ARTICLE IN PRESS

Organizational Behavior and Human Decision Processes xxx (xxxx) xxx–xxx

Contents lists available at ScienceDirect

Organizational Behavior and Human Decision Processes

journal homepage: www.elsevier.com/locate/obhdp

Using behavioral insights to increase attendance at subsidized preschool programs: The Show Up to Grow Up intervention

Ariel Kalil[a,*], Susan E. Mayer[a], Sebastian Gallegos[b]

[a] Harris School of Public Policy, University of Chicago, 1307 E. 60th Street, Chicago, IL 60637, United States
[b] Inter-American Development Bank, 1300 New York Ave, NW, Washington, DC 20577, United States

ABSTRACT

We implemented a field experiment called Show Up to Grow Up designed to increase attendance and diminish chronic absences at subsidized preschool programs in Chicago. We sent personalized text messages to parents targeting malleable factors that potentially drive absences from preschool. Using administrative records from preschools, we find that the intervention increased attended days by 2.5 (0.15 standard deviations) and decreased chronic absenteeism by 9.3 percentage points (20%) over an 18-week period. Our results suggest that the treatment impact is stronger among those in the bottom quantiles of the attendance distribution. Survey data collected at baseline suggest that our intervention made the importance of preschool more salient to parents who initially reported lower expectations for attendance and weaker beliefs about the importance of attendance to their children's development. Preschool centers may save resources by implementing low-cost light-touch interventions to meet attendance requirements.

1. Introduction

Absenteeism is a problem in most organizations. In the workplace organizations literature, absenteeism, along with lateness and turnover, are often referred to as "withdrawal behaviors" because they represent physical removal from the organization that signifies dissatisfaction and lack of commitment to the organization, or a preference for doing something else rather than attending (or some combination of these potentially interrelated factors; Berry, Lelchook, & Clark, 2012; Koslowsky, 2009). No matter the origin, absenteeism imposes significant costs (both financial and nonfinancial; e.g., diminished morale) to organizations, the person who is absent, and others in the organization (Navarro & Bass, 2006).

Preschools are important organizations in part because they help establish norms of behavior including consistent attendance (Bowles & Gintis, 1976). Preschools serving low-income families in particular suffer from problems of chronic absenteeism and lateness[2] on the part of the young children who attend them. Children's chronic absenteeism from preschool imposes costs on the schools, the children's peers who do show up, and the absent children themselves (Balfanz & Byrnes, 2012; Connolly & Olson, 2012; Ehrlich, Gwynne, & Allensworth, 2018; Ehrlich, Gwynne, Pareja, & Allensworth, 2013; Jacob & Lovett, 2017), which is partly why regulations for publicly supported preschools set attendance targets and mandate plans for managing absenteeism.[3] Because preschool children's attendance is governed by the decisions their parents make, the problem of chronic absenteeism from preschool offers an opportunity to understand the decision-making processes that influence absenteeism and the extent to which these decisions arise from malleable factors.

One important similarity between children's absenteeism from preschools and absenteeism in other organizations is that although some share of absenteeism may be due to structural factors (i.e., illness, transportation problems, lengthy commutes, unexpected events) another share may be due to potentially malleable factors (i.e., expectations for or commitment to attendance or beliefs about its importance) that shape decisions. Attendance in kindergarten is substantially higher

* Corresponding author.
E-mail address: akalil@uchicago.edu (A. Kalil).

[2] The percent of chronically absent children in Head Start programs is 25% in Washington, DC, 25.6% in Baltimore, 36% in Chicago, and 49% in New York (Katz, Adams, & Johnson, 2015). The percentages cited here consider students as "chronically absent" if they miss 10% or more of the school year, which is the modal definition across states and districts for older students (Gershenson et al., 2017). Ehrlich et al. (2018) analyze different thresholds of chronic absenteeism for pre-K children in Chicago, given the more volatile nature of enrollment and attendance for younger children. In this paper, we provide statistics for two measures of chronic absenteeism: below 90% and below 85% attendance over the period of our field experiment.

[3] See Head Start Program Performance Standards, Program Operations, part 1302.16 on attendance. Retrieved from https://eclkc.ohs.acf.hhs.gov/sites/default/files/pdf/hspps-final.pdf.

https://doi.org/10.1016/j.obhdp.2019.11.002
Received 16 July 2018; Received in revised form 29 August 2019; Accepted 8 November 2019
0749-5978/

Please cite this article as: Ariel Kalil, Susan E. Mayer and Sebastian Gallegos, Organizational Behavior and Human Decision Processes, https://doi.org/10.1016/j.obhdp.2019.11.002

than attendance at preschool only one year prior to kindergarten among similar families attending preschool and kindergarten in similar neighborhoods.[4] It is unlikely that changes in structural barriers alone explain this dramatic increase in attendance in kindergarten compared to preschool. Instead, this difference points to some role for potentially malleable factors. An important institutional difference between preschools and kindergarten is that preschool is voluntary whereas kindergarten is typically mandatory. This distinction may shape parents' expectations for attendance, beliefs about its importance to their child's development, or consequences of being absent, and thus shape parental decisions about bringing their child to school every day.

The Show Up to Grow Up (SUGU) intervention targeted potentially malleable factors driving children's absences from preschool. Our randomized controlled trial shows qualitatively important treatment impacts of the intervention on increasing the number of attended days and decreasing chronic absenteeism among the parents' children. Further, our results suggest not only that the treatment impact is stronger among those children in the bottom quantiles of the attendance distribution but also that our intervention made the importance of preschool more salient to parents who reported at baseline lower expectations for attendance and weaker beliefs about the importance of attendance to their children's development. Together, these results provide evidence that malleable barriers play some role in parents' decisions influencing their young children's attendance at school. We conduct this research in a population that is highly policy relevant (low-income parents of young children attending subsidized preschool centers characterized by high rates of chronic absenteeism) but little studied.[5] Our findings have important implications for organizational practice. In particular, results point to the potential to help preschool centers save resources by implementing low-cost light-tough interventions targeting malleable barriers in order to achieve the schools' attendance requirements and may provide insights into absenteeism in other similar organizations.

2. Background

Factors Related to Absenteeism among Schoolchildren. Chang and Romero (2008) report that about 60% of absences from preschool are due to illness, at least according to the reasons that parents give to the school. Children's absences from preschool are also correlated with family demographic characteristics, such as living with a single parent, having a young parent, and having parents with limited education. The correlation of absenteeism with factors associated with poverty suggest that the high rate of absenteeism among low-income preschool children may reflect structural factors including environmental and social conditions like residential instability, unreliable transportation, inflexible parental jobs, and community violence (Ready, 2010).

Nonetheless, absenteeism in preschools may also reflect parents' expectations for or commitment to attendance, their potentially incorrect or biased beliefs about children's attendance, and about the importance of preschool. For instance, Ehrlich, Gwynne, Pareja, and Allensworth (2014) report that parents who thought attendance mattered less or not at all in the preschool years had children who missed preschool more often. Specifically, children of parents who believed that regular attendance in early grades is as important as in the later grades had an absentee rate of 7.5%, compared to children whose parents did not believe that regular attendance in early grades is important, who had an absentee rate of 13.2%. Rogers and Feller (2018) suggest that parents (in this case, of older school children) find it hard to keep accurate track of their child's absences from school but may also be biased toward underestimating their child's absences as a self-enhancement motive to preserve a favorable view of their child along with their identity as a "good parent." According to Rogers and Feller, these factors may stand in the way of student attendance.

Prior Efforts to Increase Attendance. Prior interventions designed to boost attendance and reduce chronic absenteeism have often focused on relatively "heavy-touch" models that increase staffing or the duties of current staff members. For example, the Check & Connect intervention from the University of Minnesota requires a trained mentor (typically a professional social worker) who continually reinforces the message that education is crucial to disengaged elementary and middle school students and their families. An experimental evaluation of this program suggests that Check & Connect did not reduce absenteeism on average, though it did so for the subgroup of middle school-aged students (Anderson, Christenson, Sinclair, & Lehr, 2004; Guryan et al., 2017).[6]

The core of the Check & Connect intervention is the relationship between the mentor and the enhanced levels of communication between the family and the school. In contrast, we hypothesize that in addition to structural obstacles there could be other, potentially more malleable, factors that drive low attendance, including commitment to the organization or beliefs about its value for learning and development. Identifying malleable factors that affect parent decision-making is not only scientifically important but also policy relevant because these factors can potentially be managed with low-cost and light-touch approaches (for a review see Mayer, Kalil, Oreopoulos, & Gallegos, 2018). Identifying these factors is also policy and economically relevant because of the analogies to other types of organizations in which malleable factors may account for a substantial share of absenteeism and its associated costs.

A few recent studies (Robinson, Lee, Dearing, & Rogers, 2018; Rogers & Feller, 2018; Smythe-Leistico & Page, 2018) have tested behaviorally informed interventions designed to address malleable, cognitive barriers to school attendance, generally with promising results.[7] However, none of these studies is focused on preschoolers or low-income children in particular, and therefore we see our work as contributing new important information and complementing existing studies on older children.

In particular, parents of young children in preschool may be especially susceptible to malleable barriers affecting children's attendance because preschool is not compulsory and can therefore be easily dismissed as unimportant. This could be reflected in lower expectations for regular attendance, a lack of attention to a child's accumulated absences, or to the adoption of beliefs that children will not suffer any learning losses if they miss preschool. Any of these explanations suggests that a significant number of parents could overcome barriers to attendance if they have the motivation and support to do so. In our prior studies of low-income families with preschoolers, we have shown that behavioral tools can boost parental engagement in the home

[4] In Chicago the rate of chronic absenteeism in kindergarten is 20%, half the rate as among preschoolers (Ehrlich Gwynne, Pareja, & Allensworth, 2014; Rogers & Feller, 2007)

[5] There is, however, a good amount of evidence studying absenteeism and educational outcomes for older (primary school) children, mostly using natural experiments and longitudinal data. See, e.g., Gershenson et al., 2017; Gollwitzer & Sheeran, 2014; and Gottfried, 2009, 2011, 2014. See also the recent special issue on combating chronic absence at the *Journal of Education for Students Placed at Risk* (Introduction by Gottfried & Ehrlich, 2018).

[6] Guryan et al. (2017) did not find effects for elementary school-aged children. They hypothesize that the reason might be linked to the fact that middle school–aged students have more agency over school attendance than elementary school–aged students, and Check & Connect primarily focuses on interactions between the mentor and the student.

[7] Smythe-Leistico and Page (2018) report on a pilot, nonexperimental text-based intervention to parents that reduced kindergarten absenteeism by 11 percentage points in one school in Pittsburgh. Romero and Lee (2018) conducted a large-scale randomized experiment providing parents of K-12 children with information about their attendance rates, which reduced chronic absenteeism by 10%. Robinson et al. (2018) conducted a similar randomized field experiment in grades K-5, where the intervention decreased chronic absenteeism by 15%. Another study, by Balu et al. (2016) did not find effects on attendance for high school students.

environment (Mayer et al., 2018). Here, we test the impact of a behaviorally informed intervention on reducing absenteeism among low-income children attending federally subsidized preschool programs.

3. Conceptual framework

Behavioral science has provided tools that have been shown to change behaviors that individuals want to but cannot seem to change. Tools designed to manage time preference include goal-setting (Locke & Latham, 2002), planning prompts (Gollwitzer & Sheeran, 2006; Rogers, Milkman, John, & Norton, 2015) and timely reminders (Karlan, McConnell, Mullainathan, & Zinman, 2016). Tools designed to correct inaccurate or biased beliefs include information and receiving objective feedback about one's own behavior (Rogers & Feller, 2018). Work in behavioral science further shows that the framing of information can shape beliefs and preferences. Specifically, individuals generally prefer avoiding losses to acquiring equivalent gains (Kahneman & Tversky, 1979). Thus, the same opportunity presented as a loss is more powerful in motivating people than the equivalent opportunity presented as a gain. Studies have shown that that these tools can increase savings (Meier & Sprenger, 2010), increase college attendance (Castleman & Page, 2014), reduce smoking and drug use (Rodgers et al., 2005; Giné et al., 2010), decrease weight (Chabris, Laibson, Morris, Schuldt, & Taubinsky, 2008), and change a variety of other behaviors.

A recent randomized controlled intervention (Mayer et al., 2018) found that a suite of such behavioral tools more than doubled the amount of time that parents spent reading to their children using a digital library.[8] This suggests that when parents want the best for their children and know what to do to improve their children's skills, behavioral tools can sometimes assist them in changing their behavior in ways that help the child and make the parent feel more effective. Behavioral tools cannot solve all the structural issues faced by disadvantaged families, nor can they change behavior that a parent does not want to change, but they can help parents overcome cognitive roadblocks to achieve their own goals.

Following is a brief description of the behavioral tools that are central to our intervention to reduce chronic absenteeism among low-income preschoolers.

Goal Setting. Goal setting involves the development of an action plan designed to motivate and guide a person (or group) toward a goal. A central tenet of Locke (1968) goal-setting theory is the importance of setting specific and measurable (as opposed to general) goals to boost performance. According to Locke and Latham (2002), goal-setting is thought to affect performance through multiple mechanisms, including by focusing attention on the specific goal-related activity, increasing effort and persistence, and by changing behavior itself through the development of new knowledge and strategies. Locke and Latham (2002) also discuss the importance of feedback as a complement to goal setting. As noted below, our intervention also includes an objective feedback component.

Planning Prompts. Procrastination frequently prevents individuals from engaging in beneficial behaviors. Parents may know that getting their child to preschool today is important, and they may want to do it, but they may need assistance in implementing their good intentions. Recent work in the public health field has shown that text message reminders to low-income urban parents helped to increase the rate of flu vaccinations among their children (Stockwell et al., 2012). Research has also shown that simple techniques like designating a time and place for a new behavior can increase the likelihood of engaging in the new behavior (Rogers et al., 2015). For example, having individuals write down the date and time of a planned action has increased both voter turnout (Nickerson & Rogers, 2010) and vaccination rates for influenza (Milkman, Beshears, Choi, Laibson, & Madrian, 2011). By creating specific, actionable plans for getting their children to preschool regularly, parents may be able to see their plans through to fruition.

Helping Parents to Correct Inaccurate Beliefs. As noted by Robinson et al. (2018) and Rogers and Feller (2018), parents of grade school-aged children severely underestimate how many days their children have been absent from school; at the same time, they overestimate their children's attendance relative to that of other children. Limits on parental attention can interfere with parents' ability to accurately remember their children's absences for an entire school year (Chugh & Bazerman, 2007; Simons & Chabris, 1999). Parents may also be biased toward underestimating their own children's absences from school. Bringing this information to the top of parents' minds can reduce uncertainty, counteract this bias, and prompt behavior change.

A second type of inaccurate belief, which may be especially germane in preschool, is the belief that preschool experiences matter little for success in primary school. As Ehrlich et al. (2014) show, some parents may believe that preschool is simply "childcare" and may not understand that children are engaged in a wide variety of school readiness activities during the day. They thus may not be aware of what their children are missing when they miss school.

Timely Reminders. Reminders can also overcome problems of procrastination and self-control by getting people to focus attention on the task. A reminder can change time allocation today by drawing attention to the relationship between future outcomes and current choices. Text messages are the most common way to communicate reminders and are now a common feature of many interventions (see, for example, Richburg-Hayes et al., 2014; Castleman & Page, 2014; Bergman, 2015). It is possible that simply receiving a text message related to goal setting, a planning prompt, or information to correct a misbelief might serve as such a timely reminder and bring the importance of preschool attendance to parents' "top of mind." Thus, our intervention does not include separate messages that we deem to serve uniquely as timely reminders. Rather, we view all of the text messages as potentially serving this purpose.

4. Methods

Head Start centers in Chicago. We focus on children in Head Start programs because of the statutory requirement that Head Start programs maintain an average rate of attendance of 85% or risk sanctions. Programs are required to manage systematic program attendance issues by tracking attendance and using the data to address problems with chronic absenteeism. If a program's monthly attendance falls below 85 percent average daily attendance, Head Start programs are required to make necessary changes to their program performance and continuous improvement plans (United States Department of Health and Human Services, 2016). As reported to us by directors of Head Start centers, the fact that many programs do not achieve this level of attendance is worrisome to centers, and attempts to increase attendance consume large amounts of resources that could be used for other educational purposes.

Eligibility and Recruitment. Parents whose primary language was either English or Spanish, who had a child aged 3–5 years old enrolled in a subsidized preschool program, and who had access to a mobile phone were eligible for the experiment. Parents were informed that the preschool was participating in a new program to improve attendance but that they had the option not to participate. They were also told that some parents will experience no change and that others will receive several weekly text messages.

We recruited parents during the pick-up and drop-off hours at the preschool centers. We made special efforts to minimize the likelihood of recruiting only higher attending families. First, we put up flyers at the centers a month in advance of recruitment and we asked preschools to

[8] Although in Mayer et al. (2018) we cannot definitively rule out that digital reading is not substituting for print book reading, survey data about parental reading routines in our target population suggests that there is not much scope for such substitution.

help us identify families most at-risk of chronic absenteeism and obtained the preschool's support in encouraging the families' participation. Second, we spent between twelve and twenty hours in each center inviting parents with age-eligible children to participate, and canvassed sites during drop-off and pick-up each day at each site for two to three weeks straight. Thus, we only miss the opportunity to recruit families if they missed school for two to three consecutive weeks.

Parents were asked to sign a consent form that permitted us to collect attendance data on their children, which we also used to collect some basic demographic information. Across all nine centers, we estimate that about 1,000 children were eligible for the study. Of those eligible, a total of 741 participants signed consent forms and became actual participants in SUGU intervention.[9] All participating parents were texted three administrative messages, including a welcome, reminder about a survey, and a thank you for their participation at the conclusion of the study. We implemented our intervention at nine preschool centers[10] that shared their attendance records during the intervention period with our research team. Four participating school centers also had preintervention attendance data to share with our team. Since 2016, the Head Start Performance Standards require programs to collect attendance (United States Department of Health and Human Services, 2016).[11]

Procedures and Intervention. Within each center and classroom, we randomized half of the eligible parents to the treatment group and half to the control group. Parents of siblings were randomized with all of the siblings to either treatment or control groups. As documented in our preregistered analysis plan, we estimated we would need a sample of 700 parent-child dyads to achieve a minimal detectable effect size of 0.20 standard deviation on attended days at the 0.05 level of statistical significance and with an 80 percent of power, with no covariates. We learned from the actual data that the standard deviation of attended days was higher than what we conservatively assumed in our power calculations (17 days instead of 10 days) and hence we were able to detect an effect of 0.15 standard deviations in practice.

Parents in the control group continued with the preschool's standard procedures regarding attendance. Parents in the treatment group received a series of three to four text messages per week. The messages included ones focusing on setting goals to attend school, planning prompts, and correcting beliefs and expectations with objective feedback on attendance as well as information on what children would miss if they missed school.[12]

In our treatment group, goal setting messages prompted parents to focus on meeting the goal of having the child attend school every day. An example is, "It's almost the end of the month! Are you meeting your goal of daily attendance for Alex?" and "Are you and Alex meeting the goal of attending school every day?" and "Remember Alex's attendance goal. Get Alex to school every day!" Note that we did not offer parents to set goals in the traditional sense, insofar as we did not ask them to write down a goal for a specific number of days attended nor to respond to us by text with such a pledge. Instead, parents were prompted with goal-oriented messages designed to focus their attention on attendance and enhance their likelihood of getting their child to school each day.

Planning prompt messages encouraged parents to identify and make a plan to address some of the impediments to attendance. These are designed to focus parents' attention and bring any potential problems to parents' top of mind. An example of such a message is, "Who is able to help you drop off or pick up Alex if you are not able to? Reply with your answer."

Feedback messages designed to correct misbeliefs about attendance provided objective feedback on children's actual attendance at school in the prior month. The purpose of these messages is to help parents to correct potentially inaccurate beliefs about the actual level of absenteeism of their child in the prior period (see Robinson et al., 2018; Rogers & Feller, 2018). An example of a feedback message is, "Here is your monthly feedback: Alex missed 3 days last month including excused and unexcused absences."

Messages designed to help parents correct inaccurate beliefs about learning opportunities in preschool told parents what their children were learning in preschool, framed to emphasize what children would miss out on if they were absent. Examples of such messages are: "Preschool helps Alex develop early math skills to succeed in kindergarten. Don't let him miss this opportunity!"; "Attending school regularly helps children feel better about school and themselves. Don't give up this opportunity!"; "One of the most important things children learn in preschool is how to get along with other children. Don't give up this opportunity!"; "Tomorrow your child could be learning important reading skills. Don't let Alex lose this opportunity!"

5. Data

Sample Characteristics. Our 741 participating parents signed consent forms, from which we collected information on child and parent gender, language spoken at home (Spanish or English), whether children had siblings in the preschool, and whether children attended preschool on a full-day or half-day basis. We also asked about parental education, commuting time from home to the preschool, and child birth weight. These three variables had a joint response rate of 92% (N = 684), with nonresponse uncorrelated with treatment status.[13]

Table 1 describes our sample. Column (1) shows the number of observations with nonmissing values for each variable in the rows, and column (2) presents the mean for the total sample. Our participating parents were mainly mothers (90% female), with children equally split by gender. About 20% of the children were born with low birth weight (less than 5.5 lb), 11% of the children had siblings in the same preschool, and 68% attended on a full-day basis. The average commuting time from home to the preschool is about 17.5 min.

In terms of schooling, our sample of parents is similar in characteristics to a national sample of parents of children in Head Start programs collected in the 2010 Head Start Family and Child Experiences Survey (FACES) (the latest year for which such data are available). In FACES, 31.8% of parents had less than a high school education, compared to 31% in our sample. Another 27% reported to have a high school diploma or GED, 30% had attended some college or had associate degrees, and the remaining 10% declared they had a bachelor's degree. In FACES 36% of parents are Latino/Hispanic, and in our sample 41% of the participating parents answered consent forms in Spanish.

Table 1 also shows demographic data by treatment status in

[9] From the 741 parents, 10% (77 parents) stopped their participation during the intervention period, balanced by treatment status. From those 77 parents, 52 stopped because they left the preschool.

[10] We implemented our intervention in three rounds. The first round occurred in the spring of 2016, when we partnered with one center. Our second and third rounds occurred in the fall of 2016 and the spring of 2017. We partnered with four preschool centers for each of these subsequent rounds.

[11] The way attendance records were collected varied across sites. Some sites were using an online system and would email our research team electronic reports exported from this system. Other sites would scan and send paper attendance copies. One site transferred their records to a spreadsheet document that they created specifically for the SUGU study. We worked carefully with the data to generate our measures of attendance, as we explain in the Data section.

[12] Most text messages were sent between the hours of 4 and 7p.m., at the times we assumed parents would most likely be with their children. We sent messages about attendance goals on Sunday night, assuming families were getting ready for the week.

[13] The nonresponse rates for control and treatment groups were, respectively, 6% and 4% for parental education; 4% and 5% for home-preschool commuting time, and 6% and 7% for child birth weight. The joint nonresponse rates were 8% and 7% for control and treatment groups, respectively.

Table 1
Mean characteristics: Total sample, treatment, and control groups.

Variable	(1) N	(2) Total sample mean	(3) Treatment mean	(4) Control mean	(5) P-value of (3) – (4)
Intervention in the Fall	741	0.34	0.34	0.33	0.920
Female (Child)	741	0.50	0.51	0.48	0.530
Female (Parent)	741	0.90	0.88	0.92	0.100
Spanish	741	0.41	0.41	0.40	0.681
Sibling in the Preschool	741	0.11	0.10	0.11	0.706
Full Day	741	0.68	0.69	0.67	0.607
Non-response on the three variables below	741	0.08	0.07	0.08	0.597
Birth weight < 5.5 lb (Child)	684	0.20	0.21	0.19	0.440
Commuting Minutes home–preschool	684	17.49	17.66	17.30	0.580
Education, scale 1–4 (Parent)	684	2.23	2.20	2.25	0.620
No HS diploma	684	0.31	0.32	0.30	0.800
HS or GED	684	0.27	0.28	0.27	0.510
Some college/AA	684	0.31	0.29	0.33	0.330
Bachelor's degree or more	684	0.11	0.11	0.11	0.910

Notes: "Intervention in the Fall" takes value one if the intervention started in October, and zero if it started in February.
"Female (Child)" takes value one if the child is female, and analogously for the variable "Female (Parent)." "Spanish" takes value one if the parent answered the consent form in Spanish, and zero if it was in English. "Sibling" takes the value 1 if the parent had more than one child enrolled in the same preschool and zero if not. "Full Day" takes the value 1 if the child attended on a full-day rather than half-day status. "Non-response on the three variables below" takes the value 1 if the parent did not answer questions about birth weight, commuting time, or education, and zero if they did. "Education, Scale 1–4 (Parent)" takes the value 1 of the parent reported to lack a high school diploma, 2 if she completed high school or has a GED, 3 if she reports to have some college or an associate degree (AA), and 4 if she reports to have a bacherlor's degree or postgraduate studies. The next four variables are dichotomic variables that take value 1 if the parent reports to have achieved the respective educational level.

columns (3) and (4). No difference between the treatment and control group is statistically significant at the 5% level, as shown in column (5). As expected, conditioning on the characteristics in Table 1 (such as timing of the intervention, full or half day, or demographics) makes no difference to the results.

Enrollment and Attendance Data. Our program uses outcomes that are measures of actual behavior based on attendance records provided by the preschool centers. We measured individual attendance rates using the administrative enrollment and attendance records of the individual preschools.[14] Fig. 1 illustrates our procedure. We first counted all the days during the intervention period of 18 weeks. We refer to this as intervention days. Next, we classified days as either weekends/holidays or *potential* school days (days when the preschool was open) over the time span under analysis. We then categorized potential days into days enrolled in preschool or not. This distinction is important because there is considerable turnover of students in Head Start programs making it important to separate enrolled but absent children from those who are no longer enrolled. A potential school day counted as a day enrolled for a particular child if she appeared on the class attendance sheets we received from the centers on that day. Conditional on enrollment, we counted days as either attended or absent. We computed the attendance rate for each child over the intervention period by taking the ratio of number of days attended to the number of days enrolled in preschool.[15]

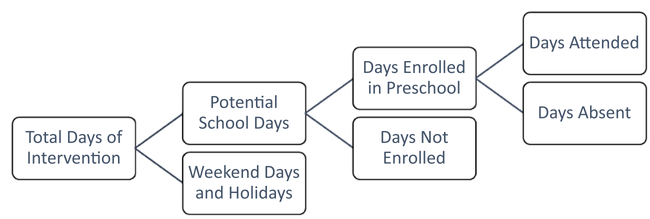

Fig. 1. Days of intervention, enrollment and attendance. Notes: Fig. 1 shows our procedure to measure individual attendance from administrative records. We first counted all the days during the intervention period of 18 weeks. We refer to this as intervention days. Next, we classified days as either weekends/holidays or *potential* school days (days when the preschool was open) over the time span under analysis. We then categorized potential days into days enrolled in preschool or not. This distinction is important because there is considerable churning of students in Head Start programs making it important to separate enrolled but absent children from those who are no longer enrolled. A potential school day counted as a day enrolled for a particular child if she appeared on the class attendance sheets we received from the centers on that day. Conditional on enrollment, we counted days as either attended or absent. We computed the attendance rate for each child over the intervention period by taking the ratio of number of days attended to the number of days enrolled in preschool.

Table 2 shows summary statistics for our attendance measures. The intervention lasted for 18 weeks, averaging 121.1 intervention days.[16] There were on average 77.3 potential school days during that period. On average, children in our sample were enrolled for 70.4 days (90% of potential days) and attended preschool for 59.2 days. The attendance rate, measured as the quotient between attended days and enrolled days, averaged 82% over the four months, which is below the 85% federal attendance requirement for Head Start programs. However, the median attendance rate of 87% is above the threshold. We plot the frequency distribution of attendance in Fig. 2. The distribution of attendance is skewed to the right, with a mass concentrated after 80% attendance.

We are particularly interested in the children who were chronically absent during our period of analysis. In Table 2 we include two attendance thresholds (90% and 85%) to define chronic absenteeism. The 90% threshold is obviously more conservative; according to this definition, 59% of the children were chronically absent during our analysis period. When the 85% threshold is assumed, the rate of chronic absenteeism is 41%. Fig. 3 plots the cumulative distribution function of attendance, which is useful because it provides the probability of attendance being less than any chosen threshold. For example, if we let 85% be the cutoff, then the probability of attendance being less than

[14] The measurement of enrollment and attendance patterns has proven to be challenging. Hutt (2018) provides an informative review of the historical precedents for the measurement and use of attendance records to evaluate schools.

[15] In the sample there are 12 children (6 in the treatment group and 6 in the control group) who were enrolled zero days during the intervention period. These are children that were reported as enrolled by the centers when we performed our randomization, but then left the preschool centers. For those children, we imputed an attendance rate of zero. Our results remain unchanged when we drop them from the sample.

[16] The implementation of the intervention in different rounds and in different preschool centers explains the variation in the number of days of intervention and the number of potential school days. Table A1 in the Appendix provides further details on these variables by center, month, and round.

Table 2
Summary statistics for days of intervention, enrollment, attendance, and absenteeism.

Variable	Mean	Std. Dev.	Median	Min	Max
Intervention days	121.11	1.38	120	120	123
Potential school days	77.30	5.24	78	63	85
Enrolled days	70.38	15.68	77	0	85
Attended days	59.24	16.71	61	0	85
Attendance rate[(a)]	0.82	0.17	0.87	0	1
Chronic absenteeism[(b)]					
Attendance rate ≤ 0.90	0.59	0.49	1	0	1
Attendance rate ≤ 0.85	0.41	0.49	0	0	1
Observations	741				

Notes: All variables are constructed with information over the whole intervention period—about four months. (a) In the sample there are 12 children (6 in the treatment group and 6 in the control group) who were enrolled zero days during the intervention period. These children were reported as enrolled by the centers when we performed our randomization, but then left the preschool centers. For those children, we imputed an attendance rate of zero. Our results remain unchanged when we drop them from the sample. (b) The definitions of "chronically absent" vary across states and districts, with the modal definition being absent on at least 10 percent of school days over the year for older students (Gershenson, Jacknowitz, & Brannegan, 2017). Ehrlich et al. (2018) uses the 10 percent threshold for pre-K children in Chicago, but also analyzes different thresholds given the nature of enrollment and attendance in pre-K. We provide statistics for two measures of chronic absenteeism, at the 90 percent level of attendance, which is used in much of the previous research on school-aged children, and at the 85 percent level of attendance, which is the statutory requirement for Head Start programs.

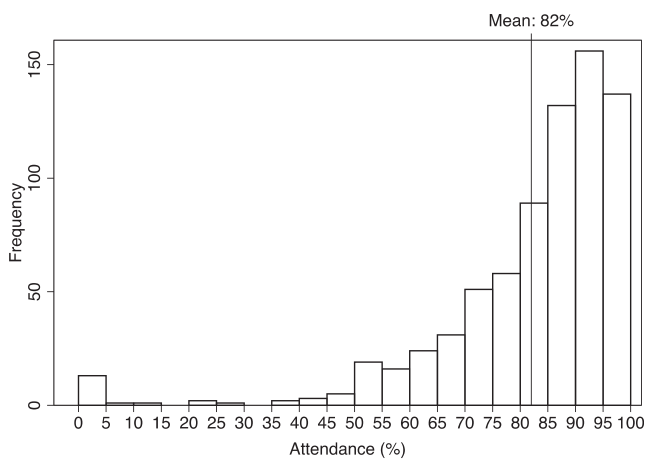

Fig. 2. Distribution of attendance during the 18-week period. Notes: The Figure plots the frequency histogram of attendance over the period of intervention (18-weeks). Each vertical bar has a width of 5 percentage points (pp) and counts the number of children within the particular 5 pp range. For example, about 150 children had an attendance rate ranging from 90% to 95%.

85% is exactly 41%.

6. Results

We turn next to our main treatment impacts. This section is organized as follows. First, we show the intention-to-treat effects of our intervention on a variety of cumulative attendance measures from the start of the intervention to the end of the 18-week period of intervention. Second, we take advantage of our detailed administrative records to study how the estimated effects evolve over the time of the intervention. Third, we explore mechanisms of the effects using results from a baseline survey.

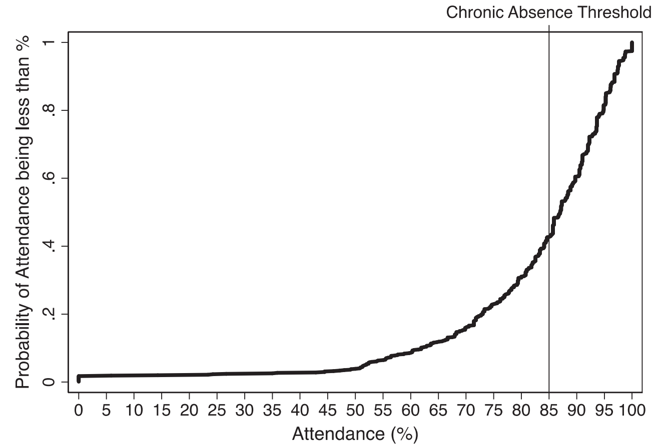

Fig. 3. Cumulative distribution of attendance during the 18-week period. Notes: The Figure plots the cumulative distribution function (cdf) of attendance over the period of intervention (18-weeks). The vertical line indicates one attendance rate threshold that determines chronic absenteeism (85% of attendance). The graph shows that the fraction of children that were chronically absent (attended less than 85% of the time) during the period of intervention is 41%, as shown by the intersection of the vertical line and the cdf.

6.1. Intention-to-treat effects

Our main outcome of interest is children's attendance and, in particular, chronic absenteeism. Prior to estimating our main results, we checked for potential differences in attendance rates for students enrolled in half day versus full day programs. Finding none, we pooled all students for the analyses that follow. To estimate the effect of SUGU on these outcomes we use the following standard equation:

$$Y = \beta_0 + \beta_1 T + \varepsilon \quad (1)$$

where Y is an outcome variable, T is an indicator for random assignment to the treatment group, and ε is an idiosyncratic error term. Our parameter of interest is β_1, which is our intention-to-treat effect or, equivalently, is the average difference for parents randomized to the treated group compared to those randomized to the control group.[17]

Table 3 shows results for the estimation of Eq. (1) on the measures presented in Table 2. As expected, there are no statistical differences in the days of intervention (column 1), the number of potential school days (column 2), or the number of enrolled days (column 3) over the four-month period of intervention between the treatment and control group. This demonstrates that there are no effects on attendance measures where we would not expect effects, which is especially important because effects on these measures would drive effects on the measures where we do expect effects.

Column 4 shows that children randomized to the treatment group attended on average 2.5 more days over the 18-week period than those randomized to the control group, who attended 58 days on average. The 2.5 treatment effect is equivalent to an effect size of 0.15 standard deviation on attended days, or about a 4% increase as a percent of the mean. The intention-to-treat effect on the attendance rate is close to 1.5 percentage points (over a base of 82%), though it is measured with noise (see column 5).[18]

[17] As noted previously, we randomized our intervention at the individual level within the preschool center and classrooms (see "procedures and intervention"). Had we randomized preschools (or classrooms) to treatment, then we would need to cluster by preschools (or classrooms), but that is not the case in the present design (see Abadie, Athey, Imbens, & Wooldridge, 2017). Also, as customary in well implemented RCTs, adding covariates (or fixed effects) to the estimation of Eq. (1) increases the precision of our estimate but does not change its magnitude.

[18] Table 2 presents Huber-White sandwich estimator standard errors that

Table 3
Intention-to-treat effects, full sample.

Dependent variable	(1) Days of intervention	(2) Potential school days	(3) Enrolled days	(4) Attended days	(5) Attendance rate	(6) 1(Rate < =90)	(7) 1(Rate < =85)
Treatment	0.0129	0.347	1.838	2.460**	0.0148	−0.0769**	−0.0932***
	(0.101)	(0.385)	(1.151)	(1.226)	(0.013)	(0.036)	(0.036)
Constant	121.1***	77.13***	69.45***	57.99***	0.814***	0.627***	0.458***
	(0.072)	(0.274)	(0.820)	(0.873)	(0.009)	(0.026)	(0.026)
Dependent variable: Mean	121.11	77.3	70.38	59.24	0.82	0.59	0.41
Dependent variable: Std. Dev.	1.38	5.24	15.68	16.71	0.17	0.49	0.49
Observations	741	741	741	741	741	741	741

Notes: Each column shows the estimated coefficients of regressing the respective dependent variable on an indicator for random assignment to the treatment group. Huber-White sandwich estimator standard errors are in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results shown in columns 6 and 7 indicate that the treatment generated a sizable decrease in chronic absenteeism, measured using either the 90% or the 85% threshold. According to column 6, chronic absenteeism decreased by 7.7 percentage points (from a control mean base of 63%) due to the treatment when the 90% threshold is considered, whereas column 7 shows that it decreases by 9.3 percentage points (from a control mean base of 46%) when the 85% threshold is considered. These effects represent a reduction in chronic absenteeism of 12% and 20%, respectively.[19]

The results suggest that SUGU had differential effects along the distribution of attendance. Therefore, we plot in Fig. 4 the cumulative distribution function of attendance over the period of intervention (18 weeks) by treatment status. Fig. 4 illustrates effects along the distribution because it provides the probability of attendance being less than any chosen threshold. For example, at the 85% attendance threshold, the vertical difference between the treatment and control distribution is about 9 percentage points.

Finally, we perform quantile regressions of attendance rate on treatment status. The coefficient estimates for the 10th, 25th, 50th, 75th, and 90th quantiles are shown in Table 4A.[20] Consistent with Fig. 4, the results indicate that the average effect is driven by changes between the bottom quartile and the median of the attendance distribution. At the 10th quantile of attendance the difference between children randomized to the treatment and control groups is 3.2 percentage points but is not statistically distinguishable from zero. At the 25th quantile of attendance the attendance for children randomized to the SUGU intervention is 4.1 percentage points (significant at the 5% level) higher than those randomized to the control group, whereas attendance among treated children is 2.3 percentage points (significant at the 5% level) higher at the median. The differences are 0.3 percentage points and 0.0 percentage points for the 75th and 90th quantiles, respectively; neither coefficient is statistically different from zero. Table 4B shows results of quantile regressions of attended days on treatment status, which follow the same pattern.

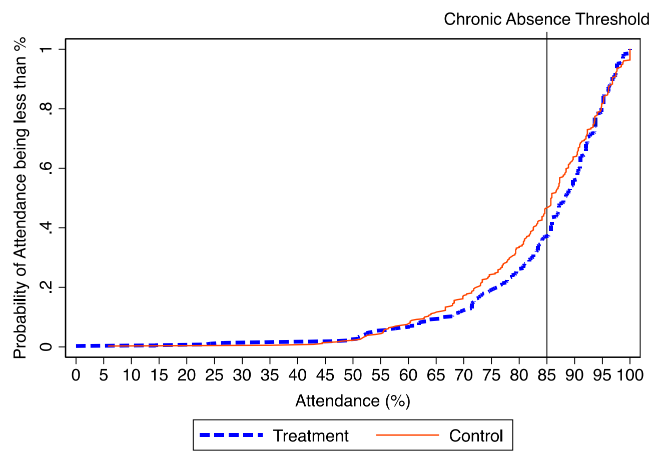

Fig. 4. Cumulative distribution of attendance by treatment status. Notes: The Figure plots the cumulative distribution function of attendance over the period of intervention (18-weeks) for both the treatment and the control groups. The largest difference between the distribution functions is 0.0964, with an approximate asymptotic p-value of 0.032, which is significant at the 95% level. A Kolmogorov-Smirnov test fails to reject the null hypothesis that the distributions are equal with a P-value of 0.058.

6.2. Treatment effects over time

Our detailed data allow us to track treatment effects over time. We view this analysis as exploratory as it is unclear a priori how immediately the effects would arise or at what rate they would persist. For instance, based on our other behaviorally informed intervention directed at parents (Mayer et al., 2018) we might expect to find an immediate response; this could persist after the intervention if parents form new habits or it could diminish if parents become habituated to the messages. On the other hand, our other intervention was focused on book reading, and if preschool attendance is different from book reading then the effects could take time to unfold as parents get feedback on their child's absences, strengthen their beliefs about the importance of preschool, and begin to respond to the behavioral tools. Table 5 presents the results of estimating the intention-to-treat effects on attended days, attendance rate, and chronic absenteeism by month. Fig. 5 plots the estimated coefficients, with 90% confidence intervals.

The results for these three outcome variables describe a similar pattern over time. Treatment effects appear in magnitude (but not in terms of statistical significance) by the end of the first month and increase further toward the end of the second month. By the third month, effects are statistically distinguishable from zero as remains true to the end of the intervention—that is, by month four.

To further assess the timing of the effects of SUGU we use data for a subsample of children for whom we have three months of pre-intervention attendance information. The information allows us to compute differences in attendance before and after the intervention was implemented. The results of those regressions are shown in Table 6, and Fig. 6 plots the intention-to-treat coefficients on attendance rate per each month, before and after the intervention started. Before the intervention, there are no detectable differences between treatment and

(*footnote continued*)
correspond to our most conservative computation. If we cluster at the preschool or classroom level, the treatment effect becomes significant at the 10% level in column 5.

[19] As a robustness check, we replicated Table 3 for the subsample with non-missing values on covariates presented in Table 1. Table A2 in the Appendix shows the results, which are almost identical to those presented in Table 3.

[20] As is customary, the quantile regression results provide differences between the (marginal) distributions of the outcome at different quantiles and do not provide the distribution of treatment effects, unless we impose a rank invariance assumption.

Table 4A
Quantile regressions of attendance rate, full sample.

Dependent variable: attendance rate	(1) Quantile 10	(2) Quantile 25	(3) Quantile 50	(4) Quantile 75	(5) Quantile 90
Treatment	0.032	0.041**	0.023**	0.003	0.000
	(0.042)	(0.020)	(0.011)	(0.010)	(0.007)
Constant	0.603	0.746	0.857	0.933	0.968
	(0.025)	(0.014)	(0.006)	(0.009)	(0.005)
Attendance rate at each quantile	0.62	0.77	0.87	0.94	0.97
Observations	741	741	741	741	741

Notes: Each column shows the estimated coefficients of quantile regressions of attendance rate on an indicator for random assignment to the treatment group, at the 10th, 25th, 50th, 75th, and 90th quantiles, respectively. Bootstrapped standard errors (100 replications) are in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4B
Quantile regressions of attended days, full sample.

Dependent variable: attended days	(1) Quantile 10	(2) Quantile 25	(3) Quantile 50	(4) Quantile 75	(5) Quantile 90
Treatment	4.00	4.00***	2.00*	1.00	1.00
	(4.095)	(1.454)	(1.198)	(1.093)	(1.135)
Constant	38.00***	51.00***	61.00***	70.00***	75.00***
	(3.400)	(1.012)	(0.659)	(0.796)	(0.772)
Attended days at each quantile	40.00	53.00	61.00	71.00	80.00
Observations	741	741	741	741	741

Notes: Each column shows the estimated coefficients of quantile regressions of attended days on an indicator for random assignment to the treatment group, at the 10th, 25th, 50th, 75th, and 90th quantiles, respectively. Bootstrapped standard errors (100 replications) are in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

control groups. The effect of our intervention goes up steadily and becomes statistically different from zero after three months of intervention, remaining constant in the last month.

Overall, the results on treatment effects over time suggest that it takes several weeks to induce behavior change in this particular realm of parental actions—namely, bringing children to preschool—but once induced the changes remain throughout the experimental period.

6.3. Exploring mechanisms

We implemented a baseline survey for parents assessing their expectations for preschool attendance as well as their beliefs about the learning opportunities at preschool. The survey did not collect data that would allow us to test all plausible mechanisms through which the treatment yielded a positive impact, but it did ask parents to report the number of preschool days they expected their child to miss during the next three months, the number of days they think is acceptable for their child to miss, whether the child is sent to preschool if she is sick, and whether parents thought that missed days of preschool would have a negative impact on the child's social and academic skill development. We also implemented a time-preference task, designed to estimate both a discount rate for parents and their present bias, as in Mayer et al. (2018).

Our survey take-up was 71% (525 out of 741 parents answered), which did not differ for the treatment and control groups (71.2% and 70.4%, respectively, with a p-value of 0.796). As we show in Table 7, the set of baseline variables available for the whole sample show no statistically significant differences between respondents and nonrespondents on average. However, and perhaps naturally, children of parents who did not answer the survey have lower levels of attendance.[21]

For the group of survey respondents, we replicated our quantile regression analysis from Table 4. The results shown in Table 8 indicate that treatment effects appear at the bottom of the distribution of absences for the survey sample, up to quantile 0.20, with smaller or null effects for higher quantiles. Thus, to help understand mechanisms we compared parents' scores on the survey measures in two different groups: the relatively smaller (n = 108) group with strong treatment effects (at or below the quantile 0.20) versus the remaining, relatively larger share of survey respondents above quantile 0.20. The idea behind this exercise is to try to understand how characteristics of these two groups of parents differ in ways that provide insight into why the SUGU intervention yielded the treatment impacts that it did.

Given that our intervention targeted potentially malleable factors driving children's absences from preschool we expected treatment effects to be greater for those parents facing higher malleable barriers at baseline. Table 9 shows the average values for baseline parental characteristics by group (i.e., higher and lower treatment effects) and the p-value of the difference. The results show that indeed parents in the high treatment effects group reported lower expectations for attendance on the baseline survey compared to the parents who experienced a weaker treatment effect. Specifically, parents in the high treatment effects group reported at baseline that they expected their child to miss more days of preschool in the next three months; they also reported a higher number of acceptable missed preschool days (by a factor of almost two and 1.5, respectively). Parents in the high treatment effect group were also less likely at baseline to disagree with the statements "it is acceptable to miss preschool if the weather is bad" or that "it is acceptable to miss preschool if the child stays with family." Next, parents in the high treatment effects group appeared at baseline to place less value on preschool as a place for learning academic and social skills. Specifically, they were less likely to concur at baseline that their child would be worse off in social and academic skills if the child missed more days of

[21] Our treatment effects appear to be higher for the nonrespondents than respondents, which is consistent with our intervention being more effective for those with greater absences. However, we cannot distinguish the difference from zero at conventional levels of statistical significance (p = 0.05). The effects on the attendance rate display a similar pattern, as we show in Table A3 in the Appendix.

Table 5
ITT effects on measures of attendance by month of intervention.

Panel 1: Attended days

Dep. variable: Attended days	(1) Month 1	(2) Month 2	(3) Month 3	(4) Month 4
Treatment	0.397	0.532	0.557*	0.980**
	(0.309)	(0.371)	(0.331)	(0.398)
Constant	15.15***	16.18***	11.70***	14.94***
	(0.227)	(0.279)	(0.241)	(0.300)
Dep. Variable: Mean	15.35	16.45	11.98	15.44
Dep. Variable: Std. Dev.	4.21	5.05	4.51	5.43
Observations	741	741	741	741

Panel 2: Attendance rate

Dep. variable: Attendance Rate	(1) Month 1	(2) Month 2	(3) Month 3	(4) Month 4
Treatment	0.013	0.024	0.034*	0.044**
	(0.014)	(0.016)	(0.018)	(0.019)
Constant	0.831***	0.794***	0.773***	0.764***
	(0.010)	(0.012)	(0.014)	(0.015)
Dep. Variable: Mean	0.84	0.81	0.79	0.79
Dep. Variable: Std. Dev.	0.19	0.22	0.25	0.26
Observations	741	741	741	741

Panel 3: Chronic absenteeism (85% threshold)

Dep. variable: Chronic Absenteeism (85% Threshold)	(1) Month 1	(2) Month 2	(3) Month 3	(4) Month 4
Treatment	−0.044	−0.048	−0.097***	−0.075**
	(0.036)	(0.036)	(0.037)	(0.036)
Constant	0.395***	0.441***	0.504***	0.477***
	(0.026)	(0.026)	(0.026)	(0.026)
Dep. Variable: Mean	0.372	0.417	0.455	0.439
Dep. Variable: Std. Dev.	0.484	0.493	0.498	0.497
Observations	741	741	741	741

Notes: Each column on each panel shows the estimated coefficients of regressing the respective dependent variable on an indicator for random assignment to the treatment group, by months after the beginning of the SUGU intervention. Huber-White sandwich estimator standard errors are in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

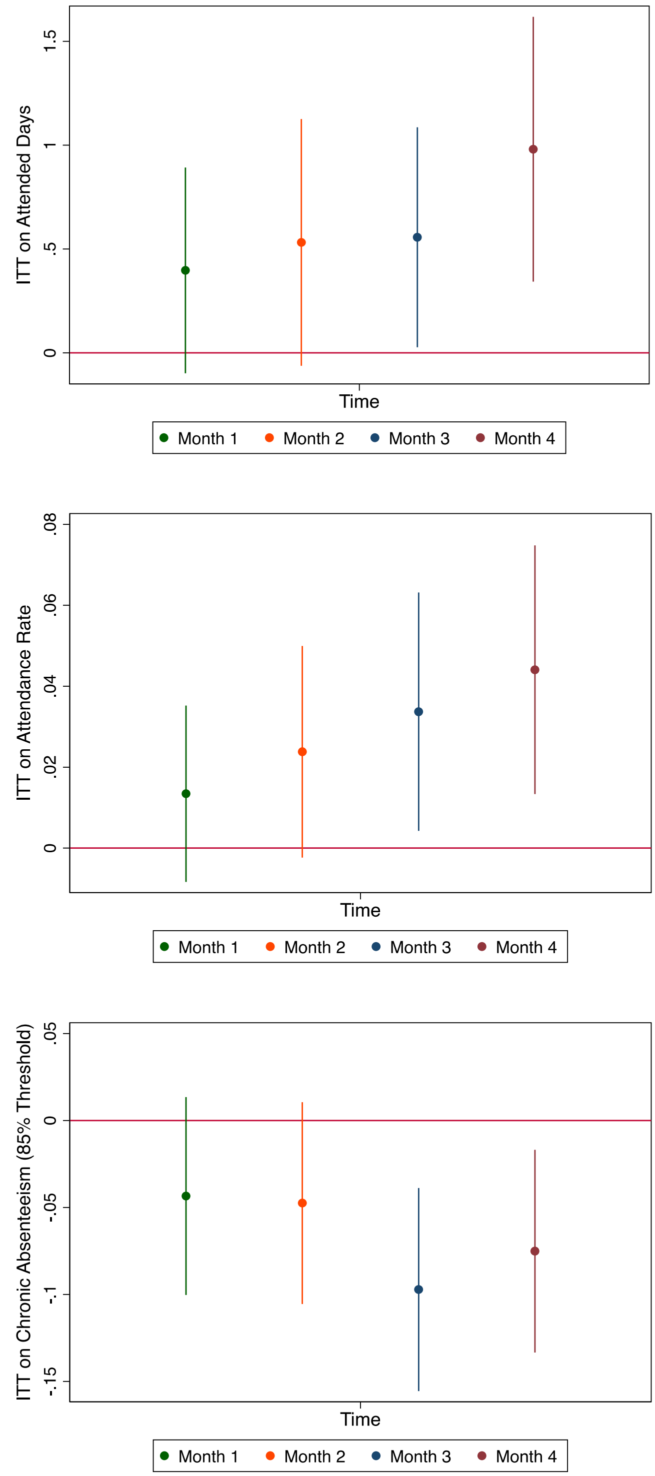

Fig. 5. ITT effects on measures of attendance by month of intervention. Notes: The Figure plots the estimated coefficients (with 90% confidence intervals) of attendance rate on the dummy variable indicating randomization to treatment, by month, for our full sample.

preschool. However, parents in the high and low treatment effects group did not differ at baseline in impatience (or time discounting).[22]

7. Discussion

Student absence from school is a problem across all grades but it is an especially critical and vexing problem in preschool programs serving low-income children. In our contemporary sample of children in Head Start centers in Chicago, for example, the rate of chronic absenteeism is either 59% or 41%, depending on a threshold of 90% or 85% attendance, respectively. Head Start centers generally use a cut-off of 85% and this performance standard is linked in principle to the programs' funding streams and is thus an important concern. It is no surprise then, that programs expend considerable time and energy trying to manage this problem. However, according to program directors and national data, absenteeism and especially chronic absenteeism continued to be a problem.

The fact that parents are able to ensure their children's kindergarten attendance far more often than their preschool attendance suggests that one or more malleable factors may shape parents' decisions regarding their children's preschool attendance. The fact that preschool is not compulsory may make it especially easy to dismiss and this may make it

[22] We also analyzed parental beliefs and preferences in a factor analysis with principal components, with similar results, as shown in Table 10. We performed the rotation of the loading matrix using the varimax and oblimin methods to produce orthogonal factors. The survey variables from Table 9 loaded onto four factors related to attendance expectations, beliefs about preschool importance, time preferences, and beliefs about absences' impact on skills. Our results show larger treatment effects for children with weaker attendance expectations at baseline.

Table 6
Intention-to-treat effects, event study—pre-intervention sample.

	Pre-intervention period			Intervention period			
	(1) November	(2) December	(3) January	(4) February	(5) March	(6) April	(7) May
Treatment	0.004	0.011	−0.004	0.001	0.022	0.058**	0.054**
	(0.015)	(0.020)	(0.016)	(0.015)	(0.021)	(0.023)	(0.024)
Constant	0.857***	0.778***	0.854***	0.848***	0.811***	0.799***	0.807***
	(0.010)	(0.014)	(0.011)	(0.012)	(0.015)	(0.018)	(0.019)
Dep. Variable: Mean	0.858	0.784	0.852	0.848	0.822	0.829	0.835
Dep. Variable: Std. Dev.	0.146	0.194	0.151	0.145	0.194	0.215	0.227
Observations	363	363	363	363	363	363	363
Effect size	0.004	0.011	−0.004	0.001	0.022	0.058	0.054

Notes: Each column shows the estimated coefficients of regressing attendance rate on an indicator for random assignment to the treatment group, by months before and after the beginning of the SUGU intervention. The subsample used to produce the estimates corresponds to four out of nine preschools centers, which were the ones that provided us pre-intervention information. Huber-White sandwich estimator standard errors are in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

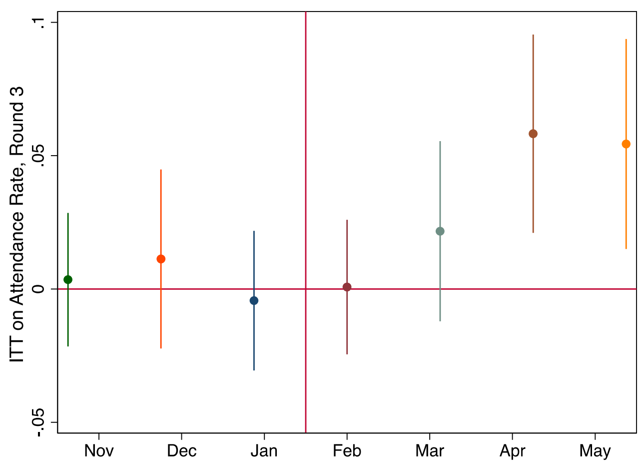

Fig. 6. Event study plot: ITT on attendance rate, pre-intervention sample. Notes: The Figure plots the estimated coefficients (with 90% confidence intervals) of attendance rate on the dummy variable indicating randomization to treatment, by month, for our pre-intervention sample (round 3). The Figure shows coefficients for three months before the start of the SUGU intervention (November, December and January) and four months after it started (February to May). The vertical line depicts the beginning of the SUGU intervention.

hard to adopt and stick to habits of regular attendance. Parents might also be less likely to ensure their children's attendance at preschool because they fail to understand that the child's daily activities are important building blocks of kindergarten school readiness.

Accordingly, we developed a light-touch and low-cost behaviorally informed approach that, if successful, could easily be adopted by programs willing to send texts to families for a multimonth period and tap into their own administrative data. We know of only two field experiments that have tested a similar approach and neither included preschoolers nor did either focus on low-income families. As noted previously, chronic absenteeism is twice as prevalent among preschoolers, making it critical to understand the effectiveness of this approach in this age group. It is also critical to understand how low-income families respond to such an approach given that these families are at the highest risk for chronic absenteeism. Besides these practical considerations, our study collected unique survey data designed to test theories about the nature of the potentially malleable barriers standing in the way of attendance. As we discuss further below, our results suggest a lesser role for present bias (in contrast to other work in this area) and a potentially greater role for parents' expectations for their children's attendance and parents' beliefs about children's learning opportunities. These findings are new in the literature.

Table 7
Mean characteristics: survey non-respondents and respondents groups.

Variable	(1) Non-respondents mean	(2) Respondents mean	(3) P-value of (1)–(2)
Baseline variables			
Intervention in the Fall	0.36	0.33	0.450
Female (Child)	0.53	0.48	0.180
Female (Parent)	0.89	0.90	0.880
Spanish	0.41	0.41	0.870
Sibling in the preschool	0.09	0.11	0.470
Full Day	0.68	0.68	0.950
Outcome variables			
Attended days	54.56	61.17	0.000
(Std. deviation)	(20.17)	(14.65)	
Attendance rate[(a)]	0.77	0.84	0.000
Chronic absenteeism[(b)]			
Attendance rate ≤ 0.90	0.69	0.55	0.000
Attendance rate ≤ 0.85	0.52	0.37	0.000
Observations	216	525	

Notes: "Intervention in the Fall" takes value one if the intervention started October, and zero if it started in February.
"Female (Child)" takes value one if the child is female, and analogously for the variable "Female (Parent)." "Spanish" takes value one if the parent answered the consent form in Spanish, and zero if it was in English. "Sibling" takes the value 1 if the parent had more than one child enrolled in the same preschool and zero if not. "Full Day" takes the value 1 if the child attended on a full-day rather than half-day status.

Using the administrative records from the preschools, we find that a low-cost behaviorally informed intervention increased attended days by 2.5 (0.15 standard deviations) and decreased chronic absenteeism by 9 percentage points (20%) over an 18-week intervention period. The magnitudes of these effects are similar to those reported by Rogers and Feller (2018) and Robinson et al. (2018) for children in elementary and high school. Robinson et al. (2018) decreased chronic absenteeism by 15% using a threshold of 90% attendance; the corresponding figure from our study is 12%. These similarities are striking given large differences between the two samples. For instance, in their socio-economically heterogeneous sample of children in California, the chronic absenteeism rate (using the 90% threshold) is only 5.5% in the control group; the corresponding figure from our low-income sample of preschoolers is approximately 10 times that at 59%. Robinson et al. (2018) achieved these treatment effects by sending, over the course of the school year, approximately five personalized postcard mailings to families containing information designed to correct parents' inaccurate beliefs about school attendance. In contrast, our similarly-sized treatment impact was achieved by sending approximately four SMS

Table 8
Quantile regressions of attendance rate, survey sample.

Dependent variable: attendance rate	(1) Q.10	(2) Q.20	(3) Q.30	(4) Q.40	(5) Q.50	(6) Q.60	(7) Q.70	(8) Q.80	(9) Q.90
Treatment effect	0.052	0.052*	0.022	0.012	0.016	0.004	−0.006	−0.004	−0.001
	(0.035)	(0.027)	(0.016)	(0.013)	(0.013)	(0.010)	(0.011)	(0.009)	(0.007)
Control mean	0.641	0.730	0.810	0.847	0.873	0.907	0.933	0.952	0.974
	(0.026)	(0.023)	(0.014)	(0.010)	(0.009)	(0.008)	(0.007)	(0.005)	(0.005)
Observations	525	525	525	525	525	525	525	525	525

Notes: Each column shows the estimated coefficients of quantile regressions of attendance rate on an indicator for random assignment to the treatment group, at the 10th through 90th quantiles. Bootstrapped standard errors (100 replications) are in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9
Baseline parental beliefs by higher or lower treatment effects (survey sample).

Variable	(1) Higher effects	(2) Lower effects	(3) P-value of (1)–(2)
Parental beliefs about preschool importance			
Number of days expected to miss next 3 months[(a)]	1.85	0.97	0.00
Number of days that are ok to miss	1.87	1.20	0.00
Have you ever sent your child to school even when he/she is sick? (1 = yes; 0 = no)	0.21	0.23	0.66
Preferences for preschool attendance (closer to 1, higher preference)			
Child should never miss a day of preschool? (Agree/St.Agree = 1, 0 otherwise)	0.62	0.66	0.39
Preschool is not as important as 1st grade (Disagree/St.Disagree = 1, 0 otherwise)	0.59	0.56	0.67
Ok to miss preschool if child stays with family (Disagree/St.Disagree = 1, 0 otherwise)	0.54	0.64	0.06
Ok to miss preschool if weather is bad (Disagree/St.Disagree = 1, 0 otherwise)	0.30	0.39	0.08
Ok to miss preschool if I do not have to work (Disagree/St.Disagree = 1, 0 otherwise)	0.70	0.76	0.23
Time preferences task			
Median choice (1 more impatient – 4 more patient)	2.65	2.62	0.85
High discount parents (1 = yes; 0 = no)	0.43	0.47	0.48
Parental beliefs about skills and absences			
How does your child compare to others in social skills (0–10 scale. where 0 is worse and 10 is better)	6.55	6.22	0.22
Would your child be worse off if she misses 5 days of preschool? (1 = yes; 0 = no)	0.50	0.60	0.07
Would your child be worse off if she misses 3 days of preschool? (1 = yes; 0 = no)	0.43	0.49	0.30
How does your child compare to others in academic skills (0–10 scale where 0 is worse and 10 is better)	6.49	6.33	0.57
Would your child be worse off if she misses 5 days of preschool? (1 = yes; 0 = no)	0.45	0.59	0.01
Would your child be worse off if she misses 3 days of preschool? (1 = yes; 0 = no)	0.37	0.50	0.02
Observations	108	417	

Notes: Table 9 shows the average values for a host of baseline parental beliefs and preferences over attendance, by group (with higher and lower treatment effects), and the p-value of the difference.

Table 10
Baseline parental beliefs by higher or lower treatment effects (survey sample).

Composite score	(1) Higher effects	(2) Lower effects	(3) P-value of (1)–(2)
Attendance expectations	−0.483	0.124	0.000
Beliefs about preschool importance	−0.112	0.029	0.240
Time preferences	0.005	−0.001	0.962
Beliefs about impact on skills	−0.113	0.029	0.237

Notes: Table 10 shows the average values for composite scores on baseline parental beliefs and preferences over attendance, by group (with higher and lower treatment effects), and the p-value of the difference. Composite scores come from a factor analysis with principal components. This exercise reduces the dimensionality of the survey variables from Table 9 to four composite scores related to attendance expectations; beliefs about preschool importance; time preferences; and beliefs about the impact of absences on skills. We provide details of the compsite score computation in Table A4 in the Appendix.

messages per week for 18 weeks; these messages also addressed inaccurate beliefs (not only about actual attendance but also about what children were learning in preschool) and, additionally, included goal setting and planning prompt messages designed to overcome procrastination.

However, in contrast to our study and those of Robinson et al. (2018), Balu, Porter, and Gunton (2016) evaluated a text message intervention designed to reduce absenteeism among high school students and found no statistically significant or meaningful effect on attendance. In that study parents received daily absence updates and weekly attendance summaries via text. One explanation for the differences in effectiveness between this intervention versus SUGU (besides the difference in the age of the study population) and the studies by Robinson and colleagues is that the Balu study was purely informational and did not rely on behavioral tools. For instance, whereas SUGU did include feedback on attendance similar to Balu and colleagues, SUGU also included goal setting, planning prompts, and messages focusing on the learning children would miss if they were absent. The effective treatments in Robinson et al. also included behaviorally informed messaging to parents in addition to simply information about children's total absences. In sum, it may be the case that simply providing information to parents about their child's behavior is insufficient to meaningfully change that behavior, especially if parents think their child's behavior is typical or carries no consequences, or the parent feels powerless or uncertain about what they can do to affect that behavior.

Our analysis suggested that the effects of SUGU emerged over time. Treatment effects appear but are not statistically significant by the end of the first month; the effects become statistically distinguishable from zero by the end of the third month and persist at this magnitude until at least the end of the intervention—that is, by month four. These results are consistent with the idea that SUGU gradually helped to change attendance patterns, and they provide some insight into how long it takes to induce behavior change in this realm of parent behavior.

Finally, our survey data suggest that the effects were stronger among those with higher malleable barriers to attendance as measured at baseline. First, the treatment was stronger among parents with lower expectations about attendance, as indicated by parent baseline survey reports of expected absences in the future and acceptable levels of absenteeism. Our goal-setting messages may have worked by making attention more salient for parents. However, it does not appear that this particular element of the treatment worked through managing present bias, insofar as parents who experienced higher and lower treatment effects had similar levels of time preference at baseline.

Second, the results indicate that the treatment was stronger for parents who thought (before the intervention) that absenteeism was not very harmful for their children in terms of social and academic skills. These results suggest that differences in parents' beliefs about the importance of preschool to their children's learning influences their efforts to ensure their child's attendance. It is possible that this barrier is an informational one and that simply informing parents of what is happening within their children's classroom would shift attendance. However, the loss aversion framing of our messages could also have shifted behavior through a cognitive channel. That is, parents could have become more motivated to boost their children's attendance once they were prompted to focus on the loss to children's learning that resulted from not attending school.

Our intervention also included a feedback component that provided parents with accurate information about their children's absences. It is possible that this element of the intervention contributed to the positive treatment effect because it is a form of a personalized information that strengthens parents' capacity to hold accurate information in mind or because it corrected parents' tendency to underestimate their children's actual absences from school (e.g., Rogers & Feller, 2018). This element of the intervention could also have motivated behavior change because it gave parents information that was incongruent with their identity as a "good parent," motivating parents to want to bring their behavior in line with their identity.

Finally, the planning prompts could have contributed to a positive treatment effect, perhaps because they provided tangible new information that made it easier for parents to get their children to school every day, or perhaps because messages of this type help parents focus on attendance and make it a higher priority. The intent of this study was to address a broad range of possible malleable factors that might lead to higher absenteeism. This led us to bundle many motivating tools into our treatment. Consequently, we are not able to adjudicate between all of these possibilities. Nonetheless, in many organizations attendance is a problem and the motivation for absenteeism may be similar to the motivation for absenteeism from preschools. This study suggests a low-cost and effective way to address malleable causes of absenteeism. Future research should both try to untangle which tools are the most effective at addressing absenteeism and test these in a broad range of organizational types.

7.1. Limitations and future research

Because multiple types of behavioral messages were bundled together in our intervention, we cannot determine which of them (either singly or jointly) drove our significant treatment impacts. Future work with larger samples could separate these messages into distinct treatment arms to better understand which was driving the treatment impacts.

Second, although our intervention was successful, the rates of chronic absenteeism remain high in this population. Even with our treatment, about one-third of the children attended less than 85% of school days. Clearly, there is more work to be done to address this problem. Head Start and similar centers could easily adopt our approach, but they may also have to consider additional approaches to layer on top of it.

While the SUGU study results strongly suggest the promise of using behavioral tools to motivate low-income parents to support their children's attendance at Head Start, many questions for future research remain. Among these are how long a behavioral intervention must last before parents no longer need the behavioral tools to reinforce the new behavior, which specific behavioral tools lead to the greatest change in parental behavior, and how much altering parental behavior alters child outcomes. Finally, the results of SUGU—like the results of all research—should be replicated. If replications support the evidence found in SUGU, finding ways to widely implement behaviorally informed programs to alter parental engagement is also a high priority.

8. Conclusion

Absenteeism is a problem in most organizations and solving it with traditional tools like incentives, monitoring, or fines has shown to be challenging and expensive (Chaudhury, Hammer, Kremer, Muralidharan, & Rogers, 2006; Gneezy & Rustichini, 2000; Gneezy, Meier, & Rey-Biel, 2011). In this paper we test the effectiveness of a set of low-cost behavioral tools in reducing absenteeism at an important type of organization—namely, preschool centers serving low-income children. Absenteeism is an important problem given the well-known gaps in children's readiness for formal schooling by family background. Moreover, managers of these organizations (preschool centers' directors) reported that attempts to increase attendance consume large amounts of resources that could be used for other educational purposes. We designed and implemented a randomized controlled trial to address this problem. Our results shows qualitatively important treatment impacts on increasing attended days and decreasing chronic absenteeism, and provide further evidence that malleable barriers play a role as determinants of attendance. Future interventions aiming to reduce absenteeism in organizations could benefit from identifying and targeting malleable barriers with low-cost light-touch behavioral tools.

Acknowledgements

We wish to thank seminar participants at Princeton's Education Research Section Workshop, the APPAM and SRCD annual meetings, and members of the Behavioral Insights and Parenting Lab for their contributions to study design, data collection, and copyediting support, including William Delgado Martinez, Keri Lintz, Jill Gandhi, Sophie Milam, Beth Ordaz, Paula Rusca, Jessica Chen, and the field research assistant team. Gallegos thanks Jacob Shapiro and the ESOC (Princeton University) and the Center for Studies of Conflict and Social Cohesion (CONICYT/FONDAP/15130009). This experiment is listed in the American Economic Association Registry for Randomized Controlled Trials— #AEARCTR-0002549. The authors gratefully acknowledge financial support from the Center for Human Potential and Public Policy and the Joyce Foundation. The views expressed in this paper are those of the authors and should not be attributed to the Inter-American Development Bank.

Appendix A

See Tables A1–A4.

Table A1
Number of days, weekend, holidays and potential class days per month.

Round 1	Months in Year 2016				
Number of days	February	March	April	May	Total
In the month	29	31	30	31	121
Weekends/holidays	9	8	9	10	36
Potential class days	20	23	21	21	85
Round 2	Months in Year 2016–2017				
Number of days	October	November	December	January	Total
In the month	31	30	31	31	123
Weekends/holidays	10/11	10	15	10	45/46
Potential class days for each Center:					
Center G	21	20	16	21	78
Center 3	21	20	16	20	77
Center 2	21	20	16	20	77
Center P	20	20	16	19	75
Round 3	Months in Year 2017				
Number of days	February	March	April	May	Total
In the month	28	31	30	31	120
Weekends/holidays	9	8	11	9	36
Potential class days	19	23	19	22	83
Potential class days (a)	19	23	15	21	78
Potential class days (b)	15	18	12	18	63

Notes: February 2016: 29 days, Monday 1 to Monday 29, minus 8 weekend days and President's Day (Feb 15), leaving 20 class days.
March 2016: 31 days, Tuesday 1 to Thursday 31, minus 8 weekend days, leaving 23 class days. April 2016: 30 days, Friday 1 to Saturday 30, minus 9 weekend days, leaving 21 class days. May 2016: 31 days, Sunday 1 to Tuesday 31, minus 9 weekend days & Memorial Day (May 30), leaves 21 class days.
Notes: October 2016: 31 days, Saturday 1 to Monday 31, minus 10 weekend days and Columbus Day (Oct 10), leaving 20 class days. Center P was closed on Oct. 10, but centers G, 3, and 2 were open on Columbus Day (Oct 10), so they have 21 class days. However, attendance was on average about 57 percent that day, vs. high 80 s on Tuesday 11 and other Mondays in the month.
November 2016: 30 days, Tuesday 1 to Wednesday 30, minus 8 weekend days & 2 Thanksgiving days (Nov 24 and Nov 25). All centers were open on Veterans Day (Nov 11).
December 2016: 31 days, Thursday 1 to Saturday 31, minus 9 weekend days and 6 holidays (Dec 23, and Dec 26–30), leaving 16 class days. On Dec 9 attendance was very low at Center G (30%) compared to Dec 8 (84%) and Dec 2 (88%) and Dec 16 (84%).
January 2017: 31 days, Sunday 1 to Tuesday 31, minus 9 weekend days, and MLK Day, and New Year's Day observance Jan 2, leaving 20 class days. Center G was open on MLK day, but attendance was low that day (Jan 16; 33%), compared to 82% the next day, 87% the previous Monday (Jan 9) and 78% the following Monday (Jan 23). Center P only gave information aggregated by month, and they say they had 19 class days in January.
Notes: February 2017: 28 days, Wednesday 1 to Tuesday 28, 8 weekend days and President's Day (Feb 20), leaving 19 class days.
March 2017: 31 days, Wednesday 1 to Friday 31, and 8 weekend days, leaving 23 class days.
April 2017: 30 days, Saturday 1 to Sunday 30, 10 weekend days and Good Friday (Apr 14), leaving 19 class days.
May 2017: 31 days, Monday 1 to Wednesday 31, 8 weekend days and Memorial Day (May 29), leaving 21 class days.
(a) Three out of four centers gave Easter Week (Mon, Apr 10–Thu, Apr 13) as a holiday, in addition to Good Friday (Apr 14) and also no class on Friday, May 26.
(b) These are potential days for children who did not have class on Fridays, who also had Easter Week (Mon, Apr 10–Thu, Apr 13) as a holiday.

Table A2
Intention-to-treat effects, subsample (92% of full sample).

Dependent variable	(1) Days of intervention	(2) Potential school days	(3) Enrolled days	(4) Attended days	(5) Attendance rate	(6) 1(Rate < =90)	(7) 1(Rate < =85)
Treatment	0.0157	0.351	1.707	2.481**	0.0154	−0.0650*	−0.0868**
	(0.107)	(0.393)	(1.156)	(1.240)	(0.013)	(0.038)	(0.038)
Constant	121.1***	76.88***	69.52***	58.08***	0.817***	0.621***	0.451***
	(0.077)	(0.289)	(0.881)	(0.929)	(0.009)	(0.027)	(0.027)
Dep. Variable Mean	121.12	77.06	70.39	59.34	0.83	0.59	0.41
Dep. Variable: Std. Dev.	1.40	5.14	15.08	16.21	0.17	0.49	0.49
Observations	684	684	684	684	684	684	684

Notes: Each column shows the estimated coefficients of regressing the respective dependent variable on an indicator for random assignment to the treatment group.The subsample used to produce the estimates has available information for child low-birth weight, parental education, and home–preschool commuting time (92 percent of our full sample). Huber-White sandwich estimator standard errors are in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3
Treatment effects interacting with survey response.

	(1) Attended days	(2) Attendance rate	(3) 1(Rate < =90)	(4) 1(Rate < =85)
Treatment (β_1)	3.648	0.0219	−0.185***	−0.204***
	(2.734)	(0.029)	(0.062)	(0.067)
Survey response (β_2)	7.462***	0.0723***	−0.214***	−0.231***
	(2.199)	(0.021)	(0.051)	(0.056)
Treat*Survey Response (β_3)	−1.757	−0.0108	0.155**	0.158**
	(3.020)	(0.032)	(0.076)	(0.079)
Constant (β_0)	52.74***	0.763***	0.778***	0.620***
	(1.975)	(0.019)	(0.040)	(0.047)
Dep. Variable: Mean	59.24	0.82	0.59	0.41
Dep. Variable: Std. Dev.	16.71	0.17	0.49	0.49
Effect on respondents ($\beta_1 + \beta_3$)	1.891	0.0111	−0.0306	−0.0458
	(1.282)	(0.013)	(0.044)	(0.042)
Effect on the total sample	2.460**	0.0148	−0.0769**	−0.0932***
	(1.226)	(0.013)	(0.036)	(0.036)
Observations	741	741	741	741

Notes: Each column shows the estimated coefficients of regressing the respective dependent variable on an indicator for random assignment to the treatment group, an indicator for survey response, and the interaction of both. The estimated equation is $Y = \beta_0 + \beta_1 T + \beta_2 SR + \beta_3 (T * SR) + \varepsilon$. Huber-White sandwich estimator standard errors are in parentheses.

Table A4
Factor analysis details.

Panel A: Factor Analysis, principal components with ortoghonal varimax rotation

Factor	Variance	Difference	Proportion	Cumulative
Factor 1	2.698	0.896	0.193	0.193
Factor 2	1.801	0.154	0.129	0.321
Factor 3	1.647	0.178	0.118	0.439
Factor 4	1.469	.	0.105	0.544

Panel B: Rotated factor loadings (pattern matrix) and unique variances

Variables from Table 9	Factor 1	Factor 2	Factor 3	Factor 4	Uniqueness
Variable 1	0.007	0.005	0.022	0.831	0.310
Variable 2	0.252	0.050	0.162	0.725	0.382
Variable 3	0.140	0.004	−0.278	0.263	0.834
Variable 4	−0.003	0.039	0.137	0.125	0.964
Variable 5	0.195	−0.056	0.445	−0.156	0.736
Variable 6	0.049	−0.017	0.734	0.015	0.459
Variable 7	−0.031	0.049	0.550	0.237	0.638
Variable 8	−0.007	−0.053	0.691	0.158	0.495
Variable 9	0.037	0.935	−0.022	−0.001	0.124
Variable 10	0.071	0.929	−0.005	0.033	0.131
Variable 11	0.821	0.033	0.078	−0.058	0.316
Variable 12	0.788	−0.102	−0.004	−0.011	0.369
Variable 13	0.800	0.175	−0.001	0.143	0.308
Variable 14	0.795	0.099	−0.014	0.195	0.320

Notes: We also analyzed parental beliefs and preferences in a factor analysis with principal components. This exercise reduces the dimensionality of our 14 survey variables from Table 9 and generates indexes with variables that measure similar concepts. The variables loaded onto four factors related to attendance expectations; beliefs about preschool importance; time preferences; and beliefs about absences' impact on skills. We performed the rotation of the loading matrix using the varimax and oblimin methods to produce orthogonal factors. The percentage of variance explained by the factors is 54%. The factor loadings indicate that the three first variables from Table 9 were the more relevant variables for Factor 4, so we relate it to attendance expectations. The next five variables were more relevant for Factor 3, and hence we call that factor "preschool importance." The two variables on median choice in our time preference task and our impatience indicator are more relevant for Factor 2, so we related it to time preferences. The final four variables in Table 9 are more relevant for Factor 1.

Appendix B. Supplementary material

Supplementary data to this article can be found online at https://doi.org/10.1016/j.obhdp.2019.11.002.

References

Abadie, A., Athey, S., Imbens, G. W., & Woolridge, J. (2017). *When should you adjust standard errors for clustering? NBER Working Paper No. 24003.* Cambridge: The National Bureau of Economic Research.

Anderson, A. R., Christenson, S. L., Sinclair, M. F., & Lehr, C. A. (2004). Check & connect: The importance of relationships for promoting engagement with school. *Journal of School Psychology, 42*(2), 95–113.

Balfanz, R., & Byrnes, V. (2012). Meeting the challenge of combating chronic absenteeism: Impact of the NYC Mayor's Interagency Task Force on chronic absenteeism and school attendance and its implications for other cities. Baltimore, MD: Everyone Graduates Center, Johns Hopkins University School of Education.

Balu, R., Porter, K., & Gunton, C. (2016). *Can informing parents help high school students show up for school? Results from a partnership between New Visions for Public Schools and MDRC.* New York: MDRC.

Bergman, P. (2015). Parent–Child Information Frictions and Human Capital Investment: Evidence from a Field Experiment. CESifo Working Paper 5391.

Berry, C., Lelchook, A. M., & Clark, M. (2012). A meta-analysis of the interrelationships between employee lateness, absenteeism, and turnover: Implications for models of withdrawal behavior. *Journal of Organizational Behavior, 33*(5), 678–699.

Bowles, S., & Gintis, H. (1976). *Schooling in capitalist north America: Educational reform and the contradictions of economic life.* Chicago: Haymarket Books.

Castleman, B. L., & Page, L. (2014). *Summer nudging: Can personalized text messages and peer mentor outreach increase college going among low-income high school graduates? Center for education policy and workforce competitiveness working paper 9.* Charlottesville, VA: University of Virginia.

Chabris, C. F., Laibson, D., Morris, C. L., Schuldt, J. P., & Taubinsky, D. (2008). Individual laboratory-measured discount rates predict field behavior. *Journal of Risk and Uncertainty, 37*(2/3), 237–269.

Chang, H. N., & Romero, M. (2008). *Present, engaged, and accounted for: The critical importance of addressing chronic absence in the early grades.* New York: National Center for Children in Poverty, Mailman School of Public Health, Columbia University.

Chaudhury, N., Hammer, J., Kremer, M., Muralidharan, K., & Rogers, F. H. (2006). Missing in action: Teacher and health worker absence in developing countries. *Journal of Economic Perspectives, 20*(1), 91–116.

Chugh, D., & Bazerman, M. H. (2007). Bounded awareness: What you fail to see can hurt you. *Mind & Society, 6*(1), 1–18.

Connolly, F., & Olson, L. S. (2012). *Early elementary performance and attendance in Baltimore City Schools' pre-kindergarten and kindergarten.* Baltimore: Baltimore Education Research Consortium.

Ehrlich, S. B., Gwynne, J. A., & Allensworth, E. M. (2018). Pre-kindergarten attendance matters: Early chronic absence patterns and relationships to learning outcomes. *Early Childhood Research Quarterly, 44*, 136–151.

Ehrlich, S. B., Gwynne, J. A., Pareja, A. S., & Allensworth, E. M. (2013). *Research summary: Preschool attendance in Chicago public schools.* Chicago: University of Chicago Consortium on Chicago School Research.

Ehrlich, S. B., Gwynne, J. A., Pareja, A. S., & Allensworth, E. M. (2014). *Preschool attendance in Chicago public schools: Relationships with learning outcomes and reasons for absences.* Chicago: University of Chicago Consortium on Chicago School Research.

Gershenson, S., Jacknowitz, A., & Brannegan, A. (2017). Are student absences worth the worry in US primary schools? *Education Finance and Policy, 12*(2), 137–165.

Giné, X., Karlan, D., & Zinman, J. (2010). Put your money where your butt is: A commitment contract for smoking cessation. *American Economic Journal: Applied Economics, 2*(4), 213–235.

Gneezy, U., Meier, S., & Rey-Biel, P. (2011). When and why incentives (don't) work to modify behavior. *Journal of Economic Perspectives, 25*, 191–210.

Gneezy, U., & Rustichini, A. (2000). A fine is a price. *Journal of Legal Studies, 29*(1), 1–18.

Gollwitzer, P. M., & Sheeran, P. (2006). Implementation intentions and goal achievement: A meta-analysis of effects and processes. *Advances in Experimental Social Psychology, 38*, 69–119.

Gottfried, M. A. (2009). Excused versus unexcused: How student absences in elementary school affect academic achievement. *Educational Evaluation and Policy Analysis, 31*(4), 392–415. https://doi.org/10.3102/0162373709342467.

Gottfried, M. A. (2011). Absent peers in elementary years: The negative classroom effects of unexcused absences on standardized testing outcomes. *Teachers College Record, 113*(8), 1597–1632.

Gottfried, M. A. (2014). Chronic absenteeism and its effects on students' academic and socioemotional outcomes. *Journal of Education for Students Placed at Risk (JESPAR), 19*(2), 53–75.

Gottfried, M. A., & Ehrlich, S. B. (2018). Introduction to the special issue: Combating chronic absence. *Journal of Education for Students Placed at Risk (JESPAR), 23*(1–2), 1–4. https://doi.org/10.1080/10824669.2018.1439753.

Guryan, J., Christenson, S., Claessens, A., Engel, M., Lai, I., Ludwig, J., & Turner, M. C. (2017). *The effect of mentoring on school attendance and academic outcomes: A randomized evaluation of the Check & Connect Program. Working Paper No. WP-16-18.* Evanston, IL: Institute for Policy Research, Northwestern University.

Hutt, E. L. (2018). Measuring missed school: The historical precedents for the measurement and use of attendance records to evaluate schools. *Journal of Education for Students Placed at Risk (JESPAR), 23*(1–2), 5–8.

Jacob, B. A., & Lovett, K. (2017). Chronic absenteeism: An old problem in search of new answers. Washington, DC: The Brookings Institution. https://www.brookings.edu/research/chronic-absenteeism-an-old-problem-in-search-of-new-answers.

Kahneman, D., & Tversky, A. (1979). Prospect theory: An analysis of decision under risk. *Econometrica, 47*(2), 263–291.

Karlan, D., McConnell, M., Mullainathan, S., & Zinman, J. (2016). Getting to the top of mind: How reminders increase saving. *Management Science, 62*(12), 3393–3411.

Katz, M., Adams, G., & Johnson, M. (2015). Insights into absenteeism in DCPS early childhood program. Washington, DC: Urban Institute. https://greaterdc.urban.org/sites/default/files/publication/39151/2000083-insights-into-absenteeism-in-dcps-early-childhood-program_0.pdf.

Koslowsky, M. (2009). A multi-level model of withdrawal: Integrating and synthesizing theory and findings. *Human Resource Management Review, 19*(4), 283–303.

Locke, E. A. (1968). Toward a theory of task motivation and incentives. *Organizational Behavior and Human Performance, 3*(2), 157–189.

Locke, E. A., & Latham, G. P. (2002). Building a practically useful theory of goal setting and task motivation. *American Psychologist, 9*, 705–717.

Mayer, S. E., Kalil, A., Oreopoulos, P., & Gallegos, S. (2018). Using behavioral insights to increase parental engagement: The parents and children together intervention. *Journal of Human Resources,* 0617–8835R. https://doi.org/10.3368/jhr.54.4.0617.8835R.

Meier, S., & Sprenger, C. (2010). Present-biased preferences and credit card borrowing. *American Economic Journal—Applied Economics, 2*(1), 193–210.

Milkman, K. L., Beshears, J., Choi, J. J., Laibson, D., & Madrian, B. C. (2011). Using implementation intentions prompts to enhance influenza vaccination rates. *Proceedings of the National Academy of Sciences, 108*(26), 10415–10420.

Navarro, C., & Bass, C. (2006). The cost of employee abstenteeism. *Compensation and Benefits Review, 38*(6), 26–30.

Nickerson, D. W., & Rogers, T. (2010). Do you have a voting plan? Implementation intentions, voter turnout, and organic plan making. *Psychological Science, 21*(2), 194–199.

Ready, D. D. (2010). Socioeconomic disadvantage, school attendance, and early cognitive development: The differential effects of school exposure. *Sociology of Education, 83*(4), 271–286.

Richburg-Hayes, L., Anzelone, C., Dechausay, N., Datta, S., Fiorillo, A., Potok, L., ... Balz, J. (2014). *Behavioral economics and social policy: Designing innovative solutions for programs supported by the administration for children and families. OPRE Report 2014-16a.* Washington, DC: US Department of Health and Human Services, Office of Planning, Research and Evaluation, Administration for Children and Families.

Robinson, C. D., Lee, M. G., Dearing, E., & Rogers, T. (2018). Reducing student absenteeism in the early grades by targeting parental beliefs. *American Educational Research Journal, 55*(6), 1163–1192.

Rodgers, A., Corbett, T., Bramley, D. M., Riddell, T., Willis, M., Lin, R.-B., & Jones, M. (2005). Do u smoke after txt? Results of a randomised trial of smoking cessation using mobile phone text messaging. *Tobacco Control, 14*(4), 255–261.

Rogers, T., & Feller, A. (2018). Reducing student absences at scale by targeting parents' misbeliefs. *Nature Human Behaviour, 2*(5), 335.

Rogers, T., Milkman, K. L., John, L. K., & Norton, M. I. (2015). Beyond good intentions: Prompting people to make plans improves follow through on important tasks. *Behavioral Science & Policy, 1*(2).

Romero, M., & Lee, Y. S. (2007). *A national portrait of chronic absenteeism in the early grades.* New York: National Center for Children in Poverty, Mailman School of Public Health, Columbia University.

Simons, D. J., & Chabris, C. F. (1999). Gorillas in our midst: Sustained inattentional blindness for dynamic events. *Perception, 28*(9), 1059–1074.

Smythe-Leistico, K., & Page, L. (2018). Connect-text: Leveraging text-message communication to mitigate chronic absenteeism and improve parental engagement in the earliest years of schooling. *Journal of Education for Students Placed at Risk (JESPAR), 23*(1–2), 139–152. https://doi.org/10.1080/10824669.2018.1434658.

Stockwell, M. S., Kharbanda, E. O., Martinez, R. A., Vargas, C. Y., Vawdrey, D. K., & Camargo, S. (2012). Effect of a text messaging intervention on influenza vaccination in an urban, low-income pediatric and adolescent population: A randomized controlled trial. *JAMA: Journal of the American Medical Association, 307*(16), 1702–1708.

United States DEPARTMENT OF HEALTH AND HUMAN SERVices. (2016). Head Start Program performance standards and other regulations. Washington, DC: US Department of Health and Human Services, Administration for Children and Families, Head Start Bureau.