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A behavioral intervention to increase preschool attendance in Uruguay☆

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ABSTRACT

This paper presents the results of a nationwide, low-cost intervention that used messages informed by behavioral economics and delivered through the government's official mobile app to increase preschool attendance in Uruguay. We document null results for attendance and child development outcomes. We also estimate conditional average treatment effects (CATE) across individuals using causal forest algorithms. We present exploratory evidence that absenteeism and some measures of cognitive development might have improved for children around the median of the baseline distribution of attendance rate.

1. Introduction

Preschool attendance is crucial for child development and has long-term effects on individuals' academic performance, adult human capital, and economic self-sufficiency (Berlinski et al., 2008; Conti et al., 2016; Bailey et al., 2021). Although a vast literature shows that low-cost behavioral strategies could effectively reduce absenteeism among primary or secondary students (Berlinski et al., 2016; Bergman and Chan, 2021; Bergman, 2021), evidence on preschool children is scarcer and mostly focuses on developed countries (Robinson et al., 2018; Rogers and Feller, 2018; Kalil et al., 2019). Moreover, although there is evidence of the effect of several behavioral interventions on child development (for instance, interventions to improve parenting practices; see Barrera et al. (2020)), the interventions that focus on reducing absenteeism do not typically analyze the impact on child development outcomes, which are important predictors of academic performance and individuals' labor market outcomes over a person's lifetime.

In this paper, we aim to fill this gap by providing the results of a nationwide, low-cost behavioral experiment designed to increase preschool attendance and potentially improve child development in a developing country (Uruguay). We evaluate a government program that sent messages informed by behavioral economics to parents of preschool children using the official mobile app (*GURÍ*) the government uses to communicate with parents. The messages were sent to parents for 13 weeks and were automated (that is, they were based on students' administrative information that was already uploaded to the system). The messages described the short- and long-term benefits of preschool education, gave parents feedback on their child's absences in the previous three weeks, and helped families plan the week in order to minimize absenteeism.

We first document the intervention's null effect on measures of absenteeism and child development. We then estimate conditional average treatment effects across individuals using a causal forest algorithm (Athey and Imbens, 2016; Athey et al., 2019; Wager and Athey, 2018). Our exploratory analysis shows suggestive evidence that the treatment led to a drop in absenteeism and an increase in some measures of the cognitive domain of child development for children around the median

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[1] For attendance rate, we also identify a significant effect for less-than-higher-income schools (school-level SES below the highest quintile). We do not find similar effects for child development outcomes.

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of the baseline distribution of attendance rate. For attendance, we also document a significant effect for schools with socioeconomic status (SES) below the highest quintile. More specifically, we identify positive effects on attendance only around the median of the pre-treatment attendance rate (deciles 4, 5 and 7). This effect of approximately 1.8 days (control mean of 50.64) is significant after correcting for multiple comparisons. This effect translates to an increase in attendance rate of approximately 2 percentage points (the mean of the control is 0.80). Consistent with these results, we document a positive effect on language development of between 0.25 and 0.40 standard deviations in the same deciles of pre-treatment attendance rate (the mean of the control is 0.48). We also find an effect on cognition, which after adjusting for multiple comparisons, remains significant only for the seventh decile. Although the analysis of heterogeneity is data driven, these results should be interpreted as exploratory and suggestive, as they were not registered in our original pre-analysis plan.[1]

Our results emphasize the limits of the ability of behavioral interventions to reduce absenteeism in preschool. First, the average effect of the intervention was null in our sample. Second, the exploratory heterogeneous analysis by baseline attendance rates suggests that children with pre-treatment absenteeism rate that are too high (absences on over 25% of days) were immune to the intervention. Children in this group typically come from lower-income families (about half of those in the lowest pre-treatment attendance rate decile are also in the lowest two quintiles of school socioeconomic status, SES). One plausible interpretation of our results is that relatively high poverty levels require more structural/intensive interventions (for instance, interventions that aim to remove structural barriers instead of just reducing psychological constraints). In contrast, the null effect among children with relatively low pre-treatment absenteeism rates (of 15% or less) suggests a possible upper bound for these types of policies.

We conducted this research in a context that lent itself to testing this type of intervention. Although Uruguay has increased preschool enrollment, reaching almost universal coverage for four- and five-year-old children, attendance levels remain low. In 2018, more than a third of Uruguayan children enrolled in public preschool centers had insufficient attendance (meaning they were absent on more than 25% of school days). Students enrolled in schools in lower socioeconomic areas had even higher levels of absenteeism.[2] Moreover, the existence of a government mobile app (*GURÍ*, an educational information monitoring system for families that allows educational centers and families to communicate) made the intervention easy, inexpensive (virtually free), and scalable.

Our paper contributes to a growing literature on the effects of informational messages or messages informed by behavioral science on student absenteeism. For instance, Bergman and Chan (2021) show that a light-touch intervention that sent automated messages with students' information (absences, missed assignments, and grades) to parents reduced absenteeism and the number of failed school years among middle and high-school students. A similar experiment by Bergman (2021) shows that an SMS intervention to adjust parents' misperceptions about their children's efforts affected students' performance. In a related work by Berlinski et al. (2016), the authors show that a texting intervention in elementary schools in Chile (containing information about children's test scores, grades, and attendance) positively affected attendance and test scores.[3] Unlike these studies, our research focuses on preschool children.

A few other recent studies share our focus on preschool children. For instance, Robinson et al. (2018) find that a message-based intervention that sought to change parents' false beliefs about pre-primary education reduced absenteeism and chronic absenteeism in California. Kalil et al. (2019) show that the "Show Up 2 Grow Up" program in Chicago, which consists of sending text messages informed by behavioral science to parents for 18 weeks (reminders, feedback on absenteeism, messages about the importance of preschool education, planning prompts), had a positive effect on attendance.[4]

Our paper makes several contributions to the research documented in previous papers. Like (Robinson et al., 2018; Kalil et al., 2019; Doss et al., 2017), and unlike (Berlinski et al., 2016; Bergman and Chan, 2021; Bergman, 2021), our focus is on preschool children instead of elementary or post-elementary students. However, in addition to testing the effects on school attendance, we test the effects on child development outcomes. This set of outcomes is crucial for predicting children's long-term development. Moreover, while most of this literature focused on preschool children from developed countries (specifically the U.S.), our intervention was implemented in a developing country.

Another crucial aspect of our intervention is that all messages we sent were automated and used administrative data already uploaded to the system. Moreover, our intervention does not rely on third-party SMS providers to deliver the messages, since we leverage an existing system (*GURÍ*). Not only is this characteristic relevant to the feasibility of policies for scaling up this type of intervention, it also meant the intervention was implemented at practically zero cost. This feature is similar to Bergman and Chan (2021), but stands in contrast to most other related experiments.

The rest of this paper is organized as follows: Section 2 describes the experimental design. Section 3 presents the econometric model. Section 4 lays out the main results, and Section 5 contains conclusions.

2. The experiment

2.1. Context

Uruguay has dramatically increased its preschool coverage in recent years, reaching almost universal education for children ages four and five. This progress reflects major investment in infrastructure and educational personnel to increase enrollment. However, attendance is still a problem. In 2018, 81% of students were chronically absent (attending 90% of classes or less). Meanwhile, 38% had insufficient attendance (attending just 70–139 of the school year's 187 days), up from 30% in 2013. The average number of absences rose from 34 days in 2013 to 41 days in 2018. This paper's experiment was implemented nationwide, in collaboration with the Consejo Educación Inicial *y* Primaria (CEIP) of the Administración Nacional de Educación Pública (ANEP). CEIP oversees national policy for preschool and primary education. To improve its management capacity, in 2011 CEIP launched the *GURÍ* system, a unified management system for records and information. This web information system registers information on students, parents, and teachers, as well as information on enrollment and students' attendance and grades. Parents can use *GURÍ* to access information on their children. The app also allows teachers and parents to communicate with each other. Fig. A.1 in the Appendix shows a screenshot of the *GURÍ* mobile app. Our intervention was purposely designed to be conducted through *GURÍ* and use already existing data so that, if successful, it could be easily scaled up.

[2] While students in schools in the highest SES quintile attend 84% of classes on average, in the lowest quintile this value drops to 75%.

[3] In a related paper, Cunha et al. (2017) analyze whether communication with parents works because it provides personalized information about students' absences or because it reinforces the importance of school attendance. Messages that share information about children's absences had small effects. Messages stating the importance of attendance accounted for 89%–126% of the effects of messages with feedback about attendance.

[4] Doss et al. (2017) also focus on preschool children in the U.S. and show that using differentiated information rather than generic messages improves results.

2.2. Design

The intervention consisted of a text message campaign delivered using the mobile app *GURÍ*. There are several advantages of using a mobile app (instead of SMS) for communication campaigns. One is the low cost of implementing and scaling up the campaign. Once the messages have been programmed, it costs almost nothing to expand and replicate the intervention. Moreover, the messages use administrative data already uploaded to the system (days missed by each student), which makes the process easily scalable.

Another benefit is that the mobile app is not tied to a person's cellphone number, which helps those in charge of the intervention maintain regular contact with parents. People change numbers frequently in Latin America, which poses a significant challenge for interventions delivered through text messages (Bloomfield et al., 2019). The app may also help increase parents' trust in the messages, as they receive them through an institutional channel. However, low uptake of mobile apps limits this technology's effectiveness as a vehicle to deliver information to parents.

We designed 43 messages to be sent to the treatment group of parents during the last three months of the school year.[5] Parents in the control group did not receive messages. As some parents agreed to participate in the program after it started, we ended up delivering 34 messages per parent on average. Table A.1 in the Appendix presents the numbers of messages per type of message. The following sub-section describes the content and rationale of each message.

Cunha et al. (2017) find alternating the delivery time to be more effective than sending messages at a fixed time. We therefore varied the day and time of delivery to keep parents from anticipating the message. We varied the frequency of messages every week: one week, we delivered three messages, on Tuesday, Thursday, and Sunday; the following week, we sent four messages, on Monday, Wednesday, Thursday, and Sunday. We limited the number of messages to a maximum of four, as more messages have been found to reduce the effect of the intervention (Cortes et al., 2021). We also combined weekend and weekday deliveries, as the literature suggests heterogeneous effects conditional on which messages are sent (Cortes et al., 2021). The timing of delivery also varied, with messages sent at either 5 p.m. or 7 p.m. We always sent a message on Thursday because Friday is the day students are most likely to miss school (see Fig. A.2 in the Appendix).

2.3. Messages' content

Several factors may influence pre-primary attendance (Chang and Romero, 2008; Jacob and Lovett, 2017). Some are structural, associated with students' characteristics and background (including parents' level of education, household income, community infrastructure, transportation, and school- and community-related factors). Others are tied to cognitive biases that influence parents' decisions. Our intervention was designed to lower absenteeism by reducing certain cognitive biases or psychological barriers preventing parents from taking their children to preschool.

We focused on the cognitive component and designed messages based on the psychological biases that several studies have identified as potential barriers for caregivers, especially in low-income contexts, as well as on the results from 10 focus groups we conducted in different regions of Uruguay with a total of 79 parents. The focus groups explored parents' perceptions and attitudes. They reported behavior on different dimensions that the literature has shown to be linked to student attendance (the instrument of the focus groups is shown in Table A.2 in the Appendix). Results from the focus groups suggested that although some absences are produced by structural factors (such as illness or unexpected events), many absences (such as those related to bad weather, family events, and medical appointments) are preventable. An intervention that targets malleable components of absence could, therefore, increase student attendance. For instance, the focus groups revealed that false perceptions and beliefs play a role in how parents think about attendance, with parents underestimating the number of days their children missed school. The focus groups also revealed that parents value preschool education in general but underestimate the short- and long-term cognitive and life gains it yields, which may translate to lower investment in their children's preschool education.

We designed messages to tackle the following potential cognitive biases or anomalies: mistaken beliefs related to number of absences (Bergman and Chan, 2021), present bias (related to the cost–benefit of not missing preschool days), mismatched identity (as Gennetian et al. (2016) show, parents might not believe in their own effort to affect their children's lives), and limited attention (parents are forgetful, especially if the cognitive bandwidth is limited, a situation particularly likely among lower-income families, as Mani et al. (2013) show). We conveyed the messages for all these biases or anomalies through four behavioral tools:

(i) ***Feedback.*** Every three weeks, we sent a feedback message to parents that included the number of times their children were absent.[6] If a child did not miss any days of school, the message ended by congratulating the parent. The idea of these messages was to correct parents' potentially mistaken beliefs, which could be driven by limitations in their attention or by their bias with respect to their children. Feedback messages can correct parents' mistaken beliefs about their children's attendance rate and have proved helpful in increasing school attendance (Kalil et al., 2019; Robinson et al., 2018; Rogers and Feller, 2018; Keren and Wu, 2015). An example feedback message is "*[Parent name], [child name] was absent [number] days in the last three weeks. Help [him/her] develop a habit of responsibility by not missing more days the rest of the year!*".

(ii) ***Planning prompts.*** We sent planning prompts to help parents tie their goals to concrete actions to achieve them or to identify potential events that might prevent them from achieving their goals. Parents may mean to bring their children to school every day but fail to do so if they forget about their intention or procrastinate when they were supposed to take a specific action. Planning prompts can work in cases of limited attention. Researchers have also shown them to be an effective way to reduce student absenteeism (Kalil et al., 2019). An example of this type of message is, *"[Parent name]: Think about the reasons that may have kept your child from attending school last year. Create a plan to avoid them this school year!"*

(iii) ***Positive parental identity.*** We included messages affirming parents' ability to ensure their children attended preschool to increase their receptiveness to the message campaign. As Gennetian et al. (2016) show, mismatched identity (which causes parents not to believe that they can change their child's attendance through their efforts) is a common bias parents face when making decisions about their children. Affirming parents' parental identity and their capacity as parents can increase their involvement in parenting support programs (Gennetian et al., 2016; Rogers et al., 2017). An example of this type of message is: *"[Parent name], what [child name] learns in preschool will last a lifetime. Help [her/him] go to preschool. You play an important role in improving [her/his] attendance!"*

[5] The campaign started on September 22 and ended on December 22, 2019.

[6] Feedback was given every three weeks to make it more likely that it would show that the child had missed least one school day that month. Pre-treatment data showed that 53% of students missed at least one day every three weeks.

(v) ***Short- and long-term gains.*** We designed messages that underlined the socio-emotional and cognitive skills children gain by attending preschool. We also mentioned how missing days of school hampers these gains. The hypothesis underlying this message is that parents may face intertemporal decisions in parental investment that could be problematic for present-biased parents (Bloomfield et al., 2019). The intervention delivered two variants of these messages. The first combined negative and positive framing. The second disaggregated the benefits of preschool education in the short-run (e.g., math skills) and the long-run (e.g., future job prospects). Examples of this type of message are: *"Hello [parent name]. Have you noticed the change in the development of [child name] since [she/he] began attending preschool? Imagine what it would be like if [she/he] went every day. Don't let the rain be an excuse, take [her/him]!"; "Hello [Parent name]. Preschool attendance is associated with better achievements in children's school careers. It is important that [child name] attend daily!"*

Table A.1 in the Appendix shows all the messages we sent. Table A.3 describes each behavioral bias we sought to counter and how the intervention addressed it using one of the four models presented above. Our design has two shortcomings. First, although we conducted focus groups to identify potential cognitive biases and behavioral anomalies, we were not able to formally test them. For instance, we do not know if the parents in our sample indeed had mistaken beliefs about their children's absenteeism and/or if our intervention helped correct them. Second, we are not able to differentiate between the different behavioral hypothesis that could explain absenteeism. We anticipated a limited sample size, so we preferred to combine different messages designed to attack different potential biases instead of including various arms to test each hypothesis separately.

2.4. Participant recruitment and take-up

The experiment includes the 194 public schools in Uruguay that have only preschool classes. Using CEIP administrative data, we determined that 39,438 parents and children at those schools were registered in the *GURÍ* system. 19,272 parents (49%) with children at the 194 schools accessed the *GURÍ* mobile app at least once during the school year. We sent those parents a message informing them that their school was eligible to participate in a communication campaign to increase attendance and that they could choose whether or not to participate. The message included a consent form. A total of 6799 (17% of all parents registered in *GURÍ* and 35% of eligible parents) responded. Of the parents who responded, 4098 (10% of all parents and 21% of eligible parents) agreed to participate in the campaign. We randomly assigned 97 preschools to treatment and 97 to control groups. We randomized at the school level to preclude potential spillovers that could contaminate the control group because of the inter-dependency of observations at the classroom level. We stratified randomization by three variables: (i) **The number of absences at each school.** First, we calculated the median of absences for the 194 preschools. Then, we created a variable (high_abs) which takes a 1 if the school was above the median of absences and a 0 if it was not. (ii) **School district.** Each school is located in one of the 23 districts. We created a dummy for each district to indicate whether the school belongs to it. (iii) **Treatment status in a previous experiment.** A few months before ours, the government of Uruguay implemented another experiment designed to encourage families to install the GURI app through talks and campaigns delivered by teachers at the school level. Approximately 97 schools were treated in that experiment (and the rest were control). We created a dummy "participated_prev" that takes a 1 if a given school was in the treatment group of that experiment and a 0 if it was not. A "stratum" is therefore the interaction among these three variables: high_abs * district * participated_prev. Misfits were all placed in one specific stratum.

2.5. Treatment implementation

The intervention lasted 13 weeks, with a total of 63 school days. Table A.4 presents descriptive information on the messages delivered and read. We delivered a total of 43 messages to parents who enrolled before the intervention started. Some parents agreed to participate after the intervention had started and received fewer messages. The mobile app metadata shows whether parents read the messages. On average, parents read 70% of the messages sent. Fig. A.3 in the Appendix plots the distribution of the messages received by all parents and by parents who joined the intervention after treatment started.

2.6. Data and balance

We accessed information on student attendance using *GURÍ*. The system also has basic information on caregivers, such as their relationship to the child and their use of the mobile app. The *GURÍ* system registers students' absences. We counted the school days from early March to December (187 days) and subtracted the total days the student was absent to calculate attendance during the intervention. We also deployed a unique and rich database from the Child Development Inventory (INDI for its Spanish acronym) in Uruguay (Vásquez-Echeverría et al., 2021). This database contains child development outcomes, which we matched with our sample at the child level. INDI data covers the entire sample of four and five-year-old children in our original dataset. However, the tests were not administered to three-year-old children, who are thus missing from our INDI dataset (and not from our attendance dataset). The INDI was designed to assess school readiness, and it covers several domains of child development. Our dataset that was merged at the individual level resulted in a final sample of 2800 observations. INDI scores were standardized for each age level using a nationally representative sample of children attending classrooms for four- and five-year-olds. This sample was used as the norm-reference group (Vásquez-Echeverría, 2020). To present our results, we group the child development outcomes into two domains: the *cognitive domain (cognition, language, math, executive function, self-projection, cognitive total score)* and *general domain (motor, attitudes toward learning)*.

Table A.5 compares the characteristics of and outcomes for parents who have access to *GURÍ* with those who did not. It shows that students whose parents had access to the *GURÍ* mobile app have better overall outcomes. They attended school 8.8 days more per year on average and are 4.7 percentage points more likely to attend school. They were also 5.5 percentage points less likely to fall into chronic absenteeism. Students in the sample attended 145 of the 187 school days (77%). Chronic absenteeism is prevalent: 79% of students have an attendance rate of under 90%. Although this is not a problem for the internal validity of our estimates, it does limit the interpretation of the external validity of our results.

Tables 1 and A.6 (Appendix) compare the baseline characteristics and outcomes of students, parents, and schools in our sample. We ran two comparisons. The first compares treatment and control groups for parents who enrolled (or did not enroll) in the campaign. The second compares parents who were eligible (ineligible) to participate in the campaign. There are no statistically significant differences between the treatment and control groups in either subsample.

Table 1
Sample characteristics. Parents enrolled in the campaign.

	(1) Control	(2) Treatment	(3) Sample mean	(4) (1) vs. (2), p-value	(5) *N*
District	11.31	12.17	11.76	0.37	4098
	(0.71)	(0.66)	(0.48)		
School SES	3.36	3.26	3.31	0.69	4026
	(0.17)	(0.19)	(0.13)		
Age 3	0.29	0.28	0.29	0.70	4098
	(0.02)	(0.01)	(0.01)		
Age 4	0.38	0.38	0.38	0.83	4098
	(0.01)	(0.01)	(0.01)		
Age 5	0.33	0.34	0.34	0.52	4098
	(0.01)	(0.01)	(0.01)		
Father access GURI	0.39	0.38	0.39	0.85	4098
	(0.03)	(0.03)	(0.02)		
Both parents access GURI	0.38	0.37	0.37	0.88	4098
	(0.03)	(0.03)	(0.02)		
Student gender	0.50	0.49	0.49	0.35	4098
	(0.01)	(0.01)	(0.01)		
Average number of parents registered	253.25	236.12	244.33	0.35	4098
	(15.49)	(9.98)	(9.14)		
Take-up ratio (accepts/access)	0.23	0.25	0.24	0.12	4098
	(0.01)	(0.01)	(0.01)		
Pre-treatment answers	0.59	0.53	0.56	0.09	4098
	(0.02)	(0.02)	(0.02)		
Agreed before treatment began	0.59	0.53	0.56	0.08	4098
	(0.02)	(0.02)	(0.02)		
Baseline days in attendance	99.97	98.82	99.38	0.33	4098
	(0.85)	(0.80)	(0.59)		
Baseline attendance rate	0.81	0.80	0.80	0.33	4098
	(0.01)	(0.01)	(0.00)		
Baseline chronic absenteeism	0.74	0.75	0.75	0.67	4098
	(0.02)	(0.02)	(0.01)		
Previous treatment assignment	0.58	0.58	0.58	0.99	4098
	(0.06)	(0.06)	(0.04)		
N	1964	2134	4098		

Notes: Columns 1–3 present estimated averages for all subjects in the sample (treatment and control groups). Column 4 presents the two-sided p-value for a test of the hypothesis that the control and treatment group means are equal. Column 5 presents the number of observations for each indicator. Figures in parentheses are standard deviations.
*** Significant at 1% level ($p < 0.01$), ** significant at 5% level ($p < 0.05$), * significant at 10% level ($p < 0.1$).

3. Econometric model

We estimate the following equation using OLS:

$$Y_{ij} = \beta_0 + \beta_1 T_j + \beta_2 X_{ij} + \epsilon_{ij} \tag{1}$$

where Y measures the outcome of interest for student i in school j, T is a dummy variable that takes a value of 1 if the school is part of the treatment variable, and X is a vector for control variables. The estimated parameter β_1 captures the causal effect of the treatment on the outcomes of interest. We cluster standard errors at the school level and estimate the effects controlling for the individual outcome in baseline and stratum dummy variables. We run our model using two set of outcomes: attendance and child development. Increased attendance is supposed to affect child development because it provides longer exposure to learning opportunities. Missed days of school mean missed opportunities for problem solving, motor development, and specific language and math stimulation, all of which are important foundations of child development.

For the attendance outcome, we analyze days in attendance. For ease of interpretation, the main results also show the effect on attendance rate, defined as the number of school days attended divided by the total number of school days (providing a re-scaled version of days in attendance). For the child development outcome, we analyze the following standardized scores: cognitive domain (language, math, executive function, and self-projection, cognitive total score) and general domain (motor and attitudes toward learning).

4. Results

Table 2 displays the average effect of our intervention. We document null effects for each of our outcomes. These results should be interpreted as an intention-to-treat effect. The effect could have been higher among those who read the messages. We thus present a set of results instrumenting the opening of messages with random assignment to treatment to test whether more messages read by parents translated into more days of preschool attendance for their children. For this analysis, we created a binary variable to identify parents who read 24 or more messages, which is the average number of messages read in the treatment group. If a parent reads 24 messages or more, the variable takes a value of one; if he or she read fewer than 24 messages, the value is zero. As the assignment to treatment arms is the result of randomization, the exogeneity of the instrument is ensured. We document an average null effect (Table A.7).

As other research has found (Kalil et al., 2019), average results could mask interesting heterogeneous effects. For instance, as Mani et al. (2013) show, the type of cognitive biases our intervention attempts to attack tend to be more relevant for poorer individuals who are likely living in more stressful conditions and thus have less cognitive bandwidth. If this were the case, we would expect to find a more substantial effect among poorer individuals. On the other hand, our focus groups revealed that many structural factors, such as illness or lack of financial capacity to deal with unexpected events, explain absenteeism in our context. It is unlikely that a purely behavioral intervention would lift these barriers, which are typically more powerful for lower-income parents.

Given that we did not anticipate any heterogeneous analysis in a pre-analysis plan, we limit our discretion to select the dimensions for which heterogeneity matters by using causal forest estimators (Athey et al., 2019). More specifically, we follow the *Honest* approach developed by Athey et al. (2019) to estimate conditional average treatment effects (CATE) for each individual in our sample using a generalized random forest (we use the *grf* R package).

Table 2
Treatment effect of the campaign (OLS).

Panel A: Attendance

	Days in attendance		Attendance rate	
	(1)	(2)	(1)	(2)
Treatment	−0.12	0.33	−0.00	0.01
	(0.75)	(0.39)	(0.01)	(0.01)
Observations	4098	4098	4098	4098
Controls	No	Yes	No	Yes
Mean control	50.64	50.64	0.80	0.80
SD control	10.26	10.26	0.16	0.16

Panel B: Cognitive domain

	Language		Cognition		Math		Executive function		Self-projection	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Treatment	0.09	0.06	0.02	0.02	−0.03	−0.04	−0.02	−0.04	−0.00	0.01
	(0.08)	(0.05)	(0.07)	(0.05)	(0.06)	(0.04)	(0.07)	(0.04)	(0.06)	(0.04)
Observations	2807	2740	2780	2683	2806	2713	2827	2788	2817	2769
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean control	0.38	0.38	0.48	0.48	0.57	0.57	0.36	0.36	0.42	0.42
SD control	1.05	1.05	0.95	0.95	1.01	1.01	0.95	0.95	0.87	0.87

Panel C: General domain

	Motor		Attitudes toward learning	
	(1)	(2)	(1)	(2)
Treatment	−0.07	−0.02	−0.05	−0.07
	(0.05)	(0.04)	(0.06)	(0.05)
Observations	2813	2731	2838	2801
Controls	No	Yes	No	Yes
Mean control	0.38	0.38	0.39	0.39
SD control	0.83	0.83	0.88	0.88

Notes: This table presents the estimated treatment effect for students in the sample for different outcomes. Column 1 shows estimates without controls and Column 2 includes the following controls: stratum fixed effects and the baseline value for the outcome. Figures in parentheses are robust standard errors clustered at the school level.
*** Significant at 1% level ($p < 0.01$), ** significant at 5% level ($p < 0.05$), * significant at 10% level ($p < 0.1$).

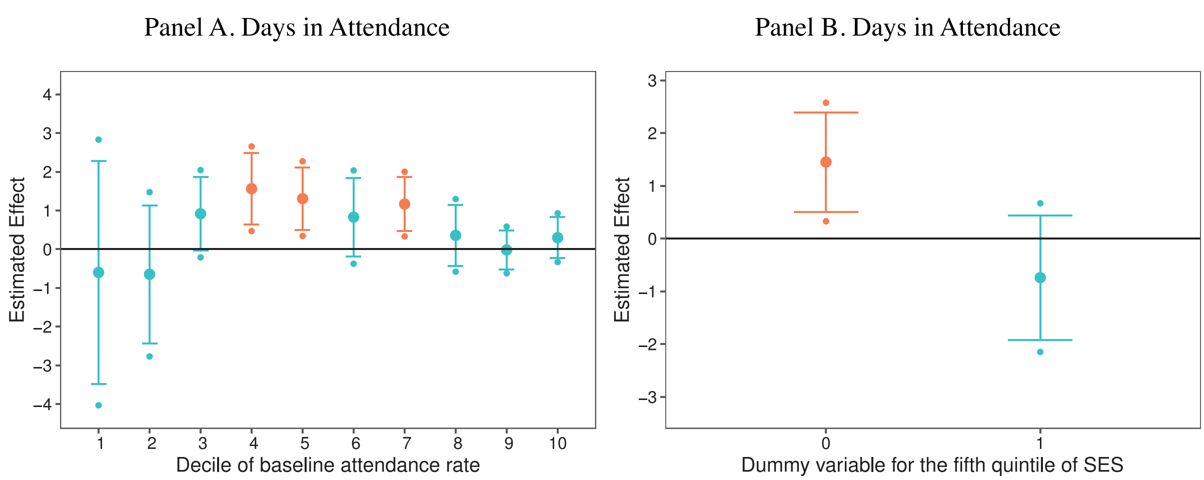

Fig. 1. Heterogeneous treatment effects on attendance. Notes: Each figure shows the conditional average treatment effects (CATE) for different levels of a specific covariate while keeping the other covariates fixed at their median values. In panel A, we evaluate the baseline attendance rate across the means of each decile. In panel B, we evaluate the dummy variable for the fifth SES quintile, while keeping the baseline attendance rate at its median value. Each point represents the estimated CATE. The bars show 90% confidence intervals, while the small dots represent 95% confidence intervals. Coral color indicates that a CATE is significant after adjusting for multiple comparisons. For those adjustments, we use the Benjamini–Hochberg procedure (Benjamini and Hochberg, 1995), setting a false discovery rate of 10%. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Causal forest algorithms are adaptations of random forests and, more specifically, regression trees (Breiman et al., 2017), which are classification methods widely used in machine learning. Regression trees are recursive partitioning algorithms that split a sample in order to maximize heterogeneity across splits. Simply put, a forest is a group of trees, and each tree is grown from a portion of the data drawn randomly from the full sample.

We proceed as follows. For each tree, we draw a random subsample, without replacement, from the full sample of our experiment. Each node is recursively split into child nodes until an entire tree has been grown. The splits are determined by the algorithm in order to maximize heterogeneity in terms of the average treatment effect in each subgroup. When a new node does not improve fit, that node is not split and thus forms a final leaf. To avoid over-fitting, Athey and Imbens (2016), recommend the honest approach in which each randomly selected subsample is split in two halves: one used to grow each tree and the other used to estimate the average treatment effect (ATE) within each leaf. The honest estimation helps reduce an overestimation of the goodness of fit of the models. However, this approach is costly in terms of statistical power, so our results should be interpreted cautiously, especially in the case of child development outcomes, which have a significantly smaller sample size.

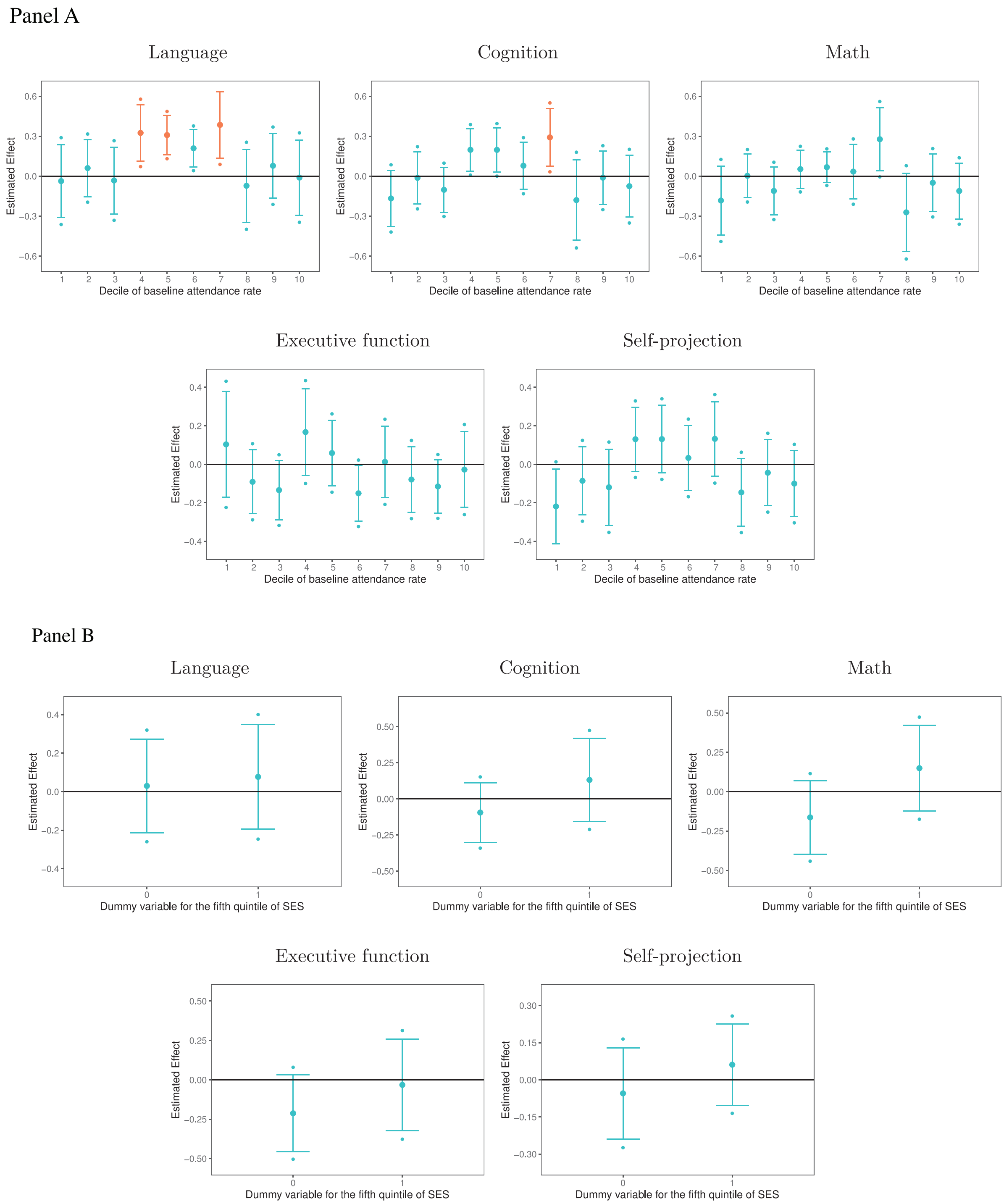

Fig. 2. Heterogeneous treatment effects on cognitive domain. Notes: Each figure shows the conditional average treatment effects (CATE) for different levels of a specific covariate while keeping the other covariates at fixed at their median values. In panel A, we evaluate the baseline attendance rate across the means of each decile. In panel B, we evaluate the dummy variable for the fifth SES quintile, while keeping the baseline attendance rate at its median value. Each point represents the estimated CATE. The bars show 90% confidence intervals, while the small dots represent 95% confidence intervals. Coral color indicates that a CATE is significant after adjusting for multiple comparisons. For those adjustments, we use the Benjamini–Hochberg procedure (Benjamini and Hochberg, 1995), setting a false discovery rate of 10%. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

To make our analysis more precise, we follow the implementation method of Athey and Wager (2019), which is inspired by Basu et al. (2018). We first train a random forest on all available covariates for the attendance outcome.[7] This part of the process is called the pilot stage. We then train final forests for each outcome, only including the covariates that had a reasonable number of splits (above the expected mean, considering all the trees in the forest) in the pilot stage.

Fig. A.4 (Appendix) shows the distribution of predicted treatment effects using the main outcome (days in attendance). For 25% of individuals, the effect is positive and significant at the 10% level. The algorithm identified two variables that appear in a reasonable number of splits: pre-treatment attendance rate and a higher-income school

[7] We only exclude district dummies, to avoid having groups with too few observations.

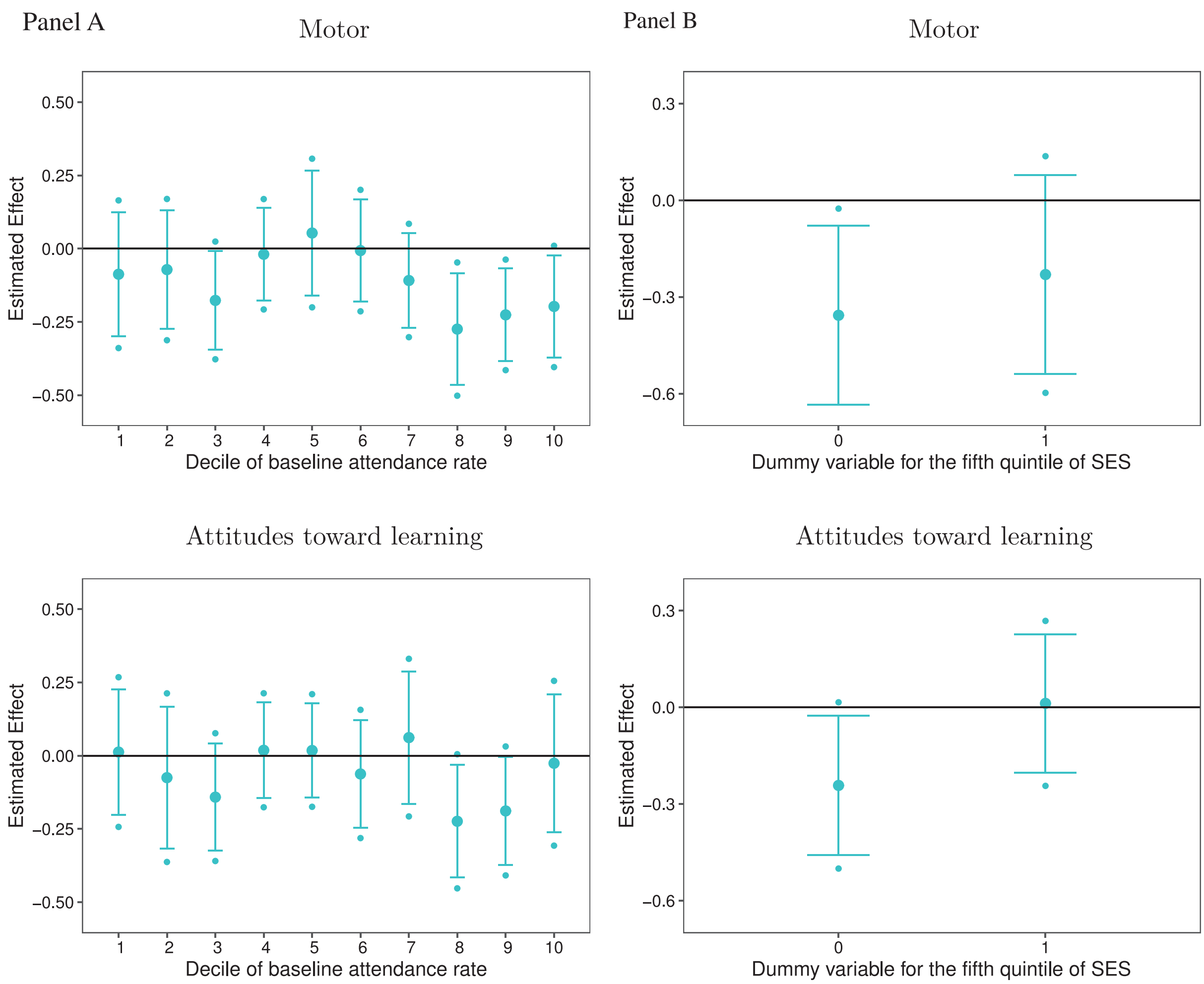

Fig. 3. Heterogeneous treatment effects on general domain. Notes: Each figure shows the conditional average treatment effects (CATE) for different levels of a specific covariate while keeping the other covariates at fixed at their median values. In panel A, we evaluate the baseline attendance rate across the means of each decile. In panel B, we evaluate the dummy variable for the fifth SES quintile, while keeping the baseline attendance rate at its median value. Each point represents the estimated CATE. The bars show 90% confidence intervals, while the small dots represent 95% confidence intervals. Coral color indicates that a CATE is significant after adjusting for multiple comparisons. For those adjustments, we use the Benjamini–Hochberg procedure (Benjamini and Hochberg, 1995), setting a false discovery rate of 10%. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

dummy that takes a one if the school is in the fifth quintile of an SES indicator. Importantly, we do not have access to the continuous SES measure used to build this dummy as we do for pre-treatment attendance. In the original (pilot) forest, these two covariates drove 53% and 11.5% of the sample splits, respectively, well above the lower-ranking variables in this dimension (for instance, school grade drove 6% of the splits and student male dummy accounted for 3% of the splits).

Figs. 1 (attendance), 2 (cognitive domain) and 3 (general domain) show how the treatment effects vary across the two covariates identified by the algorithm. The figures show the predicted CATE for each individual, when all other covariates are held constant (at their median values) and one of the specific dimensions is changed. For ease of interpretation, we show the heterogeneous effects by pre-treatment attendance rates in deciles. In each case, we indicate whether a point estimate is significant when adjusting for multiple comparisons. To that end, we use the Benjamini–Hochberg procedure (Benjamini and Hochberg, 1995), setting a false discovery rate of 10%. Each decile represents a family of outcomes that includes all outcomes in this study (for attendance and child development, so eight outcomes per family).

An interesting pattern emerges. As Fig. 1 (panel A) shows, the effect on attendance seems to form an inverted U-shape across the pre-attendance covariate, with non-significant effects for very low and high pre-treatment attendance rates, and the largest effects for deciles 3 to 7. After adjusting for multiple comparisons, the effects remain significant at 5% only in deciles 4, 5 and 7. The significant effect in deciles 4, 5 and 7 of pre-treatment attendance rate is approximately 1.4 days. During the intervention, there were 63 days on which children could attend school, and the number of days they attended in the comparable deciles of the control group was 50.4. This represents an increase of 2.8% in the number of days in attendance. When expressed in terms of attendance rates, the effects are approximately 2 percentage points. The attendance rate in the control group was 0.8.

Fig. 1 (panel B) shows a similar pattern for the higher income dummy. The effect seems to be concentrated among the less-than-higher-income schools (those not in the richest quintile in terms of SES): after adjusting for multiple comparisons, we still identify an effect in this group. Given that schools with relatively low SES tend to have lower pre-treatment attendance rate, the effects across the two variables seem to be consistent. However, since SES is calculated at the school level, pre-treatment attendance could be a better proxy for individual-level poverty.

Figs. 2 and 3 (Panel A) show the results for child development outcomes, separating cognitive and general domains. We identify a similar pattern in terms of the pre-treatment distribution of attendance to those we identified when using attendance as the outcome, but only in language and, to a lesser extent, cognition. For language, we identify effects of between 0.21 and 0.38 standard deviations (significant after adjusting for multiple comparisons) for deciles 4, 5 and 7 of pre-treatment attendance. For cognition, we identify effects of between 0.2 and 0.29 standard deviations in the same deciles (4, 5 and 7). However, after adjusting for multiple comparisons, only decile 7 remains significant. We do not identify any significant effect on the other child development outcomes. A possible explanation for why cognition and language were the only child development outcomes affected by our intervention is that those domains are typically the most affected by socioeconomic gradients (Paxson and Schady, 2007). Unlike the figures for attendance, Figs. 2 and 3 (panel B) do not show any interesting pattern based on schools' SES.

Although suggestive and exploratory, we present some plausible interpretations with potentially interesting insights. One plausible interpretation is that the intervention was not powerful enough to change the behavior of the families with the most severe and structural barriers. Those with very low pre-treatment attendance rates (a proxy of child-level poverty) may need more intensive and expensive interventions. On the other hand, it is plausible that psychological barriers were not significant for individuals with relatively high pre-treatment attendance rates. There are two possible reasons for this. The first is mechanical: increasing a rate that was already high is more complicated. Second, if pre-treatment attendance is a reasonably good proxy for income, these families were relatively higher-income families. We expect them to be less sensitive to our interventions as they could be less affected by cognitive biases (Mani et al., 2013).

Although the average effects of our intervention are null, the magnitude of the effects of the exploratory analysis of heterogeneous effects suggests that close to the median of the pre-treatment distribution of attendance (where our treatment reaches its maximum effect), the effects were comparable to those found in similar studies in developed countries. For instance, Kalil et al. (2019) identify an effect on attendance rate of 0.04 and 0.023 in quantiles 25 and 50 (where their treatment reaches its maximum effect). We identify an effect of approximately 0.2 on the deciles close to the median of the pre-treatment distribution of attendance. Although our effect is smaller, our intervention was also shorter: 13 weeks versus 18 weeks in the other study. Similarly, the largest treatment effect identified by Robinson et al. (2018) (in decile 10 of pre-treatment absenteeism) is one day, which in their context means approximately 14.5% fewer missed days. Where our treatment reaches its maximum effect, we identify an effect of approximately 1.8 days which, in our context, represents a drop in missed days of approximately 14.4%.

5. Discussion

No matter how big of an effort governments make to expand access to preschool services, it is ultimately up to families to decide whether to enroll their children in centers and take them there on a regular basis, since preschool education is often not compulsory (Mateo-Diaz and Rodriguez-Chamussy, 2016). Structural issues—such as lack of transportation or difficulty aligning work and preschool schedules—account for some of preschool children's absences. But cognitive biases also affect parents' decisions to allow their children to miss days. The good news is that cognitive biases could potentially be modified using very low-cost text-message interventions that have proven effective (Ajzenman and López Bóo, 2019). To address cognitive barriers, we proposed a treatment to test hypotheses we built using information we gathered in focus groups with parents of preschool children.

Our intervention represents one of the first attempts to use behavioral science to address low preschool attendance in a developing country using an existing government mobile application as the channel of communication between preschool centers and families in Uruguay, instead of text-messages, which are typically more expensive.

We document an average null effect on attendance and child development. Exploratory analysis suggests positive effects on people close to the median of the distribution of attendance. One possible interpretation is that families at the low end of the distribution may also have cognitive biases, but they struggle more with structural barriers (such as lack of transportation). Children with the highest rates of attendance probably come from families with less severe cognitive barriers. Our treatment was not effective in either of these segments.

These findings, although suggestive, could be useful in tailoring future interventions and targeting public resources better. Future research could focus on understanding which pre-treatment characteristics of children and parents could be important to maximize the effectiveness of this type of intervention. Understanding which type of families are particularly sensitive to these treatments is crucial to maximizing their cost-effectiveness.

CRediT authorship contribution statement

Nicolas Ajzenman: Conceptualization, Methodology, Formal analysis, Writing – original draft, Writing – review & editing, Supervision. **Laura Becerra Luna:** Project administration, Writing – original draft, Data curation, Resources. **Juan Manuel Hernández-Agramonte:** Methodology, Investigation, Project administration. **Florencia Lopez Boo:** Conceptualization, Methodology, Writing – original draft, Supervision. **Marcelo Perez Alfaro:** Project administration. **Alejandro Vásquez-Echeverría:** Data curation. **Mercedes Mateo Diaz:** Conceptualization, Methodology, Writing – original draft, Supervision, Funding acquisition.

Data availability

The authors do not have permission to share data.

Funding

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Appendix

See Figs. A.1–A.4.
See Tables A.1–A.7.

Table A.1
Number of messages sent, by type of message.

Type of message	Number of messages
Welcome message	1
Feedback (false beliefs)	5
Importance of preschool and short-term effects of absence (present bias)	13
Importance of preschool and long-term effects of absence (present bias)	8
Positive parental identity (mismatched identity)	5
Planning prompts (limited attention)	10
Goodbye message	1
Total	43

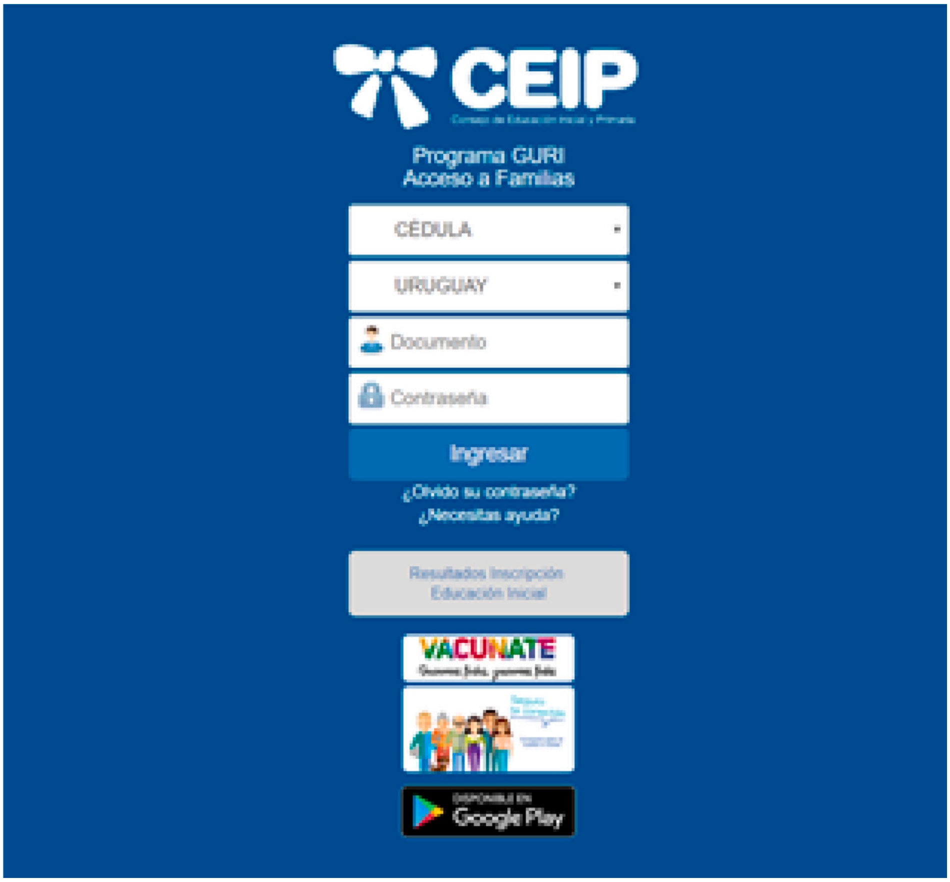

Fig. A.1. GURÍ app screenshot. Note: Example of GURÍ app used in the experiment.

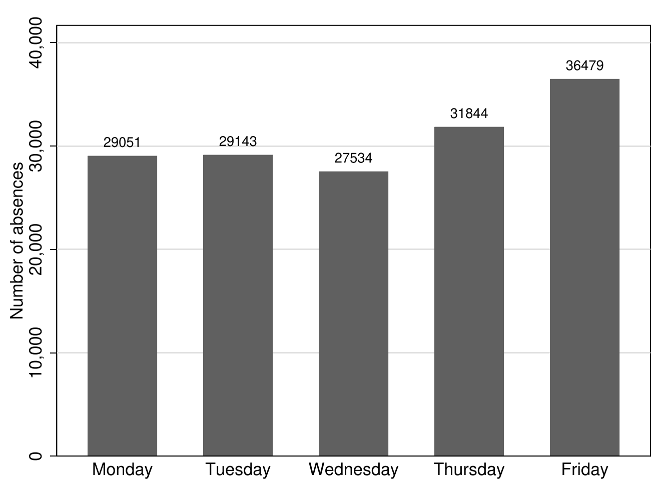

Fig. A.2. Distribution of absences by day of the week, March 4–May 17, 2019.

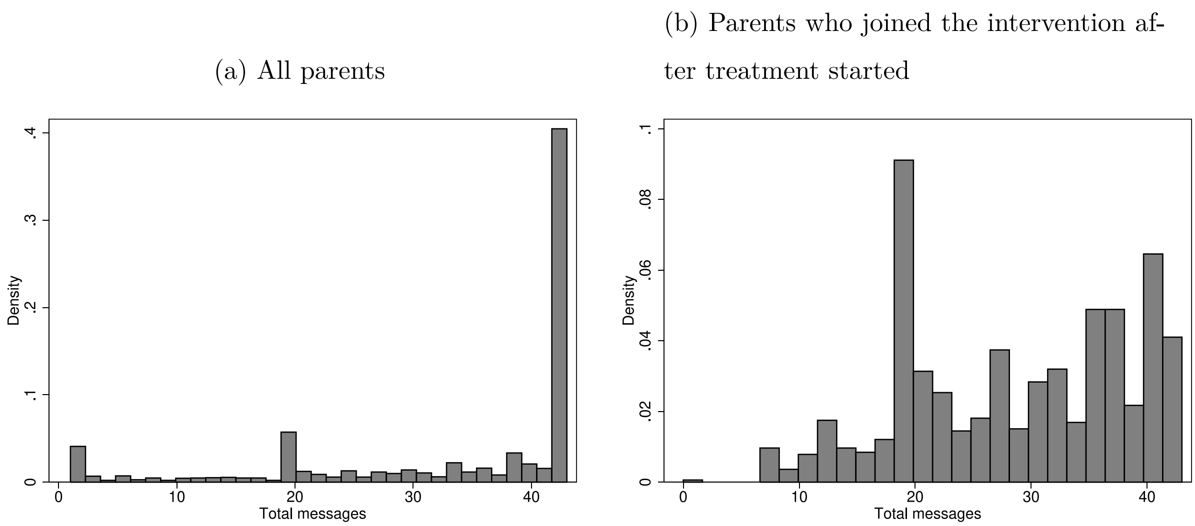

Fig. A.3. Distribution of number of text messages sent.

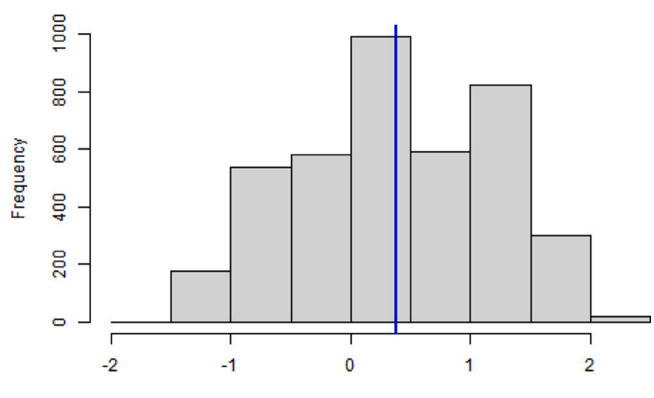

Fig. A.4. Distribution of predicted treatment effects. Notes: This figure displays the histogram of treatment effects for days in attendance. Conditional average treatment effect (CATE) predictions are trained and validated using the causal forest methods described in Section 4. The blue vertical line shows the mean CATE. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Table A.2
Topics covered in focus groups.

1. Knowledge of early childhood education and its importance
a. What do children learn in preschool?
b. Is it different from primary school?
c. What is the most important thing a child aged 3–5 should learn?
d. How important is preschool to your child's (early) education?
e. Who do you think is best able to teach your child what he or she should learn at this age?
f. How important is it to you that your child spend time interacting with other children his or her age in preschool? Why?
2. Perception of absences
a. Does your child frequently miss school?
b. If we ask you how many days per month on average your children miss preschool, would you be sure of the answer? Hint: Make sure to capture the reasons (why parents say no and why they say yes).
c. How many times a month does your child arrive at preschool late or leave early?
d. What are the most frequent reasons why your child misses preschool? What are the most frequent reasons why your child is late to preschool or leaves early?
3. Consequences for child's development of regularly missing preschool
a. What do you think are the consequences for your child's development of regularly missing preschool, if any?
b. Would you say that regular preschool attendance is lessor more important than attendance at primary school, or of equal importance?
c. What do you think are the long-term consequences for a person's school and adult years, if any, of regularly missing preschool?
d. What are the long-term consequences of someone being late for preschool? Hint: We refer to the impact on learning, mood, socio-emotional development, integration into the classroom, etc.
4. Parents' ability to influence the fate of their children (locus of control)
a. Do you believe that the decisions you make as a parent affect your child's future opportunities, or are these opportunities already fixed by their context?
b. Can you change your child's intelligence?
c. Can you change your child's personality?
5. Effect of social norms on early childhood education
a. In your social circle, how important is education?
b. In your social circle, how important is preschool education?
6. Quality of the educational center
a. What criteria did you use to choose the center where your child is enrolled?
b. Would you be interested in being able to evaluate the center and provide information in order to improve its quality?
c. Would you be willing to collaborate with such an initiative?

Table A.3
Behavioral biases addressed by the intervention.

Behavioral bias	Description	Type of message	Example
False beliefs	Parents underestimate how often their children are absent.	Feedback	[Parent name]: [Child's name] missed [number] days of preschool in the last three weeks. Daily attendance is important. Don't let [him/her] be missed!
Present bias	Most people tend to invest less than optimally in a specific activity when the reward for engaging in the activity is received only in the future. Parents can fail to internalize the future benefits of their investments and consequently make short-sighted decisions about investing in their children.	Short-term gains	[Parent name]: Did you love it when [child's name] showed you how [she/he] could tie their shoes by [him/herself?] [She/he] learns that and more every day in preschool. Don't stop taking [him/her] there!
		Long-term gains	[Parent name]: Did you know that if [child's name] attends preschool every day, [she/he] forms lasting habits that build a foundation for success in later grades? Don't let [him/her] be missed!
Mismatched identity	Parents do not believe that they can change their child's attendance through their own efforts. Parents are not receptive to the intervention's goals.	Positive parental identity	[Parent name]: What you do for [child's name] today—for example, taking [her/him] to preschool so [she/he] doesn't miss out—will affect [her/his] future. You play a key role in your child's education!
Limited attention	Parents forget to make decisions they intended to make and fail to take actions they planned to take. Day-to-day tasks may distract parents from more distant goals and cause them to pay limited attention to beneficial parenting practices.	Planning prompts	[Parent name]: Organize your time so that [child's name] can go to preschool every day. There are new lessons this week. Take [her/him]!

Table A.4
Summary statistics for messages sent and read.

Item	Mean	Standard deviation	Median	Minimum	Maximum
Number of messages sent	34	13	42	1	43
Number of messages read	24	15	24	0	43
Percent of messages read	70	40	80	0	100
Observations	2165				

Table A.5
Sample characteristics. Comparison between non-eligible and eligible parents.

	(1) No access GURI	(2) Access GURI	(3) Sample mean	(4) (1) vs. (2), p-value	(5) Observations
School SES	3.33	3.44	3.38	0.19	38 435
	(0.12)	(0.12)	(0.11)		
Age 3	0.30	0.29	0.30	0.12	39 438
	(0.01)	(0.01)	(0.01)		
Age 4	0.37	0.38	0.38	0.84	39 438
	(0.01)	(0.00)	(0.00)		
Age 5	0.32	0.34	0.33	0.10	39 438
	(0.01)	(0.01)	(0.01)		
Father access GURI	0.34	0.39	0.36	0.01	37 364
	(0.02)	(0.02)	(0.02)		
Both parents access	0.30	0.37	0.34	0.00	37 364
	(0.02)	(0.02)	(0.02)		
Student gender	0.49	0.50	0.49	0.48	37 364
	(0.00)	(0.00)	(0.00)		
Average number of parents registered	240.29	239.94	240.12	0.94	39 438
	(7.55)	(7.82)	(7.27)		
Days in attendance	140.67	149.50	144.99	0.00	39 438
	(1.11)	(0.81)	(0.92)		
Attendance rate	0.75	0.80	0.78	0.00	39 438
	(0.01)	(0.00)	(0.00)		
Chronic absenteeism	0.81	0.76	0.79	0.00	39 438
	(0.01)	(0.01)	(0.01)		
Baseline days in attendance	94.11	99.39	96.69	0.00	39 438
	(0.67)	(0.51)	(0.56)		
Baseline attendance rate	0.76	0.80	0.78	0.00	39 438
	(0.01)	(0.00)	(0.00)		
Baseline chronic absenteeism	0.80	0.74	0.77	0.00	39 438
	(0.01)	(0.01)	(0.01)		
N	20 166	19 272	39 438		

Notes: Columns 1–3 present estimated averages for all subjects in the sample (treatment and control groups). Column 4 presents the two-sided p-value for a test of the hypothesis that the control and treatment group means are equal. Column 5 presents the number of observations for each indicator. Figures in parentheses are standard deviations.
*** Significant at the 1% level ($p < 0.01$), ** significant at the 5% level ($p < 0.05$), * significant at the 10% level ($p < 0.1$).

Table A.6
Sample characteristics. Eligible parents.

	(1) Control	(2) Treatment	(3) Sample mean	(4) (1) vs. (2), p-value	(5) *N*
District	10.98	12.02	11.51	0.28	19 272
	(0.72)	(0.65)	(0.48)		
School SES	3.44	3.43	3.44	0.96	18 887
	(0.17)	(0.17)	(0.12)		
Age 3	0.29	0.29	0.29	0.80	19 272
	(0.01)	(0.01)	(0.01)		
Age 4	0.38	0.37	0.38	0.67	19 272
	(0.01)	(0.01)	(0.00)		
Age 5	0.33	0.34	0.34	0.55	19 272
	(0.01)	(0.01)	(0.01)		
Father accesses GURI	0.39	0.39	0.39	0.98	19 272
	(0.03)	(0.03)	(0.02)		
Both parents access GURI	0.37	0.37	0.37	0.99	19 272
	(0.03)	(0.03)	(0.02)		
Student gender	0.50	0.49	0.50	0.73	19 272
	(0.01)	(0.00)	(0.00)		
Average number of parents registered	249.00	231.15	239.94	0.25	19 272
	(12.77)	(8.86)	(7.82)		
Take-up ratio (accepts/access)	0.21	0.22	0.21	0.36	19 272
	(0.01)	(0.01)	(0.01)		
Pre-treatment access to app	0.29	0.28	0.29	0.87	19 272
	(0.02)	(0.02)	(0.01)		
Answers consent	0.34	0.36	0.35	0.33	19 272
	(0.01)	(0.01)	(0.01)		
Pre-treatment answers	0.20	0.20	0.20	0.57	19 272
	(0.01)	(0.01)	(0.01)		
Agreed to participate in campaign	0.21	0.22	0.21	0.36	19 272
	(0.01)	(0.01)	(0.01)		
Agreed before treatment began	0.12	0.12	0.12	0.48	19 272
	(0.01)	(0.01)	(0.00)		
Baseline days in attendance	99.87	98.93	99.39	0.36	19 272
	(0.73)	(0.72)	(0.51)		
Baseline attendance rate	0.81	0.80	0.80	0.36	19 272
	(0.01)	(0.01)	(0.00)		
Baseline chronic absenteeism	0.74	0.75	0.74	0.61	19 272
	(0.02)	(0.02)	(0.01)		
Previous treatment assignment	0.59	0.59	0.59	0.98	19 272
	(0.06)	(0.06)	(0.04)		
N	9490	9782	19 272		

Notes: Columns 1–3 present estimated averages for all subjects in the sample (treatment and control groups). Column 4 presents the two-sided p-value for a test of the hypothesis that the control and treatment group means are equal. Column 5 presents the number of observations for each indicator. Figures in parentheses are standard deviations.
*** Significant at the 1% level ($p < 0.01$), ** significant at the 5% level ($p < 0.05$), * significant at the 10% level ($p < 0.1$).

Table A.7
Treatment effect of the campaign (IV).

Panel A: Attendance

	Days in attendance		Attendance rate	
	(1)	(2)	(1)	(2)
Read 24 or more messages	−0.20	0.57	−0.00	0.01
	(1.30)	(0.68)	(0.02)	(0.01)
Observations	4098	4098	4098	4098
Controls	No	Yes	No	Yes
Mean control	50.64	50.64	0.80	0.80
SD control	10.26	10.26	0.16	0.16
F-test	1150.36	1171.16	1150.36	1171.16

Panel B: Cognitive domain

	Language		Cognition		Math		Executive function		Self-projection	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Read 24 or more messages	0.16	0.12	0.04	0.03	−0.06	−0.08	−0.04	−0.07	−0.01	0.03
	(0.14)	(0.10)	(0.12)	(0.09)	(0.12)	(0.08)	(0.12)	(0.08)	(0.11)	(0.08)
Observations	2807	2740	2780	2683	2806	2713	2827	2788	2817	2769
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean control	0.38	0.38	0.48	0.48	0.57	0.57	0.36	0.36	0.42	0.42
SD control	1.05	1.05	0.95	0.95	1.01	1.01	0.95	0.95	0.87	0.87
F-test	890.97	845.41	896.79	867.05	867.05	802.56	907.41	835.23	922.28	839.41

Panel C: General domain

	Motor		Attitudes toward learning	
	(1)	(2)	(1)	(2)
Read 24 or more messages	−0.12	−0.04	−0.10	−0.12
	(0.10)	(0.08)	(0.11)	(0.08)
Observations	2813	2731	2838	2801
Controls	No	Yes	No	Yes
Mean control	0.38	0.38	0.39	0.39
SD control	0.83	0.83	0.88	0.88
F-test	915.27	872.91	917.25	837.47

Notes: This table presents the estimated treatment effect for students in the sample for different outcomes. Column 1 shows estimates without controls and Column 2 includes the following controls: stratum fixed effects and the value of the outcome in baseline. Figures in parentheses are robust standard errors clustered at the school level.
*** Significant at the 1% level ($p < 0.01$), ** significant at the 5% level ($p < 0.05$), * significant at the 10% level ($p < 0.1$).

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