When the Effects of Informational Interventions Are Driven by Salience – Evidence from School Parents in Brazil[*]

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Abstract

Do informational interventions work because they lead subjects to merely update beliefs in the right direction or, to a large extent, because they increase the salience of the decision they target? We randomly assign parents to either an information group, who receives text messages with weekly data on their child's attendance and school effort, or a salience group, who receives messages that try to redirect their attention without child-specific information. While information has large impacts on attendance, test scores and grade promotion relative to the control group, outcomes in the salience group improve by at least as much.

Keywords: Information; Salience; Inattention

JEL Classifications: C93, D83, D91, I25, I31

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1 Introduction

In 2012, Vitória da Conquista – a municipality in a poor Brazilian State – spent over USD 700,000 on microchips embedded in public school students' uniforms. Their hope was to decrease truancy by informing parents in real-time when their children missed classes.[1] This policy was inspired by the success of informational interventions in affecting many fundamental economic decisions, including those linked to improved educational outcomes.[2] However, it is unclear if these interventions work because of the specific information they convey, tailored to the circumstances of the recipient or because such messages make particular issues salient (or top-of-mind; "when one's attention is differentially directed to one portion on the environment rather than to others, the information contained in that portion will receive disproportionate weighing in subsequent judgments", Taylor and Thompson, 1982, p. 175).[3] If that is the case, then, under certain conditions, *salience interventions*, i.e., those that do not require recipient-specific information, such as *nudges*, may do equally well – and perhaps even better. In fact, it could even be that those interventions induce individuals themselves to collect the relevant data such that they update beliefs in the correct direction, much the same way as informational interventions would do, except that at much lower cost for implementers (no microchips needed!). This paper provides first-hand evidence for this mechanism outside the lab.[4]

Whether such mechanism is at play, as well as its quantitative importance, matter. Salience interventions have the advantage of demanding less or no information. What is more, if, under certain conditions, refocusing attention is the key driver of behavior change, then salience interventions can have *even larger* effects than information disclosure. The reason is two-fold. First, informational interventions are constrained by the frequency at which information is available (often only at low frequency in developing countries like Brazil, the setting of our study) while nudging can be implemented at much higher frequency; in fact, our results show that the frequency of messaging matters greatly in the context of communication

[1] http://www.bbc.com/news/world-latin-america-17484532 (accessed on January 5, 2022).

[2] Information interventions can, for example, improve children's learning outcomes (e.g. Bergman, 2021); increase enrollment and educational attainment (Jensen, 2010); reduce employee turnover (Rockoff et al., 2012); and improve investments in labor market skills (Bursztyn et al., 2018).

[3] Dizon-Ross (2019) shows that pushing information to parents increases investments even when the information was available prior to the experiment (through school report cards).

[4] Gabaix et al. (2006) show that information directs subjects' attention while Ambuehl et al. (2017) shows that abstract information may be better than concrete information in changing financial behaviors even when knowledge is unchanged. citehussam documents that information on hygiene improved health outcomes without necessarily affecting that hygiene knowledge.

with school parents.[5] Second, nudges also allow for additional features to manipulate attention; for instance, our results show that redirecting attention to student attendance in general improves educational outcomes across different classes, in contrast to conveying child-specific information about math attendance – which affects student outcomes only in this particular class.

To study this question in the context of communication between schools and parents, the ideal experiment would evaluate the impacts of sending parents information about their children's attendance while holding their attention fixed. But this is impossible; information disclosure presumably *always attracts attention* (Golman and Loewenstein, 2018; Loewenstein et al., 2014). What we do instead is compare parents who receive information to other parents whose attention is manipulated while their beliefs about their children's behavior are *not*. To do that, we randomly assigned parents to either school messages that contained child-specific information or to school messages that tried to direct their attention to the behaviors reported on – without, however, conveying child-specific information. The idea is that, by comparing the two groups of parents, the experiment allows us to capture the additional effects of information on parent's beliefs and behavior above and beyond those that may operate through the salience mechanism.

Communication between schools and parents is a great setting to study this question for the following reasons: 1) There is a moral hazard problem between parents and children. As children grow older, their goals may drift increasingly apart from those of their future-oriented parents, and it becomes progressively harder for parents to observe children's effort at school (Cunha and Heckman, 2007; Heckman and Mosso, 2014);[6] 2) There are objective dimensions of children's effort (such as attendance) on which we can report or to which we can direct parents' attention; and 3) Administrative data on school outcomes (such as standardized test scores) allow us to track the impacts of the experiment above and beyond surveying parents about their beliefs and behavior.

Across 287 schools in São Paulo, Brazil, encompassing 19,300 ninth graders, math teachers provided weekly information about their students' behavior (attendance, punctuality and homework completion) over the course of 18 weeks. Taking advantage of a partnership with an edtech firm[7], we randomly assigned parents to

[5]Section 7 discusses the possibilities and limitations of this mechanism.

[6]Poor parents in Brazil prefer conditional cash transfers that mandate school attendance to unconditional ones (Bursztyn and Coffman, 2012). Such preference disappears when schools systematically share information about their children's attendance.

[7]Movva (http://movva.tech) delivers nudges to engage parents in their children's education across Latin America and Sub-Saharan Africa. One of the authors (Licahnd) is Movva's co-founder and chairman.

different test messages (SMS) within each classroom. Some parents received child-specific information (e.g., "Nina missed between 3 and 5 math classes over the last 3 weeks"); some received salience messages, emphasizing the importance of paying attention to that behavior (e.g.,: "It is important that Nina attends every math class"); and others received no message at all (the control group). While the salience message potentially conveyed additional information (e.g., on social expectations about parenting), the message with child-specific information presumably *did the same*.[8] Last, because we anticipated that parents' or peer interactions may generate large spillovers, we randomized treatment assignment at two levels: within and across schools, including a pure control group.

Before our experiment, parents were quite inaccurate about their children's school effort. The correlation between beliefs about absences and actual absences in math classes, reported in children's report cards, was only 0.21 (no different across treatment arms). The intervention worked. By the end of the school year, the correlation between beliefs and absences reported by teachers was up to 0.39 in the information group – a 45% increase in accuracy relative to control parents within each classroom. In contrast, in the salience group, parents were no more accurate about their children's absences at the end of the school year. If anything, the correlation between beliefs and absences was 21% lower in that group, consistent with the idea that these messages introduced noise, resulting in a *flatter* relationship between beliefs and actual absences relative to the control group.

We find that the informational intervention had large impacts on attendance (2.1 percentage points, or 2-3 additional classes; a nearly 1/5 reduction in absenteeism), math GPA and standardized test scores (0.09 standard deviation; equivalent to leapfrogging 1 quarter ahead in school), and grade promotion rates (3.2 percentage points; a 1/3 reduction in grade repetition), in line with previous findings (Bergman, 2021; Berlinski et al., 2016; Rogers and Feller, 2016).

Strikingly, most of the effects of information were driven by salience: messages without child-specific information improved outcomes by 89-126% relative to those in the information group. We also show that the effects of salience were even higher than those of information among students with lower attendance at baseline – presumably, those whose parents would benefit the most from information.

How can it be that children improved by as much in both treatment arms if parents only became more accurate about attendance levels in one condition? We document that results are consistent with higher parental monitoring in response to salience effects triggered by *both* interventions. First, we can observe monitoring

[8] See Appendix A.1 for a detailed discussion.

effort directly: treated parents in both conditions ask their children systematically more about school and incentivize studying to a greater extent relative to the control group. As a result, children in all treated households report engaging in academic and reading activities to a greater extent. Second, in both cases, effect sizes were proportional to the severity of parents' misinformation problem. The more optimistic parents were at baseline about their children's attendance relative to the truth, the larger the effect sizes of both child-specific information and salience messages on student attendance. Third, all treated parents became more accurate about *changes* in math GPA over time, relative to the control group, consistent with both interventions mobilizing parents to monitor with higher-intensity.

Results are robust to alternative explanations. First, salience effects did not fade out over time. If anything, effect sizes increased throughout the school year. As such, we can rule out that parents initially reacted to salience messages because they (wrongly) inferred child-specific information from those, resulting in higher monitoring only at first (ultimately leading to better learning outcomes), but no higher accuracy at end line. Second, randomly varying the saturation of parents assigned to the information treatment across different schools, we can rule out that salience effects were driven by spillovers from the informational intervention. Third, finer-grain information (which framed child-specific school effort relative to the classroom median, along the lines of Rogers and Feller, 2016) also had statistically identical effects to those of salience messages within a different sub-sample that included this additional randomly assigned treatment, ruling out that results were driven by child-specific information not being 'informative enough.' Last, assigning a different sample of parents to SMS engagement messages (targeting them directly with weekly suggestions of activities to do with their children, without ever involving teachers) allows us to rule out that results were driven by the fact that, in the main experiment, teachers had to fill in a platform each week with information about their students.

Our findings are consistent with parents setting monitoring effort subject to *attentional constraints*. In an additional experiment, we document that salience effects significantly increased with the frequency of engagement messages (the effect size of 3 messages per week on math attendance was nearly 2-fold that of child-specific information), and that these messages – which were not subject-specific – improved outcomes across *both* math and Portuguese classes, while the effects of math-specific information or math-specific salience messages were confined to attendance and grades in math classes. Incidentally, the additional experiment also helps us rule out that salience effects match those of child-specific information merely because of experimenter demand effects (e.g., because subjects inferred social expectations from salience messages). If that were the case, then we should see no hierarchy of effect sizes based on the frequency of messaging, and definitely not based on whether content was specific to math classes or not.

Together, these results suggest that the effects of communicating with school parents could be obtained at lower cost – and even magnified – by interventions that manipulate attention, raising the salience of the decision they target. While the relevance of the salience mechanism naturally depends on the specific circumstances of the problem – from the structure of beliefs to the monitoring horizon to the scope and the costs of independent information acquisition –, this insight may well extend beyond education. Just like parents who receive information about their child's school effort react to the salience of monitoring, employers may react to the salience of firing low-performing employees in face of information about their performance (as in Rockoff et al., 2012); clients with late payments may react to the salience of enforcement in face of information about how default affects their future access to credit (as in Bursztyn et al., 2019); and customers may react to the salience of purchasing a good in face of information about its benefits (as in Allcott and Taubinsky, 2015).

More broadly, our findings relate to an active literature connecting salience to belief updating (e.g., Bordalo et al., 2012, 2019; Enke et al., 2019; Hanna et al., 2014), extending that logic to the effects of informational interventions themselves. They also qualify the interpretation of previous results about the effects of informational interventions, particularly in the context of communication with parents (Bergman, 2021; Dizon-Ross, 2019; Jensen, 2010). Last, while a recent literature posits that the effects of information can be non-trivial when it redirects attention (Golman et al., 2017; Golman and Loewenstein, 2018; Loewenstein et al., 2014), this paper not only provides first-hand evidence for this mechanism outside the lab, but also shows that it can be quantitatively important.

2 Education in Brazil and São Paulo State

Our experiment focuses on 9th-graders across 287 school in São Paulo State, Brazil. Like most Latin American countries, while Brazil has achieved significant progress over the last 20 years in making basic education universal (over 98% of 7-14 year-olds are enrolled), it still struggles with educational quality.[9] In the 2015 PISA exam,

[9] 2015 National Household Survey (PNAD), Brazilian Institute for Geography and Statistics (IBGE).

Brazilian 15 year-old students scored 121 points below the OECD average in math, equivalent to a *two-year lag* in math skills.

São Paulo is the wealthiest and most populous Brazilian state, and its educational system encompasses the largest number of students in the country. According to the Educational Census from the Brazilian Ministry of Education, enrollment in São Paulo State amounted to 5.3 million primary and middle school students in 2015. Among those, 700,000 were ninth graders, 63% of which served by schools directly administered by the State. Despite being a relatively wealthy state accruing 40% of country's GDP, São Paulo features high inequality in access to education. While wealthy families typically enroll their children in higher-quality private schools, public schools typically serve students from disadvantaged backgrounds. In our sample, over 50% of households earned less than 3 minimum wages (i.e., lived on less than $\sim$ 900 USD/month as of September, 2017), within the income range of slum dwellers in the state capital.

National and international surveys consistently reveal a lack of family engagement in students' school life. While 20% of students in OECD report that they had skipped a day of school or more in the two weeks prior to the PISA test, in Brazil, that figure was 48%. According to the 2015 Brazilian Survey of Students' Health (*Pesquisa Nacional de Saúde do Escolar*, PeNSE), 1 in every 4 parents did not know whether their child skipped classes, 1 in every 3 parents did not systematically ask their child about problems in school, and 1 in every 2 parents did not regularly ask about homework. As in other settings, public school teachers often cite low family engagement as the leading cause of students' poor school performance.

Engaging parents in this setting is hard. Before the pandemic, the leading communication technology between schools and parents was still handwritten notes sent through students themselves, who may not face the right incentives to deliver the messages. Even though basically every parent could be reached via phone, cost control measures by Education Secretariats to prevent excessive spending by schools have made it such that their land lines often carry heavy restrictions on calls to mobile phones.[10] Above and beyond communication constraints, information on students' effort or performance in school is often not readily available to be shared. In most states, no real-time digital information systems are in place to track students' attendance or behavior. Teachers keep daily records on paper, but typically only upload such information into centralized school systems at the end of the school

[10]Less than 30% of Brazilian households own landlines while 93.4% of them own mobile phones, according to the 2015 National Household Survey (PNAD). While mobile phone penetration is high in Brazil, only 45% of active lines are systematically connected to the internet (Regional Study Center to Information Society Development, CETIC).

year.

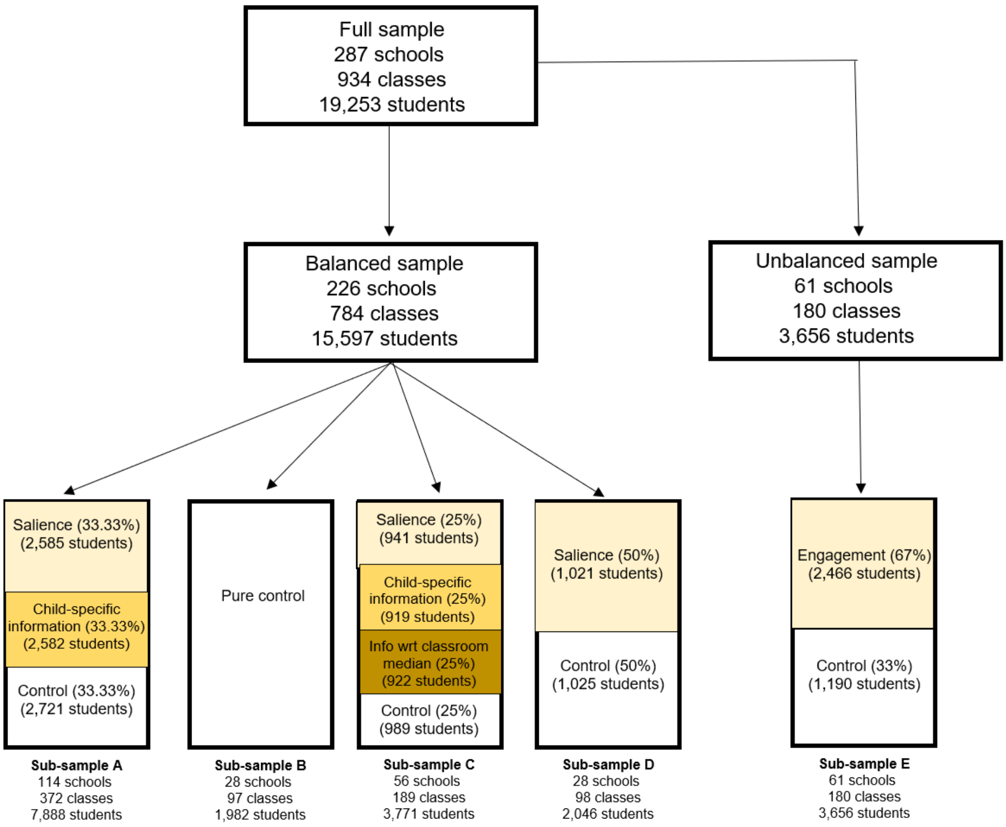
3 Empirical strategy

3.1 Experiment design

See Appendix A.1 in Supplementary Materials for a conceptual framework that informs our experiment design. All details of the design and a preliminary pre-analysis plan were pre-registered at the AEA RCT Registry (AEARCTR-0001379).[11]

Figure 1 summarizes our two-level randomization design. Sub-samples A through C allow estimating the effects of the informational intervention and the extent to which those are driven by salience; sub-sample D allows estimating the effect of salience messages without potential spillovers from information; and sub-sample E allows estimating salience effects in the absence of teacher effects.[12]

Figure 1: Experiment design



[11] See Appendix D for the pre-analysis plan in full, including an account of deviations from pre-registration.

[12] The design choice for sub-samples A through D reflects power calculations accounting for the hypothesis of interest. In the case of sub-sample E, the sample reflects the demands of the Education Secretariat.

The rationale for the two-level randomization and that for the different subsamples outlined in the Figure are discussed in the following sections.

3.1.1 Within-classroom randomization

Across 287 schools in São Paulo, Brazil, encompassing 19,300 ninth graders, math teachers reported weekly information about their students' attendance, tardiness or homework completion (see Appendix A.2 in Supplementary Materials). Within each classroom, we randomly assigned parents to different messages, shared by the platform weekly over text messages (SMS). Some parents received child-specific information; some received salience messages emphasizing the importance of attending to the child's school behavior; and others received no message at all (the control group). The assignment was held fixed over the course of the experiment. Messages were designed to be simple and clear, and to have as close as possible number of characters across treatment arms. Comparing *information* and *salience* students to *control* students allows estimating the extent to which the effects of child-specific information are driven by salience, as discussed in Appendix A.1.

We restricted communication to student effort in math classes. While standardized tests cover both math and Portuguese, the Education Secretariat thought that math teachers tend to keep more accurate records and would have an easier time using the online platform relative to Portuguese teachers. The particular dimension of student effort targeted by information and salience messages alternated weekly, rotating across three dimensions: attendance, punctuality and homework completion. We decided to rotate across those dimensions for three reasons: (1) because teachers already measure them weekly (although only on paper; data entry into the Secretariat's system only takes place at the end each quarter, or even at the end of the school year in some cases), (2) because the Education Secretariat thought it was important to inform parents about all of them (rather than just about attendance), and (3) because we thought it would be less likely that teachers' usage of the platform would die out over time if they had to report on a different behavior every week (making it seem less like just replicating the work they already do on paper). The exact wording of the salience messages varied slightly every cycle, in an attempt to prevent spam-avoiding behavior by parents, and in line with the goals of the mechanism experiment discussed in Appendix A.1. For the full script of messages sent for each treatment arm, see Appendix A.3.

Parents of all treatment arms only received text messages if the teacher filled in the platform that week. This was true even for the salience arm, in order to avoid confounding treatment effects with potential differences in teachers' compliance across conditions.[13] Perfect compliance with randomization protocols was ensured since our implementing partner (Movva) had full control over enrollment (data on all participants had to be entered by teachers into their system prior to the start of the experiment, and assignment was conditional on enrollment) and over the messages ultimately sent to parents.

3.1.2 Identification concerns and two-level randomization

There are a number of potential concerns with inferring salience effects based on experimental design outlined in the previous subsection. First, if parents already had reasonably accurate information about their children's school effort or if parents found the information conveyed through the text messages too coarse to update their prior beliefs, then we might find no treatment effects of information to start with – making it unfeasible to understand the extent to which those effects are driven by salience.

In the context of our experiment, both concerns are unfounded: Section 4 shows that parents are dramatically inaccurate about their children's attendance at baseline and that information makes them substantially more accurate at end line relative to the control group. Having said that, we did not know which would be the case by the time we designed and pre-registered the study. For this reason, we included an additional informational intervention in a randomly assigned sub-sample of schools in which parents were targeted by more informative messages – framing information on student behavior relative to the *median* of their classmates (e.g., "most students in Nina's class missed less than 3 classes in the previous 3 weeks"), analogous to Rogers and Feller (2016). The platform automatically computed each classroom median behavior once teachers submitted information on all their students, each week. Comparing the effects of salience messages to those of relative information allows us to estimate the relevance of salience effects in face of a more demanding counterfactual.

i. **Control:** No messages

ii. **Child-specific information:** Messages with child-specific information about attendance, punctuality and homework completion

iii. **Salience:** Messages highlighting the importance of school attendance, punctuality and homework completion

[13] Teachers had until Sunday of each week to fill in information with respect to the past 3 weeks (see the next subsection); parents received the message assigned to them always on the following Tuesday, according to their treatment status.

iv. **Relative information:** Messages with child-specific information about attendance, punctuality and homework completion framed *relatively* to their classmates' median behavior

Sample messages by treatment arm

Child-specific information	Salience	Relative Information
Eric missed less than 3 math classes over the last 3 weeks.	It is important that Guilherme does not miss math classes without good reason.	Nina missed less than 3 math classes over the last 3 weeks. Most students in her classroom did not miss any math class.

A second concern is spillover effects within classroom. If the interventions causally improve treated students' educational outcomes, a variety of mechanisms could lead control students within each classroom to indirectly benefit from those. In particular, control students' learning outcomes could improve because of their interactions with now higher-effort peers (e.g., Bennett and Bergman, 2021; Sacerdote, 2011), because their parents change behavior due to their interactions with treated parents (who presumably change behavior in response to information or salience messages), or because teachers increase effort in response to higher-achieving pupils (perhaps even specifically towards control students, to ensure they do not fall behind in the classroom). In the presence of spillovers, using within-classroom control students as a counterfactual would lead us to under-estimate treatment effects. Incidentally, spillovers might also lead us to under-estimate differences between the effects of information and salience messages, specifically if parents in the salience group talk to other parents about the messages and infer child-specific information from those conversations *thanks to the information treatment.*

To deal with those concerns, our design randomizes the interventions at two levels. First, we randomize *across schools* varying the specific saturation of the information and salience interventions. Those include a *pure control* group (schools where no student is assigned to either information or salience messages) and a *no information* group (schools where students are only assigned to salience or control). Second, we randomize *within classroom* assigning students across treatment arms en suite with the saturation assignment at the school level.[14] Comparing students assigned to the information and the salience groups to those in the pure control group allows us to estimate treatment effects parsing out potential spillover effects

[14] We stratified randomization at the school level based on their municipality, average Q1 math report card grades, average student attendance, and share of parents who opted into the study. At the student level, we stratified the assignment based on Q1 math standardized test scores. Since this test is not mandatory, we impute missing values based on a linear regression using all baseline covariates.

on control students within each classroom. In turn, comparing students assigned to the salience intervention across the no information group and the regular sample allows us to estimate salience effects while parsing out potential spillover effects from the information treatment on those students. Importantly, other than pure control schools, neither school principals nor teachers were aware of differences in assignment between schools or of child-specific assignments within schools.[15]

While relying on the pure control group as a counterfactual allows estimating the effects of interest while parsing out spillovers, it brings about additional concerns. Teachers in pure control schools did not have to fill in the platform (in order to avoid deception, or poor compliance once teachers eventually realized that information was not being delivered to parents). As such, having to input information into the platform could have induced teachers in the regular sample to change effort relative to pure control schools (e.g., if enough of them think that the school principal or parents might monitor them to a greater extent now that data on student effort is available at high frequency). If that were the case, one would over-estimate treatment effects of both information and salience messages when using the pure control group as a counterfactual – potentially compromising the external validity of our findings, particularly when it comes to the effects of salience messages as one considers the policy version with no data entry by teachers.

To deal with this concern, we include an additional sub-sample of schools in which parents are assigned only to either *engagement messages* or to a control group.[16] Engagement messages – which are also delivered to parents weekly over SMS – do not require any inputs by teachers. Instead, their content draws inspiration in READY4K (York et al., 2019), sharing weekly suggestions of activities for parents to do with their children. Comparing students assigned to engagement messages to those in the pure control group allows us to estimate salience effects parsing out potential effects on teacher behavior due to the teacher platform.

We excluded those schools from the possibility of sending monthly communication to parents (a feature used to convince schools in all other sub-samples to participate in the study; see Appendix A.2) since, in some schools, math teachers also handled this activity (delegated by principals) – as the goal of this sub-sample was to shut down the possibility of teacher effects. That decision, however, had a cost: the Education Secretariat required us to work on a different region of the

[15] Students in pure control schools were enrolled through the same process as those in other sub-samples. Principals of all schools, even in the pure control group, were allowed to use the platform to send monthly communication to parents about school events; see Section A.2.

[16] The saturation within those schools assigned 2/3 of students to messages and 1/3 to the control group, reflecting power calculations that had the subjects assigned to messages involved in multiple comparisons.

State whenever the communication platform was not made available to principals. As a result, students in that sub-sample are not statistically identical to those in our other sub-samples; in particular, schools in that region had relatively lower grades at baseline. To deal with those baseline differences, we take advantage of the fact that our program only started at the second half of the school year, comparing educational outcomes of different sub-samples before and after the program was introduced. The differences-in-differences estimator identifies the causal effects of engagement messages relative to the pure control group under the assumption that student outcomes would have not have changed differentially across sub-samples over time in the absence of the intervention. We show evidence for the validity of this identification strategy in section C.5.3.

3.1.3 Additional experiment

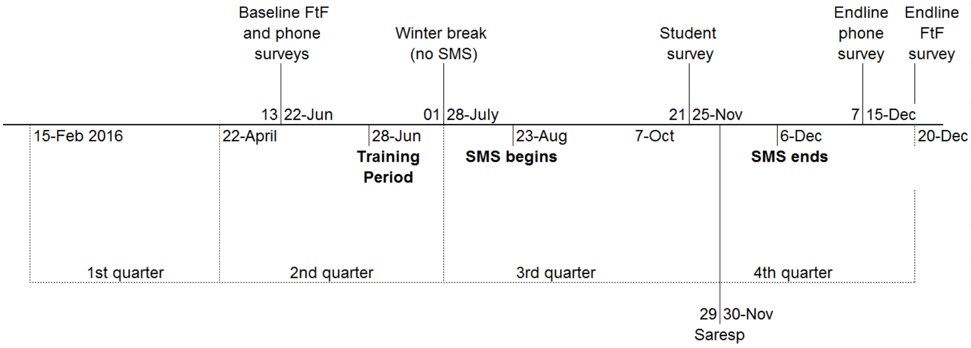
In an additional experiment, we randomized several features of communication with parents within sub-sample E. The experiment cross-randomized (1) the number of messages parents received each week (1, 2 or 3 SMS/week); (2) whether they were also targeted by an additional interactive message each week, asking whether parents undertook that week's suggested activity; (3) the time of the day messages were scheduled for delivery (during work hours or in the evening); and (4) whether messages were always scheduled for exactly the same time, or rotating to a rotating schedule (11am, noon and 1pm, for the former, and 6pm, 7pm and 8pm, for the latter).

Because the frequency of messaging is the feature with most clear predictions for its effects on attention reallocation (including results from previous experiments; e.g., Cortes et al., 2021), we restrict attention to this dimension in the main text. Results for the effects of the other features on learning outcomes are reported in Appendix B.3.

Parents randomly assigned to receive 1 message per week received a weekly SMS with a *suggested activity* for them to do with their child. Those assigned to 2 messages per week also receive a weekly SMS with a *motivating fact* that prequels the activity with some simple foundations for why the latter should support children's learning. Those assigned to 3 messages per week also receive a weekly SMS with a *growth message*, after the activity, incentivizing parents to do it regularly.

Appendix B.3 compiles all details on balance and selective non-response tests for the additional experiment.

Figure 2: Timeline of the experiment



3.1.4 Timeline

The school year in Brazil runs from February to December and is divided in four quarters, with a winter break in July. The timeline of the experiment was as follows. Parents were surveyed at baseline in mid-June, 2016, towards the end of the second quarter. All parents in our sampled schools (including pure control ones) who consented to participate in the study filled in basic characteristics of the child and their primary caregiver, followed by a baseline phone survey. Schools and students were then randomized into the different treatment arms, and teachers were trained to fill in the platform (except in pure control schools and in those where students were targeted by engagement messages) by the end of June.

Teachers started entering student data into the platform immediately after the winter break. In turn, parents started receiving messages 3 weeks after classes resumed since messages in the information group always describe student effort over the 3 previous weeks. Communication lasted until the first week of December, when final exams took place. Standardized tests took place immediately before, at the very end of November. Students were surveyed the week before the test. Parents were surveyed at end line immediately after the end of SMS communication, during the second week of December, 2016.

3.2 Data and outcomes

We rely on four different data sources to collect our outcomes of interest. First, face-to-face survey data. At baseline (mid-June), we surveyed parents who consented to participate in the program on their socio-economic characteristics and those of their children. This dataset comprises 15,597 observations, giving us access to a wide range of controls: primary caregiver's family tie with the student, their income bracket and educational achievement, and their (and their child's) gender, race and age. At end line, we surveyed students face-to-face on their perceptions about their parents' engagement in their school life, their time allocation between leisure and study, their values and aspirations, and their socio-emotional skills. This dataset comprises 9,539 observations, reflecting the fact that take-up of this final survey was optional (although strongly encouraged by schools).

Second, phone surveys, collected via robot calls (interactive voice response units, IVR) and incentivized with airtime credit. At both baseline (mid-June) and end line (December), we surveyed parents to elicit their parenting practices, beliefs about their child's math attendance and grades, and their demand for child-specific information on those dimensions. Response rates of phone surveys were 23.2% at baseline and 25.8% at end line, typical of that mode of data collection. Nevertheless, the sampling pool is large enough that we still have a large number of observations to detect relevant treatment effects on parents' beliefs and behaviors. The exact number of observations varies by outcome variable, since non-response increases throughout the call. As such, phone survey outcomes range from 4,064 to 4,471 observations at baseline, and from 3,868 to 4,974 observations at end line. Non-response was not differential across treatment arms (see Appendix B).

Third, administrative data from São Paulo State's Education Secretariat including quarterly data on students' attendance and GPA in math and Portuguese classes, standardized test scores in math and Portuguese (from the System of School Performance Evaluation of the State of São Paulo, Saresp – a State-wide mandatory exam for 9th-grade public school students), and grade promotion status, across all sub-samples. Attendance is recorded in percentage points (0–100). GPA ranges from 0 to 10 (only integers), with a passing grade set at 5 points for all disciplines (failing one or more disciplines leads to grade repetition). This dataset comprises 22,850 observations. It includes even students whose caregivers did not opt in to participate in the program, allowing us to analyze the extent of selection into the program based on students' characteristics.

Fourth, weekly data on platform scores (rotating across attendance, punctuality and homework completion, all with respect to the 3 previous weeks) for all sub-samples but pure control schools and sub-sample E.[17] This dataset features at least one week of attendance data for 12,641 students, at least one week of punctuality data for 12,208 students, and at least one week of homework completion data for 12,025 students.

Appendix B.1 in Supplementary Materials presents descriptive statistics for our

[17]Teachers' average compliance rate across sub-samples and weeks was roughly 66%; see Appendix C.1.

sample and discusses balance and selective attrition tests.

3.3 Estimation

To estimate the effects of the informational intervention and the extent to which those are driven by salience, we estimate the following equation:

$$Y_{sci} = \alpha + \beta_1 \text{info}_{sci} + \beta_2 \text{salience}_{sci} + \beta_3 \text{control}_{s \notin \{B\},ci} + \sum_{k=1}^{K} \gamma_k X_{scik} + \theta_s + \varepsilon_{sci}, \quad (1)$$

where Y_{sci} denotes the outcome of interest for student i in classroom c of school s; $\text{info}_{sci} = 1$ for students whose parents were assigned to child-specific messages, and 0 otherwise; $\text{salience}_{sci} = 1$ for students whose parents were assigned to salience messages, and 0 otherwise; $\text{control}_{s \notin \{B\},ci} = 1$ for the control group (other than in pure control schools), and 0 otherwise – with pure control schools as the omitted category. Next, X_{sci} is a matrix of student characteristics, including their gender, age and race, their baseline attendance and GPA, and their parents' gender, age, race, income and education; θ_s stands for randomization stratum fixed-effects; and ε_{sci} is the error term. When estimating treatment effects on platform outcomes, we also include classroom fixed-effects.

Coefficients are estimated using Ordinary Least Squares (OLS). We cluster standard errors at the classroom level, allowing for arbitrary correlation among residuals of students under the same teacher. Appendix C.7 documents that all results are robust to allowing standard errors to also be correlated within schools, following the hierarchical structure of our two-level randomization, adapting the recent bootstrapping procedure developed by Abadie et al. (2023). We are interested in testing $\beta_1 = 0$ and $\beta_1 = \beta_2$.

When we estimate treatment effects on the accuracy of parents' beliefs (Section 4.2), we omit the within-classroom control category, as those outcomes cannot be computed for pure control schools (where teachers did not fill in the platform). When it comes to treatment effects on administrative educational outcomes (Section 5.2), we start by estimating equation 1 only within sub-samples A and B. In robustness checks, we augment this specification with an indicator variable for students whose parents were assigned to child-specific information framed relative to their classroom median, when including sub-sample C, and allow the salience coefficient to vary in sub-sample D (where no parent is assigned to child-specific information).

Following our pre-analysis plan, treatment effects on administrative educational outcomes are analyzed individually. In contrast, when it comes to platform scores, measures of parental engagement, and students' socio-emotional skills, we handle family-wise error rates from multiple comparisons by computing standardized summary measures, following Kling et al. (2007).

4 Effects on parents' accuracy about student effort

4.1 Baseline beliefs

Parents' beliefs about student attendance and grades were elicited (non-incentivized) through our baseline phone survey. When it comes to attendance, parents were asked to provide their best estimate of how many times their child had missed math classes over the last 3 weeks. Their answers were then compared to administrative records on students' attendance over the first quarter.[18] Parents had to choose which bracket best approximated their estimate: no absences; 1 to 2 absences; 3 to 5 absences; or more than 5 absences.[19] These brackets were then coded numerically, in ascending order (1-4). Parents were also asked to give their best estimate of their child's 1st-quarter math grade. Again, parents had to choose which bracket best approximated their estimate: below average (0-5); adequate (5-6); good (7-8); or very good (9-10). Here again brackets were then coded numerically, in ascending order (1-4).

Figure 3 showcases the distribution of parents' beliefs at baseline, contrasted with actual student attendance and grades.

[Figure 3]

The figure shows that only about 1/3 of parents choose the correct bracket for either their children's baseline attendance or GPA in math classes. On average, parents are over-optimistic about their children's attendance: similarly to Bergman (2021), most parents think that their child misses fewer classes than they actually do. While over 40% of respondents think their child has missed no classes over the last 3 weeks, this is true for only about 15% of the students. This sets the stage for the typical moral hazard story, whereby information has parents monitor children more intensely (in face of lower monitoring costs), increasing student effort, and ultimately improving learning outcomes as a result. The next subsection provides direct evidence that parents indeed update their beliefs in response to the informational intervention – but not in response to the salience intervention.

[18] We asked about the 3 previous weeks to frame the question consistently with the information respondents would (potentially) receive over text messages.

[19] See Appendix D.4 for the full script.

4.2 Treatment effects on parents' accuracy about effort levels

Throughout this subsection, we restrict attention to parents' accuracy about student attendance in math classes, since the intervention did not convey information about their GPA. We defer the discussion of treatment effects on parents' accuracy about student GPA to Appendix C.5.6, in the context of information-seeking behavior by parents.

Following Dizon-Ross (2019), we compute the slope of the relationship between parents' beliefs and children's actual absences at end line, across the different experimental conditions. We harmonize the scales of parents' beliefs and that of students' actual absences such that, if parents were perfectly accurate, that slope would be equal to 1. At baseline, however, the slope of the relationship between parents' beliefs about their children's absences in math classes over the previous 3 weeks and children's actual absences in that period was only 0.22.[20] What is more, even if parents were accurate at baseline, student effort cannot simply be inferred from children's previous school standing (made available through report cards shared by schools at the end of each quarter): the correlation between 2nd-quarter math GPA and mean platform scores entered by teachers for control students over the course of the 3rd and 4th quarters is only 0.54 for homework completion, 0.32 for attendance and 0.24 for punctuality.

At the end line, we compute that slope using children's actual absences reported by teachers through the platform, since that was the content parents in the information treatment actually received. We ask parents to provide their best estimate of how many times their child had missed math classes over the 4th quarter. As mentioned, parents' beliefs are discretized just as in Bergman (2021); they had to choose which bracket best approximated their estimate: 0; 1-2; 3-5; 6-8; or more than 8 absences. In turn, teachers reported on each student's attendance every 3 weeks, specifying how many classes they missed over that interval (missed 0, 1-2; 3-5; 5 or more). We compute actual absences as the average of teachers' reports over the 4th quarter, such that the exercise compares parents' beliefs to the exact information conveyed through the child-specific information treatment without requiring additional computation.[21] Here also we harmonize the scales of parents' beliefs and

[20] We have access to children's actual absences over the 1st quarter – not over the 3 weeks prior to the baseline survey. Since a school quarter lasts 9 weeks, we divide that figure by 3 to compute our actual absences' indicator. Measurement error due to the coarse categories made available for parents to express their estimates, or due to systematic time trends in absences over that 9-week period, should be statistically identical across treatment arms.

[21] Measurement error due to the coarse categories made available for parents to express their estimates or due to differences in the scales of beliefs and actual absences in this exercise, should be statistically identical across treatment arms. See Appendix C.2 for additional details.

that of students' actual absences such that, if parents were perfectly accurate, that correlation would be equal to 1.

Figure 4 documents end-line slopes within each treatment arm through local polynomial regressions, residualizing beliefs and actual absences with respect to all characteristics of students and their caregivers that we observe.

[Figure 4]

The figure shows that, similarly to the baseline correlation, the end-line relationship between parents' beliefs and children's actual absences remains much flatter than the 45-degree line in the control group. In turn, in the information group, Panel A shows that such relationship is *steeper*: treated parents of low-absenteeism children become more optimistic about their attendance relative to those in the control group, while the opposite is true at the high-end of the absenteeism distribution.

Reassuringly, Panel B documents that the same is not the case for the salience intervention: its local polynomial regression line basically coincides with that of the control group over the entire range of actual absences.

Table 1 documents that those patterns hold in a regression framework. Columns 1 and 3 restrict attention to the information and the within-classroom control group, while columns 2 and 4, to the salience and the within-classroom control group. Columns 1 and 2 document that the slope of the relationship between parents' beliefs and children's actual absences is not statistically different across groups at baseline, and columns 3 and 4 estimate treatment effects of information and salience on that slope, respectively.[22]

[Table 1]

Columns 3 and 4 show that, at the end line, information increases the slope of the relationship between parent's beliefs and children's actual absences from 0.31 to 0.42 (column 3; significant at the 5% level). In turn, salience *decreases* it (by roughly 20% of the control average, in column 4), although the effect is not precisely estimated.

If we focus on end-line accuracy directly, only 19% of parents in the control group chose the right bracket when it comes to their children's absences at the end-line survey, compared to 19.2% of those assigned to salience messages and 21.8% of those assigned to child-specific information – a 15% increase relative to the control group. In sum, the informational intervention did make parents more accurate, while the salience intervention did not.

[22] The number of observations differs across columns 1-2 and 3-4 because of differences in response rates across the baseline and end-line phone surveys; non-response is not systematically different across treatment arms; see Appendix B.

5 Effects on educational outcomes

Having shown that the interventions affect parents' beliefs as intended, this section documents treatment effects on educational outcomes.

5.1 Within classroom treatment effects on platform outcomes

On the first time teachers entered student attendance data on into the platform, only 34.6% of students had not missed any math classes in the 3 previous weeks; in fact, 20.7% of them had missed 5 classes or more over that period. Homework completion was also underwhelming: only 46.2% of students handed in all assignments in the 3 weeks prior to the first time teachers reported on that dimension; 13% had not handed in assignments at all. In turn, tardiness was much less of a problem: roughly 80% of students were never late in any particular week when this dimension was reported on by teachers.

We estimate the effects of child-specific information and salience messages on students' attendance, punctuality and homework completion in math classes, as entered by teachers into the platform. To do that, we stack the data in a panel structure, and estimate the effects of interest absorbing classroom fixed-effects and controlling for a linear time trend (to account for systematic variation in student behavior as the school year draws to an end). To avoid the issue of multiple comparisons in face of the different dimensions of student effort, we also estimate treatment effects on all dimensions of student behavior jointly, stacking all weeks of student data while standardizing each dimension relative to the mean and standard deviation of the control group within each week. Table 2 presents the results in column (1), as well as treatment effects on each dimension of student behavior in columns (2)-(4).

[Table 2]

Column 1 presents treatment effects on the summary measure of student effort. Salience messages improve effort by 0.019 standard deviation (significant at the 5% level). Since effort increases over the course of the school year, this effect size is equivalent to skipping nearly 3 weeks ahead relative to control students. In turn, information does not significantly increase student effort. Analyzing the effects on its components, treatment effects on the summary measure are mostly driven by effects on attendance (column 2), which increases by 0.031 standard deviation in response to salience messages (significant at the 1% level) – an effect size nearly 3-fold that of child-specific information. For the other components (columns 3 and 4), the estimates for information and salience effects are very similar and not statistically different from zero, and we cannot reject that the effect sizes of the interventions are statistically identical in all four columns (at the 10% significance level).

5.2 Treatment effects on administrative outcomes

Next, we estimate the effects of child-specific information and salience messages on students' 4th-quarter attendance in math classes, 4th-quarter math GPA, math standardized test scores, and their likelihood of advancing to high school.[23] Table 3 presents the results.

[Table 3]

Average 4th-quarter attendance in pure control schools is already reasonably high. Students attend 87.5% of math classes in those schools; after all, 75% or higher attendance is a requirement for grade promotion, and 85% or higher, a conditionality for Bolsa Família payments. Nevertheless, information significantly increases it by 2.1 percentage points (significant at the 1% level), an effect size equivalent to two to three additional classes over the course of the experiment. Information also increases math GPA by 0.071 standard deviation (significant at the 5% level), an effect size similar to that of other SMS informational interventions (e.g. Berlinski et al., 2016). While math GPA is computed from tests graded by the teacher herself, child-specific information also significantly increases standardized test scores attributed by third-party graders, to an even greater extent (0.107 standard deviation, significant at the 5% level). This is a large effect size, equivalent to treated students finishing up the school year 1 school quarter ahead of the control group.[24] Those learning gains particularly benefit students on the margin of failing math class, as child-specific information leads to a significant and sizeable increase in the likelihood of advancing to high school: 2.6 percentage points (significant at the 5% level), a nearly 1/3 reduction in grade repetition relative to pure control schools.

Strikingly, we find that salience accounts for most of the informational intervention effects. Not only do salience messages significantly improve all educational outcomes, but also, their effect sizes are always statistically identical to those of child-specific information (at the 10% level). This is not a matter of statistical precision: the ratio of salience coefficients to those of child-specific information is never

[23] The analyses restrict attention to students who were still in school by the end of the year, and who had taken the standardized test, in each case. Supplementary Appendix B documents that our results are not driven by selection.

[24] Based on the average gain SARESP from ninth to tenth grade divided by 4 to compute expected quarterly learning.

below 89%. Moreover, as in the case of platform scores (Section 5.1), salience effect sizes are sometimes larger – up to 126% of those of child-specific information.[25]

Table 3 also shows that the interventions have large spillovers on control students within the classroom: students whose parents were assigned not to receive messages within classrooms where other parents were treated also improve systematically relative to the pure control group, by as much or only slightly less than those assigned to child-specific information or salience messages. Within-classroom spillovers are consistent with the clumpy nature of absenteeism: students often skip classes together (Bennett and Bergman, 2021). While our research design cannot pin down the specific nature of spillovers – potentially a combination of peer effects, parent interactions and teacher effects –, Section 5.3 and Appendix C.6 provide extensive evidence that the absence of differences between the information and salience groups is *not* driven by such second-order effects of the informational intervention.

Appendix C.4.2 compile additional results on distributional impacts and heterogeneous treatment effects. All in all, results suggest that salience interventions can do just as well as child-specific information on average, and even better among students with lower attendance at baseline.

5.3 Robustness checks

5.3.1 Did salience effects fade out over time?

We estimate dynamic treatment effects, taking advantage of the fact that we have quarterly data on math attendance and GPA from the Education Secretariat administrative records. We can test whether those outcomes did not vary systematically across treatment and control parents before the onset of the interventions, and whether treatment effects systematically increase or decrease over time.

This is particularly of interest for the effects of salience messages. Although we show that those messages do not affect parents' accuracy about student attendance at end line, it could still be that parents initially react to salience messages because they (wrongly) infer information from those. Specifically, if parents assigned to the salience intervention think that they are getting messages because their children are putting in low effort at school, they might disproportionately react to the intervention. As time goes by, however, as more and more parents realize that this interpretation is unwarranted, they would eventually stop reacting to salience messages, leading their effects to fade out over time. This alternative story – which is consistent with no differential accuracy at end line *and* better learning outcomes –

[25] Both treatment effects are concentrated on boys. See Appendix C.4.2.

would have important implications for the interpretation and generalizability of our findings. This subsection allows us to test this hypothesis.

Figure 5 displays how math attendance (Panel A) and math GPA (Panel B) vary quarterly within each treatment arm.

[Figure 5]

Panel A displays a downward trend for math attendance over the course of the school year for all groups. The interventions, however, are able to mitigate that trend over the third and fourth quarters. Differences between groups become significant only in the 3rd quarter and persist into the 4th quarter. Most importantly, differences between the salience and pure control groups *increase* over time: salience effect size increases from 1.3 to 2.1 percentage points from one quarter to the next. In Panel B, grades display an upward trend over the second half of the school year for all groups, but particularly so for those targeted by the interventions. As in the case of attendance, differences between the salience and pure control groups *increase* over time: salience effect sizes of increases from 0.092 to 0.104 standard deviations between quarters.[26] In both cases, there is no mechanical reason for effect sizes to increase over time (e.g., compounding), since attendance and GPA figures are computed quarterly. Moreover, the treatment effects of child-specific information also do not fade out over time, lending further support to the equivalence between the information and salience interventions within our experiment.

5.3.2 Are results driven by teacher effects?

As discussed in Section 5.2, spillover effects from the interventions on control students within the classroom are substantial: those students experience almost as large effects on math attendance and GPA, and statistically identical effects on standardized test scores and grade promotion rates. Since we rely on pure control schools as a counterfactual – where teachers did not have to enter data about student effort each week into the platform –, an important concern is whether our results are driven by differences in teacher behavior across different sub-samples. If that were the case, then attributing most of the effects of informational interventions to salience would be misleading, and our findings would not generalize in contexts where teachers are not responsible for high-frequency data entry.

There are two ways to address that concern. The first is to note that, even within classroom, our interventions improve educational outcomes even relative to

[26]The slight difference between the salience effect size on 4th-quarter Math GPA in Table 3 and Figure 5 stems from the diff-in-diff specification in the latter, which nets out baseline differences across the salience and pure control groups.

the within-classroom control group: that is the case for the summary measure of platform scores (significant at the 5% level), and marginally so for 4th quarter math attendance and math GPA (p=0.161 and p=0.137, respectively; see Table 3). The second is to resort to our additional experiment, in which we randomly assigned some students to engagement messages (sub-sample E; see section 3.1.2).

In those schools, we randomly assigned participants to either control or engagement messages, reaching parents directly without child-specific information or the need to involve teachers at all. Engagement messages shared weekly suggestions of activities for parents to do with their children, also through weekly SMS.[27] Messages are not linked to curricular activities; rather, those try to bring parents closer to their children's school life by having them ask about school, discuss future plans, and share how they dealt with similar issues back in the day. Engagement messages are structured around bi-weekly sequences, inspired by READY4K! (York et al., 2019) but carefully adapted to that specific age group and culturally validated by the implementing partner (see Appendix A.3 for sample sequences).

We estimate treatment effects of engagement messages relative to pure control schools, precisely because of spillovers within classroom. The main identification challenge, as discussed in section 3.1.2, is that students within sub-sample E were not statistically identical at baseline to our main sample. The Education Secretariat required us to work in a different region whenever the teacher platform was not made available for skills (for logistical reasons linked to training), and students had relatively lower 1st-quarter math grades in that other region.

Even though we can control for a wide array of students' and parents' characteristics, one may still worry that students of different profiles could have evolved differentially over time due to unobservable factors. To deal with this concern, we take advantage of the fact that our program only occurred during the second half of the school year and use a differences-in-differences strategy to compare the evolution of the different sub-samples, before and after the program was introduced. We restrict attention to math attendance and GPA, for which we have quarterly data.

The differences-in-differences estimator identifies the causal effects of engagement messages under the assumption that educational outcomes *would not have changed differentially* across message schools and pure control schools in the absence of the intervention. While the identification assumption cannot be tested, we can test whether outcomes across those groups varied differentially over the first two quarters – *before the onset of the program.*

We estimate the following equation:

[27] The program, Eduq+, is powered by edtech Movva (`http://movva.tech`).

$$
\begin{aligned}
Y_{scit} = \alpha + \beta_1 \left(\text{engagement}_{sci} \times \text{Post}_t\right) + \beta_2 \left(\text{control}_{s\in E,ci} \times \text{Post}_t\right) \\
+\theta \text{Post}_t + \gamma_1 \text{engagement}_{sci} + \gamma_2 \text{control}_{s\in E,ci} + \varepsilon_{scit},
\end{aligned} \tag{2}
$$

where Y_{scit} denotes the outcome of interest for student i in classroom c at school s on quarter t; $\text{engagement}_{sci} = 1$ for students assigned to engagement messages, and 0 otherwise; $\text{control}_{s\in E,ci} = 1$ for students assigned to the control group within sub-sample E, and 0 otherwise; $\text{Post}_t = 1$ if $t \geq 3$, and 0 otherwise; and ε_{scit} stands for the error term. We are interested in testing $\beta_1 = 0$. We can also investigate within-classroom spillovers in the absence of teacher effects by testing $\beta_2 = 0$.

Table 4 estimates the effects of engagement messages on math attendance (column 1) and math GPA (column 2) using equation 2. Columns 3 and 4 estimate a placebo exercise, restricting attention to the first two quarters – before the onset of the intervention –, and setting $\text{Post}_t = 1$ if $t = 2$, and 0 otherwise, to test for differential pre-trends between the message and pure control groups.

[Table 4]

Columns 1 and 2 show that engagement messages have significant impacts on both outcomes, increasing math attendance by 1.5 p.p. and math GPA by 0.12 s.d. relative to the pure control group (both significant at the 1% level). Strikingly, messages' effect size on learning is 50% larger than that of salience messages alone, suggesting that non-specific and more engaging content can capture parents' attention to a greater extent (see section 6.2). Last, columns 3 and 4 document no differential pre-trends between the engagement messages and pure control groups with respect to either attendance or GPA.

Both panels confirm that differences between the treated and pure control groups arise after the onset of the intervention. They also confirm the patterns we document for dynamic treatment effects of salience messages (section 5.3.1), as the effects of engagement messages do not systematically decay between the 3rd and 4th quarters. Together, our findings rule out that the effects of salience messages are driven by teacher effects.

Appendix C.5 compiles additional results on potential alternative mechanisms underlying treatment effects. In particular, it rules out that effects are driven by child-specific messages not being 'informative enough', or by spillovers from child-specific information to the salience group.

6 Mechanisms

6.1 Increased parental monitoring

Why is it that the interventions improve students' educational outcomes? To study this question, we take advantage of end-line survey data on parents' behavior and aspirations. Students were asked 12 questions about how often (never, almost never, sometimes, almost always, or always) their parents typically engage in different activities. We compute 3 summary measures of parental engagement based on those questions (standardizing their components and averaging across them within summary measure; Kling et al., 2007): *academic activities* (comprising help with homework, help with organizing school materials, participation in school meetings, and conversations with teachers); *motivation* (comprising words of incentives to attend school, to be on time, to study, and to read); and *dialogue* (comprising conversations about homework, about grades, about the day at school, and about classes). We also estimate treatment effects on parents' *aspirations*, an indicator variable equal to 1 if the student states at end line that their parents believe s/he would make it to college, and 0 otherwise. Table 5 presents treatment effects on the summary measures of parental engagement and aspirations.

[Table 5]

Both child-specific information and salience messages lead parents to ask their children significantly more about school and to incentivize studying to a greater extent than those in the control group. Incidentally, both also significantly induce higher aspirations about their children's making it to college. Across all columns, the effects of information and salience are statistically indistinguishable at conventional significance levels. Appendix C.5.1 compiles additional results for treatment effects on students' time use, which further corroborate the previous findings.

6.2 Attentional constraints

This subsection provides direct evidence of parents' attentional constraints. To do that, we turn to the additional experiment (see section 3.1.3). Specifically, we leverage variation in key features of SMS communication, namely (1) the specificity of the content of messages sent to parents (framed around math classes in the main experiment, but not in the additional experiment), and (2) the frequency of messaging (randomly assigned in the additional experiment).

First, we study the extent to which math-specific information and salience messages have effects confined to learning outcomes in that class. Table 6 estimates treatment effects on students' 4th-quarter Portuguese attendance, GPA and standardized test scores (columns 1-3).[28]

[Table 6]

While estimated treatment effects of both interventions are positive across all columns, effect sizes are much smaller and less precisely estimated than their counterparts for math classes. In particular, the effect of both child-specific information and salience messages on Portuguese standardized tests scores is *less than half* their effects on math standardized test scores.

This is not merely because it is harder to improve learning outcomes in Portuguese classes relative to math classes, as the evidence on the effects of engagement messages illustrates next. Figure 6 documents treatment effects of engagement messages on math and Portuguese 4th-quarter attendance (Panel A) and GPA (Panel B), using a differences-in-differences estimator (as in subsection C.5.3). In each panel, we also highlight the effect sizes of salience messages in the main experiment, in the left-hand side, also estimated through differences-in-differences for comparability.

[Figure 6]

We start by restricting attention to the first two sets of columns in both panels, which portray effect sizes of one message per week – varying only whether content was math-specific or not. Panel A shows that while math-specific salience messages increased attendance significantly in math classes but not in Portuguese (p-value of the difference = 0.00), non-specific engagement messages significantly increased attendance in both classes (p-value of the difference = 0.64). Panel B shows that, similarly, although math-specific salience messages also increased GPA in Portuguese classes, that effect size was significantly smaller than that for math GPA (p-value of the difference = 0.07); in contrast, non-specific engagement messages increased math and Portuguese GPA by the same extent (p-value of the difference = 0.22).

Next, we study the extent to which effect sizes increase with the frequency of messaging.[29] Figure 6 displays effect sizes of 1, 2 and 3 engagement messages per week on students' math and Portuguese 4th-quarter attendance (Panel A) and GPA (Panel B). It documents that effect sizes substantially increase with the frequency of engagement messages, for both math and Portuguese. With 3 engagement messages per week, math attendance increases by 2.5 p.p., 56% more than with 1 engagement

[28] As grade promotion depends on grades being greater than or equal to 5 *across all subjects*, we cannot assess the extent of spillovers within student for that outcome.

[29] Appendix B.3 compiles balance tests for the additional experiment. It also documents treatment effects of other communication features cross-randomized in the additional experiment.

message per week (p-value of the difference = 0.00). The same pattern holds for math GPA: the effect size is 24% larger for the latter (the difference is less precisely estimated in that case; p = 0.27). When it comes to Portuguese attendance and grades: with 3 engagement messages per week, the treatment effect of engagement messages on Portuguese GPA (0.14 standard deviation, significant at the 1% level) is nearly two-fold that of 1 engagement message per week.

Interestingly, from 2 to 3 engagement messages per week, there are already some signs of saturation effects, matching findings from the literature (e.g., Cortes et al., 2021). In particular, the effect sizes of 2 and 3 engagement messages per week are nearly identical when it comes to math attendance and math GPA. Saturation is also consistent with limited attention, as additional messages are not expected to draw additional attention when the decision domain is already top-of-mind.

These results help us rule out that salience effects match those of child-specific information because of experimenter demand. If that were the case, then we should have found no hierarchy of effect sizes based on the frequency of messaging and definitely not based on whether content was specific to math classes or not.

Appendix C.5 compiles additional results on the mechanisms underlying treatment effects. In particular, it documents that treatment effects of both child-specific information and salience messages are proportional to the severity of parents' misinformation problem, and that parents in both groups independently acquired information in response to the intervention.

Together, results not only demonstrate that attentional constraints are a key driver for how parents set monitoring effort, but also, corroborate the claim that engagement messages can magnify the effects of informational interventions by drawing on additional features to manipulate subjects' attention.

7 Concluding Remarks

While interventions that inform parents about their children's school effort tend to have large impacts on educational outcomes, we showed that alternative interventions that draw parents' attention to student effort *without making them more accurate* can improve learning outcomes by just as much. We found that salience effects were not short-lived, and they were not driven by interactions with information or by teacher effects. We also documented that effect sizes increased with the frequency of messaging, and that the intervention systematically affected learning outcomes across multiple classes only when content was not specific to math classes. All in all, our findings are consistent with parents setting monitoring effort subject to attentional constraints.

Our findings contribute to a booming literature that investigates cost-effective interventions to improve educational outcomes in developing countries.[30] While different strategies have been rigorously evaluated – from cash transfers (Baird et al., 2011; Barrera-Osorio et al., 2011; Behrman et al., 2009; Mo et al., 2013; Schultz, 2004) to scholarships (Blimpo, 2014; Friedman et al., 2011; Kremer et al., 2009; Li et al., 2014) to increasing the quantity and quality of teachers (Chin, 2005; Duflo et al., 2015; Urquiola, 2006; Urquiola and Verhoogen, 2009) and school grants (Das et al., 2013; Lucas and Mbiti, 2014; Newman et al., 2002; Pop-Eleches and Urquiola, 2013; Pridmore and Jere, 2011) –, only a few have been shown to improve student outcomes through easily scalable interventions. We provide direct evidence that interventions that manipulate attention can induce at least as large effects as informational interventions, and at a lower cost. The effects of more frequent messaging are up to 2-fold those of weekly messages with child-specific information, and engagement messages (especially at higher frequency) have significant impacts across both math and Portuguese learning outcomes, while the effects of math-specific messages are mostly confined to that subject.

In the context of the growing body of evidence that suggests that parents play a crucial role in shaping their children's behavior and school performance (e.g. Barnard, 2004; Houtenville and Conway, 2008; Nye et al., 2006), our results qualify the findings of experimental evaluations of school communication with parents (e.g. Bergman, 2019, 2021; Bergman and Chan, 2019; Berlinski et al., 2016; Castleman and Page, 2015; Dizon-Ross, 2019; Gallego et al., 2018; Jensen, 2010; Kraft and Dougherty, 2013; Rogers and Feller, 2016; York et al., 2019), suggesting that most of their effects could actually be driven by salience.More broadly, our findings suggest that the effects of informational interventions across a multiplicity of domains could be obtained at lower cost – and even magnified – by interventions that manipulate attention, raising the salience of the decision they target.

Having said that, in practice, not all informational interventions might be replicated simply with messages that try to make the decision domain top-of-mind. For information concerning *fixed* states of the world, which require no updating, or for information that is easily available, understanding the extent to which the effects of informational interventions are driven by salience becomes less relevant. Alternatively, for decisions that must be taken immediately after subjects have been informed, or for information that might be prohibitively costly for individuals to

[30]Students in developing countries learn much less than students of the same age or grade in OECD countries (Ludger et al. (2015))

acquire independently (e.g., major-specific returns to college education, as in (Hastings et al., 2015)), salience might not be very effective in changing behavior, as individuals might not have the chance to change monitoring effort, or might not come by the decision-relevant information even if they search for it.

Moreover, manipulating attention is not trivial. Yeager et al. (2019) shows that an online growth mindset intervention in US high schools improves math grades relative to a comparable online intervention that does not address beliefs about intelligence. Bursztyn et al. (2019) shows that while text messages sent by an Indonesian bank appealing to moral values significantly reduce default, other messages from the bank (even those mentioning payment reminders) do not. In those studies, even though belief updating is relevant and information could be acquired independently by participants, messages that do not address educational beliefs (in the former) or the moral implications of default (in the latter) might have failed to capture subjects' attention to the same extent as other interventions. As in any policy evaluation, the devil is the details; ultimately, designing effective and scalable interventions requires careful piloting and evaluation.

Last, when the effects of informational interventions are driven by salience, what can we conclude about their welfare implications? The answer ultimately depends on how salience affects the underlying decision process: if it expands consideration sets (such that decision-makers attend to a broader set of alternatives), it must weakly increase welfare; in contrast, if it leads to early stopping in a sequential sampling model subject to satisficing (such that decision-makers might end up with a lower-value alternative), it could lead to lower welfare (Benkert and Netzer, 2018). While it seems hard to make the case that improving educational outcomes could make children or parents worse off, in other domains, there might be no guarantee that behavior change triggered by informational interventions actually improves welfare – a point conceptually made in Loewenstein et al. (2014) and intimately connected to the ambiguity of the welfare effects of nudges pointed out by Benkert and Netzer (2018). Additional research is needed to understand how salience affects the underlying decision process and, ultimately, welfare in each case.

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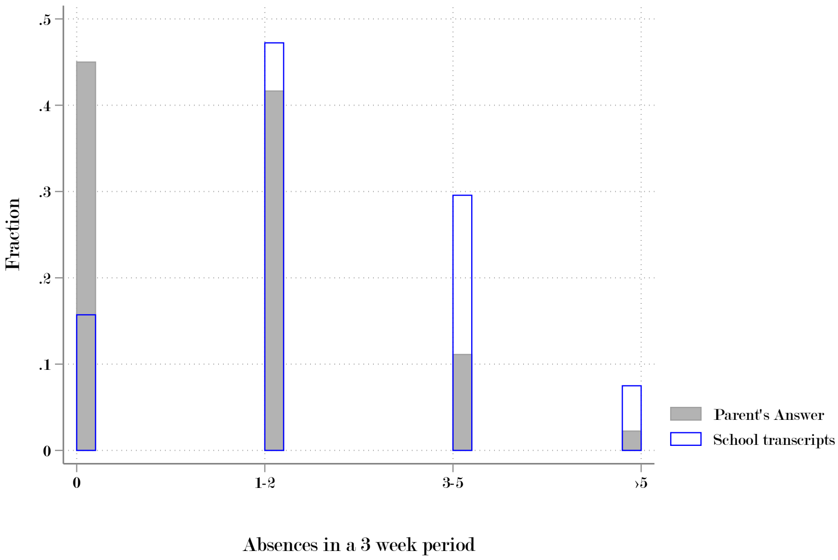
Figures

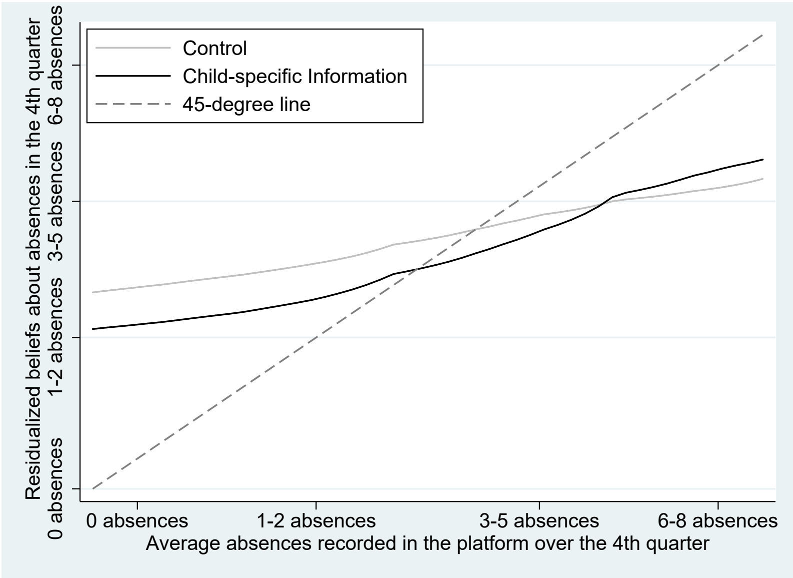
Figure 3: Parents' accuracy wrt their child's baseline math attendance



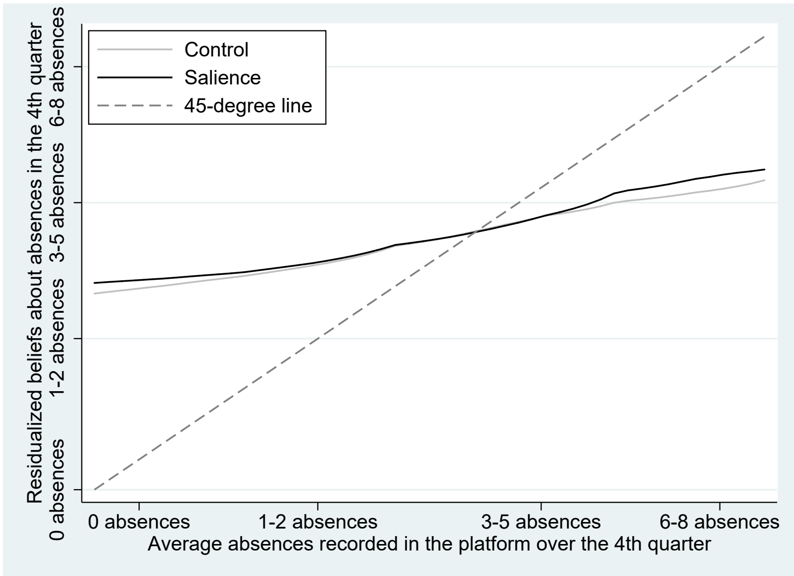
Note: This Figure displays the distribution of parents' beliefs about their child's attendance and recorded attendance. Parents were asked at baseline to give their best estimate on how many times their child missed math classes on a period of three weeks. Data was then crossed with administrative records. Four categories were available for parents' answers on attendance (missed 0; 1-2; 3-5; more than 5). Administrative data tracks student attendance on a quarterly basis (every 9 weeks). We divided Q2 attendance by 3 to compute parents' baseline accuracy gap.

Figure 4: Parents' beliefs vs. actual student absenteeism (platform data)

Panel A: Information vs. Control



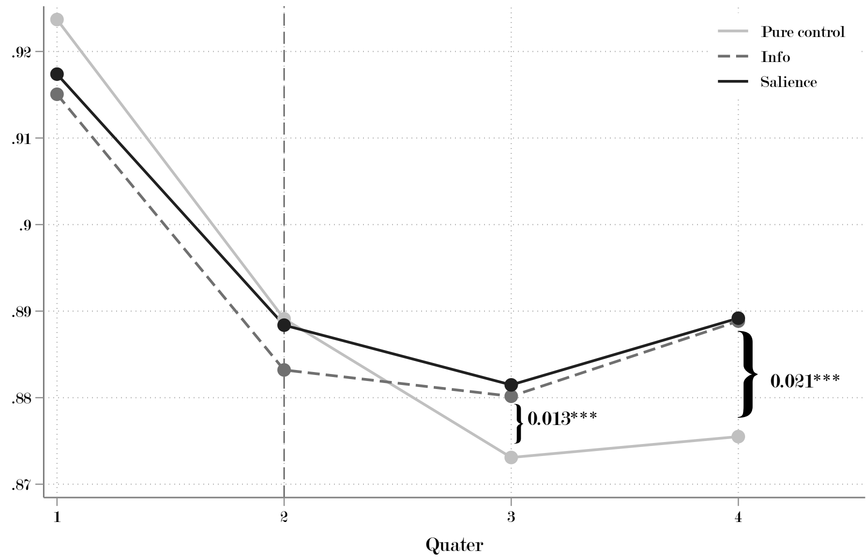
Panel B: Salience vs. Control



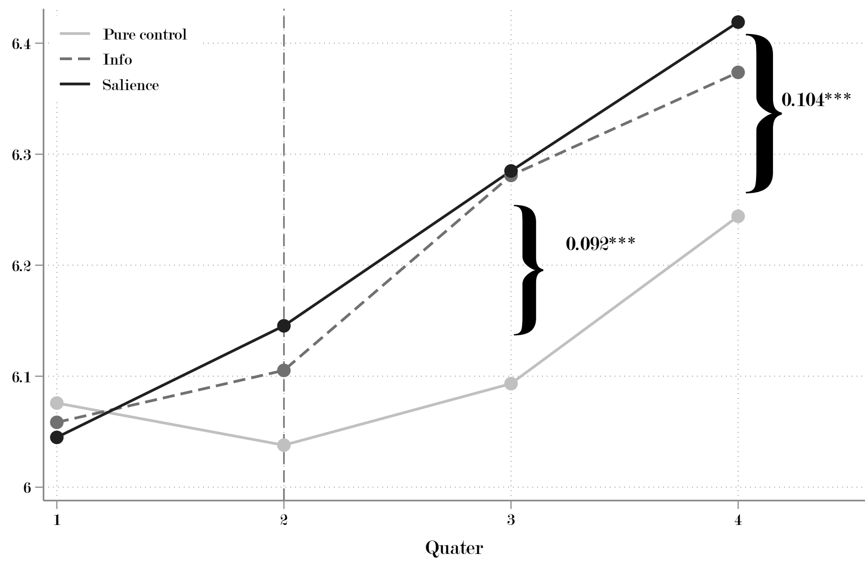
Note: Non-parametric relationship between parents' end-line beliefs about their child's absences in math classes and actual absences. At the end-line survey, parents were asked to provide their best estimate of how many times their child missed math classes over the past quarter. Parents could pick an answer from five categories (0 absences; 1-2; 3-5; 6-8; or more than 8). In this figure, we compute actual absences from the data entered by teachers into the platform ' since this is the data parents in the information group are targeted with. Teachers reported information on each student's attendance every 3 weeks, specifying how many classes they missed over that interval (missed 0, 1-2; 3-5; 5 or more). Both panels show parents' beliefs on the Y-axis, and the average of teachers' reports over the 4th quarter on the X-axis. Panel A plots a local polynomial regression for that relationship within the information group and the control group. Panel B plots a local polynomial regression for that relationship within the salience group and the control group. Both local polynomial regressions use a bandwidth of 0.6. We restrict the X-axis to the [0,3] interval in both panels since very few observations are in the (3,4] range, rendering non-parametric estimates infeasible.

Figure 5: Are effects short-lived? Salience and information effects over time

Panel A: Effect on attendance over time



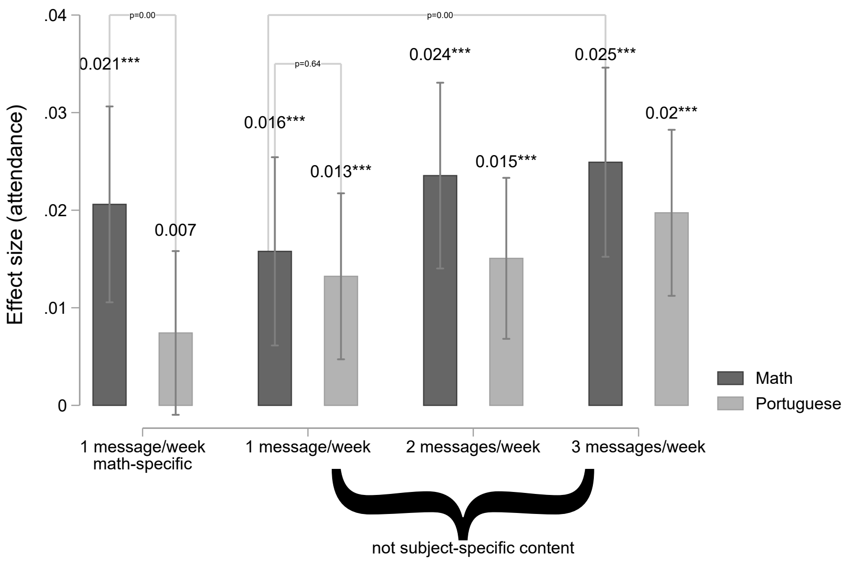
Panel B: Effect on GPA over time



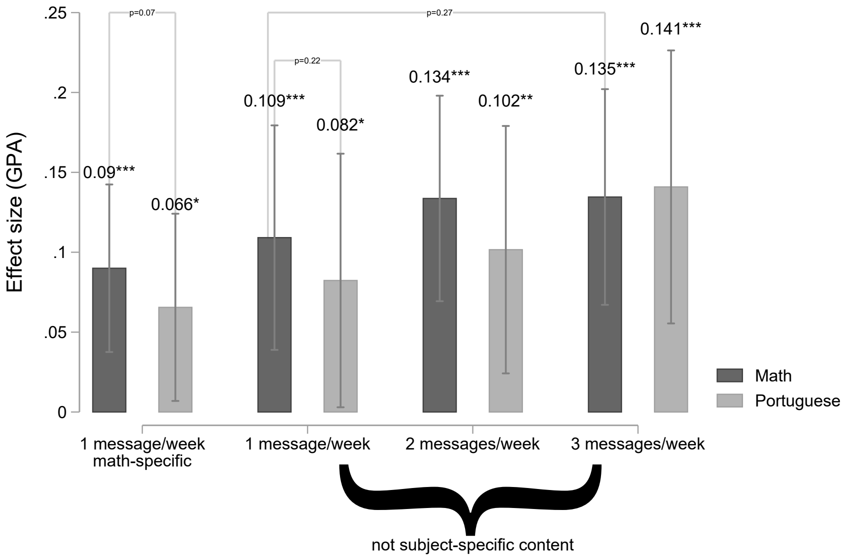
Note: Panels A and B show the attendance and GPA averages for the pre- and post-intervention period, for treatment and control groups. Attendance is recorded in percentage points (0-1 interval). GPA is in a 0-10 scale (integer increments), with 5 as the passing grade. The intervention started at the beginning of the third quarter (as shown by the vertical dashed line) and lasted until the end of the fourth quarter. Attendance and GPA are available for each of the four quarters, as part of students' transcripts. Braces highlight the differences between the salience and pure control groups at each quarter, from a model estimated with student controls, strata fixed effect and standard errors clustered at the classroom level, as specified by equation 1. Student controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. Coefficients for GPA are in standard deviations, where GPA was normalized relative to the distribution of the comparison group (pure control). Significance levels are denoted by * if p<0.1, ** p<0.05 and *** if p<0.01.

Figure 6: Effect sizes as a function of the number of engagement messages per week

Panel A: Effect on attendance



Panel B: Effect on GPA



Note: Treatment effects of salience messages (first set of columns on the left-hand side in each panel) and of engagement messages (1, 2 or 3 messages per week, respectively, for the remaining sets of columns) on fourth-quarter attendance in math and Portuguese classes (Panel A) and fourth-quarter math and Portuguese GPA (Panel B). All effect sizes estimated through differences-in-differences, with the first quarter as reference period. GPA was normalized relative to the distribution of the comparison group (pure control). Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. All estimates are from OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Tables

Table 1: Parents' accuracy about attendance levels

	(1)	(2)	(3)	(4)
	Beliefs at baseline		Beliefs at end line	
Actual absences	0.211***	0.186***	0.271***	0.267***
	[0.025]	[0.024]	[0.043]	[0.043]
Child-specific information	-0.016		-0.273***	
	[0.050]		[0.060]	
Salience		0.033		0.063
		[0.050]		[0.064]
Actual absences x Information	-0.011		0.121**	
	[0.035]		[0.057]	
Actual absences x Salience		-0.047		-0.055
		[0.034]		[0.058]
Control mean	1.42	1.38	1.46	1.40
Observations	3,085	3,174	2,136	2,032
Classroom FE	No	No	No	No
Student-level controls	Yes	Yes	Yes	Yes
R-squared	0.112	0.120	0.167	0.126

Note: Correlation between parents' baseline and end-line beliefs about their children's school attendance and actual attendance within each period. At the baseline survey, parents were asked to provide their best estimate of how many times their child had missed math classes over the past three weeks, choosing among four brackets: 0 absences; 1-2; 3-5; or more than 5. Since administrative data on students' 1st-quarter absences were only available for the whole quarter ($\sim$ 9 weeks), in columns (1) and (2) actual absences are computed by dividing that indicator by 3. At the end-line survey, parents were asked to provide their best estimate of how many times their child missed math classes over the past quarter, choosing among five brackets: 0 absences; 1-2; 3-5; 6-8; or more than 8. We compute actual absences from the data entered by teachers into the platform, since this is the data parents in the information group are targeted with. Teachers reported on each student's attendance every 3 weeks, specifying how many classes they missed over that interval (missed 0, 1-2; 3-5; 5 or more). In columns (3) and (4), actual absences are the average of teachers' reports over the 4th quarter. Columns (1) and (3) include only students in the child-specific information and the within-class control group. Columns (2) and (4) include only students in the salience and the within-class control group. Regressions include indicator variables for students in the information and salience groups and an interaction term between actual absences and the indicator for child-specific information (Columns 1 and 3) or actual absences and the indicator for salience messages (Columns 2 and 4). Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. This Table includes all students in the balanced sample, samples A, B, C, and D (See Figure 1). All columns are OLS regressions, with standard errors clustered at the classroom level. * if $p<0.1$, ** $p<0.05$ and *** if $p<0.01$.

Table 2: Effects on platform scores, within classroom

	(1)	(2)	(3)	(4)
		Specific dimensions		
	All dimensions	Attendance	Punctuality	Homework
Child-specific information	0.0108	0.0115	0.000930	0.0199
	[0.00953]	[0.0126]	[0.0123]	[0.0131]
Salience	0.0189**	0.0312***	0.00883	0.0163
	[0.00895]	[0.0118]	[0.0117]	[0.0122]
Linear time trend (weeks)	0.00703***	0.0163***	-0.00386***	0.0103***
	[0.000472]	[0.000742]	[0.000708]	[0.000679]
p-value diff. [Info] -[Salience]	0.408	0.125	0.536	0.784
Observations	158,018	53,453	52,389	52,176
Classroom FE	Yes	Yes	Yes	Yes
Student-level controls	Yes	Yes	Yes	Yes
R-squared	0.231	0.274	0.359	0.381

Note: Treatment effects of child-specific information and salience messages on different students' outcomes recorded by the teachers. The sample includes students that opted-in the study and whose parents answered the survey. Observations for students are stacked at the week-level. In columns (2) to (4), the dependent variables are: number of classes attended in the week (column 2), number classes attended on time (punctuality, column 3), and the number of completed homeworks (column 4). Each of those variables are normalized relative to the distribution of the comparison group (pure control). In column 1, we calculate a summary measure by averaging across the other three components, following Kling et al. (2007). Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for classroom fixed-effects and a linear weekly time trend. This Table includes all students in the balanced sample, samples A, B, C, and D (see Figure 1). All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table 3: Effects on attendance, grades and grade promotion

	(1)	(2)	(3)	(4)
	Math Attendance (p.p.)	Math GPA (std.)	Promotion Rate (p.p.)	Math Standardized Test (std.)
Child-specific information	0.021***	0.071**	0.026**	0.107**
	[0.006]	[0.032]	[0.012]	[0.047]
Salience	0.021***	0.090***	0.032***	0.095**
	[0.006]	[0.032]	[0.012]	[0.047]
Control within classroom	0.018***	0.070**	0.030**	0.085*
	[0.006]	[0.031]	[0.012]	[0.047]
Control mean	0.875	0.000	0.938	-0.000
p-value diff. [Info] -[Salience]	0.896	0.221	0.219	0.596
Observations	12,577	12,577	12,577	12,577
Randomization strata FE	Yes	Yes	Yes	Yes
Student-level controls	Yes	Yes	Yes	Yes
R-squared	0.21	0.62	0.10	0.34

Note: Treatment effects of child-specific information and salience messages on the following administrative outcomes: 4th-quarter attendance in math classes (Column 1); 4th-quarter math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3), and math standardized test scores (Column 4). GPA and standardized test were normalized relative to the distribution of the pure control group. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects. All columns are OLS regressions, with standard errors clustered at the classroom level. This Table includes all students in the balanced sample, samples A, B, C, and D (see Figure 1) * if p<0.1, ** p<0.05 and *** if p<0.01.

Table 4: Treatment effects of engagement messages: Differences-in-differences

	(1)	(2)	(3)	(4)
	All quarters		Placebo (first 2 quarters)	
	Math Attendance (p.p.)	Math GPA (std.)	Math Attendance (p.p.)	Math GPA (std.)
Messages x Post	0.0151***	0.116***	0.00842	0.0312
	[0.00591]	[0.0427]	[0.00735]	[0.0600]
Messages	0.00621	-0.162***	0.00130	-0.180***
	[0.00604]	[0.0610]	[0.00635]	[0.0673]
Post	-0.0333***	0.00420	-0.0389***	-0.0206
	[0.00416]	[0.0252]	[0.00499]	[0.0388]
Observations	14,775	14,586	7,347	7,376
Student-level controls	Yes	Yes	Yes	Yes
R-squared	0.072	0.060	0.074	0.062

Note: Treatment effects of engagement messages on 4th-quarter attendance in math classes (Columns 1) and 4th-quarter math GPA (Column 2). GPA was normalized relative to the distribution of the pure control group. The sample includes sub-sample E and the pure control group (we exclude parents assigned to 2 or 3 engagement messages per week). Observations are stacked (student x school quarter). All estimates use the 1st quarter as the period of reference. Regressions include interactions between a post-treatment time dummy and treated students, and between the post-treatment dummy and within-classroom control group dummy (the pure control is the reference group). In Columns (1) and (2), we estimate treatment effects using all four available quarters. In Columns (3) and (4), we estimate placebos using only the first two available quarters. All estimates use the first quarter as period of reference. Regressions include interactions between a post-treatment time dummy and treated students, and between the post-treatment dummy and within-classroom control group dummy (the pure control is the reference group). We also include in the regression indicator variables for the post-treatment period and for the treatment and within-classroom control groups, and student-level controls. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. This Table includes all students in sample E (see Figure 1). All columns are OLS regressions, with standard errors clustered at the classroom level. * if $p<0.1$, ** $p<0.05$ and *** if $p<0.01$.

Table 5: Effects on parental engagement

	(1) Academic activities	(2) Motivation	(3) Dialogue	(4) Aspirations
Child-specific information	0.092*	0.075*	0.147***	0.092**
	[0.051]	[0.042]	[0.044]	[0.036]
Salience	0.064	0.096**	0.122***	0.095***
	[0.050]	[0.041]	[0.043]	[0.036]
p-value diff. [Info] -[Salience]	0.263	0.382	0.374	0.891
Observations	9539	9539	9539	9539
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes

Note: Treatment effects of child-specific information and salience messages on parental engagement. Variables are based on students' end-line survey. They were asked to state how often their parents engage in certain activities (never, almost never, sometimes, almost always, always). Out of the 12 questions, factor analysis was performed to create 3 variables of parental behavior: academic activities (help with homework, help to organize school material, participate in school-parent meetings, talk to the teachers); motivation (incentivize to not miss school, to not be late, to study and to read); dialogue (ask about homework, ask about grades, ask about day in school and classes). We also created a dummy variable for parents' aspirations that indicates whether students answered that their parents expect them to go to college or not. Variables were normalized relative to the distribution of the comparison group (pure control). Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. This Table includes all students in the balanced sample, samples A, B, C, and D (see Figure 1) * if p<0.1, ** p<0.05 and *** if p<0.01.

Table 6: Treatment effects on Portuguese outcomes

	(1) Portuguese Attendance (p.p.)	(2) Portuguese GPA (std.)	(3) Portuguese Standardized Test (std.)
Child-specific information	0.007	0.053	0.047
	[0.005]	[0.036]	[0.043]
Salience	0.007	0.066*	0.032
	[0.005]	[0.036]	[0.043]
Observations	12577	12577	12577
Randomization strata FE	Yes	Yes	Yes
Student controls	Yes	Yes	Yes

Note: Treatment effects of child-specific information and salience messages on the parents' accuracy and students' educational outcomes in Portuguese classes. Dependent variables are 4th-quarter Portuguese attendance (Column 1), Portuguese GPA (Column 2), and Portuguese standardized test scores (Column 3). GPA and standardized test scores were normalized relative to the distribution of the pure control group. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. This Table includes all students in the balanced sample, samples A, B, C, and D (see Figure 1). * if p<0.1, ** p<0.05 and *** if p<0.01.

Online Appendix

When the Effects of Informational Interventions Are Driven by Salience – Evidence from School Parents in Brazil

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Supplementary Materials are organized as follows:

A Additional details on the experiment design

A.1 Conceptual Framework

The ideal experiment to study this research question would compare parents who receive child-specific information to other parents whose beliefs about their children's school effort are manipulated *while their attention is held fixed.* Such experiment is, however, not feasible; information disclosure presumably *always attracts attention* (Golman and Loewenstein, 2018; Loewenstein et al., 2014). Hence, to study this question, what we do instead is compare parents who receive information to other parents whose *attention is manipulated* while their *beliefs about their children's behavior are held fixed.* This alternative comparison approximates the ideal experiment by isolating the mechanism of interest, along the lines of Ludwig et al. (2011).

How could one implement this mechanism experiment outside the lab? What we do in our context is to randomly assign parents to either school messages that contain child-specific information or to school messages that try to direct their attention to the behaviors reported on – without, however, conveying child-specific information. The idea is that, by comparing the two groups of parents, the experiment allows us to capture the additional effects of information on parents' beliefs and behavior *above and beyond those that operate through the salience mechanism* (if any).

Concretely, salience messages emphasize that the dimensions of student effort we weekly report on in the information group (attendance, punctuality and homework completion) are important (e.g., "It is important that Nina attends math classes / arrives on time in math classes / hands in math homework everyday"). We match the school behavior addressed by the salience and information messages every week.

Framing salience messages in this way might raise concerns, in that claiming that a behavior is important might change preferences or beliefs above and beyond making that dimension top-of-mind. The reason why we think this is the appropriate framing is three-fold. First, informational interventions presumably do the exact same thing: being targeted by a message from the school likely makes recipients regard this dimension as important – potentially affecting their preferences and beliefs just as much. In our experiment, we can test directly if parents' beliefs are affected by the salience intervention; in particular, do salience messages lead parents to infer that their children are putting in low effort at school? Section 4 shows that is not the case: salience messages do not systematically affect the slope of the relationship between parents' beliefs and student attendance at end line.

Second, alternative framings would only imperfectly approximate those salience effects. For instance, a reminder (e.g., "You can learn about your children's attendance by asking their school") is presumably not surprising at all, and would be unlikely to draw attention comparably to the informational intervention.[31] Alternatively, a message offering parents the opportunity to receive attendance information over SMS conditional on their reply is indeed likely to make attendance salient. Having said that, such message would induce at least some parents to actually reply, making it unfeasible to disentangle the effects of the child-specific information they requested from those of salience without resorting to deception (by denying some parents access to the piece of information advertised in the original message). This is so because of selection in who takes up the information offer, preventing one from merely restricting the sample to those who do not reply. To avoid deception, the task of introducing additional variation to affect parents' likelihood of replying to the text message would once again bring about the challenge of intervening without affecting their preferences or beliefs directly – a challenge that has no obviously satisfying fix.

Third, the idea that we could send salience messages to all treated parents, and child-specific content *in addition* to the information group, would fail to cleanly separate the effects of information from those of salience. The reason is that, in the presence of inattention, additional messages would likely induce larger treatment effects even in the absence of child-specific information. This is exactly what we document in Section 6: in an additional experiment, effect sizes on attendance and grades increase with the frequency of messaging. Incidentally, other studies have documented that even message *length* matters in the case of nudges (e.g., Raifman et al., 2014). For those reasons, we not only send exactly the same number of messages across treatment conditions, but also carefully design messages to have approximately the same number of characters in each case.

Nevertheless, it could still be the case that salience effects merely capture inferred social expectations in the context of our experiment. To rule that out, we take advantage of an additional experiment that sent messages to engage parents in their children's school life, randomizing how many engagement messages per week were sent to different parents. Importantly, the content of these engagement messages was *not* specific to math classes. Due to working memory limitations and heuristics such as associativeness (Kahneman, 2011), the fact that the content of child-specific information and salience messages was restricted to school behavior within math classes suggests its effects should be lower when it comes to attendance and learning

[31] In fact, as Bursztyn et al. (2019) documents, simple reminders might not approximate well the effects of informational interventions. Moreover, reminders might just as well change recipients' preferences or beliefs above and beyond making that dimension top-of-mind.

outcomes in Portuguese classes. In contrast, engagement messages should affect math and Portuguese attendance and grades to a much more similar extent, as their content was designed to be not subject-specific.[32] Moreover, under attentional constraints, effects sizes should increase with the frequency of communication if additional messages make it more likely that children's school life becomes *top-of-mind*.

If treatment effects depend on whether content is specifically about a class or not, and on the frequency of messaging, we can safely attribute those effects to attention reallocation rather than alternative explanations.

A.2 Teacher platform

We created an online data entry platform specifically for the study, designed in a simple and intuitive way such that schools could easily manage it.[33] As discussed in the previous subsection, math teachers from treatment schools were oriented to fill in the platform every week with that week's dimension of students' behavior: attendance, punctuality or homework completion, following the scale shown below, reflecting each student behavior on that dimension over the past three weeks.[34]

Scale by dimension of student behavior

Attendance	Punctuality	Homework completion
1. Missed more than 5 classes	**1.** Was late in more than 5 classes	**1.** Did not complete any of the assignments
2. Missed 3 to 5 classes	**2.** Was late 3 to 5 classes	**2.** Completed less than half of the assignments
3. Missed less than 3 classes	**3.** Was late for less than 3 classes	**3.** Completed more than half of the assignments
4. Did not miss any class	**4.** Was not late in any class	**4.** Completed all the assignments

Scales across different dimensions were congruent – low (high) numbers meant low (high) effort across all dimensions –, and the relevant scale for each week was always visible in the platform, to minimize concerns with measurement error. The system required teachers to fill in information on *all* students in the classroom each week. Teachers were reminded to fill in the platform weekly over SMS. Teachers who failed to fill it in at any given week received an SMS alert, noting that they had not entered student data that week and encouraging them to do so in the

[32]It could of course be the case that it is harder to affect learning in Portuguese than in math. But even if that were the case, non-specific engagement messages would still provide the appropriate benchmark for salience effects when content is not domain specific.

[33]60% of Brazilian schools have access to internet, although typically only with very limited bandwidth – often below 4 mbps, shared across staff and all student computers, if any. The online platform consumed very little data, and could be accessed by principals and teachers from any computer or smartphone, even outside of the school.

[34]Students have around six math classes per week.

following week. Principals received motivational messages over SMS encouraging them to engage their teachers in the program, as well as SMS alerts in case teachers' compliance in the school was below an acceptable threshold. As a result, average compliance was high – roughly two thirds of teachers filled in the platform in any given week (see Appendix C.1).[35]

As mentioned, to incentivize schools to collect parents' phone numbers and baseline characteristics, we offered all schools (other than those in sub-sample E, where engagement messages were randomly assigned) access to the platform such that they could send parents (infrequent)notifications about school events – limited to one notification per month. Once an event was scheduled in the platform (using the principal's credentials), the system would send the SMS notification to parents one week before, and an SMS reminder one day prior to the event.

A.3 Additional details on SMS content

As described in section A.2, math teachers from treatment schools were oriented to fill in the platform every week with that week's dimension of students' behavior: attendance, tardiness or assignment completion, as shown in the table below. Teachers filled information regarding student behavior on each dimension considering the past three weeks.

Attendance	**Tardiness**	**Assignment Completion**
1. Missed more than 5 classes	**1.** Was late for more than 5 classes	**1.** Did not complete any of the assignments
2. Missed 3 to 5 classes	**2.** Was late 3 to 5 classes	**2.** Completed less than half of the assignments
3. Missed less than 3 classes	**3.** Was late for less than 3 classes	**3.** Completed more than half of the assignments
4. Did not miss any class	**4.** Was not late for any class	**4.** Completed all the assignments

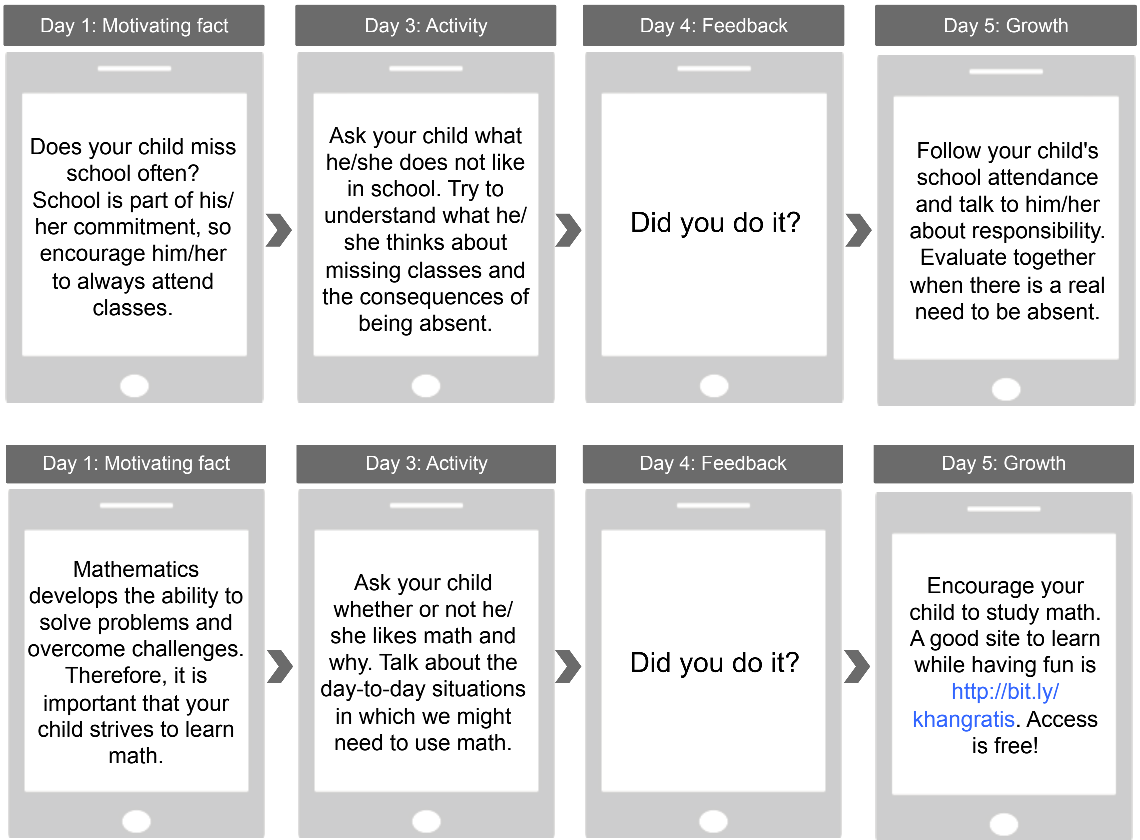
The table below shows the text messages sent in each of the 18 weeks, for each treatment arm (individual information, relative information and salience). The core text for the individual information and relative information messages were the same for each week, with only the frequency filled by the teacher in the platform and the median for the class varying (denominated by *@info* and *@info_ class* in the table). For the *relative information* arm, the platform computes the class median once the teacher submits all students' information every week. The salience messages were different each week. The messages for all the 3 groups were personalized with students names (*@name*).

[35] Despite some differences in data entry rates across the different sub-samples of schools where teachers had to enter student data into the platform, results are robust to bounding procedures that account for potential selection in unobservable student characteristics; see Appendix C.3.

Week	Individual Info.	Relative Info.	Salience
Week 1	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_class*.	If missing a class, *@name* can miss important parts of the content taught, which could impair *his/her* performance at school.
Week 2	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_class*.	When students are late for class, they can impair the progress of the group and disturb their peers' concentration. It is important that *@name* arrives on time for classes.
Week 3	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_class*.	It is important for *@name* to always turn in assignments, as they allow the student to reinforce the content taught in the classroom.
Week 4	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_class*.	Learning requires constant participation. It is important that *@name* is always present in class.
Week 5	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_class*.	For a good learning experience, it is important that *@name* is always punctual, so *he/she* doesn't miss important content taught in class.
Week 6	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_class*.	*@Name* could fall behind if *he/she* does not turn in the homework, because the teacher may not be able to help *him/her* with *his/her* specific difficulties.
Week 7	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_class*.	Participate in *@name's* education. Family engagement is essential for the student to attend classes daily.
Week 8	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_class*.	It is important that *@name* is always punctual for class so that the teacher can complete the lesson plan successfully.
Week 9	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_class*.	If *@name* does not turn in homework assignments, it may hurt *his/her* learning, as the content taught in class will not be reinforced.

Week	Individual Info.	Relative Info.	Salience
Week 10	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_ class*.	If *he/she* misses classes, *@name* may miss important parts of the content, impairing *his/her* school performance.
Week 11	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_ class*.	Arriving late impairs the progress of the class and the concentration of *@name's* peers. It's important *@name* is punctual.
Week 12	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_ class*.	It is important for *@name* to always turn in assignments, as they allow the student to reinforce the content taught in class.
Week 13	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_ class*.	Learning requires constant participation, so it's important that *@name* is always present in class.
Week 14	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_ class*.	For good learning, it is essential that *@name* is always punctual so *he/she* does not miss important content taught in class.
Week 15	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_ class*.	The teacher might not be able to help *@name* in *his/her* specific challenges if *he/she* does not turn in *his/her* homework.
Week 16	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_ class*.	Engage in *@name's* education. Family involvement is essential for the student to attend classes daily.
Week 17	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_ class*.	It is important that *@name* is always on time so that the teacher can carry out the lesson successfully.
Week 18	According to the information recorded by the teacher in the system, *@name @info* in the past 3 weeks.	In the past 3 weeks, *@name @info*. In *his/her* class, most of the students *@info_ class*.	If *@name* does not turn in the school assignments, it may be detrimental to *his/her* learning, as the content taught in class will not be reinforced.

The figure below shows two examples of the SMS sequence sent to parents assigned to the engagement messages program (described in section C.5.3). The figure displays a stylized sequence for a parent assigned to 3 messages a week and interactivity. Those assigned to the group without interactivity do not receive the feedback message on day 4 of every week. Those assigned to 2 messages a week do not receive the growth message on day 5 of every week. Last, those assigned to 1 message a week receive only the activity message, on day 3 of every week. Only parents who received one message per week were considered in the robustness tests performed in section C.5.3 [36].



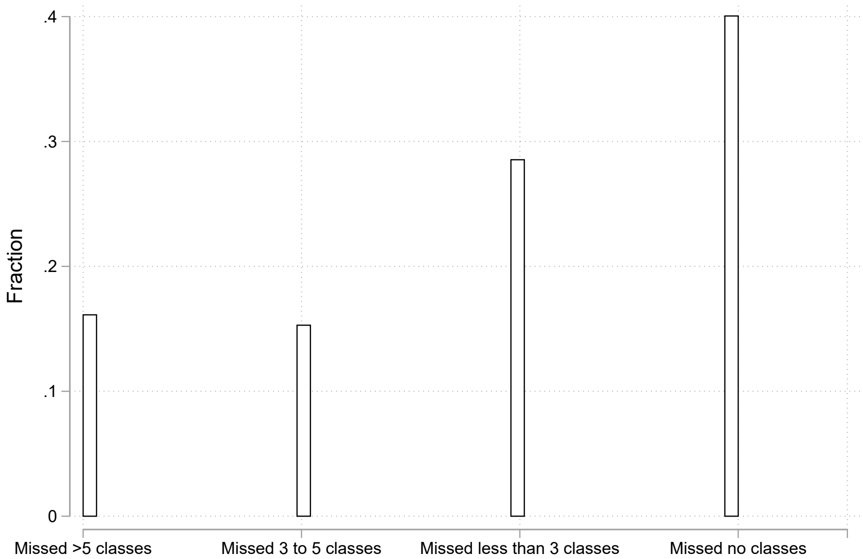
[36] The intellectual property rights of the content library of engagement messages belongs to our implementing partner, MGov Brasil, and therefore only two examples are provided here.

A.4 Distribution of child-specific information sent out

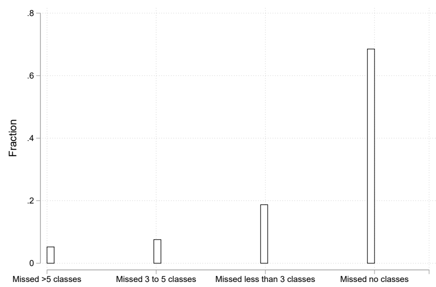
This Appendix shows the distribution of messages sent to parents targeted by child-specific information. Figure A.1 showcases the distribution of messages about attendance, Figure A.2, about punctuality, and Figure A.3, about homework. In each figure, we also showcase conditional distributions, according to the modal message received by each parent in each case.

Figure A.1: Distribution of messages sent about attendance

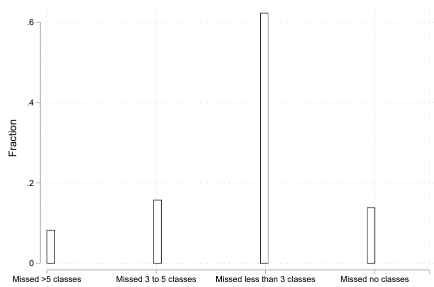
Panel A: Unconditional distribution of messages sent



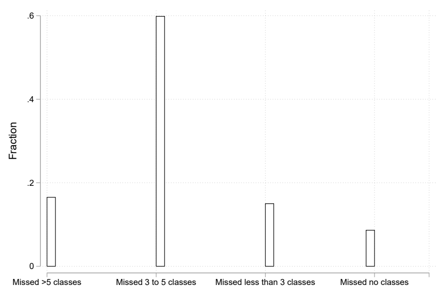
Panel B: Conditional distribution for those whose modal message received was "Did not miss any class"



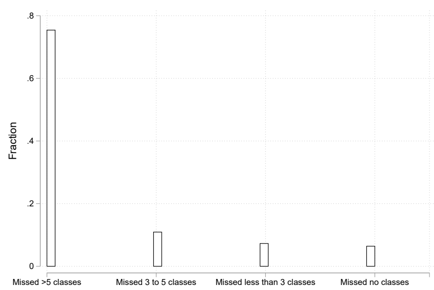
Panel C: Conditional distribution for those whose modal message received was "Missed 1-2 classes"



Panel D: Conditional distribution for those whose modal message received was "Missed 3-4 classes"



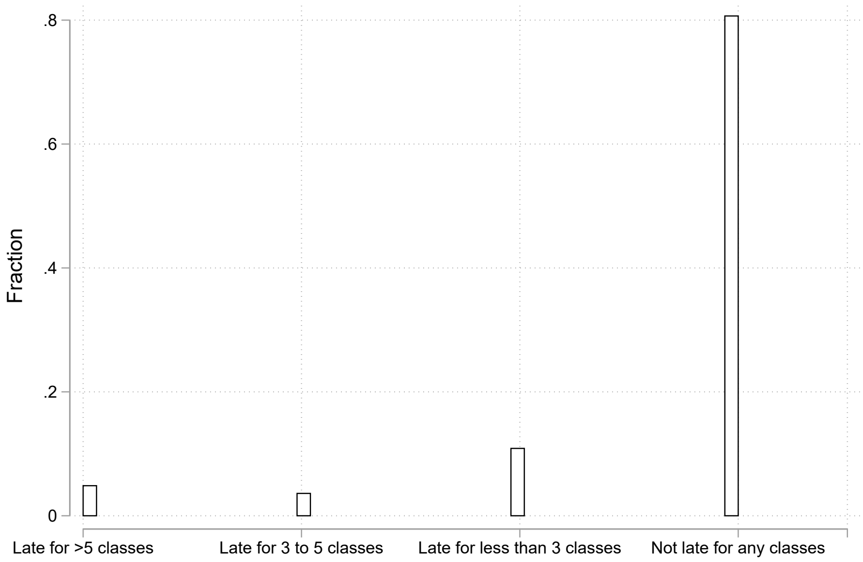
Panel E: Conditional distribution for those whose modal message received was "Missed 5 or more classes"



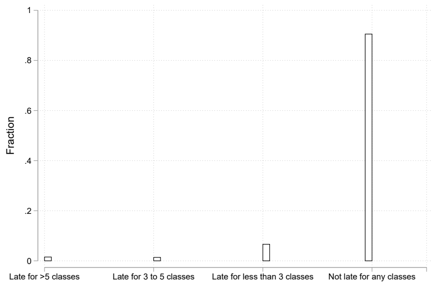
Note: Distribution of messages sent about attendance. Panel A shows the unconditional distribution of messages sent. Other Panels show the conditional distribution of messages received according to the modal message received by each parent: "Did not miss any class" (Panel B), "Missed 1-2 classes" (Panel C), "Missed 3-4 5 classes" (Panel D), and "Missed 5 or more classes" (Panel E).

Figure A.2: Distribution of messages sent about punctuality

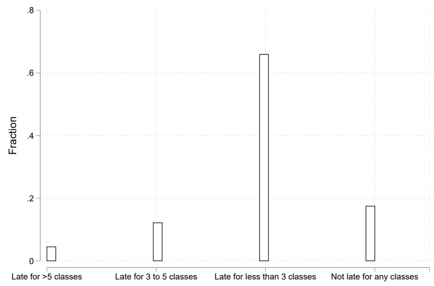
Panel A: Unconditional distribution of messages sent



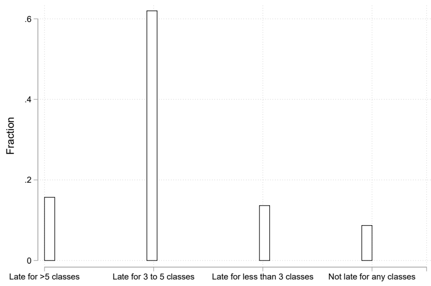
Panel B: Conditional distribution for those whose modal message received was "Was not late for any class"



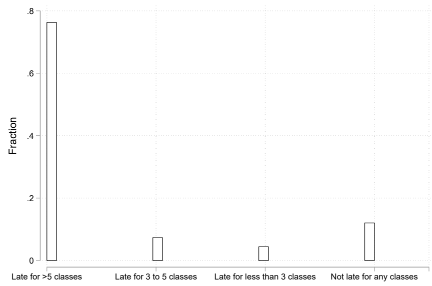
Panel C: Conditional distribution for those whose modal message received was "Late for 1-2 classes"



Panel D: Conditional distribution for those whose modal message received was "Late for 3-4 classes"



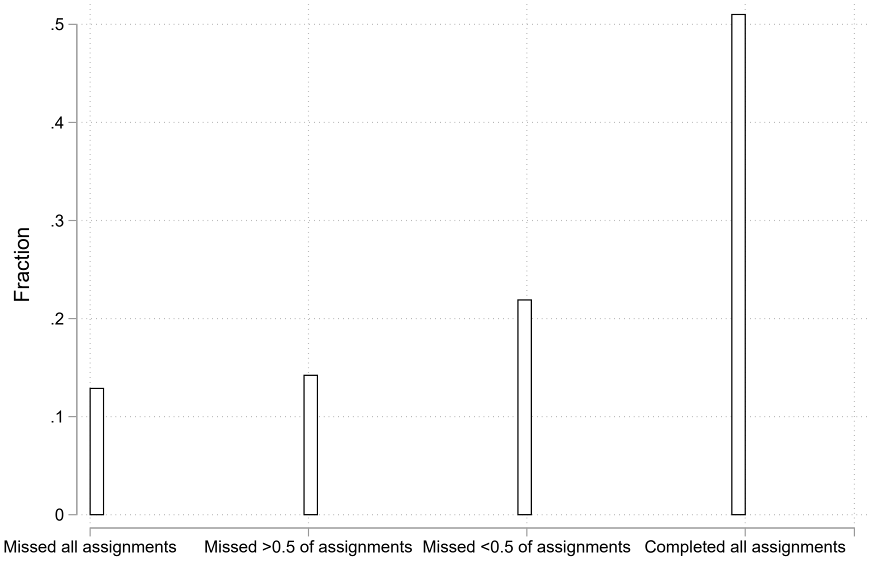
Panel E: Conditional distribution for those whose modal message received was "Late for 5 or more classes"



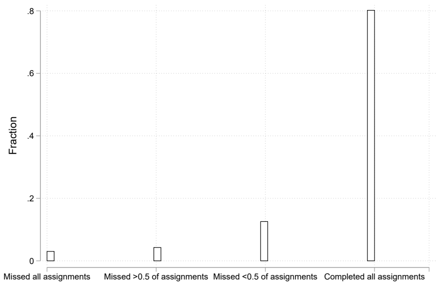
Note: Distribution of messages sent about punctuality. Panel A shows the unconditional distribution of messages sent. Other Panels show the conditional distribution of messages received according to the modal message received by each parent: "Was not late for any class" (Panel B), "Late for 1-2 classes" (Panel C), "Late for 3-4 5 classes" (Panel D), and "Late for 5 or more classes" (Panel E).

Figure A.3: Distribution of messages sent about homework

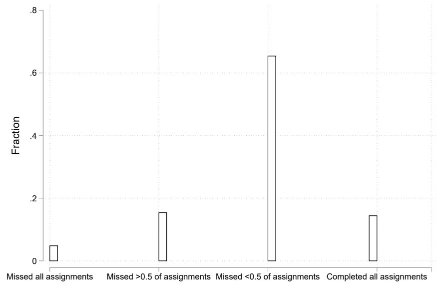
Panel A: Unconditional distribution of messages sent



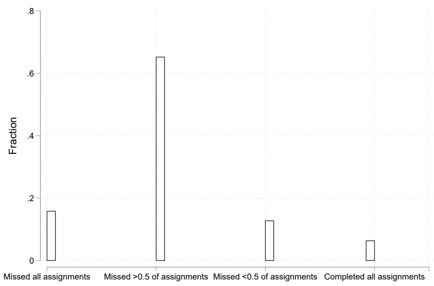
Panel B: Conditional distribution for those whose modal message received was "Handed in all homework assignments"



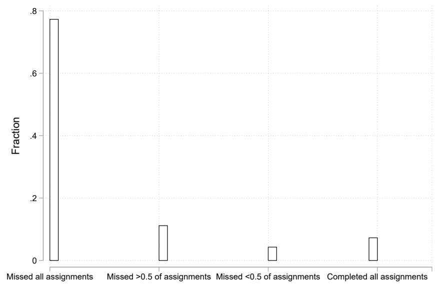
Panel C: Conditional distribution for those whose modal message received was "Handed in more than half of assignments"



Panel D: Conditional distribution for those whose modal message received was "Handed in less than half of assignments"



Panel E: Conditional distribution for those whose modal message received was "Did not hand in any assignments"



Note: Distribution of messages sent about homework. Panel A shows the unconditional distribution of messages sent. Other Panels show the conditional distribution of messages received according to the modal message received by each parent: "Handed in all homework assignments" (Panel B), "Handed in more than half of assignments" (Panel C), "Handed in less than half of assignments" (Panel D), and "Did not hand in any assignments" (Panel E).

B Descriptive statistics and balance and selective non-response tests

B.1 Descriptive statistics

Table B.1 presents the sample means of students' and primary caregivers' baseline characteristics by treatment arm, along with p-values of ANOVA tests of equality of means across groups. Panel A displays student's characteristics and Panel B, those of caregivers.

Table B.1: Descriptive statistics and balance

	Means				All differences=0 (p-value)	Pure control vs. All others=0	Sample Size
	Pure Control	Control within classroom	Salience	Child-specific information			
Panel A: Student characteristics							
Female	0.48	0.50	0.51	0.51	0.14	0.04	15589
Age	14.71	14.72	14.71	14.75	0.03	0.96	15595
Brown	0.34	0.35	0.34	0.35	0.48	0.67	15592
Black	0.06	0.05	0.06	0.06	0.45	0.34	15592
Portuguese GPA (0-10)	6.18	6.19	6.13	6.13	0.36	0.78	15437
Math GPA (0-10)	5.94	5.99	5.92	5.90	0.25	0.95	15453
Portuguese attendance	0.91	0.92	0.92	0.91	0.68	0.58	15480
Math attendance	0.91	0.91	0.91	0.91	0.30	0.74	15440
Panel B: Adult responsible for student							
Mother	0.78	0.76	0.76	0.76	0.28	0.08	15597
Age	40.43	40.25	40.34	40.42	0.86	0.90	15461
Brown	0.34	0.34	0.34	0.34	0.65	0.87	15593
Black	0.07	0.06	0.07	0.07	0.80	0.97	15593
Education	2.75	2.89	2.85	2.86	0.07	0.04	15591
Earns less than 1 MW (∼ $250)	0.17	0.18	0.17	0.18	0.63	0.38	15593
Earns between 1 - 3 MW	0.42	0.45	0.45	0.46	0.41	0.21	15593
p-value (F-statistic of joint test)					0.47	0.69	

Note: Conditional means net of randomization strata fixed effects. P-values computed using randomization strata fixed effects and with standard errors clustered at the classroom level. P-value for the joint hypothesis that all differences equal zero based on a chi-squared statistic on a multinomial logit model. Data on students' gender, age, GPA and attendance from administrative records, and data on students' race and on parents' characteristics from the face-to-face baseline survey within those who opted-in to participate in program. This Table includes all students in the balanced sample, samples A, B, C, and D (see Figure 1).

The table shows that 48% of students in our sample are girls and 40% are brown or black – a little over the State average (35.6%, according to the 2010 Census) since white families are typically wealthier in Brazil, and wealthy families typically send their children to private schools. In fact, 59% of primary caregivers in our sample earn less than 3 minimum wages (about USD 750 at the time), within the range of low socioeconomic status in the State. Students in our sample average 14.7 years old. Their math and Portuguese 1st-quarter grades average 5.9 and 6.2, respectively (in a 0 to 10 scale, with a passing grade of 5). 76% of primary caregivers are mothers, and those are, on average, at their early 40s. 69% of them have no education beyond middle school; as such, 2/3 of participating students are at least as advanced in school as their parents ever were.

The sample is balanced across treatment arms: out of 17 variables, only age features statistically significant differences across groups, at the 10% level – which is expected to happen just by chance – and numerically irrelevant. To that point, F-tests document that baseline characteristics are not jointly different across groups, when it comes to either students' or caregivers' characteristics.

Receiving messages from the school as part of parents' participation in the study borne no costs; parents just had to provide consent and a valid phone number, either directly at parent-teacher meeting towards the end of the second quarter, or indirectly, by filling in a paper form that students took home when parents were absent from the school meeting. Over 66% of the 23,398 parents invited to participate signed up for the program.

Table B.2 analyzes selection in opt-in. For parents who did not sign up, we have access to only a few student characteristics from the Secretariat of Education administrative records: their gender, age, math and Portuguese 1st-quarter attendance and grades, and their family's Bolsa Família's beneficiary status (known to schools because a high-enough attendance rate is part of the transfer's conditionality).

Table B.2: Selection at opt-in

	Sub-sample mean		Diff.	Observations
	Opt-out	Opt-in		
Female	0.45	0.50	0.05***	23372
			[0.01]	
Age	14.92	14.73	-0.19***	23398
			[0.01]	
Portuguese GPA (max 10)	5.39	6.16	0.77***	22687
			[0.03]	
Math GPA (max 10)	5.09	5.94	0.84***	22691
			[0.03]	
Portuguese attendance	0.88	0.91	0.04***	22850
			[0.00]	
Math attendance	0.87	0.91	0.04***	22753
			[0.00]	
Cash transfer beneficiary	0.19	0.16	-0.03***	23029
			[0.01]	

Note: Differences in student characteristics between those whose primary caregivers consented to participate in the SMS program and all others (refusals or those who could not be reached by the school to ask for consent). Data from administrative records on students' age, gender, 1st-quarter math and Portuguese attendance and GPA, and whether their household is a Bolsa-Família (Brazil's flagship conditional cash transfer) beneficiary. Column 3 reports differences in means between the two groups for each variable. * if p<0.1, ** p<0.05 and *** if p<0.01.

The table shows that parents who signed up for the program are from relatively better-off households: their children had statistically higher attendance and grades, and they were less likely to be Bolsa Família beneficiaries. Since any educational intervention that requires parents' consent is expected to have imperfect compliance, we focus throughout on the average treatment effect on the treated. Having said that, Appendix B shows that our results are robust to re-weighting observations by their inverse probability of opt-in.

The next sections present additional balance and selective attrition tests, focusing on the different sub-samples we analyze throughout the paper.

B.2 Main experiment

Table B.3 shows descriptive statistics and balance tests for the sample with non-missing platform scores: all students in sub-samples A, C and D for whom teachers filled in the platform in at least one week. Table B.4 then presents balance tests for the sub-sample with non-missing survey data, followed by Table B.5, which focuses specifically on those who answered the end-line survey. Those tables document no significant differences across treatment arms in what comes to baseline characteristics, regardless of sample restrictions. Last, Table B.6 presents balance tests for the engagement messages intervention, showcasing that, as discussed, there were in fact significant baseline imbalances between sub-sample E and the pure control group, which warrants the differences-in-differences strategy we pursue in Section 5.3.2.

Next, turning to selective non-response, Table B.7 documents how baseline characteristics affect the probability of survey response. Table B.8 then documents that non-response is not selective across the different treatment arms: only one coefficient out of 12 is statistically significant at the 10% level (what we would expect to happen just by chance), and we fail to reject an F-test of joint equality between the coefficients of all treatment arms even in that case. Table B.9 restricts attention to the student end-line survey to show that these findings are not sensitive to different cutoffs for survey completion. Last, because parents who opted into the program had different characteristics from those who did not (as Table B.2 shows), Table B.10 replicates our estimates for treatment effects on administrative outcomes re-weighting observations by their inverse predicted probability of opting into the program (predictions based on Table B.7's estimates).

Table B.3: Descriptive statistics and balance tests – sample with non-missing platform scores

	Means				All differences=0 (p-value)	Pure control vs. All others=0	Sample Size
	Pure Control	Control Within Class	Salience	Info			
Student characteristics							
Female	0.47	0.50	0.51	0.52	0.03	0.01	12577
Age	14.69	14.67	14.67	14.71	0.03	0.45	12577
Brown	0.36	0.35	0.34	0.35	0.14	0.11	12577
Black	0.06	0.05	0.06	0.06	0.79	0.87	12577
Portuguese GPA (max 10)	6.39	6.31	6.27	6.28	0.69	0.47	12577
Math GPA (max 10)	6.10	6.11	6.05	6.06	0.57	0.94	12577
Portuguese attendance	0.92	0.92	0.92	0.92	0.50	0.79	12577
Math attendance	0.92	0.92	0.92	0.91	0.39	0.62	12577
Adult responsible for student							
Mother	0.77	0.75	0.76	0.76	0.45	0.23	12577
Age	40.39	40.28	40.34	40.57	0.68	0.65	12577
Brown	0.36	0.34	0.34	0.34	0.15	0.05	12577
Black	0.07	0.06	0.07	0.07	0.71	0.81	12577
Education	2.84	2.92	2.90	2.98	0.19	0.08	12577
Earns less than 1 MW (1MW $\sim$ \$250)	0.17	0.17	0.17	0.17	0.80	0.56	12577
Earns between 1 - 3 MW	0.44	0.46	0.46	0.47	0.80	0.53	12577
p-value (F-statistic of joint test)					0.58	0.39	

Note: Conditional means net of randomization strata fixed effects. P-values calculated using randomization strata fixed effects and standard errors clustered at the classroom level. P-value for the joint hypothesis that all differences equal zero based on a chi-squared statistic on a multinomial logit model. Data on students' gender, age, GPA and attendance was collected from administrative records, and data on students' race and on the adult responsible for student was collected from the baseline survey took by parents who opted-in to the program.

Table B.4: Descriptive statistics and balance tests – sample with non-missing phone survey data

	Means				All differences=0 (p-value)	Pure control vs. All others=0	Sample Size
	Pure Control	Control Within Class	Salience	Info			
Student characteristics							
Female	0.50	0.50	0.52	0.52	0.18	0.26	9539
Age	14.65	14.65	14.66	14.68	0.24	0.95	9539
Brown	0.36	0.35	0.33	0.34	0.33	0.11	9539
Black	0.05	0.05	0.05	0.05	0.68	0.82	9539
Portuguese GPA (max 10)	6.51	6.45	6.39	6.39	0.51	0.53	9539
Math GPA (max 10)	6.21	6.22	6.20	6.17	0.87	0.97	9539
Portuguese attendance	0.93	0.93	0.93	0.93	0.30	0.95	9539
Math attendance	0.93	0.92	0.92	0.92	0.45	0.51	9539
Adult responsible for student							
Mother	0.78	0.75	0.76	0.76	0.43	0.22	9539
Age	40.62	40.39	40.34	40.74	0.64	0.91	9539
Brown	0.35	0.34	0.34	0.33	0.27	0.15	9539
Black	0.07	0.06	0.07	0.07	0.67	0.90	9539
Education	2.80	2.95	2.94	2.92	0.11	0.02	9539
Middle school complete	0.28	0.26	0.27	0.28	0.37	0.19	9539
High School	0.33	0.34	0.32	0.33	0.42	0.75	9539
Earns less than 1 MW (1MW $\sim$ \$250)	0.16	0.16	0.16	0.16	0.86	0.44	9539
Earns between 1 - 3 MW	0.44	0.47	0.46	0.47	0.92	0.64	9539
p-value (F-statistic of joint test)					0.54	0.67	

Note: Conditional means net of randomization strata fixed effects. P-values calculated using randomization strata fixed effects and standard errors clustered at the classroom level. P-value for the joint hypothesis that all differences equal zero based on a chi-squared statistic on a multinomial logit model. Data on students' gender, age, GPA and attendance was collected from administrative records, and data on students' race and on the adult responsible for student was collected from the baseline survey took by parents who opted-in to the program.

Table B.5: Descriptive statistics and balance tests – sample with non-missing endline phone survey data

	Means				All differences=0 (p-value)	Pure control vs. All others=0 (p-value)	Sample Size
	Pure Control	Control Within Class	Salience	Info			
Student characteristics							
Female	0.47	0.52	0.51	0.52	0.30	0.06	3857
Age	14.69	14.68	14.67	14.67	0.89	0.49	3857
Brown	0.39	0.35	0.36	0.37	0.56	0.20	3857
Black	0.05	0.05	0.06	0.05	0.81	0.62	3857
Portuguese GPA (max 10)	6.41	6.47	6.29	6.37	0.12	0.71	3857
Math GPA (max 10)	5.97	6.24	6.05	6.16	0.05	0.10	3857
Portuguese attendance	0.93	0.93	0.92	0.93	0.71	0.76	3857
Math attendance	0.93	0.92	0.92	0.92	0.41	0.25	3857
Adult responsible for student							
Mother	0.81	0.81	0.81	0.78	0.16	0.43	3857
Age	39.07	39.39	39.65	39.88	0.52	0.30	3857
Brown	0.39	0.35	0.36	0.34	0.31	0.08	3857
Black	0.08	0.07	0.06	0.08	0.39	0.78	3857
Middle school incomplete	0.29	0.26	0.29	0.28	0.57	0.59	3857
Middle school complete	0.27	0.23	0.28	0.26	0.06	0.64	3857
High School	0.37	0.39	0.33	0.35	0.01	0.54	3857
Earns less than 1 MW (1MW $\sim$ \$250)	0.19	0.16	0.17	0.19	0.12	0.57	3857
Earns between 1 - 3 MW	0.50	0.50	0.50	0.49	0.98	0.99	3857
p-value(F-statistic of joint test)					0.31	0.16	

Note: Conditional means net of randomization strata fixed effects. P-values calculated using randomization strata fixed effects and standard errors clustered at the classroom level. P-value for the joint hypothesis that all differences equal zero based on a chi-squared statistic on a multinomial logit model. Data on students' gender, age, GPA and attendance was collected from administrative records, and data on students' race and on the adult responsible for student was collected from the baseline survey took by parents who opted-in to the program.

Table B.6: Descriptive statistics and balance tests – engagement messages intervention

	Means			All differences=0 (p-value)	Pure control vs. All others=0	Sample Size
	Pure Control	Control Within Class	Engagement			
Panel A: Student characteristics						
Female	0.47	0.51	0.50	0.23	0.09	3058
Age	14.68	14.66	14.69	0.68	0.61	3058
Brown	0.36	0.32	0.31	0.05	0.02	3058
Black	0.06	0.05	0.05	0.53	0.26	3058
Portuguese GPA (max 10)	6.37	5.99	5.99	0.00	0.00	3019
Math GPA (max 10)	6.07	5.79	5.75	0.00	0.00	3021
Portuguese attendance	0.93	0.92	0.93	0.91	0.68	3037
Math attendance	0.92	0.92	0.92	0.88	0.96	2975
Panel B: Adult responsible for student						
Mother	0.78	0.76	0.74	0.14	0.07	3058
Age	40.38	40.77	40.47	0.51	0.37	3008
Black	0.07	0.07	0.07	0.88	0.02	3058
Education	2.80	3.02	3.08	0.06	0.00	3058
Earns less than 1 MW (1MW $\sim$ \$250)	0.16	0.15	0.13	0.10	0.06	3058
Earns between 1 - 3 MW	0.43	0.46	0.47	0.11	0.04	3058
p-value (F-statistic of joint test)				0.00	0.00	

Note: P-values computed from robust standard. Engagement treatment includes only parents who received one text message per week. P-value for the joint hypothesis that all differences equal zero based on a chi-squared statistic on a multinomial logit model. Data on students' gender, age, GPA and attendance was collected from administrative records, and data on students' race and on the adult responsible for student was collected from the baseline survey took by parents who opted-in to the program.

Table B.7: Marginal effects on survey completion

	(1) Parents' baseline survey	(2) Parents' end-line survey	(3) Students' end-line survey
Student characteristics			
Female	0.006	-0.010	0.015
	[0.012]	[0.013]	[0.007]
Age	-0.017*	-0.027*	-0.055*
	[0.009]	[0.009]	[0.006]
Brown or Black	-0.041***	-0.012***	-0.025***
	[0.012]	[0.013]	[0.007]
Math GPA (max 10)	0.012***	0.016***	0.027***
	[0.003]	[0.003]	[0.002]
Math attendance	0.147**	0.213**	0.774**
	[0.067]	[0.070]	[0.045]
Adult responsible for student			
Mother	0.007	0.057	-0.006
	[0.015]	[0.017]	[0.008]
Age	-0.003***	-0.002***	0.001***
	[0.001]	[0.001]	[0.000]
Brown or Black	-0.052***	-0.010***	-0.012***
	[0.013]	[0.013]	[0.007]
Low Education (middle school incomplete)	-0.070***	-0.059***	-0.042***
	[0.014]	[0.015]	[0.008]
Cash transfer beneficiary	-0.032**	-0.039**	-0.029**
	[0.016]	[0.018]	[0.010]
Sample size	12,577	12,577	12,577

Note: Marginal effects on the probability of baseline and end-line survey completion, by parents and students. Across all columns, the dependent variable is an indicator variable = 1 if the survey was completed, and 0 otherwise. Surveys were considered completed if respondents missed at most 4 questions (completion ≈ 74% or higher). Table B.9 shows that results are robust to alternative cutoffs. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income, education , and a dummy indicating whether the family is a Bolsa-Família (Brazil's flagship conditional cash transfer) recipient. We also control for randomization strata fixed-effects. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table B.8: Selective non-response tests

	(1) Parent baseline survey	(2) Parent end-line survey	(3) Student end-line survey
Child-specific information	-0.008	0.039	0.013
	[0.021]	[0.024]	[0.016]
Salience	-0.016	0.022	0.016
	[0.020]	[0.024]	[0.016]
Control within classroom	-0.006	0.045*	0.020
	[0.020]	[0.023]	[0.016]
p-value(Info=Salience=Control)	0.828	0.412	0.694
Sample Size	4862	4653	15597
Randomization strata FE	Yes	Yes	Yes

Note: Selective non-response tests for each survey. The pure control group is the omitted category. In all columns, the dependent variable is an indicator variable equal to 1 if parents/students completed the survey, and 0 otherwise. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table B.9: Selective non-response: robustness to different survey completion cutoffs (student end-line survey)

	(1) All questions answered (100%)	(2) All but one question (≈ 94%)	(3) All but four questions (≈ 74%)
Salience	-0.017	0.002	0.016
	[0.020]	[0.017]	[0.016]
Information	-0.024	0.004	0.013
	[0.021]	[0.017]	[0.016]
Control Within Class	-0.017	0.008	0.020
	[0.021]	[0.017]	[0.016]
P-value Salience=Info=Control Within	0.758	0.806	0.694
Sample Size	15,597	15,597	15,597
Randomization strata FE	Yes	Yes	Yes

Note: Marginal effects on the probability of baseline and end-line survey completion, by parents and students. Across all columns, the dependent variable is an indicator variable = 1 if the survey was completed, and 0 otherwise. Different columns consider different cutoffs for survey completion. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income, education , and a dummy indicating whether the family is a Bolsa-Família (Brazil's flagship conditional cash transfer) recipient. We also control for randomization strata fixed-effects. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table B.10: Treatment effects, re-weighting by the inverse predicted opt-in probability

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.022***	0.100***	0.038***	0.096**
	[0.006]	[0.032]	[0.013]	[0.046]
Information	0.022***	0.077**	0.031**	0.105**
	[0.007]	[0.032]	[0.013]	[0.046]
Control Mean	0.875	0.000	0.938	-0.000
P-value diff. [Info] -[Salience]	0.854	0.141	0.162	0.680
Sample Size	12,550	12,550	12,550	12,550
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes

Note: Treatment effects of child-specific information and salience messages on 4th-quarter attendance in math classes (Column 1); 4th-quarter math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3), and math standardized test scores (Column 4). GPA and standardized test scores were normalized relative to the distribution of the pure control group. Observations were re-weighted by their inverse predicted probability of opting into the program (based on Table B.7's estimates). Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

B.3 Additional experiment

This Appendix compiles balance tests for each communication feature cross-randomized in the additional experiment, as well as estimates of treatment effects of time of SMS delivery, consistency of delivery time, and interactivity, on math and Portuguese attendance and grades. The effects of the number of weekly messages on those outcomes is portrayed in the main text (Figure 6).

Tables B.11-B.14 show that assignment to different communication features across the different experiments was balanced with respect to student and caregivers' characteristics. Table B.15 documents the treatment effects of each feature on administrative educational outcomes, through differences-in-differences, along with p-values for differences in treatment effects across treatment arms. Delivering messages during work hours tends to have larger effects sizes (except for Portuguese GPA), but no difference is statistically significant. Varying the time of delivery does not seem to help make children's school life top-of-mind: its effect sizes are never larger than those of scheduling messages to be delivered always at the same time, and its coefficient is actually significantly smaller (at the 10%) when it comes to Portuguese GPA. Last, interactivity helps favourably reallocate parents' attention: its effect sizes are always larger than those of not having interactive messages, and its coefficient is actually significantly larger (at the 10%) when it comes to Portuguese attendance.

Table B.11: Balance tests – frequency

	Means				All differences=0 (p-value)	Pure control vs All others	Sample size
	Control	1 message/week	2 message/week	3 message/week			
Panel A: Student characteristics							
Female	0.50	0.50	0.54	0.50	0.21	0.49	3654
Age	14.71	14.77	14.73	14.77	0.20	0.07	3655
Brown	0.30	0.32	0.31	0.30	0.81	0.60	3656
Black	0.05	0.05	0.05	0.05	0.88	0.63	3656
Portuguese GPA (max 10)	5.83	5.89	5.84	5.77	0.71	0.98	3398
Math GPA (max 10)	5.66	5.64	5.63	5.51	0.48	0.32	3421
Portuguese attendance	0.92	0.92	0.91	0.91	0.31	0.53	3435
Math attendance	0.92	0.91	0.91	0.91	0.40	0.24	3458
Panel B: Adult responsible for the student							
Mother	0.76	0.75	0.75	0.77	0.90	0.98	3656
Age	40.74	40.64	41.14	40.84	0.54	0.63	3628
Brown	0.32	0.29	0.31	0.30	0.65	0.40	3656
Black	0.08	0.07	0.05	0.06	0.20	0.07	3656
Education	3.00	3.05	2.95	2.95	0.27	0.64	3656
Earns less than 1 MW (1MW $\sim$ \$250)	0.16	0.14	0.15	0.14	0.53	0.19	3656
Earns between 1 - 3 MW	0.45	0.47	0.46	0.43	0.57	1.00	3656
p-value (F statistic of joint test)					0.35	0.62	

Note: Conditional means net of randomization strata fixed effects. P-values calculated using randomization strata fixed effects and standard errors clustered at the classroom level. P-value for the joint hypothesis that all differences equal zero based on a chi-squared statistic on a multinomial logit model. Data on students' gender, age, GPA and attendance was collected from administrative records, and data on students' race and on the adult responsible for student was collected from the baseline survey took by parents who opted-in to the program.

Table B.12: Balance tests – time of delivery

	Means			All differences=0 (p-value)	Pure control vs All others	Sample size
	Control	Evening	Afternoon			
Panel A: Student characteristics						
Female	0.50	0.52	0.51	0.76	0.49	3654
Age	14.71	14.74	14.78	0.07	0.07	3655
Brown	0.30	0.31	0.31	0.87	0.60	3656
Black	0.05	0.05	0.05	0.88	0.63	3656
Portuguese GPA (max 10)	5.83	5.85	5.81	0.86	0.98	3398
Math GPA (max 10)	5.66	5.62	5.56	0.55	0.32	3421
Portuguese attendance	0.92	0.91	0.91	0.79	0.53	3435
Math attendance	0.92	0.91	0.91	0.33	0.24	3458
Panel B: Adult responsible for the student						
Mother	0.76	0.75	0.76	0.74	0.98	3656
Age	40.74	41.11	40.64	0.19	0.63	3628
Brown	0.32	0.30	0.31	0.52	0.40	3656
Black	0.08	0.05	0.07	0.02	0.07	3656
Education	3.00	2.98	2.99	0.88	0.64	3656
Earns less than 1 MW (1MW $\sim$ \$250)	0.16	0.14	0.14	0.42	0.19	3656
Earns between 1 - 3 MW	0.45	0.44	0.47	0.46	1.00	3656
p-value (F statistic of joint test)				0.35	0.81	

Note: Conditional means net of randomization strata fixed effects. P-values calculated using randomization strata fixed effects and standard errors clustered at the classroom level. P-value for the joint hypothesis that all differences equal zero based on a chi-squared statistic on a multinomial logit model. Data on students' gender, age, GPA and attendance was collected from administrative records, and data on students' race and on the adult responsible for student was collected from the baseline survey took by parents who opted-in to the program.

Table B.13: Balance tests – schedule consistency

	Means			All differences=0 (p-value)	Pure control vs All others	Sample size
	Control	Constant	Varying			
Panel A: Student characteristics						
Female	0.50	0.51	0.52	0.71	0.49	3654
Age	14.71	14.76	14.76	0.19	0.07	3655
Brown	0.30	0.33	0.30	0.34	0.60	3656
Black	0.05	0.04	0.06	0.19	0.63	3656
Portuguese GPA (max 10)	5.83	5.83	5.84	0.98	0.98	3398
Math GPA (max 10)	5.66	5.58	5.60	0.58	0.32	3421
Portuguese attendance	0.92	0.91	0.92	0.42	0.53	3435
Math attendance	0.92	0.91	0.91	0.43	0.24	3458
Panel B: Adult responsible for the student						
Mother	0.76	0.76	0.75	0.67	0.98	3656
Age	40.74	40.71	41.04	0.49	0.63	3628
Brown	0.32	0.31	0.29	0.36	0.40	3656
Black	0.08	0.06	0.07	0.08	0.07	3656
Education	3.00	2.99	2.97	0.84	0.64	3656
Earns less than 1 MW (1MW $\sim$ \$250)	0.16	0.15	0.14	0.35	0.19	3656
Earns between 1 - 3 MW	0.45	0.45	0.46	0.91	1.00	3656
p-value (F statistic of joint test)				0.35	0.48	

Note: Conditional means net of randomization strata fixed effects. P-values calculated using randomization strata fixed effects and standard errors clustered at the classroom level. P-value for the joint hypothesis that all differences equal zero based on a chi-squared statistic on a multinomial logit model. Data on students' gender, age, GPA and attendance was collected from administrative records, and data on students' race and on the adult responsible for student was collected from the baseline survey took by parents who opted-in to the program.

Table B.14: Balance tests – interactivity

	Means			All differences=0 (p-value)	Pure control vs All others	Sample size
	Control	Interactive	Passive			
Panel A: Student characteristics						
Female	0.50	0.51	0.52	0.77	0.49	3654
Age	14.71	14.75	14.76	0.16	0.07	3655
Brown	0.30	0.31	0.32	0.84	0.60	3656
Black	0.05	0.06	0.05	0.57	0.63	3656
Portuguese GPA (max 10)	5.83	5.89	5.78	0.39	0.98	3398
Math GPA (max 10)	5.66	5.65	5.54	0.29	0.32	3421
Portuguese attendance	0.92	0.91	0.91	0.82	0.53	3435
Math attendance	0.92	0.91	0.91	0.49	0.24	3458
Panel B: Adult responsible for the student						
Mother	0.76	0.76	0.75	0.79	0.98	3656
Age	40.74	40.69	41.06	0.49	0.63	3628
Brown	0.32	0.31	0.30	0.58	0.40	3656
Black	0.08	0.07	0.06	0.17	0.07	3656
Education	3.00	3.00	2.96	0.63	0.64	3656
Earns less than 1 MW (1MW $\sim$ \$250)	0.16	0.15	0.14	0.35	0.19	3656
Earns between 1 - 3 MW	0.45	0.44	0.46	0.45	1.00	3656
p-value (F statistic of joint test)				0.35	0.57	

Note: Conditional means net of randomization strata fixed effects. P-values calculated using randomization strata fixed effects and standard errors clustered at the classroom level. P-value for the joint hypothesis that all differences equal zero based on a chi-squared statistic on a multinomial logit model. Data on students' gender, age, GPA and attendance was collected from administrative records, and data on students' race and on the adult responsible for student was collected from the baseline survey took by parents who opted-in to the program.

Table B.15: Effects of additional features on attendance, grades and grade promotion

	(1) Math Attendance (p.p)	(2) Math GPA (std.)	(3) Portuguese Attendance (p.p)	(4) Portuguese GPA (std.)
Panel A: Time of delivery				
Afternoon x post	0.021***	0.160***	0.017***	0.103***
	[0.006]	[0.039]	[0.005]	[0.005]
Evening x post	0.018***	0.116***	0.015***	0.129***
	[0.006]	[0.041]	[0.005]	[0.047]
p-value diff. [Afternoon]-[Evening]	0.39	0.12	0.65	0.35
Panel B: Schedule consistency				
Varying x post	0.020***	0.120***	0.015***	0.092***
	[0.006]	[0.039]	[0.005]	[0.046]
Constant x post	0.020***	0.157***	0.017***	0.139***
	[0.006]	[0.041]	[0.005]	[0.048]
p-value diff. [Varying]-[Constant]	0.98	0.17	0.68	0.08
Panel C: Interactivity				
Passive x post	0.018***	0.125***	0.012***	0.100**
	[0.006]	[0.041]	[0.005]	[0.047]
Interactive x post	0.021***	0.150***	0.019***	0.131***
	[0.006]	[0.040]	[0.005]	[0.048]
p-value diff. [Passive]-[Interactive]	0.35	0.38	0.02	0.26

Note: Treatment effects of communication features on attendance in math classes (Column 1), math GPA (Column 2), Portuguese attendance (Column 3) and Portuguese GPA (column 4), estimated through differences-in-differences. GPA was normalized relative to the distribution of the pure control group. The sample includes sub-sample E and the pure control group. Observations are stacked (student x school quarter). All estimates use the 1st quarter as the period of reference. Regressions include interactions between a post-treatment time dummy and treated students, and between the post-treatment dummy and within-classroom control group dummy (the pure control is the reference group). We also include in the regression indicator variables for the post-treatment period and for the treatment and within-classroom control groups, and student-level controls. Students' controls include gender, age, race, baseline grades and attendance, and parents' controls include gender, age, race, family income and education. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

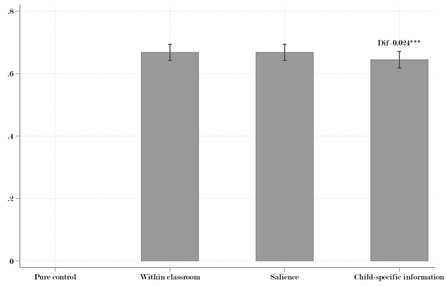
C Additional results

C.1 Manipulation checks

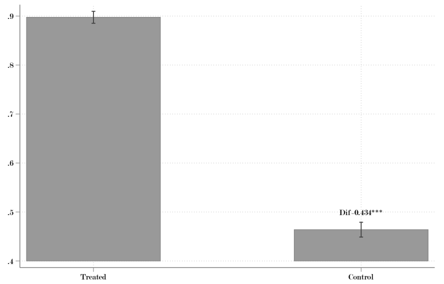
If teachers did not fill in the platform with students' information weekly, or if parents did not even acknowledge receiving text messages from the school, then there would be no hope that our experiment could allow us detecting the effects of interest. For this reason, this Appendix looks at these manipulation checks. Figure C.1 displays statistics for platform usage and receipt of text messages across treatment arms.

Figure C.1: Manipulation Tests

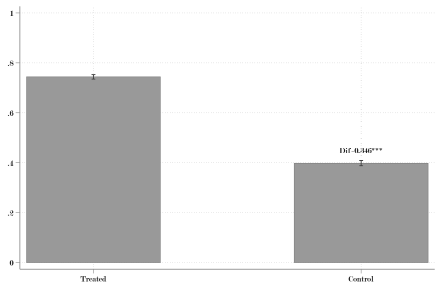
Panel A: Share of weeks teachers filled in the platform
by treatment status



Panel B: Did parents acknowledge receiving text messages?



Panel C: Did students know their parents
were receiving text messages?



Note: The Figure displays group averages across groups. The dependent variables are: fraction of weeks that teachers filled-in the platform (Panel A), fraction of parents that acknowledged receiving messages (Panel B), and fraction of students that knew their parents were receiving messages (Panel C). The Figure also displays 90% confidence interval. All panels show the average difference between groups. This difference between categories was estimated through a simple regression. Significance levels are denoted by * if p<0.1, ** if p<0.05 and *** if p<0.01.

Over the course of the 18 weeks, 66% of teachers inputted students' information through the platform in a typical week. Since this figure was slightly lower for sub-samples A and C relative to sub-sample D, students assigned to the information treatment are associated with a 2 p.p. lower messaging rate. In Supplementary Appendix C.3, we show that our results are robust to selection on unobservable variables by dropping observations from classrooms with the highest and lowest response rates, such as to equalize the rate at which teachers filled in the platform over the course of the 18 weeks across sub-samples (analogously to the bounding procedure in Lee, 2009).

At the end line surveys, we asked parents whether they had received text messages from the school, and asked students whether they knew their parents were getting such text messages. While 46% of parents in the control group acknowledge receipt of text messages (principals could send up to two notifications a month about school events to *all* parents, even in the pure control group), that figure is 90% across treatment groups – close to the expected 100%, and statistically different from the control group. Meanwhile, 74% of students across treatment arms acknowledged their parents received text messages from the school, as opposed to 40% in the control group. Since over 50% of parents reported a different mobile phone number for their child at the enrollment form, this is not just an artifact of parents and children sharing the same handset; rather, it hints at communication between parents and children as a result of the text messages.

C.2 Beliefs vs. actual report card attendance

We have two available measures of students' math attendance: actual absences reported by teachers through the platform and 4th-quarter report cards. In Table 1, we showed that the information intervention significantly increased parents' accuracy about their child's attendance. Now, in this section of the Appendix, we document the correlation between parents' beliefs and report card attendance.

We asked parents to guess their children's attendance over the 4th quarter (rather than over the previous 3 weeks, as we had done at baseline) because we wanted to compare their guess to actual student attendance included in children's report cards. Different from Dizon-Ross (2019), however, 4th-quarter report cards had still not been made available to parents at the time of our survey. This Appendix shows that, without report cards in hand, parents targeted with high-frequency information on their children's absences became no more accurate than control parents about their children's *cumulative* absences over that period. Conversely, the analysis in the main text contrasts parents' estimates at end line to students' average absences reported by teachers through the platform over the 4th quarter – matching the typical content communicated to treated parents over SMS, rather than requiring them to recall and sum over their full history of absenteeism data.

At the baseline survey, parents were asked to provide their best estimate of how many times their child had missed math classes over the past three weeks from four categories (0 absences; 1-2; 3-5; or more than 5). Thus, a comparison between beliefs and report card attendance requires the total number of report card absences. However, only the fraction of absences was registered in students' report cards. Since the total number of classes varies from one class to another (and was also not recorded), it is not immediate how to recover the absolute number of missed classes.

We propose a simple algorithm to recover the outcome of interest. It is based on the facts that absences are integer numbers, and that the total number of classes is the same for all students within each class.

Let a_{sc} be the number of absences for students s in class c and N_c. We observe the fraction of absences f_{sc}. Apart from slight rounding differences, we expect that $f_{sc} * N_c \in \mathbb{Z}_+$. Therefore, we can simulate values of N_c and, for each of them, calculate the distance between the implied a_{sc} and the closest integer. Formally:

$$\boldsymbol{N}^* = \underset{N_c \in [20,75]}{\arg\min} \sum_c \left[\left| f_{sc} * N_c - nint(f_{sc} * N_c) \right| \right]$$

where $|.|$ is the absolute value function and $nint(.)$ is the nearest integer function.

Having found the total number of classes, we can directly calculate the number of absences a_{sc} for each student.

We test this algorithm with 3rd-quarter attendance (where we have both the number of absolute absences and the fraction of absences). We are able to recover the correct number of absences for over 95% of students.

We can see in Table C.1 that neither the information or salience treatments increase the accuracy of parents' beliefs about 4th-quarter report card attendance – which had still not been made available to parents at the time of the end-line survey. Both estimates are very close to zero and statistically insignificant.

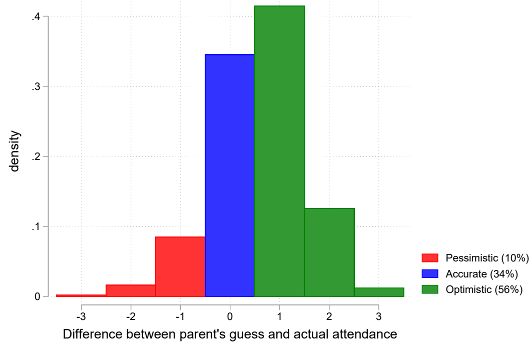
Table C.1: Parents' accuracy about report card attendance

	(1)	(2)	(3)	(4)
	Baseline beliefs		End-line beliefs	
Actual absences	0.211***	0.186***	0.150***	0.152***
	[0.025]	[0.024]	[0.024]	[0.023]
Child-specific information	-0.016		-0.137*	
	[0.050]		[0.075]	
Salience		0.033		0.029
		[0.050]		[0.079]
Actual absences x Information	-0.011		0.002	
	[0.032]		[0.057]	
Actual absences x Salience		-0.047		0.012
		[0.034]		[0.034]
Observations	3,085	3,174	2,967	2,862
Classroom FE	No	No	No	No
Student-level controls	Yes	Yes	Yes	Yes
R-squared	0.112	0.120	0.109	0.112

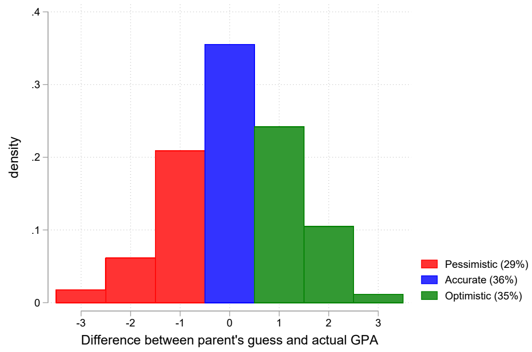
Note: Correlation between parents' baseline and end-line beliefs about their children's school attendance and actual attendance. At the baseline survey, parents were asked to provide their best estimate of how many times their child had missed math classes over the past three weeks. Parents could pick one answer from four categories (0 absences; 1-2; 3-5; or more than 5). Report card attendance was computed according to the algorithm described in this Appendix. Regressions include either an indicator variable for child-specific information and its interaction with actual absences (Columns 1 and 3) or an indicator variable for salience messages and its interaction with actual absences (Columns 2 and 4). Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Figure C.2: Parents' accuracy gap wrt their child's baseline math attendance and math GPA

Panel A: Baseline math attendance



Panel B: Baseline math GPA



Note: Panel A displays the difference between parents' guesses and baseline student attendance in math classes, and in Panel B, that between parents' guesses and baseline student math GPA. A value of 0 indicates that parents were accurate; positive values indicate that they were pessimistic; and negative values, that they were optimistic relative to the ground truth.

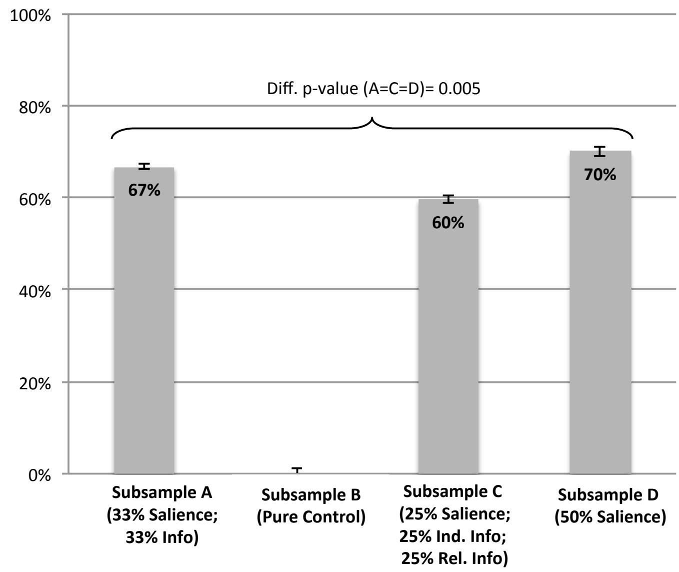
C.3 Bounding treatment effects

As shown in Figure C.3, the average number of times teachers filled in the platform over the course of 18 weeks was not statistically identical across all sub-samples. To test if ours results are sensitive to selective non-response, we trim observations (along the lines of Lee, 2009), respecting the cluster structure of the data: we drop classrooms until we equalize the average number of times teachers filled in the platform across sub-samples.

We do so by dropping 7 classrooms from schools from sub-sample D (where students were assigned to either salience or control), for which teachers had filled in the platform each and every week (over 18 weeks), and 27 classrooms from sub-sample C (where 25% of students were assigned to salience, 25% to child-specific information, 25% to information framed relatively to the classroom median, and 25% to control), for which teachers filled in the platform 3 times or less over the course of 18 weeks. This procedure maximizes sample size while eliminating selective non-response; in this new sample, the average number of times teachers fill the platform is statistically identical across sub-samples.

We then replicate our main results on school transcripts and test score (showed in Table 3) as well as the analyses testing if there is interaction between salience and information (showed in Table C.18). Results are showed in tables C.2 and C.3.

Figure C.3: Average number of times teachers filled the platform by sub-sample during the 18 week period



Note: The Figure displays average number of times teachers filled the platform by sub-sample during the 18-weeks period across samples. The Figure also displays 90% confidence interval. We show the p-value for the joint test that averages are equal across samples A, C and D. Significance levels are denoted by * if p<0.1, ** if p<0.05 and *** if p<0.01.

Table C.2: Robustness: Administrative educational outcomes (equalizing SMS received by sub-sample)

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.019***	0.085***	0.030***	0.108**
	[0.006]	[0.032]	[0.011]	[0.045]
Information	0.019***	0.070**	0.026**	0.110**
	[0.006]	[0.032]	[0.011]	[0.046]
Control Mean	0.875	0.000	0.938	-0.000
P-value diff. [Info] -[Salience]	0.994	0.368	0.323	0.929
Sample Size	11951	11951	11951	11951
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes

Note: The Table displays treatment effects of child-specific information and salience messages on the following administrative outcomes: 4th-quarter attendance in math classes (Column 1); 4th-quarter math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3), and math standardized test scores (Column 4). To equalize the number of SMS received, 7 classrooms from the salience-only sample were excluded, where teachers had filled the platform all the 18 weeks; and 27 classrooms from the sub-sample containing all treatments (25% salience, 25% ind. info; 25% relative info, 25% control), where teacher participation was low (teachers filled 3 times or less the platform) were also excluded. GPA and standardized test scores were normalized relative to the distribution of the pure control group. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table C.3: Spillovers from information (equalizing SMS received by sub-sample)

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Panel A: Full Sample				
Salience	0.016**	0.068**	0.027**	0.110**
	[0.006]	[0.033]	[0.011]	[0.047]
Information	0.019***	0.070**	0.026**	0.110**
	[0.006]	[0.032]	[0.011]	[0.046]
Salience No-information sub-sample	0.002	0.030	0.002	-0.004
	[0.004]	[0.029]	[0.009]	[0.044]
Sample Size	11951	11951	11951	11951
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes
Panel B: Sample B and D				
Salience No-information sub-sample	0.034***	0.172***	0.059***	0.106*
	[0.006]	[0.042]	[0.012]	[0.055]
Sample Size	3455	3455	3455	3455
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes

Note: The Table displays treatment effects of child-specific information, salience messages, and an interaction between treatments on the following administrative outcomes: 4th-quarter attendance in math classes (Column 1); 4th-quarter math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3), and math standardized test scores (Column 4). To equalize the number of SMS received, 7 classrooms from the salience-only sample were excluded, where teachers had filled the platform all the 18 weeks; and 27 classrooms from the sub-sample containing all treatments (25% salience, 25% ind. info; 25% relative info, 25% control), where teacher participation was low (teachers filled 3 times or less the platform) were also excluded. GPA and standardized test scores were normalized relative to the distribution of the pure control group. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

C.4 Heterogeneous treatment effects

C.4.1 Heterogeneous treatment effects by platform scores

As described in section 3, a web-platform was created specifically such that teachers could provide timely information about their students' behavior. Math teachers at treated schools were oriented to fill in the platform every week with that week's dimension of students' behavior: attendance, tardiness or homework completion, over the course of 18 weeks. Teachers were to fill in information with respect to each dimension of students' behavior accounting for the past three weeks[37]. The system required teachers to fill in information for all their students.

This appendix presents the results for treatment effects on the outcomes recorded weekly by teachers on the online platform. Because teachers did not fill in any content for pure control schools, the estimates are relative to the control group within classroom.

Each week, teachers evaluated students using a 4 point scale, where 1 was the minimum and 4 was the maximum. For this analysis, we reverse-coded scores for tardiness, to normalize estimates across dimensions such that a positive coefficient always means a positive outcome. We estimate the following model:

$$Y_{i,c,s} = \alpha + \beta_1 Salience_{i,c,s} + \beta_2 Info_{i,c,s} + \sum \gamma_k X_{k,i,c,s} + \theta_s + \varepsilon_{i,c,s}$$

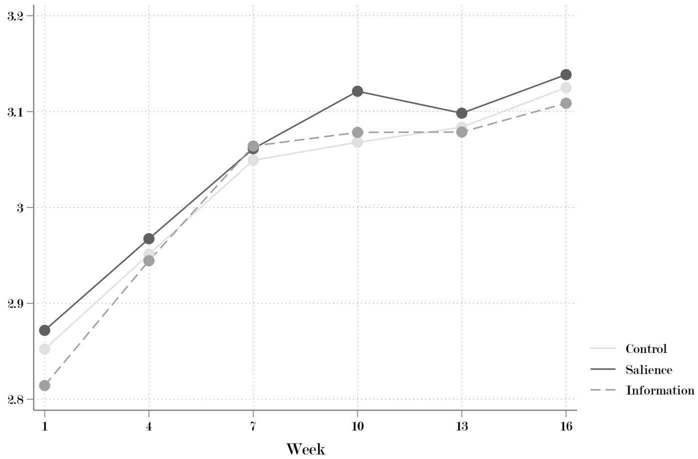
where $Y_{i,c,s}$ denotes the weekly score of each dimension for student i in classroom c of stratum s, the within-class control stand for the reference category (omitted indicator variable), $X_{k,i,c,s}$ is a matrix of student's characteristics, θ_s are randomization stratum FE, and $\epsilon_{i,c,s}$ is an error term, clustered at the classroom level.

We start by plotting coefficients week-by-week in Figure C.4. As behaviors rotate weekly, we can plot coefficients in 3-week intervals.

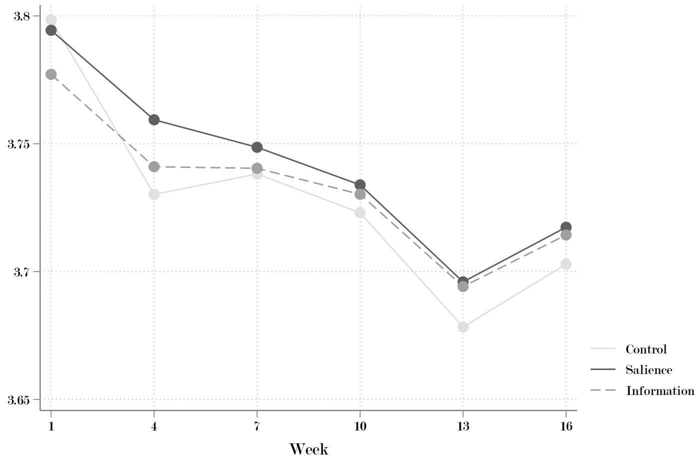
[37] Students are scheduled to have 6 Math classes per week.

Figure C.4: Weekly platform scores, by treatment arm

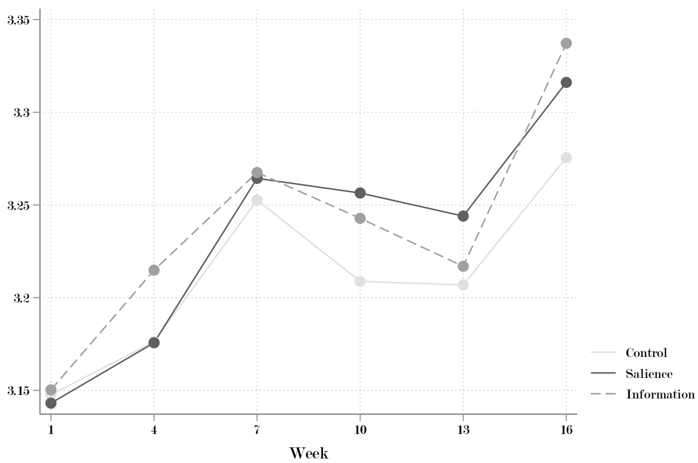
Panel A: Weekly effect on attendance



Panel B: Weekly effect on punctuality



Panel C: Weekly effect on homework completion



Note: This Figure reports weekly platform scores, by treatment arm, for three administrative dependent variables: attendance in math classes (Panel A); punctuality (Panel B) and homework completion (Panel C). Estimates include dummies for receiving child-specific information and salience messages. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for classroom fixed-effects. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Despite the large spillovers within classrooms that we document in the main text, we can see that the curves for salience and information interventions drift clearly above the control one, particularly so for punctuality and homework completion. For those two dimensions of behavior, the difference between the treatment arms (information and salience) and the control group clearly did not exist the first time teachers filled in the platform, and then gradually increased over time.

Next, we analyze heterogeneity in treatment effects with respect to attendance, punctuality and homework completion scores entered into the platform by teachers. Dizon-Ross (2019) documents that higher accuracy leads to higher inequality: the informational intervention decreases misallocation of educational investments, with parents increasing (decreasing) investments in high-(low-)performing children, relative to the control group. Is it also the case when it comes to our informational intervention? How about when it comes to salience messages?

To answer those questions, we interact treatment indicators with the average score entered by teachers into the platform for each student over the course of the experiment, estimating different regressions for each dimension of student effort. We are interested in whether the effects of child-specific information and salience messages vary systematically with average platform scores. In Dizon-Ross (2019), parents of low-performing children reduce educational investments relative to the control group, while the opposite is true for parents of high-performing children. In our regression, that would be equivalent to a negative coefficient for the treatment indicator, and a positive coefficient for its interaction with average platform scores. Tables C.4-C.6 document the results.

Table C.4: Heterogeneous treatment effects by average platform scores (attendance)

	Reported attendance			
	Math attendance	Math GPA	Math standardized test score	Grade promotion
	(1)	(2)	(3)	(4)
Child-specific information	0.00465	-0.00219	0.0135	-0.0335
	[0.0163]	[0.165]	[0.0844]	[0.0296]
Child-specific information x average reported	-0.00125	0.0142	-0.000167	0.0102
	[0.00492]	[0.0529]	[0.0274]	[0.00884]
Salience	-0.0193	0.179	0.0468	-0.0103
	[0.0172]	[0.160]	[0.0850]	[0.0294]
Salience x average reported	0.00603	-0.0544	-0.0199	0.00347
	[0.00511]	[0.0508]	[0.0269]	[0.00858]
Average reported	0.0966***	1.129***	0.563***	0.0721***
	[0.00420]	[0.0471]	[0.0244]	[0.00710]
Observations	12,641	12,337	12,230	12,519
Classroom Fixed-effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R-squared	0.411	0.319	0.299	0.193

Note: This Table reports heterogeneous treatment effects of child-specific information and salience messages by students' average reported scores on four administrative dependent variables: attendance in math classes (Column 1); math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3) and math standardized test scores (Column 4). GPA and standardized test scores were normalized relative to the distribution of the comparison group. All Columns include dummies indicating students whose parents received child-specific information and salience messages, and interactions between these treatment variables and average reported attendance. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for classroom fixed-effects. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table C.5: Heterogeneous treatment effects by average platform scores (punctuality)

	Reported punctuality			
	Math attendance	Math GPA	Math standardized test score	Grade promotion
	(1)	(2)	(3)	(4)
Child-specific information	-0.0264	-0.380	-0.203	-0.130**
	[0.0310]	[0.274]	[0.147]	[0.0519]
Child-specific information x average reported	0.00725	0.111	0.0556	0.0338**
	[0.00802]	[0.0732]	[0.0395]	[0.0133]
Salience	-0.0433*	0.0191	-0.0447	-0.0931*
	[0.0250]	[0.239]	[0.129]	[0.0527]
Salience x average reported	0.0117*	0.00199	0.00914	0.0252*
	[0.00650]	[0.0639]	[0.0344]	[0.0136]
Average reported	0.0718***	1.363***	0.675***	0.0962***
	[0.00659]	[0.0777]	[0.0482]	[0.0119]
Observations	12,208	11,913	11,808	12,096
Classroom Fixed-effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R-squared	0.248	0.279	0.263	0.193

Note: This Table reports heterogeneous treatment effects of child-specific information and salience messages by students' average reported scores on four administrative dependent variables: attendance in math classes (Column 1); math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3) and math standardized test scores (Column 4). GPA and standardized test scores were normalized relative to the distribution of the comparison group. All Columns include dummies indicating students whose parents received child-specific information and salience messages, and interactions between these treatment variables and average reported punctuality. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for classroom fixed-effects. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table C.6: Heterogeneous treatment effects by average platform scores (homework completion)

	Reported homework completion			
	Math attendance	Math GPA	Math standardized test score	Grade promotion
	(1)	(2)	(3)	(4)
Child-specific information	-0.0231	-0.115	-0.0740	-0.0462
	[0.0175]	[0.147]	[0.0802]	[0.0338]
Child-specific information x average reported	0.00650	0.0315	0.0178	0.0123
	[0.00491]	[0.0450]	[0.0244]	[0.00936]
Salience	-0.0341*	-0.00518	-0.0340	-0.0206
	[0.0177]	[0.142]	[0.0770]	[0.0352]
Salience x average reported	0.0103**	0.00474	0.00686	0.00591
	[0.00493]	[0.0432]	[0.0236]	[0.00986]
Average reported	0.0528***	1.687***	0.847***	0.0967***
	[0.00393]	[0.0425]	[0.0230]	[0.00859]
Observations	12,025	11,737	11,624	11,922
Classroom Fixed-effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R-squared	0.277	0.522	0.497	0.244

Note: This Table reports heterogeneous treatment effects of child-specific information and salience messages by students' average reported scores on four administrative dependent variables: attendance in math classes (Column 1); math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3) and math standardized test scores (Column 4). GPA and standardized test scores were normalized relative to the distribution of the comparison group. All Columns include dummies indicating students whose parents received child-specific information and salience messages, and interactions between these treatment variables and average reported homework completion. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for classroom fixed-effects. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

As one would expect, higher attendance, punctuality and homework completion positively and systematically correlate with better educational outcomes in Tables C.4-C.6. Although large spillovers from the interventions on within-classroom control students make treatment effects hard to detect, the tables document interesting patterns for conditional impacts. For nearly all outcomes across attendance, punctuality and homework completion, the coefficient of child-specific information is negative, and that of its interaction with average platform scores, positive, consistent with parents 'doubling down' on students who are already doing well, as in Dizon-Ross (2019). Having said that, the *exact same patterns* also hold when it comes to salience messages: its coefficient and that of its interaction with average platform scores have the same sign as those of child-specific information across all columns in each table. In some cases, such conditional impacts are even larger and more precisely estimated when it comes to salience messages, as in the case of treatment effects on 4th-quarter math attendance conditional on student punctuality.

In our experiment, conditional impacts manifest mostly as negative treatment effects on low-effort students targeted by the interventions, relative to low-effort students in the control group. For instance, we estimate that the informational intervention decreases the likelihood of advancing to high school by 9.6 p.p. for students who are always late, and increases it by 0.5 p.p. for those who are never late.[38] Symmetrically, we estimate that salience messages decrease the likelihood of advancing to high school by 6.8 p.p. for students who are always late, and increase it by 0.8 p.p. for those who are never late.[39]

[38] Respectively, -0.130 + (0.0338 x 1) = -0.0962 and -0.130 + (0.0338 x 4) = 0.0052, since teachers rate student effort in each dimension from 1 to 4.

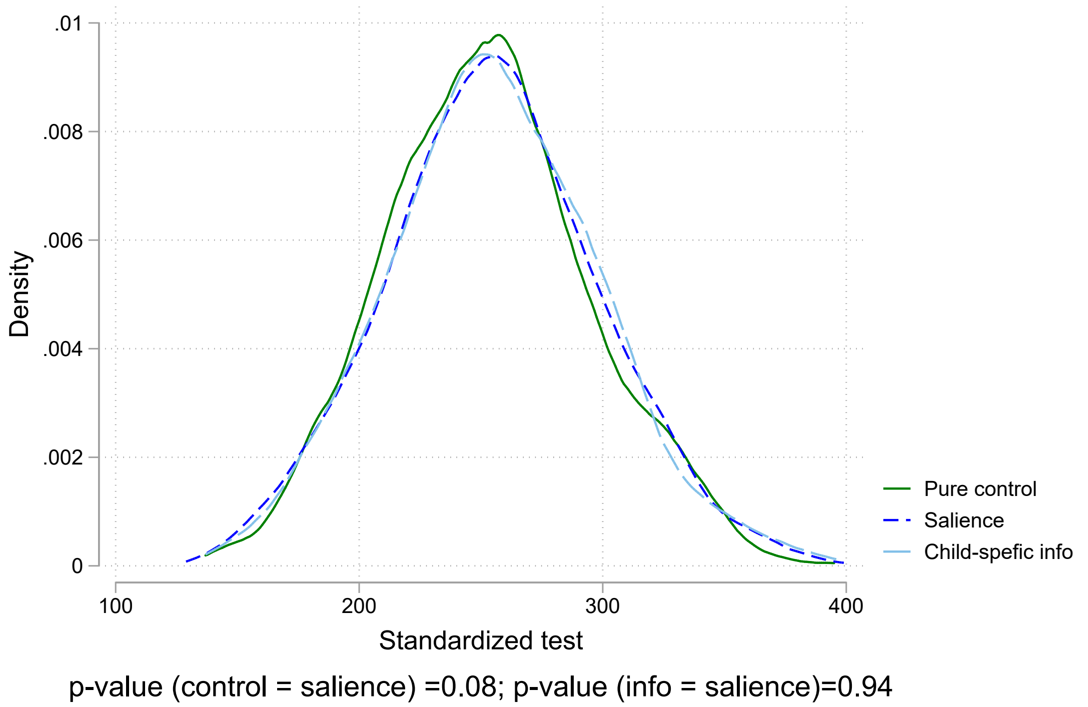
[39] Respectively, -0.0931 + (0.0252 x 1) = -0.0679 and -0.0931 + (0.0252 x 4) = 0.0077.

C.4.2 Additional results on heterogeneous treatment effects

This Appendix compiles additional results on heterogenous treatment effects.

Figure C.5 starts by plotting the densities of math standardized test scores within each treatment arm to show that the lack of differences between the average effects of child-specific information and salience messages does not mask different patterns for their distributional effects.

Figure C.5: Distributional Effects
Math standardized test scores



Note: Effect of child-specific information and salience messages across the distribution of students' math standardized test scores for each treatment arm. Data used are from administrative records. The standardized test (Saresp) has a 400-point scale, where zero is the minimum score. P-values reported for Kolmogorov-Smirnov tests of the hypothesis that pairs of distributions are not statistically different.

The figure shows that the distributions of standardized test scores of students whose parents were assigned to child-specific information or salience messages are equally shifted to the right relative to that of pure control students. A Kolmogorov-Smirnov test rejects the hypothesis that the salience and pure control test score distributions are the same (at the 10% level), and fails to reject equality of the child-specific information and salience distributions (p=0.94).

Since administrative outcomes other than standardized test scores have discrete ranges (e.g., math GPA can only take integer values between 0 and 10), another way to analyze distributional effects is estimating heterogeneous treatment effects according to students' baseline educational standing. Table C.7 replicates the analyses separately for students below and above the median 1st-quarter math attendance.

Table C.7: Heterogeneous treatment effects (by students' attendance at baseline)

	Math Attendance (p.p.)		Math GPA (std.)		Promotion Rate (p.p.)		Math Standarized Test (std.)	
	Below median	Above median	Below median	Above median	Below median	Above median	Below median	Above median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Child-specific information	0.023***	0.019***	0.066*	0.095***	0.032**	0.018*	0.095*	0.135**
	[0.008]	[0.006]	[0.040]	[0.037]	[0.016]	[0.009]	[0.052]	[0.059]
Salience	0.023***	0.018***	0.116***	0.070*	0.045***	0.016	0.107**	0.092
	[0.008]	[0.006]	[0.039]	[0.037]	[0.016]	[0.010]	[0.053]	[0.058]
Control mean	0.85	0.90	-0.23	0.25	0.92	0.96	-0.12	0.13
p-value diff. [Info] -[Salience]	0.95	0.54	0.01	0.26	0.05	0.65	0.70	0.16
p-value diff [Info below med.]- -[Info above med.]	0.48		0.49		0.26		0.52	
p-value diff [Saliece below med.] -[Salience above med.]	0.32		0.27		0.02		0.81	
Observations	6862	5715	6862	5715	6862	5715	6862	5715
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Heterogeneous treatment effects of child-specific information and salience messages by students' baseline attendance on four administrative dependent variables: attendance in math classes (Columns 1 and 2); math GPA (Columns 3 and 4); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Columns 5 and 6) and math standardized test scores (Columns 7 and 8). GPA and standardized test scores were normalized relative to the distribution of the comparison group. Odd columns report treatment effects on students who had below median baseline attendance and even columns report treatment effects on students who had above median baseline attendance. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. For each variable we report p-values for three different tests: 1) equality of information and salience coefficients; 2) equality of information coefficients below and above the median and; 3) equality of salience coefficients below and above the median. This Table includes all students in the balanced sample, samples A, B, C, and D (see Figure 1) * if p<0.1, ** p<0.05 and *** if p<0.01.

The table shows that, as in the main analyses, salience effect sizes are never statistically lower than those of child-specific information. Quite the contrary, salience effects are actually *higher* than those of child-specific information among below-median attendance students (significant at the 5% level for math GPA and grade promotion). Among those students, the effect of salience messages is almost two-fold that of information on math GPA (nearly 0.12 s.d.), and nearly 50% higher when it comes grade promotion. In effect, the effect of salience messages on the likelihood of advancing to high school among students with below-median attendance is 3-fold that among above-median students (p=0.02). Those patterns are striking, as intuition suggests that these students would be the ones most likely to benefit from informational interventions in face of parent-child moral hazard.

Next, we estimate treatment effects by student characteristics. We restrict attention to the sample of 9,539 students with non-missing data for parents' behavior and aspirations, student behavior, and administrative educational outcomes. First, we analyze heterogeneity by student gender. Table C.8 replicates aggregate treatment effects on educational outcomes for this sub-sample, followed by Table C.9, which breaks those estimates down by gender, and Tables C.10, C.11 and C.12, which compile treatment effects on parental engagement and aspirations and on students' time use by gender as well. We find that treatment effects on educational outcomes are concentrated on boys; consistently, effects on engagement and aspirations are also concentrated on boys' caregivers.

Second, Table **??** estimates heterogeneous treatment effects for students above and below the median baseline attendance. We find that students with below-median baseline attendance actually benefit to a lesser extent from information framed relative to the classroom median, while those with above-median baseline attendance benefit to a greater extent.

Third, we present regression results for heterogeneous treatment effects with respect to parents' baseline accuracy gap, in Table C.14 and Figure C.6. Consistent with the results in the main text, we find that the effects of the informational intervention increase with the extent of the moral hazard problem at baseline: the more optimistic parents were about their children's school effort before the onset of the intervention, the larger the effect sizes of child-specific information (significantly so for math attendance and the probability of grade promotion). Strikingly, the same is true when it comes to the effects of salience messages.

Last, Table C.15 presents heterogeneous treatment effects by splitting the sample according to willingness to receive information (WTR). WTR was measured at the baseline survey; parents were asked about their interest in receiving information on their child's school attendance, given the following options: no interest, some interest, or great interest (See Appendix D.4). We define low willingness to receive information as an indicator variable equal to 1 if a parent expressed no or some interest in receiving information about school attendance, and 0 otherwise. The lower sample size reflects the fact that we can only use parents who answer our baseline phone survey in this table. We find that low-WTR parents benefit the most from the interventions.

Table C.8: Average treatment effects on administrative educational outcomes

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.016***	0.072**	0.030**	0.075
	[0.006]	[0.034]	[0.012]	[0.053]
Information	0.017***	0.058*	0.026**	0.091*
	[0.006]	[0.034]	[0.012]	[0.053]
Control Mean	0.889	0.000	0.945	0.000
P-value diff. [Info] -[Salience]	0.634	0.420	0.477	0.510
Sample Size	9539	9539	9539	9539
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes

Note: Treatment effects of child-specific information and salience messages on the following administrative outcomes: 4th-quarter attendance in math classes (Column 1); 4th-quarter math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3), and math standardized test scores (Column 4). The sample is restricted to the sample of students with non-missing data for parents' behavior and aspirations, student behavior, and school transcripts and test scores. GPA and standardized test scores were normalized relative to the distribution of the pure control group. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table C.9: Treatment effects on administrative educational outcomes - Boys vs. Girls

	Boys				Girls				Diff. (Girls)-(Boys)			
	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)	(5) Math Attendance (p.p.)	(6) Math GPA (std.)	(7) Promotion Rate (p.p.)	(8) Math Standardized Test (std.)	Math Attendance (p.p.)	Math GPA (std.)	Promotion Rate (p.p.)	Math Standardized Test (std.)
Salience	0.02***	0.13***	0.04**	0.10*	0.01*	0.02	0.01	0.04	-0.01	-0.12**	-0.03*	-0.06
	[0.01]	[0.04]	[0.02]	[0.06]	[0.01]	[0.04]	[0.01]	[0.06]	[0.01]	[0.05]	[0.02]	[0.06]
Information	0.02***	0.12***	0.04**	0.13**	0.01**	0.00	0.01	0.05	-0.01	-0.12**	-0.03*	-0.07
	[0.01]	[0.04]	[0.02]	[0.06]	[0.01]	[0.04]	[0.01]	[0.06]	[0.01]	[0.05]	[0.02]	[0.07]
Control Mean	0.88	-0.22	0.92	-0.02	0.89	0.23	0.97	0.02				
P-value diff. [Info] -[Salience]	0.68	0.65	0.86	0.55	0.32	0.55	0.47	0.71				
Sample Size	4654	4654	4654	4654	4885	4885	4885	4885				
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes				
Student controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes				

Note: This Table reports heterogeneous treatment effects of child-specific information and salience messages by students' gender on four administrative dependent variables: attendance in math classes (Columns 1 and 5); math GPA (Columns 2 and 6); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Columns 3 and 7) and math standardized test scores (Columns 4 and 8). GPA and standardized test scores were normalized relative to the distribution of the comparison group. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table C.10: Treatment effects on parental engagement - Boys vs. Girls

	Boys			Girls			Diff. (Girls)-(Boys)		
	(1) Academic activities	(2) Incentives	(3) Talk	(4) Academic activities	(5) Incentives	(6) Talk	Academic activities	Incentives	Talk
Salience	0.13**	0.07	0.14***	0.00	0.11*	0.11*	-0.12*	0.04	-0.03
	[0.06]	[0.06]	[0.05]	[0.06]	[0.06]	[0.06]	[0.07]	[0.08]	[0.07]
Information	0.13**	0.05	0.17***	0.05	0.09	0.12**	-0.08	0.03	-0.04
	[0.06]	[0.06]	[0.05]	[0.07]	[0.06]	[0.06]	[0.08]	[0.08]	[0.07]
Control Mean	-0.02	-0.02	0.00	0.02	0.02	-0.00			
P-value diff. [Info] -[Salience]	0.86	0.66	0.43	0.21	0.48	0.63			
Sample Size	4654	4654	4654	4885	4885	4885			
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes			
Student controls	Yes	Yes	Yes	Yes	Yes	Yes			

Note: Heterogeneous treatment effects of child-specific information and salience messages on parental engagement by students' gender. Variables are based on students end-line survey. They were asked to state how often their parents engage in certain activities (never, almost never, sometimes, almost always, always). Out of the 12 questions, factor analysis was performed to create 3 variables of parental behavior: academic activities (help with homework, help to organize school material, participate in school-parent meetings, talk to the teachers); incentives (incentivize to not miss school, to not be late, to study and to read); talk (ask about homework, ask about grades, ask about day in school and classes). Variables were normalized relative to the distribution of the comparison group (pure control). Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table C.11: Treatment effects on parents' aspirations - Boys vs. Girls

	Boys	Girls	Diff. (Girls)-(Boys)
	(1) Parents' Aspirations College	(2) Parents' Aspirations College	Parents' Aspirations College
Salience	0.12**	0.08	-0.04
	[0.06]	[0.05]	[0.08]
Information	0.10*	0.09*	-0.02
	[0.06]	[0.05]	[0.08]
Control Mean	-0.09	0.09	
P-value diff. [Info] -[Salience]	0.76	0.79	
Sample Size	4654	4885	
Randomization strata FE	Yes	Yes	
Student controls	Yes	Yes	

Note: Heterogeneous treatment effects of child-specific information and salience messages on parental aspirations by students' gender. The dependent variable is a dummy variable for parents' aspirations that indicates whether students answered that their parents expect them to go to college or not. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table C.12: Treatment effects on students' time use - Boys vs. Girls

	Boys			Girls			Diff. (Girls)-(Boys)		
	(1) Academic activities	(2) Reading activities	(3) Other activities	(4) Academic activities	(5) Reading activities	(6) Other activities	Academic activities	Reading activities	Other activities
Salience	0.19***	0.17**	-0.09	0.06	0.06	-0.13**	-0.13*	-0.11	-0.04
	[0.06]	[0.07]	[0.06]	[0.07]	[0.07]	[0.07]	[0.07]	[0.08]	[0.08]
Information	0.18***	0.15**	-0.13*	0.12*	0.08	-0.09	-0.06	-0.07	0.04
	[0.05]	[0.07]	[0.07]	[0.07]	[0.08]	[0.07]	[0.07]	[0.08]	[0.08]
Control Mean	-0.14	-0.07	-0.18	0.14	0.08	0.18			
P-value diff. [Info] -[Salience]	0.81	0.73	0.38	0.13	0.65	0.26			
Sample Size	4654	4654	4654	4885	4885	4885			
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes			
Student controls	Yes	Yes	Yes	Yes	Yes	Yes			

Note: Heterogeneous treatment effects of child-specific information and salience messages on students' time-use by gender. Variables are based on the end-line survey. Students were requested to answer how many hours per day (0, 15 minutes, 30 minutes, 1 hour, 2 hours, more than 2 hours) they spend in each of the following activities: i. studying at home on weekdays; ii. studying at home on weekends; iii. studying at home the day before a test; iv. reading a book; v. reading the newspaper; vi. reading magazines; vii. watching TV; viii. navigating on the internet or social media; and ix. helping with housework. Factor analysis was performed to create three variables of student's behavior: academic activities (items i, ii and iii); reading activities (items iv, v and vi) and other activities (items vii, viii and ix). Variables were normalized relative to the distribution of the comparison group (pure control), such that the mean and standard deviation of the comparison group is zero and one, respectively. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table C.13: Heterogeneous treatment effects of framing child-specific information (by students' attendance at baseline)

	Math Attendance (p.p.)		Math GPA (std.)		Promotion Rate (p.p.)		Math Standarized Test (std.)	
	Below median	Above median	Below median	Above median	Below median	Above median	Below median	Above median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Child-specific information	0.023***	0.019***	0.068*	0.088***	0.036**	0.019**	0.093*	0.115*
	[0.008]	[0.006]	[0.040]	[0.037]	[0.009]	[0.009]	[0.053]	[0.060]
Relative information	0.025***	0.019***	0.057	0.120**	0.018*	0.014	0.103*	0.208***
	[0.009]	[0.007]	[0.049]	[0.053]	[0.016]	[0.012]	[0.066]	[0.076]
Salience	0.023***	0.018***	0.116***	0.070*	0.045***	0.016	0.107**	0.092
	[0.008]	[0.006]	[0.039]	[0.037]	[0.016]	[0.010]	[0.053]	[0.058]
p-value diff. [Child-specific Info]--[Relative Info]	0.69	0.97	0.09	0.50	0.19	0.57	0.84	0.10
Observations	6862	5715	6862	5715	6862	5715	6862	5715
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

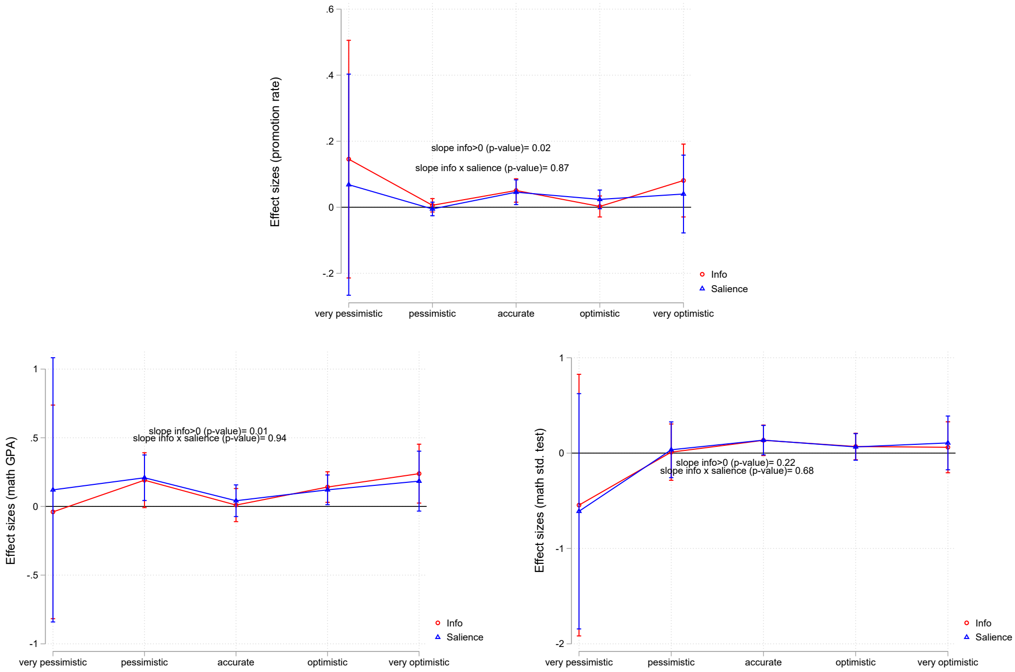
Note: Treatment effects of child-specific information, child-specific relative information, and salience messages on the following administrative outcomes: 4th-quarter attendance in math classes (Column 1); 4th-quarter math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3), and math standardized test scores (Column 4). We present the results separately for students below-median baseline attendance (odd columns) and those above-median (even columns). Parents in the relative information treatment received child-specific information framed relative to the median behavior of their classmates. GPA and standardized test scores were normalized relative to the distribution of the pure control group. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. In all columns, we present the Wald test that the estimated coefficients for child-specific and relative information are equal. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table C.14: Effects on attendance, grades and grade promotion for additional experiments

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Child-specific information	0.023***	0.063	0.026**	0.099
	[0.009]	[0.053]	[0.013]	[0.072]
Salience	0.024***	0.083	0.032**	0.088
	[0.009]	[0.051]	[0.014]	[0.072]
Information x Accuracy bracket	0.014*	0.072	0.011*	-0.031
	[0.009]	[0.045]	[0.018]	[0.064]
Salience x Accuracy bracket	0.016*	0.044	0.005*	0.005
	[0.008]	[0.043]	[0.018]	[0.063]
Control Mean	0.875	0.000	0.938	-0.000
P-value diff. [Info] -[Salience]	0.851	0.552	0.398	0.802
Observations	3556	3556	3556	3556
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes
R-squared	0.212	0.605	0.090	0.347

Note: Treatment effects of child-specific information and salience messages on 4th-quarter attendance in math classes (Column 1); 4th-quarter math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3), and math standardized test scores (Column 4). GPA and standardized test were normalized relative to the distribution of the pure control group. Student controls include gender, age, race, baseline grades and attendance, and parents' controls include gender, age, race, family income, education, interactions between each treatment dummy and accuracy bracket. We compute baseline accuracy by subtracting parents' guess from the actual number of absences. We categorize parents as 'very pessimistic' (gap $\in [-3, -2]$), 'pessimistic' (gap $= -1$), 'accurate' (gap$= 0$), 'optimistic' (gap$= 1$), and 'very optimistic' (gap $\in [2, 3]$). We also control for randomization strata fixed-effects, indicator variables for each accuracy bracket, and interactions between accuracy brackets and the within-class control group. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Figure C.6: Heterogeneous treatment effects on alternative outcomes, by parents' baseline accuracy



Note: Heterogeneous treatment effects of child-specific information and salience messages by parents' baseline accuracy with respect to attendance in math classes. At the baseline survey, parents were asked to provide their best estimate of how many times their child had missed math classes over the past three weeks, choosing among four brackets: 0 absences; 1-2; 3-5; or more than 5. Since administrative data on students' 1st-quarter absences were only available for the whole quarter ($\sim$ 9 weeks), actual absences are computed by dividing that indicator by 3. We compute baseline accuracy by subtracting parents' guess from the actual number of absences. We categorize parents as 'very pessimistic' (gap $\in [-3, -2]$), 'pessimistic' (gap $= -1$), 'accurate' (gap$= 0$), 'optimistic' (gap$= 1$), and 'very optimistic' (gap $\in [2, 3]$). Student controls include gender,age, race, baseline grades and attendance, and parents' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. We also report p-values for the hypothesis test that effect sizes of child-specific information are increasing in parents' baseline accuracy gap, and that this slope is equal between the information and salience groups. OLS coefficients and 90% confidence intervals from Table C.14.

Table C.15: Heterogeneous treatment effects by parents' willingness to receive information (WTR)

	School Transcripts and Test Scores				Parents' Beliefs	
	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)	(5) Accuracy Math Attendance (p.p.)	(6) Accuracy Math GPA (p.p.)
	Low willingness to receive information (WTR) (63.3%)					
Salience	0.03***	0.12**	0.03*	0.08	0.02	0.10**
	[0.01]	[0.05]	[0.02]	[0.07]	[0.04]	[0.04]
Information	0.03***	0.09*	0.04**	0.16**	-0.03	0.02
	[0.01]	[0.05]	[0.02]	[0.07]	[0.04]	[0.04]
Control Mean	0.86	-0.06	0.93	-0.05	0.21	0.23
P-value diff. [Info] -[Salience]	0.57	0.42	0.56	0.10	0.13	0.04
Sample Size	2578	2578	2578	2578	1071	1071
	High willingness to receive information (WTR) (36.7%)					
Salience	0.04***	0.18***	0.07***	0.14	-0.15**	0.02
	[0.01]	[0.07]	[0.02]	[0.10]	[0.07]	[0.08]
Information	0.04***	0.15**	0.07***	0.07	-0.16**	0.04
	[0.01]	[0.07]	[0.02]	[0.10]	[0.07]	[0.08]
Control Mean	0.86	0.04	0.91	0.07	0.36	0.33
P-value diff. [Info] -[Salience]	0.89	0.46	0.70	0.24	0.67	0.75
Sample Size	1317	1317	1317	1317	620	620
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Parents were asked at baseline about their interest in receiving information about their child's attendance. They could express *no interest*, *some interest*, or *high interest*. Parents who expressed no or some interest were defined as low-WTR, while parents who expressed high interest were defined as high-WTR. GPA normalized relative to the distribution of the pure control group.Parents were asked at end line for their best estimate of how many times their child missed school over the 4th quarter, and what was their child's 4th-quarter math GPA. Data was then checked against administrative records; we define accuracy as an indicator variable equal to 1 if the parent chose the right bracket for attendance/GPA, and 0 otherwise. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Willingness to receive information indicator (WTR) indeed seems to capture parents demand for information: while low-WTR parents do not update beliefs about children's attendance in response to text messages, those with high-WTR do.[40] What is more, both salience and information treatments have positive and statistically significant effects even for low-WTR parents. Third, and most strikingly, the ratio of salience to information effects is actually systematically higher for parents with high WTR, which is consistent with attention being the primary mechanism behind the effects of communication. The reason is that, in line with Chassang et al. (2012), parents with higher demand for information should be those who exert higher effort to acquire it within the setting of the randomized control trial. Salience effects are magnified among those parents to a greater extent than

[40]The negative treatment effects on accuracy about attendance are linked to the mismatch between the time span at which we conveyed information about attendance ("over the last 3 weeks") and that for which we could verify attendance at endline (over the last quarter), as in the main text.

information effects, highlighting the complementary nature between attention and decentralized information acquisition by parents.

C.5 Additional results on mechanisms

This Appendix compiles additional results on potential alternative mechanisms underlying treatment effects of the child-specific information and salience interventions. First, subsection C.5.1 documents treatment effects on students' time use. Next, subsection C.5.2 explores whether the conclusions from our experiment change when child-specific information is framed relative to classmates' median behavior. Next, subsection C.5.3 investigates whether results are driven by spillovers from the informational intervention on students whose parents were assigned to salience messages, followed by subsection C.5.4, which turns into dynamic patterns for the effects of engagement messages. Subsection C.5.5 then estimates heterogenous treatment effects by parents' baseline accuracy, to document whether effect sizes match the severity of the moral hazard problem between parents and their children prior to the interventions. Last, subsection C.5.6 documents treatment effects on parents' information-seeking behavior, by studying their accuracy with respect to end-line math GPA – as the informational intervention never conveyed child-specific information on grades.

C.5.1 Students' time use

Students were asked how many hours per day (0, 15 minutes, 30 minutes, 1 hours, 2 hours, more than 2 hours) they spend on a range of different activities. We compute 3 summary measures of students' time use based on those questions (standardizing their components and averaging across them within summary measure; Kling et al., 2007): *academic activities* (studying at home on weekdays; studying at home on weekends; studying at home the day before an exam); *reading* (reading a book; reading the newspaper; reading magazines); and *other activities* (watching TV; browsing the internet or on social media; and helping with house chores). Table C.16 presents treatment effects on those measures of students' behavior.

Table C.16: Effects on students' time use

	(1) Academic activities	(2) Reading	(3) Other activities
Child-specific information	0.151***	0.116*	-0.108**
	[0.051]	[0.065]	[0.054]
Salience	0.123**	0.113*	-0.110**
	[0.050]	[0.060]	[0.052]
p-value diff. [Info] -[Salience]	0.344	0.946	0.933
Observations	9539	9539	9539
Randomization strata FE	Yes	Yes	Yes
Student controls	Yes	Yes	Yes

Note: Treatment effects of child-specific information and salience messages on students' time-use. Variables are based on the end-line survey. Students were requested to answer how many hours per day (0, 15 minutes, 30 minutes, 1 hour, 2 hours, more than 2 hours) they spend in each of the following activities: i. studying at home on weekdays; ii. studying at home on weekends; iii. studying at home the day before a test; iv. reading a book; v. reading the newspaper; vi. reading magazines; vii. watching TV; viii. navigating on the internet or social media; and ix. helping with housework. Factor analysis was performed to create three variables of student behavior: academic activities (items i, ii and iii); reading activities (items iv, v and vi) and other activities (items vii, viii and ix). Variables were normalized relative to the distribution of the comparison group (pure control). Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. This Table includes all students in the balanced sample, samples A, B, C, and D (see Figure 1). * if $p<0.1$, ** $p<0.05$ and *** if $p<0.01$.

Students whose parents were assigned to either child-specific information or salience messages report engaging in academic and reading activities to a significantly greater extent than those in pure control school. Once again, across all columns, the effects of information and salience are statistically indistinguishable at conventional significance levels.

C.5.2 Were child-specific messages informative enough?

Is child-specific information really unnecessary, or did our experiment convey too little information to improve educational outcomes above and beyond the effects of making student effort top-of-mind? As Rogers and Feller (2016) argues, while it might be reasonably low-cost for a parent to acquire information on their child's school behavior, it might be much more costly to figure out what is the relevant benchmark against which to compare it. This subsection considers a more demanding counterfactual for salience effects, estimating the effects of framing child-specific information each week against the backdrop of classmates' *median* behavior.

Table C.17 shows the results of a regression that also includes sub-sample C, where students were randomly assigned into either control, salience, child-specific information, or *relative information*. As discussed in Section 3.3, we augment equation 1 with an indicator variable equal to 1 for children whose parents received child-specific information framed relative to the classroom median, and 0 otherwise.

Table C.17: Effects of framing child-specific information relatively to the classroom median

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Child-specific information	0.021***	0.069**	0.029**	0.097**
	[0.006]	[0.032]	[0.012]	[0.047]
Relative information	0.022***	0.078*	0.017	0.141**
	[0.007]	[0.041]	[0.014]	[0.058]
Salience	0.021***	0.090***	0.032***	0.095**
	[0.006]	[0.032]	[0.012]	[0.047]
Control mean	0.875	0.000	0.938	-0.000
p-value diff. [Rel. info] -[Salience]	0.770	0.690	0.086	0.252
Observations	12577	12577	12577	12577
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes

Note: The table displays treatment effects of child-specific information, child-specific relative information, and salience messages on the following administrative outcomes: 4th-quarter attendance in math classes (Column 1); 4th-quarter math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3), and math standardized test scores (Column 4). Parents in the relative information treatment received child-specific information framed relative to the median behavior of their classmates. GPA and standardized test scores were normalized relative to the distribution of the pure control group. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. This Table includes all students in the balanced sample, samples A, B, C, and D (see Figure 1) * if $p<0.1$, ** $p<0.05$ and *** if $p<0.01$.

The table shows that, similar to Rogers and Feller (2016), effect sizes of framing child-specific information in relative terms tend to be larger than those of not doing so. That is the case for math attendance and GPA, and notably for standardized test scores (an effect size of 0.141 standard deviations, about 50% larger than that of information itself). The exception is grade promotion, for which its effect size is actually lower, less than 60% that of child-specific information alone.

Most importantly, when it comes to the comparison between salience and relative information, the only instance for which their effects are statistically different is exactly grade promotion – for which it is the effect size of salience that is higher (significant at the 10% level). Even when it comes to standardized test scores, salience effects are still over two thirds of the effect size of relative information, and they are statistically indistinguishable.

While results suggest that the finer child-specific information is, the higher its potential to improve learning outcomes – based on the differences between framing child-specific information relative to the classroom median or not –, they also suggest that salience is still likely to play a major role behind its effects. Importantly, the informational intervention we use throughout the paper matches the typical structure of school-parents communication campaigns in developing countries (as in Berlinski et al., 2016, which also finds a 0.09 effect size of an text-message information program on students' standardized test scores).

Table **??** in Appendix C.4.2 additionally shows treatment effects of framing information relative to the classroom separately for students above and below the median baseline attendance. We find that students with below-median baseline attendance actually benefit to a lesser extent from information framed relative to the classroom median (for math GPA, the effect is significantly lower than that of child-specific information, at the 10% level), while students with above-median baseline attendance benefit to a greater extent (for standardized test scores, the effect sized of framing information relative to the classroom median is nearly 2-fold that of child-specific information, significant at the 10% level). These patterns are consistent with our results for within-classroom conditional impacts in Appendix C.4 (and with those of Dizon-Ross, 2019): parents 'double down' on high-effort students, and framing information on student effort relative to their classmates' behavior seems to reinforce that behavior.

C.5.3 Are results driven by spillovers from child-specific information?

As discussed in Section 3.1, even with a pure control group, it could still be the case that the salience and information treatments interact within treated schools. This is a specific form of contamination across treatments that does not affect control students. It could happen if parents in the salience treatment ask other parents about messages, and infer from some of those conversations information about their own child's school behavior *thanks to the information treatment*. To test this hypothesis, this subsection investigates whether salience effects are lower in sub-sample D, where students were assigned to either salience messages or control – but not to child-specific information.

Concretely, we estimate the following equation:

$$
\begin{aligned}
Y_{sci} = \alpha + \beta_1 \text{info}_{sci} + \beta_2 \text{salience}_{sci} + \beta_3 \text{control}_{s \notin B, ci} \\
+ \beta_4 \text{salience}_{sci} \times 1\{s \in D\} + \varphi 1\{s \in D\} + \sum_{k=1}^{K} \gamma_k X_{scik} + \theta_s + \varepsilon_{sci},
\end{aligned}
\tag{3}
$$

where $1\{s \in D\} = 1$ if the school belongs to sub-sample D (50% salience, 50% control), and 0 otherwise. We are interested in testing $\beta_4 \leq 0$.

Table C.18 shows the results of estimating equation 3, allowing salience effects to vary in schools where the informational intervention is absent. Panel A estimates differential treatment effects of salience messages within the no-information sub-sample through an interaction term, and Panel B estimates treatment effects of salience messages restricting attention to that sub-sample and the pure control group.

Table C.18: Differential effects of salience in sub-sample without informational intervention

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Panel A: Full sample				
Child-specific information	0.021***	0.070**	0.026**	0.108**
	[0.006]	[0.032]	[0.012]	[0.047]
Salience	0.017***	0.070**	0.027**	0.101**
	[0.006]	[0.033]	[0.012]	[0.048]
Salience x No-information sub-sample	0.001	0.049*	0.004	0.015
	[0.004]	[0.029]	[0.009]	[0.042]
Observations	12577	12577	12577	12577
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes
Panel B: Sample B and D				
Salience x No-information sub-sample	0.034***	0.172***	0.059***	0.106*
	[0.006]	[0.042]	[0.012]	[0.056]
Observations	3760	3541	3675	3455
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes

Note: Treatment effects of salience messages separately for schools where some parents were assigned to child-specific information, and those where none was. Panel A estimates differential treatment effects of salience messages within the no-information sub-sample through an interaction term, and Panel B estimates treatment effects of salience messages restricting attention to that sub-sample and the pure control group. Treatment effects on 4th-quarter attendance in math classes (Column 1); 4th-quarter math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3), and math standardized test scores (Column 4). GPA was normalized relative to the distribution of the pure control group. Students' controls include gender, age, race, baseline grades and attendance, and their caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. This Table includes all students in samples B and D (see Figure 1) * if p<0.1, ** p<0.05 and *** if p<0.01.

We find overwhelming evidence that salience effects are *not* driven by spillovers from the informational intervention. If anything, salience effects are actually larger in the absence of child-specific information, although differential effects are mostly statistically insignificant at conventional significance levels.[41]

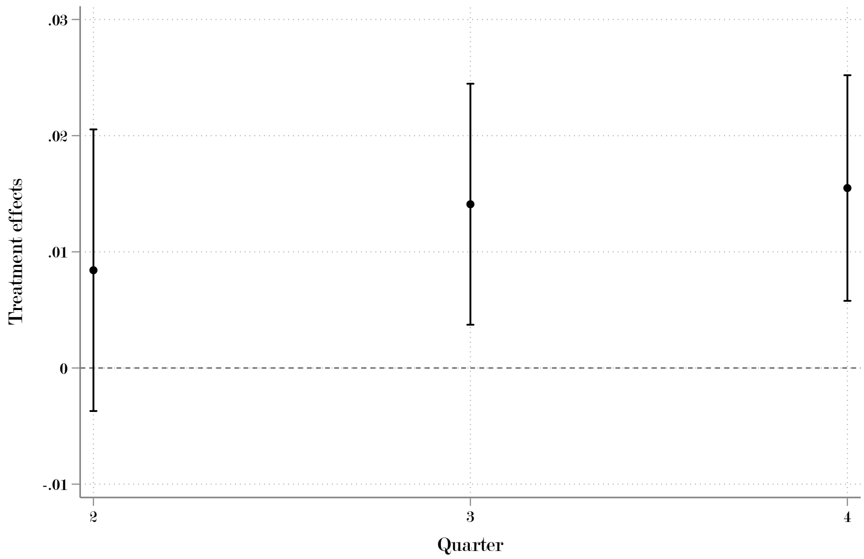
C.5.4 Additional results of the impacts of engagement messages

Figure C.7 allows treatment effects to vary more flexibly over the course of the school year, estimating non-parametric effects of engagement messages on math attendance (Panel A) and math GPA (Panel B) relative to the pure control group with the first quarter as the reference period.

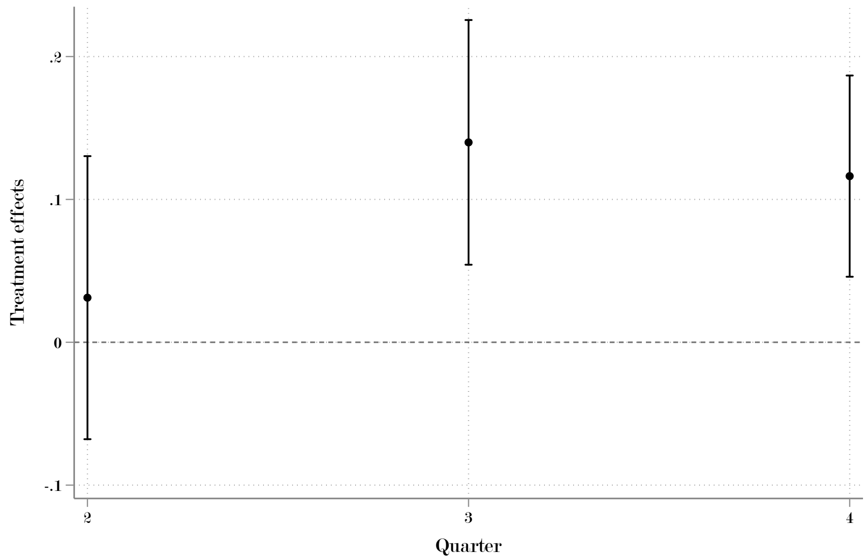
[41] The frequency at which teachers filled in the platform weekly was slightly higher in sub-sample D than in other sub-samples (statistically significant at the 1% level; see Appendix B). Results are robust to bounding treatment effects to account for selection; see Appendix C.3.

Figure C.7: Differences-in-differences coefficients of engagement messages, by quarter

Panel A: Attendance (p.p.)



Panel B: GPA (s.d.)



Note: Panels A and B show quarter-specific differences-in-differences estimates from equation 3 for the engagement messages program by quarter, with the first quarter as the reference period. Math GPA was normalized relative to the distribution of the comparison group (pure control). 90% confidence interval with standard errors clustered at the classroom level.

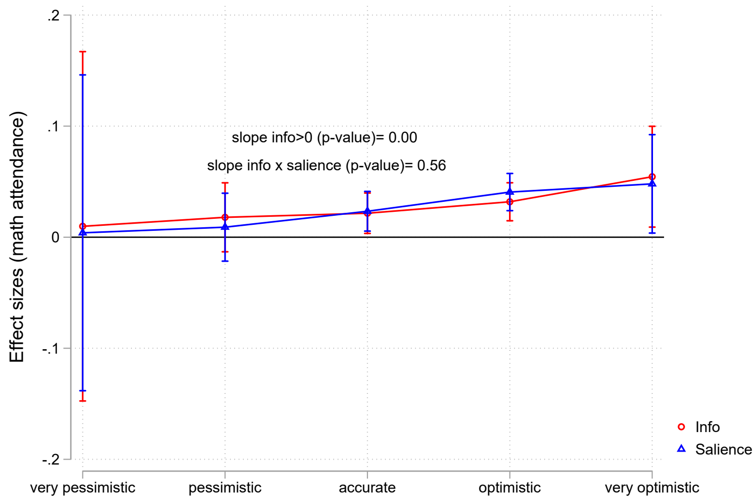
C.5.5 Effects are proportional to the severity of parents' misinformation problem

This subsection documents heterogeneous treatment effects by parents' baseline accuracy with respect to students absenteeism in math classes. The expected impacts of informational interventions naturally depend on prior beliefs, as behavior change should ultimately match the direction and the extent to which these beliefs are moved by the treatment. The more inaccurate subjects were at baseline, the larger the scope for the intervention to change their beliefs and behavior.

Concretely, in our setting, parents who were optimistic about their children's school effort (relative to ground truth) would tend to under-monitor. The larger the accuracy gap, the larger the expected impacts of the intervention. We rely on heterogeneity in baseline beliefs rather than using changes in beliefs directly because post-intervention beliefs were collected only at end line, the same horizon at which we can measure effects on student attendance – what would make it non-trivial to rule out feedback effects of student behavior into parent's beliefs.

Figure C.8 documents that, consistently, the hierarchy of effect sizes of child-specific information matches the severity of parents' misinformation problem. Treatment effects are the smallest for parents who were pessimistic about their children's attendance at baseline – as those might have even monitored less as a result of the intervention –, and increase with the extent to which parents under-estimated absenteeism prior to the intervention (significant at the 1% level).

Figure C.8: Heterogeneous treatment effects on math attendance, by parents' baseline accuracy



Note: Heterogeneous treatment effects of child-specific information and salience messages by parents' baseline accuracy with respect to attendance in math classes. At the baseline survey, parents were asked to provide their best estimate of how many times their child had missed math classes over the past three weeks, choosing among four brackets: 0 absences; 1-2; 3-5; or more than 5. Since administrative data on students' 1st-quarter absences were only available for the whole quarter ($\sim$ 9 weeks), actual absences are computed by dividing that indicator by 3. We compute baseline accuracy by subtracting parents' guess from the actual number of absences. We categorize parents as 'very pessimistic' (gap $\in [-3, -2]$), 'pessimistic' (gap $= -1$), 'accurate' (gap$= 0$), 'optimistic' (gap$= 1$), and 'very optimistic' (gap $\in [2, 3]$). Student controls include gender,age, race, baseline grades and attendance, and parents' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. We also report p-values for the hypothesis test that effect sizes of child-specific information are increasing in parents' baseline accuracy gap, and that this slope is equal between the information and salience groups. OLS coefficients and 90% confidence intervals from Table C.14 in Appendix C.4.2.

Strikingly, the same is true for the effects of salience messages; in fact, the slopes of the effect sizes of each intervention with respect to parents' baseline accuracy gap are statistically identical. Once again, results are consistent with both interventions inducing parents to increase monitoring effort. Appendix C.4.2 documents similar patterns for heterogeneous treatment effects of child-specific information and salience messages by parent's accuracy gap with respect to student baseline math attendance on additional educational outcomes.

Last, Appendix C.4 documents heterogeneous treatment effects by content of the text messages sent to parents, taking advantage of the weekly scores entered by teachers into the platform. While heterogeneous treatment effects of the informational intervention by content match the patterns in the literature, because content in our experiment reflects student behavior over the course of the school year – which is endogenous to the interventions –, we do not emphasize those results in the main text.

C.5.6 Parents acquired information as part of higher-intensity monitoring

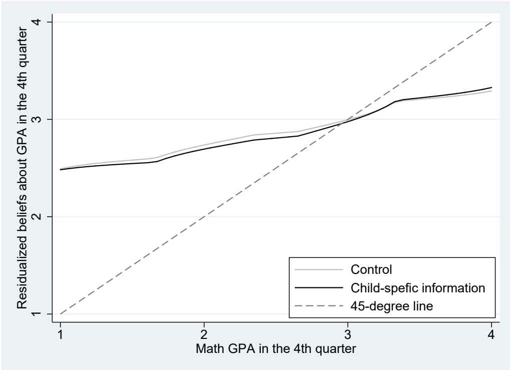
This subsection documents treatment effects on the accuracy of parents' beliefs about their children's math GPA changes in response to the interventions. Because no intervention communicated grades, changes in accuracy would be consistent with treatment effects on parents' information-seeking behavior – especially when it comes to the effects of salience messages.

At the baseline phone survey, parents were asked to provide their best estimate of their child's 2nd-quarter math grade. Parents had to choose one out of four categories: below average (0-4); adequate (5-6); good (7-8); or very good (9-10). At the end-line phone survey, we asked parents to guess their children's math GPA directly (open-ended, between 0 and 10). We build a 4-point scale variable for end-line beliefs equivalent to the baseline variable.

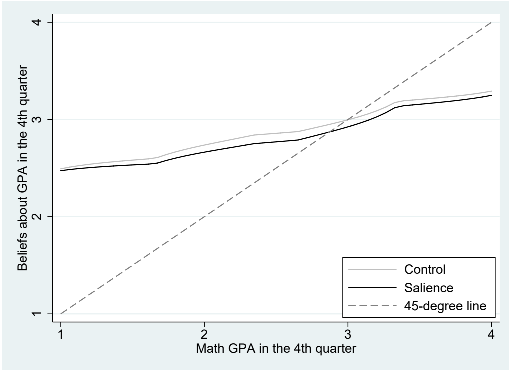
We compute the slope of the relationship between parents' beliefs and children's math GPA at end line, across the different experimental conditions. Once again, we harmonize the scales of parents' beliefs and that of students' actual absences such that,, if parents were perfectly accurate, that correlation would be equal to 1. Figure C.9 shows, however, that this correlation is not only much lower in the control group, but also, no higher in the information group (Panel A) or the salience group (Panel B) – perhaps unsurprisingly, as none of our interventions conveyed child-specific information about grades. Next, we compute the slope between *changes* in parents' beliefs and that in children's math GPA between baseline and end line, across the different experimental conditions. While in the control group that slope is nearly zero – parents seem to have very limited awareness of whether their children are improving or deteriorating throughout the school year –, it is systematically higher in both the information group (Panel C) and the salience group (Panel D). All in all, both interventions lead parents to coarsely update beliefs: they come to understand that their children's grades are going up or down, even if they are still not any better in guessing by how much, relative to the control group.

Figure C.9: Parents' beliefs vs. actual math GPA

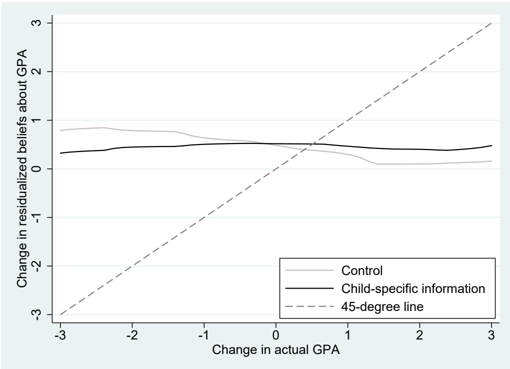
Panel A: Information vs. Control (levels)



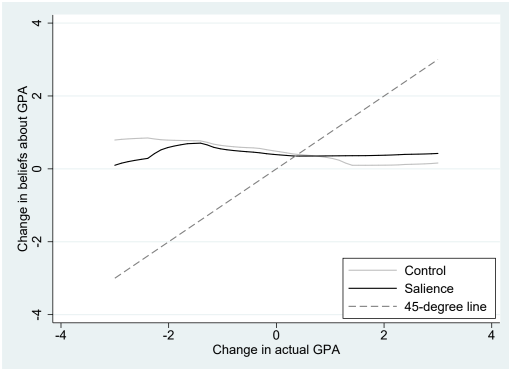
Panel B: Salience vs. Control (levels)



Panel C: Information vs. Control (changes)



Panel D: Salience vs. Control (changes)



Note: Non-parametric relationship between parents' end-line beliefs about their children's GPA and actual math GPA (Panels A and B) and that between changes in parents' beliefs between baseline and end line and changes in math GPA between the 2nd and 4th quarters (Panels C and D). At baseline, parents were asked to provide their best estimate of their child's first quarter math grade, choosing among four brackets: 0-4; 5-6; 7-8; or 9-10. At end line, parents were asked to provide their best estimate of their child's 4th-quarter math GPA, an integer between 0 and 10. We adjust end-line beliefs and actual math GPA's to the 4-point scale of baseline beliefs. The control group across all panels includes both within-class control students and the pure control group. All local polynomial regressions use a 0.6 bandwidth.

Table C.19 shows that these patterns hold in a regression framework. Columns 1, 3 and 5 restrict attention to the information and the control groups (both within-classroom and pure control), while columns 2, 4 and 6, to the salience and control groups (both within-classroom and pure control). Columns 1 and 2 document that the correlation between parents' beliefs and children's actual math GPA is not statistically different across groups at baseline; columns 3 and 4 estimate treatment effects of information and salience on that correlation; and columns 5 and 6 estimate treatment effects of information and salience on the correlation between changes in parent's beliefs between baseline and end line and changes in math GPA between the 2nd and 4th quarters.[42]

Table C.19: Parents' accuracy about math GPA levels and changes

	Baseline beliefs		Endline beliefs		Change in beliefs	
	(1)	(2)	(3)	(4)	(5)	(6)
Child-specific information	0.051		-0.055		0.052	
	[0.085]		[0.104]		[0.044]	
Salience		0.031		0.014		0.008
		[0.080]		[0.091]		[0.043]
Information x actual GPA	-0.011		0.020			
	[0.032]		[0.037]			
Salience x actual GPA		-0.013		-0.018		
		[0.030]		[0.036]		
Information x changes GPA					0.104***	
					[0.052]	
Salience x changes GPA						0.073
						[0.054]
Actual GPA	-0.050	-0.017	0.265***	0.253***		
	[0.048]	[0.051]	[0.030]	[0.039]		
Changes in GPA					0.156***	0.157***
					[0.036]	[0.039]
Baseline level					-1.216***	-1.195***
					[0.025]	[0.025]
Observations	2,815	2,878	2,296	2,178	1,160	1,140
Classroom FE	No	No	No	No	No	No
Student-level controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.152	0.154	0.248	0.242	0.790	0.783

Note: Correlation between parents' baseline and end-line beliefs about their children's math grades and actual grades as well as the correlation between changes in beliefs and math grades within each period. At baseline, parents were asked to provide their best estimate of their child's first quarter math grade, choosing among four brackets: 0-4; 5-6; 7-8; or 9-10. At end line, parents were asked to provide their best estimate of their child's 4th-quarter math GPA, an integer between 0 and 10. We build a 4-point scale variable for end-line beliefs consistent with the baseline variable. The dependent variables are: an ordinal scale of parents' baseline beliefs, between 1 (corresponding to the 0-4 bracket) to 4 (corresponding to the 9-10 bracket) (Columns 1 and 2); a 4-point scale equivalent variable for end-line beliefs (Columns 3 and 4); and the change in beliefs between the two periods (Columns 5 and 6). Columns (1), (3) and (5) include only students in the child-specific information and control groups (both within-class and pure control). Columns (2), (4) and (6) include only students in the salience and control groups (both within-class and pure control). Regressions include indicator variables for students in the information and salience groups and an interaction term between (changes in) actual math GPA and the indicator for child-specific information (levels in columns 1 and 3, and changes in column 5) or (changes in) actual math GPA and the indicator for salience messages (levels in columns 2 and 4, and changes in column 6). 2nd-quarter math GPA included as control in columns (5) and (6). Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. All columns are OLS regressions, with standard errors clustered at the classroom level. This Table includes all students in the balanced sample, samples A, B, C, and D (see Figure 1). * if $p<0.1$, ** $p<0.05$ and *** if $p<0.01$.

Columns 3 and 4 confirm that neither child-specific information nor salience messages make parents systematically more accurate about their children's 4th-quarter math GPA. In turn, both interventions make parents a lot more accurate about *changes* in math GPA over time: child-specific information increases the correlation between changes in beliefs and actual changes in math GPA by 2/3 (significant at the 1% level, column 5), and salience messages, by nearly 50% (p-

[42]The number of observations differs across columns 1-2, 3-4 and 5-6 because of differences in response rates across the baseline and end-line phone surveys, and in the number of subjects who answered both surveys; non-response is not systematically different across treatment arms; see Appendix B.

value = 0.125, column 6).

Results suggest that both interventions lead parents to acquire information independently. While accuracy gains among parents targeted by child-specific information could be partly explained by the fact that student attendance and math GPA are positively correlated, our results suggest that roughly 70% of those gains are actually due to salience effects; after all, parents in the salience group do *not* become more accurate about student effort, but do so when it come to changes in math GPA between baseline and end line. These results are also not an artifact of salience messages making parents merely less optimistic about their children's educational outcomes: our results show that is *not* the case for math attendance or GPA levels neither for changes in math GPA over time; moreover, Figure C.2 shows that while most parents were in fact too optimistic with respect to their children's baseline attendance in math classes, the same was *not* the case with respect to baseline math GPA – parents were roughly equally distributed across optimistic, accurate and pessimistic before the intervention.

All in all, our findings are consistent with parents setting monitoring effort subject to attentional constraints.

C.6 Additional results on within-classroom spillovers

This Appendix presents additional results on spillovers within the classroom. Table C.20 shows that, if anything, within-classroom spillovers were even larger in the absence of child-specific information. Next, Table C.21 shows that treatment effects of engagement messages to parents also had similar within-classroom spillovers, in the absence of child-specific information and in the absence of teacher effects driven by platform requirements.

Table C.20: Differential effects of salience and within-classroom control in sub-sample without informational intervention

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Panel A				
Child-specific information	0.021***	0.070**	0.026**	0.108**
	[0.006]	[0.032]	[0.012]	[0.048]
Salience	0.017***	0.070**	0.027**	0.101**
	[0.006]	[0.033]	[0.012]	[0.048]
Salience x No-information sub-sample	0.016***	0.080**	0.018**	-0.022
	[0.004]	[0.037]	[0.007]	[0.046]
Within-classroom control	0.014**	0.062*	0.026**	0.094**
	[0.006]	[0.033]	[0.011]	[0.047]
Within-classroom control x No information	0.015**	0.031	0.014*	-0.037
	[0.005]	[0.037]	[0.008]	[0.052]
Panel B: Sample B and D				
Salience x No-information sub-sample	0.034***	0.172***	0.059***	0.106*
	[0.006]	[0.042]	[0.012]	[0.056]
Within-classroom control	0.033***	0.013***	0.053**	0.091
	[0.007]	[0.004]	[0.013]	[0.059]
Observations	12577	12577	12577	12577
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes

Note: Treatment effects of salience messages separately for schools where some parents were assigned to child-specific information, and those where none was. Panel A estimates differential treatment effects of salience messages within the no-information sub-sample through an interaction term, and Panel B estimates treatment effects of salience messages restricting attention to that sub-sample and the pure control group. Treatment effects on 4th-quarter attendance in math classes (Column 1); 4th-quarter math GPA (Column 2); grade promotion rate (=1 if the student advanced to high school, and 0 otherwise; Column 3), and math standardized test scores (Column 4). GPA was normalized relative to the distribution of the pure control group. Students' controls include gender, age, race, baseline grades and attendance, and their caregivers' controls include gender, age, race, family income and education. We also control for randomization strata fixed-effects, and include an indicator variable for the within-classroom control group. All columns are OLS regressions, with standard errors clustered at the classroom level. * if p<0.1, ** p<0.05 and *** if p<0.01.

Table C.21: Treatment effects of engagement messages: Differences-in-differences and within-classroom control

	(1)	(2)
	Math Attendance (p.p.)	Math GPA (std.)
Message x Post	0.0151***	0.116***
	[0.00591]	[0.0427]
Message	0.00621	-0.162***
	[0.00604]	[0.0610]
Post	-0.0333***	0.00420
	[0.00416]	[0.0252]
Within-classroom control x Post	0.0156***	0.0926**
	[0.00583]	[0.0396]
Within-classroom control	0.00637	-0.151***
	[0.00589]	[0.0558]
Observations	14,775	14,586
Student-level controls	Yes	Yes
R-squared	0.072	0.060

Note: Treatment effects of engagement messages on 4th-quarter attendance in math classes (Columns 1) and 4th-quarter math GPA (Column 2). GPA was normalized relative to the distribution of the pure control group. The sample includes sub-sample E and the pure control group (we exclude parents assigned to 2 or 3 engagement messages per week). Observations are stacked (student x school quarter). All estimates use the first quarter as period of reference. Regressions include interactions between a post-treatment time dummy and treated students, and between the post-treatment dummy and within-classroom control group dummy (the pure control is the reference group). We also include in the regression indicator variables for the post-treatment period and for the treatment and within-classroom control groups, and student-level controls. Students' controls include gender, age, race, baseline grades and attendance, and caregivers' controls include gender, age, race, family income and education. All columns are OLS regressions, with standard errors clustered at the classroom level. * if $p<0.1$, ** $p<0.05$ and *** if $p<0.01$.

C.7 Robustness to clustering level

In the main paper, we cluster standard errors at the classroom level. Nonetheless, given our two-level randomization design, one might worry that it would be more appropriate the cluster standard errors at the school level instead. Nevertheless, as discussed by Abadie et al. (2023), clustering at a too coarse level might make standard errors too conservative, especially in cases with high within-cluster treatment variation, as such is the case of our experiment design. In order to examine the robustness of our findings to the choice of the clustering level, we implement the bootstrap clustering procedure developed by Abadie et al. (2023), with a small adaptation – since our design has multiple treatment arms, different from the application analyzed throughout that paper.

Let $\overline{W}_m$ and $\overline{N}_m$ be the fraction of treated individuals and the total number of students at school m, respectively. We proceed as follows:

1. For each school (henceforth, *cluster*) where at least one student was assigned to child-specific information, we draw (with replacement) a **fraction of treated units** $\overline{W}_m^b$ from the empirical distribution of treatment fractions: $(\overline{W}_1, ..., \overline{W}_M)$, where M is the total number of schools featuring the child-specific information treatment arm in the sample;

2. Then, for each cluster for which there is within-school variation in treatment assignment, we compute $\overline{W}_m^b$ x $\overline{N}_m$ as the bootstrapped number of treated students in that cluster. As such, we draw (with replacement) $[\overline{W}_m^b$ x $\overline{N}_m]$ treated observations from cluster m, where the $[z]$ function denotes the largest integer less than z. Similarly, we draw (with replacement) $\overline{N}_m - [\overline{W}_m^b$ x $\overline{N}_m]$ of observations assigned to other conditions (salience or within-classroom control);

3. For schools without students assigned to child-specific information, we simply draw (with replacement) a sample of the same size as the actual number of students in those schools;

4. We replicate the bootstrap procedure 100 times. In each replication, we estimate regressions with the stacked bootstrapped samples of observations from pure control and treated schools in steps 2 and 3, and store coefficients for each treatment arm (child-specific information and salience) and that for the within-class control group. We compute corrected standard errors as the standard deviation of the empirical distribution of each estimated coefficient.

The procedure above incorporates two adjustments relative to that Abadie et al. (2023). First, the original procedure features a single treatment arm. To stay as close as possible to it, we implement the two-step bootstrap procedure based on child-specific information. Then, within each bootstrapped sample, our regressions also estimate coefficients for the salience intervention and the within-classroom control group. Since treatment assignments are cluster-correlated, this should not affect the estimator's performance. Second, and most importantly, the procedure in Abadie et al. (2023) is not well defined in the absence of treatment variation within clusters. In our application, however, about a third of the schools feature no student assigned to child-specific information. For this reason, we add step 3 in the procedure above, whereby we bootstrap the sample of schools without within-cluster variation in treatment saturation independently.

Table C.22 re-estimates our main results with bootstrapped standard errors according to that procedure. We compare coefficients using the empirical distribution obtained from bootstrapped simulations. To help inference, the table showcases the 5th and 95th percentiles of the distribution of differences between the child-specific information and the salience coefficients.

Table C.22: Effects on attendance, grades and grade promotion with corrected standard errors

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Child-specific information	0.021***	0.071***	0.026***	0.107***
	[0.003]	[0.019]	[0.007]	[0.028]
Salience	0.021***	0.090***	0.032***	0.095***
	[0.003]	[0.021]	[0.006]	[0.027]
Control within classroom	0.018***	0.070***	0.030***	0.085***
	[0.003]	[0.020]	[0.007]	[0.028]
Control mean	0.875	0.000	0.938	0.000
p5(diff), p95(diff)	[-0.004, 0.003]	[-0.041, 0.008]	[-0.013, 0.001]	[-0.021, 0.043]
Observations	12577	12577	12577	12577
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes
R-squared	0.206	0.617	0.101	0.342

D Pre-analysis plans and survey instruments

D.1 Pre-analysis plan for the main experiment

Our pre-analysis registered at the AEA RCT Registry is presented in full in subsections D.1.1 through D.1.4. Subsection D.2 highlights the elements of the analyses that deviate from what had been specified in that pre-analysis plan.

D.1.1 Background

While there is increasing evidence that enhancing the communication between schools and parents significantly improves students' performance, less is known about what mechanisms drive those effects. Is it because, by providing parents with information about their children's effort, communication primarily alleviates the moral hazard problem between parents and children? Or is it because parents have limited attention, and communication makes parenting "top of mind"?

This paper attempts to decompose the effects of communicating with parents into those two mechanisms. Specifically, we investigate whether informing parents about their children's attendance, tardiness and assignment completion, improves students' outcomes above and beyond the effects of communication aimed at increasing awareness about those dimensions of children's effort. The distinction matters: providing timely and accurate information about children's behavior requires integrated systems and customized communication, which can be quite costly, particularly in developing countries. Conversely, simply nudging to raise awareness does not require any information systems in place.

Our experiment has Math teachers fill in information about students' attendance, tardiness and assignment completion, and then randomly assigns within classroom what information is conveyed to each parent over SMS. Parents in the control group receive no SMS; those in the awareness treatment group receive only general statements about the relevance of monitoring their child's behavior (e.g.: "Attending classes every day is important for Nina's grades"); and those in the awareness + information treatment group receive what the teacher informed about their child (e.g.: "Nina was absent less than 3 times in the previous 3 weeks"). The questions of interest are whether awareness alone improves student's attendance, grades, and drop-out rates, and to what extent adding pupil-level information further improves those outcomes.

D.1.2 Intervention, sample and outcomes

Communication interventions are randomly assigned at the school and student levels, within a sample of 223 Brazilian public schools, in order to estimate the impacts of each of those mechanisms on parental engagement and students' outcomes. The ninth grade is a crucial period in the school cycle of Brazilian schools: it is the last grade before high-school, and dropout rates are very high.

We will deliver content through sequences of text messages (SMS), alternating the dimensions of children's effort—attendance, tardiness and assignments completion. The intervention's treatment arms are as follows:

1) [Awareness treatment] General statements about attendance, tardiness and assignment completion (e.g., "attending school is important") – T1

2) [Awareness + information treatment] Child-level attendance, tardiness and assignment completion – T2

Comparing T2 to T1 and T1 to control allows separating the effects of information and awareness.

There are two main concerns about how this design may potentially underplay the effects of information. The first is that parents may already have (to a reasonable extent) information about their child, such that the key piece of information missing is how to place their child relatively to his or her classmates. In fact, other studies often focus on relative behavior: e.g., Rogers and Feller (2016) inform parents about how their children's attendance fares relatively to his/her classroom modal attendance.

To deal with this concern, we pursue two strategies. First, we survey parents at baseline about their best guess for their child's attendance, tardiness and assignment completion, so as to investigate heterogeneity of treatment effects by baseline accuracy (Annex 2). Second, for a sub-sample of schools, we add an alternative awareness + information treatment that conveys parents both with pupil- and classroom-level information, to test whether that treatment has additional effects.

3) [Awareness + relative information treatment] Child- and classroom-level attendance, tardiness and assignment completion – T3

The second concern is contamination, or peer effects. While there is a concern that assigning different treatments within the same classroom may lead to contamination, we are less worried about it in this setting parents typically have no recurring interactions at this age – most of them no longer take their children to school, and parent-teacher meetings are rather infrequent in Brazilian public schools. However, peer effects may lead us to underestimate treatment effects. To deal with this concern, our design varies the exposure to the different treatments across different sub-samples of schools, allowing us to estimate spillovers. Randomization will be performed in two steps. First, schools will be randomly assigned to 4 different sub-samples (A-D), determining the treatment arms each school will have access. Then, students will be randomized within class to each treatment arm:

A. Pure control – 25 schools
B. T1 + control – 25 schools
C. T1 + T2 + control – 100 schools
D. T1 + T2 + T3 + control – 50 schools

Sub-sample C allows separating the effects of information and awareness; sub-samples A and B allow estimating spillover effects. Sub-sample D is meant to address the concern about relative vs. absolute child-level information. In order to collect cellphones and information from parents in the control group, and also to control for the proportion of parents registered in the program, we will offer the control and the treatment group access to send school events through the platform.

		Randomization at the school level			
		A - 25 Schools	B - 25 Schools	C - 100 Schools	D - 50 Schools
Randomization at the individual level (within classroom)	T1 - [Awareness treatment]		1/2 Class	1/3 Class	1/4 Class
	T2- [Awareness + information treatment]			1/3 Class	1/4 Class
	T3-[Awareness + relative information treatment]				1/4 Class
	Control (events)	All students	1/2 Class	1/3 Class	1/4 Class

Figure D.1: Research Design

A web-platform was created specifically to this project and was designed in a simple and intuitive way so schools could easily manage it. Treatment and control schools will have access to the event feature, allowing them to notify parents of two school events per month. Once the principal registers the event, the system will send two SMS notifications to parents: one week prior and one day prior to the event. Math teachers from treatment schools will be oriented to fill in the platform every week with that week's dimension of students' behavior: attendance, tardiness or assignment completion. Teachers will fill information regarding student behavior on each dimension considering the past three weeks. The system requires teachers to fill in information for all students.

Attendance	Lateness	Assignment Completion
1 Did not miss any class	**1** Was not late for any class	**1** Completed all the assignments
2 Missed less then 3 classes	**2** Was late for less then 3 classes	**2** Completed more than half of the assignments
3 Missed 3 to 5 classes	**3** Was late 3 to 5 classes	**3** Completed less than half of the assignments
4 Missed more than 5 classes	**4** Was late for more than 5 classes	**4** Did no complete any of the assignments

Figure D.2: School Platform

Teachers and schools are not aware of their assignment, nor of parents' assignment. For treatment arm T3, the platform computes the class median once the teacher submits all students' information every week. As for treatment arm T1, although teacher will fill in child-level information every week, parents will only receive general information aimed at raising awareness about that dimension of children's effort. Parents of all treatment arms only receive the text message if the teacher had completed the platform that week. This is true even for T1, in order to avoid confounding treatment effects with teachers' non-compliance. After teachers have filled the platform until Sunday of each week, parents will receive the following message on Tuesdays, according to their treatment status:

	T1 (awareness)	T2 (awareness + information)	T3 (awareness +relative information)
Week 1	For a good school performance, it is important that Caroline doesn't miss school for no reason.	According to the information registered by the teacher in the system the past 3 weeks, Eric missed less than 3 classes.	In the past 3 weeks, Susanna missed a few classes less than 3 classes. In her class, most of the students didn't miss any class.
Week 2	Punctuality prevents Caroline from missing explanations given by the teacher that are not always in the books.	According to the information registered by the teacher in the system the past 3 weeks, Eric was late for more than 5 classes.	In the past 3 weeks, Susanna was late for more than 5 classes. In her class, most of the students were late for less than 3 classes.
Week 3	Completing the assignments is very important for Caroline to learn what was taught in class.	According to the information registered by the teacher in the past 3 weeks, Eric complete more than half of the assignments.	In the past 3 weeks, Susanna completed all the assignments. In her class, most of the students completed more than half of the assignments.

Figure D.3: SMS examples

The content of the messages are simple and clear and messages across treatment arms were designed to have a similar length (number of characters). Each week teachers will receive a text message, reminding them which dimension they should fill in that week. Moreover, teachers who miss one week will receive an alert, emphasizing they did not fill the platform that week and encouraging them to fill in the following week. Principals will receive motivational messages, encouraging them to engage teachers in the program, as well as message alters, if the usage in the school is low. The study relies on four main stakeholders, who will contribute to the success of the intervention: the São Paulo Secretariat of Education, the Regional Board of Education Directors, school principals and teachers. São Paulo is the most populous state in Brazil and it is divided in 91 Regional Boards of Education. Each Region has an Education Director. In this project, we will work with five Regional Boards of Education. Education Directors will play an import roll of engaging schools in the program.

The implementation of the intervention involves five steps. First, on April 14th we had a meeting with the five Education Directors, as well as the team of São Paulo Secretariat of Education to present the project. Second, on the following two weeks, Directors presented the project to their schools, inviting them to participate. Participation rate was 87%. Third, between May 9 and May 17 we had meetings with the school principals and Education Director, in each of the Regional Board of Education head offices, to explain the project and distribute the enrollment material and instructions. Forth, the schools organized parental meetings, to explain the project and enroll parents in the program, collecting their cell-phone, as well as other information. For parents who did not attend the meeting, the material was sent home trough the student. Fifth, Math teachers had two weeks to register parents' information in the system. Schools and students were then randomized to treatments and control groups and teachers began to fill the platform on the week of June 13th. The school year in Brazil runs from February to December, with a winter break in July. Parents will be exposed to the program during 6 months of the academic year.

D.1.3 Outcomes

We will conduct surveys through automated voice calls (Interactive Voice Response, IVR) at the end of the intervention to collect self-reported parenting practices and parents' views about their children. We conducted a baseline survey through IVR on the week of June 16th, surveying parents about their demand for information, as well their previous knowledge about their kids. At the end of the project, we will be able to investigate if treatment effects are heterogeneous by the accuracy of prior knowledge about children's behavior and the ones by ex-ante demand for information about child-level behavior.

One interesting lesson from our 2015 pilot is that, at least among 6th grades, about 1/3 of participating families' children also have cell phones, which lead us to collect student's cell phones for this study. We were able to collect cell phones for 50% of the students. Among these families, we track students' views about themselves, their parents and their teachers. At the end of the intervention, the São Paulo Education Secretariat will provide data on student attendance and grades in 2016 (per quarter), and enrollment in 2017. Moreover, the Secretariat implements an yearly standardized test to all schools in the state of São Paulo, SARESP (System of School Performance Evaluation of the State of São Paulo). All students in grades 1st, 3rd, 5th, 7th, 9th of primary school and the 3rd (final) year of high school are tested on their knowledge of Mathematics and Portuguese.

D.1.4 Timeline and milestones

#	Milestone	Target Start Date	Target End Date
1.	Meeting with the Regional Board of Education Directors and the São Paulo Secretariat of Education to explain the project	Apr-14	Apr-14
2.	Regional Board of Education Directors meet with their schools principals to explain the project	Apr-18	Apr-27
3.	Schools register to participate in the program (trough an online form)	Apr-18	Apr-27
4.	Meeting with Education Directors and school principals in each of the 5 Regional Board of Education head office to explain the project and distribute the enrollment material	May-9	May-17
5.	Schools organize meeting with parents to explain the project and obtain their cellphone and consent	May-10	May-30
6.	Teacher uploads parental enrollment information through secure website	May-10	Jun-2
7.	Randomization	Jun-3	Jun-5
9.	Baseline phone survey implementation	Jun-13	Jun-24
10.	SMS content and nudges begin	Jul-4	-
11.	End line phone surveys implementation	Dec-12	Dec-20
12.	SMS content and nudges end	Dec-20	-
13.	Impact Evaluation	Jan-30	Mar-31

Figure D.4: Timeline & Milestones

D.2 Deviations from the pre-analysis plan

In the paper, we present *all* results of the hypotheses' tests pre-specified in that document (some of which are relegated to the supplementary appendices).

There are five main differences between the analyses we undertake in the paper and those that were pre-specified.

First, terminology. For ease of exposition, in the paper we distinguish between salience messages and child-specific information, while in the pre-analysis plan we referred to the former as "awareness" messages and to the latter as "awareness + information" messages. Nothing changed in terms of the analyses; we just clarify the difference to guide the reader in their examination of the pre-analysis plan.

Second, while we had anticipated the possibility of spillovers within classroom (and this is why we included a pure control group), we did not anticipate that spillovers would be so large as to prevent us from detecting differences in administrative educational outcomes. This has led us to focus most analyses in the paper on comparisons relative to the pure control group. Having said that, we present extensive evidence that the interventions had significant effects even relative to the within-classroom control group when it comes to parents' accuracy (Section 3), platform outcomes (Section 5.1), and even administrative outcomes – conditionally on average platform scores (Section 5.3).

Third, sub-sample E (engagement messages) was not included in the pre-analysis plan. It was added later, covering a different set of schools (not statistically identical at baseline to the other sub-samples), to allow us to rule out that treatment effects were merely driven by differential teacher behavior across treated schools and pure control schools. As the analyses of treatment effects comparing educational outcomes in this sub-sample to those in the pure control group is non-experimental (rather, estimated using a differences-in-differences strategy), we did not amend the pre-analysis plan at the time.

Fourth, the number of schools assigned to each sub-sample does not correspond exactly to those in the pre-analysis plan. The reason is that we ended up having access to a larger number of schools than we had foreseen at the time. The proportion of schools assigned to each group is, however, nearly identical to that of the pre-analysis plan.

Fifth, we incorporated some additional analyses in order to generate results comparable to the literature. Specifically, the analyses of how the interventions affect the slope of beliefs as a function of actual absences, in Sections 4 and C.5.6, and of how the interventions induce conditional impacts with respect to student effort, in Appendix C.4, closely follow Dizon-Ross (2019).

D.3 Pre-analysis plan for the additional experiment

Our pre-analysis registered at the AEA RCT Registry is presented in full in subsections D.3.1 through D.3.4.

D.3.1 Background

A growing education literature suggests that supporting parents through text messages (SMS) can positively impact students' behavior and educational attainment. While those studies highlight the potential of text messages for producing cost-effective educational results, there is limited evidence on the optimal design of SMS campaigns. What it the optimal frequency of texting, so as to most effectively capture parents' attention without saturating it? At what time should messages be sent? Should parents get messages always at the same time? Is interactive content more effective? The answers to those questions are critical as governments and international organizations consider scaling up successful SMS interventions.

This paper cross-randomizes different features of the design of a typical SMS campaign targeted at making parenting a habit among families of public schools' 9th graders in Brazil. Those experiments assess the impacts of alternative campaign parameters: (i) frequency (0, 1, 2 or 3 times a week), (ii) time of the day (afternoon or evening), (iii) consistency (constant or varying time of delivery), and (iv) interactivity (in the form of a feedback flow that asks whether parents complied with the suggested activity), on student's attendance, grades, and drop-out rates.

D.3.2 Intervention and sample

Campaign parameters are randomly assigned at the student level, comprising a sample of 2500 students within of 180 classrooms at 60 Brazilian public schools. While there is a concern that assigning different treatments within the same classroom may lead to contamination, we are less worried about it in this setting parents typically have no recurring interactions at this age – most of them no longer take their children to school, and parent-teacher meetings are rather infrequent in Brazilian public schools. Having said that, both potential contamination and students' peer effects are expected to bias our estimates towards not detecting differences across the variations in the campaign parameters. The research design is outlined in Table 1. Assignment to each treatment branch across the four experiments is cross-randomized, except in what comes to the control group, since those receiving no messages cannot be assigned to other campaign parameters.

Experiment 1 randomly assigns the frequency at which SMS messages are delivered. The control group receives no messages. The decision to assign 1/3 of the sample to this group is based on maximing power for Experiments 2 through 4. Treatment 1A (1/3 of the remaining subject pool) receives 1 message a week, a suggestion of activity for parents to do along with their children (delivered on Wednesday). Treatment 1B (also 1/3 of the remaining subject pool) receives 2 messages a week, a 'fact' with information about how an activity is linked to children's development (delivered on Monday) and a suggestion of activity for parents to do along with their children (delivered on Wednesday). Treatment 1C (also 1/3 of the remaining subject pool) receives 3 messages a week, a 'fact' with information about how an activity is linked to children's development (delivered on Monday), a suggestion of activity for parents to do along with their children (delivered on Wednesday), and a reinforcement of that activity, which tries to make it a habit (delivered on Friday).

Experiment 2 randomly assigns the time of the day at which messages are delivered. Treatment 2A (1/3 of the sample) receives messages at the evening (7pm), while Treatment 2B (also 1/3 of the sample) receives messages at the afternoon (noon).

Experiment 3 randomly assigns the consistency of SMS delivery. Treatment 3A (1/3 of the sample) receives messages at always the same time of the day (either noon or 7pm), while Treatment 3B (also 1/3 of the sample) receives messages at alternating times (at the scheduled time, 1 hour before and 1 hour after, following a 3-week cycle).

Last, Experiment 4 randomly assigns whether content is interactive. Treatment 3A (1/3 of the sample) receives a follow-up message (delivered on Thursday) asking whether the parent complied with the activity suggested the day before – to which parents can reply 'yes' or 'no' –, while Treatment 3B does not receive follow-up messages.

Experiment 1 – Frequency		
Group	**Definition**	**Sample size**
Control	0 messages / week	833
Treatment 1A	1 messages / week	556
Treatment 1B	2 messages / week	556
Treatment 1C	3 messages / week	555
Experiment 2 –Time of the day		
Group	**Definition**	**Sample size**
Control	N/A	833
Treatment 2A	Evening	834
Treatment 2B	Afternoon	833
Experiment 3 – Consistency		
Group	**Definition**	**Sample size**
Control	N/A	833
Treatment 3A	Constant	834
Treatment 3B	Varying	833
Experiment 4 – Interactivity		
Group	**Definition**	**Sample size**
Control	N/A	833
Treatment 4A	Interactive	834
Treatment 4B	Passive	833

Figure D.5: Research Design

D.3.3 Outcomes

We will conduct surveys through automated voice calls (Interactive Voice Response, IVR) at the end of the intervention to collect self-reported parenting practices and parents' views about their children.

One interesting lesson from our 2015 pilot is that, at least among 6th grades, about 1/3 of participating families' children also have cell phones, which lead us to collect student's cell phones for this study. We were able to collect cell phones for 50% of the students. Among these families, we will rack students' views about themselves, their parents and their teachers, and teachers' views about their students and their students' parents.

At the end of the intervention, the São Paulo Education Secretariat will provide data on student attendance and grades in 2016 (per quarter), and enrollment in 2017. Moreover, the Secretary of Education of São Paulo implements annually a standardized test to all schools in the state of São Paulo, SARESP (System of School Performance Evaluation of the State of São Paulo). All students in grades 1st, 3rd, 5th, 7th, 9th of primary school and the 3rd (final) year of high school are tested on their knowledge of Mathematics and Portuguese.

D.3.4 Timeline & Milestones

#	Milestone	Target Start Date	Target End Date
1.	Meeting with the Regional Board of Education Directors and the São Paulo Secretariat of Education to explain the project	Apr-14	Apr-14
2.	Regional Board of Education Directors meet with their schools principals to explain the project	Apr-18	Apr-27
3.	Schools register to participate in the program (trough an online form)	Apr-18	Apr-27
4.	Meeting with Education Directors and school principals in each of the 5 Regional Board of Education head office to explain the project and distribute the enrollment material	May-9	May-17
5.	Schools organize meeting with parents to explain the project and obtain their cellphone and consent	May-10	May-30
6.	Teacher uploads parental enrollment information through secure website	May-10	June-2
7.	Randomization	June-3	June-5
9.	Baseline phone survey implementation	June-6	June-13
10.	SMS content and nudges begin	June-14	-
11.	End-line phone surveys implementation	Dez-12	Dez-20
12.	SMS content and nudges end	Dez-20	-
13.	Impact Evaluation	Jan-30	Mar-31

Figure D.6: Timeline & Milestones

D.4 Survey instruments

D.4.1 Baseline Survey: Parents

"Thank you for participating in the research about parental engagement in student education! Answer the following questions by dialing on your cellphone. This survey is anonymous and free and if you answer all the questions you will receive 5 reais in cellphone credit in your pre-paid phone. You will answer only 11 questions!"

1. How many times does your child usually miss Math class in a one-month period? If none, press 1; if between 1 and 3 times, press 2; if between 4 and 6 times, press 3; if more than 6 times, press 4.

2. How many times is your child usually late to Math class in a one-month period? If none, press 1; if between 1 and 3 times, press 2; if between 4 and 6 times, press 3; if more than 6 times, press 4.

3. How many times does your child usually hand in Math assignments on time in a one-month period? If none, press 1; if between 1 and 3 times, press 2; if between 4 and 6 times, press 3; if more than 6 times, press 4.

4. How does your child usually behave in Math class? If very well, press 1; if well, press 2; if appropriately, press 3; if inappropriately, press 4.

5. Usually, how is your child's performance in Math class? If very good, press 1; if good, press 2; if adequate, press 3; if inadequate, press 4.

If your child's school initiated a program to inform parents and guardians about the school life of students, what would be your interest in receiving information about each of the following?

6. About the number of Math classes missed? Press 1 if you would be very interested, press 2 if you would be somewhat interested; press 3 if you would not be interested.

7. About the number of Math classes he/she was late for? Press 1 if you would be very interested, press 2 if you would be somewhat interested; press 3 if you would not be interested.

8. About the number of Math assignments he/she failed to hand on time? Press 1 if you would be very interested, press 2 if you would be somewhat interested; press 3 if you would not be interested.

9. About his/her behavior in Math class? Press 1 if you would be very interested, press 2 if you would be somewhat interested; press 3 if you would not be interested.

10. About his/her performance in Math class? Press 1 if you would be very interested, press 2 if you would be somewhat interested; press 3 if you would not be interested.

11. About activities you could perform at home with your child, to increase parental engagement? Press 1 if you would be very interested, press 2 if you would be somewhat interested; press 3 if you would not be interested.

Final message: "Thank you! Your air credit will be delivered within 7 days!”

D.4.2 End-line Survey: Parents

"Thank you for participating in SMS ESCOLA research about parental engagement in student education! Answer the following questions by dialing on your cellphone. This survey is anonymous and free and if you answer all the questions you will receive 5 reais in cellphone credit in your pre-paid phone!”

1. Did you receive weekly text messages from the school in the last six-months? If yes, press 1; if no, press 2.

If the answer is 1 (yes) – 2A & 3A:

2.A. Did you talk with the professor or other parents about the text messages you received from the school? If yes, press 1; if no, press 2.

3.A. Did you show the text messages to your child? If yes, press 1; if no, press 2.

If the answer is 2 (no) – 2B & 3B):

2.B. Did you hear that some of the parents were receiving text messages from the school or did you talk with the professors or other parents about the text messages? If yes, press 1; if no, press 2.

3.B. Did any parent show you the content of these text messages? If yes, press 1; if no, press 2.

4A. Now answer how often you do each of the following things. Help your child with schoolwork or homework? If never, press 1; if almost never, press 2; if sometimes, press 3; if always or almost always, press 4.

4B. Now answer how often you do each of the following things. Help your child to organize school material, such as books, notebooks and backpack? If never, press

1; if almost never, press 2; if sometimes, press 3; if always or almost always, press 4.

5A. Incentivize your child to not miss school? If never, press 1; if almost never, press 2; if sometimes, press 3; if always or almost always, press 4.

5B. Incentivize your child to not be late for school? If never, press 1; if almost never, press 2; if sometimes, press 3; if always or almost always, press 4.

6A. Talk to your child about his day in school? If never, press 1; if almost never, press 2; if sometimes, press 3; if always or almost always, press 4.

6B. Talk to your child about his classes? If never, press 1; if almost never, press 2; if sometimes, press 3; if always or almost always, press 4.

7A. Go to school parent meetings? If never, press 1; if almost never, press 2; if sometimes, press 3; if always or almost always, press 4.

7B. Talk to your child's teachers, for any reason. If never, press 1; if almost never, press 2; if sometimes, press 3; if always or almost always, press 4.

8. Thinking about your child's Math class, answer each of the following questions with your best guess. On average, how many Math classes did your child miss in the last quarter? If none, press 0; if less than 3, press 1; if between 3 and 5, press 2; if between 6 and 8, press 3; if more than 8, press 5.

9. What was your child's Math grade in the last quarter? Press a number between 0 and 10 and then pound.

10. Now thinking about your child's Portuguese class, answer each of the following questions with your best guess. On average, how many Portuguese classes did your child miss in the last quarter? If none, press 0; if less than 3, press 1; if between 3 and 5, press 2; if between 6 and 8, press 3; if more than 8, press 5.

11. What was your child's Portuguese grade in the last quarter? Press a number between 0 and 10 and then pound.

12. If a professor suggests a list of books for your child to read during vacations,

would you buy it? If you would buy it if they were required, press 1; if you would buy it even if they were optional, press 2; or if you would not buy it, press 3.

13. Answer if you agree or disagree with the following statements. "Experiencing failure debilitates my performance and productivity." If you strongly disagree, press 1; if you disagree, press 2; if you somewhat disagree, press 3; if you somewhat agree, press 4; if you agree, press 5; or if you strongly agree, press 6.

14. "Experiencing failure inhibits my learning and growth." If you strongly disagree, press 1; if you disagree, press 2; if you somewhat disagree, press 3; if you somewhat agree, press 4; if you agree, press 5; or if you strongly agree, press 6.

15. "Experiencing failure enhances my performance and productivity." If you strongly disagree, press 1; if you disagree, press 2; if you somewhat disagree, press 3; if you somewhat agree, press 4; if you agree, press 5; or if you strongly agree, press 6.

16. "The effects of failure are negative and should be avoided." If you strongly disagree, press 1; if you disagree, press 2; if you somewhat disagree, press 3; if you somewhat agree, press 4; if you agree, press 5; or if you strongly agree, press 6.

Final message: "Thank you! Your air credit will be delivered within 7 days, and you will receive a text message confirmation when it is available!"

D.4.3 End-line Survey: Students

SCHOOL: ARMANDO COELHO – COD: 1512 **CENTRO SUL**

Check here, if the name printed above is NOT yours, notify the administrator immediately ☐

Dear student,
This questionnaire should be answered with great care. We want to know more about families' engagement habits and your study habits. You can be sure that your family, your colleagues and your school teachers will not know any of your answers, so please answer honestly. Your answers will contribute to a better future for you and other young people in our State. If you do not understand a question, please call the administrator, but do not stop answering! There are no right or wrong answers! Thank you!

1.	**Answer how often your parents or guardians:**	**Never**	**Almost Never**	**Someti mes**	**Almost always or always**
a.	Help you with homework or schoolwork.	1	2	3	4
b.	Ask if you did you homework or schoolwork	1	2	3	4
c.	Help you to organize the school material, such as books, notebooks and backpack.	1	2	3	4
d.	Incentivize you to not miss school.	1	2	3	4
e.	Incentivize you to not be late for school.	1	2	3	4
f.	Ask you about your grades in tests, activities and classes.	1	2	3	4
g.	Incentivize you to study.	1	2	3	4
h.	Incentivize you to read.	1	2	3	4
i.	Ask you about your day in school.	1	2	3	4
j.	Ask you about your classes.	1	2	3	4
k.	Go to school parent meetings.	1	2	3	4
l.	Talk to your teachers.	1	2	3	4

2.	**Answer if you agree or disagree with each of the following statements:**	**Strongly disagree**	**Disagree**	**Somewhat disagree**	**Somewhat agree**	**Agree**	**Strongly agree**
a.	How smart you are is something that you can't change very much.	1	2	3	4	5	6
b.	You can learn new things, but you can't change how smart you really are.	1	2	3	4	5	6
c.	You can always change how smart you are.	1	2	3	4	5	6
d.	You have a certain degree of intelligence and you can't really do much to change it.	1	2	3	4	5	6
e.	My parents ask me how my work in school compares with the work of other students in my class.	1	2	3	4	5	6
f.	My parents would be pleased if I could show that school is easy for me.	1	2	3	4	5	6
g.	My parents would like it if I could show that I'm smarter than other students in my class.	1	2	3	4	5	6
h.	My parents don't like it when I make mistakes in school.	1	2	3	4	5	6
i.	My parents want me to understand school concepts, not just do the work.	1	2	3	4	5	6
j.	My parents think how hard I work in school is more important than the grades I get.	1	2	3	4	5	6
k.	My parents would like me to do hard work, even if I make mistakes.	1	2	3	4	5	6
l.	My parents want me to understand homework problems, not just memorize how to do them.	1	2	3	4	5	6

3.	Answer if you agree or disagree with each of the following statements: (answer thinking about how you felt recently. There is no right or wrong answer)	Strongly agree	Agree	Disagree	Strongly disagree
a.	On the whole, I am satisfied with myself.	1	2	3	4
b.	At times, I think I am no good at all.	1	2	3	4
c.	I feel that I have a number of good qualities.	1	2	3	4
d.	I am able to do things as well as most other people.	1	2	3	4
e.	I feel I do not have much to be proud of.	1	2	3	4
f.	I feel useless at times.	1	2	3	4
g.	Sometimes I feel that I'm a worthless person.	1	2	3	4
h.	I wish I could have more respect for myself.	1	2	3	4
i.	All in all, I am inclined to feel that I am a failure.	1	2	3	4
j.	I have a positive attitude toward myself.	1	2	3	4

4.	Answer how you feel for each of the statements below. Do you like that your parents or guardians:	I like it a lot	I like it a little	I don't like it	I hate it
a.	Help you with homework or schoolwork?	1	2	3	4
b.	Ask you about your day in school?	1	2	3	4
c.	Help you to organize school material, such as books, notebooks and backpack?	1	2	3	4
d.	Ask you about your grades on tests, on assignments and in classes?	1	2	3	4
e.	Go to school parent meetings?	1	2	3	4
f.	Incentivize you to not miss school?	1	2	3	4
g.	Incentivize you to not be late for school?	1	2	3	4

5.	Indicate how much you identify with each of the statements below (there are no right or wrong answers)	Very much like me	Mostly like me	Somewh at like me	Not much like me	Not like me at all
a.	New ideas and projects sometimes distract me from previous ones.	1	2	3	4	5
b.	Setbacks (delays and obstacles) don't discourage me.	1	2	3	4	5
c.	I have been obsessed with a certain idea or project for a short time but later lost interest.	1	2	3	4	5
d.	I am a hard worker.	1	2	3	4	5
e.	I often set a goal but later choose to pursue (follow) a different one.	1	2	3	4	5
f.	I have difficulty maintaining (keeping) my focus on projects that take more than a few months to complete.	1	2	3	4	5
g.	I finish whatever I begin.	1	2	3	4	5
h.	I'm hard working and careful.	1	2	3	4	5

6.	In general, indicate how much time per day you spend in each of the following activities:	I don't do this activity	15 minutes	30 minutes	1 hour	2 hours	More then 2 hours
a.	Study at home, on weekdays.	1	2	3	4	5	6
b.	Study at home, on weekends.	1	2	3	4	5	6
c.	Study at home, the day before a test.	1	2	3	4	5	6
d.	Watch TV.	1	2	3	4	5	6
e.	Read a book.	1	2	3	4	5	6
f.	Read the newspaper.	1	2	3	4	5	6
g.	Read magazines.	1	2	3	4	5	6
h.	On the internet or social media.	1	2	3	4	5	6
i.	Help with housework in YOUR HOUSE (clean the house, laundry, dishes, take care of children…).	1	2	3	4	5	6

7. Answer if you agree or disagree with each of the following statements:	Strongly disagree	Disagree	Agree	Strongly agree
a. I like the **MATH** class.	1	2	3	4
b. I like the **PORTUGUESE** class.	1	2	3	4
Your **MATH** teacher…				
c. Doesn't like that students are late for class.	1	2	3	4
d. Doesn't like that students miss class.	1	2	3	4
e. Is strict about the delivery of homework or schoolwork.	1	2	3	4
f. Is rigorous in test grading.	1	2	3	4
g. Is rigorous in report card grading.	1	2	3	4
Your **PORTUGUESE** teacher…				
k. Doesn't like that students are late for class.	1	2	3	4
l. Doesn't like that students miss class.	1	2	3	4
m. Is strict about the delivery of homework or schoolwork.	1	2	3	4
n. Is rigorous in test grading.	1	2	3	4
o. Is rigorous in report card grading.	1	2	3	4

8. Answer from 1 to 4 how important each of the items below are to you (there are no right or wrong answers):	Not important at all	A little bit important	Important	Extremely important
a. Doing the homework or schoolwork.	1	2	3	4
b. Studying for tests.	1	2	3	4
c. Having a good performance on tests.	1	2	3	4
d. Getting a good grade on the report card.	1	2	3	4
e. Not missing class.	1	2	3	4
f. Not being late for class.	1	2	3	4
g. Finishing elementary school.	1	2	3	4
h. Finishing high school.	1	2	3	4
i. Going to college.	1	2	3	4
j. Getting a good job.	1	2	3	4

9. If it were **only up to you,** up to which level you would study?	
a. I would have already dropped out of school	1
b. Until finishing the 9^o grade.	2
c. Until finishing high school.	3
d. Until, at least, finishing college.	4

10. If it were **only up to your parents,** up to which level you would study?	
a. I would have already dropped out of school.	1
b. Until finishing the 9^o grade.	2
c. Until finishing high school.	3
d. Until, at least, finishing college.	4

11. And what do you think will really happen?	
a. I will drop out of school before finishing the 9^o grade.	1
b. I will finish the 9^o grade of elementary school.	2
c. I will finish high school.	3
d. I will finish college.	4

12. Answer yes or no for each of the questions below:	Yes	No
a. Did you hear that some parents were receiving text messages from your school?	1	2
b. Do you think your parents received text messages from your school?	1	2

13. **Answer how confident you are for each of the statements below:**		**Not at all confident**	**Slightly confident**	**Somewhat confident**	**Quite confident**	**Extremely confident**
a.	How confident are you that you can complete all the work that is assigned in your classes?	1	2	3	4	5
b.	When complicated ideas are presented in class, how confident are you that you can understand them?	1	2	3	4	5
c.	How confident are you that you can learn all the material presented in your classes?	1	2	3	4	5
d.	How confident are you that you can do the hardest work that is assigned in your classes?	1	2	3	4	5
e.	How confident are you that you will remember what you learned in your current classes, next year?	1	2	3	4	5

14.	**To answer the questions below, think of how you compare to most people. For the following statements, please indicate how often you did the following during the past school year (there are no wrong or right answers):**	**Almost never**	**About once a month**	**About 2-3 times a month**	**About once a week**	**At least once a day**
a.	I forgot something I needed for class.	1	2	3	4	5
b.	I interrupted other students while they were talking.	1	2	3	4	5
c.	I said something rude.	1	2	3	4	5
d.	I couldn't find something because my desk, locker, or bedroom was messy.	1	2	3	4	5
e.	I lost my temper at home or at school.	1	2	3	4	5
f.	I did not remember what my teacher told me to do.	1	2	3	4	5
g.	My mind wandered when I should have been listening.	1	2	3	4	5
h.	I talked back to my teacher or parent when I was upset.	1	2	3	4	5

15.	**Answer from 1 to 6 for the following questions, where 1 is a little and 6 is a lot.**	**1**	**2**	**3**	**4**	**5**	**6**
	How much do you think that your **MATH** teacher takes each of the following items into account when defining your report card grade?						
a.	Grades on tests.	**1**	**2**	**3**	**4**	**5**	**6**
b.	Grades on homework, schoolwork and activities.	**1**	**2**	**3**	**4**	**5**	**6**
c.	Classroom participation.	**1**	**2**	**3**	**4**	**5**	**6**
d.	Delivery of homework on time.	**1**	**2**	**3**	**4**	**5**	**6**
e.	Absences.	**1**	**2**	**3**	**4**	**5**	**6**
f.	Lateness.	**1**	**2**	**3**	**4**	**5**	**6**
g.	If you disturbed your peers.	**1**	**2**	**3**	**4**	**5**	**6**
h.	If you talked about non-class related subjects during class.	**1**	**2**	**3**	**4**	**5**	**6**
i.	Other characteristics of yours.	**1**	**2**	**3**	**4**	**5**	**6**
	How much do you think that your **PORTUGUESE** teacher takes each of the following items in account when defining your report card grade?						
j.	Grades on tests.	**1**	**2**	**3**	**4**	**5**	**6**
k.	Grades on homework, schoolwork and activities.	**1**	**2**	**3**	**4**	**5**	**6**
l.	Classroom participation.	**1**	**2**	**3**	**4**	**5**	**6**
m.	Delivery of homework on time.	**1**	**2**	**3**	**4**	**5**	**6**
n.	Absences.	**1**	**2**	**3**	**4**	**5**	**6**
o.	Lateness.	**1**	**2**	**3**	**4**	**5**	**6**
p.	If you disturbed your peers.	**1**	**2**	**3**	**4**	**5**	**6**
q.	If you talked about non-class related subjects during class.	**1**	**2**	**3**	**4**	**5**	**6**
r.	Other characteristics of yours.	**1**	**2**	**3**	**4**	**5**	**6**