su correo electrónico de las CPS.	Monday 7/20 8:15 AM
Week 2 Text	Friendly reminder: Summer Learning meets 830AM-1230PM M-F this week. Parent hotline: 7735535437 for any issues. Recordatorio: Summer Learning es de 830AM-1230PM L-V esta semana. Linea de padres: 7735535437 por cualquier problema.	Monday 7/27 8:15 AM
Week 3 Text	Friendly reminder: Summer Learning meets 830AM-1230PM M-F this week. Parent hotline: 7735535437 for any issues. Recordatorio: Summer Learning es de 830AM-1230PM L-V esta semana. Linea de padres: 7735535437 por cualquier problema.	Monday 8/3 8:15 AM
Week 4 Text	Welcome to the final week of Summer Learning! We're looking forward to seeing your child on Google Classroom. Bienvenido a la última semana de Summer Learning! Esperamos ver a su hijo diario en Google Classroom.	Monday 8/10 8:15 AM

15.3 Experiment 3: Content of wellness check scripts used by teacher's aides when calling families

Summer Learning Wellness Check Script, First Contact

Hi, am I speaking with [parent name]?

My name is [staff name], I am calling from the Chicago Public Schools Summer Learning Program. I wanted to introduce myself because I will be the Resident Teacher for the classroom in which [student name] will be working.

Do you have about 10 minutes to speak? I just wanted to explain my role in the classroom and answer any question you or [student name] may have about the program so far.

A **If not, schedule another call:** Is there a better time I could call you later today or this week?

- **If yes, reschedule:** I look forward to speaking with you soon. If you have any questions before I call, please contact the lead teacher over email at [email address].
- **If unsure, or soft no:** I understand. I will try to check in again later in the week. If you do find time, or have any questions before I call, please the lead teacher over email at [email address].
- **If hard no, ask:** I understand. Would it be all right if I gave you a call next week to check in and see if I can be of any help to you or [student name]?

  [**Record responses in tracker. End call.**]: Thank you for your time.

B **If yes, skip to:** Great, thank you. As a Resident Teacher, I will be partnering with [student name]'s teacher throughout the Summer Learning Program to offer additional support. I'll visit the classroom 2-3 days each week and work with the students in small groups or individually, as needed. If a student needs additional support, we might work together on an additional assignment to make sure they are caught up with the material. I'll also call periodically to check in and ask how things are going for you and [student name]. If there are ever any issues, I am happy to help troubleshoot.

  [**Schedule student for small-group review if needed.**]

  If needed, ask: Do you have access to your child's CPS email address and password? Communication about the program will be primarily directed at the student's email address, so it will be important that you check it often.

  [**If no, provide contact information to retrieve it.**]

  [**Record responses in tracker. End call.**]: Thank you for your time.

Summer Learning Wellness Check Script, Follow-Up Contact

Hi, am I speaking with [parent name]?

My name is [staff name], the Resident Teacher from the Summer Learning Program. How has the program been going so far? Have you or [student name] had any issues so far?

[Record responses in tracker. Refer to the Q&A as needed.]

[End call.] Thank you for your time. I will call back again next week to check in. If you have any questions before I call, please contact the lead teacher at [email address].

Summer Learning Missed Class Script

Hi, am I speaking with [parent name]?

A **If no previous wellness check, or unsure, introduce:** My name is [staff name], I am calling from the Chicago Public Schools Summer Learning Program. I am the Resident Teacher for the classroom in which [student name] will be working.

B **If previously introduced, skip to:** This is [staff name], I am the Resident Teacher from the Chicago Public Schools Summer Learning Program.

[Continue]: I noticed that [student name] was not in class [day]. I wanted to check in and see if everything was all right or if [student name] needed any help logging on.

[Record responses in tracker. Refer to the Q&A as needed.]

[End call.]: Thank you for your time. I will call back again next week to check in. If you have any questions before I call, please contact the lead teacher at [email address].

Summer Learning Wellness Check Script, First Contact (Spanish)

Hola, hablo con [nombre de padres]?

Mi nombre es [nombre de empleado], llamo del programa Summer Learning de Chicago Public Schools. Quería presentarme porque seré el Resident Teacher en el aula donden[nombre de estudiante] trabajará.

Puede hablar por unos 10 minutos? Por ahora, solo quiero explicarle mi rol en el aula y responder cualquier pregunta que usted o [nombre de estudiante] pueda tener sobre el programa.

A **If not, schedule another call:** Hay un mejor momento para llamarle de nuevo hoy más tarde, o esta semana?

- **If yes, reschedule:** Hablamos pronto. Si tiene alguna pregunta antes de que le llame, por favor contacte al maestro principal por correo a [correo electrónico].
- **If unsure, or soft no:** Entiendo. Intentaré llamarle otra vez más tarde esta semana. Si tiene tiempo o cualquier pregunta antes de que llame, por favor contacte al maestro principal por correo a [correo electrónico].
- **If hard no, ask:** Entiendo. Estaría bien si le llamo la próxima semana para ver cómo van las cosas y si puedo ayudarle a usted o a [nombre de estudiante]?

  [**Record responses in tracker. End call.**]: Gracias por su tiempo.

B **If yes, skip to:** Bien, gracias. Como Resident Teacher, trabajaré con el maestro de [nombre de estudiante] durante el programa de Summer Learning para ofrecer apoyo adicional. Voy a visitar el aula 2-3 días cada semana y trabajaré con los estudiantes en grupos pequeños o individualmente, como se necesite. Si algún estudiante necesita apoyo adicional, tal vez trabajaremos juntos en un ejercicio adicional para asegurar que haya recuperado el material. También llamaré de vez en cuando para ver cómo van las cosas para usted y [nombre de estudiante]. Si hay cualquier problema, con gusto le ayudo a solucionarlo.

[Schedule student for small-group review if needed.]

If needed, ask: Tiene acceso al correo electrónico y la contraseña del estudiante? Cualquier comunicación sobre el programa será dirigida a ese correo electrónico, entonces será importante que lo revise a menudo.

[If no, provide contact information to retrieve it.]

[**Record responses in tracker. End call.**]: Thank you for your time.

Summer Learning Wellness Check Script, Follow-Up Contact (Spanish)

Hola, hablo con [nombre de padres]?

Mi nombre es [nombre de empleado], soy el Resident Teacher del programa Summer Learning. ¿Cómo va el programa hasta ahora? ¿Ha tenido algún problema usted o [nombre de estudiante]?

[Record responses in tracker. Refer to the Q&A as needed.]

[End call.] Gracias por su tiempo. Volveré a llamar la próxima semana para ver cómo van las cosas. Si tiene cualquier pregunta antes de que llame, por favor contacte al maestro principal, por correo a [correo electrónico].

Summer Learning Missed Class Script (Spanish)

Hola, hablo con [nombre de padres]?

A **If no previous wellness check, or unsure, introduce:** Mi nombre es [nombre de empleado], llamo del programa Summer Learning de Chicago Public Schools. Soy el Resident Teacher en el aula donde [nombre de estudiante] trabajará.

B **If previously introduced, skip to:** Mi nombre es [nombre de empleado], soy el Resident Teacher del programa Summer Learning.

[Continue]: Me di cuenta de que [nombre de estudiante] no estuvo en clase [día]. Quería ver cómo van las cosas, si todo está bien, o si [nombre de estudiante] necesita ayuda iniciando la sesión.

[Record responses in tracker. Refer to the Q&A as needed.]

[End call.]: Gracias por su tiempo. Volveré a llamar la próxima semana para ver cómo van las cosas. Si tiene cualquier pregunta antes de que llame, por favor contacte al maestro principal, por correo a [correo electrónico].