What Mom and Dad Don't Know, Might Hurt Me: Messaging Parents to Reduce Chronic Absenteeism

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Abstract

In high resource contexts with low child absenteeism, interventions that reduce information frictions between educators and parents tend to increase children's school attendance. Can these interventions reduce chronic absenteeism in resource constrained and high-absenteeism environments, and specifically using existing data collection systems? We test this in a randomized controlled trial in South Africa. We sent low-income parents of children attending an afterschool program weekly text-messages with information about their children's previous attendance. The intervention increased children's attendance by 14% (7 percentage points) and reduced chronic absenteeism by 24% (7 percentage points). Survey evidence shows that parents in the treatment conditions had more accurate beliefs about their children's attendance, talked to their children about the afterschool program more often, and were generally more engaged with their children's afterschool activity. However, an additional treatment arm focusing parents' attention on the future benefits of education did not produce additional effects on attendance or engagement. The intervention was highly cost-effective, utilized data collection systems "as is", and treatment effects on attendance did not decay over the intervention period. Taken together, this intervention shows potential for scaling in lower-resource contexts to reduce information frictions between educators and parents.

Keywords: Field experiment; information frictions; student absenteeism; parental engagement; messaging intervention. JEL Codes: C93; D82; J18; J08; O15; I25

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1 Introduction

Parents are critical for children's educational success. An extensive literature demonstrates the importance of parental engagement[1] in facilitating children's learning and educational outcomes (Carneiro and Heckman, 2003; Cunha and Heckman, 2007; Todd and Wolpin, 2007; Cheung and Pomerantz, 2012; Desforges and Abouchaar, 2003; Jeynes, 2012; Damgaard and Nielsen, 2018). A growing literature specifically documents a strong causal relationship between parental engagement and crucial education investments (such as school attendance) and outcomes (Avvisati et al., 2014; Bergman and Chan, 2021; Bettinger et al., 2021). While most models of children's human capital development take it as given that parents fully control their education investment decisions, children often have different preferences to their parents regarding education (Read and Read, 2004; Bettinger and Slonim, 2007). In these cases, parents' capacity to monitor investments, such as school effort, become important. This is particularly true as children get older and develop greater agency (Heckman and Mosso, 2014). Given this, parents can benefit from timely and accurate information about their children's actual effort.

In reality, parents face strong information frictions regarding their children's effort at school and in learning. Bergman (2021), Bettinger et al. (2021), and others (Rogers and Feller, 2018; Berlinski et al., 2021; Bergman and Chan, 2021) show that parents have upwardly biased beliefs about their children's school attendance, assignment submission, and grades. In many cases, parents technically have access to the correct information[2], but pronounced and consistent belief biases persist. This suggests that parents face other, possibly behavioral and attentional, constraints to accessing and utilizing this information to form accurate beliefs. Dizon-Ross (2019) shows that one out of three parents in her study does not know which of their two children perform better at school, despite receiving regular report cards. It also seems that there is strong demand for this information. Bursztyn and Coffman (2012) and Berlinski et al. (2021) find that the majority of parents in Brazil and Chile respectively are willing to pay relatively large proportions of their monthly income for their children's school attendance information.

[1]Parental engagement includes a broad range of behaviors, such as encouraging children's investments verbally and with incentives, helping with homework, talking to children about school, and helping facilitate children's more accurate beliefs and favorable preferences about education.

[2]Through report cards, or technically open communication channels with teachers

Given this backdrop, interventions designed to reduce information frictions between parents and educators and improve parents' belief accuracy can have strong positive effects on children's effort and optimal education investments. Dizon-Ross (2019) shows that when surveyors helped parents (in a low literacy context) read through their children's school report cards, parents adjusted their upwardly biased beliefs about poor performing children's performance downward, reallocated resources towards their better performing children, and invested more appropriately in educational materials. Text messaging interventions, specifically, have shown a great deal of promise at providing high-frequency education-related information to parents. Bergman (2021) provides parents with detailed bi-weekly information on their children's grades, absences, and assignment submissions, and finds increased belief accuracy about their effort and improved school performance, with GPA increases of approximately 0.2 standard deviations. Several other studies demonstrate similar positive effects of simple messages on parents' belief accuracy and engagement, and increased school attendance, predominantly in developed countries (Rogers and Feller, 2018; Bergman and Chan, 2021; Bergman, 2021, 2019), but also in developing countries (Bettinger et al., 2021; Berlinski et al., 2021; Lichand and Wolf, 2021; Angrist et al., 2022).

However, in spite of the success of these messages, they have predominantly been tested in contexts where attendance rates are relatively high: in all of the above studies, baseline attendance rates were greater than 85%, and in all but two cases they were greater than 90%. Moreover, most of this work documents increases in attendance at the intensive margin for children "in the system", rather than reducing dropouts or chronic absenteeism[3]. In settings where low attendance or non-attendance is the norm, barriers to attending school may be different, or even structural, suggesting that messaging interventions might be differently effective. Similarly, children who seldom or never attend school might be differently sensitive to messaging interventions than those who attend most days and are deciding whether or not to attend on a given day. It is therefore unclear whether light-touch information interventions can work in contexts where chronic absenteeism is the norm, and to solve the problem of non-attendance rather than increasing attendance at the margins. Our paper provides the first test to our knowledge of the effect of simple text messages on absenteeism in a setting where attendance rates are low (~50% of days) and chronic absenteeism is high (~30%

[3] Some work tests the effect of messages on dropouts (Kraft and Rogers, 2015; Rogers and Feller, 2018), but this is a smaller subset of the research in this area, and none test the effect in a context where dropouts are common.

of enrolled students)[4].

The interventions tested in the literature also typically require educators to undertake additional activities, such as writing tailored messages to parents (Kraft and Rogers, 2015; Kraft and Dougherty, 2013), or compiling additional data outside of their regular reporting (Bettinger et al., 2021; Bergman, 2021). In cases where interventions tested were explicitly "light-touch'" and leveraged existing data collection systems (Berlinski et al., 2021; Bergman and Chan, 2021), these systems were relatively sophisticated and included data on specific classes missed, assignment submission details, and grades of specific outputs. There is therefore little evidence to show whether simple messages that rely on basic data collection systems and low technology can work. As such, it is unclear how effective these interventions are in low resource contexts. We thus also provide the first test to our knowledge of text messages using low-cost data collection systems "as is" in a relatively low resource setting (in low-income neighborhoods in South Africa).

We tested the effect of weekly text-messages on parents' engagement behaviors, beliefs, and children's attendance at an afterschool program in Cape Town, South Africa. Our sample consisted of 1037 children across 18 schools in low-income neighborhoods within Cape Town. Following the literature, our intervention was designed to increase parental engagement through two channels: i) reducing information frictions between educators and parents, and thereby improving parents' belief accuracy[5]; ii) increasing the salience of (and therefore directing parents' attention towards) key education investment decisions[6]. We sent treatment messages to parents on Monday evenings between July and September, 2016: the messages included reminders to encourage their children to attend the program and information about their child's attendance in the previous week. Children were randomly assigned to either a control group or to one of two treatment conditions. Both treatment conditions received the encouragement and attendance information; one of the treatment conditions received an additional sentence highlighting a documented future benefit of attending af-

[4]Baseline attendance is 51% in our sample, and at endline, 29% of the control group attended zero sessions.

[5]Bergman (2021) derives a model that shows that parents' increased belief accuracy after receiving similar messages is a key driver of increased investment optimization, and Dizon-Ross (2019) shows that improved belief accuracy directly causes more targeted and effective education investments.

[6]Bettinger et al. (2021) find that a salience treatment, which matches messages providing information on children's effort but excludes child-specific information, has the same effect as messages with child-specific information on attendance, but does not change beliefs. Similarly, Lichand and Wolf (2021), find that in Cote d'Ivoire, messages sent to parents that do not contain any child-specific information lead to improvements in children's school performance. This also aligns with literature documenting the importance of simply directing attention to key investment decisions (Karlan et al., 2016), including in education (Castleman and Page, 2015, 2016)

terschool programs. There is evidence that parents in low-income settings have downwardly-biased beliefs about the returns to education, and that correcting these beliefs can generate increases in education investments (Jensen, 2010; Cunha et al., 2013). There is also evidence that these returns might be very high in South Africa (Nikolov et al., 2020; Patrinos and Psacharopoulos, 2020; Keswell and Poswell, 2004). Given that most South African parents from previously disadvantaged backgrounds (like all parents in our study) were formally excluded from quality education and labor market opportunities during the Apartheid regime, they might face specific limitations to accessing information on the benefits to education. We therefore included this second treatment arm to draw attention to information on concrete benefits of investing in education. Treatment messages were sent each week over a total of 10 weeks.

The intervention significantly increased attendance and reduced chronic absenteeism, although there was no additional effect of the information about future benefits of education investments. Attendance rates in the pooled treatment conditions were 6-7 percentage points higher than the control group (or 13-15% relative to the control group mean), while chronic absenteeism was 6-7 percentage points (or 20-24%) lower. Notably, decomposing the treatment effect into the effect on attending the program at all compared with the effect on marginal attendance for those already attending, the treatment messages affected the decision to attend the program at all substantially more than marginal attendance. This aligns with the relatively larger effect on chronic absenteeism than overall attendance rates. We also find a larger reduction in chronic absenteeism for high school students and those with lower baseline attendance. The intervention significantly increased the accuracy of parents' beliefs about children's effort (regarding chronic absenteeism and the number of days attended): parents in treatment groups were substantially less upwardly biased than control group parents. They also increased parental engagement in the afterschool program (in the form of dialogue, reminders, and incentives), but did not change engagement behaviors with school more generally, or beliefs about the future value of education.

This paper contributes to the literature in several ways. First, it provides some of the first evidence on the effectiveness of information messages to parents that leverage data collection systems "as they are". Much of the literature to-date demonstrates the effectiveness of messaging interventions that require either substantial additional effort from educators or leverage sophisticated systems for collecting data on attendance, grades, assignment submissions, and other data. We use educators' existing attendance reports and only provide parents with attendance information. We thus contribute to understanding how similar interventions work in low-resource environments, where educators cannot adjust their effort and where data collection is relatively rudimentary. We also provide one of the first tests of the effectiveness of messaging parents in contexts where attendance rates are low and chronic absenteeism is high. These settings present a plausibly different set of barriers to school attendance and information acquisition compared to settings with high attendance where these interventions have previously been tested; our work is among the first to show that messaging parents can be similarly effective at reducing information asymmetries and absenteeism in these settings. In spite of the relatively lower level of sophistication of our intervention, we find similar effect sizes to other work in the literature, but larger than similar studies in developing countries (0.13-0.17 SD compared with 0.09 and 0.075 in Chile and Brazil respectively). Finally, our work also builds on the literature demonstrating the effect of "light-touch" remote interventions on parental engagement in Africa (Lichand and Wolf, 2021; Angrist et al., 2022). Ours complements this other work by being the first to our knowledge to reduce information asymmetries between parents and educators through child-specific data sent via text message.

The remainder of the paper proceeds as follows. Section 2 provides information on the context and setting of the experiment. Section 3 provides details of the empirical strategy, including descriptions of the intervention, data and estimation procedure. Section 4 gives the results of the experiment, and Section 5 provides a discussion of how the findings in this paper relate to the literature, and concludes.

2 Study Background and Context

We conducted the intervention in Cape Town, in South Africa, between July and September 2016. Like many South African cities, and large cities in developing countries, Cape Town is highly socioeconomically unequal, geographically segregated, and with a relatively high poverty rate. The intervention was conducted with students attending the Year Beyond (Yebo) after-school program, a government-funded initiative aimed at improving the after-school opportunities of students from low-income neighborhoods characterized by high dropout rates, gang violence, and drug use. The government deliberately selected Yebo locations in racially and geographically diverse low-income neighborhoods across Cape Town to inform a broader roll-out of the program (Western Cape Department of Cultural Affairs and Sport, 2013). At the time of the intervention, Yebo was offered in 22 primary and high schools throughout the Western Cape metropolitan area, providing structured curricula in mathematics, language, and non-cognitive skills. The sessions were held three times a week[7], for two hours in the afternoons between 2 PM and 6 PM, and students were encouraged to continue using school facilities if their parents were unavailable when the session ended. Similar afterschool programs, specifically with structured cognitive and non-cognitive materials, have been shown to improve socio-emotional behavior and anti-social behavior, academic performance, and reduce school dropouts (Posner and Vandell, 1999, 1994; Zill et al., 1995; Eccles and Barber, 1999; Miller, 2003; Heller et al., 2017).

The program is implemented by non-government operating partners in collaboration with the Department of Education, with some partners operating in multiple schools. The program recruits students in Grades 1-5 in primary schools and Grades 8-10 in high schools through voluntary enrollment, with a limit of 100 students per school. Students must obtain written consent from parents in order to participate and must attend a minimum of 65% of sessions or face exclusion from the program. In practice, however, this cut-off is not enforced, and over the 3-month period for which baseline data was recorded (beginning of April until the end of June 2016), the average attendance rate was 50%. Despite chronic absenteeism, no students were excluded from the program in the term prior to the intervention[8].

3 Empirical Strategy

3.1 Sample

The sample for this experiment was taken from the universe of students enrolled at 18 of the 22 Yebo centers in Cape Town between March 1st and June 1st 2016[9]. Yebo center locations were

[7]Or twice a week for the youngest grades.

[8]More serious cases of absenteeism had led to exclusion in earlier terms. Moreover, the government planned to increase the adherence to these cutoffs in future.

[9]This includes all but 4 Yebo centers; the remaining centers were excluded given they have different attendance record-keeping practices, making it impossible to access attendance information in a timely manner to include in weekly intervention messages.

deliberately selected by the Western Cape Government to provide access to students from a diverse set of low-income Cape Town neighborhoods and across a range of geographic areas (Western Cape Department of Cultural Affairs and Sport, 2013). Centers were specifically hosted in schools[10]. The schools included 10 primary schools and 8 high schools. All students at the respective schools in grades 1-5 and grades 8-10 were eligible to enroll in the program. Between June 1st (1 month prior to the experiment) and the end of our experimental period (September 30th) the program did not enroll any new students; thus, the full set of enrolled students at these centers was eligible for the experiment.

In the first week of the term, we sent a welcome message to each enrolled student's registered parent contact number. We then screened out all parents who opted out of receiving further messages or who had invalid contact numbers. Of the original 1688 students enrolled at the 18 Yebo centers over this period, 580 were screened out due to invalid contact numbers, and 7 due to parents withdrawing consent to receive text messages, leaving 1101 students who were randomly assigned either to control or to some treatment condition. For our final analysis, we also screened out students who lived within the same household to remove potential contamination between treatment conditions from spillover effects[11]. For robustness, we run all primary analyses including these students and report the results in the Appendix. This left us with a final sample of 1038 students across 18 Yebo centers[12].

3.2 Intervention and Random Assignment

The intervention was a weekly text message reminder sent directly to parents, with information on their child's attendance at Yebo sessions in the previous week. Attendance information (a simple "yes" or "no" for whether a child attended each scheduled day) was digitally recorded by YeBo staff at each school within the program's regular reporting practices[13]. The research team had

[10] We will use school and center interchangeably hereafter.

[11] This amounted to only 62 students in total, across 30 households. This group was randomly assigned to treatment groups, but removed from all primary analyses. We judged this group as insufficient in size to measure spillovers across three treatment conditions.

[12] One additional student was erroneously assigned to more than 1 treatment condition and was removed from the final analysis. However, all analyses are robust to this students' inclusion).

[13] Each school's YeBo staff record weekly attendance for each child on a Microsoft Excel sheet. The YeBo program leader is responsible for uploading these data to a shared Dropbox folder by the end of each week as part of reporting requirements set by the Western Cape Government. The research team was given access to this folder during the study period.

digital access to these data and used a Stata script to automatically generate messages to load onto a bulk messaging platform. Messages were sent between 6pm and 8pm every Monday over the 10-week intervention period, between July 25th and September 30th. The timing was chosen to draw parents' attention to their children's education effort at the beginning of the school week, when the attention might be most pertinent to attendance that week, and when parents were most likely to be with their children, and could thus direct their attention to some form of engagement. This practice was identical across treatment arms. The message was designed to draw parents' attention to their children's education investment and to reduce information asymmetries about children's effort (Bergman, 2021; Bettinger et al., 2021).

The intervention included two treatment arms: one where parents received the reminder message with attendance information described above, and another receiving the same message with an additional sentence with information on the potential long-term returns to attending afterschool programs. A different potential benefit was described each week and each sentence was kept brief to preserve the salience of attendance information[14]. In addition to increasing parents' attention and reducing information asymmetries, this treatment was designed to increase knowledge of, and attention to, long-run benefits, of education (See Table 1 for an example of a message for each treatment group).

Table 1: Intervention Message Examples

	Treatment 1	**Treatment 2**
Attended 1 out of 3 Sessions in Previous Week	Dear Parent/Guardian of Trevor, Trevor attended 1 out of 3 YeBo sessions last week. Please encourage him to attend all sessions this week! We appreciate your help	Dear Parent/Guardian of Trevor, Trevor attended 1 out of 3 YeBo sessions last week. Children who do better at school are more likely to get a good job in future. Please encourage him to attend all sessions this week! We appreciate your help

Children in the sample were randomly assigned to either treatment 1, treatment 2, or to a control group. Parents of children in all three groups received two messages welcoming their children to the program in the first week of the study period, before any treatment messages were sent[15]. Random assignment was stratified by school and baseline attendance. While this approach creates the potential for bias due to spillover effects within schools, the distinctiveness of schools and

[14] See the Appendix for the full list of messages.

[15] This allows us to rule out that simply being reminded of the program's existence or a child's enrollment was sufficient to generate treatment effects.

their relatively small number restricted us to randomize at the individual level. Previous work with a similar design demonstrates positive spillover effects (Bettinger et al., 2021), which suggests that attenuation bias is the more likely form of bias from this design feature (thereby potentially strengthening inference on significant treatment effects). Using exogenous variation in treatment intensity at the class level[16], we find evidence of positive spillover effects and find that they do not materially affect our treatment effect estimates[17].

We chose randomization probabilities to optimize the power to determine both an overall treatment effect (both treatments compared to control) and separate treatment effects for each treatment condition (treatment 1 compared to treatment 2)[18]. We summarize observable characteristics for each treatment group in Table 2[19]. Treatment groups are balanced across all observable characteristics: none of the pairwise comparisons for the full sample approach significance, and when we regress each treatment assignment on all observable characteristics, the F-statistic for each is less than 0.6, suggesting that the full set of baseline characteristics does not predict treatment status. Table 3 also shows balance (with one minor exception) across observable characteristics for each treatment group split by primary and high school.

3.3 Implementation

We sent the first treatment message on July 25th 2016, and the final treatment message on September 30th. This time period spanned the third school quarter. The experiment thus ran for 10 weeks and all parents in the treatment groups were scheduled to receive 10 messages, one each Monday between 18h00 and 20h30. The majority of messages were successfully delivered in each week of the intervention. Over the intervention period, parents received 8.7 messages each on average, and 91% of all messages sent were successfully delivered (See Figure 2 for the distribution of delivered messages, and Appendix Figure A1 for the delivery status of messages by week). In total, 7 con-

[16]Students were assigned to attend YeBo sessions at certain times in “classes” of between 4 and 25 students. This was prior to random assignment. This generates exogenous variation in the intensity of exposure to other treated children. The research team did not have access to class assignment prior to randomization, and as such could not assign treatment at that level, which would have been a preferred design choice.

[17]See Section 4.1.1 and Section 4.3 for a discussion of this analysis.

[18]At a ratio of 4:3:3 for control:treatment 1:treatment 2.

[19]This shows the final sample of 1038 students used for the primary analysis. As noted above, 1101 students were randomly assigned between treatments; 62 were dropped from the final sample due to living in the same household, and a further 1 was dropped due to being mistakenly assigned to two treatment conditions. We run all analyses including and excluding these data, and report the results in Appendix Table A7

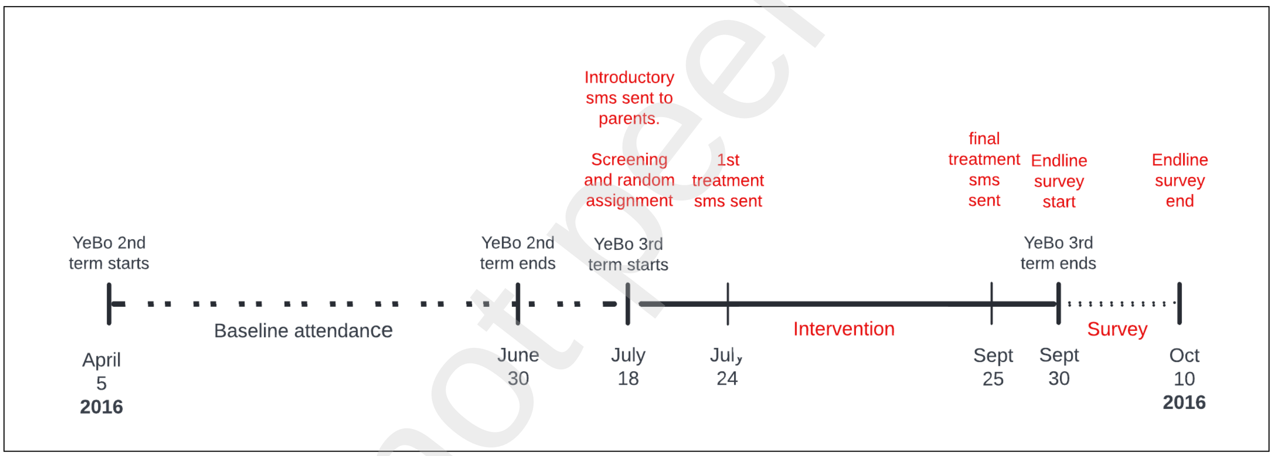
Table 2: Summary Statistics by Treatment Arm

Variable	(1) Control N	(1) Control Mean/SE	(2) Treat 1 - Attend info N	(2) Treat 1 - Attend info Mean/SE	(3) Treat 2 - Info + benefits N	(3) Treat 2 - Info + benefits Mean/SE	T-test P-value (1)-(2)	T-test P-value (1)-(3)
Baseline Attendance - %	413	0.49 (0.02)	310	0.51 (0.02)	315	0.50 (0.02)	0.44	0.83
Baseline test scores	203	50.03 (1.50)	158	49.17 (1.64)	155	50.57 (1.60)	0.70	0.81
Female	413	0.64 (0.02)	310	0.62 (0.03)	315	0.65 (0.03)	0.53	0.75
Black	413	0.57 (0.02)	310	0.57 (0.03)	315	0.59 (0.03)	0.89	0.62
IsiXhosa speaker	413	0.56 (0.02)	310	0.57 (0.03)	315	0.57 (0.03)	0.80	0.79
Afrikaans speaker	413	0.21 (0.02)	310	0.19 (0.02)	315	0.20 (0.02)	0.63	0.79
English speaker	413	0.23 (0.02)	310	0.23 (0.02)	315	0.22 (0.02)	0.82	0.84
Other language	413	0.00 (0.00)	310	0.00 (0.00)	315	0.01 (0.01)	0.74	0.45
Highschool student	413	0.39 (0.02)	310	0.40 (0.03)	315	0.38 (0.03)	0.73	0.79
Grade 1	413	0.17 (0.02)	310	0.18 (0.02)	315	0.18 (0.02)	0.78	0.61
Grade 2	413	0.16 (0.02)	310	0.15 (0.02)	315	0.16 (0.02)	0.70	0.99
Grade 3	413	0.12 (0.02)	310	0.13 (0.02)	315	0.12 (0.02)	0.75	0.91
Grade 4	413	0.10 (0.01)	310	0.09 (0.02)	315	0.09 (0.02)	0.88	0.83
Grade 5	413	0.06 (0.01)	310	0.05 (0.01)	315	0.06 (0.01)	0.40	0.88
Grade 8	413	0.19 (0.02)	310	0.18 (0.02)	315	0.19 (0.02)	0.86	0.87
Grade 9	413	0.12 (0.02)	310	0.12 (0.02)	315	0.10 (0.02)	0.84	0.49
Grade 10	413	0.08 (0.01)	310	0.10 (0.02)	315	0.08 (0.02)	0.29	0.93
Age	402	11.14 (0.16)	306	11.17 (0.19)	306	10.95 (0.19)	0.90	0.46

Notes: This table shows mean values of baseline characteristics for children randomized into each treatment condition from the sample of 1038 children. Data are relatively complete with two exceptions: baseline test scores were only collected for 516 children, and age was missing for 24 children. The value displayed for t-tests are p-values. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. F-statistics for regressions of treatment 1 on baseline conditions are 0.73 (p=0.73) and 0.45 (p=0.95), and for treatment 2 are 0.19 ($p > 0.99$) and 0.21 ($p > 0.99$).

tact numbers were removed from messaging lists over the course of the intervention due to the withdrawal of consent or wrong numbers. The significant majority (85%) of scheduled treatment messages were sent specifically during the 2 hr period described above. The remaining 15% were sent late (later on Monday evening or on the following day) due to internet connectivity problems at program sites or a public holiday falling on the Friday or Monday, delaying the submission of attendance data from the Yebo centers. The above represent the non-compliance in the treatment conditions. Given that the primary analysis in this paper measures the intention to treat (ITT) effect, these observations are included in all analyses[20]. Given that final attendance data was captured for all sites, for all enrolled students, and for all weeks of the observation period, there is no attrition in this study regarding the analysis of attendance outcomes.

Figure 1: Implementation Timeline



Note: red font describes experiment-related activities. The solid line describes the 10-week intervention period, the large dotted line describes the baseline period (from which baseline attendance is recorded and calculated for the full sample), and the small dotted line represents the period of the survey with parents. Dates are given below the line.

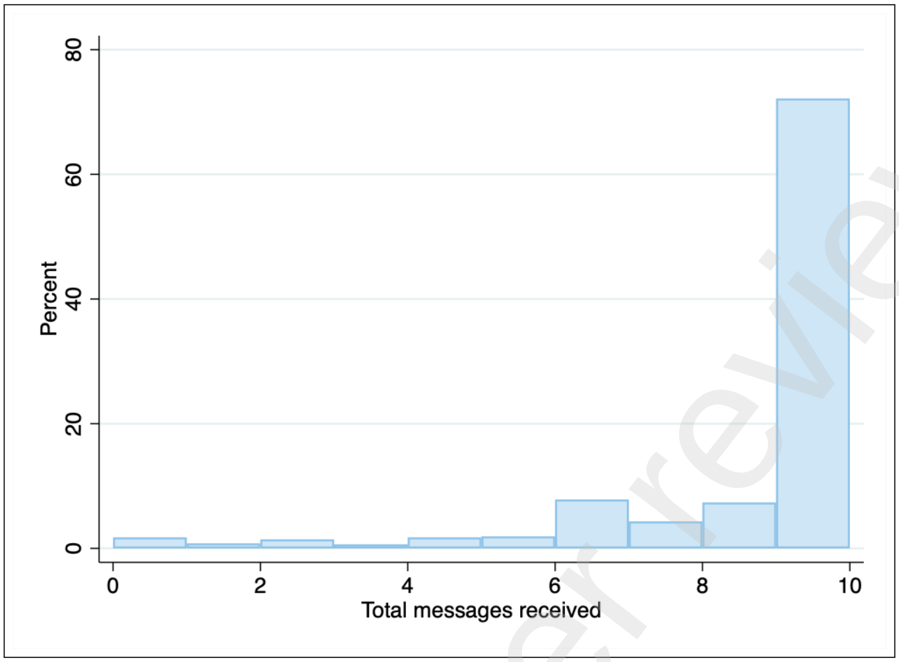
3.4 Data

Data for this analysis come from two sources: baseline and endline attendance and demographic data from program administrative data, and survey data from telephone interviews conducted with parents at endline.

Baseline data was acquired from the program's administrative data. This includes: learner attendance data for the 3 months prior to the intervention, scores from a baseline numeracy and

[20]We also analyze and report results on treatment effects on the treated.

Figure 2: Histogram of treatment messages successfully delivered



Note: parents in all schools were scheduled to receive 10 messages each. However, several schools did not operate centers some weeks, and for these, no messages were scheduled. See the Appendix Figure A1 for the proportion of actual scheduled messages delivered. Moreover, over the course of the program, some parents phones stopped receiving the messages (owing to blocking the number or to losing phones or phone numbers). Please see Appendix Figure A1 for message delivery status by week

literacy test (only for 516 out of the 1038 learners[21]), and learner data on race, gender, age, home language, parent contact number, unique school id, grade and school. These data are complete for all observations, except for test scores and age[22].

For the duration of the intervention, we acquired weekly attendance data from program administrators. These data were used to develop the treatment messages to inform parents of their child's attendance in the previous week.

This paper has two primary outcome variables for attendance. First, the proportion of sessions that a learner attended during the observation period (the "attendance rate"). This variable is constructed by adding each session a learner attended and dividing by the number of days that learner was scheduled to attend[23]. This variable is continuously distributed between 0 and 1 (See Appendix Figure A3 for the distribution) and allows us to capture any improvements in overall

[21]The other learners did not complete the test

[22]Age data is missing for only 24 students; we do not use this variable in our primary analysis.

[23]Days where sites were closed were not included in this denominator. Site closures occurred in some instances due to local protest action. This is orthogonal to treatment status

attendance rates. Second, we use a dummy variable for whether or not the student failed to attend any scheduled sessions during the observation period, which we describe as "complete absence" and use as an indicator of chronic absenteeism. As noted, chronic absenteeism is a strong predictor of dropout and thus warrants its own investigation. Moreover, as evidenced in Appendix Figure A3, a large portion of students are chronically absent.

In addition to attendance data, we conducted a structured telephone interview with parents at the end of the observation period to gather data on parents' beliefs about their children's attendance, parents' engagement behaviors, and general attitudes towards education and the Yebo program. These data were intended to identify the effect of the intervention on parent-related outcomes, and to generate insights into potential mechanisms driving changes in attendance and absenteeism. Section 4.2.1 provides more detail on the survey implementation.

3.5 Estimation

The primary aim of the paper is to identify the effect of the reminder messages on attendance and chronic absenteeism, for both a simple reminder with attendance information included, and for the same message with an additional sentence highlighting future benefits of education. We estimate the "intent-to-treat" (ITT) effects, and Equation 1 represents the model we use to estimate these effects[24].

$$Y_i = \beta_0 + \beta_1 Treat1Simple_i + \beta_2 Treat2Benefits_i + \epsilon_i \tag{1}$$

Y_i represents the dependent variable. For our primary analysis, we use the attendance rate for student i and the complete absence dummy variable for student i. β_0 represents a constant and ϵ_i represents an idiosyncratic error term for student i. $Treat1Simple_i$ and $Treat2Benefits_i$ are our treatment variables and take on a value of one if student i is assigned to receive a simple weekly reminder message with attendance info (treatment 1) or to receive the simple message plus additional information highlighting the future benefits of attendance (Treatment 2) respectively.

[24] We additionally estimate the treatment effect on the treated (ToT) using instrumental variables. We use treatment assignment as an instrument for the number of treatment messages received; we report these results in the appendix.

Each variable takes a value of zero otherwise. Thus, β_1 and β_2 represent our coefficients of interest and identify the treatment effects for treatment 1 and 2 respectively. We run a Wald test to formally test for any difference between β_1 and β_2[25].

In addition to the model in Equation (1), we run a model that pools the treatment conditions together such that all students assigned to either treatment 1 or treatment 2, and who thus receive a weekly reminder message with attendance information, are in the "Pooled Treatment" condition.[26] Equation 2 describes this model: the terms are the same as in Equation 1 except that $PooledTreatment_i$ replaces our other treatment variables and is equal to one if student i was assigned to either treatment 1 or treatment 2 and zero if the student was assigned to the control group. For robustness, in a set of specifications we also use Equation (1) and (2) including a vector of control variables for student i, multiplied by a vector of coefficients, $X_i\beta$.

$$Y_i = \beta_0 + \beta_1 PooledTreatment_i + \epsilon_i \tag{2}$$

Given that the variable for the attendance rate is censored at zero (students cannot attend less than 0% of sessions) and that we observe relatively high numbers of students bunching at this point in the distribution (see Appendix Figure A3 for the distribution), we estimate the above equation using a Tobit model for this dependent variable in our primary analysis[27]. We also estimate the model using Ordinary Least Squares (OLS), and several other estimators for robustness, and present results in the appendix. Additionally, given that the attendance variable is censored and that modal attendance is zero, the attendance decision might be constituted as both a decision to attend the program at all, and a decision of "how much" to attend once having decided to attend at all. As

[25] Additionally, we run a regression that specifies a $PooledTreatment_i$ dummy variable, as in Equation (1), and additionally with a separate dummy variable for $Treat2Benefits_i$; the test for significance of the coefficient on $Treat2Benefits_i$ provides an additional test of whether treatment 2 differs from treatment 1, and thus whether the additional sentence highlighting future benefits makes any additional impact

[26] The base message included identical content between treatment 1 and treatment 2 (the same greeting, reminder, and attendance information, only differing by one sentence), and the relative size of the treatment conditions, such that the control group was larger than each of the other treatment groups individually, were intentional design features to strengthen the test of the reminder message as a "pooled treatment". Given we observe no difference between each treatment condition, this becomes an important analysis approach throughout the paper.

[27] Censored data can bias ordinary least squares (OLS) estimates; the Tobit model uses a latent variable approach to account for this. See Greene (2005) for an elaboration

such, we estimate a Cragg's double-hurdle model, which separately estimates the decision to attend at all and the decision to attend more sessions.

We use the linear probability model (OLS) to estimate the model for the "complete absence" dummy variable, and again run a set of robustness checks by estimating the model with logistic and probit regressions, and a set of other estimators.

Table 3: Summary Statistics for Primary and High School Students

Variable	(1) Control N	Mean/SE	(2) Treat 1 - Attend info N	Mean/SE	(3) Treat 2 - Info + benefits N	Mean/SE	T-test P-value (1)-(2)	(1)-(3)
Primary School Students								
Baseline Attendance - %	278	0.62	198	0.63	205	0.60	0.85	0.52
		(0.02)		(0.02)		(0.02)		
Baseline test scores	124	61.00	88	60.16	87	60.52	0.75	0.85
		(1.65)		(2.04)		(1.97)		
Female	278	0.62	198	0.60	205	0.61	0.56	0.78
		(0.03)		(0.03)		(0.03)		
Black	278	0.57	198	0.58	205	0.58	0.79	0.87
		(0.03)		(0.04)		(0.03)		
IsiXhosa speaker	278	0.56	198	0.58	205	0.56	0.73	0.93
		(0.03)		(0.04)		(0.03)		
Afrikaans speaker	278	0.22	198	0.21	205	0.22	0.82	0.92
		(0.02)		(0.03)		(0.03)		
English speaker	278	0.22	198	0.21	205	0.21	0.85	0.80
		(0.02)		(0.03)		(0.03)		
Other language	278	0.00	198	0.00	205	0.01	N/A	0.10*
		(0.00)		(0.00)		(0.01)		
Age	271	8.75	197	8.80	202	8.71	0.71	0.79
		(0.09)		(0.11)		(0.10)		
High School Students								
Baseline Attendance - %	170	0.31	132	0.34	124	0.32	0.45	0.85
		(0.03)		(0.03)		(0.03)		
Baseline test scores	96	36.06	77	37.70	73	40.11	0.49	0.10
		(1.64)		(1.71)		(1.85)		
Female	170	0.66	132	0.64	124	0.72	0.79	0.28
		(0.04)		(0.04)		(0.04)		
Black	170	0.55	132	0.54	124	0.59	0.87	0.48
		(0.04)		(0.04)		(0.04)		
IsiXhosa speaker	170	0.54	132	0.53	124	0.57	0.93	0.53
		(0.04)		(0.04)		(0.04)		
Afrikaans speaker	170	0.20	132	0.19	124	0.19	0.82	0.76
		(0.03)		(0.03)		(0.04)		
English speaker	170	0.25	132	0.27	124	0.23	0.70	0.71
		(0.03)		(0.04)		(0.04)		
Other language	170	0.01	132	0.01	124	0.01	0.72	0.76
		(0.01)		(0.01)		(0.01)		
Age	166	14.89	128	14.86	118	14.81	0.82	0.58
		(0.10)		(0.11)		(0.12)		

Notes: This table shows mean values of baseline characteristics for children randomized into each treatment condition from the sample of 1038 children. The top panel shows balance for the primary school sample only and the bottom panel for the high schoool sample only. Data are relatively complete with two exceptions: baseline test scores were only collected for 516 children, and age was missing for 24 children. The value displayed for t-tests are p-values. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. F-statistics for regressions of treatment 1 and treatment 2 on baseline conditions are less than 1.2 ($p > 0.4$) for all specifications.

4 Results

4.1 The Effect on Attendance and Chronic Absenteeism

4.1.1 Weekly messages increase attendance

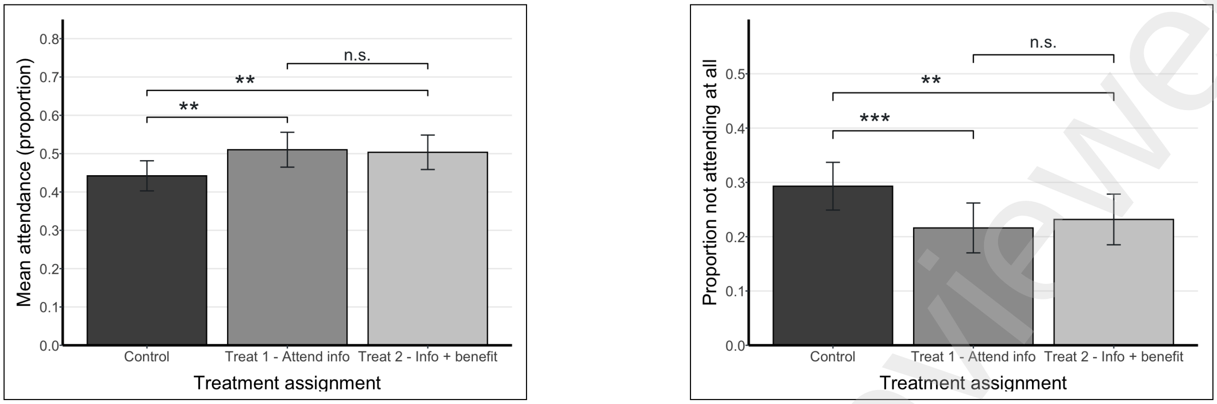
Weekly treatment messages increased the attendance rate and decreased chronic absenteeism in both treatment arms. Figure 3a. shows the attendance rate by treatment assignment: students in the control group attended 44.2% of their scheduled sessions, while those in treatment groups 1 and 2 attended 51.1% and 50.4% respectively. Pairwise t-tests show that differences between treatment groups and the control group are significant at $\alpha = 0.05$. Figure 3b. shows similar differences between treatment and control groups for the *complete absence* dummy variable: 29.2% of control group students did not attend any sessions over the whole term, what we we consider "chronically absent", while 21.6% and 23.1% of students in Treatment Groups 1 and 2 respectively were completely absent. Table 4 presents the estimates from Equations 1 and 2 and confirms these effects. Treatment 1 and Treatment 2 generate an approximately 6.2 - 7.6 percentage point increase in the attendance rate (a 13-16% increase relative to the control group), and an approximately 5.8 - 7.7 percentage point reduction in the rate of chronic absenteeism (a 20-26% reduction relative to the control group).

Across all specifications, there is no significant difference between β_1 and β_2 ($p > 0.5$ across all Wald test results[28]). Moreover, there are no major qualitative differences between the coefficients for each treatment condition across main and robustness specifications (Tables 4 and 5, Appendix Tables A3-A8). There is thus no evidence that the extra sentence, making the benefits of future attendance more salient, generated an additional effect above the simple reminder with attendance information. As such, for most of the analyses in the remainder of the paper, we estimate the model in Equation (2) using the "pooled treatment" group as our treatment group of interest, given the additional statistical power it affords us and that this was a planned feature of the design.

These treatment effects are robust to a number of specifications. First, they are robust to the inclusion or exclusion of control variables (Table 4). Second, they are robust to a range of different

[28] In an additional regression that tests Equation 2 with a $Treat2Benefits$ coefficient included, to estimate the additional effect of treatment 2 relative to the "pooled treatment", the coefficient on this variable is very near zero and does not approach significance (it always realizes a p-value> 0.5), consistent with the Wald test result.

Figure 3: Attendance by Treatment Group



(a) Mean proportion of sessions attended

(b) Complete absence (chronic absenteeism)

Note: bar heights represent mean values by sub-sample for each measure for the full sample of 1038 children. Lines represent 95% confidence intervals for the mean estimate. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level for pairwise comparisons between each sub-sample mean.

estimators, including the addition of school fixed effects, random effects, ordinary least squares (OLS), and others. The treatment effects are quantitatively similar and statistically significant for all treatment coefficients across all alternative estimators (Appendix Tables A3 and A4)[29]. Importantly, treatment effects do not appear to be driven by spillover effects to control group children. We exploit exogenous variation in the proportion of students within a class that are treated to attempt to account for spillover effects[30]. When we include variables for treatment intensity at the classroom-level[31], the estimated spillover effects on attendance and reduced chronic absenteeism are positive, and are indistinguishable for control and treatment group children (Appendix Tables A8-A9)[32]. Crucially, estimated treatment effects after accounting for spillovers remain quantitatively similar to and statistically indistinguishable from those in our primary specifications (Appendix Table A8-A9).

[29] For the attendance rate outcome, coefficients are within 1 percentage point of the effects in Table 4, statistically indistinguishable, and all are significant at $p < 0.05$. For the complete absence outcome, they are statistically indistinguishable and all significant at $p < 0.1$.

[30] Class assignment is prior to and orthogonal to random treatment assignment. Classes consist of groups of children assigned to attend YeBo sessions in the same time slots, and range from 5 to 24 students in size. Classes range from having 25% to 100% of their students in the treatment condition.

[31] We also account for school fixed effects, classroom size and other features in specifications. See Appendix Table A8 for details.

[32] This suggests that more treated children in a classroom generates positive spillover effects on attendance for both treated and non-treated children. However, we cannot fully account for non-linearities in these relationships, given that all classes include treated children and the majority of variation in class-level treatment intensity is between 44% and 73%, the 10th and 90th percentiles of classroom intensity respectively.

Table 4: Primary Treatment Effects on Attendance:
Mean Sessions per Student & Student Complete Absence

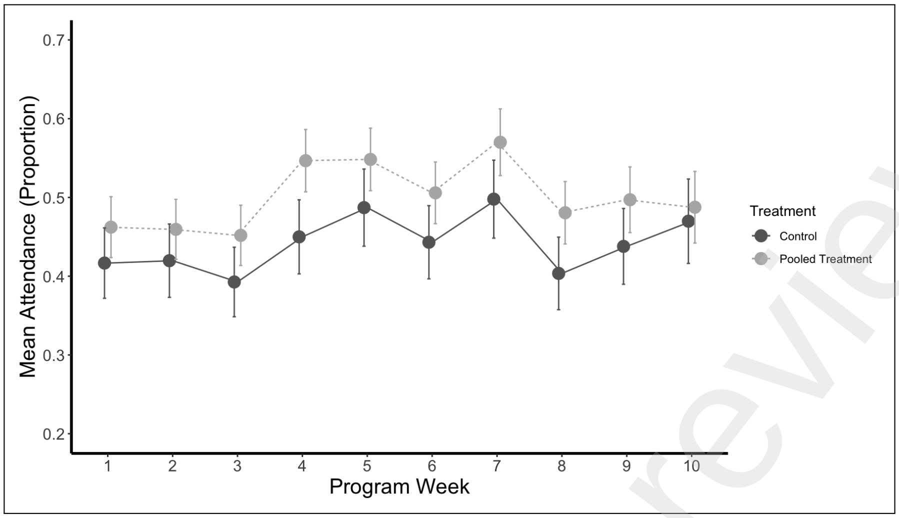
	(1)	(2)	(3)	(4)	(5)	(6)
Mean Sessions per Student						
Treat 1 - Attend info	0.076**	0.062**	0.076**			
	(0.031)	(0.024)	(0.031)			
Treat 2 - Info + benefit	0.066**	0.063***	0.066**			
	(0.031)	(0.024)	(0.031)			
Pooled treatments				0.071***	0.062***	0.071***
				(0.027)	(0.021)	(0.027)
Control Group Mean	0.442	0.442	0.442	0.442	0.442	0.442
Observations	1038	1038	1038	1038	1038	1038
Complete Absence						
Treat 1 - Attend info	-0.077**	-0.064**	-0.077**			
	(0.032)	(0.028)	(0.032)			
Treat 2 - Info + benefit	-0.061*	-0.058**	-0.061*			
	(0.033)	(0.028)	(0.033)			
Pooled treatments				-0.069**	-0.061**	-0.069**
				(0.028)	(0.024)	(0.028)
Control Group Mean	0.293	0.293	0.293	0.293	0.293	0.293
Observations	1038	1038	1038	1038	1038	1038
Baseline attendance control		X	X		X	X
Other controls			X			X

Notes: This table reports coefficients on treatment variables from regression of the primary outcomes, mean sessions per student (or the attendance rate) and complete absence, on treatment variables and various control variables, as per equations 1 and 2. The results of equation 1 are in columns 1-3, and equation 2 are in columns 4-6. Columns 2 and 5 include a control variable for baseline attendance rate. Control variables in columns 3 and 6 include a vector of grade fixed effects, a dummy variable for racial group, and a gender dummy variable (this is in addition to the baseline attendance rate). Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. A Wald test to test for the difference between treatment 1 and treatment 2 coefficients generates $p > 0.65$ for all specifications, and $p > 0.9$ for two of them, suggesting we cannot reject that the treatment coefficients are equal.

Moreover, the effect of the messages on the attendance rate is relatively persistent throughout the intervention period. Figure 4 shows the mean attendance rate in each week of the intervention period for both the Control Group and Pooled Treatment Group: from week 1, there is a small but notable increase in the attendance rate relative to the control group, which grows slightly, and this gap remains consistent throughout the full intervention period[33].

[33] This gap notably reduces in week 10. However, week 10 also realized the closure of all high school centers, and 2 of of the 10 primary school centers. This week thus included less than half, and a selected subset, of the full sample and makes it less comparable to other weeks.

Figure 4: Mean attendance by week and treatment condition



Note: points represent mean proportion of sessions attended by each sub-sample for each week of the program, for the full sample of 1038 children (with the exception of week 10). Notably, in week 10, 10 out of the 18 schools shutdown operations, including all 8 high schools and 2 out of 10 primary schools. As such, only 8 schools, all of which were primary schools, were operational. Lines represent 95% confidence intervals for the mean estimate.

4.1.2 The effect on attending at all and attending more often

Table 4 and Figure 3 show that while there is a statistically significant treatment effect on both the attendance rate and on chronic absenteeism, the effect on chronic absenteeism appears practically larger, especially when looking at the effect as a proportion of the control group mean (approximately 24% compared with 15%). Several authors have suggested that participation decisions of this kind should be analyzed separately at both the extensive and intensive margins (Jones, 1989; Ricker-Gilbert et al., 2011). That is, the decision to attend at all (the participation decision) and the decision to attend more sessions conditional on attending at all (the "consumption" decision) should be analyzed separately, as each constitutes a different decision making process and may thus be differently affected by the intervention. The Cragg's Double-Hurdle model allows us to separately estimate parameters for both the participation and consumption decision. We run the Cragg's model with the "attendance rate" as the dependent variable[34]; Table 5 gives the results. The treatment effects on the participation decision (attending at all; the inverse of the "complete absence" variable) are greater than those on the consumption decision (attendance rate conditional on attending at all) for all treatment variables. In fact, the effect on "marginal attendance", or

[34]We use bootstrapping with 1000 repetitions to generate standard errors; for a full treatment of the model and technique, see Jones (1989) and Burke (2009).

the consumption decision, is not significantly different from zero across all models and treatment variables[35]. This suggests that the treatment effect on overall attendance rates (The upper panel in Table 4) is primarily driven by the effect on participation (or reductions in chronic absenteeism).

[35]The results are similar to those from a separately estimated linear probability model on “participation”, and a truncated regression model on the attendance rate, or the test for the effect on the “consumption decision”

Table 5: Cragg's Model Results

	Participation				Conditional Attendance				Overall Attendance			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treat1-AttendInfo	0.076**	0.075**			0.026	0.028			0.068**	0.067**		
	(0.031)	(0.031)			(0.030)	(0.028)			(0.029)	(0.030)		
Treat2-Info+benefit	0.06**	0.049*			0.03	0.029			0.061*	0.052*		
	(0.031)	(0.029)			(0.029)	(0.029)			(0.031)	(0.029)		
PooledTreatment			0.068***	0.062**			0.028	0.029			0.064**	0.06**
			(0.024)	(0.025)			(0.025)	(0.024)			(0.025)	(0.025)
Control Group Mean	0.707	0.707	0.707	0.707	0.625	0.625	0.625	0.625	0.442	0.442	0.442	0.442
Observations	1038	1038	1038	1038	1038	1038	1038	1038	1038	1038	1038	1038
All Control Variables		X		X		X		X		X		X

Notes: This table reports coefficients on treatment variables from the Cragg's Double-Hurdle Model. Columns 1-4 present coefficients for treatment variables on the decision to participate, columns 5-8 on the marginal attendance conditional on participating, and columns 9-12 on total attendance. Control variables (in alternating columns) include a vector of grade fixed effects, a dummy for racial group, and a gender dummy, and baseline attendance. Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.1.3 Heterogeneity in Attendance Effects

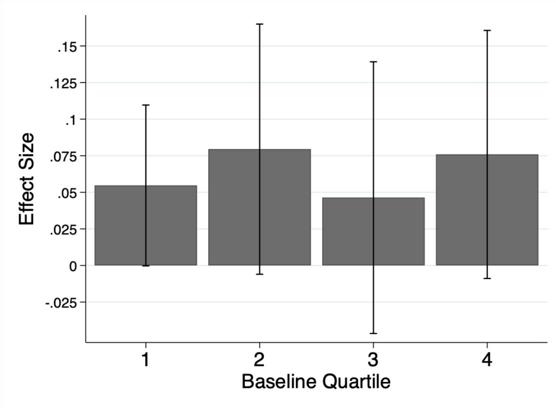
Table 6: Heterogeneous Treatment Effects

	Mean Sessions per Student				Complete Absence			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pooled treatments	0.048*	0.064**	0.033	0.061**	-0.056*	-0.076*	-0.019	-0.031
	(0.027)	(0.031)	(0.025)	(0.025)	(0.031)	(0.040)	(0.025)	(0.024)
Treatment*Male	0.041				-0.020			
	(0.044)				(0.050)			
Treatment*Black		-0.003				0.025		
		(0.041)				(0.048)		
Treatment*HighSchool			0.082*				-0.108**	
			(0.042)				(0.051)	
Treatment*LowBaseAttendance				0.002				-0.062
				(0.034)				(0.039)
Male	0.001				-0.030			
	(0.035)				(0.039)			
Black		0.088***				-0.084**		
		(0.032)				(0.038)		
HighSchool			-0.141***				0.237***	
			(0.034)				(0.043)	
LowBaseAttendance				-0.402***				0.300***
				(0.021)				(0.026)
Observations	1038	1038	1038	1038	1038	1038	1038	1038
All Control Variables	X	X	X	X	X	X	X	X

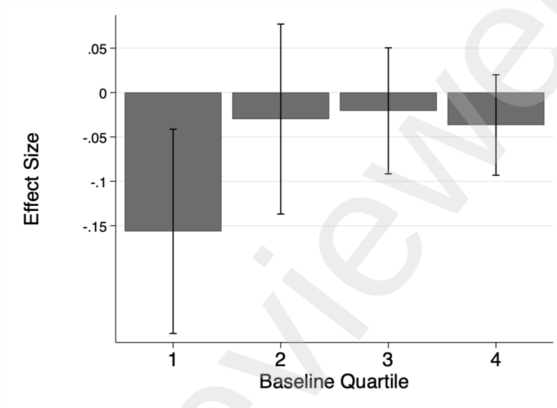
Notes: This table reports coefficients from regressions of outcomes on the pooled treatment dummy variable, dummy variables for one category of sub-sample (Male for gender, Black for racial group, High School, and a dummy variable for below median attendance), and interaction terms between the pooled treatment group and each of these categories. This represents an extension of equation 2, where interaction terms are included to estimate heterogeneous treatment effects. Columns 1-4 show results for mean sessions per student (or the attendance rate) and columns 5-8 for the complete absence outcome. The top panel shows results for the pooled treatment variable and the interaction between pooled treatment and each category; the bottom panel shows coefficients for 'level' term for each category. Control variables include a vector of grade fixed effects (except column 3 and 6, which include a dummy variable for 'high school' to document the 'level effect'), a dummy for racial group (black), a male gender dummy, and baseline attendance rate. Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

There is no difference in the treatment effect across races and across genders. Table 6 presents the results of regressions of attendance rate and complete absence on the pooled treatment, and the pooled treatment interacted with a set of baseline covariates. Columns 1 and 2 and columns 5 and 6 of Table 6 show that the interaction term with "black" and "male" (in the top panel) is not significant, suggesting that the treatment effect is the same across black and colored racial groups and across male and female students. Regarding the heterogeneity between high school and primary school students, the intervention's effect appears to be concentrated among high school students: for both the mean sessions attended and "complete absence" the treatment effect significantly differs

Figure 5: Treatment Effects on attendance and absenteeism by baseline quartile



(a) Proportion of Sessions Attended



(b) Complete Absence

Note: Bar heights represent treatment coefficient estimates of the pooled treatment variable from regressions of each outcome on treatment and a set of control variables (including grade fixed effects, a gender dummy, and a race dummy) run on each quartile of baseline attendance separately. lines represent 95% confidence intervals.

for high school students compared with primary school students. This is particularly pronounced for the "complete absence" outcome, where high school students experience a decrease in chronic absenteeism 11 percentage points greater than that experienced by primary school students. Those with lower baseline attendance show directionally larger effects on attendance, but this is not significant using an interaction with a dummy variable for baseline attendance below the median. However, when we decompose the treatment effect by baseline attendance quartile (Figure 5), we see that the treatment effect on complete absence is concentrated among students in the lowest baseline attendance quartile, with a reduction of 15 percentage points in complete absences for this group. The effect on the proportion of sessions attended across quartiles appears similar. While these comparisons suffer from statistical power limitations, the results suggest that the most susceptible to chronic absenteeism, those with the poorest record of attendance at baseline, are indeed more positively affected by this intervention.

4.2 The Effect on Parents' Beliefs and Engagement behaviors

4.2.1 Phone Survey Implementation

In order to determine the effect of our intervention on parents' beliefs and engagement behaviors, and to better understand how these might affect attendance, we conducted a telephone interview with parents immediately following the final week of the Yebo program term (and intervention period). A team of research assistants called and interviewed parents using a predefined script and a set of structured questions regarding their beliefs about attendance, beliefs about the value of the program and the value of future education, and their engagement behaviors both regarding the Yebo program specifically, and their children's education more generally. Research assistants conducted calls in parents' home language, were blind to treatment status, and used randomly assigned and randomly ordered lists. To reduce potential social desirability concerns, interviews were explicitly addressed as being part of a research project of the University of Cape Town and no reference to the Yebo team was made. The messaging intervention itself at no point mentioned the university, and reference was made to treatment messages only at the end of the interview. Research assistants completed interviews with 436 parents[36]. While this represents a relatively low proportion of parents (42%), there is no differential selection: treatment status does not predict survey response ($p > 0.85$ for both treatment 1 and treatment 2; see Table A10 in the appendix) nor does the full-set of baseline observables (F=1.34, $p = 0.14$). Moreover, within the sample of parent survey respondents, treatment groups are balanced across all observables (Table A11 in the appendix); we additionally include controls for baseline characteristics in all analyses. While we believe *ex ante* that inferences in this section are likely valid given this balance, we caution against strong interpretations and offer the caveat that these results might apply to respondents with a higher likelihood of responding to telephone interviews.

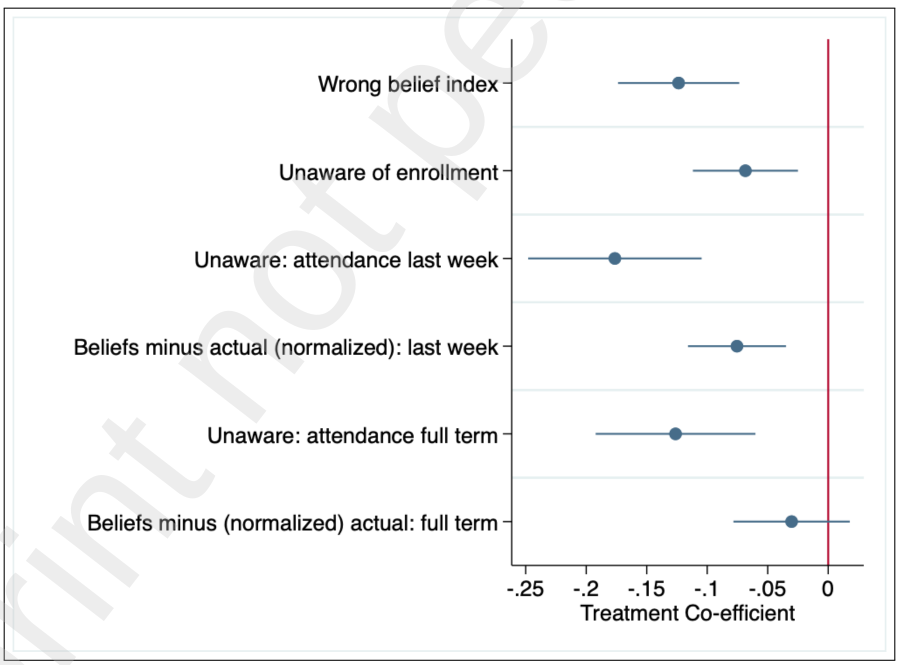
4.2.2 Parent's Beliefs

The text message intervention improved the accuracy of parents' beliefs about their children's attendance. Parents' whose children were in the pooled treatment condition were significantly less likely to be unaware of their child's enrollment in the program (9.5% of surveyed control group parents did not know their child was attending Yebo, compared to 2.7% in the pooled treatment group; $p < 0.001$; see Figure 6) and less likely to respond "no" when asked if they knew how often their child had attended in the final week of the term (28% in the control group compared to 10.3%

[36]Across all call attempts, 35 parents did not consent to be interviewed at the time; some of these offered follow-up times but did not answer the follow-ups or follow-ups could not be made; 42 parents' contact numbers yielded either wrong numbers or cases where students' guardians were not available during the survey period. The remainder received at least one call attempt but did not answer any calls.

in the pooled treatment group; $p < 0.001$). Moreover, parents in the treatment condition also held more accurate beliefs about the number of sessions (days) that their child attended ($p < 0.001$). Figure 7 shows the histogram of the absolute error in parents' beliefs about sessions attended in the final two weeks of the term: control group parents generally have larger errors in their beliefs (measured by the absolute difference between parents' estimate of the number of days their child attended and the actual number they attended). Notably, many control group parents and few treatment group parents have an error equal to 6 days[37]. This predominantly represents parents who believed their children attended all 6 scheduled sessions when they actually attended none. As such, it is more accurate beliefs about more acute absenteeism that differs most between treatment groups. Overall, an index including normalized versions of all attendance belief accuracy variables (those shown in Figure 6) shows a highly significant effect of the intervention on belief accuracy about attendance ($p < 0.001$)[38].

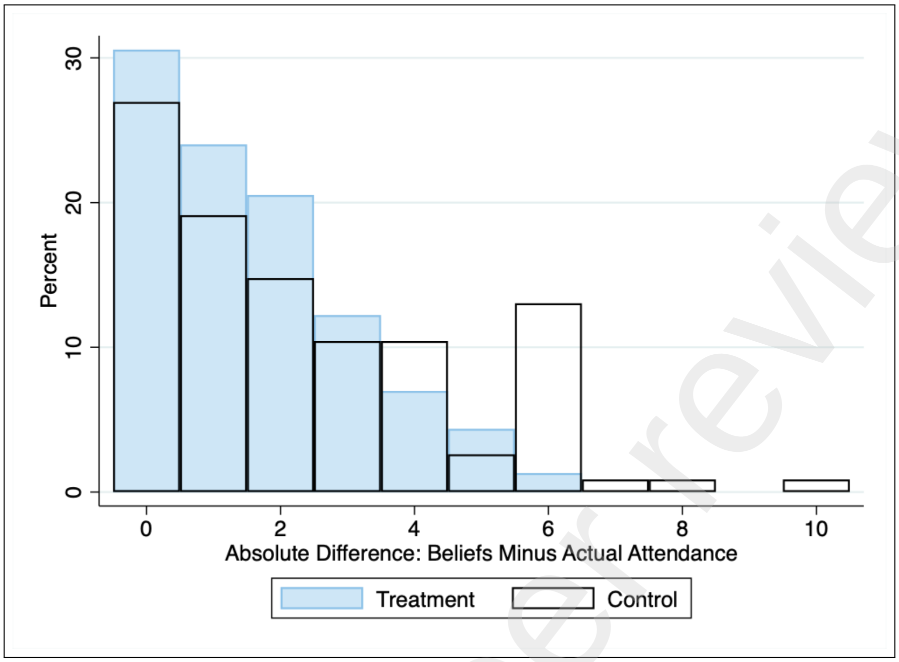
Figure 6: Treatment effects on parents' attendance beliefs



Note: dots represent treatment coefficients of the pooled treatment variable from regressions of each belief variable on treatment and a set of control variables (including baseline attendance, grade fixed effects, a gender dummy, and a race dummy). lines represent 95% confidence intervals.

[37]Recall that students attend 3 sessions per week, amounting to 6 days over 2 weeks.
[38]See Appendix Table A12 for these results.

Figure 7: Histogram of Difference Between Attendance Beliefs and Actual Attendance by Treatment Group



Note: the x-axis describes the absolute difference (in days) between the number of days parents believe their child attended and the actual days they attended in the final two weeks of the term and experimental period. Higher values represent less accurate beliefs and 0 represents perfectly accurate beliefs.

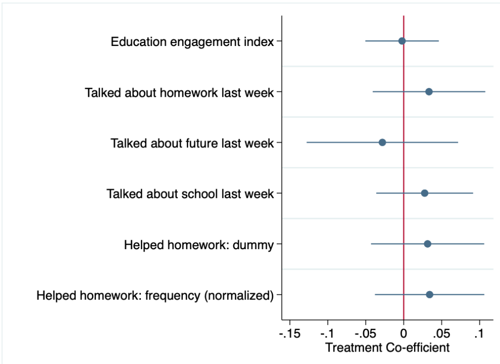
4.2.3 Parent's Engagement behaviors

The text message intervention increased a range of parent engagement behaviors regarding the YeBo program, but did not appear to affect their engagement in education more generally, such as talking about school or helping with homework. Figure 8, Panel b shows that parents in the pooled treatment group were significantly more likely to talk to their children about the Yebo program ($p < 0.001$), to remind them to attend ($p < 0.001$), and to incentivize attendance in the form of punishing non-attendance (p=0.019). Incentives in the form of rewarding attendance and the frequency of talking specifically about Yebo's learning content were both directionally higher for the treatment group but were only significant at a 10% level (p=0.066 and p=0.062 respectively)[39].

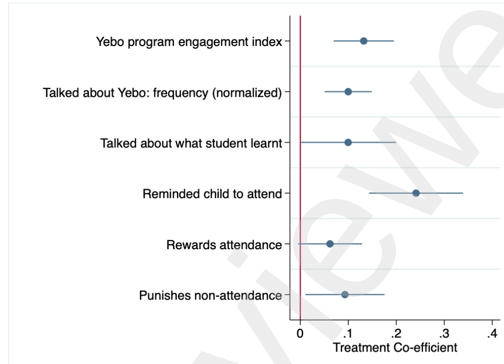
While the text message appears to have affected this Yebo-specific engagement, there is no evidence of any effect on other parent engagement behaviors. Parents receiving the intervention did not talk about homework, school, or their child's future any more than control group parents, nor did they

[39] See Appendix Table A13 for these results.

Figure 8: Treatment Effects on Parents' Engagement behaviors



(a) General Education Engagement behaviors



(b) Yebo-specific Engagement behaviors

Note: dots represent treatment coefficients of the pooled treatment variable from regressions of each behavior on treatment and a set of control variables (including baseline attendance, grade fixed effects, a gender dummy, and a race dummy). lines represent 95% confidence intervals

help with homework any more. Overall, it therefore appears that a message drawing attention to a specific education investment, in this case the Yebo program, and even more specifically attendance of the program, can increase parent engagement regarding that specific investment but that it does not spillover into more general attention to education investments as a whole.

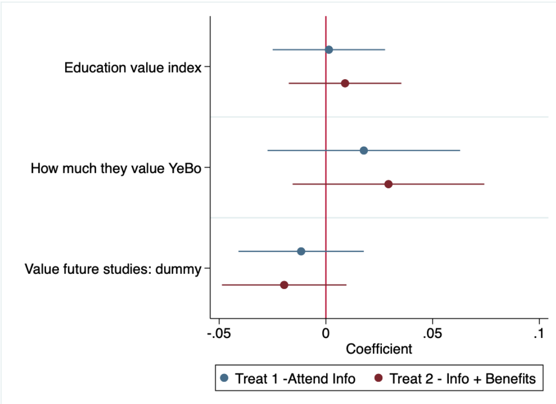
4.2.4 Parents' beliefs of the value of education

The text message did not affect the extent to which parents valued education investments, both specifically (the Yebo program[40]) and generally (the child's post-school studies[41]). Section 4.1.1. shows that Treatment 2, the text message that provides additional information about possible future benefits from attending afterschool programs, shows no additional effect over the simple reminder message with attendance information. Figure 9, Panel a shows that this message did not affect the primary belief it was designed to change. While this suffers from a ceiling effect in measurement for the "value future studies" measure (the control group mean was 99%), there was reasonable variation in the measure of the value of Yebo, which also more directly measured the specific belief that treatment 2 intended to affect. Taking these results together, this intervention does not appear to have any effect on parents' beliefs about the value of education.

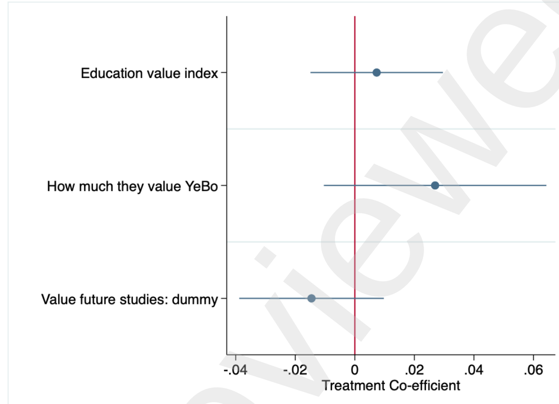
[40]Measured through a likert-type question asking to what extent parents believed that the Yebo program had a positive impact on their child's schooling outcomes

[41]A choice question asking whether the parent believed it was more important for their child to study in higher education or to work immediately after completing school, taking the choice "to study" as a positive measure of

Figure 9: Treatment Effects on Parents' Beliefs About the Value of Education



(a) Treatments 1 and 2



(b) Pooled Treatment

Note: Panel a) dots represent sub-group means for each variable and lines represent 95% confidence intervals. Panel b) dots represent treatment coefficients of the pooled treatment variable from regressions of each outcome on treatment and a set of control variables (including baseline attendance, grade fixed effects, a gender dummy, and a race dummy). lines represent 95% confidence intervals.

4.2.5 What Affected Attendance and Absenteeism?

Given the message was designed to improve belief accuracy and increase parents' attention to children's education investments (specifically in Yebo), and that it in fact generated significant effects on these outcomes, it is plausible that these documented changes mediated the relationship between the treatment messages and attendance. We conduct a mediation analysis (Preacher and Hayes, 2004) to better understand which parental beliefs and behaviors most likely affected attendance. Given that mediation methods cannot identify an exclusive causal model (Fiedler et al., 2011) we treat this analysis as exploratory and cannot rule out alternative unmeasured causal pathways. We perform the Preacher and Hayes (2004) bootstrap estimation of direct, indirect, and total effects of the treatment variable and a set of possible mediators with 1000 repetitions. We select the set of plausible mediators as those that are significantly affected by treatment, and theoretically distinct[42]. This includes: an index of accurate beliefs[43], how often parents talk to

valuing education

[42]Identifying a "mediated effect" or "indirect effect" relies on meeting following conditions outlined by Baron and Kenny (1986): 1) The intervention significantly affects attendance outcomes, 2) The intervention significantly affects mediation variables (a set of parent beliefs and behaviors), and 3) The inclusion of the mediation variables in a regression of attendance outcomes on treatment assignment shows that mediation variables significantly affect attendance and the coefficient on treatment assignment is significantly reduced. We thus select variables meeting conditions 1 and 2 and test condition 3 in this subsection.

[43]We choose an index here given that belief accuracy across measures is highly correlated and all relate to awareness of the child's involvement in the program.Figure 7 shows that this variable is highly significantly affected by the

their children about the program, how often parents remind children to attend (we see these as conceptually distinct), and a dummy variable of whether or not parents incentivize attendance (a dummy variable that takes on one if they either reward attendance or punish non-attendance, and zero otherwise[44]). We test whether these four potential mediators are responsible for an "indirect effect" of the intervention on attendance.

Table 7: Mediation Analysis

	Accurate Attendance Beliefs		Talks about the Program		Reminds Child to Attend		Incentivizes Attendance	
	Mean Sessions per Student							
Indirect Effect	0.027**	0.018*	0.023**	0.008	0.003	-0.006	0.005	0.001
	(0.011)	(0.01)	(0.011)	(0.007)	(0.01)	(0.007)	(0.006)	(0.003)
Direct Effect	0.022	0.017	0.031	0.017	0.048	0.017	0.049	0.017
	(0.044)	(0.044)	(0.042)	(0.044)	(0.044)	(0.044)	(0.039)	(0.044)
Total Effect	0.049	0.036	0.054	0.025	0.051	0.011	0.054	0.019
	(0.041)	(0.042)	(0.041)	(0.044)	(0.041)	(0.043)	(0.039)	(0.044)
	Complete Absence							
Indirect Effect	-0.026**	-0.013*	-0.028***	-0.010	-0.019*	-0.003	-0.010	-0.002
	(0.01)	(0.008)	(0.01)	(0.008)	(0.011)	(0.006)	(0.006)	(0.003)
Direct Effect	-0.061	-0.036	-0.055	-0.036	-0.073*	-0.036	-0.08**	-0.036
	(0.04)	(0.04)	(0.037)	(0.04)	(0.04)	(0.04)	(0.038)	(0.04)
Total Effect	-0.087**	-0.049	-0.083**	-0.045	-0.091**	-0.038	-0.089**	-0.038
	(0.04)	(0.039)	(0.038)	(0.04)	(0.039)	(0.039)	(0.039)	(0.04)
N	429	390	411	390	398	390	413	390
All Controls included	X	X	X	X	X	X	X	X
All mediators included		X		X		X		X

Notes: This table reports coefficients of indirect, direct and total effects of the specific mediators on attendance, using the Preacher-Hayes (2004) Bootstrap mediation analysis procedure with 1000 repetitions. All specifications include the following controls: a vector of grade fixed effects, a dummy variable for racial group, and a gender dummy variable, and baseline attendance rate. Columns 2, 4, 6, and 8 additionally include all mediators as control variables other than the specific one being tested. Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7 shows the results of the bootstrapping procedure for direct, indirect, and total effects, for models that include only the key mediator of interest, and models including the other three mediators as controls. Parents' accurate attendance beliefs and talking to their children about the program show significant indirect effects on both the attendance rate and chronic absenteeism in the models that exclude other mediators. While this effect disappears when we add other mediators as controls for talking to their children about the program, it remains significant for accurate atten-

intervention ($p < 0.001$).

[44]We test this separately to see if it fulfills condition b), and it is significantly affected by the intervention (p=0.007)

dance beliefs. Parents' reminding the child to attend and incentivizing attendance, while correlated with attendance outcomes, do not show any robust indirect effects. Taken together, it appears that accurate beliefs about children's attendance and talking to their children could have mediated the intervention message's effect on attendance. We note, however, that this analysis is exploratory. First, it is possible that these mediating beliefs and behaviors correlate with unobserved variables that causally affect attendance. Second, these variables are highly correlated with one another and given their self-reported nature are likely relatively noisy, making interpretations of the mediating effect of one compared with another sensitive to this measurement error. Ultimately, we cannot make any definitive claims about the appropriate causal model.

4.3 Limitations

While the results above are relatively robust, this study's conclusions have several limitations. First, given that we conducted the experiment in only 18 schools, it was not possible to randomize at the school level due to statistical power concerns. We were also unable to observe class assignment until after the experiment was completed and thus could not assign treatment at that level, either. Treatment effects might thus be biased by spillovers as treatment and control group children were in the same classrooms. We exploit exogenous variation in the proportion of children treated at the classroom level to attempt to measure and control for spillover effects. We find that higher classroom-level treatment intensity is significantly positively related to higher attendance and lower chronic absenteeism for both control and treatment group children (Appendix Tables A8-A9). This suggests that spillover effects on attendance are most likely positive. The inclusion of these variables, and a set of classroom-level controls, does not materially affect our treatment effect estimates. This result is consistent with related work: Bettinger et al. (2021) find that control group children in the same classrooms as treatment group children (whose parents received similar messages to those used in this study) were absent less and performed better in school. While our study shows significant positive spillover effects on treated and control group children, this analysis has limitations. Notably, all classes included some treated children, and 75% of classes had half or more of assigned children treated. As such, we do not have a "pure control group", and if treatment intensity has non-linear effects on attendance, we are unable to show or account for these. Our results should be read with this caveat in mind.

Next, though we observe a significant treatment effect on attendance, the intervention and attendance data only cover 10 weeks. While we see the effect across most weeks in our study (Figure 5), and that it does not diminish over time, we cannot definitively claim that the effect would remain over substantially longer periods. Other papers show that the effect of similar interventions can actually increase after several months (Berlisnki et al., 2021), but future research should test this in contexts closer to ours.

Another limitation of the study is the absence of academic performance data for this sample. While we therefore cannot make strong claims about the effect of messaging on learning outcomes, there is some evidence to suggest that increasing attendance in this context might have improved academic outcomes. First, there is strong evidence from other contexts (Goodman, 2014; Gottfried, 2011; Jacob and Lefgren, 2003) and South Africa (Branson et al., 2014; Osuafor, 2021) that reduced absenteeism is associated with reduced dropout, improved academic performance, and reductions in crime and drug-use. Second, a study using attendance data at an afterschool program in Cape Town shows that attendance at the program, using a difference-in-difference estimation procedure, causes increases in math (but not language) systemic test results for Grade 6 learners (Western Cape Department of the Premier, 2016). This study had several limitations[45], but provides suggestive evidence that attending YeBo could improve academic outcomes.

5 Discussion & Conclusion

To our knowledge, this is the first paper to study the effect of simple text messages on parent beliefs and behaviors, and on children's attendance in the context of chronic absenteeism in Africa. Our results are consistent with those in developed countries that find substantial reductions in absenteeism from simple text-messaging interventions (Bergman et al., 2021; Bergman, 2021; Rogers and Feller, 2018; Kraft and Rogers, 2015; Bergman and Chan, 2020). Given the range of documented effect sizes of similar interventions on attendance (between 0 - 0.21 SD; Berlinski et al., 2021), our effect of 0.13-0.17 SD is on the larger side, and is relatively larger than those in other developing countries, such as 0.09 SD and 0.075 SD in Chile and Brazil respectively (Berlinski et al., 2021; Bettinger et al., 2021). This is especially encouraging, given that this intervention used only atten-

[45] It is limited to Grade 6 learners and lacks data to robustly support the parallel trends assumption

dance information and a basic encouragement. As such, it suggests that in this context, relatively simple text message interventions can effectively improve attendance.

The intervention is also highly cost-effective. The weekly cost of each message was approximately R1.03 ($0.07US[46]) per child, or $2.38 per child for the full school year[47]. Set-up costs, including message translation, welcome texts, and the generation of scripts to produce weekly intervention messages, amounted to R4209 in total ($290). The total cost per learner per year is therefore $3.91. Using a conservative treatment effect of 6%, each additional day learners attend due to the intervention costs $0.74. This compares favorably to the cost-effectiveness of similar interventions in the United States, at $6 per additional day attended[48], and to those in Chile, at approximately $1.25 per additional day attended (Berlisnki et al., 2021)[49]. Our calculations suggest that the variable cost per additional child is approximately $1.30 per annum, and should decrease with scale. Given the relatively high level of per-child expenditure on education in South Africa, and its relatively low-effectiveness (Spaull, 2013), such low-cost interventions could prove extremely fruitful to getting children into quality learning facilities. Indeed, this intervention compares highly favorably to alternative means of increasing attendance, such as one-on-one mentoring (Guryan et al., 2021), which costs approximately $500 per additional day attended in the United States.

The intervention is also relatively simple to implement. We used only attendance data and existing systems for collecting and collating data. Organizations with existing data collection systems thus only require a simple script and can use very affordable messaging services to communicate personalized information to parents. Regarding the effects' sustainability, our data show that treatment effects increased after the first few weeks of the intervention, and then remained consistent for the remaining weeks. This is similar to Berlinski et al. (2021) who find that attendance treatment effects increased over the course of a year-long messaging intervention. However, good quality data collection systems and regular updates of parent contact information are important for the success of similar interventions over longer periods.

Regarding costs and implementation, the intervention shows strong potential for scaling. However,

[46] using the 2016 USD to ZAR exchange rate of 0.069.

[47] weekly costs include the cost of sending the SMS, and weekly labor of approximately three hours per week, which amounts to R0.56 and R0.47 per learner per week respectively.

[48] this is partly attributable to our slightly larger percentage point treatment effect and to lower labor costs

[49] calculation given effect size and costing in Berlinski et al., 2021)

successful scaling also depends on the external validity of this paper's key findings. Several features of the study suggest that it has potential across multiple contexts. Firstly, the targeted areas share similar characteristics to most at-risk areas and schools in Cape Town: they were deliberately selected for representativeness to inform a more extensive roll-out of the afterschool program. Second, the selected cross-section of schools are representative of racial demographics of Cape Town's low-income groups. The intervention was also equally effective for black and colored families. On the other hand, YeBo participants are self-selected, as learners must actively register and parents must provide written consent to participate in YeBo. This group could thus have different characteristics to the general population of parents and learners in these areas, such as higher motivation. Thus, messaging could have a different impact for groups where enrollment is mandatory, such as school classes, compared to where learners self-select.

Notably, the effects on attendance were larger for high school students, who are able to exercise more agency in their decision to attend or not relative to primary school children, and for students with lower baseline attendance. This finding aligns with those in similar papers, where older children and more at-risk children benefit more from the intervention (Bergman, 2021; Bergman and Chan, 2021; Berlinski et al., 2021). When we decompose the effect on attendance, we find that the significant majority of the effect is driven by those shifting from not attending at all to actually coming to the program, and that this is concentrated among high school students. The shift from complete absence to attendance is a crucial first step in being able to reach disaffected young people in low-income neighborhoods and connect them to the support they need. This intervention provides evidence that increasing this connection with a support system may be possible at relatively low cost and with relative speed.

The intervention also specifically improved the accuracy of parents' beliefs about their children's attendance. This adds to a growing literature on the importance of information interventions to correct belief biases, and induce investments (Bursztyn et al., 2020; Jensen, 2010), including in education (Bergman, 2021; Dizon-Ross, 2019). While mediation analysis suggests that improved belief accuracy might have driven the effect on attendance, this analysis does not provide definitive causal evidence for this mechanism over alternative mechanisms, such as salience (Bettinger et al., 2021). The intervention had no effect on parents' beliefs about the future value of education. Additionally, our second treatment arm, which included an additional sentence highlighting plausible future benefits of investing in education, showed no effect relative to the message with attendance information alone. It is possible that the attendance information drew attention away from this additional "future benefits" information[50]. It is also possible that children's attendance information and future benefits information do not generate monotonic increases (Lichand and Wolf, 2021). Given we did not have a treatment condition exclusively describing these benefits, we cannot definitively determine this. Finally, parents might already be acutely aware of the future benefits of education, and thus not require reminding. Several authors show that low-income parents often place a high value on education (Bursztyn and Coffman, 2012; Edmonds, 2006)[51].

Moreover, increased parent engagement regarding Yebo specifically did not lead to more general attention to education investments as a whole. As discussed earlier, the parent survey results should be read with caution given selection in this sub-sample. The parental engagement literature shows that engagement is highly complex and includes subtle behaviors that may not be captured in the self-reported survey, yet could be correlated with engagement. Thus, similarly to the rest of the parental engagement literature, this paper cannot definitively determine the true parental behavior responses that affect attendance. This leaves open the question of which other interventions might heighten parental engagement, providing an opportunity for further experimentation.

[50]The average number of words per message for treatment 2 was 44.3 compared to 30.3 for treatment 1, suggesting that this could be the case. The information on future benefits may have especially lacked salience as it came towards the end of the message.

[51]For example, Bursztyn and Coffman (2012) find that some of Brazil's poorest parents were willing to spend up to 30% of their monthly income on a mechanism to commit children to attending school.

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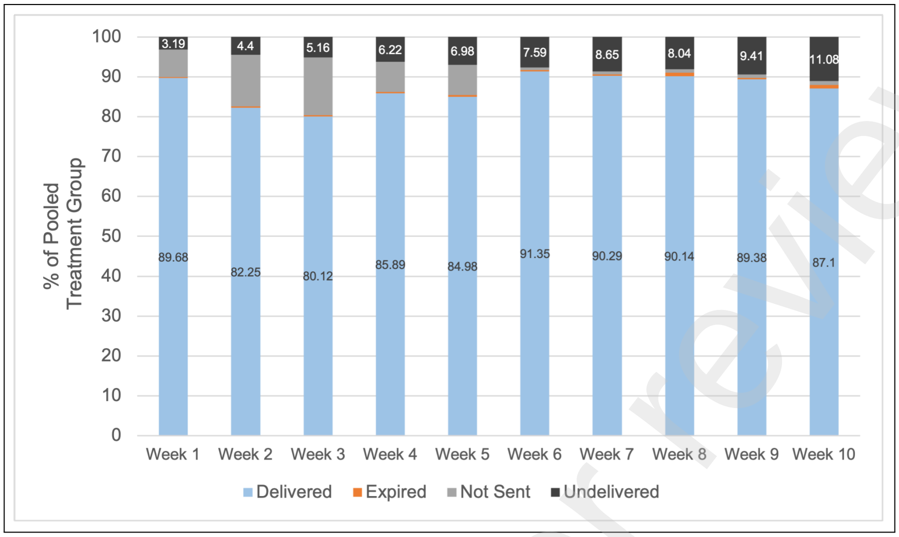
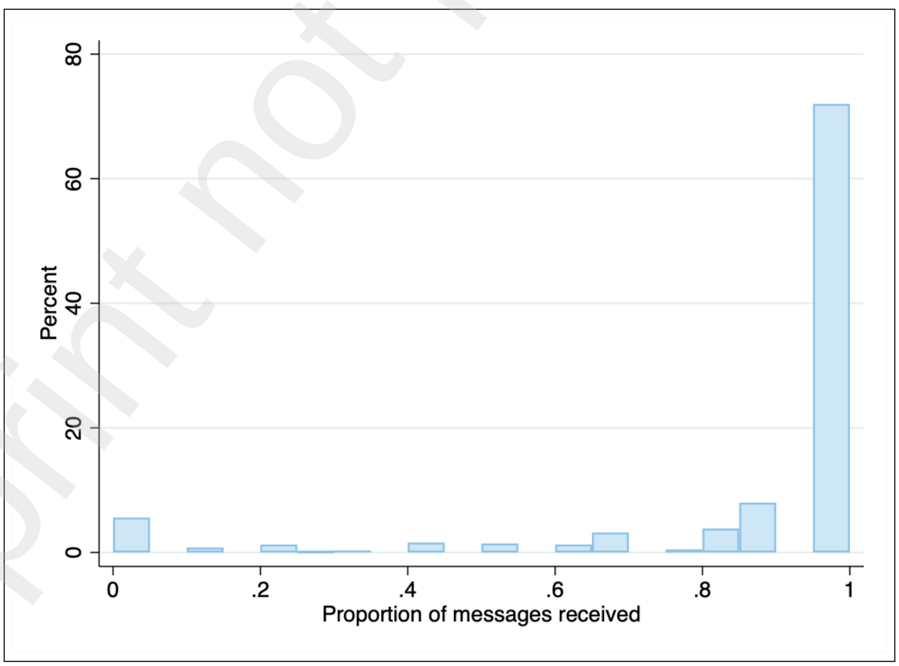
Appendix

Figure A1: Weekly message delivery status during intervention period



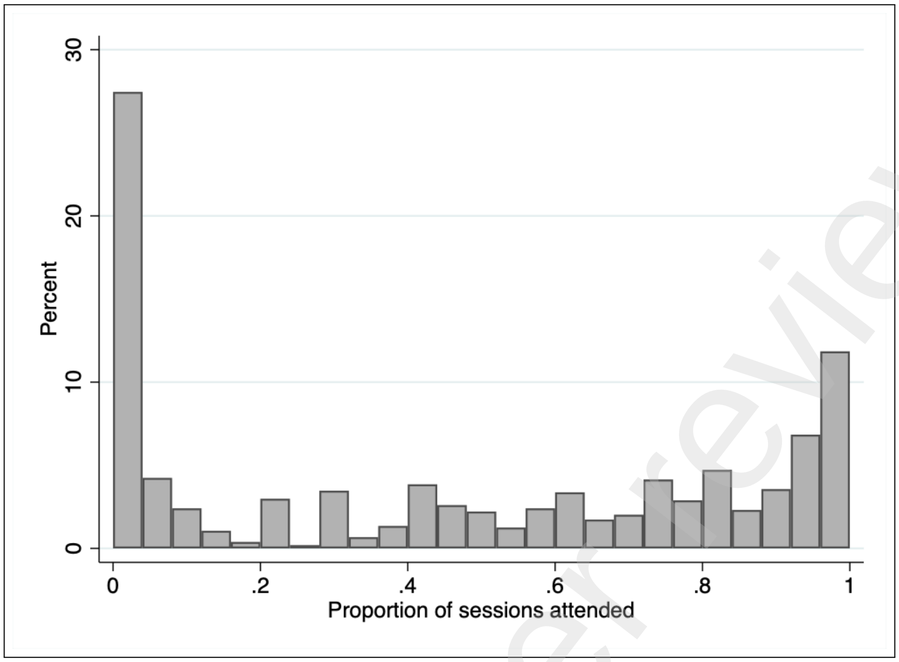
Note: This shows the status distribution of each intervention message by week in the intervention period. Parents in all schools were scheduled to receive 10 messages each. However, several schools did not operate centers some weeks, and for these, no messages were scheduled. This, and express requests to not receive further messages, are represented by the "Not Sent" category. "Expired" and "Undelivered" both represent messages sent but that did not deliver to parents' phones.

Figure A2: Histogram of proportion of treatment messages successfully delivered



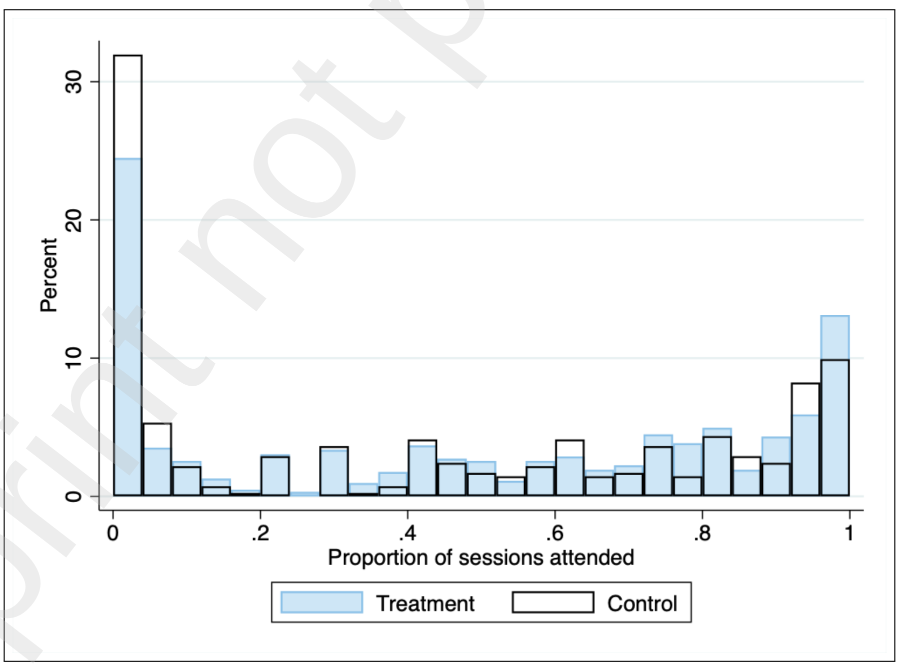
Note: this represents the distribution of the proportion of intervention messages (out of 10) that parents assigned to treatment conditions received.

Figure A3: Histogram of proportion of sessions attended



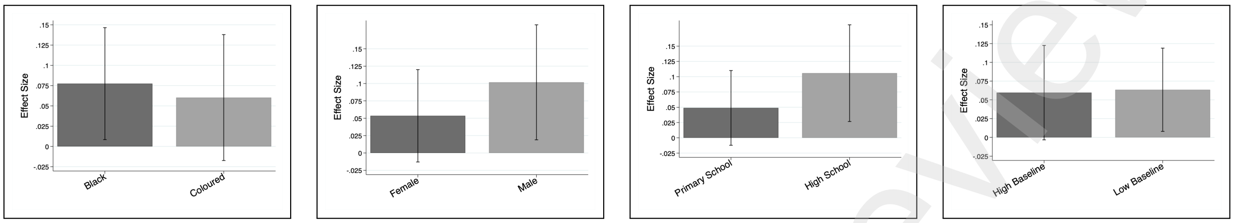
Note: this represents the distribution of the proportion of YeBo sessions (out of the total scheduled available sessions at each center) that each student in the sample of 1038 attended.

Figure A4: Histogram of proportion of sessions attended by treatment group



Note: this represents the distribution of the proportion of YeBo sessions (out of the total scheduled available sessions at each center) that each student in the sample of 1038 attended, decomposed by treatment group. Light blue bars represent the distribution for students in the pooled treatment group and white bars for those in the control group.

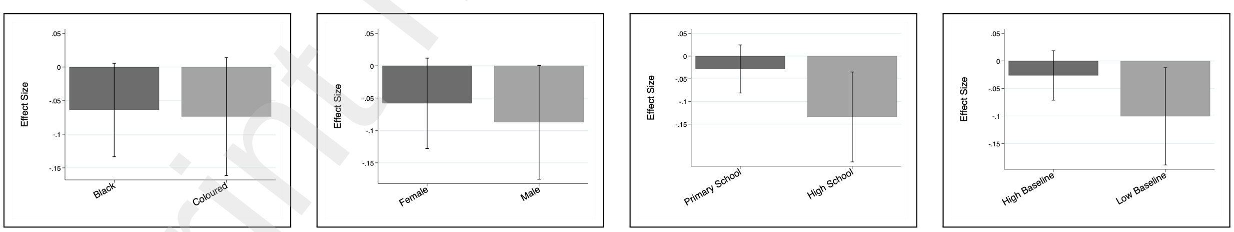
Figure A5: Treatment Effects by Subgroup for Proportion of Sessions Attended



(a) Race (b) Gender (c) School Type (d) Baseline Attendance

Note: bar heights represent treatment coefficients of the pooled treatment variable from regressions of the "mean sessions per student" outcome (attendance rate) on treatment and a set of control variables (including baseline attendance, grade fixed effects, a gender dummy, and a race dummy) for each subgroup, separately. lines represent 95% confidence intervals.

Figure A6: Treatment Effects by Subgroup for "Complete Absence"



(a) Race (b) Gender (c) School Type (d) Baseline Attendance

Note: bar heights represent treatment coefficients of the pooled treatment variable from regressions of the "complete absence" outcome on treatment and a set of control variables (including baseline attendance, grade fixed effects, a gender dummy, and a race dummy) for each subgroup, separately. lines represent 95% confidence intervals

Table A1: List of additional sentences for Treatment 2 messages

Children who attend after-school programmes are more likely to stay out of trouble in school.
Children who attend after-school programmes are more likely to behave better at home.
Children who attend after-school programmes are more likely to go to school.
Children who attend after-school programmes are more likely to do better at school.
Children who attend after-school programmes are more likely to listen to teachers and parents.
Children who do better at school are more likely to get a good job in future.
YeBo can help [StudentName] succeed at school.
YeBo can help give [StudentName] the skills he needs for a successful life.
YeBo is an investment in a better life for [StudentName].

Notes: This table lists the additional sentences used in Treatment 2 messages designed to focus on potential 'future benefits' of attending afterschool programmes. This is derived from the evidence on afterschool programme attendance's relationship with children's future education and labor market outcomes. These were randomly assigned to different weeks over the course of the program.

Table A2: Intervention Costs

Cost	Amount (ZAR)	Amount (USD)
Marginal Costs		
Treatment message total cost	3037.98	209.62
Labour Cost	3600.00	248.40
Total Marginal Costs	6637.98	458.02
Marginal cost per learner per week	1.01	0.07
Fixed Costs		
Welcome and Test Messages	1529.02	105.50
Set-up Labour Costs	1680.00	115.92
Translation Services	1000.00	69.00
Total Fixed Costs	4209.02	290.42
Fixed Cost per learner per week	0.64	0.04
Total Costs	10247.00	707.04

Notes: This table calculates the cost of the programme using total amounts spent in running the program, converting into per-child costs by dividing by the total number of treatment group students (653) the intervention was administered with. USD amounts are calculated using the 2016 USD-ZAR exchange rate of 1 ZAR = 0.069 USD.

Table A3: Robustness: Mean Sessions per Student
Different Estimators

	OLS		Clustered by School		Random Effects		Fixed Effects	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat 1 - Attend info	0.068**		0.076***		0.076***		0.077***	
	(0.031)		(0.025)		(0.029)		(0.028)	
Treat 2 - Info + benefit	0.061**		0.066***		0.064**		0.064**	
	(0.030)		(0.026)		(0.028)		(0.028)	
Pooled treatments		0.065**		0.071***		0.070***		0.071***
		(0.026)		(0.021)		(0.024)		(0.024)
Control Group Mean	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442
Observations	1038	1038	1038	1038	1038	1038	1038	1038
All Control Variables	X	X	X	X	X	X	X	X

Notes: This table reports coefficients on treatment variables from regression of the mean sessions per student outcome (or the attendance rate) on treatment variables and various control variables, as per equations 1 (odd numbered columns) and 2 (even numbered columns). All specifications include the following control variables: a vector of grade fixed effects, a dummy for racial group, and a gender dummy (in addition to the baseline attendance rate). This table produces the results using different estimators. Columns 1 and 2 use a standard OLS estimator with heteroskedasticity robust standard errors. Columns 3 and 4 use a Tobit model, and cluster standard errors by school. Columns 5 and 6 use a random effects model and columns 7 and 8 a fixed effects model, with random and fixed effects for schools, to account for the hierarchical data structure. Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Robustness: Student Complete Absence
Different Estimators

	Clustered by School		Random Effects		Fixed Effects		Logistic		Probit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat 1 - Attend info	-0.077**		-0.083***		-0.083***		-0.076**		-0.076**	
	(0.032)		(0.030)		(0.030)		(0.033)		(0.032)	
Treat 2 - Info + benefit	-0.061		-0.059**		-0.059**		-0.059*		-0.060*	
	(0.035)		(0.030)		(0.030)		(0.032)		(0.032)	
Pooled treatments		-0.069**		-0.071***		-0.071***		-0.068**		-0.068**
		(0.032)		(0.025)		(0.025)		(0.027)		(0.027)
Control Group Mean	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442
Observations	1038	1038	1038	1038	1038	1038	1038	1038	1038	1038
All Control Variables	X	X	X	X	X	X	X	X	X	X

Notes: This table reports coefficients on treatment variables from regression of the complete absence outcome on treatment variables and various control variables, as per equations 1 (odd numbered columns) and 2 (even numbered columns). All specifications include the following control variables: a vector of grade fixed effects, a dummy for racial group, and a gender dummy (in addition to the baseline attendance rate). This table produces the results using different estimators. Columns 1 and 2 use an OLS estimator with standard errors clustered by school. Columns 4 and 4 use a random effects model and columns 5 and 6 a fixed effects model, with random and fixed effects for schools, to account for the hierarchical data structure. Finally, whereas the primary models in this paper use OLS (the Linear Probability Model) for this outcome, columns 7 and 8 and 9 and 10 provide results from estimation with the logistic regression and probit estimators. Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Treatment Effects on the Treated: Instrumental Variable Estimate

	(1)	(2)	(3)	(4)	(5)	(6)
			Mean Sessions per Student			
Treat 1 - Attend info	0.079**	0.079**	0.079**			
	(0.035)	(0.035)	(0.035)			
Treat 2 - Info + benefit	0.069**	0.069**	0.069**			
	(0.034)	(0.034)	(0.034)			
Pooled treatment				0.074**	0.074**	0.074**
				(0.029)	(0.029)	(0.029)
Control Group Mean	0.442	0.442	0.442	0.442	0.442	0.442
Observations	1038	1038	1038	1038	1038	1038
			Complete Absence			
Treat 1 - Attend info	-0.089**	-0.074**	-0.089**			
	(0.037)	(0.033)	(0.037)			
Treat 2 - Info + benefit	-0.069*	-0.065**	-0.069*			
	(0.037)	(0.032)	(0.037)			
Pooled treatment				-0.079**	-0.070**	-0.079**
				(0.032)	(0.027)	(0.032)
Control Group Mean	0.293	0.293	0.293	0.293	0.293	0.293
Observations	1038	1038	1038	1038	1038	1038
Baseline attendance control		X	X		X	X
Other controls			X			X

Notes: This table reports estimates from a two-stage least squares regression of each outcome on the proportion of messages received, where treatment assignment is used as an instrument for the proportion of treatment messages received, which serves as an endogenous measure of treatment. The coefficients represent the average treatment effect of receiving all weekly messages. Columns 2 and 5 include a control variable for baseline attendance rate. Control variables in columns 3 and 6 include a vector of grade fixed effects, a dummy variable for racial group, and a gender dummy variable (this is in addition to the baseline attendance rate). Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: Main Effects: 65% attendance dummy outcome variable

	(1)	(2)	(3)	(4)	(5)	(6)
Treat 1 - Attend info	0.062*	0.047	0.062*			
	(0.037)	(0.031)	(0.037)			
Treat 2 - Info + benefit	0.068*	0.064**	0.068*			
	(0.036)	(0.031)	(0.036)			
Pooled treatments				0.065**	0.055**	0.065**
				(0.031)	(0.026)	(0.031)
Control Group Mean	0.293	0.293	0.293	0.293	0.293	0.293
Observations	1038	1038	1038	1038	1038	1038
Baseline attendance control		X	X		X	X
Other controls			X			X

Notes: This table reports coefficients on treatment variables from regression of the '65% attendance' outcome variable on treatment variables and various control variables, as per equations 1 and 2. The '65% attendance dummy' represents a dummy variable that takes on a value of '1' if the student attended 65% or more of their scheduled sessions, and '0' otherwise. This represents the government's attendance rate target for eligibility for continue participation. However, this standard is not enforced in practice. The results of equation 1 are in columns 1-3, and equation 2 are in columns 4-6. Columns 2 and 5 include a control variable for baseline attendance rate. Control variables in columns 3 and 6 include a vector of grade fixed effects, a dummy variable for racial group, and a gender dummy variable (this is in addition to the baseline attendance rate). Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A7: Robustness: Including Intra-household Spillovers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Mean Sessions per Student														
Treat 1 - Attend info	0.076**		0.076**		0.071**		0.068**		0.068**		0.070**		0.065**	
	(0.031)		(0.031)		(0.032)		(0.031)		(0.031)		(0.031)		(0.031)	
Treat 2 - Info + benefit	0.066**		0.066**		0.059*		0.053*		0.053*		0.059*		0.052*	
	(0.031)		(0.031)		(0.031)		(0.031)		(0.031)		(0.031)		(0.031)	
Pooled treatments		0.071***		0.071***		0.065**		0.061**		0.061**		0.064**		0.058**
		(0.027)		(0.027)		(0.027)		(0.026)		(0.026)		(0.026)		(0.027)
Shared contact spillover					-0.195	-0.195							-0.203	-0.203
					(0.124)	(0.124)							(0.125)	(0.125)
Intra-household spillover											-0.089	-0.090	-0.092	-0.093
											(0.118)	(0.118)	(0.118)	(0.118)
Control Group Mean	0.442	0.442	0.442	0.442	0.442	0.442	0.459	0.459	0.459	0.459	0.459	0.459	0.459	0.459
Observations	1038	1038	1038	1038	1038	1038	1100	1100	1100	1100	1100	1100	1100	1100
Complete Absence														
Treat 1 - Attend info	-0.077**		-0.077**		-0.068**		-0.068**		-0.068**		-0.065**		-0.057*	
	(0.032)		(0.032)		(0.033)		(0.031)		(0.031)		(0.031)		(0.032)	
Treat 2 - Info + benefit	-0.061*		-0.061*		-0.052		-0.058*		-0.058*		-0.059*		-0.049	
	(0.033)		(0.033)		(0.033)		(0.031)		(0.031)		(0.032)		(0.032)	
Pooled treatments		-0.069**		-0.069**		-0.060**		-0.063**		-0.063**		-0.062**		-0.054**
		(0.028)		(0.028)		(0.028)		(0.027)		(0.027)		(0.027)		(0.027)
Shared contact spillover					0.222	0.223							0.228	0.229
					(0.137)	(0.137)							(0.136)	(0.136)
Intra-household spillover											0.170	0.170	0.175	0.176
											(0.114)	(0.114)	(0.114)	(0.114)
Control Group Mean	0.293	0.293	0.293	0.293	0.293	0.293	0.282	0.282	0.282	0.282	0.282	0.282	0.282	0.282
Observations	1038	1038	1038	1038	1038	1038	1100	1100	1100	1100	1100	1100	1100	1100
Baseline attendance control			X	X	X	X			X	X	X	X	X	X
Other controls			X	X	X	X			X	X	X	X	X	X
Spillover participants included							X	X	X	X	X	X	X	X
Shared Contact Controls					X	X							X	X
Intra-house Controls											X	X	X	X

This table reports coefficients on treatment variables from regression of the primary outcomes, mean sessions per student (or the attendance rate) and complete absence, on treatment variables and various control variables, as per equations 1 and 2. The table provides robustness checks for the average treatment effect of treatment messages by attempting to account for possible spillover effects within households and between children with shared contact numbers. In the primary analyses in this paper, children from within the same household (n=62) are excluded from analysis, leaving n=1038. Columns 1-4 reproduce these results. Columns 5 and 6 add a control variable for children with a shared contact but who do not live together, and a variable 'Shared contact spillover' that takes on a value of 1 if the child is in the control group and has a shared contact with a treatment group child (indicating that there might be a spillover from treatment). Columns 7-14 include children within the same household who are randomly assigned to a treatment group or control (and who were previously omitted due to possible spillover effects), in addition to the original sample, n=1100. Columns 11-14 add a control variable for children within the same household, and a variable "Intra-household spillover" that takes on a value of 1 if the child is in the control group and lives with a treatment group child (indicating that there might be a spillover from treatment). Regarding control variables, columns 3-7 and 9-14 include a control variable for baseline attendance rate, a vector of grade fixed effects, a dummy variable for racial group, and a gender dummy variable. Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A8: Spillover Analysis: Class-Level Treatment Intensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean Sessions per Student								
Pooled treatment	0.080***	0.106	0.070***	0.110	0.075***	0.141	0.070***	0.113
	(0.026)	(0.121)	(0.020)	(0.093)	(0.025)	(0.122)	(0.019)	(0.093)
Spillover (Class Treatment Intensity)	0.340***	0.365**	0.175**	0.214*	0.330***	0.392***	0.175**	0.216*
	(0.098)	(0.149)	(0.077)	(0.116)	(0.099)	(0.150)	(0.077)	(0.116)
Spillover * Pooled Treatment		-0.043		-0.067		-0.110		-0.072
		(0.198)		(0.151)		(0.200)		(0.151)
Observations	976	976	976	976	1038	1038	1038	1038
Complete Absence								
Pooled treatment	-0.088***	-0.110	-0.087***	-0.144	-0.070***	-0.141	-0.072***	-0.142
	(0.027)	(0.129)	(0.023)	(0.111)	(0.027)	(0.129)	(0.023)	(0.111)
Spillover (Class Treatment Intensity)	-0.309***	-0.329**	-0.235**	-0.286**	-0.281***	-0.350**	-0.210**	-0.277**
	(0.105)	(0.159)	(0.093)	(0.139)	(0.106)	(0.160)	(0.093)	(0.139)
Spillover * Pooled Treatment		0.037		0.096		0.121		0.118
		(0.212)		(0.182)		(0.212)		(0.182)
Observations	976	976	976	976	1038	1038	1038	1038
Class size control	X	X	X	X	X	X	X	X
Baseline attendance control			X	X			X	X
School Fixed Effects			X	X			X	X
Non-assigned students included					X	X	X	X

This table reports coefficients on the "pooled treatment" and on a measure of class-level treatment intensity (and their interaction) from regressions of the primary outcomes, mean sessions per student (or the attendance rate) and complete absence, on these and several sets of control variables. The "Spillover (Class Treatment Intensity)" variable represents the proportion of *other* children in a given student i's assigned class that are treated (excluding student i), and theoretically ranges from 0 to 1 (none or all of the other children in the class are treated). Children were assigned to classes of between 5 and 25 students prior to random assignment. Children's assignments remain relatively stable post-treatment (approximately 15% of assigned sessions change from initial assignment until the end of the term), although 62 students change their class assignment during the term. In columns 1-4 we exclude these students. In columns 5-8 we include them and use the proportion of students in their "grade group" (the level on which classes are assigned; a higher level) as a best estimate of exposure to treated children. Changes in assignment during the term are not related to treatment status. The "Spillover * Pooled treatment" variable measures the interaction of pooled treatment with the above measure of class-level treatment intensity: this tests differences in the effect of class-level treatment exposure for those assigned to the treatment group compared to those in the control group. All specifications include controls for class size. Columns 3-4 and 7-8 also include control variables for baseline attendance and school fixed effects. Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A9: Spillover Analysis: Weighted Class-Level Treatment Intensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean Sessions per Student								
Pooled treatment	0.075***	0.085	0.070***	0.010	0.070***	0.105	0.069***	0.013
	(0.026)	(0.104)	(0.020)	(0.079)	(0.025)	(0.105)	(0.019)	(0.079)
Spillover (Weighted class treatment intensity)	0.208**	0.217*	0.181***	0.125	0.193**	0.227*	0.174***	0.120
	(0.084)	(0.127)	(0.066)	(0.099)	(0.085)	(0.128)	(0.066)	(0.099)
Spillover * Pooled treatment		-0.017		0.100		-0.060		0.094
		(0.169)		(0.129)		(0.171)		(0.129)
Observations	976	976	976	976	1038	1038	1038	1038
Complete Absence								
Pooled treatment	-0.080***	-0.006	-0.083***	0.027	-0.062**	-0.022	-0.068***	0.033
	(0.027)	(0.111)	(0.023)	(0.095)	(0.027)	(0.111)	(0.023)	(0.095)
Spillover (Weighted class treatment intensity)	-0.099	-0.029	-0.148*	-0.041	-0.074	-0.036	-0.131	-0.035
	(0.090)	(0.135)	(0.080)	(0.118)	(0.090)	(0.136)	(0.080)	(0.119)
Spillover * Pooled treatment		-0.125		-0.184		-0.068		-0.171
		(0.181)		(0.155)		(0.181)		(0.155)
Observations	976	976	976	976	1038	1038	1038	1038
Class size control	X	X	X	X	X	X	X	X
Baseline attendance control			X	X			X	X
School fixed effects			X	X			X	X
Non-assigned students included					X	X	X	X

This table reports coefficients on the "pooled treatment" and on a measure of class-level treatment intensity (and their interaction) from regressions of the primary outcomes, mean sessions per student (or the attendance rate) and complete absence, on these and several sets of control variables. The "Spillover (Weighted class Treatment Intensity)" variable represents the proportion of *other* children in a given student i's assigned class that are treated (excluding student i), weighted by their baseline attendance. As such, it measures the proportion of other children in a child's class that are assigned to treatment (as it does in Table A8), but reweights this depending on the relative baseline attendance of treated vs control group children within the class. This is to account for the plausibly greater exposure to children with higher baseline attendance, and as such plausibly higher spillovers from higher attenders. As in Table A8, children were assigned to classes of between 5 and 25 students prior to random assignment. Children's assignments remain relatively stable post-treatment (approximately 15% of assigned sessions change from initial assignment until the end of the term), although 62 students change their class assignment during the term. In columns 1-4 we exclude these students. In columns 5-8 we include them and use the (baseline attendance weighted) proportion of students in their "grade group" (the level on which classes are assigned; a higher level) as a best estimate of exposure to treated children. Changes in assignment during the term are not related to treatment status. All specifications include controls for class size. Columns 3-4 and 7-8 also include control variables for baseline attendance and school fixed effects. Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A10: Summary Statistics by Treatment for Survey Response

Variable	(1) Survey Non-Response N	Mean/SE	(2) Survey Response N	Mean/SE	T-test P-value (1)-(2)
Treat 1 - Attend info	602	0.30 (0.02)	436	0.30 (0.02)	0.87
Treat 2 - Info + benefit	602	0.30 (0.02)	436	0.31 (0.02)	0.82
Baseline Attendance - %	602	0.49 (0.01)	436	0.52 (0.02)	0.18
Baseline test scores	296	49.16 (1.21)	220	50.96 (1.38)	0.33
Female	602	0.63 (0.02)	436	0.64 (0.02)	0.62
Black	602	0.59 (0.02)	436	0.56 (0.02)	0.24
IsiXhosa speaker	602	0.58 (0.02)	436	0.55 (0.02)	0.42
Afrikaans speaker	602	0.21 (0.02)	436	0.18 (0.02)	0.22
English speaker	602	0.20 (0.02)	436	0.26 (0.02)	0.01**
Highschool student	602	0.40 (0.02)	436	0.38 (0.02)	0.50
Grade 1	602	0.18 (0.02)	436	0.17 (0.02)	0.42
Grade 2	602	0.15 (0.01)	436	0.17 (0.02)	0.33
Grade 3	602	0.13 (0.01)	436	0.11 (0.02)	0.32
Grade 4	602	0.08 (0.01)	436	0.12 (0.02)	0.03**
Grade 5	602	0.06 (0.01)	436	0.06 (0.01)	0.96
Grade 8	602	0.19 (0.02)	436	0.19 (0.02)	0.91
Grade 9	602	0.11 (0.01)	436	0.12 (0.02)	0.69
Grade 10	602	0.10 (0.01)	436	0.07 (0.01)	0.07*
Age	592	11.16 (0.14)	422	11.00 (0.16)	0.47
F-test of joint significance (F-stat)					1.34

Notes: This table shows mean values of baseline characteristics and treatment status for children whose parents responded to the endline survey (N=436) compared to those who did not (N=602). Data are relatively complete with two exceptions: baseline test scores were only collected for 516 children, and age was missing for 24 children. The value displayed for t-tests are p-values. The value displayed for F-tests are the F-statistics. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table A11: Summary Statistics by Treatment for Survey Respondents

Variable	(1) Control N	(1) Control Mean/SE	(2) Treat 1 - Attend info N	(2) Treat 1 - Attend info Mean/SE	(3) Treat 2 - Info + benefits N	(3) Treat 2 - Info + benefits Mean/SE	T-test P-value (1)-(2)	T-test P-value (1)-(3)
Baseline Attendance - %	173	0.50 (0.03)	129	0.55 (0.03)	134	0.51 (0.03)	0.19	0.62
Baseline test scores	86	49.23 (2.31)	68	50.26 (2.43)	66	53.94 (2.43)	0.76	0.17
Female	173	0.65 (0.04)	129	0.63 (0.04)	134	0.65 (0.04)	0.65	0.94
Black	173	0.55 (0.04)	129	0.59 (0.04)	134	0.52 (0.04)	0.55	0.57
IsiXhosa speaker	173	0.55 (0.04)	129	0.58 (0.04)	134	0.52 (0.04)	0.65	0.57
Afrikaans speaker	173	0.18 (0.03)	129	0.18 (0.03)	134	0.19 (0.03)	0.98	0.74
English speaker	173	0.27 (0.03)	129	0.24 (0.04)	134	0.28 (0.04)	0.62	0.73
Highschool student	173	0.38 (0.04)	129	0.41 (0.04)	134	0.34 (0.04)	0.61	0.41
Grade 1	173	0.17 (0.03)	129	0.18 (0.03)	134	0.15 (0.03)	0.81	0.66
Grade 2	173	0.14 (0.03)	129	0.16 (0.03)	134	0.22 (0.04)	0.66	0.10
Grade 3	173	0.12 (0.02)	129	0.12 (0.03)	134	0.10 (0.03)	0.99	0.76
Grade 4	173	0.13 (0.03)	129	0.10 (0.03)	134	0.12 (0.03)	0.48	0.84
Grade 5	173	0.06 (0.02)	129	0.03 (0.02)	134	0.07 (0.02)	0.20	0.70
Grade 8	173	0.18 (0.03)	129	0.21 (0.04)	134	0.18 (0.03)	0.60	0.90
Grade 9	173	0.13 (0.03)	129	0.13 (0.03)	134	0.09 (0.02)	0.98	0.24
Grade 10	173	0.06 (0.02)	129	0.07 (0.02)	134	0.07 (0.02)	0.83	0.90
Age	167	11.17 (0.25)	127	11.10 (0.29)	128	10.69 (0.28)	0.85	0.20
F-test of joint significance (F-stat)							0.42	0.97

This table shows mean values of baseline characteristics for children randomized into treatment condition, conditional on their parents responding to the endline survey (total N=436). Data are relatively complete with two exceptions: baseline test scores were only collected for 220 of these children, and age was missing for 14 children. The value displayed for t-tests are p-values. The value displayed for F-tests are the F-statistics. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. The value displayed for t-tests are p-values.

Table A12: Treatment Effects on Beliefs

	(1) Control Group Mean	(2) Pooled Treatment Mean	(3) Mean Difference	(4) Co-efficient (with controls)
	0.196	0.073	-0.124***	-0.124***
Wrong belief index	(0.323)	(0.205)	(0.028)	(0.028)
	0.095	0.027	-0.068***	-0.068***
Unaware of enrollment	(0.294)	(0.162)	(0.025)	(0.025)
	0.280	0.103	-0.176***	-0.176***
Unaware: attendance last week	(0.450)	(0.305)	(0.040)	(0.040)
	0.235	0.159	-0.075***	-0.075***
Beliefs minus actual (normalized): last week	(0.226)	(0.152)	(0.023)	(0.023)
	0.214	0.088	-0.126***	-0.126***
Unaware: attendance full term	(0.412)	(0.284)	(0.036)	(0.036)
	0.248	0.218	-0.030	-0.030
Beliefs minus (normalized) actual: full term	(0.245)	(0.203)	(0.026)	(0.026)

This table shows group means, mean differences, and regression coefficients specifically for the set of variables capturing parents' beliefs about their children's attendance from the parent survey, N=436. Column 1 shows the control group mean and column 2 shows the pooled treatment group mean. Numbers in parentheses represent standard error of the mean. Column 3 shows the difference between treatment and control group means: ***, **, and * indicate significance in the t-statistic from a T-test between means at the 1, 5, and 10 percent critical level. Column 4 represents the coefficient of the pooled treatment variable in a regression of the outcome relevant to each row on pooled treatment and a vector of control variables (that includes baseline attendance rate, a vector of grade fixed effects, a dummy variable for racial group, and a gender dummy variable). ***, **, and * indicate significance in the treatment coefficient at the 1, 5, and 10 percent critical level. The first row, 'Wrong belief index' represents a normalized index comprized of normalized versions of the other five attendance belief measures. Each of the other five rows represent responses to survey questions in the parent survey and represent respectively i) a dummy variable for not knowing about their child's attendance, ii) a dummy variable for not having any information on their child's attendance in the previous week, iii) difference between beliefs about attendance and actual attendance in the final two weeks, iv) a dummy variable for not having any information on their child's attendance in the full term, and v) difference between beliefs about attendance and actual attendance over the full term.

Table A13: Treatment Effects on Engagement behaviors

	(1) Control Group Mean	(2) Pooled Treatment Mean	(3) Mean Difference	(4) Co-efficient (with controls)
General Education Engagement Behaviors				
Education engagement index	0.775 (0.245)	0.772 (0.236)	-0.002 (0.025)	-0.002 (0.025)
Talked about homework last week	0.812 (0.392)	0.846 (0.362)	0.033 (0.038)	0.033 (0.038)
Talked about future last week	0.538 (0.500)	0.510 (0.501)	-0.028 (0.051)	-0.028 (0.051)
Talked about school last week	0.868 (0.340)	0.896 (0.306)	0.028 (0.033)	0.028 (0.033)
Helped homework: dummy	0.812 (0.392)	0.844 (0.364)	0.032 (0.039)	0.032 (0.039)
Helped homework: frequency (normalized)	0.513 (0.373)	0.547 (0.354)	0.034 (0.037)	0.034 (0.037)
Yebo-specific Engagement Behaviors				
Yebo program engagement index	0.348 (0.293)	0.480 (0.298)	0.132*** (0.032)	0.132*** (0.032)
Talked about Yebo: frequency (normalized)	0.227 (0.250)	0.327 (0.246)	0.100*** (0.025)	0.100*** (0.025)
Talked about what student learnt	0.494 (0.502)	0.593 (0.492)	0.100* (0.051)	0.100* (0.051)
Reminded child to attend	0.380 (0.487)	0.621 (0.486)	0.241*** (0.050)	0.241*** (0.050)
Rewards attendance	0.093 (0.291)	0.154 (0.362)	0.062* (0.032)	0.062* (0.032)
Punishes non-attendance	0.155 (0.363)	0.248 (0.433)	0.093** (0.040)	0.093** (0.040)

This table shows group means, mean differences, and regression coefficients specifically for the set of variables capturing parents' general education and Yebo-specific engagement behaviors from the parent survey, N=436. Column 1 shows the control group mean and column 2 shows the pooled treatment group mean. Numbers in parentheses represent standard error of the mean. Column 3 shows the difference between treatment and control group means: ***, **, and * indicate significance in the t-statistic from a T-test between means at the 1, 5, and 10 percent critical level. Column 4 represents the coefficient of the pooled treatment variable in a regression of the outcome relevant to each row on pooled treatment and a vector of control variables (that includes baseline attendance rate, a vector of grade fixed effects, a dummy variable for racial group, and a gender dummy variable). ***, **, and * indicate significance in the treatment coefficient at the 1, 5, and 10 percent critical level. The top panel includes variables regarding parent engagement towards their children's education generally, and includes a normalized index of all parent-engagement behaviors, and one row for each self-reported behavior. The second panel includes variables regarding parent engagement towards their children's experience in Yebo specifically, and includes a normalized index of these parent-engagement behaviors regarding Yebo, and one row for each self-reported behavior.

Table A14: Treatment Effects on Value of Education

	(1) Control Group Mean	(2) Pooled Treatment Mean	(3) Mean Difference	(4) Co-efficient (with controls)
	0.911	0.918	0.007	0.007
Education value index	(0.097)	(0.112)	(0.011)	(0.011)
	0.828	0.855	0.027	0.027
How much they value YeBo	(0.174)	(0.185)	(0.019)	(0.019)
	0.994	0.979	-0.015	-0.015
Value future studies: dummy	(0.079)	(0.143)	(0.011)	(0.011)

This table shows group means, mean differences, and regression coefficients specifically for the set of variables capturing parents' beliefs about the value of education from the parent survey, N=436. Column 1 shows the control group mean and column 2 shows the pooled treatment group mean. Numbers in parentheses represent standard error of the mean. Column 3 shows the difference between treatment and control group means: ***, **, and * indicate significance in the t-statistic from a T-test between means at the 1, 5, and 10 percent critical level. Column 4 represents the coefficient of the pooled treatment variable in a regression of the outcome relevant to each row on pooled treatment and a vector of control variables (that includes baseline attendance rate, a vector of grade fixed effects, a dummy variable for racial group, and a gender dummy variable). ***, **, and * indicate significance in the treatment coefficient at the 1, 5, and 10 percent critical level. The first row, "Education value index" represents a normalized index comprized of normalized versions of the other two belief measures. Each of the other rows represent responses to survey questions in the parent survey and represent respectively i) a likert-type question regarding how valuable Yebo is ii) a dummy variable for whether or not they see tertiary education as a better investment than immediate paid work.

What Mom and Dad Don't Know, Might Hurt Me: Messaging Parents to Reduce Chronic Absenteeism

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